Developing Research Questions: The Social Lives of Ideas, Interests and Questions

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Research questions, however practically understood, appear central to the research enterprise. They may be explicitly marked as 'research questions' or implicitly orientated to through concepts such as 'aims' and 'objectives' or phrases such as 'we sought to explore …', 'I identify how …', 'the purpose of this study was to …'. You only need to consider where you may routinely encounter them – in journal articles, book chapters, books, final reports of projects, dissertations, theses, conference presentations, seminars, etcetera. So, research questions emerge at a specific end phase in the research cycle, as part of the process of dissemination, of 'writing up', in publishing, academic and educational contexts. They can also emerge much earlier in the research cycle, tied to more bureaucratic and procedural contexts – those of research applications for funding bodies, PhD applications, MA and BA dissertation outlines sent to supervisors, as well as applications for ethical approval, etcetera. These appearances, at earlier stages, are clearly less open to a more public gaze.

If we step back, we can see how in all these types of contexts, the action of outlining a research question is designed to do specific forms of work. Research questions often act as a gloss, a form of shorthand that demonstrates to the reader the specific direction and focus of this piece of research. They act as signposts to help you as a reader make sense of what is to unfold in the text, to prospectively manage expectations. So, they do some classification work, defining the limits of possibility. They are also clearly focused towards shaping the judgements of actors, whether they are formally reviewing the work for a specific publication, a funding panel or an ethical board, signing off on or marking the work for a specific assessment

or seeking to enrol and align the interests of readers to entice them into continuing to read on. So, they also do some persuasion work, seeking to engage.

In this way, research questions are part of the pragmatics, the politics and poetics of various moments of public formal presentations of research work. However, research questions are not only tied to shaping the action and reasoning of a range of (external) people and organisations but are also central to shaping our own conduct. Punch (2013: 64) outlines that research questions are 'central' because

- they organise the project, and give it direction and coherence;
- they delimit the project, showing its boundaries;
- they keep the researcher focused during the project;
- they provide a framework for writing up the project;
- they point to the data that will be needed.

Research questions, to borrow a concept from Gomart (2002), should offer 'generous constraints'. Through initially carefully crafting our research question, we constrain some elements of the potential trajectories of the work and so enable and focus our thoughts, ideas and actions. As such, they are a central part of the pragmatics of designing, directing and coordinating our field- and desk work.

We need to remember that these are only brief moments in the broader trajectory of the social life of research questions. When we engage with them in contexts such as funding proposals or dissertations – where they are often given their own specific demarcated section – or finally finish the process of agreeing on and writing 'my research questions', they (hopefully) appear as relatively coherent, settled and stable things. But we are missing the extensive work that has gone on, that is routinely rendered as invisible prior to and after these moments. We miss out on how they change, warp and reshape over time in and through a range of interactions with people, documents, technologies, situations and idea(l)s.

This chapter seeks to recover (some) elements of the social life of research questions, to describe the range of ways that qualitative researchers work to formulate, develop, refine and, at moments, (radically) reframe their research questions. It seeks to show how we often start with embryonic ideas for research and how, over time, some of these get transformed into more substantive research questions that direct the shape of our research.

Initially, I focus on how our ideas can emerge from a range of sources – from personal experience and reflection, engagements with others and existing research. I then show how we can work to refine these ideas, especially through conversations with a range of people and more focused and directive engagements with the research literature. I then focus on how we can shape our ideas into specific research questions, outlining some styles of questions we might want to ask. I show how these questions are refined over time, sometimes changing in quite substantive ways, as we engage with and reflect on our fieldwork. Finally, I close by returning to the central role of conversations with others – that we may want to work more directly, in collaboration, with those who have some direct interest or stake in the outcome of the research.

THE SOCIAL LIVES OF IDEAS AND INTERESTS

Personal, practical, conceptual and contextual issues can guide and shape our initial and embryonic research ideas. With relatively few exceptions, we rarely get access to the (initial) motivations of researchers for focusing on a specific piece of work. Other than informal conversations with people, a key source is monographs (often positioned in the preface, introduction, postscript, footnotes or appendices of such books). Research articles rarely offer such (confessional)

opportunities, in part due to issues of custom and practice and in part the pragmatics of word counts. For example, Goode (1994: 7–8), near the start of his book *A world without words*, an ethnography with children born with Rubella syndrome, offers us a brief insight:

> In doing this research I had no specific a priori theoretical or methodological issues; rather, these concerns emerged during the course of the study. I was confident that the ward was an extremely interesting place, but did not know what my involvement with ward personnel or residents would come to in the long run. My primary motivation, as I perceived it at the time, was a genuine fascination with interaction between children who were congenitally deaf-blind and without formal language and adults who heard, saw and spoke normally [*sic*]. I must confess that as a senior graduate student I had read enough sociology to understand that sociologists assigned language a critical role in the organization of social relations and human behaviour. Thus, I realized at the time that I had stumbled upon a society in which shared formal language could not play such a function, and perhaps this represented a general research motivation. In the end the research covered many topics, not all of which […] were related specifically to language.

