Approaching, Contacting, and Grasping: An Annotated Methodology

Christopher R. Matthews[1] 🄳

Abstract

Social scientific methods are not as they should be. In this extended essay, I offer a way of framing and writing about methodological discussions that can tackle some of the problems I have been butting my head up against for some time. Building on a twin paper recently published in *Qualitative Inquiry* and extracts from an unpublished, pre-peer-reviewed manuscript, I offer commentary and explanatory annotations. The main thrust of the work is to demonstrate the utility of reconsidering the ways we think and write about methods using the interconnected notions of approaching, contacting, and grasping. Doing so should encourage us to deliver what I think are the most important features of a methodological discussion—that is, *we* should say what *we* did, why *we* did it and how. When scholars do this well, a methods section can "sing" with an *intentional and, at times, intense focus on the practical, precise, and descriptive* rather than the abstract, vague, and intangible. Stated differently, *it puts the onus on us, our processes, and our understanding of them*, while also addressing the central features that sit at the core of sound research. And this, to me at least, is how we can focus on clearly communicating *what we did when we did our social science*.

Keywords

methodologies, epistemology, qualitive research, research philosophy, neurodegeneration

Social science is not as it should be—I think we have lost contact with what *matters*. I have made this case elsewhere in relation to the moral foundations that mark out our work as important (Matthews, 2024). The claim is that various social currents that shape academia, the "business" of university, and, especially Western, life more broadly, have resulted in situations that undermine our ability to do the most meaningful work that we can. I will not relitigate those arguments, instead, I will take up a connected problem—*that social scientific methods are not as they should be*.

My focus is directed toward problems that I have been butting my head up against in terms of the delivery of qualitative research methods for two decades. This includes (a) the use of unclear, piecemeal and banal discussions about so called philosophical "paradigms." (b) Academic "word salads" delivered in place of clear methodological descriptions of what was done. (c) Poor evidence of ethics as a personal, practical, and ongoing process rather than as a bureaucratic tick boxing exercise. (d) The veneer of rationality casting a conceptual shadow over what most social scientific researchers recognize to be the partly unknowable, often intuitive, and invariably emotional process of developing human understanding. And (e) the delivery of references to "how-to guides" which do very little in terms of detailing the specific interpersonal and intellectual journeys that scholars have been on to come to their findings, claims and conclusions.

I do not offer substantiation to the rather personal and strawman feel to these claims. To add such detail would be a worthy but distinct contribution to what I am trying to achieve here, and it would distract from what I consider to be a more positive way forward. Strawman critiques can, of course, be puerile, immature and unconsidered, but they are not necessarily so. They can, and often do, speak to quite fundamental and diffuse problems, and in focusing at those underlying features important pedagogical and epistemological outcomes *can* flow. It is toward such contributions that I point as the justification for the approach I take here. As a way to scene-set, let me briefly mention my style.

I appreciate the approach I take to writing will not resonate with all readers (some might have already taken a disliking to the tone before I have got anywhere near my thesis). But I have worked hard to develop a style which is in some ways distinctly my own and is, more importantly, founded in personal, political, and epistemological reflections. While some implicit substantiation to these points does follow, I do not aim to justify myself in that regard. Rather it must suffice to say that I am working in a way

[1]Nottingham Trent University, UK

Corresponding Author:
Christopher R. Matthews, Nottingham Trent University, Nottingham NG1 5LT, UK.
Email: dr.christopher.r.matthews@gmail.com

which I hope centers me, my ways of thinking/working, and my personality at the core of the scientific knowledge I am producing. And, demonstrates the possibility of working in a way to undermine what I understand to be the *fetishisation of references*. Regarding the former point—I am simply trying to extend the well-developed tradition of accounting for and partially managing researcher positionality and influence in social science. But instead of providing you a brief and hackneyed "positionality statement," I want you to get a sense for me, and how I have developed these ways of thinking and working, as you read along.

Regarding the latter point, good academic training encourages us to add substantiation to our claims with recourse to data, logic, and/or other published work. Of course, this is right and proper, but my sense is that many scholars, peer reviewers and editors alike have sleepwalked into an unconsidered position wherein references and referencing have been given undue power. This is particularly the case in methodology sections of papers, because, and I will return to this below, what we should mainly be offering as substantiation is not reference to other's work, or approaches, but ours—that is, *what we did, how we did it and why.* I have lost track of how many methods sections I have read which were filled with references to this debate, that debate and the other, but told me little of what actually happened during the work. The words Mike Atkinson once uttered to me (during a discussion about the methods section of my Master's thesis) ring out when I read such work: "Chris, you gotta tell me something more about what *you* did, man."

So, while some readers might understand my style as unacademic, perhaps self-indulgent and overly confident, and I will certainly engage with those who make such critiques in a considered way, it is not delivered unreflectively. I am acutely aware of the thin line I am treading in relation to debates around researchers' attempts at writing "reflexively" turning into insipid, egotistical and self-congratulatory accounts with little, to no, academic merit (for great discussions around these points please see, Ryang, 2000; Whitaker & Atkinson, 2021). What I am trying to do in the style, grammar and tone of my work, is press the point around the need to have space for different ways of writing and thinking about science. At the core of such openness is not a simple acceptance of difference for differences sake, but rather, a critical assessment of the legitimacy, importance, and utility of such approaches.

Put another way, my style may sit in tension with what passes as "normal," conformative, and obedient ways of working, but that does not necessarily point to it being incorrect, academically poor, or unuseful. Assessments, that you, or other readers, might make around such points, must flow from the substance of the work rather than the method of delivery. And if the style that is adopted allows and encourages more of that substance to come forth in an accessible way, I hope you will join me in accepting, if not fully promoting, that approaches to writing such as the one developed here can be useful in terms of reframing how good social scientific and particularly, methodological discussions, are delivered. With those comments in mind, let us get to the crux of the matter.

Approaching, Contacting, and Grasping (Neurodegeneration)

I am confident that a useful way of contemplating, understanding, designing, doing, and then writing about social scientific research can be gained by considering the ways in which we approach, contact, and grasp our work. I have marked these points out in an explicitly but relatively abstract sense in a twin paper elsewhere in this journal, while what follows can be read in isolation it is designed as a companion piece to that essay (see Matthews, 2025a). I want to reduce this level of abstraction while offering a clear example of why I am so convinced of this utility. Extracts from an unpublished and pre-peer-reviewed manuscript about the experiences of family members who care(d) for someone living with neurodegeneration demonstrate how these ideas help me outline our team's methodological toils, challenges, and successes.

