

Academic Knowledge Production

Output, Impact and Quantity

John Bendix

2009

Introduction: a naïve view

Ideally, what results do we really want from academic research? We often say we want scholars or researchers to make creative or original discoveries. In an industrial, technologically-driven era, suffused by an ideology of progress, we often mean by that knowledge that serves human betterment and improvement. The stereotypical image is of the results from experimental laboratory work in medicine or the natural sciences. We may also expect that laws, or at least law-like or predictable regularities, are found in this process.

Some scholars are in disciplines, by contrast, whose focus is on the interpretation of the human past or human thought. They more typically provide a freshness of insight that may be no less creative or original, though it more commonly comes out of archival or library work. Those who focus on contemporary human society, economy, culture and politics fall in between, some working to find law-like regularities based on empirical results, others engaging in re-evaluating past assertions, and some doing both.

No advance is guaranteed from the process or the results reported. We can try to provide scholars with the freedom and resources we think will aid discovery and lead to new insight, but all we really have is faith their findings will prove of value. In at least some fields, that value may be measured increasingly in direct, economic terms. Typically, results are disseminated in the form of publications, many nowadays containing implicit or explicit claims that what they report is an advance in knowledge. Inasmuch as what is published is subjected to a system of peer review – which is not always the case – we presume that before being made public, knowledgeable others have judged that what is asserted is at least worth being published and disseminated.

Much of that is actually in answer to other assertions that are in print, so the academic enterprise is an ongoing debate between the present and the immediate or more distant past. Karl Popper called these ‘conjectures’: over time they will either be shown to be incorrect, and hence refuted, or they will become the accepted interpretations – until such time as they, too, are reformulated. A case in point is the conjecture by Alfred Wegener in the early 20th

century about continental drift. Though rejected largely for lack of corroborating evidence in the years immediately after being published, his conjecture became the basis for plate tectonics several decades later. It illustrates both that ‘publication as ongoing conversation’ and that a full evaluation of the worth of a published conjecture may simply not be possible at the time it is published. Some disciplines operate with very short time spans, particularly if it is possible to relatively quickly replicate a reported finding, as an experimentally-based, laboratory-oriented discipline can, while other disciplines may take a generation or more for a substantial shift in perspective and interpretation to occur. That may well come about not as the result of the work of a single rare individual but instead be the product of a network of thinkers (Collins 1998).

Then there is the matter of age. A number of early studies (in particular, Lehmann 1953), drawing their information from the natural sciences, asserted that creativity or innovation is associated with youthfulness. We award science Nobel Prizes, in general, for work that was done by individuals, often in their 30s, rather than for groups of researchers. In art and literature, however, one needs to consider not just the brilliance of a youthful Picasso, Orson Wells or James Joyce but the mature talent of a Michelangelo, an Alfred Hitchcock, or a Daniel Defoe as well (Galenson 2006; Gladwell 2008). Perhaps that kind of creativity is different from the creativity felt necessary for academic work, but it is also true that in many humanities and social sciences, it is possible to identify the “grand old men” – and still, unfortunately, far too few “grand old women” – whose creative insights took decades to fully formulate but whose impact was decisive.

The unforgiving view

These are generous but naïve views. Here are some stark counter-propositions:

- I. Most scholars publish little and publish infrequently. The issue isn’t new discovery; the issue is any discovery at all, because there is simply little output providing evidence for it.
- II. Most who do publish remain uncited or are barely cited: they have little or no impact, at least not as measured by citation rates.
- III. Though we want quality work, increasingly what is produced and evaluated is quantity. Herewith, an anecdote from the U.S. along these lines:

A friend who teaches at a large Midwestern school says that salary increases correlate with book and article publication to the dollar, and he hopes that his next book comes out before year-end recommendations are due. “What if your book isn’t any good?” I ask with a half-smile. “Doesn’t matter,” he replies. “When I returned to my own institution after two and a half years of government work and wondered how much credit I would get for pieces appearing during my time away, a dean skipped the quality question and replied, ‘Well, you have lots of titles, but how many pages do they amount to?’ ” (Bauerlein 2008).

So first one needs to establish whether there is evidence which supports for these unforgiving views. Then one needs to address the implicit, nagging (if not nasty) suspicions that lie behind them. If there is little output, what *are* scholars doing with their time – and are we paying them for essentially doing nothing? If so little of what is published resonates, should we have publication impact or citations as a criterion at all? If there is this over-supply, can one do anything about it? And there is an underlying aspect: *in our exaggerated focus on the very few – perhaps 20% – who have high output and high impact, we ignore assessing the worth, value, or contribution of the vast majority who have neither.* This is one the major, and largely ignored, issues in evaluation, and it matters a great deal in assessing the work produced in departments and institutes, as well as by individual scholars.

Hence, some suggestions:

1. We may need two different assessment tools, one suited for distinguishing among the high-flyers, the other suited for the large mass. The U.N. found it necessary split its original Human Development Indicator into two, one that was sensitive enough to measure the comparatively small differences between the most highly developed countries, while the other could rank all countries. The difference between Zambia and France on the sensitive first HDI indicator was stark; distinguishing Denmark from Germany on the second HDI indicator was nearly impossible.
2. Output and impact may need to be conceived in terms of a narrow band (measured, say, by citation indexes) and a broader band (possibly measured by productivity over time, or by including forms of publication like book reviews and encyclopedia articles, or by finding a measure of public impact – perhaps conference attendance? editorials? newspaper mentions?). The latter would surround the former, much the way the European Monetary System that was once (colorfully) called “the snake in the tunnel.” Implicit that both bands are linked together and need each other.

3. It may help to think of incentives built into a model of ‘normal progress’ through the ranks of an academic career. In a differentiated or tiered system which allows for rising through the ranks of instructor, assistant, associate and full professor, this can be done by regular reviews every two or three years (in a tenure-oriented system) or every five years (in a contract-based system). Rewards and punishments – and increase in pay or decrease in workload for above-average performance; no increase in pay or staying longer at the same rank for below-average performance – are also conceivable. This is more difficult in European systems that have tried to reduce differences in rank, but there could be incentive systems that would do something similar (e.g., offer greater rewards for participating in work the institutions deems necessary, such as self-management/administration, or desirable, such as awards for good teaching).

