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What was sociology?

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Abstract

This article is about the future of sociology, as transformations in the digital and biological sciences lay claim to the discipline's jurisdictional claim over 'the social.' Rather than analyse the specific of these transformations, however, the focus of the article is on how a narrative of methodological crisis is sustained in sociology, and on how such a narrative conjures very particular disciplinary futures. Through a close reading of key texts, the article makes two claims: (1) that a surprisingly conventional urged towards disciplinary reproduction often sometimes animates accounts of sociology's crisis; (2) that, even more surprisingly, these same accounts are often haunted by a hidden metaphorical architecture centred on biology, vitality, vigour, and *life*. The central gambit of the article is that, perhaps in spite of itself, this subterranean image of life actually hints at less reproductively conventional ways of understanding – and intervening in – sociology's methodological 'crisis.' Drawing, empirically, on the author's recent work on urban stress, and, theoretically, on Stefan Helmreich's (2011, 2016) account of 'limit biologies,' the papers ends with a call for a 'limit sociology' – a form of attention that could, similarly, expand rather than contract the present moment of transformation. At the heart of the article is a hope that thinking with such a limit may at least help sociologists to imagine a less deadening future than that on offer from a canonised discipline cathected by endless crisis-talk.

Keywords

crisis, life, method, sociology, urban

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Introduction

In 2007, Mike Savage and Roger Burrows published a paper called ‘The Coming Crisis of Empirical Sociology’, which would go on to become one of the landmark British sociology papers of the decade.¹ The paper was published in *Sociology*, the foremost publication of the British Sociological Association (BSA), and centred on what Savage and Burrows took to be a transformational moment in the generation, collection, and analysis of sociological data—as that data was becoming more and more entangled in new digital technologies, and the private institutions that controlled them. The paper asked: what happens to the methodological jurisdiction of academic sociology, when central nodes of the digital and algorithmic economy can gather infinitely more social data, in in an infinitesimally smaller amount of time, than even the most dedicated team of university-based sociologists?²

The authors described a salutary incident in 2005, when Mike Savage, then Professor of Sociology at the University of Manchester, went to a methods festival organised by the UK's Economic and Social Research Council (ESRC). Savage was presenting on a social network analysis that mapped the ties and affiliations of hundreds of people working in voluntary organisations. After his talk, he bumped into a man who was also interested in social network analysis – but who was not, as it turned out, an academic. In fact, this man worked for the research arm of a large telecommunications company; and as Mike Savage talked to him, it became apparent that, in putting together his own social network studies, the man had access to the records of every phone call, ever made, by everyone connected to his company's network. Compared to Savage's own study of 320 people, this man, and the large commercial organisation he worked for, were sitting on a data mine of, literally, billions of social ties. 'This is data,' lamented Savage and Burrows, 'which dwarfs anything that an academic social scientist could garner' (2007: 887). Worse: 'it was data that did not require a special effort to collect, but was the digital by-product of the routine operations of a large capitalist institution' (ibid.).

How are we to think about the future of sociology in the face of such challenges? How, indeed, should we imagine that future not only in the face of *these* particular developments, but in relation to a much wider suite of more-or-less 'social' transformations, now taking place in the digital and biological sciences? (Marres, 2012; Meloni, 2014). This is the question that preoccupied Savage and Burrows in 2007 – and it preoccupies me too, more than ten years later. But not because I think it is necessarily still worth answering (or that it is actually answerable). My interest, rather, is in what laboring under such a preoccupation does to sociology – in how we should actually relate to a discipline that has become so unerringly preoccupied by its own constrained horizons. I am concerned, in other words, with how we should think about an intellectual practice, a discipline, and a set of scholars, for whom such

fundamental – indeed, existential – questions of jurisdiction and purpose are now coming into view. So, rather than offering yet another analysis of sociology’s shrinking intellectual territory, in this article I want to focus on how sociologists have actually thought, narrated, figured and become anxious about this same terrain. That may look narrow, and a bit parochial. But my gambit is that such a focus might help us to think through the history and present of ‘discipline’ more generally, and to do so, perhaps, in more generative registers than are currently available. First, and this will remain largely implicit in what follows, I want to draw attention to what I call, following Lee Edelman (2004), the ‘reproductive futurism’ of British sociology; in using this term, my goal is to explore some ways of thinking about the methodological future of academic sociology *without* the gestures through which sociologists’ try to reproduce themselves, and their institutions, and their methods, in the face of encroachment and crisis (see e.g. Holmwood, 2010). The first question that guides me here is: what would come into view if we could re-imagine the future of sociological practice, but do so in the absence of a will to disciplinary reproduction? Second, and this is very much on the surface of what follows, I will show that there is another figure haunting these discussions – which has not yet been made explicit, but which may nonetheless offer very different resources for thinking about the temporal contingency of sociology and sociological method – and this is the figure of *life*. In what follows, I argue that a sense of life – its precarity, its fragility, its ebbing-away – sits at the centre of these discussions, though it is a figure rarely acknowledged there. A second guiding question then is: how might explicitly re-centering this discussion on life, or more accurately on *the limits of life* (cf. Helmreich, 2011), open up a more compelling framework for making sense of precisely the moment of transformation that these discussions are trying to understand? What would happen if we re-read reflections on methodological crisis as ways of thinking about sociology’s relationship to living things? What if we re-diagnosed

what is, for Mike Savage (2010), an ongoing question in the ‘politics of method’ as, rather, a slanted intervention in the politics of life (cf. Rose, 2007)?

