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EMANCIPATING SCHOLARS: RECONCEPTUALIZING SCHOLARLY OUTPUT

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Abstract

This research describes the current state of a scholarly publication machine that is highly dependent on journal rankings. Through a critique of the current system and the methodologies used to measure the notion of quality in scholarly research one can discover that the current system is set up to limit a system of open democratic discourse. The process of journal ranking is inherently political and we show how the use of these rankings can stifle the discourse, thereby allowing only a select elite few to be participants. By identifying the constructs of ideational influence and social influence we attempt to create a composite measure for scholarly quality. We draw from past works on communicative theory and democratic discourse to propose a system that has greater transparency, more equal access, open participation, increased truthfulness, and lower power differences.

Keywords: critical social theory, democratic discourse, scholarly influence, ideational influence, social influence, institutional influence, Habermas

Introduction

In all academic institutions the evaluation of scholarly output determines faculty recruitment, retention and re-appointment, promotion, funding, and tenure decisions. This process has pragmatic consequences when comparing the influence of one researcher, or of a set of researchers, to another researcher, department, or discipline. Indeed, scholarly evaluation impacts on the very survival of academic programs.

In the absence of a true 'theory of quality', the pragmatic standard of publication in "A" journals has become the de facto metric. As a consequence, the literature has become a battleground over the development of a gold standard for academic journals. The situation has become one in which different groups use different surrogate measures to compare the 'quality' of one researcher, collection of researchers, or of one journal to another. Measures such as journal rankings, rejection rates, citation counts, impact factors, and other bibliometrics are used to assert the supremacy of a particular journal venue.

But a growing literature also exists expressing concern over the 'received' measures in common use complaining that they are biased or merely schemes to preserve power regimes already in place (Chua, Cao, Cousins, & Straub, 2002; Gallivan, 2009; Singh, Haddad, & Chow, 2007; Truex, Cuellar, Takeda, & Vidgen, 2011; Walstrom, Hardgrave, & Wilson, 1995). The most critical of those is Macdonald and Kam (2007) who identify essential inconsistencies and circuitous logics undergirding all of the received approaches. Our research proposes that extant approaches are reified by repetition and are power laden and biased (Truex III, Cuellar, & Takeda, 2009). We believe that these issues stem from the nature of the construct being measured. We argue that 'quality' like 'success' is a slippery and socially constructed concept, not an objective measure. We submit that, rather than assess quality by the venue of publication, we might better assess quality by examining the behavior of the research community. By looking at that which the community references in their research, we may determine those publications that the field views as being influential. Accordingly, by substituting the concept of "influence", i.e., the uptake of ideas by the field, we democratize and improve the evaluation of scholarly output. The improvement in the evaluation approach arises from the use of the aggregate 'vote' by the entire field rather than by the dictates of an entrenched minority.

This research is therefore aimed at identifying and developing a composite (multi-dimensional) set of measures allowing a transparent, fair, replicable, and comparable indication of scholars' influence. To date we have identified two classes of measures: those we call ideational influence measures, that represent a scholar's productivity of published works and the uptake of the ideas expressed in those works by the community of scholars, and social influence measures, representing the position in social co-authorship networks. Taken together we contend that these measures are closer to the four conditions – equal access, open participation, truthfulness, and low power differences – required in an ideal communicative act as proposed by Jürgen Habermas (1985a, 1985b) and described as a 'democratic discourse' Mingers and Walsham (2011).

Hegemony – the prevailing view of academic quality

Nowadays in most academic circles the bankable unit of academic capital is the publication, and it is operationalized by two questions: "How often have you published?" and "Where have you published?" (Dean, Lowry, & Humpherys, 2011; Karuga, Lowry, &

Richardson, 2007; Lowry, Karuga, & Richardson, 2007; Lowry, Romans, & Curtis, 2004) In this view the production of publications is valued, but the strength of publications is either discounted or enhanced by the venue in which those publications appear. And often, the publication venue is a surrogate for the presumed quality and the value of the paper itself. With "…some journals simply hav[ing] more quality than others." (Macdonald & Kam, 2007, p. 641). This approach is a pragmatic concession to the fact that it is impossible for the evaluators of scholarly output, who are often in different fields, to properly evaluate the quality of articles and render an informed decision on the merits. The system has therefore evolved to defer to the editorial process of journals on the theory that editors and editorial boards are experts in the field and should be able to determine whether an article is of high quality or not. Certain journals have been designated as "premier" journals, others as "high quality" with the rest presumably not so high quality. While this pragmatic approach conceptually seems to provide a rigorous yet very time efficient process for evaluation of the quality of articles, we find that it is subject to very serious flaws in execution. These flaws are seen to be 1) an inability to articulate the concept of quality, much less operationalize it; 2) issues with selecting which journals are of high quality; 3) the inability of the journals to select and publish "quality" papers; 4) the opening of the process to biases; and, 5) Deleterious effects on research and publication resulting in the closing of the discourse.

Academic quality: fitness for purpose or transcendence?

The fact that there is no theory of quality or operationalization of quality that has been advanced for academic literature (Dean, et al., 2011; Locke & Lowe, 2002; Straub & Anderson, 2010) is generally conceded. In perusing the literature on evaluation of scholarly quality, we find that the construct is constantly referenced, but is always used in an implicit manner. Quality is not defined but assumed that what is meant by the term is understood and accepted. This definition of quality is defined by Garvin (1984) as the *transcendent view*.

