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Shallow science or meta-cognitive insights:

A few thoughts on bibliometrics

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Correct citation for final version:

Holden, G., Rosenberg, G., & Barker, K. (2005). Shallow science or meta-cognitive insights: A few thoughts on bibliometrics. *Social Work in Health Care, 41*, 3/4, 129-148.

Keywords:

Informetrics, scientometrics, citation analysis, sociology of science  
tenure, promotion

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As preface to our reactions, we want to thank our colleagues for their insightful and reasoned commentaries on our work and the current state of affairs in bibliometrics. Individually and collectively, they have enhanced our undertaking on bibliometrics by raising issues and posing questions that we will respond to below. We will begin by addressing a general critique of the use of bibliometrics in social work. Then we will weave our responses to our colleagues comments into material that has either appeared since we wrote the three main articles in this volume or that we missed in our initial literature search. Our goal in doing this is to provide you with the most comprehensive and current view of bibliometrics in social work.

A broad criticism that has been raised regarding bibliometrics asks if it is simply 'keyboard driven, shallow science' (e.g., Kreuger, 1999). As Kreuger might inquire, are these bibliometric studies too remote from the basic mission of the profession? Perhaps. Bibliometric studies will not give us new incidence or prevalence data regarding new or existing conditions; nor will they describe the features of some new client population; nor will they tell us which practice intervention, research methodology or policy approach is most effective. Yet, are these the only questions a maturing field needs to ask?

As educators, it is important for us to understand what knowledge and which scholars appear to be having an impact on the larger community of scholars. Understanding the dissemination of knowledge and its acceptance by practitioners (translational research) has recently been a target of federal funding

(e.g., Hudgins & Allen-Meares, 2000; National Institute of Mental Health, 2000).

Understanding the dissemination and acceptance of knowledge by scholars is important as well. Why does some work enter into the discourse (as represented by citations) almost immediately, while other work reclines as virtual 'sleeping beauties' waiting to be discovered many years later (e.g., vanRaan, 2003)? Which scholars, journals, topics, and methodologies appear to have greater impact? Are there article structural factors, journal factors or author factors that predict impact (e.g., Meittunen & Nieminen, 2003)? Are there correctable biases in the publication process that can be discovered via bibliometric analyses? Are those in charge of the publication process (e.g., editorial boards) the most appropriate to carry out those responsibilities (e.g., Lindsey, 1976; 1992; Pardeck, et al., 1991; Pardeck, 1992a; 1992b; 2002; Pardeck, Chung & Murphy, 1995; Pardeck & Meinart, 1999a; 1999b). What scholarship enters into the profession via the textbooks used by students (e.g., Christopher, Dobbins, Marek & Jones, 2004)? In general, bibliometric data regarding the entry of an article into the profession's knowledge base, and its ongoing life therein, may provide insights about the scientific communication process that lead to improvements of that process.

How can we improve the development and dissemination of knowledge without study? Take the data reported by Green (2005) in this issue. A question that arises is whether it is satisfactory for the profession to move ahead thinking all is well (or not well) with faculty scholarship? Social work faculty may have had the sense that we do not publish much – but nothing focuses our collective

attention like an estimate of .28 articles per year. What explains this level of WoS article production? Although a multivariate explanation is obvious, it may be that social work faculty are writing and submitting articles for publication but that work is not being accepted for publication in WoS journals. If this assumption was true, faculty could, for instance, focus on improving the quality of the literature reviews conducted by doctoral graduates. This would likely lead to improved journal articles for years to come, and thus provide higher quality knowledge for practice. In our view, weak literature reviews in social work have been a self-limiting feature of the profession for years. Our hope is that by bringing additional data to the examination of social work scholarship via bibliometrics the field will improve its scholarship.