The Evidence

I. Infrequent Publication

Self-reporting by a large sample of U.S. faculty members concluded that on average, a professor writes about two articles and a book review every two years (Middaugh 2001:22-24). Across all U.S. universities, output is both somewhat higher and more differentiated: 2.6 refereed articles, 1.3 non-refereed articles, plus 6 conferences and 0.5 books, monographs, or reports over two years, this last a figure one can interpret as ‘work-in-progress,’ since it implies one book, monograph, or report completed in four years.

Table 1

Average number of publications and presentations over 2 years (2003-4),
full-time instructional faculty and staff, by program area,
all public and private Title IV degree-granting institutions, U.S.

	<u>Publications</u>				
	Refereed or juried	Non-refereed or non-juried	Published reviews	Books, reports, monographs	Presentations and exhibits
Engineering	4.4	2.5	0.7	0.9	5.9
Nat. Sci.	4.1	1.0	0.7	0.4	5.0
Agriculture	3.4	3.9	0.8	0.9	8.7
Health Sci.	3.1	1.2	1.0	0.5	6.6
Mean	2.6	1.3	0.9	0.5	5.9
Soc. Sci.	2.3	1.1	1.1	0.7	5.1
Business	2.0	0.9	0.5	0.4	3.6
Humanities	1.7	1.0	1.6	0.5	4.2
Education	1.4	1.2	0.6	0.6	6.4
Other	1.2	1.3	0.8	0.6	4.4
Fine Arts	1.1	1.0	0.8	0.4	14.7

Source: 2004 National Study of Postsecondary Faculty (Table 23, p. 33), National Center for Education Statistics. Available at: <http://nces.ed.gov/pubs2006/2006176.pdf> (accessed 4 Mar 2009).

Still, because these figures come from a sample of 1,000+ colleges and universities, more than one-third of which are two-year institutions whose faculty focus is on teaching, not research and publication, these figures are low. When one examines those most likely to be publishing, namely full-time, tenure-track and tenured faculty at 4-year institutions, the averages are higher.

Table 2

Full-time tenure-track faculty publications, 4-year U.S. institutions, over a 2-year period (1992-93)

<u>Mean</u>	Above mean	Below mean
3.67	Health Sciences 5.08	Humanities 3.49
	Natural Sciences 4.89	Business 2.80
	Agriculture 4.74	Other 2.79
	Engineering 4.63	Education 2.75
	Social Sciences 4.09	Fine Arts 1.49

Source: Fairweather (2002), Table 2 - Productivity Measures by Program Area

Averages are misleading, inasmuch as they are summary measures of an underlying distribution. In the U.S., for example, scholars in agriculture publish in non-refereed journals while those in the fine arts put on exhibits or do presentations, at rates three times the overall averages, which skews their patterns of productivity (see bold figures, Table 1) and a mean value does not capture this. So a proper understanding of publication rates needs to be with reference to the distribution of publications *by discipline*, or in other words, using a cross-sectional view (Najman and Hewitt 2003).

One also needs a longer view, as an academic book often takes more than two years to write and publish; during that time, the active publication of journal articles may very well take a back seat. Hence, the measure of publication across disciplines also cannot be how many articles appear within two years but must instead include judgment, likely qualitative, which takes the type of academic product into account. For books, those judgments might ask about the type of book and its contribution or ambit, about its audience or publisher, and ask to see what reviewers have judged. Evaluation may well need to be in the context of an academic career of publishing or production (e.g., using a periodic review). This matters, as there is some, if contradictory, evidence that publication varies *by the author's rank*.

Does the discipline matter?

Fairweather (2002) asked the question: “what percentage of the faculty are productive in teaching or research?” and used data from the 1992-93 National Survey of Postsecondary Faculty in answer. His analysis was based on a sample of 25,780 full-time and part-time faculty in 817 U.S. institutions, ranging from research universities (which train most U.S. doctoral students) to two-year colleges. His focus was only on full-time, tenure track faculty from 4-year institutions, and he left out departmental chairs since their work patterns are often different from those of ordinary faculty. While faculty themselves often defined their productivity in terms of refereed publications and research grants, Fairweather used a more expansive definition, the “number of refereed publications during the previous two years, where publication includes articles in refereed journals, published reviews of books, articles or creative works; books; textbooks; monographs; and chapters in edited volumes” (2002: 33).

One finding was that the type of institution itself correlated with research productivity. In the two-year period studied, faculty at research universities produced six publications, while those at teaching-oriented liberal arts colleges produced two. Another finding, and the actual focus of the investigation, was that there was a clear trade-off: Either one could be a highly productive scholar or a highly productive teacher, but it was the rare faculty member (only about 1 in 5) who was both at once.

Analyzed by disciplinary area, Fairweather found a mean of 3.67 publications across the disciplines in a two-year period, a figure not far from Middaugh’s ‘two articles and a book review’ mean. The natural sciences, medicine, agriculture, and engineering exceeded that mean, while fine arts, education, and business were below it. Both social sciences and humanities were close to the mean, reflecting the fact that these are conglomerations of disciplines, some of which are more likely to have refereed publications than others. Two years also may be too short a period of assessment purely for reasons of process in some fields. The peer review process for articles and books can be very slow, academic norms mitigate against multiple submission of the same material to different journals or publishers, and if a paper is returned for a ‘revise and resubmit,’ the time lag between creation and publication grows even longer. This is one reason for the growing attractiveness of ‘publication’ on the Internet, though the status of such output vis-à-vis print publication remains unclear, even when an online journal is peer-reviewed.

Table 3

Full-time tenure-track faculty conference presentations, 4-year U.S. institutions, 2-year period (1992-93)

<u>Mean</u>	Above mean	Below mean
4.41	Agriculture 7.42	Social Sciences 3.71
	Engineering 6.18	Natural Sciences 3.56
	Education 6.03	Other 3.39
	Health Sciences 5.41	Business 2.94
		Humanities 2.82
		Fine Arts 2.72*

*averaged 17 exhibitions or performances

Source: Fairweather (2002), Table 2 - Productivity Measures by Program Area

Because publication is not the only means of disseminating results, Fairweather also looked the number of conference presentations or workshops. While the finding is generally similar, not only is the range larger, but disciplinary uniqueness is also more clearly displayed. Academics in education publish at levels below the mean, but their conference attendance is above average; the pattern is the reverse in the natural sciences. Fine arts are a special case, for while the mean in both publishing and conference attendance, their energies are instead directed to putting on a large number of exhibits (or performances).