In the first and second sections of this paper, I approach these questions through in-depth readings of two influential accounts of the present and future of sociological method – one by Mike and Savage and Roger Burrows (2007), introduced above, and another by Les Back (2012). I will show that these accounts are, in fact, shot-through with strikingly vital, biological, and organic imagery. My central argument in these sections is that this imagery is not incidental, nor is it merely illustrative of the (in fact) quite different desires of the two sets of authors. I argue, rather, that the parapraxical emergence of biological language and metaphor in these texts, at this precise moment, should be read as a certain kind of ontological anxiety – an emergent and still unarticulated sense that very different ways of inhabiting the social world, and thereby of intervening in it, are just over the horizon. In the third and fourth sections, I attach this observation to my own empirical interest in ‘life’ as a concern for sociological method, and here I will reflect on some of my current work, which is broadly about the biopolitical entanglements of urban stress in contemporary megacities. Situating myself in a very specific sociological scene, and drawing especially on the work of the anthropologist Stefan Helmreich (2011, 2016), I will argue that it is less a sociology of reproductive futures and more a *limit sociology* that can guide us through this ongoing moment of methodological transformation. Before moving on, let me note – and it is perhaps already obvious – that there are two different objects sliding across one another in this article, but perhaps never quite meeting – one concerns the life of sociology, the other the sociology of life.³ To be clear, it is not my claim that one follows necessarily from the other. But having identified it, my method in what follows is to take the metaphorical structure of sociological crisis-talk very seriously. How might things be different if we started to understand the biosocial metaphors that suffuse these texts as not only metaphors – indeed, perhaps not as metaphors at all?

More than twenty years ago, in this journal, Thomas Osborne and Nikolas Rose (1997) offered a set of theses about the forms of articulation and voice in which social thought has moved, historically, and within which social problems emerge. Towards the end of their paper, Osborne and Rose asked how we might make sense of the articulations of social thought in our own era, at a moment in which ““society” is no longer social, or, at least, not social in quite the same way’ (1997: 100). Their proposal was to attend to the ethical and practical inventiveness of contemporary social technicians, and the forms of ‘living thought’ that those techniques articulate: ‘one learns more about the conditions under which we have come to be able to understand our experience as “social”’, Osborne and Rose argued, by attending to an ‘applied ethics of investigation and intervention,’ rather than ruminating on ‘biographies and schools, or by reconstructing a theoretical canon’ (ibid.: 101). In this article, I will pick up the threads of this suggestion. I will explore what a new ethics of investigation, in this mode, might look like, and I will ask if it cannot yet move us beyond the reproductive futures of ‘crisis’ and ‘canon’.

Touching the nerve

Let me return to the Savage and Burrows paper with which I began. Certainly, the ‘Coming Crisis’ paper does a lot of interesting things in not much space. In one reading, its contribution turns on an argument about the epistemic designs of commercial institutions under late capitalism (what Savage and Burrows call, after Nigel Thrift, ‘knowing capitalism’). What does it mean, they ask, when the privately-owned digital fallout of everyday interaction turns out to be our primary means for ‘thinking the everyday’ (Thrift, 2005: cited in Savage and Burrows, 2007)? This is a really interesting question, but Savage and Burrows don’t really go into it. In fact, their overall focus is not so much on capital but on method—and, maybe more specifically, on the provenance of method (ibid.: 886). Savage and Burrows are attentive in

their paper to questions of methodological jurisdiction, including (and they are of course alive to this⁴) the very active politics of boundary-policing through which jurisdiction gets made. As they point out: for all its theoretical desires, British sociology actually survived the twentieth century mostly by virtue of its methods (ibid.: 888). Indeed, it is the research technologies of sociology – the questionnaire, the interview, the community study – which have garnered whatever limited prestige the discipline now enjoys.

Hence the concern: massive social-data-gathering technologies, and the private interests that own them, which are, simultaneously, engines of digital capitalism, enablers of social interaction, and repositories of high-quality data *on* that interaction, clearly have limited interest in the data-resources or empirical skills of university-based sociologists. And if this analysis holds fairly obviously for quantitative sociology, it is no less a problem for qualitative methods: precisely because of how these same enterprises have recast social life, outmoded research tactics such as interviewing, life history, ethnography, etc.—i.e. the basic methodological ground of most contemporary British sociology—cannot grasp the ‘myriad mobilities, switches, transactions and fluidities’ in which contemporary social life takes place (2007: 894). Interviewing is all well and good for ‘mid-range typifications of social actions’—but is not especially helpful ‘for generating sophisticated understandings of the diverse *weltanschauung* that pertain in contemporary societies’ (ibid.).

