Building Reliable Test and Training Collections in Information Retrieval

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by

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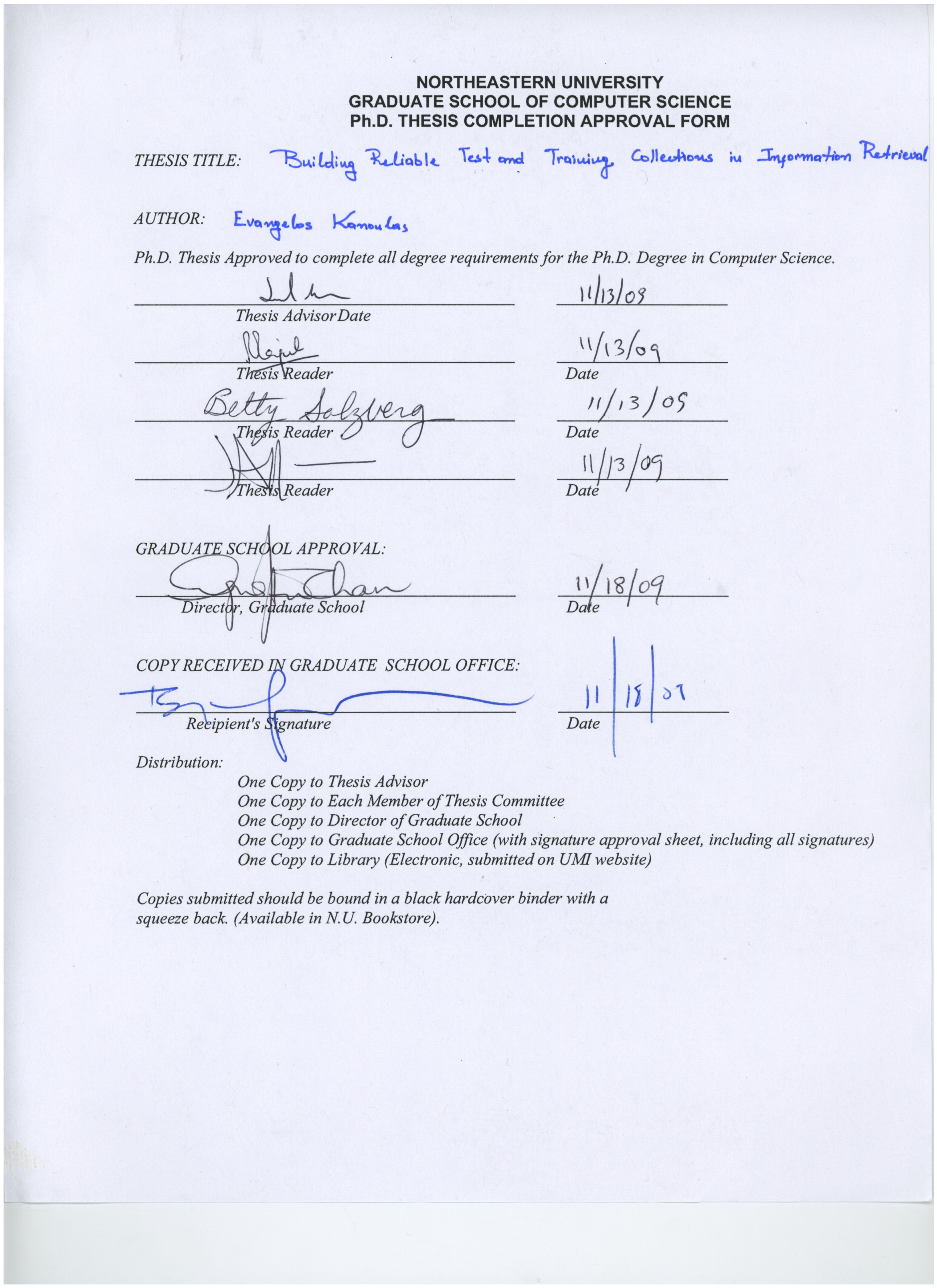

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Abstract

Research in Information Retrieval has significantly benefited from the availability of standard test collections and the use of these collections for comparative evaluation of the effectiveness of different retrieval system configurations in controlled laboratory experiments. In an attempt to design large and reliable test collections decisions regarding the assembly of the document corpus, the selection of topics, the formation of relevance judgments and the development of evaluation measures are particularly critical and affect both the cost of the constructed test collections and the effectiveness in evaluating retrieval systems. Furthermore, recently, building retrieval systems has been viewed as a machine learning task resulting in the development of a learning-to-rank methodology widely adopted by the community. It is apparent that the design and construction methodology of learning collections, along with the selection of the evaluation measure to be optimized significantly affects the quality of the resulting retrieval system. In this work we consider the construction of reliable and efficient test and training collections to be used in the evaluation of retrieval systems and in the development of new and effective ranking functions. In the process of building such collections we investigate methods of selecting the appropriate documents and queries to be judged and we proposed evaluation metrics that can better capture the overall effectiveness of the retrieval systems under study.

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CHAPTER 1

Introduction

Information retrieval (IR) is the study of methods for organizing and searching large sets of heterogeneous, unstructured or semi-structured data. In a typical retrieval scenario a user poses a query to a retrieval system in order to satisfy an information need generated during some task the user is undertaking (e.g. filing a patent). The retrieval system accesses an underlying collection of searchable material (e.g. patent text documents), ranks them according to some definition of relevance of the material to the user's request and returns this ranked list to the user. *Relevance* is a key concept in information retrieval. Loosely speaking, a document is relevant to a user's request if it contains the information the user was looking for when posing the request to the retrieval system [46]. Different *retrieval models* have been constructed to abstract, model and eventually predict the relevance of a document to a user's request.

Traditional retrieval models measure the relevance of a document to a user's request (user's query) by some similarity measure between the language used in the document and the language used in the query. If both the document and the query contain similar words and phrases then it is highly likely that they both describe the same topic and thus the document should be relevant to the query. Tools and techniques to enhance the ability of retrieval models to predict the relevance of documents to users' requests have also been developed. An example of such techniques is pseudo-relevance feedback and query expansion, where retrieval systems are first run over the original query, they retrieve a ranked list of documents, they consider the top k documents relevant and expand the original query with terms out of these pseudo-relevant documents to overcome issues such as language mismatch between the original query and a relevant document.

However, the notion of relevance is not that simple. There are many factors other than the topical agreement between a query and a document that can determine a user's decision as to whether a particular document is relevant or not. The quality of the document, its length, its language, the background of the user, whether similar documents have already been returned and read by the user can all influence the user's decision. Different document- and query-dependent features capture some of these aspects of relevance. Hence, modern retrieval systems combine hundreds of features extracted from the submitted query and underlying documents along with features of past users' behavior obtained from query logs to assess the relevance of a document from a user's perspective. Learning-to-rank algorithms have been developed to automate the process of learning how to combine these features and effectively construct a ranking function. Learning-to-rank has recently gained great attention in the IR community and it has become a widely used paradigm for building commercial retrieval systems. Training ranking functions requires the availability of *training collections*, i.e. document-query pairs from which a set of features can be extracted. Implicit or explicit feedback can be utilized as labels in the training process. Click-through data and query reformulations are examples of implicit feedback that has been used in the training of ranking functions [99]. Such data is easy to obtain from query logs that record the interaction of users with the retrieved results of a search engine. However, this data is usually noisy and correspond to a limited number of documents per query that the user examines. On the other hand, training with explicit feedback requires the relevance of each document with respect to each query in the collection to be assessed to be used as a label in the training process. In this work we only consider the construction of training collections with explicit relevance judgments.

Research and development in information retrieval has been progressing in a cyclical manner of developing retrieval models, tools and techniques and testing how well they can predict the relevance of documents to users' requests, or in other words how well the response of the retrieval system can fulfill the users' information needs. The development of such models, tools and techniques has significantly benefited from the availability of *test collections* formed through a standardized and thoroughly tested evaluation methodology. Systematic variations of key parameters and comparisons among different configurations of retrieval systems via this standardized evaluation methodology has allowed researchers to advance the state of the art.

Constructing test and training collections requires the user to judge the quality of the response of a retrieval system to his/her request. Since users are rarely willing to provide feedback on the quality of the response of a retrieval system, judges are hired to examine each document in the collection and decide the degree of relevance of the document to the user's request for a number of queries. This introduces a steep cost in the construction of test and training collections which limits the ability of researchers and engineers to develop and test new models and methods in information retrieval. Reducing the cost of constructing test and training collections reduces the resources used in such collections (in terms of queries used, documents judged and judges) and may eventually reduce the generalizability of the conclusions drawn by the evaluation or the effectiveness of the trained retrieval system. It is hence essential that queries and documents that constitute the test and training collections are intelligently selected to reduce the overall cost of constructing collections, without reducing the reliability of the evaluation or harming the effectiveness of the produced ranking function. Along with the documents and queries used in the construction of test and training collections, the evaluation metric utilized either to summarize the overall quality of the retrieval system or to be optimized in the construction of ranking functions also affects the cost and the reliability (effectiveness) of the evaluation (ranking function).

The broad goal of this work is to provide a methodological way of constructing test and training collections in an effective, reliable and efficient manner. To that end, we focus on particular issues raised by the current collection construction methodology.

1.1 Evaluation

The evaluation methodology currently adopted by the IR community is based on the *Cranfield* paradigm of controlled laboratory experiments [40]. First a collection of documents (or other searchable material) and user's requests – in the form of topics or queries – is assembled. The retrieval system under evaluation is run over the collection returning a ranked list of documents for each user's request. Since real users are rarely willing to provide feedback on the performance of the systems, human judges are hired to examine each one of the returned documents to decide on its relevance. The performance of the retrieval system is then evaluated by some effectiveness metric that summarizes the quality of the returned results [141]. To eliminate noise in the effectiveness scores, metrics are often averaged over all user's requests. Further, to ensure the reliability of the comparisons among different systems or system configurations, hypothesis testing is employed [120]. Most of the traditional and still popular evaluation metrics are functions of *precision* and *recall*. Precision is the proportion of retrieved documents that are relevant. Recall is the proportion of relevant documents retrieved. When recall is used in the evaluation there is an assumption that all the relevant documents in the collection are known. In other words, judges not only have to assess the relevance of documents returned by a system as a response to a user's request, but also the relevance of documents in the collection that have not been returned by the system.

The current evaluation paradigm abstracts retrieval from the real-world noisy environment and simulates different retrieval scenarios by controlling (1) the material to be searched (e.g. document corpora), (2) the user's requests, (3) the judging process, and (4) the effectiveness metric. Abstracting the retrieval process via the aforementioned variables allows reproducible evaluation experiments, while controlling the noise in test collections via these variables allows formal experimental comparisons between systems or system configurations and scientifically reliable inferences. Furthermore, to amortize the steep cost of obtaining relevance judgments (due to the human effort required to assess the relevance of each document to every query in the collection) it is a current practice to build general purpose test collections. The Text REtrieval Conference (TREC) organized by the U.S. National Institute of Standards and Technology (NIST) was the first attempt by the IR community to construct large scale test collection [141]. TREC set the example for other similar forums (CLEF [19], INEX [55], NTCIR [74]).

The prominent success of information retrieval along with the constant increase of available information in all kinds of environments and the explosion of tasks that require access to the right information at the right time has made IR technology ubiquitous. IR tools and methodologies are employed in tasks such as spam filtering, question-answering, desktop search, scientific literature search, law and patent search, enterprise search, blog search, recommendation systems. Nevertheless, the ability of researchers and engineers to develop new tools and methodologies or simply customize and adapt already successful techniques to the particular needs of different retrieval scenarios is limited by the lack of appropriate test collections. Hence, although successful, the current practice of building general purpose test collections cannot accommodate the increasing needs of the IR community for a highly diverse set of test environments customized to their specific needs. At present, researchers will often test new models and techniques intended for innovative retrieval environments against inappropriate test collections, which makes any conclusions drawn unreliable. The lack of customized test collections and evaluation frameworks constitutes a significant barrier in the progress of information retrieval.

Constructing customized test collections for each particular retrieval scenario, however, requires extensive human effort (in acquiring relevance judgments) which makes the development of such test collections practically infeasible. For instance, even a small scale TREC collection requires 600-800 hours of assessor effort to provide relevance judgments for only 50 queries. Researchers, small companies and organizations cannot afford the development of proprietary test collections that match their particular needs, while vendors of retrieval systems cannot afford the development of different test collections for each one of the tens or hundreds of their customers.

Thus, it is absolutely critical to develop techniques that make evaluation *efficient*. High efficiency, however, comes at a price of low reliability. Reducing the cost of evaluation, i.e. the available resources (queries, judges and documents to be judged), reduces the generalizability of the constructed test collection and thus the generalizability of the evaluation outcome. For instance, in the extreme case of evaluating retrieval systems over a single query, any projections of the relative performance of retrieval systems in this experiment to the general case is questionable. Hence, any proposed technique for efficient evaluation should also account for the *reliability* and *generalizability* of the evaluation outcome.

1.2 Learning-to-Rank

As in the case of test collections constructing data sets for learning-to-rank tasks requires assembling a document corpus, selecting user information requests (queries), extracting features from query-document pairs and annotating documents in terms of their relevance to these queries (annotations are used as labels for training). Over the past decades, document corpora have been increasing in size from thousands of documents in the early TREC collections to billions of documents/pages in the World Wide Web. Due to the large size of document corpora it is practically infeasible (1) to extract features from all document-query pairs, (2) to judge each document as relevant or irrelevant to each query, and (3) to train learning-to-rank algorithms over such a vast data set. Furthermore, as mentioned earlier retrieval systems are employed in all kind of different environments for a large variety of retrieval tasks. Since different retrieval scenarios are modeled by controlling documents, queries, judgments and metrics, training retrieval systems for different retrieval scenarios require training over different and customized for the particular needs of the target retrieval scenario training collections.

Thus, it is absolutely critical to develop techniques that make learning-to-rank *efficient*, by reducing the cost of obtaining relevance judgments. However, the construction of learning-to-rank data sets along with the choice of the effectiveness metric to optimize for greatly affects the ability of learning-to-rank algorithms to effectively and efficiently learn. Thus, any proposed technique for efficient learning-to-rank should also account for the *effectiveness* of the resulting retrieval system.

1.3 Evaluation metrics

Evaluation metrics play a critical role in the development of retrieval systems either as metrics in comparative evaluation experiments, or as objective functions to be optimized in a learning-to-rank fashion. Due to their importance, dozens of metrics have appeared in IR literature. Even though different metrics evaluate different aspects of retrieval effectiveness, only a few of them are widely used, with average precision (AP) being perhaps the most

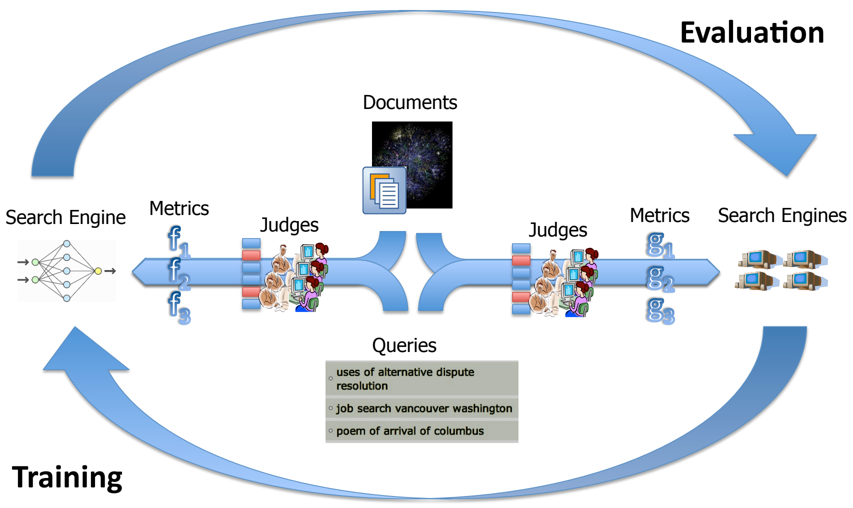


Figure 1.1: Training and evaluating retrieval systems.

commonly used such metric.

One of the main criticism traditional evaluation metrics, such as average precision, have received is due to the assumption they make that retrieved documents can be considered as either relevant or non-relevant to a user's request. In other words, traditional metrics treat documents of different degrees of relevance as equally important. Naturally, however, some documents are more relevant to a user's request than others and therefore more valuable to a user than others.

Thus, evaluation metrics that can utilize this graded notion of relevance are required to better capture the quality of the returned to the user documents and to better guide the construction of ranking functions.

1.4 Duality between evaluation and learning-to-rank

As illustrated in Figure 1.1, constructing retrieval systems can be thought as an iterative process of a training phase and an evaluation phase. In both phases, a collection of documents and queries needs to be assembled and accessed in the case of training by the machine learning algorithm, while in the case of evaluation by the already constructed retrieval systems. Relevance judgments need to be obtained by human judges and a metric of effectiveness needs to be selected to summarize the quality of the retrieval system as a function of these judgments. In both cases, one needs to decide how to select the appropriate queries, the appropriate documents to be judged and the appropriate evaluation metric to be used for an efficient, reliable and effective evaluation and learning-to-rank.

1.5 Contributions

This work is devoted to the development of a low-cost collection construction methodology that can lead to reliable evaluation and effective training of retrieval systems by carefully selecting (a) queries and documents to judge, and (b) evaluation metrics to utilize. In particular, the major contributions of this work are,

- In constructing test collections:
  - A stratified sampling methodology for selecting documents to be judged that can reduce the cost of judging. Based on this stratified sampling methodology and the theory of statistical inference standard evaluation metrics can be accurately estimated along with the variance of their scores due to sampling.
  - A framework to analyze the number and the characteristics of queries that are required for reliable evaluation. The framework is based on variance decomposition and generalizability theory and is used to quantify the variability in evaluation scores due to different effects (such as the sample of the queries used in the collection) and the reliability of the collection as a function of these effects. Based on that, the minimum number of queries along with the distribution of queries over different query categories (e.g. long vs. short queries) that can lead to the most reliable test collections can be found.
- In constructing training collections:
  - A study of different techniques for selecting documents for learning-to-rank and an analysis of the characteristics of a good training data set. Different techniques include low-cost methods used to select documents to be judged in the context of evaluation. Characteristics such as the similarity between the selected documents, the precision and the recall of the training set are analyzed and their correlation to the effectiveness of the resulting ranking functions is analyzed.
- In utilizing evaluation metrics:
  - A study on the reliability of one of the most popular evaluation metrics, the normalized Discounted Cumulative Gain (nDCG). NDCG is a functional of a gain

and a discount function. Different gain and discount functions introduce different amount of variability in the evaluation scores. The same framework based on variance decomposition and generalizability theory is used here to quantify this variability. Based on this framework efficiency-optimal gain and discount functions are defined.

– A novel metric of retrieval effectiveness, the Graded Average Precision (GAP) that generalizes average precision (AP) to the case of multi-graded relevance and inherits all the desirable characteristics of AP: (1) it has the same natural top-heavy bias as average precision and so it does not require explicit discount function, (2) it has a nice probabilistic interpretation, (3) it approximates the area under a graded precision-recall curve, (4) it is highly informative, and (5) when used as an objective function in learning-to-rank it results in good performance retrieval systems.

CHAPTER 2

Background Information

2.1 Retrieval Models and Relevance

One of the primary goals in IR is to formalize and model the notion of relevance. For this purpose a number of retrieval models have been proposed. Even though completely understanding relevance would require understanding the linguistic nature of documents and queries, traditional retrieval models simply treat documents and queries as *bags of words* and measure the relevance of a document to a user's request by some similarity metric between the term [1] occurrences in the document and the term occurrences in the user's requests. If both the document and the user's request contain similar terms then it is highly likely that they both describe the same topic and thus the document is relevant to the query. Three of most significant retrieval models that have appeared in the literature are (a) the boolean model, (b) the vector space model, and (c) the probabilistic model.

The underlying mathematical framework of the *Boolean retrieval model* is Boolean algebra. A query is represented as a Boolean expression of keywords. Often proximity operators and wild characters are also used. Documents are represented as binary vectors that indicate the existence or absence of a word from the overall corpus in the document. A document is considered relevant if its representation satisfies the boolean expression. Thus, the output of a Boolean retrieval system is a set of documents as opposed to a ranked list of documents. Boolean retrieval models are still employed in high-recall tasks where the goal is to find all relevant documents in a collection (e.g. legal documents retrieval or patent retrieval).

The *Vector Space Model* was introduced by Salton in the 60's and 70's [113, 114]. In this model documents are represented as vectors in a k-dimensional space, where k is the number of unique terms in the document corpus. To account for the importance of a term in a document, terms are weighted by their frequency in the document (*term frequency*).

[1]A term may be a word, a stem or a phrase.

The more times a term occurs in a document the more probable it is that the topic of the document is about this term. Term frequencies are usually normalized so that short documents are not penalized when compared to long documents. Further, to account for the general discriminative power of a term, that is the ability of the term to discriminate relevant from nonrelevant documents, terms are also weighted by the inverse of the number of documents that contain these terms in the entire corpus (*inverse document frequency*). The more documents that contain a term the less useful this term is to discriminate relevant from nonrelevant documents [102]. Queries are also represented as term vectors in the k-dimensional space. The similarity between a query and a document is then measured by the cosine of the angle between the query and the document vector. Documents are ranked by their similarity to the query and the ranked list is returned to the user.

Probabilistic Models employ probability theory to model the uncertainty of a document being relevant to a user's request. The development of such models were highly motivated by the Probability Ranking Principle [101] which states that, given that the relevance of a document is independent of the relevance of other documents, the optimal overall effectiveness of a system is achieved by ranking documents in order of decreasing probability of relevance to a user's request. Different ways of computing the probability of a document being relevant gave rise to different probabilistic models. The *BM25* ranking algorithm [73] was developed on the basis of decision theory and Bayesian statistics. Similar to the Vector Space Model, BM25 has a term frequency and an inverse document frequency component. *Language Models* were developed on the basis of probability distributions. The distribution of terms in a document (query) defines a language model that represents the document's (query's) topic. The simplest language model, the unigram model, is a probability distribution over the corpus' vocabulary contained in a document (query). Higher-order models that consider word dependencies, e.g. bigram or trigram models, have also been used in Information Retrieval. The relevance of a document to a given query is then measured either by measuring the similarity between the document and query language models or by computing the probability of generating the query (document) given the document's (query's) language model. Language models were first introduced in IR by Ponte and Croft [95]. Different versions of language models have appeared in the literature [65, 123, 16, 47, 153, 57, 82].

Despite the success of the afore-described models, the concept of relevance cannot always be captured by the topical agreement between a query and the documents in the corpus.

First, different retrieval tasks dictate different definitions of relevance. The most rudimentary retrieval task is *ad-hoc* retrieval, where a user submits an arbitrary query and the system returns a ranked list of documents to match this query. Typically, a document is considered relevant if it contains any information about the topic of the query. In *known-item* retrieval [15] users look for a particular document that they know it exists in the collection and which constitutes the only relevant document. Similarly, in *home-page* and *name-page retrieval* [63] users are looking for a particular site, which also constitutes the only relevant document. In *topic distillation* users are looking for web pages that are good entries to relevant sites, while in *question answering* users are expecting a natural language answer to their question.

Even within a certain retrieval scenario, though, relevance remains hard to define [85]. The aforementioned retrieval models only consider the topical agreement between the query and the document. However, there are many factors that can determine a user's decision as to whether a particular document is relevant or not. The quality and popularity of the document, its language, the background of the user, whether similar documents have already been returned and read by the user and many other factors can all influence the user's decision.

Different document and query dependent features capture some of these aspects of relevance. Simple features like term frequencies over different sections of the document (e.g. title, body), web features such as the number of incoming and outgoing links or term frequencies over anchor text in case of web corpora, or more complex features such as scores given from the aforementioned models, document popularity given by PageRank [92] or click-through data are all combined by modern retrieval systems to assess the relevance of a document to a user's request. *Learning-to-rank* algorithms have been developed to automate the process of learning how to combine these features and effectively construct a ranking function.

2.2 Evaluation

Research and development in information retrieval have been progressing in a cyclic manner of developing retrieval models and testing how well they can predict the relevance of documents to users' requests. This progress has benefited significantly from an extensive effort of the IR community to standardize retrieval system evaluation by building test collections and developing evaluation metrics.

Karen Spärck Jones and Keith van Rijsbergen [124] underlined the necessity for large test collection. They pointed out that the inadequacy of the small in size early collections (e.g. Cranfield and Communications of ACM collections) to demonstrate the ability of retrieval systems to operate in real-world information retrieval environments was a major barrier into commercializing laboratory technology [141]. Large-scale evaluation was realized only in the early nineties. The Text REtrieval Conference (TREC) organized by the U.S. National Institute of Standards and Technology (NIST) was the first attempt by the IR community to construct large scale test collection [141]. TREC was designed to provide the infrastructure necessary for large-scale evaluation of information retrieval technology by introducing over the years test collections of tens of millions of documents, thousands of user requests and corresponding relevance judgments in an effort to mimic a real-world environment and standardized metrics to evaluate the retrieval systems effectiveness. TREC set the example for other similar forums (CLEF [19], INEX [55], NTCIR [74]).

The current evaluation methodology is based on the Cranfield paradigm of controlled laboratory experiments [40]. First a collection of documents and user requests is assembled. Retrieval systems are then run over the collection returning a ranked list of documents for each user request. Human judges examine each one of the returned documents to decide on its relevance. The performance of the retrieval system is then evaluated by some effectiveness metric that assesses the balance of relevant to non-relevant documents returned [141]. Effectiveness scores are averaged over all user requests to assess the overall performance of the retrieval systems. To ensure the reliability of the comparisons among different systems or system configurations, hypothesis testing is employed [120].

The Cranfield evaluation paradigm makes three basic assumptions [138]. First, it assumes that the relevance can be approximated by topical agreement between the document and the query. This implies that all relevant documents are equally important, that the relevance of a document is independent of the relevance of any other document and that the user information need is static. The second assumption is that a single judge can represent the entire user population. The final assumption is that relevance judgments are complete [41]. Even though these assumptions are not true in general, they define a laboratory type of reproducible experiments.

In an attempt to design large and reliable test collections decisions regarding the assembly of the document corpus, the selection of topics, the formation of relevance judgments and the development of evaluation metrics are particularly critical.

2.2.1 Document corpora

Different retrieval scenarios often require different in nature corpora [90, 51, 62, 58]. Thus, document corpora are obtained on the basis of their suitability to the retrieval scenario under study and also on the basis of their availability [141].

Over the past 40 years, Information Retrieval research has progressed against a background of ever-increasing corpus size. From the 1,400 abstracts in the Cranfield collection, the first portable test collection, to the 3,200 abstracts of the Communications of the ACM (CACM), to the 348,000 Medline abstracts (OHSUMED), to the first TREC collections of millions of documents, to the web—billions of HTML and other documents—IR research has had to address larger and more diverse corpora.

When constructing test collections, especially those that simulate web retrieval, it is infeasible to obtain all available documents and thus typically test collection corpora consist only of a subset of the available documents. Hawking and Robertson [64] investigated the relationship between collection size and retrieval effectiveness. By considering subsets of an 18 million documents collection, they concluded that retrieval effectiveness measured by early precision declines when moving to a sample collection. Hence, the limited size of the document corpora often times can negatively affect the accuracy and quality of the retrieval systems evaluation when absolute scores matter.

In the case of web collections different crawling [2] techniques have been proposed in the literature. Crawlers begin with a number of seed web pages, extract their content and collect their out-links. The out-link pages constitute the candidate pages to be crawled next. Given that only a subset of the web can be obtained a good crawling technique should crawl *good pages* as early as possible. Different crawling strategies follow by different definitions of a *good page*. Criteria for the selection of pages to be crawled include link-based popularity [38], topicality [35], user interests [93], avoidance of spam [59]. Recently, Fetterly et al. [52, 52] introduced an evaluation framework, based on measuring the maximum potential NDCG (a popular evaluation metric for web retrieval) that is achievable using a particular crawling policy. They considered two crawls of the scale of 1 billion and 100 million pages respectively. Employing their evaluation framework they showed that crawling does affect evaluation scores and that crawl selection based on popularity (PageRank, in-degree and trans-domain in-degree) allows better retrieval effectiveness than a simple breadth-first crawl of the same size.

2.2.2 Topics

Traditionally, the topics used in TREC-like collections are mainly developed by the judges hired to assess the relevance of documents. There has been no attempt in TREC to develop topics that match any particular characteristics, e.g. length of the topic or number of relevant documents found for the topic, mainly because it is not clear what particular characteristics would be appropriate [141]. TREC topics generally consist of four sections,

[2] Obtaining the contents of a subset of the web is called *crawling*.

(a) an identifier, (b) a title (usually a set of query words), (c) a description of the judge's information need when posing the query, and (d) a narrative of what makes a document relevant.

In order for the evaluation of retrieval systems to reflect real world performance however queries in the test collection should be a sample of the real search workload. It is questionable whether this is the case for the TREC-like made-up queries [106].

To address this issue, the queries comprising recent test collections are selected from actual query logs from commercial search engines [3]. Typically, the selected query set consists of *torso* queries, that is queries that are neither very popular, – such as *yahoo.com* or *wikipedia* – to avoid a bias towards navigational queries, nor too rare – such as *Evangelos Kanoulas phone number* – to avoid any privacy issues raised by the release of tail queries. However, a general theory of how to select or construct good queries for evaluation remains an open issue and it certainly depends on the retrieval scenario one wants to evaluate her retrieval system on.

Furthermore, to increase the coverage of user requests, some of the recent collections contain a much larger number of topics than the 50 topics that typical TREC test collections contain. In the Million Query track [3] ten thousand (10,000) topics were released in TREC 2007 and 2008, with about 1000 of them being used in systems evaluation each year, while in 2009 40,000 queries were released, with about 700 of them being used in systems evaluation.

2.2.3 Relevance Judgments

Obtaining complete relevance judgments is prohibitively expensive in large-scale evaluation where the relevance of millions of documents with respect to each topic needs to be assessed by a human judge. To deal with the problem of acquiring relevance judgments, TREC employs the *pooling method* [125]. Rather than judging every document to every topic, the union of the top k documents retrieved by each retrieval system submitted to TREC per topic and only the documents in this depth-k pool are judged. All the remaining documents are considered nonrelevant under the assumption that if a document is not retrieved by any of the systems contributing to the pool in any of the top-k ranks it is unlikely to be relevant. TREC typically employs depth-100 pooling.

Incompleteness in relevance judgments: Pooling clearly violates the rudimentary assumption of the Cranfield paradigm that judgments are complete. This raises serious concerns about the *reusability* of the constructed test collections. Ideally, relevance judgments in a test collection should be complete enough to allow the evaluation of new systems that did not contribute to the pool. If judgments are incomplete, relevant documents retrieved by new systems but not retrieved by the contributing to the pool systems will be considered nonrelevant introducing a bias in the effectiveness scores.

Harman [61] tested the TREC-2 and TREC-3 collections and demonstrated the existence of unjudged relevance documents. In particular, a pool formed by the second top 100 documents in the ranked results was judged with one relevant document per run found on average. The distribution of the new relevant documents was uniformly distributed across runs but skewed across topics. Zobel [155] also reached similar conclusions by evaluating each submitted run twice, once with the complete set of judgments in the collection and once without the relevant documents found by the run under evaluation. Even though pooling failed to find up to 50% of the relevant documents, the missing judgments were shown not to be biased against the runs that did not contribute to the pool and thus the comparative evaluation results were still reliable.

Recent work, however, suggests that due to the growth of the size of document collections these pools are inadequate for identifying most of the relevant documents [22]. Furthermore, even though a depth-100 pool is significantly smaller than the entire document collection, it still requires extensive judgment effort. In TREC 8 for example, 86,830 judgments were used to assess the performance of 129 runs in response to 50 queries [134]. Assuming that assessing the relevance of each document takes 3 minutes and assuming that a judge works 40 hours per week and that there are about 50 working weeks per year, obtaining 86,830 relevance judgments requires 2.14 labor-years of effort [145].

Recent research has attempted to reduce the human effort required to evaluate retrieval systems. Soboroff et al. [122] proposed a technique of ranking retrieval systems without relevance judgments by forming a depth-100 pool, randomly sampling documents from this pool and assuming them to be relevant (*pseudo-relevant*). Systems are then evaluated by these pseudo-relevant judgments. Although system rankings by pseudo-relevant judgments appeared to be positively correlated with the ranking of systems when actual relevance judgments were used this methodology failed to identify the best performing systems.

In a similar line of research, Efron [50] also proposed the use of pseudo-relevance judgments. According to his proposed methodology, a number of query aspects were manually generated for each given query. These aspect represent different articulations of an information need. Then a single IR system was run over each one of these query aspects and the union of the top k documents over the query aspects was considered relevant. The correlation of the system ranking over this set of pseudo-relevance judgments with the system rankings over the actual relevance judgments was shown to be higher than the one achieved by Soboroff et al. [122].

While evaluating performance without any human relevance judgments is obviously appealing, it has been argued that such an evaluation process tends to rank systems by *popularity* rather than ”performance” [7].

To reduce the total number of judgments Zobel [155] suggested judging documents in an incremental fashion by judging more documents for topics with many relevant documents so far and fewer for topics with fewer relevance documents so far. In a similar manner, Cormack et al. [44] suggested judging more documents from runs that have returned more relevant documents recently and fewer from runs that have returned fewer relevant documents recently (*move-to-front pooling*). Similarly, Aslam et al. [6] employed the Hedge algorithm to *learn* which documents are likely to be relevant from a sequence of on-line relevance judgments. In their experiments using TREC data Hedge found relevant documents at rates nearly double that of benchmark techniques such as TREC-style depth pooling. Nevertheless, all of the above methods create biased judgment sets. For instance, when Hedge or move-to-front pooling are employed to select documents to be judged, the judgment set is biased against the poorly performing systems.