Note here the range of topics he covers in his post hoc reflection on possible sources of motivation, be it those that initially instigated the work – his 'genuine fascination' about the topic – as well as those that emerged over time – theoretical and methodological – through the period of fieldwork and through the prism of Goode's prior reading of sociological work.

Initial (Research) Ideas

In very general terms, our initial research interests can and do emerge from an intersection of different types of trajectories. They often include the following:

- Some aspect of direct personal experience or reflection. My own PhD work on interaction in interviews (Rapley, 2001) emerged in part

from reflecting on the interesting dynamics of the first qualitative interview I ever undertook as part of my Master's thesis. And Glaser and Strauss' (1965) classic work, *Awareness of dying*, emerged from their personal experiences of witnessing their own mothers' deaths in hospitals.

- Discussing, witnessing or learning about the others' experiences, norms or practices. For example, part of the initial impetus for one of my PhD students to undertake a study designing digital support with and for people who self-harm (Birbeck et al., 2017) was discovering that a friend had a history of self-harm. So, that initial awareness or interest may emerge directly through interactions with friends, colleagues or strangers or be technologically mediated through the web, social media, books, films, radio, television, etcetera. And for many of my PhD students, their interest initially emerged from working in specific roles in health and social care.

- Research questions are also often aligned to some form of engagement with prior academic work, be that the empirical, methodological or theoretical work of others or un(der)explored areas of your own prior work. For example, my own interest in young people's decision-making around whether to take specific drugs to manage their arthritis (Hart et al., 2015, 2016) emerged in part from a theoretical interest in exploring relational autonomy (and working with young people offers a really interesting space to explore this issue). I was also interested in testing and expanding ideas I had developed in my prior work on distributed decision-making (Rapley, 2008), as well as exploring questions that emerged around treatment options from my work observing consultations in this area (Foster et al., 2011).

These interests can also be further shaped and mediated by political, ethical or moral interests. Really, anything might spur that initial interest.

I should note that most things have the potential to fascinate me. In part, that may be tied to the style of (conceptual) thought that my work initially emerged from, that of ethnomethodology. Given its focus on exploring the local production of social order, anything becomes interesting – from doing the washing-up to the work of maintaining a satellite in

orbit. I cannot stress enough how important it is to be open and to be (potentially) fascinated by the world. Ideas can emerge from any quarter – a brief conversation you overhear, a moment in a documentary, a particular thread in social media or a situation you observe that helps you make a connection to something else. Reading outside the areas you normally are interested in, especially outside your discipline, can be key, as this can help reframe your taken-for-granted thoughtways. You need to be prepared to be led down novel and unexpected paths and, importantly, you need to remember to make a note about them. Those ideas that persist, those that you continue to think with over time, that emerge in and enthuse your discussions with others, are potentially worth pursuing further. Our interests are often constrained by very practical concerns. The key one for me is time. We all only have so much time and energy to focus on specific things. And given my day job, as someone who researches and teaches in the context of health, that further constrains some directions (at least until I retire!).

Shaping Initial (Research) Ideas

Moving 'a general curiosity … into concrete terms' (Flick, 2018: 21), from a research idea or interest into something that feels more coherent also routinely emerges from the intersection of a range of trajectories. The emerging situation of the coronavirus (COVID-19) pandemic, which is happening at the time of writing, offers an example. Clearly, for many, this is a radical breach in taken-for-granted ways of living in the world, not least in terms of health and social care. So, I can see a whole of range of potential issues to research that would really interest me. However, I questioned whether I should be designing a study that needed me to undertake fieldwork with people. Given the broader existential, emotional and practical impacts that the emergent situation has on many people, especially those with health

conditions and health and social care workers, are any of my ideas really so important that I can justify designing another study that takes up the time of others? My answer, at that point, was no; none of my ideas felt vital enough to directly disrupt their lives with my research activities. Given this concern, one option was to design a study that focuses on the digital and document-based life of COVID-19, so minimising my direct impact on others. On thinking about a new government announcement, it sparked an interesting idea that, in part, made connections with an idea I noted down many years ago (an idea which keeps returning, in itself a good sign). I sent a message to a friend, and we discussed it. However, I chose not to pursue it, in part because I had other work on-going, and in part because it could wait; there was no immediate need. Over this period of time, various people have contacted me to discuss potential ideas, none of which I pursued beyond initial discussions.

