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Research questions

What's worth asking and why?

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'What's worth asking and why?' is without doubt a key question for all researchers. Behind it lies a complex array of issues, some specific to the particular field of research and others more general. The question is of particular pertinence to postgraduate researchers embarking on or working towards a doctorate. The doctorate not only constitutes the highest level of earned academic award, but also arguably involves the highest level of personal investment and greatest level of risk, particularly in the light of high levels of non-completion across the disciplines and apparently increasing time to completion for those who persist (see Halse 2007, for discussion of contemporary concerns about the doctorate and the rhetoric of crisis, and McWilliam 2009, for consideration of the impact of risk consciousness on doctoral education). At every point in the journey, from application for a university place to embarking on the research to completion and award of the doctorate, the issue of whether or not the question you are answering, the problem you are addressing, or the hypothesis you are testing is worthwhile will continue to be a concern (and maybe, even, for a good time after the journey to the award of a doctorate is brought to a successful conclusion).

In this chapter I am going to consider a range of factors underpinning or influencing judgement of 'what's worth asking?' with a particular focus on doctoral research projects. It is clearly going to be difficult to address the issue of the 'worthwhileness' of a specific question in a manner that encompasses the specialised knowledge of the full range of disciplines, subjects and fields of research covered in the modern university (let alone the late- or postmodern university, with its complex offer of interdisciplinary, hybrid and practice-oriented regions of knowledge and research). Indeed, the question of 'field' is of central concern in determining whether or not a research question is worth asking, an issue to which I will return later in this chapter when considering what it means to make an original contribution to knowledge (a common requirement for the award of a doctorate), and the imperative to be able to identify the field to which you are aiming to make a contribution.

The task is further complicated by institutional, national and regional differences in the form and content of doctoral programmes and the criteria for the award. An additional level of complexity is added by the increasing diversity of types of doctorates within any given system of awards, with many institutions offering a range of professional doctorates as well as doctorates by publication and performance and practice-based awards

alongside more established PhD programmes. These awards, such as the EdD in education, DBA in business, the DEng in engineering and the DClinPsy in clinical psychology, can embody very different expectations about research and the relationship between research and practice (both academic and professional). Consequently, they attract postgraduate researchers with very different backgrounds and aspirations, and offer very different opportunities for research and development (see Scott *et al.* 2004, for an exploration of the professional doctorates in a range of areas of professional practice). My strategy, in the face of this diversity, is to address the more generic issues, and when straying into more specialised concerns to stick, at least initially, to the social sciences, and in particular to my own quasi-discipline of education, and broadening out to consider other disciplines and regions of research as appropriate. My approach will also clearly be coloured by my own sociological and educational interests. In following this path, I aim to be able to say something that is of genuine general interest, and, I hope, utility to postgraduate researchers from diverse disciplines, programmes and contexts.

The research question

It is common for higher education institutions to require a clearly defined research question at the point of application to study for a research degree. In most cases this question is crafted by the applicant, based on previous studies at first degree or masters levels, other academic work they have done, professional and personal interests and experience, or some combination of these. How these influences are weighted may vary according to the form of doctorate (with, for instance, professional concerns coming more to the fore in identifying potential research questions for a professional doctorate such as the EdD or DEng). In some cases, for instance for a scholarship related to a larger funded research project, the research question might already be defined.

There are obvious benefits to having a clearly defined question at the outset of a piece of research, particularly where time and resources for the conduct and completion of the research are limited, as they are for the vast majority of postgraduate researchers. It is vital, however, to acknowledge that, in the course of conducting the research, this question is likely to change. This change might involve anything, from the refinement and clarification of the question as the researcher becomes more familiar with the literature in the field of enquiry, to the outright rejection of initial focus of the research in favour of a completely different question. The degree of latitude available in precision of definition of the initial question and in the degree to which the research can depart from this question will vary between disciplines and with the particular circumstances of the researcher (for instance, the condition of the funding of the research or supervision of the project). The key issue here is that although postgraduate researchers are commonly required by institutions, for understandable reasons, to appear to commit themselves at the very outset of the research to a tightly defined question, in practice it is likely that in the course of the research the question will change. Researchers and their supervisors have to be prepared for interests to shift, and indeed it can be argued that dynamism and flux in 'the research question' are fundamental features of the research process. This does not mean that the definition of an initial research questions serves no useful purpose. It does, for instance, set a direction for the research and place useful limits on the reading to be done in the initial stages of the project. It also enables supervision arrangements to be made that are appropriate to the specialised nature of a doctorate.