A solution to the problem of extensive judgment effort and incompleteness of the depth-pools that does not introduce bias in the evaluation results came from methodologies utilizing statistics to infer either standard evaluation metrics or the comparative performance of retrieval systems [31, 5, 32, 30, 33, 146, 148, 110, 148].

Carterette et al. [31, 32] and Moffat et al. [87] selected a subset of documents to be judged based on the benefit documents provide in fully ranking systems or identifying the best systems, respectively. In particular, given a pair of systems, the Minimal Test Collections (MTC) method [31, 32] assigns a weight to each document indicating its importance in determining whether there is a difference in performance of systems by some evaluation metric; the highest-weighted document is judged and that judgment is used to update all other weights. The MTC's formal framework was extended to better estimating the probabilities of relevance of unjudged documents [30] resulting in a more effective evaluation. Even though the aforementioned approaches are shown to reduce the relevance judgments required, these methods are not guaranteed to compute or estimate the actual values of standard evaluation metrics. Hence, the values of metrics obtained by these methods are difficult to interpret.

Yilmaz and Aslam [146] and Aslam et al. [5] instead used random sampling to estimate the actual values of effectiveness metrics. Both of these methods are based on treating incomplete relevance judgments as a sample drawn from the set of complete judgments and using statistical methods to estimate the actual values of the metrics. Yilmaz and Aslam [146] used uniform random sampling to select documents to be judged from a depth-k pool. On the other hand, in Aslam et al. [5] samples are drawn according to a carefully chosen non-uniform distribution over the documents in the depth-100 pool. Even though this latter method is more efficient in terms of judgment effort than the former, it is very complex both in conception and implementation and therefore less usable.

All the aforementioned methods attempt to reduce the number of required relevance judgments by reducing the number of judgments per query. Recent work has shown that some queries are more useful in evaluation than others [86]. Thus, by selecting the appropriate queries one may also reduce the judging effort and/or improve the reliability of the test collections. Zhu et al. [154] employed Modern Portfolio Theory to dictate the query set that best reduces the uncertainty of the evaluation scores calculated over a subset of topics while preserving the overall difficulty of the query set to a predefined value. The resulting query set includes queries that are the least correlated with each other in terms of the systems performance score. On the other hand, Cattelan and Mizzaro [34] used difficult queries to evaluate good systems and easy queries to evaluate bad systems and ranked systems by normalizing standard evaluation metrics by query hardness. Although both methods suggest a mechanism of selecting queries to be included in a test collection, they both perform a post-hoc analysis, i.e. they both require the knowledge of system performance over the original query set.

Inconsistency in relevance judgments: In standard settings, the relevance of documents is assessed by a single assessor. However, relevance judgments are known to differ across judges and even for the same judge at different times [117, 136]. Hence, this inconsistency in relevance judgments raises the question as to whether the retrieval systems are correctly evaluated using the judgments from a single judge. Test collections with relevance judgments from additional assessors have been developed to test this inconsistency. Voorhees [136] showed that even if there is a large variation in what is relevant and what is not when different relevance assessors judge documents for relevance, the relative performance of retrieval systems is extremely stable. Therefore, she concluded that obtaining relevance by a single assessor is adequate for comparative evaluation. Recent studies, however, have shown that test collections are not completely robust to changes of judge when judges vary in task and topic expertise or when the relevance threshold of judges drops in response to difficult topics [13, 2, 118].

2.2.4 Reliability of Evaluation

As it has become apparent a test collection is a sample from a general collection of user activities over searchable material. The manner in which documents, queries and judgments are collected significantly affects the quality of the evaluation and thus the reliability and generalizability of any conclusions. However, most of the reliability studies only consider the query effects, while some also consider the inconsistency among assessors.

Banks et al. [14], by fitting an analysis of variance model into TREC-3 results, demonstrated that topic and system effects as well as the interaction between the topic and the system were all highly significant sources of variability, with the topics effect being the largest. In other words, the differentiation between effectiveness scores of different systems per topic is mainly due to the topic itself and the way the retrieval system deals with the particular topic and less due to the difference in the quality of retrieval systems.

To compensate variability, effectiveness scores are typically averaged over a number of topics (even though this practice has also received criticism [104]). The larger the number of topics effectiveness scores are averaged over the less the variability of the mean scores, however, the larger the judgment effort.

Buckley and Voorhees [24, 140] tested the consistency of performance comparisons done on given test collections as a function of the topic set size. Their method was based on calculating the *swap rate* of individual effectiveness comparisons — or, in other words, the likelihood that the decision that one retrieval system is better than another would change if tested over a different set of topics — as a function of the topic set size. The size of a topic set that can guarantee a small fixed swap rate was also computed as a function of the evaluation measure employed and the size of the difference in effectiveness scores between systems. The empirical results suggested that researchers should be sceptical for evaluation conclusions even over 50 topics and that multiple test collections should be used to evaluate systems reliably. Sanderson and Zobel [115] refined the swap rate so it only considers comparisons with statistically significant differences in the performance of the two systems. They concluded that statistically significant results over 50 topics with relative score differences greater than 10% can be considered reliable. All conclusions of the aforementioned work were drawn by using topic sets of up to 25 topics and extrapolating to topic sets of 50 topics. Recently, Voorhees [139] repeated the same study with 50 topics and concluded that neither statistical significance nor score normalization [142] can guarantee the reliability of the conclusions especially when user-oriented metrics (discussed below) are employed in evaluation. Finally, using different number of topics and different amounts of relevance judgments Sanderson and Zobel [115] suggested that more topics and shallow pools can lead to more reliable evaluation than few topics and deep pools.

In the same line of the work by Banks et al. [14], Bodoff and Li [17], also fit an analysis of variance model in TREC results, considering not only the topic effect in the variability of effectiveness scores, but also the assessor effect and its interactions with system and topic effects. Their work also suggested that topic effect is the most significant and therefore they concluded that given a fixed number of available judgments one is better off judging more topics than having more than one assessors judging the same documents over the same topics.

2.3 Learning-to-Rank

2.3.1 Learning-to-rank algorithms

Learning-to-rank has recently attracted a great deal of attention both in the information retrieval and in the machine learning community. As a result, dozens of learning-to-rank algorithms appeared in the literature.

Typical retrieval metrics are a function of the relevance and the ranking of documents returned by a search engine. The value of such measures changes only if two documents of different relevance are flipped in the ranking. Thus, typical IR metrics are not smooth nor differentiable. Ascribed to this, early proposed learning-to-rank algorithms optimized ranking functions for measures loosely related to measures of retrieval effectiveness, e.g. classification error, accuracy or area under the ROC curve. Classical machine learning methods, such as SVM, boosting and neural networks were directly applied to learning-to-rank task, resulting in algorithms such as Ranking SVM [71], RankBoost [54], RankNet [26] and many others [29, 56, 98, 132, 151]. In later development, learning-to-rank algorithms that endeavor to directly optimize IR measures were developed. Since typical IR metrics are non differentiable, these algorithms either optimize for some upper bound of the metric (e.g. SVMmap [152] and AdaRank [143]) or some surrogate of it (e.g. LambdaRank [27] and SoftRank [129]). Recently, Donmez et al. [49] empirically showed that LambdaRank (a learning algorithm which smoothly approximates the gradient of the target effectiveness measure) finds a locally optimal solution for three of the most popular IR metrics with a 99% confidence rate. These results, to some extent, indicate that the learning-to-rank algorithms have resolved the issue of not optimizing for an evaluation metric directly. However, ranking functions still achieve suboptimal performance. Thus, the next critical step is to identify the right features to be extracted and combined and the right data set to train ranking functions on.

2.3.2 Learning-to-rank collections

Relatively little research has been conducted on the choice of queries and documents for learning-to-rank data sets neither on the effect of these choices on the ability of a learning-to-rank algorithm to "learn", effectively and efficiently.

Constructing data sets for learning-to-rank requires assembling a document corpus, selecting user information requests (queries), extracting features from query-document pairs and annotating documents in terms of their relevance to these queries (annotations are used as labels for training). Over the past decades, document corpora have been increasing in size from thousands of documents in the early TREC collections to billions of documents/pages in the World Wide Web. Due to the large size of document corpora it is practically infeasible (1) to extract features from all document-query pairs, (2) to judge each document as relevant or irrelevant to each query, and (3) to train learning-to-rank algorithms over such a vast data set.

The main bottleneck in constructing learning-to-rank collections is annotating documents with relevance grades. It is essential therefore, both for the efficiency of the construction methodology and for the efficiency of the training algorithm, that only a small subset of documents be selected. Yilmaz and Robertson [149] recently demonstrated that given a fixed total judgment budget training over many queries and few documents per query results in more effective ranking functions than training over few queries but many judgments per query. The document selection, though, should be done in a way that does not harm the effectiveness of learning.

LETOR [80] is the only attempt made to construct a publicly available learning-to-rank collection. Documents, queries and relevance judgments were obtained from the OHSUMED and TREC test collections. Since there are many documents in these collections, in order to reduce the computational effort required to extract features and train ranking functions over these data sets, only a subset of them was chosen in the following way: Documents were first ranked by their BM25 [73] score, which is known to correlate well with the relevance of a document to a query. Features then were extracted only from the corresponding top 1000 documents, in an effort to include as many relevant documents as possible in the learning-to-rank dataset. Features were also extracted from documents that were not ranked in this top 1000 but were judged as relevant in the corresponding TREC collections. In essence, Liu et al. [80] employed this certain document selection mechanism for the efficient construction of a learning-to-rank collection with the intuition that relevant documents are more useful than nonrelevant documents in training ranking models.

The extracted features cover most of the standard features in IR, including classical features (such as term frequency, inverse document frequency, BM25 and language models for IR), along with features recently proposed in the literature (such as HostRank, Feature propagation and Topical PageRank) [153, 78, 89, 91, 97, 105, 66, 119, 144].

Even though LETOR has been widely used by many researchers, recent work demonstrated bias in this document selection methodology that could harm learning-to-rank algorithms [83, 96]. When the LETOR collection was built, the fact that documents with low BM25 score were selected only if they were relevant resulted in BM25 being negatively correlated with relevance in the LETOR collection. This is a highly counterintuitive outcome. To avoid the aforementioned implication, these extra documents with low BM25 scores were dropped in the latest LETOR release [128].

For the OHSUMED learning-to-rank collection only judged documents were selected for feature extraction. As pointed out by Minka and Robertson [83], this selection methodology results in an atypical proportion of relevant and non-relevant documents in the collection. Further, the nature of the non-relevant documents in the learning-to-rank collection is not representative of that in the entire OHSUMED collection.

These issues clearly manifest the effect a document selection methodology may have on the effectiveness of the learning-to-rank algorithms, and thus, on the performance of the resulting retrieval systems. Furthermore, the conclusions about the relative quality of different learning-to-rank algorithms may not be reliable.

2.4 Evaluation Metrics

Evaluation metrics play a central role in the construction of effective information retrieval systems. In a typical setup, key parameters of a retrieval system are systematically varied in controlled laboratory experiments and the different configurations are then compared against each other on the basis of some measure of retrieval effectiveness. In an alternative setup, machine learning techniques are applied to “learn” a ranking function that optimizes some metric. It is hence apparent that the quality of the evaluation metric employed in either setups directly affects the quality of the resulting retrieval system.

Due to their significance, dozens of evaluation measures have been proposed in the IR literature. The proposed measures are based on the assumption that the quality of a retrieval system is reflected on the quality of the ranked list of documents retrieved to satisfy a certain user information need. Thus, evaluation measures are a function of the relevance of the retrieved ranked list of documents.

Two of the most common evaluation metrics introduced in the Cranfield studies are *precision* and *recall*. Precision is the the proportion of retrieved documents that are relevant and recall is the proportion of relevant documents that are retrieved. Both metrics assume *binary relevance*, i.e. a document is either relevant or nonrelevant. Further they operate over sets of documents as opposed to ranked lists of documents.

There is a trade-off between precision and recall. A retrieval system could achieve maximum recall by returning all documents in the collection, but the precision of the returned set would be very small. On the other hand, a system could achieve maximum precision by returning just a single relevant document but the recall in this case (given that there are multiple relevant documents in the collection) would be very small. Thus, the goal of a system is to achieve both a high precision and a high recall, i.e. to retrieve as many relevant documents and as few nonrelevant documents as possible.

The *F-measure* gives a single score to each system by trading-off precision and recall. It is defined as the weighted harmonic mean of the two measures,

$$F = \frac{1}{\alpha \cdot \frac{1}{\text{Precision}} + (1-\alpha) \cdot \frac{1}{\text{Recall}}}$$

The F-measure is often transformed using $\alpha = 1/(\beta^2 + 1)$,

$$F_\beta = \frac{(\beta^2 + 1) \cdot \text{Precision} \cdot \text{Recall}}{(\text{Recall} + \beta^2 \cdot \text{Precision})}$$

The most common version of the F measure is F_1, which gives equal weight to precision and recall. In some evaluations, precision or recall is emphasized by varying the value of β. Values of $\beta > 1$ emphasize recall.

Most retrieval systems, though, return a ranked list of documents instead of a set of documents. To account for that, precision and recall values are computed at each rank (cut-off) by only considering the set of documents above this rank as the documents returned by the system. This series of precision and recall values can be visualized by plotting precision against recall each time a new relevant document is retrieved, i.e. each time the recall value changes, resulting in a *precision-recall curve*. A precision-recall curve fully characterizes the performance of a system, i.e. given a precision-recall curve along with the total number of relevant documents in the collection one can exactly reconstruct the list of relevant and nonrelevant documents returned by the system.

Most of the traditional evaluation metrics are a function of precision and recall [11, 81, 135]. Three of the most commonly used evaluation metrics in information retrieval are *precision-at-cutoff* k, *R-precision*, and *Average Precision*. All of these metrics produce values in the range $[0, 1]$.

Precision-at-cutoff k is the proportion of relevant documents after the first k documents are retrieved. For example, precision-at-cutoff 10, PC(10), is the fraction of documents among the first 10 in a list which are relevant. This may, for example, correspond to the accuracy of the first page of a web retrieval systems results. PC(k) can be calculated for any k; however, the most commonly reported cutoffs k are 5, 10, 15, 20, 30, 100, 200, 500 and 1000. R-precision, RP, is defined as the precision-at-cutoff R, where R is the total number of documents relevant to a topic.

Perhaps the most widely reported overall metric of retrieval effectiveness is average precision, AP. The average precision of a list is the average of the precisions at each relevant document in that list. If we let *isrel(k)* be a boolean operator that returns 0 if the document at rank k is nonrelevant and 1 otherwise, then AP can be defined as,

$$\text{Average Precision} = \frac{1}{R}\sum_{k=1}^{N} isrel(k) \cdot PC(k) = \frac{1}{R}\sum_{k=1}^{N} isrel(k) \cdot \frac{\text{\# of relevant docs up to k}}{k}$$

where N is the total number of documents returned by the retrieval system and R is the total number of relevant documents in the collection.

For example, given a topic with three relevant documents retrieved at ranks 2, 5, and 8 in a list, the average precision would be, $AP = (PC(2) + PC(5) + PC(8))/3 = (1/2 + 2/5 + 3/8)/3 = 0.425$. Precision at unretrieved relevant documents are assumed to be zero, and thus average precision is effectively the sum of the precisions at retrieved relevant documents divided by R.

R-precision and average precision have been shown to be highly correlated. Through a geometric interpretation of the two metrics Aslam et al. [8] has shown that R-precision and Average Precision both approximate the area under the precision-recall curve [100].

In an attempt to characterize evaluation metrics one can divide them into two categories: *user-oriented* and *system-oriented* [48]. User-oriented evaluation metrics evaluate the quality of the output of a retrieval system based on its utility to an end user [43]. On the other hand, system-oriented metrics capture the overall quality of a retrieval system. For example, in the case of web retrieval systems, a user might primarily be interested in the top k documents of the output of a search engine. Therefore, a user-oriented metric would consider only the top k documents of the output, whereas a system-oriented metric would evaluate the quality of the entire output. Based on these definitions, it can be seen that average precision and R-precision are system-oriented metrics while precision-at-cutoff k is a user-oriented metric. *Reciprocal rank*, RR, is another user-oriented metric defined as the reciprocal of the rank of the highest-ranked relevant document. Alternatively, the reciprocal rank can be defined as the precision at the first relevant document in the ranked list. Based on the retrieval scenario under study some evaluation metrics are more appropriate than others. For instance in the case of homepage retrieval the reciprocal rank could be a more appropriate metric to be used than average precision while in the case of legal document retrieval, where there are potentially hundreds of relevant documents that a user may be interested in, average precision could be a more appropriate metric to be used than precision-at-cutoff k. Given that there is no ”optimal” metric, researchers usually report a large number of metrics when evaluate their retrieval systems.

Robertson [103] has recently shown that the distinction between system- and user-oriented metrics is misleading since all metrics based on relevance can be considered user-oriented with respect to the appropriate *user model*. In other words, most evaluation metrics are designed to measure the satisfaction of an end user and their main difference is the associated user model that models how users interact with the retrieval system.

Following Cooper in his proposal for the Expected Search Length measure [42], Robertson [103] envisages a user stepping down a ranked list until some stopping point. Given this, average precision can be explained by a stochastic user model according to which a user terminates browsing only after visiting a relevant document.

Metrics that attempt to better capture the user interaction with the retrieval system have recently been developed. Moffat and Zobel [88] proposed *rank-biased precision* using a rudimentary user model according to which the user is assumed to walk down the ranked list of documents with a fixed probability. Turpin et al. proposed an evaluation metric that also considers the relevance of the snippets [133], Chapell et al. [36] employed the *cascade model* [45] to model the user search behavior considering different stopping probabilities at different ranks and based on this model they proposed the *expected reciprocal rank* metric, while Yilmaz et al. [150] considered a more accurate model than the cascade one along with snippet relevance and based on this model they defined the *expected browsing utility*.

2.4.1 Evaluating evaluation metrics

Attempts have been made to evaluate and compare evaluation metrics on different bases. Buckley and Voorhees [24] proposed a framework that can be used to analyze the evaluation metrics with respect to their stability (accuracy). In particular, they evaluated evaluation metrics in terms of the number of topics needed to reach a stable conclusion about the relative quality of retrieval systems. Aslam et al. [9] has described a framework based on the maximum entropy for evaluating the quality of a performance metric in terms of the amount of information it captures. Given an informative metric one should be able to infer back the ranked list of relevant and nonrelevant documents while giving an non-informative metric that will not be possible. According to their experimental results system-oriented metrics like average precision are more informative than user-oriented metrics since they are sensitive to all document flips. Yilmaz and Robertson [149] have shown that the informativeness of a metric is a significant characteristic when the metric is used as an objective function in learning-to-rank and that informative metrics can better guide the construction of ranking function even though they are less correlated with user satisfaction. Sakai [109] introduced a framework based on Bootstrap [116] and compared metrics on the basis of their sensitivity to evaluation score differences between systems (discriminative power). Finally, recently Yilmaz et al. [150] proposed a methodology that evaluates metrics on the basis of how well they predict users' search behavior modeled by click-through data.

2.4.2 Evaluation metrics for incomplete relevance judgments

Standard evaluation metrics such as the ones described in the previous section have shown not to be robust to incomplete judgments [25]. As a solution to this problem, Buckley and Voorhees [25] proposed *bpref*. Sakai [110] instead applied traditional metrics to condensed lists of documents obtained by filtering out all unjudged documents from the original ranked lists and showed that these versions of metrics are actually more robust to incompleteness than bpref. Carterette et al. [32] and Moffat et al. [87] proposed techniques aiming at accurately inferring the ranking of systems or identifying the best systems, respectively. Yilmaz and Aslam [146], Aslam et al. [5], Yilmaz et al. [148] and Pavlu [94] instead used random sampling to estimate the actual values of average precision when relevance judgments are incomplete. The metric proposed by Yilmaz and Aslam [146], *infAP*, became a commonly used metric by the information retrieval community [18, 121] and was used in TREC VID and Terabyte tracks in 2006 [79, 28]. Furthermore, the metrics proposed by Carterette et al. [32], *MTC*, and Pavlu [94], *statAP*, were used in the Million Query track [3].

2.4.3 Multi-graded evaluation metrics

One of the main criticism traditional evaluation metrics, such as average precision, have received is due to the assumption they make that retrieved documents are either relevant or nonrelevant to a user's request. Naturally, however, some documents are more relevant to a user's request than others.

The nDCG measure [68, 69] has proven to be one of the most popular measures of retrieval effectiveness that utilizes graded relevance judgments. The underlying model of user search behavior on which nDCG is based makes two assumptions: (1) highly relevant documents are more valuable to the user than marginally relevant documents, and (2) the greater the rank at which a relevant document appears the less valuable to the user that document is.

In the framework used to define nDCG, first relevance scores are mapped to relevance grades, e.g. a score of 3 is given to highly relevant documents, a score of 2 to fairly relevant documents and so on. Relevance scores are viewed as the *gain* returned to a user when examining the document. Thus, the relative value of relevance scores dictates how much more valuable for instance a highly relevant document is to a user than a marginally relevant. Even though, relevance scores were used directly as gains when nDCG was originally introduced, alternative gain functions that map gain values to relevance scores have appeared in the literature [26]. To account for late arrival of relevant documents gains are then discounted by a function of the rank. The discount function is viewed as a measure of the patience of a user to step down the ranked list of documents. As in the case of gains, a number of different discount functions has appeared in the literature. The discounted gains are then summed progressively from rank 1 to k producing the *discounted cumulative gain* (DCG). DCG may take arbitrary large values since it depends on the number of relevant documents retrieved as a respond to a topic. Therefore, averaging DCG values over topics is not statistically reliable since the size of recall bases change by topic [69, 67, 107]. Thus, DCG is divided by the DCG of an ideal ranked list of documents to normalize it to a 0 to 1 range, resulting in the *normalized discounted cumulative gain* (nDCG).

More formally, let G denote a relevance grade and $\textit{gain}(G)$ the gain associated with G. Also, let $g_1, g_2, \dots g_N$ be the gain values associated with the N documents retrieved by a system in response to a query q, such as $g_i = \textit{gain}(G)$ if the relevance grade of the document in rank i is G-relevant.

Then, the nDCG value for this system can be computed as,

$$\textit{nDCG} = \frac{\textit{DCG}}{\textit{optDCG}} \text{ where } \textit{DCG} = \sum_{i=1}^{N} g_i / \log_b(i+1)$$

The normalization factor, *optDCG*, for a query q can be defined as the maximum possible DCG value over that query.

Based on cumulative gain, Sakai [111] introduced the *Q-measure* as a better mechanism to control the penalty to late arrivals of relevant documents. Let *isrel(k)* be a boolean operator that returns 0 if the document at rank k is irrelevant and 1 otherwise. Moreover, let N be the total number of documents retrieved and let R be the total number of relevant documents in the collection for a given topic. The Q-measure can be calculated as,

$$\textit{Q-measure} = \frac{1}{R} \sum_{i=1}^{N} \textit{isrel(k) BR(k)} \text{ where } \textit{BR(k)} = \frac{\beta \cdot CG(k) + \textit{count}(k)}{\beta \cdot \textit{opt}CG(k) + k}$$

with *count(k)* returning the number of relevant documents up to rank k and *CG(k)* being the sum of gain values up to rank k. The parameter β controls the penalty upon late arrivals of relevant documents.

2.5 Summary and Directions

Test and training collections are a sample of the user activities over a general corpus of searchable material. The manner in which documents, queries and judgments are obtained along with the evaluation metric utilized either in evaluation or in learning-to-rank affect the reliability of the evaluation conclusions and the effectiveness of the constructed ranking functions. To overcome these issues the IR community constantly attempts to increase the size of the collections and employ a large number of evaluation metrics. This however results in an overly large cost in constructing test and training collections.

In what follows we first propose an intelligent way of sampling documents per query to be judged. Our document selection methodology significantly decreases the cost of evaluation. Then, given a fixed budget of total number of judgments (or labor hours) we examine how should one allocated the available resources among queries and documents per query to be judged. Moreover, we explore whether some query categories are more useful in evaluation that others, i.e. whether some queries could lead faster (in terms of total number of judgments) to reliable evaluation results and thus further decrease the cost of evaluation.

After proposing a reliable and efficient manner of constructing test collections we investigate whether low-cost document selection methodologies developed for the construction of test collections can also be employed in the construction of collections for the purpose of learning-to-rank. We explore a number of such techniques and describe the characteristics of a good training data set in terms of the documents included in the training collection.

Finally, we optimize nDCG, one of the most popular evaluation metrics, to efficiently discriminate good from bad systems (in terms of the number of queries required) and we also propose a new evaluation metric that outperforms nDCG both in the amount of information it captures about the system under evaluation and in the effectiveness of the resulting ranking function when used in the context of learning-to-rank.

CHAPTER 3

Test Collections

The size of collections used in Information Retrieval research has been growing at an astonishing pace, with every decade seeing at least an order-of-magnitude increase in research collection size. Evaluation of retrieval systems requires *test collections* that augment these collections with a set of information needs and judgments of the relevance of documents in the collection to those needs. While collection sizes keep increases, the budget for relevance judgments does not. Reliable evaluations will rely more and more on the relevance judgments being selected intelligently and the inferences about systems made robustly despite many missing judgments. In Section 3.1 we devise a low-cost evaluation methodology based on stratified sampling to intelligently select only a subset documents per query to be judged. In Section 3.2 we employ two low-cost evaluation methodologies, the first proposed by Carterette et al. [32] and the second proposed by Pavlu and Aslam [94], and investigate two questions (a) how many queries are necessary for a reliable evaluation when low-cost techniques are employed, and (b) given a fixed budget of judgments (or labor hours) how should one optimally allocate judgments among queries and documents per query to be judged. The method proposed by Pavlu and Aslam [94] is based on the same principles of stratified sampling as the one described in Section 3.1.

3.1 Document Selection Methodology for Efficient Evaluation

We consider the problem of large scale retrieval evaluation. Recently two methods based on *random sampling* were proposed as a solution to the extensive effort required to judge tens of thousands of documents [5, 146]. Both of these methods are based on treating incomplete relevance judgments as a sample drawn from the set of complete judgments and using statistical methods to estimate the actual values of the measures. These methods are both shown to (1) produce unbiased estimates of average precision even when relevance judgments are incomplete and (2) be more robust to incomplete relevance judgments than any other measures such as bpref [25] or the condensed versions of the measures [146].

The first method was proposed by Yilmaz and Aslam [146], and became a commonly used low-cost evaluation methodology by the information retrieval community [18, 121]. It was used in TREC VID and Terabyte tracks in 2006 [79, 28]. According to this methodology, a uniform random sample of documents over the depth-k pool is obtained, and only the documents in the sample are assessed for relevance. Unbiased estimators of standard evaluation measures are then obtained via statistical inference. The authors demonstrated the their methodology over the estimation of average precision, with the estimator being called *infAP*.

Although the estimated average precision, infAP, is unbiased in expectation, in practice, when calculated using a single sample of relevance judgments, it may vary in value. This necessitates the derivation and use of confidence intervals around the estimated values in order to allow confident conclusions regarding the actual value of average precision and thus the ranking of retrieval systems. In this work we first derive confidence intervals for infAP and validate them using TREC data. We show that infAP along with the corresponding confidence intervals can allow researchers to reach confident conclusions about actual average precision, even when relevance judgments are incomplete.

A limitation of infAP accrues from the measure's design to only to account for incomplete relevance judgments that are a *uniform random* sample drawn from the set of complete judgments. Typical evaluation measures, however, give more weight to documents retrieved towards the top of a returned ranked lists. Therefore, a *top-heavy* sampling strategy could lead to more accurate results, i.e. narrow confidence intervals, with higher efficiency in terms of judgment effort needed.

Such a top-heavy sampling strategy is employed by the second low-cost evaluation method proposed by Aslam et al. [5]. Samples are drawn with replacement according to a carefully chosen non-uniform distribution over the documents in the depth-k pool. Even though this method is more efficient in terms of judgment effort than infAP, it is very complex both in conception and implementation and therefore less usable.

Instead, in this work, we employ a stratified random sampling methodology and extend the simple infAP measure to incorporate relevance judgments created according to *any* sampling distribution. This extended infAP combines the simplicity of random sampling with the efficiency of stratification and thus it is simple and easy to compute while, at the same time, it is much more efficient than infAP in terms of reducing the judgment effort. We further claim that the same methodology can be applied to other evaluation measures and demonstrate how nDCG [69]) can be estimated using incomplete relevance judgments.

3.1.1 Confidence Intervals for infAP

The inferred average precision, infAP, by statistical construction, is an unbiased estimator of average precision and thus it is designed to be exactly equal to average precision in expectation. However in practice, it may be low or high due to the nature of sampling (especially when the subsets of documents whose binary relevance is available is small). In other words, there is variability in the values of infAP because different samples from the collection of documents give rise to different values of infAP. The amount of the variability in infAP is measured by its *variance*.

Before computing the variance of infAP let's revisit the random experiment whose expectation is average precision [146] and identify all sources of variability in the outcome of this random experiment. Given a ranked list of documents with respect to a given topic:

1. Select a relevant document at random and let the rank of this relevant document in list be k.
2. Select a rank, j, at random from the set $\{1, ..., k\}$.
3. Output the binary relevance of the document at rank j.

In expectation, steps (2) and (3) effectively compute the *precision* at a relevant document and in combination, step (1) computes the *average* of these precisions.

The aforementioned experiment can be realized as a two-stage sampling. At the first stage — step (1) — a sample of *cut-off levels* at relevant documents is selected. The infAP value is computed as an average of the estimated precision values at the sampled cut-off levels. Even if we assume that these precision values are the actual precision values, infAP varies because different samples of cut-off levels will result in different values of infAP. Therefore, computing infAP using precision values only at a subset of cut-off levels introduces the first component of variability.

Let *rel* be the set of the judged relevant documents of size r. This first variance component can be estimated as[1]

$$\text{var. comp. } 1 = (1 - p) \cdot s^2 / r$$

where $p \cdot 100\%$ is the sampling percentage and s^2 the variance among the precision values at the judged relevant documents calculated as $s^2 = \left(\sum_{k \in \text{rel}} (\widehat{PC}(k) - \text{infAP}) \right) / r$.

At the second stage — step (2) — for each one of the selected cut-off levels, a sample of *documents* above that cut-off level document is used to estimate the corresponding to the cut-off precision value. Therefore, even for a given sample of cut-off levels, infAP has variability because different samples of documents give rise to different values of precisions

[1]The complete formula of infAP variance along with the derivation can be found at the Appendix A.

and thus different values of infAP. Hence, computing the precision at some cut-off using only a subset of the documents above that cut-off introduces a second component of variability.

Assuming that precisions at different cut-off levels are independent from each other, this second variance component can be estimated as,

$$\text{var. comp. } 2 = \Big(\sum_{k\in \text{rel}} var[\widehat{PC(k)}]\Big)/r^2$$

where $var[\widehat{PC(k)}]$ is the variance of the estimated precision at cut-off k.

According to the Law of Total Variance, the total variance of infAP can be computed as the sum of the two aforementioned variance components; hence,

$$var[\text{infAP}] = (1-p)\cdot\frac{s^2}{r} + \frac{\sum_{k\in \text{rel}} var[\widehat{PC(k)}]}{r^2}$$

The first term of the right-hand side of Equation 3.1.1 corresponds to the variance due to sampling *cut-off levels* while the second term corresponds to the variance due to sampling *documents* above each cut-off level.

When evaluating retrieval systems, the average of infAP values across all topics (mean infAP) is employed. The variance of the mean infAP can be computed as a function of the variance of infAP as

$$var[\text{mean infAP}] = \sum var[\text{infAP}]/(\text{\# of queries})^2$$

According to the Central Limit Theorem one can assign 95% confidence intervals to mean infAP as a function of its variance. A 95% confidence interval centered at the mean infAP intimate that with 95% confidence the actual value of MAP is within this interval.

We used TREC 8, 9 and 10 data to validate the derived variance of the mean infAP when relevance judgments are incomplete. We simulated the effect of incomplete relevance judgments as in [146]. That is, we formed incomplete judgments sets by sampling from the entire depth-100 pool over all submitted runs. This is done by selecting $p\%$ of the complete judgment set uniformly at random, where $p \in \{10, 20, 30\}$.