I wrote this work, as best as I could at the time, with regard for *only* what I consider to be the most important parts of a methodological discussion—that is, I focused on clearly communicating *what we did when we did our social science*. After each of the three sections, I offer some commentary and explanatory annotations, including where I might have gone too far and/or got it wrong. In conclusion, I will highlight some problems with the work, and give some thoughts as to what would be needed for the opportunities that might come from this approach to be fully realized. That is what lies ahead, but first, some more personal and academic context.

The Project

My career has involved, among other things, researching a sport that gave me brain damage.[1] Experiencing the reality of regular and chronic headaches, impaired vision and judgment, increases in aggression, decreases in my ability to teach, and the erosion of my short- and long-term memory, while also loving my sport and wanting so much to continue, has always stuck with me. Working through what this meant for my life, and my wife's, as you might expect, resulted in some existential concerns for us both. While we have largely tackled these, this process has left an imprint on my body, brain, life, and academic career. In the latter regard, I am compelled by a sense of moral duty to work in a direction that tackles the problems associated with sport-acquired brain injuries.[2]

Former athletes in certain sports have an increased likelihood of developing neurodegeneration (see McKee, 2020; Mckee et al., 2023, Ueda et al., 2023). The experiences of those living with, and being affected by, such diseases are not well understood, and therefore, the care and support people receive seems to be inadequate in various ways. Given my personal interest in this area, and expertise in conducting phenomenologically/experientially focused qualitative research, it seemed to offer a logical and politically important next stage for my career. Therefore, I led the development of various projects which sought to explore the experiences of spouses or adult children who had caring responsibilities for husbands/fathers who were former professional athletes. It is within such an important social space that I, and the team supporting me, was drawn to grasp something significant of people's lives and experiences of caring for someone with neurodegeneration. In doing so, we discovered powerful, emotional, and sometimes traumatic accounts.

This work noticeably mattered to the research team personally, politically, and academically. When I began the process of writing it up, I had a commitment to transparently detailing the moral and ethical work we had been doing, while also presenting important empirical findings. All of this was on my mind as I led the process of capturing our methodological plans, problems, doings, and reflections. I did this using the heuristic frame I often employ, wherein our process of approaching, contacting and grasping something of social life would be a useful way to structure a clear discussion of what we did, how we did it and why. In the following extracts, I present that work in its original, pre-peer reviewed format. (It was written on behalf of the team, this means there will be a slight shift in the pronouns I use and I must also refer to myself and colleagues by name which might feel slightly odd considering the way I have been writing thus far.)

Approaching

We were drawn to research this area following personal and political motivations to deliver social science which seeks to reduce avoidable human suffering and/or enhance human flourishing. This, we argue, following Andrew Sayer's (2012) excellent work, provides an ethical and moral foundation upon which meaningful and impactful research "that matters to people" can be produced. Such an axiological starting point guided the development of our research with community partners and led us to seek various elements of stakeholder involvement. This began with a public engagement symposium (please see Matthews et al., 2023, for a proceedings paper), out of which a research steering group was formed. Over the course of a year this group worked with the project lead Christopher Matthews to shape various funding applications. Our methodological and ethical approach springs from this process and resulted in us engaging people with various

experiences associated with caring for someone with neurodegeneration. Our reading of extant literature, and understanding of anecdotal experiences from steering group members, highlighted the emotional and physical trauma that such people can face in their daily lives, and, as such, we felt focusing our work in this direction provided an opportunity to deliver research that matters in its potential to positively impact people.

We tentatively approached the project using foreshadowing theoretical tools from a broadly social constructionist position, largely influenced by symbolic interactionist theory (Blumer, 1969). This, for Matthews, at least, meant that a process of seeking to understand how people live within, and construct the world around them, was an initial focus. As such, we paid particular attention to social relationships, because we know how important they are for understanding health, wellbeing and the ways people care for each other. From this approach we became interested in exploring caring responsibilities, the experience of living with someone with neurodegeneration and how this might relate to football. We anticipated discussions with prospective participants would be emotionally challenging, uncomfortable, and present various ethical issues. In balancing our obligations as researchers to avoid producing discomfort in our participants, with our motivations to document the painful realities upon which such experiences were based, we hoped to be given the chance to grasp something of significance about their lives at the centre of professional football.

Each author has diverse experiences both academically and personally within sport and/or brain health, and while these might be of interest for helping to situate this work, space constraints mean that we will limit ourselves to a discussion primarily of Jane Rowley's background due to her central place in recruitment, data collection and the effective delivering of the project. Rowley is a white British woman living in the West Midlands of England. She has a background in dementia support as both a manager of a local dementia charity and as a researcher. She has dual and overlapping careers as an academic and practitioner. This has resulted in praxis-orientated research with older people affected by malnutrition, ex-service personnel with traumatic injuries, and people at the end of life (see Rowley et al., 2021). Her current work with people affected by dementia offers a nuanced understanding of the potential challenges families face. These experiences were central to her ability to lead the recruitment and data collection in an efficient, sensitive and ethically robust manner. However, she has no background personally or academically in sport, or sport related brain health. The wider research team acted to fill in some of this information, if necessary, but invariably her sporting "naivety" was useful as a means of encouraging participants to speak fully about often taken-for-granted features of football community/subcultural life.

Within this section, I outlined how we approached the work. These are the foundational positions that I felt were most important for readers to understand as they sought to make

assessments about the assumptions and logics that led us to our research. In three paragraphs, I tried to provide a discussion of how we philosophically, theoretically, personally, and politically approached our attempts to know something about neurodegeneration in former athletes. They underpin our methodological choices and, as such, they are essential in contextualizing and making sense of what follows in the rest of our reflections, empirical contributions, and knowledge claims.

