Advances in IR Evaluation

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Microsoft Research

Course Outline

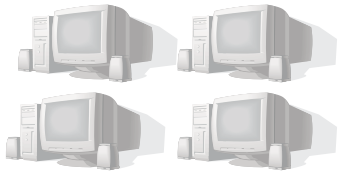
- Intro to evaluation
  - Evaluation methods, test collections, measures, comparable evaluation
- Low cost evaluation
- Advanced user models
  - Web search models, novelty & diversity, sessions
- Reliability
  - Significance tests, reusability
- Other evaluation setups

Judgment Effort

- uses of alternative dispute resolution
- job search vancouver washington
- poem of arrival of columbus

Results

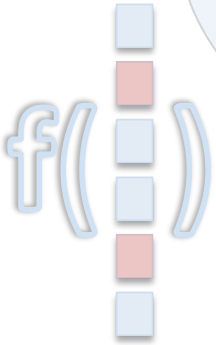
Search Engines



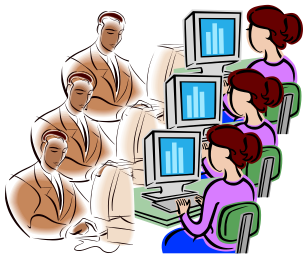
Cost

vs.

Reliability

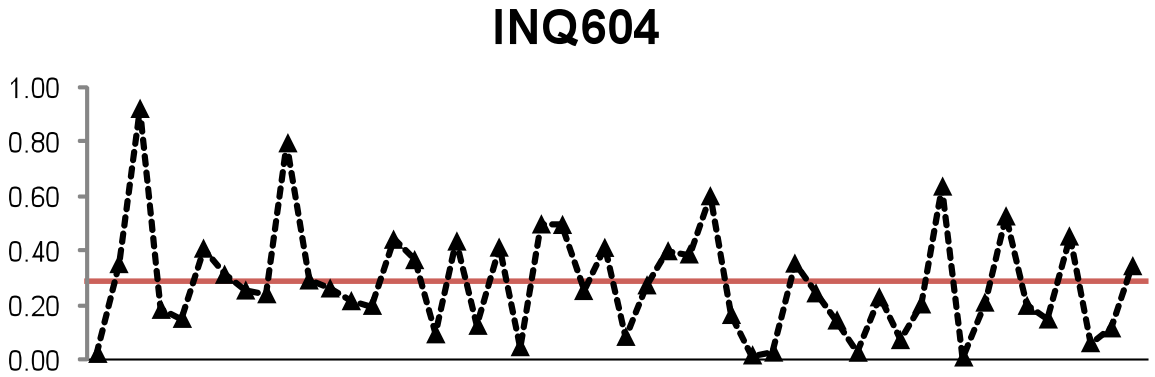


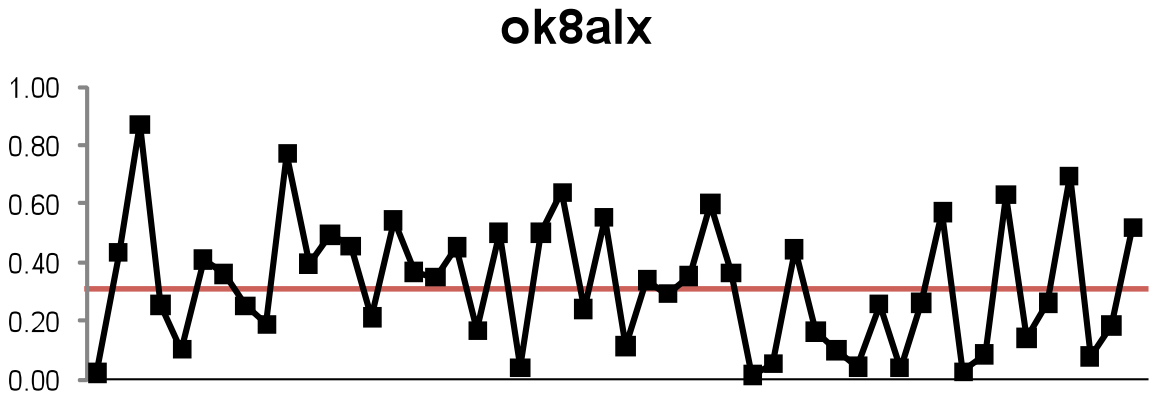
Judges

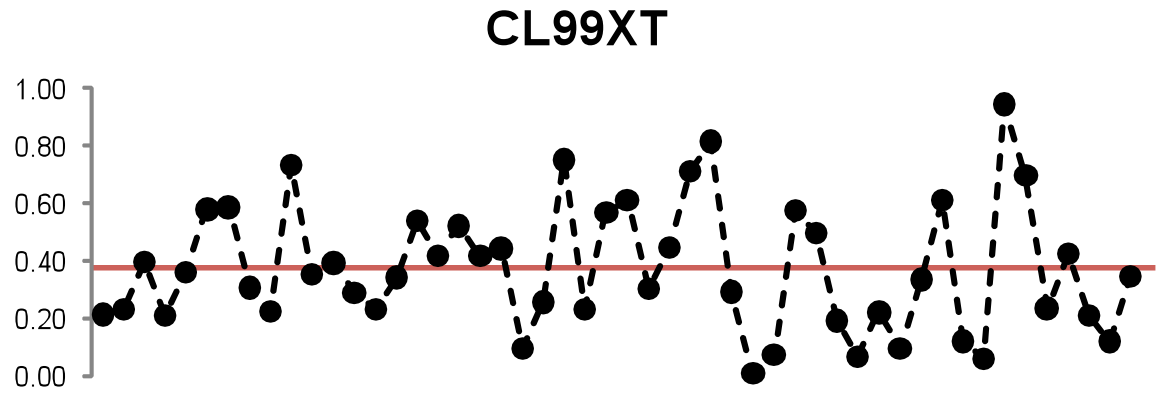


Average?

- INQ604 - 0.281
- ok8alx - 0.324
- CL99XT - 0.373







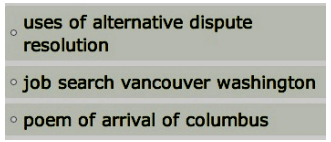
