Fieldwork as technique for generating what kind of surprise? Thoughts on *Post-Soviet Social* in light of "Fieldwork/Research"

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> "[O]nce fieldwork is no longer understood in terms of ethnography, as the study of ethnos, but rather as a surprise generating practice of exposure out of which new, un-thought of research questions emerge (research questions that escape the reach of ethnography), fieldwork is no longer an actual site of research. It is a practice for generating research questions—but it does not provide the information necessary to address these research questions."
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> Tobias Rees, "Fieldwork/Research"

Tobias Rees' "Fieldwork/Research" advances a provocative claim: in much recent anthropology fieldwork no longer functions as a self-contained method but as a practice, a technique for generating surprise that provokes, inflects, and indeed shapes anthropological research. For Rees, this development is linked to the emergence of an anthropology beyond *ethnos*—that paradigmatic object of anthropological inquiry that made it possible for the question of method in anthropology to be posed as a question of fieldwork through the figure of ethnography.

Two challenges confront an anthropology thus conceived. First, it must rethink fieldwork outside the comfortable categories of society and culture—or complementary terms like (cultural) difference and specificity—that have grounded much reflection on fieldwork in American anthropology and that have been invoked to identify anthropology's distinctive kind of knowledge-making. Second, the question of method has to be thought beyond the question of ethnography or fieldwork. Anthropology beyond society and culture, one concerned first of all with difference in time rather than difference in space, must ask: What is the distinctive mark of anthropological method if it is *not* fieldwork?

Rees has taken my book, *Post-Soviet Social*, to exemplify this emerging tendency in recent anthropological work. In doing so he reads my book into disciplinary developments that I have not thought about for a long time and with which, indeed, my book is not directly engaged. As such, his intervention is certainly welcome. It has caused me to think about my work in new ways, and in relation to a new sub-disciplinary space. If there is an anthropology beyond society and culture I am happy to be part of it.

I was particularly struck by one dimension of Rees' argument that strikes at the central theme of this workshop. It relates to an issue on which Rees places a great deal of emphasis: the "surprise" that arose during my fieldwork, which, as I describe in my book's introduction, served to reorient my research in unanticipated ways.

At the time, I experienced this surprise as a happy accident, and described it as such in *Post-Soviet Social*. It was therefore useful to be reminded by George Marcus (2009: 6) that this is an entirely familiar "rhetorical frame": "the common indication at the beginning of [anthropological] texts that the research fortuitously took different turns from the way it had been conceived before fieldwork." This is, he argues, "one key way of establishing authority for an ethnography through the venerable aesthetic in anthropology of knowing by discovering." So my surprise was entirely predictable—and distinctively anthropological.

But Rees pushes us to think about surprise beyond this rhetorical function. He proposes that we understand fieldwork as a surprise-generating *practice*. Surprise, he suggests, can be and ought to be purposively sought out through a structured and normed technique or set of techniques. The aim of this practice is to produce a result that can be anticipated, even if this anticipated result is, somewhat paradoxically, the discovery of something unexpected.[i] I am particularly intrigued by the connection between this analysis of fieldwork as a surprise-generating practice and the broader question of research that Rees poses. If fieldwork as a surprise-generating practice is structured and normed, these norms must relate to, and be conditioned by, the broader enterprise of anthropological research in which it is deployed.

Rees summarizes the approach to these issues in *Post-Soviet Social* as follows: If fieldwork functioned as a method to generate surprise—"unexpected, previously un-thought of curiosities that break open the possibilities of research"—then research functioned as "the reconstruction of the field of inquiry in terms of the surprise generated by the field—and the clarifying, knowledge producing effort to write the history of...the surprising configuration that emerged out of fieldwork." This seems to me just right, and I will add a couple notes to what he has written about how this problem of research *after* and *in light of* fieldwork relates to an anthropology beyond society and culture. But initially I want to raise a different question: What *precedes* the deployment of fieldwork as surprise-generating practice? What, other than just "being there," conditions the type of surprise that this practice aims to generate?

This question suggests one of the central points I would like to explore, concerning the exact nature of the surprise generated by my fieldwork. The surprise I experienced was not that of sheer contingency. Nor—and here I want to slightly modify the formulations suggested by both Rees and Marcus—did it open up entirely new questions or fields of inquiry. Rather, it was conditioned by at least three things:

(1) A prior *orientation* to problems and questions, given in part by a broader intellectual tradition, and in part by more specific disciplinary tendencies;

(2) Choices about *field sites*—where to conduct fieldwork and what to focus on;

(3) What might be provisionally called (though I hope someone could propose a more apt term) a *problem-space* that is shaped by some conventional understandings—in whatever universe of discussion—in relationship to which a surprising, confounding, or disorienting discovery might be made, and in which such a discovery might provide the grounds for an intervention.