You may have picked up that, this section details issues that might more commonly feature in a discussion of one's philosophical "paradigm" or position. For me, philosophy of research—that is, discussions of ontology, epistemology, axiology, and the like—are central and practical features of the way I think about, do and write up my work. And I try to reflect that by writing in clear, grounded and applied ways. Those who have experience of advising, reviewing, and examining postgraduate research, will likely recognize that when research philosophy is understood well, it provides a frame from which "joined up" thinking about, and doing of, research flows. And, in that regard, such discussions tend to offer substantiation to a scholar's sound philosophical choices, and this can be "felt" when reading the work—put differently, such work, often quite intuitively, makes logical sense. These feelings can and must be rationalized and captured when critiquing research or when conducting peer review, but that should not hide the fact that many of us do indeed get a "sense" of when someone is working in ways that do or do not make sense philosophically.

So, when social science with a sound philosophy is delivered it will be tied to quite clear and practical methodological and theoretical choices that add up. When this is the case, I do not think journal articles require an explicit methodological subsection titled *Research Philosophy* or similar—hence no such section was forthcoming from me. What is more, in my experience, having such a section can lead scholars to reproduce banal, uninsightful, and incorrect statements about these topics—after all, they are expected to write, in a few hundred words, with coherence and resolution about philosophical tensions and wranglings that have rumbled on for more than a couple of millennia. In this regard, it is usual that scholars use key references in quite vague ways which do not point to a keen reading nor understanding of such work. When this is the case, such research philosophy sections serve to obfuscate, confuse, and otherwise hide what scholars actually understand and did in relation to their philosophical approaches.

So, rather than providing piecemeal references to say, "critical realism," "interpretivism," or simply leaving the statement about the influence of "symbolic interactionism" vaguely floating, I tried to lock this tentative and broad "approaching" down in specific details. This means that after reading the first three paragraphs you should understand something of why I was personally drawn to this work, what I think makes it matter, something of my intellectual proclivities which shaped the initial funding applications and research strategy, and key elements of the researcher's academic and practical background.

I note that our work is referenced lightly in comparison to what might be considered normal academic practice. This is because, where possible and appropriate, I referred to what we did and, as such, the call to other's ideas, at least in this work, becomes, logically and empirically, of less utility. In this regard, I was trying to put the teams ways of working "out there" for you, and other readers, to assess and consider as coherent. And in leading the process of writing about methods like this, the aim was to provide the practical clarity which I think is missed when scholars provide a reference to a philosophical "paradigm" without detailing how such ideas played out in specific, personal, and pragmatic terms.

This is then a simple and clear example of how a team of scholars approached their work. It is not complex, it does not hide behind unnecessary academic jargon, but it does provide a clear start point that is useful for understanding why we did what we did. Perhaps you are thinking, "big deal Chris, all that is obvious." I would not be writing this if there was no need for some scholars to be made aware of the requirement for such "obviousness" to be delivered in their work.[3] And the clarity that is at the core of my discussion should not be confused with an overly simplified analysis. Rather, years of reading, thinking, and practicing the practical dimensions of research philosophy and methodology have resulted in the coherent and considered writing above.

While I think there is much to be gained from starting a methodology section by describing our ways of "approaching" scientific projects, my work is clearly open to critique.[4] So, in my attempt to tackle problems embedded in how scholars often write about methods, you will find further problems—such is the nature of academic knowledge development. For example, as a reviewer pointed out, the last paragraph actually ends on a quite vague point. I should have provided specific details as to how the researchers "naivety" was useful or not. I have here fallen foul of my own opening critique, and it will not be the last time. In that regard, you could reject, revise, or refine parts of my argument, but I will have at least opened the possibility of thinking anew about how to jump into one's methodological reflections. Building from this, I consider how the researcher attempted to make contact with social life. You will see this flows on from our previous discussions in context and style.

Contacting

Our initial practical conversations around the ethical recruitment and delivery of the work focused on Rowley's experience as the manager of a charity. She is often approached

by researchers wanting to speak with those affected by dementia, as such, she is acutely aware that people access services for support, and this means that they may not have the inclination nor capacity to take part in interviews, especially if they are approached frequently. Keeping this in mind and starting with a suggestion from a project steering group member, Rowley cautiously attended sport-specific "memory groups" and then broadened these visits to more generic dementia groups, sharing flyers with contact details and speaking to attendees about the study.

These meetings led to conversations with people who wanted more information, and several home visits were requested from prospective participants. Whilst none of these initial visits led directly to interviews, they did aid Rowley in understanding more about the networks of support and connections that existed between groups and within this community. Following this, she spoke to cultural "insiders" who appeared to hold something akin to a "gatekeeper" role. These were people who others looked towards for guidance as to the motivations, ethics and intentions of journalists, researchers and charity campaigners/activists who appeared to frequently contact former sports persons' families. They were quite influential and could act to hinder or facilitate access to networks of friends seemingly connected via football. Once these insiders leant their support, recuitment seemed to flow more easily via word of mouth being passed around the community.

On average it took two months and two or three conversations before people were ready to participate in a formal interview. All potential participants wanted an initial chat about the work, and it was at this stage that Rowley found high levels of distress within the community. Four wives[5] became so upset as they relayed some of their experiences in an informal chat that, while Rowley maintained limited contact to ensure they could access support, she also took the decision to request another pre-interview conversation. At this point, in mutual agreement with these four participants, it was decided that it was not the right time for them to participate in a more comprehensive and recorded interview.

The distress people expressed seemed to come from extended periods of time when they had felt unable to tell anyone of the changes in their relationships with their husbands. For example, one person responded to a flyer distributed at a memory cafe; she said she was eager to share her experiences of caring for her husband up until his death two years previously. However, initial conversations led by the prospective participant, which quickly touched on spousal abuse and suicide, were taken by Rowley to be too distressing to be followed up on ethically. This was a "judgement call" that she made based on her experience in the area, the feel she got for the social situation and attempts to rationally reflect on the outcomes of such a decision. These were challenging decisions, taken with upmost care. Rowley ensured each person had access to support and has taken steps to maintain the relationship so that they can return to a discussion about the potential for conducting an interview in the future.

Six wives and four adult children of former professional footballers were interviewed—they all had care-giving roles connected to the neurodegeneration of their husband/father. At the request of the participants one interview was in person and the rest were conducted using video conferencing software. The interviews ranged from 40 to 80 minutes in length and were transcribed verbatim. All interviews were conducted by Rowley and recorded with the written and verbal consent of each participant. Key identifying features of the participants have been removed, modified or made vague, without losing their broad importance for understanding something of the person's life, and we refer to everyone using pseudonyms.

