SENTIO
an interdisciplinary social science journal
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Dear Sentio readers,

We hope you enjoy the inaugural issue of Sentio, a new, interdisciplinary, online journal for the social sciences, founded and led by doctoral researchers from the ESRC-funded South-East Network for the Social Sciences (SeNSS) consortium. The journal was inspired by discussions that began during the SeNSS Summer Conference 2018 (and have continued ever since) regarding the need for an interdisciplinary space of open discussion among doctoral and early-career researchers. The wider Sentio community working on the inaugural issue (including section editors and reviewers) comprises 23 SeNSS doctoral students from seven of the SeNSS institutions: Sussex, Royal Holloway, Reading, Essex, Roehampton, Kent, and UEA.

The journal’s title Sentio (‘I sense’) reflects our belief in the necessity of varied modes of knowledge production, including those rooted in personal experience and (bodily) sensation, and a commitment to affirming the validity of multiple and dispersed archives. We are concerned that the onslaught of austerity, precarity, and marketization are fueling a research climate that dangerously circumscribes what counts as ‘valid’ and ‘legitimate’ social science, privileging policy-facing, impact-generating research to the detriment of boundary-pushing and emancipatory scholarship with different epistemological and methodological moorings. Sentio is also the product of a particular political moment witnessing a confluence of violent and racist border policing (both in and outside the university), an assault on the rights and dignity of trans people, a crescendo of jingoistic nationalism and nostalgia for Empire, and growing electoral support for right-wing populist parties and politicians. Sentio recognizes that a policing of the boundaries of ‘proper’ science functions to marginalize and delegitimize those already labouring under conditions of heightened precarity – those who are racialized, trans, disabled, queer, or from working-class backgrounds – and wants to push back by affirming diverse forms of knowledge production. Sentio is intended to function as a forum for conversation and exchange between different social science approaches, while upholding a firm commitment to anti-racist, trans-inclusive, queer, and feminist politics.

Validity, the theme of Sentio’s inaugural issue, was chosen with these struggles and aspirations in mind. On the one hand, validity as a concept is familiar from the empirical social sciences, where it designates a necessary characteristic for research output seeking to constitute a truth claim. The means by which validity can be claimed, contested, and affirmed are multiple, and Sentio seeks to reflect a wide range of methodological and epistemological engagements with it. On the other hand, ‘validity’ can also be used as a lens through which to refract and reflect some of the clashes we are observing both within and outside of the academy. The last few years have seen an uptick in attacks on gender, critical race, queer, and critical disabilities scholarship, specifically through denial of the ‘validity’ of their methodological and epistemological commitments. Clearly, these charges are not exclusively scholarly in nature, as they are happening in a context of emboldened (far) right politics. An engagement with validity that both explores its multiplicity and maintains an awareness of its weaponization in current political struggles is a core endeavour of Sentio’s inaugural issue.

Practically, Sentio aims to facilitate the ‘first steps’ of early-career researchers as they approach the challenging field of academic publishing. We hope the journal offers a friendly and respectful environment for emergent ideas, thereby fostering possibilities for improvement, collaboration, and new professional connections.
In order to include diverse submissions and contributions, and at the same time position ourselves differently from other established journals, we created three different sections: Articles, Features, and Reflections. The Articles section presents short academic articles that deal with philosophical and theoretical discussions, methodological approaches and early empirical insights. The Reflections section was designed to highlight moments of a researcher’s experience often left out of the final text, including instances of personal realization, emotional revelation, or field research encounters, as well as experiences of applying different methodological approaches. Lastly, the Features section was conceived as an outward-focused section for pieces such as reportage of current events and interviews with scholars at the forefront of critical perspectives in the social sciences.

The collected articles in this issue offer varied and fertile perspectives on the notion of validity, presenting views from a range of onto-epistemological, disciplinary, and national contexts.

A number of articles question the nature and topography of validity. Foster-Collins’ work on modelling ‘trustworthiness’ and Huber’s consideration of big data and emotion research offer alternative ways of assessing rigour that still adhere to the ‘generalisable’ standards of positivist epistemologies. Other articles unsettle the seemingly stable foundations of validity. For example, Nielsen’s ethnography illustrates how claims to objective neutrality go hand in hand with capitalist processes of alienation and the wielding of power, which ignores embedded understandings of the world, whilst Pantalone’s research into public-key cryptography questions the trust placed in assumedly valid concepts which continue to be based on unproven hypotheses. Demonstrating the pressure on public funds to be used only on interventions with verifiable and quantifiable results, Speer and Murphy’s reportage on the new ‘What Works Centre for Children’s Social Care’ explores the challenges of applying the logic of other scientific disciplines—Randomised Controlled Tests—to the area of social work, where reality cannot easily be captured by a binary result. Offering a departure, Costa encourages us to practice ‘slow science’ when listening to our data, and Franklin’s interviews with arts-based researchers in Colombia make the case for bringing emotions and the impact of research on our informants to the centre of our scholarship.

Other contributions more explicitly foreground the relations of power that are at play within constructions of validity. Franco and Mustafa’s article on racialised archival silences and Mansey’s work on historical sociology both highlight how allowing the past to speak through our research can help us interrogate Eurocentric, Western-centric narratives of the social, disrupting situated understandings of validity. Meghji’s article on racialised regimes of truth, Blair’s piece reflecting on attacks on their dual position of parent and researcher, and Jung’s interviews with scholars in the fields of gender studies, critical race, and postcolonial research highlight the exclusion and derision of research on the grounds that it is too personal, too subjective, or too marginal. Further articles highlight that these exclusions can produce affective and embodied experiences, such as Place’s reflections on the selection of the anthropological field site, Simon’s observations on danger in the field, and Church’s ruminations on the tensions between personal beliefs and youth work practice.
Establishing, coordinating, and leading a journal is both a daunting and exciting endeavour. Managing the work involved, alongside the work of a PhD and other commitments, is no easy feat. From the outset, we have done things a little differently, be it by design, or perhaps at times through naiveté. For starters there are four general editors, all women, who take decisions collectively and by consensus. We had never worked together previously and are currently associated with four different disciplines, albeit in social science. It soon became apparent that we all had our own working styles and ways of approaching team work. It was, at times, difficult to ensure we all stayed on the same page. Communicating our (sometimes intuitive) assumptions and ideas about the direction for the journal was particularly challenging. This was exacerbated by the fact that we were not always in the same geographical place, with three of us away from our university for (demanding and stressful) fieldwork for most or part of the time. A certain well-known messenger application was the space of much daily discussion, sometimes heated. We had to work hard at times to resolve a lack of consensus. Other challenges included one of us suffering a decline in health and having to take leave from their PhD programme, as well as having to deal with challenging field research. Yet, we find it important to highlight that despite all these challenges, the journal project has also produced a space for solidarity and esprit de corps. Working as part of a team has clear benefits in the face of (frequently isolating) PhD research, fostering a sense of sisterhood and camaraderie that we have come to cherish.

In closing, we would like to thank those without whose support and encouragement Sentio would not have been possible: Pam Cox, Director of the SeNSS DTP, who first broached the idea of a journal led by SeNSS doctoral students, and who provided invaluable feedback, expertise, and warm encouragement; Felicity Szesnat and Paul Newman, the SeNSS Manager and Coordinator (respectively), who were tireless in their logistical and administrative support for the journal; the SeNSS Management Team, who provided support and advice in the final, critical stages of the journey to publication; our contributors, who graciously endured the complications arising from a first-time editorial collective putting together an inaugural issue; as well as our anonymous peer reviewers. We would also like to thank the SeNSS-DTP for providing Sentio with funding, enabling us to outsource a small portion of the labour (copy editing and layout) required to publish this journal.

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1. Articles
‘Hello from the other side’: Listening to Data, Slow Science and the Quest for Validity in Qualitative Content Analysis Processes

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Abstract
This paper explores examples used by the author in a pedagogical, higher education context as a way to discuss strategies of enhancing validity in qualitative content analysis. ‘Listening to data’ is presented as a strategy that can uncover ‘hidden truths’ and soundness in research, specifically when engaging in a qualitative content analysis of transcripts of oral interviews. To listen to data is about giving it sufficient time and to be attentive to the data both inside and outside. Listening to the data inside entails looking at utterances and their structure, as well as attending to the rhythms and sequences of the speech. Listening to the data outside refers to how data echoes and resonates in sensory images through which the readers are transported by way of colours, sounds, smells, gestures, and emotions. While ‘listening to data’ might appear to be a passive and asynchronous exercise between the speaker (there/then) and the listener (here/now), by using this strategy the researcher can dialogue with the data and hear surprising, revealing, and unexpected voices from that ‘other side’.

Learning to listen
Some years ago, in a National Congress of Pedagogical Practices in Higher Education¹, I spoke with a teacher who introduced me to the challenging task of teaching students from health sciences how to hear a patient’s breathing and heartbeat. I often recall this discussion when, as a teacher, I am frequently confronted with the need to teach research methodologies for undergraduate sociology students. How should one teach the qualitative strategy of ‘listening to data’? And what can such a listening exercise add to the existing knowledge whilst interpreting data and seeking validity?

This paper advances some answers to such questions which intertwine with the search for soundness in qualitative inquiry. Methodologically, it brings together examples used by the author in a higher education context while working with students who want to become qualitative researchers (Clesne, 1999). Original data were collected through pedagogic exercises developed in the framework of a course titled ‘[Inter]views: designing, conducting and analysing interviews in social research’, offered as a summer course at the University of Évora (Portugal), which has been running since 2014². Using the technique of role playing, students were asked to take on the roles of interviewer and interviewee, using a short interview script. Data collected through audio recordings were subsequently subjected to a verbatim transcription and then explored using qualitative content analysis techniques (Krippendorff, 2018). While these various tasks comprised the development of the knowledge and competencies set for the course, they allowed students to gain a finer insight into how the different stages of research are intertwined, namely the phases of data collection and data analysis and interpretation.

Showcasing the author’s previous empirical work allows discussions of both the potentialities and obstacles presented by the strategy of ‘listening to data’ in the broader context of seeking ‘validity’ in qualitative studies. This article concludes by providing a short yet striking contribution on the place of ‘listening to data’ as a strategy to uncover ‘hidden truths’ and claiming validity in social sciences, specifically when engaging in a qualitative content analysis of transcripts of oral interviews. While ‘listening to data’ appears as a passive and asynchronous exercise between the speaker (there/then) and the listener (here/now), experience shows that by formulating and using the right questions, the researcher can dialogue with data and hear surprising, revealing, and unexpected voices from that ‘other side’³.

Play, pause. Searching for validity in the transcripts
Validity issues are crucial when referring to the scientific procedure. There is a common understanding that scientific research is considered valid if it provides a ‘true picture’ of reality (Creswell, 2018). Internal validity usually refers to the degree to which an instrument measures what it is intended to measure, whilst external validity refers to the degree to which an instrument can be generalized to other people, situations, or times.

² The course has been taught in b-learning with a total number of 15 hours. Detailed information about the 2018 edition can be found online via the School of Social Sciences: https://www.ecs.uevora.pt/divulgacoes/cursos_livres/Entre-vistas-desenho-aplicacoes-e-analise-de-entrevistas-em-investigacao-social-4a-edicao
³ The title of this paper is inspired by the verse ‘Hello from the other side’, from the song ‘Hello’, by the British musical artist Adele. The song appeared on her third studio album, 25 (XL, 2015).
validity pertains to the generalizability or broader applicability of the study's findings, results, and conclusions beyond the immediate study (Miles and Huberman, 1994; Silverman, 2011).

Despite the different interpretive frameworks and philosophical underpinnings, qualitative inquiry questions the framework of validity that is commonly accepted in the quantitative or ‘hard-science’ research paradigm. Qualitative inquiry is based on some general assumptions, including: there is no observation without a subject who observes within a particular social time and place; the researcher is part of the study and constructs the reality he or she observes; and each researcher brings a different and unique perspective to it (Mason, 2002; Denzin and Lincoln, 2018). As a consequence, when envisaged from a more quantitative paradigm, validity and qualitative research are often seen as fundamentally incompatible, that is, as “an oxymoron”, in the words of Onwuegbuzie and Leech (2007).

That said, over the years, qualitative methodologists have presented and described different or alternative criteria and categories with which to judge and define validity in line with the qualitative paradigm (Miller, 1986; Silverman, 2011). To this end, issues regarding the ‘truth’ or ‘falsity’ of an observation are usually replaced by discussions concerning the ‘goodness’ (Miles and Huberman, 1984: 277) of the research, since ultimately ‘some accounts are better than others’ (Miles and Huberman, 1984: 277). What, then, are the different standards for judging the worthiness of such research?

In a seminal work on the naturalistic inquiry, Lincoln and Guba (1985) proposed four criteria for judging the soundness of qualitative research. They explicitly offered these criteria as an alternative to more traditional, quantitatively-oriented criteria: credibility; transferability; dependability; and confirmability. Pairing traditional terms with those alternatives for assessing the degrees of ‘trustworthiness’ and ‘authenticity’ in naturalistic and qualitative research, Miles and Huberman (1994) discuss five main issues regarding the criteria: the objectivity/confirmability of qualitative work; reliability/dependability/auditability; internal validity/credibility/authenticity; external validity/transferability/fittingness; and utilization/application/action orientation. As for Patton (2002), the credibility of qualitative inquiry depends on three distinct yet related topics: rigorous methods for conducting fieldwork; the credibility of the researcher; and a philosophical belief in the value of qualitative inquiry. More recently, Onwuegbuzie and Leech (2007) have introduced the ‘Qualitative Legitimation Model’, through which they attempt to integrate the various types of validity identified by qualitative researchers. They described twenty-four methods for assessing the true value of qualitative research (Onwuegbuzie and Leech, 2007).

Although the criteria and terminology for judging soundness vary among qualitative researchers, the quality of the transcripts – particularly the option for a verbatim transcript – is often given as an example of a specific procedure when assessing trustworthiness, authenticity or credibility (Arksey and Knight, 1999; Gillham, 2005; Kvale, 2007). Two excerpts of verbatim transcriptions were selected as empirical illustrations for this paper. Whether used for the purpose of exemplifying how the researcher should ‘listen to data’, both ‘inside’ and ‘outside’, the two quotations describe how listening to the data as represented in the transcript constitutes a way of enhancing the validity of interpretation in qualitative content analysis processes. As discussed below, it is true that to transcribe is a common procedure when dealing with data collected through oral interviews, yet rather than seeing this act as an end in itself, it ought to be understood as allowing for a richer, deeper, more colourful and audible representation of social reality. Ultimately, the act of ‘listening to data’ can increase the levels of validity within qualitative content analysis.

More broadly, this paper can be situated alongside recent calls in academia for more time. Slow Science is part of the broader ‘slow’ movement and draws attention to the ‘slow timescale’ in which science moves, declaring: ‘Science needs time to think. Science needs time to read, and time to fail’ (The Slow Science Academy, 2010). In the domain of sociology, the work of Les Back (2007 and 2008) has been particularly important in denouncing the pitfalls created by the quantitative measurement of academic value and performance that has become so characteristic of the accelerated science of the early twenty-first century. Moreover, it calls for an urgent turn towards listening, paying ‘attention to the fragments, the voices and stories that are otherwise passed over or ignored’ (Back, 2007: 1). Taking inspiration from the work of Back, it is argued here that time is a key factor for listening, and that validity, in turn, depends on listening.
Listening to the data inside: Utterances, structure, rhythm, and sequence

To work with interview transcripts is a demanding process of attentive, mediated listening of the data. In this article, it is argued that the listening exercise is comprised of two moments: one of listening to the data inside; the other of listening to the data outside. Both moments are vital in searching for meaning and, as such, in establishing the basis for the validity of data interpretation. Specifically, listening to the data inside implies looking at the utterances and the structure of these utterances, yet it also involves paying attention to the rhythms and sequences that mark the pace of the speech.

The following excerpt was drawn from an exercise that aimed to explore whether individuals remember their first day of work and, if so, to collect such descriptions. João⁴, a Portuguese man, born in the Alentejo region, aged 45 and working in agriculture, gave the following answer to the guiding question from the interviewer⁵:

**Interviewer:** Let’s talk about your first day at work. Do you remember how it went? How was it? What did you do?

**Interviewee:** So … in agriculture … you know how it is … we are born for this, and we start from an early age helping our father, digging … We are …, we are here …, so …, now you ask me when did it begin? Uhm, it started … well, it started since I started playing there on the ground and all that … uhm … I don’t know, I don’t know, I don’t know, I don’t know, I don’t know when it started. It began from the time I was born, since I began to walk, since I began to help my father to water a few … a few plants, and now …, having to say when it started … […] Let’s say … it has begun, it started, started many years ago [emphasis], oh … I don’t know … now, now, now I cannot say to you, I cannot say to you, that’s it, that’s it, I cannot say to you when it was.

At first sight, reading this excerpt leads to the conclusion of a non-answer. It would appear that the interviewee is saying that he does not remember his first day at work. And in fact, he uses several language resources to ‘convince’ the interviewer of this, for instance, when he insistently repeats negative statements such as ‘I don’t know’ or ‘I cannot say to you’.

What is the interviewee saying? To be able to answer this question implies listening more deeply to the data⁶. It means paying greater attention to what is being read, trying to link the sentences, now separated in the written narrative by suspension points, commas or end points. In their place, the researcher should reintroduce the pace that the orality has lost since the audio has been transcribed, that is to say, the various hesitations and pauses, the suspension and resumption of speech.

The result of this process can be as surprising as it is theoretically rooted. When reread in such a way, the data tell us more and in more complex ways. Multi-layered meanings can be extracted from this quote. Without entering into theoretical considerations in the framework of sociology and socialization processes, being attentive to the interviewee’s repetitions allows us to conclude, in this particular case, when, after all, ‘it’ all started. Simply put, ‘it’ was always there. In this particular case, there was never, in fact, such a thing as the ‘first day at work’. Since he was a child, João has lived the life of someone deeply immersed in the countryside, just as his father was. In fact, this is (also) what he is saying in the interview. João was, in fact, focused on answering the interviewer’s question, using expressions such as ‘I don’t know’, ‘I don’t know when it started’, ‘I don’t know’, ‘I cannot say to you’, ‘that’s it, I cannot say to you when it was’, while also answering by way of other expressions: ‘in agriculture […] we are born for this’, ‘we start from an early age helping our father, digging’, ‘it started since I started playing there on the ground and all that’, ‘it began from the time I was born, since I began to walk, since I began to help my father to water a few … a few plants’, ‘[it] started many years ago’, etc.

Being attentive to the utterances that form the narrative, and to the structure of such utterances, as well as paying attention to the sequence and rhythm of the speech entails reading but also stopping and resuming. Following Les Back, in the exercise of listening, it is important to focus on ‘the fragments’ and ‘the voices and stories’ (Back, 2007: 1). As illustrated above, making those voices audible and being able to tell those stories via rich and thick description greatly depends on the quality of the verbatim transcripts. The relationship between the quality of the transcription – sometimes seemingly chaotic, confusing, and repetitive – and the quality of

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⁴ All names used hereafter are pseudonyms for data anonymization.
⁵ Data collected on 2 July 2014.
⁶ When teaching this topic and working with students, and this excerpt, in particular, I often make a gesture that challenges them to bring their ears closer to the data, with the latter figuratively represented by the computer.
intermediate is crucial in assessing the trustworthiness, authenticity, and credibility of qualitative research, as well as judging its soundness (Arksey and Knight, 1999; Gillham, 2005; Kvale, 2007).

**Listening to the data outside: Colours, sounds, gestures, and emotions**

When the process of looking for validity means obtaining a true picture of reality by reading interview transcripts, one needs to go beyond listening to data inside. This implies being attentive and able to listen to the data outside. In doing so, data, as an echo, resonates in sensory images to which the reader is led through colours, sounds, smells, gestures, and emotions. In this way, the researcher is brought closer to the reality described and thereby to the ‘truth’.

The following quotation is the start of a dialogue between the interviewer and interviewee in an exercise developed for the purpose of collecting an in-depth narrative about the first day of classes. Eduardo, a 57-year-old Portuguese man born in Inhambane, Mozambique, described that day as follows:

**Interviewer:** Do you remember the day you entered the school?

**Interviewee:** 10 September 1963.

**Interviewee:** Was there any reason for you to remember that date?

**Interviewee:** Yes, there was. A colleague of mine who was not silent, redheaded, whose name I cannot remember, he kept interrupting, with his finger in the air, asking Professor António if he was going to give him a notebook. Because he had no notebook, and his mother had told him that the teacher was going to give him a notebook. ‘Mis::ter!’9, he had a sibilant ‘S’, ‘Mis::ter, Mis::ter, my mother told me that Mis::ter would give me a notebook’. I never forgot it! The permanent interruption that boy was making ... I was very small ... looking like ... as any small child. Standing there, very shy and looking at that ... for that ... and that, in fact, impressed me a lot. And still today, I remember ... I don’t know what happened to that person, the little redhead, who sat down ... a little ahead of me, on the left side. I remember as if it was today.

In this case, the researcher is confronted with a specific situation in which the interviewee is quite affirmative. He immediately answers ‘Yes, there was’, leading the interviewer through a detailed description of what he remembers about the first day of school. Yet, is there more to see in this account beyond Eduardo’s detailed description? The answer here seems to be just as definitive as that given by Eduardo. Again, since the aim of this text is not to undertake a sociological interpretation of the data, for our purposes it is worth drawing attention to how this quote says something deep about the experience of the first day of school for Eduardo. Behind such an impressionistic memory around the behaviour of the ‘redheaded boy’ seems to be the fact that in 19639 the school was a space of visibility for Eduardo, confronting the difference between himself and that other boy. Though not direct or sequential, the description does include clues that signal such difference; these clues are not simply physical or behavioural but predominantly social. The differences between the two boys seem to be as noteworthy as they are unwieldy, as the boy ‘was not silent’, ‘redheaded’, ‘kept interrupting’, ‘had no notebook’, and ‘had a sibilant “S”’, while Eduardo ‘was very small’, ‘looking like ... as any small child’, was ‘standing there’, and ‘very shy’.

As illustrated in this quote, data carries with it multiple sensory images, colours, sounds, smells, gestures, and emotions. Again, the quality of the transcript is central in portraying such a sensory landscape. Yet, the task of listening to data goes beyond being impressionistically touched by the data. As Les Back argues, listening is not just about hearing more carefully; rather, it involves ‘a mode of thought that works within and through a “democracy of the senses”’ (Back, 2007: 25). The transcript analysed above clearly exemplifies how such a democracy of the senses must be used to question data and to unveil the clues in ‘searching for latent meanings’ (Costa, 2015). ‘Within and through’ data, it is possible to develop a ‘true picture’ of reality (Creswell, 2018), and therefore to reach soundness in research.

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1 Data collected on 8 July 2015.
2 In the transcription, :: is used to highlight the stretching of the consonant (Costa, 2014). The number of points is usually proportional to the duration of the extension. In this case, two points were used because the interviewee pronounced the word with a stretching of the consonant ‘S’; moreover, he explicitly stated that the other boy had a sibilant ‘S’.
3 In this period, and until 1975, the year of independence, Mozambique was a Portuguese overseas colony.
Rewind, forward. Finding validity with the voices from the field

This paper was written to provide empirically grounded insights into the importance of listening to data as a strategy for enhancing the search for validity when teaching and learning qualitative research methodologies. Based on the author’s own experience of researching and teaching qualitative content analysis techniques, the text brings together short excerpts of verbatim transcripts from oral interviews, which allow for considerations of listening to data while discussing current debates about content analysis and issues around validity.

The current way of carrying out qualitative content analysis – highly computerized through the widespread use of qualitative software such as NVivo (QSR, International) or MAXQDA (Verbi GmbH) – favours a kind of analysis that appeals to the speed of processing information, rapid coding and categorizing, and easy data reading and visualization (e.g. through summary charts, tables, graphs or word clouds) (Krippendorff, 2019). Focusing on the participants’ own words is one way of reconnecting the flow of the speech that is otherwise broken through the act of transcription. Given that it is sometimes difficult to listen back to the recorded audio files, the researcher should allow sufficient time to listen to data and to practice slowness and attentive listening.

Whether technological advancements are revolutionizing forms of dissemination in qualitative research, allowing us to better listen ‘to the sounds of research’ (Salmons, 2016) and to use hypermodal dissemination possibilities – such as embedding audio clips and infographics in written papers (Chandler, Anstey, and Ross, 2015) – this text underlines the importance of time, slowness, and focus. In the context of qualitative content analysis processes, being able to listen to data entails being attentive both ‘inside’ and ‘outside’. Listening to the data inside implies looking at the utterances and their structure, as well as to the rhythm and sequence of speech. Researchers should also dedicate more time to listening to data outside. As an echo, data resonates in sensory images where readers can be transported by way of colours, sounds, smells, gestures, and emotions. In this way, it is possible to hear the actual voices of the research participants while transforming a passive and asynchronous exercise into a rich and colourful dialogue that seeks to enhance the soundness of research.

Listening to data is a key strategy in enhancing validity in qualitative content analysis processes. When given sufficient time, it is possible to search for – and indeed to find – latent (hidden) meanings. In so doing, the researcher comes closer to the reality he/she intends to study and, as a result, the conclusions become more trustworthy, authentic, and credible. In this sense, listening to data is rooted in a profound ethical commitment between the researcher and the subjects of the analysis. In an accelerated global culture, ‘The Art of Listening’, as Back aptly calls it, is a commitment that ought to be pursued and strengthened, as it realizes ‘the value of sociological attentiveness, a compound of dialogue and critique that is the hallmark of sociological imagination’ (Back, 2008: 1.20).
References


Trustworthiness

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Abstract
The terms validity and reliability have been contested within naturalistic research, with some attempts to redefine these concepts to increase their relevance for qualitative research. For my own research on informal workplace support for new teachers, I chose the alternative term ‘trustworthiness’ and additionally drew upon texts which discussed ‘rigour’ in qualitative contexts. I used the five strands outlined by Guba and Lincoln as a useful starting point for thinking about trustworthiness during my methodological design. In this paper, I outline these five criteria and how I have applied them in my own research. I also consider their strengths and limitations and briefly appraise the use of criteria-based measures of quality in general. I argue that reflection on trustworthiness of research is an ongoing process, comparable to that of the continued imperative to consider ethics in a responsive and unfolding way. Ensuring that we remain reflective during all stages of qualitative research may help to increase trust: in the overall quality of research, the results of data analysis, and any conclusions and recommendations based on these.

Introduction
It is important to conduct research studies of the highest quality, given that we draw conclusions and make recommendations for action on the basis of our findings. Whether that be for new interventions, changes to policy and practice, or informing theory, our research has real-world implications. Moreover, for academics and the public to have confidence in the results of research, they should be able to feel confident that it has been carried out rigorously. Practices such as double-blind peer review, where experts scrutinize the methodology, findings, and conclusions of research before publishing, can help build confidence. Such practices are difficult to carry out, however, if insufficiently detailed information is provided on how research was conducted. Finally, although qualitative research methods are used widely within social sciences, historically there have been questions over their value compared to quantitative methods (Lincoln and Denzin, 2000). For all these reasons, qualitative research communities should continue to engage in discussions about how best to promote quality.

Paradigmatic Assumptions Underlying ‘Quality’ Assessments
Most qualitative research is based within naturalistic, interpretivist paradigms (Guba and Lincoln, 1982), having the social world as its object, and using methods such as observation, interviews, and textual analysis to explore designated topics in all their complexity. Unlike quantitative research, it does not try to describe the world as it ‘really is’. There is no desire to manipulate variables, determine causal relationships, draw generalisations or make predictions (Golafshani, 2003). Instead, qualitative researchers see interpretation as inherent to our understandings of the world, with these interpretations situated within wider socio-cultural contexts of shared beliefs, meanings, and practices (Schwandt, Lincoln and Guba, 2007), and resting upon collaboratively constructed language concepts (Schwandt, 1994). It is therefore common for qualitative researchers to explicitly acknowledge possible influences on data production, such as social, cultural, and political climates, researcher-participant dynamics, and individual researcher standpoints. They also recognize that alternative interpretations of their data are possible.

