

VWPE
No. **2**



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Vienna 2014
ISSN 2311-231X

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WHERE THEORY WORK IS DONE IN THE PRODUCTION OF CONTEMPORARY ANTHROPOLOGICAL RESEARCH – AND HOW IT MIGHT BE MADE ACCESSIBLE IN INTERMEDIATE FORMS OF RECEPTION IN ETHNOGRAPHY¹

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Abstract

Through suggesting how theoretical/concept work develops in the various ways that research programmes manifest themselves in contemporary social/cultural anthropology, this paper argues that theory work is an integral part of ethnographic method and the fieldwork process. It is more about work undertaken inside of fieldwork than a 'professional society' activity that surrounds or results from fieldwork at a distance. This is in turn an effect of the increasingly collaborative and mobile strategies of fieldwork, which move among many micro-publics. Therefore, this paper encourages the creation of more open forms and new media for the reception and construction of theory and analytic work instead of the previous lonely confines of fieldwork communication, and reflects on this issue as a current problem of method. Fieldwork requires patience, but theory work requires more churn inside it.

Introduction

This Working Paper makes an argument in dialogue with a provocation by James Faubion regarding the absence of classic programmatic framings in anthropology toward which individual research projects might be developed and oriented – and in terms of which theory, distinctive of a discipline, might be thought to emerge conventionally. Instead he offers an intriguing typology of the various ways theory ‘works’ in the diversity of anthropological research today. I address this typology in some detail in order to arrive at his alternative to the ‘diagnostic’ which is perhaps the most ambiguous notion of the programmatic from a disciplinary perspective since its cogency so much depends on the concept work that the ethnographer is able to do with his or her subjects, interlocutors and diverse partners in contemporary fieldwork situations.

¹ This is the text of a lecture that I presented at the University of Vienna in June 2014. It was originally prepared as a chapter for a forthcoming volume on the nature of theory in contemporary anthropological research (*Theory is More Than It Used To Be*, edited by Dominic Boyer, James Faubion and George Marcus, Cornell University Press).

From such research, any ‘theory’ of a second order or derived nature that anthropologists themselves are able to develop in their own conclusive discussions and debates seems to me to depend so much more than ever before on prior or first order contexts of theory or concept work in field research.

This concept or theory work in the field (or ‘in the wild’ in Michel Callon’s terms) does not just precede the production of ethnography as texts as a mode of communication to colleagues but surrounds this professional context, on all sides, so to speak. Professional discussions of research in anthropology seem increasingly to define themselves in the middle of projects without making the actual dimensions of research clear enough. Other forms are needed.

How, then, to begin to give this theory work of the field presence in the classic forms and formats of ethnography, how, even, to supersede these classic forms? This is really the methodological issue of core interest to me, not only in this paper, but more generally in the evolution of my thinking from *Writing Culture* and *Anthropology of Cultural Critique* of the 1980s, through the emergence of multi-sited conditions of ethnographic research of the 1990s, forward. Collaboration, modes of participation, new technologies of communication – these are the keywords (if not the buzzwords!) that suggest renovations in the forms in which ethnography is authored, presented to subjects, interested publics and for the authoritative discussion of scholars building knowledge through standards. It is these standards that are changing – or perhaps need methodological reformulation or restatement in changing ecologies of disciplines that anthropology grew up in relation to, and new ones, or reformulated ones, that it perhaps is meeting for the first time.

What is unique to ethnography, I believe, is the building of its ideas – and its concepts and theories – from those of its subjects and found partners in fieldwork. In this sense, theory is a primary form of data – not its result – but as such it must be located in the sites and situations of fieldwork. This requires (dialogic) forms of reception that the anthropologist has to make, stage, design and incorporate into classic notions of fieldwork and the production of ethnographic texts from them. How all of this can be staged, mediated and circulated in a ‘standard’ project of contemporary anthropological research is a matter of keen interest to me as I have emphasised in recent writings, tried to experiment with in a modest Center for Ethnography at my university and work into my contribution to the volume, which constitutes the following text that I offer to readers of the Vienna Working Papers in Ethnography.

Types of the programmatic condition of theory work: the referential, the model-theoretic, the tendential, and the diagnostic

James Faubion's crucial move in the concluding pages of his *An Anthropology of Ethics* (Faubion 2011: 268-76) is to link the question and standing of theory in the practice of anthropological research today to theory's relation to 'the programmatic', which I take to be its systematic reception and development in a collectivity, usually conceived of as a community of interested scholars, experts and professionals. In short, theoretical work is significant to the extent that someone else makes something systematically of it.² When we observe the work of theory in anthropology, in whatever discursive form or genre (for example, typically as it provides a frame and structure for both argument and narration in much contemporary ethnographic writing), we should ask: in relation to what 'generative' project, or to what, as Faubion terms it, 'technology of disciplined question-formation' does it speak? Without some systematic connection to a notion of the 'programmatic', the substance of theory making, in its manifestation – concept work – in mainly ethnographic research/fieldwork-based inquiry in anthropology, is much reduced in value as a foundational category for evoking what holds a discipline together.

The substance and significance of an interest in theory in any particular research project are thus linked to its reception and the contribution that it makes to the collective thinking of a community, a public or publics. The question today is whether theory as it applies to individual research in anthropology can have such robust programmatic significance, especially with reference to a disciplinary or scholarly professional community, and if this is in doubt (as I believe it is, and as it certainly is for Faubion as well), then is there an alternative and distinctive sense or process of inquiry, perhaps specific to anthropology in its modern tradition as a field science developing through ethnography, in which the work of theory in the production of research can be considered programmatic? Faubion suggests that there is by engaging in an acute typological exercise.

The first sense of the programmatic that Faubion evokes – the referential – is the most mythic (at least for the positivist-minded social sciences), thoroughly deconstructed (at least

² To foreshadow a bit: my point will be that anthropology cannot simply rely on its professional community in reception only to do or shape this programmatic work. Somehow, reception must be blended granularly into the classic practice and expectation of fieldwork research and method. For me, in recent years, this operation means linking or extending the intellectual functions of the professional community of anthropologists into the field of inquiry in research projects themselves. What forms of new assemblages of methods and research practices this would require, and what their feasibility and political problems would be in implementation, has been a primary and growing interest of mine over the past decade (Marcus 2012).

in anthropology), but still powerfully influential one: the status of theory work dependent on its relation to empirical methods of some demonstrable reliability. These are theories that grow and are changed by studies, controlled experiments, reproducible investigations and growing accumulations of variable-defined, factual materials. In its central tendencies, anthropology has not tried to literally produce this sort of project for a very long time (excepting perhaps some subspecialties like latter day sociobiology or semiotics and linguistic anthropology, but even these, though often formal in concept and deeply empiricist, are tolerably unruly in their research projects). There are indeed legacy form comparative projects in the history of anthropology to take up again, but it is very unclear in the present era of data science if they would ever be taken up referentially again, in which the value of the ethnographic study could be theorised in terms of a research programme in the positivist ethos (as isolating limited variables and proving a hypothesis with some logic of replicability).