I want to say something about each of these factors that conditioned the surprise produced by fieldwork. In some way, I am simply rehearsing the framing of my book in light of Rees' paper, or what I have understood of it. My hope is that in doing so I might provide some material for thinking about, following Marcus (2009: 25), "broader, elaborated view and model of the anthropological research process" in an anthropology beyond society and culture.

Orientation

My orientation to the Russian field was initially shaped by an anthropology of modern government that was just taking shape in the early 1990s when I began to study anthropology as an undergraduate. Among the texts that most influenced me were Jim Ferguson's *Anti-Politics Machine*, James Holston's *Modernist City*, and Paul Rabinow's *French Modern*. In light of the history that Rees outlines, one might note two ways (no doubt among others) that these studies were innovative for their time. First, they unapologetically took modern government, modern expertise, and modern governmental rationality as objects of anthropological investigation. They weren't the first or only ones to do so; they were part of a broader call for "anthropologies of modernity" at this time (Rees 2013a). But they played a catalytic role in bringing such studies into the anthropological mainstream, and in establishing within anthropology a set of methods for conducting them. Second, if these studies pointed to an anthropology of modern political rationality they did not suggest it need be primarily ethnographic or fieldwork-based. Rabinow's book did not involve any fieldwork at all, though it was partly motivated by prior fieldwork that sparked an interest in French colonial planning. Holston's book had a more prominent fieldwork component—and fieldwork was central to the book's critical project—although that was combined with historical work on city planning and documentary analysis. Ferguson's book was based on initial fieldwork of a rather traditional sort that was also central to his critical project, though it is worth thinking through how precisely that fieldwork contributed to its very broad reception and influence. How many of its readers recall what precisely his book had to say about attitudes towards cows among the miners of Lesotho?

Anticipating something I return to below, I think that there is an important tension running through this work that I did not fully perceive when I first engaged it. All of these books were attempts to understand the constitution of the social field, in part through forms of "social" knowledge, whether urban planning, development, or epidemiology. Although all of these books thereby "anthropologized" the social (to borrow Rees' phrase), none of them constituted social knowledge as an object of *ethnographic* investigation or even a site fieldwork per se. So the difference made by fieldwork (or its absence) is notable. Ferguson and Holston used fieldwork to ground a distinctively anthropological knowledge about the social that grasped things other kinds of social knowledge producers could not grasp. We might say that their authority stemmed, in part, from a claim to know society better. Rabinow, by contrast, refused to claim any epistemic privilege for anthropological knowledge *viz* these modern experts because his anthropology was not a form of social knowledge. If his book was a work of anthropology, it was, indeed, anthropology beyond society and culture.

I wasn't particularly attuned to these distinctions when I first encountered these books. At the time, they simply provided a point of entry—not just into my own research project but into anthropology as such. That is to say, I wasn't interested in these books because they suggested a particular way to do anthropology. Rather, I was interested in anthropology because these books suggested that anthropology was a discipline in which one could study the kinds of things they were studying and conduct the kind of inquiry they were conducting. As I came to think of it later, they provided a way to take long running themes concerning government and rationality—drawn broadly from Weberian and Foucaultian traditions—and to constitute them as topics for a critical inquiry into the present. These were anthropological inquiries that asked how, today, have life and population become targets of expert knowledge and political administration?

If these studies were contributing to an emerging picture of modern political rationality, a couple things stood out, and provided for me points of orientation. First, the absence of the Soviet case from this literature was conspicuous. The Soviet Union was the great experience of planning that loomed over the entirety of the 20th century. It profoundly influenced 20th century urban planning, 20th century development thought and practice, and post-war economic planning even in rich, nominally capitalist countries like France. And yet there was no account of Soviet governmental rationality equivalent to the one that these authors had provided for Brazil, Lesotho, and France. Second, there was a curious fact related to the timing of this literature. These books were published long after the forms they were analyzing had undergone profound critique. Holston's book was published thirty years after Jane Jacob's *Death and Life of Great American Cities*; the "total planning" he analyzes was long since discredited in many quarters. The centralized, state-led, infrastructure-intensive development planning analyzed in Jim Ferguson's book had undergone many waves of critique by the time his book was published in 1990; indeed, even the *successor* to the state-centric development paradigm of the post-World War II period had already come under severe criticism.[ii] Paul Rabinow's book, meanwhile, was explicitly presented as a history of a present that was already receding into the past.

