Factors affecting the selection of foci for radiography research

In the second in this series of articles on transformation Peter Hogg et al continue the outline of transformation in the research focus.

Introduction

This article is the second in a series of three which, when taken as a whole, will outline how the Directorate of Radiography within the University of Salford changed its research focus. The first article[1] focused into the leadership style which was adopted to facilitate change. This article focuses into the contextual factors that played a significant role in the discussions which led to the selection of our research topics. This article draws heavily on literature published within the business sector[1,2,3,4,5,6,7]; literature that outlines how universities might engage meaningfully to produce research which has value.

The article commences with a short reflection on the contribution that radiography academics working in universities make to radiography related research and practice innovation. Building on this the transfer of research into practice is considered. Finally, against this background an insight is given into our strategy for research topic selection.

University contribution to radiographic professional knowledge and practice advancement

Central to the selection of the research topics there was a requirement to understand the university contribution to radiographic professional knowledge and practice advancement – Here, professional knowledge is described as research which can be used to inform practice. Establishing whether universities do contribute to *professional knowledge* was relatively straight forwards and as an indicator, ten years' worth of *Radiography*[8] was assessed to determine the contribution that university staff made to that body of literature.

We established that approximately 60% of the original research articles emanated from universities or collaborations with a university. Given that a minority of radiographers are employed within universities this suggests that universities contribute a disproportionally high component of our professional knowledge base, per radiographer capita.

Similarly, on this basis it could be argued that universities provide a disproportionally large amount of evidence on which practice – educational, clinical or otherwise – *could* be based. The word *could* is emphasised because generally speaking we simply do not know whether practice has changed as a consequence of any particular research.

We acknowledge that examples exist where university-generated research can be tracked though to professional policy. A notable example would be the Society and College of Radiographers, Child Protection Policy and its reliance on research publications from the mid 1990s onwards[9].

This policy cited peer-reviewed 'radiography' – not radiology – articles which all emanated from university collaborations. In the same vein one could deduce that the abundance of university generated practitioner ability – formative and advanced – literature has likely played a part in driving forwards practice standards, including the widespread uptake of advanced practices[10]. It is important we recognise that other factors, such as NHS policy and Regulation, have also played a major role in practice advancement.

It is reasonable to conclude that university contribution to professional radiographic knowledge is identifiable and in abundance. It is also fair to surmise that such literature may have value within practice. Determining what contribution specific pieces of university generated research have made to practice advancement is much more complex and beyond the scope of this article. On the basis of these arguments, we hypothesise that universities are well placed to continue providing evidence on which practice might be based.

Relationship between university related research and its value within the community

In the business sector, universities appear to be valued for their ability to generate novel ideas and to think differently to industry based innovators. Business recognises that universities have the flexibility to engage with *curiosity driven research* that may not have an immediate impact on practice, By contrast, business tends to engage in *near to market research*, which usually has value to their core ambitions. It might be that universities can afford the risk associated with curiosity driven research whereas business cannot.

Valuing and utilising this difference could result in an innovative approach to research. In one of our recent research meetings we observed an example of this phenomenon:

The discussion was within a research meeting for the refinement for our novel deformable breast phantom. The team comprised clinical scientists and practitioners and university staff. The university staff explained that one of the research questions should involve the comparison of analogue and digital images of the phantom acquired under 'similar' conditions, eg. compression levels.

The purpose of the research would surround determining cancer lesion in order to quantify any difference. Some of the clinical team members said that this work had limited value as the way forwards was to use only digital recording media. The university staff responded by saying the work was a good intellectual exercise and it would likely have historic value; it may also confirm or refute anecdotal remarks made by practitioners about lesion pick up as they transferred from analogue to digital systems. The clinical staff agreed with the sentiment and work has already commenced on this curiosity driven project.

Another interesting difference is that university solutions often tend to focus on achieving results that are robust and repeatable whereas business can be satisfied with an 80%[11] solution which is in line with commercial opportunities. This business-university research analogy could have similarities with hospital based research in which service evaluation would give an answer which could resolve a practice question, but which would not necessarily satisfy the rigour of research required by a university. Of course 'rigour' and 'near to market' are not incompatible and the two do coincide in many instances, both in business and in radiographic practice.

