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When is a Problem a Research Problem?

ABSTRACT

Various definitions of and approaches to research and research problems are explored with numerous examples given. Guiding criteria for applied research are also discussed, along with potential pitfalls, the role of intuition in the process, and the qualities that are needed to make a good researcher.

PROLOGUE

“The Brains Trust” was one of the earliest and most popular TV shows broadcast by the BBC. It featured a panel of highbrows and academics who fielded questions from the general public on every conceivable subject. The questions were not especially abstruse, more like the kinds of questions posed by my three-year-old daughter: seemingly simple, but ultimately confounding. Prominent among the pioneering panelists was the late philosopher, C. E. M. Joad, who, if my memory serves me right, unfailingly began his reply to each question with the phrase, “Well, it all depends what you mean by” Such, indeed, was my internalized reaction on receiving the title of the present paper. It is not one I would have chosen, and the question is certainly not one I have ever posed or been posed. Problems, large and small, domestic and professional, are everyday features of my life, and research (funded and independent, basic and applied) is something I have been doing for the last fifteen or so years. But I haven’t given a great deal

of thought to the nature of problems, at least not since reading Bertrand Russell's (1959) *The Problems of Philosophy* as a freshman, and my theorizing about research has tended to focus on issues of style, methodology, and management rather than root definitions.

PROBLEM OR OPPORTUNITY?

There is a perfectly simple reason why this has been so. Since I began researching in this field, I have never had to look for a research problem: ideas for research tumble naturally out of workplace experiences, literature immersion, and routine intellectual trading. The things I do, read, hear, and say provide the inspiration for my personal research. Since this conference is concerned with practitioner aspects of research, let me illustrate: In 1978 I was employed as an entry-level professional in a small/medium-sized public library in London. The library was sited in an area with (a) high ethnic diversity, (b) poor quality housing stock, (c) multiple social deprivation, and (d) low income levels. The library was keen to reach out to nonusers in the local community. There are two ways of looking at this: The library's objective was to reach those who could not be reached, or the library's problem was not being able to reach those it wanted. Objective, problem, challenge, opportunity. The word is largely irrelevant. At the time, I was interested in the marketing of library services, so I designed an experiment using five different direct mailing shots to compare the relative effectiveness of the five different packages/approaches (Cronin, 1980). In the longer term, my goal was to identify predictors of positive response to advertising campaigns of this kind.

A presurvey of the target population used construct clustering techniques to evaluate different kinds of promotional materials and guide the design process. The five experimental groups were painstakingly matched in terms of (a) type of accommodation, (b) social class, and (c) proximity to library. New library registrations from the family units of all those included in the five groups were monitored for four weeks to gauge the relative effectiveness of each promotional package. Less than 1 percent of those mailed joined the library over the four-week period. Hardly an experimental success, but, with hindsight, hardly surprising. Nonetheless, a good example of how we can learn from negative results.

As a piece of practitioner-conceived and conducted research, the study was not without charm and ingenuity. And it was low cost. It highlighted the inappropriateness of direct mail advertising for a certain kind of nonuser population. But it was driven by a curiosity to see whether the factors that influence nonuse could be modulated by a

particular form of targeted advertising. Nonuse may, for some, be a problem, but as I look back, I realize that my motivation was curiosity rather than problem resolution. I did not conceptualize my study in terms of a research problem, though if someone had referred to it in such a manner I would not have batted an eye.

COLD WATER . . . COLD FUSION

“The deep secrets of Loch Ness are to be laid bare in what is claimed will be the first full and credible scientific exploration of its depths” ran the July 19, 1991, story in the *Glasgow Herald*. A funding package of almost \$3.5 million is being assembled by the Natural History Museum (London) and the Freshwater Biological Association to “determine how the loch works and how it supports its plant and animal populations.” Project Urquhart, as the investigation will be known, acknowledges “that there have been a number of interesting observations at the loch which have yet to be explained” and that it “is highly likely that species new to science will be discovered.” As one who has had an interesting, if fleeting, observation while on the loch, and who thus runs the risk of being dismissed as a crank, it is a relief to find that the apparatus of scholarly research is finally being marshalled in a serious and determined effort to separate fact from fancy.

