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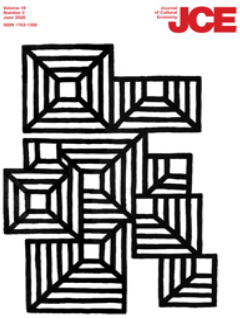
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## Symposium on ‘Do ethnographers make markets?’

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### Introduction to the symposium

This special issue has been a truly collective inquiry. It began with a simple intuition: the fact that social researchers, who normally study how practitioners make markets, are invited to collaborate in market interventions is something worth thinking about. Those who contributed with their articles generously agreed to work with this premise (Björklund Larsen 2023; Geiger and Gross 2022; Onto and Fernandes 2025; Ossandón and Pallesen 2025; Våland, Pallesen, and Møller 2024). While in their work, they usually report on what they find about those they study – the practices, instruments, and issues of the practitioners they investigate – and reflect on how these findings connect and challenge ongoing academic conversations – in anthropology, sociology, market studies, marketing, etc. – here they wrote on their own experiences as collaborators in market interventions. As we all realized, this was more difficult than how it first seemed. Not only did contributors have to write new papers for the issue, but they also had to conceptualize a type of problem that was new to them.

The introduction to the issue (Ossandón and Onto 2026) is like our collective inquiry’s final report. We reflect on how this minor transition, from observing to collaborating, *does something* to the concepts we use, to how we understand our practice, and to how we think about market making. More generally, rather than a clear recipe on how to collaborate, how we should behave in market interventions, the issue reports on a shared sense of confusion, ambiguity, and frustration. Our hope is that these reflections will nevertheless help colleagues in similar situations to reflect on their conundrum and to see that what they encounter is a shared and difficult problem.

As a CODA, we have this symposium. What contributors to the symposium have in common is that they have, not necessarily knowingly, greatly helped us think about the common problems emerging from this special issue. With this symposium we thought we could invite them to properly join the conversation. In terms of the format, we asked them to provide no full papers but rather more flexible short essays or reflections. We also told them that we were not looking for a debate on the quality of the papers. This was not an invitation to *review* the issue, like in a book symposium, but rather to use the symposium to take the conversation on ethnography, collaboration, and market intervention further.

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Because the work of each contributor helped us in different ways, when we contacted them, we posed different questions to them. Even though each of the following texts is self-contained, we thought that it could be a good idea to close this introduction with an edited version of their invitation.

**Daniel Beunza.** As we write in the introduction to this issue, your work has become important to us. We think that if we take your monograph, *Taking the Floor* (Beunza 2019), Gillian Tett's (2021) reading of your work, and your own interventions afterwards, for example your interview in BBC's podcast *Thinking Allowed*, we find an important proposal. To say it in the language we use in this issue, you propose a *script* for the ethnographer that collaborates in finance. In this script, the ethnographer is the person that can help practitioners deal with those issues they articulate as cultural problems. In the introduction, we use your script and Tett's reading of it as a yardstick to compare the often much less neat situations in which we encounter the ethnographers in the case studies in this issue. What we do not discuss however is that your script is also a figure you have tested in practice in your own collaboration with banking institutions. For your contribution to this symposium, we would like to ask you if you could use the discussions in this special issue to reflect on your own experience in market collaboration.

**Eugênia Motta.** You and your work have been closely intertwined with the trajectory that underpins this special issue. You have been part of collaborative engagements with institutions such as the Brazilian Central Bank and the Brazilian Federation of Banks. In fact, you also participated in the Study Group on the Anthropology of Finance organized within the Brazilian Securities and Exchange Commission that is discussed in Onto and Fernandes' paper in this issue. And you were part of the workshops and events in Copenhagen and Rio de Janeiro, where many of the questions explored in this issue were first developed and debated. In these occasions, you always brought with you a particular sensibility to the multiple parts ethnography can play, which, in fact, connects with one of the main issues we highlight in the introduction to this issue. The *ethnographer* in the title takes many roles in the different collaborative situations reported in this issue. For your contribution to this symposium, we would like to ask if you could reflect on ethnography, in, with, and after market intervention.

**Philip Roscoe.** You and Olga Loza (Roscoe and Loza, 2019) published a piece in this journal that has been important to us. We mentioned it already in the special issue proposal we submitted years ago, and we go back to it in the introduction. In a way, we could say that we began with the situation outlined in your paper. If our work – in market studies, social studies of finance, etc., – pay so much attention to the performativity of economic knowledge, – that is if we work from an approach that focuses on knowledge less as a representation and more on how it shapes the reality it intervenes – we should also reflect on the performativity of our knowledge and the obligations that come with that. In the end, however, the papers in the issue went elsewhere, collaboration did not translate into performativity, the market or the economy in the different cases did not become more like the assumptions and figures mobilized by the social researchers, which, in turn, pushed authors to search for other concepts to think about their own experiences. For your contribution to this symposium, we would like to ask you if you could go back to yours and Loza's proposition and reflect on it in relation to what we find in the issue and to your own experience since your piece was published.