Well. How should we read this paper now – ten years after its first publication? In 2014, Savage and Burrows themselves (now writing as Burrows and Savage) published a follow-up in a new journal, *Big Data and Society*—the subsequent emergence of which, they are not slow to notice, at least counts as part vindication. As they point out, what is perhaps most remarkable about their original paper is how commonplace its arguments have since become: what was once ‘innovative and important’ has since become ‘a pretty mainstream position, not just in sociology but also across the cognate social sciences more generally’ (Burrows and Savage,

2014: 1).⁵ This is surely true. But another reading would say that a great deal of what is stake here (none of this will surprise nuanced scholars of jurisdiction, such as Savage and Burrows) is a certain kind of jostling for methodological, conceptual and institutional ground, among a particular generation of British sociologists, at specific moments in their individual careers. One way to read the 2007 paper, then—including the figures it critiques in passing, the scale of its reception and influence, its authors’ attempts to read it in retrospect, the methodological interventions that came after it, the (often strikingly sharp) debates around those interventions—would be as a familiar, anxious labour of legacy-leaving and career-making, as well as the larger work of discipline-shaping that is a necessary condition of this labour.⁶ And this all taking place among a group of (mostly male) senior sociologists, either then inhabiting, or coming to inhabit, professorial chairs at one or more of the self-consciously elite departments in the UK. Reproduction, we might say, sits at the centre of this scene.⁷ As Mike Savage himself points out elsewhere: the history of a discipline like sociology needs to be understood in ‘a messy, competitive context, whereby the roles of different kinds of intellectuals, technical experts, and social groups are at stake’ (2010: 237). It seems important, from such a perspective, to read interventions around the ‘Coming Crisis’ paper not only as analyses of the counting-work done by individual sociologists (quantitative or otherwise)—but as claims about the kind of sociology that is, in the future, going to count.

But what is most striking to me about Mike Savage and Roger Burrows’ retrospective re-reading of their paper, ‘The Coming Crisis of Empirical Sociology’, is that they root its legacy in the second half of its central compound: ‘empirical sociology’. Whereas the object that I find more prescient and urgent, but which is nonetheless largely neglected (maybe even part repudiated) in this later memorialization, is the first half of the title: the ‘coming crisis’. Indeed, I want to say that it is this feeling of crisis—and not a debate about the collection of data—that forms the paper’s ground, and accounts for its influence. The word ‘crisis’ itself, in

this reading, is not incidental. Not only is it a long-sustained trope in sociological lamentations about the state of sociology itself (see Gouldner, 1971), but the specific term, ‘crisis’, of course recalls histories of pathology and disease: in this scenario, the crisis, as the sociologist Robert Holton reminds us, marks ‘a particular stage in the development of an illness which is decisive for the future. The resolution of the “crisis” will determine whether the “patient” will recover or die’ (1987: 504).⁸

Recover or die. Does it over-read these texts to say that the very life of sociology is at stake here? I don’t think that it does. For example, noting that the discipline is far from the critical vortex that it was in the 1960s and 1970s, Burrows and Savage, in their 2014 paper, suggest that there is scope to re-think the assumption ‘that the discipline of sociology was bound to exist’ (2014: 2). What they describe is thus not simply a parallel world of quasi-sociological work in the commercial sector, but in fact a ‘major nail in the coffin of academic sociological claims to jurisdiction over knowledge of the social’ as such (ibid.). And what is needed in the face of such existential threat is not an academic debate, but rather ‘a sort of sociological call to arms’—a campaign that will ‘reinvigorate a sociological imagination’ for the twenty first century (ibid.: 2-3). The authors evoke the same imagery in the face of critique: ‘whatever the quality of our article might have been,’ they say to one respondent, ‘we certainly seemed to have touched a nerve’ (Savage and Burrows, 2009: 764).

It seems to me that such imagery—existential, vigorous, *nervous*— reveals important stakes of this discussion; it suggests that an intense collective anxiety about sociology’s life, about the prospect of the discipline as both a lively and a lifely endeavour, lurks below the surface of these methodological lamentations, and the anxious disciplinary atmosphere into which they have been received. Savage and Burrows are insistent that theirs is an intervention in the ‘politics of method’ (2014: 4), but as I argued in the introduction, we would do better to read their paper as a statement about the politics of life in and around sociology today. When I

draw attention to a politics of life, I mean it (as I think Savage and Burrows mobilize it, albeit implicitly) as an urgent attention to the possibility of any kind of lively, animated, buoyant, sentient, sociological discipline—a discipline that is alive to, and sustained in, the material, digital, and technoscientific hubbub of contemporary social relation. But I also mean it (again, I think this is already in the work of Savage and Burrows, just not on the surface) in the rather crass sense of *being alive*. Which is to say, what I think we are really talking about here is the simultaneously institutional and existential politics of being any kind of vaguely sustainable intellectual enterprise—of being a discipline that, in the future, will have any methodological niche to call its own.

