More than thirty years ago there was good evidence to suggest that information retrieval involved conceptual problems of greater subtlety than is generally recognized. The dramatic development and growth of online services since then seems not to have been accompanied by much interest in these conceptual problems, the limits they appear to impose, or the potential for transcending such limits through more creative use of the new services.

In this article, I offer a personal perspective on automatic indexing and information retrieval, focusing not necessarily on the mainstream of research but on those events and ideas over a 34-year period that have led to the view stated above, and that have influenced my perception of important directions for future research.

Some experimental tests of information systems have yielded good retrieval results and some very poor results. I shall explain why I think that occurred, why I believe that the poor results merit special attention, and why we should reconsider a suggestion that Robert Fairthorne put forward in 1963 to develop postulates of impotence—statements of what cannot be done. By understanding such limits we are led to new goals, metaphors, problems, postulates, and perspectives.

The Limits of Information Retrieval

In the beginning was Calvin Mooers, who in 1950 introduced the term “information retrieval” (IR) into the literature of documentation [1]. For better or worse the name stuck, notwithstanding the question it raises of what an information retriever might be, or the image it invites of a bird-dog fetching the New York Times. Bar Hillel’s warning a few years later that Mooers had confused IR with literature searching led to Mooers’ rebuttal that Bar Hillel had confused IR with question-answering, both failing to notice that the bird-dog was neither searching literature nor answering questions [2,3]. In any event, the problem context in which the dispute arose was that of subject access to information, and how to index scientific articles and reports. Apropos of indexing and access, the first use of “information retrieval” in Volume 1 of this journal did not become an entry point in the index to that volume [4]. Automatic keyword-in-context indexing was not yet born, though its conceptual origin dates back at least to the year 1247 when Hugo de St. Caro employed 500 monks to compile the first concordance of the bible [5]. Hugo’s monk-powered ecclesiastical data processing system (allegedly called the MP/EDP-1, or possibly System 1 — history is obscure on this point) must have been worth seeing.

The decade of the 1950s was marked by endless disputes among proponents of various breeds of indexing schemes, classification systems, and bird-dogs. In 1953, the first large scale experiment intended to test retrieval effectiveness only inflamed the controversy. In effect, the test was a contest between the uniterm system of Documentation, Inc. (aka DocInc, a company founded by Mortimer Taube in 1951) and the Armed Services Technical Information Agency (ASTIA) subject heading system, using 98 questions applied to a test collection of 15,000 technical documents. Before the results could be evaluated, it was necessary for the contestants to decide jointly which among the retrieved documents were or were not relevant to the questions. As reported by Dake Gull, both teams agreed that 1390 documents were relevant to one or more of the 98 questions, but there were another 1577 documents that one team or the other, but not both, considered to be relevant—a colossal disagreement that was never resolved [6]. Both sides claimed victory, but the performance of the uniterm system attracted more notice, for it was novel and challenged the more conventional system. In any event, the partisan spirit seemed to overshadow what for me was the more compelling question of why the two teams disagreed on the matter of relevance, on so massive a scale. That hiatus was the first clue that the idea of a “relevant” document might be more problematic than it appeared.

That an IR system should be judged by two measures — how well it captures relevant documents, and how well it rejects the irrelevant — was implicit in the experiment. However, the idea was not formalized until 1955 when Kent, Berry, Luehrs, and Perry defined the two measures as
"recall" (proportion of relevant documents that are retrieved) and "pertinency factor" (proportion of retrieved documents that are relevant) [7]. The latter measure was later to be called "relevance ratio" and then, about 1965, "precision ratio" became more or less standard. These authors, and hundreds of IR researchers since, seemed to accept uncritically the idea that relevance is additive. The possibility, for example, that two irrelevant documents might become relevant if put together has never been adequately considered, so far as I know.