However, Kyvik's 1989 study of Norwegian scholarship, surveying all tenured academics at Norway's four universities, and using methodologically different premises than Fairweather, concluded there were no real differences in publication productivity by disciplinary area. Kyvik defined productivity in terms of all scientific publications produced in the three-year period from 1979 to 1981. The definition of publication used here included articles, books and reports, but excluded encyclopedia articles, book reviews, and scientific reports unless they were part of a series. As absolute output was the focus, no distinction between refereed and non-refereed publications was noted.

Two productivity measures were used, one a raw score of total publications, the other an index designed to "facilitate relevant comparisons across fields of learning" in which all publications were regarded as article equivalents and correspondingly weighted (see Table 4). For this second index, "an article in a journal or book is given the value of 1, while a book

receives 2 to 6 points according to the number of pages and type of book,” in a ranked, descending scale from research book to textbook, edited book and translation, with proportional points awarded for multiple authorship (Kyvik 1989: 206). This values the channel rather than of the quality of an individual publication (Moed, Luwel, and Nederhof 2001), and the ‘article equivalence’ index yields what Kyvik calls “substantially altered”

Table 4

Average Number of Publications and Article Equivalents per Norwegian Researcher, 3-year period (1979-81)

	Humanities	Soc. Sci.	Nat. Sci.	Medicine
Publications	3.5	4.6	5.1	8.2
Article equivalent	4.7	5.9	3.9	5.2
N	434	299	482	354

Source : Kyvik (1989 : 208), Table 1

patterns that increase the score in the humanities (by .40) and social sciences (by .43), due to the larger number of books published in these fields. It decreases scores for the natural sciences (by .40) and medicine (by 1.00, due to the larger number of co-authors in these fields). One can debate the specifics of Kyvik’s weighting, but it does give the greater effort writing a book involves its due, and it reduces the potentially inflated significance of the mere fact of having one’s name listed as co-author in a multi-authored article. Striking, however,

Table 5

Annual publications, selected fields, Norway and U.S.

	Humanities	Soc. Sci.	Nat. Sci.	Medicine	
Norway (1979-81: 3 yrs)	1.16	1.53	1.70	2.73	Kyvik publications
Norway (1979-81: 3 yrs)	1.56	1.96	1.30	1.73	Kyvik article equiv.
U.S. (1992-93: 2 yrs)	1.75	2.04	2.44	2.54	Fairweather
U.S. (2003-04; 2 yrs)	1.60	2.05	2.75	2.40	NSOPF 2004

Note: Norwegian categories were articles, books, and reports, but not book reviews, encyclopedia articles, and scientific reports not part of a series. Fairweather categories were refereed publications, book reviews, books, textbooks, monographs, and chapters in edited volumes. NSOPF 2004 categories were publications, both refereed and non-refereed, and books, reports, monographs. Comparison across countries therefore means comparing categories which are not identical.

is not, as one might assume, how much more the natural sciences and medicine publish annually than the social sciences and humanities, but rather how close all productivity rates are. The same can be said of the U.S. across these same categories: it does not appear to differ much from Norway, other than that Norwegian natural scientists seem to have a slightly lower output than do U.S. scientists. These figures underscore the initial assertion:

On average, there is simply little academic productivity, per year and overall.

There is a subtler point. It is not across disciplines (though see Baird 1991) or across nations one should be looking: one should instead pay attention to what is typical *within disciplines*. Unfortunately, this is not just difficult to establish but also difficult to generalize about.

One of the few quantitative efforts to try was launched by Academic Analytics in an effort to establish a “Scholarly Productivity Index” by discipline and university in the U.S. In figures reported for 2007, the index covered around 165,000 faculty in U.S. Ph.D. programs. Five factors, depending on discipline, were used to judge faculty on a 100-point scale: books published, journal publications, journal citations (together, these three accounted for 60 points), federal grant dollars awarded (30 points), and honors and awards (10 points). Books are counted for the previous five (2002-06; Baker & Taylor database), journal articles for the previous three (2004-06) and citation counts for the previous four (2003-2006; Scopus) years. Books were included in some fields (business, education, humanities, social sciences) but not others (agriculture, biology, engineering, physical and mathematical sciences), and were more heavily weighted in humanities than in other fields.

A rather worrisome comment is also to be found in the methodology description: “institutions that pay for the data have an ability to re-weight the variables in any category, according to their preferences.” Furthermore, what is publicly available online from Academic Analytics only shows the top universities in a given field, hence is unrepresentative of the 395 institutions they surveyed. Data is incomplete in some cases, the databases themselves do not provide complete coverage of all publications, disciplines are not represented at every institution, and the focus is only on Ph.D.-granting institutions. Given that 40% of this index is devoted to federal grant dollars as well as other grants and awards, one can also ask whether this shouldn’t better be called a “Scholarly Productivity, Honors, Awards, and Grants Success Index.” For any particular discipline at any particular institution, this index is no more helpful than any other similar effort to rank programs and institutions.

Despite these rather crippling caveats, this source does contain the useful categories of ‘books per faculty’ and ‘journal publications per faculty.’ I have selected ten disciplines in

the humanities, largely based on the completeness of information available online, to examine. It is noteworthy is that only *between one-quarter and one-third* of the faculty in these ten humanities disciplines even have a journal publication, let alone citations – and

Table 6

Top Ph.D. Programs, Selected Humanities Fields, Journal Publications

	% faculty w. jrn. pub.	journal publ. per faculty	% faculty w. citation	citations per faculty	citations per paper
	Range	Range	Range	Range	Range
American Studies	7 – 27	.07 – 1.07	22	0.07 – 12.3	0.33 – 9.7
Area Studies	9 – 46	.09 – 1.60	41	1.00 – 10.1	1.30 – 5.5
Art History	4 – 33	.04 – 2.30	25	0.06 – 18.4	0.25 – 9.9
Classics	0 – 22	.13 – 2.90	22	0.07 – 38.9	0.33 – 14.9
Comp. Lit.	7 – 27	.30 – 2.30	20	0.90 – 17.1	1.90 – 19.7
Engl. Lang. & Lit.	6 – 25	.06 – 2.23	21	0.06 – 33.5	1.00 – 10.8
History	15 – 37	.25 – 1.06	29	0.13 – 10.9	0.36 – 8.8
Philosophy	28 – 100	.63 – 8.40	77	1.30 – 40.3	1.00 – 5.4
Religion	3 – 23	.06 – 2.00	16	0.03 – 33.6	0.20 – 16.2
Theater	0 – 12	.08 – 0.30	20	0.15 – 3.6	0.75 – 6.0
Means	8 – 35	.17 – 2.42	29	0.37 – 21.8	0.74 – 10.7
(minus Philosophy)	6 – 28	.12 – 1.72	24	0.27 – 19.8	0.71 – 11.2

Source: Academic Analytics; own summaries

bear in mind these are faculty at the top-ranked institutions and disciplines in the country. With the notable exception of Philosophy, these are not fields in which most faculty write articles; overall, the numbers are quite low.