In this article, I take the work of Mike Savage and Roger Burrows very seriously. But I am working to diffract it in a particular way: I want to take the diagnosis of ‘empirical crisis’ as a way into thinking much more squarely about the vitality of sociology within the transformations of the present — transformations which, such a formulation already reminds us, are not only digital and computational, but are wrapped up in emergent forms of biology too. In so doing, I am trying to shift us beyond the reproductive futurism of British sociology, to ask if there are not less conventional registers in which we might understand what’s happening here. I want to ask if we cannot say something bolder about what it might mean, for sociologists, when we somehow find ourselves touching a nerve.

Life methods

Perhaps the most compelling response to the ‘Coming Crisis’ paper came from Les Back, Professor of Sociology at Goldsmith’s College, in a paper that Back published five years later — this time in *The Sociological Review*.⁹ At the heart of Back’s paper, ‘Live Sociology’ (2012), is an argument that we are indeed confronted by a new reality in the production and appreciation of social relations—a reality that is non-linear, emergent, processual, digital,

mobile, and so on—but that the way to deal with these developments is not to play around at the edges of method; instead, and somewhat more directly, Back argues that ‘there are some aspects of sociological practice that we need to bury’ (Back, 2012: 20). Indeed, not only a burial, but first ‘an autopsy on dead sociology’ is called for — in order to eliminate practices that are ‘no longer vital’ to the sociological enterprise (ibid.).

What follows in Les Back’s paper is a combination of post-mortem and reanimation, in which Back roots out ‘lifeless conceptions’, ‘zombie concepts’ and (in a phrase borrowed from Albion Small), ‘fossil facts’ (Back, 2012: 21). ‘Dead sociology’ says Back, is ‘objectifying, comfortable, disengaged and parochial’; what is needed, by contrast, is a ‘vital sociological future’ — which will come from a kind of embodied, multi-sensory openness to the mobility of social life, and from a renewed attention to the craft through which that life gets registered (ibid.: 23). Such modes of attention, Back argues, are necessary for producing ‘vital texts,’ which will not only help us to bring sociology to life, but actually help sociologists — as, now, ‘organic intellectuals’— both ‘to live,’ and even to ‘sustain the life of things’ (ibid.: 34, 360).

There are many things that might be taken from this proposal.¹⁰ And if I am riding roughshod over the subtlety of his paper, I want to stress that I am much in agreement with Back’s diagnosis of the intensely stultifying nature of much sociological work, and especially the relationship of his diagnosis to the preferences of sociology journals that imagine themselves, somehow, the bearers of prestige. But again, I want to focus on the rhetorical undercurrent, and to draw attention to how Back constructs his argument through images of living and dying: Fossils. Zombies. Autopsies. Burials. Organic intellectuals. Vital texts. Lively things. An impetus for ‘assassination’. A desire ‘to live.’ What can we say about the stakes of such a vocabulary? It is possible, of course, to read too much into metaphor – both here and in the discussion above.¹¹ It is also possible that moving from a particular metaphorical architecture to a claim that there is gain in reading these debates as debates that are *also* about

the life and death of the discipline is a stretch too far. But the central gambit of my paper is that there is no coincidence in the fact that the most pressing empirical problem in sociology, in a moment of transformation and crisis, turns out to be, at the same time, a question of life. What strikes *me* as strange, rather, is that these images of life, which are intended to underwrite a reanimation of the present, nonetheless seems so peculiarly ... lifeless. Because it is remarkable to me that a 'live sociology,' as compelling a response to the 'crisis' as it surely is, is not a sociology of flesh and blood; it involves organic intellectuals, but not organic subjects; it draws on sensory methods, but not biological ones; there are well-laid plans for autopsies and burials—but none for measuring vital signs.

