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A Relatively Rational Analysis of Interpretative Analysis

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Abstract

This essay takes aim at normal academic practice in interpretative analysis. Specifically, the ways methodological discussions are often stilted, wooden and don't sufficiently attempt to trace the intellectual journeys which scholars have traveled to arrive at their understandings. Based on this I consider some of the epistemological under-laboring that sits at the foundation of social scientific analysis of much qualitative data. With a sound understanding of what it means to understand, we can grasp something of the "rational shadow" that is often cast over the realities of human knowledge production. That is because, while we scholars might organize our time so that we can have a moment when we are "doing our analysis," such a moment, when understood as a bounded, linear and rational event or series of progressive steps, *is an epistemological fiction.* It is, then, more rational to acknowledge the diffuse, tacit, and non-rational features of human understanding, and build methodological and analytic strategies which lean into these realities. And, in that regard, we can provide a more rational analysis of our relatively rational analysis.

Keywords

interpretative analysis, research philosophy, understanding, epistemology, knowledge

There appears to be a misunderstanding or misrepresentation at the core of how some scholars frame their ability to understand. That is, attempts to rationally know a

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thing—to "grasp" something of it intellectually using a logical explanation supported by evidence and an appropriate level of epistemological doubt—while essential to the academic endeavor can lead some to ignore how doing such a thing is fundamentally impossible in a pure sense. This is because although we humans can claim to be the only known animals capable of rational thought, our lives, experiences and understandings of the world are still importantly shaped by our pre-reflective existence (MacIntyre, 2009). In this sense, while we can point to relatively detached processes of knowledge development as a distinctly human enterprise, we must also point to our fundamental inability to know outside of our nature as intuitive and feeling beings who often tacitly navigate our ways through the world.[1]

And while all scholars must try to detail the relatively systematic, orderly and logical foundations of their explanations and reason, doing so to the exclusion of non-rational, or as of yet not rationalized, intuitions, "gut feelings" and assumptions, actually undermines the rationality of one's analysis. What is more, implicitly framing methods of analysis, knowledge production and empirical claims as fully knowable, rides roughshod over the pre-reflective, perhaps unknowable, elements of human social life. And, therefore, that which isn't currently, and maybe can't be, intellectually grasped, won't even be known as something unknown.

Building from these statements, this essay springs from a collegial disagreement about how to approach the process of doing various forms of interpretative social scientific analysis of qualitative data and my ongoing disappointments with what often passes for "normal academic practice" when writing about methods. In a broad sense, I'll make the claim that there is something of a fiction at the core of how some scholars tend to think and/or write about how they have come to understand and interpret data. That is, while the ways in which someone has come to intellectually "grasp" their empirical findings are discussed as inherently or implicitly coherent, rational, linear, bounded and unproblematically systematic, such apparent clarity acts to cast a "rational shadow" over the more or less non-rational or irrational[2] features of such a process.

And, if my arguments are accepted, at least four things should follow. First, some scholars will be encouraged to think anew about what it means to know something about something. Second, the non-rational realities at the core of doing interpretative analysis should be considered as leading to, flowing across, and continuing after, the research process. Third, an approach to analysis which aspires to be systematic should, of course, be developed, while acknowledging the inherent impossibility of implementing such a thing in any simple sense. And, fourth, the key features of this process, including those that don't neatly fit a simplified system, should be described and analyzed in methodology sections of papers with reference to the specifics of what happened in honest and practical terms.

While this essay contributes in clear ways to these points, it is in my concluding arguments that I highlight some specifics of what should be done to tackle the problems which have drawn me to develop this work. I outline these problems in the following section in order to partially substantiate my position. This is done by offering something of a caricature of what stands as normal academic practice in some interpretative social

science. A more rational and systematic approach to justifying my critiques would be a worthy exercise, but it would detract from what I think is a more positive endeavor. That is, outlining in relatively accessible terms how I think scholars should understand understanding and interpretative analysis and, from this, how they should go about practically using that knowledge in their future efforts in thinking about and writing up their social science.

In that respect, this essay should be thought of as doing some of the *epistemological under-laboring* that sits at the foundation of specific academic approaches, research methods and forms of analysis in interpretative social science. As such, its focus is general in various places, this is precisely because much of the ideas I explore are general in nature.[3]

A Caricature of Normal Discussions of Interpretative Analysis

Tim Rapley provides a jumping off point for this essay. He tells us that forms of qualitative data analysis can't:

> … be adequately summed up by using a *neat tag*. They can also never be summed up by a list of specific steps or procedures that have been undertaken. Above all, you need to develop a working, hands-on, empirical, tacit knowledge of analysis. This should enable you to develop what I can only think to call, 'a qualitative analytical attitude'. (2003, 274, my emphasis)

From my reading of qualitative research which draws on forms of interpretative analysis (some details below), I can't be sure that such an attitude is indeed developed in many, perhaps most, scholars.

In that regard, I think it is quite common to read a methods section in a qualitative social science paper, which employs a systematic approach to interpretative analysis, and find a relatively underdeveloped discussion of the actual process that was undertaken. This means that scholars who are disposed, through reading or experience, to understand the underlying features of qualitative research, or specifically for this essay, elements of interpretative analysis, are often left unsatisfied by the lack of detail, clarity and transparency of methodological reflections.

While I understand this to be a quite general problem, it appears to be an issue in the following ways:

1. In academic disciplines which straddle natural and social sciences. In such instances it can be quite common for scholars to be "trying out" qualitative research without being well versed in the epistemological foundations of such work. This might be seen when a psychologist, for example, who was trained to do research using experimental/predictive study designs develops their initial attempts at interpretative analysis.
2. In subdisciplines that function as something of an academic silo within which mainstream disciplinary or general methodological debates (such as those which

take place in this journal) can be more or less ignored. Despite some examples of rigorous scholarship, "coaching science" is an example of a pocket of academia where it's quite common for confused and underdeveloped methods to be published. In this instance, the issue is compounded due to the multidisciplinary nature of the topic which means scholars look toward various disciplines, and therefore various philosophical foundations, for inspiration/leadership.

3. In journals across the sciences that appear to welcome qualitative research, and with it various forms of interpretative analysis, but which are largely set up mimicking a quantitative approach to writing about methods with overly restrictive formatting, structuring and word count expectations. The giveaway here is usually that the editors insist on a limit of 5000 words.
4. When the deployment of a specific and codified version of interpretative analysis such as framework analysis (Ritchie et al., 2003), thematic analysis (Braun & Clarke, 2019), interpretative phenomenological analysis (Smith & Osborn, 2008) or constructivist grounded theory (Charmaz, 2006),[4] which are favored in social scientific disciplines such as, for example, criminology, psychology, social psychology, or sociology, happens in an overly reductive and piecemeal way.[5]

I appreciate these statements are quite broad, but they are necessarily so because I am confident these issues, while playing out in specific ways in pockets of academia, are also quite diffused across almost all the social sciences.

It appears, then, that something of a "shorthand" approach to explicating forms of interpretative analysis has taken hold in some academic social sciences. Based on my reading across various disciplines, despite some important exceptions,[6] I think the ubiquity of such a way of working is relatively self-evident. Specifically, what is quite commonplace is some combination of the following:

1. The "empty" deployment of a taken-for-granted reference (Thorpe, 1973) to a form of analysis followed quickly by a statement about the empirical findings that logically flowed from that process.[7]
2. The use of such a reference, but with some details about how that process played out in an unproblematic way, perhaps covering a paragraph or two.
3. An unproblematic discussion of such an approach but detailing how different scholars took different roles within what appears to be a smoothly running process.
4. A more involved discussion which shows elements of the process were challenging, but a broadly linear, bounded and rational discussion is maintained.
5. A scholar outlines some of the theoretical and/or axiological[8] starting points from which they approached their work in perhaps a "positionality" or "researcher reflexivity" statement, but little discussion follows as to how this process might have concealed more than it revealed across the practical delivery of the work.