However, one conversation, a brief aside in a video-call, did make me stop and think. A colleague had been talking with a friend, who was a doctor, about their experiences of delivering care at this time. They were having to contact people with long-term and life-limiting conditions, some near the end of life, by phone to discuss emergency healthcare plans – plans about what treatment actions people would like undertaken in specific emergency situations. Normally, people would be invited ahead of time to take part in a face-to-face discussion. However, at the start of the COVID-19 situation, many doctors and some nurses were asked to phone people up, without prior notice, to discuss the potential content of emergency healthcare plans. This was having a large emotional impact on all those involved, and no guidance and support were available for doctors on how best to do this work remotely.

We then arranged a virtual meeting, between a few people with different areas of expertise – the doctor who had initially discussed the issue and two colleagues with

interests in different aspects of end-of-life care and long-term conditions. We talked through the issue, including why research might be important, some tentative ideas about potential focus and practical outcomes of the research. In the short term, by designing a study that focuses only on fieldwork with doctors and nurses, we felt we may be able to produce something useful and relevant resources to support and guide them through the process, as well as resources to help them recognise and share the emotional impact, whilst also minimising the disruption for those we involved in the research. After the meeting, we exchanged some emails, contacted another colleague with interests in doctor–patient interaction and kept in touch. It was only after one of us saw some specific funding calls that felt appropriate both in terms of scope and timeline that we further developed our ideas, over virtual meetings and emails, into more refined research questions that might align with the funder's expectations (see Cheek, Chapter 21, this *Handbook*).

Now, this extended example is useful as it highlights a range of issues. Firstly, you will note I have talked about whether the research idea is 'important', 'useful' or 'relevant' (and not just to me but to a variety of stakeholders) and whether I feel I can 'justify' working in an area at this time. Questions of relevance and usefulness are key. However, creating impact through generating knowledge, tools or resources tailored to directly inform or support people or organisations or through generating knowledge for policy work is only one potential direction. As important is the development of theoretical, conceptual and methodological knowledge as well as substantive, empirical knowledge about the dimensions of a specific phenomenon. But you need to note that such ideas at the start of the project are more 'hope than truth'. We cannot really know a priori what, if any, useful results will actually be generated, especially given the emergent nature of the research process.

Secondly, developing and refining research ideas is best done in and through discussion. As with all areas of qualitative research, talking with and working with other people really (really) helps. Discussing ideas, be that in pre-arranged meetings or informal, emergent conversations, is central. Being asked to develop a trajectory of thought in and through discussion (as well as writing your ideas down) helps to (re)shape ideas and make them more concrete. It is important to note that nearly all of my own research ideas have either been initiated or more often crystallised or realigned in and through encounters with others, be they researchers or people outside academia. Often, these encounters and conversations happen by chance.

Thirdly, developing your curiosity into something more concrete takes time. You need time to develop and refine ideas, you need time to have conversations and to reflect on them, and, as we will explore below, you need time to read around the topic to support that work. You may also need time to access the appropriate support. Goode's (1994) work may not have been feasible without initial access to funding. He was a graduate student at the time and notes funding from two grants. For many empirical projects, as soon as you move beyond undergraduate work, the pragmatics of funding opportunities and specific calls for research on topics are often central to helping you refine your interests into specific questions. Many ideas you have may not emerge – some will forever remain things you would 'love' to work on, but due to practical, capacity or funding issues, they may be put to one side, delayed (hopefully only for now) or even abandoned.

Reviewing Initial (Research) Ideas

The range of work outlined above seeks to shape your ideas as well as help you feel more confident that your idea has potential. However, aligned with this work, you need to engage in some more formal ways with the

substantive research literature on the specific topic of interest. Engaging with the literature is often woven into the work described above. In a general sense, all you need to do is some reading around the area to make sense of the different trajectories of thought.

Researchers have outlined a range of ways that this work is and can be done. For example, Alvesson and Sandberg (2011) argue within management studies that most researchers undertake a practice of 'gap-spotting'. Through a critical reading of the studies of research questions (see Sandberg and Alvesson, 2011) and the broader how-to literature within management studies, they argue that people work to render an issue, topic or area as 'being incomplete, inadequate, inconclusive, or underdeveloped' (Alvesson and Sandberg, 2011: 249). As they note, this is as much tied to the norms of presentation in journals articles – the expectations of editors, reviewers and readers that research should and is shown to be filling an (important) gap – as it is to the potential conduct of researchers. They argue that the process of gap-spotting leads to 'systematic and incremental additions' (Alvesson and Sandberg, 2011: 251) to the field. However, they feel that it is problematic in that it fails to generate research that develops new theoretical ideas (see Alvesson and Sandberg, Chapter 2, this *Handbook*).