Faubion then discusses two derived types of the relation of theory work to the condition of the programmatic. Both might be uncharitably understood as hedges, as what theoretical practice for individual projects can be when the conditions for operating in the realm of the referential programmatic are absent. These are the model-theoretic and the tendential. The former is not practiced much, at least in social/cultural anthropology, where theory work in reports, essays or monographs would be a formal, abstract exercise of constructing models in which to organise and narrate the data of an ethnographic report. It gestures toward comparison, and appeals to rigorous, factorial thinking, but mostly remains the distinctive 'mark' or signature of a particular researcher. The model-theoretic, which was the common form of producing work when I came of age as an anthropologist (during the late 1960s through the 1970s), sets up thinking for the programmatic without following through.

What Faubion terms the tendential is the most common form that theory work takes in contemporary anthropological research. It gestures even more remotely to the conventional idea of the programmatic. In its practice, it is highly variable in the way that it uses theory to set ethnographic work within a form of argument, narration and description sensitive to emplacement in a particular location, condition of fieldwork and historical or contemporary context. At best, the tendential use of theory does a lot of imaginative, creative work in the making of argument out of the exploration of a case or problem found in fieldwork. The tendential mode of theory making creates concepts close to materials, develops an analytics (yet not approaching a model) around it and stands for an ethnographic work discursively as it travels in recognition and discussion. However, the tendential is very far indeed from a

referential notion of the programmatic. It gestures toward such a classic notion of the programmatic but is farthest from it. The diverse efforts, of varying longevity, to build topical research programmes in anthropology today mostly circulate arguments, connections and associations through the power of theoretical usages developed in the tendential mode within the frame of exemplary presentations of ethnographic material. As such, the tendential mode of theory work is a weak source to establish a condition of the programmatic that sustains disciplined, systematic inquiry.

Still, it should be noted that for the tendential to thrive as widespread disciplinary practice, to be effective in the pedagogy of apprentice anthropologists and to serve, stimulate and reward the diversity and variety of research initiatives – of varying ephemerality – that characterise anthropology today, a seat of programmatic knowledge about traditions, genealogies, currents and fashions of theory, in the mode of scholarly projects and commitments associated with intellectual historians and philosophers, is indeed necessary to inform the practice of the tendential and keep it going. But this specific and necessary programmatic function exists, I would argue, alongside, or laterally, to most research projects. It serves to fire up the engine of the tendential so to speak.

So, there are deep and close readers of theoretical traditions and innovations (like, for example, Faubion, Boyer and Glaeser, among others) who follow them systematically for their own interest and within the frame of evolving questions that anthropology asks. This kind of systematic theory interest on the part of some anthropologists helps to give theory work a powerful standing in the production of individual research projects, especially in graduate school curricula, but it does not serve to define the programmatic in its classic referential sense, collectively pursued by a discipline. This offside exercise of the programmatic when it comes to the intellectual history of theory is thus of immense importance in giving theoretical competence to students to produce the tendential in their projects, but it does not make them programmatic in any of the senses that Faubion evokes as defining the significance of theory in contemporary anthropological research.³

³ I offer an anecdote here about how a certain programmatic influence of theory/theories in the training of anthropological graduate students manifests itself as a literal, perhaps eventually productive (!), contradiction in the process of making anthropologists. If I were producing this anecdote as a short essay, I would entitle it 'Beautiful Theories and the Precipice of Fieldwork'. 'Beautiful Theories' refers to a brilliant book by Elizabeth Bruss (1982), deserving to be remembered, which questioned the programmatic authority of theories and theoretical discourse that were very much then in fashion in shaping the doing and discourse of literary criticism. In anthropological graduate training today, the centrality of 'the theory course' and the prominence of theoretical exegesis in almost all other courses (along with the absence of rigorous field methods courses) communicate the importance and authority of theory as a programmatic construction or

Finally, Faubion explores a fourth type or potential source of the programmatic condition of theory work that differs in kind from the other three in not depending on a derivation from, or relation to, the referential type. He labels it ‘the diagnostic’ (perhaps with an inflection from where the influence of Foucault has led the application of anthropological thought in recent decades), but also folds into it the more commonly known and practiced interpretive tradition of theory in anthropology, associated most famously and recently with Clifford Geertz. What is bold in Faubion’s inclusion of this otherwise familiar, even standard, type of theory work in his typology is his suggestion that such work can be strongly programmatic as well – that it can be more than the theory work in the tendential pose, and that it can have broader, more intensive and lasting reception than is otherwise supposed or expected.

This move of expecting a ‘disciplined technology of question-formation’ in such projects of ethnographic research, and a different mode of reception for them both within the realm of the ‘field’ of fieldwork activity and within the realm of their professional, scholarly community consideration as well, suggests a different ambition (modality) for theory work in contemporary ethnographic research on which I want to focus.

pillar in the otherwise diffuse curricula of training in many departments. I learn this repeatedly in attending the unfolding mini dramas of oral examinations as they are conducted in my present department. The department to which I moved from Rice – anthropology at the University of California, Irvine – schedules oral examinations at the conclusion of two or three years of coursework before the student leaves to do a year or more of fieldwork. The basis of the orals are elaborate documents consisting of a research proposal and two synthetic papers on topical and theoretical subjects that define the research. This moment of oral examination is pivotal since upon return and writing up, our department has no oral defence of the dissertation. The returned student works mainly with his or her supervisor and upon committee approval of the dissertation exits the programme. What I have noticed in our oral examinations is almost a ritual ‘undoing’ of the theoretical language of the student on her way to the field in favour of a much more pragmatic prompting to speak literally of what one anticipates doing in fieldwork: a kind of compensatory focus on operationalising the thinking that has gone into the project that is likely to be wound in theoretical circles, rationales and key concepts. Recently, I personally found the pulling back on the theory elegance of a particular student, especially gifted in this way, in favour of prodding him about how he anticipates collecting data in a literal field of neighbourhoods and marketplaces, to be a bit harsh, but also supremely evocative of widespread contradictions in pedagogical practice. In anthropology, mastering theory (and its beauty!) is extremely important in training and evaluating students – as a mode for demonstrating intellectual capacity – but it threatens as well the production of fieldwork – notoriously, as a method, not taught as theory is, in most anthropology departments. This divide between the programmatics of theory and the lack of such programmatics in the technology of question-asking before or after fieldwork is something that I regularly see students being caught in – especially those with an affinity for theory – at least temporarily as they move between the seminar room and fieldwork. And I am regularly reminded of this by the ritual of the particular oral examination that we schedule in our department. Interestingly, orals that come at the end, on the submission of the dissertation, have a different quality to the way they manage the theory-fieldwork divide (for a memorable, personal anecdotal example of this see Marcus 2010: 38-40). By then the tendential crafting of theory specific to a dissertation’s analytics is clearly in play, and the programmatics of theory substituting for the programmatics of research is less dramatic than when the orals occurs before fieldwork.