Over the years, there have been many attempts to pin down the elusive Nessie, some of which have produced interesting, if ultimately inconclusive, results. The Loch Ness “mystery” is researchable and certainly seems to provide a challenge for a variety of researchers, some more sophisticated and serious-minded than others. There is a hypothesis; there exists a variety of evidence, from folklore to home movie footage; there are many eye witness accounts, albeit of variable reliability; there are investigative techniques (from naturalistic to experimental) that could be used to determine the nature and scale of subaquatic life in the loch; and there is a range of technologies that can be wheeled into action (e.g., sophisticated sonar testing devices and image-enhancing systems to facilitate tracking and analysis). In that sense, Loch Ness has many of the features of a research problem. For some, there is a desire to know unequivocally whether Nessie exists or not; for many others, the answer, affirmative or negative, will sound the death knell for magic realism in the Highlands.

During 1989 and 1990, cold fusion was a hot issue. Pons and Fleischmann's high-profile media announcement of their “discovery” (via TV newscasts and the pages of the world's financial press) broke the unwritten rules of the scientific community. In the race to be first, Pons and Fleischmann cut corners, sidestepped the scholarly press, and

withheld information from their peers. The cold fusion saga shows what happens when commercial considerations (the potential payoffs from patentable discoveries in cold fusion would have been massive, as both the University and State Government of Utah fully realized) collide with the essentially cautious and consensus-seeking nature of the scientific communication process (Close, 1990).

This deviant behavior provoked physics labs around the world to put their claims to the test. Replication proved impossible, and the duo fled the limelight. Because cold fusion offers the prospect of cheap, safe, and abundant energy, Pons and Fleischmann's claims generated feverish and unprecedented speculation inside and outside the scientific community. Cold fusion became a research problem. And the mainstream scientific community responded with a battery of corroboration-seeking research.

PUZZLES AND DIFFICULTIES

And so to root definitions. There are at least two kinds of problems: puzzles and difficulties. Puzzles are things for which there are solutions (e.g., a crossword, conundrum, jigsaw, Rubik's cube); difficulties are things we have to cope with, but for which convincing or lasting explanations should not necessarily be expected (e.g., explaining apparent regularities in underlying macroeconomic behavior, dealing with the depletion of the earth's natural resources). We may not solve a puzzle for any one of a number of reasons (e.g., failure to grasp a clue; we misread the rules of the game), but in theory a puzzle is soluble. Not necessarily so with difficulties. Difficulties exercise our ingenuity; they are also relative. What is difficult for me may not be difficult for you. And the nature of difficulties may be redefined or better understood as a result of research (e.g., corn circles, quarks), but the fundamental problems (e.g., the nature of matter and of the universe) remain as challenging and resistant to full explanation as ever. Problems, then, can have final or potential solutions. The Loch Ness monster is more of a puzzle, while cold fusion remains, despite repeated failures to replicate the results, a difficulty.

In library and information science research, we have puzzles and difficulties. Reasons for collection nonuse, user failure at the shelf or at the catalog, and communication breakdowns in the reference negotiation process are puzzles for which in specific instances we should be able to come up with plausible explanations and solutions. Trying to define what we mean by information, or determine what constitutes the basic unit of information, or put a monetary-equivalent value on information are difficulties—they are the hardy annuals of research in

this field, and the best we can hope for is a greater appreciation of the complexities and nuances of the problem domain. Even the simplest library use survey is hamstrung by the difficulty of defining use in a meaningful manner: Surrogate measures (e.g., document exposure time) tell us nothing about the nature of the interaction between user and text; nothing about the amount of intellectual capital (if any) that was transferred; nothing about the degree of cognitive enrichment. The lack of a basic metric of information means that much of our research, and assumptions about the value of information interventions, rest upon questionable premises and approximate measures. Until now, research in our field has virtually ignored the motivational triggers that influence an individual's decision to use or not to use a particular quantum or parcel of information.