A Criteria Approach to Ensuring Quality
However, it is still important to establish some guidance for quality and rigour if we are to avoid an ‘anything goes’ attitude towards qualitative research (Silverman, 2006). We have the same need to demonstrate that our work has value, that our findings and conclusions are ‘credible’, and that they genuinely answer the questions we set out to explore (Kline, 2008). One approach to addressing rigour has been to draw up sets of evaluative criteria specifically relating to qualitative research. The best-known of these criteria were set out by Guba and Lincoln (1981, 1989) to demonstrate what they called ‘trustworthiness’. Despite having been developed almost forty years ago, and notwithstanding the fact that Guba and Lincoln have since challenged their own criteria to some degree (Lincoln, 2010), this set of ‘standards’ is still widely used and cited. Each set of criteria was intended to correspond to accepted measures of quality in positivist paradigms and address similar concerns, but they were adapted for use within naturalistic paradigms. Four main strands of trustworthiness were initially described: credibility; transferability; dependability; and confirmability. A fifth element – authenticity – was
later added (Guba and Lincoln, 1986). These five aspects formed the main starting point for thinking about trustworthiness, and so I will both define these and illustrate how I have sought to apply them in my own research. I will also evaluate this approach to trustworthiness, and discuss how reflecting upon quality continues to challenge my understandings.

Informal Workplace Support for Early-Career Professionals

My own PhD research explores early-career professionals’ experiences of workplace support during their first year, with a particular focus on informal support. These ‘stories of support’ are being analysed with the aim of discovering: who provides support; what types of support they give; and which socio-cultural and organizational factors influence this. Having previously analysed secondary data from first-year postgraduate doctors, my current focus is the collection and analysis of new narrative interviews from recently trained secondary-school teachers in their first year of practice.

Reflecting on the Five ‘Criteria’ of Trustworthiness

Table 1 below outlines each of the criteria of trustworthiness, as devised originally by Guba and Lincoln and further developed by others. It also includes illustrations of how I have implemented these and some reflections on that process.10

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<th>Criteria</th>
<th>Criteria</th>
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<td>Credibility is analogous to internal validity in qualitative research and seeks to answer the question: to what extent do the data and research findings show ‘a true picture of the phenomenon under scrutiny’? (Shenton, 2004). Guba (1981) warned that data could be ‘distorted’ if researchers failed to challenge their own preconceptions and suggested that data could be influenced by factors such as a lack of rapport, or participants anticipating researchers’ aims. Some suggested solutions have included: ‘prolonged engagement’ with participants and context, peer discussion and co-analysis, and rich descriptions of data collection contexts (Morrow, 2004).</td>
<td>In face-to-face interviews, I tried to develop rapport by bringing refreshments, using informal interview schedules, and describing how narrative methods aimed to capture teachers’ own unique stories, with no right or wrong answers. I also reassured respondents regarding anonymity and confidentiality in the hope that this would enable them to speak more freely. In my reflective notes, I noted that rapport may be more difficult to achieve during telephone interviews due to the absence of body language cues. Active discussions with my supervisory team were used to try and uncover my own preconceptions about teacher support, such as how I might draw on my own experiences of working within education as a teaching assistant. Prolonged engagement is not usually deemed necessary in interview research (Morse, 2015).</td>
<td>The concept of ‘rapport’ has recently been problematized (Lincoln, 2010). It is a difficult thing to define or demonstrate to others but encompasses ideas such as empathy and respect. Within my study, it is perhaps better considered as a conscious intention to put participants at ease. Rapport may also be viewed as something which occurs not just during interviews but throughout all participant contacts (Miller, 2007). The idea of a ‘true picture’ of social phenomena ‘independent of us that can be discovered’ is suggestive of objectivity and realist ontologies (Smith and McGannon, 2018). However, I can reflect upon and acknowledge my own subjective perspectives where possible and aim to represent the varying perspectives of my teacher participants in research outputs. Given opportunities to ask questions, some participants did enquire about the wider aims of the research and what their data would be used for. However, more did not, giving me little access to their perceptions of the research as a whole.</td>
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10 Due to limitations of space, it is not possible to include all of my reflections here.
Dependability is analogous to reliability in quantitative research, which is concerned with stability of findings (Anney, 2014). In qualitative research, it is not possible to replicate findings, but it may be possible to replicate methodology to an extent. Therefore, dependability in this case is chiefly concerned with research processes being transparent and ‘auditable’ (Guba and Lincoln, 1989).

If research methods are explicitly described in sufficient detail, avoiding vague statements, e.g. that data ‘was analysed’ or ‘themes emerged’ (Anfara, Brown and Mangione, 2002), this allows others to critique the methods and conclusions drawn, and repeat methods if desired (Shenton, 2004; Aguinis and Solarino, 2019). This may include timelines of activities, notes on thematic development, and influences on data collection and analysis (Morrow, 2004). It is also desirable that published quotes be chosen to represent the variety, ‘range and tone of responses’ found in the data (Roberts, Priest and Traynor, 2006).

I am using Framework analysis, a type of thematic analysis described by Ritchie and Spencer (1994; 2003). This was primarily chosen for its transparent procedure, which is systematically applied to the whole data, and is easily referenced, understood, and repeated by others (Lacey and Luff, 2001).

I have clearly described the research process undertaken, including the extent to which analysis was driven by data and previous theory. The number of quotes included in publications is necessarily constrained. However, they will be carefully selected to try and represent all major themes identified and the full variety of participant viewpoints.

Inevitably, there will be aspects of the research process which remain ‘invisible’ to others, given the messy complexity of data analysis. Additionally, difficult decisions may have to be made regarding what is shared, for instance, due to space limitations or considerations of confidentiality and anonymity. A high level of methodological detail will be shared in my thesis. This is more challenging when writing for journals, but supplemental data will be made available (e.g. my coding framework) via online appendices where possible.
<table>
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<td>Confirmability</td>
<td>Confirmability</td>
<td>There are some practical issues with co-analysis, with suggestions that power differentials can influence which interpretations of the data are privileged. It is not possible to claim that no power differentials exist within my supervisory relationships. However, I feel that in my research team, there is a good balance between experienced researchers acting as ‘critical friends’ (Costa and Kallick, 1993) and support to develop my own ideas.</td>
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<td></td>
<td>In my own research, a systematic process is being implemented, with an analytical framework being applied to the entirety of the data. This, it is suggested, may help to avoid ‘cherry-picking’.</td>
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<td>Transferability</td>
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<td>Guba and Lincoln’s final aspect</td>
<td>‘Member-checking’ in its truest sense – where raw transcripts are shared</td>
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<td>of trustworthiness is somewhat</td>
<td>with participants for feedback – is not included in my research design.</td>
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<tr>
<td>multifaceted but, broadly</td>
<td>Although some researchers see this as an essential step, it can raise</td>
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<td>speaking, it addresses issues of</td>
<td>further methodological dilemmas (Carlson, 2010). For instance, how one</td>
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<td>fairness and ethical treatment</td>
<td>resolves divergences of interpretation by participants and researchers,</td>
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<td>of participants and stakeholders</td>
<td>whether participants have time to actively engage with re-reading</td>
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<td>(Johnson and Rasulova, 2016).</td>
<td>transcripts, whether participant perspectives on the data have changed</td>
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<td>since collection or are influenced by personal interests, whether power</td>
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<td>differentials encourage participants to defer to researchers’ ‘expertise’</td>
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<td>(Estroff, 1995), and whether clear evidence exists that such practices</td>
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<td>truly increase credibility (Thomas, 2017).</td>
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<td>A number of steps were taken to address such concerns in my own research.</td>
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<td>All teacher participants were informed as to the uses their data would be</td>
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<td></td>
<td>put in the information sheet.</td>
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<td>Events are also in planning for new teachers and educators to hear</td>
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<td>summaries of results and provide feedback on the practicality of</td>
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<td>proposed recommendations.</td>
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**General Limitations of the Five Criteria**

A general limitation of Guba and Lincoln’s concepts is an apparent overlap between the five criteria. Various authors have also defined these criteria differently, making their own suggestions for the actions required to ensure quality. Despite these differences, the central tenets of trustworthiness appear broadly the same: providing thorough descriptions of methodological processes to allow transparency, and requiring that researchers engage in reflexivity at every stage. Some researchers reject the idea that unified guidelines can be formulated, due to the wide variations in qualitative research methodologies and lack of consensus on what these criteria should be (Rolfe, 2006; Sandelowski and Barroso, 2002). However, this is not a reason to abandon all thought of quality standards, given that academics frequently fail to reach consensus and yet continue to explore contested topics through lively debate. Instead, we might see quality criteria more as guiding concepts, which aid researchers’ thought processes during the stages of design, analysis, and evaluation (Hammersley, 2005). A further approach to quality has been to develop ‘lists’ of characteristics deemed desirable in qualitative research, the idea being that these do not represent the last word but remain open to reinterpretation and reconstruction over time (Smith and Deemer, 2000). This approach has the advantage of allowing greater flexibility in how we implement quality, depending on the methodology and underlying epistemological stance, whilst providing constructive guidance for researchers and reviewers.

**Further Reflections on Trustworthiness**

The idea of a ‘universal’ set of criteria is highly appealing. This is particularly the case for new researchers, who may have sincere intentions to produce the highest standards of research but lack confidence in how to achieve this practically. This is exacerbated by a multitude of approaches which outline ‘the way’ to achieve quality research (Creswell and Miller, 2000). I would argue that using Guba and Lincoln’s criteria was indeed a useful starting point for me as a researcher. However, critically reflecting on them and looking beyond to other conceptions of rigour continues to challenge my understandings of quality, and this emergent thinking can hopefully be integrated into my research practice. I would liken this process of thinking about trustworthiness to how we consider ethics in research. The use of ethics forms, ratified by academic institutions, can encourage a way of thinking in new researchers that ethics is a once-only process. A hurdle which one must ‘get over’ and then never return to, rather than a set of principles and continued self-reflection which applies to the entirety of the research process (Guillemin and Gillam, 2004).
The Ethics of Trustworthiness

In this regard, I was inspired by a presentation by Brannelly and Barnes (2018), who put care at the centre of their research ethics. Drawing on Tronto (1993; 2013), they argued that caring for participants requires a degree of attentiveness, responsibility, competence, responsiveness, and solidarity with participants. The idea of caring for one’s participants is somewhat abstract, yet we have all read papers in which the researcher’s passion for their topic is clear and conveys a desire to represent their participants. When thinking about trustworthiness, this is not simply a process of laying out a trail of the research path we have taken for others to examine and critique, though clearly this is important. It is an ethical process which involves asking ourselves some piercing questions: Why am I doing this research? Who is it for? Am I telling the stories that participants want to be told? Relating this to the ethics of care, I can see that in order for my research to be trustworthy, this involves aspects of caring for participants and my research, ensuring my research skills are sufficient to do the data justice, recognising the varying aims and needs of participants, and remaining responsive to feedback. That is not to say that I can meet every need of every participant in every way; their views will invariably come into conflict with those of other participants and my own, acting as researcher-interpreter for their stories. In my own research, I face difficult choices regarding which of the many findings are most relevant to publish and which participant quotes should be selected to illustrate these. I am therefore required to ask myself: Am I fully conscious of any motives and interests which might influence this process? Have I favoured findings which support my own beliefs, are more interesting to me personally, come from a ‘favoured’ participant, incite controversy or play it safe, or for which it is easier to suggest solutions? That is not to say that we should present results which are vague or boring, that court or avoid controversy, or that difficult problems be presented with no apparent solutions (except that ‘more research is needed’). However, participants entrust us with their personal experiences and stories for a reason. Therefore, it is important that we approach the research process with a clear intent, to ensure our participants’ voices are heard and that we try to represent their best interests.

Final Conclusions

As a relatively new researcher, my thinking around trustworthiness is continually developing. Guba and Lincoln’s criteria offered a useful guide during my research design, but perhaps should not be viewed as being ‘set in stone’. Alternatively, ‘list’ approaches to rigour may allow greater flexibility when identifying which characteristics of trustworthiness apply to particular methodologies, topics, and settings. Furthermore, the use of criteria does not absolve us from deeper reflections on whether we can go above and beyond them; to imbue our research with genuine concern for participants and the impact of our research. In short, that as researchers, we are indeed ‘trustworthy’.
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Coping with Epistemological Restrictions in Interdisciplinary Emotion Research

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Abstract
Based on the findings of an EU Horizon 2020 funded project investigating Knowledge Complexity and bias in (big) data (K-PLEX: www.kplex-project.eu), this article focuses on the validity of ‘emotion data’. By means of a series of in-depth interviews and an online survey, researchers in various disciplines were asked what data they are in a position to validate and how they adjust to methodologically induced knowledge gaps. Social scientists use a multitude of qualitative and quantitative methods to explore emotions and affects of individual persons, groups, and even entire societies. Epistemological questions thus arise as to whether valid data can be obtained at any scale. The longstanding issue of sample representativeness in sociology re-emerges in data-driven approaches yielding big data on emotions and affects. Closely connected to the matter of scope, this article explores the generalizability of research findings. Framed as the WEIRD-problem of psychological data created in laboratory settings, the criterion of external validity can also be applied to the question of cross-validation with a mixed method design. Especially with regard to the sharing and integration of ‘emotion data’, the consequences of information loss through datafication are addressed. From a critical epistemological perspective, validity is finally discussed in terms of reflexivity fostered by participative research designs. As communicative validation of data and interpretations with research subjects can entail both methodological and ethical challenges, researchers’ responses to the limitations in establishing credibility are also analysed.

Introduction
In the humanities and social sciences, discussions on what constitutes good scientific practice date back to the beginnings of empirical research. With the differentiation and specialization of the disciplines, some of the principles have shifted in focus, but some controversies still persist. The tension between science and art falls into this ongoing discussion. Do scholars in the social sciences need to follow strict criteria of demonstrating and justifying their decisions within the research process, or should they be granted the freedom to artfully create an oeuvre that also meets aesthetic demands? Another fundamental debate centres on the conflict between the positivist criteria of accuracy and the criteria of accountability prevalent in critical ontological debates.

This article investigates how scholars from the social sciences, undertaking research on emotions and affects, encounter questions related to validity. What criteria do they regard as important? How should other researchers be convinced to trust the data and the conclusions drawn from them? And what kind or degree of validity results from data constructed by the researchers (they themselves being an ‘instrument’ in observing and listening) in contrast to data yielded by machines and algorithms? When researchers identify the limits and shortcomings of validity claims, how do they circumnavigate these limits?

Research on emotions and affects is particularly well-suited to an investigation of the various dimensions of validity, since the object of research manifests itself both explicitly and implicitly in a variety of phenomena. For instance, one can point to the role of the emotions and affects in decision-making processes or their shaping of cultural-historical practices and differences. One can even use applied research in the computational modelling of emotions. Based on primary data collected via 15 in-depth interviews and a survey with 123 participants from quantitative and qualitative backgrounds, this article seeks to highlight some of the current challenges and strategies in conducting solid research and reporting on it convincingly. Part of an EU Horizon 2020 funded project entitled Knowledge Complexity (K-PLEX), the interdisciplinary team located at the Freie Universität Berlin focused on epistemic cultures and issues of datafication and information loss among scholars carrying out research on emotions and affects.

Dimensions of Validity in Emotion Research
Validity in social-scientific research has been discussed in a range of ways. On the one hand, the concept of validity is associated with choosing the right methods in order to collect accurate data. On the other, it concerns data analysis, that is, the process of evaluating the data collected and their potential for meaning-making. The third dimension of validity, which complements both descriptive and interpretive validity,
theoretical validity. The latter pertains to the applicability of theoretical statements to a certain range of contexts. In quantitative approaches, most attention is drawn to the accuracy of measurement. Researchers are preoccupied with the question of whether their approaches actually measure what they want to measure. A multitude of labels such as construct validity, content validity or internal validity have been offered in response to methodological claims to validity. Attempts to translate the validity criteria for qualitative approaches have been undertaken by several researchers, e.g. Margaret LeCompte and Judith Goetz (1982).

In qualitative research, the ideas behind these labels are usually well-known but seldom articulated explicitly. In contrast to quantitative approaches, researchers face greater difficulties in showing how the evidence contributes or relates to the theory put forward.

More recent propositions differentiate between first-order criteria such as credibility, authenticity, criticality, and integrity, and second-order criteria such as explicitness, vividness, creativity, thoroughness, congruence, and sensitivity (Whittemore, Chase and Mandle, 2001). The ranking suggests that once researchers have made sure that they comply with the primordial principles of transparency and confirmability, they can pay attention to more fluid and flexible criteria. Especially for researchers who embrace a perspective that foregrounds the emic aspects, this ranking is problematic. Scholars in the field of emotion research who participated in the K-PLEX project formulated the challenge of representing the internal perspectives of research subjects in two respects: First, capturing collective emotional experiences in historical subjects was associated with high degrees of uncertainty; Second, defining distinct emotions in linguistic terms and in that sense referring to learned cultural concepts, rather than felt or sensed inner states, was identified as potentially distorting and simplifying.

If ‘emotion, like poetry, gets lost in translation’ (Beatty, 2019: 10), providing supplementary contextual information becomes indispensable. Social scientists unanimously agreed that the transferability of the meaning of words uttered in a certain context was highly problematic. As a way of comparing and translating, this is related to the question of external validation. Depending on the research object and the methodological approach, different answers have been given to the question of what additional contextual information is necessary to judge the validity of data and conclusions in emotion research (cf. Lehmann and Huber, 2018).

Some researchers were chiefly preoccupied with the validity of conclusions. Paul Ekman’s attempt to compare emotions in a cross-cultural way and to identify a set of universally valid basic emotions was mentioned as an example. Validity was found to be only temporary, as Ekman himself had to revise the list of basic emotions several times. Although the data were not collected in laboratory settings, the dialogic interactions with research participants with the aim to induce emotions were criticized as artificially constructed situations that did not correspond with locally established speech-event forms. Generalization for real-life situations could therefore not be assumed. Moreover, some linguists and psychologists pointed to the fact that people do not behave in ways that can be so neatly categorized, as complex interactional behaviour patterns and ritualized interaction strategies demonstrate. The analysis of emotions must not be reduced to controlled experimental situations that exclude any alternative explanations, or one accepts results that are no longer valid. Cross-checking in interviews was then suggested as a strategy for verification. By means of research subjects’ self-assessment, validity turns into validation and intersubjective agreements can be reached. Potential difficulties encountered – be it comprehensibility, memory or social desirability, or more fundamental ethical questions regarding the possibility that researchers and research participants might not necessarily pursue the same interests, commitments, and goals (Sandelsowski, 1993) – have been touched on but not explored in detail.

Even if many researchers agree that only detailed narrative accounts can do full justice to the complexity of emotions, how to establish validity in narrative research remains a controversial issue (cf. Creswell, 2013). In the interviews and survey of the K-PLEX project, anthropologists often pointed to the provenance and context of origin and assured validity by taking protocols and reflecting on how the data were gathered. A sociologist working on the relationship between scandalization, moralization, and emotions by conducting qualitative analyses of social media content went so far as to say that the meaning of emotions depended entirely on the context. Computational social scientists, however, deplored the fact that emotion recognition in speech analysis based on machine learning was exclusively relying on predefined categories and examples. As the machine cannot deal with exceptions or context, researchers need to cope with the majority bias in identifying emotions. This point is well illustrated by the statement: ‘We know that it is only valid if there is
one really prototypical situation’. A coping strategy intended to compensate for this problem was to be found in the collection of more ‘noisy’ data. However, this may give rise to other challenges. In a data-poor environment that consists mostly of noise, missing data can lead to falsely rejecting a valid hypothesis. Using data yielded by instruments developed at the initial stage of a research project and data from refined instruments in later stages clearly affects comparability as these data must be evaluated differently.

With big data, the question of validity is raised in new ways, in terms of both the data quality and the conclusions that can be drawn from the analyses. In big data research, less deterministic approaches referring to patterns instead of essentialist categories and classifications might alleviate some of the traditional social scientists’ challenges. In addition, non-intrusive methods can yield more valid results as social desirability and interviewer effects become insignificant. Yet, other criteria such as sample representativeness are not met, since some groups do not leave data traces. Are these findings nevertheless valid? Referring to the second-order criteria mentioned above, I want to reiterate that methodological and theoretical rigour is not the only means through which validity can be achieved. Patterns detected do not necessarily reflect what is in the external world, but they often build upon the researchers’ conceptualizations and creative imaginations. This is not to say that researchers invent data unrelated to phenomena and events, but reducing scientific practice to blindly clinging to data is not considered professional mastery. Big data research reformulates the relationship between validity and creativity in new ways. Data-driven approaches imply a high degree of serendipity as analyses produce findings that were not actively searched for. Big data analyses conducted by the interview partners in the K-PLEX project had found correlations between the Tsunami in the Indian Ocean in 2004 and the first recognizable wave of hate speech in Finnish social media. A second example pertains to German political parties on Facebook and the different emojis associated with each party. The right-wing party had received most of the angry faces while the social democrats scored higher with hearts. Interpreting the statistical correlations between political parties and emojis is not possible without a certain degree of creativity, unless complemented with qualitative studies to control for other intervening variables like gender. Relying on big data alone is not sufficient to ascertain, for instance, whether a laughing face was meant seriously or ironically, and therefore whether the person in question was laughing along with the joke or laughing at the person.

One common strategy to minimize bias and increase validity consists in cross-validation with a mixed method design. Several researchers from different disciplines reported seeking the confluence of evidence. Whereas a sociologist made efforts to compare the evidence of self-reflexive accounts of how people feel with the measuring of arousal by means of skin conductivity when using a smartphone, research teams composed of anthropologists and psychologists opted for a combination of long-term participant observation and experiments adapted to the local situations in order to obtain a variety of data on the socialization of emotions in different collectives. This approach enabled an expanded scope in considering mutual shapers and contextual factors, as well as more depth in the form of data intensity. While these two examples only point out the most innovative realizations of mixed method designs, data plurality is common in most ethnographic approaches. During field research, an interview can yield an audio recording, interview transcript, observation protocol, and entries in a field diary. Data plurality, then, includes not only speech data and written narratives, but also observations of facial expressions and body language. Although mixed methods approaches and source triangulation have been criticized for their failure to include full context information, the aim to strive for rich and comprehensive accounts should be acknowledged.

Moreover, when publishing results, scholars usually embed their data in theoretically founded arguments and commit themselves to theoretical candour. Together with fieldnote evidence and the elucidation of the ethnographer’s path, this constitutes the three paths for ethnographic validity (Sanjek, 1990). Real difficulties are faced when research data are shared with other scholars. Secondary analysis as a form of recontextualization nevertheless requires additional information on the particular purpose for which the data were constructed so as to enable researchers to draw ‘valid’ conclusions. Categorical, dimensional or appraisal models to describe emotional behaviour can all be legitimate because validity also depends on the research questions and aims. Certain
questions – such as what might constitute context-sensitive validity criteria and whether recontextualization is only valid when preceded by reconstruction – will need to be addressed in the future. Findings from the K-PLEX project indicate that little or no information on emotional aspects of the exchange between interviewer and interviewee is provided when data are shared (cf. Lehmann, Stodulka and Huber, 2018). Suggestions to place more emphasis on relational and situational aspects follow from this.

Patti Lather’s (1993) fourfold approach to validity – subsuming situated, rhizomatic, ironic, and neo-pragmatic validity – can be useful in this regard. Situated validity refers to the situated, positioned, partial role of the researcher. It can be defined as a form of accountability as it calls for self-reflexivity and engagement, and connects epistemology and ethics. Several authors within the critical epistemologies’ tradition have taken up these claims. Reflexive scholarship is then understood as engaging in discourse and encouraging research participants to articulate themselves. Validity can be formulated as a moral question of whose knowledge it is, how it is obtained, by whom, and for what purpose(s). Researchers and the research community cannot escape from their responsibility to make decisions on what is valuable and useful to study (Angen, 2000). To enhance the trustworthiness of research by reflexive positioning, four strategies have been suggested: identification with respondents; emotional enmeshment; personal associations; and auto-ethnography (Possick, 2009).

Scholars in the field of emotion research, when performing the balancing act of caring about the research participants while remaining impartial to the data, have reported on the difficulties associated with such a dual strategy. Key challenges and loss of information due to methodological or ethical restrictions have mainly concerned the embodied and relational dimensions of affective and emotional practices through discourse, body language, gesture, and tone.

Rhizomatic validity that attempts to undermine authority, regularity, and common sense, and replace these with interactive co-construction and co-theorizing has been discussed above in relation to communicative validation and member checks. Ironic validity, the third component of Lather’s approach, is associated with a transformative capacity whereby the author can position herself in a liberating way, emphasizing the ephemeral, rhetorical function of data. In emotion research, for example, scholars are well aware of the fact that data can have descriptive, performative, educative or still other functions. In particular, scholars who conducted research on emotion tracking acknowledged that individuals might ‘attempt to play-act a little bit in line with these measurements, either to send the right messages to the machines or to kind of subvert or fake it, in order to navigate it’. Whether emotion tracking yields valid results depends on whether or not the person perceives the feedback to match one’s own expectations. With self-tracking devices, the ways in which emotions are narrated continue to depend on habitus-based demands and social desirability. Depending on the object of research, the different dimensions of validity can intermingle in conflicting ways. Take, for example, the case of applied research in psychology that aims to design a self-tracking app to monitor and regulate one’s emotions. At what point does a product designed for emotion recognition provide sufficiently valid data to be used for therapeutic purposes? If patient-centred validation is prioritized, then the assessment criteria might differ from those of the researchers. Patients might want to feel that they are engaged in social interaction when using the app, whereas the accuracy of the data might be the researchers’ primary concern.

The fourth dimension of Lather’s validity concept (neo-pragmatic validity) stresses the researchers’ ability to tolerate the incommensurable, to deal with limits, paradoxes, and complexities. According to this principle, reporting ambiguities and contradictions is required if research is to be reported fairly. Going beyond the question of representation, validity can be defined as a ‘space of constructed visibility of the practices of methodology’ (Lather, 1993: 676). In the K-PLEX project, interviews with scholars in the field of emotion research highlight this challenge by disclosing how speech, texts, and images do not indicate emotions, but are rather proxy measures for emotions. To address emotions in their full complexity, researchers differentiated between enduring affective atmospheres and concrete emotional states. Tensions between purism and pluralism (Whittemore, 2001) can thus also be found in emotion analysis. Strategies to circumvent measurement biases that only consider acute intensities and no background emotions include: working in teams; comparing results with those of neighbouring disciplines; peer review; and combining deductive and inductive techniques. Thus, standards of judgement are as much representational as they are normative.