The situation is different when we turn to books. Here, the overall comparison shows a vastly higher percentage of faculty with book rather than article publications (see the last column, Table 7; the table also includes the mean number of faculty, since some scholars have hypothesized that there is a correlation between the size of a department and productivity).

Table 7

Top Ph.D. Programs, Selected Humanities Fields, Book and Journal Publications

	Faculty mean number	Faculty mean w. book	Faculty mean w. art.	Books per faculty	Ratio article to book
Philosophy	21	66%	46%	2.0	.69
Religion	36	68%	13%	1.9	.19
History	58	75%	25%	1.7	.33
Comp. Lit.	31	62%	17%	1.7	.27
Engl. Lang. & Lit.	37	64%	16%	1.6	.25
<i>mean of 5 fields</i>	37	67%	23%		.35

Classics	15	61%	16%*	1.4	.26
Art History	20	52%	17%	1.3	.33
Theater	17	49%	9%	1.2	.18
Area Studies	36	47%	26%	1.0	.55
American Studies	37	41%	17%	1.0	.41
<i>mean of 5 fields</i>	25	51%	17%		.35

*only 6 cases; 4 had no articles reported

Source: Academic Analytics, own calculations

There do seem to be two groups. One has more faculty, on average (e.g., philosophy, religion, history, literature), places greater importance on publishing books, and has a somewhat higher book production rate. The second group has smaller faculties (an average of 25 versus 37), and book publication is in a lower range (1 – 1.5 versus 1.6 – 2.0 in the first group). The contrast is most evident when comparing Philosophy to Area Studies.

But while philosophers stand out, inasmuch as they are clearly expected to publish articles, the importance of articles per se is clearly low in other humanities. Indeed, in the ten humanities fields listed in Table 7, the production of articles – and it is worth noting that this is by faculty in the top-rated U.S. Ph.D.-granting humanities departments – is the exception rather than the rule. During the 1980s and 1990s, in fact, the role of journals stagnated in the humanities, as Larivière et al. (2006) pointed out in their longitudinal study. Another way to put this is that publishing books matters, publishing articles not so much.

This has interesting implications if natural (or social) science citations models are to be used as indicator of productivity or impact in humanities fields. Citations in the humanities, at least to journal articles, are simply not that significant:

Table 8

Percentage citations to journal articles in the Thomson SSCI and AHCI databases by discipline, 1996-2000

Discipline	Percent Citations
Psychology/Psychiatry	68
Law	59
Economics/Management	55
Other Social Sciences	45
Education	44
History	33
Other Humanities	28
Literature	22
SSCI – AHCI average	48

Source: Archambault and Gagné 2004, Table XI (p. 54)

Does rank or age matter?

Price, in an extensive demographic study of authorship, concluded “that authors differ not so much in their *rate* of publication of papers, but in the span of *time* they spend at the publication front” (Price 1976: 304). One-third continued to publish, while two-thirds were ‘transients’ who “emit only their initial publications” and who had only a one-in-four chance of being cited. A study of publication impact in psychology came to a similar conclusion. If you control for rate and place of publication, “scholarly impact is independent of [chronological] age” (Over 1989: 225). What mattered more was “professional age,” defined as the years between Ph.D. graduation and publication (in the past, this had been associated with academic level or rank).

Price’s ‘transients’ likely include those in doctoral programs who are encouraged, to be competitive on the job market, to publish even though they have not yet completed their Ph.D.s. Many never will. A survey of 30 U.S. universities conducted by the Council of Graduate Schools found that for those who entered graduate study in 1992-94, Ph.D. completion rates after 10 years ranged from 64% for engineers to 49% for humanities fields (Jaschik 2007). With an attrition rate of 40-50% among Ph.D. students, there will be many ‘transients’ moving through the system, and they will publish – but often only a little. Price implies that in hierarchically organized academic systems, lower-ranking newcomers (Ph.D. students, lecturers, instructors) will have less output, or publish at lower rates, than those who have been around longer (tenured associate and full professors).

Plausible though this sounds, it is difficult to find confirming evidence. The opposite argument, that those who are trying to join the club of faculty members, such as assistant professors or those in tenure-track positions, are under considerable pressure to publish. Indeed, to prove themselves that may need to be at rates equal or above those of existing members of the club. The typical demands made on those who aspire to become professors in the humanities at German universities (not just one but two dissertations, preferably published and reviewed, along with a substantial number of articles) could serve as supporting evidence for this argument. In a sense, the German expectation is that one hires those who already have achieved a substantial ‘professional age,’ though it is in a system that in comparison with the U.S. lacks a clear academic career progression.

Evidence from the 2004 U.S. National Survey of Postsecondary Faculty, showing that part-time faculty published at rates comparable with those of full-time faculty (the only significant exception was in peer-reviewed publications, with a mean of 0.7 for part-timers vs.

2.6 for full-timers) also suggests employment pressures are increasing the pressure on those lower on the academic ladder to publish. One can also read this as a reflection of sharply worsened career prospects. In 2005, 48% of all U.S. faculty were part-time (in 1975, this was 30%), and to be hired, even part-timers need to have a solid publication record in addition to a completed terminal or Ph.D. degree.

One of the few large-scale, quantitative studies of academic stratification, conducted a generation ago and focused specifically on European research laboratories in the natural sciences, concluded that “position within a research organization” was the major explanatory variable accounting for publication productivity differences (Knorr et al. 1976; Knorr and Mittermeir 1980). This study argued that higher productivity was a function or consequence of higher position in the internal hierarchy. The association of a senior scientist with particular research results was far less dependent on that scientist actually conducting the research himself (goal-executing), because high rank meant being associated with larger research agendas (goal-setting), which in turn permitted him to be associated with more research tasks than before. In one particularly striking finding, “productivity seemed to be more or less independent of how much research a scientist was actually doing.” Those in supervisory positions reap the publishing rewards of the research of others, and publish at rates four times higher (7-8 articles) than the researchers in the laboratories (1-2 papers) (Knorr et al. 1976: 25-26), though this might reflect a past habit of putting the name of the senior scientist on the paper regardless of actual contribution. The data was drawn from 1,222 research units and 4,057 scientists working in six European countries, and while suggestive, is not directly comparable to situations in the social sciences or humanities. A recent study has cautioned that one needs to be careful about disentangling the effects of age from those of cohort and period, because “identifying an age-related productivity effect...relies crucially on what we are willing to assume about the variation in the other two dimensions” (Hall, Mairesse and Turner 2005: 19).