Institutional and personal demands for a research question continue throughout the life-course of the research project. Shaping your concerns and interests into a question helps you to gauge your progress according to the extent to which you are moving towards providing an answer to the question. A commonly given piece of advice to postgraduate researchers is to write your research question on a card, which you pin close to your computer screen or other prominent place in your work space. Periodically you should look at the card and consider whether or not what you are currently writing addresses the question written on the card. If it does not, then either adjust what you are doing to address the question, or flip the card over and write on it a question that you are addressing. Although this is crude advice, it makes sense to have some way of assessing the extent to which your activity at any given time is advancing your research. Having a defined research question to which to refer your activity is one way of doing this.

A clearly defined research question also helps in communicating what it is that you are doing to others: from fellow researchers working in the same or a related field, to interested friends and family with no detailed knowledge of your work. Suppose you meet someone in an informal setting, such as a party, who asks what you do. On hearing that you are studying for a doctorate, they ask what your research is about. The challenge here is to provide a succinct description of your research for a non-specialist audience. A clear research question helps provide a way in to understanding what it is you are doing and should give a clear sense of direction to the activity that can be grasped by a non-specialist, though they may not, of course, appreciate the finer details of the research, nor indeed see the importance or fascination of your research endeavour at all. Here, the research question is a device for codifying and communicating what you are doing to a range of different audiences.

The point here is that the formulation of a research question can, and should, have utility to you as a researcher, as well as meeting more formal, institutional and other requirements. It is, though, dynamic, and in many cases will, and should, change, to a greater or lesser extent, as the research develops, and as your knowledge of the field in which you are working (which itself will be in a state of flux) increases. Brown and Dowling (1998; see also Dowling and Brown 2010) present the development of a research project as a dialogue between the theoretical field (where the research question, or problem, is formulated as a relationship between a number of concepts) on the one hand and the empirical field (where research strategies are operationalised and outcomes produced) on the other. Neither side necessarily has the upper hand, with developments on one side warranting adjustments on the other, in a dialogue that continues throughout the process of research.

Question form might not be the best way of encapsulating the focus of your research. In some disciplines, the formulation of a hypothesis to be tested is more appropriate. In other areas of work it might be more appropriate to identify a problem to be addressed, or an issue to be explored, which will be broken down into more specific questions that are formulated as the research progresses. In some cases, the production of the question being addressed, in its final form, might not be possible until a relatively late stage in the research, as it is formulated and reformulated in an iterative process in dialogue with a progressive programme of empirical and/or analytic work. The latter is clearly a high-risk strategy if you want to be confident that the question you are addressing is worthwhile.

What must be avoided at all costs is tyranny of the research question. Here, researchers, under pressure to capture their work as a clearly defined question, formulate it in a way that misrepresents or is tangential to their interests. Subsequently, attempts to answer the

question lead them away from their interests, or even expertise, and begin to dictate a particular approach or form of work with which they feel uncomfortable, uninterested and unprepared. The research question should guide, but not dictate.

Contribution to the field

One common feature of doctorates, certainly in the UK at the time of writing, is that they should make an original contribution to knowledge in their field. *The Framework for Higher Education Qualifications in England, Wales and Northern Ireland* (Quality Assurance Agency for Higher Education 2008: 23), for instance, states that

> doctorates are awarded to students who have demonstrated: the creation and interpretation of new knowledge, through original research or other advanced scholarship, of a quality to satisfy peer review, extend the forefront of the discipline, and merit publication.

This means that, whatever the discipline or area of research, to begin to get a sense of what is worth asking entails engagement with what constitutes original research and new knowledge within your field of work, and getting a sense of the principles of evaluation of the quality of research. Clarity regarding the field in which you are working is essential, not least because your supervisor, colleagues and examiners will be members and representatives of that field, and will have particular expectations about the manner in which research is conducted, the kinds of knowledge produced and the manner in which the process and outcomes of research are presented in the thesis. For some researchers, identifying the field in which they are working is straightforward. They might, for instance, be working in a highly specialised and well-established area of work that is strongly institutionalised with clear boundaries, a well-defined academic community and a strong sense of disciplinary identity. Their project could, for instance, be a part of the work of an established research team, which has a clear sense of what constitutes legitimate research in the field and where the limitations in the current state of knowledge lie. This form of 'lab culture', in which postgraduate researchers are inducted into the academic culture of the field in which they are working, is more common in the natural sciences than in the social sciences and humanities. Although this has the advantage of presenting a strong sense of field membership and the security, often, of an already specified research question, it does place constraints on the extent to which postgraduate researchers can take control of or influence the direction their research takes.