12 These principles can be seen as equivalents to the four standards of evaluation, as there are interpretive, procedural, emancipatory, and postmodern.
Conclusion

Validity affects all stages of emotion research: perception; processing; evaluation; interpretation; and (self-) report. The kind of knowledge perceived as valid – whether introspective knowledge, empirical knowledge or practical knowledge – cannot be defined in general terms, but rather depends on the research purpose. Criticisms have been directed at the narrow focus on individual consciousness, experiences, and expressions. In order to increase the validity of research data, the embeddedness of individuals in social situations, discourses, and social structures should be taken into account. This kind of framework builds on long-established discussions about scale and the opposition of public and private fields. It reiterates the notion that common behaviour and official discourse in a group does not determine the behaviour and speech of individuals. Doing justice to the complexity of both the collective and the individual scales can help to prevent scholars in the field of emotion research from reproducing and validating cultural stereotypes. Data and especially 'emotion data' can be better understood, integrated, and shared when framed and contextualized as fluid, contextual, and relational. Institutionalizing doubt and remaining modest when seeking to verify correlations, always cognizant of the fact that ambiguity cannot be dissolved entirely, can guarantee a reasonable reporting of data and results. This article suggests that techniques for establishing or demonstrating validity might not always be feasible. Coping strategies can be found in altering techniques. Emphasizing the credibility of data analysis and presentation can compensate for impaired validity in the research design and data generation.
References


A Defence of Historical Sociology

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Abstract
Since gaining popularity as a research tradition, historical sociology’s validity has been questioned on a number of fronts. Some have critiqued its focus on the past, arguing that sociologists should investigate the present. Its authenticity, as a field of study, has also been challenged on the basis that historical sociology shares certain features with the discipline of history. Commentators have ridiculed the methods associated with historical sociology, accusing the latter of being unsystematic and unreliable. This article offers a response to these criticisms and defends historical sociology as a valid research tradition. A historical sociological analysis of the tradition is presented and the complexities and contradictions of the critiques levelled against it are exposed.

Introduction
Historical sociology is a tradition within the field of sociology that seeks to understand how the social world has changed over time. The tradition itself, much like the social history that it strives to negotiate, has developed since the founding of the sociological discipline. With these changes, historical sociology has faced numerous criticisms. The aim of this article is to offer a response to three of the most prominent challenges to historical sociology’s validity.

The article is divided into three sections, each of which deals with critical issues. The first section considers a criticism made famous by Goldthorpe in 1991, which maintains that sociologists should not study the past, but rather focus on the present. To build evidence for why historical sociology should exist, a historical sociological outline of the tradition is offered, which summarizes its origins and social history. This outline also supplies context, in terms of the tradition’s developments, while illustrating why it is prudent and useful for sociologists to study past historical shifts. In the second section, the idea that the disciplines of history and sociology are one and the same is challenged by looking at how each of the respective disciplines has different goals and research styles. The third section focuses on the critique of the investigative tools used by historical sociologists. In response, it is argued that different methods are suitable for different research questions, and that historical sociology still produces valuable research.

The article concludes that it is reductionist to limit sociology’s output to nomothetic, present-centred results, produced using modern quantitative methods. To restrict the discipline in such a way negates the reasons why sociology was developed in the first place, while also narrowing the scope of knowledge creation. In consequence, this work advocates diversity and collaboration in the social sciences. Researchers should not be limited by differences, but instead ought to utilize them so as to advance academic understanding.

Sociology and the Scientific Study of the Past
In his widely cited critique of historical sociology, Goldthorpe argued that sociologists ‘should leave history to the historians’ (Mennell, 2017: 1). His justification for this was that sociological data collected in the present allows for ‘rigorous’ testing and the opportunity to grapple with ‘reliability and validity’ (Goldthorpe, 1991: 216). To exemplify his controversial point, Goldthorpe stressed that ‘social change’ can be studied by ‘life-course, cohort or panel studies’, which all take place in the present (ibid). For him, such studies do not have the same limitations as historical enquiry, as relics are always ‘partial and lacunary’, meaning that so too are the ‘inferences that can be drawn from the fragmented remains of the past’ (Bryant, 1994: 4). Thus, Goldthorpe implied that historical sociology is unscientific because it unsystematically attaches theories to incomplete fragments of social pasts. This article does not share Goldthorpe’s view and instead will argue that historical sociology is needed to scientifically understand social change. Evidence in support of this can be found by considering the origins of sociology.

Comte (1798–1857) was among the first to observe a need to study the social world in a scientific manner. Developed between 1830 and 1842, his two laws – the law of three stages and the encyclopaedic law of scientific development – led to his classification of physique sociale, ‘social physics’, which he would later rename sociologie, ‘sociology’, in 1839 (Gane, 2006): ‘logie’ referring to a subject or body of knowledge, and

13 ‘Relics’ is the term Goldthorpe adopted to refer to ‘existing available traces, artifacts and documents’ (Šubrt, 2012: 406).
'socio' from the Latin ‘societus’, which translates to ‘society’ (Taneja, 2005 [2003]; Walia, 2008). ‘Sociology’ is therefore the scientific study of society and social matters, and the body of work that is devoted to doing so.

The founding fathers of sociology, Karl Marx (1818–1883), Émile Durkheim (1858–1917), and Max Weber (1864–1920), followed in Comte’s footsteps by considering the social world scientifically. That said, Marx was less concerned with the academic discipline to which he contributed, famously complaining that the philosophers had spent too much time interpreting the world, rather than changing it (1969 [1888]). He believed that his scientific socialism was ‘a theoretical praxis and a practical theory’ (Löwith, 2003 [1993]: 46). For Durkheim, ‘sociology is not the appendage of any other science; it is itself a distinct and autonomous science’ (1892 [1895]: 162). Weber is famed for likening the study of the social to a science because of its ability to identify causal relationships and explanations for social actions/behaviours.

All four men, despite their differing aims and intentions, employed the tools of the new discipline to contribute to a systematic, scientific understanding of the social world. Yet, this is not the only connection that linked these scholars; they all used socio-historical methods. As Lachmann noted, ‘[s]ociology at its beginnings was historical because of the questions its founders asked’ (2013: 1). Comte looked at historical shifts in order to understand human development and present his case for the positivist period, from which sociology was born. Marx analysed the historical shifts that led people to live under capitalism and how major social change could lead them to a new age of equality (ibid). Durkheim’s concern was the historical shift from mechanical to organic solidarity (ibid). Weber focused on the historical shifts that helped to shape religious practice, human action, and capitalism (ibid).

Sociology’s founders had developed new ways of scientifically thinking about the social world by looking at past historical change. This, in essence, is the definition of historical sociology. Despite this, after their deaths, the sociology that these scholars had pioneered faced a political threat. In Europe, from the 1920s–1950s, fascism and totalitarianism stunted the growth of historical sociology. ‘Regimes which “knew” the future and invented the past rejected historical sociology’ (Smith, 1991: 2). With strict control over academia in Europe, sociology developed in the United States of America. Adams, Clemens and Orloff claimed that this aided the institutionalization of the discipline, and that it ‘took shape in the United States’ (2005: 4). However, they also acknowledged that sociology’s ‘historically-informed theoretical vision gave way to more ahistorical models of social and cultural change’ (ibid). ‘In America, sociology became a discipline from which scientific outputs with clear practical use were expected’ (Šubrt, 2017: 5). Theory concerning the social world was of little interest; research had to be scientific, practical, rational, empirical, usable, and, more often than not, quantitative. In the words of Smith, ‘[b]ig business and big science were the main repositories of value and knowledge in the leading capitalist democracy’ (1991: 2).

Despite the institutional (economic) worth granted to sociology that had practical utility, scholars of the 1950s and 1960s readopted historical sociology. According to Šubrt, ‘[b]y this point, sociologists had recognized that sociology could not be based on empirical research alone’ (2017: 5). Lipset (1950; 1963), Bendix (1964), Tilly (1964), and Moore (1966) all ‘engaged in fundamentally historical questions’, which would lead the way for ‘the next generation of historical sociologists’ (Adams, Clemens and Orloff, 2005: 5). In the 1970s and 1980s, historical sociology was on the rise in both America and Europe. Wallerstein (1974; 1983), Paige (1979), Tilly (1975; 1978; 1981), and Skocpol (1979) all made breakthroughs in studying social change by adopting historical sociological standpoints. With this resurgence came criticism. As noted by Calhoun, these self-established historical sociologists gained attention from ‘many leaders of the dominant quantitative, scientific branch of the discipline’ who were to ‘dismiss their work as…somehow not quite “real” sociology’ (1991: 305).

As is evident from Goldthorpe’s critique, since the resurgence, some commentators still do not see the value of historical sociology. Mennell (2017) has argued that these views may be the product of physics envy, which has been a lasting problem in sociology and other disciplines within the social sciences. Yet, this article posits that such commentators are missing the point of sociology in general. Since its origins, sociology has always been the scientific study of past historical shifts and changes. In fact, historical shifts and social changes have done much to benefit sociology as a discipline, in more than just the subject matter that it covers. Due to the European academic constraints of the 1920s–1950s, sociology was to further develop as a discipline and incorporate new methods. Life-course, cohort, and panel studies, which provide valuable results, are examples of such methods. Nonetheless, it is worth remembering that these methods were built on the foundation of historical sociology.

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46 ‘Physics envy’ refers to a perceived need for research to be considered as ‘valid’, ‘logical’, ‘quantifiable’ or ‘empirical’ as that which is found in the discipline of physics.
The development of methods that allow a researcher to study the present does not mean that historical sociology should, itself, be left in the past. As Calhoun has rightly stated, ‘[t]he most compelling reason for the existence of historical sociology is embarrassingly obvious (embarrassingly because so often ignored). This is the importance of studying social change’ (2003: 383). The world existed in a state of flux prior to the development of sociology as a discipline and, as a result, sociology still needs to understand historical shifts and social change. This view was echoed by Mikl-Horke, who believed that Goldthorpe ‘overlooks the fact that every social reality has an historical nature and all sociological data will finally become part of history’ (cited by Šubrt, 2012: 405). Indeed, as Mennell (2017) has made clear, Goldthorpe’s own sociological work is now a relic. The present of today will always become the past of yesterday, and there exists no method to preserve all relics.

History and Sociology
If, since its creation, sociology has existed to study change in the social world, it is not surprising that sociologists such as Runciman (1970), Giddens (1979), and Abrams (1982) have claimed that it is no different from history. After all, at their core, both fields focus on human change. Yet, as argued by Mann (1994), Lachmann (2013), and, to an extent, Goldthorpe (1991), there are some similarities between history and sociology, but there are also many differences between the two disciplines. History is defined as the study of the past using written records and, because of this, it remains an incredibly broad discipline. It is linked to prehistory, which is the study of the human past before written historical records. Other disciplines, like sociology, focus on humans, but despite this shared connection, (pre)history and sociology remain highly distinct. As Lachmann has posited, ‘history and sociology have their own histories, and the past intellectual, institutional, and career decisions made by historians and sociologists shape the questions asked, the methods employed, the data analysed, and the arguments offered within each discipline today’ (2013: 6). This view is echoed by Šubrt, who notes that ‘the fact remains that sociologists and historians do not speak a common language’ (2017: 7).

One of the key reasons why sociology and history are so divergent as disciplines is that (pre)historians often specialize in a particular time period, which is linked to a geographical location, e.g. British Mesolithic or Ancient Egyptian (Tilly, 1991; Burke, 2005; Lachmann, 2013). Conversely, sociologists tend to focus on a specific question that concerns social matters. For example, Durkheim questioned the cause of the shift from mechanical to organic solidarity, while Marx analysed how capitalism developed as a mode of production. While their enquiries turned to history for answers, they were not grounded in a precise time period or geographical location. The historian would not ask questions about Russian communism if they were researching tribal equality in the Mesolithic period, but the sociologist might do so. The way in which studies within the respective disciplines are structured is drastically different too. There are advantages and disadvantages to both methods, but it is clear that ‘[w]hat distinguishes historical sociology from history is that it uses history for an explicitly sociological purpose’ (Delanty and Isin, 2003: 3).

Historical Sociological Research
Despite agreeing that history and sociology are independent disciplines, Goldthorpe argued that sociologists should focus on the present, while historians should focus on the past. He held that present-centred sociological data was of a superior scientific quality. Yet, he also thought that sociologists are not limited by time and place in terms of evidence collection: ‘Historians…are concerned with finding evidence from among a stock of relics. In contrast…sociologists have open to them the possibility that is largely denied to historians…[sociologists] can generate evidence…when they engage in “fieldwork”’ (Goldthorpe, 1991: 214). In consequence, Goldthorpe was of the opinion that sociologists should produce their own evidence, in the present, which could be tested empirically in order to develop general social laws.

Although Goldthorpe’s style of sociology can be said to produce valid results, it is reductive to condemn other ways of understanding the social world. As previously noted, sociology is necessarily historical and needs to study the past so as to understand historical shifts and social change. Even so, Goldthorpe would counter this by saying that these shifts and changes can be investigated in the present. He illustrates this by claiming that if a sociologist is studying industrial societies, it is only ‘sensible’ for them to look at contemporary industrial societies (Goldthorpe, 1991: 214). This is problematic insofar as industrialization has existed, throughout the ages, in many different forms and guises. To illustrate, the Peruvian industrialization seen today is not the same as the Peruvian industrialization of the nineteenth century. In turn, it remains controversial to believe that a sociologist’s aim would be to develop a theory that is applicable to ‘all industrial societies’ (ibid). Indeed, it is

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5 History, prior to the appearance of the human species, is typically explained by those who study earth science, which is linked more closely to the discipline of geology than history.

6 See Burke (2005) for a detailed account.
argued that many early sociologists did have nomothetic goals, and some sociologists – historical or otherwise – still strive to produce generalizable macro theories. However, sociology can also view the social world on a micro scale. Not being confined to time or place, sociologists can study past and/or present forms of industrialization, in specific locations, without the aim of creating a theory that would apply to all industrial societies.

Researchers have autonomy in respect of the questions they ask, and they are free to select the method that is most suitable for answering them. As Mann has stated, ‘there are differences between methods appropriate for studying the past and present…sociologists of the present cannot go out and create whatever data they like’, and nor can a historical sociologist (1994: 38). ‘[A]s E. P. Thompson said, “you can’t interview tombstones”’ (Gittins, 2003 [1998]). Equally, you cannot collect survey data from some, very alive, elites. As such, if studying successful industrialists, for example, it could be more fruitful to turn to history for answers than to receive no response from elite contacts. No single research method is free from limitations or problems, but it is the responsibility of the researcher to select the most appropriate method.

The point that Goldthorpe misses is that, depending on the object or area of research, historical methods can be more appropriate than methods used to study the present. In light of this, his argument invokes sociological reductionism by placing a greater value on methods that are deemed to be more valid or of a higher quality due to their application in the present and their reproducibility. He also overstates the importance of creating new data, when, if relics are perfectly accessible, there is little to no value to be found in generating more ‘evidence’ when said ‘evidence’ already exists and can provide a better means of study. This is not indolence on the part of the sociologist; it is simply logical and systematic.

Conclusion

Goldthorpe’s argument is reductionist from the outset. Not only is he critiquing historical sociology, but he is also trying to re-centre the discipline of sociology more generally. For Goldthorpe, sociology should be present-centred, nomothetic, and testable, and it should make use of modern (quantitative) methods. Sociology has undergone a number of social changes of its own, which have positioned it as a rich and diverse field of study. Sociologists now adopt an assortment of research methods, align themselves with varying schools of thought, ask wide-ranging questions about the past, present, and future of the social world, and work in collaboration with people in industry and other academic disciplines. Thus, in agreement with Bryant, who rejects Goldthorpe’s ‘call to halt or reverse the trend towards interdisciplinary synthesis’, sociologists should instead push this trend forward (1994: 15).

In closing, this article calls for researchers to embrace their differences in order to further inform the globalized academic world. To illustrate, in order to rectify Eurocentric and Western-centric perspectives in social-scientific accounts of human development, a sub-branch of historical sociology has recently emerged known as ‘international historical sociology’. The expansion of this tradition, which allows for more inclusive and informed voices, is one of the many advantages of academic globalization. Historical sociologists are not limited to the confines of sociology itself, and can easily pool their resources with colleagues from other fields, including historians, economists, psychologists, geographers, and physicists from across the globe. It is high time that researchers stopped subjecting their disciplines to reductionist frameworks in order to claim to be the pinnacle of validity. Instead, an emphasis should be placed on collaboration, via inter-departmental, inter-disciplinary, inter-institutional, and inter-cultural research. Rather than viewing difference as a weakness and something to criticize, it can be used to diversify and strengthen the validity of knowledge.
References


Abstract
In this article, I consider how dominant sociological conceptions of validity reproduce the racial status quo. I argue that sociology has its own racialized regime of truth, which marginalizes research that is critical of the racial status quo, and denies such research any validity. I explore this, first, from a historical perspective, focusing particularly on how Du Bois’ critical work was denied validity by his contemporaries (while other scholars’ research reproducing structural racism was welcomed with open arms). I then focus on the present, examining how race critical research is often recast as ‘mesearch’, and consequently denied validity in a way that disproportionately affects scholars of colour. I then reflect on the problem that we need validity to do ethical research, but so long as the racialized regime of truth shapes what is valid, race critical sociology will continue to be marginalized. I finish the article by considering how this marginalization exemplifies Toni Morrison’s idea of ‘racism as distraction’, where race critical scholars must overcome a validity deficit. Our work is automatically assumed to not be valid, and we have to struggle to have it recognized as such.

Philosophy and Validity, Sociology and Truth
There is a tension between philosophical approaches to validity and sociological approaches to truth. Philosophy teaches that an argument is valid when it is impossible for all the premises to be true and the conclusion false (Marcus, 2018). However, sociology teaches that truth and falsity are more than matters of correspondence, and are embedded in networks of power (Du Bois, 1898). Thus, in contrast to the abstract philosophical approach, sociology shows that validity is embedded in networks of power, and thus often serves to reproduce the status quo. As such, it is important to recognize that while sociologists may think that our discipline is the critical discipline, it is shaped by a ‘regime of truth’.

In this article, I consider how dominant sociological conceptions of validity are shaped by a ‘racialized regime of truth’, which marginalizes race critical research as non-science (and nonsense). Thus, since the beginnings of sociology through to the present day, research that critiques the racial status quo is denied validity. In this article, I analyse this invalidating of sociological research by starting with a discussion of W. E. B. Du Bois and Black sociology, showing how in sociology’s beginnings, the research that reproduced the racial status quo was deemed valid, while more critical approaches in racism studies were denied this validity. I then proceed to show how this dynamic carries into the present day, reflecting on Toni Morrison’s notion of ‘racism as distraction’. Here, I draw on my experiences of the reception of my work on the Black middle class to show how race critical research has a ‘validity deficit’ and that scholars operating in this research area must work much harder than their colleagues to prove the validity of their research. I conclude by arguing that in order to get to a stage where we can have a working conception of validity that allows for race critical research to be construed as valid, we need to dismantle sociology’s epistemological commitment to White supremacy – that is, the discipline’s racialized regime of truth.

Regimes of truth
The notion of a ‘regime of truth’ was articulated by Foucault (1994: 131), when he claimed:

(...each society has its regime of truth, its ‘general politics’ of truth – that is, the types of discourse it accepts and makes function as true; the mechanisms and instances that enable one to distinguish true and false statements; the means by which each is sanctioned; the techniques and procedures accorded value in the acquisition of truth; the status of those who are charged with saying what counts as true.)

Central to Foucault’s (1994) argument was a recognition that societies have a system of truth production, with various institutions, practices, and people serving as legitimate truth producers. As with most of Foucault’s work, there is little mention of racism, and no citations of previous theorists with similar ideas. Addressing these gaps within Foucault’s approach, I theorize the ‘racialized regime of truth’. This racialized regime of truth is responsible for ordering which institutions, which people, which arguments, and which types of knowledge are accepted as ‘true’ on racial matters. First and foremost, the racialized regime of truth works to reproduce the racial status quo. It functions to legitimize knowledge and ways of thinking that reproduce the racial order, and to exclude or marginalize knowledge and ways of thinking that are critical towards the
racial order. My argument is that this racialized regime of truth is not just ‘out there’, but it is fully functioning within sociology, and it works to deny validity to research that is critical of the racial status quo. Indeed, this is a process that sociologists have been struggling with since the discipline’s creation.

**Du Bois, Black sociology, and racialized regimes of truth**

Du Bois, for instance, regularly lamented how his contemporaries in the nineteenth and twentieth centuries denied the validity of critical research. Du Bois’ critique was twofold. Firstly, Du Bois (1898) points out that many social scientists were so convinced of the natural inferiority of Black people that they did not see the study of racialization and racism as a valid area for sociological investigation. Secondly, Du Bois (1898) argued that the social scientists who were studying ‘race’ were doing so in ways that reproduced racism. In *The Philadelphia Negro*, Du Bois (1967 [1899]) refers to these people as ‘car-window sociologists’. Critiquing these sociologists, Du Bois (1898: 13–14) argues that:

> […] we reverently receive a column of figures without asking who collected them, how they were arranged, how far they are valid and what chances of error they contain; we receive the testimony of men without asking whether they were trained or ignorant, careful or careless, truthful or given to exaggeration, and, above all, whether they are giving facts or opinions.

Continuing this critique, Du Bois (1898: 14–15) concludes:

> It is so easy for a man who has already formed his conclusions to receive any and all testimony in their favor without carefully weighing and testing it, that we sometimes find in serious scientific studies very curious proof of broad conclusions.

This critique laid out by Du Bois (1898) highlights the first side of validity and the racialized regime of truth. Du Bois shows how much research on Black Americans was merely an exercise in White confirmation bias; the ‘valid’ research was the research that reproduced racism. However, the issue of validity then became even further exacerbated. In response to the lack of critical research on Black Americans, Du Bois co-founded the Atlanta School of Sociology in the 1890s (Wright II and Calhoun, 2006). This Atlanta School was dedicated to the sociological study of social problems affecting Black Americans – not only was it the first empirical sociology institute in the US, but it was also the first to make methodology sections a prerequisite for all papers (Wright II, 2016), and the whole ‘laboratory’ was dedicated to producing anti-racist research which would challenge the racial status quo (Du Bois, 1990 [1944]).

Nevertheless, given that it was critical to the racial status quo, the work produced by Du Bois and the Atlanta School was never seen as ‘valid’ social science by the mainstream (Wright II, 2016). Indeed, even now we are taught that the first school of empirical sociology was the Chicago School, with Robert Park being the pioneer of sociological methods. Of course, once we appreciate how the racialized regime of truth works, this makes perfect sense. While the Atlanta School was showing how racism constrains Black Americans, and the various measures that can be taken to ameliorate these conditions (Du Bois, 1968), the Chicago School developed a ‘race relations paradigm’ which instead saw Black Americans as a social problem of the state, with White racist violence being rationalized as a natural by-product of encountering ‘different’ people (Frazier, 1947). Given the racialized regime of truth, we ought not be surprised that seminal work critiquing the racial status quo has been forgotten, while work reproducing the racial status quo is construed as essential to the development of sociology. Furthermore, with the rise of quantitative social science in the twentieth century, many mainstream sociological studies merely treated ‘race’ as an independent variable, which allowed for sociologists – including those working in the ‘race relations paradigm’ – to espouse ‘valid’ arguments around Black inferiority, further squashing the potential for critical scholarship to be taken seriously (Zuberi and Bonilla-Silva, 2008).

**A Century On and We’re Still Not Valid**

The question then turns to whether the situation is any better now, a century on from Du Bois’ mission against uncritical scholarship. The answer is that sociology is still shaped by a racialized regime of truth, and this regime still results in race critical studies being denied validity. In fact, the racialized regime of truth has been one of sociology’s constants since its inception. Thus, in the early days of the discipline, Du Bois’ critical work was seen as invalid sociology; however, there were also prominent Black feminist sociologists, including Cooper (1990 [1892]) and Wells (2014) whose work was even further marginalized to the extent that it was not
just seen as invalid sociology like Du Bois’ work, but it was not seen as sociology at all. Nearly one hundred years later and Patricia Hill Collins (1986: 26) makes the same point when she critiques how sociology’s ‘white male insiderism’ pushes Black feminist standpoints to the margins of the discipline, making Black women ‘outsiders within’. Moving on another half a century and we see critiques of ‘white sociology’ (Bhatt, 2016; Meghji, 2019), ‘methodological whiteness’ (Bhambra, 2017), a ‘possessive investment in white sociology’ (Brunsma and Wyse, 2018), the ‘White rule in sociology’ (Bonilla-Silva, 2019), and the discipline’s ‘white logic, white method’ (Zuberi and Bonilla-Silva, 2008). As such, it is quite clear that those of us within critical race (or race critical) studies are well aware of sociology’s ongoing racialized regime of truth.

This contemporary racialized regime of truth prevents much race critical research from being accepted as valid by mainstream sociologists, and consequently devalues the sociological contributions of many race critical scholars (many of whom are scholars of colour). In fact, it is often because much race critical scholarship is produced by scholars of colour that the validity of such research is denied. Through sociology’s racialized regime of truth, such research is converted into me-search; mainstream sociologists see this work as scholars of colour simply researching their own lives or ‘people like them’ (Bonilla-Silva, 2017). Through this devaluation of race critical research, mainstream sociology separates the ‘sociology of race’ or ‘critical race studies’ into its own subdiscipline where they hope it cannot infect the rest of the discipline (Virdee, 2019). While there may now be more institutional space for the sociology of race than there was in Du Bois’ time, and indeed even than when Collins was critiquing White male insiderism, a constant remains: sociology’s racialized regime of truth still pushes race critical research to the disciplinary margins, to a position of invalidity.

**Theoretical and Interpretive Validity**

One of the key issues in this respect is that even liberal, well-intentioned approaches to validity in sociology make it very difficult for race critical research to be accepted in the mainstream. We can see this, for instance, in the rise of analytic induction, where validity is construed as the accuracy of one’s research and yet remains a property that we work towards rather than one that we fully realize (see Katz, 2015).

At the heart of the analytic induction paradigm is the idea that the concept of validity can be taken out of its quantitative association with ‘generalizability’ and reoriented towards qualitative research. Through this redirecting of validity, we get the doubled notions of interpretive validity, that is, how accurately the researcher interprets their participants’ narratives and ethnographic observations, and theoretical validity, that is, how accurately the researcher goes from what participants do (for example, a student throwing a pen at a teacher), to the wider class of which these actions are representative (for example, student resistance to authority) (Maxwell, 1992). I do not wish to argue that this doubled conception of validity is useless, and that qualitative researchers ought to ignore it. In fact, working towards both interpretive and theoretical validity is an essential practice for ethical research.

My focus is on a more systemic form of critique. Namely, both interpretive validity and theoretical validity are concepts that we work towards in our research, but the extent to which one’s work achieves theoretical and interpretive validity is defined by the wider scholarly community. This is one of the main reasons why we have peer review, and, indeed, it underlines much of the reason for making one’s data publicly available – so that others can holistically see how you came to your research findings (see Contreras, 2019; Khan, 2019; Lamont, 2009). A problem thus arises in that if one is publishing race critical work that challenges the status quo, the wider scholarly community will likely dismiss this work’s theoretical and interpretive validity. The problem, therefore, with this approach to validity is not the definition of validity itself, but the fact that theoretical and interpretive validity can be, and are, shaped by the racialized regime of truth.

Of course, the problem is then exacerbated by the fact that much race critical research is qualitative, as we often use qualitative methods to produce ‘counter stories’ to the status quo (Delgado and Stefancic, 2001). In virtue of critiquing the racial status quo through qualitative research, whole sections of sociologists, sociological thought, and research are either being denied validity or forced to work especially hard to prove that our work as valid. In this regard, I want to finish this article by reflecting on one instance in which this racialized regime of truth, which structures how one’s theoretical and interpretive validity is judged, has affected my own research.

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17 Of course, as Ray (2016) puts it: ‘White scholars do mesearch all the time. In many disciplines, that is simply called the canon.’
Racism as Distraction, and the Epistemological Arm of White Supremacy

As a qualitative researcher exploring how racism affects Black middle-class people, every single one of my peer-reviewed articles has had at least one reviewer say something along the lines of ‘How common was this to your participants’ experiences?’ Again, this claim itself need not be problematic – it reflects a ‘healthy scepticism’, as one reviewer once rightfully put it to me. However, there is a specific dimension to racism that turns the question of ‘How common was this to your participants?’ into a (sometimes unintentional) derogatory stance towards one’s research. Especially now that post-racialism is hegemonic, accusations of racism are often met with disbelief (Meghji and Saini, 2018). Given this post-racialism, when reviewers critique my work by asking about its ‘generalizability’ to the overall sample, it seems to reproduce the structure whereby my participants’ experiences and navigations of structural racism are simply denied. This was particularly apparent to me when the British Sociological Association were working with me on a press release about my work, where I was questioned about my study’s thirty-two participants:

Can you say what proportion of the thirty-two said they felt uneasy at being stared at or confronted by security or other such discriminatory behaviour? […] If you’re able to say roughly what proportion of your interviewees had attended some kind of cultural event and been made to feel uncomfortable […] roughly how many felt that way solely because they were the only black people in the audience (and for no other reason), and how many because they were stared at or questioned, or overhead remarks, etc.?