It has been said that business needs to realise it must engage actively to get the best out of university collaborations[12]. Of those businesses that do interact actively with universities, they have recognised they benefit from an assortment of intellectual resources which have been built up over time. Examples of these include: experience in methods design; the ability to gather data faithfully; the ability to analyse data objectively, in relation to the research question.

In the context of radiography research it can be speculated that meaningful hospital/university collaborations will offer much more than simply the project outcome alone. Also, if university related research is to be a catalyst for practice advancement then the research activity should focus as near to market as possible and with this in mind there might be a need to implement a strategy for effecting change as a result of the research outcome/s.

Transferring research into practice

Radiography is an applied field and on reviewing radiography related publications across several journals, it is clear that radiography research tends to be applied and not theoretical. Given its applied nature, one can speculate that the intention of a university radiography researcher would be to publish their findings in anticipation that somebody would adopt the research into their practice. Sadly there are chance elements to this occurring:

- Will it be read?
- Will it be understood?
- Will the relevance to practice be evident?
- Is the culture right for transferring research into practice?
- Did the research get translated into practice?

These chance elements bring into focus the difference between *'dissemination of research'* and *'knowledge exchange'*. Dissemination is a relatively simple task; for instance publishing a journal article about the research. Knowledge exchange involves transferring the research knowledge in a more meaningfully fashion. This implies it would be used in practice and achieving this is much more complex than dissemination.

Previously, universities have been judged on the quality of research they conduct and disseminate and this has been quality assessed through Research Assessment Exercises (RAE)[13], a process of profiling each discipline of research, enabling the grading of universities on a national scale. Recently there has been a shift in emphasis through the introduction of the Research Excellence Framework (REF)[14,15], which replaces RAE. Through REF, universities are expected to ensure knowledge transfer occurs with a view to practice development or market innovation. REF describes market innovation and practice development as 'impact'.

The implication of REF means that there is now a drive for universities to engage in research that is highly likely to have impact within the communities they serve, since REF argues that research should have value. Considering the earlier points made about practice advancement – notably the lack of clarity between research and practice advancement – it has become necessary for university research teams to plan for impact from the onset. To achieve high impact a university would need a strategy that would heighten the probability of knowledge effecting change. Let us now consider some factors that would heighten the probability of impact occurring.

Research that is near to market, eg applied research, will likely be adopted more quickly and strategies can be implemented to bring the research even nearer to the market. Very near to market research can also help minimise transition costs, which may be a barrier to adoption. Transition costs are considered to be the overheads required to translate research into practice. The business sector acknowledges that the transition time for the innovation may detract from delivering the normal business so again, strategies should be considered to minimise this problem. Collaboration, from research conception, between business or hospital, can reduce distance to market and help to minimise transition costs.

Absorptive capacity (AC) is a term used in the business sector and it relates to successful market innovation. It concerns the successful transference of research into practice. A company with high AC is much more likely to adopt research into practice. Steps can be taken to improve AC; companies, and hospitals, require their own internal capacity and/or external help to understand and value how research could benefit them. Workplace cultures which are open to innovation are more likely to benefit from university research.

With this in mind it is worth remembering that the NHS has an evidence based practice requirement[16] and consequently the NHS culture should be open to innovation, but even so, within the confines of a research project, further steps can be taken to improve AC. Ways of improving AC can include undertaking collaborative research and centrally involving hospital based gatekeepers in the research collaboration. For applied and near to market research, collaboration would be an essential requirement, from conception to translating outcomes into practice.

Given the nature of radiography it is certain that most research

Figure 1: Impact pathway for an aspect of mammography breast imaging research.

1. Potential outcomes
- a. Enhanced machine and practitioner capability
- b. Discovery of new facts
- c. Predictions which have clinical and professional relevance
- d. Improved cancer pick up
- e. Improved patient experience

2. Nature of impact of the research
- a. Technology change; knowledge transferred into product development
- b. Enhanced staff technical and interpersonal abilities
- c. Service development, including enhanced patient experience and reduced none attendance rates

3. Potential interested users of the research
- a. Industry
- b. Patients
- c. Practitioners
- d. Educators
- e. Professional bodies
- f. NHS Managers
- g. Funding bodies

Figure 2: Broad aims for research topic selection.