At about the same time as the Doclnc/ASTIA contest, Oliver Lilley at the Columbia library school was investigating the extent to which subject headings assigned to books was predictable by prospective users of the catalog. The results were amazing. 340 graduate students in the library school, asked to chose subject headings appropriate to six books, came up with an average of 62 headings per book, of which 61 were different from the headings actually used on the catalog card. Whether that says more about subject headings or Columbia students isn’t clear; nonetheless here was the second clue that subject-oriented indexing and retrieval might be either far more or far less than it seemed [8].

Interest in the problems and process of indexing was stimulated during the latter part of the fifties by the work of Hans Peter Luhn at IBM who devised a method of automatic abstracting (more accurately, extracting) with obvious implications for indexing and searching [9,10]. His method was based on the selection of words according to frequency, avoiding those that were either too rare or too common. Phyllis Baxendale at IBM San Jose tested a similar method for indexing by comparing human with machine procedures for word extraction [11].

Bar Hillel seemed to enjoy intellectual skeet shooting; he would often toss out good ideas and then blast them to bits. Others would pick up the pieces and find them interesting. In November, 1958, at the Teddington conference, in criticizing the Luhn approach to automatic extracting, he proposed an alternative approach that he thought superior to any other that he knew of, but about which he nonetheless had the gravest doubts — namely to base index words, or, even better, word-pairs, on the ratio of relative frequency within the document to relative frequency within the language [12]. Substantially the same idea was developed at about the same time, and carried much further, by Harold Edmundson, Vic Oswald, and Ron Wyllys at the Planning Research Corporation [13]. Soon after its birth this neat new theory collided with a few untidy facts. John O’Connor at the University of Pennsylvania, who, around 1960, was studying the relationship between word frequency and indexing in the drug literature, noted among other things that 11 out of 23 documents properly indexed with the term “toxicity” did not contain any word at all with the stem “toxi” [14]. Thus O’Connor provided a third category of clues that nothing was going to be simple about automatic indexing and retrieval.

The Edmundson-Wyllys ideas, together with early work on associative indexing by Lauren Doyle at System Development Corporation (SDC) and by Ed Stiles at an unmentionable government agency, as well as work on automatic classification by Harold Borko at SDC, and especially the seminal work on probabilistic indexing and ranked-output retrieval by Bill Maron and Lary Kuhns, at Thompson Ramo Wooldridge, had by 1960 set the stage for subsequent research based on statistical approaches to IR [15–18]. I have mentioned only a few of the people and projects that I knew at the time and that influenced my work. Mary Stevens wrote a definitive review of the early work on automatic indexing, citing 662 references [19]. (Regrettably, she neglected to mention Hugo).

My own approach to investigating the mysteries of indexing and retrieval, during the late 1950s, was to dump a small collection of physics articles into a computer word by word, along with a thesaurus or synonym dictionary, and then try to retrieve articles relevant to a set of test questions. For small enough a collection, I thought I could settle in advance the matter of which articles were and which were not relevant to each question. Automatic indexing, information retrieval, text searching, and literature searching in effect could then be studied as one and the same problem. Judgments of relevance were made with some care by subject experts, with a three-person consensus to resolve disagreements prior to any retrieval tests. The point of using a computer was mainly to force a mode of description that was rigorous, and that would separate purely mechanical problems — what a computer can do — from conceptual problems, or problems of meaning. As Christine Montgomery then put it, we were trying to distinguish the temporal from the eternal.

Early in 1959, my colleagues and I at Thompson Ramo Wooldridge put these ideas before Verner Clapp, then President of the Council on Library Resources. Not only was financial support quickly forthcoming, but the project was sustained as well by Verner’s enthusiasm and encouragement — for he did not take lightly the bets he placed. In August 1959 we were awarded a nine-month contract that was completed more or less on schedule, with the help of a team that included Paul Garvin, Lary Kuhns, Donald Black, and twelve physicists who participated in relevance assessments and in formulating searches.

Two publications, in 1960 and 1962, reported the outcome of this work [20,21]. I was impressed with how poor our retrieval results were and the kind of difficulties that we encountered. To anticipate the words that would be used in a document to express the idea in a search question was as difficult in practice as it was in principle.