Underlying this questioning we see the logic of the racialized regime of truth, and its shaping of interpretive and theoretical validity. Namely, this racialized regime of truth creates a logic by which if only a minority of participants have an experience construed as ‘racist’, then it does not merit sociological investigation – it is not a ‘valid’ finding. It shifts the ‘burden of proof’ onto the oppressed rather than aiming towards a critique of the people and structures responsible for this oppression. This shows exactly what Toni Morrison (1975) was critiquing in her notion of ‘racism as distraction’, where the racially subordinate must constantly prove and refute racist tropes:

The function, the very serious function of racism is distraction. It keeps you from doing your work. It keeps you explaining, over and over again, your reason for being. Somebody says you have no language and you spend twenty years proving that you do. Somebody says your head isn’t shaped properly so you have scientists working on the fact that it is. Somebody says you have no art, so you dredge that up. Somebody says you have no kingdoms, so you dredge that up. None of this is necessary. There will always be one more thing.

Expanding on Morrison’s (1975) work, racism often means that the racially subordinate must consistently prove that racism exists; we have to constantly defend our experiences from structures and people who downplay the continuing significance of race(ism). This fight against the distracting nature of racism is no different in sociology. Within sociology it is through these community-defined concepts of truth, accuracy, theoretical and interpretive validity that we see the distracting, devaluing nature of racism. The problem is not the concepts themselves, but the fact that they acquire their meaning through a racialized regime of truth. Sociologists critical of the racial status quo are playing a losing game, because while sociology is supposed to be the most critical of academic disciplines, its dominant epistemology is more reproductive than radical. This is precisely the case with the concept of validity. On the one hand, retaining a concept of validity is absolutely essential for ethical research. Yet, on the other, the validity of race critical research will only be recognized once we manage to move beyond the discipline’s racialized regime of truth which itself shapes the kinds of research methods, questions, and findings that are deemed valid to begin with. On this note, I end this call to battle sociology’s epistemological commitment to white supremacy – the discipline’s racialized regime of truth – with a reflection from Bonilla-Silva (2019: 18), who puts it far better than me:

I know most White sociologists believe we do not have serious racial issues in sociology, or worse, think that whatever problems we have are caused by sociologists of color. Doubters should check the data […] White sociologists must get serious about race matters even if doing so hurts […] The question before us then is this: will we face our racial issues and work to create a truly inclusive and multicultural sociology, or will we continue believing like Pangloss that ours is ‘the best of all (sociological) worlds’? I sincerely hope we choose door number one.
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Invalidating the Archive: Interpreting Silences and Inconsistencies

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Abstract
This paper questions whether and how the historical narrative that is validated in the institutional archives can be dislocated, while adhering to the scientific requirements of the law faculty in which the authors’ doctoral research is situated. Through a joint auto-ethnography, the authors explore their discomfort and anxieties prompted by the need to interpret silences, inconsistencies, and undertones in official state archives during their archival research on racial thinking in the context of the postcolonial regulation of intimacy. Both researchers struggle with the interpretation of archival silences and inconsistencies, as they attempt to work through the question of how their own positionality deeply influences their interpretation. This gives rise to certain questions around how one understands silence and inconsistency within a specific historical time and space, and how this might impact the researcher. In responding to such questions, new ways of reading the archive have been developed within the humanities that aim to uncover the power dynamics inherent in the production of the archive. In this paper, the authors reflect on the usability of these practices in their multidisciplinary research environment.

Introduction
The archive is no longer a privileged space for historians: over a decade ago, scholars from various disciplines have turned to the archive to understand the present. With the growing interest in the archive as an object of knowledge, rather than as a source of knowledge, different modes of reading and interpreting the archive have enabled scholars to shed light on the production of history and power. This underlines the importance of interrogating the telling of history, and the archives that enable us to do so, and has given rise to the productive field of critical historical analysis, which has attempted to invalidate, correct, and complement (national) historical narratives.

In the same line of work, our PhD research explores whether and how ‘interracialized intimacies’18 (Haritaworn, 2012: 90) were regulated in the decolonization and immediate postcolonial period in France and the United Kingdom. The aim of our respective research is to contribute to critical readings of postcolonial histories in order to understand the racialized exclusions encountered in contemporary France and the UK. Our research is part of an interdisciplinary project within the law department. Our work turns to state archives, as one of the locations in which power dynamics are negotiated, to question how racialized, sexualized, and gendered logics have functioned to categorize and regulate society during and in the wake of decolonization.

Turning to the archive to analyse and broaden the validated historical narratives has forced us to reflect on the methodological challenges presented by the need for validity in interpreting archival findings. In the research on France, Rébecca encountered fragmented material and inconsistencies that reflect France’s ambivalence in treating ‘race’ as a category of difference. Nawal, on the other hand, was confronted with one-dimensional representation and painful silences regarding the legacy of Black19 nurses. In this paper, we will reflect on the workability of critical theoretical interventions across the social sciences and humanities on our own archival practices, as we seek to reconceptualize the meaning of validity.

Situating Our Research
The challenges we have encountered are, to an extent, specific to the national-historical context in which we carried out our research, but they are similar in the sense that they require a methodological approach that overcomes disciplinary boundaries and narrow definitions of validity. The specificity of France’s historical national narrative as a ‘colour-blind’ society causes a particular challenge for research on racialization in France. Research that attempts to understand racialization in France has argued that the French construct themselves as a universal civilization while remaining unconscious of the racialized mechanisms of repression

18 Building on the work of Jinthana Haritaworn we consider ‘Interracialized intimacies’ to be romantic relationships between two people that are considered to belong to different ‘races’. These types of relationships are ‘Interracialized’ because an external gaze subjugates and encapsulates them within the artificial binary paradigm of ‘race’ thinking, therefore, they are considered to disrupt racial hierarchies.

19 We acknowledge that the term Black is contested. For the purpose of this article, notions of Blackness are not limited to phenotypical features.
that are integral to the French nation (Stovall and Van Abbee, 2003; Peabody and Stovall, 2003; Saada, 2007). To understand the mechanisms of racialization, then, requires the reinterpretation of fragmented and implicit racial thinking in the French archives, instead of uncovering explicitly racist policies. Rébecca's research looks at archives of the different ministries, local governments, and law enforcement that were involved with the regulation of postcolonial immigration in France. In these archives, she attempts to understand the subtle racialized logics that underpin the policies on postcolonial immigrants.

In the contemporary context of the UK, interracial heritage has been celebrated for signifying the 'multiraciality' of the UK (Ifekwunigwe, 2015). However, the colonial archives tell the story of the unwanted and inferiorized 'halfcast', which has travelled into the metropole. Scholars of colonialism have argued that ‘miscegenation’ threatens the construction of British identity, which brings the sexual encounters of white women with Black men into the public and regulatory domain (Bush, 1999: 25). Generally, the contributions made by Black women to the British national life have not been substantially researched; this is especially the case in relation to ‘interraciality’. This is striking because of the large-scale immigration of Black nurses from the 1950s to the 1970s.

Nawal is particularly interested in archival materials such as official documents from the labour and health ministries, church archives that focus on issues relating to immorality or indecency, private archives of the nurses and their family members, and hospital archives.20 By collecting materials from numerous archives, Nawal hopes to comprehend the impact of the imbrications of gender, ‘race’, and sexuality in British history and nation-making.

The Archives’ Juridical Command

As our research is carried out under the auspices of the law faculty and pertains to laws and regulation, the issue of what is considered ‘usable data’ – that is, which archival documents are valid to respond to the questions posed in our research – is a contentious one. Generally speaking, the field of law has been late to the archive party. While some legal historians have challenged what should be considered as a legal source, there has been no significant questioning of what counts as law (Mawani, 2012). Hence, within the European legal context, the administrative and bureaucratic archive has only sporadically been explored in relation to ‘race’ and intimacy.

The law and the state archives are closely connected. Derrida has argued that the beginning of the law, that which underpins it, resides in the archive (Vismann, 2008). According to Derrida, the archive itself has juridical command as it ‘speaks the law’, and as such, the files that constitute the archive should be considered and analysed as objects of knowledge within the field of law. The law differentiates between allowable and unallowable action. The state archive, however, marks the difference between authorized and disqualified knowledge that underpins law (Stoler, 2012: 20).

Thus, the ways in which the archive qualifies specific knowledge claims as true and false, or, perhaps even more revealing, as irrelevant, reflects the structures of power that regulate society. Whereas the British colonial archives show that the regulation of relationships between white women and Black men were a topic of interest to authorities, sexual encounters between Black women and white men remain unmentioned21. This indicates that Black male sexuality was seen as dangerous to white women, while white men’s sexuality was not seen as dangerous to Black women (McClintock, 2013). An analytical focus that takes into account how the archive itself categorizes and thereby regulates relationships helps us to understand how meanings of ‘race’, gender, and intimacy were constructed.

Similarly, in the French archives, sex, ‘race’, and gender played a role in the regulation of immigration. Rébecca found that in the French state archives, ‘interracialized intimacies’ were casually invoked as self-evident objects of concern. She did not encounter any explicit regulations or laws on ‘interracialized intimacies’. Instead, ‘interracialized intimacies’ were problematized in a more subtle way. For example, at times ‘mixed couples’ were promoted while other types of ‘interracialized’ sexual relationship were problematized in an anecdotal manner in numerous official and unofficial letters, governmental research, and various documents of different ministries and (sub-)committees. This shows that the meaning of ‘race’ and intimacy cannot be pinned down to something static. Rather, their meaning can only be understood as a circulation of signifiers that are co-constituted through the acts of categorization, archiving, and regulation.

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20 I have looked a variety of documents at the National Archives in Kew in London; for example, minutes of the National Committee for Commonwealth immigrants HO 231/1, Racial discrimination CO 103/2/478, Recruitment and the employment of immigrants NH 165/375

21 Typescript guide to the Race Relations Bill 1968 and pamphlet ‘Colour and Immigration in the United Kingdom 1968’ by the Institute of Race Relations/Facts paper and flyer ‘Proof in black & white’ by the Lincoln and County Committee for Human Rights Year 3AMS/B/16/11, the status of half-casts CO 822/36/16, minute by Mr Maclennan, ‘Passports: Mr Oladele Adebayo Ajosie and his White Wife Beatrice’ CO 554/103/3. In order to be jointly repatriated with their black wives, this group of men had to wait until ships with special accommodation for women became available. The CO 554/103/3, Repatriation of Natives with White Wives Minutes as to policy to be adopted in regard to CO 137/375, no. 48782.
By searching only for regulations and laws, the creation of sexualized, racialized, and gendered meaning would have been lost. In this regard, our research requires that we depart from our institutional context concerning the internal validity of our research methods: it should favour the analysis of the state archive as a regulatory practice, instead of reinforcing the differentiation made by legal scholars between law and governmental policy. Consequently, we aim to uncover specific regulations on ‘interracialized intimacies’, analysing the absence of regulation and investigating the archival practice as a site on which history is created.

In/valid Archives
Our work is informed by critical interventions in archival research that have sought to remedy the erasure of versions of the past, to uncover those voices that cannot be heard in the letters typed by officials, carefully preserved in the boxes that determine the telling of the past. Subaltern Studies have been influential in postcolonial theory as they intervened in Indian history-making around the 1980s to argue not only that subalterns make histories, but also that the relationship between power and knowledge needs to be interrogated, as the archive is a technology of power that serves to create specific forms of knowledge (see e.g. Guha, 1983; Guha and Spivak, 1988; Chakrabarty, 2000). However, Subaltern Studies have been problematized (at times from within their own ranks) in response to certain issues that arise as a result of seeking to represent the subaltern voice (see the acclaimed text by Spivak, 1994). Namely, the ‘authentic’ voice cannot be retrieved because this effort is always embedded in systemic structures of power. Other, later interventions call for reading ‘along the grain’, to uncover the rationales that weave the archive together, enabling an analysis of the mechanisms that validate historical narrative creation, rather than uncovering an impossible truth (see e.g. Stoler, 2012; Levine, 2004).

Archival labour with the aim of uncovering that which has not been validated throughout history is, according to Arondekar, an ‘unrepresentable search for an impossible object’ (2009: 10), precisely because the archive has made it un-enunciable. In discussing sexuality in the colonial archives, Arondekar shows the difficulties in tracing homosexuality in the colonial archives, even though its regulation was an integral part of colonial politics. Homosexuality was always mentioned anecdotally as an undeniable problem, but it was never actually substantiated in official archival form (Arondekar, 2009: 19). Accordingly, Foucault has argued that the archive is the law of what can be said: it is an unruly system of enunciability of the past, present, and future (1972). What is included and preserved in the archive determines what narratives can be articulated, and what must be excluded. In our research, we attempt to understand why and how certain narratives on ‘interracialized intimacies’ are made enunciable while others are excluded.

Silences, Hypervisibilities, and Inconsistencies
While these critical interventions are helpful to understand how to approach the archive, they present some challenges in the interpretation of the archive. The pertinent silencing of Black women’s private lives in the history of the United Kingdom and in Black history of the UK reveals how the archive works to exclude Black women from the British national narrative. During the official tribute paid to Black nurses by the National Health Service in 2018, their trails, tribulations, and triumphs were shared on diverse platforms. Yet, the reconstruction of this story disregarded the impact of the conditions of the nurses’ professional lives on their private lives. The silencing of the nurses’ complexity occurs on multiple levels. The private lives of the nurses were invisible in the historical documents and the relevance of their lives is academically under-researched. At the same time, these women are hypervisible. In the public imagination they are often represented as hardworking professionals who seem to be the embodiment of the ‘good’ immigrant (Shukla, 2016: 93). Their image is used to suppress public anxiety that is replete with colonial, stereotypical ideas about the other. This juxtaposition of simultaneous hypervisibility and hyper invisibility (Alcoff, 2005: 8; Al-Saji, 2010: 891) raises the question of how to ‘validly’ interpret and understand archival silences.

Like silences, inconsistencies also necessitate interpretation. Rébecca only found ‘interracialized intimacies’ to emerge in the archive at unexpected moments, without substantial explanation or explicit regulation. In the French archive, inconsistencies around the categorizations of immigrants and mixed couples abound. For example, the activity reports of the so-called ‘Muslim Service’ discuss the problem of ‘Algerian families’ as a problem of immigrants, while another section of the reports show that actually in a third of these families, the wife was French. The same type of report invokes mixed couples as evidence of French tolerance, while the very same report mentions the sexual threat posed by too many Algerians in a single neighbourhood.22

22 These reports can be found at the archives of the Ministry of Social Affairs, at the National Archives in Paris: 1976003314, ‘Synthèse des rapports trimestriels établis par les Conseillers techniques aux affaires musulmanes (1958–1963).’
As Rébecca attempted to recover and decode these references and inconsistencies, she struggled with the question of how the researcher can undertake a complete analysis out of an incoherent archival narrative, without interpreting the narrative from her own standpoint, and thereby giving a ‘biased’ perspective.

Validity as Positionality
Mawani has argued that the interpretation of the archive involves processes of translation, as the reader interprets the documents outside of the political and spatiotemporal context, through present-day concerns (2012: 352). Similarly, Stuart Hall has shown how the archive stands in ‘an active, dialogic, relation to the questions which the present puts to the past’ (2001: 92). The researcher enters the archive not as a blank interpreter, but as an individual with an epistemic and political positionality. Hence, methodologies that are presented as objective and outside of the researcher’s interaction with the archive cannot grasp the dynamics of the creation of meaning that is always ongoing. Doing archival work that aims at critique involves necessarily conceptual and methodological risks (Macharia, 2015). In the archive, we do not follow its grain nor do we go against it; rather, we communicate with the grain of the archive.

In that sense, critical archival work requires expanding the meaning that is validated in the archive, rather than providing a ‘complete’ and ‘unbiased’ analysis. The researcher inserts herself in the circulation that creates meaning by the act of reading and interpreting the archival documents. Accounting for the specific spatiotemporal context and our political and epistemic standpoint is not sufficient, due to the fact that the act of archival research involves a process of interpretation and translation of the knowledge produced through the archives. We need to be aware that the meaning of the archive itself changes as the circulation of the implications of signifiers in the archive are ‘tampered with’ by the reader-scholar.

The call to attend to positionality has had an important impact across the social sciences and many disciplines within the humanities, yet it has not been given as much attention in the fields of history or law. However, as both of us enter the archives from a particular standpoint, we are confronted with understanding our positionality in relation to the archive. In order to do critical archival work, therefore, we believe that attention to positionality is crucial.

Rébecca embarked on her archival visits in an attempt to ‘uncover’ the logics that underpin the archived documents, as she felt the will to know better than the archive. However, she was not prepared for the emotional vicissitudes she felt while rummaging through thousands of papers. The many inconsistencies in the archives made her anxious. She felt conscious of the projections she made, which made her feel too ‘biased’ to interpret the logics that underpinned the inconsistencies. However, her specific agenda of understanding the covert racial signifiers helps her to reveal the workings of racial enunciability.

Nawal’s experiences of entering the archives as a Black woman interested in the lives of other Black women were also emotionally conflicting, albeit for different reasons. The lack of explicit evidence of the existence of these women beyond their recruitment and migration undeniably prompted her to reflect on her own trajectory. She wondered whether she will also be forgotten or erased. The connections she felt through denominators such as ‘race’ and gender force her to question the silences in the archives. Nawal’s reading of the archive becomes inextricably connected to her own history, location, and understanding of the politics of the contemporary moment. She felt pain upon seeing the erasure and denial of the complexity of these lives. As Saidiya Hartman writes, ‘a history of the present strives to illuminate the intimacy of our experience with the lives of the dead’ (2008: 4). Therefore, researching the history of the nurses is personal, since this history ‘engenders’ her, because ‘the knowledge of the other marks her’, as the frustrations experienced in the encounter with the archive push her to continuous self-questioning and self-critique (Hartman, 2008: 4; Aguinaldo, 2004). Yet, this causes Nawal to become aware of the silences, rather than perpetuating them.

Conclusion
Valid research is that which questions power and narratives. Our research expands the boundaries of what is considered to be ‘laws and regulations’, by re-conceptualizing the meaning of ‘valid data’. We argue for the validity of our research because it identifies the gaps, the forgotten and disregarded variables (Burton, 2005: 12). Encounters with the archive thus cannot be neutral, given that researchers endlessly ‘tamper’ with the materials they find. We consider the archive to be a ‘contact zone’ between past and present, between the researcher and the local, national construction of the past, a past that has come into being through power struggles (Sahadeo,
2005: 54). In reconstructing the past, the archive should not serve as a place where evidence is found, but rather as a site of ongoing negotiation. Interpreting information found (or indeed the lack thereof) on specific demographics in the archive then becomes a practice of negotiation between the archive and the researcher. Positionality forces the researcher to see the archive in a distorted way, which has the ability to complement and/or question the existing historical narratives. For researchers who identify with their research demographics due to political and/or personal markers of identification, conducting archival research can be an emotional endeavour since it can force them to reflect on and interpret the archive in relation to their own biographical trajectories. In this way, valid knowledge can then only be knowledge that is situated and that which emphasizes the particular in relation to the universal (Haraway, 1988). Thus, archival research sets in motion a set of processes that select, interpret, negate, produce, and reproduce (Aguinaldo, 2004). These processes are intimately connected to the researchers’ positionality, the history of the archive itself, and the contemporary moment that influences the interpretation of the (lack of) information found in the archive.
References


Limits of Validity: Alienation and the Power to Ignore

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Abstract
Under conditions characterized by widespread unease about the emergence of a ‘post-truth’ world in which the credibility of facts and expertise are under attack, trying to understand how social science can achieve a measure of validity through research has emerged again as a central concern. Perspectives aligned with positivist, social constructionist, and critical epistemologies have tended to approach the issue of validity in terms of how best to establish, open up, or bring to light more credible knowledge claims. This article argues that we need to pay attention to social-material entanglements and processes that seek to ignore social-materially embedded everyday knowledge and establish disentangled claims of validity that tie in with capitalist processes of alienation. To illustrate the argument, I use ethnographic evidence from a North London warehouse belonging to a global retail company to demonstrate how processes of inventory making and the establishing of valid facts in that particular context were indifferent to the particular entangled moments within which those processes unfold and relied on the power and ability to ignore embedded understandings of the world.

What can we learn about validity from a warehouse bin in North London? In a time of heightened anxieties about the emergence of a ‘post-truth’ world in which no validated fact can survive a tweet, where democratic political systems are crumbling under capitalist pressures and populist lies, where the whole scientific project is under threat from flat earthers and climate deniers (Sismondo, 2017), it might seem that talking about validity in relation to warehouse bins represents yet another sociological distraction from what is really important. In addressing these issues, some would argue, surely we need to start by focusing on bigger phenomena, with new transformative techno-methodological developments (Berghel, 2017), widespread government reform and intervention (Suiter, 2016), and a realignment of the political-educational-media landscape (D’Ancona, 2017). Why even bother with a bin in a North London warehouse?

It is tempting to seek system-level understandings of problems that occur on a massive scale. However, by starting from something relatively uncomplicated (in terms of making authoritative and valid knowledge claims), we can begin to get a better sense of the processes that shape our understandings of the world and how they relate to validity. I will argue that by paying attention to this humble warehouse bin, and by noticing how it is embedded in everyday entanglements, we can learn more about how to approach, understand, and deal with validity and the challenges of a post-truth world than we can from approaches that seek to understand and establish new global orders of systematic and reified truths. Entanglements bring attention to how our knowledge, practice, and bodies are caught up in ‘dense material–semiotic networks’ (Law, 2004: 68) and how ‘we are caught up in sets of relations that simultaneously have to do with meanings and with materials’ (ibid). At the same time, noticing entanglements also highlights the importance of paying attention to the consequences of processes that seek to disentangle life-worlds, whether that is in the form of scientific methods that lay claim to objective facts by disentangling complex social and material relations (Law, 2004) or capitalist processes of accumulation that ‘imbue both people and things with alienation, that is, the ability to stand alone, as if the entanglements of living did not matter’ (Tsing, 2015: 5). From the everyday unfolding entanglements engulfing this warehouse bin, I will demonstrate both how knowledge emerged out of the everyday practices centred on the bin, and I will also draw out how capitalist processes create and enforce disentangled and alienating claims to validity (Tsing, 2015).

Understanding validity is not just important in connection with post-truth political and social anxieties. Questions around how to understand and achieve validity are central to social science and connect up with wider concerns about the role that social science and social researchers should play in society (Lather, 2007). It is beyond the scope of this article to examine the full range of diverse approaches to validity in the social sciences, but to situate our understanding of the warehouse bin I will tease out a few general characteristics of particular epistemological approaches to validity within the social sciences and the concerns that motivate them.
Within the social sciences, one loose set of positivist-aligned approaches conceive of validity in terms of how to establish objective, credible, generalizable, and transferable facts and knowledge claims out of the particularities from which they emerge (Cho and Trent, 2006). When it comes to achieving validity, discussions and practices in this area tend to focus on methodological issues – valid findings are treated as the product of valid research methods. Attempts to determine such methods by delineating the strengths and weaknesses of a range of different approaches thus becomes the central concern in ongoing attempts to establish social-scientific validity. Whether it is discussions about the use of triangulation, persistent observation, negative case analysis, recording and coding of data, and how best to ensure strong links between research categories and the phenomena being studied, the goal is to establish normative social-scientific practices that can yield more robust and pure knowledge claims (Erzberger and Prein, 1997; Lather, 2007; Onwuegbuzie and Leech, 2007).

The positivist promise of universally unproblematic and consistent valid truths arrived at by applying the right methodological principles has proven hard to deliver. Fine-grained explorations of how particular scientific knowledge claims gain importance and validity have shown, contrary to positivist credos, that validity is not obtained by merely applying the right methods; rather, it emerges from specific standpoints embedded in social, political, and cultural discourses, practices, and processes and the authority granted to them. For those who draw on constructionist and interpretive approaches (Holstein and Gubrium, 2011), the emphasis has to shift – namely away from decontextualized principles and towards how knowledge claims are shaped, constituted, and made powerful through historically and spatially contingent discourses (Kvale, 1995). On this view, validity is not something that can be achieved by a discerning social scientist applying the right methodology. Instead, validity is treated as one among many variably connected discursive attempts to establish truths that are themselves bound into wider practices that function to separate, bound, and police what can be considered knowledge under particular conditions. As such, the role of the social scientist should be to deconstruct these powerful knowledge claims and open up space for the subversive and playful voices of multiplicity and difference (Scheurich, 1996; Cho and Trent, 2006).

Although these social constructionist approaches to validity have been helpful in unpicking assumptions around the (im)possibility of establishing objective valid knowledge claims, they can end up overlooking the importance of lived, embedded, everyday knowledge treated on the same basis as any other exercise in knowledge and power (Siebers, 2001). To move closer to an understanding of validity, then, it is necessary to pay attention to the entanglements of everyday life and examine the ways in which certain knowledge claims are made valid.

The concept of reification (Chari, 2010) can help when it comes to understanding some of these processes. This is because reification foregrounds the relationship between socially grounded materialities and how capitalist, political, and cultural processes obscure these underlying realities to create purified singular knowledge claims and objects. The notion of reification, with its links to Marx’s analysis of commodity fetishism – whereby commodities take on detached, thing-like properties that conceal how they are produced through labour and social relations – has undergone multiple developments and reinterpretations. These include the work of Lukács who focuses on how commodity fetishism and modern rationality have led to subjects passively accepting the capitalist reification of the world (Lukács, 1971), as well as Honneth’s view that reification is intersubjective and based on forms of collective misrecognition of the conditions of life (Honneth, 2008). Nonetheless, while there are several differences within these approaches (Chari, 2010), when it comes to validity they urge us to reflect on how embedded and entangled knowledge claims are reworked and disentangled from their lived circumstances and how they take on new properties in their purified forms that are re-imposed on the world. Moreover, writing in this tradition illustrates the need to examine the link between claims to validity and the alienating effects they can have once those claims have become separated from and are subsequently imposed back onto the everyday experiences they profess to reflect, remoulding them in the process (Kalekin-Fishman and Langman, 2015; Hall, 2018). Theories of reification thus encourage us to remain attentive to powerful underlying processes within capitalism and the alienating effects that can result from the reification of lives. Where such analyses become problematic, however, is when they claim that these processes are hidden and obfuscated from the people who are affected by them and that it is only through critical methods that valid understandings of the real world can be revealed (Latour, 2004).
The problem is that there are situations in which nothing is hidden and where embedded understandings of the world are actively ignored so as to enable claims to validity to be enforced. To illustrate the kind of situation I am talking about, we need to return to the warehouse bin in North London. The bin I have in mind is not for refuse, as a ‘bin’ in the UK would be outside of the warehouse. Instead, it is a demarcated space on a shelf where items are stored. The bin is one of more than 100,000 equivalent spaces located within a warehouse owned and run by IROHA (pseudonym), an online global retail company. During my ethnographic fieldwork study of precarious work and living in London, I spent months working at IROHA. The job of most people in the warehouse consisted of either taking items away from the bins or putting new items into the bins. But my job, together with around sixty other people in the quality assurance unit, was to count the items in the bins so that the company could validate the real number of items in any given bin.