Each interview focused on various issues related to life prior to, and following, a diagnosis of neurodegeneration. While an interview schedule was produced, Rowley facilitated and encouraged the family carer in leading the conversation where possible. She started each interview with a discussion about consent and specifically how the data would be de-identified. This created an environment where the aims of the interview were clear. Each participant was first asked to talk about their life and family situation, Rowley then took social cues around what the person was comfortable discussing, while also, if necessary, asking about the circumstances that led to the diagnosis, their experience of becoming a carer, the sports career of their loved one and their thoughts on what contributed to the illness. Despite a cautious and careful approach, each participant became distressed at some stage during the interview, Rowley took time to pause, reassure the family carer and/or offer the option to reschedule. She also ensured that after the interview each person had time to talk and relax, until they were feeling calm. Later in the day, or at some other agreed upon time, she checked in with them to see how they were doing and discuss options for further support.

We initially aimed for 20 participants, this was a relatively arbitrary and aspirational number derived from our experience delivering work on other similar projects and knowledge of the limited research time available to us. In particular, Rowley was on a part-time six-month research fellow contract which meant we had around 4-months to recruit and conduct data collection—this short period was balanced by our existing connections/partnerships with various communities of practice which we knew would help with recruitment. Despite time constraints, when approaching interviewees, Rowley took a considered and "slow" approach which prioritised assuring she was confident that the family carers understood what the research involved and helped them feel as comfortable as possible. Quite early in the process, as it became apparent recruitment was very involved, we accepted that a smaller number of interviewees might need to suffice. However, alongside this pragmatic acknowledgment was also a quick realisation from Matthews and Rowley that the data was emotionally powerful. This meant two main things, 1) they were confident the findings from the small number of existing participants would provide an important academic contribution and 2) the emotionality of organising and collecting data took a personal toll on Rowley. As our academic outputs in this area

will evidence the first of these claims, we will focus on considering the second.

The emotional toll and labour involved in conducting (especially social) science has been well documented in recent decades (Bondi, 2012; Holland, 2007; Reed & Towers, 2023). And to those of us who build a career on seeking to understand people as distinctly social beings — as we try to reduce unnecessary suffering and enhance flourishing the emotional realities of research are well known. Rowley was recruited to deliver this research in part because Matthews was convinced she has the temperament, experience and personal ethics to approach this research in the considered, professional and caring ways that it requires. Her permanent job working in local dementia support means she is well accustomed to the harsh realities that can accompany living with neurodegeneration. And Matthews has discussed how his ability to collect data on such emotive and personally significant topics has been eroded by his time in the field, but that he is simultaneously compelled by these experiences to lead and support such important work (Hiemstra & Matthews, 2025). This means there exists a neat alignment between Rowley's and Matthews' capacities and personal motivations. But with all that said, the interviews and the preparatory chats and discussions needed to recruit and research ethically (and subsequent relistening during the analysis phase), became increasingly challenging for Rowley.

Of course, as a committed and experienced researcher she could have continued to recruit more participants, but as the findings were of such clear significance, Matthews decided on ethical grounds to end the data collection after the tenth interview. Alongside his motivation to shield Rowley from unnecessary discomfort he was confident that sufficient data had been produced for an important and powerful empirical account that could do justice to the participants experiences. We also felt that the work Rowley had put in represented "value for money" for the funding agency. This resulted in there being sufficient time left in the project to begin seeking the funding required to extend this work. In the latter regard, while we know that what we report from this data is empirically novel and theoretically useful in various ways, we are also very clear that this is a foundation from which we will deliver a more comprehensive scheme of work in the coming years—here, we align with Nobert Elias' discussion of social scientific knowledge as a "symptom of a beginning" (Dunning, 1992), rather than anything like the final word. So instead of reaching towards some trite comments about empirical "saturation" (see Braun & Clarke, 2021, for a critique around this idea) or simply nodding to the "limits of funding/time" to address why we spoke with "only" ten people, it is to the utility/importance of our findings and the emotional toll associated with producing them, that we point to as robust and ethical justification.

Here, I provided further insight into the researcher, who she is and how she shaped the work—the practicalities of her position—rather than the potential vagaries of a detached positionality statement. I wanted you, and other readers, to *feel* some of the difficulties we faced when doing this work; that there were ups and downs, and tough ethical/moral decisions that were sweated over as the work progressed. I hope you get a sense for how our work developed, emerged from our approach but also could not have been predicted and thus proposed to have followed this path at the outset.[6]

If this point is accepted, a simple process of gaining ethical clearance is insufficient to demark ethical practices within such emergent research methodologies. You might have noticed we did not discuss our institutional ethics procedures. Of course, we gained ethical clearance, doing funded work in the United Kingdom without engaging in that side of the research process is almost impossible. Simply put, my job would have been at risk if I had not attended to this side of the process before the research commenced. But as I argue with Reem AlHashmi, the reality of why most of us do ethical research is not connected to such a bureaucratic process, "but because we care and develop caring relationships" with our participants and those we work with closely (Matthews & AlHashmi, 2024). Given that position, I hope it is apparent from our discussions how a clear and committed ethical/moral stance and practice articulated with what we did.

You see, I care very little for you knowing that I ticked an "internal admin box" that denotes my academic institution considers our work to be ethically sound, and, as such, this section was written so you could try to feel and intellectually grasp parts of our personal and practical ethics. I hope you get a sense for the ethical caring that sits at the foundation of how I worked with the researcher and how she worked with the family carers. I hope in reading this that you are assured that we went into this work with our personal commitment to ethical practice and delivering ethical and meaningful work at the forefront of our minds. If we had referred to this process using some empty statement about "internal ethics processes were followed throughout" you would have gained next to no understanding of this vital dimension of our work.