Nearly a year before these experiments were initiated, Vic Yngve, at the International Conference on Scientific Information (ICSI) held in Washington, D.C. in 1958, presented a paper on the feasibility of text searching. His insights on the ubiquity of ambiguity and on the need to find "formal connections between widely divergent ways of saying essentially the same thing" must be considered prophetic [22, p. 977–978]. Bar Hillel a few years later similarly stated:
Though scientific and technological writers may not make full use of the theoretically unlimited number of ways of expressing their thoughts, put at their disposal by natural languages, they do make use of a large enough number to defeat any system based upon simple matching of expressions [23].

A recent experiment in text searching reported by Blair and Maron corroborates these basic difficulties and offers an illuminating discussion of their nature and causes for specific types of search questions [24]. Comparison of the measured effectiveness of retrieval in my 1960 Science paper with this 1985 work by Blair and Maron is also of interest. First, I should correct a misunderstanding. Blair and Maron indicated that their results were at variance with more favorable results that I had reported in 1960. In fact, in both cases the average proportion of relevant documents retrieved—that is, “recall”—was very low, about 20%, at comparably high levels of “precision” (about 70%). The following statement by Blair and Maron is similar to my explanation of why the results were so poor.

Stated succinctly, it is impossibly difficult for users to predict the exact words, word combinations, and phrases that are used by all (or most) relevant documents and only (or primarily) by those documents, as can be seen in the following examples [24, p. 295].

In my own experiments, I did obtain substantially higher recall, but lower precision, with thesaurus-aided methods; my point is that the reported 20% is a magic limit, but that retrieval even at best is seriously problematic, and it is worth trying to understand why that is so.

My experiment shares with the Blair-Maron experiment two design features that might explain the similarly low values of recall. First, each test dealt with fairly homogeneous material—mine with nuclear physics scattering problems, and Blair & Maron with litigation support material relevant to a particular case. Second, in both cases a high level of expertise was applied to the judgment of relevance. Thus relevance judgments tended in many instances to reflect subtle distinctions that could not be discerned by anticipating word usage. The Blair-Maron experiment, based on a collection of some 400,000 documents, offers valuable insight into the nature and the subtlety of what I call the “conceptual” problems of IR—the problems of meaning. To expect that machines can be instructed to solve such problems may be an illusion without a future.

The Crucial Role of Relevance Judgments

Among the many retrieval tests conducted between 1960 and 1985, I have reviewed four of the more notable ones [25–27]. Although there have been many reports of high values of recall and precision, I know of no solutions that have been offered to the problems of meaning—problems that account for the low values reported by me and by Blair and Maron. Consistently high values of recall and precision seem to be attributable to the conditions under which questions are created and relevance is judged. For example, some experiments used relevance judgments based only on inspection of titles, or titles and abstracts. Others involved circular reasoning based on “source” documents, and still others the delegation of pre-screening, hence irrelevance judgment, to nonexperts. Under these and similar circumstances the possibility for taking into account subtle relevance relationships based on expert judgment appears to be precluded at the outset.

Thus I suspect that the outcome of retrieval tests depends more strongly on the nature of the questions and the circumstances of the relevance judgments than on the characteristics of the systems under test. The best retrieval results are obtained when relevance judgments are hasty or simplistic—that is, based on obvious words or phrases, particularly in titles. The poorest results are obtained when relevance judgment depends on any subtle process of extracting meaning from text by persons with expert knowledge of the subject.

For the foregoing reasons, I believe we stand to learn more from the poor retrieval results reported by Blair and Maron than from the reportedly successful tests. I have not, however, attempted an exhaustive review of the literature on retrieval experiments, and so will quote the opinion of Karen Sparck Jones who did conduct such a review, covering the period 1958–1978. “Overall, the impression must be of how comparatively little the nonnegligible amount of work done has told us about the real nature of retrieval systems” [28, p. 245]. In the same book, Bill Cooper, commenting on the state of theorizing in document retrieval, says pretty much the same thing: “Deep down... it’s shallow” [29, p. 201].