In terms of living in a complex, intertwined world, establishing how many items there are in a bin might be thought of as one of the simpler tasks that might emerge in such circumstances. For social scientists studying the complex social relations that encompass endlessly interconnected elements, the matter of establishing how many items a given bin contains surely comes nowhere near such levels of complexity. Yet, the very real problem for us whose job it was to quantify the items in the bins was that it was not the sort of thing that could be settled with certainty. On numerous occasions, we were informed by management that we had made an error. Although the bins had the capacity to hold hundreds of items, most of the time there were only a handful of items and sometimes we were told that we had made an error when we reported bins containing just one or even zero items.

The fact that we had made such a mistake was further compounded in light of the elaborate procedures put in place to minimize such errors. A management software system had been developed to keep track of how many items were supposed to be in a bin. This software was then tied in with our scanners, which told us what bin we were to check. After scanning the correct bin, we then counted the number of items and typed the number into the scanner. If the number we had counted was different from what was supposed to be in the bin, we had to count again by taking out all items in the bin and putting them on our trolleys to make sure we had not overlooked anything. If there was still a discrepancy, we would then be asked to confirm the count again before moving on. To double-check our count, another person would then arrive to scan each individual item one by one. If there then continued to be a difference between what we had counted and what the second person (known as the ‘control person’) had counted, we would ‘get an error’.

‘Getting an error’ was not something to be taken lightly, since if you were to get more than a couple of errors per week, you would get in trouble. People were disciplined as a result of errors. They were sent to retraining because of errors. They lost the opportunity for lucrative overtime work because of errors. They were laid off because of errors. Despite the seriousness of errors and the obvious incentives people had for avoiding them, people were regularly told that they had miscounted bins that contained either one or zero items.

There are various ways in which we could attempt to analyse this situation. We could approach it from a positivist standpoint and assume that given all the technological methods and practices implemented by IROHA to ensure a valid count, human error can explain any counting mistakes. The workers had simply miscounted the number of items (twice) or pushed the wrong button on the scanner (twice). This certainly sounds plausible. Getting the count wrong was entirely feasible and when a worker was told that they had miscounted bins with 60–70 items in them, they often had few complaints. However, when different workers were consistently told that they had miscounted bins containing either no items or just a few items, this suggested that something else was going on. This ‘something else’ was not immediately clear; only after having worked there for a while did it become evident that the warehouse was not just rows of neatly ordered bins that could be counted and ordered as was imagined within the systematizing software that guided and evaluated our work.

The first thing that started to pick away at the software’s ordered worlds was the dawning awareness that the bin was not really a bin in and of itself, and that it was inseparable from the intertwined flows that pervaded a warehouse in which nothing was entirely solid or self-contained. That is, the warehouse was constituted by flows of people fluctuating between being hungry, bored, stressed, overworked, apathetic, playful, and always on the move, and items that were not just blank, interchangeable, docile units/entities waiting to be counted, but materials with multiple shapes and weights, endowed with properties of temptation, allure,
and transformability. There were luxury chocolates that over the course of a 12-hour shift screamed out to be tasted. There were wigs that, after counting or picking or stowing thousands of items, invited you to try them out for a quick laugh amidst all the boredom. There were gaps in the shelves through which items would fall out of sight. In other words, the warehouse was a world constantly changing, pushing on, affecting and being affected, connecting and breaking apart, and re-emerging in new shapes. To maintain the perfect separation of bins and items, beyond the particular moment in which they happened to be counted, was impossible. There were countless ways that items could be opened, consumed, pushed on, grabbed, moved, misplaced, and found, and if any of this happened between the first count and the control count – a period in which hours or weeks could go by – it would lead to an error for the person who had made the initial count.

By understanding the everyday flows and entanglements of the warehouse, we can then explain why people continued to count items that, according to the systemized software validations, should not exist in that bin. However, this knowledge was not hidden from or unknown to anyone who had been part of the inventory assurance unit for some time. It was widely known among both the workers and the managers (who often themselves had worked years on the floor before becoming managers) that this was how things worked. The problem was not that people did not have enough grounded and valid knowledge about their immediate circumstances. They knew very well about the arbitrariness of the management software system and the unfairness of the error system. They knew that the real, lived experience of the warehouse was nothing like the software’s pure encoded visions of a world of self-contained and manageable bins and items.

The issue was not a lack of credible knowledge. Getting a better understanding of what was going on would not have solved anything. There were no illusions to dispel, no false consciousness to raise. The workers knew that when they counted zero items in a bin, twice, there were no items in that bin at that particular time and place. It was as valid a fact as anything constituting our world. They knew that trying to disentangle that fact from its embedded context so that it could be processed within the management software system meant that it lost connection with what was really going on. They also knew that the alternative reality of management and software validity was what would ultimately matter, and that any discrepancy would be classified as their error, as their misapprehension of the world.

This process is not unique. Tsing argues that although capitalism is adept at dealing with all kinds of embedded complexities of the world, one of the central premises of the capitalist economy is its ability to disentangle items from the embedded context from which they emerge and to make them into transferable items (Tsing, 2015). For the workers in the warehouse, the enforcement of the error system was also just one of many everyday encounters where their own embedded, lived reality was dismissed as invalid, and where a system view of the world was enforced by either state agencies or capitalist enterprises. Being told that you cannot count to zero was frustrating, but hardly unique. In all the other jobs the workers had experienced, similar processes were part and parcel of the role, and whenever they had to engage with state-capitalist assemblages outside of work – from migration officers to bank officials – their lived experience again meant nothing when set against and assessed relative to documented and ordered systems.

If the establishment of validity is bound up with state-capitalist powers to disentangle, transform, and ignore embedded knowledge, how should we then approach validity? In terms of anxieties about the role of social science in a post-truth world, we could start by paying attention to how people’s embedded everyday knowledge is continuously ignored within the processes that make claims to validity so powerful. In a world where validated knowledge claims about your life are consistently at odds with what you know to be the case, it should not be surprising to find that people start to question other supposedly validated knowledge claims regardless of how many social scientists say that something is true. Indeed, if we turn attention to our practices as social scientists – whether tied in with state, academic, or commercial assemblages – we should ask whether we also rely on our ability to ignore, purify, and move knowledge claims outside of the deeply embedded contexts in which they develop. If we do so, then it is an open question as to why our claims to knowledge and validity should be treated as less problematic than the enforced validity claims that emerge in state-capitalist assemblages. I do not pretend to have any easy response to such questions, but if we want to get closer to properly addressing the most troubling questions about the role of validity, then reflecting on our relations with some of the simple complexities of the world (such as a warehouse bin) may not be a bad place to start.
References


Valid Proof: Understanding the Politics of Public-Key Cryptography
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Abstract
This paper considers the political implications of the public-key cryptosystems which secure communication over popular messaging programs such as WhatsApp, and underwrite the blockchain infrastructures of cryptocurrencies like Bitcoin. While the distribution of privacy and security through access to cryptography has been a salient topic in academic and public discourse, less has been written about cryptographic mechanisms themselves. Public-key cryptography makes use of a mathematical concept called the one-way function. One-way functions are assumed to be easy to compute in one direction, but difficult to reverse. However, one-way functions have an unusual relationship to validity: they rely on conjectures about computing which have not been proven. This property is considered in relationship to Michel Foucault’s concept of the unthought to suggest that it reflects the condition of the modern episteme. This takes on a political dimension in the uncertain conditions of cryptographic design, where national security interests often lead to the restriction of research. Furthermore, cryptographers must design systems which are resistant to both external adversaries as well as malicious users. Cryptosystems must therefore be designed against the users who they nominally protect. The confluence of epistemic uncertainty and practical insecurity makes distrust endemic in cryptographic research. This outlook enacts a logic of suspicion at the level of its design; as applications based on cryptography attain wider circulation, this will have increasing social effects.

The politics of cryptography is typically framed as a debate about the extent to which personal privacy must be balanced with collective security. By encrypting a message, one can protect themselves against surveillance; however, encrypted messages may not be accessible to law enforcement apparatuses even when valid search warrants are issued. As a result, access to encryption technologies is framed as a zero-sum game: there can be either security or privacy, but not both. Although encryption has been a salient topic for academic and public discourse, efforts to legislate cryptosystem design reveal that this is an equally relevant issue. In the United Kingdom, for instance, section 253 of the 2015 Investigatory Powers Act allows for the government to issue notices which compel service providers to redesign their encryption protocols to comply with surveillance warrants (Open Technology Institute, 2017: 5). The political importance of these mechanisms will only grow as variations of public-key cryptography become increasingly commonplace on popular messaging programs such as WhatsApp, in evidentiary mechanisms like digital signatures, or cryptocurrencies such as Bitcoin. This paper departs from the privacy-security framework to analyse how the mechanisms of contemporary cryptography reflect the political practices of the security apparatuses which make use of encryption technologies. There is a parallel effort in both projects to manage the risk of uncertainty. Highlighting this connection explains cryptography’s relevance for security politics and provides a basis for analysing emergent applications of encryption processes.

The Mechanisms of Public-Key Cryptography
Contemporary cryptography uses a public-key encryption scheme with pairs of keys – one private and one public – to allow users to communicate securely. Although anyone may use a public-key to encrypt a message, the message can only be decrypted by reversing this process using the information contained in the private-key. There is therefore no need to distribute shared keys over secure channels as with earlier symmetric methods. The two keys are linked by mathematical processes which are easy to compute, but infeasible to reverse; it is therefore exceedingly difficult to find the private-key based on the public one. These mechanisms, called one-way functions, were brought to wider attention in 1976 by Whitfield Diffie and Martin Hellman in their germinal paper, ‘New Directions in Cryptography’. Responding to the need for both privacy and authentication in communication over open-channels, such as radio waves, where transmissions can be easily intercepted or falsely planted, Diffie and Hellman (1976) propose using one-way problems to send messages securely even if the data are transmitted over public channels.
Although Diffie and Hellman use a code compiler as an illustrative example of a one-way function, their paper does not offer a practical scheme for public-key cryptography. The first openly-published public-key cryptosystem would be developed by American researchers Ron Rivest, Adi Shamir, and Leonard Adelman two years later in 1978. As befits the secretive nature of the discipline, it was later revealed that British researchers employed at the Government Communications Headquarters (GCHQ) developed the very same scheme in 1973 (Levy, 1999). The method itself is both devilishly simple and fiendishly difficult. The Americans suggest that multiplying two large prime numbers (at least 100-digits long) can act as a one-way function. Because it is trivial to perform the multiplication – it could be done with pen and paper – but exceedingly difficult to find the factors of the resulting product without knowing them in advance, the product can be published as a public-key and the primes can be used as a private-key (Rivest, Shamir and Adelman, 1978). This process (now referred to by its inventors’ initials, RSA) became a cryptographic standard and remains in use.

The importance of one-way functions for modern cryptography cannot be overstated. In addition to providing secret communication, one-way functions are also used to authenticate transmissions. For these digital signature schemes, a signer uses their private-key to generate information that can be verified though use of their public-key; this validates their message because it would be infeasible to replicate the process and forge the message without knowing the private-key (Sako, 2011). One-way functions are also used for pseudorandom number generators which increase cryptographic strength by preventing predictable key choices (Ha Stad et al., 1999). Finally, they can be used as hash functions which output a string of bits of a fixed size from an input string of an arbitrary length (Naor and Yung, 1989: 34). Hashing a message compresses it and thereby reduces the computing burden required to encrypt it. In sum, the one-way function is both the theoretical basis of modern cryptography and critically important for its practical implementation.

The Validity of the One-way Function
While cryptosystems ostensibly offer a demonstrable means for security, an examination of the principles upon which they are based reveals that this is not so clearly the case. Mathematically speaking, one-way functions are operations which are thought to have asymmetrical computational demands: they are easy to perform in one direction, but are infeasible – meaning they require impractical computational effort – to invert (Robshaw, 2011). These form the basis of many cryptographic primitives from which more complicated cryptosystems are constructed (Kaliniski, 1999: 92). If the difficult element of a one-way function (called a hard-core predicate) can be identified, then it may be used to design cryptosystems which are considered more provably secure (Goldreich and Levin, 1989). Finding and discovering one-way functions remains an important research focus in cryptography. In spite of this, one-way functions are not proven. Although RSA, for example, has been the subject of intense research to define best practice, its strength is the absence of a known algorithm for factoring large composite numbers (Boyter and Moore, 1984: 182). One-way functions exist as unverifiable conjectures and therefore exemplify the search for difficult or intractable problems, which, according to Claude Shannon, the godfather of information theory and pioneer of cryptography, distinguishes the field (Shannon, 1949: 704).

This has complicated the notion of proof in the discipline of cryptology. In mathematics, a proof is traditionally a rigorous argument that can be verified by a human and deductively validates a logical relationship (Krantz, 2010: 242). Within computing, however, proof means that the theoretical model of a computer system aligns with its enacted design, regardless of whether the underlying assumptions are true or if the design is adequate (MacKenzie, 1995: 48). This is verified mechanically by automated theorem-testing programs that determine whether the propositions are valid under the specified conditions; however, these programs themselves have not been entirely formally verified (MacKenzie, 1995: 51). Because of these limitations, proof becomes a problem insofar as it increases the dependability of a system through automatic mathematical investigation to manage the risk that there may be unknown complications (MacKenzie, 2004: 8). As there is always some degree of doubt, proof is never absolute or exhaustive; rather, it is probabilistic. Validity is constructed through successive but limited verifications that diminish uncertainty. The development of provable security in cryptography similarly relies on mechanical proof. Rigid step-wise protocols have been developed to standardize the requirements of cryptographic design as a guarantee of security (Sprenger and Basin, 2010). But because one-way functions are unproven, their reliability can only be tested indirectly, which requires making further assumptions (Dent, 2006). The result is called a refinement paradox: attempts to develop demonstrably secure systems cannot evaluate that quality directly (Alur, Černý and Zdancewic, 2006). Consequently, although mechanical proof enhances the validity of existing systems, it also compounds the underlying problem by adding propositions. As a result,
cryptographers are in the unenviable position of designing secure systems from indemonstrable propositions; the possibility of errors can be mitigated but not entirely eliminated.

In public-key cryptography, validity is not only a problem of managing uncertainty to ensure that a system works. Counterintuitively, it is the limitations of computing and the unknown properties of the one-way function that make cryptography possible. Indeed, if research showed that one-way functions were reversible, they would no longer be useful. Mathematician and computer theorist Oded Goldreich reveals how this presents a fundamental challenge for secure design. His treatise, Foundations of Cryptography, is unique in cryptographic discourse because it eschews the development of new systems in favour of formalizing the core principles of cryptography. The basic problem is that while ‘one-way functions are the very minimum needed for doing most sorts of cryptography […] our current state of understanding of efficient computation does not allow us to prove that one-way functions exist’ (Goldreich, 2001: xiv). The contradiction at the heart of cryptography, then, is that the inability to resolve whether one-way functions are directional is both the condition and limitation of their usefulness.

This is not an idle concern because uncertainties are vulnerabilities. The safest assumption in designing a cryptosystem is to assume that an adversary has effectively unlimited capabilities (Goldreich, 2001: 22). In a separate meditation on the relationship between proof and cryptography, Goldreich stresses the need to make minimal assumptions about the validity of one-way function to ensure that analyses are sufficiently rigorous. He summarizes the dilemma which the discipline faces, writing: ‘intuition invoked in time of need (as any feeling that arises in time of distress) is to be suspected, certainly not trusted. For sure, such intuition can serve as a basis for knowledge, and things are even worse in cryptography’ because there is always a presumption of adversarial behaviour, and therefore cause for concern (Goldreich, 2006: 12). Designing cryptography requires being suspicious of knowledge because the lack of definitive deductive proof may be exploited in an attack. Goldreich thus reveals the confluence between epistemic validity and political security in cryptography: uncertainty is tantamount to risk.

Proof as an Epistemic Problem

The paradoxical status of the one-way function in cryptography can be more fully understood if its validity is seen as part of a larger epistemic problem. Indeed, one-way functions have not always been cryptographic: in their earlier formulation by William Stanley Jevons, they are treated as a broader class of logical problems whose irreversibility is of general importance (Jevons, 1877: 121–196). Accordingly, the one-way function intersects with the broader question of how valid knowledge develops in the face of the unprovable. In an essay examining the bases of modern science, Martin Heidegger shows that the limit of thought is also the condition of knowledge. This is exemplified by mathematics. Returning to the term’s Greek roots, mathēsis (learning) and mathēmata (what is learnable), Heidegger suggests that mathematics is ‘coming to know actually and to the very foundations what we already know’ (2008: 275). Mathematics is thus a practice of self-investigation. This reflexive practice gives birth to experimentation in modern science which establishes axioms as the standard of knowledge. Although this liberates thought by making everything subject to evaluation, it also imposes ‘a binding with obligations that are self-imposed’ (Heidegger, 2008: 291). In mathematics, ‘thinking thinks itself’ by both questioning and enacting limits (Heidegger, 2008: 302). Heidegger’s characterization of mathematics continues in the discipline of cryptography which must perpetually re-examine its propositions according to the programs of mechanical proof.

Michel Foucault’s archaeology of modern thought, The Order of Things, connects the concept of the limit with the unknown. For Foucault, mathēsis in Classical thought refers to a qualitative science of order that seeks to establish a continuity of knowledge (Foucault, 1994: 56). However, mathēsis also articulates a transition toward the modern episteme because it makes possible empirical disciplines which do not conform to an a priori order, for example a divine design, but instead refer only to their internal cohesion (Foucault, 1994: 243). This restructures knowledge: as mathēsis becomes dissociated from a universal order, there is no longer a given coherence between objects and their representations. Instead, all knowledge is made in relation to the thinking subject who, Foucault argues, occupies an ‘ambiguous position as an object of knowledge and as a subject that knows’ (1994: 312). The result is that empirical knowledge is always incomplete because it is ‘a fundamental finitude which rests on nothing but its own existence as fact’ (Foucault, 1994: 315). This analytic of finitude is not a stable arrangement because the thinking subject is always aware of the finitude of their thought. There is an obligation to unceasingly question knowledge so as to affirm its validity. Modern thought must continually confront its limit – the impossibility of absolute certainty – to retain its concrete form. Consequently, the modern cogito encounters what Foucault terms the unthought: ‘an irreducible, an insuperable exteriority’ which exceeds it but is nevertheless integral.
to its thinking (1994: 324). The unthought is neither the opposite of thought nor its negation. It is, rather, the indispensable condition of the thinking subject’s relentless reflexivity and that which it can never overcome. Foucault’s analysis reveals that the central dilemma of cryptography is also that of the modern episteme. One-way functions’ resistance to deductive proof is not an aberration, but a manifestation of how thought encounters its finitude in the form of an unthinkable but necessary exterior. Contemporary public-key cryptography can be characterized as what Foucault calls a science of the unconscious, which is directed not ‘toward that which is below consciousness’, but instead ‘towards that which, outside of man, makes it possible to know with a positive knowledge, that which is given to or eludes his consciousness’ (Foucault, 1994: 378).

In this regard, public-key cryptography constitutes an effort to generate stability from conditions of epistemic uncertainty. The unprovable nature of one-way functions does not foreclose the possibility of security, but instead demands a set of epistemic practices, for example the use of mechanical proofs, which work within the finitude of computing knowledge. Out of this indeterminacy, cryptographers produce standards from which theoretical apparatuses and practical applications arise. There is, however, no possibility of certain proof on account of the underlying epistemic uncertainty. As a result, all cryptographic developments are by necessity subject to persistent suspicion which indefinitely endeavours to secure itself.

**Cryptography as a Political Technology**

The goal of analysing the disparity between valid mathematical proof and working cryptographic design is neither to reveal a disciplinary deficiency nor to resolve the discrepancy between the two. Rather, triangulating the gap between the theory and practice of public-key cryptography reveals that the question of security is not confined to how encryption technologies are used, but also linked to their epistemic mechanisms. If these functions are seen as inherently political rather than neutral, then it is possible to understand why these technologies are important for practices of security without recourse to familiar and reductive narratives about the rise of electronic communication in a networked society. Encryption technologies make possible secret communication. These are political relations because, as Georg Simmel argues, secrets must be ‘to a certain extent recognized by the other’ and as such serve to distinguish between members of a group and outsiders (1906: 462). As part of digital communication, cryptography forms part of what Alex Galloway terms protocol. Protocols, for instance the TCP/IP standards through which the Internet operates, enable flexible connection between many networked nodes, but only under the absolute and rigidly coded rules (Galloway, 2006: 8). As a result, protocols are political because they modulate and manage the flow of information according to their programming; they govern relationships by creating the occasion for action within the confines of encoded possibilities.

Cryptography is therefore a means of managing political relations. Technology theorist Jean-François Blanchette affirms this is the case. In one of the few political considerations of public-key cryptography, he observes that ‘cryptography is code created with the sole purpose of regulating behavior. Cryptographic technologies are specifically designed to provide confidentiality, authentication, anonymity, accountability – in short to implement the locks and keys of cyberspace architecture’ (Blanchette, 2012: 95). Encryption technologies do not replace existing institutions but become integrated with them. For instance, digital signatures gain evidentiary value through their association with state bureaucracies and actors like notaries public (Blanchette, 2012: 156). However, cryptography also reflects an overriding desire to avoid relying on any trusted third party by implementing systems which offer security on the basis of mathematical proof alone (Blanchette, 2012: 40). Public-key cryptography is seen as a way of validating information even when those who provide it cannot be trusted. For example, encryption technologies guarantee security independently from the promises of national security apparatuses, which may try to control or censor them (Blanchette, 2012: 38). They must also be resistant to tampering from users who might try to falsify information (Blanchette, 2012: 80).

Public-key cryptography must therefore be designed against attackers as well as against abuse by those who it nominally protects. As a result, efforts to mitigate uncertainty at the level of computing are paralleled by political practices which work to reduce risk by treating any user as an adversary who might exploit a weakness. As Foucault observes in a lecture, ‘the primary, imparable law of both modern governmental and historical science [is] that man henceforth has to live in an indefinite time’ (2007: 356). Political theorist Michael Dillon extends this convergence, arguing that modern security constitutes a political analytic of finitude because it perpetually governs lives that will necessarily end (2011: 782). In the absence of an extra-historical standard by which to judge political decisions or which promises a coming salvation, ‘finite regimes of government and rule have indefinitely to secure themselves against the realization of their very own finitude’ (Dillon, 2015: 37). More than that, everything which politics seeks to secure is ‘dangerous because, in its very know-ability, which is to say its governability, everything also proliferates ungovernably’ as all knowledge is
implicated with the unthought which by definition cannot be fully contained (Dillon, 2015: 39). The very task of politics in an analytic of finitude is managing uncertainty and endlessly securing the validity of that venture. Thus the epistemic mistrust of mathematical proof is doubled as a political maxim in which nothing can be fully assured and therefore everything must be securitized anew. Public-key cryptography is of contemporary importance because it resonates with this political project. Although encryption technologies are deployed as a response to endemic suspicion and pervasive risk, these mechanisms of public-key cryptography reproduce a logic of security which enacts the same mistrust it is meant to overcome.
References


Introduction
When I was about to start my long-term ethnographic fieldwork on human-environmental relations, my research area was shaken by numerous attacks ascribed to an Islamist militant group. While the area has a longstanding history of ethno-political conflicts around elections between farmers and pastoralists, such a form of violence, which consisted of attacks on vehicles and villages across the area and in the course of ten incidents left dead around a dozen locals, travellers, and security agents, was unprecedented.

Making sense of this kind of danger that was both absent and present, as the militants were neither rooted in the communities nor following known patterns, and relating it to one’s individual positionality and experience proved difficult for all those affected. In consequence, ‘truth’, rather than being stable and shared, was always situational, partial, and relational.

In ethnographic fieldwork, validating is a continuous practice not to be separated from data production and left for analysis. To proceed in research, one is constantly and often tacitly testing and making decisions about ‘reality’, largely by searching for alignment, overlap, and commonality with others. And so it is especially in moments of intersubjectivity that we feel deeply and uncontradictedly connected to and knowledgeable about the world.

In this light, the present contribution seeks to examine a form of validation that might precede processes of validation in other scientific practices but is central to ethnography. In describing the attempt to make fieldwork succeed against the odds, I will trace my navigation through different (self-)perceptions of danger and my use of different methods in the search for validity and control. This endeavour turned into an epistemic and emotional borderwalk, raising questions about the limitations of ethnographic validation in personally demanding research settings.23

Front-line Entanglement
On a cloudy afternoon, my friend and assistant Jonas picked me up in Gatetown, and we took one of the last minibuses to Haventown, which with around 3,000 inhabitants was the biggest town in my research area. Traffic was now structured around the dusk-to-dawn curfew that had been put in place in response to the attacks. Shortly after Gatetown, we reached the first checkpoint. Whereas on the way back, we would have our bags searched, here only an identification document was needed. There were fewer restrictions entering the danger zone than leaving it.

Upon my arrival in Haventown, I was told that the curfew would surely be lifted on the proclaimed date, that ‘they cannot strike here’, and that the attacks occurred ‘down there’ or ‘not in this county but in the other county’ (the county border passes right through my research area). From governmental officials, security forces, and local media outlets, I first faced silence and placation. And from the loudspeakers at the celebration of International Peace Day, I heard the words ‘peace means business, peace means development, peace means tourism’, accompanied by admonitions to respect each other despite ethnic or religious differences. Not a single public statement about the ongoing attacks.

However, in other, less public spheres of discourse and practice, it soon turned out that the terror was indeed present. Moreover, the way people engaged with it revealed ambivalence and contradictions. And so I was advised to lodge only in the middle of the town and to stay indoors when it was dark, while at the same time I was reassured of my safety. I was told not to type notes into my phone because it would make people suspicious that I was working with the security forces. I witnessed how prices rose because trade had slowed down, or how people hesitated to travel. Or, in the case of some relatively high-ranking officials whom I talked to in private, I was first asked, ‘Have you seen someone spraying bullets with your own eyes?’ Moments later, again, I was being told that they would and could not take any responsibility for me.

23 The names of people, places, and the country in which my research was undertaken have been changed or intentionally left unspecified. The piece takes its title from Robben and Nordstrom’s edited volume Fieldwork Under Fire: Contemporary Studies of Violence and Survival (1995). I would like to thank Jonas, my interlocutors, and my colleagues.
I soon also encountered various rumours that there was ‘more’ behind the attacks. These ranged from notions of weak and unorganized security forces clashing over smuggling and competition for funding and resources within their own ranks, to suspicions about the involvement of ethnically affiliated groups, and/or local and non-local bigwigs such as politicians, businessmen, security officers or elders that wanted to ‘scare away farmers’ in order to access their land. The most prominent narrative was that ‘it would and should be easy to get rid of the militants, so there must be more to it’.

These narratives could be understood as attempts to assign meaning to apparently meaningless incidents, to create a more stable reality via the discursive (see Oldenburg, 2010). They appear also to have been informed by the lack of trust or even the fear that the people had towards parts of the state apparatus, or their feelings of negligence and vulnerability based on a longstanding experience of political marginalization, top-down (infrastructure) projects, arbitrary police and military actions, and the prevailing insecurity around land tenure and resource extraction. Furthermore, the experience of the displacement of communities that lived in the nearby forest and were flushed out by a military intervention targeting the militants’ hideouts, and the suffering of the communities that had been attacked, reminded people of former clashes between pastoralists and farmers. In these clashes, villages were attacked, people killed, and others displaced. Thus, in a setting of mistrust between the people and the state, former experiences of violence were activated and met with new experiences of a different type of violence. This led me to the impression that people (still) had to navigate between denial, horror, and the quest to ‘routinize’ violence in spite of being ‘routinized by violence’ (Oldenburg, 2010). Similar to my colleagues, supervisors or the embassies and foreign ministries, the officials’ and civilians’ response-ability was limited and the town appeared to be caught in a sort of tripartite liminal stage between the containment or repression, the emergency, and the banality of danger.