FIVE CONDITIONS

So back to the original question: "When is a problem a research problem?"—the wording of which seems to imply a need for more formalism and semantic precision than the Loch Ness and cold fusion cases provide. Sociologists of science have analyzed the ways in, and reasons for, which scientists select particular problems for research (Gieryn, 1978):

Problem choice is defined as the decision by an individual scientist to carry out a program of research on a related set of problems, or more simply, in a problem area. . . . *Problem area* is defined as the accepted knowledge and recognized questions associated with a substantive object of study or with an instrumentational means of inquiry. A problem area is made up of a number of related though discrete problems, and a number of related problem areas are said to make up a specialty. (p. 97)

This kind of definition begs our question: It is as if scientists merely have to dip their hand into a pork barrel and pluck out a problem topic from a predetermined set, safe in the knowledge that such problems are "substantive" or susceptible to "instrumentational means of inquiry." For a brief moment after Pons and Fleischmann's announcement, funds flowed into cold fusion research. Once the bubble burst, the funds dried up: Cold fusion was in effect ejected from the pork barrel, as the scientific establishment reasserted its control over its research agenda. The establishment's reaction can be viewed as either a perfectly natural self-correcting mechanism or as an exclusionary strategy. Ortega y Gasset (1960) would, I suspect, favor the latter interpretation:

All the individual sciences begin by marking off for themselves a bit of the Universe, by limiting their problem, which, once limited, ceases in part to be a problem . . . they start by knowing, or believing that they know,

the most important aspect of it in advance. Their task is reduced to investigating the interior structure of its object, its fine innermost texture, we might say its histology. (pp. 61, 77)

The question could perhaps be paraphrased as: "What conditions have to obtain for a problem to have research problem status?" Consider then the following five generic conditions: pragmatism, instrumentality, reliability, credibility, and allocation. These are offered as a tentative rather than a definitive listing. In the case of pragmatism, the following conditions have to be satisfied:

- Curiosity is stimulated. ("Why is it that . . . what would happen if . . .?")
- The answer is not to be found in the literature. ("We have the question, but not the answer.")
- Conventional wisdom is defeated. ("Beats me.")
- Research funds are available (the cart before the horse approach).

All of these can apply as much to fundamental as to applied research: Curiosity may be the driver of a basic research program (e.g., defining the nature or value of information) or the trigger for a piece of amateur problem-solving research (how do we make local business more aware of library services; how can stock utilization be increased?).

The second condition, instrumentality, is triggered when

- an issue is tractable.

Research is thus defined as that which is researchable, and a research problem is one that enables the apparatus of systematic investigation to be mobilized in order to probe and to analyze data/subjects/phenomena. This, of course, is a circular definition, but if the parties involved dispute what constitutes admissible evidence or procedure, the circle can be broken. The Logical Positivists, for example, would not admit any kind of metaphysical speculation. For them, there could be no God, therefore there could be no problem. And if there is not a problem, there is no need for research.

A problem (e.g., a problem of morals or ethical behavior) can exist independently of results or of research methods: The status of a problem is not dependent upon the state of the art in research. For the members of the Vienna Circle, a problem may be a pseudoproblem, while for others (like Ortega y Gasset) it may simply be a problem for which the answer does not yet (or may never) exist. There are problems in science and in the social sciences for which adequate tools and reliability measures are lacking (e.g., the definition and measurement of human intelligence), but the problems remain problems.

However, in big science, little science, and parascience, problems are only deemed to have been solved when the results can be verified.