Questions of novelty, or rather demonstrating originality, are clearly an essential part of the broader organisation of research, be that educational assessment, funding, ethics or publication. As Denscombe (2010) outlines, demonstrating originality can be conceived in quite modest terms, through focusing on a new topic, adopting a design that uses alternative methods on a topic already explored, identifying unique information and offering new analytic trajectories. Within many areas, including management studies, novelty in theory development and theory-building is held in high esteem. However, for most of us, developing a coherent substantive theory (one focused on a specific bounded domain,

such as 'case work in social work') or a formal theory (a generic theory of, for example, 'routines' that can be used in a range of areas) is a relatively rare achievement. It generally takes place over time, over a large range of studies on the same and related issues. As Kislov et al. (2019) note, some work is theory-informed, and some work is theory-informing; and that, for me, is fine. We generally need more modest expectations, albeit with the hope that we can be surprised by what emerges.

Alvesson and Sandberg (2011) offer a relatively more formal process, an alternate to gap-spotting which they refer to as 'problematisation', to support and enable the generation of research questions. Whether or not we choose to follow their process step-by-step, what is helpful about the idea(l)s embedded in this approach is that they seek to remind us to challenge our taken-for-granted thinking, to slow down how we read and think with the research literature we are interested in exploring. Alvesson and Sandberg (2011: 256) outline six key methodological principles:

> (1) identifying a domain of literature, (2) identifying and articulating the assumptions underlying this domain, (3) evaluating them, (4) developing an alternative assumption ground, (5) considering it in relation to its audience, and (6) evaluating the alternative assumption ground.

In practice, this process is enacted in an iterative fashion and focused on enabling you to start to actively challenge the assumptions that are embedded in specific domains of research. Alvesson and Sandberg (2011) outline five key areas of assumptions, ranging from what they refer to as 'in-house' assumptions, those ideas that are taken-for-granted in a specific conceptual area (e.g. that we should research and evaluate medical decision-making research as one-off events between patients and doctors), to 'field' assumptions, those ideas shared across a discipline (e.g. that we can identify a 'discourse of medicine').

So, in this sense, practices of 'gap-spotting' or 'problematisation' are methods to help direct our engagement with a body of literature. And engaging in a body of work is key to enabling you to make sense of whether your specific research idea can and should be developed into a more formal research question. So, returning to the example of developing research questions on remote conversations about emergency healthcare plans outlined above, we simply searched some databases and found a small body of work on discussing emergency healthcare plans in various face-to-face contexts, as well as a body of work on telephone and online consultations in other contexts. However, we only found a few examples of guidance on remote emergency healthcare plans produced by a range of professional bodies and charities – generated rapidly in response to the COVID-19 situation – that were not informed by any empirical research. So, clearly, we had identified a 'gap', albeit a modest one, where we can, at the very least, build on and develop prior work, as well as generate some new knowledge on elements of this practice.

Dixon-Woods (2016) highlights a key contemporary distinction that she caricatures as the 'authorship' and the 'contractual' approach to the (critical) engagement with the research literature. By authorship approach, she means the norms and traditions of how we routinely think about, at least in the social sciences, of gathering, reading and thinking with literature – searching for papers, following up references and ideas, reading around a topic and using our embedded expertise to make sense of what is and is not relevant for our argument. I outlined that approach above in relation to developing the research questions for our project on remote conversations about emergency healthcare plans. We went back to some of the literature we knew existed, rapidly searched some databases, notably Google Scholar, and followed up some references in the papers, as well as papers that had cited the paper we were interested in (known as citation chaining or reference mining). This was an emergent, ad hoc affair. However, the more contractual approach has emerged within and alongside the rise of 'evidence-based' policy and practice in the educational, medical, political and social sciences. And with this, we see a proliferation of a set of very procedural techniques.

The more contractual approach involves systematisation of the searching, evaluation, extraction and presentation of papers. So, with the rise of practices of systematic reviews, content from papers is now 'data-extracted', and this 'data' is then synthesised in ordered ways (see Moreira, 2007, for an ethnography of this work). A range of these much more procedural approaches – techniques like scoping reviews (Arksey and O'Malley, 2005), rapid reviews (Khangura et al., 2012), critical interpretative synthesis (Dixon-Woods et al., 2006), meta-ethnography (Noblit and Hare, 1988) meta-synthesis (Jensen and Allen, 1996), etcetera (see Timulak and Creaner, Chapter 33, this *Handbook*) – are now available to support the development of your research ideas and questions. They offer a range of very ordered (and ordering) methods to construct a more 'transparent' narrative about a body of work. Clearly, you could formally apply one of these methodologies to develop an overview of your area of interest, as well as reading published reviews (if they, or related ones, already exist), to support elements of your understanding.

Whatever your view of these approaches, and they are heavily critiqued, we could all learn something from the sheer persistence with which they seek to discover papers on a specific topic – developing, testing and using structured search terms, searching multiple databases, and following-up references in an ordered manner. For example, I am in the process of having some very initial conversations about undertaking some work on the (re)organisation of care pathways for people with specific forms of liver disease. As part of that conversation, an expert in this style of work offered to conduct a quick search for any qualitative work in this specific area.