Figure 3.1 illustrates the mean infAP values computed from a single random sample of documents per topic for each run against the actual MAP values for $p \in \{10, 20, 30\}$ (left, middle and right column, respectively) for TREC 8, 9 and 10 (top, middle and bottom row, respectively). The 95% confidence intervals are depicted as error bars around the mean infAP values. As one can observe, the greatest majority of the confidence intervals intersect the 45^o dashed line indicating that the greatest majority of the confidence intervals cover

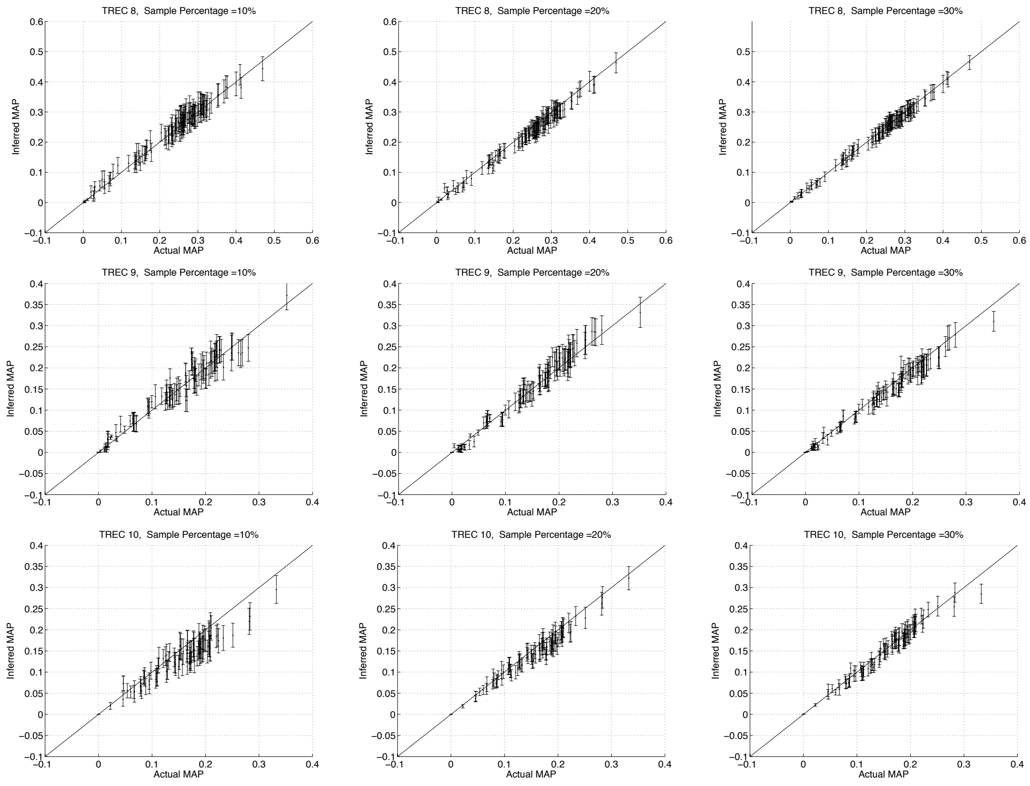


Figure 3.1: TREC 8, 9 and 10 mean inferred AP along with estimated confidence intervals when relevance judgments are generated by sampling 10, 20 and 30% of the depth-100 pool versus the mean actual AP.

the actual MAP values.

Furthermore, we computed the mean infAP values and the corresponding confidence intervals for 100 different sampling trials over TREC 8 data and we accumulated the deviation of the computed mean infAP values from the actual MAP values in terms of standard deviations. This way we generated a Cumulative Distribution Function of divergence of mean infAP values per system. According to the Central Limit Theorem each of these CDF's should match the CDF of the Normal Distribution. Figure 3.2 shows how the two CDF's compare to each other. As it can be observed the CDF for the mean infAP closely approximates the CDF for the Normal Distribution and as so it validates our derived theoretical results.

We also performed a Kolmogorov-Smirnov test of fitness and for 90% of the systems the hypothesis that the two CDF's match could not be rejected ($\alpha = 0.05$) which validates our derived theoretical results.

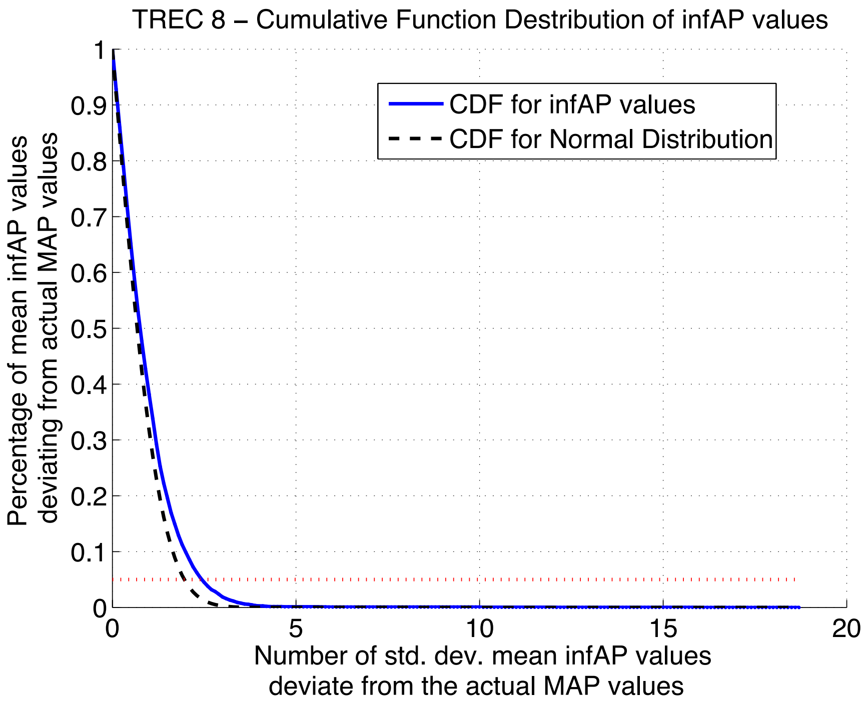


Figure 3.2: Cumulative Distribution Function of the mean infAP values

3.1.2 Inferred AP on Nonrandom Judgments

In the previous section we derived confidence intervals for infAP in a setup where documents to be judged were a random subset of the entire document collection. Confidence intervals can be further reduced (i.e. the accuracy of the estimator can be improved) by utilizing a top-heavy sampling strategy. In this section we consider a setup where relevance judgments are not a random subset of complete judgments and show how infAP can be extended to produce unbiased estimates of average precision in such a setup. We denote the extended infAP measure as *xinfAP*.

Similar to the infAP paradigm, consider the case where we would like to evaluate the quality of retrieval systems with respect to a complete pool and assume that relevant judgments are incomplete. Further assume that the set of available judgments are constructed by dividing the complete collection of documents into disjoint contiguous subsets (strata) and then randomly selecting (sampling) some documents from each stratum to be judged. The sampling within each stratum is performed independently, therefore, the sampling percentage can be chosen to be different for each stratum. For instance, one could choose to split the collection of documents into two strata (based on where they appear in the output of search engines), and sample 90% of the documents from the first stratum and 30% of the documents from the second stratum. In effect, one could think a large variety of sampling strategies in terms of this multi-strata strategy. For example, the sampling strategy proposed by Aslam et al. [5] can be thought as each stratum containing a single document, with different sampling probabilities assigned to different strata.

Let $\widehat{AP}$ be the random variable corresponding to the estimated average precision of a system. Now consider the first step of the random experiment whose expectation corresponds to average precision, i.e. picking a relevant document at random. Note that in the above setup, this relevant document could fall into any one of the different strata s. Since the sets of documents contained in the strata are disjoint, by definition of conditional expectation, one can write $E[\widehat{AP}]$ as:

$$E[\widehat{AP}] = \sum_{\forall s \in \text{Strata}} P_s \cdot E_s[\widehat{AP}]$$

where P_s corresponds to the probability of picking the relevant document from stratum s and $E_s[\widehat{AP}]$ corresponds to the expected value of average precision given that the relevant document was picked from stratum s.

Let R_Q be the total number of relevant documents in the complete judgment set and R_s be the total number of relevant documents in stratum s if we were to have all complete relevance judgments. Then, since selecting documents from different strata is independent for each stratum, the probability of picking a relevant document from stratum s is, $P_s = R_s/R_Q$.

Computing the actual values of R_Q and R_s is not possible, since the complete set of judgments is not available. However, we can estimate their values using the incomplete relevance judgments. Let r_s be the number of sampled relevant documents from stratum s and n_s be the total number of sampled documents from stratum s. Furthermore, let N_s be the total number of documents in stratum s. Since the n_s documents were sampled uniformly from stratum s, the estimated number of relevant documents within stratum s, $\hat{R}_s$, can be computed as $\hat{R}_s = (r_s/n_s) \cdot N_s$. Then the number of relevant documents in query Q can be estimated as the sum of these estimates over all strata, i.e. $\hat{R}_Q = \sum_{\forall s} \hat{R}_s$. Given these estimates, the probability of picking a relevant document from stratum s can be estimated by, $\hat{P}_s = \hat{R}_s/\hat{R}_Q$.

Now, we need to compute the expected value of estimated average precision, $E_s[\widehat{AP}]$, if we were to pick a relevant document at random from stratum s.

Since the incomplete relevance judgments within each stratum s is a uniform random subset of the judgments in that stratum, the induced distribution over relevant documents within each stratum is also uniform, as desired. Therefore, the probability of picking any relevant document within this stratum is equal. Hence, the expected estimated average precision value within each stratum, $E_s[\widehat{AP}]$, can be computed as the average of the precisions at judged (sampled) relevant documents within that stratum.

Now consider computing the expected precision at a relevant document at rank k, which corresponds to the expected outcome of picking a document at or above rank k and outputting the binary relevance of the document at this rank (steps 2 and 3 of the random experiment).

When picking a document at random at or above rank k and outputting the binary relevance of that document, one of the following two cases may occur. With probability $1/k$, we pick the current document, and since this document is by definition relevant the outcome is 1. With probability $(k-1)/k$ we pick a document above the current document, in which case we need to calculate the expected precision (or expected binary relevance) with respect to the documents above rank k. Thus,

$$E[\widehat{PC(k)}] = \frac{1}{k} \cdot 1 + \frac{k-1}{k} E[\widehat{PC}_{\text{above } k}]$$

Let N_s^{k-1} be the total number of documents above rank k that belong in stratum s, n_s^{k-1} be the total number of judged (sampled) documents above rank k that belong to stratum s and r_s^{k-1} be the total number of judged (sampled) relevant documents above rank k that also belong to stratum s.

When computing the expected precision within the $(k-1)$ documents above rank k, with probability $N_s^{k-1}/(k-1)$ we pick a document from stratum s Therefore, the expected precision above rank k can be written as:

$$E[\text{prec above } k] = \sum_{\forall s} \frac{N_s^{k-1}}{k-1} \cdot E_s[\widehat{PC}_{\text{above } k}]$$

where $E_s[\widehat{PC}_{\text{above } k}]$ is the expected precision above rank k within stratum s. Since we have a uniform sample of judged documents from stratum s, we can use these sampled documents to estimate the expected precision within stratum s. Since the incomplete relevance judgments from each stratum is obtained by uniform random sampling, this expected precision can be computed as r_s^{k-1}/n_s^{k-1}.

Note that in computing the expected precision in stratum s, we may face the problem of not having sampled any documents from this stratum that are above the current relevant document at rank k. Adapting the same idea used in infAP, we employ Lindstone smoothing [37] to avoid this problem. Therefore, expected precision above rank k can be computed as:

$$E[\widehat{PC}_{\text{above } k}] = \sum_{\forall s} \frac{N_s^{k-1}}{k-1} \cdot \frac{r_s^{k-1} + \epsilon}{n_s^{k-1} + 2\epsilon}$$

It is easy to see that when complete judgments are available, xinfAP is exactly equal to average precision (ignoring the smoothing effect). Further, note that infAP is a particular instantiation of this formula with a single stratum used.

Overall, the advantage and real power of the described stratified random sampling and the derived AP estimator, xinfAP, is the fact that it combines the effectiveness of the sampling method proposed by Aslam et al. [5] by employing stratification of the documents and thus better utilization of the judgment effort with the simplicity of infAP by employing random sampling within each stratum.

3.1.3 Inferred AP in TREC Terabyte

As mentioned, xinfAP can be used with a large variety of sampling strategy. In this section, we focus on the sampling strategy used in TREC Terabyte 2006 [28] and we show that (1) *xinfAP* is highly effective at estimating average precision and (2) it better utilizes the judgment effort compared to infAP.

First, let's briefly consider the sampling strategy used in TREC Terabyte 2006. In this track, three different sets of relevance judgments were formed, with only two of them being used for evaluation purposes. Out of these two sets, the first set of judgments, constructed by the traditional depth-50 pooling strategy, was used to obtain a rough idea of the systems average precision. The second set of judgments was constructed using random sampling in such a way that there are more documents judged from topics that are more likely to have retrieved more relevant documents. Since, in Terabyte track, the size of the document collection is very large, the systems may continue retrieving relevant documents even at high ranks (deeper in the list). This set of judgments was created to obtain an estimate of average precision if complete judgments were present.

To estimate average precision, infAP was used as the evaluation measure. Since, by design, infAP assumes that the set of relevance judgments is a random subset of complete judgments, even though the entire depth-50 pool was judged, infAP was computed only using the random sample of judgments (second set) without utilizing judgments from the depth-50 pool. Therefore, many relevance judgments were not used even though they were available.

Note that xinfAP can easily handle this setup and it could be used to utilize all the judgments, obtaining better estimates of average precision.

To test how xinfAP compares with infAP we simulate the sampling strategy used in TREC Terabyte 06 on data from TREC 8. The TREC Terabyte data was not used due to the fact that in TREC Terabyte the actual value of average precision is not known since ”complete” judgments are not available.

To simulate the setup used in TREC Terabyte, we first form different depth-k pools where $k \in \{1, 2, 3, 4, 5, 10, 20, 30, 40, 50\}$ and obtain judgments for all documents in each one of these pools. Then, for each value of k, we compute the total number of documents that are in the depth-k pool and we randomly sample equal number of documents from the complete judgment set[2] excluding the depth-k pool. After forming these two sets of judgments (depth-k and random) we combine them and compute xinfAP on these combined judgments.

This setup exactly corresponds to a sampling strategy where complete judgments are divided into two strata and judgments are formed by uniformly and independently sampling within each stratum.

Note that in TREC, there are some systems that were submitted but that did not contribute to the pool. To further evaluate the quality of our estimators in terms of their robustness for evaluating the quality of unseen systems (systems that did not contribute to the pool), when we form the incomplete relevance judgments, we only consider the systems that contribute to the pool but we compute the xinfAP estimates for all submitted systems.

Figure 3.3 demonstrates how xinfAP computed using judgments generated by combining depth-10 (top row, left), depth-5 (top row, right) and depth-1 (bottom row) pools with equal number of randomly sampled judgments compares with the actual AP. Each of these depths correspond to judging 23.1%, 12.7% and 3.5% of the entire pool, respectively. The plots report the RMS error (how accurate are the estimated values?), the Kendall's τ value (how accurate are the estimated rankings of systems?) and the linear correlation coefficient, ρ, (how well do the estimated values fit in a straight line compared to the actual values?). [3] The dot signs in the figures refer to the systems that were used to create the original pools and the plus signs refer to the systems that did not contribute to the pool.

The results illustrated in these plots reinforce our claims that xinfAP is an unbiased estimator of average precision. Furthermore, it can be seen that the measure can reliably be used to evaluate the quality of systems that were not used to create the initial samples, hence the measure is robust to evaluating the quality of unseen systems.

Figure 3.4 illustrates how xinfAP computed on a non-random judgment set compares with infAP computed on a random judgment set for various levels of incompleteness. In a

[2]Throughout this section, we assume that the complete judgment set corresponds to the depth-100 pool as the judgments we have are formed using depth-100 pools and assuming the remaining documents are nonrelevant.

[3]A more detailed description of these statistics is given in Appendix C.

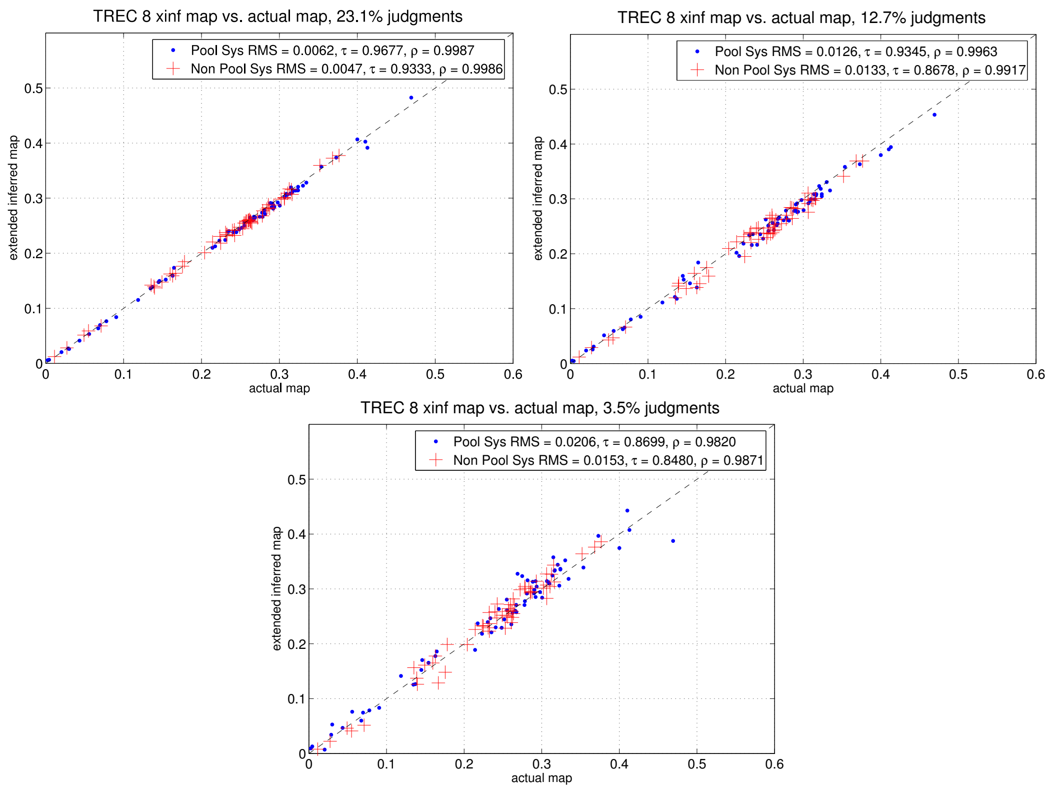
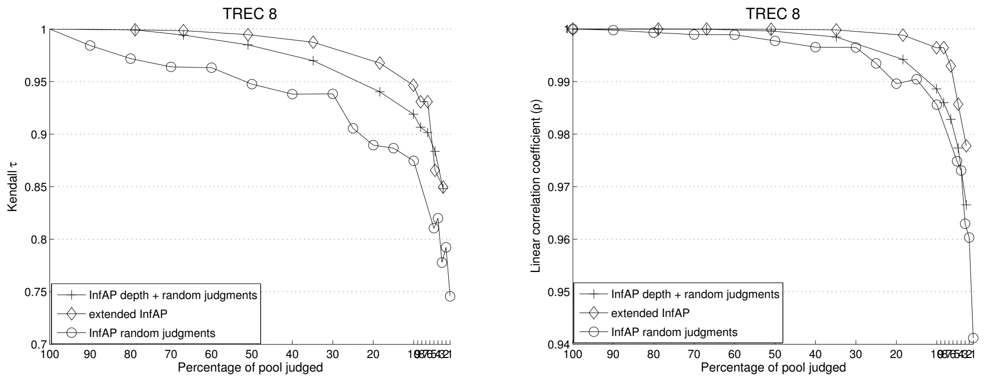


Figure 3.3: TREC 8 mean xinfAP when relevance judgments are generated according to depth-10, depth-5 and depth-1 pooling combined with equivalent number of randomly sampled judgments versus mean actual AP.

similar manner to the experimental setup of the original infAP work, for each value of k, we generated ten different sample trials according to the procedure described in the previous paragraph, and for each one of the ten trials we computed the xinfAP for all systems. Then, all three statistics were computed for each one of the trials and the averages of these statistics over all ten trials were reported for different levels of judgment incompleteness. Using the same procedure, we also created ten different sample trials where the samples were generated by merely randomly sampling the judgment set and the infAP values were computed on them. For comparison purposes, to show how the original version of infAP behaves when this randomness assumption is violated, we also include infAP run on the same judgment set as extended infAP (marked as infAP depth+random judgments in the Figure).

It can be seen that for all levels of incompleteness, in terms of all three statistics, xinfAP is much more accurate in estimating average precision than the other two measures.

We further compared xinfAP to the sampling method proposed by Aslam et al. [5]. The robustness of xinfAP to incomplete relevance judgments is comparable to (and in some cases even better than) this method.



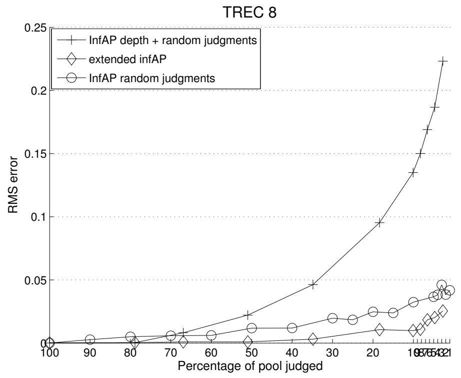


Figure 3.4: TREC 8 change in Kendall's τ, linear correlation coefficient (ρ), and RMS errors of xinfAP and infAP as the judgment sets are reduced when half of the judgments are generated according to depth pooling and the other half is a random subset of complete judgments and of inferred AP when the judgments are a random subset of complete judgments.

3.1.4 Estimation of nDCG with Incomplete Judgments

Before we continue to estimate nDCG with incomplete judgments, we revisit the definition of nDCG given in Chapter 2.

Let G denote a relevance grade and *gain*(G) the gain associated with G. Also, let $g_1, g_2, \dots g_N$ be the gain values associated with the N documents retrieved by a system in response to a query q, such as $g_i = \textit{gain}(G)$ if the relevance grade of the document in rank i is G-relevant.

Then, the nDCG value for this system can be computed as,

$$\textit{nDCG} = \frac{\textit{DCG}}{\textit{optDCG}} \text{ where } \textit{DCG} = \sum_{i=1}^{N} g_i / \log_b(i + b - 1)$$

and $optDCG$ denotes the DCG value for an ideal ranked list for query q.

The estimation of nDCG with incomplete judgments can be divided into two parts: (1) Estimating optDCG and (2) Estimating DCG. Then, the DCG and the optDCG values can be replaced by their estimates to obtain the estimated nDCG value.[4]

3.1.4.1 Estimating optDCG

The normalization factor, optDCG, for a query q can be defined as the maximum possible DCG value over that query. Hence, the estimation of optDCG can be derived in a two-step process: (1) For each relevance grade G such as $gain(G) > 0$, estimate the number of documents with that relevance grade; (2) Calculate the DCG value of an optimal list by assuming that in an optimal list the estimated number of documents would be sorted (in descending order) by their relevance grades.

Using the sampling strategy described in the previous section, suppose incomplete relevance judgments were created by dividing the complete pool into disjoint sets (strata) and randomly picking (sampling) documents from each stratum to be judged, possibly with different probability for each stratum.

For each stratum s, let $r_s(G)$ be the number of sampled documents with relevance grade G, let n_s be the total number of documents sampled from strata s and N_s be the total number of documents that fall in strata s. Since the n_s documents are sampled uniformly from strata s, the estimated number of documents with relevance grade G within this strata can be computed as

$$\hat{R}_s(G) = \frac{r_s(G)}{n_s} \cdot N$$

Then, the expected number of documents with relevance grade G within the complete pool can be computed as

$$\hat{R}(G) = \sum_{\forall s} \hat{R}_s(G)$$

Once these estimates are obtained, one can estimate optDCG.

3.1.4.2 Estimating DCG

Given N documents retrieved by a search engine with relevance gain g_i for the document at rank i, for each rank i, define a new variable x_i such as $x_i = N \cdot \frac{g_i}{log_b(i+b-1)}$. Then, DCG can be written as the output of the following random experiment:

1. Pick a document at random from the output of the search engine, let the rank of this document be i.
2. Output the value of x_i.

[4]Note that this assumes that $E[nDCG] = E[DCG]/E[optDCG]$, i.e., that optDCG and DCG are independent of each other, which is not necessarily the case. This assumption may result in a small bias and better estimates of nDCG can be obtained by considering this dependence. However, for the sake of simplicity, throughout this section, we will assume that these terms are independent.

It is easy to see that if we have the relevance judgments for all N documents, the expected value of this random experiment is exactly equal to DCG.

Now consider estimating the outcome of this random experiment when relevance judgments are incomplete. Consider the first step of the random experiment, i.e. picking a document at random. Let N_s be the number of documents in the output of a system that fall in stratum s. When picking a document at random, with probability N_s/N, we pick a document from stratum s.

Therefore, the expected value of the above random experiment can be written as:

$$E[DCG] = \sum_{\forall s} \frac{N_s}{N} \cdot E[x_i|\text{document at rank } i \in s]$$

Now consider the second step of the random experiment, computing the expected value of x_i given that the document at rank i falls in strata s. Let $sampled_s$ be the set of sampled documents from strata s and n_s be the number of documents sampled from this strata. Since documents within stratum s are uniformly sampled, the expected value of x_i can be computed as

$$E[x_i|\text{document at rank } i \in s] = \frac{1}{n_s} \sum_{\forall j \in sampled_s} x_j$$

Once E[optDCG] and E[DCG] are computed, infNDCG can then be computed as infNDCG $= E[DCG]/E[optDCG]$.

3.1.5 Overall Results

Until now, we have shown that using a similar sampling strategy as the one used in TREC Terabyte 06 (complete judgments divided into 2 different strata), xinfAP is highly accurate. In this section, we show that (1) this claim is consistent over different TRECs for both xinfAP and infNDCG and that (2) the two measures can be used with the complete judgments divided into more than two strata.

In order to check (2), we use a different sampling strategy than the one in Terabyte; we divide the complete judgment set (assuming depth-100 pool is the complete judgment set) into 4 different strata. The first stratum is the regular depth-k pool, fully judged. Instead of randomly sampling equal to the depth-k pool number of judgments from the remainder of the collection, we now divide the rest of the documents into three other strata and distribute the remaining judgments with a ratio of 3:1.5:1 (judge 55% of the documents in the top depth stratum, 27% of the documents in the middle depth stratum and 18% in the lowest depth stratum). This way, more weight is given to judging documents retrieved towards the top of the ranked lists of the search engines. Note, however, that as the number of strata

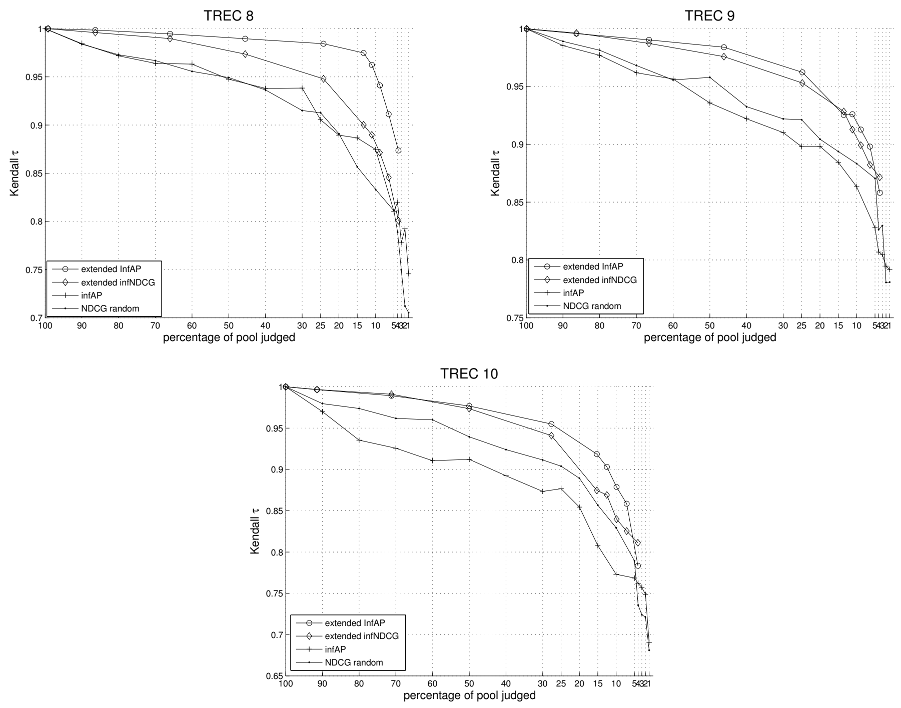


Figure 3.5: Comparison of extended inferred map, (extended) mean inferred nDCG, inferred map and mean nDCG on random judgments, using Kendall's τ for TREC 8, 9 and 10.

increase, there values of the estimates may slightly deviate from the actual values since the effect of smoothing also increase (smoothing is needed for each stratum).

Figures 3.5, 3.6 and 3.7 show the quality of xinfAP and infNDCG (referred as extended infNDCG to avoid confusion) computed on these samples according to Kendall's τ, linear correlation coefficient (ρ) and RMS Error statistics, respectively, for TRECs 8, 9 and 10. For comparison purposes, the plots also contain infAP and nDCG (the standard formula computed on random judgments, assuming unjudged documents are nonrelevant).

Looking at all plots, it can be seen that according to both statistics, using the same number of judgments, the extended infAP (xinfAP) and infNDCG consistently outperform infAP and nDCG on random judgments, respectively. The high RMS error of nDCG on random judgments is due to the fact that nDCG is computed on these judgments as it is, without aiming at estimating the value of the measure.

3.1.6 Conclusions

In this work, we extended inferred AP in two different ways. First, we derived confidence intervals for infAP to capture the variability in infAP values. Employing confidence intervals enables comparisons and eventually ranking of systems according to their quality measured

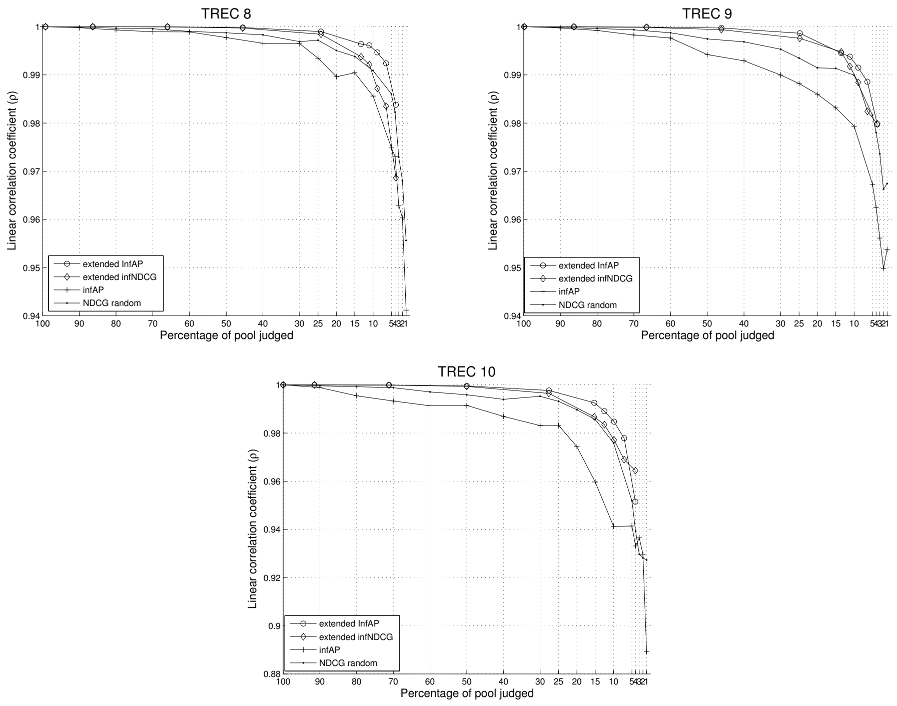


Figure 3.6: Comparison of extended inferred map, (extended) mean inferred nDCG, inferred map and mean nDCG on random judgments, using linear correlation for TREC 8, 9 and 10.

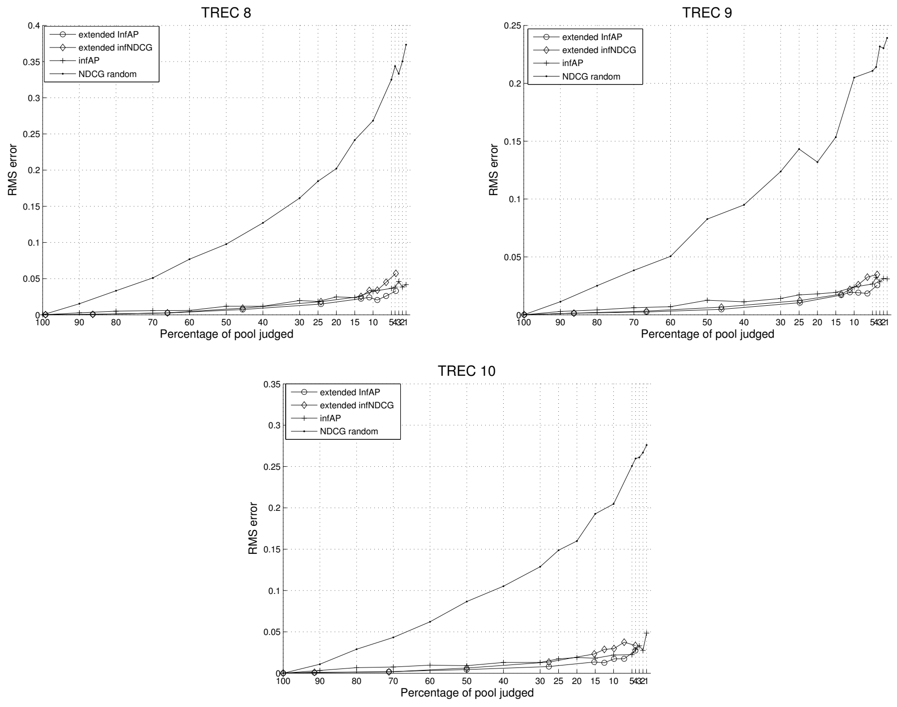


Figure 3.7: Comparison of extended inferred map, (extended) mean inferred nDCG, inferred map and mean nDCG on random judgments, using RMS error for TREC 8, 9 and 10.

by AP with high confidence. Second, we utilized a stratified random sampling strategy to select documents to be judged and extended infAP to handle the non-random samples of judgments. We applied the same methodology for estimating nDCG in the presence of incomplete non-random judgments. Stratified random sampling combines the effectiveness of stratification and thus better utilization of the relevance judgments with the simplicity of random sampling. We showed that xinfAP and infNDCG are more accurate than infAP and nDCG on equal number of random samples.

Note that the sampling strategy (i.e. the number of strata, the size of each stratum and the sampling percentage from each stratum) used here is rather arbitrary. The confidence intervals as described in the first part of this section could be used as an objective function to determine an optimal sampling strategy. The sampling strategy is highly important for the quality of the estimates and identifying an optimal strategy is a point of future research.