Although commenting on somewhat different facets of these topics, both Green (2005) and Ligon and Thyer (2005) ask if all of this effort on bibliometrics has real utility. One might ask in response: What would we know without the systematic information that bibliometrics has provided? Perhaps in the past a person who was relatively knowledgeable about social work scholarship might have known: that some journals, some authors, some schools, produced more articles than others; that some articles, some authors, some schools, were cited more frequently than others; that some editorial board members were publishing and being cited rather infrequently; etc. But there was probably no one in the field that knew all of the specifics and could convey them with the stark clarity that this accumulating body of bibliometrics data provides us. Is there utility

beyond such clarity? While we know of no data related to the following we would assume that increased use of bibliometrics in social work has led, and will lead, to more effective library planning, more attention to publishing articles in journals of higher quality (as defined by ISI), and a concomitant decrease in publishing in non-peer reviewed venues, especially books done with proprietary publishers.<sup>1</sup>

*New and previously undiscovered bibliometric work*

Since we finished writing the three primary articles in this special issue in January of 2004, the field of bibliometrics has moved forward. Although we had hoped to capture nearly all the work on bibliometrics in social work, we knew that we would not be able to cover all the potentially relevant articles in bibliometrics, and so therefore termed the review article a selective review (Holden, Rosenberg & Barker, 2005a). Since that time we have uncovered a few older articles that we missed and newer articles that have been published subsequently, which deserve mention.

New research has appeared that applies bibliometric analyses to a single journal (e.g., Quinones-Vidal, Lopez-Garcia, Penaranda-Ortega & Tortosa-Gil, 2004), as we did in our examination of *Social Work in Health Care* (Rosenberg, Holden & Barker, 2005). Early in the review article, we included a brief overview of applications of bibliometrics to topics in social work beyond the productivity and impact of individuals and academic institutions. Subsequently, we also came across Thomas's (2000) study which demonstrates a local application of

bibliometric techniques by a social work librarian. In an effort to provide empirical support for the library's journal acquisition decisions, Thomas examined masters degree theses at California State University: Long Beach, (see Nicholson (2003) for a more general discussion of *bibliomining*). She found 22,183 references to 1,964 journals in her sample of theses. An important finding from a librarian's perspective was that almost 25% of the social work journals in the library were referenced less than ten times. The sad finding was that the 11<sup>th</sup> most frequently cited source was the Los Angeles Times.

In terms of the use of bibliometrics in studies of journals, Sellers, Mathiesen, Perry and Smith (2004) compared journal rankings across various indices: ISI impact factors scores (for the year 2000) and ratings of the quality and prestige (a combination of familiarity and quality rating used previously by Cnaan, Caputo & Shmuely, 1994). Utilizing a survey (n=556, response rate = 26%) they examined 38 journals. Their data (extracted from Table 5) revealed statistically significant Spearman correlations of  $r_s = .49$  ( $p < .05$ ) and  $.45$  ( $p < .05$ ) between the journals' impact factor score ranking and the rankings of journal quality and prestige, respectively. The authors state that "[t]his finding is not surprising because the two approaches differ in terms of focus, emphasis, and audience" (p.156). They proceed to discuss the possible use of journal quality ratings by promotion and tenure committees. While we agree with Sellers, Mathiesen, Perry and Smith's spirit of multidimensional assessment for such decisions (and they do caution readers about using ratings as the main indicator

of the quality of scholarship), we repeat Frank's (2003) admonition from earlier in this issue:

Frank (2003) cautions us that because of inter- and intra-journal variations, citations to a scholar's articles are a better indicator of that scholar's work than the impact factor of the journals in which they are published (cf., Furr, 1995; Garfield, 1996; 1999; Seglen, 1997; Whitehouse, 2001) – (Holden, Rosenberg & Barker, 2005b, p. //).

In general, using quality ratings of journals (especially when it is unclear as to what time period respondents are rating) based on low response rate surveys, done at specific points in time (compared to impact factor scores computed yearly) seems even more problematic than using impact factor scores as a proxy measure for the quality of a scholar's work. Using either group level measure (quality ratings of journals or impact factor scores) as a proxy measure of the quality of scholarship of an individual author risks an incorrect inference because of the *ecological fallacy* (i.e., inferring something about an individual based exclusively on data from a group to which they belong).

Puckett's (2003) report on a study of authors appeared after our search attempts were completed for the three primary articles in this issue. He examined publications and citations for the 1998-2000 period for 215 university based Australian social work authors and their schools. Puckett's group of most frequently cited authors (n = 11) received an average of 12.6 citations for the period (min.-max.: 5-41; literal self-citations were excluded). In terms of quantity

of article productivity, this frequently cited group of authors published from 1 to 13 articles during the period.