As Bernstein (1975) has observed, in his discussion of forms of knowledge in the field of sociology in the 1970s, some disciplines comprise a number of distinct, and sometimes competing, sub-groups. These groups can differ not only in the form that research takes, but more fundamentally in how the social and/or physical world is conceived, what constitutes a legitimate question and how the outcomes of research are mediated and evaluated. A worthwhile question or research focus for one of these sub-groups may not be recognised as worthy of any attention at all by another. The growth of inter-disciplinary research and increasing interest in practice-oriented research complicates the situation further by bringing together disciplines and areas of professional practice in a multiplicity of ways, which makes it impossible to provide straightforward explicit guidance on how to identify your field. It also makes it more likely that your research can be positioned in

not one but a number of fields. The relative maturity of a field of research can have an impact on the consensus on and stability of the principles of evaluation of research in the field. The bringing together of fields or disciplines in new configurations of inter-disciplinary or interprofessional work also entails the need to resolve diverse and possibly conflicting criteria for evaluation of research. In making decisions about questions to address and approaches to take, postgraduate researchers need to be aware of possible areas of instability, uncertainty and contestation, and the associated opportunities and risks.

In order to gain a sense of what constitutes a worthwhile question, we, as researchers, have to position ourselves and our work in relation to what has already been done and is currently in progress. This means that we have to engage with existing research and other writing, encompassing our theoretical, empirical and practical academic, personal and professional interests. What is commonly referred to blandly as a 'literature review' in the social sciences is, more accurately, a form of active engagement with writing and other academic and professional artefacts in order to gain a sense of the landscape in which we are working, and figure out where we stand in this landscape, and, importantly, a clear sense of the other individuals and groups standing alongside us. That said, fields are dynamic, with differing degrees of coherence and stability, making it more or less easy to gain a sense of where one stands. As both the field and our own research develop, where we stand and who stands alongside us may change, and we need to have a clear sense of when and how these changes take place and what the consequences are of any given shift. Thus, although an initial identification of and engagement with our field of research and sharpening of our sense of its dynamics and limits are necessary to provide an initial orientation, this engagement has to continue throughout the life-span of the project, and, in the development of an academic career, beyond the project. Working at the outer edges of current knowledge in the field is, by definition, unstable and risky. Continuing engagement with work in the field is necessary in managing this uncertainty and maximising confidence that a worthwhile contribution is likely to be made to the field, in a way that is recognised as original and rigorous by other researchers.

No matter how pertinent and interesting the question being addressed in the research, not knowing the field in which you are working and not knowing it sufficiently well to be able to position your work and establish its originality and rigour, clearly constitutes a major threat to the perceived value of the project. There are clearly ways of mitigating this risk. A field is sustained by a community of researchers, and so interaction with members of the community and participation in its activities, for instance, help to ensure the relevance and value of the research, and help, in turn, to shape the field. The characteristics of this community might, indeed, influence the decision to conduct research in a particular area.

Ultimately, the examiners of the thesis, as representatives of the field of research, have to make a judgement about the extent to which your thesis makes a legitimate and original contribution to knowledge in the (that is, their and your) field. In cases where the research spans more than one academic field, or combines professional and academic interests, the examiners, if they represent different disciplines, fields or interests, may come with very different expectations. In research of this sort, it pays from the outset to be clear about the extent to which these expectations are likely to be compatible. In the social sciences, for instance, it is common for a thesis to address a particular specialised area of practice from a particular theoretical perspective or from a specific methodological position. A thesis that examines the processes and practices of inclusion of children with specific special education needs in primary schools from a sociological perspective

may lend itself to examination by experts in both the substantive area of special needs education and in the sociology of education (or a more specialised sub-group or either or both these fields). The combination of examiners (as representatives of the fields and legitimate evaluators of the thesis) will be very different if the topic is approached from, say, a psychological, historical or philosophical perspective. Bringing together a set of substantive professional interests with a particular disciplinary perspective requires mutual recognition of the value of the accounts produced by the other. For a postgraduate researcher deciding on whether a question or topic is worth asking, it is necessary to be confident that such areas of mutual recognition exist, or can be created, or that potential differences can be resolved with respect to the project and the approach adopted.