And an associated thing which I find seldom happens:

6. Scholars will rarely detail how they worked to take feelings and intuitions they had about their participants/research setting and came to rationalize them into a more formal analysis. Such often happens via engaging in a dialogue with colleagues, during moments of personal reflection, the further collection of data and/or the peer review process, but are not discussed formally as a part of the intellectual journey scholars have taken.

One of the reasons I can so confidently claim that such ways of working are relatively normal, is because not only have I seen this on countless occasions in other scholars' work, but I have also done elements of these points when I have written my papers—despite knowing I was being overly reductive I've had to swallow this bitter "intellectual pill" on my way to developing a profile as a scholar.[9]

As my ways of working, teaching and advising have improved, I've butted my head against sometimes quite senior academics who are unwilling to see the point in me "swimming against the academic tide." This has involved editors, reviewers and colleagues encouraging me to reduce my methodological discussions by simply including a reference to a form of analysis (something similar to points one and two above—see Matthews & Channon, 2016, for an example of where this happened). Reasons for this have been 1) to cut back on the number of words that have been used in a paper, 2) to avoid confusing readers and reviewers by providing details of the complexity of the analysis, and 3) to not undermine the knowledge claims that followed in the findings. Let me briefly unpick these justifications in turn.

Working to reduce word count is often an important academic exercise. It can help scholars "trim the fat" from their analysis on the way to producing a more streamlined, coherent and impactful piece of writing. Many academic papers are overly "wordy" and are thus in need of being chopped at by the "academic axe" (yes I appreciate the irony of me saying that in this very long essay). But shorter doesn't necessarily mean better nor more scientifically valid. Indeed, especially in the sort of work I've developed and enjoy reading which makes intertwined theoretical, methodological and empirical claims, based on quite an involved approach to producing qualitative data, the limit of 8,000 words which is common in social scientific journals can be very restrictive even to an analysis which is concise. When this happens our ability to develop the best work we can is hindered by the need for relative brevity. And at this point, rather than limited word counts helping the scientific endeavor, they are actively hindering it. This is especially the case when valuable methodological details are edited out, or even worse, when such details are increasingly normalized as being superfluous to a rigorous discussion of social scientific methodology.

Seeking to avoid confusing readers is also important. Clarity and precision in writing are foundational to the development and delivery of all good (social) scientific knowledge. But such an approach should not be conflated with infantilizing those who might be drawn to our work. In fact, writers of science can reasonably expect their

readers to be relatively informed and capable of doing some intellectual work while reading. That is because some of the ideas we use and the things we talk about are quite complex, and as such, despite our best efforts, some of our writing must also share the same qualities. This is certainly the case in how scholars can be thought to arrive at their interpretations during the process of analysis—more on this below. Of course, it is possible to describe a piece-meal and shallow account of such things (Rapley's "neat tag"), but that would conceal the more complex phenomenological, existential and epistemological nature of how such processes unfold. So, I think, we must, as writers and readers, accept the need to depict some/much of that complexity in order for a fuller description of our work to be grasped.

The fact that interpretative social science relies on human consciousness, which is not fully understood, nor perhaps ever fully understandable, can be thought to undermine knowledge claims that flow from such work (of course, all articulable knowledge, including that connected to the natural sciences falls under this critique). This often happens when qualitative social science is held to the (impossible) epistemological standards of experimental, statistical and predictive natural sciences.[10] Once we embrace the fact that all reflectively conscious human knowledge has processes of interpretation at its core—the "universality of hermeneutics"[11]—and that such an endeavor must also maintain space for distinctly human foibles, weaknesses, lacks, and problems, it becomes logical to outline, as best we can, features of such knowledge that make it contingent, fallible and partial. And in attempting to do this, rather than undermining our claims, we're more capable of accounting for problems in our analysis, and, therefore, we become better situated to present bold accounts of what we can/do know, acknowledge the limits of our research and suggest future avenues for developmental work.

I appreciate why people are drawn to the above (false) justifications for reducing analysis into more "manageable" discussions. They are connected to traveling down the "path of least resistance" toward getting a paper published, satisfying a reviewer and/or just getting on with detailing important empirical findings. But, they don't represent valid reasons if one's aim is to produce the best, clearest, and most rigorous account of the social science that has been conducted. As someone whose job it is to do, and support the doing, of just that, I can't accept them as a feature of normal academic practice. And, therefore, a different way of framing how scholars discuss our methodological business is required. As a step toward figuring out what should be done, a bit of "ground clearing" is useful, so I will consider what it means to understand something about something. Here, I'm outlining foundational epistemological processes that under-labor specific methods of data collection and forms of analysis.

Understanding Understanding

At the core of all human knowledge is a quite complex process of understanding.[12] Briefly, we can split the knowledge that we have of a thing into two intertwined and overlapping components, 1) that which implicitly helps us move through life

effectively without us necessarily being able to explain it and 2) that which can be articulated in more or less explicit and accurate ways. The former is most usually prior to the latter—that is, our tacit understanding of something is developed before our ability to reflect on it.

This is a central take away from Micheal Polanyi's classic account in *The Tacit Dimension* (1967). Tacit knowledge is the stuff we use mostly in daily life as we navigate interactions, chores, driving, shopping, reading, writing and the like. When we "unthinkingly" react to a given situation with tact and skill, that shows our ability to understand and deliver practical behaviors in ways that we have learnt and embodied over time. There is a "practical reason" to such phenomena—that is, they may be effective, but they happened for currently unknown and potentially unknowable *reasons*.

If you want to understand something of this, next time you're driving, try to tell someone how you know when to change gear, how hard to push the clutch pedal down, and how that pedal requires different force to the accelerator. These are things that many of us know well and can *sense* and *feel* in our daily interactions, but they also more usually remain outside of our explicit intellectual grasp unless you're a driving instructor. And, of course, instructors will tell you that learning to drive can't be done in some purely rational and explicable manner, one must get on with doing the thing[13]—practicing driving and developing the feel for it—while under the supervision of someone who can correct you if you mess up.

What I think is often missed when developing social science scholars consider their methods and analysis is how the vast, and I mean *vast*, majority of life, is lived in a tacit, practical and pre-reflective mode of being. Think, for example, how many times you've asked yourself "why did I do that, like that?" in the last month. Unless you live with chronic self-doubt, it will not have been very often. And if you do start to ask such questions regularly you will find yourself quickly advancing toward a state of "paralysis by analysis"—that is, you'll think so much about why you're doing things the ways you are, that you might never do the things you need to do in the first place. Human social life simply could not function if it was largely based on such a reflective approach. Our humanity, after all, is largely to be found in our doing of acts, not the act of thinking about such acts.[14]

We can try to turn our attention to tacit knowledge by making it the focus of conscious reflections—that is, we can dwell on/in it and by doing so get to know something of it (Polanyi, 1967). Here, questions and statements that try to make explicit what is implicit are the key—"why do you pronounce that word like that, do you think?" "That's an odd way to eat, how did you learn that?" "You shouldn't behave like that here, consider your actions in future." Notice how the goal here is to "jolt" the person who is being questioned out of their unconsidered and comfortable ways of doing social life (see footnote 14 re breaching in ethnomethodology). At that point, their previously unthought mode of being, in which they had been "in," is brought into conscious light. And it is when that process is successful that reflection and consideration can commence.