Science, broadly defined, has its rules that must be observed. The quality and admissibility of evidence and the means whereby it was derived matter a great deal. Whether we are talking about a proportionately stratified sample of cancer sufferers with a matched control group in the context of a NIH-sponsored (National Institutes of Health) study or a local survey of randomly selected library patrons in a busy shopping mall, users of the resultant research are entitled to know the assumptions, survey methods, and confidence limits employed. None of the conditions listed below has anything to do with the status of a problem, but they will have a bearing upon the perceived status of the results arising from the investigation of the problem. Reliability (and legitimacy in the eyes of many peers) will only have been achieved when

- results can be reproduced (unlike those of Pons and Fleischmann);
- results can be generalized or reasonable extrapolation made (as with basic informetric laws [Bookstein, 1990a, 1990b]);
- methods can be applied in other contexts (portability);
- resultant models have predictive power (e.g., Zweizig's [1973] analysis of predictors of library use/nonuse).

Credibility is another dimension that merits consideration. If our lawyer, doctor, or realtor is confronted with a problem in the professional domain, we have certain expectations that he/she will apply his/her forensic or technical skills in a systematic way to resolve that problem (e.g., the Center for Disease Control's Guidelines for Health Care Workers "encourage research to identify modifications for medical, surgical and dental procedures and develop equipment to reduce the risk of injuries to workers that might result in exposure of patients"). Here, of course, we are generally talking about quite different kinds of problems and research from those characteristic of the world of science. Professionalism creates a certain set of assumptions and expectations, which, in my view, includes the ability and willingness to conduct research and to solve problems. The condition of credibility is thus activated when

- perceived professional status creates the expectation among client groups that problems can be resolved by the application of appropriate research tools.

In other words, both the public and funding bodies are entitled to expect that professionally qualified librarians would have a research capability and a commitment to improving their services through focused investigation and experimentation, typically via problem solving or developmental research initiatives.

There are many occasions when trade-offs have to be made: A doctor may be faced with a choice between saving the child's or the mother's

life; the librarian may have to choose between extended weekend opening hours and subsidized online services for the local business community. The trade-offs will not always be binary, but may involve an array of variables. In such cases, it may be necessary to carry out complex conjoint analysis to arrive at a weighted assessment of outcomes and implications. Research will therefore be necessitated when

- trade-offs are required (more of A and less of B, or vice versa?);
- questions regarding allocative inefficiencies are raised (what return on investment/yield is being generated?).

DEFINITIONS

But perhaps the problem is not so much with the word “problem,” as with the term “research.” The latter has acquired certain connotations (rigor, repeatability, measurement, etc.) and is powerfully associated with scientism in the popular mind. But this need not be the case. Overholser’s (1986) definition, with its distinction between probable and probative, is a helpful corrective to this kind of myopia:

Research is a far broader concept than science. Like science, it must be careful, systematic, insightful, persistent. But unlike science it need not be precise nor based on a theoretical construct, nor need it be subject to proof. Its findings need only be probable not necessarily probative. (p. RC-10)

And it is by no means a lone view. Patton (1986) offers an essentially qualitative definition of inference and extrapolation:

Unlike the usual meaning of the term ‘generalization’, an extrapolation clearly connotes that one has gone beyond the narrow confines of the data to think about other applications of the findings. Extrapolations are modest speculations on the likely applicability of findings to other situations under similar, but not identical, conditions. (p. 206)

For the library practitioner (at whom my remarks are addressed), this is reassuring stuff. Findings need only be “probable” and extrapolations are categorized as “modest speculations,” which in many working environments will be perfectly adequate to ensure that the results of research can be translated into actionable outcomes.

Let me illustrate not with a library case study, but by briefly describing an analysis of the strategic information needs of a Fortune 500 corporation’s sales and marketing division. Our brief was open-ended: We were invited to define our research agenda. Basically, we investigated how a large manufacturing company supported the technical, market, and product information needs of its sales and marketing headquarters personnel and of its nationwide salesforce. Information was gathered through on-site observation of facilities, technologies, and information resources, and through interviews with

senior and middle management and members of the salesforce. Convenience rather than representativeness was the criterion for selecting interviewees. We also drew upon a mass of background information on the company and its mainline competitors in order to provide contextualization.