Faubion's delineation of this fourth type folds the question of the possibility of programmatic theory work into questions of method, but it is perhaps unhelpful to use this category, since 'method' is traditionally understood in distinction to 'theory' with some version of the referentially programmatic in mind. What Faubion instead suggests is that the strength of theory work in contemporary ethnographic investigations is in the possibility of producing sustained and diverse reception for them in their production. How theory travels in research as analytics is critical to forging the programmatic contexts of the remarkably diffuse and varied explorations of anthropological research today. What is lacking are the forms and means in research practice to establish a programmatic context for it, project by project.

The diagnostic, as a structure of research, incorporates diverse audiences and receptions for its pursuit (in analogy with theatre, it breaks 'the fourth wall' of the imagined, enclosed *mise-en-scène* of anthropological fieldwork taking place elsewhere), involving design strategies (and increasingly new digital-based technologies) of intervention, scenarios or events where different modalities of theory work blended into fieldwork can literally take place. (See, analogously and suggestively, Saunders 2008 for a deeply ethnographic account of disciplined 'diagnostic intrigue' and its elaborate production in the interpretation of CT scans). Theory work, or analytics, then, would not so much be the product of this research, blended into ethnographic writing and forms of argument, but its means. It needs accessible forms for reception that are performative, more raw and prototypical (Marcus 2013) in relation to ways that anthropologists have articulated theory in conventional reporting genres to the academy – the ethnographic based text or article. Such collective diagnostic thinking, that each project makes accessible, becomes programmatic in the field before it becomes programmatic in disciplinary discussion. However, at present, this programmatic of and within the field remains a potential, largely invisible, anecdotal or relegated to the now almost canonical reflexive framing of ethnographic writing.

Research results are thus what different communities of reception do with them, including the professional one that is incorporated as a second order, still perhaps authoritative community, which now must take reception itself as a theorised and empirical dimension of what is presented to it as research, as ethnographic cases.

This suggests that a lot more is going on of a theoretical nature in many projects than anthropology currently has a vocabulary for, or genres of access to (Fischer gestures in this direction through his evocation of 'third spaces', 2003, but in my case there is a more material

and literal concern with the design of such spaces as research practices within longstanding norms of fieldwork, Marcus 2012).

Clarifications of the changing forms of questions, thought of as analytics or theory in play, thus become the sustained programmatic endeavour of the myriad morphing diagnostic projects of research that abound in contemporary anthropology. The task of the programmatic played out in the field in professional reception and pedagogy is to systematically and recursively reprocess and rearticulate the questions on moving contextualised ground – to create a collective and sustained discourse and framework for organising such apparently unruly arrays of individual inquiries.⁴

This alternative notion of the programmatic is indeed a viable collective research programme, centred on a disciplinary tradition, in which explanation, in its classic ambition, as Faubion says, plays no role. The work of theory or analytics in ethnography is to pose continuously and empirically the questions as they are asked and articulated (of course, the posing of questions situationally is never just that, though it is a sustaining fiction of theory work). This view of the collective work of contemporary anthropology depends on the connectivity of the many research projects of diffuse curiosity that anthropologists are currently pursuing under the long established regulative professional norms of fieldwork

⁴ It is worth mentioning an evolution in the work of Michel Callon, one of the founding thinkers of what became influential actor-network theory. His work on markets and his argument that economic theory (the science of economics) does not describe or explain markets from an intellectual distance, but actually participates in their making has similarities to the movement of theory that informs ethnographic research into the immersed dynamics of fieldwork itself envisioned in this paper. To argue in this way, Callon depended on an attractive and significant notion of ‘theory in the wild’ as the kind of stuff that research is after and engages with. In an unpublished paper (2013), Andrew Barry traces a late 1990s /early 2000s distancing by both Bruno Latour and Callon from what they wrought and had become canonical actor-network theory. Latour’s movement away had complex trajectories (his movement into collaborations with artists and his concern with design are of interest as well to what is argued in this paper), but Callon’s development seems even more instructive for the direction in which this paper is moving. ‘Theory in the wild’ led Callon for a time to explore forms, which Andrew Barry describes as ‘hybrid forums’, composed of intellectuals, researchers, activists, social movement organisers and a more diffuse ‘public’. The suggestion is that they were efforts to practice, investigate and explore the construct of ‘theory in the wild’, to understand theory as social effects, to realise in another sphere an insight parallel to the one that theory did not analyse markets but made them. Interestingly, Barry remarked that what Callon in fact created was ‘social movement theory/practice for the age of experts.’ The implication is that these were less experiments in form in the immersion of investigation that sought to study ‘theory in the wild’ while participating in it, than a rationale or ideology for the merging of expertise – its forms of theoretical, conceptual thinking – with activist projects. Frankly, I know little more about this move in Callon’s work than what I learned from Barry’s very brief treatment. Still, without the activist or social movement associations, hybrid forums seem close to how interventionist forms are being conceived by those who are indeed experimenting with ‘theory in the wild’ as the core production of ethnographic research built around diagnoses circulating and morphing in circuits of reception that themselves become the data of scholarly research enterprises which programmatically organise their activity, independent of identification with activist or social movement goals.

research. This diagnostic programmatic still depends very much on the sort of theory work and interest that anthropologists have developed for themselves to shape and revitalise in recent decades their discipline's ethnographic form and argument as writing genres, but the forms that this theory work takes in fieldwork itself are very different – platforms, stages, designed interventions as necessary for falling into the liveliness, variety and collective curiosities that compose the empirical 'stuff' of ethnographic research projects so much in evidence over the past three decades.

The contemporary ethos or spirit of the way that theory informs inquiry is well captured in a remark by Jamer Hunt, who has made a distinctive career in teaching design methods informed by the same sources of theory that have now inspired several generations of ethnographic research in anthropology: "theory is a frame for anthropology intervening in arenas new to it" (Boyer/Faubion/Marcus forthcoming). Indeed, such a remark is a spur for anthropologists to ask questions both new and relevant to the contexts in which they work, and to enter distinctively into arenas of social and cultural life that have been already diversely represented (or even theorised) by actors on the ground, including other kinds of experts who have gotten there earlier. But what theory becomes in practice and product in the course of research – as fieldwork, or in Hunt's case, design projects – very much depends on its specific scenes of emergence in fieldwork, its recursive circulations, as the fieldworker himself moves his research to different sites and locations and, finally, to its second order disciplinary receptions that are both continuous with and self-defining from the theory work in and as fieldwork research.