A *transcendent* view considers quality to be something that cannot be articulated; we know a quality product when we see it but find it hard to pinpoint particular characteristics that make it a quality product:

> "Quality is neither mind nor matter but a third entity independent of the two … even though quality cannot be defined, you know what it is." (Pirsig (1974) as quoted in Garvin (1984))

The transcendent view of quality is concerned with quality aesthetics and can be likened to primitive Platonic concepts such as "beauty" and "truth"; by being exposed to a succession of quality objects we develop *a sensitivity* for quality. Editors may indeed recognize good research when they see it, as a result of the sensitivity they have developed through exposure to many research publications. This is indeed the approach of many editors. For example, Straub and Anderson (2010) indicate:

> let us suggest that a concept like journal quality lies almost completely in the minds of scholars because quality itself is highly abstract, …. Without clearly mapped physical markers, we can come up with a set of metrics that will approximate this construct, but never tap into it without a large dose of humility … . It is not even remotely similar to the construct of something physical like ball bearing quality, where we can measure with small degrees of precision the variances of machine tools in creating the balls, their housings, and the processes that assemble these (Straub & Anderson, 2010, p. x).

Given that there is no generally accepted or even articulated theory of the quality of scholarly output (at best a transcendent concept of quality is applied), the designation of some journals as "quality journals" may be, in large part, a political decision. There exist many justifications and descriptions of the process by which journals are selected and admitted to different journal ranking lists; e.g., the AIS 'basket of six or of eight', the ERA lists in Australia, the ABS list in the UK among others. (ABS, 2011; AIS, 2011; ARC, 2011; Barnes, 2005; Lowry, Humpherys, Malwitz, & Nix, 2007; Lowry, et al., 2004; Mylonopoulos & Theoharakis, 2001; Nerur & Sikora, 2005; Peffers, Avison, Ein-Dor, & Zmud, 2003; Rainer & Miller, 2005; Schwartz & Russo, 2004; Serenko & Bontis, 2009; Walstrom & Hardgrave, 2001; Walstrom, et al., 1995) In general these lists are assembled by "panels of experts" that have been invited to adjudicate. But, as there is no theory of quality for academic literature, there has been no empirical study that has determined the level of quality of a set of journals by objective measures. How did journals such as MIS Quarterly (MISQ) or Information Systems Research (ISR) get the reputation of being "quality" journals? At some point, some suitably influential elite entity designated these journals as quality journals and their dictum was accepted by the field. Their opinion has been perpetuated in the field by inculcation of the notion during doctoral training, reinforcement during the review process, and rewarded by the tenure and promotion process. We refer to this process as reification by repetition. The constant repetition of this opinion in many different places and channels reinforces the received opinion. Additionally, the subtle, but highly significant, linguistic turn from referring to 'journal ranking lists' as 'journal quality lists' is given as true and accurate via appeals to the power of definition possessed by these "panels of experts."

In their advocacy of the transcendent view, Straub and Anderson (2010) specially reject another of Garvin's view of quality: quality as product based or conformance to specification. We concur with this rejection, as it is difficult to the point of impossibility to establish a "specification" for quality output. However there is another of Garvin's views that might be a more satisfactory definition of quality; that of quality being defined as "user-based" (Takeda, Truex III, & Cuellar, 2010; Truex, et al., 2011; Truex III, et al., 2009). This view of quality is concerned with meeting the expectations of the customer. Juran (1974) holds that 'quality is fitness for use', a definition that is followed supported by Feigenbaum's (1983) view of quality as:

> "The total composite product or service characteristics of marketing, engineering, manufacture, and maintenance through which the product and service in use will meet the expectation of the customer."

When considered this way, academic literature can be considered as having quality if it is taken up and used by other members of the field, i.e. cited. In this view, a citation is a "vote" or an expression of belief in the quality of the paper for some use, such as the basis for construction of a theory, an example of the use of a theory, source of data for an argument, etc.

In other publications, we have proposed more open and measurable criteria to judge scholarly influence as 'ideational influence' and 'social influence' (Cuellar, Takeda, & Truex, 2008; Takeda, et al., 2010; Truex, et al., 2011; Truex III, et al., 2009). This criterion could be considered to be in line with the 'fitness for use' or 'user based' definition of quality. In this paper we explore how we might widen and 'democratize' the question of quality. We hold that the user-based view of quality is most pertinent, i.e., the research should meet the needs and expectations of the 'customer'. This is particularly true when the user base is an educated, well-informed, and engaged group capable of making judgments about the efficacy and value of a given paper. In this instance the notion that the 'wisdom of the crowd' may provide a meaningful surrogate for the value, if not quality, of a paper must be considered (Surowiecki, 2004, 2005; Surowiecki & Silverman, 2007). An immediate consequence of taking this view is that research that is not used, i.e., not built upon by other researchers and not applied in practice, cannot – by definition – be of high quality, regardless of where it is published.