Let me offer two caveats at this point. First, there has of course been a great deal of discussion, in sociology and elsewhere, about rethinking intellectual practices as relations to and from living bodies. Les Back, indeed, has made major contributions to drawing out the conceptual and methodological heart of this discussion.¹² But when I ask about 'a sociology of flesh and blood', my question is not so much about, for example, the body 'as a political field' (see Back, 2013: 73-6) but something much more viscerally interior, more densely cellular, than these kinds of analysis typically allow. As Elizabeth Wilson points out, we have lately learned to be astute about 'the body' in social and cultural theory, and yet we remain somewhat willfully ignorant about *anatomy* (2016: 49). Second, when I call attention to a certain 'lifelessness' in this text, I don't mean this as any kind of aesthetic judgement—I only mean to say that, in these papers, 'life' seems to be taken as determinate, and as binary, and thus as an object of adjudication. Which is to say, if I may add my own gloss, that these are analyses in which sociology somehow must be dead *or* alive; that it has a future *or* it doesn't; that existence is sustained *or* quenched; that concepts are vital *or* fossilized; that the future is digital *or* analogue; that 'knowing capitalism' will put us out of work *or* make us rich; that we are all, in the final analysis, silicon *or* carbon, possible *or* impossible, buried *or* resurrected,

incorporated *or* zombified. This is what the image of life is made to do here: it not only adjudicates the future, but does so with reference to a set of conventional taxonomies drawn from the recent past.

And yet, as the anthropologist Stefan Helmreich (2011) reminds us, the theoretical object of biology, ‘life’, has lately become unmoored. This is not to say that the concept of life has dissolved into nothingness, but that the methodological and conceptual limits of life science—what Helmreich calls ‘limit biologies’—now push at the edges of what we think it might actually mean to be alive. By limit biologies, Helmreich means practices like astrobiology (the search for biological traces beyond earth) or oceanic microbiology (the study of deep-ocean microbial life) – i.e. endeavours that push at the edges of what biologists think is possible for sustaining vitality. At such limits, it is not simply that we find living things or only dead ones. It’s that what we come into contact with, what we encounter, and measure, and parse, actually expands our account of what life *is*, and what might constitute degrees of life in the first place. At the heart of these practices, says Helmreich, life ‘moves out of the domain of the given into the contingent, into quotation marks, appearing not as a thing-in-itself but as something in the making in discourse and practice’ (2011: 774).

Which is a useful reminder that when we fix on life (the theoretical object of biology) in our attempt to understand what’s happening to the social (the theoretical object of sociology), we find ourselves in difficult territory. In what follows, I want to explore this territory in parallel with Helmreich, who takes this development—this move ‘into quotation-marks’—not as an object of anxiety, but as a data-point. I especially want to think with Helmreich’s notion of a limit biology, and to ask what such a notion might do for the social sciences, at a moment when social life—now digital, developmental, embodied, processual, mobile, and so on—seems to be, once again, on the move.¹³ Temporality is at the centre of this analysis: Helmreich’s article, ‘what was life?’, is also included in his collection of essays concerned with

(among other things) ‘genres of time’ – from the premonitory time of the anthropocene, to soundwaves reverberating across time, to the simultaneous rapidity and languor of ocean time (Helmreich, 2016: 108). This wide-ranging attention to temporality reminds us that if the concept of life is now ‘wearing away’ and ‘coming apart,’ it is nonetheless doing so while entangled in practices that unpick ‘the very difference between now and the future’ (ibid.: 107-108, xiii, xx-xxii). In other words, to identify something with the category of the ‘was’ is not especially to condemn it to oblivion. To be crude, I am not here saying here that sociology is ‘over’, whatever such a claim could even mean. I *am* saying that theoretical objects and methodological practices – including ‘sociology’; including ‘the social’ – are co-produced with and through temporal relations that are specific even if they are not fixed, and that making better sense of those relations might help us to get some purchase on just what is going on with those same objects and practices at a moment of particular mobility, and do so without necessarily shoring up a now somewhat diminished present.

Where might we seek the limits of sociology then? What are the practices, objects and territories that are exploring, and pushing on, the limits of what we have called, and might continue to call, ‘social’ in the first place? To once again paraphrase Elizabeth Wilson (2016): might it be possible to think the social through vital (and even biological) agencies — but to do so while expanding, rather than contracting, the present moment of transformation? What might it mean to think the material present of sociology as a question of life, but to do so in the absence of a convention that takes biological data to be so determinedly binary — that takes biological concepts to be so ontologically inert?

Be calm

In March 2016, in its Rockefeller-sponsored online ‘Cities’¹⁴ section, the *Guardian* reported on a survey about the number of panic attacks experienced by people in different urban areas

in the UK (Fleming, 2016). The report and survey caught my attention for a number of reasons: first, they identified the cities of South Wales, where I live, as the most stressed in the country. In a table compiling the percentage of residents in a range of cities who reported a panic attack at least once a week, Swansea and Cardiff – the two largest cities in Wales, about 40 miles apart along the southern coast – occupied numbers one and three respectively (for contrast: London came a sanguine 13th). According to the survey, in Cardiff about 7% of residents experience a panic attack every week; in Swansea, the figure is over 8% (ibid.). There are of course many ways in which we might think the landscape of panic, panic attack, and panic disorder, in a story like this one.¹⁵ What particularly caught my attention, however, was how skillfully the journalist, Amy Fleming, wove a relationship between city living and stress around this finding: ‘it’s no surprise’, Fleming wrote, ‘that urban environments can contribute to the onset of panic disorder. Noise, jostling crowds, treacherous and painfully slow-moving traffic, lack of green, open spaces, filthy pollution, high crime rates and living costs, and social anonymity are some of the factors city dwellers say make them uneasy’ (Fleming, 2016). The article goes on to describe how an individual city-dweller’s brain mediates these relationships: activity in the amygdala is associated with city stress; an increase in the acidity level in synapses around the amygdala associates with this activity; and elevated levels of carbon dioxide (CO₂) in the air increase the acid. A chain of associations begins to emerge: stress and jostling in the urban milieu, competition, traffic, poor air quality, carbon dioxide, brain acidity, increased amygdala activity... panic.