**Don Slater.** There are two articles you and Joanne Entwistle co-authored that became important to us in this special issue. In the first one (Entwistle and Slater 2019), relying on your experience as an ethnographer-collaborator, you question the optimism you see in more theory driven reflections concerning the concrete ability of the social scientists to make their own work performative. In the second (Entwistle and Slater 2014), you reflect on how the notion of culture was too easily dismissed in the ANT-inspired economic sociology, especially considering that culture is not simply a concept we can choose to rely on or not, but that it is a very important part of the conceptual repertoire that the practitioners we have to collaborate with use. As we discuss in the introduction, these two problems became very salient in the cases discussed in the papers in this issue. For your contribution to

this symposium, we would like to ask you if you could follow up on these discussions in relation to your own experience in ethnographic collaborations.

## **Ethnography, translation, and the political contingency of reform in UK banking**

**Daniel Beunza**

The papers in this special issue speak eloquently to the fears, uncertainties, and practical challenges I encountered when deploying fieldwork to advance reform in the UK financial sector. The contributions summed up by Ossandón and Onto (2026) – including liminality, para-ethnography, cultural re-turn, and controlled equivocation – shed incisive light on my own experience as an aspiring ethnographer and change-maker. However, there were additional dynamics at broader societal levels that are also relevant to explain the fate of market-making by ethnographers, and I turn to these below.

But first, my story. As Ossandón and Onto (2026) correctly note, my monograph *Taking the Floor* (Beunza 2019) committed a strange form of intellectual treason against STS-informed economic sociology. While the leading scholar of this literature advocated abandoning culture as an explanation of markets (Callon 1998), the concept of culture was prominently displayed on the book's back cover. Was this disloyalty, cynicism, or misunderstanding?

As it turns out, none of the above. The book makes it clear that the 2008 global financial crisis prompted the widespread appropriation of the concept of organizational culture (in short, 'bank culture'). This appropriation was led by financial regulators and bank supervisors, from the UK's Financial Conduct Authority to the Federal Reserve Bank of New York. Indeed, the President of the New York Fed, William Dudley, famously used the word 'culture' as many as 44 times in the now-famous speech in which he announced the creation of a 'Conduct and Culture' program of supervision that would target the values and beliefs of Wall Street banks, not simply their actions.

But as my book insists, this native use of the word 'culture' to denote ethics and misconduct was deliberate. It was a strategic misuse, aimed at avoiding references to ethics and therefore making a program of reform and intervention more palatable to the banks and their supporters. After all, it would be far more difficult for a bank to agree to take part in a program improving its ethics than one improving its culture. Bank culture was a form of what Callon (1984) calls 'translation,' that is, defining, enrolling, and linking actors to form an actor-network.

My book, in other words, knowingly adopted the regulators' appropriation of culture. I chose to be, so to speak, an accomplice. This came at the expense of theoretical consistency, and indeed loyalty to the very theoretical turn that my scholarly fellow travellers were attempting. Yet using 'bank culture,' I felt, was the appropriate move.

To see why, turn the clock back to 2016.

At the time, I was working as a lecturer in management at the London School of Economics. In February of that year, I was contacted by a newly formed industry initiative in the UK, the Banking Standards Board. This coalition of several banks had just been set up with a public-service objective, i.e. 'help raise standards of behaviour and competence across the UK banking sector' (Lambert 2014, 2). It was to accomplish this by conducting an annual survey of organizational culture at the banks. However, the leadership at the Standards Board was aware of the limitations of such a quantitative tool and was interested in complementing it with additional insight.

In our meeting, the Standards Board employees asked me if ethnography could be used to help them figure out 'what is really going on' at the banks. I informed them that their plan might be seen as planting a mole in the banks and would not be possible, as it contravened ethnographic research ethics. But I offered something else instead: the Board could train bank executives in ethnography, hopefully creating a sensitivity to culture within the banks. Over time, successive cohorts of ethnography-trained bankers might come to know each other, develop a common approach, and assist in the Standards Board's goal of improving the banks' culture.

Notice the long sequence of translations here. The Standards Board relied on three euphemisms in framing its mission: from ethics to culture, from misbehaviour to behaviour, and from incompetence to competence. There was an additional practical translation: in realizing its mission, it was to rely on an organizational survey of its members, as traditionally used by organizational psychologists. However, organizational scholars do not refer to these surveys as ‘culture’ but as ‘climate,’ with the term culture traditionally reserved for qualitative studies (Denison 1996). Nevertheless, the Standards Board made the case that it could fix culture by surveying climate. In other words, private regulation of the banking industry staged an industry version of the cultural re-turn of the kind described by Ossandón and Onto (2026). In my 2019 book, I decided to adopt this controlled equivocation (while extensively acknowledging its problems) so as to align myself with this program.