Issues in Journal Selection

This transcendent approach to quality is visible in the methods used to identify journal ranking lists (Karuga, et al., 2007; Lowry, Karuga, et al., 2007; Lowry, et al., 2004). 'Premier' journals are usually chosen by surveying selected members of the field. In this method, there are a number of biases that occur. Survey respondents are authors, editors, reviewers and potential authors, editors, and reviewers, who might have the interests of their own journals in view. Also, when making their ratings, respondents may take other considerations into account. Respondents may consider differential weights given to rigor vs. relevance, methodological approaches, as well as personal preferences and whether they have been published in the journal (Podsakoff, MacKenzie, Bachrach, & Podsakoff, 2005). Additionally, Walstrom, Hardgrave, and Wilson (1995) found that underlying discipline, familiarity, and research interest influenced ranking decisions. Survey approaches are also biased by the 'anchor' effect. The 'anchor' effect occurs when respondents are given a list of journals to select from, in this case, they are inclined to select from the list provided rather than add other journals to it. This holds even when the list is incomplete and the respondents are given the instructions to add any journals that may not be on the provided list (Chua, et al., 2002).

Errors in Selecting Quality Papers

Even after lists of "quality journals" are selected and anointed as 'the best', there exists evidence that these journals are consistently not very good as selectors of "high quality" articles. Singh, Haddad & Chow (2007), using citation rates, have argued that the "top-tier" management journals have significant error rates in selecting the best articles. They found the type 1 error rate was approximately 25%, i.e., a quarter of articles accepted for publication in a top-tier journal were not those considered to be "high quality" by the field. They found that 63% of "top articles" were not published in the "top-tier" journals either due to type 2 errors (a deserving article is rejected by a top-tier journal) or because the authors submitted it to a "lesser" journal to begin with.

Additionally, assessing scholarly ability based on publication in "quality journals" has other deleterious effects and often leads to circuitous reasoning in justifying this approach. Table 1 below, derived from Macdonald and Kam (2007), summarizes prevalent claims and the ways that the reasoning may be suspect. Macdonald and Kam also provide compelling arguments as to why the mindless goal of publishing in only the "quality journals" leads to some egregious examples of gamesmanship that in the end undermine the notion of quality scholarship and negatively impacts the journals, the scholars, and the discipline.

Warrant to Journal Quality designation	**Circuitry of the rational**	**Implications**
Authors from Top Business Schools (only/primarily) publish in Top Journals	Top business schools are often ranked 'top' in part because of the journals in which their faculty publish	Narrows the set of acceptable venues, constrains the discourse towards commonly accepted topics and values
Quality journals publish authors of the highest reputation	Author reputation is often determined by publishing in quality journals	Topics, research questions and ultimately research programs become constrained by a focus to what is safe and publishable in those venues.
Quality journals contain quality papers	Papers are deemed to be quality papers because they are published in quality journals	Journals are fallible. Not all papers in top-quality journals are of the same quality or value.
Quality journals have high rejection rates	The more authors are encouraged to submit only to high quality journals the more rejection rates will increase	As rejections rates exceed 90% the reliability of screening plummets (Miner, 2003) High rejection rates fosters gaming the system.
Highly cited papers are of high-	High citations may be self-	Leads to a concentration of

quality	referential. The more a paper is cited the more likely it is to be cited again: it is cited because it is cited. (de Solala Price, 2005; Redner, 2005)	citations to certain venues and gamesmanship by editors in expecting authors to cite work from the journal they are submitting to.
Powerful and influential departments have journal quality lists	Copy-cat lists. Lists are often created in reference to other lists. The likelihood of being on a list is increased by being on the list of another influential school or program.	Journals are often affiliated with departments and schools. The danger is that authors affiliated with those programs may receive preferential treatment, and that 'recognition swapping' on lists occurs between schools or programs.
Peer review guarantees quality. The best peer reviewing is at the best journals	Peer reviewing is fallible - type two errors can occur anywhere; Nobel laureates have been denied publications in top journals.	The reviewing system is stressed: too few reviewers; many papers are screened out by editors (not reviewers) as "The only practical way to maintain high rejection rates" and to cope with the volume of submissions. Editors know the authors so blind refereeing is not absolute. Choice of referee can doom or bless a paper.
Quality journals publish only the most important research.	Quality research is that published in quality journals.	Some evidence exists that quality journals publish mainstream rather than breakthrough research.

Table 1: Circuitous logics underlying claims to journal quality (Macdonald & Kam, 2007)

Some examples of this gamesmanship include behaviors such as self-citations, citation swapping, small community citations, and citation rigging. Self-citations happen when authors abuse the ability to cite themselves in order to garner citations counts for their previous work. While in some cases self-citing to extend your own work is a legitimate method of citing, the difference between legitimate use of self-citations and the abusive use of self-citations is hard to differentiate. Citation swapping and small communities of citations are similar to self-citations but are with larger number of researchers. With citation swapping two researchers are in cahoots to cite each other to gain citation numbers and with a small community of citations there are several researchers citing each other. With citation rigging an editor or reviewer of a journal or conference might 'suggest'

to an author that they need to add several citations, typically articles of the reviewer or editor or articles that were published in the target journal or conference. While these citations may again be legitimate, sometimes this is a tactic used to increase citations for the target venue and/or the authors being suggested by reviewers and editors.
Because of these concerns there are schools and promotions and tenure (P&T) committees that eschew publication counts and journal listings in favor of actually reading papers and making direct judgments of those individual contributions despite the cognitive burden and difficulties associated with understanding the concerns, problems, research topics, and research nuances in other disciplines. Never the less, in most schools and for the field as a whole the journal ranking list and publication counting metric still holds sway.