Now, of course, I could simply add a line acknowledging that such internal processes were followed, and in response to a recent peer review I did exactly that because the reviewer pointed out, quite correctly, that such practices are still developing in certain parts of academia. While I am all for "nodding" to this process, I am not for such a nod standing in for a more developed and considered account of the personal, political, pragmatic, and iterative realities of ethics. So, I purposely did not mention our passing of ethical clearance because I wanted to "prove" our ethical "chops" in our detailing and describing of the ways we made contact with people, and worked to understand their experiences, in a moral way. This, at least to me, seems to be a good way to enable readers to make an assessment about our ethical intentions and practices.

However, in my focus on substantiating our work in this way, I did not sufficiently join up with ongoing debates about ethics. In that regard, I should have highlighted that our work was influenced in large part by feminist scholarship (see Edwards & Mauthner, 2012, and particularly the work of Nel Noddings 2013/1986). This was picked up during the review process of this paper and it points to a problem with seeking to challenge some of the norms of academic working practices. While doing so can clearly have some utility, in this case, as I try to undermine the "fetishism of references," I went too far. And in various places by me directing my attention in such a way, I undermined (a) the readers ability to more fully know more about the origins of our ways of working and, perhaps even worse, (b) I reduced opportunities for readers to go and seek out such knowledge for themselves by not providing references to an important and underused body of work. So I did not get it quite right here.

As the work was exploratory in nature and small in scale, we did not consider applying for dedicated psychological support for the participants and research team to be economically viable. However, the researcher's experiences, and those of the family carers, during the recruitment, data collection, and initial phases of analysis, highlight the importance of ensuring such support is in place in our future research endeavors. We did operate a "distress protocol" which provided information about wider sources of support for the participants, and the researcher had access to counseling through our institution's wellbeing scheme. Her challenges were discussed, and partly managed, via regular contact with the team when "debriefing" after each interview.

Of course, there is a balance here between the ability for us to gain funding to do impactful research on traumatic topics, and the need to fully finance the management of the emotional and mental health consequences which follow on from that work. We encourage colleagues who are interested in developing, especially qualitative, studies in this, and connected areas, to think deeply about the emotional toll on those involved and to strive to have clear support in place. And I led the development of this writing with an honesty that I hope shows this emotive side quite clearly if not in all its quite challenging detail.

Alongside this broad understanding of how we ethically made contact with the carers lives and experiences, you also got a sense for how we recruited people for interviews. Some of the information we have given certainly does little for informing our empirical findings, but they were included as I felt they were essential features of our research process. There is a moveable line of significance here which people might shift depending on what they find to be important in methodological discussions. My sense, however, is that we need to trust scholars to give *their* readings on what was important in *their* research process and use that to guide the discussion of important moments of *their* work.

In that regard, I do my best to reject what others try to impose on me, and those I advise, in our methodological reflections, during the peer-review process. This is because, while you and other scholarly readers might have your own personal experiences of your research, and abstract knowledge of research more broadly, you have only the practical knowledge of our research that we provide you. And as such, I want to be trusted, and I think we need to trust scholars more broadly, to give *our* account of what mattered, and why so, as we help readers understand *our* methodological contacting with the worlds we share with participants. Of course, there are parameters of good science we need to consider here, but a little more flexibility in how reviewers come to understand such a process, I argue, could go a long way in helping the transparent discussion of ethics and associated issues.

Unfortunately, I could not include our quite detailed discussions of our participants. I did this because they were quite fully weaved into our analysis sections and do not particularly work in isolation. This is unfortunate as a rich account of participants' lives is another feature that I often find to be lacking in some research, but I have to pick my battles and that is one for another day. Based on our approaching and contacting, I move next to how we grasped something of our participants' worlds.

Grasping

Our process of analysis was informed by Blumer's (1969) classic symbolic interactionist discussion of 'sensitising concepts'; Bob Prus (1996) captures our use of this idea neatly:

Blumer uses the term sensitising concepts to refer to these tentative, analytical notions. Sensitizing concepts suggest subsequent lines of inquiry and assessment, but in each case the researcher has the obligation of making the concept match up with the circumstances at hand rather than making the data fit the concept (p. 132).

Blumer then encourages scholars to ensure a thorough interaction between their foreshadowing approaches, tentative academic ideas and data (for a fuller discussion see Matthews, 2025b, especially Part One).

Following a commitment to this broad epistemological approach, our attempts to intellectually grasp something of the family carers experiences started out quite informally— Matthews and Rowley met every week to discuss each interview. From these conversations, an intellectual and emotional "feel" for important insights that Rowley was developing began to come forth. We appreciate that "feel" does not translate well in written format and is often understood in (falsely) dichotomous terms as oppositional to the rationalised approaches favoured in scientific publications (for this latter point please see Barbalet, 2001, 2002 and for a broad discussion in relation to

doing research please see Hiemstra & Matthews, 2025). But we accept and welcome that, often implicit, emotional communication and understanding is essential to most human interactions and that such ways of being were indeed central to Rowley's ethical and caring approach to understanding the lives of family carers. As such, we consider such "feel" to be part of our analysis and we work to rationalise this way of knowing into something clear, coherent and social scientific during our more formal analysis (see Matthews, 2025c).

There was fluidity to these initial attempts to understand the empirical contact Rowley had made with the family carers worlds, but as two experienced researchers we used this process to help refine subsequent interviews in terms of sharpening their academic focus. This included asking about the support the carers had access to from the broader football community, the specifics of how the former footballer's conditions developed and a commitment to capturing some of the trauma associated with their experiences. Also, these chats encouraged ongoing reflections about how Rowley's expertise might enable but also constrain her interpretations of data. This meant considering how her assumptions about dementia, although informed by literature and experience, might lead her to miss opportunities for detailing commonalities and differences that came from the family carers experiences.

Building on this quite relaxed, collegiate, and iterative process, Rowley began the more formal stage of analysis by rereading interview transcripts and listening back to the interviews. This process, and our discussions about what she felt she was finding, resulted in Rowley highlighting reoccurring ideas across the interviews and identifying where the participants spoke to important experiences, as part informed by our initial axiological and theoretical approach discussed above. But also, following Blumer, we ensured this work allowed for an openness to the lives of the family carers to come forth and shape the ways data collection and subsequent use of concepts might develop. This is then how we approached our analysis—we worked tentatively to sensitise what we felt to be important concepts in relation to our participants experiences while staying open to subsequent lines of enquiry.