The key issue here is knowing the audience, or audiences, for our research, and being clear about who can, and ultimately will, evaluate the work. A major threat to successful completion of a doctoral project is, in relation to this, not having an audience, or having an audience that is not in a position, in the eyes of the accrediting institution, to evaluate the work. A postgraduate researcher who is convinced that her or his work is so innovative that there is no one who is in a position to evaluate it, is clearly going to have trouble bringing her or his doctorate to a successful conclusion. Similarly, there are projects that are seen as being of vital importance to a community, but where members of that community are not in a position, with respect to the criteria for examination held by an academic institution, to evaluate the research. This is not an argument for a conservative orientation. Academic fields, like fields of practice, have differing appetites for risk, challenge and change, and clearly change does take place. It is, however, an argument for recognition that the doctorate is an academic award and that academic fields are regulated and institutionalised, albeit to differing degrees. In attempting to assess whether or not a research question is worth asking, it is important to bear this in mind. We have to assess our own appetite for, or tolerance of, risk and be aware of strategies that are available to mitigate this risk.

Processes and products

Though the feature that distinguishes the doctorate from other earned academic awards is making an original contribution to knowledge, how that contribution is achieved and how it is presented in the thesis is also important. The criteria for the award of a PhD at my own institution, for instance, include a formidable list of other requirements: that the thesis consist of the candidate's own work, that it 'be an integrated whole and present a coherent argument', that it includes a full list of references, that it is written in English, that it demonstrates the capacity for autonomous research, that it provides a critical assessment of relevant literature, that it demonstrates a deep understanding of the field, that it demonstrates relevant research skills and that it is 'of a standard to merit publication in whole or in part or in revised form', and more. And all in 80,000 words, and taking just the equivalent of three years full-time study, and presented in print with specified line spacing and page margins, and so on.

The issue here is that although the question being addressed must be of current interest in the field of study, and hold the potential to give rise to an original contribution to knowledge, this is not necessarily sufficient for it to be worth asking. For a postgraduate researcher working towards a doctorate, the question must also be 'do-able' within the constraints and expectations of a doctoral thesis. It must also be do-able given

the institutional context and the skills, interests, circumstances and dispositions of the researcher and his/her supervisor, who has a key role in supporting and enriching the process of research. Furthermore, it has to have the potential to give rise to a process through which specialised knowledge and research skills can be developed and deployed alongside generic, transportable and employment-related knowledge, dispositions and skills. Finally, the question must engage you as a researcher, and the process of research must have the potential to meet your immediate and longer term aspirations. Here, we are focusing on the process of research, and addressing the extent to which the research question will give rise to a process that delivers more than just a completed thesis.

Working towards a doctorate should be an educational experience. Your supervisor plays a key role in supporting you in the development of your project and in providing guidance and feedback on progress towards successful completion. Your supervisor also has a more general educative role to play, in helping you to explore and understand the field in which you are working and in enabling you to acquire both the specialist and generic skills and knowledge you require not only to complete the thesis successfully, but also to become an active participant in the relevant research community or communities and to meet your own academic and/or professional career aspirations.

Clearly, it is important that your supervisor has the expertise and experience to be able to assess the value of your project and advise you on how it might be developed. With less experienced supervisors, or with experienced supervisors who are supervising on the edges or outside their immediate field of expertise, it is important that some kind of additional expert support is provided, in the form of either joint supervision, a supervisory panel or supervisor mentoring. It is, in any case, advisable to have people in addition to your principal supervisor involved in support of your project, or at least involved in the monitoring of progress. This is commonly achieved by appointing a second supervisor to work in a supporting role, or the appointment of a supervisory panel to support the supervisor.

As research into the process of supervision has consistently demonstrated (see, for instance, Delamont, Atkinson and Parry 2000), the strategies used by supervisors, and the level and quality of support provided, is highly variable. Institutions also vary in the form and quality of support provided for postgraduate researchers, both in terms of the facilities offered and the scope and quality of the research training provided. For a postgraduate researcher, the lack of suitable institutional and/or supervisory support for a project can render a potential worthwhile project highly problematic. The context has to be right to realise the full potential of a project.