Furthermore, confidence intervals as a function of the sample size could be used to determine the appropriate number of documents to be judged for an accurate MAP estimation which is a point we plan to investigate.

3.2 Evaluation over Thousands of Queries

With fewer judgments available, estimates of evaluation measures as the ones described in Section 3.1 will have higher variance. One way to cope with this is to evaluate over more queries.

In this section we describe an evaluation over a corpus of 25 million documents and thousands of queries, the Million Query Track that ran at the Text REtrieval Conference (TREC) in 2007 [3] and 2008 [4]. Using two recent method for selecting documents, (a) statAP [94] (which is very similar to the method described in Section 3.1), and (b) MTC [32], we evaluated 24 systems from 10 sites in 2007 and 25 systems from 8 sites in 2008. The goal in 2007 track was to test whether low-cost methods produce reliable evaluations when used to select documents, and how many queries are needed to draw robust conclusions.

In the 2008 track, queries were categorized by length and a proxy measure of their appropriateness to the corpus [4]. This allowed a more detailed analysis of what the query sample of a test collection should look like, both in terms of query size but also in terms of query characteristics. In addition, queries were assigned different target numbers of judgments, which allows more detailed analysis of the proper level of budgeting for relevance judgments within that query sample.

3.2.1 Methods

We selected two recently proposed low-cost evaluation methodologies. The two methods we used differ by the aspect of evaluation that they attack. The Minimal Test Collection (MTC) algorithm, proposed by Carterette et al. [32], is designed to induce rankings of systems by identifying differences between them, without regard to the values of measures. StatAP, proposed by Pavlu and Aslam [94], similar to xinfAP described in Section 3.1, is a sampling method designed to produce unbiased, minimum-variance estimates of average precision. Both methods are designed to evaluate systems by average precision.

3.2.1.1 Minimal Test Collections

Minimal Test Collections (MTC) is an adaptive on-line greedy algorithm for selecting documents to judge. It assigns a weight to each document indicating its importance in determining whether there is a difference in performance of systems by some evaluation measure; the highest-weighted document in judged and that judgment used to update all other weights. For most widely-used evaluation measures, MTC is optimal: no other algorithm would be able to reach the same conclusion about whether a performance difference exists with fewer judgments [32].

MTC's formal framework can be extended by the use of probabilities of relevance of unjudged documents. Using these probabilities and computing expectations over all possible assignments of relevance to unjudged documents, we compute quantities such as the probability that two systems are different given an incomplete set of judgments, the probability that the magnitude of the difference is greater than some value, the expectation of an evaluation measure, and the probability of a complete ordering of a set of systems. The probabilities can be estimated using classic information retrieval approaches to determining relevance. In this way we can understand the relative ranking of systems with high confidence while also obtaining estimates of their performance. We refer to these estimates as expected average precision ($\mathbf{E}AP$), expected precision (**E***prec@k*), and so on, to differentiate them from the true measures.

3.2.1.2 Statistical Average Precision (statAP)

In statistical terms, average precision can be thought of as the *mean* of a *population*: The elements of the population are the relevant documents in the collection, and the population *value* of each element is the precision at this document's rank within the ranked list being evaluated. Given an appropriately chosen sample of documents, one can statistically *esti-*

mate the number of relevant documents, the precisions at sampled relevant documents, and the average of the precisions at relevant documents, i.e., *average precision*. This principle is the basis for several recently proposed evaluation techniques [147, 148, 5]. The infAP technique [147, 146], for example, uses a simple uniform random sample to estimate the values in question; however, uniform sampling techniques, while simple, are far less than optimally efficient. StatAP [94, 33, 3], on the other hand, similar to xinfAP, employs *stratified sampling* [21, 127], and adapts the *generalized ratio estimator for unequal probability designs* [131] in order to estimate the values in question. These techniques are widely used in polling, surveys, market research, and the like [131], and while slightly more complex than simple uniform sampling techniques, they are far more powerful and efficient. Employing these techniques, statAP, by design, produces unbiased estimates of average precision and other evaluation measures of interest (precision-at-cutoff, R-precision, nDCG), as well as statistical *confidence intervals* for these estimates. As such, statAP can be used to both *estimate* the performance of retrieval systems and *rank* these systems by their performance.

3.2.2 Million Query 2007 : Experiment and Results

The participants in the Million Query Track were provided a set of queries to run through their retrieval engines, producing ranked lists of up to 1,000 documents from a given corpus for each query. The submitted runs were used as input to the MTC and statAP algorithms for selection of documents to be judged.

Corpus: The corpus was the GOV2 collection, a crawl of the .gov domain in early 2004 [39]. It includes 25 million documents in 426 gigabytes. The documents are a mix of plain text, HTML, and other formats converted to text.

Queries: A total of 10,000 queries were sampled from the logs of a large Internet search engine. They were sampled from a set of queries that had at least one click within the .gov domain, so they are believed to contain at least one relevant document in the corpus. Queries were generally 1-5 words long and were not accompanied by any hints about the intent of the user that originally entered them.

Retrieval runs: Ten sites submitted a total of 24 retrieval runs. The runs used a variety of retrieval models and techniques: BM25, language modeling, dependence modeling, model combination; some used query expansion.

Assessors: The main set of judgments were made by NIST assessors (sites that submitted runs and undergraduate work-study students also made few judgments). They were presented with a list of 10 randomly-chosen queries from the sample. They selected one query from that list. They were asked to develop the query into a full topic by entering an information need and a narrative describing what types of information a document would have to present in order to be considered relevant and what information would not be considered relevant.

Each query was served by one of three methods (unknown to the assessors): MTC, statMAP, or an alternation of MTC and statMAP. For MTC, documents weights were updated after each judgment; this resulted in no noticeable delay to the assessor. StatMAP samples were selected in advance of any judging. The alternations proceeded as though MTC and statMAP were running in parallel; neither was allowed knowledge of the judgments to documents served by the other. If one served a document that had already been judged from the other, it was given the same judgment so that the assessor would not see the document again.

Documents were displayed with query terms highlighted and images included to the extent possible. Assessors could update their topic definitions as they viewed documents, a concession to the fact that the meaning of a query could be difficult to establish without looking at documents. Judgments were made on a tertiary scale: nonrelevant, relevant, or highly relevant. Assessors were not given instructions about the difference between relevant and highly relevance.

Assessors were required to judge at least 40 documents for each topic. After 40 judgments they were given the option of closing the topic and choosing a new query.

Judgments: The judging process resulted in 69,730 judged documents for 1,692 queries, with 10.62 relevant per topic on average and 25.7% relevant overall. This set comprises three subsets: 429 queries that were served by MTC, 443 served by statAP, and 801 that alternated between methods. Details of these three are shown in Table 3.1 as “1MQ-MTC”, “1MQ-statAP”, and “1MQ-alt”.

Because the 10,000 queries in this experiment had previously been used in the TREC Terabyte 2005 track, The TREC queries in this set had already been judged with some depth. A total of 135,352 judgments had been made, with 180.65 relevant documents per topic (29 of which were highly relevant) and 19.89% relevant overall. These queries and judgments, details of which are shown in Table 3.1 as “TB”, were used as a “gold standard” to compare the results of evaluations by MTC and statAP. It should be noted that these queries are not sampled from the same source as the other 10,000 and may not be representative of that space. They are, however, nearer to “truth” than any other set of queries we have.

Table 3.1 shows details of the judgment sets. The full 1MQ set is shown along with its

set	topics	judgments	rel/topic	% rel
TB	149	135,352	180.65	19.89%
1MQ	1,692	69,730	10.62	25.78%
1MQ-MTC	429	17,523	11.08	27.12%
1MQ-statAP	536	21,887	10.42	25.47%
1MQ-alt	801	33,077	10.32	24.99%

Table 3.1: Judgment sets.

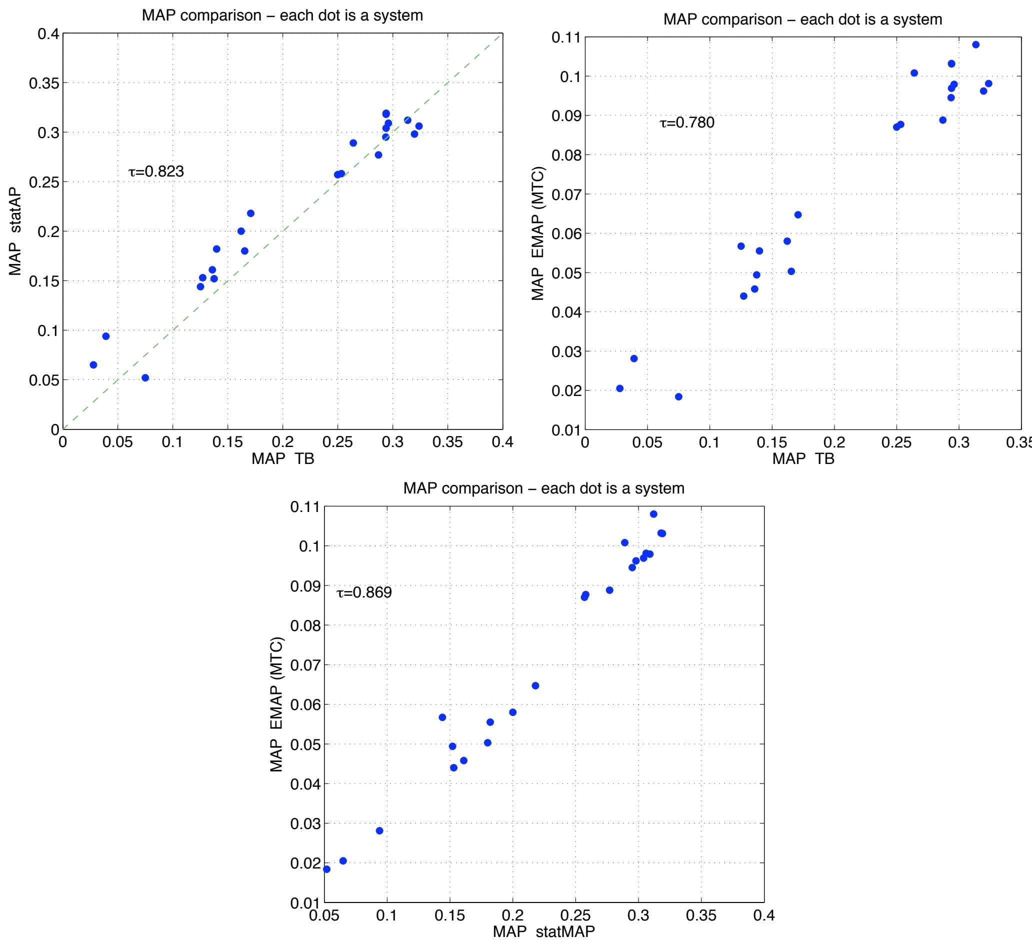


Figure 3.8: From left, evaluation over Terabyte queries versus statMAP evaluation, evaluation over Terabyte queries versus **E**MAP evaluation, and statMAP evaluation versus **E**MAP evaluation.

three subsets 1MQ-MTC, 1MQ-statMAP, and 1MQ-alt.

Results: The 24 runs were evaluated over the TB set using `trec_eval` and over the 1MQ set using **E**MAP and statMAP. If TB is representative, we should see that **E**MAP and statMAP agree with each other as well as TB about the relative ordering of systems. Our expectation is that statMAP will present better estimates of MAP while **E**MAP is more likely to present a correct ranking of systems.

Overall, the rankings by $\mathbf{E}MAP$ and statMAP are fairly similar, and both are similar to the "gold standard". Figure 3.8 shows how statMAP, $\mathbf{E}MAP$, and MAP over TB queries correlate. All three methods have identified the same three clusters of systems; within those clusters there is some variation in the rankings between methods. For statMAP estimates (Figure 3.8, top row, left plot), besides the ranking correlation, we note the accuracy in terms of absolute difference with the TB MAP values by the line corresponding to the main diagonal.

The agreement between the two low-cost methods, which are radically different in terms of the documents they select to be judged, as well as the high correlation between these methods and the TB evaluation indicates that sampling a small set of documents per query can still lead to reliable evaluation, as it was suggested in Section 3.1.

3.2.3 Million Query 2007 : Analysis

As described earlier, the performance of each system per topic is expressed in terms of average precision of the output list of documents while the overall quality of a system is captured by averaging its AP values over all topics into its mean average precision. Systems are then ranked by their MAP scores.

Hypothetically, if a second set of topics was available, the systems could be run over this new set of topics and new MAP scores (and consequently a new ranking of the systems) would be produced. Naturally, two questions arise: (1) how do MAP scores or a ranking of systems over different set of topics compare to each other, and (2) how many topics are needed to guarantee that the MAP scores or a ranking of systems reflect their actual performance?

We describe an efficiency study, based on analysis of variance (ANOVA), of the stability of rankings induced by subsets of queries. Appendix B gives a detailed description on analysis of variance and variance decomposition.

3.2.3.1 Analysis of Variance Studies

We ran two separate analysis of variance studies; one over the MAP scores estimated by the MTC method given the set of 429 topics exclusively selected by MTC and one over the MAP scores estimated by the statAP method over the set of 459 topics exclusively selected by statAP (both methods utilized 40 relevance judgments per topic). For both studies we reported (a) the ratio of the variance due to system and the total variance and (b) the ratio of the variance due to system and the variance components that affect the relative MAP scores (i.e. the ranking of systems), both as a function of the number of topics in the topics set. The results of the two studies are illustrated in Figure 3.9. The solid lines correspond to the ratio of the variance due to system and the total variance and expresses how fast (in terms of number of topics) we reach stable MAP values over different sets of topics of the same size. As the figure shows, the statAP method eliminates all variance components (other than the system) faster than the MTC method, reaching a ratio of 0.95 with a set of 152 topics, while MTC reaches the same ratio with 170 topics. The dashed lines correspond to the ratio of the variance due to system and the variance due to effects that can alter the relative MAP scores (rankings) of the systems. The figure shows that the MTC method produces a stable ranking of systems over different sets of topics faster (in terms of number of topics) than the statAP method reaching a ratio of variance 0.95 with a set of 40 topics, while statAP reaches the same ratio with 85 topics.

These results support the claims that the statAP method, by design, aims to estimate the actual MAP scores of the systems, while the MTC method, by design, aims to infer the proper ranking of systems.

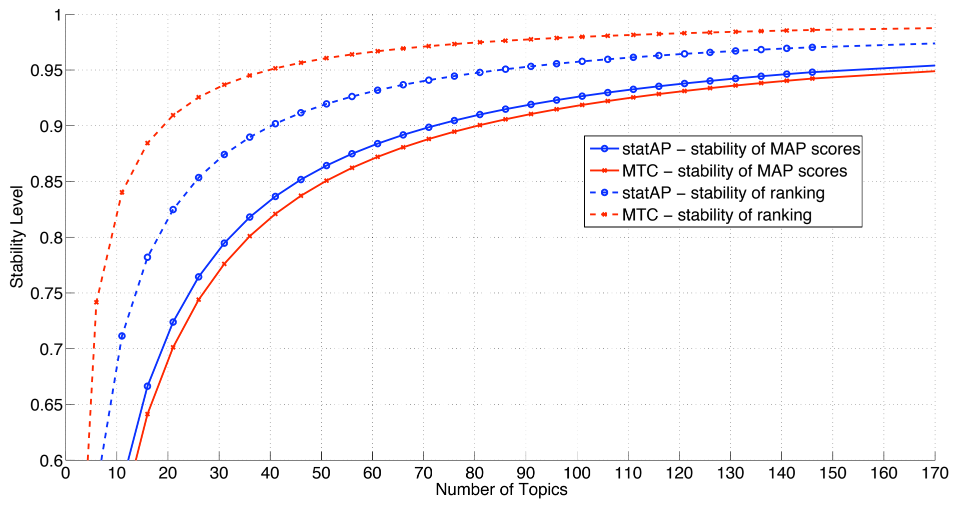


Figure 3.9: Stability levels of the MAP scores and the ranking of systems for statAP and MTC as a function of the number of topics.

3.2.4 Million Query 2008 : Experiment and Results

As with the 2007 MQ track, the corpus is GOV2 and we started with a sample of 10,000 queries from the log of a commercial search engine. Assessors were allowed to select a query from a list of 10 to "backfit" into a topic definition and then judge. Eight participating sites submitted a total of 25 runs based on various retrieval strategies (BM25, metasearch, inference networks, etc).

	judgment target					
category	8	16	32	64	128	total
short-govslant	95 (7.87)	55 (15.58)	29 (29.93)	13 (58.85)	4 (117.50)	196 (18.92)
short-govheavy	118 (7.85)	40 (15.18)	26 (30.27)	10 (58.60)	3 (117.67)	197 (16.54)
long-govslant	98 (7.72)	52 (15.60)	26 (30.38)	13 (58.31)	8 (116.88)	197 (20.56)
long-govheavy	92 (7.79)	57 (15.32)	21 (29.95)	14 (59.29)	10 (114.40)	194 (21.61)
total	403 (7.81)	204 (15.43)	102 (30.14)	50 (58.78)	25 (116.08)	784 (19.40)

Table 3.2: Number of queries and number of judgments per query in parentheses.

Queries and Judgments: Queries are categorized by two features:

- **long/short:** queries with more than 5 words are considered "long".
- **govheavy/govslant:** all queries used have at least one known user click in the .gov domain; the ones with more than 3 clicks are considered "heavy".

To ensure a uniform distribution over categories, assessors picked from 10 queries with the same category. The category rotated round-robin. To ensure that there were queries with varying numbers of judgments, a target number was assigned after the assessor selected a query and completed topic definition. The targets increased by powers of 2 (8, 16, 32, 64, or 128), and were assigned to ensure a roughly equal amount of total judging effort for each target: the number of queries with 8 judgments would be twice the number with 16, which in turn would be twice the number with 32, and so on.

Selection methods alternated to pick documents to judge. Because of "collisions" (both methods selecting the same document) the total number of judgments could have been less than the target. In the end we obtained 15,211 judgments for 784 queries; Table 3.2 shows the distribution of queries and judgments by category and target. Note that the frequency of collisions is quite low.

Of the 15,211 judgments, 2,932 (19%) were relevant. "Govheavy" queries had substantially more relevant documents than "govslant" queries (24% to 15%), indicating that it is a good proxy for appropriateness to corpus. "Short" queries had more relevant documents than "long" queries (21% to 18%), perhaps indicating that a topic definition based on a short query is more fluid than one based on a long query. There were 220 queries for which no relevant documents were found; 198 of these had a target of 8 or 16 judgments.

Table 3.3 shows the proportion marked relevant broken out by query category and maximum number of judgments. Note that the "govheavy" categories had a significantly greater proportion of documents judged relevant than the "govslant" categories. The difference between "short" and "long" is not as clear, but we can see from the table that the increase from "short-govslant" to "short-govheavy" is quite a bit larger than the increase from "long-govslant" to "long-govheavy".

category	8	16	32	64	128	total
short-govslant	0.1872	0.1214	0.2028	0.1399	0.0298	0.1459
long-govslant	0.2021	0.1702	0.1734	0.1201	0.1369	0.1597
short-govheavy	0.2462	0.3081	0.3037	0.2338	0.3739	0.2832
long-govheavy	0.2887	0.2039	0.2226	0.1361	0.1600	0.1958
total	0.2313	0.1928	0.2251	0.1524	0.1575	0.1928

Table 3.3: Percent of documents judged relevant.

The length of judged documents varied by category. Measuring length by number of characters (a loose measure that also includes HTML tags, Javascript, metadata, and more that would not be visible to the user), documents judged for short queries had an average length of 38,730 characters, while those judged for long queries had an average length of 43,900 characters. There is a smaller difference for govslant and govheavy: an average length of 40,456 characters for the former, and 42,175 for the latter.

Evaluation Results: Both evaluation methods estimate average precision for each run and each query. We calculated a weighted mean of APs to account for the fact that we have 16 times as many queries with 8 judgments as with 128; we did not want queries with 8 judgments to dominate the evaluation as they would with a straight average. Weighted MAP is calculated as $\mathit{wMAP} = \frac{1}{5}\sum_{j=1}^{5} MAP_j = \frac{1}{5}\sum_{j=1}^{5}\frac{1}{Q_j}\sum_{q\in j} AP_q$, where MAP_j is averaged over all queries at level j ($= 2^{j+2}$ target judgments) and Q_j is the number of queries at level j.

In the absence of any traditional evaluation (in which many more documents are judged for each query), the best indication of being close to the "true" ranking is the correlation of the two evaluation methodologies. Their mechanisms for estimation are fundamentally different, so any correlation is much more likely due to correct estimation rather than other reasons. Figure 3.10 illustrates the weighted MAP scatterplot, with a Kendall's τ=.93. This tracks the results observed in the previous year's track [33].

Furthermore, the two methods continue to correlate well even when breaking queries and judgments into subsets by category, target, or method. Perhaps the best indicator that we have found the "true" ranking is that the two methods correlate well even when evaluated over only the documents they selected ($\tau = 0.87$), and even when evaluated over only the documents selected by *the other method* ($\tau = 0.91$)—despite very little overlap between the two methods.

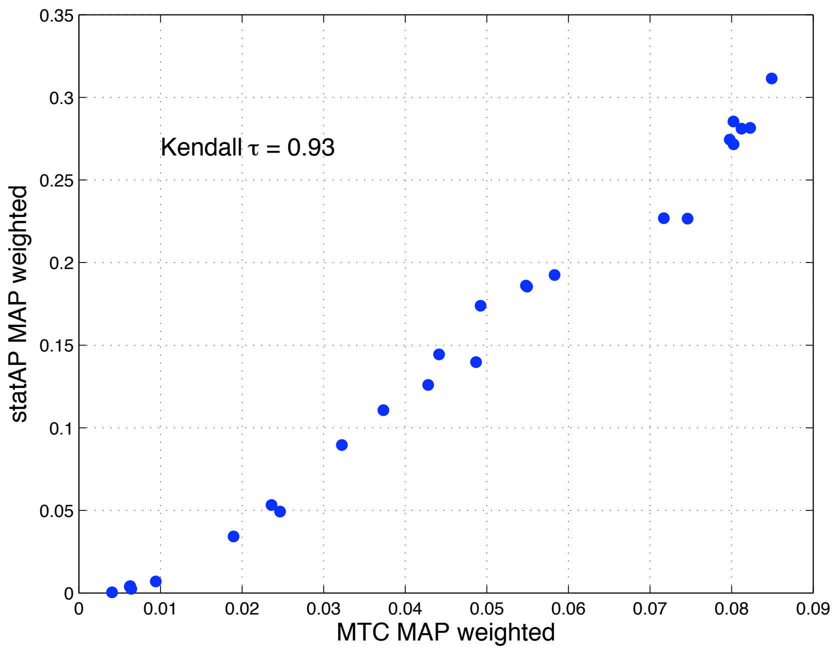


Figure 3.10: MTC and statAP weighted-MAP correlation

3.2.5 Million Query 2008 : Analysis

The above results are based on a large sample of 784 sparsely-judged queries distributed uniformly over four categories. The next step is to determine the extent to which the number of queries and judgments can be reduced, and how to sample the categories, to achieve similar results with less overall effort.

Our aim is to answer two questions: (1) what is the number of queries needed for different levels of relevance incompleteness to guarantee that, when systems are run over this many queries, their MAP scores reflect their actual performance, and (2) given a fixed budget of total number of judgments (or total hours of labor), what is the ratio between number of queries and number of judgments per query that maximizes stability?

3.2.5.1 Analysis of Variance Studies

Stability of MAP and induced rankings: We ran two separate variance decomposition studies for the MAP estimates produced by each method. In both cases systems were run over the same set of 784 queries.[5] and each one of the methods utilized all available judg-

[5]Note that statAP does not report scores for queries with no relevant document found; studies for statAP are on the 564 queries for which statAP returned scores.

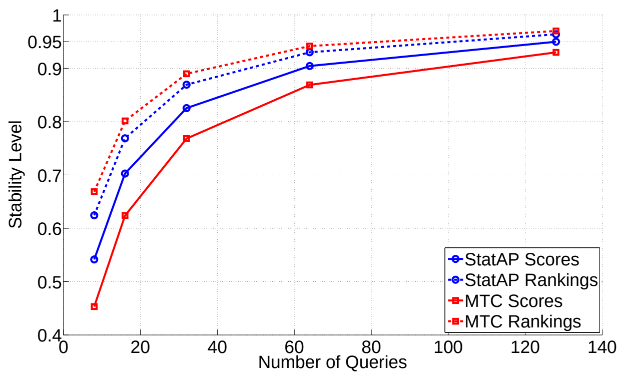


Figure 3.11: Stability level of MAPs and induced ranking for statAP and MTC as a function of the number of queries.

ments per query in the estimation of MAP scores.

As mentioned before, first MAP scores were computed over each one of the judgment-levels classes of queries separately (MAP_c), and then averaged to produce the final MAP scores (wMAP). The variance in wMAP is a function of the variance of the MAP within each query class and the covariance of the MAPs among query classes. Thus, instead of fitting a single ANOVA model in APs over all queries [14, 17], we used a Multivariate Analysis of Variance (MANOVA) [20]. The variance of MAP within each query class was decomposed into the aforementioned variance component, while the covariance of the MAPs among the query classes was solely attributed to *system* effects, since the query classes are disjoint.

For both studies, we report (a) the stability levels of the MAPs (system variance over total variance) and (b) the stability levels of the systems rankings (system variance over system and system/query interaction), both as a function of the total number of queries in the query set.

Figure 3.11 shows the results: the solid lines correspond to stability levels of MAPs while the dashed lines correspond to stability levels of system rankings. As the figure indicates, statAP reaches a MAP stability level of 0.95 with a set of 129 queries, while MTC reaches the same level with 204 queries (not observed in the figure).[6] MTC reaches a ranking stability level of 0.95 with a set of 83 queries, while statAP reaches the same level with 102 queries.

The solid lines correspond to the ratio of the variance due to system and the total variance and expresses how fast (in terms of number of queries) we reach stable MAP values over different sets of queries of the same size.

The dashed lines correspond to the ratio of the variance due to system and the variance due to effects that can alter the relative MAP scores (rankings) of the systems and expresses

[6]We have observed in our experiments that a stability of 0.95 leads to a Kendall's tau of approximately 0.9.

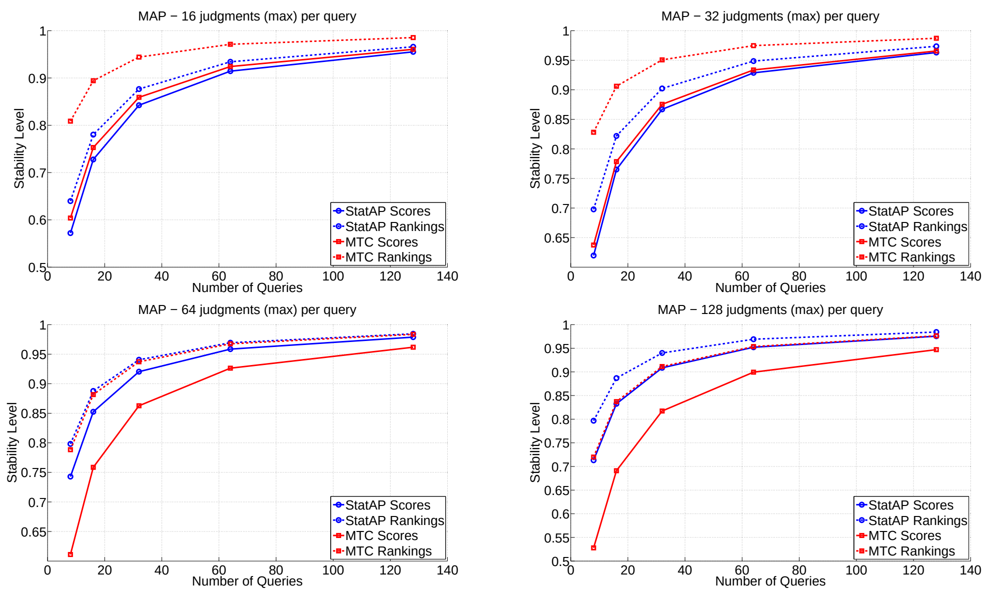


Figure 3.12: Stability levels of MAP scores and induced ranking for statAP and MTC as a function of the number of queries for different levels of relevance incompleteness.

how fast (in terms of number of queries) we reach stable system rankings.

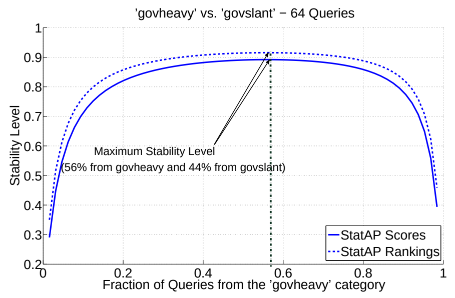
Note that the queries in the query set are distributed in the same manner as in the original data, i.e. the number of queries in each class is inversely proportional to the level of relevance incompleteness.

These results again support the claims that the statAP method, by design, aims to estimate the actual MAP scores of the systems, while the MTC method, by design, aims to infer the proper ranking of systems.

Stability with incomplete judgments: To illustrate how stable the MAPs returned by the two methods are with respect to different levels of relevance incompleteness, we ran ANOVA studies for the each one of the query classes separately. Figure 3.12 demonstrate the stability levels for both methods when 16, 32, 64, and 128 judgments are available, respectively.

According to stability levels illustrated in these figures, MTC leads to both more stable MAPs and induced rankings than statAP when 16 or 32 relevance judgments are available per query, while the opposite is true when 64 or 128 relevance judgments are available.

Note that the stability of the MAP scores returned by statAP degrades with the relevance incompleteness, as expected. On the other hand, the opposite is true for MTC. For the estimation of MAP scores, MTC is employing a prior distribution of relevance which is calculated by combining information from all queries, which violates the query independence



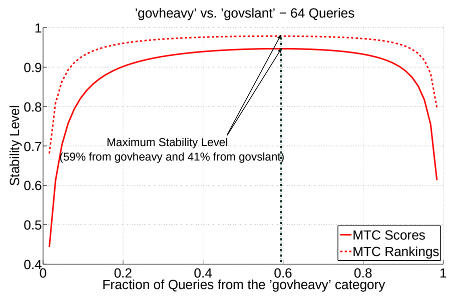


Figure 3.13: Stability of MAPs and induced rankings returned by statAP and MTC as a function of different subsampling ratios for "govheavy" and "govslant", given a fixed sample of 64 queries.

assumption ANOVA makes. The fewer the relevance judgments, the larger the weight on the prior distribution, and thus the more the assumption is violated. Consequently, MAP scores seem to be more stable than they should.

Stability for query categories: Here we consider how sampling queries in different proportions can affect the stability of measures and induced rankings. Unlike the above studies that consider stability merely as a function of the query set size, the following studies consider how different characteristics of queries can affect the stability of measures and induced rankings. For each pair of categories (short/long and govheavy/govslant), we fit a MANOVA model to the MAPs and calculated the optimal ratio of queries to be subsampled from each category.

Results from these studies are illustrated in Figure 3.13. In both plots, we gradually change the ratio of "govheavy" to "govslant" queries in a sample of 64 and plotted the stability level achieved for each ratio. Both statAP and MTC demonstrate a slight preference towards "govheavy" queries. In particular, selecting $55 - 60\%$ "govheavy" results in the optimal stability level for both scores and rankings. The results for "short" and "long" queries indicated no particular preference for one or the other.

3.2.5.2 Cost-Benefit Analysis

We measured the elapsed time assessors spent performing various interactions with the judging system. From these we can construct a cost function which we can then use to identify an optimal operating point: how many and what type of queries and judgments are needed to reach a point where the ranking would not be expected to change much with additional judgments or queries.

					judgment times					
category	refresh	view	lastview	topic	8	16	32	64	128	average
short	2.34	18.0	25.5	67.6	15.0	11.5	13.5	12.0	8.5	12.5
long	2.54	24.5	31.0	86.5	17.0	14.0	16.5	10.0	10.5	13.0
slant	2.22	22.5	29.0	76.0	13.0	12.5	13.0	9.5	10.5	12.0
heavy	2.65	20.0	27.5	78.0	19.0	13.0	17.0	12.5	8.5	13.5
average	2.41	22.0	29.0	76.0	15.0	13.0	15.0	11.0	9.0	13.0

Table 3.4: Average number of query lists viewed and median seconds spent viewing each list, viewing the final list, and defining the topic for the selected query.

Assessor Time: Assessor spent the majority of their time on three activities: (1) selecting a query; (2) backfitting the query to a topic definition; and (3) judging documents. Selecting a query can further be broken down into the following: the number of times the assessor refreshed the display (to see a new set of 10 queries), the time the assessor spent looking at each list of 10, and the time the assessor spent looking at the last list of 10 just before selecting one for topic definition. These numbers are shown in Table 3.4 along with the time spent on topic definition. Note that all numbers are median seconds—the mean is inappropriate because the distribution is heavily skewed by long breaks. Note also that time varied with query type: in particular, choosing queries and defining topics took quite a bit longer with long queries than with short.

Time to make judgments is also shown in Table 3.4. Here too time varied by query type, and by the target number of judgments. The fact that each judgment was made faster when more were requested suggests that assessors have some "ramp-up" time on a topic, after which they can make judgments faster.

Cost Given a query q of category c with target number of judgments j, the total time spent on that query is $((nr_c - 1)tl_c + tf_c + tt_c + tj_{cj}j$ where nr_c is the number of refreshes for query type c, tl_c is the time spent looking at a list of 10 queries for query type c, tf_c is the time spent looking at the final list of 10 for query type c, tt_c is the time spent on topic definition for type c, and tj_{cj} is the time spent on a judgment for query type c with target j.