In this regard, several key empirical details quickly became apparent and as such prompted further reading. These included, but were not limited to, connecting neurodegeneration to heading; a seemingly high rate of violence and aggression; issues relating to the physical fitness and strength of the former footballers; family carers health suffering; obsessive behaviours towards children; the unpredictability of dementia following diagnosis and the lack of any treatment pathway/ plan and lack of support; the financial and emotional costs associated with caring; various worries that came with the footballers 'fame'; shame and guilt about considering residential care for someone who was physically fit and relatively young; lack of support from former clubs and the footballing community; and distrust of the players union and Football Association.

Matthews and Rowley conducted a further review of literature around these and connected topics. This reading informed the refinement of our analysis and led to the grouping of data into two parts: 1) that which was insufficiently evidenced and required further consideration and/or data collection, and 2) that which we felt was robust enough to form the basis for some important initial findings. Robust in this sense is taken to mean that the data provided a relatively clear and coherent empirical basis from which important observations could be substantiated in academic publications.

Based on this latter data grouping, and in partnership with the research team Matthews and Jack Hardwicke led a process whereby a coherent analytical frame was built, around which two initial empirically focused papers could be developed. The goal here was to place the data in dialogue with existing work in the field as a means of advancing, in some small but significant way, our understanding of the outcomes of athletic labour and experiences of living with neurodegeneration. This process resulted in three broad empirical findings which could be usefully framed by, and in turn help further develop, sociological analysis, this included the family carers thoughts about the occupational origins of the former footballers neurodegeneration as an outcome of their athletic labour; the financial and emotional costs that families had to "pick up" and the failure of sporting institutions to support families despite generating vast amounts of money from former footballer's labour.

In the above, I do not describe our use of systematic content analysis, thematic analysis, grounded theory, or the like. That is because, we did not use any of those processes in a way as described in the common or garden references that people tend to point to for such work. Instead, I provide a foundational reference to Blumer's approach, as captured by Bob Prus. I did this because I believe such ideas can provide a foundation for the development of good social science and, as such, they informed how I led the analysis.

Building on that, we described in specific terms, some key and systematic elements of *our* process, that is, parts of the intellectual journey *we* went on to come to the key features of *our* empirical focus. This included some stuff we did not think we had enough data on, or clarity of thought about. I provided that information because it gives insight to what happened as we sought to pull a coherent scholarly story together—in that regard, some ideas work, others might need more time to come together, more still never make it "over the line" for one reason or another.[7] In past reviews of similar work, I have been told to simply provide generic references and say less about my process of doing, advising and leading such work. *These types of statements when offered by other scholars represent a willful attempt to reduce the clarity of methodological discussions in favor of the deployment of empty references* (see Thorpe, 1973, and Matthews, 2025c, for discussions).

It is here that we can re-join with my earlier comment about the "fetishism of references." Broadening out from my own rather personal take above toward a consideration of the areas of scholarship associated with where my students and I regularly publish our research, one of the most frequently abused "empty references" is to that of Virginia Braun and Victoria Clarke's various works on thematic analysis. I expect you might have seen the somewhat ubiquitous, and often vague, use of "Braun and Clarke" dotted around in various papers. Thankfully, the editors, and some reviewers, in important journals, have now heeded Braun and Clarke's (2019) own warnings about such unconsidered usage, and begun pushing people for more clarity in their use of such a reference.

Personally, despite considering their work to have some application, and encouraging scholars new to thinking in social scientific ways to read parts of it, I do not refer to them when discussing my process of analysis. This is because I do not think what they outline takes into consideration some of the underpinning epistemological realities of grasping something about social life, so, err, obviously, I do not cite them (see Matthews, 2025c for more on this point). However, I draw on some of their thoughts on "saturation," so, err, obviously, I did cite them for that. This way of working seems self-evident to me, but it clearly is not what passes for normal practice in some academic circles.

You see, *I know my own approach well, because I do it and reflect on that doing*. This "process of knowing," of developing "knowingness," is based on trying to objectify one's work—that is, turning *your* lens of critical thought back on what *you are* doing, and how *you are* doing it, so as to make your work an object of which *you* can try to gain knowledge. As above, and in this paper's twin (Matthews, 2025a) we must try to know what we do when we do social science. I must say, I know quite senior scholars, who lead junior academics and groups, who cannot do this themselves—this is then the analytically "blind" leading the analytically "naïve"—and it is troubling.

When scholars travel down such a path their destination can be an unthinking recreation of a "well this is what my supervisor taught me" sort of approach to academia, wherein they struggle to critically grasp what they do, and why they do it, and thus are unable to think beyond the ways they were taught. This is, then, a "recipe following" approach to methodological thinking, which can, in isolation and in the correct conditions, be of utility, but will also drain away one's ability to leverage critical thought toward the research process—and I do not think critical scholarship easily flows from uncritical methodologies. This is why I dislike "how-to guides" to analysis (see Matthews, 2025b, Ch. 11 for a discussion), and methods in general—they help people cut out the philosophical understandings that enable the coherent, dedicated and flexibility of methodological approach, design and delivery that I encourage in those who come under my influence. Part of my work in general and specifically in this essay's focus on approaching, contacting and grasping, is to help others develop knowingness of their work by reframing foundational features of the research process. In so doing we can work to make the familiar feel strange.

When a scholar drops in a "how-to guide" as the supporting reference for their analysis, without a detailed discussion of their intellectual journey, it can have a *deus ex-machina* feel to it. With little explanation or specific detailing, "the guide" was followed, and, as if by epistemological magic, our scholarly protagonists' ideas leap, neatly, coherently, with little fussing, from the empirical world. And it is also here at which the pervasive creep of theoretical determinism becomes apparent—if one goes into the research expecting to find something, perhaps the utility of Bourdieu's or Foucault's theoretical approach?—one should not be surprised if "lo and behold," again, as if by epistemological magic, such a thing is indeed found (see Matthews, 2025b, Ch. 6). When research plays out in such ways—as the symbolic and legitimizing shadow of a "how-to guide" is cast over idiosyncratic analytical, interpersonal, intellectual processes—*we readers are left with little to go on in terms of trying to understand and assess how the researchers did indeed come to grasp something of social life in specific, personal, and pragmatic ways*.