Such ambiguous, conflicting, and emergent thoughts and feelings about or towards violence, the individuality and positionality of the experience of violence, and its complexity and multifariousness across different scales mean that violence is a ‘layered’ phenomenon that can be seen through various analytical lenses – for example, one could focus on the political or the psychological aspects of violence (see Oldenburg, 2013; Robben and Nordstrom, 1995). In moving towards Haventown, however, I had arrived at the ‘front line’ (Robben and Nordstrom, 1995). This meant getting entangled in the ‘front line’s’ visceral situatedness and to approach other, more distanced ways of looking at the violence from there.

In difficult research settings, different methods influence, (in)validate, reform, and bring out one another (Diphoorn, 2012). Furthermore, they serve as ‘defence mechanisms’ that promise a degree of steadiness and stability amid disorientating conditions (Jackson, 2010). Confronting a difficult setting via methods with some level of rationality can be comforting (Nilan, 2002), yet methods might not (only) create rationality, but also evoke what is commonly perceived as its opposite, namely emotions (Davies, 2010). The consequence is a specific, entangled, and ultimately murky epistemology where one rapidly shifts between different states of thinking, feeling, and acting. For example, I sometimes caught myself measuring distances on maps and calculating the time that the militants would need to cross the area from point X to Y. This then led me to engage with the development of the security governance of the state over time. When I became worried about the inactivity of the state, I again shifted towards the ‘front-line’ narratives around our personal safety, and the logic, patterns, and aims behind the attacks. Hence oscillating between the different realms of violence based on my ‘front-line’ situatedness was an everyday, inevitable practice, which reflected and again (re-)informed my methodology and sense of self.

**Do You See Something That I Cannot See?**

Doing ethnography means to meander and to strike a balance between detachment and immersion; proceeding in a more directed way and collecting as much as possible (Nilan, 2002). In my situation, there was, on the one hand, a strong underlying impetus to evaluate the danger and engage with the affected people with the aim to search for validity, to establish a certain truth about the situation and a basis for my decision to stay or to leave. Yet, on the other hand, I had to rely heavily on everyday life, and on the ways danger and violence surfaced in or were strikingly absent from day-to-day practices. This also related to the specific quality of the violence.

In attempting to understand violence, Giphoorn (2012) proclaims, one has to be a participant in its manifestations. In my fieldsite, there was, however, hardly any experience of violence itself; it existed more as a distinct possibility or omnipresent ambient, background, or anonymous danger (Lee, 1995; Yancey and...
Rainwater, 1970). This danger was ‘already there’ as opposed to situational or presentational danger – a danger caused by the researcher’s activities (Lee, 1995; Yancey and Rainwater, 1970). As such, my capacity to reduce the (situational) danger, to assess and validate my own actions and their consequences was very limited. However, even though there was no physical manifestation of violence for the majority of people (there were indeed some people who survived an attack and reported on it), the anxiety about and the fear of violence were socially relevant and formative.24 I, too, came to be gripped by it, yet in order to assess it, I was not able to draw on a certain, commonly acknowledged object ‘out there’. My own perceptions, interpretation, and emotions and those of my interlocutors remained ambivalent and were constantly shifting. But in the absence of a clear object ‘out there’, these mutable phenomena became the only entry point for me to gain deeper insights into how safe we really were. And so I tried to relate and align my perceptions, interpretation, and emotions to those of others.

Fearing Together, Fearing Alone

The relations and alignments to and with others, if possible at all, were always fragile and fleeting, and they also depended on the ways in which we experienced and communicated them. Anxiety, latent and murky, continuously remained in the shadow of my consciousness as well as in my body, for example in the form of palpitations, insomnia, or the feeling of being perpetually restless. These sensations were not so easily assessable by myself, and difficult to relate to others. And so it was only in retrospect that I came to conclude how I sometimes resonated with and therefore also learned about the restlessness in others; for example when an interlocutor did not want to stroll about the village while talking, as often experienced on my previous visit, but instead steered determinately from one place to another. In the realm of the discursive again, I noticed ‘irregularities’ in the form of unfinished sentences, hesitant answers, long pauses or contradictory statements. In the moment, I was rarely able to pinpoint or verbally address them, but I experienced both irritation and tacit empathy. Only later, having shifted my attention in the process of writing up my notes (and even more so in revisiting them later on) was I able to relate those ‘irregularities’ to my interlocutor’s anxieties, and reflect on them. The basis to do so was thereby my own experience of anxiety – assembling, among others, hesitancy, flightiness, otherness, and ambivalence – which in situ had been hard for me to grasp (see also Hage, 2009). Conversely, reflecting on the anxiety of others also allowed me to reflect on my own anxiety.

In this case, it was thus ex-post reflections that served as stimuli and led to a cognitivization, assessment, and validation of my preceding experience. This experience had also been possible because it did not need to be pinpointed; because our differences could remain open. Such a tacit understanding beyond words, which might rather divide and distinguish (Jackson, 1989), was enabled by way of a certain non-predicatedness or unconsciousness, immersion and bodylines. That said, some doubts remained: ‘Did I interpret those experiences correctly? Did my interlocutors and I really experience a tacit communality?’

In more predicated communications, which were also revolving around fear rather than anxiety, such doubts were easier to confront, yet the outspoken communications often served to also painfully reaffirm the differences between people as well as the inexplicability of the situation. One such example was the question of how militants target their victims. It turned out that different people had very different perceptions of the extent to which they or others were at risk. Some felt quite safe as members of an international first aid NGO; some felt that Christians would be more at risk than Muslims; others claimed that the militants did not distinguish much between beliefs; and I voiced my concerns about being an especially attractive kidnapping victim. All of our own positionalities entailed the possibility of situational danger. Yet, they were more part of who we were than what we did, and as such they appeared far from manageable. And so these discussions on the risks of being targeted highlighted again and again the limitations of fearing together, to speak about fear, to protect each other, and to establish a shared, stable perception of and a discourse on the danger and our emotions (see also Lutz and Abu-Lughod, 1990).

In addition, however, there were some instances when fearing was something shared. For example, Jonas and I would often indulge in reminiscences about our formerly widespread motorbike trips through the area, lauding how safe we used to feel. Sharing the impression that we could no longer embark on the same travels because it had become too dangerous, we were thinking back, and this reassessment of our experiences resulted in a shared imaginary of what would hopefully be once again. Our shared past experiences and our common imagination of a future also allowed us to experience fearing together.

Drawing on these examples, I would argue that the experience of fear and anxiety, even in the absence of

24 Both fear and anxiety are enacted and experienced with and through our mindful body as something we simultaneously experience and do, which includes the body, the social, material, and semiotic (Scheper-Hughes and Lock, 1987; Scheer, 2012). Yet, a (gradual) differentiation between them appears useful for inquiring into attention and habitualization, as well as intersubjectivity. In this light, I take fear as being something more directed towards an immediate and cognitively classifiable threat, and anxiety as being something that relates more to an apprehensive or anticipated, vague and often conflictual or ambiguous threat, which can have an insidious character (Sadock, Sadock and Ruiz, 2014; Sylvers, Lilienfield and LaPrairie, 2011; Blanchard et al., 2008).
physical violence must not but indeed can be shared – in which case it fosters intersubjectivity. Intersubjectivity has been described as a shared judgement of aspects of the world between two or more persons, or as overlapping perspectives on the life-world that entail the possibility of trading places, which emerges through the simultaneity of experience, for example when actors participate in the same practice (Kesselring, 2015; Förster, 2011). Intersubjectivity can therefore also mean to establish moments of a commonly accepted reality and to test and (in)validate one’s own perspective so as to align it with those of others. Over time, intersubjectivity can lead to sedimentation, routinization, and habitualization – which are themselves processes of testing, (in)validating, and then incorporating or spacing out certain aspects of life for oneself.

Consequently, when we fear together over time, we can go through both a change in our personal and interpersonal experiences (i.e. we can experience, deepen, and prolong intersubjectivity) and a change in the fearing itself (i.e. sedimentation, routinization, habitualization, and loss of attentiveness). Thereby, fear can morph into something chronic, or anxiety.

Some authors have stressed the ‘volatility’ of danger and violence while at the same time problematizing the fieldworker’s ability to become accustomed to them. Green (1994; 1999: 59), for example, notes that ‘subjectively, the mundane experience of chronic fear wears down one’s sensibility to it’ and that she and her interlocutors were ‘swinging wildly between controlled hysteria and tacit acquiescence’ (these words are reminiscent of Taussig, 1992). Oldenburg (2013) recalls how her interlocutors’ contradictory practices, relativizing language, and ironic comments signified and constituted ‘a normal state of emergency’, and how she started to imitate their behaviour in order to tame the challenging conditions of her research. And Hage (2009) states that the more deeply we are immersed in the ‘emotional borderline’, the more (emotional) effort it takes to remove ourselves from participation and shift to the observational/analytical plane.

My attempt to make fieldwork work largely foregoes what Green (1994; 1995) or Oldenburg (2013) were experiencing over time. I was still in a mode of searching for control, testing and validating different relations and positions. As I encountered an array of perceptions and opinions, I also (still) experienced rapid and severe shifts between different emotions, thoughts, and actions. The choices, opportunities, and offers both between others and myself, and within myself, were manifold. Moments of interpersonal and personal coherence as well as fragmentation were always ephemeral. And intersubjectivity remained forever fragile and was continually ruptured. Rather than a regular pendular movement between different states of being that would be comparable to the ethnographer’s ‘swinging’, it thus appeared as if I had (still) been caught up in an individual, volatile, arrhythmic, and shapeshifting state of being in itself.25

Coda

In ethnographic fieldwork, there are always many situated uncertainties and unpredictable variables. These are also relevant because the self, with all its senses, is part and parcel of the scientific endeavour, and while the process of trial and error is inevitable its effects can be extremely far-reaching, irreversible, and non-reproducible. In fieldwork under the possibility of fire, one might have just a single chance. Consequently, all decisions and actions take on a heightened relevance and amplitude, all the while remaining partial and somewhat suspicious. Furthermore, all experiences can potentially be linked to the danger. One keeps an eye out for danger, or, like a black hole, feels constantly pulled towards it. Even more, this black hole might appear more as a sort of phantasm than as a clear object. The difficulties in identifying, ordering, and communicating about and making sense of danger might thus even lead one to doubt one’s own perception of reality (Green, 1999).

When the danger is not so immediate and objectively present, but is rather present-absent, bound to unpredictable actions by unknown actors, different people are and/or feel differentially targeted or exposed, largely independently of their capabilities or their doings and non-doings. Narratives, explanations, and strategies diversify, while categories such as Muslims, Christians, People of Colour, Whites, Women, Men, Pastoralists, Farmers, Anthropologists, Interlocutors, Assistants, Civilians, NGO workers, or Military personnel become reinforced, all of which contributes to the fragmentation of sociality. This fragmentation, in turn, further hampers the response-ability and the establishment of common perceptions and interpretations of, or strategies towards, danger.26

26 In contexts of a less present-absent threat, common mitigation strategies alongside interlocutors or direct negotiations with perpetrators can indeed be possible (Rodgers, 2007; Oldenburg, 2010; 2013; Dolnik, 2011).
In such a situation, sedimentation, habitualization, and routinization can be key (Blanchard et al., 2008). On the one hand, shared extreme experiences in the past might (still) function as a future-oriented bond and as a useful form of guidance (Geertz, 1983). On the other hand, sedimentation, habitualization, and routinization can further be expanded when a past that somehow resounds in the present becomes activated, and when emotions take their cue from what has been (Beatty, 2010). Yet, those who do not share this past experience, or who are not completely involved in re-evoking, re-(in)validating, and re-formulating it, might also not be able to fully relate to it (Beatty, 2010). And those again who unsuccessfully try to find, validate, and stabilize a ‘truth’ on the one hand, but do not need to stay in place on the other hand, might end up leaving – such as me, the anthropologist with European citizenship. Hence, especially when positionalities are diverse and the danger and violence remain present-absent, it is not the case that one stable ‘truth’ is established, but rather an array of emergent assemblages of possible ‘truths’ that allow for (shared) moments of living and moving on or might also drive some away.
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2. Features
Connecting with Emotions: Exploring the Arts and Valid Research with Displaced Communities in Colombia

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Before starting my PhD in the autumn of 2017, I followed an impulse to leave my job in London and take some time off to travel. Having a soft spot for mountains, I was drawn to spend a month cycle touring in the truly breath-taking landscapes of Colombia’s tropical Andes. Yet my experience in Colombia was in many ways shaped by a deeply moving visit to the Museo Casa de la Memoria in Medellin during my first few days there. At that time, it was less than a year since the signing of a peace agreement between the government and the Revolutionary Armed Forces of Colombia (FARC). The museum starkly confronts visitors with the immense toll five decades of civil war has taken. Its exhibits include harrowing first-hand accounts of people’s experiences living amid physical and sexual violence, with the disappearance or murder of relatives, and forced recruitment into armed groups. Many fled their homes to escape the violence and seek security for themselves and their families. There is now thought to be some eight million people internally displaced in the country, which makes Colombia second only to Syria in the size of its displaced population.

When I returned to the UK to start the PhD, I found that some of my new colleagues had been working with displaced people in Colombia. The topic caught my attention, yet it was their innovative research approach – blending the creative arts with more conventional social science methods – that most interested me. Music, theatre, dance, and painting were at the centre of the research team’s engagement with these marginalized communities. The project makes for a thought-provoking example of how novel approaches and methodologies are being used that reconsider the definition of validity in social research. For this feature piece, I will explore this by considering their approach in greater detail; speaking with two members of the research team, Teresa Armijos and Viviana Loaiza, on how the creative arts, emotions, and ethics interweave in their work.

The research project’s title, ‘Moving with Risk’, reflects a focus on how forced displacement due to conflict has often brought people to be exposed to other forms of livelihood risks. While Colombia’s tropical, mountainous geography is spectacular, it is also notorious for natural hazards such as floods, landslides, and volcanic eruptions. The Armero tragedy in 1985 stands out as a reminder of this. Over 20,000 people died when a volcanic eruption triggered a vast torrent of mud to cascade down its slopes, devastating the settlements in the valley below. With little real choice in the process, those displaced by the civil war have often rebuilt their lives in areas where they are exposed to these kinds of natural hazards. Their resettlement has effectively meant exchanging one form of catastrophic risk for another.

The research team wanted to better understand displaced people’s life trajectories coping with these different forms of risks. The creative arts were used in different ways to explore risk trajectories as both individuals and communities. Individual stories were first elicited, using music as a vehicle for this process. Participants were asked to pick a song to share at the beginning of the interview, and their choice of song did not have to be relevant to any particular theme. It could simply be one they liked, or which resonated with them in some way and that they wanted to share. Reflecting on the music – what thoughts, memories, and emotions it evoked – was a way of opening up personal narratives of displacement and resettlement. The research team then invited communities to come together to produce collective stories of coping with risk. In a series of facilitated workshops, they produced either theatrical productions, group dances or painted murals that gave a creative expression to their experiences.

As social scientists, we tend to think about validity as a property of knowledge. The creative arts have not conventionally been seen as compatible with producing this. Valid knowledge entails the rigour of a systematic, scientific approach. But the ‘Moving with Risk’ approach calls certain aspects of this view into question. A scientific epistemology that attempts to separate subjective emotions from objective truth appears dubious in this context where the research explores what are profoundly sensitive and emotive experiences of traumatic events. Understanding the lived experience of displacement is not reducible to a series of events and life decisions, as there is also a need to connect to the emotional truths that are intimately bound to these. Yet
emotive memories around life traumas are not easy to express, especially in the context of a research interview with a stranger. The researchers used the creative arts precisely for their power to enable people to access and express emotions.

But in the research process, emotions were not taken as simply moving in one direction. Hearing people’s stories also provoked strong emotions among the research team. For instance, Teresa tells me how the team would each reflect on what they felt they had learnt from the stories shared with them. She gives an example, describing how ‘after interviewing one lady, we wrote down that we had learnt of how much she cared for her family and how strong she has been to keep the family together and put the children before her. As people we learnt that from them – as people we were touched by this’. Rather than trying to remain detached observers, the context demanded that they connect to the emotions being opened up to them. Viviana also emphasizes the importance of having trained psychologists as part of the team: ‘You cannot just be bringing up these emotions and then say “see you later, go home”’. In utilizing arts as a methodological tool there was a need to make sure that emotions were processed and channelled responsibly.

Teresa explains another aspect of their choice of approach: ‘Through using creative arts, they [participants] could express the stories they wanted to tell in a way they had control over. They could express what was important to them from these traumas – which allowed for a different conversation to take place around displacement’. This is significant given the way in which the voices of displaced people in Colombia are normally heard. Individuals have been called on to prove their displacement in order to access support or compensation from the state. To do so they face an interrogative process, which prompts them to crudely recount their stories of the violence in ways that reinforce their status as victims. Teresa noted how using the creative arts to express their stories in a new way had the potential to be empowering: ‘It helped them seem not only victims of displacement, of arriving and living somewhere where there is risk, but that they are also capable. They have been really resourceful in managing this. In a way that was part of the ethical heart of the project, making them feel not victims but also as agents of change’.

The point to stress here is that using the creative arts was about more than building knowledge of their trajectories of risk. It was also about working collaboratively to find ways to build their capacity to cope with risks they continue to face. In part, this was achieved through a ‘psychosocial’ approach of helping individuals process and come to terms with the intense traumas they had lived through. Viviana described how for some this process seemed almost cathartic, while for others it had a less immediate impact. At a community level, they were able to present their collective murals, dances or theatre pieces to a public audience that included representatives of local and national authorities. These artistic workshops were also combined with legal training and capacity building for community mobilization. Together these activities are helping to build a more productive dialogue between these communities and authorities, where before there was disengagement.

All of which brings us back to the concept of validity. I brought up earlier how the creative arts were used to connect with emotions in a way that expanded conventional ideas of valid knowledge. But their approach draws on a notion of valid social research that goes beyond its relation to knowledge outputs. They make a compelling case that validity resides in part in what our participants get more directly out of the research process. In this way ethics was more than just an obligatory consideration; it was at the very heart of their research approach. As Teresa makes clear: ‘we are not just extracting information. They are getting something out of that process as well. That is at the core of why we think using the arts is important. It allows us and the people we work with to get something out of it’. It invites researchers to more seriously ask: According to whom is this research valid? What are the participants’ experiences of the research? Do they see any benefits?

As social researchers, we want our work to bring about positive change for those individuals and communities we work with. This is conventionally seen to come through the indirect impact of the knowledge we create on influencing policy or institutional practices. But often we overlook how the most direct impact we have arises out of participants’ experience of the research process. The ‘Moving with Risk’ project foregrounds how when we think about what counts as valid research, we should also be more seriously considering how our research effects its participants – not just in minimising potential harm, but in bringing real benefits. Validity here is broadened to be an ethical matter as much as it is epistemological. Moreover, their work demonstrates how the expressive, transformative potential of creative arts makes for a powerful tool social research can employ in striving to achieve this end.
Progressive politics and critical scholarship are under attack. While this is neither new nor unusual, these attacks have reached a new pitch and intensity in the current context of the global rise of the (far) right. They target individual scholars, as seen in the public hounding of Judith Butler and Kimberlé Crenshaw, purge degree programmes from universities, as witnessed by the exiling of the Gender Studies MA from the Central European University, and seek to humiliate entire branches of scholarship by decrying them as ‘constructivist sophistry’, as the ‘Grievance Studies’ hoax article has done.

The charge raised against this scholarship – usually in gender, queer, post-colonial, and critical race studies – is that it is not ‘proper’ science. According to its critics, it lacks objectivity and rigour, fails to produce independently verifiable results, uses incomprehensible jargon, and is ultimately rooted in the subjective political commitments of its authors instead of ‘simply’ researching and theorizing social and political reality ‘out there’.

This espousal of positivist principles of ‘proper’ science can also be found in reactionary scholarly efforts to re-appraise Empire. Prominent examples of this approach include the ‘Ethics and Empire’ project at Oxford University, as well as a 2018 article in Third World Quarterly. Both stake the validity of their undertaking on the objectivity of their approach and the universality of their methods, which proceed by ‘test[ing] the critiques against the historical facts of empire’ (Ethics and Empire) and using ‘simple epistemic virtues’ to conduct an ‘objective cost/benefit analysis’ of colonialism (TWQ).

What are the politics of knowledge at work in these confrontations? How should progressive, critical scholarship respond to charges of lack of scientific integrity and rigour? What kinds of politics of knowledge are necessary to produce scholarship that is both valid and emancipatory? I conducted interviews with three scholars in gender, critical race, and post-colonial studies who are actively involved in movements to decolonize the university and end sexual violence on campus: Dr Akanksha Mehta (Lecturer in Gender, Sexuality, and Cultural Studies and co-director of the Centre for Feminist Research at Goldsmiths), Prof Gurminder K. Bhambra (Professor of Postcolonial and Decolonial Studies at the University of Sussex), and Prof Alison Phipps (Professor of Gender Studies at the University of Sussex). The interviews were conducted separately in April 2019, and appear here in the order in which they took place.

This interview with Akanksha Mehta is the first of three interviews. The other interviews in this series can be found (here) and (here).

L: I wanted to start by talking about the politics of knowledge in some of the recent scholarship on the history of Empire. Some of these projects have openly stated their aim as re-appraising Empire as an ethical, humanitarian undertaking, and generally a force for good. Historical fact, objectivity, and empirical method feature prominently in their project descriptions to highlight claims to scientificity and truth, and their self-portrayal as detached, disinterested purveyors of fact. What is your view of the mobilization of historical fact and objectivity in this context?
AM: There is a lot of discussion on so-called historical facts these days. But of course the most important question here is what counts as a ‘historical fact’ and what doesn’t. When ‘historical facts’ are brought in about slavery and racism, they are often discarded as not ‘fact’ enough, not historical enough. So the fundamental thing to think about is – who decides what is a historical fact and how does something become a historical fact. And this links to who is considered as being able to occupy the positionality of being ‘objective’ and who is considered the holder of historical knowledges or ‘historical facts’ and this, of course, is dictated by whiteness. It’s also important to think about Gurminder Bhambra’s work on ‘methodological whiteness’ here. When the assumptions are produced through coloniality, when concepts, ideas, and knowledge productions are happening through a lens of whiteness, when the archive, or ‘facts’, or ‘history’, or ‘objectivity’ are all being produced through whiteness – then how do we move forward? For example, take the case of the archive. If we are thinking of the archive as a place of knowledge production but limiting the idea of the archive to a physical space containing ‘valid sources of data’ and using ‘evidence-based’ methods to convert this data into knowledge, then we are producing colonial knowledge through colonial means. That archive will always be colonial – naming and classifying, excluding and leaving out – that will be the history of such an archive. The very assumption that there is such a thing like an ‘objective’ method that is somehow not rooted or situated in the positionality of the researcher or the material and that any knowledge is truth, is false. Decades of feminist work by black, indigenous, and women of color will tell you that you always have to situate yourself in your work, or situate your ‘sources’ and think about how something becomes a ‘source’ or a site of knowledge and about how legitimacy is granted to some knowledges and how some knowledges become legible. How can we still be having these debates?

Some of these recent projects and writings that have attempted to justify Empire and show it as a force of good have been framed as going against the grain of some larger project of ‘political correctness’ and these researchers see themselves as going against some homogenous grain. But actually, feminists, queer theorists, postcolonial and critical race scholars have been the ones that have been saying necessary and uncomfortable things – but what they say is seen as disruptive in a negative way. Even in the everyday context of the university, teachers and lecturers who question whiteness and hegemonic assumptions are seen as a ‘problem’ and as ‘disruptive’ and those that embody white structures are seen as adding to robust ‘debates’.

L: You mentioned that decades’ worth of critical scholarship critiquing notions of empiricism and objectivity already exist. Yet somehow, its ‘critics’ get away with claiming that it doesn’t engage in knowledge production but in navel-gazing, self-absorbed pseudo-science. It doesn’t have any of the positive attributes of scientific enquiry but is often characterized, by means of feminized and racialized tropes, as naively approaching these topics.

AM: There’s so much gatekeeping in how knowledge is produced, how it should be produced, who is producing it, that what you just said is what happens all the time. Certain people are seen as being able to produce academic knowledge, especially what is considered worthy academic knowledge or impactful and legible academic knowledge, academic knowledge that is seen as going somewhere and doing something, and several others are just seen as disruptive, doing ‘identity politics’, being divisive, and making everything about what is seen as an agenda on race or gender or sexuality. The thing is, though, the neoliberal universities needs this ‘disruptive’ group, needs us, to fill their diversity agendas and to portray themselves as being tolerant and benevolent enough to give us a space and a career and resources to produce knowledge. But the university thinks that merely providing this tokenistic space is enough – it checks the boxes – but actually engaging with these critiques and even enabling them is not on the table. Even in the everyday context of the university, teachers and lecturers who question whiteness and hegemonic assumptions are seen as a ‘problem’ and as ‘disruptive’ and those that embody white structures are seen as adding to robust ‘debates’.
oppression. So in that sense the university manages to work really well, and a lot of people in these positions (often poc) also become invested in protecting the institution and whiteness.

L: I also wanted to talk about the ways in which this form of knowledge production is then mobilized as a form of doing progressive politics. Scholarship defending historical and contemporary colonialism often seeks to strengthen its argument through claims to stronger epistemological foundations, or producing ‘better science’. Of course we also find this in scholarship that defends naturalized gendered and racialized hierarchies. Could you talk more about that, and what it means for a science to elevate itself as better, and what are the effects of that claim?

AM: I think that you already said it – this is knowledge and science that not only defends but is also produced through naturalized gendered and racialised hierarchies. There is no discussion or questioning of the assumptions that this knowledge is based on, on colonial methods and methodologies that underpin it. And yet, or hence, this work is pitched as progressive. In the current political discourse where phrases like ‘politically correct’ and ‘identity politics’ are used to discredit struggles on gender, sexuality, race, caste – this becomes ‘progressive’. Because progress is seen through liberal frames where we ‘move beyond the past’ and learn to ‘live together in peace’ and have the ‘freedom’ and ‘choice’ to say what we want and do what we want and anyone who doesn’t like that is ‘fragile’, ‘divisive’, and ‘gets offended easily’. But this whole freedom of speech argument is constructed through gendered and racialised hierarchies and liberal values so it will always uphold colonial binaries and projects.

And this whole idea of ‘objectivity’ – what is that? Feminist and decolonial/anticolonial knowledge production teaches you time and again, that there no ‘objective answers’ and that it is colonial knowledge production that follows this path of evidence-based ‘objective’ research. ‘Objective’ and even ‘scientific’ research is built from going to the formerly colonized world and reducing centuries of oppression and violence and extraction to certain numbers/experiments that make up a ‘cost/benefit’ analysis that is inherently racialised and gendered. And this is then pitched as ‘progressive’ and ‘better’.

L: I also wanted to discuss the proliferation of attacks on gender studies and gender studies scholars. Why is this happening now, with this intensity and frequency?

AM: The easy answer to your question would be to think of the way the right wing has developed force in recent years. A lot of these right-wing movements mobilize gender and seek to reify gender norms and positionalities. Anything that dislodges that cis-het gender binary that remains crucial to these movements is seen as dangerous by them. Fundamentally gender studies, critical race studies, the scholars that you’ve mentioned – Butler, Crenshaw etc. – are all pushing and chipping away at what gender is and how we understand it. They’re making you unlearn everything that you knew about the world and about gender, and about the way societies and bodies and everything has been organized through gender. So I think there’s a backlash there and at these scholars/fields that becomes an easy way to project the anxieties against what’s happening.