Granted, the banking industry had strong reasons to accept an intervention. Following the 2008 global financial crisis, the Royal Bank of Scotland required the largest bank bailout in UK history, and the cash capital injected by the UK government to keep the banks liquid added up to £65–70 billion. Its previous regulator, the Financial Services Authority, had been deemed too weak to handle the banks and had been reorganized into the sharper Financial Conduct Authority (FCA) as well as the Prudential Regulation Authority. A number of prominent FCA officials had promoted the idea of culture as a key explanation for the banks’ problems (Financial Conduct Authority 2018). Furthermore, the FCA had also been promoting the use of behavioural economics and other psychological approaches such as surveys. Finally, the CEO of the Banking Standards Board, Allison Cottrell, was a former official at the FCA.

The relationship between the FCA and the Banking Standards Board was intriguing. The former was a regulator but was constrained by the sheer economic importance of financial services to the UK: tasked with fixing banking, but not breaking it. The Banking Standards Board, on the other hand, was an industry initiative – i.e. a form of private regulation. What was the point of having two different bodies simultaneously intervening on bank culture? Why have public as well as private regulation? At the time I felt, as Ossandón and Onto (2026) suggest, that everyone but the ethnographer (myself) knew the script of the play I was part of.

Why, then, was I tapped to collaborate with the Banking Standards Board? I had been following the global financial crisis unfold since my arrival at the brand-new Department of Management at the London School of Economics in 2009. My hiring complemented that of Yuval Millo in Accounting and Juan Pablo Pardo Guerra in Sociology (among others) to create an LSE team of scholars in the social studies of finance. Once the 2012 Financial Services Act was passed, it became clear to me that the British government’s response to the crisis was insufficient. With measures such as ring-fencing, bonus caps, or divestment, the Act was based on structural reforms to buffer risks and alter incentives. But if the goal was to change the behaviour of banks, years of research in organization theory strongly suggested that purely structural reforms would not be enough (Kotter 2012). Cultural, not simply structural, reform was also needed. In the absence of cultural change, bankers would simply find ways to game the new structures. But to my surprise, few people seemed to publicly voice this worry. I started to share my concern by organizing public events at the LSE’s newly created centre to structure research on the financial crisis, the Systemic Risk Centre, in 2013 and 2015 (Beunza 2015; Beunza, Andreeva, and Zigrand 2013).

In sum, by the time the Standards Board and I crossed paths, our alignment was not just the outcome of multiple translations, but also of broader economic and regulatory developments – the financial crisis, the creation of the FCA – as well as other local events such as the founding of a Department of Management and the Systemic Risk Centre at LSE, not to mention my public events. The outcome differed from the liminality noted by Geiger and Gross (2022) because I remained a full-time member of the LSE, but was in other ways similar because I was never an employee of the Standards Board.

At this point, it is worth dwelling on an ontological question that Ossandón and Onto (2026) bring up. How should the new economic sociologist behave? The Callonian dictum, in their gloss, is ‘do not argue against economists.’ But this barely offers an exhaustive guide in meetings.

I filled in the blanks to the best of my abilities. My demeanour was professional, neutral, and constructive. I wore a suit and tie to the meetings with the BSB, arguably with a level of ease that comes from wearing formal dress regularly in the classroom. Further, in my conversations with BSB employees, I translated Callon as ‘do not rally against bankers.’ I instead asked ‘how’ questions, such as how to better understand bank culture.

After some negotiations, we agreed on a module titled ‘Ethnography Workshop Series,’ comprising five two-hour sessions with 20-odd attendees from six different banks over several months. A key design decision was to include a group project that entailed actual fieldwork, ideally in their own bank.

I started teaching in February 2017. The first session, held at LSE, felt exhilarating. I was originally concerned that any critical comment on the City’s culture would be met with antagonism, but I soon realized the participants seemed to be on my side. They were self-critical. They were also political allies: participants came from all the large retail banks, typically from areas such as HR, risk management, and audit – in other words, the functional (not profit-making) areas. One large bank sent as many as seven members. Further, the participants seemed to understand my accounts of Malinowski’s motivations for doing fieldwork, as well as Agar’s idea of ethnographic breakdown. They first practiced observation by wandering around Lincoln’s Inn Fields and taking notes. Although one third of participants informally stopped attending, when, after four months, the surviving participants presented their observations, I was impressed.

Following positive participant evaluations and media interest, the workshop was offered again in 2020. It was delivered online, reoriented towards Equality, Diversity, and Inclusion, and a third edition in 2021 focused on Employee Resource Groups (i.e. internal corporate networks). These two workshops also gained a specific orientation towards change and were specifically labelled ‘Change Ambassadors.’

My collaboration ended soon thereafter. In 2021, the Banking Standards Board was renamed the Financial Services Culture Board in an attempt to attract memberships among insurance companies and other non-bank organizations. In 2022, I received a phone call from one of the Board’s leaders explaining that the organization would be winding down. One of the largest sponsoring banks, it turned out, had decided to pull out its funding. The gap was too much for the others to fill. In addition, he mentioned a speech given by the Tory Chancellor of the Exchequer Jeremy Hunt in which he signalled a post-Brexit gradual shift away from the previous focus on banking regulation and financial stability and toward competitiveness and growth; this speech subsequently came to be known as the Edinburgh reforms.