So an obvious question presented itself: what comes next? How to update this literature in relationship to more recent developments? This question inescapably raised the problem of neoliberalism, which at that time was barely on the radar of the critical human sciences in the United States, though attention to it was about to explode. I do think that in the first instance I understood my interest in neoliberalism as, following Rees, an interest in difference with respect to time: What difference did neoliberalism make for the forms of Soviet social modernity? How, more generally, might the emergence of neoliberalism be understood to mark a point of inflection in the history of biopolitical government? How might a study of neoliberalism tell us something, as Rees likes to quip, about the difference that today makes with respect to yesterday? And do we have the tools to think discerningly about this difference?

So this was my starting point, my initial orientation. Since we are concerned here with surprise encountered through fieldwork, it bears emphasizing that it is also where I ended up. In this sense my experience is not quite adequately described by the trope in ethnographic writing that Marcus (2009: 12) calls the story of "correction", in which "the anthropologist starts out with the idea of researching one thing, but good, promising fieldwork…leads to something completely different, unexpected, and more interesting." If fieldwork presented me with surprises, it didn't present me with entirely new topics or problems. Looking back, it strikes me that my book really is about the things I set out to study.

Field Sites

If fieldwork is a surprise-generating practice, how does one decide *where* to deploy it? While planning for fieldwork in Russia I decided to focus on small industrial cities. No doubt there were other possible choices, but this one seemed compelling for at least two reasons.

First, small cities were the ideals of Soviet city planning. They were consistently held up as a model form for urban development throughout the Soviet period. Borrowing from a longstanding tradition of thinking about "garden cities", Soviet urban planners thought that in small cities it would be possible to create a carefully planned balance between industrial enterprises, residential areas, leisure facilities, nature, and social services, thus avoiding the pathologies of urbanization and industrialization under capitalist circumstances. Despite the myriad problems with Soviet planning, the Soviet urban pattern was indeed characterized by an unusual preponderance of these cities. This is not to say that they were typical. Rather, they were ideal-typical. Small cities brought out a certain dimension of the Soviet experience in a one-sided and exaggerated fashion; they brought its contours into relief.

Second, I thought these cities might be interesting places to study post-Soviet transformation, and in particular the project of neoliberal reform. My reasons had to do, at least in part, with the specific structure of these cities. As I argue in *Post-Soviet Social*, the entire project of social modernity in small industrial cities was "enterprise-centric." These towns were planned and built around one or two major enterprises that, in their turn, were responsible for most aspects of local life: employment, social services, housing, infrastructure, leisure facilities, and so on. So what would the collapse of planning institutions mean in these cities that, due to their small size, geographic remoteness, and economic homogeneity, seemed particularly ill-equipped to deal with markets? Here, too, small cities seemed not so much typical of the post-Soviet experience as ideal-typic. In them, I thought, one could discover, in nearly ideal purity, a confrontation between a human settlement shaped by institutions of socialist planning and "neoliberalism" – whatever that was.

Perhaps, in any case, there more to be said about case selection, the exemplar or ideal-type, and anthropology beyond society and culture oriented to difference in time.

Problem-Space

By the time I began to think about Russia in the mid- to late-1990s, debates over post-Soviet transformation had taken a particular shape that was both typical of and exemplary for broader discussions of globalization and neoliberalism. During the 1990s reform focused on the big issues of macroeconomic management and economic governance: privatization; interest rate and exchange rate policy; fiscal balance; and so on. Advocates of "transition" argued that such policies, if implemented properly, could quickly turn Russia into a market economy that would at least be better than socialism for most people. Critics of transition argued that these policies would result in catastrophe for human communities shaped by planning and exposed suddenly to markets. As I argue in *Post-Soviet Social*, these critics cast the stakes of post-Soviet transformation—sometimes implicitly, often explicitly—the terms Karl Polanyi used to describe the birth of the liberal creed and the creation of market society in early 19th century Britain. They saw advocates of transition as exponents of a new liberalism that revived classical liberalism's unwarranted faith in the "self-regulation and harmony" of market society. And they pointed to myriad examples of reactions against this process, understanding them as instances of what Polanyi described as a self-protective reaction of society defending itself against the ravages of the market.