And I think for a long time, academia was fairly closed off. It still is of course, no doubt about that, but more and more you are seeing academics speaking on social media and taking on public roles, making their work visible and sometimes even mainstreamed. With regards to gender studies, ‘popular’ and mainstream white feminism has also done a huge disservice here. It has ‘popularized’ feminism in such hollow ways, removing the politics from the word, whitewashing the term, and yet at the same time drawing attention to it from those who stand against it.

Regarding the ‘Grievance Studies’ hoax article, on the one hand I was really happy that the entire episode showed that these academic journals are not the best place to share our work and that we should be actively resisting all of these publishing models, and that we should be thinking about how knowledge production can take on many forms and shapes. On the other hand, I was really unhappy that the group went after gender and race work. It’s hard enough when gender studies departments are shutting down across the world, when gender studies work is still not seen as knowledge production by other ‘disciplines’, and when people are always asking you how is gender relevant, even after 30-40 years of feminist theorizing. In that sense, the hoax wasn’t doing anyone a favour by wanting to expose the foolishness of academia by choosing to do it through attacking gender studies and related critical work. To be honest, I’m just really tired of it – feminist, queer,
critical race, postcolonial theories are not just theories, they are your life – you literally survive, make meanings and worlds through them, you heal through them and it is really harrowing to see that these are the spaces that people want to discredit.

L: This is a point I wanted to raise with you because many of these so-called critics claim that they’re making these interventions to make these disciplines better. They say that there’s a fraudulent portion within these disciplines that don’t do proper science, and if we expose them as methodologically unsound, it would be better for everyone involved. That’s not possible, of course, because if we had the same ideas about methodology and epistemology as the people conducting these hoaxes, we wouldn’t have critical theory.

AM: Exactly! I think what was really upsetting was how this entire project was disguised as making ‘disciplines better’ but was actually about showing how gender studies, sexuality studies etc. was a ‘joke’. Yes, there is a need to call out some of the work that happens within these fields, for example, some feminist and queer work is clouded in whiteness and perpetuates racist narratives and ableist frameworks and we need to call that out, there is no question about that. But the hoax was not interested in that kind of calling out. If we really want work in these fields to be more rigorous and less (white) Eurocentric, then we can call it out through sustained critiques. For example, I am thinking of feminist work in/from South Asia that continues to privilege upper-caste voices, knowledges, and frameworks and omits discussions on caste carefully. Or of white feminist work that erases race from discussions on gender and sexuality and relies on racist frameworks. There is always an urgent need to call this work out, to question its frameworks and omissions, to situate the ‘knowledge’ it produces and the problems with it. And scholars have been doing that. But this hoax and other such projects are not about that. They are not about that kind of calling out and that kind of political project. What these kinds of hoaxes end up doing is making things difficult for everybody, and especially for black, indigenous, women of colour scholars and anyone who is already marginalized in academia – people who are doing really amazing and critical work. This hoax narrative discredits entire disciplines and fields, potentially cutting down funding pathways, employment opportunities, and departments. And what that leads to is privileged (white feminist) scholars, whose work is the one that should be called out, continuing to inhabit powerful positions and marginalized scholars bearing the brunt of the discrediting and potential cuts. So this exercise is quite pointless, especially in this global moment right now where things are already so contested and tense around gender – is this really the best possible project to undertake now? For whom is this project being undertaken?
Progressive politics and critical scholarship are under attack. While this is neither new nor unusual, these attacks have reached a new pitch and intensity in the current context of the global rise of the (far) right. They target individual scholars, as seen in the public hounding of Judith Butler and Kimberlé Crenshaw, purge degree programmes from universities, as witnessed by the exiling of the Gender Studies MA from the Central European University, and seek to humiliate entire branches of scholarship by decrying them as ‘constructivist sophistry’, as the ‘Grievance Studies’ hoax article has done.

The charge raised against this scholarship – usually in gender, queer, post-colonial, and critical race studies – is that it is not ‘proper’ science. According to its critics, it lacks objectivity and rigour, fails to produce independently verifiable results, uses incomprehensible jargon, and is ultimately rooted in the subjective political commitments of its authors instead of ‘simply’ researching and theorizing social and political reality ‘out there’.

This espousal of positivist principles of ‘proper’ science can also be found in reactionary scholarly efforts to re-appraise Empire. Prominent examples of this approach include the ‘Ethics and Empire’ project at Oxford University, as well as a 2018 article in Third World Quarterly. Both stake the validity of their undertaking on the objectivity of their approach and the universality of their methods, which proceed by ‘test[ing] the critiques against the historical facts of empire’ (Ethics and Empire) and using ‘simple epistemic virtues’ to conduct an ‘objective cost/benefit analysis’ of colonialism (TWQ).

What are the politics of knowledge at work in these confrontations? How should progressive, critical scholarship respond to charges of lack of scientific integrity and rigour? What kinds of politics of knowledge are necessary to produce scholarship that is both valid and emancipatory? I conducted interviews with three scholars in gender, critical race, and post-colonial studies who are actively involved in movements to decolonize the university and end sexual violence on campus: Dr Akanksha Mehta (Lecturer in Gender, Sexuality, and Cultural Studies and co-director of the Centre for Feminist Research at Goldsmiths), Prof Gurminder K. Bhambra (Professor of Postcolonial and Decolonial Studies at the University of Sussex), and Prof Alison Phipps (Professor of Gender Studies at the University of Sussex). The interviews were conducted separately in April 2019, and appear here in the order in which they took place.

The following interview with Gurminder K. Bhambra is the second of three interviews. The other interviews in this series can be found [here] and [here].

L: There have been a number of projects in recent years that have sought to re-evaluate the history of empire, often mobilizing epistemic notions like universality and objectivity to make their claims. Could you discuss the role of historical fact and the empirical method in this context?

GKB: History is always for some purpose, or the writing of history is always for some purpose. So, if we know what that purpose is, that will also give us some measure by which to understand how valid and correct or accountable those histories are. I wouldn’t wish to argue against notions of objectivity or of rigour or of method. I think those things are fundamentally important to how we develop our understandings across
the social sciences, let alone about history itself. But then we also need to think about how that knowledge isn’t produced in a vacuum. It’s produced both in a context of histories that have gone before it and it’s also produced in a context of other conversations that are happening. This means that the questions of legitimacy and the standards against which those narratives need to be judged are not just in terms of rigour in relation to evidence, but also in terms of other accounts that are being produced, which might call into question our central precepts. In that sense, people produce the history that they do, and to the extent that they’re doing it in good faith, they produce what they hope to be accurate, adequate accounts. If somebody comes up and says, ‘well you haven’t taken this other aspect into consideration’, then I think it’s incumbent upon all of us as researchers to take those critiques seriously, and think about whether the narratives we’re producing are able to account for the questions that are being raised.

L: I wanted to discuss the different ways in which grievance is discussed in the current moment – on the one hand, there is the ‘Grievance Studies’ hoax article, which situates it as something that is the property of a decadent and easily hurt science, whereas in your article ‘Brexit, Trump, and “Methodological Whiteness”’, grievance is something that is used to construct a legitimate object of racialized identity politics. Could you talk about this tension?

GKB: What I say in that piece is that the way in which some claims are argued to be legitimate, or some grievances are argued to be legitimate, is part of a broader politics that situates some people as legitimate in making grievances. So the issue is not about the grievance.

L: But how is this legitimacy produced? In your article, you argue that this legitimate subject of grievance is fabricated through shoddy methodology.

GKB: Well it’s not the subject of grievance, it’s the claim; and the claim is produced in a particular way, usually without sufficient attention to history. If people don’t know their history, they don’t know why society is configured in the way it is, and if they don’t know that, then they assume particular ways in which it’s supposed to be configured. If you imagine Britain to be a nation, and if you imagine it historically having been a nation, then it would be quite plausible to imagine it as always having been white. To think that that’s what Britain is, to think that Britain historically was white, would be the legitimation for why it ought to be white in the present. But if you were to understand that Britain hasn’t been a nation, and has, instead, always been an empire, therefore it’s never been white, it’s always been multicultural, then that claim to whiteness no longer has any historical legitimacy.

L: Would you say that we can draw a parallel with the construction of the social sciences? That their formation as colonial and imperialist disciplines has largely been forgotten, and for that reason they can more easily present themselves as universal sciences?

GKB: Disciplines emerge out of particular histories. If we don’t understand the histories out of which the disciplines emerge, we think of them as universal. For me, having a historical understanding, in terms of being able to think better about how the present is configured, is central, and that is as much for the social sciences and disciplines as it is for societies, and other phenomena.

L: But then, why do you think it is so difficult to produce these counter-histories and have them accepted – specifically those that foreground colonial violences?

GKB: I don’t think of them as counter-histories; these are shared, or connected, histories. But people often don’t wish to account for the shared histories that produce the present because that would require a rethinking of politics that has thus far been based on disaggregated histories.

L: The interests, reasons, are to defend entrenched positions of power.

GKB: Richard Drayton wrote an article about this quite a few years ago, looking at the writing of the history of empire in Britain. One of the things that he discovered is that, at the point of writing, no white British historian of empire had made violence central to their narrative of empire. I think it’s quite extraordinary that you have a whole tradition of history writing by white British historians which fails to recognize the constitutive aspect
of violence in their accounts of empire. That, to me, points to a very particular political purpose that these histories serve, which is precisely to justify empire, to justify the past, and to justify present configurations of inequality in relation to that past. So, to challenge that, and to open up the writing of that history, on the basis of a different understanding, it isn’t just another perspective, it would have material consequences.

L: Can you think of or identify instances in which either specific historians or movements of historical sociological scholarship have intervened in collective memories in a way to change them in the way that you are describing? I’m thinking of the case of German historical scholarship, which so massively foregrounds the genocidal violence of the Holocaust. But this, of course, has also been used to elevate the concept of what German statehood is now, as very much a reformed and repentant state.

GKB: I think that was useful in the sense that it worked to a certain degree; however, the consensus around this now seems to be breaking down. There are political parties saying, ‘why should we apologize for this aspect of our history?’ So, to the extent that there was a political commitment to repair what had happened, this allowed for an openness and the construction of an understanding of the past that produced positive social-democratic political possibilities. These are now under threat. We do also need to consider the general failure of scholars to address the genocide of the Herero and Nama people, which happened only forty years earlier. As such, there is a failure to broaden the scope by way of which we could understand the development of the state, and to think about how lives in other places matter as much lives in this place. Not necessarily in terms of comparing the events, but thinking about how they connect and relate.

L: I’m wondering about the potential for historical scholarship. In the German case, this was made possible through the end of one state and the beginning of another. In Britain, this state has been continuous. What is the potential for historical scholarship to intervene in collective consciousness without a break in the form of state?

GKB: Well, maybe Brexit produces such a break?

L: So there’s a sense of optimism there.

GKB: Britain has never had to confront the end of empire because at the high point of decolonization, Britain ended up joining the European Economic Community, and so went from having been a global empire and Commonwealth to being part of another powerful transnational federation. Therefore, it’s never had to deal with what it means to become a small state. Now, through Brexit, and if Britain actually leaves, and even if it doesn’t, it’s having to confront itself; and its immediate conceptualization, at least in some quarters, was to go back to Empire, this time conceptualized as Empire 2.0. Once it became clear that this wasn’t a possibility, the idea of CANZUK was floated, but even this disappeared quite quickly as Canada, New Zealand, and Australia have other interests and they are no longer as caught up in the relationship with Britain as they might have been under the Commonwealth. So the question is: what next for Britain?

L: This would be a future opportunity. What impacts have you seen this scholarship have so far?

GKB: None whatsoever. Most political and media commentators, as well as many academics, refuse to recognize that Britain was an empire and to think through the implications of that past on the present political configurations. This is especially the case in relation to issues of citizenship.

L: In this very pessimistic assessment of the role and impact of scholarship, how do you situate your own work and your own teaching?

GKB: It’s not pessimistic, it’s just a recognition that the structures of knowledge within which we operate have been in place for at least the last 500 years and dismantling them won’t happen in the blink of an eye. It will take work. I see the scholarship that I do, in collaboration with a lot of other people, as deconstructing and then reconstructing other, more adequate, ways of thinking about the world in which we live.

L: Could you speak in more detail about how this happens in your classroom? Could you give a concrete example of forms of knowledge that are deconstructed to then be reassembled differently?
GKB: When I first started teaching, I was asked to deliver a module on Modernity, and the first book that I had written was *Rethinking Modernity*. The module outline was basically everything I had critiqued in my book. I thought, well I can’t really just teach this module in the way that it’s been given to me to teach, but it was also my first year in a new job and I wasn’t able to do a wholesale revision of the module. In my first year, I taught the standard narratives in the first half of the module, and taught the critiques in the second half. All the student essays came back basically on the first half of the module. In the second year, I tried to intersperse the critique with the standard account – the French Revolution followed by the Haitian Revolution; industrialization, and then colonialism. Still, all the essays came back on the standard topics, none on the critique. Eventually, after a few years of teaching this module and trying to figure out different ways of doing it, I decided to rewrite the module as if it started with the knowledge that I had gained through the research that I had done on the topic. This became ‘Race and the Making of the Modern World’, which started with dispossession, enslavement, expropriation, extraction, and went on from there. The essays for this module were much more varied and engaged with the broader material of the module.

L: I wanted to also talk about your involvement in writing about, and participating in, processes of decolonizing the university. This term refers to a number of different practices – reading lists, hiring practices, removing symbols of empire from campuses, etc. – but it is also specifically about addressing colonial relations that are produced through scholarly practice. What kind of work needs to be done in this context?

GKB: There are two specific issues for me. One is to transform the concepts and categories that we use within the social sciences. Second, in more practical terms, is addressing the BAME marking gap which means that BAME students who enter university on the same grades as white students, leave with a 20–50% less chance of getting a good degree. This is something that will follow them through the rest of their lives and will have an impact on their ability to get graduate level jobs and earn equivalent to their white peers. There is no gender gap and there is no socio-economic gap, but there is a race gap. This is not simply a reflection of broader injustices and inequalities within society; it’s produced by our institutions. Given that I work in this institution, I have a responsibility to take that seriously and to work to eliminate it.

L: Could you speak in more detail about the concepts you addressed?

GKB: One of the things I’ve been looking at recently is the way that citizenship is standardly understood in the literature as something that is the property of those within the nation. However, when Britain established citizenship, it gave citizenship to everybody who was within Britain and within all its colonies. This was a single shared citizenship, and it also gave citizenship to everyone who had ever been in a former colony and was now in the Commonwealth. In this way, British citizenship, when it was first established in 1948, was given to over 800 million people. Its political practice since that time has been to take citizenship away from people. In the words of Rieko Karatani, Britain has turned citizens into immigrants, as opposed to it being the other way around. If we don’t understand that citizenship within Britain has been established on the basis of processes of purification, then we don’t understand what citizenship is actually about, we don’t understand why Shamima Begum has been stripped of her citizenship, or why Commonwealth citizens have been stripped of their citizenship, in what has been commonly termed the Windrush scandal. Or even what’s happening with Brexit, so the fact that now non-UK EU citizens are being deprived of their full citizenship rights. This is not new or distinct; Britain did this to its Commonwealth citizens in the 1960s and ’70s. So, to transform our social-scientific understanding of the concept of citizenship would be to take seriously its history in terms of when it emerged, how it emerged, what it emerged in relation to, and how it’s been transformed over the last few decades.

L: In closing, I wanted to return to the notion of objectivity and universality, which are often contested in critical scholarship.

GKB: Much of postmodernism has been about deconstruction, about deconstructing grand narratives. But if you simply deconstruct a grand narrative, it remains in place as the implicit common sense, it’s still there in the background. For me, the problem is not the grand narrative in general, it’s the particular form of grand narrative that is currently being used. I think grand narratives are necessary, it’s just that the ones we currently have in place are inadequate. Instead there is a need for reconstructed and reformed grand narratives, which take into account the connected histories of different peoples in different parts of the world, and provide a different frame through which to look at these issues. I’m not against grand narratives.
Progressive politics and critical scholarship are under attack. While this is neither new nor unusual, these attacks have reached a new pitch and intensity in the current context of the global rise of the (far) right. They target individual scholars, as seen in the public hounding of Judith Butler and Kimberlé Crenshaw, purge degree programmes from universities, as witnessed by the exiling of the Gender Studies MA from the Central European University, and seek to humiliate entire branches of scholarship by decrying them as ‘constructivist sophistry’, as the ‘Grievance Studies’ hoax article has done.

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What are the politics of knowledge at work in these confrontations? How should progressive, critical scholarship respond to charges of lack of scientific integrity and rigour? What kinds of politics of knowledge are necessary to produce scholarship that is both valid and emancipatory?

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This interview with Alison Phipps is the third of three interviews. The other interviews in this series can be found [here](#) and [here](#).

L: I’d like to start by talking about the politics of knowledge in a number of recent struggles in and outside of academia. These have included both efforts to re-situate empire as a worthy, ethical project, but also anti-trans activism. I wonder whether we could call their approach a kind of ‘brute positivism’, where claims are deemed valid because they’re supposedly common-sensical, and where arguments are called ‘better’ because they gesture towards certain indisputable facts, like anatomy or a
historical record. Would you agree? Is this politics of knowledge a common thread we see in right-wing, conservative scholarship and activism?

AP: I think so, and I think that probably gender studies and critical race studies are the canary in the coal mine. The hoax on gender studies and critical race journals mentioned a number of other disciplines as well – I think sociology, anthropology, there may have been others. So I think it’s an attack on all of the humanities, some of the social sciences, but obviously they’re trying to pick off what they see as the weak links first.

L: Could you talk in more depth about what it is about these disciplines that is identified as weak?

AP: I think it’s a couple of things. First of all, I think it’s the identity politics trope, which acts as a cipher for people who think that equality’s gotten out of hand, or within the more academic attacks, or those attacks masquerading as more respectable, is packaged up in opposition to what they see as an obsession with difference, which threatens the ideals of the enlightenment. So someone like Jordan Peterson calls himself an enlightenment liberal, who is just opposed to identity politics because it’s too particularistic.

The second thing is the grievance studies thing, the idea that we’re all focusing too much on our wounds, we’re all whingeing about our oppressions all the time. I think what that has done, which is a shame, is repackage a lot of valid feminist discussions around the politics of the wound. I’m thinking about Wendy Brown’s States of Injury, for example, which I think is a fantastic text which explores some of the drawbacks of having a politics which is wound-focused. I actually think what Wendy Brown is critiquing primarily is white feminism, which she doesn’t acknowledge, but for me her critique, when applied to white feminism, is valid. But especially in the backlash against campus activism against sexual violence in the US, critiques like Brown’s were repackaged into an accusation that victimization and trauma was made up.

And there’s been also a rejection of postmodernism in various different ways, with the idea that postmodernism is anti-science and anti-reality, or the source of ‘gender ideology’, which means feminists, LGBT or specifically trans people, depending on the context. The religious right have been targeting postmodernism for a long time. And the alt-right hate postmodernism, even though they don’t really understand what it is. So I think all of that has come together and put gender studies in the crosshairs, and critical race studies and queer studies as well. The word queer has become a particular target of both the right and anti-trans feminists, because they associate it with postmodern identity politics.

L: I’m interested in what you said about this theme of the enlightenment liberal that crops up in these debates. It seems to me like a clawing back of the narrative of progress, where the left have had that mantle for a few decades, and now we’re witnessing a countermovement from the right.

AP: Yes, exactly. And they call us the ‘recessive left’ or the ‘oppressive left’. Anti-trans feminist Julie Bindel uses the phrase ‘the trans Taliban’, which is a very clear position-taking that is incredibly problematic in lots of different ways. I think that’s particularly been a characteristic of some elements of the alt-right (the ones defined as ‘alt-lite’) – not wanting to be defined as conservative or reactionary. Instead they’re saying, ‘we know the way forward, and that way is universality, enlightenment ideals, progress, democracy – not identity’. Anti-trans feminists use a similar strategy. It’s very clever, in some ways – it’s probably exactly the right thing to do.

L: I also wanted to discuss with you a number of attacks on gender studies scholars and scholarship – for instance, the vilification and personal attacks on Judith Butler and Kimberlé Crenshaw, as well as that ‘Grievance Studies’ hoax article. Why are these things happening now? This scholarship and the antipathy towards it are not new, but it’s reached a new pitch.

AP: It has, and I think it’s been made possible by the global swing to the right. The Internet also has a role to play – the alt-right have had massive success on online and social media, which has created more spaces for these types of things to appear! So there is a definite audience and market for these types of views. It’s also very telling that anti-trans feminists are most often platformed by right and far-right publications like Spiked, the Spectator, and Quillette.
That being said, the write up of that ‘Grievance Studies’ article was very misleading because a number of their attempted publications were rejected. But also going back to your questions about knowledge, a number of the articles were actually defined as hoaxes because they rested on different assumptions about what counted as knowledge – knowledge that we in gender studies would find perfectly acceptable. So the idea that you could go to Hooters and explore the social construction of masculinity through the behaviour of the men in that bar is actually something we would do! But because they come from this, what did you call it – ‘brute positivist’ – place [changes voice] where men just like to look at tits! It’s no more complicated than that, everybody knows that! It’s common sense! [voice returns to normal] Because they were coming from that place, the article seemed ridiculous, but that premise is not ridiculous to me. And they had also studied so hard to develop these hoaxes that in some cases they produced articles that were actually half decent.

L: The charges brought against gender studies by the right are two-fold: On the one hand, it’s called frivolous and discredited as science. On the other, it’s positioned as dangerous and powerful, capable of destroying the family and corrupting your children. What’s your assessment of the first claim, and how does feminist theorizing accrue this power?

AP: It’s not frivolous! There’s a huge body of feminist epistemology and theory around the problems of the idea of objectivity. All perspectives are partial, it’s just that the perspectives of the dominant group become universal, become factual, become true. And that’s not just in feminism, that’s in Foucault and a lot of postmodern thinkers. It’s not new and it’s not even particularly revolutionary any more – many scholars in the humanities and social sciences operate with these assumptions, and some in the natural sciences as well.

We are dealing with very clear fascist tropes here – defining your enemies as both weak and strong at the same time, and obsessing about plots and conspiracies. This is compounded by misogyny, queerbphobia and transphobia, because gender studies is mostly done by women and LGBT people. It’s ‘girls’ things’, or it’s all about sex or feelings or experiences, which can’t constitute real knowledge. I think that’s a problem for other social sciences and humanities subjects as well, because people both dismiss expertise on ‘personal issues’ and simultaneously also think they have it. And to some extent they’re right – we are all experts on our own experience. So to validate experience as knowledge is both necessary and dangerous, because it means that people can play their experiences as intellectual capital in whatever way they want. So if we don’t have other criteria for evaluating knowledge, that becomes quite dangerous.

The right does this all the time. There’s been any number of privileged white men saying, ‘I feel threatened!’ Piers Morgan, for example. That’s an experiential knowledge claim. But of course they don’t acknowledge it as that, and because of their dominant social position, it just becomes accepted as fact. So it’s not frivolous – what gender studies scholarship is saying is effectively common sense. Everybody uses experience as a form of knowledge-making, but we get the blame for it when people want to dismiss ‘the feels’.

The idea of gender studies as all-powerful is partly about white men’s fear, which is elevated at a time when white fear is centre stage. The fear that women are taking over, that entitlements are being threatened, because deep down, privileged white men know that their entitlements are not god-given gifts – they’re things that they don’t deserve. So that can be very frightening. With MeToo, the idea that sexual harassment might suddenly be called out is deeply threatening, because most men have probably done something in that area that they’re not proud of. That doesn’t mean they should all be locked up – that doesn’t mean they should all lose their jobs. But being in a position of privilege makes you hyper-alert to threat. And the monstering of feminism is part of the backlash. Women have taken over...I mean, who? How have we taken over? The world is still run by men. There are other fears at play as well, which very often involve privileged white women – the fear of intersectionality, which seems to be a fear that feminism will be overrun by queers and trans people, and which expresses colonial values. The tropes of ‘female erasure’, ‘male obsolescence’, and ‘white genocide’ are quite similar in their origins and make-up.

L: What should our response to these charges be? Should we double down on fact and objectivity [Alison interjects: ‘No!’], or do we reject the terms of the debate?

AP: I don’t know exactly what the response is. I don’t think we need to double down and say actually we are objective, because we’re not! I think we definitely reject the terms of the debate – or any debate at all – with fascists. ‘Reasonable debate’ is not a way to counter unreasonable and dangerous ideas, it’s a way to give them
oxygen. And we’ve seen that happen – as the mainstream media have platformed far-right figures, their politics have become more acceptable and gained more power. In terms of the more ‘respectable’ figures involved in this, they often don’t act in good faith, so debate becomes extremely difficult. Anti-trans feminists, for example, tend to change their claims depending on what part of the argument they want to win. So sometimes trans women are too feminine and are shoring up oppressive norms. Other times they’re too masculine which means they are perverts and sexual predators. And the ‘alt-lite’ don’t seem to believe half of what they say, it’s all a big game. So debating with them would be like trying to hit a moving target.

There is perhaps work to be done with people who might be convinced or not convinced, depending on how much information they have and are given. Especially with younger people, and that is where we as academics are in a good position, to be able to raise these questions with students about the nature of knowledge, and what counts as valid knowledge. Also to educate them on how to think for themselves in a ‘post-truth’ context where the claim and counter-claim has reached new heights. I think that can be done in all disciplines, not just ones that have a methodological focus.

L: As a university professor you’re speaking with students all the time.

AP: Yes exactly, and I would certainly engage with students who express these views in my classes, and other students would do that as well. But it’s a very contested, dangerous field at the moment, because you can be accused of indoctrination very easily as a university lecturer. I’ve been accused of being too political, of trying to indoctrinate students into my views, even though I’m very careful to reassure students that they can disagree with me. The Overton window has shifted, so just speaking in a left-wing register has become oppressive and censorious and indoctrinatory. I think it’s useful to present students with examples and data, for instance about equal pay, or universal credit, or the hostile environment, or trans healthcare. This doesn’t have to be done with strong claims to ‘objectivity’, but students can be encouraged to evaluate different sources and make up their own minds. Because we’re in an era of post-truth where people can make up facts to suit themselves, and broadcast them very loudly to a very large audience, perhaps the best thing to do is to give people the capacity to think for themselves. So not just to say ‘you’re wrong’, but explore the bases on which claims are being made.

L: In your recent article ‘The Fight Against Sexual Violence’, you discuss how a lot of feminist organizing is compromised by a stance of ‘political whiteness’, where both injury and redress are only seen in a context of affluent whiteness. Can you talk about the requirements for a progressive politics of knowledge and struggle? What do we do specifically, in our scholarship and activism, to counter that stance?

AP: I think an obvious way to counter what I call political whiteness is to make sure women and non-binary people of colour are at the forefront of our movements, to make sure that we’re reading their scholarship and other writings. We need to take our lead from people who have been doing this kind of alternative politics for a long time, because they understand the nuances and complexities. The prison abolition movement in the US especially is a really good example of this. Black feminists have been working for a long time in that movement, in the spaces between sexual and domestic violence and state violence. This is where we all need to be.

There is also what Mariame Kaba calls the ‘jailbreak of the imagination’, which is about imagining the kind of society and politics we want. That doesn’t mean we have to be utopian and idealistic, it can be very practical. We can create a map, against which we can evaluate whether we want to support certain reforms or not. Kaba talks about ‘non-reformist reforms’, which in the context of prison abolition are reforms which shrink, rather than grow, the state’s capacity for violence. You could apply that to individual institutions as well – reforms that shrink rather than grow the institution’s capacity for violence. And one of the main features of political whiteness is its willingness to call on the oppressive power of the state and institution to redress individual injury. Privileged white people feel safe with punitive technologies – we’re perfectly happy to say people should be fired or put in prison, because we know it’s probably never going to happen to us. But if we try to imagine the kind of society we would like to ultimately have, a society that works for everyone, does it have prisons in it?