Indeed, at the time, banking deregulation was not limited to the UK but seemed instead to be a sign of the times. The first Trump Administration had eased restrictions for small and regional banks in 2018, relaxed the constraints barring proprietary trading imposed by the Volcker Rule in 2019, and the Federal Reserve lowered certain capital and stress-testing requirements, particularly for small and regional banks. At the time, I was already familiar with Abolafia’s (2001) finding that financial regulation is often followed by deregulation in a way akin to Polanyi’s double movement (expansion of the market, followed by societal pushback). For this reason, I was not surprised by the Edinburgh reforms nor by the closure of the Standards Board – though I disagreed with the former and was saddened by the latter. More importantly, I felt that the script of my personal play had finally been revealed. Mikes and Power (2024) go as far as suggesting that the bank culture agenda was purposefully put in place to deflect stricter structural banking reform.

There were two additional ethnography workshops. By 2021 I had moved from LSE to the business school of City University of London. After years of conversations with my colleague André Spicer, one large clearing bank hired our business school to deliver another workshop. In 2024 I delivered another workshop to yet another large clearing bank. There was no follow-up contact in either case; one possible explanation is the lack of industry interest in banking reform.

To conclude, the scale and scope of my efforts at shaping the UK’s financial sector through ethnography were not determined solely by the internal dynamics of translation, equivocation, or para-ethnographic collaboration. They were also structured by political cycles that alternately privileged

reform and competitiveness. The Banking Standards Board emerged during a reformist moment in which culture functioned as a politically viable language for intervention; it dissolved when that moment gave way to a renewed emphasis on growth and deregulatory flexibility. If this account holds more broadly, then market-making by ethnographers is not merely a matter of methodological ingenuity or relational positioning. It is temporally and politically contingent. Ethnography can rub against markets, and perhaps reshape them, but only within the windows opened by regulatory conjunctures. Understanding those conjunctures is therefore as important as mastering translation.

## **Frustration, naivety and uselessness of ethnography**

***Eugênia Motta***

Ethnographies are processes of encounter and of writing about them. To do ethnography is to build relationships and describe them, in the hope that we might learn something about our existence as humans. That is what ethnographers do. Despite the question driving this special issue – whether ethnographers also make markets – the authors, appropriately it seems to me, do not attempt to answer it. The articles deal, in fact, with the frictions produced in encounters with engineers, economists and architects, who are, apparently, quite convinced that yes, they do make markets.

Frustration seems to be the common thread running through the articles, each one trying to draw lessons from it about the social world of market-making, and so move closer to understanding how markets are made. It is a typical ethnographic move: drawing on what necessary reflexivity can produce that is useful, allowing the initial questions to be bent. Continuing the authors' analytical movement, I propose one more turn, in a spiral, to carry the reflection they proposed a step further, but turning the question back onto the assumptions underlying it. Frustration begins and is produced by expectation. Expectations are made up of more or less informed anticipation and something of hope. My argument is that they rest on a certain kind of naivety, a hallmark of ethnography as an intellectual practice, which is worth thinking about.

Naivety is necessary for the ethnographic project and, in part, it is by being naive that we learn as a necessary basis for analytical estrangement and openness to the relationships we build 'in the field.' As a disposition to be learned and cultivated, it is what sensitizes us both to fieldwork as everyday practice and to nuanced analysis of data that only becomes ethnographic when treated with due surprise. Beyond that, naivety is also a common element in the rhetoric of ethnography as a textual style, something we also learn to reproduce and then incorporate as a discursive practice more broadly. Personal discovery and analytical conclusion blend and blur, at times dangerously.

There is still a third form in which the naivety of ethnographers appears, one that comes from our limited imaginative capacity in relation to certain social worlds, not coincidentally, those that are, in some ways, closest to us: the universe of experts and scientists like ourselves. With a lower capacity to project about these relationships, the hope of being accepted and having one's voice heard may prevail. This may be the most important issue to confront. Why is it easier to believe in spirits embodied in animals that come to bring predictions about the future to the living than in the sincerity of a statistician working at the ministry of finance? It is quite possible that this relates to a fundamental contradiction on which ethnography rests: a somewhat positive and systematic claim alongside a comprehensive and highly relativist disposition, with both poles duly identified respectively, with solitary analysis on one side, and on the other, with a relationship to an otherness, in the field, that stands in opposition to this style of thinking. Positivist and Cartesian natives do not fit so well into that scheme.

Encounters with them force us to raise uncomfortable questions about our relativism. To what extent do we take seriously what we hear in the field? If we know that the self-regulating market is a fiction and a probably destructive one, what do we say about other people's and other groups' fictions, about their historicity, situatedness, and perhaps their role in maintaining hierarchies that disturb us morally? The questions that these 'others,' precisely because they are so close to

us, send back as an echo of our own comprehensive disposition and intellectual morality, threaten our image as generous intellectuals. But it is not only our relativism that market-makers challenge. They also force us to acknowledge our own realism, which is necessary for empirical research. People, places, and at least some things have to exist for us to go out and engage with them.