My expectations and questions were structured by this "Polanyian" framing of post-Soviet transition. At the very least, it provided a baseline interpretation of the stakes of neoliberalism, both in the post-Soviet context and more generally: markets versus the existing substantive organization of society. Certainly, it suggested a way to make quick sense of the situation in small industrial cities when I arrived. Following the collapse of Soviet planning institutions the enterprises in small industrial cities across Russia experienced dramatic collapse. Since everything depended on the major industrial enterprises, everything was affected by these enterprises' rapid decline; they seemed threatened, indeed, by what Polanyi called "death from exposure" to market forces. At the same time, a range of mechanisms served to prop up these cities during the hard years of the 1990s, whether these were government subsidies to enterprises, the much discussed barter economy, various "informal" activities by individuals and households, or government social welfare payments. This dynamic was, again, readily legible in Polanyian terms. The devastation of neoliberal marketization—a profoundly *anti-social* force—triggered self-protection. The critical response (for both foreign scholars and many Russians of various stripes): society must be defended.

(The Obligatory) Surprise!

Had I conducted fieldwork in the mid-1990s it is entirely possible that I would have simply confirmed this Polanyian story – or confirmed, at least, that this was how the politics of neoliberal reform were being played out in post-Soviet Russia. But my fieldwork began only in the second half of 1999, by which time the situation had changed in important ways. The ruble was devalued in the summer of 1998, immediately changing the terms of trade for Russian producers, and triggering sharp recovery in many small industrial cities, including the ones I worked in. This economic recovery combined with a rise in global oil prices to relive pressure on government budgets at the federal, regional, and local levels. As a consequence, social welfare payments and public sector wages – both of which were crucial in these towns – began to flow with greater regularity, and vital social services were better funded. Many cities whose very survival seemed in question during the 1990s began to revive.

At the same time, the reform agenda shifted. The big economic governance items like privatization and liberalization were either completed or had gotten as far as they were going to get. In any case, without pressure from external creditors—whose money was no longer needed—the high profile battles over such reforms were a thing of the past. Instead, the dominant topics of reform in the 2000s related to ongoing concerns of state administration: budgetary management; infrastructure reform; social welfare reform; and so on. If there was an ethnographic present for my research it was this moment of "second wave" reform was just taking shape. Proposals for budgetary reform, communal service reform, and various types of social payment reform were just being discussed, and in some cases tentatively implemented, at the local level.

These reforms were of particular interest since they grappled directly with the institutions of social modernity whose planning, creation, and reform I had set out to study in the first place. The surprise, simply, was that these reforms did not follow the Polanyian pattern. Some features of these reforms seemed distinctly neoliberal: their emphasis on “commercial” principles; their concern with incentive problems; their grounding in a critique of the inefficiency of existing governmental practice; their focus on individuals rather than collectivities as the beneficiaries of social welfare. But other features of these reforms confounded standard understandings of neoliberalism. In many cases they accepted and reinscribed the norms of Soviet social modernity, and simply reengineered existing patterns of provisioning. Moreover, they criticized the existing social welfare systems that had been inherited from the Soviet Union not just for their inefficiency or their “socialization” of things that ought to be left to the market but also for neglecting the poorest and most vulnerable households, and for lavishing social welfare benefits on rich families. Working through these reforms on a local level, the small industrial city began to appear, if only tentatively, in a new light. It was no longer simply a relic of planning holding out against the tide of neoliberal marketization. Instead, I began to understand the small industrial city as an exemplary space in which these reforms could be seen to engineer a tricky articulation between the norms of Soviet social modernity and the principles of market organization and liberal government. It was not the social *as such* that was at stake or under threat. Rather, it was a particular variant of social modernity that was being replaced in partial, somewhat contingent, and extremely interesting ways by *another social modernity*, another vision of social citizenship, a different form through which life and population are constituted as governmental problems.

Derailment and reconstruction: research after fieldwork…

I finished my year of fieldwork with lots of insight into many things about post-Soviet Russia. But I was at sea with respect to the central question with which I began: neoliberalism and its relationship to the history of Soviet biopolitics. How to conceptualize these reforms? Were they simply not neoliberal? Were they examples of a neoliberalism that had been modified through their translation into the

Russian scene? Was the apparent commitment to social welfare I discovered in them just an ameliorative adaptation of a neoliberal project still defined, at its core, by a logic of capital accumulation and class power? Or was neoliberalism something different from—or at least dramatically more complicated than—what most critical commentators said it was?

At the time, two models from within anthropology suggested themselves for reengaging neoliberalism through ethnography and through an anthropology of society and culture. One followed from the suggestion that Jean and John Comaroff made in their work on occult economies: that one could study, through ethnography, the dialectical interplay between localities and the global culture of neoliberalism or the global effects of millennial capitalism. In this case, anthropology would be reinscribed as an expertise of the local, the social, and the cultural, though these are transected by global vectors. Another possibility—exemplified in a book on Russian reforms by the anthropologist Janine Wedel—involved tracing the "translational" work of transnational networks of actors ("transactors", in her terms) who brought neoliberal ideas and schemas of reform into the Russian context. Here the objects are networks, social ties, and mechanisms of cultural translation, and so on. Either option suggested more fieldwork, since neither question had been clearly formulated in my original fieldwork, which had focused on the institutions of social modernity and their post-Soviet transformation. And indeed, in the long interregnum between finishing my dissertation (which I did very quickly) and finishing my book (which I did slowly, painfully, and intermittently) it occurred to me on many occasions that I really ought to return to the field to make my project properly anthropological.