The form of support provided by the supervisor constitutes one part of the institutional environment that facilitates the 'do-ability' of the project (and therefore the 'answerability' of the question). As the project develops, other factors come into play that influence the extent to which a project is do-able, both generally and more specifically within the confines of the journey to completion of a doctoral thesis.

Ethical considerations, for instance, clearly impact on whether or not a project is do-able, and, if it is, whether or not the conduct of the project is advisable or desirable. An investigation of, say, the educational progress of children identified as having a particular kind of learning difficulties, which requires access to data collected routinely and confidentially by schools for purposes other than research, is going to confront a number of difficult ethical issues relating to access to and use of data. Likewise, a study by a senior manager in an organisation, which involves interviews with members of staff about their aspirations and perceptions of decision making in the organisation, is going to involve

difficult conversations in which interviewees may feel they are compromising their position in the organisation and endangering their prospects by revealing aspects of their lives and selves that would normally have been kept private. It also opens up the possibility that this unease will lead to partial accounts that compromise the quality of the data collected. In both these cases, the research questions being addressed may be, from the perspectives of researchers and/or practitioners in the field, well worth asking. The means by which they have to be, or are being, addressed may render the research questions, from an ethical and practical perspective, 'not worth asking'.

As studying for a doctorate is likely to span a number of years, it is important that the initial enthusiasm that you have for your project can be sustained (and that the interest of others in your field can also be assured over the period of research). Is the topic of research sufficiently engaging? Is the approach you are adopting one with which you feel comfortable? Does the mode of data collection lead you to engage in forms of activity that you enjoy, and thus are more likely to be able to sustain (for instance, a project in which data is collected through interviews is more likely to be sustainable and successful if the researcher particularly enjoys talking to people). Sustainability also has to be considered with respect to issues such as access to empirical settings over the period of research and meeting the costs of the research. Whatever the potential of the question to give rise to a contribution to knowledge in the field, it has to be possible to complete the project itself with the time, resources, expertise and support available to you, as the researcher, bearing in mind the circumstances you are in, your own particular skills, interests and dispositions and so on. And, ideally, the process should be enjoyable and fulfilling.

'What's worth asking' is also influenced by what you hope to gain from the process of working towards a doctorate, and from the eventual award of the doctorate. Analysis of UK first destination occupational data (UK Grad Programme 2008) shows that graduates from doctoral programmes take up a wide range of forms of employment across diverse sectors. Although a significant proportion of graduates do take up research-related employment, it can no longer be assumed that a doctorate is principally preparation for a research or academic career. This has fuelled a greater emphasis on generic and transferable employment related skills in PhD programmes. It has also led to an acknowledgement that people enter PhD programmes with a broad range of aspirations and follow diverse occupational trajectories on graduation. Diversification in the types of doctorate offered, for instance the growth of professional doctorates that act as either initial training or a form of further professional development for particular professional groups, has also increased awareness that what postgraduate researchers hope to gain from a programme is increasingly varied. This diversity of aspirations is catered for in the structure and content of programmes, with professional doctorates, such as the Doctor of Engineering (DEng), offering a range of taught courses, which combine specialised skills and knowledge relating directly to the area of research, with more explicitly employment-related components, such as input on business practice, production processes, research management and knowledge transfer.

Career, and other personal aspirations, should also be taken into account in ascertaining whether or not a research question is worthwhile, for you personally, to address. This involves a balancing of academic field-related and professional concerns with personal dispositions, skills and circumstances. Particular questions offer specific opportunities, for skills development and new experiences in the short term, and for work and life opportunities in the longer term. Analysis of particular forms of data may, for instance,

provide opportunities to use and develop expertise with certain forms of analysis software, which, in turn, may place you in an advantageous position with respect to future occupational choices. This also holds for the substantive focus of the research. Knowledge and expertise developed through your research may have greater or lesser utility to you in future career development.

Current circumstances also influence the selection of a question to research. Your personal and professional commitments determine the time you have available to study and how this time is distributed. For postgraduate researchers studying part-time, professional commitments and conditions can both create opportunities, for instance to carry out a study focusing on the setting in which you work, and render some lines of enquiry impossible, for instance research that requires an extended period of time in another setting. From the possible array of potentially worthwhile research questions in your field, there are going to be some that are not, through personal and professional circumstances, possible to take up, and others, through the opportunities that they afford in relation to interests and aspirations, which would be particularly worth addressing.