It's important to grasp here that tacit knowledge might help us navigate through the world effectively—that is, be useful, skilled, refined, and practical—but unless we can bring it into our conscious awareness, we can't even begin the process of trying to rationalize it in ways we can express to others. This is the foundation of Polanyi's classic refrain *that we know more than we can tell.* And, if such ways of knowing remain outside of our conscious reflection they also remain outside of many of our rationalized, social scientific methods of empirically contacting and intellectually grasping something.

This discussion of the tacit dimensions of life should help us recognize the limited nature of our attempts to know something of the social worlds we share with others. Van Manen discusses this in relation to the "mysterious" features of life that although unknowable in their entirety actually call forth our attempts to understand. That is, "human life needs knowledge, reflection, and thought to make it knowable to itself, including its complex and ultimately mysterious nature." And the key here is that when we do try to grasp something of life we can't hope to know the fullness of such experiences "with a concept of rationality that is restricted to a formal intellectualized interpretation of human reason" (1990, 17). Or, as I suggest above, seeking rationality to the exclusion of non-rational, or as of yet not rationalized, intuitions, "gut feelings" and assumptions, actually undermines the rationality of one's analysis.

In bringing forth our tacit knowledge, into something that can be more or less considered, we can do more than simply try to know it. We can use that information to speak and think back to our tacit ways of being. As Polanyi argues in another text, "articulation does not merely make us better informed [about our tacit knowledge]: it enriches us more by increasing our mental power over the given piece of information" (1959, 24). Therefore, in reflecting on "why I said that?" to someone, I might find that I'm acting in an unthinking way which recreates the behaviors I was socialized into as a child. I might have made a statement that is outdated in contemporary social relations—for example, people brought up in the 80s and 90s will remember how prevalent casual homophobic language was. And those who embodied this language will have had this pointed out to them and had to work consciously to change their unconsidered ways of speaking. So, while tacit knowledge is prior to reflective and articulated knowledge, our nature as beings who can consider things in relatively detached ways means we can work to change how we unthinkingly go through the world.[15]

These unarticulated dimensions of our knowledge pose a problem for scholars seeking to do forms of interpretative analysis—how might we go about interpreting someone's interpretation of a thing that they haven't yet tried to interpret? Of course, not all social science operates at the level of exploring such phenomena, but while unthought and unarticulated ways of knowing are left unacknowledged and unconsidered, especially in projects that might reasonably be expected to be empirically contacting something of them, there is a clear methodological issue.

What it means to reflectively grasp something about a material, phenomena, event, person, thought, one's self or whatever, requires not only what might be thought of as a "discrete moment of conception"—like realizing someone is walking toward you with

a dog—but also years of socialization into social worlds of language, ideas, and interactions which enable such thoughts to be thought in the first place. Without the linguistic term "dog," nor the idea of a "four-legged friend"—which can both be thought of as examples of socially produced stocks of knowledge that existed prior to us being born—no such "discrete" moment is possible. Understanding is then not a single, "internal," static feature of "the mind," but rather, an emergent phenomenon which builds on history, culture, and various intertwined social, psychological, and biological processes.[16]

There is then "no knowledge from nowhere."[17] That is, all ways of understanding are situated, contextualized, and fundamentally connected to history[18]—that which has preceded us—and our personal place in the world—that which has been central to producing "us." Understanding is then processual and developmental in at least two important ways. 1) All knowledge builds in dialogue and/or disagreement with that which has already been known/established/disproven. 2) Each person who seeks to know something of social life, goes on an "epistemological journey" within which they establish their own understanding from those pre-existing ideas.

Within that personal process there is room for creativity and the production of more or less novel ways of thinking, but such agentic "impulses" emerge from the intellectual resources one has access to in the first place. For example, the ability to think critically about one's thoughts, is a process which is in part learnt as people are educated about the ability to do such a thing. There is then no place for "purely" subjective and novel thought, and, as such, even "genius" must be grasped as something that emerges from "the social."[19]

All understanding must, by definition, bear something of the hallmarks of its emergence as a social phenomenon. For example, I, as someone who was brought up by English parents and schooled in the English "comprehensive" system reached adulthood with no second language. As such, all my thoughts, all my theorizing, all my attempts to understand the world are inherently enabled and constrained by that language. If there isn't currently a way of conceptualizing an idea in English, I won't have thought it. And when I do think, I do so in a grammatical arrangement which is distinctly English. When I read social theory, I do so in that language, and this affects how I understand those ideas and how I can then use them to understand the world around me. In this respect, I don't simply think through English as a passive vehicle, rather, my thoughts and "mind" are formed and reside *in* my knowledge and ability to use that language. Or as George Herbert Mead reminds us in his classic text *Mind, Self and Society*, out of language emerges the "field of mind" (2015 [1934], 133).

This is not some purely linguistic phenomenon of course, because my schooling and upbringing shaped the ideas I had access to and the intellectual endeavors I pursued as interesting. In that regard, I did much more sport than reading and listening to teachers/parents when I was young. As such, I didn't start reading about social theory, research philosophy or methodology until I was in my late 20s (perhaps that's telling in my analysis☺). Of course, we can all point to similar and different personal processes which sit at the foundation of how we think about things. The point here is to establish a

foundational principle—any time we do indeed understand a thing, the outcome of that epistemological process is *inherently unbounded*. That is, a moment of intellectually grasping an idea and applying it to the worlds we share with others, is just that, *a moment*, *within an otherwise historical*, *cultural*, *shifting and ongoing process that we (re)produce within social interactions.*

What is more, this process, although I have above traced two elements of it in relation to my life, and I'm sure you can do similar for yourself, is fundamentally unknowable in its entirety—van Manen's "mysteriousness." To more fully grasp how we come to understand would require us to have a complete record of how, and in what ways, we were socialized into language and ideas, alongside all the interactions where we "sorted" those words, sentences and meanings into things that make sense by using them in practical situations. So, how I come to understand can be partially understood by considering the constraining and enabling features of English, the forms of knowledge I was taught at home and school, and then after that by appreciating parts of my higher education, for example, what degree I studied, who supervised my PhD, and the books I have read since then. But, such a tracing of the development of my thought could never be complete nor used in a confident nor determinative way to predict what I might think next.

To do such a tracing of one's ways of knowing also requires us to think about our thought using the thoughts we have access to (and, of course, we do not fully understand what conscious thought is). There is an inherent impossibility here in that the act of thinking about thought necessarily requires one to shift outside of the thought of which one is trying to think. The question "why did I think that?" is a rational attempt to intellectually grasp something of a thought that is now passed and can only be accessed by an interpretation of "that which once was." And, as above, generating such an understanding requires us to draw on pre-existing stocks of knowledge we have been socialized into overtime. An infinite regression flows from this process, because to think of the origins of that thought, requires us to think of the origins of the thought just thought and so on and so forth (even writing and reading these sentences requires a flexibility of thought that is difficult to hold on to, let alone attempt to do in practice).

To be clear, I am not calling for scholars to engage in trying to deliver an ultimately detailed rendition of their own biography and the social, historical relations that are formative to their ways of thinking. Of course, some of this sort of detail can be usefully tied to the foundational ways we think when we approach and try to understand the world, but much would be superfluous and distracting. The point here is to highlight both the futility of producing such a thing in a *perfect* sense, but also how these features of our social nature are fundamentally tied to the ways we do and think about our research. In that respect, we need to acknowledge and then *reasonably* interrogate our ways of understanding—providing more and more descriptive details will not result in more and more utility. Therefore, there must be a justifiable end point which scholars, advisors, editors, and reviewers must define and defend based on the parameters of the project under question.