Then the total cost of Q queries of which q_{c_i} are from category i ($1 \leq i \leq k$) is *cost* $= \sum_{i=1}^{k} q_{c_i} \left((nr_{c_i} - 1)\, tl_{c_i} + tf_{c_i} + tt_{c_i} + t_{c_i j} j \right)$, assuming every query has the same target j. This cost function can accommodate arbitrary splits between query categories, and even take into account differing target numbers of judgments. When doing so one should take into account the variance in the time estimates. If that variance is high, it may be prudent to average categories together and not distinguish between them in the cost analysis.

Figure 3.14 shows the Kendall's τ correlation between the baseline ranking by weighted MAP and the ranking over a subset of queries (all with the same target number of judgments) versus the total cost of judging that set of queries. In this plot we used average

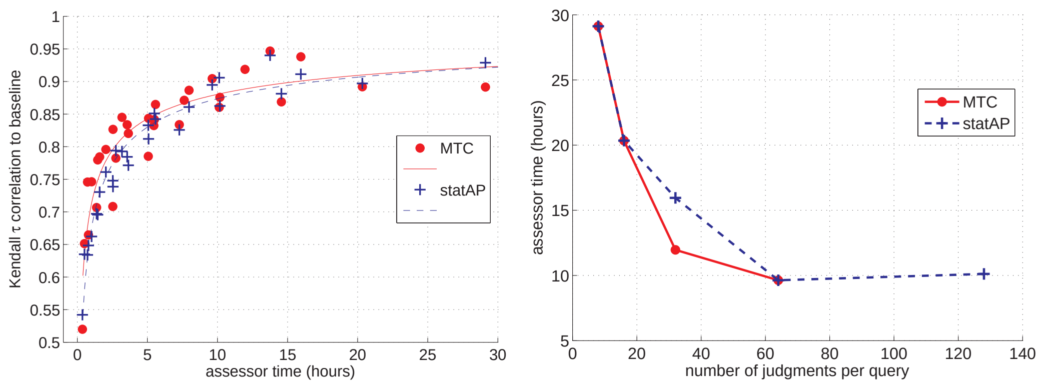


Figure 3.14: Cost plots: on the left, total assessor time to reach a Kendall's τ rank correlation of 0.9 with the baseline; on the right, minimum time needed to reach a τ of 0.9 with increasing numbers of judgments per query.

times only; the cost is not based on query category. Note that τ increases logarithmically with total cost (as the fitted lines show clearly). The fit lines suggest that a little over 15 hours of assessor time are needed in order to reach an expected τ of 0.9. This corresponds to 100 queries with 32 judgments each. A mean τ of 0.9 is first reached after only 9 hours of assessor effort. However, the standard deviation of τs is fairly high; any given sample of topics could produce a τ anywhere between 0.84 and 0.96. To ensure a τ of 0.9 with 95% probability requires around 15 hours of assessor time.

The right plot in Figure 3.14 shows the minimum time required by each method to reach an expected Kendall's τ of 0.9 as the number of judgments per query increases. When there are few judgments per query, many queries are needed; a long time is required. As the number of judgments increases, the number of queries needed decreases, and less time is required. There is a tradeoff, though, as the time to make judgments begins to exceed the time spent on query selection, and the total time begins to rise again. The minimum time for both methods is achieved at 64 judgments.

Using the times in Table 3.4 and the full cost function, we may consider whether a τ of 0.9 could be reached with less effort when query types are sampled in different proportions. Figure 3.15 shows the tradeoff between cost and Kendall's τ when short queries are sampled 25%, 50%, and 75%, and the tradeoff when "govheavy" is sampled 25%, 50%, and 75%. Note that the empirical data to generate different splits is limited; since for example there are only 50 queries with 64 judgments each, and they are roughly evenly split between categories, we cannot measure a τ with 75% of one category with 64 judgments.

The left plot suggests a slight (but not significant) preference for sampling long queries, while the right suggests a preference for heavy queries. Combining the two in such a way as to guarantee 75% long queries and 75% heavy queries (i.e. sampling long-heavy

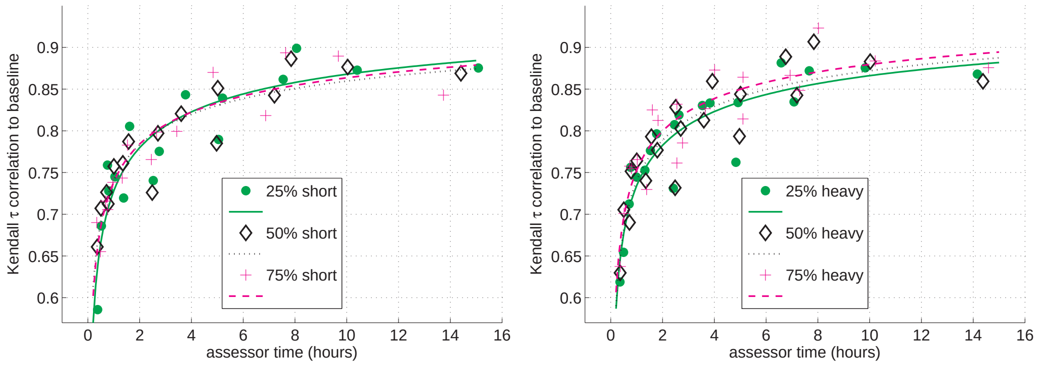


Figure 3.15: Cost plots. On the left, sampling short versus long in different proportions. On the right, sampling heavy versus slant in different proportions.

$0.75 \times 0.75 = 0.56$, long-slant 0.19, short-heavy 0.19, and short-slant 0.06), an expected Kendall's τ of 0.9 is reached after a little over 4 hours of assessor effort.

3.2.6 Conclusion

We put in practice two recently developed evaluation techniques that, unlike standard evaluation, scale easily and allow many more experiments and analyses. We experimented with 24 submitted systems and 10,000 queries evaluating 1,692 of them with about 70,000 judgments in 2007 and with 25 submitted systems over 10000 queries, evaluating 784 of them with only about 15000 judgments in 2008.

Evaluation stability. The setup allowed an analysis of evaluation stability with fewer judgments or queries. Using ANOVA, we concluded that MTC needs about 50-100 queries with approximately 2000 total judgments for a reliable ranking, while statAP needs about 130-150 queries with approximately 2800 total judgments for a reliable estimate of MAP.

Many queries; categories. In 2008, we investigated system performance over pre-assigned query categories. There is some evidence that over-sampling some types of queries may result in cheaper (if not substantially more efficient) evaluation: over-sampling long and govheavy queries resulted in a good ranking with just a little over four hours of simulated assessor time. More investigation to find the right tradeoffs is a clear direction for future work.

Cost analysis; optimal budget. Finally, in 2008, queries were randomly assigned 5 different target numbers of judgments such that the total number of judgments for each class is roughly constant. This split facilitated a derivation of optimal budgeting for IR evaluation via cost (in assessor hours) analysis. We concluded that 30-60 judgments per query with around 100 queries is optimal for assessing systems' performance ranking.

3.3 Overall Conclusions

The size of test collections used in IR evaluation has been growing at a tremendous pace. While collection sizes keep increases, the budget for relevance judgments does not. Reliable evaluations will rely more and more on the relevance judgments being selected intelligently and the inferences about systems made robustly despite many missing judgments. In this chapter we first proposed a low-cost evaluation methodology based on stratified sampling to intelligently select only a subset of documents per query to be judged. Our experiments demonstrated that one can reliably evaluate retrieval systems with about 5% of the total judgments that would be required if the traditional depth-pooling method was employed. Then, by utilizing document selection methodologies similar to our proposed one we concluded that given a fixed budget evaluating systems over many queries with few judgments per query leads to more reliable and thus more efficient evaluation than evaluating systems over few queries with larger number of judgments per query. Finally, we demonstrated that some query categories can indeed be more useful in evaluation than others. For instance oversampling long queries with many user clicks (which, to some extent, designate that the query is appropriate for the subset of searchable material that constitute the collection) can lead to more reliable and thus more efficient evaluation.

CHAPTER 4

Training Collections

Ranking is a central problem in information retrieval. Modern search engines, especially those designed for the World Wide Web, commonly analyze and combine hundreds of features extracted from the submitted query and underlying documents in order to assess the relative relevance of a document to a given query and thus rank the underlying collection. The sheer size of this problem has led to the development of *learning-to-rank* algorithms that can automate the construction of such ranking functions: Given a training set of (feature vector, relevance) pairs, a machine learning procedure learns how to combine the query and document features in such a way so as to effectively assess the relevance of any document to any query and thus rank a collection in response to a user input.

Given that document corpora have been increasing in size it is practically infeasible (1) to extract features from all document-query pairs, (2) to judge each document as relevant or irrelevant to each query, and (3) to train learning-to-rank algorithms over such a vast data set.

Even though extracting features and training ranking algorithms is computationally expensive, annotating documents with relevance grades is the main bottleneck in constructing training collection, since human effort is required. It is essential therefore, both for the efficiency of the construction methodology and for the efficiency of the training algorithm, that only a small subset of documents be selected. The document selection, though, should be done in a way that does not harm the effectiveness of learning.

Unlike document selection for learning-to-rank, where little work has been done, a significant volume of work has appeared in the literature regarding document selection for efficient and effective evaluation of retrieval systems [5, 32, 33, 146, 148, 110], including the methodology presented in Section 3.1. Here, we explore the duality between document selection methodologies for evaluation and document selection methodologies for learning-to-rank. The main question we ask is : “Can any of the methodologies designed for efficient evaluation also be used for constructing effective learning collections? If yes, which one of these methods is better for this purpose?”

We employ five different document selection methodologies that are well studied in the context of evaluation, along with the method used in LETOR for comparison purposes. Subsets of documents are chosen according to the six methods at different percentages of the complete document collection (in our case the depth-100 pool), and features are extracted from the selected query-document pairs. State of the art learning-to-rank algorithms are used then to train ranking functions over each one of the data sets the six selection methods have produced, and the resulting functions are compared with each other in terms of their performance.

In particular, (1) we explore whether certain document selection methodologies are better than others in terms of both efficiency and effectiveness; that is, how fast, in terms of documents, can ranking functions learn to combine features in a meaningful way over the corresponding data sets such that their performance is not significantly worse than the performance of functions trained over the complete collection (depth-100 pool), and (2) we isolate certain characteristics of the selected subsets of documents (e.g. the percentage of relevant documents, or the similarity among relevant documents in the subsets) and study their effect on the efficiency and effectiveness of learning.

4.1 Methodology

In order to investigate the effect of document selection on the ability of learning-to-rank algorithms to effectively and efficiently learn a ranking function, five different document selection methodologies, widely used in retrieval evaluation, are studied.

Our *complete* document collection consists of the depth-100 pools from TREC 6, 7 and 8 adhoc tracks [1]. This collection consists of 150 queries in total, along with the corresponding relevance judgments. Features are extracted from all query-document pairs. Using different document selection methodologies, for each query, documents from the complete collection are selected with different percentages from 0.6% to 60%, forming different sized subsets of the complete collection for each methodology.

Features and relevance judgments pairs are then partitioned into five parts in order to conduct five-fold cross validation. For each fold, three parts are used for training, one part for validation and one part for testing. The documents in the training and validation sets are samples of the complete collection, as described above. The testing set consists of the complete set of documents.

State of the art learning-to-rank algorithms are then trained and the quality of the resulting ranking models is assessed by mean average precision (MAP).

[1] In the sections that follow in this chapter we use the "depth-100 pools" and "complete collection" interchangeably

4.1.1 Data sets

The document corpus, the queries and the relevance judgments are obtained from TREC 6, 7 and 8 ad-hoc retrieval track.

The document corpus consists of approximately half a million documents (528,155) from the Financial Times, the Federal Register, the Foreign Broadcast Information Service and the LA Times document collections [134].

The queries were developed by NIST assessors and they were chosen based on their estimated number of relevant documents in the corpus. Collectively, 150 such queries were developed. A number of retrieval systems were run over these queries and for each query the depth-100 pools of the returned documents were judged as relevant or irrelevant by the same NIST assessor that issued the query.

We extract features from all query-document pairs. The features extracted are shown in Table 4.1.1 and they are a subset of the LETOR3.0 features [128].

For the computation of the IDF (inverse document frequency) of a query term q_i, as in LETOR3.0, we adopted the widely used formula,

$$idf(q_i) = log\frac{N - n(q_i) + 0.5}{n(q_i) + 0.5}$$

where $n(q_i)$ is the number of documents containing q_i and N is the total number of documents in the collection. Since a query may contains more than one term, the final value of IDF is the sum of the IDF of each query term:

$$idf(Q) = \sum_{q_i} idf(q_i)$$

Note that IDF is document independent, and so all the documents under a query have same IDF values.

The BM25 score of a document D for a query Q, containing the terms $q_1, q_2, ..., q_n$ is computed as:

$$\text{BM25(D,Q)} = \sum_{i:f(q_i,D)>0} idf(q_i) \cdot \frac{f(q_i,D)\cdot(k_1+1)}{f(q_i,D) + k1\cdot(1-b+b\cdot\frac{|D|}{\text{avgdl}})} \cdot \frac{(k_3+1)f(q_i,Q)}{k3 + f(qi;Q)}$$

where $idf(q_i)$ is defined as above, $f(q_i, D)$ is the occurrences of q_i in the document D, $f(q_i, Q)$ is the occurrences of q_i in the query Q, $|D|$ is the length of the document D (i.e., the number of words), and *avgdl* is the average document length in the entire document collection. The parameters k_1, k_3 and b are set to $k_1 = 2.5$, $k_3 = 0$ and $b = 0.8$. The language modeling features are implemented according to Zhai and Lafferty [153].

A full description of these features in the LETOR documentation [128]. Each feature is computed over the document text (without the title) and over the document text combined with the title, resulting in 22 features in total. Note that web features such as PageRank are not computed since the document corpus is not a web corpus.

	Features
1.	BM25
2.	LogBM25_Feature
3.	LM_ABS_Feature
4.	LM_DIR_Feature
5.	LM_JM_Feature
6.	LogNormalizedTF_Feature
7.	SumLogTF_Feature
8.	TF_Feature
9.	TF_IDF_Feature
10.	LogTF_IDF_V2_Feature
11.	NormalizedTF_Feature

Table 4.1: Feature Set

4.1.2 Document selection

For each query, six different document selection methodologies are employed to choose documents from the complete collection:

- **Depth-k pooling:** According to the *depth pooling*, the union of the top k documents retrieved by each retrieval system submitted to TREC in response to a query is formed and only the documents in this depth-k pool are selected to form the learning-to-rank collection. The intuition behind depth-pooling is that most relevant documents appear at the top of the ranked list and therefore depth-k pools contain most of them [60, 155].

- **InfAP sampling (uniform random sampling):** InfAP sampling [146] utilizes uniform random sampling to select documents to be judged. In this manner, the selected documents are representative of the documents in the complete collection.

- **StatAP sampling (stratified random sampling):** StatAP sampling [94] employs stratified sampling. Using a prior of relevance induced by the average precision measure, each document is selected with probability roughly proportional to its likelihood of relevance.

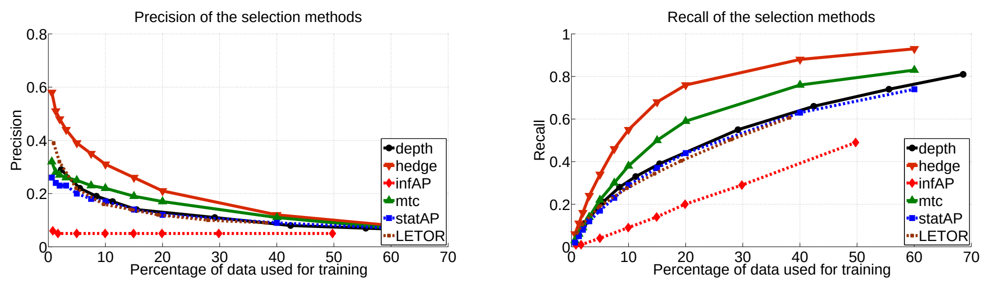


Figure 4.1: Precision and Recall of the selected documents for different selection strategies and for different sampling percentages.

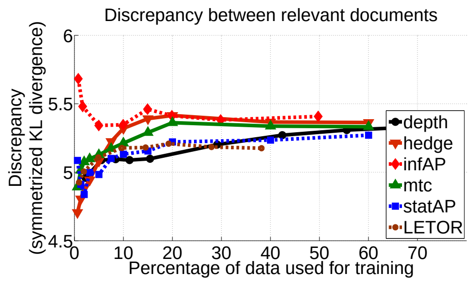
- **MTC (greedy on-line algorithm):** MTC [32] is a greedy on-line algorithm that selects documents according to how informative they are in determining whether there is a performance difference between two retrieval systems. Our intuition is that many of the documents selected by MTC, relevant or not, are "interesting" for learning by being relatively close to the decision surface of the classifier, similar to active learning.

- **Hedge (greedy on-line algorithm):** Finally, hedge is an on-line learning algorithm used to combine expert advice. It is essentially a feedback-metasearch technique, which, when applied to the document selection problem, aims at choosing documents that are most likely to be relevant [6]. Hedge finds many relevant documents "common" to various retrieval systems, thus documents likely to contain many of the query words.

- **LETOR:** For comparison purposes, a LETOR-like document selection methodology is also employed. According to this methodology, documents in the complete collection are first ranked by their BM25 scores for each query and the top-k documents are then selected for feature extraction. This method is designed to select documents that are considered relevant to the query by BM25.

When the properties of the above document selection methodologies are considered, one can see that infAP creates a representative selection of documents, statAP and depth-k pooling aim at identifying more relevant documents utilizing the knowledge that retrieval systems return relevant documents at higher ranks, the LETOR-like method aims at selecting as many relevant documents according to BM25 as possible, hedge aims at selecting only relevant documents, and MTC greedily selects discriminative documents.

For all these strategies, the precision, computed as the ratio of the number of relevant documents in the document sample to the total number of documents in the document



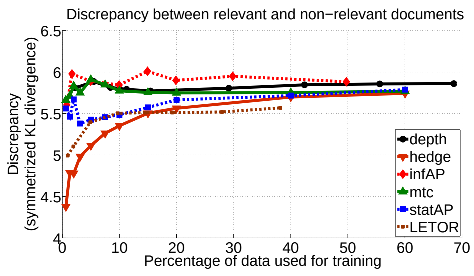


Figure 4.2: Discrepancy among relevant documents and among relevant and non-relevant documents selected by different selection strategies and for different sampling percentages.

sample, and the recall, computed as the ratio of relevant documents in the document sample to the total number of relevant documents in the complete collection, are illustrated in the left and right plots of Figure 4.1, respectively. As expected, hedge achieves the largest values of precision and recall, followed by MTC. Pooling and statAP follow by achieving similar values of precision and recall. Since infAP is based on uniform random sampling, the precision of infAP stays constant while the recall grows linearly with the sample percentage. The LETOR-like selection achieves both high precision and recall at small percentages of data used for training (up to 5%) and then it drops to the levels of statAP and depth pooling.

Further, the discrepancy among the selected relevant documents, along with the discrepancy among the selected relevant and non-relevant documents for the different selection methods is illustrated in Figures 4.2. The discrepancy is measured for each pair of documents by the symmetrized Kullback-Leibler divergence between the documents' (smoothed) language models, then averaged over all pairs in a set. As it can be observed at the leftmost plot, for all methods except infAP, the selected documents are very similar to each other. For small percentages, it can be seen that the relevant documents picked by hedge are very similar to each other. As more documents are selected according this algorithm, relevant documents with different properties can be identified. Depth-pooling and statAP select similar relevant documents due to the underlying retrieval systems that return similar relevant documents at the top-end of their ranked lists, while hedge picks similar relevant documents by design. In particular, hedge selects very similar documents regardless of their relevance, as it can be observed in the right-most plot. At the other end of the discrepancy scale, infAP for small sampling percentages selects the most diverse relevant documents while it converges fast to the average discrepancy between documents in the complete collection. The LETOR-like selection methodology also selects very similar documents, since the documents selected are those that give high BM25 values and thus have similar characteristics.

4.1.3 Learning-to-rank algorithms

We employ five different learning-to-rank algorithms to test the document selection methodologies,

- **RankBoost (boosting):** RankBoost is a very well known ranking mechanism based on the AdaBoost algorithm [53] for supervised learning. RankBoost training is performed using pairwise preferences, essentially combining several "weak" rankers into a master one using on-line learning. Typical weak learners are features of the data (in our case extracted features from documents) with a threshold that best differentiates the relevant documents from non-relevant ones; however, in general, the weak rankers can be very complicated retrieval/ranking functions.

  Rankboost is widely reported in many learning-to-rank publications [128], primarily as a baseline ranking algorithm. In our experiments, we run the algorithm for 50 epochs. In some tests we trained Rankboost for larger number of epochs (up to 1000) and concluded that performance after 50 epochs was stable, even for small datasets.

- **Regression (regression):** We implemented a baseline linear regression ranker, with the purpose of studying the change in learning performance across various training sets. The procedure basically fits a linear regression model to the training set, and then it uses the learned model to predict (and rank) the test documents.

- **Ranking SVM (SVM):** The implementation of Support Vector Machines used is based on SVM-perf [72, 70] and is similar to the ones reported in ranking literature [128]. We use a polynomial kernel of degree 2 (sparse approximation with 400 basic functions), and a tradeoff constant of $c = 0.01$. We experimented with several loss functions; the results presented here use as loss function a variant of ROC area, specifically the percentage of swapped positive/negative pairs. The structural learning algorithm uses a shrinking heuristic[2] in order to speed up the training.

- **RankNet (neural network):** RankNet [26] is one of the basic learning-to-rank algorithms based on neural networks. The algorithm is based on training the neural net on pairs of documents (or feature vectors) that have different relevance. During training, single feature vectors are first forward propagated through the net and are sorted based on their scores. The RankNet cost is a sigmoid function followed by a cross entropy cost that evaluates the difference of the *learned* probability that a document pair will be ranked in some order from the *actual* probability. In our experiments, the

[2]http://svmlight.joachims.org/svm_perf.html

training was run for 300 epochs (no significant improvement was observed if more epochs were used).

- **LambdaRank (neural network):** LambdaRank [27] aims at directly optimizing an IR metric, in particular, NDCG. Since the IR metrics are not smooth as they depends on ranks of documents, it uses the approach of defining the gradient of the target evaluation metric only at the points needed. Given a pair of documents, the gradients used in LambdaRank are obtained by scaling the RankNet cost with the amount of change in the value of the metric obtained by swapping the two documents. Similar to RankNet, LambdaRank training was also run for 300 epochs.

As a summary, RankBoost optimizes for pairwise preferences, Regression optimizes for classification error in the relevance judgments, and SVM optimizes for the area under the ROC curve. RankNet aims to optimize for the probability that two documents are ranked in correct order in the ranking. Finally, LambdaRank directly optimizes for nDCG and even though the gradients are virtually defined, the method is shown to find the local optimum for the target metric.

All the algorithms, with the exception of Regression, are “pair-wise” because they consider pairs of documents while training, either directly in the learning mechanism or indirectly in the loss function.

4.2 Results

The performance of the learning-to-rank algorithms when trained over the different data sets produced by the six document selection methodologies is illustrated in Figure 4.3. The x-axis on the plots is the percentage of documents sampled from the complete document collection (depth-100 pool). The performance of the rankers (y-axis) is measured by the mean average precision (MAP) of the ranked list of documents returned by the rankers in response to the queries in the testing data sets. Each one of the document selection methods employed corresponds to a curve in the plot.

As one can observe in Figure 4.3, for most of the cases, the learning-to-rank algorithms reach almost optimal performance with as little training data as 1% of the complete collection. The Student’s t statistical test was employed in order to test whether the difference among the achieved MAP scores of the ranking function for different sampling percentages and the maximum MAP score the ranking functions obtain (MAP using full training data) are statistically significant. The t-test exhibits no significant differences for ranking functions trained over infAP, statAP, depth-pooling and MTC at any of document sampling percentage above 2%.

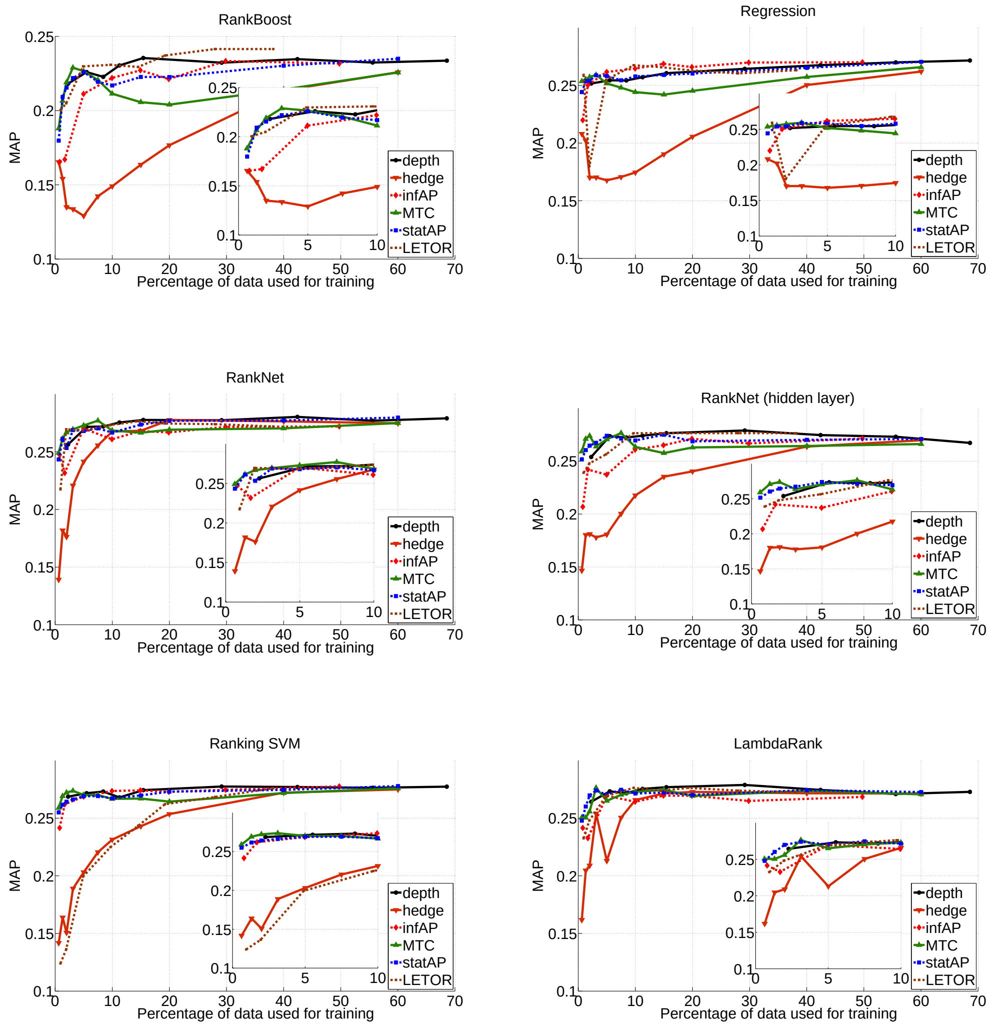


Figure 4.3: RankBoost, Regression, Ranking SVM, RankNet (linear and nonlinear) and LambdaRank ranking performance across several document selection methods at various sampling percentages.

Therefore, training data sets whose sizes are as small as 1% to 2% of the complete collection are just as effective for learning-to-rank purposes as the complete collection. Thus, one can train much more efficiently over a smaller (though effectively equivalent) data set or at equal cost one can train over a far “larger” and more representative data set either by increasing the number of queries of by selecting documents deeper in the rankings. Note that the number of features used by the learning-to-rank algorithms may well affect the efficiency of these algorithms to learn an effective ranking function [130]. In our experiments only twenty two (22) features where used, most of which are ranking functions of their own (e.g. BM25 or language models). Therefore, in the case where hundreds of (raw) features are employed, ranking functions may need more than 1% of the complete collection to achieve optimal performance. Nevertheless, in a setup similar to

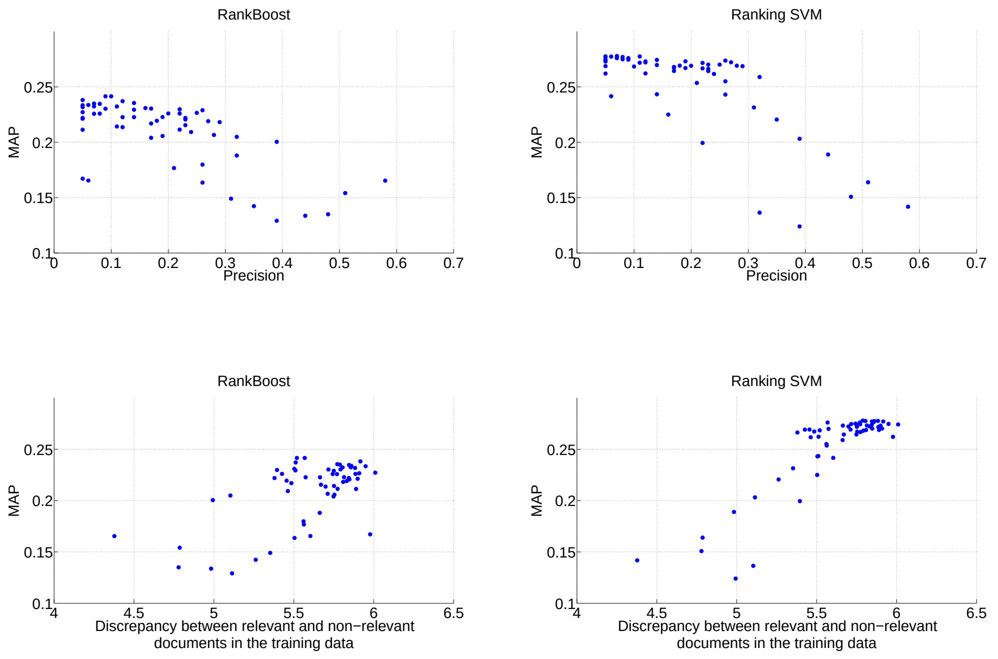


Figure 4.4: Scatter plots of the performance of RankBoost and SVM ranking functions versus the percentage of relevant documents and the discrepancy between relevant and non-relevant documents in the training data.

LETOR setup, as in our experiments, we show that substantially less documents than the ones used in LETOR can lead to similar performance of the trained ranking functions.

Furthermore, it is apparent from Figure 4.3 that the effectiveness of small data sets for learning-to-rank purposes eminently depends on the document selection methodology employed. The most striking example of an inefficient document selection methodology is that of hedge. Ranking functions trained on data sets constructed according to the hedge methodology only reach their optimal performance when trained over data sets that are at least 20% of the complete collection, while in the worst case, the performance of some ranking functions is significantly lower than the optimal one even when trained over 40% to 50% of the complete collection (e.g. the performance of RankBoost, Regression and RankNet with the hidden layer).

Ranking functions exhibit their second worst performance when trained over data sets constructed according to the LETOR-like document selection methodology. Even though LETOR data sets have been widely used by researchers, our results show that the document selection methodology employed in LETOR is neither the most effective nor the most efficient way to construct learning-to-rank collections.

The deficiency of learning-to-rank data sets produced according to hedge and LETOR-like document selection methodologies may seem counterintuitive results. One would expect relevant documents to be much more "informative" than non-relevant documents for the purpose of learning-to-rank, and both hedge and LETOR-like document selection methodologies are designed to choose as many relevant documents as possible. Clearly this is not the case according to our results.

In what follows, we try to give an explanation of these counterintuitive. Figure 4.4 illustrates the performance of two of the learning-to-rank algorithms (SVM and RankBoost) for which data sets created according to hedge and LETOR-like methods seem the least effective. The y-axis corresponds to the performance of the ranking functions. The x-axis in the top-row plots corresponds to the percentage of relevant documents in the learning-to-rank data sets, while at the bottom-row plots, it corresponds to the discrepancy among the selected relevant and non-relevant documents. Each dot in these plots corresponds to a training data set at some sampling percentage, regardless of the document selection algorithm employed. As can be observed, there is a clear negative correlation between the percentage of relevant documents (above a certain threshold) and the performance of both ranking functions. Further, a strong positive correlation is exhibited between the dissimilarity among relevant and non-relevant documents and the performance of the two algorithms. This is a strong indication that over-representation of relevant documents in the training data sets may harm the learning-to-rank algorithms. Furthermore, when relevant and non-relevant documents in the training data set are very similar to each other, performance of the resulting ranking functions decline.

Both hedge and LETOR-like document selection methodology, by design, select as many relevant documents as possible. As shown in Figure 4.2, the documents selected by the two methods also exhibit very high similarity to each other.

In contrast to the aforementioned selection methodologies that are designed to select as many relevant documents as possible, infAP, statAP, depth-pooling and MTC tend to construct data sets that are more representative of the complete collection. In particular, infAP randomly samples the complete collection, and thus, all documents in the collection have equal probability to be included in the training data set, regardless of their relevance grade. Even though depth-pooling tends to select documents from the top end of the ranked lists of the underlying participating systems, it treats all systems in a fair manner regardless of their quality, giving poor underlying systems an equal opportunity to contribute to the constructed data sets. Thus, many non-relevant documents are included in the resulting training sets. StatAP selects the documents that are commonly retrieved by multiple systems due to the way the sampling prior is computed. Hence, the quality of the sampled documents depend on the quality of the underlying systems. If a non-relevant document is retrieved by many retrieval systems, then this document is still very likely to be sampled by the statAP sampling. Finally, MTC, by design, selects the most informative documents for the purpose of evaluation, regardless of their relevance.

Therefore, the percentage of relevant documents in the learning-to-rank data sets along with the similarity of relevant and non-relevant documents appear to be good explanatory parameters for the efficiency and effectiveness of all the aforementioned document selection methodologies.