What emerged, in a nutshell, was the centrality of field intelligence; intelligence that was unstructured, unvalidated, hot, speculative, and short-lived, and which was routinely gathered by members of the 250-strong salesforce. Our principal recommendations centered on the creation of a marketing knowledge base, which would pull together field intelligence on other players, products, technologies, key accounts, competitor pricing strategies, and third-party vendors, and permit this street-level information and intelligence to be integrated with other corporate information.

The study overthrew some of our safe assumptions about the importance of traditional information tools, sources, and resources in the context of a highly competitive and dynamic manufacturing environment. It highlighted the importance of social exchange, networking, and the leverage effect of distributed salesforce intelligence. In subsequent work for the company, we conducted a qualitative analysis of the impact of laptop computers on salesforce productivity (Cronin & Davenport, 1990). The study was to be two-part: part one predicting likely impacts and benefits; part two matching outcomes against benefits expected. For a variety of reasons the follow-up study could not be completed, but the insights that emerged from the exploratory phase (e.g., the longer term implications for space planning, property management, and relocation decisions) forced us to rethink the set of measures (hard and soft) that could be used to assess the downstream impact of support tools, such as laptop computers and cellular phones, on workforce attitudes, behaviors, and performance.

The conclusion to be drawn from all of this is that there is not (and probably does not need to be) a definitive answer to the question "When is a problem a research problem?" A more productive approach may be to consider how research (rather than problems) can be classified, and the following categorization is therefore suggested as a means of structuring essentially preliminary (and practicable) research ideas.

Contexts

Is the focal issue political, technical, managerial, scholarly, organizational, or personal in nature? It is important to be clear, as the answer will influence the style, conduct, and likely outcomes of the research. For example, a field-based survey of library nonuse among

ethnic minorities will require a different style and approach from a systems audit in the technical services department of a major research library.

The Problem

Here there is a set of epistemological questions to be addressed: What is knowable? What do you need to know? How do you know when you know? What is the nature of knowledge in the problem domain? What are the chances of the problem being solved successfully? For instance, studies that attempt to measure the value or downstream effects of information will need to consider these kinds of questions.

Purpose

What are you planning/hoping to do with the results of your research? Will it be possible to apply the results? How will they be used? In what form must the results be gathered so that they have value-in-use? What, for instance, is the rationale for monitoring traffic flow through service points if the ability and willingness to reschedule personnel or opening hours are absent? What is the point of bibliometrically analyzing the use made of a journal collection, if, for political reasons, weeding and justifiable cancellations cannot subsequently be implemented?

Techniques

Which research methods and techniques (e.g., naturalistic, historical, action, ethnographic, experimental, content analysis) are most suited to the problem at hand? What combination of approaches would be most potent? What special capabilities will be required? How amenable is the problem to conventional or traditional lines of inquiry? Are the techniques commensurate with the problem? What particular sensitivities need to be taken into account? For example, a survey of OPAC use could combine audit trailing with direct observation and structured interviewing. An evaluation of scholarly performance in a research university might collocate weighted publication data and citation counts with peer review and receipt of honors and awards (the partial converging indicators approach), rather than rely upon a single measure. But a word of caution is called for:

One should beware of researchers who collect research methods like others collect stamps and who tend to regard each project as an opportunity to add another method to their collection. (Moore, 1987, p. 10)

Validity

Validity can be of various kinds (e.g., construct, instrumental, apparent). What are the bases of inferential confidence you are employing? For example, what do citations measure, and can we legitimately count and compare such data? How reliable is the peer review/refereeing process? What precisely does the concept of relevance denote in the context of information retrieval?

Management

How is the research to be conducted: in-house and on a do-it-yourself basis, by hiring consultants, on a multiclient basis? On an agreed customer-contractor basis? Is the study premium quality or quick-and-dirty in character? Who “owns” the results?

Kind of Research

How should the research be characterized: basic, pure, strategic, applied, problem solving, developmental?