In the following section, I want to outline briefly what forms – both digital and conventional – seem to be developing that make theory work both programmatic, in the sense of the diagnostic potential for it that Faubion evokes in his typology, and a more explicit core activity of contemporary fieldwork. But with which paradigm of classic fieldwork in mind? Is there any other one than the dominant Malinowskian exemplar, which despite the actual complexities of its conduct by Malinowski himself, has nonetheless shaped the regulative ideals that have guided field research in the Anglo-American tradition for generations in the direction of the referential type of the programmatic or one of its derived variations? Is there an alternative tradition of fieldwork to evoke in inventing the diagnostic type of the programmatic that Faubion has proposed?

Here I am inspired by an essay by Matti Bunzl (2004) who, under the influence of Foucault, revives and re-imagines an alternative Boasian paradigm of fieldwork that serves

much better than the dominant Malinowskian one as a frame to think through and propose forms of field research that accommodate at its core a project of collaborative theory work.⁵ Boasian fieldwork never became methodological doctrine, since it served most explicitly museum and linguistic science rather than social science, in which the reference of the programmatic was to the patient accumulation, classification and interpretation of collections in temporal frames of history and evolution. Yet in Bunzl's Foucauldian revival of its core mise-en-scene of anthropologist-subject relation, Boasian regulative norms of research are a better version to think with than dominant Malinowskian ones in proposing the development and accessibility of prototypical forms of the diagnostic type of programmatic potentiality in the conduct of contemporary fieldwork. In short, they imagine more open and dynamic dialogic conditions of inquiry that encourage the kinds of constructions and interventionist forms that create ethnographic data as theory work that punctuate and extend fieldwork as a programmatic exercise.

⁵ To give a sense of the difference of a Boasian paradigm of ethnographic/field research and its resonance for conceiving an alternative mise-en-scene of the contemporary, I quote Bunzl (2004: 438): "In Boas's fieldwork, a constitutive epistemological separation between ethnographer and native was absent...From Boas's perspective, neither anthropologist nor informant had immediate access to the history he hoped to reconstruct. In this situation, anthropologist and informant were united in a common epistemological position vis-a-vis the real Other of Boasian anthropology. That Other, ultimately, was the history that had generated the present condition, a history that eluded immediate description due to the absence of written records. In practice, this meant that Boas was just as happy if Native Americans generated ethnographic data themselves..." Then... (439) "...Insiders and outsiders were thus differentially positioned at the onset of the ethnographic project. What is central in the present context, however, is that Boasian ethnography not only did not rest on that distinction but was also designed to efface it. Guarding against alternating sounds, outsiders would produce the same ethnographic data as insiders; at the same time, the critical awareness of secondary explanations would guide insiders (and the anthropologists who derived their information from them) toward the actual histories of contemporary ethnic phenomena. Conceptually, this meant that insiders and outsiders would generate the same kind and attempt the same kinds of historical reconstructions..." While the particular kinds of knowledge quests have changed considerably in contemporary ethnographic research, the core Boasian relation in fieldwork inquiry, as evoked by Bunzl, is certainly close to the kinds of prototypes of collaborative thinking (Marcus 2013) upon which a diagnostic programmatic guiding contemporary ethnographic research would depend. Beyond this neo-Boasian mise-en-scene, fieldwork would progress in the contexts of circulations and receptions of the collaborative thinking that fieldwork produces. By a quite transitive relation between insider and outsider, in sum, the Boasian style of fieldwork gives the programmatic construction of contemporary anthropological research more to work with than what is imagined in the Malinowskian alternative.

Platforms, para-sites, installations, prototypes and the like... Toward the condition of programmatic theory work in ethnography as technologies of disciplined question-formation

The key innovation in method is that reception is folded into ethnographic strategies of inquiry, and thus requires making accessible to real and imagined publics for a project (who might also serve as its subjects) sustained and variable concept work alongside norms and expectations of investigation. New forms of enabling, applied theoretical discourse – as granular public discourse in formation – thus become the primary discourses-in-use of the actual conduct of inquiry and investigation. These forms become the latter's basis for its paths of circulation and reception which it continually incorporates until the projects stops, wears out or merges with another. At present such a research process can only be imagined in terms of a referentialist derived notion of the programmatic. It needs something else.

The professional scholarly and expert community which practically exercises authority and control over research (through controlling channels of funding, recognition, career prestige, determining what is to count as knowledge, etc.) finds itself, if not just another public in reception, then a second-order one, obliged to think through, making programmatic, a body of research, not just through what the researcher argues and offers, but through being presented with, and perhaps integrated into, a lively arena of primary and derived receptions that an ethnographic inquiry ignites and spreads merely by being in the field. Still, while this might be the reality of contemporary research – embedded in the conditions of the 'found field' however it is constructed as fieldwork for research – it is not how it is presented or received in traditional genres of ethnographic writing and reporting, and how these are received and rewarded professionally.

Forms are thus needed to make visible and accessible these levels and recursions of reception as fieldwork moves and develops. Intermediate expressions of knowledge, collaboratively and sometimes self-consciously composed and designed, are necessary to expose the kinds and forms of thinking that a contemporary fieldwork project assimilates. These expressions take the form of debates, proofs, experiments in the field – designed interventions –, more than single authored data recording and interpretation of fieldnotes, which are produced alongside them. Ultimately, these forms capture layered receptions for cumulative theory and concept work that has various mise-en-scenes of their own that run parallel to the more solitary theory work that comes from the traditional recording and

observing of classic fieldwork, and that eventually reach back to the academy and encourages it to think differently about how knowledge presented to it in raw and prototypical forms is certified, if it is, or continues on as a lively programmatic of question-asking that establishes novel sorts of relationship with long-standing modes of receiving ethnographic cases as the basis of theoretically defining anthropology.⁶ How does the academy participate in projects that it is pulled into by forms of promoting and developing theory work as fieldwork – not just in a location, but in its recursions and trajectories of movement among granularly defined publics of reception, of which the receiving authority of academic debate and assessment is one, and perhaps not the final one?