**Postulates of Impotence**

In 1963, Robert Fairthorne, arguing that information science was a branch of science, not of mathematics, pointed out the necessity of formulating principles valid in the outside world, and suggested that such principles might be statements of what cannot be done—of what Whittaker called “postulates of impotence” (PI) [30,31, p. 58–60].

The fundamental laws of physics and cosmology can take the form of PI, statements about what it is impossible to achieve. Whittaker noted that any well-developed branch of physics can be exhibited as a set of logical deductions from PI. Numerous examples can be cited, including the impossibility of “perpetual motion”. Some progress has been made in other fields as well; the theory of economics follows from the nonexistence of a free lunch, and political theory from the impossibility of fooling all of the people all of the time.

So far as I know, no one has explicitly accepted Fairthorne’s challenge to come up with such postulates for information science, but a number of perceptive writers—Bar Hillel [12,23], Lauren Doyle [32], Robert Taylor [33], and
no doubt others — laid at least some of the groundwork even before Fairthorne’s paper. Whether useful and rigorous PI for “information science” can be invented is difficult to say, for that really depends on just what we are trying to explain, but it seems worthwhile at least to stimulate argument on the matter. In that spirit I propose the following postulates. They apply only to the problem of subject-oriented information retrieval — that is, the problem of meeting information needs with items the exact nature and even existence of which are unknown to the requester.

**PI 1:** An information need cannot be fully expressed as a search request that is independent of innumerable presuppositions of context — context that itself is impossible to describe fully, for it includes among other things the requester’s own background of knowledge. Indeed, because the search is for something that is unknown at the outset, the question cannot be precisely formed until the answer is found.

**PI 2:** It is not possible to instruct a machine to translate a stated request into an adequate set of search terms. Search terms are hypotheses, inventions, or conjectures; there are no rules.

**PI 3:** A document cannot be considered relevant to an information need independently of all other documents that the requester may take into account. Relevance is not fixed, it is judged within a shifting framework. With each document examined, a requester is faced with a continually branching web of associations. Two documents that are thought similar in one context, or in the light of one hypothesis, may be quite different in another. The phenomenon of “changing salience as a function of the object set considered” is well known in the behavioral sciences. For example, two objects, such as Pepsi and Coke, are judged more similar if a third object, 7up, is introduced into the situation [34]. Two separate pieces of a puzzle might be irrelevant, but, when fit together, reveal a relevant pattern.

**PI 4:** It is never possible to verify whether all documents relevant to any request have been found, for relevance can be ascertained only by examining the relevant document and one can never in practice or even in principle examine all documents. Any claim or conjecture that all relevant documents have been found is, however, open to potential refutation by someone who might exhibit a relevant document that was not found [35]. This postulate is parallel to the principle that the universal laws of empirical science can in principle be refuted but never verified.

**PI 5:** Machines cannot recognize meaning and so cannot duplicate what human judgment in principle can bring to the process of indexing and classifying documents. Corollary: Some indexers all of the time, and all indexers some of the time, also cannot duplicate what human judgment in principle can bring to the process of indexing.

**PI 6:** Word-occurrence statistics can neither represent meaning nor substitute for it. Such data, however, can be used, with occasional success, to signal or point out potentially fruitful areas of text where a human being might then seek meaning or relevance.

**PI 7:** The ability of an IR system to support an iterative process cannot be evaluated in terms of single-iteration human relevance judgment. For repeated iterations, new criteria emerge, such as the ability of the system to stimulate creative revision of the question.