Imagining a society that works for everyone also requires what Angela Davis calls the ‘intersectionality of struggles’. What if MeToo were to have a closer relationship with Black Lives Matter? What would be the
position on prisons then? What if people working against sexual harassment in workplaces were to have a
closer relationship with the sex workers’ rights movement? Who can teach us more about sexual harassment
in the workplace than sex workers? Who can teach us more about the nuances of consent than sex workers?
And wouldn’t it be great to lend our solidarity to their struggles? What if we were to join up campaigns against
reproductive coercion with trans rights – who can teach us more about bodily autonomy than trans people?
And that intersectionality can be done in really practical ways. Sometimes it might be about having new
conversations, following a few people on social media and listening to what they have to say, trying to reach
out in small ways. Political whiteness is a default setting – for me as well. We all make mistakes. But taking our
lead from more marginalized people, and trying to have a joined-up politics, is the way to go.

L: We’ve talked about the ways in which feminist theory is powerful and the ways in which it’s perceived
as threatening because it successfully destabilizes naturalized embodiments. In closing, could you
reflect more on that: How is feminist theory, particularly intersectional feminist theory, useful in these
struggles? What does it bring to the table?

AP: Not all feminist theory is that great, actually. The more one-dimensional analyses of patriarchy are not
particularly helpful, because of all the things they miss. But intersectionality is fantastic because it’s just a
way of seeing the world. Kimberlé Crenshaw has said that it’s not a theory, it’s not a methodology – it’s a
framework. And it is! Once you’ve seen how things are connected, and not just identities or positionalities but
structures as well, it’s quite hard to unsee. Patriarchy, capitalism, and colonialism are symbiotic systems and
you have to think them together. Seeing this doesn’t mean you don’t make mistakes in your thinking, or that
your thinking is complete – it never is. But once you have crossed that line into thinking more intersectionally, it
is difficult to go back. It becomes part of who you are.

I also think feminist theory is powerful because gender is so taken for granted and so naturalized. Gender is so
deeply embedded in capitalist and colonial structures that it has become part of the ‘common sense’ of who we
are. That’s why deconstructing it is so frightening and threatening. The shift to the right has involved a massive
reassertion of binary gender – men are men, and women are women, Brexit means Brexit [laughs]. This is a
deply capitalist and colonial logic – capitalism needs binary gender and the heteronormative nuclear family,
and colonialism exported, and continues to export, this as a means of controlling people, land, production, and
resources. Feminism threatens all that – when it’s done intersectionally. So perhaps this is a plot, after all!
What Counts as Valid Research in Social Work?

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Abstract

The notion of social work research impacting directly on practice is nothing new. This article explores the challenges within the recent trend which privileges randomized controlled trials (RCTs) – the ‘gold standard’ of research within medical science – and a relatively new approach to evaluating the effectiveness of interventions with children and families. The What Works Centre for Children’s Social Care is also discussed in terms of what policymakers count as ‘valid evidence’ when allocating financial resources within Children’s Services. The appeal of the RCT is clear to those in charge of budgets, offering a binary, conclusive approach to finding out whether or not something works. Yet an RCT methodology will not capture the nuances of relationships, trust, and social meanings – all of which are integral to delivering effective interventions in social work with children and families. The simple picture painted by government and what they circumscribe as valid research is not the reality of where social work interventions take place. The What Works Centre for Social Care does, however, offer some reasons for hope in working towards a richer research landscape, where the knowledge base is more diverse, valuing and yielding both qualitative and quantitative insights.

What is the role of research in areas where there is clear and pressing social need? The number of children in care in England has steadily risen since 2008. In January this year, it was found that 88% of Children’s Social Care departments in England had overspent their budget. What evidence should inform government spending on Children’s Social Care? Here we will consider how the UK government, in its attempt to answer this question, is currently enlisting research evidence, albeit with an arguably circumscribed focus on what it considers to be ‘valid’ research, a position which is largely contested within social work scholarship.

In 2013, the government set up the What Works initiative, seeking to ‘ensure that spending and practice in public services is informed by the best possible evidence’, proudly claiming this to be the first time that any government has taken a national approach to prioritizing the use of evidence in decision-making (Cabinet Office, 2019). There are seven existing research centres, looking at areas such as health and social care, education, and policing. They use a combination of systematic reviews of existing evidence and the commissioning of their own research to provide an easy ‘toolbox’ of information for practitioners to apply. A review of the first five years points to the successes they have had in identifying effective practice in classrooms and police forces across the country, while also highlighting some of the challenges encountered in ensuring the adoption of best practice (Cabinet Office, 2018).

The What Works Centre for Children’s Social Care (WWC), currently in development with funding from the government’s Department for Education, is due to launch in 2020. It is already creating waves in the sector with their review on Signs of Safety – a widely used, government-supported approach to safeguarding children and working with families (Turner, 2018). The review identified that there is little clear evidence that this intervention has any impact on reducing the number of children in care. By evidence, the WWC considers the contextual factors in research, but places a high value on quantitative studies, ideally Randomized Controlled Trials (RCT). RCTs are an experimental design, considered the ‘gold standard’ of validity in medical science research and amongst other ‘hard’ sciences, but they constitute a relatively new approach to evaluating interventions in social care. The objective of an RCT research methodology is to conclusively examine the effects of an intervention – for example, a new drug treatment – and to this end, it will randomly allocate participants between two groups. One group will receive the intervention under investigation, while the other group – the control group – will continue to receive ‘treatment as usual’, that is, they will not receive said intervention. RCTs aim to enable valid claims as to whether the intervention under investigation makes a difference by excluding all other potential explanations, enabling conclusive comparisons between groups as to the effectiveness of the intervention.
The appeal of the RCT is clear to those in charge of budgets, since they offer a binary, conclusive decision on whether or not something works. 

WWC acknowledges that currently almost no studies in Children’s Social Care met their standards to gain a meaningful evaluation, and that building this evidence base in social care will be particularly challenging (DfE, 2019). A review of all research articles published in three UK social work journals over a decade, covering all areas of social work (i.e. also work with adults), identified only six RCTs. The author of the review, Sheppard (2016), suggests that concepts of validity play a part in these low numbers and that social work might lean towards an interpretivist understanding of the world – where numbers are not seen as providing a meaningful understanding of how people experience their lives. Perhaps this reflects the motivations of a profession trained to see the individual or family unit’s unique needs and circumstances, rather than to think in terms of population. But this notion is being challenged by some within the WWC network.

On 29 May, the Chair of the WWC, Alan Wood (@woodsgolem), tweeted:

“Too much research in educ and children’s social care is inconclusive, leaving the front line staff frustrated and curious as to why. We should fundamentally rethink the models and methodologies we have favoured and look more closely at promising practice in need of evidence.”

There have been multiple voices on Twitter raising objections to this statement, generating a debate on a number of fronts. Professor of Social Work at the University of Sheffield, Sue White (@ProfSueWhite) firmly asserted that Alan’s sentiments seem to fundamentally misunderstand research, suggesting that it ignores years of already existing research, a rich knowledge base of ‘inconclusive’ findings which offer ‘useful cumulative insights, tiny and often serendipitous’. Other responses drew attention to the significance of structural issues outside specific interventions, such as high staff turnover and austerity which, unlike inconclusive research, do actually frustrate front-line staff. Alan suggests in his initial tweet that the value of research lies in whether the findings can provide a clear basis for practice. The importance of research being relevant to practice can hardly be overstated within the field of social work, but is it ever possible to offer certainty?

Looking more closely at the use of RCTs within Children’s Social Care, some of the challenges in applying this methodology and developing ‘conclusive’ research for practice become starker. There is always a question of the validity of applying an RCT design to evaluating the effectiveness of social work interventions with children and families: for instance, do the findings truly reflect the phenomenon they are claiming to represent? Firstly, RCTs are only concerned with what works, but the question of why can be equally vital – particularly when thinking about how to improve practice in children and family social work. Can social work interventions ever, realistically, be free from extraneous psychosocial factors impacting the child or family under investigation? Secondly, another key concern in social work research has been to explore what it feels like to be subject to these interventions, not merely trying to discover whether or not they ‘work’. Any time the state makes an intervention in a family’s life, there is the potential for oppression, and research into service-user perspectives allows this to be explored. An RCT methodology, as Featherstone, White and Morris (2014) argue, will not capture the nuances of relationships, trust, and social meanings. Questions have been raised about the appropriateness of using RCTs in some situations. An evaluation is planned of Family Group Conferencing, a structured meeting which allows families time to develop their own plan for children who may be at risk. These meetings were developed from a rights-based perspective, to give power back to families within the child protection process. Some academics have raised concerns in this regard, claiming that this opportunity should not be randomly assigned to some families whilst denying it to others (Turner, 2019).

Undertaking RCTs in a field that is arguably unfamiliar with this methodology can be complex. One RCT looked at a model of foster care for adolescents. A particular challenge in the study was that social workers and managers were reluctant to make referrals since they had no control over whether the intervention requested would be allocated to their young person (Dixon et al., 2014). This reflects similar findings from an RCT that looked at a parenting programme, where practitioner ‘buy-in’ was given as one possible reason for variation in recruitment rates (Rushton and Monck, 2010). Social work practice takes place within the often chaotic and unpredictable context of relationships, both with families, social work teams, and within diverse local areas and communities; all of which makes this methodology rather more challenging to apply outside of the controllable laboratory setting. An RCT cannot reveal if the intervention under examination will work in different contexts, with different groups, or when it is confronted with complex and non-standardized situations.
Perhaps practitioners are likely to need time to be convinced of the value of knowledge generated via RCTs. The WWC seeks to aid workers by undertaking this initial evaluation of research for them. A new research approach, dubbed the ‘pragmatic stepped wedge’, has just been announced and will be pioneered by the WWC (Sanders, 2019). Their partner Local Authorities will all be trained in delivering a new intervention, but crucially the order in which they start this intervention will be random, allowing them to act as each other’s control group in the research. We will have to wait to see if this will be an effective response to the issues raised by the application of RCTs in other social work areas, and indeed whether it will meet their standards of valid research. A key factor in this process will be ensuring that social workers are aware of this new resource. In a highly scientific sample (texting two friends who are still in front-line work) neither had heard of the WWC.

It is understandable that a government would wish to secure its legacy by claiming to have found the key to ‘fixing’ welfare issues. Social work is a highly politicized and moralized area. Research is, of course decisively socially arbitrated and, as such, it has the potential to be oppressive. The simple picture painted by government, and arguably underpinned by the government parameters for what constitutes valid research, is not the reality of where and how social work interventions occur. The What Works Centre for Social Care does, however, offer some hope in working towards the creation of a rich research landscape, where the knowledge base is more diverse, hopefully offering, utilizing, and valuing both qualitative and quantitative insights.
References


3. Reflections
‘Oh. My. God. You’re crazy!’: The Struggle for Validity Whilst Parenting and Doing a PhD

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Over the past year, I have reflected upon the struggle to feel acknowledged as a valid member of the academic world, primarily due to my dual roles as mother and student. The societal pressures placed on parents, especially mothers, when they start a family are felt across the world. Everyone has different values, perceptions, and opinions about how children should be raised. Trying to keep up with the range of varying and sometimes competing expectations can be incredibly stressful, especially in the first few years. In theory, these differences allow for generations of children to be exposed to a diverse set of values as they grow up. However, in practice, this relies on us accepting others’ values, and not assuming that our own way is the only way. Intergenerational pressure is perhaps the most complex issue in this regard. It is almost a guarantee that any new parent will hear the words ‘well, it was fine in my day!’ and ‘they just keep changing the rules!’ And whilst in most cases such comments are well-intended, they can be hard to stomach because we have the power of hindsight. Yes, it was ‘ok’ not to use a car seat or a seatbelt 50 years ago, but we also know that the number of infant mortalities in car accidents has plummeted since the introduction of safety regulations. A divisive issue that certainly has an intergenerational precedent, and one that has more recently become a sticking point for many families, is the dual role of parenting and working. Whilst I speak from my own experience as a woman in a heterosexual relationship, I fully appreciate that others will take on this work-life/family burden in different ways, so when I say mother/woman, I hope readers will assume their own role in the family dynamic.

The expectations placed on parents, particularly mothers, to balance the commitments of work and childrearing are significant, especially in a society where, unfairly, the job of raising children is still very much the woman’s ‘role’. The Office for National Statistics reports that on average women still do 60% more unpaid work than men. This is especially true for women with caring responsibilities. But since the cost of living has become so high now, in contrast to 50 years ago, households cannot always afford to have a stay-at-home parent. The pressure on mothers to work (or indeed to not work) are immense, and there’s an expectation that women can work as if they don’t have children to care for, and parent as if they don’t have to work. The cries of ‘women can have it all!’ are loud, but they are not backed up with societal support. Some women may well be able to have it all, but they will be judged harshly by many for doing so. Additionally, ‘having it all’ can also mean shouldering all or the majority of the invisible, mental, and emotional burdens of caring for a family, where the traditional roles of women as caretakers of the household are still subliminally and subtly reinforced. Doctor’s appointments, birthdays, sorting out clothing sizes for your rapidly growing offspring, ensuring that they eat more than just biscuits, extra-curricular activities… the list is endless. And individually, none of these tasks are particularly taxing, but taken together, on top of work, studies, hobbies, sleep, staying healthy, keeping up with friends and family, life can quickly become incredibly hectic. And unfortunately, such responsibilities are still disproportionately held by women as the traditional caregivers.

So, what does this have to do with validity? Well, I wanted to write this piece because I have noticed an alarming trend in my own personal experiences, which I think needs to be addressed.

Three weeks after my second daughter was born, I received the unbelievable news that I had succeeded in obtaining a full scholarship to do the PhD that I had been dreaming of for almost a decade. In the following months, I heard the words ‘Oh my God, you’re crazy’ so frequently that I started to pre-empt the response by saying ‘Oh yeah, I’m starting a PhD in September, I know, I’m totally crazy!’ But those words wore me down like a voracious earworm. I’m not crazy. I chose to do this. I knew I wanted to start this PhD while our children were young. For our family (my husband is also a student), it was a no-brainer. We have flexibility, childcare grants, and a drive to make the world a better place for our daughters. We’re in a unique position as students where procrastination is a luxury we can’t afford, and this is just one of the many benefits of studying while parenting. Of course, I know that most of the comments were not made with any malicious intent; they came from family, friends, and colleagues who meant well, and who perhaps sympathised with the challenges ahead of me. But it still felt invalidating, and was certainly not something my husband ever heard, who has experienced wonderfully positive responses to the news of his return to education as a mature student. Anecdotal yes, but

1 https://www.ons.gov.uk/employmentandlabourmarket/peopleinwork/earningsandworkinghours/articles/womenshouldertheresponsibilityofunpaidwork/2016-11-10
painful nonetheless. Why should one parent (regardless of gender) be seen as a superhero, whilst the other one is deemed crazy? We casually use language that refers to mental illness, and I find that problematic. I wonder whether new mothers who decide to, say, run a marathon or start a business also receive similar replies from their well-meaning family and friends. Words like ‘crazy’, ‘insane’, ‘mental’ are frequently used in casual conversation, but the connotations behind those words are powerful.

Despite much progress being made, we are still in need of significant societal change before many working and studying mothers feel that their place in the academic world is valid. So, let’s challenge the use of language that refers to mental illness directed towards mothers (or indeed anyone else) taking on some of the most exciting journeys of their lives, and give them validity. I am not crazy. I’m excited. I’m motivated. I’m valid.
Bending and Flexing Around Valid Youth Work

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Abstract
This reflection is the result of working on a UK-wide applied research project called the Youth Programme Quality Intervention (YPQI). The piece explores my own struggle with the concept of validity and reflects on how my personal value system and view of what constitutes ‘valid knowledge’ have had to change as a result of the realities of youth work.

Bending and Flexing Around Valid Youth Work
I am a researcher at the Centre for Youth Impact, where I am working on a project called ‘The Youth Programme Quality Intervention’ (YPQI). The YPQI is a continuous, quality-improvement initiative, which can help us to re-frame how we value and evaluate quality youth work. It's based on a research-validated tool that has been subjected to experimental trial and is made up of 70 items, capturing quality youth work at the granular level so that youth workers have effective criteria against which they can measure their own practice. The tool has been tried and tested in over 4,700 youth work sites in 20 US states. The evidence shows that youth organisations that participate in the YPQI see improvements in youth worker skills and outcomes for young people.

In this reflection, I will focus on the validity of the Programme Quality Assessment tool (PQA), which includes 70 items that describe quality youth work. Despite the compelling research findings, I struggled at first with the idea of a research-validated quality youth work tool. My feelings about the tool were formed by my post-positivist values, as well as my experience as a support worker with adults and young people, which made me sceptical of the findings. I found it hard to believe that quality youth work could be captured in a tool comprised of 70 items. I wondered whether the positivist in me could take this on board. It took a while for my irritation with unexplored biases, assumptions, and unacknowledged power, all of which contribute to the validating or invalidating of knowledge, to move over and make space for the realities of the system I was working with. To me there were multiple ways in which youth work could and should take shape. The idea of a single tool being used to measure all types of youth work was jarring to me. These thoughts also stemmed from my time at university, during which I delved into an epistemology of thought that explored nuance, difference, relationality, and how all of these things constantly interact with one another, proliferating and producing new categories, ways of being, ways of living, interacting, and understanding. I also considered how language is inadequate to capture and communicate all of these concepts in the first place. I still hold on to these thoughts and they influence my approach to work in many ways – for example, by not taking miscommunication too seriously, expecting it even, and understanding that context will affect the relevance of the tool and the motivation of different youth workers taking part. Such thoughts mean that it is more important to me to support those using the tool to find their own meaning for it. However, as I’ve come to find out, holding on to these values too rigidly, without any bend or flex, does not work when it comes to the practicalities of embedding an intervention within a system, and they do not fit with what many people want either.

Since starting my job in October 2018, I’ve realized that the practical realities of the wider context of youth work require agreed upon definitions and commonalities, and that people want or need to be singing from the same hymn sheet – or at least have one to compare to. Not everyone has an opinion about what constitutes quality youth work, and most would probably agree that it’s extremely tricky to achieve. This diverse group of people includes youth workers, funders, and policymakers, all of whom have different priorities and perspectives and rarely communicate with one another. This is not a wilful act of distancing; it merely arises out of the practicalities of our massively bureaucratic system.

Whilst I was busy trying to get on board with the idea of validity, I was also setting up calls with youth workers who had contacted the Centre for Youth Impact to talk about the YPQI. It was at this point when my views on ‘valid quality youth work’ began to change. Despite my reservations, the youth workers I was speaking to were telling me that they wanted an agreed upon, tested, validated definition of quality youth work, which would support them to embed continuous quality improvement in their organizations, as well as demonstrate their impact to funders. Later, as youth workers were trained in using the tool, it turned out that this particular concept of quality youth work was not experienced as quashing difference and violating alternative modes
of knowing. In practice, there was scope for difference in the use of the tool and, for the most part, it reflects what youth workers understand as quality youth work and clarifies their route to achieving it. I began to see that youth workers were on board with the tool’s validity and concluded that I should be too. I also saw the potential of this tool to bring youth workers together, unite players across geographical locations, as well as improve knowledge sharing and learning, precisely because of its validity.

There will always be cases where micro-experiences on the ground will not correspond with the wider system of principles, or the YPQI in this case. I listen out for these dissonances to see how we can improve the YPQI and to flag up what simply may not be working. But the lesson I am learning in practice is that embracing commonality over difference is sometimes necessary in order to bring people together and learn, and that my values cannot extend into all contexts. My own epistemological values often lead me to recognise difference rather than unity, and I’ve realized that such values can sometimes cause me to ignore the realities of the wider context in which I am working. In the youth work context, there has been an impact agenda for the past 10 years, which forces youth workers to measure themselves against a set of standards that undermine their work, and which is widely thought to contribute to the massive funding cuts that the youth sector has suffered. A research-validated tool, which youth workers themselves agree is valid, is potentially transformational, despite what I think about validity and the complexity of individual experiences, which I still stand by. The YPQI is about achieving a vision where youth workers have access to an evaluation process that is empowering and practical. In better understanding and appreciating this, my perspective on validity has had to bend and flex. In this instance, although I will be looking to respect and appreciate the details and differences between organizations that utilize the YPQI, I will also be making space to seek out the commonalities that demonstrate the YPQI is improving youth work across sites and, as a result, outcomes for young people.
I grip the steering wheel tightly, navigating the mountainous roads. Our van screeches as I push hard on the accelerator, but it doesn’t muffle out the phone ringing.

‘Sí?’ my partner answers. I know from his tone that the news is bad. As we feared, his grandmother has just passed away. Our rush was not enough to get him home in time to say goodbye. The feeling of guilt spreads across my stomach.

I’m at the beginning of my doctoral fieldwork, researching dementia and elder care experience in Andalusia. I’m exploring the strong family care networks this region is known for. My partner is Andalusian, and his grandmother was part of the inspiration for the research. I was interested in how, like many older Andalusians, she rotated her days between her children’s homes. Her family believe this shared care helped her live well with dementia until the age of ninety-seven.

I’d been staying with my partner’s family whilst conducting various scoping trips, visiting dementia day centres throughout Andalusia, seeking a suitable field site. My partner and I hesitated over this last trip, as his grandmother had been unwell, but thinking that she had improved, we decided to go. Then just after leaving a meeting with a day centre, we heard that her health was deteriorating rapidly, so we began the three-hour drive home, but didn’t make it back in time.

Days later, writing field notes about the trip, I found myself drawn back to where we’d been when we received the call. The town fulfilled the research criteria: a small population; far from the tourist-filled coastline; a high elderly population; and a day centre willing to participate. Yet despite other towns meeting these criteria, I leaned towards this one. The guilt of the trip causing my partner to miss his grandmother’s death influenced me to choose this town as the main field site as this would mean he had not missed her death in vain. My partner, who was accompanying me on fieldwork, said that choosing this place seemed somehow to honour his grandmother.

Could I claim to have made a purely scientific assessment of an appropriate field site in choosing this town? Or were my emotions invalidating the research process? Should the anthropologist’s personal life really influence methodological decision-making? Taking this experience as an example, I reflect on these types of dilemmas that anthropologists find themselves facing in the everyday messiness that makes up ethnographic fieldwork.

Ethnography is about ‘being there’, yet the ‘there’ of ‘being there’ no longer relates to a concrete place but rather to an abstract ‘field’ of the phenomena studied. Anthropology’s early conceptualization of the ‘field’ has been criticized as a ‘taken-for-granted space in which an “Other” culture…lies waiting to be observed’ (Gupta and Ferguson 1997: 2). The concept has hence been transformed into an understanding of the ‘field’ as something anthropologists actively ‘construct’ themselves (Coleman and von Hellerman 2012), merging aspects of personal and professional fieldwork (Amit 2000: 3). However, this blurring of research with personal life raises certain questions around validity. If the field is no longer considered a concrete place, investigated objectively, how do we assess a valid field site? Rigid validity criteria designed for quantitative research have been critiqued for ‘kill[ing] the spirit of qualitative work’ (Sandelowski 1993: 2). Instead, ethnography’s validity, by approaching the field as constructed, suggests a need to demonstrate how the anthropologist’s own experiences influence methodological decision-making.

Unlike other disciplines, where familiarity between the researcher and the subject are considered an impediment to validity, anthropology considers intimacy ‘a fundamental medium of investigation’ (Amit 2000: 3). Moser has suggested that qualitative researchers engage in fieldwork suitable to their personality and interests, facilitating emotional connection with informants (Moser 2008). Having previously lived in and having a partner from Andalusia gives me an emotional connection to the research context. It was during my
time living there that I noticed an approach to elder care that appeared different from what I had experienced working as an occupational therapist with older adults in London. This pre-interest, with personal regional links gives me a closeness to the field. This has aided me not just practically in finding informants more easily through the use of pre-established regional social networks, but also through an emotional connection and investment in local culture. Such emotions filter into ethnographic engagement, and this closeness to the phenomena can facilitate productive questions and curiosities.

Including the anthropologists’ own experiences can thus enhance validity by demonstrating how emotions influence inquiry. The use of diaries during fieldwork to reflect on how emotions affect one’s research can reveal the ‘often hidden struggles in the production of knowledge’ (Punch 2012: 87). Anthropologists have utilized evocative descriptions of personal experiences to show how they generated understandings of phenomena. Rosaldo described how he only fully understood the rage of his Ilongot informants, who severed heads to deal with grief, when he became bereaved himself. He criticized ethnography for ‘wiping away the tears and ignoring the tantrums’ in excluding the anthropologist’s own emotions, suggesting that the inclusion of personal experience can provide a vehicle to understanding (Rosaldo 1989: 162). Meanwhile, Ellis’ piece about her brother’s death makes use of emotional recall to generate cultural insights into grief and kinship. She argues for ‘feeling and participatory experience as dimensions of knowing’ (Ellis 1993: 726). The inclusion of anthropologists’ own personal experiences and emotions can thus help us to better understand the range of processes involved in knowledge production, and through this transparency validity can be enhanced.

The personal experience of my partner’s grandmother’s death thus could be reflected on to convey its impact on the research process and knowledge production. Guilt affected the decision to select the town as the primary field site, as my partner and I felt emotionally connected to the place, as if by basing the research there we had not missed her death without reason and were honouring her memory. Yet this guilt also led to deeper empathy with informants as I felt an emotional connection to the phenomena of kinship elder care. Months later, discussing kinship elder care with informant family caregivers, the notion of guilt kept emerging in conversations. Informants spoke of intense guilt about getting on with their lives and not being constantly with an elderly dependent family member. One informant described feeling intense guilt around her mother’s care, recalling how she had gone out with friends and returned home to find her elderly mother lying on the floor with a broken hip. The guilt, she said, came from prioritizing her own needs over her mother’s. Her words resonated with me as the experience of causing my partner to miss his grandmother’s death for my own academic ambitions made me feel intense guilt for placing my needs over those of my partner and his family. I felt an empathy towards informants, which led me to further scrutinize and explore my own emotions that in turn prompted new research interests.

Yet, I was very conscious of not wanting to transplant my emotions onto informants. Their guilt was absolutely not my guilt. As Beatty states, ‘we cannot rely on our own emotions for insights into the emotions of people living very different lives’ (2010: 432). In the case of my partner’s grandmother, I was not immediate family; I had only known her for five years and was never her main caregiver. My own emotional experience of her death was entirely different from that of my partner or her children. Making any comparison between my experience and theirs could be deemed invalid and unethical. However, I am not arguing that my emotions afforded me an enhanced understanding of informants; rather, my own emotional experience allowed me to uncover new lines of inquiry. My partner’s grandmother’s death, by making me participatorily experience the emotional force of guilt, led me to delve into my own guilt and ask questions about it, asking myself what it was, why it was there, and where it had come from. I questioned whether my guilt resulted from an emotional indebtedness to my partner’s family. They had welcomed me into their family, despite linguistic and cultural hurdles, and they had shown me warmth and care. By not being there and keeping their son from them when they needed emotional support, had I left this debt unrepaid? Was emotional debt something children felt in relation to their elderly parent’s care? Delamont has distinguished between using reflexivity purely autobiographically and using the self in relation to research (2009: 60). Reflecting on my own emotions led me to ask myself questions that facilitated useful lines of inquiry with informants. Outlining emotions and the steps of thinking they cause can thus enhance the transparency of knowledge production, which strengthens validity in qualitative research.
Emotional experiences shape the self we bring to the field and the questions we ask. The inclusion of the anthropologist’s emotions in ethnography can make more visible the complex processes that influence inquiry and lead to knowledge production, enhancing validity through greater transparency. Just as weather transforms landscapes – rain turns grass to mud, wind shakes leaves off trees – so our emotions affect the formation of the ethnographic ‘field’ we construct. Including emotional experiences in our research can uncover the processes at play in knowledge production, validifying ethnographic research by constructing an honest field.
References


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