But here was the most interesting thing of all: as I read on, it became apparent that the team that carried out the research was headed by the co-founder of a device designed to actually addresses precisely this link between CO₂ and panic attack – a device called ‘bcalm’.¹⁶ Bcalm is a small, inhaler-like object; it works by giving the urban dweller a space to expel her potentially stress-causing CO₂-laden breath, and to inhale, in exchange, air that has been

filtered by the device to have the CO₂ level of ‘forest air’. The bad air of the city goes out; clean, pure, forest air comes in:

In effect, your air supply has been ‘scrubbed’ of high CO₂... this CO₂ ‘scrubbing’ process is very safe, and is also used in anesthesia machines and in rebreathers used by divers. After about six or seven breaths, the CO₂ levels in your throat are back to a much lower level. This means your CO₂ receptor now sends a message to your brain, saying ‘relax, it's okay’. It feels as though you just stepped out into a forest, but you haven't needed to go outside....You can now get on with enjoying your day, without worrying about having another episode – after all, you didn't have an episode. Bcalm stopped your panic developing further.¹⁷

I want to pause, here, to acknowledge this one small artefact of what we might call ‘knowing capitalism’, and its knowledge of, as well as its proposed intervention in, the urban scene: how should we think about the affective, psychosocial and physiological weight of urban life in a world where a device like the bcalm makes a certain kind of neuropolitical and technosomatic sense? At stake here is a longstanding but still poorly understood ethnographic and historical object, which for the sake of convenience I will call ‘urban stress’ — i.e. the idea that there is an epidemiologically significant relationship between urban living and mental ill-health, a claim that is in turn embedded in an intellectual history that is woven through a very particular set of urban affects, dispositions, and experiences (see Söderström *et al.*, 2016). For the last couple of years, with my collaborators, I have been trying to think about urban stress in this way— to understand how such stress, as a social, historical and physiological experience, has come to matter across a range of scientific and cultural practices (see Fitzgerald *et al.*,

2016a, 2016b). My collaborators and I have especially been trying to think about the varied inheritances of urban ecology in that history, which we have then tried to connect to contemporary work on the embodied stresses of urban experience, and on the city as a space in which it seems increasingly hard to disentangle an interior from an exterior, a citizen from her milieu, a brain from a street. Of course we have been working to describe this landscape because of its intrinsic importance. But we have also drawn on it as a space for thinking this through the methodological practices of the social sciences themselves – and, in particular, the relationship of those practice to the technological and biological mutations in which they now find themselves.

It is worth recalling here that ‘the urban’ has long been an experimental testing-ground for sociological theory and method: perspectives as different from (indeed, hostile to) one another as human ecology (Faris and Dunham, 1939), Marxist and critical theory (Lefebvre, Kofman and Lebas, 1996), poststructuralist work (Jameson, 1992), as well as broadly non-representational (Amin and Thrift, 2002) and environmental theories (Heynen, Kaika and Swyngedouw, 2006), all at least partly took root in urban studies. In particular, the city has long been a testing-ground for a diverse range of scholars, from a range of disciplines and backgrounds, trying to figure out how the variously social and *biological* dimensions of human life might get into one another. We might think of, on the one hand, figures as unlike as Patrick Geddes (see Welter, 2002), Octavia Hill (1970[1883]) or WEB DuBois (2003[1906]) – all of them, in one way or another, concerned with ‘health’ in/and the city, among their many other preoccupations. But we might also look to contemporary mainstream epidemiology, where scholars still draw on the work of figures like Louis Wirth to establish connections between city life, wellbeing and happiness (Okulicz-Kozaryn and Mazelis, 2016). As Orit Halpern and her colleagues remind us, there is nothing new in ‘test-bed urbanism’, a logic of practice in

the which the city gets situated as a ‘development environment [for testing] the operability of new technologies, processes, or theories for large systems’ (2013: 290).