**PI 8:** You can have either subtle relevance judgments or highly effective mechanized procedures, but not both.

**PI 9:** In sum, the first eight postulates imply that consistently effective fully automatic indexing and retrieval is not possible. The conceptual problems of IR — the problems of meaning — are no less profound than thinking or any other form of intelligent behavior. There is no evidence yet that thinking can be reduced to rule-ordered manipulation of a database of facts. Our relevance judgments and our thinking entail, among other things, artful leaps of the imagination unconstrained by logic, reasoning, or the clammy hand of consistency; more important, they entail knowing who we are, what we are, the kind of world we live in, and why we want what we seek. It is hardly imaginable that a mechanism other than a human could acquire such self-knowledge, be given it, or do the job without it.

All of that is not to deny that machines are unsurpassed as an aid to information retrieval, and that continued research to improve such capability is warranted. That we can retrieve more information faster, however, does not by itself mean that we have learned how to do it better. My postulates of impotence are not precise or rigorous but perhaps they can serve as reminders of the enormity of our presuppositions. I hope they will start arguments.

**Transcending the Limits**

Beginning about 1955, I participated for eight years in various studies of problems related to military intelligence analysis. These studies were focused not on collection of data but on the process by which massive amounts of raw or low-level fragmentary data are transformed into much smaller quantities of finished, high-level intelligence. That focus creates a new perspective within which information retrieval can be seen as part of a broader process of information exploration, correlation and synthesis. Our purpose was to develop machine techniques for aiding and stimulating these and other creative activities associated with intelligence analysis.

An illuminating comparison can be drawn between the intelligence analyst and the scientist, with respect to their use of recorded information. The scientist typically sees new knowledge as originating in measurements on the physical world; such knowledge is then recorded, stored in libraries and retrieved as needed — usually keeping the chore of literature searching to the absolute minimum insofar as possible and then delegating to an assistant whatever cannot be avoided. Such a generalization is of course hazardous, but I suspect that my impression is at least more right than it is wrong. The Weinberg report of 1963 similarly notes: “To the working scientist or engineer, time spent gathering information or writing reports is often regarded as a waste-
ful encroachment on time that would otherwise be spent producing results that he believes to be new” [36, p. 9]. More recently, Weissberg, Caponio, and Lunin state: “Despite the enormous growth of information, there remains for many scientists the belief that they can maintain awareness of developments in their field by very conventional means; that is, attendance at scientific meetings, direct communication with other scientists, and the review of the literature in their own specialization” [37, p. 52].

The intelligence analyst, by contrast, is much more intimate with the available base of recorded information. New knowledge, or finished intelligence, is seen as emerging from large numbers of individually unimportant but carefully hoarded fragments that were not necessarily recognized as related to one another at the time they were acquired. Use of stored data is intensively interactive; “information retrieval” is an inadequate and even misleading metaphor. The analyst is continually interacting with units of stored data as though they were pieces selected from a thousand scrambled jigsaw puzzles. Relevant patterns, not relevant documents, are sought.

Imagine for a moment that the scientific community adopted the culture, attitudes, and metaphors of the intelligence community with respect to recorded information. Scientists might then take seriously the idea that new knowledge is to be gained from the library as well as from the literature. Only by retrieving the two literatures and bringing them together can one assume that their logical relationship had not been seen by reflecting on the historical fact that the various branches of science have always fertilized each other” [38, p. 42]. Fertilization was also of interest much earlier to J. D. Bernal at the 1958 ICSI conference: “Here what science loses by such enforced specialisation is the cross fertilisation of ideas from different fields such as lead to all great discoveries” [39, p. 80].

Perhaps these two authors are implying that we need postulates of fertility. Because that idea would seem to merit at least as much attention as impotence, I offer three postulates just for a start.

**Postulates of Fertility (PF)**

**PF 1:** The literatures of different scientific specialities tend to develop independently of one another, but the connectedness inherent in the physical world suggests that there are many fertile, unintended logical connections between these distinct literatures.

**PF 2:** Owing to inadequate communication between scientific specialities, these literatures tend to be mutally isolated bibliographically; that is, they do not adequately cite one another, and are not co-cited — they are noninteractive. There is reason to think therefore that the logical connections between them may be not only unintended, but unnoticed, unexplored, and unfertilized.