Errors Bias in Reviewing and Editing

Beyond errors in selecting the journals and errors of the journals in selecting "quality" papers, there is the issue of bias in editing. Our field is no more immune to some of the darker features of human nature than any other discipline. For more than 30 years studies have described how the reviewing and academic selection processes are not blind to personal demographic features and, for instance, "that peer reviewers cannot judge scientific merit independent of gender" (Wennerás & Wold, 1997). Gender stratification operating "… through formal and informal relationships" (Light, 2009) and continuing gender differences in academic output (Symonds, 2006) are both thought to "contribut[e] to the inequality we continue to find in academic science." and continues to be a subject of investigation in reference disciplines (Betsey, 2007; Cole & Zuckerman, 1984a; Cole & Cole, 1973; Davenport & Snyder, 1995; Zuckerman, 1970, 1971). The subject of academic reviewing and editorial bias likewise continues to receive research attention (Betsey, 2007; Cole & Zuckerman, 1984b; Menges, 1983; Perna, 2001) in mainstream sociological and higher education journals in the IS literature (Gallivan & Benbunan-Fich, 2007) as well as those venues focusing on race and gender issues (Xie & Shauman, 1998).

Blind and double blind procedures combined with strong codes of ethics try to protect against these temptations. However, the authors submitting papers are known at the editorial level and it is unsurprising that people tend to favor those authors and topics they know, or simply that relational networks matter in the reviewing process and the publication game in general. This is sometimes summed up in the simple statement: "You don't submit papers to journals; you submit papers to people."[1]

Given the problems associated with using venue of publication to evaluate the quality of articles, we concur with the conclusion of Singh, et al. (2007) in which they state of this methodology to evaluate management journals:

> *Taking the preceding results as a whole, the conclusion is inescapable that using journal ranking (specifically, distinguishing between the top five and non-top five) can lead to substantial misclassification of individual articles and, by extension, the performance of the faculty members who authored them. (Singh, et al., 2007,p. 327)*

The Discourse is Closing –A Critical Theoretic Concern

The current process is therefore problematic because it puts so much power in the hands of a few who are relying on arbitrary and unmeasurable decision-making criteria. It has also produced a noticeable closing of the discourse. Despite the stated goal of the process to

[1] This was a direct quote from a very powerful and influential colleague, who was referring to the fact that paper submissions should not be thought of as being sent to impersonal 'journals' but rather to editors and reviewers. While the intention was constructive the statement is we believe still prescient and reminds us that the process of being published is still, in some significant measure, a social process.

identify “quality” scholarly production, in the absence of a theory of quality the pragmatic standard of publication in “A” journals has become the measure of quality. Hence, publication in the right places has become the capital of the academic realm.

The problem with this approach is that, to hold its value, some attribute of the good must be scarce, i.e., its availability must be limited. Such limitation may come in several forms: limited production, limited supply, limited access to the good, or adding restrictions making only certain goods of value. The scarcity of “valued venues” has been well documented (Adams & Johnson, 2008; Dennis, Valacich, Fuller, & Schneider, 2006; Katerattanakul, Han, & Rea, 2006; Mylonopoulos & Theoharakis, 2001).

> “Although the top-tier journals have increased the number of articles published in recent years, the number of articles being published in the top-tier journals compared relatively with the number of scholars seeking publication has declined, thus creating an academic environment where top-tier publications have become harder and harder to produce.” (Cuellar, et al., 2008)

In the information systems (IS) discipline this discourse on ‘what counts’ for P&T and credibility as a researcher tracks previous discourses in which IS scholars were concerned with: 1) the relative paucity of IS specialty academic journals outlets and 2) the question of the independence of the field as a separate discipline in its own right (Katerattanakul, et al., 2006; Larsen & Levine, 2005; Straub, 2006; Wade, Biehl, & Kim, 2006). The former was an expression of concern that too many IS scholars were seeking access to relatively few extant IS journals or IS reference discipline outlets. Because of the scarcity of outlets, publication in these peer-referred outlets was considered *bona fide* evidence of scholarly productivity.

As the field has matured, and more publications opportunities have been created, other means of maintaining the value of capital in a political economy have also had to evolve. Scarcity may now be maintained by the way we ‘count’ journal hits or admit published papers to the list of 'counted hits' (Ashkanasy, 2007; Cronin, 2001; Cuellar, et al., 2008; Culnan, 1986; Kateratanakul & Han, 2003; Straub & Anderson, 2010). With the maturing of the field the expectation of what constitutes methodological quality, or the development of theory as well as standards of narrative development have become higher. Additionally, as the field has struggled to deal with challenges to its own integrity and distinctiveness, it has dealt with issues of ontological and epistemological openness of research methods and of valid research topics. In our view much of this discourse has been emblematic of closure and of protecting of political boundaries and existing academic capital. As these discussions have played out in conferences, journals, and faculty settings, they have affected the way that publications are admitted to the "high quality journals", a designation which in turn limits access to participation in discourse in the IS field.