The articles in this issue thus help us think about ethnographic styles and objects more broadly. It is common for ethnographers to be far more interested in deviant and marginalized people and groups, and few choose to do research on the rich, the powerful, and the technocrats. Beyond the obvious difficulty of accessing these people, the issue is one of interest. These people and spaces challenge us in uncomfortable ways or rather, they make us uncomfortable in a way that we do not find as interesting as the discomfort generated by encounters with other, minority, subaltern, dominated forms of otherness. I think at least part of the answer lies in a kind of frustration that, unlike prevailing fashions and traditions, this group of authors has decided to confront.

The necessary naivety of the comprehensive effort par excellence, which in a certain sense defines ethnography, finds its antithesis in the professionals of social transformation. They are quite certain about the existence of the world, of causes and effects that are potentially controllable. The task is to describe them well and calculate the costs of intervention. For this true architecture to be assembled, many ethnographies have already shown that the question is not about carving out an object properly (that is what we do), but about rendering irrelevant whatever does not fit within it. In this sense, the difference is radical, perhaps irreconcilable. It seems easier for someone trained in holism and complexity to believe in the spirits of the dead walking the earth than in the synthetic truths of charts and tables.

So far, I have dealt with principles of worldview, epistemology, and intellectual dispositions. But ethnographers have always done research in the real world of money needed to fund research, of contacts that allow access to the field, of strategic adaptation of vocabulary. It is true that this side of our practice is generally kept hidden, because it lays bare the utilitarian, self-interested, and politically ambiguous character of many of the relationships we establish with people and institutions. This is courageously confronted by the authors in this issue, and it should serve as an invitation to be accepted in all its radicalism. In times of denialism and anti-intellectualism, it is good that we ourselves are the ones to disenchant academic practice, openly addressing the conditions that make it possible, so that we may (re)build collective commitments that have been eroded in the name of putting an end to hypocrisy.

The immediate effect of these texts, for most readers, should be the discomfort generated by identifying with the authors' misadventures. Perhaps alongside that, a certain despondency about our capacity, as social scientists, to be relevant. But for me, it is also a relief that ethnography can still be, not despite, but sunk deep in its own contradictions, a way of seeing, thinking, and speaking about the world that does not fit well into the effort to manage and format human life, and even less so through 'markets.' That for this purpose, it may be, perhaps, useless. Even more heartening is the fact that, accustomed as we are to acknowledging our ambivalences, ethnographers do not shy away from trying to make the world better, even if that is (almost) impossible.

## Things ethnographers can do (if they can't do ethnography)

### *Philip Roscoe*

To read the opening paragraphs of Ossandón and Onto's introduction to this engaging, if slightly depressing, special issue is like standing in a hall of mirrors. We have the editors as ethnographers of ethnographers, synopsisizing the contributions. Their text begins with an excerpt from Gillian Tett's piece in the *Guardian*, itself excerpted from her book *Anthro-Vision* (2021). Tett, as any fule kno, predicted the credit crisis thanks to a grounding in Tajik tribal rituals gained during her doctoral studies at Cambridge. (It's a good story, but Tett's investment bankers were perhaps not such an unfamiliar tribe – it's hard to imagine a kid from Middlesbrough dropping into that planning retreat on behalf of the mighty *FT*, anthro PhD or none.) In José and Gustavo's excerpt, Tett is

using our colleague Daniel Beunza's work as a way into a promo for her own 'anthro-vision' take on life, her book, and doubtless her consultancy services and after-dinner repartee. Daniel was justifiably delighted at finding himself described in the *Guardian* as a 'voluble social scientist' but one can't help feeling that Tett's summary of 'sensemaking' did not do proper justice to 20-plus years of careful ethnographic work in the social studies of finance. Tett, who clearly has patience with middle-aged male scholars of the social studies of finance tradition, soon found herself sipping 'sparkling water in a midtown café' with associate editor Koray Caliskan; the interview was reproduced in the JCE (Caliskan 2023). Her take? It's culture, innit!

The hall of mirrors is an apt metaphor for the issue. We are dealing in reflection, translation, obscurity. Tett, by her own admission, is prepared to make compromises: 'I know that a lot of people may be seeing me as a sell-out because in their mind I work for capitalists ... . Unfortunately, there's a trade-off. You can't communicate what you really think about the world without some kind of a compromise with it' (Caliskan 2023, 133–134). She is right that senior managers in organizations suffer from 'mono-vision,' their understanding restricted by the reports and numbers that structure organizations. From an actor-network inflected perspective more attuned to this issue we might call them 'cyborg managers,' their cognition tangled in the performativity of the organization's sociotechnical devices (Roscoe 2020). Tett sees compromise as a mechanism to get beyond this tunnel vision, a deal signed off at the revelation of the cultural aspect of any decision (the engineers didn't see the meeting like this, but like that!) where culture becomes, like behaviour, a factor to work into the service design workshop, if not the spreadsheet.