This is not to sanction or encourage a purely instrumental view of selection of a topic to research, but rather to emphasise that 'what's worth asking' is not just a function of the field of research, but also relates to who you, the researcher, are, and how your circumstances and aspirations position you in relation to the processes and outcomes of doing research.

Conclusion

Formally, 'what's worth asking and why?' could be seen as a function of the current state of the field of study – what is the current state of knowledge, where are the gaps, what constitutes an interesting and productive study in relation to other work in the field? So, to answer the question 'what's worth asking?', we could say 'a question is worth asking if it makes an original contribution to knowledge in the field'. Fields are not always quite so simple to identify, and exploration of the field, in order to position one's own work, constitutes a major part of the project of completion of a doctorate. Fields are not discrete, nor are they defined by or limited to academic disciplines, but incorporate, relate to, overlap with, influence and are influenced by each other and domains of professional practice. You have to know the field in order to be confident that the question you are intending to address will give rise to knowledge that is considered within the field to be new, original and interesting. Although this does not enable us, here, to make a positive, non-contingent statement about what is worth asking, it does carry a very important message with respect to doctoral projects. You must be able to identify a field, in the sense of a recognisable community of researchers with common interests and approaches, in which to locate your work. It is from this community that representatives will be drawn (as examiners) in order to evaluate the extent to which your work advances knowledge in the field, and does so in a way that demonstrates a good understanding of the field (substantively and methodologically) and adopts a form of research that is recognised as appropriate and legitimate by other members of the field of research.

A topic or question might appear to a researcher to be worthwhile in itself, but without a field or community, its value cannot be recognised. Although doctoral theses can, should and do play an important part in the development of new fields of study and new approaches to research, it is unlikely that a new field will be initiated, though it can

be signalled, in a doctoral thesis. The nature of, and risks associated with, the examination of a doctoral thesis place limits on novelty. The 'attack on the taken for granted world of their audience' that Davis (1971: 311) associates with 'interesting' social theories (and, for Davis, all 'great' social theories are considered as such because they are seen within the field as 'interesting', not because they are seen to be 'true') carries a huge risk for a postgraduate researcher. So, although originality is essential in a worthwhile doctoral research project, there are distinct dangers in being, or at least seeing oneself as being, so far ahead of the cutting edge of contemporary research that no one is in a position to judge its value or quality. For originality to be established, someone has to be in a position, other than the researcher her- or himself, to make that judgement. There has to be a readership that is knowledgeable and respected in the field.

For a question to be worth asking, it also has to be answerable, and answerable within the limits imposed by a doctoral programme and thesis. These relate, as we have seen, to the resources available to do the research (in terms of time, money and equipment, for instance), the institutional setting (in terms of supervisory expertise, training to do the research and access to sites of research, for instance) and the form and format of the thesis (in terms of both how the research has to be physically presented in thesis form, and the requirements of the thesis to demonstrate broader competence, expertise and knowledge than may be strictly required for completion of the project and answering the question, exploring the problem or testing the hypothesis). As the foundation for a doctoral project, to be worth asking the question has to be answerable within the confines and affordances of a doctoral programme. This is in addition to other 'do-ability' issues that concern all research, for instance relating to ethical research practice.

A worthwhile project must not only be do-able in relation to the doctoral programme and the support it provides, it must be personally engaging and sustainable for you, as the postgraduate researcher, in your particular circumstances. This personal dimension should not be under estimated. A worthwhile question that is not personally engaging, or that requires expertise and attributes that you do not have, is unlikely to give rise to a process of research that can be brought to a successful conclusion. Losing motivation, either through loss of interest in your work, or through the trials of meeting constant personal and academic challenges, can be fatal for a doctoral research project. Making a judgement of 'what's worth asking' must thus take into consideration the researcher her- or himself and her or his circumstances. It is not just 'the field' that renders a project worthwhile; who is asking and in what circumstances is equally important in figuring out 'what's worth asking?', and 'why?' is as much an issue of the extent to which it is answerable in personal, practical and situated terms, as it is a matter of the state of knowledge of the field of research.

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