How much is the average

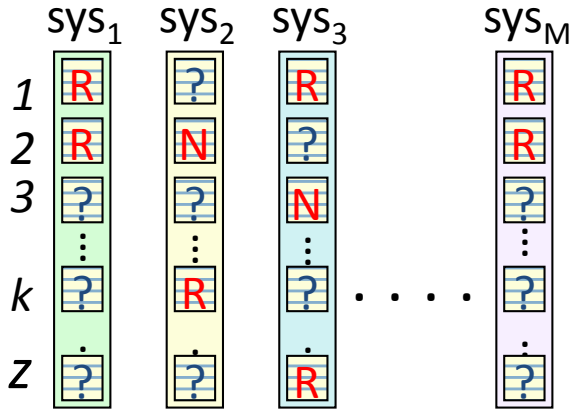
- A product of
  - your IR system
  - chance?
- Slightly different set of topics?
  - Would the average change?

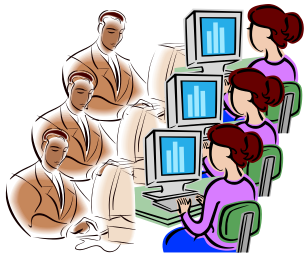
Reliability

- Reliability : the extent to which results reflect real difference (not due to chance)
- Variability in effectiveness scores due to

1. Differentiation in nature of **documents** (corpus)
2. Differentiation in nature of **topics**
3. **Incompleteness** of relevance judgments
4. **Inconsistency** among assessors







Reliability

- Variability in effectiveness scores due to
  1. Differentiation in nature of **documents**
  2. Differentiation in nature of **topics**
  3. **Incompleteness** of judgments
  4. **Inconsistency** among assessors

- Document corpus size

  [Hawking and Robertson J of IR 2003]

- Topics vs. assessors

  [Voorhees SIGIR98, Banks et al. Inf. Retr.99, Bodoff and Li SIGIR07]