The empirical studies in this issue suggest that this compromise is more of a condition, at least in the minds of the market designers, the engineers and the policymakers. The compromise has been unilaterally done before the ethnographers even walked through the door. The market ethnographers are there to supply 'culture' as an exogenous variable, where and when it fits. The clients or users are always other: there's a telling outburst from the Danish engineers asked by Pallesen what they would do in the circumstances. As Ossandón and Onto point out, culture is often experienced in the world as having a concrete quality. Culture as imported by the market ethnographer is too rich, too sociotechnical, too disruptive. It is too slow and expensive. Rather than an answer (why are those Brazilian consumers so profligate? It must be their culture!), it is an identity, a 'legitimizing artefact,' a strategic resource for actors on both sides. It is a source of discord, failing to deliver the KPIs, or sparking the imagination in ways that a head of unit cannot tolerate. The only episode that records even partial success is Våland et al.'s (2024) story of the 'industrial researcher' in architectural practice. We notice that the ethnographer was previously – already – an architect. Perhaps she was more able to make something useful from the dissonance that occurred on that account.

Anything more than 'anthro-vision' seems too much. The cyborg manager is just too strong. One wonders how much even those legendary economist-engineers are successful, and how much we market ethnographers are guilty of doing exactly what the critics have always said and writing economists' whig histories for them (Christophers, 2014). What did happen to that strawberry market in the end? Geiger and Gross' testimony suggests that all of the researchers, not just the ethnographers of markets, suffered under the yoke of the KPIs and the lash of the centre Director. The papers, as the introduction makes clear, 'dwell on tensions, uncertainty and ambiguity;' in the hall of mirrors, all is distorted and obscured.

There also emerges from the contributions a deep disquiet with the positionality of expertise. I find this discomfort again on rereading my own paper, with Olya Loza, which is so generously quoted in the introduction (Roscoe and Loza 2019). It recounts my failed flirtation with practice (seven years on, still a failure). Notwithstanding the shared gloom and discomfort, I recall that we worked on the piece as a positive intervention. We hoped to foreground the power of words to change the world, even one as noisy as the one we live in. If, we wrote, the central claim of market studies:

is that market knowledge is performative – that it participates in bringing markets into being – and if economics offers just one kind of market knowledge, it follows that the economists' markets – the markets of high modernity – are just one kind of market, and there are as many others possible as we can conjure in our writing. (216)

The operative word in this passage is not knowledge, or performative, but conjure. The act of writing, of putting words on paper or text on a screen, is a work of magic. The battlegrounds of our present world, be they crypto, gender wars, or even populist politics, have genealogies that link them to communities of the written word. So do cyberspace and the metaverse; as Neal Stevenson (1994) remarks in the foreword to his charmless *Snow Crash*, many people have spent a great deal of resources trying to build the things he imagined.

Rereading the paper, I was surprised to see something else emerging from the autoethnographic vignette. I recalled that we were talking about words, when in fact we wrote about a text. The article tells of the birth of this text, which is subsequently set free to travel. We write that ‘the author [me] has done something in giving rise to the text,’ the anxious italics very much in the original. The text travels and is translated; it is enrolled in networks and appears in odd places; it is agency-filled and capable. The text is a socio-technical entity. It is out of control: ‘the author wrestles with his text and loses,’ we add, while the text ‘enacts novel performances’ (p 223). None of this would be surprising to students of literature, for whom criticism is a practice of contesting authorial interpretations of a text and texts are a fluid medium through which other points can be made. It may be a surprise to social scientists, but words, having been set down, can assume an independent life.

Collectively, the papers can be taken as a call to discover and inhabit a different kind of identity from the one that is assigned by the kind of relationships set out in the papers and – unfortunately – envisaged in the metrics of government funders and our own employers. I am struck by a line in Geiger’s vignette, after the project PIs have been chastised for being ‘too academic’: ‘What else would we be if not academics?’ she wonders, ‘How strange it was to be criticized for the one performance I truly knew how to inhabit.’ (Geiger and Gross 2022, 6). Geiger finds a way as an academic activist, ‘relieved ... of the need to pose as expert’ but still very much in possession of her expertise. Activism is just one of the performances or identities available to us, and like any other, it demands its textual interventions. We have our texts. Academics in literary studies, critical theory, political economy, geography and numerous other fields are exploring this possibility, drawing on the notion familiar to readers of the *JCE* that the constitution of markets is narrative, textual, and discursive. The imaginary futures of markets are rich in possibility, perhaps, so long as we recognize that they depend also upon an imagined past.

At the end of *Markets in the Making* (2021), Michel Callon makes the argument that changing the way a market works is a matter of changing its framing. This is a radical move from someone whose career was so tied to the careful, emic critique of the STS method. But it is essential. To do otherwise is, as Geiger and Gross (2018) have shown, just applying a sticking-plaster to structures that are fundamentally unfair. As writers of markets our texts can resonate (Bresnihan and Millner 2023) – through the curriculum, the seminar room, the activist engagement, the practitioner forum. We can seek out examples, or we can imagine possibilities. We can reverberate with parallel projects such as the ‘postcapitalist politics’ of Gibson-Graham (2006). The notion of resonance (Bresnihan and Millner 2023) pushes against tidiness, abstraction, managerialism, and the search for unified answers (it’s culture!). It is comfortable with tension, uncertainty and ambiguity. Pallesen and Ossandón propose that market studies have not yet found its ‘psycho-social type,’ the social persona that unifies a discipline, and the papers suggest that it won’t be the market ethnographer as cultural engineer. Market civilizer? Anthro-seer? I’m hoping for something wilder, darker, unruly, less boardroom-sanitized – and that, whatever it is, it resonates.