It is precisely this strange, test-bedded nexus of social theory, biosocial relationality, and experiment, that draws me to think the ‘empirical crisis’ of sociology through the urban and through urban studies. It moves me to think the bcalm, for example, not simply as a by-product of ‘knowing capitalism’ but actually as a much more interesting assemblage of materials, spaces, affects, bodies, interests, and capitals—a device that has the central attribute of being able to stage and switch a politics of urban space, a sensation of panic, a measure of CO₂, a dream of a forest, a moment of breath, an acid in a synapse, a panic in Swansea. And if we can incorporate a device like this, without great difficulty into long, tangled histories of thinking the social and biological through one another in urban space, then there is an important question implicit in that narrative, and which I am trying to make explicit here, about the precise social techniques and practices that it holds together, and how those techniques and practices may or may not then get traced into the present, through what devices and economies, with what consequences, and for whom. In other words, I am trying to figure out how ‘the social’ comes to matter in a device like the bcalm; and I am trying to answer that question, not by re-enacting a firm cut between these two, but by thinking through some, in fact, much less bifurcated histories of urban sociology — an intellectual legacy that is so much more lively and vital, so much more risky and experimental, so much more weird, and animal, and panicked, than we often acknowledge.

Limit sociology

Is it not the case that, just as with the work of the astrobiologists described by Stefan Helmreich, we are not simply reaching the end of that well-worn theoretical object, ‘the social’, nor are we only left gawping at its contingency; rather we are beginning to get a firmer analytical hold of the objects and practices that might push at the edges of its current limit – objects and practices that help us to stretch it, and extend it, and maybe even then to seek new agencies and new assemblages on the other side of it. Might we not say then, and also in a way that is largely derivative of the limit biologies described by Helmreich, that these dilemmas signal no more than the emergence of a *limit sociology* – in other words, that they mark the emergence of a procedure, or a set of procedures, that actively expand the ontological terrain of what it might mean to be social in the first place? And might we not *also* then conclude, at the same time, that limit sociologies and limit biologies do not simply run parallel to one another, that they are in fact kin of a sort— and that, indeed, the most obviously proliferating agencies one quickly encounters beyond the bounds of reproductive-futurist sociology are precisely those ‘imploded entities’ already well identified by Donna Haraway, and which are as much assembled of ‘particular sorts of historically situated machines’ as they are of ‘historically situated organisms’ (2016: 104)?¹⁸ My basic concern in this paper is that such entities will remain stubbornly invisible to a sociological practice that has its eyes firmly fixed on the reproductive business-as-usual. It seems to me that some kind of limit-practice, or at least a practice that has trained itself to be attentive to limits, is going to be vital for, in one sense, getting to grips with the proliferation of non-obvious techniques and entities that constitute ‘social’ life today; and, in another sense, the actual continuation of a ‘social’ science that has some sort of claim to the analysis of these techniques and entities in the first place. At least I want to suggest what I have here

called ‘limit sociology’ should be central to the ambitions of those for whom such continuation seems to matter.

Let me conclude with one final reflection: for the last couple of years, my collaborators and I, including colleagues at Fudan University in Shanghai,¹⁹ have spent some time in the migrant-oriented new towns at the edges of that city, key sites in the social, technological and industrial momentum of contemporary China (Greenspan, 2014). Our project is a collaboration between a set of researchers based in Chinese and UK universities, trying to get some purchase on the mundane hassles of everyday migrant life in Shanghai, by drawing together a complex range of ethnographic and epidemiological methods, and then triangulating these into some thicker account of what that life is like (see Li and Rose, 2017; Richaud and Amin, in press). Although the precise relationships are complex and multifaceted, the status of being a migrant, with its attendant stresses and dislocations, has long been associated with poor urban mental health (Bhugra, 2004; Li *et al.*, 2006). What is at stake in a city like Shanghai, then, still in the midst of an enormous rural to urban migration, is a complex and still not well understood assemblage of urbanisation, migration, and stress (but not only stress), which is taking place at the same time as a ‘boom’ in psychological attention, and even in new forms of ‘psychological governance’, in China (Yang, 2017). Without pathologizing or reducing this very varied and complicated experience, in our project we have been trying to think collectively about the politics of stress in migrant areas of Shanghai – and especially to think the sensory, social and biological *sequelae* of that stress; which is to say, the *sequelae* of competition and alienation, of uprooting and physical distance, of long hours and variable housing, of sometimes patchy and bureaucratically complex access to services, and so on. Our interest is in how migrant life in Shanghai gets incorporated – and, although the empirics of the study are still in progress, we are

working to assemble a combination of ethnographic, epidemiological and digital methods, through which we might somehow trace the social life of the city through the body and brain of the stressed city dweller.