Although we mentioned measurement problems in bibliometrics such as incorrect citation citations and spelling errors, we by no means fully explored this topic. Spivey and Wilks (2004) have opened a new line of inquiry in which they investigate the accuracy of reference lists in social work journals. They examined 100 references from five social work journals from the year 2000 (N = 500). They found that statistically significant differences in the number of errors per journal (*Social Service Review* had the lowest number of errors per reference). Conversely, they found no relationship between the age of the reference and errors, and no relationship between the number of authors (sole vs. multiple authorship) and errors. More recently, these authors have explored the perceptions of social work authors and editors regarding the accuracy of reference lists in journal articles (Wilks & Spivey, in press). This is clearly a line of research and possible intervention that would improve the validity of bibliometrics research.

#### *New research on problematic areas in bibliometrics*

A number of the problematic areas in bibliometric research that we have noted in this issue have also received recent attention. We will now discuss a few of those in greater detail.

*Data sources.* In our review article we mentioned a number of the problematic issues associated with the data sources used in bibliometrics. Hood

and Wilson (2003) have recently produced a more comprehensive and in depth treatment of these concerns. Although they tend to view the current state of the art as positive, they urge those doing bibliometric analyses to demand better data from the data sources they use (e.g., WoS, DIALOG). In fact, as we write this conclusion, we have begun to see announcements for a new database (Scopus from Elsevier) as well as changes to an existing data source (Thomson ISI's collaboration with NEC on a web citation index) that may mean substantial changes for bibliometric research (Hane, 2004; Quint, 2004). In a similar vein, Morrisey (2002) has made important suggestions regarding the development of *Uniform Author Identifiers* and *Uniform Concept Identifiers* that might improve the reliability and validity of bibliometric analyses.

*Scholarship coverage in the WoS.* While we pointed out potential sampling problems such as the fact that the WoS may not cover all the relevant journals or all of the volumes of journals that are included, we did not go as far in discussing these issues as Nisonger (2004). In this 'citation autobiography' Nisonger examined a variety of print and electronic sources as well as the web in order to determine the proportion of citations to his work in various venues. He reported that the WoS captured 44.6% of the citations appearing in print to his work (although as he notes this may be an overestimate because it may have not been possible to capture all citations outside of the WoS). Even though we don't know if this proportion would be similar to what we would find in social work,

Nisonger's work serves as a reminder of this limitation of WoS based bibliometric analyses (cf., Reid, 1995).

*Impact factor scores.* Journal impact factor scores are discussed in each of the three primary articles in this issue. They are defined and some of the pros and cons regarding their use are noted. Impact factor scores continue to prompt questions within bibliometrics though, and Garfield (2003) has recently published another set of responses to common questions about them. Although impact factors scores are determined in part by self-citations this topic has not been explored in social work to our knowledge (c.f., Anseel, Duyck & DeBaene, 2004 – re: psychology).

*Self-citation.* In this issue, Klein and Bloom (2005) note concerns regarding self-citation. Although we have proposed an alternative definition of self citation when using bibliometrics to augment academic employment decisions (literal self-citation vs. co-author citation), the common view is employed by Glanzel, Thijs and Schlemmer (2004) in their recent work in this area – that is if the citing and the cited article share at least one author, it is considered a self-citation. Glanzel, Thijs and Schlemmer found, in part, that for 1999 WoS publications (using a three year citation window), that the bulk of the science publications had authors from the U.S. and the proportion of diachronous self-citations was lowest in the U.S. (22.1%). In a follow up study, examining WoS publications from 2000, Glanzel and Thijs (2004) found that the category of fields containing

social work (Social Sciences I (General Regional & Community Issues)) had a diachronous self-citation rate of 23%.

Similarly, Gami, Montori, Wilczynski & Haynes (2004) examined citations to a sample of articles on diabetes published in the year 2000. The rates of diachronous self-citation were slightly less than those reported by Glanzel and Thijs for the U.S. for 1999 (mean 18%, median 7%; c.f., Kovacic & Misak, 2004).

The evidence from prior research including our own, combined with this new work strengthens our belief that the view noted by Glanzel, Thijs and Schlemmer (2004) is correct.