There is then something within the way I, you, we, and all other human beings, understand things that can't be rationally understood. There is an inherent irrationality and non-rationality to the process of knowing. You feel this when you make statements like "I just know...," "I feel like...," and/or "I have a sense that...." And when someone asks you to substantiate these intuitions the challenges that invariably flow marks out parts of the impossibleness I'm pointing to. When answers are provided, it's likely that a post-hoc analysis that is "good enough" will follow—"oh, well, erm, I think that's probably something my mum taught me." This is largely fine for going about the daily business of social life, but when it comes to developing rigorous social scientific claims based on qualitative methods, it is clearly an issue which must be considered in more detail and from a very different perspective. And this is why scholars have produced *relatively* systematic ways of framing the process of developing our analysis.

Understanding Interpretative Social Scientific Understanding

It is right and proper that we develop coherent attempts to systematize human understanding as a means of marking out key parts of how we come to make interpretations of data—this is one of the ways we put the "scientific" into interpretative social scientific knowledge and thereby make it a distinctive form of knowing. Indeed, elsewhere, I have adamantly pushed back on scholars who claim that the process of doing social science "is messy" (Matthews, 2025). This is a phrase that in some pockets of academia has taken hold. The reality is that while all forms of interpretative analysis certainly don't, nor can't, follow the systematic processes that can be conducted in predictive and experimental science, most social scientific scholars do, or must try to, produce some relatively systematic approach, which we can describe and justify to those who assess, critique, and try to understand our work. Without such efforts we wouldn't be able to document our methodological choices as rational in anything resembling a coherent fashion.

Many scholars have done the necessary job of discussing various relatively systematic approaches to doing interpretative analysis. And when one reads the common-or-garden discussions of say content analysis, basic qualitative data coding, thematic analysis, interpretative phenomenological analysis, grounded theory and the like, the authors are invariably careful to avoid false epistemological statements that undermine the realities of human understanding that I have discussed above—statements such as "do my approach and you'll produce unproblematic knowledge" are wisely not offered. But as I have argued, when interpretative analysis is discussed in methodology sections of papers in ways that reduce the process to a mere reference, or a shallow, unproblematic account, the complexity, partiality and problems that are inherent in the production of knowledge are drained away. There is then a difference between the ways that academics who develop common-or-garden systematic approaches to interpretations of data frame them and the ways in which some scholars discuss these ideas in their methodology sections.

So, What Does Happen When Someone Does Interpretative Analysis?

There are at least five *intellectually distinct* but fundamentally intertwined and overlapping "moments" that I think must be accepted as central to *all* forms of interpretative analysis. And I also think it's only common for one of them to be discussed partially well during many methodology sections. These can be understood as our *approach*, *pre-analysis*, *a formal "block" of analysis*, *post-analysis analysis*, and *ongoing analysis*. It might feel like there is a coherent unfolding process in how I discuss these in turn below. But, this is simply an artefact of how I must proceed during a clear articulation of such ideas, so, please do try to avoid considering this in anything like a bounded or clearly defined series of "stages." Doing that would be part of the problem I'm seeking to tackle here, and I will expand on this comment below.

Approach

Our "approach"—"the ways in which we approach our attempts to understand something" is more accurate—can be considered as the collection of social processes that sit at the core of how we are drawn to develop, design and do our studies. There is no discernible starting point to the intellectual work that is done before we enter "the field" or our moments of analysis. And, as above, our approach, like all ways of knowing, emerges from the historical processes that produced stocks of knowledge that existed before we were born and which we have learnt as we've matured. As such, *fully* tracing the influence of how we approach the topics we want to explore, the questions we ask, and even our motivation to be researchers in the first place, is an important, aspirational, but ultimately, impossible task. It is important because by engaging in such a process we're able to reflect on *some* elements of social life, and our development as scholars, that are connected to our interpretations and understandings.

This inherently historical, cultural, personal, and practical process is shaped by our development as agents who make creative, non-rational and irrational decisions about what we find interesting, desirable and concerning. This means that when we seek to understand such dimensions of academic understanding—which I argue we must (Matthews, 2025)—we will need to make a rationalized and knowingly reductive judgment call as to what we consider to be the most valuable features of the process.

Often our approach will be connected to existing empirical and theoretical work and social problems in our field that act to foreshadow what we might expect to find, while also marking out what areas of social life require further analysis.[20] There will be distinctly personal issues here which might be connected to the theory we're aware of, or are (overly)comfortable using, and which therefore often frames our thoughts. This is also connected to the values we bring to our research which frame the sorts of questions we are drawn to ask and how we might seek to answer them.

Exploring and outlining these features of our work are a necessary part of providing our readers with an honest, relatively transparent and clear understanding of our work. To my mind, many scholars do not put enough time into tracing, reflecting on and

outlining such foundational parts of their analysis. If we accept that it is us—not the relatively systematic methodological process we outline—that *does* our interpreting, we must also accept the importance of doing as much as possible to explore this essential part of "us" in our research. While also acknowledging that some, perhaps quite significant, parts of such a process must remain beyond our ability to know them—there are known and unknown unknowns that undermine our ability to trace how and in what ways we approach a topic, study, or phenomenon.

Pre-analysis

"Pre-analysis" can be considered as our thoughts and feelings about our data collection. They emerge from our socialization into ideas and ways of approaching our work. Think of how, even within our first interview within a new project, we are *always already* doing analysis—as someone answers a question my pre-existing understandings will draw me to think of certain probe questions ahead of others, to ignore or miss some comments as unimportant and make assumptions about what is being said and why. This process happens largely in an implicit and pre-reflective way as ideas "pop into our heads" about what we should ask next.

Alfred Schütz' discussion of "mutual tuning in" is a good example of what is happening during such interactions (1970). Here, the development of understanding flows not out of a rational detached mode of being, but from shared moments of being "in" conversation with each other (see Matthews & Smith, 2024). Of course, some may question my use of the term "analysis" and they may suggest something like "pre-reflective understanding" is more useful (see Crossley's, 1999, discussion of inter-subjectivity for more on this point). For the context of the argument presented here, I prefer to use "pre-analysis" exactly because it marks out a direct and essential connection to the more formal component of understanding that is yet to be undertaken.

One of the most obvious and common forms of pre-analysis happens as a part of conversations that developing scholars have with senior colleagues. For example, I regularly encourage doctoral candidates who I advise to sum up their initial observations, thoughts and "gut feelings" based on data collection. This is me pushing them to do some informal analysis. Based on this I will offer methodological critiques, assessments of their claims and make connections to bodies of literature and theory that they might want to explore. In this way, we are having a dialogue between existing bodies of knowledge and their initial feelings about their time in the field.

It is in such shared moments that tacit, non-rational, irrational and pre-reflective ideas can be dwelled upon and then "shifted" into relatively rational and explicit attempts at understanding. As such, they are a formative part of what will later become more clearly and concisely evidenced or dismissed as logical or illogical interpretations of data. These processes might help shape further rounds of data collection, encourage scholars to see things they might have missed and open up new ways of thinking and approaching their projects. Or in Polanyi's words from above they can enrich our

understanding "by increasing our mental power over the given piece of information" (1959, 24).

As I understand it, the job of a scholar is to work diligently to avoid our approach and pre-analysis from skewing our findings into a biased interpretation. That is, if we are so confident in the utility of our theoretical approach, or we're so committed to our values, or unable to critically see assumptions that blind us to certain data that undermines our initial tacit understandings, then our ability to consider our data set in *relatively* detached ways is undermined. When such processes play out, instead of doing good social science, we are uncritically recreating theory, ideology or beliefs (see my discussion of "pet theories" for more on this, Matthews, 2025).