In the not too distant past of the first author, the show-stopping and bone-chilling question asked of journeymen scholars and young academics, and a measure of their success, was whether or not they had published regularly in peer-reviewed publications. The focus of that discourse was on publishing and on the peer reviewing process as the guarantor that a piece of research was worthy and would be of value to others when published. This author has witnessed the transition in this discourse from 'have you published in peer reviewed *venues'*, wherein high-quality conferences (measured in part by acceptance or, more correctly, rejection, rates) were counted as important to a more restrictive version such as 'have you published in peer reviewed *journals*' and finally to an even more restrictive declaration 'have you published in *high quality* peer reviewed journals' wherein only a relative handful of journals may count as relevant. Of late an even more divisive discourse centers around the question of which one, two, or possibly three journals are to be counted.

In these settings advocates of the more selective list choose to ignore the IS field's own designation of a so called "Basket of journals" as an admissible set of journals that should be considered high ranking when making critical academic evaluations such as P&T decisions (Myers & Liu, 2009; Saunders, Brown, Sipior, Zigurs, & Loebbecke, 2009). The overflowing attendance at the panel presentation and subsequent audience discussion during the 2010 ICIS conference in St Louis provides an indication that many in our field are concerned about the notion of 'endorsing' a list of journals and about the process by which journals are admitted to the list.

Attempts to increase the number of publication venues in order to reduce the backlog in IS publishing and to allow for additional publishing opportunities has met with resistance by those arguing that additional journals will "dilute quality". In addition to describing certain journals as superior to others, there are academics who lament the plethora of research outlets and of too much new research, fear a diminution in the 'quality' of the output and thus actually seek to close access even further:

> "…the last few decades have seen astounding growth in the sheer output of research findings and conclusions... [and] that the number of "refereed academic/scholarly" publications grows at a rate of 3.26 percent per year (i.e., doubles about every 20 years). The main cause: the growth in the number of researchers." (Bauerlein, Gad-el-Hak, Grody, McKelvey, & Trimble, 2010)

In a recent *Chronicle for Higher Education* Commentary (Bauerlein, et al., 2010), we are told that there are *too many journals* chasing too few good articles, that the academy cannot sustain the reviewing pace to maintain high quality and the solution is to limit the number of journals and accepted publications. That is, their solution is to limit access, much in the manner the American Medical Association once controlled access to medical school education to control the number of physicians produced, limit competition, and protect their position of power and wealth.

> "We need policy makers and grant makers to focus not on money for current levels of publication, but rather on finding ways to increase high-quality work and curtail publication of low-quality work. If only some forward-looking university administrators initiated changes in hiring and promotion criteria and ordered their libraries to stop paying for low-cited journals, they would perform a national service." (Bauerlein, et al., 2010)

Curiously, we note that in this article asserting that intellectual and article *quality* cannot be maintained in the face of a knowledge explosion, new media and means of dissemination of ideas, and in the 140 comments posted following the on-line version (as of August 14 2010) there was not one single definition of the construct 'quality' (a concept so central to their core argument). This is unsurprising because, to our knowledge at least, theories of journal quality or of paper quality do not exist. In our view many of the surrogate measures, such as those discussed above, are arguments for protecting extant power structures and an extant status quo all neatly wrapped up in pragmatic academic argumentation.

A Habermasian Investigation of Scholarly Influence

Jurgen Habermas's Theory of Communicative Action (Habermas, 1984) has had considerable traction in research in IS development (Klein, 1986, 1991) and IS research in general (Asif & Klein, 2009; Brooke, Cecez-Kecmanovic, & Klein, 2005; Cecez-Kecmanovic, Klein, & Brooke, 2008; Klein & Truex, 1996; Lyytinen, K. & H. K. Klein, 1985; Lyytinen, Kalle & Heinz K. Klein, 1985; Mingers & Walsham, 2010; Ngwenyama & Klein, 1994; Truex, et al., 2011). It is our intention to use this critical social theory to explore the implications for assessing research. In particular we point to Habermas's notion of discourse ethics as developed by Mingers and Walsham (2010) and the notion that the present system of adjudicating scholarship is an instance of a systematically distorted discourse.

For Habermas there are three conditions required to achieve a pure communicative act or an ideal speech situation. Habermas' requires that participants in communications exhibit a kind of intentional attitude towards fairness and truth. This is illustrated by his three validity claims of "Truth", "Rightness" and "Sincerity" that, if/when challenged must be defended by a speaker. The conditions required for an ideal speech situation are: 1) any party to a discourse must have an equal opportunity to participate in a discourse to introduce or challenge claims; 2) differences in power between the participants will not overtly or covertly restrict participants from open participation in the discourse; and finally 3) the participants must be truthful with each other, seeking agreement and not deception. When these conditions are met, in Habermassian terms, there is the possibility of true argumentation, consensus and an open discourse. Consensus is not the only presumed outcome of an open discourse, but is a goal. One may still have disagreement or dissensus, but it will have been reached openly and fairly in the realm of the communicative action (Wijnia, 2004). Whether consensus or dissensus is the outcome of the communicative act, space is left for continued open and fair development of the discourse at a later time. Mingers and Walsham (2010) elevate the standard a step further in arguing from Habermas' notion of discourse ethics, that a discourse has an ethical and moral dimension. Of a discourse they ask:

> What if the goals or ends to be achieved are themselves in question, or if the means to be used raise ethical or moral issues? Here we are concerned with the core values and the self-understanding of a person or a community. What kind of person am I, or what kind of group are we, that we should have these particular values and behaviors? … Within the pragmatic domain, efficacy is the test: Does the action work? Does it have the desired effect? But within the ethical domain, goodness or virtue is at issue. Does the action accord with and develop the actor's own existential identity and self-understanding? This clearly picks up on the Aristotelian and communitarian positions that emphasize the importance of developing the good life within one's community. Although the pragmatic and the ethical have very different concerns—the efficacious and the good—they are similar in that they are both oriented toward the self-interests of particular individuals or groups: the question is, what is effective or good for *us*? It is when one goes beyond that perspective to consider what might be good for *all* that one moves into the

> domain of moral questions. And this is really the focus of discourse ethics. (Mingers & Walsham, 2010, p. 841.)

Following from the previous discussion, our position is that the characteristics of the present academic discourse about scholarly influence are systematically distorted in the following three ways. **First**, parties do not have equal access to participation. The gate-keeping functions in the reviewing processes restrict access. **Second**, there are distinct power differences present throughout the academic assessment and publications process. These power distances in creating, reviewing, editing, and evaluating publications, bias the production toward the standards and topics of those reviewing, editing, and evaluating publications. **Third**, there is evidence suggesting that the 'truthfulness and agreement conditions' of an ideal discourse are marred by the 'gamesmanship' described by Macdonald and Kam (2007). This distortion extends to the peer-reviewing process. For instance, in the realm of peer-reviewing Mahone (1997; 1977) determined the presence of confirmatory bias, the tendency to emphasize and believe experiences which support one's views and to ignore or discredit those which do not, wherein reviewers were strongly biased against manuscripts which reported results contrary to their theoretical perspective.

If one accepts that the discourse might be systematically distorted then this begs the question: How might we improve the discourse?

Striving for ideal speech situations

We need to ask what can we do to arrive at a field with an ideal speech situation in regard to evaluation of scholarly contribution. To reiterate, the conditions required to approximate an ideal speech situation are that: 1) any party to a discourse must have an equal opportunity to start and participate openly in a discourse, 2) the participants must be truthful with each other, seeking agreement and not deception and finally, that 3) any differences in power between the participants must not covertly or overtly diminish or obstruct the rights to participation by others. Therefore, we subject our proposal to the test of these conditions.

1) **Openness**. Our work opens the discourse about alternative methods for adjudging scholarly influence. We advocate a movement away from an evaluation of the venue of publication to the reception of a publication itself; the scattering of many seeds on the ground to let the field sort out the wheat from the chaff. Under this proposition the "wisdom of the crowd" (Surowiecki, 2005) is in effect the collective wisdom of the field unbounded by limits on which venues may be cited. The current 'authority-view' focus on the venue itself limits the discourse to those who pass the gate keeping of journal editors. In short the process could become more democratized. If democratized the process could also begin to respect that the technologies now in hand, technologies allowing for fuller and more complete searches of topics, ideas, and authors in print, and which did not exist when the current systems of evaluation were created have changed the nature of the discourse itself. We argue that publications should be open and with modern search technologies such as Google scholar, access to these publications would be made free to all. This creates a true opportunity for all to begin to discourse on a topic.

The openness notion also addresses the controversy over whether to count only academic citations versus practitioner and popular press citations. We argue that all forms of influence are important not just those where academics speak to other academics. As business school faculty whose positions exist in part to help improve business practice, it seems that when our ideas are taken up by practitioners and the popular press this should count as much as when an academic takes up an idea. The relative importance of our ideas is filtered out by whether they endure in the discourse. As Cronin & Shaw say: " …one needs at the very least to distinguish between, on the one hand, enduring scholarly impact, as suggested by a cumulating citation record–and, on the other hand, Web-based measures of 'street cred' or transient group interest… a digital equivalent of Andy Warhol's fifteen minutes of fame…" (Cronin & Shaw, 2002)

2) **Participation**. With the openness described in point 1, equal participation in creating discourse is provided. To provide openness in contributing to participation in the discourse, evaluation systems must shift from evaluation based on venue to evaluation based on citation. With the use of Google Scholar and Hirsch family of indices and possible extensions, this becomes possible. Google Scholar and h stats are egalitarian, anyone can speak because it includes all venues not only those tightly 'gate kept' Journals. Using the Hirsch family statistics removes the elites from positions of determining what is good and bad research leaving that to the judgment of the field. It is more democratic and it acknowledges some aspects of the "wisdom of crowds" (Surowiecki, 2005) and current knowledge management notions that knowledge resides in networks of many people, not in individuals. Indeed, we may see "crowdsourcing" developments (Howe, 2008) where research problems are proposed by industry and proposals and solutions are then offered by members of the research community (e.g., the InnoCentive scientific community) who then work collaboratively to investigate research questions of relevance to business.