Table 16.1 outlines the 'results' of a search of a single database, MEDLINE, for potential papers from 1964–2020. From those 219 potential papers, only six were qualitative studies in this area! You need to be aware that MEDLINE is focused on biomedicine and the life sciences. Other databases, like Scopus and Google Scholar, are more effective at accessing qualitative research, as are CINAHL and PsycINFO. So, I also conducted a very rapid and more loosely structured search on Google Scholar and found little else of relevance. These more formal and structured forms of searching helped us understand the lack of work on this very defined area. Now, this may feel like a lot of technical work for little reward, but I would always suggest talking to someone who knows how to construct searches; they can develop searches in minutes, whereas it can take hours (and hours) trying to make sense of the systems and options. For me, at the times I've undertaken it – or rather, most often, the times when someone has done it for me – this process enables me to breach my norms and expectations. At times, it opens my eyes to new papers, authors, journals or ideas that my usual routines may overlook – and that, in itself, is important.

THE SOCIAL LIVES OF QUESTIONS

So, part of the social life of research questions is the moments of initiation, shaping and refinement – in and through discussion and, as we just focused on, reading around the area. During this iterative process, I would be thinking about – and, importantly, writing specific notes and memos about – the emerging ideas. Throughout the cycles described above, you are trying to develop tentative, candidate research questions. And we will now focus on exploring some of the pragmatics of research questions, how they are shaped and change over time.

Initially, we need to ask ourselves: given we are undertaking qualitative work, why do we need a research question? Qualitative work, across the range of traditions and ideals, is meant to be centred on more emergent,

Table 16.1   An example of a search strategy and results from a single database search

#	Searches	Results
1	exp Hepatitis, Autoimmune/	3554
2	exp Liver Cirrhosis, Biliary/	8051
3	"type 1 AIH".tw.	141
4	"type 2 AIH".tw.	80
5	"lupoid hepatitis".tw.	172
6	"autoimmune chronic active hepatitis".tw.	305
7	"autoimmune hepatitis".tw.	5576
8	"Primary Biliary Cholangitis".tw.	913
9	"primary biliary cirrhosis".tw.	7564
10	or/1-9	16150
11	((("semi-structured" or semistructured or unstructured or informal or "in-depth" or indepth or "face-to-face" or structured or guide) adj3 (interview* or discussion* or questionnaire*)) or (focus group* or qualitative or ethnograph* or fieldwork or "field work" or "key informant")).ti,ab. or ((patient? or clinic*) adj3 (view* or experience?)).tw. or "barriers and facilitators".tw. or interviews as topic/ or focus groups/ or narration/ or qualitative research/	578317
12	10 and 11	219

discovery-based work, where we seek to follow the phenomena – be that over time, space or actors – where we are open to being surprised, to being amazed and to change our assumptions and ideas. Given such a tradition, why not just have an interest or idea, enter the 'field' and see what emerges? As we saw above with Goode's (1994) confessional, you could argue that he did undertake, in some ways, this style of work. However, he did design the study to be focused in terms of a very specific domain: child–adult interaction in a specific context, an organisation with wards for children with Rubella syndrome, as well as an analytic focus on language as understood through the lens of a body of sociological work. And Silverman (2010) highlights that the idea of entering the field, hanging out and just seeing what emerges is an overly naïve position – what he refers to as 'simplistic inductivism' (2010: 84). We all have a 'professional vision' (cf. Goodwin, 1994); we always enter with some analytic ideas, see some elements over others as relevant.

Initial (Research) Questions

So, what actually makes a research question a research question? Lewis (2003: 48) offers an overview, drawn from a range of sources, of the core 'requirements' that she suggests research questions 'need to meet':

- clear, intelligible and unambiguous
- focused, but not too narrow
- capable of being researched through data collection: not too abstract, or questions which require the application of philosophy rather than data
- relevant and useful, whether to policy, practice or the development of social theory
- informed by and connected to existing research or theory, but with the potential to make an original contribution or to fill a gap
- feasible, given the resources available
- of at least some interest to the researcher.

And we have seen some of these elements introduced and discussed above. However, I want to briefly focus on a few of these 'requirements'. Firstly, I would strongly suggest that you develop a research question that is more than 'of at least some interest' to you! Given you are going to spend an extended period of time, energy and effort working on the topic, make sure it inspires you in some way; note that Goode (1994) described his 'genuine fascination' in the area he focused on. We all have to design studies to work within specific trajectories of resources, be that limited time, capacity, money, access, etcetera. So, make sure you focus on a question that (hopefully) brings you some joy and that you are curious about. Field- and desk work can be frustrating – all projects have moments when you are less motivated, as well moments of excitement – so you need to choose wisely.