What stabilises knowledge making in such projects is precisely the crafting of forms of interventions where emerging claims, concepts and ideas can be received. Much depends on skills of curating and organising ethnographic forums that punctuate and move alongside the sort of classic probing by the individual's research in the traditional vision of how fieldwork proceeds. The problem is how to coordinate and relate the two. Over the past decade, and continuing, there are two modalities – the first, necessarily narrow and directed in its range; the second more diffuse and expansive – in which I have been interested, primarily from the vantage point of activities sponsored by the Center for Ethnography, founded in 2005, at the University of California, Irvine. The first experiments with the developing affordances of digital technology to create commons of organised research and participation; the second operates conventionally with the protocols of doing research, but experiments with incorporating forms and exemplars that recognise affinities with varieties of design thinking and methods, on the one hand, and, on the other, with arts such as theatre, museum curation, site-specific installation, conceptual and performance genres and the craft of filmmaking.

Of digital experiments, I have been most interested in those that structure the entire project through the construction, care and tending of platforms as the core apparatus of a

⁶ Certain earlier, ambitiously programmatic projects of anthropology entertained notions of the ethnographic field that extended from investigations in cultural settings elsewhere seamlessly to receptions in the context of everyday work in the university – I have in mind here the ethnosience/cognitive programme in U.S. anthropology of the 1950s and 1960s – but they developed within a referentialist commitment to the programmatic that constrained what could be data from the field. The question in the same experimental or interventionist ethos is what would work today. The disciplinary apparatus simply does not exist, at least in anthropology, as it does in the sciences, to produce such a referentialist programmatic. Of course, I do not believe this is circumstantial. Anthropology itself does not have the means or the apparent will and inspiration, in its own court, to produce a rigorously referentialist programmatic. What is open to it, not as a consolation, but as a strong alternative, is what I have been labelling, following Faubion, a programmatic of the diagnostic, built out of the persistently and creatively strong theory driven interpretive character of ethnographic research.

research project. While such a project may be minimally a modality of dynamic, continuous archiving or more the engine of a topically conceived research project that accumulates vast sources and types of data for it, for my interest here, the platform is a greatly enhanced ethnographic/fieldwork project of the ambition and variety that have been expressed in the most original, but conventional ethnographic writing over the past three decades. In a sense, platforms can literally develop the kinds of expanding receptions and engagements that ethnographies now can only imagine.

At the core of such platform experiments are novel theoretical imaginations and applied theory work that is challenged by the care and development of a variable and expanding commons of participation, without conventional boundary or reach (such as ‘a readership’ or the evaluation of peers in the academy). This is work engineering the spread of the sort of commons that Chris Kelty (2008) so attractively terms ‘recursive publics’ in conceiving contemporary online communities of mutual interest, participation and co-construction. A platform is thus a machine for knowledge making ‘in the wild’ that generates research, brings together its subjects and others as publics and creates the means of novelty in individual and collective participatory thinking with recognisable connections to the ‘diagnostic’ (tendential?) theory and concept work so prevalent in post 1980s ethnography. And in terms of the interest of this essay in the means for establishing the grounds of the programmatic in independent fieldwork research, platform projects operate through imaginaries and means for the production of disciplined technologies of question-asking – a core theoretical function within research protocols that otherwise have been difficult to achieve in current exemplars of ethnographic research practice.

How practical is it to produce such a modality of research today and under what conditions? I have followed two projects of particular interest to me. One is ‘The Asthma Files’ developed by Kim and Mike Fortun over the past seven years, on the basis of very little funding but strong and expanding networks of collaboration (see asthmafiles.org, for example Fortun 2012). While ‘asthma’ might seem a very specialised topic – which indeed it is though expansive in its empirical manifestations – it becomes much less so when developed through the platform affordances designed by the Fortuns and their collaborators. To develop these has involved both a continuing practical education and the recruitment of expertise in digital technology and also the rethinking and reforming of the modalities of theory work that so influentially shape ethnographic texts (indeed, it is an interesting exercise to follow the quite original way that theory is used to compose Fortun’s highly regarded ethnographic work,

Advocacy After Bhopal (2001), and how related and extended techniques of theory work emerge as aspects of the design, making and protocols of use and participation in The Asthma Files platform). Eventually, the activities of research, reception and concept work in The Asthma Files forums, incorporating traditional concepts of the field and fieldwork, would attract the distanced critical attention of the authorising discussion of the academy and evolve new modes of incorporating them as well. At least this is its promise.

The other platform project of continuing interest that I have discovered more recently is Bruno Latour's 'An Inquiry into Modes of Existence'. It is not only ironic, but indexical of the times that many in academia will discover this dynamically online platform project that has invented attractive protocols of participation by first gaining notice of it in a largish tome recently published by Harvard University Press (Latour 2013). It is more a reporting (a reception?) of the theory work and research of the platform than yet another theoretical meditation on the career evolution of a famous mode of inquiry, and distinctively opens with 'A User's Manual for the Ongoing Collective Inquiry.' This text thus might be understood as both the culmination of and the transition from the author's long series of texts, many evoking or performing the ethnographic voice of reporting on fieldwork-like researches, of his own and others. What is important to note here is that theory work for Latour has not so much reached an end, an exhaustion, climax or final version or synthesis of older ideas (this might already have indeed happened a while back for him in his prolific output within the terms and means of the conventional genres of discourse and textuality available) but a new medium of making, and certainly new communities of reception, and therefore new stakes, ambitions, continuities and foremost forms (or in Faubion's term, disciplined technologies of question-asking) and partners for working through those ideas. Though prominent in his example, the stakes for any such shift of an established line of theory in ethnography to platform affordances would be the same. With the development of platforms for inquiry and theory making, researchers become project managers and members intellectually of recursive overlapping communities of varying commitments and agendas.

In both examples, the participatory ethos of the two platform experiments are the same, as is the centrality of open theory work, and the idea of a coming alternative modality of method. There are also significant differences. The Fortuns' platform is built collectively and with minimal resources, on volunteer labour and learning as you go; Latour's is the production of considerable funding support and prestigious institutional connections and reputation. Characteristic of the digital age, who – and if a significant number – can be

attracted to what protocols are what the programmatic ethnographic value of these projects depends upon. Practically, they depend on social capital and reputation of the traditional sort, as well as considerable continuous funding and patronage (although the Fortuns impressively show what can be done in the kind of recursive community, or commons that Kelty evokes; they actively are moving to creating a technology that moves beyond their originating project and that can be applied to any number of modest projects conceived as ethnographic inquiry).

Most importantly, both squarely make shifts in the gravity of theory making, depending on how platforms granularly develop publics of participation, and they both challenge the traditional academy in its standard-keeping to adapt to its role as second-order reviewers of, and indeed late entry participants into, the knowledge making processes emergent in how platforms operate socially. Perhaps when the technologies for producing platforms are more streamlined, cheaper and easier to implement, the question will be how they will become ‘method’ in pedagogy and more advanced research practice. Their central product is not only the data that they can amass, organise and concentrate, but the constant invention of theoretical and conceptual frames for giving them diagnostic/interpretive shape.