Of course, if a scholar provides a reference to a "how-to guide," with its systematic steps, diagrammatic procedures and whatnot, this might well have the alluring feel of being more "sciency." But let us not forget, at the core of what we are doing here is a distinctly human process of understanding, of interpretation, of cognitive grasping, which is tied to social, personal, and metaphysical processes we do not fully understand—that is, the manifestation of human consciousness. So, I also prefer to accept that elements of how we do interpretative analysis are, by necessity, unknowable, have intuitive and idiosyncratic dimensions, and can often reside in how we "feel" about something.

This means the description above of how we came to think what we think, is, of course, not perfect. I would love to tell you more of how it happened, of how we came to our interpretations of data, but anyone who has tried to trace their own psychological processes will know there is an ultimately impossible nature at the core of such an undertaking—simply put, one cannot fully know one's own mind. As such, following Becker (2017), I always try to honestly frame my analysis as consisting "of guesses, that seem plausible to me, and I hope you, on the basis of evidence I've provided" (p. 6).

When scholars seek to underplay the unknowableness at the core of (interpretive) social science, I do get it, I know they want to build a defensible and apparently bulletproof rationality. But, such a commendable way of working is an *epistemological fiction* which can only ever be aspirational

and, instead, in its place, all we can really do is try our best to trace what we did, how we did it, and hope that others see some utility, importance and logic in our work when taken as a whole. In that regard, one's methodological discussion of analysis is simply a starting point, which must be further explored, detailed, and substantiated within a coherent presenting and discussing of findings.

Some Problems and Opportunities

Moving to a broad conclusion—the claim I am making about these extracts from an unpublished pre-peer-reviewed manuscript is that they can help us grasp certain elements of good practice in relation to writing about our social science. To be clear, **the inference is that I think this work is better (in many ways) than that which was edited and reduced to be submitted, and that I also think what I have presented here is more transparent and honest, and because of that more rigorous, than some significant proportion of published methodological reflections in social science.**

Of course, I will be off the mark in some regards, the work most certainly needs some refining and editing (that I accept would, in part, have come via the review process), and there is excellent writing about qualitative research methodologies that is way better than mine. But, just like a stopped clock—there will be key moments when I am on the mark, and in this way, these ideas will be useful as a framework for some scholars to learn from, follow, and develop.

I happily accept that I have not done the necessary work to fully substantiate the personal critiques I level at the way some scholars write about their research methodologies. This critical foundation of the approach I am proposing, if not accepted, undermines what I have tried to do here. However, to my mind, and this is based on several years of experience working in this area, and discussions with many colleagues, the thinking I have presented here will be of utility to a variety of scholars, both junior and more senior.

We must tread with caution, because, in the same way as no findings section can paint a perfect picture of the empirical world, no method section, no matter how long or well developed, can provide a complete discussion of all aspects of the research process. This is because human understanding—that which sits at the foundation of what we write about when we try to publish our work—is reductive and finite, but the experiences, lives, and cultures that social scientists try to grasp are expansive and functionally infinite. This tension does not mean we should not aspire, to our best ability, to capture key moments of our methodological doings and reflections—that is what I have tried to provide here, in the relatively long-form structure that is afforded me.

I appreciate these extracts from our methodological reflections seem to go on a bit, and some readers will understand parts of our approach as unnecessary. You might think, for example, that I wrote at too much length about the way we approached the work, or our ethics as a practical process with some important personal "judgment" calls at its core. Maybe my discussion of flourishing and suffering as a moral starting point felt underdeveloped? And I expect my way of telling what we did, rather than providing citations to others' discussions of ways of doing things, will feel, to some readers, like I am disconnected from ongoing academic debates. Perhaps my link to Blumer's work feels strangely dated to you? There is certainly well considered critiques you could level at my work.

And, even within the extended space I have used here to outline what I believe to be a detailed account of our methodology, I have still had to cut short our discussions in various ways. For example, I did not include a full elaboration of how our work was produced in partnership with communities of practice; we presented a fairly sanitized account of how much of an emotional undertaking the research was; and the local and wider politics involved in researching this topic area have been omitted. But with all that said, I am confident that what we provided gives insights into the broad processors which were important to our philosophy, ethics, methodology, and key moments from our analysis, alongside specific insights into our work that mark it out as distinctive. And I propose them, even with their problems, as an example of good practice in developing, doing, and writing about social science.

After looking at an earlier and longer version of this admittedly quite long paper, a trusted colleague asked me, "Chris if this methods stuff is such a problem isn't the answer for academics to write a book about their research, rather than squeezing it into a paper?" I took his comment seriously and wondered, "am I wasting my time here?" The answers that came from that reflection helped me capture more of the importance of what I am trying to do. You see, publishing books is not that easy, and there is various limitations which the capitalist structure of academic publishing places on what will be published and by whom. In that regard, despite journals being open to gatekeeping, and in various ways restrictive and overly formulaic, they are far more accessible, and therefore, dare I say it, egalitarian and democratic, than submitting an idea to an academic book publisher.

Also, as I have been frustrated to be told at various times in my career, the "currency" of academia is "pounds" and "papers" (i.e., external funding and high impact factor peer-reviewed journal articles). I will not lament this further, although I certainly could, it suffices to say I think we must work to ensure that relatively long-form, rigorous, and considered qualitative research can find a home in *most* of our leading journals.

For the opportunities that I think are embedded in the approach I am advocating here to be fully realized we must

have space to let our methodological reflections "breathe." That is, attempting to "cram" our methods into 1,000 or so words simply will not do. Yet, in many social scientific journals, this is exactly what is required when we are expected to make intertwined theoretical, empirical, and methodological contributions in less than 8,000 words. There is a quite easy win for editors—provide a clear system for significantly upping word count in your journals. This must be done *judiciously* and *cautiously* because more words can lead to rambling and incoherent analysis (yes, the irony of me stating that in this 11,500-word paper is not lost on me).