But to work to avoid such poor analysis, is not the same as claiming that we don't work from an approach or undertake elements of pre-analysis. Because whatever reflections upon such processes do happen, and however well we manage and mitigate them, they are *inherent* parts of doing any version of interpretative analysis. Therefore, such methods of analysis can't be thought to exist outside of its emergence from, and merging with, these parts of the process. This means they should feature within our methodological discussions and, in my experience, its rare they do.

A Formal "Block" of Analysis

So, understanding (academic or otherwise) is not a bounded activity which has a clear beginning. Yet, I'm in favor of scholars carving out space in which they do move into various moments where they conduct a more "formal block of analysis." In this regard, drawing on previous attempts to systematically undertake such a process *can* be useful. It is here, if anywhere, that academic papers tend to focus reasonably well on describing analysis.

But there are two problems. First, in framing ones work as following a certain method of analysis, scholars can be lured into uncritically and unknowingly forcing a rational system over a fluid process which has intuitive dimensions. That is, as above, it is common to get a *feel* for what is going on in the world one is researching, and when such understandings are "coded out" as coming from say three or more layers/steps/stages of thematic analysis, the reality of how we did come to understand our data is cast in a "rational shadow."

Second, my sense is that how people read and then follow such systems is often as something akin to a "how-to guide" to analysis. That is, when scholars follow some logically laid out steps, but do not engage in the process in a more abstract sense so as to understand the underpinning and emergent features of human knowledge production, they will most likely not grasp the key epistemological under-laboring of such ways of working. When this is the case the ability to think critically and deeply about such a process and therefore the findings that flow from it is undermined. In this way, the unknowable and unknown dimensions of human knowledge can sit obscured behind someone's practical use of a "how-to guide"—knowledge appears to come from "the system" rather than the (more or less non-rational and irrational) people "doing" the system.

A result is that scholars are less likely to critically see the absences, lacks, and problems that are inherent in their data. They are also at risk of becoming beholden to such approaches as they don't necessarily grasp the foundations of human understanding upon which they could build *their* relatively systematic approaches to analysis based on the needs of their participants, research questions, and the parameters of their project. There is a distinct potential for "learnt helplessness" that can come from such ways of working and this, to me, is the antithesis of Rapley's "qualitative analytical attitude" and the social scientific endeavor.

Despite my critical comments, formal "blocks" of analysis do often yield important insights into data that were not, and perhaps could not, be grasped from the informal processes that preceded it. And, in this most organized, systematized and dedicated moment of analysis, the hunches, feelings and intuitions that have developed from our time "in the field" or with our participants, will often be put under the most rational scrutiny. New observations will become apparent, previous understandings will be confirmed or denied by the data, and some feelings about what's going on won't be substantiated.[21]

Interestingly, the veracity of those feelings is not necessarily undermined by the lack of their alignment with data, this is because it is often that we simply can't "empirically reach" phenomena in a way that lends itself to formal interpretative analysis—for example, an interview, no matter how well developed and delivered, due to the limits of language, simply can't be expected to bring forth the richness and depth of some experiences. And it is here that we must accept, and when appropriate acknowledge, the limitations in the methods and analysis we've chosen to employ.

Post-Analysis Analysis

"Post-analysis analysis" relates to the processes that follow on from a formal "block" of analysis. It's rare, at least for me and those I advise, that the ideas that come from a moment of formal analysis see the light of day in academic publications in the manner they were first conceived. Rather, the ideas are dwelled upon, played with, sharpened, reworked, revisited and in many more ways shifted and refined over time (and they may even trigger a move back to any one of the previous moments I've described above). One of the most common features of this is during the writing up of academic papers. The process of getting one's thoughts "down on paper" and drawing together empirical and theoretical points in "black and white," are often an important part of working out how we understand a topic, how we interpret data and even how we (re)think in quite foundational ways about our work.[22]

It's common that when we try to write up our analysis there are empirical findings that were first considered to be significant, but which don't work for one reason or another in an academic paper/thesis. Perhaps they needed further substantiation in data, perhaps our first readings of them didn't actually fit with the theory that was developed or perhaps the limits of academic brevity mean they simply couldn't be squeezed in while maintaining a coherent paper in response to a reviewer's comments. As such, it

seems to me that it is important for us to speak about this dimension of our work when we discuss our analysis. And more so, it is a "game of pretend" to hide such processes from our readers.

Ongoing Analysis

There is no end point to the development of human understanding.[23] So, while there is a formal ending to academic analysis in the publication of a paper or book, this is a temporary fixing of an ongoing process. We must do this so as to say something about our research in relatively confident and explicit ways. But as we move toward our next paper, project, lecture, funding bid, our understanding continues to develop. This "ongoing analysis" is then the collection of processes that feed directly into our approach and pre-analysis in our next piece of work and beyond. We carry with us the experiences of our previous work and we can't divorce ourselves from them in any simple sense.

There is then a cyclical, spiraling way in which we should be thinking about interpretative analysis, and how our process of knowing more broadly, emerges and merges. I began this section by remarking that these "components" were *intellectually distinct.* The inference is that while we can consider them in this way, there are no clear empirical/phenomenological separations in how our understanding is developed. In that respect they are inherently plural. In the same way as a coin's "head" can be focused on intellectually, but can't be known in reality to have an existence without its "tail," so it is with these features of understanding. They emerge from, and merge into, one another—we flow through, in between and back and forth across them.[24] So, while we might mark out a moment, perhaps even a whole month, in our diary as when we are "doing analysis," such a moment when understood as a purely bounded, linear, rational event, *is an epistemological fiction.*

So, What's to be Done?

To offer a solution via a system, protocol or standardized set of actions would fall foul of the overly rationalized approach which I have called a fiction above. But there are some quite clear practical consequences that logically flow from the preceding argument. I'll detail these in turn, but broadly speaking what I've tried to outline here is the need for us to start our process of thinking about analysis from a solid epistemological foundation of what it means to understand something. Such issues are so fundamental to one's ability to develop research, critically assess the strengths/weaknesses of methods and make confident but balanced claims about data, that it seems implausible to me that a scholar can do good social science independently without such background philosophical knowledge in their "academic toolbox." I hope my epistemological under-laboring has highlighted this and offered clear points of departure.

But I think such fundamental understandings are undermined in current and future scholars if space is not made for more detailed discussions of the emerging and merging processes of knowledge production in methodology sections.[25] This, to me at least, must be something that editors, research leaders, and PhD advisors encourage more readily of scholars at all levels, and I expect that parts of my argument will be important in demonstrating and justifying exactly that.