**PF 3:** Sufficiently creative use of online systems can lead to detection of both the logical connections and the bibliographic isolation of the connected literatures, and so eventually to the impregnation of these virginal connections.

These postulates add up to this; the fragmentation of science into specialities, and therefore the relative isolation of specialized literatures from one another, suggests that there may exist numerous unintended and unnoticed relationships among these isolated literatures. If that is so, then a systematic search for such connections ought to be a worthwhile pursuit—a pursuit that goes beyond information retrieval, and may resemble the correlation and synthesis functions of intelligence analysis.

**Implications for Future Research**

In a series of recently published studies [35,40—42], I have tried to confirm my three postulates of fertility by analyzing two separate, noninteractive or bibliographically isolated, chunks of literature — sets of journal articles — that together contain implications that cannot be seen within the two sets considered separately. That is, the two literatures taken together contain a chain of reasoning, with some of the links being in one literature and other links in the other literature. Only by retrieving the two literatures and bringing them together can any one person become aware of the complete argument, and so be led to the implied conclusion. (And so it is, incidentally, that articles which taken alone are irrelevant become relevant when put together.) Moreover, because the two literatures were noninteractive it was plausible to suppose that they had not before been brought together, and so one can assume that their logical relationship had not
been noticed, an assumption supported by the fact that apparently the conclusion had not before been published.

I have described then how new discoveries might in principle be extracted or constructed from existing literature through retrieving, assembling, and understanding the individual parts that make up the whole, parts that had not previously been assembled.

The metaphor of the jigsaw puzzle should not go unnoticed in connection with the postulates of fertility; fitting the pieces together results in fertilization. Any pattern thus revealed may assume was not designed by anyone. The pieces fit together neither intentionally nor by chance, but because of the inherent connections within the physical world. In principle, new patterns can emerge that have never before been seen.

Perhaps the discovery of new knowledge within the scientific literature is commonplace, and I am doing no more than describing some obvious ways that it might happen. Indeed, I have found instances of such discoveries by others, but I am not yet sure how common it is. One characteristic feature of the kind of discovery to which I refer is the bibliographic pattern that supports it. The author making the discovery would have to bring together literatures that have not before been seen as related.

In any event, the main problem is to systematize and describe a process by which new connections can with some regularity and consistency be discovered in the scientific literature. I have taken an initial step toward a solution, in the form of a structured trial and error-elimination process that appears to hold promise; it is, in effect, a paradoxical quest for the absence of retrieval clues. The trial part consists of browsing among article titles that are selected in such a way as to enhance their suggestiveness of logical connections. Connections that are well known are considered “errors” and eliminated at one level by simple online search for certain co-occurrences, and more deeply by analyzing co-citation patterns [42].

Co-citation, or rather the absence of co-citation, plays a central role in this process. As a matter of historical interest, here is probably the first suggestion (1957) that co-citations might have significance — another good idea put forward, and dismissed, by Bar Hillel:

And one can think of many other easily establishable relationships between documents that stand a better chance [than co-requests] of being a useful approximation, e.g., co-occurrence of their references in reference lists printed at the end of many documents, co-quotatation, and so on [2, p. 111].

This offhand remark does not of course diminish the importance of Henry Small's independent development of the co-citation idea in 1973 [43]. Incidentally, this Bar Hillel paper has not before now been co-cited with any paper by Henry Small according to Scisearch® and Social Scisearch®, on Dialog® (June, 1986), suggesting therefore that the passage quoted has gone unnoticed.

Although the goal of much advanced IR research is to make computers do humanlike things, the possibility that people can be humanlike should not be overlooked. The dazzling capability that the online services already offer opens new horizons for human creativity in exploring the world of recorded knowledge, and new opportunities for research into humanized information retrieval. The potential for improving human interaction with recorded knowledge is immediate, but there seem to be few signs of interest in so practical a goal. Waiting for Godot, we fail to grasp what is now in reach.

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