So, some basic process for "applying" for more words, if the analysis warrants it, is needed. For example, if a scholar can show the need for extended ethnographic extracts, field notes or a long-form to-and-fro from an interview, and they also need to detail a complex discussion of their methods, I expect they will easily pass 10,000 words. Of course, there are journals, such as this one, that take such work already. But I think this should be the standard not the exception and the ability to extend beyond the recommended limits should be clearly communicated to authors and be considered as part of the review process. If we are to tackle the problems we face in methodological discussions in social science, the first thing we need is the space to engage in that process. So, this upping of word count seems like an obvious and necessary first step you can take asap.[8]

Another way forward was suggested by one of the reviewers. That is to embrace the possibilities of "open science" by having a long-form methodological discussion hosted online. While I can see potential here, and I am advising others to do a variation of just that, I am wary of some colleagues (unconsidered) attempts to provide an audit trail for such work, as I am concerned about the ways such ideas might fall into an overly rationalized discussion of what is often a non-rational and irrational process of knowledge production (*pace* de Kleijn and Leeuwen, 2018). But, with that said, if designed and delivered from epistemologically rigorous foundations, I can certainly see a merit in documenting study design and development (approaching), recruitment, ethical (contacting) and analytical (grasping) processes outside of a more streamlined methodological discussion in a published paper. If this was to be the case we would need to normalize such efforts as a part of the review process so as to not let them turn into a "dumping ground" for unconsidered, underdeveloped, and/or overly rationalized methodological reflections.

Finally, let me lock in my thesis. If we are able to interrogate, understand, and then articulate how we have approached, contacted, and grasped, we will usually be in a good place to know what we have done when we have done our social science. These quite broad and underpinning terms can lead people to think in broad and underpinning ways. And this should encourage us to deliver what I think are the most important features of a methodological discussion—that is, *we* should say what *we* did, why *we* did it and how.

When scholars do this well a methods section can "sing" with an *intentional and, at times, intense focus on the practical, precise and descriptive*, rather than the abstract, vague, and intangible. Here, the underpinning and flexible nature of the framing of social science I have outlined provides (a) space for the specifics of how scholars actually developed and did their research to come to the fore and (b) requires us to not rest on the recreation of "normal," common, or trendy approaches adopted by others in their methods sections. Stated differently, *it puts the onus on us, our processes, and our grasping of them*. And this, to me at least, is how we can focus on clearly communicating *what we did when we did our social science*.

ORCID iD

Christopher R. Matthews [iD] https://orcid.org/0000-0001-8561-2863

Notes

1. I use the term "brain damage" as opposed to "concussion," or terms that are often using in sports subcultures like "buzzed," "rocked," "wobbled," and so on, because I want to highlight

the status of the *damage* that is done to *brains*. Such lay terms have a greater potential to be used to symbolically minimize brain injuries, even the sort of relatively run-of-the-mill short/medium term ones that I experienced (see Matthews, 2021, 2025b, ch. 10 for more work around these points).

2. See Hiemstra and Matthews (2025) for long-form discussion about how this process relates to emotional experiences that often sit at the axiological foundation of meaningful scholarship.

3. I could provide many citations to research that that does exactly what I am critiquing here, but I think such work is so common, that I would be "picking on" certain scholars, some of whom I call my friends, if I did that. Please, if you doubt what I am arguing, go back over the big pile of papers you have downloaded but not had time to read and check some methods sections and I expect you will see examples. See Matthews (2025c) where I make a similar contention but provide some limited details of where I find these problems to be more prevalent.

4. Remember, this is an extract from a pre-peer-reviewed manuscript which of course means it's not had the benefit of being picked at by the dreaded "reviewer two"—the reviewers of this paper did look at these sections, but as they are presented as pre-peer-reviewed any problems they point to have been retained and then discussed as part of the analysis. The point is that while I present this work in its early development on purpose and with good reason, of course, that also means there will be places it could benefit from refining and I am happy to be critiqued in that regard, especially where I fall foul of my own critical comments in the opening of the paper.

5. When appropriate in what follows we refer to these women as wives rather than "partners" or "spouses," this denotes how they identified their relationship status, and overrides the terms we might have chosen to use ourselves which were less embedded in traditional gendered language.

6. Often when reading methods sections in social science, the work seems to unfold as if it was as simple as A–B–C, but I have certainly never been involved in such a project and I want, especially junior, scholars to understand that reality. The roadblocks, the mistakes, and the apparent "failures" we all face on the way to doing good social science are an essential, but often hidden, or at least minimized, parts of our research processes. Please see Kahryn Hughes and colleagues' forthcoming text on this topic; *Tales of the Unexpected: Learning from Doing Social Sciences Research.*

7. Of course, I could write a few more thousand words in that direction as we dwelled, reconsidered, confused, stressed, and strained, over this process in our weekly meetings and beyond. This would present the honest discussion of analysis I am calling for here, but I would also be at risk of distraction from the matter at hand in a method section, that is, the description and consideration of what was done, how and why, in order for the reader to be able to assess the findings and argument that *are* presented, not the ones that did not make the final cut. Clearly, balance is needed.

8. A fair counter point came from Brett Smith, in a personal communication: "The word count points are important. As a former editor the reality is that you are given 'X' number of words per year (when I was editing that was 100000) by the publisher. That meant every issue had a maximum number of words, and if you exceeded the total amount at the end of the year you as the editor were fined! This is why, at least in the first submission, word count is tight. I considered moving to 12000 words but that would have meant losing approximately 5 papers per year. At the time we made the judgment—with advice—that it was best to have a few more papers rather than increase the word count. The assumption was that if you give people 12000 words, they will use it—we did ask many people and, while a few disagreed, most agreed with this assumption. Maybe times have changed? The other issue is that, as I say, word limits apply at the first submission. Often in a revise and resubmit in multiple journals I've been involved with they give you an extra 1000 words. Not the full amount you are asking for but better than 8000. Of course, I cannot speak for other Editors. Practices may have changed also. Maybe it's time for a discussion."

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Author Biography

Christopher R. Matthews, is a social scientist and epistemologist with years of experience advising doctoral candidates. His latest sole-authored book, *Doing Good Social Science*, was published by Routledge in April 2025. He co-edited *Teaching with Sociological Imagination in Higher Education* (Springer) and *Global Perspectives on Women in Combat Sports* (Palgrave Macmillan). He is currently leading the editorial team for *The Routledge Handbook of Sport, Concussion and Brain Injuries*, while also preparing the manuscript for his next book *The Act of Social Theory*. More information about his various contributions to academia are available here: https://www.immersiveresearch.co.uk/