So, my main recommendation is for editors and reviewers to avoid recreating and reinforcing systems that attempt to "flatten," or "code out," human epistemological processes into falsely systematic methods. Doing so removes space for the emergence of rational analysis from tacit, non-rational ways of knowing and the fundamentally spiraling nature of knowledge development outlined above. This might mean extended word counts, or encouraging alternative structures for articles, where appropriate. Of course, some colleagues and journals do this already, but it should become standard academic practice.[26]

Some more specific actions:

1. The ways in which we approach our work are fundamental to our analysis—interpretative or otherwise. As such, scholars must make clear attempts to account for how their approach was tied in very specific ways to *practical decisions*—as opposed to vague and vacuous reflections on a research philosophy or paradigm—they made when developing, designing, and doing their research. Some attempt this in a positionality statement, but such ways of writing tend to focus on researcher reflexivity in relation to their biographic positionality, rather than the details of how one's approach effects the whole research process (see Whitaker & Atkinson, 2021, for a wide-ranging discussion around this point).
2. Where clear elements of pre-analysis can be traced, they should be described in frank terms. If conversations between scholars happened during data collection, in the pub, at a conference or via email, which are related to how they understood the study broadly, and the data specifically, I see no reason why some of the content but, more importantly, the analytic outcomes of those moments shouldn't be featured in a methodology section.
3. A practical discussion of formal "blocks" of analysis should detail the specifics of how that process played out. Systems come in many shapes and sizes, and there are many justifiable reasons as to why one might shift the processes outlined by other scholars, so as to make them more coherent for one's study. Detailing and justifying *your* system in clear, practical and unbounded terms should be the order of the day and using, or being inspired by, others' systems *might* help in that regard. Of course, sound epistemological principles must form the basis of such a way of working.
4. Where elements of post-analysis shifted interpretations, discussions of findings and claims within your work, that process should be detailed. Rather than hiding moments of reconsidering, rethinking, and retheorizing, I suggest

acknowledging and exploring them as a part of the intellectual journey you map out in your methodology section. There are usually logical, theoretical, axiological, and empirical justifications for such shifts which I think should be detailed.

5. Across this whole research process scholars should be open about how they have sought to rationalize and make explicit, tacit, implicit, and what can be non-rational understandings. Dwelling in such *knowhow* is the first step and important content and outcomes of that process should be discussed. How did you go from "feeling" like something was the case, to clearly defining it and demonstrating it in data? Want did you sense was happening, but were unable to rationalize and how might you explain that process? It is within a discussion of those efforts that I find much of the intellectual work we do as scholars can be detailed and then presented to our readers and colleagues. For as Polanyi shows us, it is in the process of articulating the previously unarticulated that both forms of knowledge can grow (1959, 1967).
6. Methodological strategies should be produced that lean into the reality of human understanding rather than forcing it into a falsely bounded and overly rationalized "block." This might mean multiple "moments" of data collection and analysis that can provide more opportunities for a dialogue between data, theory and evidence. I see no legitimate epistemological reason to design or adhere to a linear and bounded system and force it over what is an inherently emergent, merging and spiraling process. Of course, limits of time/money might constrain this, but we should be resistant to such strictures impinging on our ability to do the best science we can, rather than simply accepting them as "par for the academic course."
7. Delivering dedicated moments of partial analysis and then writing up "bits" of data *might* work better, especially for developing scholars, than trying to do a single separate block where the entirety of a data set is approached in one (unmanageable) go. That is, while, of course, we must acknowledge the "unknowable wholeness" of the empirical worlds we're trying to grasp, it is an inherent part of the scientific process to reduce the complexity and mystery of the human condition and our social nature into "bite sized" chunks.
   One way I encourage people to work is to take what *feels* like important initial ideas, which can be justified as significant in some meaningful way, and focus on substantiating them while paying less attention to wider features of the work. This provides a clarity of focus which *can* help get a key part of the analysis, or perhaps a paper, sketched out. The interpretative processes that happen in such limited ways of working are *usually* more manageable and are therefore easier to describe and analytically interrogate. Once an initial bit of analysis is reasonably well worked up a second important focus can be developed, and so on and so forth. This can be repeated numerous times, and, in this way, the unending and developmental nature of understanding is maintained as scholars know they are only doing bits at a time.

8. If such "bits" are focused on, there will be non-rational or currently not rationalized, but knowable features of the wider data set—feelings and observations about the empirical world—that must be "pinned" for later consideration. Being organized as to how one collates this dimension of the analytical process is part of one's system. And, once various parts of the analysis have been sketched out and organized in this way, approaching the other pinned bits and/or the data set as a whole should be more manageable. This is reductive, but it is a way that we can organize our efforts to hopefully grasp something of the phenomenological magnitude and infinitude of human life. At this point synthesis is required, that is, the recombination of these separate bits of analysis—academic papers, pinned ideas, sections in a thesis—into something which seeks to speak in valuable ways to the data sets in a more "whole" way.[27]
9. Avoid the "fetishism of references"—that is, instead of simply deploying an empty reference to a relatively systematic approach spend more time describing in real terms what you did. You should refer in your method, to my mind at least, *mainly* to the specifics of what you did, not that of what other scholars say you *should* do. If you did indeed take inspiration from someone else's approach provide details of how that worked and didn't work for you.
10. A useful, but obviously fallible, way of being able to trace how you came to interpret your data and to recall the system that you develop is to use an "analysis/thinking diary" of sorts. Some version of this is relatively common of scholars doing ethnographic work and this is often connected to field note taking. Here, I'm recommending this for anyone as a means of tracing parts of their intellectual journey and, importantly, the act of writing down ideas, feelings and reflecting on tacit knowledge, is a great way of dwelling on such phenomena. And this will often act as in initial step in articulating and thus trying to rationalize tacit and implicit ways of knowing. In such a diary you would focus on documenting how your thoughts shifted and developed as you read, observed, thought, discussed, and theorized across the course of your studies and beyond.

    But, this will not be an overly useful process if it is merely descriptive. There should be an analytical focus on ones attempts to do analysis—that is, thinking about your thinking, assessing, and critiquing your development of ideas, and considering them in relation to extant theory and analysis should be the order of the day. This means your reflections can be written into your methodology at a level of abstraction that has more utility than simply trying to describe a tracing of what happened "in your brain."[28]

These ten points outline clear ways that some of the problems that I think we face in developing discussions of interpretative analysis in social science can be overcome. I don't anticipate that addressing them outside of a well-developed understanding of epistemology would be a successful endeavor.[29] It is then on those of us who lead

disciplines and have influence over scholars to ensure we are encouraging knowledge of research philosophy and critical methodology to be developed in doctorial education programs for both students and supervisors alike.

Some Final Thoughts

An unfaithful reading of my work may lead someone to critique it as "flattening" the place of Theory in interpretative social science—that I'm making the case for naive empiricism. Such a misunderstanding would most likely come from a place that finds significance in "big T" theorizing. I tend to give short shrift to such approaches because I find academics who enjoy them often shift into esoteric nasal-gazing which acts to exclude many scholars. Following on from this I try to discuss theory and theorizing in terms designed to undermine its sometimes fetishized place in academia. So, when I talk of academic ideas, intellectually grasping, explanations, or relatively rational understandings, I am discussing theory.

When my work is read via this knowledge it becomes more obvious that I place theorizing—the doing of relatively rational academic understanding—at the heart of all I have outlined above. I find very little separation between theory and other ways of knowing which require relatively abstract forms of interpreting the worlds we share with others. So, for clarity, this is an epistemologically informed essay which focuses on interpretative analysis. But theory—however you define it—is central to such analysis in that no such thing can exist outside of some degree of theorizing.

And an underdeveloped reading of my work may lead some to think I'm calling for a version of auditing that scholars have suggested as a means of enhancing quality in social science (Akkerman et al., 2008; de Kleijn & Van Leeuwen, 2018). While some of what I've argued does have that feel, in that I'm asking for more of our processes to be documented and detailed, much of what I have read on the topic of auditing in academia appears to fall into the broad critique I've made above about scholars presenting an overly rationalized account of their work. In the examples of auditing, and the systems that are proposed, there seems to be very little room marked out for the tacit, unarticulated and non-rational parts of the process to be explored. So, if parts of developing an "audit trail" and engaging in decisions with an "auditor," do help in the process of dwelling and thus making that which is currently non-rational shift toward a more rational grasping, than I would be cautiously in favor of such a process. But much more work is needed to mark this side of the process out in the extant literature on the topic.