Secondly, research questions have to manage the tension between overly focused and overly generous. Silverman (2010: 86) notes that some people have an approach that he refers to as 'the kitchen sink gambit', where they develop a very broad set of questions to cover all potential aspects of the phenomena. As he argues, it is much better to say 'a lot about a little […] than a little about a lot' (Silverman, 2010: 86). Focusing down is essential to explore and coherently describe the complexity of the social world. However, being too focused, being too specific, especially during field- and desk work, can also overly constrain analytic possibilities. And as we will see later, we need to be willing and able to shift our focus and refine our questions.

Finally, our questions need to be accessible. As highlighted at the start of this chapter, we need to account to others (and ourselves) about what we are doing. Over the whole research process, you routinely find yourself being asked about what you are researching (as well as 'why' and 'how' questions). Even though the answers will be tailored to the situation and often, following rounds of data collection and analysis, shift over time, they need to be clear and intelligible to a range of

audiences. Do not assume everyone speaks the same conceptual, theoretical or disciplinary language as you, so make it accessible.

So far, you have seen very few examples of actual research questions. This is deliberate. The specific architecture of research questions is shaped by a range of factors, including conceptual, theoretical and disciplinary affiliations, alongside methodological traditions, as well as the expectations of thesis examiners and funding bodies. However, we can begin to explore this in relatively simple terms, through thinking about the types of outcomes that they seek to generate. Ritchie (2003) offers a broad overview of four functions, outlined in Table 16.2, and I've offered some examples, written in very broad and general terms, of a style of question that might be used to generate that outcome (see Ritchie, 2003, for a more extensive list of examples of questions; see also Marshall and Rossman, 2014, for a different yet related typology: exploratory, explanatory, descriptive and emancipatory).

Some traditions have key words or phrases that they routinely draw on that may mark a specific conceptual approach. For example, Beck (1992) was interested in exploring 'the lived experience of postpartum depression', and phrases like 'lived experience' and 'lived meaning' may mark work as part of more phenomenological traditions. Beck also undertook another study (see Beck, 1993: 43), drawing in part from the same data set, where she tells the reader that the purpose of the study was to 'investigate […] the social psychological process used to resolve' postpartum depression. What is key here is the use of 'process' as a potential marker of a focus on (one of the versions of) grounded theory. And we see this elsewhere, with terms like 'explore the construction of depression in …', with terms like 'construction' and 'discourse' potentially highlighting one of discourse analytic traditions. However, be aware that there is often no direct correlation between the use of specific terms and

the study designed around using a specific approach.

The research questions outlined in Table 16.2 are quite generous, in that each one could be enacted in a variety of ways. And we always need to move from these more overarching, general questions to more specific and focused questions. So, I want to briefly return to the emergency healthcare plans study, in order to explore elements of this process. In Table 16.3, I have outlined three moments: an email summary after our initial conversation that outlines the idea; a research question, written early in the process of writing the grant application; and then the final version we went with.

Note that the actual research questions are shaped not only to demonstrate a specific focus but are also tailored to our expectations of the style of question that this funder, a relatively small medical charity, might expect. As you can see, the final question contains multiple foci. Drawing on Ritchie's (2003) typology, we can see a focus on 'contextual' issues, exploring the conduct of the conversation, 'evaluative', exploring the challenges to people, and 'generative', exploring a solution to those challenges. The funder also wanted some aims and objectives, and I show the final, revised objectives below:

1   Assess current guidance for remote emergency healthcare plan(s) (EHCP) conversations in primary care.
2   To identify, characterise and evaluate the impact of remote conversations in primary care on EHCP.
3   Develop, test and refine recommendations for remote EHCP conversations in primary care.

Again, these work to focus on different aspects of the research process, as they seek to break down and organise that process. If funded, we would seek to ask a group of patients with experience of the calls to work with us on the project. We would then start with reviewing the current literature, then explore the impact of this work with professionals through interviews and then develop

Table 16.2   A classification of the functions of qualitative research (functions from Ritchie, 2003: 27)

Functions (Ritchie, 2003: 27)[1]	Focus	My examples of general research questions
Contextual – describing the form or nature of what exists	Exploring meaning, configuration and structures	• How do people experience participation in qualitative research studies? • What are the different ways that people are recruited to qualitative research studies?
Explanatory – examining reasons for, and associations between what exists	Exploring motivation, origins and contexts	• Why do people agree to take part in qualitative interview studies? • How have interviews become the dominant method in qualitative research?
Evaluative – appraising the effectiveness of what exists	Exploring impact, engagement and organisation	• What factors promote or inhibit recruitment to qualitative research studies? • How do qualitative research participants understand research concepts like 'informed consent' or 'anonymisation'?
Generative – aiding the development of theories, strategies or actions	Exploring new ideas, solutions and tactics	• How can we work more creatively with younger children in qualitative research studies? • How can we increase the impact of qualitative research on local government policy?

[1] Note that Ritchie's functions are not parsimonious, in that pragmatically you cannot have explanatory focus without engaging with a contextual focus, or, as a reviewer helpfully noted, generate theory without explanation.