So platforms, then, are one interesting development of the ethnographic method that we have known beyond its textual genre and its tendential gesture toward the programmatic. A second such, perhaps more accessible, development lay in ethnographic practices’ relation to, by alliance, kinship and incorporation, more experimental forms, appropriate to their longstanding disciplines.

Without primarily working with and through digital technologies as the primary forms of research and theory work, there are then the many sources of inspiration in collaborations with designers and artists that might inspire studio or installation interventions integrated into, or significantly alongside, the conduct of fieldwork according to its regulative ideals (of a preferred Boasian rather than Malinowskian origin here). In terms of experiments with implementing forms toward a diagnostic programmatic for contemporary fieldwork research, one might view ethnographic practice as shifting between two poles of attraction today, that of design (see Gunn, Otto and Smith 2013) and that of traditions of site-specific installation and conceptual art and their curation (see for example Papastergiadis 2012). Each have forms – imagined, adapted and theorised spaces and materialities – to offer ethnographic research a means to develop a programmatics within its present protocols of research. Dubbed ‘parasites’ by me (see Deeb/Marcus 2011), ‘third spaces’ by Fischer (2003) or zones of ‘lateral reason’ by Maurer (2005), through such interventions in fieldwork (usually in collaboration

with designers, installation artists or theatre and film makers, who know what they are doing!) spaces are forged and occasions are designed that permit the systematic creation of the middle-range, prototypical exposure of thinking, and theory-making in fieldwork. Such endeavours make theory undertakings in the field visible and accessible, giving it extended publics and receptions and setting research on a path, literally and figuratively, with theory work as its core activity, creating nested levels of constituencies, collaborations and receptions along the way.

My own recent experience of a project that has thought through a range of the kinds of design and art world derived experiments in form, within and in relation to ethnographic research, would be an excellent case to foreground this second realm of creating forms for the expression of theory and concept work in (and as) the field, if its climax had not occurred recently, and I were not presently in the throes of considering how to present or report on it to a conventional readership. So in the notes of this paper, I offer some extended (and indulgent) notes about it. This concerns a 'second act' or 'after-life' period of research at the World Trade Organization in Geneva, following a collaborative team ethnography project, extending from 2008 until 2010, organised by Professor Marc Abeles (see Abeles 2011) at the invitation of Pascal Lamy, Director-General of the World Trade Organization (until the end of June 2013).

Lamy had a special intellectual fascination with anthropology, and a certain despair about the state and purpose of the WTO during his two term tenure as D-G (including the 2008 world economic crisis and the failure to conclude a long awaited, since 2001, global trade agreement – the Doha Round). To the puzzlement and annoyance of many of his colleagues, he thought for a time that anthropological ethnography might locate, define and suggest by what means the WTO could shift and broaden its functions by showing through ethnographic observation and insight where this might already or potentially be happening.

There were some interesting methodological questions about how the ten of us in the project worked both individually and collaboratively, but only I used the project as an opportunity to experiment with the para-site form as a sort of meta-context to our challenging work to understand the everyday life of trade as diplomatic and bureaucratic thinking and operations – that is how a certain post-war vision and theory of world order (of the GATT) matured and evolved through the micro-concept work of the WTO. Toward the end of the project, I staged a series of seminar-like meetings with DG Lamy (creating the right mood or tone for these as something different from the usual genres of meeting at the WTO – an

interview, a conference, a committee report, etc. – required the skills of scenographers and installation artists whom I consulted) that tried to articulate and probe his stakes in anthropology, and his own deeper thinking that lay within his notion of anthropology and its relevance. The results (see Deeb/Marcus 2011) were interesting to all concerned, but inconclusive and rather were left dangling by the project coming to end.

That is where I left it until 2012, when in the midst of my increasing interest in the concerns described in this paper – the making of forms to do theory work inside or alongside fieldwork projects – I became motivated to produce an experiment at the WTO that would create a collaborative project between art and anthropology, and that would run for a duration along parallel and intersecting lines in the same space of research and exhibit, with possibilities of creating quite different as well as overlapping constituencies or publics for the co-operating projects of fieldwork and installation art. Our time constraint was that Lamy was leaving the WTO at the end of June 2013, and it was not clear whether after that any more research of an experimental kind could be done. Further, one of his chefs de cabinet had a taste for such a project and therefore was willing to facilitate it in June.

Perhaps the varied discussions around this project, planning for it, imagining scenarios that might work among various artists, designers, museum curators, anthropologists formerly associated with the research, and others who were not, with whom I consulted, was the most valuable experience for me in producing it. These have richly filled in with content and detail my schematic thinking about a theory of producing forms (like para-sites) in the course of fieldwork projects. Finally, there were five developed scenarios considered for producing an anthropology/art event at the WTO that would occur during the last three weeks of June 2013 at the Centre William Rappard in Geneva. The one we settled on entailed the production off-site (at Pace University in New York City) of three scenarios enacting problems of trade at the WTO, performed by improv dancers. Film of these scenarios was projected continuously at the WTO headquarters for three weeks, curated on site by the theatre artists with whom we collaborated, while two of us, Jae Chung and myself, conducted intensive interviews, conversations and had reunions with those we had worked with before as well as with new interlocutors. We did not meet face-to-face with Lamy again. The WTO was at a remarkably different place than it had been when we left in 2010, and we registered but stayed away from the current tense politics, especially about the organisation's future, as Lamy leaves, and many of the veterans from the time of GATT retire.

In any case, this all is to be written up in the tradition and current styles of ethnographic reporting (for example, I am imagining an account with the title “Lamy’s Charge...”). But in parallel to this convention, the 2013 art/anthropology intervention has established its own trajectory and requires its own accounting, focused on the form we created and what it produced, what connections it opens and where it is still to move. The ethnographic reporting, on the one hand, is a summation of the former project, but it is still a lively medium in relation to what the intervention generated. When we were still in Geneva, and as both a summary of our thinking, and a memo to ourselves we produced a document that Jae Chung thought to call ‘Our Theory of the WTO Case’ – considered as a kind of token or prestation, a parting gift, to Lamy (see Annex). His simple response to us, the fact that he cc’d it to his still most trusted and respected staff, and the fact that he indicated that he had already ‘said it’ in a recently published report on the future of world trade was almost a ritualistic endorsement from our patron of the entire project, from 2008 onward, about which he had remained unofficially silent (he had written a bland preface to Abeles’ book). Certainly not the last word for us either (actually about Chung it should be said that she was the most expert, original thinking anthropologist among us – and continues to be; her insights about how the art was influencing the co-occurring ethnography were the most acute). This case represents well that the language and stakes of inquiry were always in a deeply theoretical idiom, which in anthropologists’ hands always exceeded the narrow reference or else backroom existence of theory work and curiosity among the more pragmatic and technocratic appearances of those varied persons at the WTO concerned with the business of trade. Such a theory of the case, building it, speaking of it, as a core idiom of ethnographic work was a way to get at the communicative rather than calculative concerns at the WTO (for a shrewd approach of this sort to central bankers see Holmes 2013).