## Dealing with devils

### Don Slater

I have to start with a confession: as an old guy who cut his academic teeth in the 1970s, I was intellectually raised in an environment of Marxist theoretical purism, one in which all the research relationships covered in this collection came under the headings of co-optation, complicity and

collaboration (in the pejorative, wartime sense of ‘fraternizing with the enemy’ or ‘selling out’). This kind of engagement was generally met with the deepest suspicion of colleagues and comrades, at least until the rise of ‘impact’ metrics. Entering into such projects – which I’ve done throughout my career, from PhD onwards – was always constructed in my head as ‘doing a deal with the devil’: I would barter promises of ‘useful knowledge’, ‘findings’ or ‘good PR’ in return for research access to the belly of various beasts (in my case, advertising agencies, market research, digital enterprises, development agencies, now lighting designers, city councils and urban design practices). In the early days, I saw myself as a kind of gonzo guerilla academic, working undercover, pledged to give back to these ‘collaborators’ as little as possible. I was entirely clear about my position: I didn’t want to provide any useful fuel for the capitalist machine; I wanted to get back to my department as soon as possible and tell them what I had found in order to feed the theoretical machines we were building backstage.

In most respects, I have shifted entirely away from this stance. Particularly over the past 15 years in which I’ve been involved with lighting and lighting designers, every project has been conceptualized from the start as negotiated collaborations and tactical alliances with practitioners and their clients. However, what has always stayed with me is the sense of ethical and political danger, the ambiguity of my positions and positioning in these relationships, and the need for both a deep ethical anchor and, at the same time, opportunistic and fancy footwork to keep at least roughly on an acceptable path. I still don’t necessarily want to be ‘helpful’ or ‘relevant,’ or only in relation to my own critical red lines. In doing any ethnography – not just the kinds of collaborations under discussion here – the process involves continuous and unresolvable pragmatic calculations and negotiations as to the terms on which I enter a field, why I would want to enter it, what I want to give, and what I hope to take. These are simultaneously political, ethical and personal choices as to what worlds I want to support and on what terms. They are also calculations as to what might emerge from the collaborations and with what cost to myself and others. And they are matters of negotiation because I am situated in my own academic world with my own sources of power, identity, anxiety, advancement and career: for years, I was negotiating from an academic base that coupled the privileges of ‘academic freedom’ with the constraints of performing an academic market (or in Bourdieuan terms, calculating forms of academic capital (the normative forms of CVs and REFs)); indeed, I think some of the articles in this collection could spend more time on the institutional fields that we come from as they do on the ones in which we seek to intervene.

This collection makes for seriously useful if sometimes painful reading precisely because it is largely written from a similarly pragmatic sense of what happens when you get your research hands dirty. I’m not going to disagree with anything. What I’d like to underline more strongly is the sense of danger and ongoing negotiation, of how we try to maintain a political/ethical rudder as we negotiate these engagements. I sometimes feel that the ‘performativity’ lexicon somewhat neutralizes what’s at stake as we move through these spaces.

On the one hand, ‘performativity’ entered analytical language via a critique of representation: our intellectual outputs are not so much truth claims as disturbances in the fields we study. This was a critique of false objectivity (and we should remember that it came from multiple sources – not just ANT but analytical philosophy, phenomenology, feminist theory, Foucault); and it was a *mea culpa*; it was about recognizing our complicity in the construction of a world that we pretended merely to observe. On the other hand, however, the concept of performativity has increasingly taken a more positive form: the complicity of representation can also be the basis of engagement with the world: the concept now links with co-design, co-construction, participatory methodologies, citizen science to promise new forms of articulation and empowerment. But with that re-birth of academic activism come challenges that the concept of performativity doesn’t necessarily help us deal with, above all ethics and power.

The move to engagement raises the oldest questions about ethics and value in social research. My older Marxist self was confidently guided by a political gyroscope toward some kind of emancipation; clearly not an option for many decades now. What’s left are basic questions of what I am trying

to do, to further, to support, and why? Where are the lines I won't cross? Who will I work with and up to what ethical cost? And in terms that the older Marxist would recognize, when does a dodgy collaboration look permissible as a means to a valid political or intellectual end? I would dearly love to see more of the pragmatic pitfalls that are discussed here posed at a higher level of ethical and political values and dangers. Recognizing that I am participating in 'inventing the social' doesn't resolve questions as to what social forms I ultimately want to support or construct. We come full circle to Weberian questions of the sources and choices of substantive rationality as we negotiate formally rational systems.