Table 16.3   An example of how research questions can change over time

Early draft research idea	Exploring the impact COVID-19 has had on primary healthcare professionals, primarily focusing on planning end-of-life care.
Early draft research question	How are remote conversations about withdrawing or withholding life-sustaining treatments conducted between primary care clinicians and patients, what are the challenges and how might they be overcome?
Final research question	How are remote conversations about emergency healthcare planning conducted between primary care clinicians and patients, what are the challenges and how might they be overcome?

(test and refine) some specific resources to support them, through rounds of design workshops, interviews and a survey, and the detail of this work is then explained in the grant, in another section.

Above all, initial research questions need to strike a balance between specificity and generosity in scope and need to demonstrate, at a glance, the focus of your work. They are driven as much about personal style and habit as learning to make sense of the norms of the specific discipline and conceptual area you work in, alongside the expectations of those who will pass judgement on them, be that examiners, reviewers or grant panels. So, you need to discover and work within the genre of research questions in your area (as well as the genre of aims and objectives, if they are also part of the norm). However, the initial process of crafting your research question is just one moment in the trajectory of their social life.

Refining and Reframing (Research) Questions

Our initial research questions are routinely (re)shaped over time in and through engagements with field- and desk work. As soon as we begin the process of 'formally starting' the research, be that through applying for ethical review or collecting our first piece of 'data', the research focus and potentially the question will start the on-going process of changing, warping and reshaping over time. MacIntosh et al. (2016: 69) differentiate between two trajectories of this work – what they refer to as refinement and reframing:

> Refinement involves adjustment to the particular focus of a research question following engagement with the research setting in a way that leaves the original intention of the research intact. […] Reframing, on the other hand, involves a substantive and potentially discontinuous shift in the focus or nature of the research question.

They outline how this refinement and reframing work can occur in and through 'contextual triggers' – so, shifts in the setting, be that tied to, for example, practical issues around more or less access to a site or broader issues around organisational changes – and 'reflexive triggers' – so, the development of additional insights and ideas through working in the area.

As I have discussed in a range of ways above, qualitative research is an iterative practice, and its strength lies in the process of comparison, constantly exploring similarities and differences and being open to new trajectories of thought. In a sense, the work of refinement and reframing is core to the idea(l)s of qualitative research; it is essential work, at the heart of the programme of work. And I want to briefly outline a few examples that begin to explore these ideas. Note that I have drawn on examples from my own and my colleagues' work, because, as MacIntosh et al. (2016: 69) note, these features are rarely described in published work.

- Elsewhere (Rapley, 2014), I have discussed how discovering a 'critical case' (Patton, 2002; see Patton, Chapter 70, this *Handbook*) was central to shifting the focus of our research question. We were undertaking research that focused on delay in diagnosis for children with Juvenile Idiopathic Arthritis (JIA). An interview with the parents of one child made me realise that rather than needing to understand the phenomenon under study as 'delay in diagnosis', we needed to focus on delay in diagnosis *and* receiving appropriate care. So, the research question (and concomitant analysis) was expanded to focus on dimensions of 'inappropriate care'.

- With another study I was part of, on how primary care practitioners detect, discuss and manage patients' alcohol problems (Rapley et al., 2006), I was very aware of the prior work on the topic. During the research process, I began to feel some elements had changed little over time. So, we adjusted one of the research questions to focus much more explicitly on this aspect, using a core paper in the area (Thom and Téllez, 1986), to enable a comparative analytic approach. The main difference across this time period was that the definition of what could constitute abnormal alcohol consumption had expanded, so the range of consultations in which they may have to negotiate these difficulties also expanded. However, the core conceptual issues that Thom and Téllez's (1986) work initially identified, 20 years earlier, still held. I want to note that within the qualitative field, we rarely encourage, design and/or undertake such explicit replication work, yet it was a fascinating experience.

Now, both of these examples are clearly tied to reasonably modest refinements in the design of work – (re)focusing elements of the data collection and analytic work. However, they can feel more dramatic, as one of my colleagues, Tiago Moreira (personal communication) recounts:

> I was in the third year of my PhD, with a writing-up plan approved by both my supervisors. I was working up to handing in my PhD in September for a viva in December or January. Around April I was invited to give a paper and I thought it was a good idea to use this to write up and discuss a draft of what would be my last chapter before the conclusion. The chapter would focus and

critically discuss the concept of illness trajectory (Strauss et al., 1985) taking into account the data analysed in the previous empirical chapters. As I was writing the draft of the paper, I slowly realised that the concept of trajectory should be the point of departure of the whole thesis, from where my questions of the data should be formulated. I had a bit of a crisis, thinking that I might not be able to transform the entire thesis and re-analyse the whole data set. But as I wrote the chapter, it became clear that the new question extended, and deepened, rather than completely reconfigured, my analysis of the data. The new question was an opportunity to continue to analyse the data. By July I had realised that I needed six more months to do this and asked for an extension from my funder. I was lucky that the extension was given, as most funders would not accommodate.