The fact that scholars have been calling for such auditing since the 1980s and that they have not taken hold highlights several things. 1) That parts of academia are quite conservative and resistant to change. 2) The extra workload that is involved in such a process. And 3) the epistemological naivety there is at the core of seeking out even more rational processes as the solution to the problems of human knowledge production that I've outlined in this essay. It is only point three that I find to be a logical reason to reject and/or modify auditing as an appropriate approach. Possible paths forward should certainly not focus on more and more rationality on its own, but rather, we must

seek to acknowledge the non-rational and irrational nature of our attempts to know something of the world using social science. From here we *may* add more moments of rationality, but we may also seek out non-rational ways of working. It is in that latter focus that those colleagues who draw on creative and artistic methodologies are leading the way.

I think we must also encourage scholars who incorrectly understand or write about interpretative analysis as a purely linear, bounded and rational exercise, to see how a more considered epistemological under-laboring is foundational to their development and ability to function as relatively free-thinking, free-wheeling and potentially "single minded" academics (see Matthews, 2024, for a discussion on that latter point). That is, once philosophical understandings as to what makes rigorous and critical analysis have been more fully grasped, specific systematic approaches can be developed in various guises to suit the needs of participants, scholars, data and research questions in innovative and creative ways. This, to me at least, is a far better way of doing good social science, and if more "rank and file"[30] academics can do this, it would undermine some of the problems that I see in the current normal practice in how scholars think and write about their work. And, this is how we can go about developing what Rapley refers to as a "qualitative analytical attitude."

What I've presented here is some work designed to help produce such an attitude, via a relatively rational analysis of interpretative analysis. I have not tried to explicitly substantiate all my points, nor rationally prove all the foundational critiques upon which my argument is built. This is because I am confident my *feelings* on these topics will have broad utility to scholars who are not well versed in research philosophy, foundational understandings of human understanding and therefore interpretative analysis.

In that respect, while this essay certainly could be described as offering a strawman framing of normal academic practice—indeed I have accepted that in my own discussion of this section of my work as a "caricature"—I'm convinced that there are problems in how many people write and think about analysis as a linear, bounded and rational scientific enterprise. As such, I hope colleagues and future scholars will use some of this work to help them think more deeply about how understanding comes about and employ some elements of my recommendations in how they approach writing about such things in their methodology sections. This will enhance the rationality of our accounts of how we developed interpretative analysis which builds upon tacit, implicit and non-rational ways of knowing. And in that regard, we will be providing a more rational analysis of our relatively rational analysis.

Acknowledgments

As I remarked in the essay, the impetus to get these ideas down on paper came during a collegiate disagreement about normal academic practice in social science. Many of my colleagues are great scholars, and wonderful people, but that doesn't change the fact that I think they're wrong in various ways, and they reciprocate that feeling toward me. The ability to disagree, doubt, and be

dubious are essential features of developing a scholarly sensibility, and our relationships with colleagues must allow space for the growth of such features within our interactions. But many scholars (especially quite senior one's who should know better) are loathed to be questioned and challenged. Such unconsidered personal protectionism is antithetical to developing knowledge and understanding. So my gratitude goes out to those who have the wherewithal to disagree, debate and call each other wrong, while still being able to share a moment or more in the pub together. Recently, I've been fortunate to have some really considered reviewers and editors help me develop a series of papers that I expected would get a negative reception. This was certainly the case with this work (I submitted it and ducked for cover because I thought my approach would be hammered). The reviewers help me see some basic errors and pushed me to refine my thoughts without trying to fundamentally change the project. So I owe a debt of gratitude to the editorial and reviewer team at IRQR for their efforts. Cheers, CRM.

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Notes

1. Norbert Elias did much to establish this point in *Involvement and Detachment* and his work on the emotions (1987b). My own development of those ideas in relation to ethnography can be read at Matthews (2016, 2018). Unfortunately, these dimension of Elias' work, especially his discussion of "the hinge"—the intertwining of our sociological and biological nature—are not often explored within in contemporary scholarship (see Matthews, 2025, Ch18).
2. There is an important difference between the terms irrational and non-rational which I think springs from the common way in which "irrational" is used in English speaking societies. Specifically, when someone is being "irrational" they are often thought to be acting based on an inaccurate emotional interpretation of something. For example, becoming offended due to misunderstanding something someone has just said to you. Given this usage, the term "non-rational" provides space to mark out a dimension of someone's understand which is not guided by reason, logic, and explainable forces, but which is not "emotionally"/irrationally produced. For example, from the perspective of a (social) scientific rationality (that which broadly frames how we must proceed if we're scholars) beliefs, assumed traditions, magic, mysticism, and unexplained intuitions would be considered non-rational. There is a dichotomy here which we must be cautious of, but for the parameters of what is developed in

this essay, I think this discussion gets us most of the way. My thanks go to Marit Hiemstra for pushing me be more clear with my language and ideas on this topic, more on this at Matthews (2025, Ch16).

3. A note on focus, tone, and style: During the review process I was encouraged by colleagues' comments to reflect on the place of such a focus within leading academic journals. Of course, as the traditional home for advances in science, and academic knowledge more broadly, journals tend to prize work which offers contributions to the *avant-guard*, the "razors edge," the current pinnacle of empirical, theoretical and methodological debates. Much of my recent solo work, and certainly this essay, does not operate at that level. As one reviewer told me correctly, "there's nothing new in this argument." What then is my contribution? I hope readers will see a few things that give my work utility. 1) While the arguments are not new, and I use various quite dated references to help illustrate that, the current state of "normal academic practices," means that I suggest these foundational issues are lost on some developing (and more senior) colleagues. 2) I have worked hard to develop a relatively accessible approach to writing about potentially complex ideas, which I like to think is both encouraging and forthright. This means my arguments are clear but also open to challenge and critique—my goal here is push readers to develop and advance beyond my underlaboring work. 3) When taken together I think my focus, tone, and style offer something important (if not new) which I know some scholars find really useful and it is in that utility, that a refinement in their analysis, and contributions to academia, can be demonstrated.
4. Here, I am following Tim Rapley (2011) discussions of these four relatively systematic outlines of qualitative data analysis. For those readers who are not well versed in interpretative analysis I recommend reading that chapter as it adds quite specific details to the more general arguments I'm trying to make in this essay.
5. This is far from an exhaustive list. But these are the disciplines I am best qualified to comment on. I imagine readers will be able to find many examples of the problem I am highlighting here and below.
6. Within academic journals and disciplines that are welcoming to what might be called more artistic or creative approaches—narrative inquiry, auto-ethnography, poetic inquiry, arts-based research, phenomenology—my broad critical observations are less applicable. If social science could be considered as a spectrum, I would be taking aim at the more "sciency" of the social sciences. As a specific example of three journals that I'm familiar with which, while certainly not perfect, I think offer examples of where it is normal to find consistently well considered and refined discussions of methods, please see the *Gender and Society*, *Sociology of Health and Illness*, and *Qualitative Research in Sport*, *Health and Exercise*. Although work within these journals is rarely able to address all of the concerns I have about the misunderstanding or misrepresenting at the core of how some scholars frame their ability to understand.
7. Some readers will know of one of the most frequently abused "empty references" is to citations to Virginia Braun and Victoria Clarke's various works on thematic analysis. I expect you've seen the somewhat ubiquitous, and often vague, use of "Braun and Clarke" dotted around in various papers. Thankfully some editors, and some reviewers, have heeded Braun and Clarke's own warnings about such unconsidered usage (2019), and begun pushing

people for more clarity in their use of such a reference. Indeed, in a more recent book chapter the authors present a discussion of the "ebbs and flows" of qualitative research which matches some of the broad discussions I outline below (Braun & Clarke, 2021).