As to the power issue, the situation is really rather clear, and each of these articles exemplifies it. The answer to the question, 'Do ethnographers make markets?' is very obvious: No, of course not. Ethnographers are brought into economic and other institutions on very specific terms and on sufferance after the main structures have already been solidified. Callon originally applied performativity to these debates in a very specific way: to diagnose the role of economics. Market structures are not natural law but institutions whose production includes circulating discourses and the construction of specific devices on the basis of rather generalized blueprints. In 1990, you could literally watch Chicago economists arriving in Prague, Warsaw, Berlin, and Moscow to script the new regime of truth and power. You did not see sociologists and anthropologists on the same planes. Similarly, we've seen the rise and rise of forms of behavioral and biological psychologies that could perform versions of the 'human' as a natural calculating machine, one that tallied with these markets and the marketization of just about everything. Again, we have not seen culturalist and social versions of personhood performatively significant in economic policy or practice. Instead, we got a couple of decades of 'nudge.'

So the question for ethnographers is clearly what confers the power to be performative and on what terms? This is both an empirical and analytical question; it is also one which I have to answer differently in each research engagement. This collection is very strong and vivid on the matter of how little formatting of economic behaviours and devices the authors could effectively do; it is less strong on analysing why we are so limited. Or put differently, why are we more often performed than performative. I think we need a more macro account of why economic and behavioural forms of knowledge are so preeminent and so able to position socio-cultural knowledges in unthreatening nooks and crannies of institutional processes, or to radically misidentify our forms of knowledge ('surely you can provide me findings that will provide an evidence base for this lighting infrastructure investment'). In my own research experience, the basic answers are simple enough to fit into a short response piece and lead me back to long-term critiques of economic and technocratic rationality: socio-cultural relations appear from the standpoint of governmental and economic rationalities as an externality, as a residuum or artifact or variable or deformation that is appealed to after economic models have reached their limit or failed. So on the one hand we are tasked with supplying all the nebulous stuff like values and meanings and people – the glue which holds a monadic world together – and on the other hand, we are asked to reduce that stuff to a pseudo-numerical input, or finding, or datum that can be bolted onto an already existing managerial model. Part of me still believes, like Comte, that sociology is the queen of the sciences because it integrates everything else; the people I work with believe precisely the opposite. Indeed, I have gotten more research opportunities by fitting into a PR role than by promising useful knowledge: many firms and public bodies are willing to pay a researcher in order to be able to say, in a press release, that 'according to research from the London School of Economics ... ..;' or to say to a council or community that, 'according to a consultation organized by sociologists from LSE.' I supply a signifier or simulacrum of 'the social' and its presence within their governmentality. Again, what kind of deal am I making with what kind of devil, and to what extent can I justify or indeed live with it? And again, the question is much less about mulling over my own performativity in making markets and much more about how I am being performed by a much larger institution-making apparatus.

The crunch generally comes over questions of methodology and complexity, at the point where I need to say, I'm not here to answer your question with findings or evidence-based policy

recommendations, but rather to say that you are asking the wrong questions, that you need more complexity, not reductionism. Often in the form of new methodology: for example, although most public lighting decisions and investments are driven by claims about their impact on safety and security, there simply is no generalized correlation between light and crime or fear of crime, and there never will be. The relationship between, for example, gendered access to public space and urban design elements like lighting, can be entirely different from one street to the next, one demographic to the next, one season to the next. It cannot be resolved abstractly; it has to be responded to by designers and planners on the basis of a location-specific, ethnographic understanding of this social space. Tell that to a lighting engineer tasked with maintaining 100,000 lights across a medium-sized city with zero budget or staff and locked into a PPP contract for the next 25 years.

The reality of negotiation and collaboration in these, and most, cases generally comes from another dynamic that I want to conclude with: the heterogeneous nature of the collaborators with whom we work and, therefore the opportunities for alliances with specific groupings or departments, and the strategic ambiguity we can activate in order to get on with joint work or to create a framework for developing a common understanding. In almost every case, the actual effective ‘collaborator’ is one department, working group or individual who identifies their ‘problem’ in more or less socio-cultural terms, and sometimes – e.g. in my lighting research – as part of an internal struggle with economists, engineers, or managers that very much parallels my own. It is not so much that the non-academics we work with are themselves ethnographers (after all, everyone needs to make sense of their lifeworld); it is more that the situated knowledges that are most important in their actual work, the knowledges on which they act, are tacit, informal, or even illicit, everyday understandings that are not admissible in meetings, reports, or policy documents, understandings that are often coded as ‘subjective.’ Ethnographic alliances are often formed on the basis that reality lies here rather than in the virtuality of formal models. I present myself as the potential ally who can legitimate their way of knowing, as much as they appear to be a good way in for me. I actually started working with lighting designers precisely because their work is based on a deep engagement with the way in which light can support very diverse and specific forms of action, with the pragmatics of using space; therefore, there was a kind of natural affinity with ethnography; at the same time, they were in a subaltern role in relation to engineers and project managers who only allow them to articulate the language of generic standards and regulations.

I would like to think that our collaborations could be characterized by an opportunistic but responsible embrace of ethical and political danger, one that requires us constantly to question ourselves and refine our position as to whose interests and values we are supporting and what kind of good we think we can do.

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