Some studies have found a relationship between rank (or tenure) and research productivity (Blackburn, Behymer and Hall 1978), while others (Wanner, Lewis and Gregorio 1981) found academic rank strongly affected article count in the natural and social sciences but not in the humanities (where, unsurprisingly, book count was higher). The argument of ‘cumulative advantage’ posits that early opportunities received (mentoring, early publication, training at prestigious institutions, formal and informal recognition) pay out in higher publication rates later on (Zainab 1999: 88) – though there is disagreement about whether academic rank causes high productivity or whether it is the other way around. This theory

has been tested with respect to productivity and prestige in sociology departments as well (Keith and Babchuk 1994). Bland (2005), in a study of research productivity at a specific medical faculty found that two factors did significantly predict research productivity: rank and appointment type (tenure-track faculty were more productive than those on other appointments).

Tien and Blackburn (1996), finally, found assistant and associate professors did not differ in productivity, but were lower overall in productivity than full professors, who in turn had significantly greater variance in their productivity than did those at lower ranks. Those at the lower ranks who had less publications, esp. assistant professors, stayed longer in their ranks. One can put this in terms of necessary and sufficient conditions. Those at the pre-tenure stage need publications to advance in rank but it may not be sufficient for tenure (e.g., there need to be ‘enough’ or the ‘right type’ of publications, or they may need to be forthcoming regularly). At the associate and full professor rank, it is not as (or at all) necessary to have publications for reasons internal to an institution – unless a ‘post-tenure review’ system is in place that associates rewards and punishments with continued productivity – so the criteria for what is sufficient may increasingly become a external to a given institution, such as prestige or standing among peers in the discipline.

II. Minimal Citation

The evidence that about half of all articles published are never, or quite rarely, cited is strong. Herewith a few examples:

- of 32.7 million papers listed in the Science Citation Index (1945-1988), 18.2 million (55.6%) were either never cited or were cited only once (Garfield 1990)
- in a study of a total of 783,339 papers published in 1981, 368,110 (46.9%) remained uncited between 1981 and 1998 (Redner 1998)
- of 38 million items indexed from 1900-2005, half were not cited at all and about a quarter were not substantive articles but editorial ephemera (Garfield 2007a)
- “In a given year, about 35 percent of all the existing papers are not cited at all, and another 49 percent are cited only once” (Price 1965: 511).

If citation means impact, then half the articles published have no impact – and more

than half the journals have no impact either. Testa reported that “an analysis of 7,622 journals covered in the 2003 Journal Citation Reports revealed [that]...a core of 3000 journals [39%] accounts for...nearly 95% of cited articles,” with only about 300 of those journals accounting for more 50% of what was cited (2006: 135-36). More than a generation ago, a similar finding led one of the founders of scientometrics to gloomily conclude that “a very large fraction of the alleged 35,000 journals now current must be reckoned as merely a distant background noise” (Price 1965: 515).

The situation is still more skewed when one looks at citation distribution. Various studies have found proportions suggesting that only about 15% of researchers are responsible for more than half the publications in a given period (Ramsden 1994; Cole, Cole and Simon 1981; Reskin 1977). That converts, as another founder of scientometrics bluntly put it, into a situation where “in most fields,...20% of the articles may account for 80% of the citations” (Garfield 2007b: 67). The distribution of citations, in other words, do not follow a normal (Gaussian) distribution, but follow a long-tailed, heavily right-skewed distribution that is characterized by a power law or follows conjectures made by Zipf and Lotka. More bluntly, “*most scientific work never gets noticed (and)... most scientific papers are read by almost no one*” (Surowiecki 2004: 170).

It is thus appropriate to speak of those one in five articles/scholars who are citation-rich and the four out of five articles/scholars who are citation-poor. Vilfredo Pareto first noted this proportion in a different context, when he discovered that 80 percent of Italy’s wealth was in the hands of only 20 percent of its population. It is not just Italy in the past: a 2007 study of the distribution of private wealth in Germany, surveying persons in private households aged over 17 (examining property and company ownership, savings and insurance while omitting debts, cars owned, and personal furnishings), concluded that “the wealthiest 10 percent of the German population owns 61.1 percent of total private wealth, and the next wealthiest 10 percent owns 19 percent” (Niejahr and Rudzio 2009: 27) – or in other words, the top 20 percent owns 80.1 percent of total private wealth in Germany. This imbalanced wealth distribution is in fact global. The 1998 UN Human Development Report concluded that “the 20% of the world’s people in the highest-income countries account for 86% of total private consumption expenditures – the poorest 20% a minuscule 1.3%,” with the richest fifth, for example, also consuming more than half the worlds’ energy (UNHDP 1998: 2). Aptly dubbing it the “champagne glass effect” for the shape of the distribution when diagrammed, they also noted that “the average income of the top 20% of the world’s population is about 50 times the average income of the bottom 20%” (UNHDP 2005: 36).

In fact, and especially when represented logarithmically, wealth, like academic publication, seems to follow a power law distribution running from the very few with extremely high values down to the very many with extremely low values. The distribution even fits subsets, such as the wealth distribution found among the Forbes 400 list (from 1988 to 2003) of the richest Americans (Klass et al. 2006), in which the top value may be double the second (one assertion of Lotka's law), or have values for the highest cases vastly above the mean of the entire distribution. A paper by Lowry, for example, the most highly ranked in the 1961 to 1972 period, had 29,655 citations, while the next two papers had "only" 6,281 and 5,825 citations, respectively, prompting Price to suggest half the scientific papers are contributed by the top square root of all scientific authors (Price 1976: 302).