In the bibliometric literature, there is an ongoing debate on the interpretation and role of author self-citations in the process of scientific communication. This debate has resulted in a certain polarisation. Particularly, users in science policy, but sometimes even the researchers themselves are condemning author self-citations as possible means of artificially inflating citation rates and thus of strengthening the authors' own positions in the scientific community. Bibliometrists are, on the other hand, inclined to regard a reasonable share of author self-citations as a natural part of scientific communication. According to this view, the almost absolute lack of self-citations over a longer period is just as pathological as an always-overwhelming share (p. 63).

Based on their findings, Glanzel and Thijs (2004) conclude "at the macro level - there is no need for excluding self-citations in evaluative bibliometrics" (p. 310).

We continue to believe that self-citations are often appropriate and therefore a less problematic aspect of bibliometrics than some authors assert. Regardless self citation is an aspect of scholarship that deserves further study in general as well as particular attention in academic employment decisions.

*Multiple authorship.* The issue of multiple authorship was addressed in our articles in this issue as it has been by many preceding authors. Trueba and Guerrero (2004) recently presented the development and testing of a new approach to this problem based on a view very similar to the view used in the development of our *Multiple Author Qualifier* (MAQ), which we apply to both authorship and citation counts. Their *Refined Weights* ( $W_f$ ) approach differs from ours in the way they derive and test the credit assigned to authors at various positions, and also in that  $W_f$  credits the first, second and last authors differently than the MAQ and more than their own "uncorrected" formulas would (these differences are clearest when there are more than four authors). Trueba views this method as superior to the MAQ approach and also thinks that the calculation of the MAQ weights could be more precise (Personal Communication, 6/21/04).

While there has been some discussion about the possibility that senior authors may cede position to junior authors to help them out even though they were more substantial contributors than their authorship position conveys (c.f., Epstein, 2005), we have seen no evidence regarding this practice in social work. Furthermore, if it does occur, those senior authors should be discouraged from

this practice because it violates an important norm of science that we discussed – that is that authorship credit should be assigned according to the relative contribution of the authors. To do otherwise simply clouds the reader's (and citation analysts') understanding.

*Theory.* While our articles spend virtually no space on theory of bibliometrics that does not mean that work is non-existent in this area. Beyond the references provided, the interested reader should see Small's (2004) recent tribute to Robert Merton, in which he continues Merton's normative view with his presentation of a citation classification system.<sup>2</sup>

*Meaning in bibliometrics.* In our review article we noted misspellings as well as factors such as authors who could "be citing work that is incorrect, not citing the best work, not correctly citing satisfactory work or may be failing to cite work that influenced them" that might present problems (Holden, Rosenberg & Barker, 2005a, p. //). As we were about to finish this manuscript a colleague gave us an intriguing paper to consider that suggests a problem we did not address directly (T. Festinger, Personal Communication, 5/4/04). Simkin and Roychowdhury (2003) report an application of bibliometrics as a challenge to bibliometrics (cf., Simkin & Roychowdhury, 2004). Their reasoning is that if an incorrect citation is repeated in subsequent papers, those repeated mistakes represent instances where the citer did not read the original article. Morrisey (2002) has referred to these instances as '*hollow citations*'. Based on the analysis of a highly cited physics paper, Simkin and Roychowdhury conclude that: "[o]ur

estimate is that only about 20% of citers read the original" (2003, p. 269). While we disagree with some of the premises and the conclusion of Simkin and Roychowdhury's work, they do a great service by raising yet another potential caution about the use of citation analysis. Even though they may not demonstrate the low level of reading primary documents that they claim, Simkin and Roychowdhury prompt us to reconsider what we think a citation indicates. How can we assume that a citation is an indicator of impact if the writer did not read the paper?

It goes back to the general question that a number of our colleagues in this issue raise in one form or another. What do these bibliometric measures mean (e.g., Epstein, 2005; Kirk, 2005; Klein & Bloom, 2005; Ligon & Thyer, 2005)? As with many constructs in the social sciences, we know that, validity is an ongoing issue (c.f. Spriggs & Hansford (2000) for a discussion of the psychometric properties of Shepard's Citations for legal research). Aksnes and Taxt's (2004) recent findings regarding the positive relationship of bibliometric indicators to expert ratings builds on previous validity studies. Oppenheim's (1997) findings (which replicate his earlier studies) provide yet another example. He found high positive correlations between the U.K. Funding Council Research Assessment Exercise ratings (expert panel ratings where higher scores equal greater excellence) of university departments and the number of citations received. Regardless, more psychometric work on bibliometrics indicators is needed.