In order to quantify the explanatory power of these two parameters, we formulate a linear model, with the performance of RankBoost and SVM as measured by MAP over the testing data sets expressed as a linear function of the percentage of relevant documents and the dissimilarity among relevant and non-relevant documents in the data sets. Both the adjusted R^2 and the F-statistic of the resulting linear models indicate an excellent goodness of fit, with the former being equal to 0.99 and with the p-value of the latter being equal 10^{-16}.

Further, to assess the relative importance of the two explanatory parameters, with respect to the performance of the learning-to-rank algorithms, we also fit an ANOVA model to the data and performed a variance decomposition analysis, according to which the percentage of relevant documents accounts for about 60% of the variance in the MAP scores across all data sets and the discrepancy between relevant and non-relevant documents accounts for about 39%. Therefore, we conclude that both the proportion of relevant documents and the dissimilarity between documents are highly important for the quality of a learning-to-rank collection, with the former affecting the quality more than the latter.[3]

Finally, by revisiting Figure 4.3, one can observe that some learning-to-rank algorithms are more robust to document selection methodologies than others. In particular, LambdaRank and RankNet seem to be more robust than Regression, RankBoost and Ranking SVM. To assess the relative importance between the learning-to-rank algorithms' effect on MAP and the selection methodologies' effect on MAP, we fit a 2-way ANOVA model to the MAP values and again perform a variance decomposition. According to ANOVA 26% of the variance in the MAP scores is due to the selection methodology and 31% due to the learning-to-rank algorithm, while 5% is due to the algorithm-selection methodology interaction. When we perform the same analysis to MAP values over datasets up to 10% of the complete collection, then 44% of the variance in the MAP scores is due to the selection methodology and 23% is due to the learning-to-rank algorithm, while 10% is due to the algorithm-selection methodology interaction.

[3]Note that we have tested the effect of other explanatory parameters to the variability of MAP values (e.g. recall, total number of documents, total number of relevant documents, similarity between relevant documents, interactions between these parameters). None of these parameters appeared to be significant.

4.3 Overall Conclusions

We have analyzed the problem of building document collections for efficient and effective learning-to-rank. In particular, we explored (1) whether the algorithms that are good for reducing the judgment effort for efficient evaluation are also good for reducing judgment effort for efficient learning-to-rank and (2) what makes one learning-to-rank collection better than another.

For this purpose we constructed different sized learning collections employing depth pooling, infAP, statAP, MTC, hedge and the LETOR methodology. We then ran a number of state of the art learning-to-rank algorithms over these training collection and compared the quality of the different methods used to create the collections based on the relative performance of the learning algorithms.

We showed that some of these methods (infAP, statAP and depth pooling) are better than others (hedge and the LETOR method) for building efficient and effective learning-to-rank collections. This is a rather surprising result given the wide usage of the LETOR datasets as it suggests that using the same judgment effort, better collections could be created via other methods.

Furthermore, we showed that both (1) the proportion of relevant documents to non-relevant documents and (2) the similarity between relevant and non-relevant documents in the data sets highly affect the quality of the learning-to-rank collections, with the latter having more impact.

Finally, we observed that some learning-to-rank algorithms (RankNet and LambdaRank) are more robust to document selection methodologies than others (Regression, RankBoost and Ranking SVM).

CHAPTER 5

Evaluation Metrics

Evaluation metrics play a critical role both in the context of comparative evaluation of the performance of retrieval systems and in the context of learning-to-rank (LTR) as objective functions to be optimized. Many different evaluation metrics have been proposed and studied in the literature. Even though different metrics evaluate different aspects of retrieval effectiveness, only a few of them are widely used, with average precision (AP) being perhaps the most commonly used such metric. AP has been the dominant system-oriented evaluation metric in IR for a number of reasons:

- It has a natural top-heavy bias.
- It has a nice probabilistic interpretation [146].
- It has an underlying theoretical basis as it corresponds to the area under the precision recall curve.
- It can be justified in terms of a simple but moderately plausible user model [103].
- It appears to be highly informative; it predicts other metrics well [10].
- It results in good performance ranking functions when used as objective in learning-to-rank [152, 143].

The main criticism to average precision is that it is based on the assumption that retrieved documents can be considered as either relevant or non-relevant to a user's information need. Thus, documents of different relevance grades are treated as equally important with relevance conflated into two categories. This assumption is clearly not true: by nature, some documents tend to be more relevant than others and intuitively, the more relevant a document is the more important it is for a user. Further, when AP is used as an objective metric to be optimized in learning to rank, the training algorithm is also missing this valuable information.

For these reasons, a number of evaluation metrics that utilize multi-graded relevance judgments has appeared in the literature (e.g. [126, 68, 69, 111, 108]), with nDCG [68, 69] being the most popular among them, especially in the context of learning-to-rank as most learning to rank algorithms are designed to optimize for nDCG [27, 26, 129, 143].

In the framework used to define nDCG, a relevance score is mapped to each relevance grade, e.g. 3 for highly relevant documents, 2 for fairly relevant documents and so on. The relevance score of each document is viewed as the *gain* returned to a user when examining the document (utility of the document). To account for the late arrival of relevant documents, gains are then discounted by a function of the rank. The discount function is viewed as a measure of the patience of a user to step down the ranked list of documents. The discounted gain values are then summed progressively from rank 1 to k. This discounted cumulative gain at rank k is finally normalized in a 0 to 1 range to enable averaging the values of the metric over a number of queries, resulting in the *normalized Discounted Cumulative Gain*, nDCG.

The nDCG metric is thus a functional of a gain and a discount function and thus it can accommodate different user search behavior patterns on different retrieval task scenarios. It has been illustrated by a number of correlation studies, different gain and discount functions lead to radically different rankings of retrieval systems [137, 76, 75]. Therefore, nDCG with different gain and discount functions evaluates radically different aspects of retrieval effectiveness and correspond to radically different notions of user utility.

Despite the great flexibility nDCG offers, defining gain and discount functions in a meaningful way is a difficult task.

With respect to gain functions this difficulty comes from the fact that the value of a document is user dependent. For instance, some users might get frustrated with marginally relevant documents while others might consider them as informative. A similar problem is associated with the discount function since different users have a different way of interacting with ranked lists (browsing patterns).

Given the infinite number of possible discount and gain functions, the vast differences in users search behavior, the many different possible retrieval tasks and the difficulty in measuring user satisfaction, a complete and rigorous analysis of the relationship between different gain and discount functions and user satisfaction under different retrieval scenarios is prohibitively expensive, if at all possible.

For this reason, in the past, the selection of the gain and discount functions has been done rather arbitrarily, based on speculations of the search behavior of an average user and speculations of the correlation of the metric to user satisfaction. For instance, Burges et al. [26], introduced an exponential gain function ($2^{\text{rel(r)}} - 1$, where rel(r) is the relevance score of the document at rank r) to express the fact that a highly relevant document is very much more valuable than one of a slightly lower grade. Further, the logarithmic discount function ($1/log(r+1)$) dominated the literature compared to the linear one ($1/r$) based on the speculation that the gain a user obtains by moving down the ranked list of documents does not drop as sharply as indicated by the linear discount.

Despite the reasonable assumptions behind the choice of the gain and discount function that dominates nowadays the literature, recent work [1] demonstrated that cumulative gain without discounting (CG) is more correlated to user satisfaction than the discounted cumulative (DCG) and nDCG (at least when computed at rank 100). This result not only strongly questions validity of the aforementioned assumptions but mostly underlines the difficulty in specifying gain and discount functions in a meaningful manner.

In Section 5.1 we propose a methodological way of selecting a gain and a discount function for nDCG without trying to correlate the metric with user satisfaction. Instead, the resulting nDCG metric is efficiency-optimal, that is it requires the least number of queries to effectively rank retrieval systems.

Due to the above difficulties associated with the current multigraded evaluation metrics, even when multigraded relevance judgments are available, average precision is still reported (together with the multigraded metrics) by converting the relevance judgments to binary [13, 12]. Thus, despite the invalid assumption of binary relevance, average precision remains one of the most popular metrics used by IR researchers (e.g. in TREC [12, 23, 4]). Furthermore, even though AP is wasting valuable information in the context of learning-to-rank, since it ignores the swaps between documents of different positive relevance grades, it has been successfully used as an objective metric [152]. Given that, in section 5.2 we extend average precision to graded relevance judgments.

5.1 Gain and Discount Function for nDCG

Given the fact that nDCG variations result in different rankings of systems and thus they evaluate different aspects of retrieval effectiveness along with the difficulty in studying gain and discount functions with respect to user satisfaction, one can compare different variations of nDCG based on other desirable properties of the resulting measure. For instance for different gain and discount functions, one can investigate how informative the resulting variations of nDCG are, i.e. how well do they summarize the relevance of the underlying ranked list of documents [9], how discriminative they are, i.e. how well do they discriminate good from bad systems [109], or how stable they are, i.e. how different the rankings of systems are over different sets of queries [24]. Sakai [111] compared the effect of a number of different gain and discount functions based on the discriminative power of nDCG.

Here, we adopt the variance component analysis framework proposed by Bodoff and Li [17] to measure the stability/efficiency of the resulting nDCG measure when different gain and discount functions are utilized. Based on this framework, we define a stability- or efficiency-optimal gain function by treating gain values of relevance grades as unknown variables and optimizing for the aforementioned stability/efficiency measure. We compare the resulting function to both the linear and the exponential variates that have appeared in the literature, both in terms of stability/efficiency and induced rankings of systems. Similarly, we also define a stability- or efficiency-optimal discount function and compare it against the Zipfian, the log and the linear function. Further, we define a Pareto optimal combination of gain and discount function, i.e. the combination of gain and discount function that maximizes the minimum stability. Finally, we explore whether the stability- (efficiency-) optimal gain and discount functions lead also to an nDCG measure with high discriminative power [109].

5.1.1 Methodology

In this section, we describe a methodology to numerically derive stability- (efficiency-) optimal gain and discount functions. First, we adopt the methodology used by Bodoff and Li [17] to assess the reliability of an IR test collection.

Given an evaluation measure, a number of retrieval systems, a set of queries and a document collection, Bodoff and Li considered two sources of variability in the observed system scores of the retrieval systems when they are run over the given queries, (a) the actual performance differences between systems, and (b) differences in the nature of the queries themselves. This way, Bodoff and Li [17], quantified the quality of the test collection as the proportion of the total variability observed in the scores of the retrieval systems that is due to actual performance differences among these systems.

In a similar manner, different sources of variability can be considered and quantified. For instance, earlier than Bodoff and Li, Banks et al. [14] considered, as an additional to the systems and queries source of variability, the judges that assess the relevance of the documents to the queries.

In this work, we consider the evaluation measure itself as a source of variability. In particular, given a number of retrieval systems, a set of queries and a document corpus, we consider gain and discount function of nDCG as unknown variables and we select the ones that maximize the proportion of variability due to actual performance differences among systems. The proportion of variability reflects the stability of the evaluation measure, and thus by maximizing this proportion we maximize the stability of the measure. Furthermore, the more stable a measure is the fewer queries it requires to reliably evaluate the retrieval systems. Thus, by maximizing stability we also maximize efficiency in terms of required queries.

We numerically computed the stability optimal gain and discount function by employing (a) variance decomposition analysis of the nDCG scores [14, 17, 20] and (b) optimization. The variance decomposition analysis is described in details in Appendix B. In the following subsection we describe the optimization component of our methodology in details.

5.1.1.1 Optimization

$$\underset{\{gain(grade_j)\}}{\text{argmax}} \frac{\sigma^2(\text{sys})}{\sigma^2(\text{sys}) + \sigma^2(\text{topic}) + \sigma^2(\text{sys:topic})}$$

Subject to:

$$\begin{array}{ll} 1. & \sum_{j=1}^{k} gain(grade_j) = 1 \\ 2. & gain(grade_j) - gain(grade_{j+1}) \leq 0 \forall j : 1 \leq j \leq k-1 \end{array}$$

where k is the number of relevance grades.

$$\underset{\{disc(rank_r)\}}{\text{argmax}} \frac{\sigma^2(\text{sys})}{\sigma^2(\text{sys}) + \sigma^2(\text{topic}) + \sigma^2(\text{sys:topic})}$$

Subject to:

$$\begin{array}{ll} 1. & \sum_{r=1}^{r} disc(rank_r) = 1 \\ 2. & disc(rank_r) - disc(rank_{r+1}) \leq 0 \forall j : 1 \geq j \leq N-1 \end{array}$$

where N is the cut-off rank at which nDCG is calculated.

Figure 5.1: Optimization setup for gain values and discount factors, respectively.

In the optimization process employed, we use the dependability coefficient [1] , Φ, as the objective function to maximize with respect to the gain values/discount factors employed in nDCG.

Note that nDCG is a scale-free measure with respect to both the gain values and the discount factors in the sense that multiplying either the gain or the discount with any number does not affect the nDCG score. For this reason, we enforced the gain values to be a probability distribution over relevance grades and the discount factors to be a probability distribution over ranks. This way we limit the range of values both for the gain and the

[1]The dependability coefficient is defined in Appendix B

discount within the $[0, 1]$ range and reduce the unknown parameters by one. Furthermore, it so happens that there maybe some fluctuation in the values of the optimal discount factors, e.g. the discount factor on a certain rank may happen to be larger than the one on a lower rank. This is not justifiable from an IR perspective and thus, we also enforce that the discount factors are non-increasing with the rank. The same may be true for the gain values, hence we enforce them to be non-decreasing with the relevance grade. Further, we set the gain value for non-relevant documents equal to zero.

Moreover, note that the coefficient Φ in Equation B.2 is a monotonically non-decreasing function of the number of queries. In other words, the gain or discount function that is optimal for n queries is also optimal for $n + 1$ queries. Therefore, in the optimization process we set the number of queries equal to 1.

The optimization setup for the gain/discount function is mathematically expressed in Figure 5.1. When we optimize for the discount factors we consider the gain values as given, while when we optimize for gain values we consider the discount factors as given. We also perform a multi-objective optimization to simultaneously optimize for the gain and the discount function. For the purpose of the optimization, we used the **fmincon** MATLAB function for the normal optimization and the **minimax** MATLAB function for the multi-objective optimization. Both functions employ Sequential Quadratic Optimization.

5.1.2 Results

The afore-described optimization framework was applied to the TREC 9 and 10 Web track collections and the TREC 12 Robust track collection. The number of participating systems for the three collections is 105, 97 and 78 respectively. All systems were run over 50 queries[2]. Documents returned as a respond to these queries were judged by a single assessor in 3 relevance grades scale: highly relevant, relevant and non-relevant. The task in all tracks was the usual ad-hoc retrieval task.

For each one of the three test collections, we calculated the optimal discount factors (given a linear gain function), the optimal gain values (given a logarithmic discount function) and the Pareto-optimal gain values and discount factors. We compared the optimal gain and discount functions with the a number of commonly used gain and discount functions both with respect to the stability/efficiency of the resulting nDCG measure and with respect to the induced by the resulting nDCG ranking of systems.

[2]The TREC 12 Robust track collection includes 100 queries, however the first 50 of them were obtained from TREC 6, 7 and 8, where documents were judged as either relevant or non-relevant. For this reason, we did not use these 50 queries in our studies.

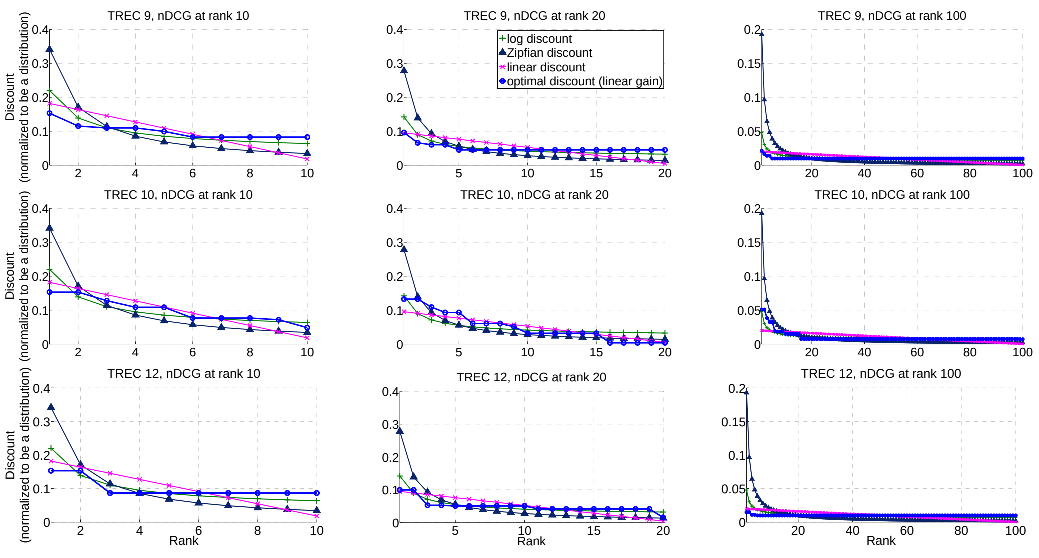


Figure 5.2: Discount factors at each rank for (a) the optimal discount function, (b) the Zipfian discount function (1/rank), (c) the log discount function (1/$\log_2$(1+rank)) and the linear discount function ((cut-off rank+1-rank)/(cut-off rank)).

5.1.2.1 Optimal discount function

In this section we present the results of the optimization for the discount function. The gain values were set to 0, 1 and 2 for non-relevant, relevant and highly relevant documents respectively and they were treated as constants during the optimization. We performed the optimization over the TREC 9, 10 and 12 data sets for nDCG computed at rank 10, 20 and 100 and we report the results in Figure 5.2. We compare the optimal discount factors – blue solid curve with circles as markers in the figure – (a) with the Zipfian discount function (1/rank) – green solid curve with plus signs as markers, (b) with the log discount function (1/$\log_2$(1+rank)) – dark blue solid curve with triangles as markers and (c) with the linear discount function ((cut-off rank + 1 - rank) / cut-off rank) – magenta solid curve with crosses as markers. For comparison purposes, we transformed the linear, log and Zipfian discount factors to probability distributions over the ranks.

As it can observed in Figure 5.2, the optimal discount function is the least steep one among the discount function considered. The log discount function is the one closest to the optimal, while the Zipfian drops much faster than the optimal. The linear discount also appears to be close to the optimal one, at least when only the top ranks are considered..

Looking at the right-most plots for each TREC, that is the plots corresponding to nDCG at rank 100, one can observe that the top ranks are the ones that mainly matter and thus they are given higher discount factor, while the rest of the ranks are given a rather small and constant discount factor. The number of the top-most ranks that really matter seems

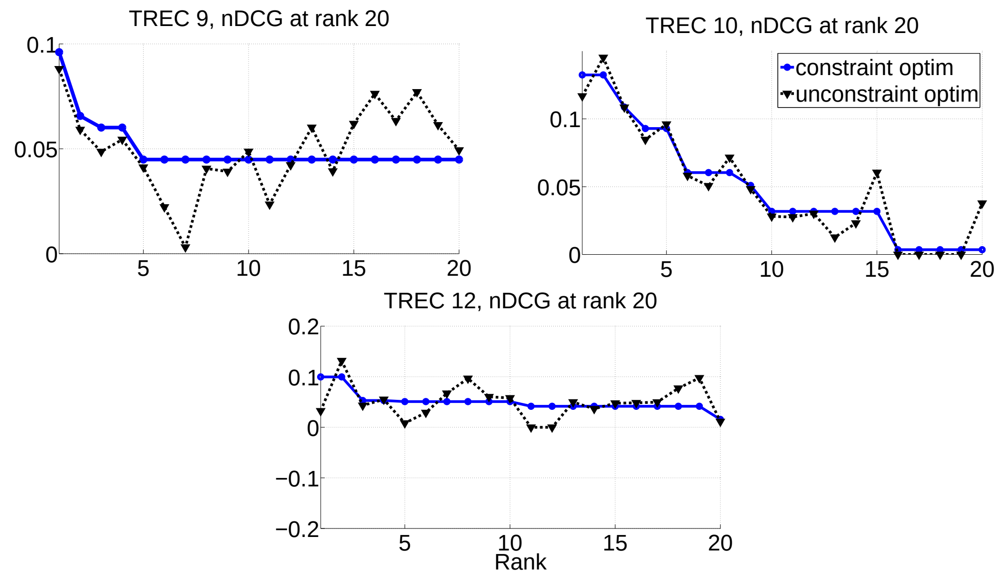


Figure 5.3: Discount factors for the optimal unconstrained discount function. The optimal constrained discount function is also included for comparison purposes.

to be collection dependent, with the top 10 ranks being the important ones for TREC 9 and 12 and the top 20 ranks being the important ones for TREC 10. A further observation one can make is that, even though the rest of the ranks are given a rather constant discount factor, this constant is far from zero (or at least farther than the discount factors the log and the linear discount function assigns to those ranks) suggesting that documents lower at the ranked list may also be useful in discriminating systems efficiently. This further suggests that computing nDCG at top ranks is sub-optimal since computing nDCG at some cut-off rank implicitly assigns zero discount factors to the ranks below that cut-off.

For the purpose of completeness, Figure 5.3 illustrates the results when we optimized the stability (efficiency) of nDCG without enforcing the non-increasing constraint for the discount factors. We only report results for nDCG at rank 20. One may observe that the optimal unconstrained discount factors are not strictly decreasing with the rank. Intuitively, these fluctuations are due to the fact that often times similar systems return relevant documents at the top ranks and thus the only way to discriminate them is by looking deeper in the ranked list of documents. Thus, once again, this indicates that lower ranks may very well help in discriminating systems.

Figure 5.4 illustrates the stability of the nDCG measure (i.e. the fraction of the variance in the mean nDCG values due to actual performance differences between systems) when computed using (a) the optimal, (b) the log, (c) the Zipfian, and (d) the linear discount function. As expected, the optimal discount function eliminates all variance components other than the one due to systems, faster (in terms of queries) than the rest of the discount

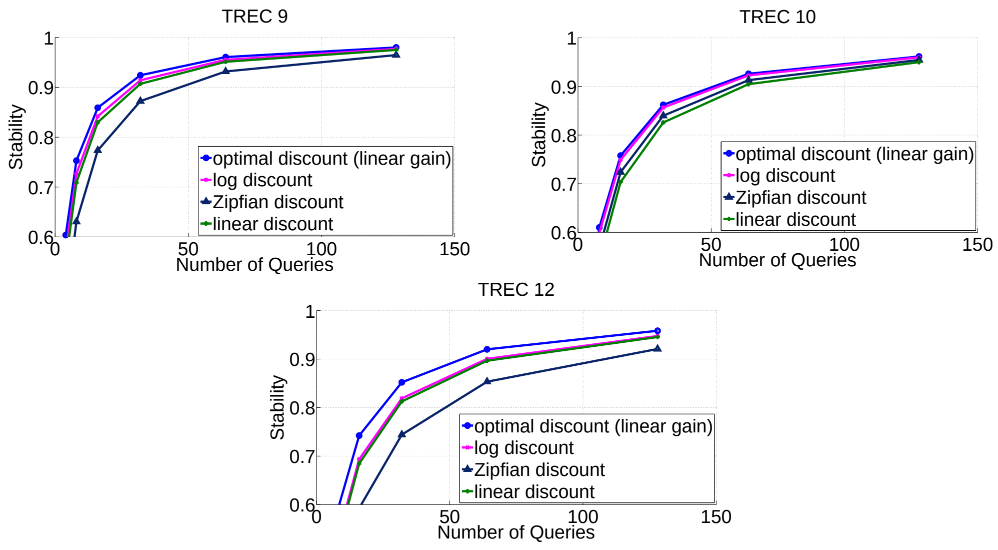


Figure 5.4: The stability of nDCG at rank 100 (or the fraction of the variance in the mean nDCG values due to actual performance of system) when nDCG is computed with the (a) optimal, (b) log, (c) Zipfian, and (d) linear discount function.

functions. The log discount function is the second most stable one while the Zipfian and the linear lead to the least stable nDCG measure.

Finally, in Table 5.1 we compare the efficiency of the nDCG when the optimal discount function is employed with the efficiency of nDCG when the log, the Zipfian and the linear discount functions are employed. To calculate the efficiency, we fit an ANOVA model into the resulting nDCG scores, for each one of the discount functions. Then, setting the value of Φ equal to 0.9, that is 90% of the total variance in the nDCG scores being due to the actual performance differences between systems, and using Equation B.2 we computed the necessary number of queries to reach the given stability. As expected the log discount function is the closest to the optimal one.

To conclude, the stability- (efficiency-) optimal discount function is less steep than any of the commonly used discount functions. The widely used log discount function is the one closest to the optimal discount function, while the Zipfian and the linear ones are the least stable. Furthermore, the optimal discount factors over low ranks are far from zero which suggests that looking further down at the ranked list of documents (regardless of the underline user search behavior and patience to step down the ranked list) can improve the reliability of system comparisons.

Table 5.1: Number of queries required to achieve 0.95 stability in evaluation.

$\Phi \geq 0.95$	Zipfian	linear	log	**Optimal**
TREC 9	45	31	29	**25**
TREC 10	58	64	51	**49**
TREC 12	104	70	67	**53**

Table 5.2: Ratio among the gain values of highly relevant and relevant documents for TREC 9, 10 and 12

TREC9	optimal ratio	optimal ratio	optimal ratio	hrel/rel	$\frac{2^{\text{hrel}}-1}{2^{\text{rel}}-1}$
	(unconstraint)	(log discount)	(optimal discount)		
nDCG@3	1.1	1.1	1.1	2	3
nDCG@10	1.3	1.3	1.4	2	3
nDCG@20	1.5	1.5	1.6	2	3
nDCG@100	1.2	1.2	1.9	2	3
nDCG@200	1.1	1.1	2.7	2	3

TREC10	optimal ratio	optimal ratio	optimal ratio	hrel/rel	$\frac{2^{\text{hrel}}-1}{2^{\text{rel}}-1}$
	(unconstraint)	(log discount)	(optimal discount)		
nDCG@3	1.2	1.2	1.3	2	3
nDCG@10	1.6	1.6	1.8	2	3
nDCG@20	2.0	2.0	2.0	2	3
nDCG@100	1.8	1.8	1.8	2	3
nDCG@200	1.5	1.5	1.6	2	3

TREC12	optimal ratio	optimal ratio	optimal ratio	hrel/rel	$\frac{2^{\text{hrel}}-1}{2^{\text{rel}}-1}$
	(unconstraint)	(log discount)	(optimal discount)		
nDCG@3	1.2	1.2	1.2	2	3
nDCG@10	1.2	1.2	1.1	2	3
nDCG@20	1.0	1.0	1.0	2	3
nDCG@100	0.8	1.0	1.0	2	3
nDCG@200	0.7	1.0	1.0	2	3

5.1.2.2 Optimal gain function

We also performed an optimization for the gain values assigned to the different relevance grades of documents. In this case, the discount factors were treated as constants. The log discount function, the closest to the stability- (efficiency-) optimal discount function, was utilized. Further, we set the gain value for the non-relevant equal to zero and optimized for the gain values of the relevant and highly relevant documents. We performed the optimization over TREC 9, 10 and 12 data sets for nDCG at ranks 3, 10, 20, 100 and 200. Instead of the gain values themselves, we report the ratio between the gain value assigned to the highly relevant documents and the gain value assigned to the relevant ones. The results can be viewed in Table 5.2. As in the case of the discount function, we performed both an unconstrained and a constrained optimization. In the constrained optimization we enforced the gain value of the highly relevant documents to be greater than or equal to the gain value of the relevant ones. The optimal gain value ratios for the unconstrained optimization are reported in the first column of Table 5.2, while the ones for the constrained optimization are reported in the second column. The last two columns show the ratio of the gain values when the linear and exponential gain functions are utilized.

By comparing the first two with the last two columns of the table one can observe that the utility of relevant documents in comparative evaluation of systems is underrated by the commonly employed gain functions. The optimal ratio of the gain values for highly relevant and relevant documents is in most of the cases much smaller than 2 or 3. Intuitively, this means that relevant documents are almost equally discriminative to the highly relevant ones. Good systems will retrieve both highly relevant and relevant documents while bad systems will have difficulties in retrieving either highly relevant or relevant documents. Thus, discriminating systems regarding their performance can be similarly done with either relevant or highly relevant documents. Note that this is true for the particular TREC collections under study and the systems run over these collections and it may not be true in the general case.

In the unconstrained optimization column, highly relevant documents still appear more discriminative than relevant documents for most of the cases. However, there are cases, e.g. in TREC 12 with nDCG computed at low ranks, that relevant documents appear to be more discriminative than highly relevant documents. An intuitive explanation of this behavior may be given by fact that the total number of highly relevant documents retrieved by systems in TREC 12 is quite small and highly relevant documents tend to appear at the very top of the ranked lists, while they are almost absent from the deeper ranks. Thus when deeper ranks are considered, highly relevant documents lose some of their discriminative power. The percentage of relevant and highly relevant documents on average (over all queries) at each rank for TREC 12 can be viewed in Figure 5.5.

Finally, for both TREC 9 and 10, one can observe a trend in the optimal ratio between the grades for relevant and highly relevant documents, with the ratios originally increasing by the rank nDCG is computed at and then dropping. This phenomenon needs to be further explored.

In Table 5.3 we compare the efficiency of the nDCG measure calculated at rank 100 when the optimal gain function is employed with the efficiency when the linear or the exponential gain functions are employed. As in the case of discount functions, to calculate the efficiency of each measure, we fit the ANOVA model into the resulting nDCG scores, for each one of the discount functions. Then, setting the value of Φ equal to 0.95, that is 95% of the total variance in the nDCG scores is due to the actual performance differences

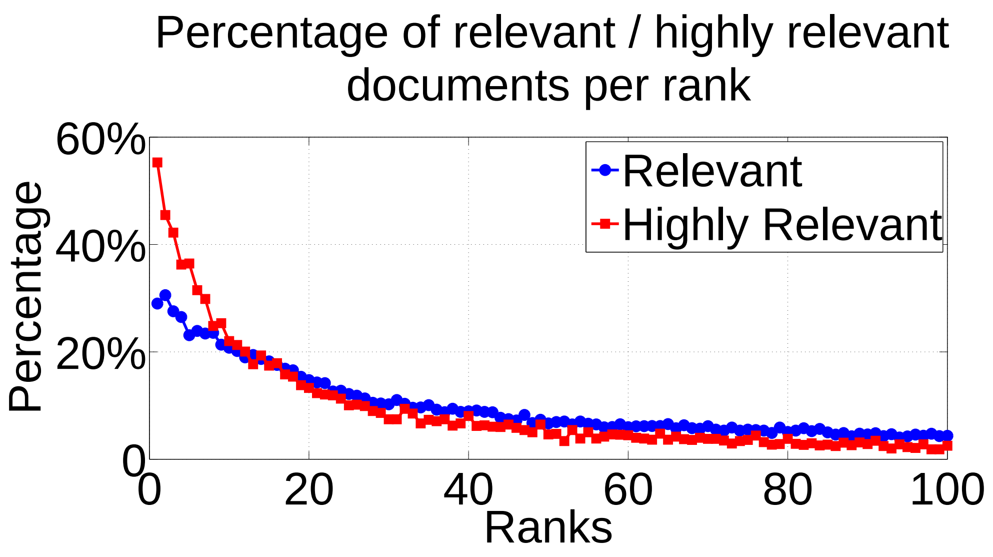


Figure 5.5: The percentage of documents that are relevant and the percentage of documents that are highly relevant on average (over all queries) at each rank for TREC 12.

between systems, and using Equation B.2 we compute the necessary number of queries to reach the given stability. Interestingly, the values in the table are almost identical for all gain functions for TREC 9 and 10, while only for TREC 12 the optimal gain is significantly better than the linear or the exponential ones in terms of efficiency.

Comparing Table 5.3 with Table 5.1 one can observe that the choice of the discount function affects much more the efficiency (stability) of the resulting nDCG measure than the choice of the gain function. As mentioned before, intuitively this means that at least in these particular collections when a system is good it retrieves both many highly relevant and many relevant documents, while when a system is bad it fails to retrieve either. Even though, this is true for the given test collections, this may not be the case for other test collections and in particular for collections with more than three relevance grades, where for instance retrieving enough marginally relevant documents may not necessarily mean that the system can also retrieve enough excellent documents (where excellent is more than 1 relevance grade away from marginally relevant). Unfortunately, currently we do not possess any such collection and thus we leave this as a future work.

Table 5.3: Number of queries required to achieve 0.95 stability in evaluation.

$\Phi \geq 0.95$	exp	linear	Optimal
TREC 9	30	29	**28**
TREC 10	52	51	**51**
TREC 12	72	67	**63**

5.1.2.3 Pareto-optimal gain and discount functions

Finally, we performed multi-objective optimization in order to optimize efficiency (stability) for both the gain and the discount functions simultaneously. To do so, we utilized the minimax MATLAB function, which produces the Pareto optimal discount and gain functions. That is, the discount and gain functions that maximize the worst case value of nDCG stability. We performed the optimization over TREC 9, 10 and 12, concluding that the Pareto optimal gain and discount functions are very close to the optimal gain and discount functions when the optimization is done independently for gains and discounts. The multi-objective optimal discount function for TREC 9 when nDCG is computed at rank 20 is shown in Figure 5.6. For comparison reasons, the optimal discount function when linear gain is used is also shown in the figure. As it can be observed in all cases the discount factors obtained from the multi-objective optimization are almost equal to the ones obtained with linear gains used. The multi-objective optimal ratio between highly relevant and relevant documents is reported in the third column of Table 5.2. As it can be observed, except for the case of TREC 9, when nDCG is computed at very low ranks, the multi-objective optimal ratio is very close to the one obtained with the log discount function. This may be an indication that gain and discount functions independently affect the stability of the measure. Similar plots are obtained for all TREC's and all ranks nDCG is computed at.