Such an approach where, given new (surprising) analytic insights and ideas, we are willing to forgo our prior analytic trajectory is central to the tradition of qualitative research. However, as Moreira highlights, when this emerges relatively late in the process, this can be mediated by some very mundane contextual concerns – in this case, questions of time and funding.

Adjustments in your research questions can clearly occur as a result of shifts in the context – for example, through a specific site you are working with no longer being able to grant access, or a shifting policy context meaning that a specific process or practice is no longer supported or delivered. The recent COVID-19 pandemic has highlighted the impact of such contextual factors. For example, I am currently undertaking a study embedded within a randomised controlled trial of yoga for older people with multiple long-term conditions. Our research questions are written as objectives for this funder:

Objectives:
1: To identify, describe and explain barriers and facilitators to set-up, recruitment and trial processes.
2: To describe recipients' and providers' experiences of the yoga intervention and study process.
3: To identify optimal implementation strategies for embedding and normalising the yoga intervention in preparation for wider roll-out.

The study design involves interviews with yoga participants, yoga teachers and the trial team as well as observations of classes and collecting documents. The face-to-face yoga classes were stopped as soon as the potential impact of the pandemic was understood. The study was initially put on hold, and then we started to discuss the potential of offering the yoga online, both internally and with the funder. Since that point, all organisations involved, including the ethical review board, have agreed for a shift to online delivery of yoga classes. So, in some senses, our general research questions remain the same; however, the online element refines and radically reframes elements of the design, methods, reading and analytic opportunities and focus.

So, in and through the life of a study, our research questions need to be constantly returned to and reviewed. MacIntosh et al. (2016) note that one impact of such practices can be abandoning a specific question and/or the research per se. Abandoning research feels like a relatively rare occurrence. However, the work of refinement and reframing – in terms of adjusting and finessing elements of your research question, as well as more substantive changes that involve more radical reorientations – is everyday, routine work.

Engaging (Research) Questions

In this chapter, I have briefly shown how research ideas, interests and more formal research questions develop and adapt over time. I have outlined how they are constantly reshaped through a range of interactions with people, situations and ideas. Above all, we need to be open and willing to change our ideas, questions and ultimately field- and desk work practices. You need to engage with the work in your specific area of interest, as well as relevant methodological or theoretical work. We also need to find ways to slow down how we discover, read and think with the research literature we are interested in exploring.

When we find similar work to that which we are planning – and this is often the norm – we need to learn from it and build on it. You can always design your study to draw on different methods, analytic approaches, theories or contexts. As we develop our research questions, we need to be aware of the specific genres of research questions in our area of work, the specific words and phrases that are routinely used. You can then tailor them to the conceptual, theoretical and disciplinary contexts you work in, as well as to those reviewing and/or funding our work. Finally, we need to try to design questions that strike a balance between specificity and generosity. They need to enable a trajectory for the organisation and focus of our field- and desk work and that is not overly narrow and restricting but that is also workable given questions of limited time, capacity, money and access.

Given these directives, I want to close this chapter by briefly returning to focus on another core aspect of the development of our research questions, to return to the vitally important role of active engagement and collaboration with others. I have routinely highlighted, and at points briefly shown in my narrative, that discussion with others is a core element of our work. In more emancipatory and activist research traditions (sometimes marked in relation to a version of action or participatory approaches) and a new generation of applied, often policy research (often marked through labels such as co-design and co-production), very active engagement with participants or 'stakeholders' is positioned as essential and ideal practice. The term 'stakeholders' is a gloss here for a vast potential array of actors, generally focused on those with some interest in the outcome of the research. So, that can include citizens, consumers, experts by experience, patients, publics, marginalised citizens, service users, etcetera. In the context of health research, Martin (2008) highlights that the rationale is both technocratic in terms of accessing expert knowledge and increasing potential for impact, as well as democratic in relation to egalitarian and political trajectories. For some, such work can also include a different set of actors, such as policy-makers, practitioners and professionals, as well as formal and informal groups and organisations tied in some way to the issue.

As others have argued, such collaborative work can be done in a range of ways, sometimes generously and engagingly and sometimes in less egalitarian ways (see a recent debate on co-production: Oliver et al., 2019; Williams et al., 2020). However, what is central here is that we understand that dialogues with a range of actors are potentially central to directly formulating, developing and refining research questions. These are not only dialogues in the frame of research – for example, dialogues we have in interviews or focus groups, or dialogues we observe in situations or documents (so research 'on' and 'for') – but rather moments we create to engage more explicitly with people, to think 'with' them, where we research 'with' them. And in some cases, that also means moments where they become co-researchers. Clearly, we all need to be open to such engagements, open to collaborate and discuss and open to reframe our research ideas, interests and questions.

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