Time Horizon

What time frame is envisaged: short-term versus long-term; a one-off snapshot versus time lapsed; rolling versus longitudinal data gathering?

Even a nonexhaustive classification such as this can be beneficial. It helps you map out the range of research options in terms of inputs, processes, and outputs, and thus achieve a better fit between problem and investigative strategy.

PICKING PROBLEMS

Defining a research problem as anything that rouses curiosity, or as any activity for which research funds are forthcoming, is perhaps a trifle disingenuous. In effect, the flood gates are open, and almost any kind of puzzle or difficulty achieves research status. This may not matter greatly, though purists and the priesthood may sometimes bridle at what passes for research.

Why should librarians be interested in research? Such a question invites a potential litany of Motherhood and Apple Pie statements, but it can also be answered by the word “survival.” To quote Swisher and McClure (1984):

The myriad constraints which librarians must confront in the foreseeable future will demand greater accountability for decision making. . . . Research

that directly supports decision making is not an altruistic pursuit, only for those who have the time and the interest; it is a survival skill, essential for the continued vitality of library/information services. (p. xiii)

Line (1991), however, is less apocalyptic, preferring to speak in terms of a general research-mindedness or disposition, which, of course, results in the admission of virtually any kind of inquiry or investigation no matter how local, focused, trivial, or small-scale:

Practitioners need to look critically at all activities, past, present and possible future, and approach their work in a constantly experimental and enquiring frame of mind: what would happen if I tried so-and-so, how best can I do so-and-so, and how can I find out how well we are doing so-and-so and how well it worked? Research-mindedness should be an automatic mode of thought, a way of life. Not all of this will result in research, and much of it will be of purely local interest, but some will be of much wider interest. (p. 6)

With the justification firmly in place, the next step is to identify candidate problems that can be researched. Numerous checklists and guidelines can be found in the general survey research literature and in the literature of librarianship. Typical guiding criteria for applied or action research will include:

Actionability

Is change (as suggested by the research) within the control of the library, and can appropriate recommendations be implemented as desired? If we can't do something with what we've done, why do it?

Definition

Can the problem be clearly formulated and its essence conveyed to others?

Congruence

Does the problem under investigation relate strongly to the mission and objectives of the library or to those of the parent institution?

Centrality

Does the problem domain/focus account for a significant consumption of resources (human, material, financial, or technical) or is it of marginal concern?

Externality

Does the problem under investigation impact significantly on the activities, needs, or perceptions of users?

Utility

Is it reasonable to assume that the results of the research effort will have value-in-use?

Communicability

Can the import of the research results be transmitted clearly and effectively to the target audience in such a way as to ensure effective adoption?

AVOIDING PITFALLS

Assuming satisfactory answers are forthcoming for each of the above, the next step is to anticipate as many as possible of the pitfalls that await the unsuspecting researcher. If common sense is not enough, there are textbooks aplenty with solid advice on what to do and what not to do. The list of caveats and problems that follows is an adaptation of Swisher and McClure (1984):

Problem Statement

- Lacks focus . . . too diffuse
- Poorly expressed
- Low organizational relevance/salience
- Assumptions underlying the problem are ignored

Prior Art

- Failure to conduct cross-field literature searches (n.b., Swanson's [1990] concept of logically related but noninterconnecting literature sets)
- Unintentional duplication of research
- Ignores grey literature (e.g., in-house/unpublished studies)
- Not invented here (NIH) syndrome

Definitions

- Unanchored terminology
- Lack of consistency or precision in data categorization or analysis
- Terms may be defined but not operationalized (i.e., cannot be measured)
- Definitions at variance with existing standards (i.e., idiomatic usage)

Methodology

- No formal or agreed research plan/agenda
- Investigative methods/tools inappropriate to problem
- Failure to identify hidden costs
- Deficient know-how/technical expertise