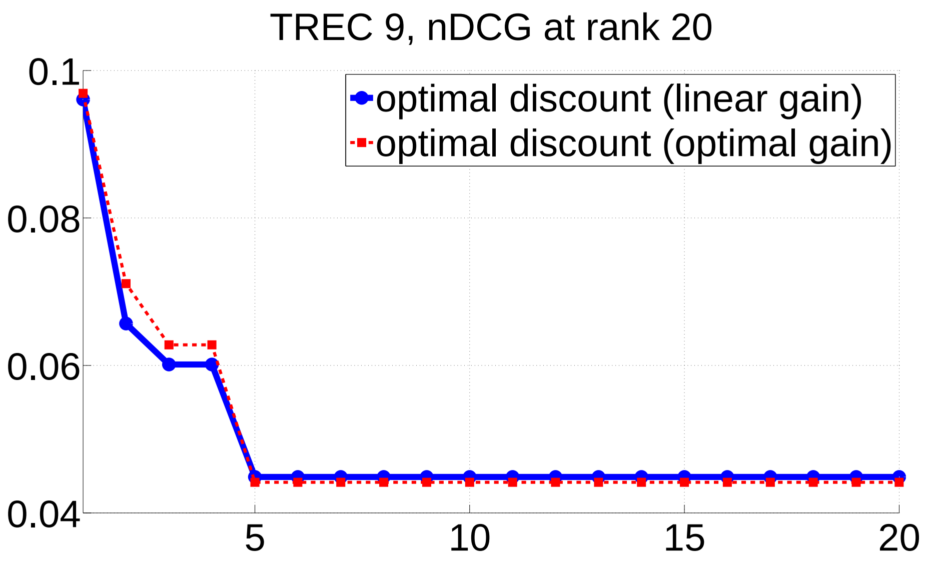


Figure 5.6: The multi-objective optimal discount function along with the optimal discount function when linear gains are used for TREC 9 and nDCG computed at rank 20.

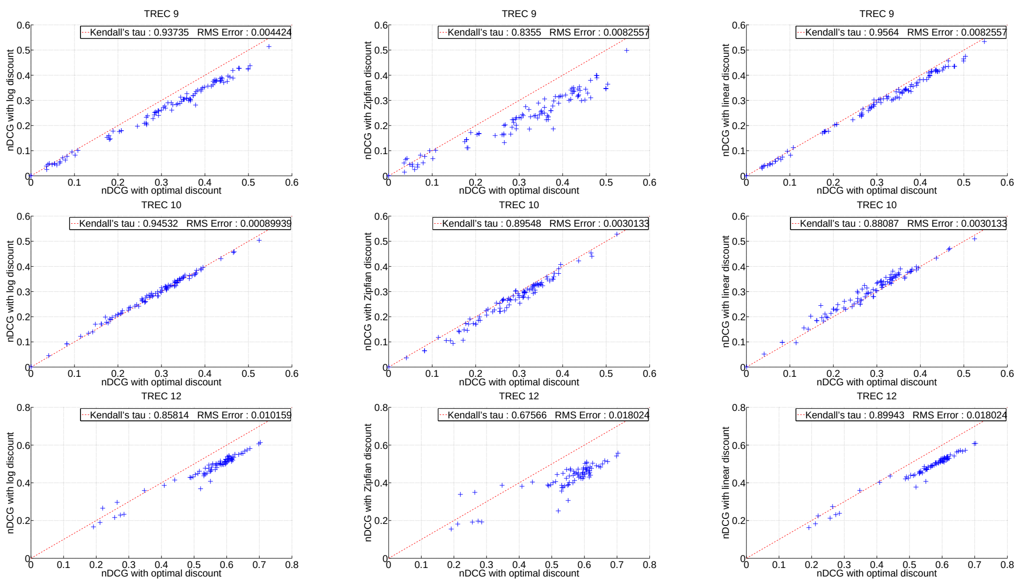


Figure 5.7: Scatter plots of the mean nDCG scores for the optimal discount function versus the log, Zipfian and linear discount function.

5.1.2.4 Correlation study

Different gain and discount functions employed in the calculation of nDCG may result in different mean nDCG values and therefore different rankings of the systems. To investigate how gain and discount functions affect the nDCG score and thus the induced ranking of systems, we calculated the mean nDCG at rank 100 for different gain and discount functions and computed the Kendall's τ between the induced rankings.

The scatter plots in Figure 5.7 illustrate the mean nDCG scores for the optimal discount function ($x-axes$) computed at rank 100 against the mean nDCG scores for the log, Zipfian and linear discount functions respectively ($y-axes$) for TREC 9, 10 and 12. The RMS Error and Kendall's τ are reported in the plots. The Kendall's τ between the rankings of systems induced by any two discount functions are also reported in Table 5.4.

By inspecting both the scatter plots in Figure 5.7 and the Kendall's τ values in Table 5.4 one can observe that both the rankings by the linear discount function and the rankings by the log discount function are very close to the rankings by the optimal discount function. As illustrated in Figure 5.2, these two discount functions are the closest to the optimal one. The rankings by the Zipfian discount function are widely different than the ones by the optimal discount function, especially in TREC 12.

This wide difference between the induced rankings by the optimal discount function and the Zipfian one can be explained by revisiting Figure 5.4. As it can be observed, for the Zipfian discount function, only 80% of the differences in the mean nDCG scores over a set of 50

Table 5.4: Kentall's τ

		$Zipfian$	$linear$	log	Optimal
TREC 9	$Zipfian$	1.0000	0.8315	0.8960	0.8355
	$linear$	0.8315	1.0000	0.9282	0.9564
	log	0.8960	0.9282	1.0000	0.9374
	Optimal	0.8355	0.9564	0.9374	1.0000
TREC 10	$Zipfian$	1.0000	0.7886	0.8625	0.8955
	$linear$	0.7886	1.0000	0.9184	0.8809
	log	0.8625	0.9184	1.0000	0.9453
	Optimal	0.8955	0.8809	0.9453	1.0000
TREC 12	$Zipfian$	1.0000	0.7136	0.8149	0.6757
	$linear$	0.7136	1.0000	0.8828	0.8994
	log	0.8149	0.8828	1.0000	0.8581
	Optimal	0.6500	0.8994	0.8581	1.0000

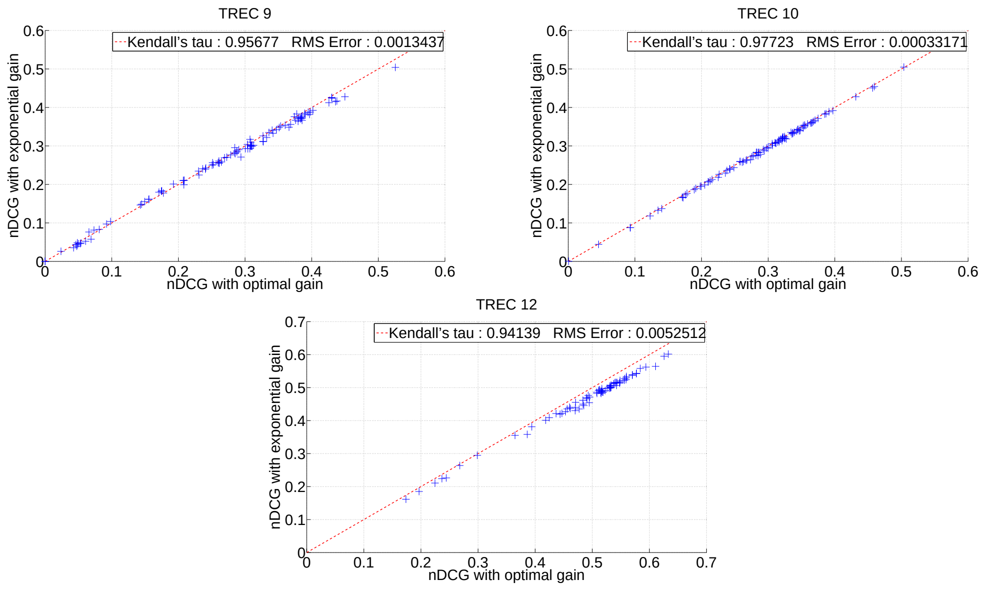


Figure 5.8: Scatter plots of the mean nDCG scores for the optimal gain function versus the exponential discount function.

queries (which is the case in all scatter plots here), is due to actual performance differences between the systems, while the corresponding percentage for the optimal discount function is about 90%. The corresponding percentages for TREC 9 (where the ranking of systems for the two discount functions are closer to each other) are 90% and 95% respectively, while for TREC 10 (where the ranking of systems are almost identical) the percentages are around 88% and 90%, respectively. Therefore, the ranking by the Zipfian discount in TREC 12 incorporates a lot of noise which is reduced in the case of TREC 9 and 10.

The scatter plots in Figure 5.8 illustrate the mean nDCG scores computed at rank 100 for the optimal gain function ($x - axes$) against the mean nDCG scores for the exponential gain function ($y - axes$) for TREC 9, 10 and 12. The RMS Error and Kendall's τ are also reported in the plots.

As it can be observed in Figure 5.8 the rankings by the optimal discount function are almost identical with the rankings by the exponential gain function. This is one more indication that for the particular test collections with the three grades of relevance the ratio between the gain values for relevant and the gain values for highly relevant documents does not affect the ranking of systems (at least for the ratio values examined in our studies, i.e. ratio values less than 3). What is particularly striking is that even for TREC 12, where the optimal gain function gives the exactly same gain value to both relevant and highly relevant, and thus essentially conflates the two relevance grades in one, the Kendall's τ between the rankings is 0.94, with the top 6-7 systems ranked in the exact same order by both gain functions. This states that good systems do equally good in retrieving relevant and highly relevant documents, while bad systems do equally bad in retrieving either relevant or highly relevant documents.

The corresponding scatter plots for the linear gain function look very similar to the ones in Figure 5.8 and for this reason they are not reported here.

5.1.2.5 Discriminative power

As mentioned before, intuitively, efficiency and stability seem to correlate well with discriminative power, since the variability in a measure that discriminates systems well will most probably be due to actual performance differences between systems. In this section we perform some basic experiments to test whether this hypothesis is correct.

Sakai [109] proposed a methodology to compare evaluation methods in terms of their ability to discriminate between systems based on *Bootstrap Hypothesis Tests*. According to his framework, all pairs of systems are considered and the hypothesis that their mean scores over a set of queries are the same is tested. To test this hypothesis Sakai [109] employs a bootstrap test, creating 1000 bootstrap samples. The achieved significance level (ASL), that is the significance level required to reject the zero hypothesis that two systems have the same mean score, is computed for each pair of systems. Finally, evaluation measures are compared in terms of ASLs. The smaller the ASLs a metric achieves the more discriminative the metric is.

To optimize for discriminative power, one would need to minimize the obtained ASLs while treating gain and discount function as unknowns. This is not a trivial optimization and it seems at least computationally inefficient. However, if stability (efficiency) is well

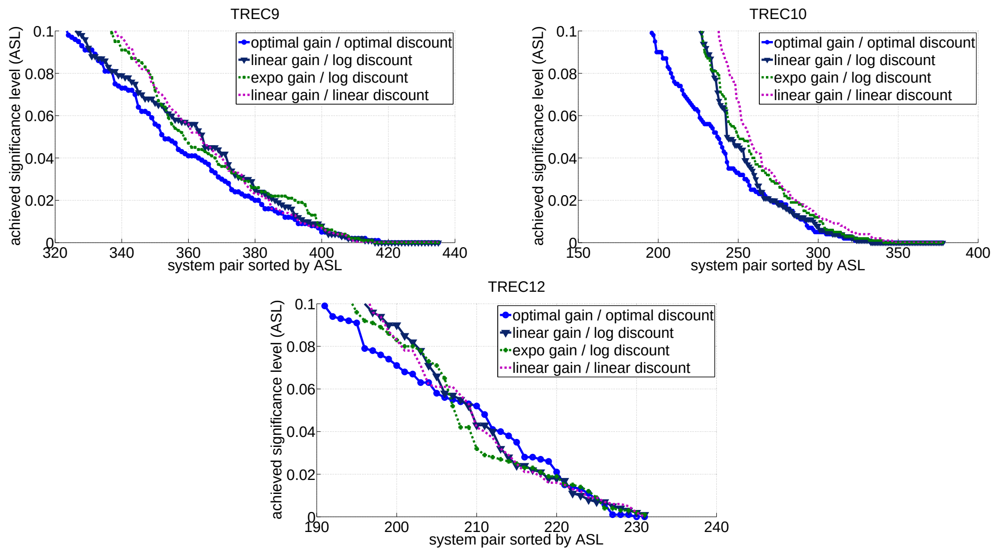


Figure 5.9: ASL curves for TREC 9, 10 and 12 with nDCG computed at rank 20.

correlated with discriminative power, then the stability-optimal nDCG will also demonstrate high discriminative power.

To test out thesis, we adopted the bootstrap hypothesis testing methodology, and compared 4 variations of nDCG, (a) nDCG with optimal gain and optimal discount, (b) nDCG with linear gain and log discount, (c) nDCG with exponential gain and log discount, and (d) nDCG with linear gain and linear discount. We followed the experimental setup in Sakai [109] and used only the top 30 systems from each data set (TREC 9, 10 and 12), since "near-zero" runs are unlikely to be useful for discussing the discriminative power of the measures. We considered all the remaining pairs of systems and for each one of the pairs we created 1000 bootstrap samples and calculated the achieved significance level (ASL) for all aforementioned nDCG measures. Figure 5.9 illustrates, for each one of the nDCG measures, the ASLs of systems pairs. The horizontal axis represents all system pairs sorted by ASL. The pairs of systems at the left of a given ASL level, are those that the measure cannot discriminate.

As it can be observed from the plots, when the stability- (efficiency-) optimal gain and discount functions are utilized nDCG outperforms all other variations with respect to discriminative power. The linear/exponential gain and log discount nDCG measures appear to be the next most discriminative ones, while the linear gain and linear discount nDCG appears to be the less discriminative one.

5.1.3 Conclusions

Despite the flexibility nDCG offers in the selection of the appropriate gain and discount function, so far this selection has been done rather arbitrarily, based on speculations of the search behavior of an average user and speculations of the correlation of the measure to user satisfaction. Recent work [1] has shown that the most commonly employed gain and discount functions are loosely related to user satisfaction which underlines the need for a more methodological selection of gain and discount function. However, given the infinite number of possible gain and discount functions, the vast differences in user search behavior, the many different possible retrieval tasks, a complete analysis of the different gain and discount functions with respect to the user satisfaction is prohibitively expensive, if at all possible.

In this work, we numerically computed a stability- or efficiency-optimal gain and discount function by treating gain values and discount factors as unknowns and optimizing for a stability/efficiency measure defined based on Generalizability theory. We compared the resulting gain function to both the linear and the exponential functions and the resulting discount function to the log, Zipfian and linear ones.

According to our results, the optimal discount function is less steep than all commonly used discount functions, giving reasonably high weights to lower ranks, while the relative difference between gain values is much smaller than the commonly used ones, giving almost equal weights to both relevant and highly relevant documents. The latter was rather striking, since weighting relevant and highly relevant documents equally did not seem to alter the ranking of systems. Note that this is true for the particular collections and systems under study and it may not reflect the general case.

Finally, we demonstrated that the stability- (efficiency-) optimal nDCG measure outperforms the dominant in the literature nDCG measure with respect to discriminative power as well.

5.2 Extension of Average Precision to Graded Relevance Judgments

Given that average precision remains one of the most popular metric of effectiveness, here we generalize average precision to the multigraded relevance case in a systematic manner, proposing a new metric, the *graded average precision* (GAP). The GAP metric is a direct extension of AP and thus it inherits all the desirable properties that average precision has:

- It has the same natural top-heavy bias average precision has.
- It has a nice probabilistic interpretation.
- It has an underlying theoretical basis as it corresponds to the area under the ”graded” precision-recall curve.
- It can be justified in terms of a simple but moderately plausible user model similarly to AP
- It appears to be highly informative.
- When used as an objective function in learning-to-rank it results in good performance retrieval systems (it outperforms both AP and nDCG).

The incorporation of multi-graded relevance in average precision becomes possible via a simple probabilistic user model which naturally dictates to what extend documents of different relevance grades account for the effectiveness score. This user model corresponds to one of the approaches briefly discussed in Sakai and Robertson [112]. This model offers an alternative way of thinking about graded relevance compared to the notion of utility employed by nDCG and other multi-graded metrics.

Sakai [111] introduced the Q-measure as a better mechanism to control the penalty to late arrivals of relevant documents than nDCG. It has been shown that for ranks above R Q-measure behaves similarly to AP (where R is the number of relevant documents in the collection). Nevertheless, the incorporation of graded relevance by the Q-measure follows the same model with nDCG. GAP, on the other hand, is based on the well-trusted notions of precision and recall as is AP. Thus, we propose GAP as a complement to the existing multi-graded metrics.

In this work we do not aim at proposing a family of measures that accommodate different user behavior, as is nDCG (even though GAP offers some flexibility). Instead, by generalizing average precision we propose a more system-oriented measure, that apart from comparing the overall performance of retrieval systems in a similar to average precision way, it can be utilized in the critical task of learning to rank and improve the performance of the resulting ranking functions.

In what follows, we first describe the user model on which GAP is based and define the new metric. We then describe some desirable properties GAP possesses. In particular, we describe a probabilistic interpretation of GAP, generalize precision-recall curves for the multigraded relevance case and show that GAP is an approximation to the area under the graded precision-recall curves. Further, we evaluate GAP in terms of informativeness [10] and discriminative power [109]. Finally, we extend two popular LTR algorithms, SoftRank [129] and LambdaRank [27], to optimize for GAP and test the performance of the resulting ranking functions over different collections.

5.2.1 Graded Average Precision (GAP)

5.2.1.1 User Model

We start from a rudimentary user model, as follows: assume that the user actually has a binary view of relevance, determined by thresholding the relevance scale $\{0..c\}$. We describe this model probabilistically – we have a probability g_i that the user sets the threshold at grade i, in other words regards grades $i, ..., c$ as relevant and the others as non-relevant. We consider this probability to be defined over the space of users. These should be exclusive and exhaustive probabilities: $\sum_{j=1}^{c} g_j = 1$.

5.2.1.2 Definition of GAP

Now, we want some form of expected average precision, the expectation being over this afore-defined probabilistic event space. Simple interpretation of this (just calculate average precision separately for each grade and take a probabilistically weighted combination) has problems; for instance, in the case of an ideal ranked list, when there are no documents in some grades, the effectiveness score returned is less than the optimal value of 1. So, instead, we extend the non-interpolated form of AP; that is, we step down the ranked list, looking at each relevant document in turn (the ”pivot” document) and compute the expected precision at this rank. With an appropriate normalization at the end, this defines the graded average precision (GAP).

In particular, suppose we have a ranked list of documents, and document d_n at rank n has relevance $i_n \in \{0..c\}$. If $i_n > 0$, d_n, as pivot document, will contribute a precision value to the average precision calculations for each grade j, $0 < j \leq i_n$, since for any threshold set at grades less than or equal to i_n, d_n is considered relevant. The binary precision value for each grade j is, $\frac{1}{n}(|d_m : m \leq n, i_m \geq j|)$, while the expected precision at rank n over the aforementioned probabilistic user space can be computed as,

$$E[PC_n] = \sum_{j=1}^{i_n} \left(\frac{1}{n} |d_m : m \leq n, i_m \geq j| \right) \cdot g_j$$

Let $I(i,j)$ be an indicator variable equal to 1 if grade i is larger than or equal to grade j and 0 otherwise. Then, the expected precision at rank n can also be written as,

$$\begin{aligned} E[PC_n] &= \sum_{j=1}^{i_n} \left(\frac{1}{n} |d_m : m \leq n, i_m \geq j| \right) \cdot g_j \\ &= \frac{1}{n} \sum_{j=1}^{i_n} g_j \sum_{m=1}^{n} I(i_m, j) \\ &= \frac{1}{n} \sum_{m=1}^{n} \sum_{j=1}^{min(i_n, i_m)} g_j \quad \text{if } i_m > 0 \end{aligned}$$

By observing the new form of calculation of $E[PC_n]$, we can compute the contribution of each document ranked at $m \leq n$ to this weighted sum for those grades $j \leq i_m$. Thus we define a contribution function:

$$\delta_{m,n} = \begin{cases} \sum_{j=1}^{min(i_m, i_n)} g_j & \text{if } i_m > 0 \\ 0 & \text{otherwise} \end{cases}$$

Now the contribution from the pivot document can be defined as, $E[PC_n] = \frac{1}{n} \sum_{m=1}^{n} \delta_{m,n}$.

The maximum possible $E[PC_n]$ depends on the relevance grade i_n, it is the probability that this document is regarded as relevant by the user, $\sum_{j=1}^{i_n} g_j$. We must take account of this when normalizing the sum of $E[PC_n]$'s. Suppose we have R_i total documents in grade i (for this query); then the maximum possible value of cumulated $E[PC_n]$'s is, $\sum_{i=1}^{c} R_i \sum_{j=1}^{i} g_i$, which corresponds to the expected number of documents considered relevant in the collection, with the expectation taken over the space of users, as above.

The graded average precision (GAP) is then defined as:

$$GAP = \frac{\sum_{n=1}^{\infty} \frac{1}{n} \sum_{m=1}^{n} \delta_{m,n}}{\sum_{i=1}^{c} R_i \sum_{j=1}^{i} g_i}$$

The user model that GAP is based on dictates the contribution of different relevance grades to the GAP calculation by considering the probability of a user thresholding the relevance scale at a certain relevance grade (the g values). This allows a better understanding and an easier mechanism to determine the relative value of different relevance grades to an average user than the underlying model for the current multi-graded evaluation metrics. For instance, given the relevance grades of documents, click through data can be utilized to conclude relative preferences of users among documents of different relevance grades [72, 84]. Assuming that the user only clicks on the documents he finds relevant, the g values correspond to the probability that a user clicks on a document of a particular relevance grade, given all the documents clicked by the user.

In this work, given that our goal is to develop a good system-oriented metric, we propose an alternative way of setting the g values by considering which $g = \{g_i\}$ makes the metric most informative 5.2.3.1.

5.2.2 Properties of GAP

In this section, we describe some of the properties of GAP and show that GAP inherits all the nice properties of average precision (extended to the multi-graded case) that make the metric understandable and desirable to use.

First, it is easy to see that GAP generalizes average precision – it reverts to average precision in the case of binary relevance. With respect to the model described in Section 5.2.1.1, binary relevance means that all users find documents with some relevance grade $t > 0$ relevant and the rest non-relevant (i.e., $g_j = 1$ if $j = t$, for some relevance grade $t > 0$ and 0 otherwise).

Furthermore, GAP behaves in the expected way under document swaps. That is, if a document is swapped with another document of smaller relevance grade that appears lower in the list, the value of GAP decreases and vice-versa. As a corollary to this property, GAP acquires its maximum value when documents are returned in non-increasing relevance grade order.

Finally, GAP possesses all the desirable properties of average precision:

- It has the natural top-heavy bias of average precision and so it does not require explicit discount function.
- It has a nice probabilistic interpretation (Section 5.2.2.1).
- It approximates the area under a ’graded’ precision-recall curve (Section 5.2.2.2).
- It is highly informative (Section 5.2.3).

In the following sections, we describe a probabilistic interpretation of GAP and show that GAP is an approximation to the area under a graded precision-recall curve.

5.2.2.1 Probabilistic interpretation

In this section, we describe the probabilistic interpretation of GAP. We show that GAP can be seen as a probability value, which makes the measure more intuitive. In particular, we define GAP as the expected outcome of a random experiment, which is a generalization of the random experiment whose expected outcome is average precision [146], for the case of graded relevance.

Probabilistic interpretation of AP: Recently, Yilmaz and Aslam [146] showed that AP corresponds to the expected outcome of the following random experiment:

1. Select a relevant document at random. Let the rank of this document be n.
2. Select a document at or above rank n, at random. Let the rank of that document be m.
3. Output 1 if the document at rank m, d_m, is relevant.

In expectation, steps (2) and (3) effectively compute the precision at a relevant document, and in combination, step (1) effectively computes the average of these precisions. Hence, average precision corresponds to the probability that a document retrieved above a randomly picked relevant document is also relevant.

Probabilistic interpretation of GAP: Consider the case where graded relevance judgments are available. We claim that GAP corresponds to the expected outcome of the following random experiment:

1. Select a document that is considered relevant by a user (according to the afore-defined user model), at random. Let the rank of this document be n.
2. Select a document at or above rank n, at random. Let the rank of that document be m.
3. Output 1 if the document at rank m, d_m, is also considered relevant by the user.

Hence, GAP can be seen as the probability that a document retrieved above a randomly picked 'relevant' document is also 'relevant', where relevance is defined according to the user model previously described.

Consider computing the expectation of the above random experiment to show that it corresponds to GAP.

In expectation, step (3) corresponds to the conditional probability of document d_m being considered as relevant given that document d_n is also considered as relevant. To calculate this probability, let's consider all possible cases of the relative ordering of the relevant grades for documents d_n and d_m.

- **($i_n \leq i_m$) :** Since the relevance grade of d_n is smaller than or equal to the one for d_m, if d_n is considered relevant then d_m will also be considered as relevant.

$$\begin{aligned} & Pr(d_m = \text{rel}|d_n = \text{rel}) = \\ & = 1 = \frac{\sum_{j=1}^{i_n} g_j}{\sum_{j=1}^{i_n} g_j} = \frac{\sum_{j=1}^{min(i_n,i_m)} g_j}{\sum_{j=1}^{i_n} g_j} \end{aligned}$$

since $min(i_n, i_m) = i_n$.

- **($i_n > i_m$) :** By applying the Bayes' Theorem,

$$\begin{aligned} & Pr(d_m = \text{rel}|d_n = \text{rel}) = \\ = & \frac{Pr(d_n = \text{rel}|d_m = \text{rel}) \cdot Pr(d_m = \text{rel})}{Pr(d_n = \text{rel})} \\ = & \frac{1 \cdot \sum_{j=1}^{i_m} g_j}{\sum_{j=1}^{i_n} g_j} = \frac{\sum_{j=1}^{min(i_n,i_m)} g_j}{\sum_{j=1}^{i_n} g_j} \end{aligned}$$

since $min(i_n, i_m) = i_m$

In expectation, steps (2) and (3) together, correspond to the value the 'pivot' document d_n will contribute to GAP,

$$\frac{1}{n} \cdot \sum_{m=1}^{n} \frac{\sum_{j=1}^{min(i_n,i_m)} g_j}{\sum_{j=1}^{i_n} g_j}$$

In step (1), the probability that a document d_n is considered relevant is $\sum_{j=1}^{i_n} g_j$. Thus, the probability of selecting this document out of all documents that are considered relevant is,

$$p_{d_n} = \frac{\sum_{j=1}^{i_n} g_j}{\sum_{i=1}^{c} R_i \sum_{j=1}^{i_n} g_j}$$

Therefore, step (1) in combination with steps (2) and (3) effectively computes the average of the contributed values, which corresponds to GAP,

$$\begin{aligned} GAP &= \sum_{n=1}^{\infty} \frac{1}{n} \sum_{m=1}^{n} \frac{\sum_{j=1}^{min(i_n,i_m)} g_j}{\sum_{j=1}^{i_n} g_j} \cdot \frac{\sum_{j=1}^{i_n} g_j}{\sum_{i=1}^{c} R_i \cdot \sum_{j=1}^{i_n} g_j} \\ &= \frac{\sum_{n=1}^{\infty} \frac{1}{n} \sum_{m=1}^{n} \sum_{j=1}^{min(i_n,i_m)} g_j}{\sum_{i=1}^{c} R_i \sum_{j=1}^{i} g_i} \end{aligned}$$

Note that based on the user model described in Section 5.2.1.1, an alternative random experiment could be described by selecting a user at random in the first step. Selecting a user at step (1), defines the threshold at the relevance scale. Given this threshold, in expectation, the following steps effectively compute average precision [146], and hence, in combination, this random experiment computes a weighted combination of average precisions for the different thresholds. As mentioned in Section 5.2.1.2, however, such a weighted combination of average precisions has certain problems.

5.2.2.2 GAP as the area under the graded precision-recall curves

In this section we first intuitively extend recall and precision to the case of multi-graded relevance, based on the probabilistic model defined in Section 5.2.1.1. Then we define the graded precision-recall curves, and finally show that GAP approximates the area under the graded precision-recall curves, as AP approximates the area under the binary precision-recall curves.

Precision-recall curves are constructed by plotting precision against recall each time a relevant document is retrieved. In the binary relevance case, recall is defined as the ratio of relevant documents up to rank n to the total number of relevant documents in the query. In the graded relevance case, a document is considered relevant only with some probability. Therefore, recall at a relevant document at rank n can be defined as the ratio of the expected number of relevant documents up to rank n to the expected total number of relevant documents in the query (under the independence assumption between numerator and denominator).

In particular, according to the user model defined in Section 5.2.1.1, documents of relevance grade i_m are considered relevant with probability $\sum_{j=1}^{i_m} g_j$, and thus, the expected number of relevant documents up to rank n is, $\sum_{m=1}^{n} \sum_{j=1}^{i_m} g_j$, while the expected total number of relevant document is, $\sum_{i=1}^{c} R_i \sum_{j=1}^{i} g_j$.

Hence, the graded recall at rank n can be computed as,

$$\text{graded Recall@n} = \frac{\sum_{m=1}^{n} \sum_{j=1}^{i_m} g_j}{\sum_{i=1}^{c} R_i \sum_{j=1}^{i} g_j}$$

The recall step, i.e. the proportion of relevance information acquired when encountering a 'relevant' document at rank n to the total amount of relevance, is, $\sum_{j=1}^{i_n} g_j / \sum_{i=1}^{c} R_i \sum_{j=1}^{i} g_j$. This corresponds to the expected outcome of step (1) of the random experiment described in Section 5.2.2.1 and expresses the probability of selecting a 'relevant' document at rank n out of all possible 'relevant' documents.

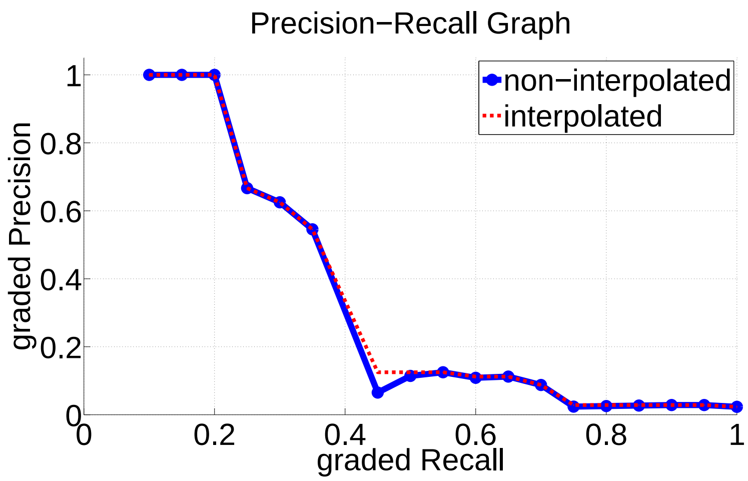


Figure 5.10: Graded relevance precision-recall curve.

In the binary case, precision at a relevant document at rank n is defined as the fraction of relevant documents up to that rank. In the multi-graded case, precision at a 'relevant' document at rank n can be defined as the expected number of documents at or above that rank that are also considered as 'relevant' This quantity corresponds to the expected outcome of steps (2) and (3) of the random experiment in Section 5.2.2.1,

$$\text{graded Precision@n} = \frac{1}{n} \cdot \sum_{m=1}^{n} \frac{\sum_{j=1}^{min(i_n,i_m)} g_j}{\sum_{j=1}^{i_n} g_j}$$

Therefore, graded average precision can be alternatively defined as the cumulated product of graded precision values and graded recall step values at documents of positive relevance grade, as average precision can be defined as the cumulated product of precision values and recall step values at relevant documents.

Given the definitions of graded precision and graded recall, one can construct precision-recall curves. Figure 5.10 illustrates an example of a graded precision-recall curve for a system/query pair from TREC 10 data, with the blue solid line corresponding to the non-interpolated curve and the red dashed line corresponding to the interpolated one. Now it is easy to see that GAP is an approximation to the area under the non-interpolated graded precision-recall curve as AP is an approximation to the area under the non-interpolated binary precision-recall curve.

Note that Kekäläinen and Järvelin [77] have also proposed a generalization of precision and recall. The way they generalized the two statistics is radically different than the one we propose; in their work precision and recall follow the nDCG framework where gain values are assigned to each document.

5.2.3 Evaluation Methodology and Results

There are two important properties that an evaluation metric should have: (1) it should be highly informative [10] – it should summarize the quality of a search engine well, and (2) it should be highly discriminative – it should identify the significant differences in the performance of the systems. We evaluated GAP in terms of both of these properties. Further, we used nDCG as a baseline for comparison purposes. Given that our goal is to propose a good system-oriented metric that can be used as an objective function to optimize for in LTR, in what follows we mostly focus on the informativeness of the metric since it has been shown to correlate well with the effectiveness of the trained ranking function [149].

In particular, when a ranking function is optimized for an objective evaluation metric, the evaluation metric used during training acts as a bottleneck that summarizes the available training data. At each training epoch, given the relevance of the documents in the training set and the ranked list of documents retrieved by the ranking function for that epoch, the only information the learning algorithm has access to is the value of the evaluation metric. Thus, the ranking function will change on the basis of the change in the value of the metric. Since, more informative metrics better summarize the relevance of the documents in the ranked list and thus better capture any change in the ranking of documents, the informativeness of a metric is intuitively correlated with the ability of the LTR algorithm to ”learn” well.