In the production of a ‘second act’ fieldwork experiment with form, ‘The Theory of the Case’ as a polyvalent expression (with an interesting legal doctrinal standing – see, for example, Wikipedia) of what we were trying to construct during the last weeks of June 2013 – first as an expression of the parallel and intersecting work of artists and ethnographers in the same literal as well as project space, and then as an expression for symbolic purposes as a prestation to Lamy –, is a fascinating exemplar of the arguments of this paper about how experiments with form settle into fieldwork projects, about the centrality of theory work in their production and about how they constitute a working programmatic of inquiry – a technology of disciplined question-asking.

A retro/prospective note in conclusion

The programmatic that Faubion wants to restore are consistent with a highly fragmented, lively and creative realm of contemporary research in anthropology, and that is also consistent with how the individualistic quest character of the ethnographic fieldwork method depends upon instilling discipline – a more systematic collective stake – in that durable questing modality. His provocation is that such a programmatic cannot arise from without, from supra paradigms formulated by the scholarly community itself, but must lay in experimenting with expansive and changing relational forms (e.g. encounters with diverse kinds of experts at almost every turn in the pursuit of contemporary fieldwork) already as a potential within the classic mode and norms of ethnographic inquiry, facing new contemporary relations and conditions of production. One could say that much of the theory attraction and its use by anthropologists in their production of ethnographic projects and its distinctive textual outcomes have been an effort, by limited means, to make this argument time and again in ethnographic writing over the past three decades. It has been difficult to exceed more than a gesture toward a referentialist programmatic in its tendentialist guise.

As this paper (and Faubion) argues only a material/pragmatic interruption in the kind of theory relation to the production of ethnography would provide an alternative programmatic – actually the only functioning one – and this involves a different sense of theory work as an activity at the core of the experience of fieldwork research, with a recognition of different participants, different roles, different circulations, different accountabilities and receptions – for which new forms of production are needed in retaining the durable ethnographic paradigm as the emblematic core of anthropological inquiry. As outlined here, these are already being attempted in experiments with new technologies of making research that emphasise the accessibility of raw, prototypical forms of knowledge that are the essence of theory-making and concept work in anthropology today, continually unsettled in the making and remaking of questions as a driver of where ethnographic inquiry as fieldwork literally goes.⁷

⁷ These lines evoke for me my own personal history with the ‘multi-sited’ construct of the emerging future for ethnographic research that I introduced in 1995 (Marcus [1995] 1998) – basically one of a number of reflections constructed at that moment that put ethnographic research in motion, the most prominent and successful of which was actor-network theory. Mine had the misfortune (but my own fault) to be understood in a literal way as the reproduction and multiplication of sites of research where the modes and standards of inquiry applicable to one would be produced in each. Of course this was open to obvious critiques of feasibility, which I anticipated in the original article. What I was personally more interested in was how work in one place evoked often hidden routes to others precisely through the theory or concept work that the

Without new forms of theory work, professionally sanctioned and implemented in pedagogy, and especially ‘method’, the difference that Faubion (and I) have been trying to delineate could easily be assimilated to the habits of the ghosts of referentialist programmatic that still haunt anthropology and the hopes of some of its practitioners.

Annex

“The Theory of the Case”

Good piece, thanks. See also, as a confirmation of your analysis, recently published report on "the future of world trade: the challenges of convergence", which can be seen as an attempt to overcome the mercantilist approach of trade offs. PL -----Original Message-----

From: marcus [mailto:gmarcus@uci.edu] Sent: 24 June 2013 23:18 To: Lamy, Pascal Cc: Jae Chung; Christine Hegel; gmarcus@uci.edu

Subject: Our Theory of the WTO Case...In Conclusion

Dear DG Lamy, As our anthropological project draws to a close, it seems to be the right point to present to you, from those of us who have continued after 2010, our theory of the WTO case. It was a rare gift to have open access to the DG cabinet and the Secretariat. And, the rules of social relations dictate that one gift must be answered with another. Although we have very little to offer that can be considered a gift that the WTO would value highly, we do have a story to tell. But, first, thank you. No one else would have been creatively confident enough to let the anthropologists roam among the rather tough legalistic crowd of the Secretariat, but you did. Some time after 2004/2005, and certainly after 2008, the organization, mostly the DG Cabinet, realized that impediments to negotiation consensus were multiplying in structural, political and cultural ways: the triple digit membership introduced unmanageable complexity to the interest based negotiation calculations; the US, once a leader in the system, had become distracted, defensive, and rigid in its offers; the growing assertive confidence of middle power Members, while a great moral victory, slowed down the process by bringing the negotiation down to deeply contested details; the NGO-led discourse of equity, meant to be productive, deepened the suspicion of any proposal from

ethnographer could do with specific subjects and not others (the key informant becoming an epistemic partner in complicit relations – a construct with which I was working by the late 1990s, Marcus 1997 [1998]). In this trajectory I indeed saw the multi-sited construct becoming something like the emergent connectivities and paths of recursion that were generated by collaboratively produced and distinctive ideas of ethnography emerging in the scenes of fieldwork – as a technology of question asking that sent one on a trajectory that was in fact multi-sited. What was missing was thinking about the literal forms that might materialise this sense of fieldwork process then. Changes in the way the world presents itself to ethnographers for fieldwork projects and dramatic changes in media and communication technologies have finally made the question of doing things differently with the classic method explicit and pressing. In the original multi-sited formulation, this question was not far under the surface, but it only became gradually and never clearly sayable until the present and the recent past.