Among those papers that are cited, here with specific reference to the natural sciences where citation practices are most widespread, one can define a precise number of citations that separate the many (the 80% who are "rarely cited") from the few (the 20% who are "more frequently cited"): **five** citations. What is striking is the pattern with which the frequencies diminish in the distribution. A little less than half as many items (7.8 million, 24%, or 1 in 4) received 2 to 4 citations as received none or 1 (18.2 million, 56% or 1 in 2). Half again as many received 5 to 9 citations (2.9 million, 9% or 1 in 11). The first two categories (0-1, 2-4)

Table 9

Citation frequency distribution for papers in the SCI 1945-1988

No. of citations	No. of items receiving that number of citations	Percent of entire SCI file	
	32,728,729		All SCI papers, 1945-1988
0 and 1	18,255,577	55.78	
2 - 4	7,877,213	24.07	
0 - 4	26,132,790	79.95	
		80%	
5 - 9	2,955,984	9.03	
10 - 14	1,207,577	3.69	
15 - 24	1,089,731	3.33	
5 - 24	5,253,291	16.05	<i>sub-total</i>
25 - 49	842,950	2.58	
50 - 99	348,537	1.06	+
100 - 199	112,299	0.34	
25 - 199	1,303,786	<u>3.98</u>	<i>sub-total</i>
		20%	

200 - 299	21,952	0.07
300 - 399	7,736	0.02
400 - 499	3,406	0.01
Over 500	5,767	

Source: Garfield (1990: 4)

account for 80% of the citations, so receiving five or more citations already places a publication among the top 20% and receiving 50 citations, among the top 1%.

In light of such numbers, the definition of what a “highly cited” paper is may need revising. This distribution suggests that while citation may well be a way of separating the 20% of the wheat from the 80% of the chaff, it is necessary – even in the sciences, and more so in humanities fields that lack a strong tradition of article publication or of citation – to develop other yardsticks for assessing the work of the vast majority of scholars, and not just in the humanities.

III. Quantity over Quality

Bauerlein noted that the number of publications in the Modern Language Association’s International Bibliography has increased six-fold between 1960 and 1980, and put the blame for sinking quality squarely on career pressures: “we cannot blame graduate students and young scholars for rushing manuscripts into submission and cutting corners on research when the hustle for jobs and tenure urges them to *produce, produce!*” (Bauerlein 2008). There is certainly pressure on those who are competing for the ever-scarcer full-time appointments at U.S. universities to convince not by their potential (e.g., the prestigious universities they have attended or their grades) but by evidence of their publication(s).

At the same time, one should bear in mind that one source of the increase in academic production is the increase over time in the number of institutions and hence also of faculty.

Table 10

Increase in U.S. Institutions, Faculty, and SCI papers

Year	Institutions	Faculty	SCI papers	Faculty to SCI ratio
1960	2,021	380,554	100,000	0.26
1970	2,556	450,000	350,000	0.77

1980	3,231	675,000	550,000	0.81
1995	3,706	931,706	800,000	0.85
2005	4,236	1.2 million	1.2 million	1.00

Note on Institutions: before 1980, branch campuses were excluded, and figures after 1980 include 2-year colleges, which constitute 40% each of the number given in 1980, 1995, and 2005. Note on Faculty: for 1960 and 1970, measured as ‘different individuals’ rather than full-time equivalents; for 1980, as ‘instructional faculty with rank of instructor or above’, for 1995 and 2005 as ‘faculty (instruction and research)’. For SCI papers, these approximate figures are drawn from a chart showing the growth in the number of papers indexed by the SCI presented by Garfield (2007a).

Sources: Historical Summary of Faculty, 1870-1988 (Table 156), Digest of Educational Statistics 1990, available at <http://nces.ed.gov/pubs91/91662.pdf>; Total and full-time equivalent staff in degree granting institutions, fall 1976, fall 1995 and fall 2005 (Table 234) and Number of Educational Institutions (Table 5). Digest of Education Statistics 2007, available at http://nces.ed.gov/programs/digest/d07/tables/dt07_234.asp and [...dt07_005.asp](http://nces.ed.gov/programs/digest/d07/tables/dt07_005.asp): Garfield, E. 2007c, “Charting the Growth of Science,” Presented at the Chemical Heritage Foundation, May 17 available at <http://garfield.library.upenn.edu/papers/chartinggrowthofsciencd2007.pdf> (all accessed Mar. 8, 2009).

A very rough guide to this compares the increase in the number of U.S. institutions of higher education and the number of faculty over the last decades to the total number of papers indexed in the SCI – the numbers are gross approximations and the categories (for example, of who is counted as ‘faculty’) not wholly consistent over time. Nevertheless, the message is clear: everything is increasing, and the best indication is that the ratio of faculty to publication keeps rising each decade. It is not just the quantity of production that is going up, but that each faculty member, at least in the sciences that the SCI index mirrors, is increasingly expected to be producing.

There have been some attempts to look at this from the point of view of the publication “opportunity structure” faculty face. Using bibliographies, periodical directories, journal issues, national faculty surveys, and information on total faculty, Bieber and Blackburn (1993) tried to assess whether there was more or less space in which faculty in biology, psychology and English, could publish in 1988 than in 1972. Their conclusion was that in real terms, faculty had to produce 14.65, 5.05, and 1.18 articles, respectively, in the two years before 1988 to have been as productive as in the two years before 1972.

One can put this differently: faculty are not being expected to produce *less* than their predecessors, though how *much* more they are expected to do varies considerably by field. As the anecdote at the outset illustrates, an advantage for evaluators is that it is far easier to judge quantity – how much have you produced? – than quality of output. If one kind of demand (for output) is creating supply (of publication pages), then a better alternative may be a different kind of demand (such as for quality). Behind the demand for output may lie the notion that if much is produced, at least some will be of quality. Not everything a Nobel Prize winner publishes is stellar, after all.

There is a problem that is harder to address behind the demand for quality: who will supply the quality assessment? As Bauerlein (2008) notes,

we cannot blame senior professors who recognize that invoking old-school quality controls too diligently earns them only resentment from colleagues who regard criteria such as handling of evidence, validity of inference, and clarity of prose as constricting—or who are just plain uncomfortable with judgment. Besides, everybody is too focused on their own productivity to question that of others.

And even Garfield (2007b: 69) rather dispiritedly observed that

Obviously, a better evaluation system would involve actually reading each article for quality, but ... when it comes to evaluating faculty, most people do not have or care to take the time any more.

Discussion

On Section I: Scholars are not doing *nothing*, but what they are doing is not readily quantifiable in terms of “productivity”. The time spent in a laboratory conducting an experiment that doesn’t work (and then trying to figure out why not), or the time spent in a library (or on the Internet) trying to find appropriate material or sources isn’t any more directly ‘productive’ than it is for a secretary in an office to spend all day on the telephone trying to arrange a meeting between five businesspeople with busy schedules. A successful end result in all these cases does not reflect the actual amount of time invested, and in fact, actively ignores it.