Klein and Bloom (2005) criticize our work on another measurement issue.

They state:

For example, these authors claim to assess “impact.” And yet, even as they share that bibliometrics offers but one indicator, they are prompt in operationalizing this construct in terms of citations to a given work. The results of this operationalization may be an instance of what Donald Campbell has termed the “unmitigated disaster [of] the advice to employ designated operational definitions for theoretical terms” . . . This process results in the richness of the term “impact” being reduced to an enumeration of citations (//)

We agree that operational definitions should be clearly distinguished from the concepts that spawned them. Operationalization involves moving from the concept to the specific indicators of the concept. On reflection, we think the problem described by Klein and Bloom is more the lack of clarity in our writing than in our operationalization of impact. In our review article (Holden, Rosenberg & Barker, 2005a), we stated that the focus is on *impact operationalized as citations to journal articles*. In that article we also state that “[c]itation analysis may not reflect the impact of unpublished scientific work or the impact a journal or article has on professionals who are reading it (but not writing and citing it)” (Table 1, p. \ \ ). Similarly, in the SWHC article we note:

While the authors fully understand that impact can take many forms, in the current study it has been narrowly conceptualized as the impact of

articles, operationalized as citations . . . That is, the number of articles that cite the target article. Criticisms of this approach will be considered in the Discussion section. . . . In terms of caveats, some readers may be thinking that the current study misses some of the impact produced by social workers' ideas. It does. Social workers' ideas have impact on the field via activities such as discussions with students and colleagues; teaching and supervision; presentations at a local, national or international conferences; publication in newsletters, monographs, books or in a variety of Internet outlets. But the mechanisms for studying the impact of such venues are less developed (p. | | |, // /).

Impact does entail more than citations in the WoS – but any study circumscribes its focus. The article describing that study should explain the operationalization, the authors' justifications for the approach and the implications of those choices. Our work could have been more clearly explained.

It is clear that adding bibliometric analyses will not remove all subjectivity from academic employment decisions. But we do think that bibliometric analyses can help us to increase the ratio of empirical to subjective factors in these decisions. Kirk asks (2005)

what is a personnel committee to make of the fact that a candidate's MAQ adjusted total cites per year is .77, or 1.77 or 2.77? Their usefulness is only in relation to some standard that may provide some meaning. We don't yet have such standards and so we are left with ambiguities (p. //)

Most approaches require a determination of the performance of an individual and dichotomous decision about that performance (e.g., hire / do not hire; tenure / do not tenure; promote / do not promote). There are always standards in the mind of the decision maker. Our complaint is that in the current situation there is excessive murkiness on both the performance assessment and the standards side. *Our proposal simply seeks to reduce the murkiness on the performance assessment side.* There are some normative data regarding faculty publications and citations in social work and more is needed (e.g., Bloom & Klein, 1995; Green, 1998; Green & Hayden, 2001; Green, Baskind & Bellin, 2002; Klein & Bloom, 1992; Rothman, Kirk & Knapp, 2003). Our suggestion is an incremental approach to the problem and we do recognize its limitations and set forth these limitations explicitly, for example, in the case of junior faculty (cf. Klein & Bloom, 2005). Furthermore, we agree with Aksnes and Taxt:

Our results indicate that a bibliometric analysis can never function as a substitute for a peer review. However, a bibliometric analysis can counterbalance shortcomings and mistakes in peer judgments. In this way a bibliometric study should be considered as complementary to a peer evaluation (p. 40).

*The future of bibliometrics in social work.* Along with methodological evolution comes topic evolution. Glanzel (2002) categorizes the current topical areas succinctly.

Present-day bibliometric research is aimed at the following three main target-groups that clearly determine topics and sub-areas of "contemporary bibliometrics".