8. Axiology is the study of values. While some hackneyed understandings of science would suggest it should be "value free," the reality is that human beings can't exist outside of the values they attach to certain things. Even the notion of being committed to scientific "neutrality" is, in and of itself, a value. See Andrew Sayer's *Why Things Matter to People*, Matthews (2024), and Matthews (2025) for more on this.
9. My own reasons for doing this have changed over the years, considering them gives some insight as to why this happens more broadly. I first wrote about methods (see Matthews, 2014, 2016) by uncritically following what other scholars did—I had little ability to reflect on the (in)appropriateness of how published academics seemed to happily go about their methodological business. Later, as I considered how I advise others about what it means to write up good social science I found myself questioning the status quo, but ultimately accepting it as a necessary part of "the game" I was teaching them to play. Such uncritical acceptance has never sat well with me as an academic or as a person. So, in recent years I have read, thought and written about such issues as I developed various elements of "my approach" which I could more happily point people toward as a part of the scholarly training I provide (Matthews, 2025).
10. See Barnes (1974), Bernstein (1983), Flyvbjerg (2001), Harding (1991), Sayer (2010), and Stones (1996), for various important discussions around this point.
11. I take this phrase from Flyvbjerg call for social scientists to embrace the philosophical foundations of our disciplines to with groups and communities in various ways that matter (2001). Here, he is extending a long thread of well-informed social science. For example, Cicourel's *Method and Measurement in Sociology* (1964), Sayers' *Method in social science*: *A realist approach* (2010 [1984]), and Abbott's *Methods of Discover*: *Heuristic's for the Social Sciences* (2004) are all books that develop useful accounts of hermeneutics that broadly align with the arguments I develop here.
12. See Dewey's classic *How we think* (2012 [1920]) as a great example of thinking about thinking that can help more beyond assumptions that tend to drain away the complexity that is inherent in cognitive processes. Crossley's *Intersubjectivity* (1999) is also a great book for grasping how it's possible for people to understand and share experiences with each other.
13. Gilbert Ryle's separation between a rational *knowing that* and the lived development of *knowhow* is useful here (1984 [1949]). I've used this idea as the foundation for understanding the difference between being able to intellectually confirm you understand that getting hit in the head is bad for your health, and having a much richer and more detailed knowledge of how that act actually plays out as a result of boxing (see Matthews, 2020).
14. That we *do* social life in often unthinking, habitual, and embodied ways is something of a social theoretical truism that can be found in various scholars work including Margret Archer, Peirre Bourdieu, Mary Douglas, Norbert Elias, Ervin Goffman, Marcel Mauss, Maurice Merleau-Ponty, Michael Polanyi, and many more. What I'm talking about in this paragraph and below can also be found in Harold Garfinkel (1967), and followers, development of breaching experiments, wherein social norms are purposely violated, or more

subtly pushed at, or order to reveal the more or less pre-reflective, naturalized, and normalized ways in which people have learned to make their way through the world. Also see Margret Archer's book (from which I borrowed the end of the previous sentence) for more on such understandings (2000).

15. Margret Archer's work on the shift toward more reflexive ways of being, specifically in relation to liberal Western social worlds, provides excellent theorizing around this point in a broad sense (2012).
16. Gilbert Ryle's *Concept of Mind* and George Herbert Mead's *Mind*, *Self*, *and Society*, are books I would direct readers toward who want to explore this idea in more detail. Nick Crossley's *Intersubjectivity*, and Ian Burkitt's *Social Selves* are a more recent and, in some ways, more accessible ways into these ideas. I would also align these works with Lakoff and Johnson's *Philosophies in the Flesh*, which can be read as situating the body within Ryle and Mead's focus on the playing out of social processes from which "the mind" emerges.
17. Academia owes a debt of gratitude to feminist scholars who did, and continue to do, so much to establish this point as a central feature of a more rigorous, refined and politically progresses epistemology. See Harding (1991, 2009) and Sprague (2016) for excellent discussions around this point.
18. See Fleck's *Genesis and Development of a Scientific Fact* for how this relates to the social process of science.
19. On this last point, Norbert Elias' *Mozart* is especially enlightening.
20. For a foundation understanding of "foreshadowing" problems see Malinowski's classic *Argonauts of the Western Pacific*.
21. I'm indebted to Marit Hiemstra who, after reflecting on an earlier version of this essay and her own process of analysis, help me realize I had not given enough attention to such important outcomes of the formal block of analysis.
22. Van Manen's discussions of writing about hermeneutic phenomenological analysis are insightful here (1990). And in a more general sense please see three texts from Howard S. Becker—*Evidence*, *Telling about Society*, and *Writing for Social Scientists*—which especially when read together offer great insight into how writing is inherently tide to thinking about, representing and evidencing the human condition and our social nature.
23. Of course, it may feel like certain people's understanding does not "develop" as in progressing to toward a more accurate and reality congruent form, but I'm not using the term "development" here as a value judgment. Rather, I'm denoting the shifting, moving, nature of this process. And, of course, there is something of an "end point," because, as far as we currently understand, human knowledge development does sadly stop when we die. I hope readers will grant me some poetic license here due to you hopefully appreciating that I didn't feel it necessary to detail the nuances and specifics of our eventual demise as a central feature of the main body of my argument.
24. I deliberately avoided the terms "stages," "phases," and "steps" due to them being locked into a false linear temporality. I do appreciate that there is *something* of an unfolding process here, but when considered in that way, it can lead people to overemphasize the bounded nature of such a swirling process. Scholars who outline systematic approaches to interpretative analysis are usually careful to describe how no such linearity exists in reality. What

I think happens is people read about such things, but when a term like "stage" is used, even cautiously, it reduces the emerging and merging circularity which are essential features of a fuller understanding of human understanding. And this can lead to poor conceptualization and discussions within methodology sections of papers.

25. Some discussions around the online archiving of research materials, data and reflections might offer some cautious ways forward here (Corti, 2019). But, as my initial critical comments were aimed at poor discussions in methods sections my focus as to what should be done is also aimed at what I think can reasonably be delivered there.
26. I think most journals should offer authors the ability to apply for extended work counts if a logical and justifiable case can be made as a part of the review process. Simply extending word counts as a standard might lead to some people developing overlong papers which should be more concise. As such, offering some process whereby a judgment call can be made as to the appropriateness of extended word counts seems logical to me.
27. For an example, please see where Alex Channon and I did just this using Stebbin's discussion of "concatenated studies" as a methodological and analytical framework (Channon & Matthews, 2022).
28. Glaser and Strauss discuss something like this in the form of memo-writing for theory development in *The Discovery of Grounded Theory* (1967). And many more recent reincarnations of such ideas can be found (see for example, Borg, 2001; Engin, 2011; Janesick, 1999).
29. Andrew Sayer's methods books are particular favors of mind to help develop such knowledge (2000, 2010). However, a more manageable first step can be taken by reading Howard S. Becker's *Evidence*. I have a soft spot for this book and many others from Becker, and I recommend them to anyone who will listen to me.
30. I consider myself and most scholars to be part of this "rank and file." Some colleagues may take acceptation at me using this term as they feel it is a pejorative but is has only a positive value judgment in mind. This is because I have very little interest in writing for those who are at the vanguard of methodology and theory in social science—especially supposed "academic rock stars," bleurgh. Those scholars are getting on just fine. My interest lies in those of us who work "at the wings," that is, the vast and relatively silent majority who largely do the "heavy lifting" of academic business by diligently getting on with our research as best we can. It is in this understanding of the "rank and file," that I focus most of my efforts as I'd like to be part of raising the quality of social scientific scholarship across the board.

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