3) **Truthfulness**. By this we mean open, transparent, and reproducible. Evaluation systems should be open and visible to all parties. Today small groups make subjective judgments based on criteria not known to all parties or even to themselves. The evaluation for academic rewards such as tenure and promotion is based on publication in selected venues chosen by panels of experts, possibly based on surveys of people using disparate, often contradictory thoughts whose criteria are unknown. The movement to the use of citation statistics creates an open, known, and reproducible standard that lets all parties to the discussion understand the criteria. We advocate the use of the Hirsch family of statistics and the social network analysis (SNA) measures of centrality for this purpose since they are public, accessible, stable, and reproducible. In effect they are transparent unlike other measures, such as journal impact factors, that are based on a limited set of publication venues, are unstable, are not transparent, and may be amenable to manipulation (Gallivan, 2009).

4) **Power differences.** The movement from publication in selected venues to the Hirsch family of indices and SNA centrality measures also help level the power differences in the evaluation process. By de-centering the venue from the evaluation process in favor of influence measures, the process reduces the power of those who would block dissident discourse in the journal venues. It also puts the power in the hands of the field to determine how important some research is rather than in the hands of an elite.

It is thus our belief that the evaluation of scholarly production should not be predicated upon publication in an exclusive and limited set of journal venues, but rather based on a more open and egalitarian set of measures of scholarly influence. Firstly, those measures should properly include measures of productivity and published participation in the scholarly discourse in places and ways that are meaningful to scholars and others. Secondly, the set of measures should include ways of examining the degree in which a scholar contributes to the "tending of the commons" in part at least by working with other junior scholars and in discipline as well as knowledge and reputation building activities. And, finally, the set should of course consider some merit to the ideas and topics one is investigating. Influence, based on citation and co-authorship, removes the prejudice toward establishment and opens the discourse to dissident and counter-cultural ideas and arguments. As Cronin and Shaw remind us:

> "Within the political economy of academia, citations are a highly regarded form of symbolic capital; in the vernacular you can bank on citations."(Cronin & Shaw, 2002, p. 1267) citing (Cronin, 2001)

We need to consider influence not just from academics but also from the practitioner's point of view. This leads us to the concept of open publication standards (Easton, 2007). As Cronin and Shaw say further:

> "The world of citation is the closed world of the clerisy; we trade citations with other scholars, not with the public at large. The world of the web, by contrast is more open and egalitarian in character (equal opportunity invocation…) here we are linked to, mentioned by our peers, but also, on occasion, by practitioners...and sundry others who may have a special or passing interest in the issues we address as academics and/or public intellectuals. The web extends the discursive space within which scholars operate." (Cronin & Shaw, 2002, 69)

Our vision is that we discount the venue of publication and move toward truly open publication where the discourse is open and unfettered and the conversation joined by any and all who have an interest in it (Howe, 2008). We do not believe that scholars need to be "vetted" by "gatekeepers" but rather through the use of critical thinking and judgmental rationality (Archer, Collier, & Porpora, 2004) that a true progression toward discovery of aleithic truth can be made.

Conclusion: Do We Advance the Deliberative Democracy?

Mingers and Walsham (2010) extract from Habermas' principles of discourse (c.f., fig 1 below) and identify three issues that they argue are embodied and addressed in the notion of deliberative democracy.

> First, the issues may involve moral questions, that is questions that need to be solved in the interests of all. Second, they could involve ethical questions that may differ between different communities and may not be generalizable. … Third, the problem may not be resolved either through general assent or the strength of a particular value because of the range of different communities and interests involved. In these cases, one has to turn to bargaining rather than discourse. The parties involved need to come to a negotiated agreement or accommodation rather than attain a consensus. This is not a rational discourse (in Habermas's terms) since the parties involved will be acting strategically and may well employ power, and because the parties may agree for different reasons, whereas with a moral consensus the parties will agree for the same reasons. Nevertheless, rationality and the discourse principle can be applied to the process of negotiation if not its actual content. (Mingers and Walsham, 2010, p. 843)

If one is open to the idea that the project to evaluate scholarly contribution based on venue of publication is a failed policy then we have to ask if there are better methods to measure contribution and if these approaches are more consistent with the principles of an open discourse and with discourse democracy.

We think there are. We argue that the evaluation of contribution should de-center the predominant publication-centric analysis to a view that also considers the uptake of the ideas published in the articles. By de-centering the venue of publication in favor of reception of the articles, we open the discourse to all comers, allowing the field to select the scholars and works who are the most important rather than having that 'gold standard' set by a small set of evaluators. The most common method of assessing the uptake of ideas has been to use citations. Using citation data allows us to create a proxy for the uptake of ideas from the scholar in the field. In essence, citation data allows us to make an assessment of the influence of a scholar and by inference the quality of the scholar's ideas. This we term "*Ideational Influence*".