*Bibliometrics for bibliometricians ("Basic research" in bibliometrics)*

This is the domain of basic bibliometric research and is traditionally funded by the usual grants. Methodological research is conducted mainly in this domain.

*Bibliometrics for scientific disciplines (Scientific information)*

The researchers in scientific disciplines form the bigger, but also the most diverse interest-group in bibliometrics. Due to their primary scientific orientation, their interests are strongly related to their speciality. . . .

*Bibliometrics for science policy and management (Research evaluation)*

This is the domain of *research evaluation*, at present the most important topic in the field. Here the national, regional, and institutional structures of science and their comparative presentation are in the foreground (no p.).

Increased use of bibliometrics will likely bring a number of outcomes, including:

- 1] evolution of bibliometric methods (c.f., Klein & Bloom, 2005)
- 2] increased sophistication of critiques
- 3] changed citation behavior in the scholarly literature

Consider this third potential outcome. Science moves forward aided by the corrective influence of professional norms including personal, professional society and funding agency sanctions for misconduct (c.f., Merton, 2000). As new or old methods (e.g., bibliometrics) are employed, certain types of misconduct may be illuminated. While we have suggested increased use of bibliometrics for hiring, retention, promotion and tenure decisions (Holden, Rosenberg & Barker, 20005b), we are well aware that such calls may prompt misconduct, as some individuals might try to influence the outcome of such decisions by engaging in inappropriate citation behavior. Yet at the same time, more refined bibliometric methods will provide more precise descriptions of authors' citation behavior. While the motivation to cheat the system may increase as use of our system (or other systems) increases, increased use and refinement of such systems would similarly increase the chances of detecting cheaters and thereby reduce motivating for cheating. For instance, in this volume we introduced both a refined citation statistic (self-citation split into literal self-citation and co-author citation), as well as the MAQ adjustment for multiple authorship in response to issues like 'inappropriate self-citation' and 'inappropriate assignment of authorship.' For instance, if committees routinely used the MAQ, authors would have less motivation to get themselves added as additional authors on multi-authored articles. While having some superficial logic it remains to be seen if this discussion represents anything more than idle speculation.

Kirk (2005) and Green (2005) ask the important question which all of our colleagues ask directly or indirectly. That is, will the proposed method that is designed to improve academic employment decisions actually do so? As comments within this volume demonstrates, tenure criteria linked to bibliometrics elicits a range of reactions but caution appears to be commonly threaded throughout. At the same time, it is notable that there is an unhappiness expressed in terms publishing and social work's scholarly productivity. These concerns are hardly accidental and demonstrate an underlying tension in the evaluation of productivity. In our view, productivity in academe should not be measured strictly by "counting," or with other measures that are similarly corporate in nature. Academe exists so that smart people can "think" about the world, reflect, and write. We believe that bibliometric methods are a natural extension of those activities. Still, more research is needed to better understand the actual utility of these methods.

In conclusion, while continued growth and development of bibliometric research outside of the field of social work seems probable, the likelihood of such growth within the field is uncertain. We hope that our efforts and the thoughtful comments of our colleagues in this volume will add to the knowledge base that social work researchers will use to make decisions about whether or not to pursue bibliometric studies in the future. We look forward to learning what happens to these ideas.

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<sup>1</sup> This last possibility is perhaps the most important, because until academics focus their publication efforts on peer reviewed articles in venues which can be wrested from the grip of the for profit-publishers (see Harnad (2001) re: university based pre-print servers), we will continue to have less control over the scholarly publication process than we rightly deserve given that we are discussing our intellectual product.

<sup>2</sup> We found Epstein's (2005) comments about Merton consonant with others' allegories. Although exploration of Merton's conceptual contributions to bibliometrics is beyond the scope of this article, it should be noted here that Cole (2004) has just reported data that demonstrates that Merton was larger than life.

A few years later when I was his teaching assistant, it occurred to me and to his other students that Merton seemed larger than life. Consistent with my training, I tested that hypothesis in a survey of students in that course, Analysis of Social Structures. Over 150 responded to the question: How tall is Robert K. Merton? There was little variance in opinion. The class average had Merton at 6 feet 3 and a-half inches in height -- a full two inches taller than he actually was. It was true. Merton was, in fact, larger than life (Cole, 2004, p. 39).