Relatively few scholars are pure researchers. The remainder, who typically teach, need to devote time to that enterprise, including reading papers and grading, advising students, planning, or preparing classroom hours – and anyone who thinks a semester-long series of lectures can be quickly dashed off should sit down and try to do it themselves sometime. Besides, before one can publish, one has to generate the formulations, or do the research, that is reflected in that publication, and that often is a slow process or may call for quiet, reflective time. We can’t increase productivity by trying to make the process of creation go faster, and we shouldn’t, because not only does that pressure undermine the motivation to engage in the academic enterprise, it is guaranteed to produce work whose quality needs improving.

The evidence on scholarly productivity is that differentiation is more revealing than aggregation. It is not just that individual humanities scholars tend to publish books and

natural scientists co-author articles, but also that there is a pattern such that the “publication profile” of an engineer will differ considerably from that of a historian, and that of a professor of theatre. Professional activities, as reflected in attending and presenting at conferences, are also not uniform, and may or may not be linked to a publication. The “professional age” of a scholar may well be correlated with productivity, even when the causality is unclear. Nevertheless, if one aggregates, it is hard to escape the impression that scholars, in general and across disciplinary areas (and even cross-nationally) are simply not publishing much,

On Section II: The strong evidence that about half the articles published are never cited, or are cited only once, and that another quarter are only cited two to four times, indicates that a **very** small proportion of academic authors are reaping the majority of the attention, if one means by that being cited even ten times. “Famous scientists find their work cited vastly more often than scientists who are less well known. When famous scientists collaborate with others, they’re given a disproportionate share of the credit for the work” (Surowiecki 2004: 170). Citations are useful primarily as a means of distinguishing the small minority, the charmed circle, from the vast majority.

It does little good for every institution to try to ensure its scholars be among the top 20%. There simply aren’t enough members of that charmed circle to go around. Not only that, those institutions which pursue (and can afford) the strategy of only hiring the most cited (or productive) scholars they can attract, typically discover that star performers have their difficulties getting along with other star performers. One cannot and should not expect all members of a department to be at the level of the top performer in the department. So the 80% need to be judged not by their citation counts but by other criteria – progress in career, contributions to the institution or to the wider academic community, where their book is published and how it is received, public impact, overall pattern of steady publication, teaching excellence, etc. – than those applied to the top 20%.

On Section III: The question of quantity vs. quality is often conflated with career issues. The supply of publications continues to rise, not in response to a societal demand for them but to the pressures of trying to make a living in a context where there are few permanent positions. Lack of sufficient publications is detrimental to advancing, so the more publications, the better. Yet economics teaches us that increasing supply (of publications) lower the value of each additional (publication) being supplied. The more applicants at the

door, the more selective an academic employer can get. If one then only accepts the ‘top quality’ publications from prospective future faculty members, or only focuses on the ‘best’ publications, then those in the best position to provide them – those who already have the most ‘cumulative advantages’ – will also reap the most rewards. The call for higher quality may well result in some version of the Matthew Effect.

A Few Closing Suggestions

1. Systematic, regular evaluation of scholars, not just at hiring and tenure but throughout the academic career (post-tenure reviews, or regular performance evaluations). Provide incentives to keep the teaching and scholarship of faculty members at acceptable levels. This assumes particular ‘career paths’ or certain kinds of expectations to be fulfilled at each step or rank in order to advance.
2. Seriously consider adopting, and acting on, the Carnegie Foundation recommendation that one recognize and reward four different dimensions of scholarship: the scholarship of discovery, the scholarship of integration, the scholarship of application, and the scholarship of teaching (Glassick, Huber and Maeroff 1997: 9). This meta-level perspective would go a long way to transcending disciplinary differences by placing the emphasis not so much on the specific work a discipline does but by placing that work inside categories of scholarship all scholars engage in.
3. Because there is such a sharp distinction that can be drawn between those who are noticed and those who are not, assessment tools need to be disaggregated into those which measure finer differences (such as citations) among a small minority (of those who actively publish) and a tool which offers broader measures (say, of regular progression up an academic ladder, accepts ‘non-standard’ output as evidence of productivity, values and validates contributions to the whole (a department) and not just publications, and so forth). It would more accurately reflect the patterns most scholars display.
4. Introduce a *meso*-level of expectations. Some improvement in productivity and some improvement in quality, a satisficing strategy that argues that most people can do somewhat more and somewhat better. If we find that, on average, our arts faculty in

five years is producing one book and three articles, we could say: wouldn't it be good if you could produce five articles in that time and have another book in progress? An academic dean obsessed with excellence might say: my faculty needs to move from having their papers cited four times or less to being cited five to 24 times, because that distinguishes the bottom 80% from the top 20%...

5. In the composition of an academic department, one needs to rely on the “wisdom of crowds” (Surowiecki 2004) rather than the excellence of stars. The tasks of an institution call for a variety of abilities, and what makes someone a good department chair may well not be what makes them a good teacher. Though our attention is on the star forward who kicks the ball into the soccer net, he will only have been able to with the help from the other players on the team. Medical examples, such as the rapid identification of the SARS virus, a successful international collaboration between laboratories in 11 countries make the point (Surowiecki 2004: 158-62). On factual knowledge, the results are more accurate when one polls many opinions than just one: expertise does have its limits. In a moderately frivolous example, Surowiecki cites the fact that when ‘experts’ are polled on *Who Wants to be A Millionaire?* they do

okay, offering the right answer – under pressure – almost 65 percent of the time. But they pale in comparison to the audiences. Those random crowds of people with nothing better to do on a weekday afternoon than sit in a TV studio picked the right answer 91 percent of the time (2004: 4).

These are arguments, in other words, for what those 80% who produce work that goes unnoticed can nevertheless contribute.