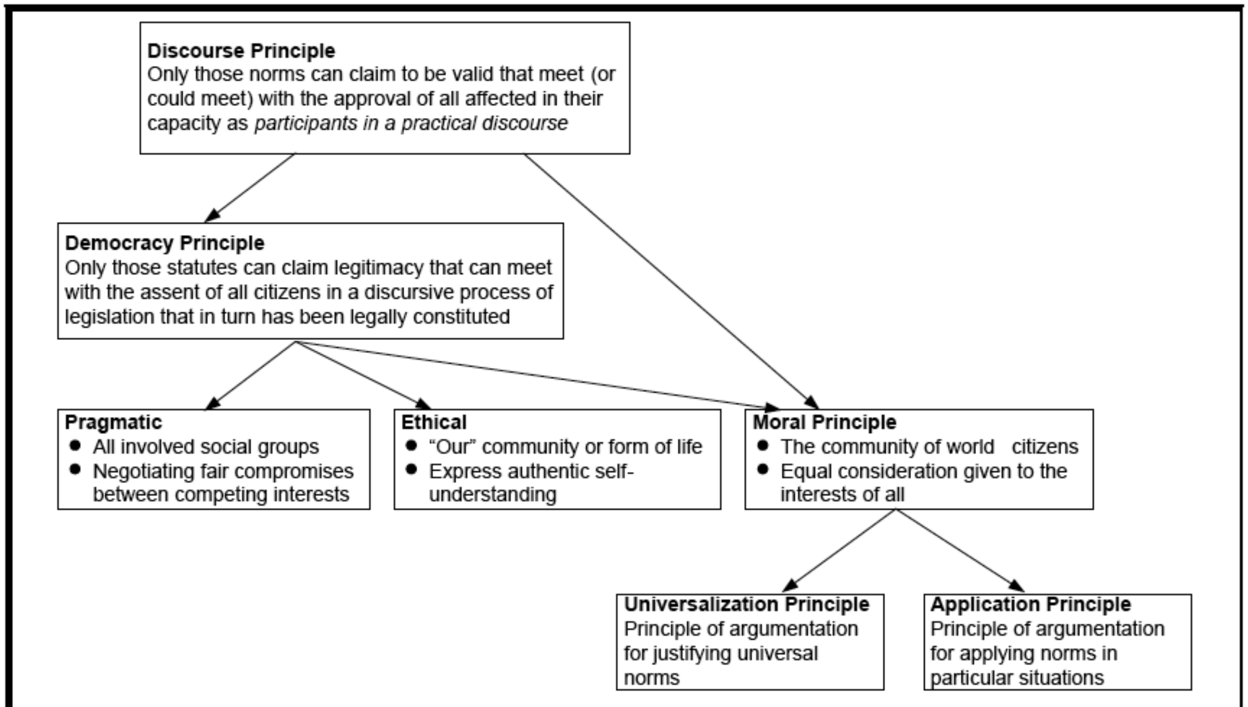

Figure 1. Relationships between Principles of Discourse

Figure 1. Relationships between Principles of Discourse from *Mingers & Walsham 2009, Toward Ethical Information Systems*

The development of scientific knowledge is well recognized as being a social activity (Bhaskar, 1997; Latour, 1987; Pinch & Bijker, 1984). As researchers work together, they interact with each other to help flesh out theories and test these theories either formally through the publication process or informally through interactions at conferences and other meetings or through media such as telephone and email. These interactions mold and shape the ideas of those interacting and eventually help foster the consensus that determines what the field regards as "truth". As these interactions take place, the informal interactions sometimes lead to formalization of relationships in terms of co-authoring: becoming a doctoral student-advisor, joining a faculty and becoming co-workers on the same faculty, forming virtual research teams, co-editing conference proceedings and the like.

In this process of interaction, some scholars are more persuasive than others, in terms of influencing others as to the validity of their ideas. Others are less capable of the social type of influence. The differences in these levels of influences arise through differential social skills, varying comfort levels in social settings, affinity between scholars, commonality of thought and so on. This ability to influence others through the processes of social interaction we term "*Social Influence.*" A scholar may be said to have higher social influence if he/she is able to change the thought of other scholars through their social interactions with them. Ideational influence, on the other hand, is in view when the influence is exercised strictly through their published works, i.e. through the force of their ideas without their social interaction.

Our critique is not to be taken as a condemnation of any particular body of faculty or administration but as a question for a field of scholars as a whole. That is, do we wish to contribute to a system that emancipates and supports critical consideration of the field's scholarship or do we prefer to narrow and close discourse to a select few? In a real sense all faculty are victims of an unjust system. They are locked into an academic establishment that privileges the status quo, normal science, and establishment opinions and disadvantages proposals for change, paradigmatic shifts, alternative truth claims, and new approaches to academic inquiry. Our deliberations lead us to believe that none of the extant dominant approaches toward assessing a scholar's influence is sufficient to the task. We believe that these issues with measurement of influence stem from the nature of the construct being measured. We therefore argue that rather than attempting to assess quality by venue of publication, we assess quality by assessing the behavior of the research community. By looking at what the community references in their research, which publications that the field views as being of high quality may be determined. We therefore argue that by using the concept of "influence" (ideational and social), the uptake of ideas by the field, those needing to evaluate scholarly output can measure its perceived quality. This yields an evaluation by the entire field (and can include industry) and not just by a handful of scholars.

On a final note of caution, we recognize that measures of ideational influence (e.g., citations) can be 'gamed' (Table 1), as can measures of social influence (e.g., co-authorships). In a very real sense, all of our theories and measures of scholarly influence can be said to be performative, i.e., they contribute to the construction of the reality that they describe (Callon, 1998).

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