This is much too short an account of a complex endeavour. But I raise it here because I want to say that, if I had originally understood this project as a novel attempt to ‘connect’ the social and biological in an interesting and creative way, increasingly I have started to think of what we’re doing (and I am aware of the affectation involved in this comparison) as a kind of ‘limit sociology’ – which is to say: as an attempt to gently, but seriously, push on our sense of what social life is, on where it might be sought, and on what forms of action, practice, and feeling might be organized under its sign. I am not claiming that our project, barely described here, has invented anything new, or that it is profoundly novel, or that it is the sociological equivalent of deep ocean microbiology. I *am* suggesting that its empirical assumptions and techniques (and it is far from alone in this) nonetheless arrange social life as an assemblage that is as amenable to app-mediated psychological self-ratings as it is to ethnographic field-notes, and epidemiological surveys, and perhaps even, in future work, to direct biological measures. And that, at least in the sense of a reproductive-futurist sociology, and the limited temporal horizons within which it makes sense of itself, this places our project at the limit, perhaps even beyond the limit, of what counts as normal science. This is sociological method, in other words, that is founded on the hope of locating some more ambitious procedure for confronting the epistemological conundrum with which I started – a procedure for thinking the material transformations of the present not as a cause for alarm, nor the sign of crisis, nor the absence of a future, nor still a premonition of death. But rather as a nudge to think less conventionally about just these kinds of binaries; even

to let go the sense of doom, to dial down the panic, to breathe in the forest air, to be calm.

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Notes

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1. According to Google Scholar, the paper had been cited more than 800 times by May 2017, while two successor papers (Savage and Burrows, 2009; Burrows and Savage, 2014) had between them amassed more than 300 more.
2. An early version of some of the ideas in this paper appeared on the *Sociological Review* blog under the title, ‘Life Methods.’ See: <https://www.thesociologicalreview.com/blog/life-methods.html>
3. I am grateful to a review of the paper for this neat formulation.
4. Mike Savage’s *Identities and Social Change in Britain since 1940* (2010), which was published a couple of years after this paper, is *inter alia* a piercing analysis of the history of British sociology in these terms.

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5. In their follow-up, Burrows and Savage take the opportunity to trace some of the methodological developments that have taken place since. A subsequent, large-scale data project, the ‘Great British Class Survey,’ is their central example, although, as they admit, this has run into some method-trouble of its own: see Mills (2014). For Savage and Burrows, the work of recalling their 2007 paper is thus the work of tracing the methodological developments that it either predicted or anticipated. The way to read the paper, now, for them, is as an account of the methodological future—a guide to developments in (mostly quantitative) research innovation that have been taken, could have been taken, or might still be taken, in the years since.
 6. I don’t intend this observation as a way of castigating anyone involved in these debates. Let she or he who is innocent of career-making cast the first stone!
 7. I am grateful to an audience at CRAASH, at the University of Cambridge, and especially to Sarah Franklin, for helping me to foreground reproduction in my reading of these texts.
 8. I am grateful to Kate Moles for pointing me towards Holton’s paper.
 9. It is worth noting that Back’s paper, while it comes in the wake of the methodological anxiety occasioned by Savage and Burrows, and it addresses that paper squarely in its opening lines, is hardly a direct ‘response’ to Savage and Burrows. In fact, such a description does scant justice to Back’s rich and wide-ranging paper, which I am treating somewhat narrowly here for my own ends.
 10. And, indeed, the important essays that come before and after it, in what is in fact a special monograph issue of *The Sociological Review* dedicated to ‘Live Methods,’ co-edited by Les Back and Nirmal Puwar
 11. And, further, that reading a paper through its metaphors produces a fairly skewed account of its argument. For the avoidance of doubt: I don’t think that Mike Savage and

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- Roger Burrows are of the opinion that sociology will literally die, or that Les Back advocates burying texts underground. I am reading against the grain here, and so encourage the reader not familiar with these texts to not simply follow my reading, which I am sure many would regard as eccentric.
12. Back's *The Art of Listening* (2013) contains, among other things, a rich and vivid account of tattoos as particular forms of political and affective inscription, while *The Auditory Culture Reader* (2003), co-edited with Michael Bull, is a compelling call for attention to the ear as a piece of sociological apparatus.
 13. Which is not to say, of course, that 'the social' was taken as an unproblematically ahistorical category previously – see Donzelot (1988) - but only to say that (1) it seems fairly undeniable, now, that technological and other changes have produced fairly obvious mutations in where and how social life takes place, and that (2) these changes have produced a discussion on the nature of the social right on the surface of the contemporary social sciences (in addition to the papers I discuss in the main article. See Couldry (2012) and Rose (1999).
 14. It is perhaps worth noting in passing that the Rockefeller Foundation has its own history of helping to negotiate the line between the social and the biological in twentieth-century British social science. See Renwick (2014).
 15. For an account of the historical geographies of psychiatry and psychopharmacology at stake here, see Callard (2016).
 16. See <https://bcalm.co/>
 17. <https://bcalm.co/how-it-works/>. Accessed December 2017.
 18. But see Lewis (2017) for an important critical reading of the reproductive trouble of Haraway's recent writing.

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19. <https://www.kcl.ac.uk/sspp/departments/sshm/research/Research-Groups/BIOS/BIOS-Projects/M3/About-Us.aspx>

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