Findings/Results

- Limitations of results are not stated explicitly (e.g., sampling error, confidence levels, experimenter bias, reliability)
- Significance or implications of findings not clearly perceived or stated
- Inability to translate results into actionable recommendations (e.g., politically unacceptable, nontransferrable across cultures)

Utility

- Presentation of results lacks clarity
- Results do not lead to improved organizational effectiveness
- Results evoke "So what?" response

SOFT FACTORS . . . SOFT CITATION

The kind of literature alluded to in this paper makes little or no reference to the role of intuition in either the conception or prosecution of research. Words like "aha," "eureka," "insight," "hunch," "epiphany," are noticeable by their absence. This is unfortunate. What we say elsewhere with respect to information management practice applies equally to the research process:

You cannot teach people intuition, but you can help them trust their own judgment by making them aware of how it has been formed, and of the biases and prejudices which are brought into play. . . . We believe that intuition is as valuable to management as scientism. The soft models we invoke (from metaphor to matrix) can be used to foster intuition. (Cronin & Davenport, 1991, p. 185)

Metaphor, for example, encourages people to see things in a different light, to seek out echoes and parallels, and to think laterally. It is a valuable modeling tool that can be put to good use in the formulation and conduct of research. The researcher who is a whiz at discriminant function analysis and linear programming may still lack the necessary sensitivity and flexibility to spot really interesting research issues or to interpret the full significance of his/her results. Research, in other words, is not a mechanistic activity (beware what Ortega y Gasset [1960]

calls the “terrorism of the laboratories”). Ideally, it combines an enquiring mind with investigative literacy. But let me illustrate what I mean about metaphor with a personal example.

One of my current research interests is exploring the social and cognitive significance of acknowledgments in the scholarly communication process (Cronin, 1991). Until now, the role and status of acknowledgments have been virtually ignored in the literature of our own field: Our attention has focused instead on citations and how they can be used to measure research performance and communication patterns among scientists and researchers. I have been carrying out citation studies intermittently for a decade, but I had never thought of the acknowledgment as a logical extension of my interest in citation analysis. I was not looking for a fresh research topic, nor was I trying to build upon my previous bibliometric work when it dawned on me that there was a degree of functional equivalence between citation and acknowledgment. When we cite another’s work we are, to a greater or lesser extent, acknowledging the influence of that individual’s thinking on our own cogitations. When we include a personal acknowledgment at the end of a published paper, we are making a public statement of gratitude for services rendered, which may be technical assistance, intellectual stimulation, or whatever.

Acknowledgments often function as “soft citations,” metaphorically speaking. The mere act of reconceptualizing acknowledgment as soft citation has dragged acknowledgment practice out of the penumbra and opened up a potentially rich research vein for myself and others. But, to return to the title of this talk, at no time did I view acknowledgment as problematic; at no time did I reflect on the problem status of research into the communication role of acknowledgments. Now, however, I can see that there may be a logical (even moral) problem in excluding acknowledgments, but not citations, from individual and institutional evaluation exercises, and that further research is called for if this apparent anomaly is to be resolved.

What qualities, then, are needed to be a good researcher? Apart from the obvious (e.g., proficiency in research techniques), I would cite three from my own experience: curiosity, passion, and deep knowledge of one’s field. But that is a highly personal view, one that reflects the fact that the longer I remain in this field and the more I learn, the greater the number of research topics that suggest themselves. But I shall leave it to my mentor and former colleague, John Martyn (Moore, 1987), to define the attributes that go to make up a good (funded) researcher:

What makes a good researcher is firstly a total determination to keep to the deadlines in the project, secondly a decent respect for the tax payers’ money that he or she lives on, thirdly a desire to do something genuinely useful as opposed to merely interesting, fourthly a combination of objectivity,

a legalistic view of what constitutes evidence, a mind open to different interpretations of what the evidence may mean and a lot of imagination, fifthly a degree of numeracy, sixthly the ability to write up the results clearly, concisely and preferably amusingly, and seventhly a well-developed awareness that most people, especially researchers, have got it wrong most of the time. (p. vii-viii)

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