5.2.3.1 Informativeness

We use the *maximum entropy framework* by Aslam et al. [10] in order to analyze the informativeness of the evaluation metrics.

The maximum entropy framework is based on the assumption that the quality of a list of documents retrieved in response to a given query is strictly a function of the relevance of the documents retrieved within that list. The question that naturally arises is how well does a metric capture the relevance information of the output list and consequently the effectiveness of a retrieval system? In other words, given the value of a metric, for a particular system on a particular query, how accurately can one predict the relevance of documents retrieved by the system. In order to compute how well a metric predicts the relevance of the returned documents, Aslam et al. [10] form a distribution over all possible sequences of relevance and find the maximum entropy distribution over these lists given the value of the metric. Using the maximum entropy distribution, they guarantee that they use no extra information other than the value of the given metric. Thus, using only the information given by the metric, they form a distribution over the sequence of relevant/nonrelevant docu-

Maximize: $H(\vec{p}) = \sum_{n=1}^{N} H(p_n)$

Subject to:

$$1.\quad \sum_{n=1}^{N}\sum_{\xi=0}^{c} \frac{Pr(i_n = \xi)}{n} \cdot \left(\sum_{j=1}^{\xi} g_j + \sum_{m=1}^{n-1}\sum_{\zeta=0}^{c} \left(\left(\sum_{j=1}^{min(\zeta,\xi)} g_j \right) Pr(i_m = \zeta) \right) \right) / \left(\sum_{i=1}^{c} R_i \sum_{j=1}^{i} g_i \right) = gap$$

$$2.\quad \sum_{n=1}^{N} Pr(i_n = \xi) = R_\xi \qquad \forall \xi : 1 \leq \xi \leq c$$

$$3.\quad \sum_{\xi=0}^{c} Pr(i_n = \xi) = 1 \qquad \forall n : 1 \leq n \leq N$$

Maximize: $H(\vec{p}) = \sum_{n=1}^{N} H(p_n)$

Subject to:

$$1.\quad \sum_{n=1}^{N}\sum_{\xi=0}^{c} \frac{(e^{g(\xi)} - 1) \cdot Pr(i_n = \xi)}{lg(n+1)} / \,(\text{optDCG}) = ndcg$$

$$2.\quad \sum_{n=1}^{N} Pr(i_n = \xi) = R_\xi \qquad \forall \xi : 1 \leq \xi \leq c$$

$$3.\quad \sum_{\xi=0}^{c} Pr(i_n = \xi) = 1 \qquad \forall n : 1 \leq n \leq N$$

Figure 5.11: Maximum entropy setup for GAP and nDCG, respectively.

ments of length N. Assuming that the distribution over lists is a product distribution, i.e. the probability of a particular sequence of relevance is the product of the probability of different relevance grades at each rank, they infer the probability of relevance of a document at a particular rank (probability-at-rank distribution) given the value of a metric.

Since given a precision-recall curve along with the total number of relevant documents one can exactly reconstruct the returned ranked list of documents that corresponds to the given precision-recall curve, to quantify how informative a metric, as in Aslam et al. [10], we used the following criterion: how do the precision-recall curves obtained from the inferred probability-at-rank distribution compare with the actual precision-recall curves of the system?

To utilize the maximum entropy framework we first derive the expected GAP and nDCG over the probability-at-rank distribution. The maximum entropy formulations for GAP and nDCG are shown in Figure 5.11. Both of these formulations are constraint optimization problems and numerical methods (MATLAB) were used to determine their solutions.

We then test the performance of GAP and nDCG using data from TRECs 9, 10 and 12.

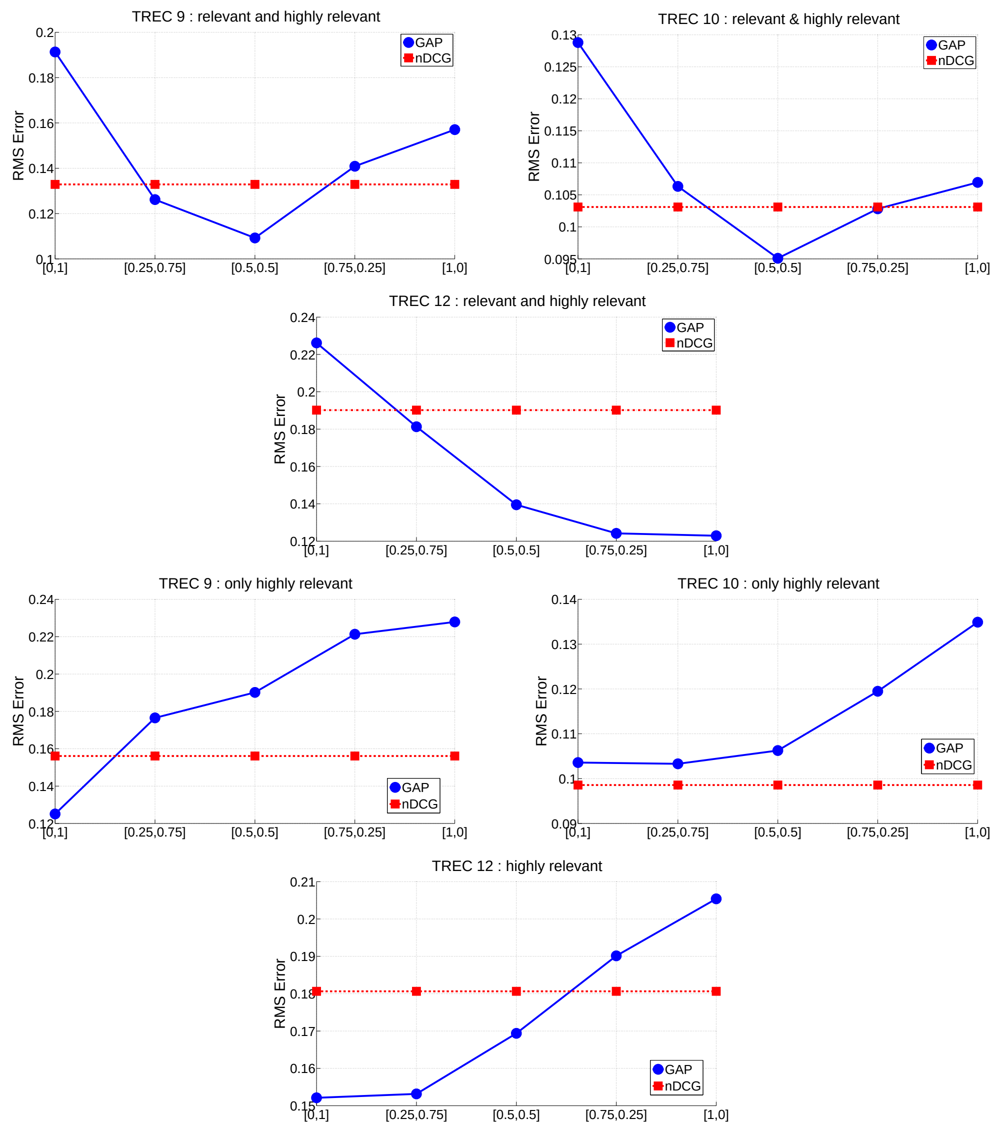


Figure 5.12: Mean RMS error between inferred and actual PR curves when only highly relevant documents are considered as relevant and when both relevant and highly relevant documents are considered as relevant.

Using the setup described above, we first infer the probability-at-rank distributions given the value of each metric and then calculate the maximum entropy precision-recall curves when only highly relevant documents are considered as relevant and when both relevant and highly relevant documents are considered as relevant (the graded PR-curves described in Section 5.2.2.2 are not utilized due to their bias towards GAP). As in Aslam et al. [10], for any query, we choose those systems that retrieved at least 5 relevant and 5 highly relevant documents to have a sufficient number of points on the precision-recall curves. We use different values for g_1 and g_2 to investigate their effect on the informativeness of GAP.

The mean RMS error between the inferred and the actual precision-recall curves, calculated at the points where recall changes, is illustrated in Figure 5.12. The x-axis corresponds to different pairs of threshold probabilities, g_1 and g_2. The blue solid line corresponds to the RMS error between the actual and the inferred precision-recall curves subject to GAP, while the red dashed line indicates the RMS error of the inferred precision-recall curves subject to nDCG.

As it can be observed (1) the choice of g_1 and g_2 appears to affect the informativeness of GAP; when g_1 is high GAP appears to summarize well the sequence of all relevant documents independently of their grade, while when g_2 is high GAP appears to summarize well the sequence of all highly relevant documents, (2) choosing g_1 and g_2 to be relatively balanced (around 0.5) seems to be the best compromise between summarizing well the sequence of all relevant documents independent of their grade and highly relevant documents only, and (3) with g_1 and g_2 to relatively balanced GAP appears to be more informative than nDCG in most of the cases[3].

Finally, note that when the thresholding probability $g_1 = 1$ (the right-most point for GAP curve in all plots), GAP reduces to average precision since relevant and highly relevant documents are conflated in a single grade. Therefore, one can compare the informativeness of GAP with the informativeness of AP by comparing the right-most point on the GAP curve with any other point on the same curve. For instance one can compare GAP with equal thresholding probabilities ($g_1 = g_2 = 0.5$) with AP by comparing the point on the blue line that corresponds to the [0.5,0.5] on the x-axis with the point on the blue line that corresponds to the [1,0] on the x-axis. This way we can test whether graded relevance add any value in the informativeness of the metric on the top of binary relevance. What is striking about Figure 5.12 is that in TREC 9 and 10 GAP (with $g_1 = g_2 = 0.5$) appears more informative than AP when relevant and highly relevant documents are combined (top row plots). That is, the ability to capture the sequence of relevance regardless the relevance grade is benefited by differentiating between relevant and highly relevant documents.

5.2.3.2 Discriminative Power

A number of researchers have proposed the evaluation of effectiveness metrics based on their discriminative power. That is, given a fixed set of queries, which evaluation metric can better identify significant differences in the performance of systems? The same question

[3]Different gain (linear vs. exponential) and discount (linear vs. log) functions used in the definition of nDCG were tested. The ones that utilized the log discount function appeared to be the most informative, while the effect of the gain function on informativeness was limitted. The nDCG metric used here utilizes an exponential gain and a log discount function.

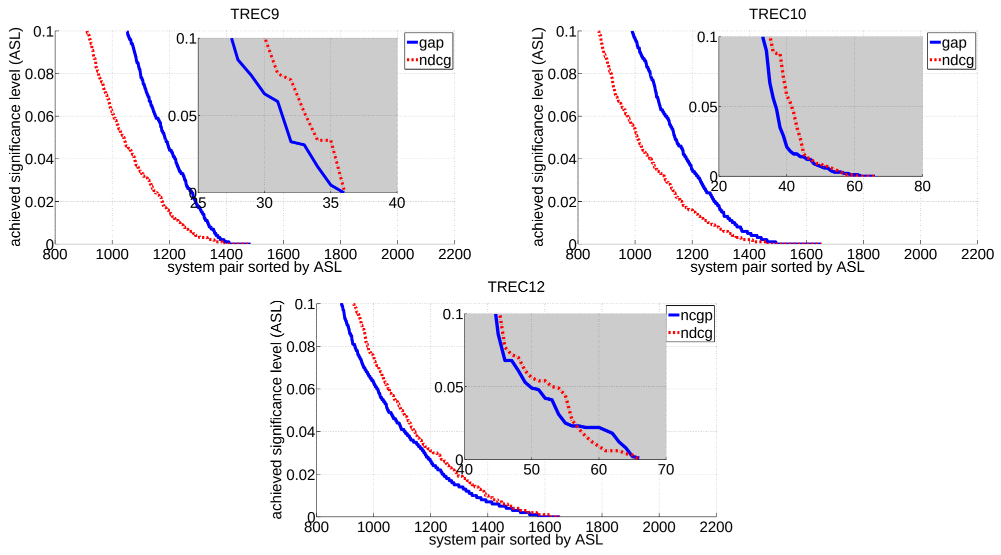


Figure 5.13: ASL curves based on bootstrap hypothesis tests for TREC 9, 10 and 12.

can be posed in an alternative way: what is the necessary topic set size to guarantee a given degree of consistency in the performance comparisons over different topic sets?

In our experiments we adapt the bootstrap hypothesis testing methodology described in Section 5.1.2.5. We use data from TREC 9, 10 and 12. We follow the same experimental setup as in Sakai [109] by dropping the bottom 30% of the runs from each data set, since "near-zero" runs are unlikely to be useful for discussing the discriminative power of the measures. We consider all the remaining pairs of systems and for each one of the pairs we created 1000 bootstrap samples and calculated the achieved significance level (ASL) for each systems pair for GAP and nDCG. The thresholding probabilities for GAP were both set to 0.5. Figure 5.13 illustrates, for each one of the evaluation metrics, the ASLs of systems pairs. The horizontal axis represents all system pairs sorted by ASL. One can observe that GAP outperforms nDCG with respect to discriminative power in TREC 12, while nDCG outperforms GAP in TREC's 9 and 10. We repeat the same experiments for the intersection of the top 15 systems according to the two metrics to test our original hypothesis that GAP is more discriminative among the best systems. The results are illustrated in the sub-plots inside the plots of Figure 5.13. As it can be observed, GAP outperforms nDCG in all TREC data sets, which confirms our original hypothesis.

Note that discriminative power cannot be solely used to compare evaluation metrics. An evaluation metric may exhibit great discriminative power but capture no information regarding the performance of a system. An extreme example is an evaluation metric that outputs a unique identification number for each retrieval system. Such a metric exhibits absolute discriminative power, but captures no information regarding the quality of the retrieval system.

5.2.4 GAP for Learning to Rank

Finally, we employed GAP as an objective function to optimize for in the context of LTR. For comparison purposes we also optimized for AP and nDCG. In our experiments we employed two different learning algorithms, (a) SoftRank [129] and (b) LambdaRank [27] over two different data sets, (a) a Web collection with 5K queries and 382 features taken from a commercial search engine, and (b) the OHSUMED collection provided by LETOR [128]. The relevance judgments in the both data set are in a 3 grade scale (non-relevant, relevant and highly relevant). Five-fold cross validation was used in the case of OHSUMED collection.

Since the informativeness of the metric is well correlated with the effectiveness of the constructed ranking function, we select g_1 and g_2 based on the criterion of informativeness. As we observed in Section 5.2.3.1, the values of g_i that result in the most informative GAP variation is $g_1 = g_2 = 0.5$. Intuitively, these values of g_i indicate that highly relevant documents are "twice as important as relevant documents.

LTR algorithms: *SoftRank* [129] is a neural network based algorithm that is designed to directly optimize for nDCG, as most other learning to rank algorithms. Since most IR metrics are non-smooth as as they depend on the ranks of documents, the main idea used in SoftRank to overcome the problem of optimizing non-smooth IR metrics is based on defining smooth versions of information retrieval metrics by assuming that the score s_j of each document j is a value generated according to a Gaussian distribution with mean equal to s_j and shared smoothing variance σ_s. Based on this, Taylor et al. [129] define π_{ij} as the probability that document i will be ranked higher than document j. This distribution can then be used to define *smooth* versions of IR metrics as expectations over these rank distributions.

Based on these definitions, we extend SoftRank to optimize for GAP by defining SoftGAP, the *expected* value of Graded Average Precision with respect to these distributions and compute the gradient of SoftGAP.

Given the probabilistic interpretation of GAP defined earlier and the distribution π_{ij}, the probability that document i will be ranked higher than document j, SoftGAP can be computed as follows:

Let PC_n be:

$$PC_n = \frac{\sum_{j=1}^{i_n} g_j + \sum_{m=1}^{N} \pi_{mn} \sum_{j=1}^{min(i_m,i_n)} g_j}{\sum_{m=1,m\neq n}^{N} \pi_{mn} + 1}$$

then,

		Test Metric		
		nDCG	AP	PC(10)
SoftRank	Opt nDCG	0.6162	0.6084	0.5329
	Opt GAP	**0.6290**	**0.6276**	**0.5478**
	Opt AP	0.6129	0.6195	0.5421
LambdaRank	Opt nDCG	0.6301	0.6158	0.5355
	Opt GAP	**0.6363**	**0.6287**	**0.5388**
	Opt AP	0.6296	0.6217	0.5360

Table 5.5: Test set performance for different metrics when SoftRank and LambdaRank are trained for nDCG, GAP, and AP as the objective over 5K Web Queries from a commercial search engine.

		Test Metric		
		nDCG	AP	PC(10)
SoftRank	Opt nDCG	0.4665	0.4452	0.4986
	Opt GAP	**0.4747**	**0.4478**	**0.5001**
	Opt AP	0.4601	0.4448	0.4900
LambdaRank	Opt nDCG	0.4585	0.4397	0.5005
	Opt GAP	**0.4665**	**0.4432**	**0.5042**
	Opt AP	0.4528	0.4408	0.4881

Table 5.6: Test set performance for different metrics when SoftRank and LambdaRank are trained for nDCG, GAP, and AP as the objective over the OSHUMED data set.

$$SoftGAP = \sum_{n=1}^{N} \frac{PC_n}{\sum_{i=1}^{c} R_i \sum_{j=1}^{i} g_i}$$

Optimizing for an evaluation metric using neural networks and gradient ascent requires computing the gradient of the objective metric with respect to the score of an individual document $\bar{s_m}$. To compute the gradients of SoftGAP, we use a similar approach as the one Taylor et al. [129] used to compute the gradients of nDCG.

LambdaRank [27] is another neural network based algorithm that is also designed to optimize for nDCG. In order to overcome the problem of optimizing non-smooth IR metrics, LambdaRank uses the approach of defining the gradient of the target evaluation metric only at the points needed.

Given a pair of documents, the virtual gradients (λ functions) used in LambdaRank are obtained by scaling the RankNet [26] cost with the amount of change in the value of the metric obtained by swapping the two documents [27].

Following the same setup, in order to optimize for GAP, we scale the RankNet cost with the amount of change in the value of GAP metric when two documents are swapped. This way of building gradients in LambdaRank is shown to find the local optima for the target evaluation metrics [49].

Results: Tables 5.5 and 5.2.4 show the results of training and testing using different metrics. In particular the rows of the table correspond to training for nDCG, GAP and AP, respectively. The columns correspond to testing for nDCG at cutoff 3, 5 and 10, AP and precision at cutoff 10. As it can be observed in the table training for GAP outperforms both training for nDCG and AP, even if the test metric is nDCG or AP respectively. The differences among the effectiveness of the resulting ranking functions are not large, however, (1) most of them are statistically significant, indicating that the fact that GAP outperforms AP and nDCG is not a results of any random noise in training data, (2) GAP consistently leads to the best performing ranking function over two radically different data sets, and (3) GAP consistently leads to the best performing ranking function over two different LTR algorithms. Thus, even if the differences among the constructed ranking functions are not large, optimizing for GAP can only lead to better ranking functions.

These results strengthen the conclusion drawn from the discussion about the informativeness of the metrics. First, it can be clearly seen that even in the case that we care about a binary measure (AP or PC at 10) the utilization of multi-graded relevance judgments is highly beneficial. Furthermore, these results suggest that even if one cares for nDCG at early ranks, one should still train for GAP as opposed to training for nDCG.

5.2.5 Conclusions

We constructed a new metric of retrieval effectiveness (GAP) in a systematic manner that directly generalizes average precision to the multi-graded relevance case. As such, it inherits all desirable properties of AP: it has a nice probabilistic interpretation and a theoretical foundation; it estimates the area under the non-interpolated grade precision-recall curve. Furthermore, the new metric is highly informative and highly discriminative. Finally, when used as an objective function for learning-to-rank purposes GAP consistently outperforms AP and nDCG over two different data sets and over three different learning algorithms even when the test metric is AP or nDCG itself.

5.3 Overall Conclusions

Evaluation metrics are employed both to summarize the effectiveness of a retrieval system and as objective functions to be optimized in learning-to-rank. Traditional evaluation metrics, such as average precision, even though they are very popular, assume that documents are either relevant or nonrelevant to a user's information request ignoring the fact that naturally some documents are more relevant than others. Consequently, in evaluation they do not capture all available information about the effectiveness of a system and in learning-to-rank they ignore swaps between documents of different relevance grade and thus they do not guide the training of new ranking functions in the most optimal manner.

The nDCG metric has proven to be one of the most popular multi-graded relevance metric, however defining the appropriate gain and discount function with respect to user satisfaction is a hard problem. In this work, instead we proposed a methodological way of defining the gain and the discount function of nDCG by optimizing the efficiency of the metric, that is by minimizing the number of queries required to reliably discriminate a good from a bad systems. Different gain and discount functions however may lead to nDCG variants that evaluate different aspects of a system's retrieval effectiveness and since the choice of gain and discount functions appear to be dependent on the data collection and the participating systems, this could lead to inconsistencies in the evaluation over different collections. To overcome the difficulties and uncertainties regarding the choice of gain and discount functions for nDCG, we proposed an extension of average precision to multi-graded relevance judgments. The new metric, GAP, has a nice probabilistic interpretation and a theoretical foundation. Further it is highly informative and discriminative. Finally, we compared GAP with nDCG when used as objective function for learning-to-rank purposes and GAP clearly outperforms nDCG even when the test metric is nDCG itself.

CHAPTER 6

Conclusions

In this work we have considered the construction of reliable and efficient test and training collections to be used in the evaluation of retrieval systems and in the development of new and effective ranking functions. In the process of building such collections we investigated methods of selecting the appropriate documents and queries to be judged and we proposed evaluation metrics that better capture the overall effectiveness of the retrieval systems under study.

In particular, we demonstrated that low-cost evaluation, by carefully selecting documents per query to be judged and also queries to be included in the test collections, can lead to effective and reliable evaluation. By utilizing our evaluation methodology IR researchers and engineers can construct new and customized test collections with minimal cost to test new ideas, while customers can easily compare different retrieval system options and select the one that best fits their needs. Furthermore, with the same budget as the one currently used by multinational corporations and governmental organizations like NIST, we have shown that larger and thus more diverse and more representative and at the same time more reliable test collections can be constructed. Our work on query selection is a preliminary step towards this direction and more work is needed to methodologically understand and decide what queries are better to evaluate on. Therefore, we plan to carry on this line of research, (a) in a top-down approach by examining different query categories and whether some query categories are more useful than others (e.g. in Million Query at TREC 2009 we plan to examine what distribution over query hardness and query intent can lead to more effective and efficient evaluation), and (b) in a bottom-up approach through item-response analysis that can reveal how well a single query or a set of queries can discriminate different systems.

Further, we illustrated that the construction of training collections needs to be carefully studied. Specifically, we showed that the way documents are selected to be included in training collections affect the effectiveness of the constructed ranking functions. Some preliminary results of on-going research has also shown that queries in training collections also affect the effectiveness of the obtained ranking functions. Even though the construction of training collections faces similar issues with the construction of test collections the solutions are not necessary the same and thus using test collections for the purpose of learning-to-rank may not be the optimal way to construct new ranking functions. In learning-to-rank a large number of factors and especially their interactions may affect the effectiveness of the final ranking function. In particular we have shown preliminary results on the interactions between the machine learning algorithm used and the document selection methodology with some algorithms being more robust than others both regarding the incompleteness of the constructed training collection and the methodology to select those documents. We plan to continue investigating different factors that affect the effectiveness of the ranking functions along with their interactions.

Finally, we have shown that evaluation metrics also affect both the effectiveness of the evaluation and the effectiveness of learning-to-rank. We plan to continue investigating the effectiveness of different evaluation metrics following two different directions: (a) for the purpose of evaluation we plan to move towards evaluation metrics that better associate with user satisfaction by utilizing tools like Amazon's Mechanical Turk that can be used to provide inexpensive judgments and query logs that provide useful information about users' interactions with the retrieval system, and (b) for the purpose of learning-to-rank we plan to continue our work on metrics of overall effectiveness even though they may not best correlate with user satisfaction. In particular, we intent to investigate how newly proposed metrics that can effectively evaluate new IR tasks, such as novelty retrieval, can be optimized to result in ranking functions that best fulfill users' need, e.g. by diversifying the ranked list of documents returned to the user.

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Appendix

APPENDIX A

Confidence Intervals for Inferred Average Precision

Let s_d be a sample of cut-off levels at relevant documents. According to the Law of Total Variance, the variance in infAP can be calculated as,

$$var[\text{infAP}] = var[E[\text{infAP}|s_d]] + E[var[\text{infAP}|s_d]]$$

Let's consider the first term of the right-hand side of the above equation, which corresponds to the variance due to sampling cut-off levels.

Let r the number of relevant documents in s_d. Then, the conditional expectation of *infAP* is,

$$E[\text{infAP}|s_d] = \frac{1}{r}\sum_{k\in s_d} E[\widehat{PC(k)}|s_d] = \frac{1}{r}\sum_{k\in s_d} PC(k)$$

where $\widehat{PC(k)}$ and $PC(k)$ denote the estimated and actual precision at cut-off k, respectively. Thus,

$$var[E[\text{infAP}|s_d]] = var\left[\frac{1}{r}\sum_{k\in s_d} PC(k)\right] = (1-p)\,\frac{\sigma^2}{r}$$

where $p100\%$ is the sampling percentage of documents from the entire depth-100 pool and σ^2 is the actual variance among the precision values at all cut-off's of relevant documents and it can be estimated by, $\left(\sum_{k\in s_d}(\widehat{PC(k)} - \text{infAP})^2\right)/(r-1)$.

Now, let's consider the second term of the right-hand side of the equation deduced by the Law of Total Variance, that is the variance due to sampling documents above a cut-off level in order to estimate the precision at that cut-off level,

$$var[\text{infAP}|s_d] = var\left[\frac{1}{r}\sum_{k\in s_d}\widehat{PC(k)}\right] = \frac{1}{r^2}var\left[\sum_{k\in s_d}\widehat{PC(k)}\right]$$

Considering $\widehat{PC(k)}$ independent from each other

If we consider precisions at different cut-off levels independent from each other the variance of infAP for a given set of sampled cut-off levels depends on the summation of the precision variances at each individual cut-off level,

$$var[\text{infAP}|s_d] = \frac{1}{r^2} \sum_{k \in s_d} var[\widehat{PC(k)}|s_d]$$

The precision at cut-off 1 is always 1 and therefore the variance is 0. Moreover, the precision at relevant documents not in the retrieved list is always assumed to be 0 and therefore, the variance at those cut-off levels is also 0. In all other case $\widehat{PC(k)}$ is calculated as, $\widehat{PC(k)} = 1/k + ((k-1)/k) \cdot \widehat{PC}_{\text{above k}}$ and therefore,

$$var[\widehat{PC(k)}|s_d] = \left(\frac{k-1}{k}\right)^2 var[\widehat{PC}_{\text{above k}}]$$

Let r_{k-1} and n_{k-1} be the number of relevant documents and total number of documents sampled above cut-off k, respectively and let $|\text{d100}|_{k-1}$ be the number of documents in the depth-100 pool above cut-off k. The precision above cut-off k is estimated by [1],

$$\widehat{PC}_{\text{above k}} = \frac{|\text{d100}|_{k-1}}{k-1} \cdot \frac{r_{k-1}}{n_{k-1}}$$

which follows a hypergeometric distribution and its variance can be calculated as,

$$var[\widehat{PC}_{\text{above k}}|s_d] = \left(\frac{p(1-p)}{n_{k-1}}\right) \cdot \left(1 - \frac{n_{k-1}-1}{|\text{d100}|_{k-1}-1}\right)$$

By considering the expected value of $var[\text{infAP}|s_d]$ over all samples of cut-off levels we get,

$$E[var[\text{infAP}|s_d]] = \frac{\sum_{k \in s_d} var[\widehat{PC(k)}|s_d]}{r^2}$$

Considering $\widehat{PC(k)}$ dependent to each other

If we do not consider precisions at different cut-off levels independent from each other the covariance between precisions can be calculated as,

$$cov[\widehat{PC(k)}, \widehat{PC}_m] = \frac{k}{m} var[\widehat{PC(k)}] \text{ where } k < m$$

[1]For simplicity reasons we ignore the effect of smoothing that is introduced in the formula of infAP. Smoothing was considered in all experiments ran and it was observed that the effect of smoothing in variance is negligible.

APPENDIX B

Variance Decomposition Analysis

Variance components

Assume an experimental design that involves n_s systems run over a sample of n_q queries resulting in a set of $n_s * n_q$ ranked lists of documents. Further assume that each list of documents is evaluated by some evaluation metric (e.g. average precision) and the overall quality of a system is captured by averaging the values of this evaluation metric over all topics. Systems, then, are ranked by their mean scores, e.g. mean average precision, MAP.

Hypothetically, if a second set of topics was available, the systems could be run over this new set of topics and new mean scores (and consequently new ranking of the systems) would be produced. The question that naturally arises is, how many topics are necessary to guarantee that the mean scores do not change radically when two different query sets are used, or alternatively how many topics are necessary to guarantee that the mean scores of systems reflect their actual performance?

Given different sets of topics one could decompose the amount of variability that occurs in the mean scores (as measured by variance) across all sets of topics and all systems into three components: (a) variance due to actual performance differences among systems—*system variance*, (b) variance due to the relative difficulty of a particular set of topics—*topic variance*, and (c) variance due to the fact that different systems consider different sets of topics hard (or easy)—*system-topics interaction variance*. Note that among the three variance components, only the variance due to *systems* and *system-topics interactions* affect the *ranking* of systems—it is these two components that can alter the relative differences among the mean scores, while the topic variance will affect all systems equally, reflecting the overall difficulty of the set of topics.

Ideally, one would like the total variance in the mean scores to be due to the actual performance differences between systems rather than the other two sources of variance. If this would be the case, running the systems over different topic sets would result in each system having identical mean scores regardless of the topics used, and thus the mean scores over a single set of topics would be 100% reliable in evaluating the quality of the systems.

In practice, retrieval systems are run over a single set of topics. The decomposition of the total variance into the aforementioned components in this case can be realized by fitting an ANOVA model into the evaluation scores [14, 17, 33]. Given the variance components tools from Generalizability Theory [20] can be used to quantify the stability of the evaluation.

Stability coefficients

There are two coefficients that predominate in Generalizability Theory to quantify the stability of the evaluation, the generalizability coefficient and the dependability coefficient, with the former reflecting the stability of the system rankings and the latter the stability of the system effectiveness scores. They both lie in a zero to one range.

The former coefficient is the ratio of the system variance and the variance in relative scores (i.e. in system rankings), that is the summation of the system and system-topic interaction variance,

$$E\rho^2 = \frac{\sigma^2(\text{system})}{\sigma^2(\text{system}) + \frac{\sigma^2(\text{system:topic})}{\#\text{ of topics}}} \tag{B.1}$$

and it can be interpreted as an approximation to the squared correlation between the relative mean scores observed over the given set of topics and the relative mean scores that would be observed if infinite number of topics was available.

The dependability coefficient, Φ, is the ratio of the system variance and the total variance,

$$\Phi = \frac{\sigma^2(\text{system})}{\sigma^2(\text{system}) + \frac{\sigma^2(\text{topic})+\sigma^2(\text{system:topic})}{\#\text{ of topics}}} \tag{B.2}$$

and it can be interpreted as an approximation to the squared correlation between the mean scores observed over the given set of topics and the mean scores that would be observed if infinite number of topics was available. Note that both Φ and $E\rho^2$ decrease with the topic set size. Further note that $E\rho^2$ is always larger than Φ. In our experiments we employ only the latter coefficient since stable scores infer stable rankings.

Also note that the computation of the two coefficients is done independently of the estimation of the variance components. That is, first the variance components are estimated over a set of available topics (50 topics in our experiments). Then, the two aforementioned coefficients are using these estimates to project reliability scores to topic sets of any size. The topic set size in the computation of the coefficients does not need to be the same as the topic set size used to estimate the variance components (it can even be larger).

Appendix C

Statistics

Root Mean Squared Error

The *Root Mean Squared Error* quantifies the difference between the estimated and the true values of some quantity. Let θ be the quantity to be estimated and $\hat{\theta}$ an estimator; the Root Mean Squared Error can be calculated as,

$$\text{RMS Error} = \sqrt{E\left[(\hat{\theta} - \theta)^2\right]}$$

The RMS Error has the same units as the quantity being estimated.

Linear Correlation Coefficient (ρ)

The *Linear Correlation Coefficient*, ρ, (or Pearson's correlation coefficient) indicates the strength and direction of the linear relationship between the estimated and the true values of some quantity. Geometrically, it can be interpreted as the the cosine of the angle between the two vectors of the true and the estimated values after the data points are centered. It is calculated as,

$$\rho = \frac{\sum(\theta - \bar{\theta})(\hat{\theta} - \bar{\hat{\theta}})}{(n-1)s_\theta s_{\hat{\theta}}}$$

where n is the number of measurements, $\bar{\theta}$ and $\bar{\hat{\theta}}$ are the means of θ and $\hat{\theta}$, s_θ and $s_{\hat{\theta}}$ are the sample standard deviations of θ and $\hat{\theta}$ and the sum is from i = 1 to n.

The linear correlation coefficient takes values in the range of -1 to 1, with -1 indicating a perfect negative linear correlation between the θ and its estimator and 1 indicating a perfect positive linear correlation between θ and its estimator.

Kendall's tau (τ)

The *Kendall's* τ statistic is a non-parametric statistic used to quantify the correlation between two rankings. It is a function of the minimum number of pairwise adjacent interchanges needed to convert one ranking into the other. Given two ranked lists of length N, let C be the total number of concordant pairs (pairs that are ranked in the same order in both rankings) and D be the total number of discordant pairs (pairs that are ranked in opposite order in the two rankings). Then, the Kendalls τ can be computed as,

$$\tau = \frac{C - D}{N(N-1)/2}$$

If the two rankings are exactly the same, then the Kendalls τ value is 1. If the two rankings perfectly disagree with each other, then the Kendalls τ value is 1 and if the two rankings are completely independent of each other (no correlation), then the Kendalls τ value is 0.