certain Members. So fractured, there was no organizing leader, and thus no agency, in the system. Denied leadership and shared values, the system had to decide to redesign itself but it turns out that the problem of decision making was the very problem it had to solve first. There was no system, just individual parts in it. Given this tautological problem, once defined by a retiring DSB judge as "many hands but no one voice", the DG innovated (a problematic word we know) by redesigning the forum, without rewriting the rules, to bind the Members to multilateralism. So the project of change of culture from within began as a means to create a new community of trade. How to bind the Members as a community? The new forum a) What kind of problem is this? The anthropological case emerged after the July 2008 Ministerial when a deal among the major Members, seemingly so close, vanished. After this heartbreak, what was the prognosis for WTO, for multilateralism without the forward energy of the successive Rounds? In one sense, nothing: its future remains its present. Ships come and ships go, carrying goods to their destinations across borders. Post 2008 crisis numbers suggest only a slight and benign growth in protectionism, a sign of the vitality of the current WTO framework. But, as you know better than any one of us, while the work of TRP and DSB continued unabated, new trade issues found their consensus elsewhere. The resulting siphoning of personnel, political interest, and "energy" from the WTO Geneva suggests that perhaps the new danger to multilateralism might be found not as a competition among nations, but among trading blocs. If the optimists have it right, the bilateral agreements will return to the multilateral forum, and all will be well. If the pessimists are right, the competition to create the new standards on trade issues, like IP, may spark friction and worse, and trade will become again overtly a political tool. As a keeper of the system, what to do? The textbook answer is clear. As some in the Secretariat told me, almost in rebuke, the system was designed to have only one source of agency, the members, so the responsibility lies with them. While that is a good enough of a procedural answer, that is not good enough for operational intervention. For those who understand themselves as caretakers of the system, the task was to untie the knots and to push the levers. Was one lever at the level of the domestic lobby groups, as is in the case of the United States? Or was an important knot the complexity of the negotiation process among one hundred fifty some ministers? The case is then: how does a system, designed to be spoken in the passive voice, change actively? It was time to tap into the power of the weak. To experiment with lateral strategies of organizational movement. b) Why was it necessary to deploy the lateral move? Of course, there is no WTO as a coherent, independent organization. It is a linguistic and legal fiction, made of parts coordinated through the rules of the organization. Members decide and the organization, DSB, DG Cabinet, and Secretariat, execute that decision within the framework of the Agreements. Change in any legal sense can come only from the Members, but the question that occupied the DG was, can there be any other source of change? c) Why do the Major players not initiate change? It is the tragedy of the commons. In the past, the leaders either offered concessions to Members to overcome their defensive positions, "or they moved with such speed, others felt compelled to follow. But recently the "owners" suddenly found themselves in a house they thought they owned but only leased. Due to domestic governance difficulties and realignment of trade powers, the major players adopted a pure defensive

position and no longer offered concessions on desired issues. Instead they introduced counter moves elsewhere. Importantly, the United States must have seen that now is the last instance in which they could move to create another US centered trade regime and the WTO impeded that goal. As a result, in the absence of that ownership or power, defensive interests remain interests and do not coalesce into what the system calls consensus. d) With no leaders, what are the organization's options? As a response, the DG Cabinet seems to have engaged in three different genres. The first, the game of diplomatic persuasion, of exerting "peer pressure." The second is to embed the WTO within the network of IOs. The third, which is the art of the 'nudge', to move things along incrementally, even glacially, as a strategy of the long game, one of change in the culture of the organization. This one requires faith, hope, and charity, and an immense tolerance for disappointment, and satisfaction with private pleasure in small apparent forward moves. 1) Expanding the language of trade to reimagine it as a system of shared values in the speeches of the DG and the visual, photographic history of multilateralism since 1947 2) Speaking a vocabulary of contemporary openness, an homage of architecture stripped of British club ambiance as a way of reforging the narrowly legalistic and procedural idea 3) Bringing and binding the members into the building as an act of communitas, if only to share open meeting spaces and improved amenities 4) Recreating the secretariat in the image of the globe and not so much the anglo past 5) Negotiating the resistance within the ranks of the GATT based contingent, who saw the lateral strategy as the recasting trade from a pure economic issue to an interrelated one. The game has changed. It is no longer possible to calculate the offensive and defensive interests of the entire trading nations within the purely transactional framework of GATT. In recognizing this fact and the undeniable weak powers of the organization, the end game was to change the practices at the WTO, so that the organization may help bind the separate Members into a trade community whose commitment to multilateralism can transcend the interest-based calculations. It was a long term gambit to create the opportunity for the emergence of trust by forging a shared experience in the off chance that shared values can decrease negotiating complexity by increasing trust, and community can calculate the costs and benefits over a longer duree. In other words, to change the system without changing its rules required an investment in manifesting a utopic dream of a new political and cultural unit. Its actual emergence is slow and tenuous but the articulation of this dream may defend an increasingly vulnerable space of multilateralism. To those who see GATT as a way of defending against the nightmare of nationalism, aligning the weak powers may be the organization's best chance to move the organization from within.

Sincerely, Jae Chung, Christine Hegel, George E. Marcus

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Zusammenfassung

Im vorliegenden Aufsatz wird auf die verschiedenen Formen theoretischer bzw. konzeptioneller Arbeit in zeitgenössischen sozial- bzw. kulturanthropologischen Forschungsprogrammen eingegangen und die These aufgestellt, dass Theoriearbeit integraler Bestandteil der ethnographischen Methode und des Feldforschungsprozesses ist. Es geht eher um Arbeit innerhalb der Feldforschung, denn um die Aktivität eines professionellen Kreises, die die Feldforschung umgibt oder aus dieser in Distanz herrührt. Dies ist wiederum Folge der zunehmend gemeinsamen und mobilen Feldforschungsstrategien zwischen vielen kleinen Teil-Öffentlichkeiten. Daher unterstützt der vorliegende Aufsatz die Herstellung neuer, offenerer Formen und Medien für die Konstruktion und Rezeption von Theorie und analytischer Arbeit – statt der ehemals einsamen Grenzen der Feldforschungskommunikation – sowie ihre Reflexion als ein aktuelles Methodenproblem. Feldforschung verlangt Geduld, aber Theoriearbeit verlangt darin mehr unkonventionelle Arbeit.

Biographical Note

George Marcus currently holds the position of Chancellor's Professor at the University of California, Irvine, where he established a Center for Ethnography, devoted to experiments and innovations in this form of inquiry. He is also a founding editor of the Journal *Cultural Anthropology*, and chaired the anthropology department at Rice University for 25 years.

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George Marcus ist Inhaber einer Chancellor's Professur an der *University of California*, Irvine, wo er das *Center for Ethnography* gegründet hat, das sich Experimenten und Innovationen in ethnographischer Arbeit widmet. Er ist auch Gründer der Zeitschrift *Cultural Anthropology*, und leitete 25 Jahre lang das anthropologische Institut der *Rice University*.

George Marcus

Where Theory Work Is Done in the Production of Contemporary Anthropological Research – and How It Might Be Made Accessible in Intermediate Forms of Reception in Ethnography Vienna

Working Papers in Ethnography, No. 2, Vienna, 2014

Wiener Arbeitspapiere zur Ethnographie, Nr. 2, Wien 2014

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