But I did not undertake more fieldwork; indeed, I never returned to Russia to do research of any sort (and, in a sense, I stopped worrying at that point about whether what I was doing was anthropology). The reason, beyond the logistical barriers, was that I was not convinced that doing so was the best way to make progress on the question I had set out with, and that ultimately interested me the most: What is neoliberalism? And what difference does it introduce with respect to the existing forms of social modernity? From this perspective, it seemed that the available models of anthropological work—anthropologies of society and culture—would of necessity take for granted and thus obscure precisely what had emerged as the most interesting finding of, or should I say hypothesis generated by, my fieldwork: that neoliberalism might be thought not in opposition to social modernity but as a kind of social modernity, perhaps a reflexive moment of social modernity. To address *that* question it seemed necessary to turn to research *beyond* fieldwork: a study of certain traditions of neoliberal thought that proved relevant to the Russian reforms I examined; genealogical work on neoliberalism and the social state; concept work. This is a crucial moment: if we understand fieldwork not as the essence of anthropological method but as a practice, and if we must also therefore develop a more serious theory of its value in research design, we should also allow for the possibility that, in some cases, it is the wrong tool for the job.[iii]

…fieldwork in anthropology beyond society and culture

To briefly conclude, it bears asking what, precisely, the value of fieldwork is in the kind of study I was engaged in. Here I would like to refer back to my earlier suggestion that it is possible to distinguish two ways of approaching the study of modern government, governmental rationality, technical expertise, and the production of social knowledge.

In one model, the anthropologist positions him or herself as a contending producer of knowledge about the social. Here anthropological authority rests on the ability to know the social better. Fieldwork in the form of ethnography (however *ethnos* is configured) plays a privileged role: it gets close to practices, to experience, to the quotidian, the anecdotal, the local, the circumstantial.

In the other model the anthropologist resolutely refuses any epistemic privilege. So what is the role of fieldwork? I am not convinced that there is any necessary role for fieldwork (again the example of *French Modern*, as well as my own work current work on security – perhaps these are not works of anthropology?). But I am convinced by Rees' claims that if fieldwork functions in such an anthropology it functions to generate surprise, and the particular kind of "radical" surprise that belongs to difference with respect to time. Moreover I identify entirely with the way that Rees describes the *experience* of this surprise: "the experience that one's concepts don't work any longer—that an opening has occurred, a mutation, a rupture; that we live in a situation we have not yet learned to come to terms with (largely because the conceptual presuppositions implicit in the already existing concepts are inadequate to the new situation…)."

But keeping in mind how this surprise is conditioned by what comes *prior to* fieldwork, I think there is some cause for caution in placing too much emphasis on surprise. Some discussions of the "surprise" of fieldwork create the impression that the fieldwork experience is *sui generis* – that the surprise is merely circumstantial, purely serendipitous. But as I have indicated, this is not precisely what goes on, and there is much to say about how surprise is conditioned and about how fieldwork, as surprise-generating technique, is *deployed*. The surprises of fieldwork do not provide us with entirely new questions and problems. Instead, they provide us with unanticipated and previously unknown ways of putting questions, problems, and concepts into play in a field of inquiry; or in observing *how they have been put into play* in particular circumstances. As I put it in *Post-Soviet Social* (and it still seems right to me), what fieldwork offers is an orientation to a "grouping of sites and a set of problems that I simply could not have stumbled upon otherwise" (Collier 2001: 29). Fieldwork is thus seen as one piece of a broader arc of inquiry whose function is to specify problems, rectify (or simply problematize) concepts, and orient further research.

[i] A characteristic feature of recent history is the invention of new techniques—replicable practices with results that can be anticipated if not known or predicted in advance—for generating surprising results.
[ii] As I show in *Post-Soviet Social*, by the late 1980s and early 1990s many of the problems of early structural adjustment policies had been recognized, and the paradigm of structural adjustment was already beginning to shift, though not as dramatically as it has in the period since the late 1990s.
[iii] In this sense it seems to me that Rees' claim that anthropology beyond society and culture is quintessentially a field science could use more discussion. Certainly there are other kinds of surprise-generating techniques (genealogy?) and methodological problems other than surprise-generation that require equal attention.