1. Investigate technologies and imaging practice to improve image quality and diagnostic reliability
2. Investigate the factors that enhance or compromise patient and carer safety and well-being in the context of diagnostic imaging
3. Evaluate the contribution of diagnostic imaging services within patient pathways

would require more than one discipline on a collaborative research team, as such a multi-disciplinary approach would normally be essential to reflect the demands of research question/s.

To do this, the university radiography academic would need to work across discipline boundaries. It has been proposed that they should have a firm knowledge of their own area and a conceptual understanding of other areas together with effective leadership skills to bring the project together[2]. This approach is consistent with good practice in the business sector. As part of this process and with these requirements in mind there is a need to identify and empower those academics that are capable of engaging with external partners. Leadership ability is considered an essential requirement in this process[17].

Whilst size of impact is not yet debated in the literature, this may require consideration. For instance, conducting research into a niche area will likely have small, but perhaps important benefit to a sub-component of society, eg. a cure for a rare disease. If there is a need to demonstrate magnitude of impact then this must be considered during research topic selection. Figure 1, adapted from Ternouth et al[18], illustrates an impact pathway for an aspect of our mammography breast imaging research. As can be seen there is scope for magnitude of impact to be factored into discussions.

Strategy for selecting our research topics

At the onset we acknowledged that our staff had a diverse research skill set and our research interests were equally diverse. As previously explained[1], we knew we had to focus into clinical imaging and that we must work in collaborative research teams. As part of the focusing process and at an early stage, we set broad aims to help direct where our research should lie (Figure 2).

This meant we would have to move away from existing research, develop additional skills and move into new areas. This would require reaching consensus on the new foci and then working on those. On selecting the foci there was a need to think creatively. Our debates were not about identifying a set of isolated research projects, nor were they about one staff member having a research idea and pursuing that idea in isolation of others. The debates sought to identify topics which had value to practice; importantly we tried to identify ones which had an element of future proofing with considerable scope for addressing a set of inter-related research questions. Such inter-relation would have to consider how each research question could relate to, and help partially unlock, the answer to another research question.

Consequently, our research became multi-layered and multi-faceted. Our strategy for topic selection turned out to be one of speculation, opportunism and iterative refinement; it was pragmatic and highly reflective. Pilot research activity tended to surround literature analysis and testing theories by gathering and analysing empirical data. The process we followed was not without an evidence base; it was guided by using knowledge transferred from a highly successful research unit within our own university.

We were mindful not to select topics in which we may have to compete with highly successful research teams from other universities. Hard decisions had to be taken about what not to pursue and this involved identification and cessation of aspects of our early research which did not appear to be thriving.

Some staff found these false starts frustrating, however we tried to view these activities as re-definition and focusing of our scope and purpose. Importantly these activities helped us to stop investing resource in areas which did not make adequate progress. Our low resolution CT research, in the context of SPECT-CT, was identified through deductive reasoning and by contrast, the selection of our breast research was highly opportunistic.

Consistent with planning for transferring research into practice, the potential for collaboration was assessed and where potential existed, we put together multi-disciplinary collaborative research teams. To oversee our research we established two steering groups; one for breast and one for SPECT-CT – low resolution CT.

External funding was important and at all stages we had our mind on grant applications. However, until we had characterised our research identity, we did not engage in this activity because we did not have a track record in our new areas. That era has now passed and we now have a large body of empirical material to submit to journals.

We continue to be mindful of external and internal agendas. For instance we feel that our low resolution CT research will have value beyond the original context, as our methods and results are likely to have importance to optimising dose and image quality in diagnostic CT. For our mammography research we recognise that extending the screening programme age will mean our work could impact on a larger proportion of the population.

Final thoughts

In this article we have set out what we believe to be the important factors that should be considered when defining a new area of research which would likely have value to our profession. We acknowledge that the range of areas we have selected is limited and they do not reflect our entire educational portfolio, or the scope of radiographic practice per se. This is the nature of quality research; it should have a clearly defined focus because depth is more important than breadth.

Building on this article, next month we will give an overview of one of our research areas – breast. In that article we shall outline how the specific strands of the research inter-relate to form a cohesive approach to investigating a particular problem.

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The first article in this series was: *Transformational leadership in changing a research culture: a personal reflection* (*Synergy I&TP*, April 2011).

References for this article can be found under 'Synergy resources' at *www.sor.org/members/pubarchive/synergy.htm*

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