References

- Archambault, E. and E. Gagné, E. 2004. *The Use of Bibliometrics in the Social Sciences and Humanities*. Report prepared for the Social Sciences and Humanities Research Council of Canada. Montreal: Science-Metrix.
- Baird, L. 1991. "Publication productivity in doctoral research departments." *Research in Higher Education* 32(3): 303-318.
- Bauerlein, M. 2008. "The Future of Humanities Labor." *Academe* (Sept/Oct).
- Bieber, J. and R. Blackburn. 1993. "Faculty research productivity 1972–1988: Development and application of constant units of measure." *Research in Higher Education* 34(5):
- Blackburn, R., Behymer, C. and D Hall. 1978. "Correlates of Faculty Publications." *Sociology of Education* 51: 132-141.
- Bland, C. et al., 2005. "A Theoretical, Practical, Predictive Model of Faculty and Department Research Productivity." *Academic Medicine* 80(3): 225-237.
- Cole, S., Cole, J. and G. Simon. 1982. "Change and consensus in peer review." *Science* 214:
- Collins, R. 1998. *The Sociology of Philosophies. A Global Theory of Intellectual Change*. Cambridge: Harvard University Press.
- Fairweather, J. 2002. "The Mythologies of Faculty Productivity." *The Journal of Higher Education* 73(1): 26-48.
- Galenson, D. 2006. *Old Masters and Young Geniuses: The Two Life Cycles of Artistic Creativity*. Princeton: Princeton University Press.
- Garfield, E. 1977/78. "The 100 Articles Most Cited by Social Scientists, 1969-1977." *Current Contents* 32 (Aug. 7): 5-14.
- Garfield, E., 1990. "The Most-Cited Papers of All Time, SCI 1945-1988." *Current Comments* 7 (Feb. 12): 3-14.
- Garfield, E. 2007a. "Evaluative Measures for Resources Quality: Beyond the Impact Factor." Presented at the Medical Library Association Meeting, Philadelphia (May 22). Available at: www.garfield.library.upenn.edu/papers/mla2007.pdf. Accessed Feb. 16, 2009.
- Garfield, E. 2007b. "The Evolution of the Science Citation Index." *International Microbiology* 10: 65-69.
- Garfield, E. 2007c, "Charting the Growth of Science," Presented at the Chemical Heritage Foundation (May 17). Available at <http://garfield.library.upenn.edu/papers/chartinggrowthofscience2007.pdf>. Accessed Mar. 8, 2009.

- Gladwell, M. 2008. "Late Bloomers. Why Do We Associate Genius with Precocity?" *The New Yorker*, Oct. 20.
- Glassick, C., Huber, M. and G. Maeroff. 1997. *Scholarship Assessed: Evaluation of the Professoriate*. San Francisco: Jossey-Bass.
- Hall, B., Mairesse, J., and L. Turner. 2005. Identifying Age, Cohort and Period Effects in Scientific Research Productivity. NBER Working Paper 11739. Available at: www.nber.org/papers/w11739. Accessed 14 March, 2009.
- Jaschik, S. 2007. "Why and When Ph.D. Students Finish." *Inside Higher Ed* (July 17). Available at <http://www.insidehighered.com/news/2007/07/17/phd>. Accessed 4 March 2009.
- Keith, B. and N. Babchuk. 1994. "A longitudinal assessment of productivity in prominent sociology journals and departmental prestige." *The American Sociologist* 25(54): 4-27.
- Klass, O. et al. 2006. "The Forbes 400 and the Pareto wealth distribution." *Economics Letters* 90 (2): 290–295.
- Knorr, K. et. al. 1976. Individual Publication Productivity as a Social Position Effect in Academic and Industrial Research Units, Research Memorandum No. 117. Paper presented at the PAREX-IAS Colloquium on "The Role of Research Organizations in Orienting Scientific Activities," Vienna, July 5-6. Available at: <http://www.ihs.ac.at/publications/ihsfo/fo117.pdf>. Accessed on 5 March 2009.
- Knorr, K. and R. Mittermeir. 1980. "Publication Productivity and Professional Position: Cross-National Evidence on the Role of Organizations." *Scientometrics* 2(2): 95-120.
- Kyvik, S. 1989. "Productivity Differences, Fields of Learning, and Lotka's Law." *Scientometrics* 15(3/4): 205-214.
- Larivière, V. et al. 2006. "The Place of Serials in Referencing Practices: Comparing natural sciences and engineering with social sciences and humanities." *Journal of the American Society for Information Science and Technology* 57(8): 997-1004.
- Lehman, H. 1953. *Age and Achievement*. Princeton: Princeton University Press.
- Middaugh, M. 2001. *Understanding Faculty Productivity: Standards and benchmarks for colleges and universities* (San Francisco: Jossey-Bass).
- Moed, H., Luwel, M., and A. Nederhof. 2002. "Towards Indicators of Research Performance in the Humanities." *Library Trends* 50: 498-483.
- Najman, J. and B. Hewitt. 2003. "The Validity of Publication and Citation Counts for Sociology and other Selected Disciplines." *Journal of Sociology* 39(1): 62-80.
- Niejahr, E. and K. Rudzio. 2009. "Die andere soziale Kluft." *Die Zeit* (22 January).

- Over, R. 1989. "Age and Scholarly Impact." *Psychology of Aging* 4(2): 222-225.
- Price, D. de Solla. 1965. "Networks of Scientific Papers." *Science* 149 (July 20): 510-15.
- Price, D. de Solla. 1976. "A General Theory of Bibliometric and Other Cumulative Advantage Processes." *Journal of the American Society for Information Science* 27(5/6): 292-306.
- Ramsden, P. 1994. "Describing and explaining research productivity." *Higher Education* 28: 207-226.
- Redner, S. 1998. "How Popular is Your Paper? An Empirical Study of the Citation Distribution." *European Physics Journal B4*: 131-134.
- Reskin, B. 1977. "Scientific Productivity and the Reward Structure of Science." *American Sociological Review* 42: 491-504.
- Surowiecki, J. 2004. *The Wisdom of Crowds*. New York: Anchor Books.
- Testa, J. 2006. "The Thomson Scientific Journal Selection Process." *International Microbiology* 9:135-138.
- Tien, F. and R. Blackburn. 1996. "Faculty Rank System, Research Motivation, and Faculty Research Productivity: measurement refinement and theory testing." *Journal of Higher Education* 67(1): 2-22.
- UNDP. 1998. Consumption for Human Development (Human Development Report). New York: Oxford University Press. Available at <http://hdr.undp.org/en/reports/global/hdr1998/chapters/>. Accessed Feb. 23 2009.
- UNDP. 2005. International Cooperation at a Crossroads (Human Development Report) New York: UNDP. Also at http://hdr.undp.org/en/media/HDR05_complete.pdf. Accessed Feb. 23, 2009.
- Wanner, R., Lewis, L. and D. Gregorio. 1981. "Research Productivity in Academia: A comparative study of the sciences, social sciences and humanities." *Sociology of Education* 54: 238-253.
- Zainab, A. 1999. "Personal, Academic and Departmental Correlates of Research Productivity: A Review of Literature." *Malaysian Journal of Library and Information Science* 4(2): 73-110.