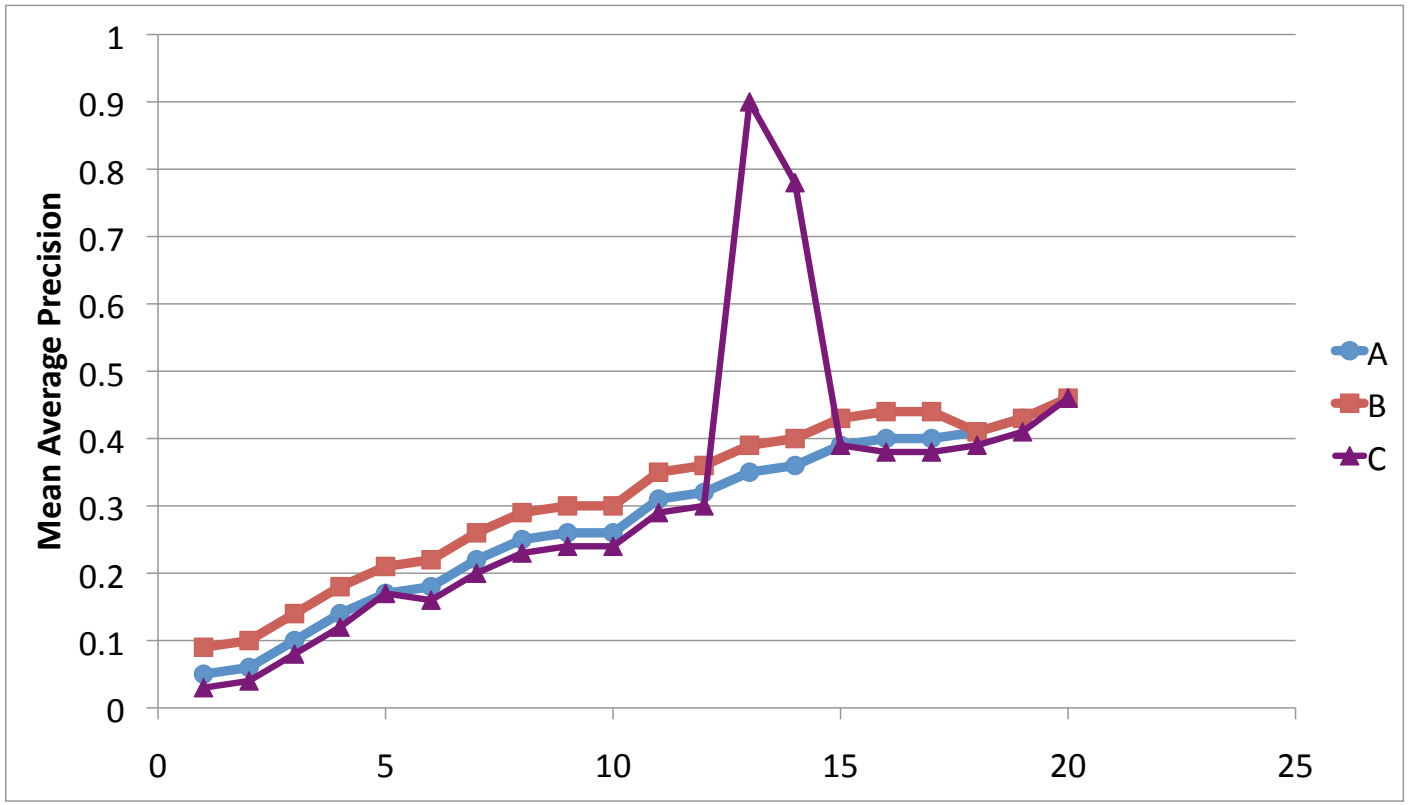
- Effective **size** of the topic set for reliable evaluation

  [Buckley and Voorhees SIGIR00/SIGIR02, Sanderson and Zobel SIGIR05]

- Topics vs. documents (Million Query track)

  [Allan et al. TREC07, Carterette et al. SIGIR08]

Is C really better than A?



Comparison and Significance

- Variability in effectiveness scores
- When observing a difference in effectiveness scores across two retrieval systems
  - Does this difference occur by **random chance**?
- Significance testing
  - Estimates the probability *p* of observing a certain difference in effectiveness given that H_0 is true.
  - In IR evaluation
    - H_0 : the two systems are in effect the same and any difference in scores is by random chance.

Significance Testing

- Significance testing framework:
  - Two hypotheses, e.g.
    H_0: $\mu = 0$
    H_a: $\mu \neq 0$
  - System performance measurements over a sample of topics
  - A test statistic T computed from those measurements
  - The distribution of the test statistic
  - A p-value, which is the probability of sampling T from a distribution obtained by assuming H_0 is true

Student's t-test

- Parametric test
- Assumptions
  1. effectiveness score differences are meaningful
  2. effectiveness score differences follow normal distribution

- Statistic : $t = \frac{\overline{B-A}}{\sigma_{B-A}}.\sqrt{N}$

- t-test performs well even when the normality assumption is violated [Hull SIGIR93]

Student's t-test

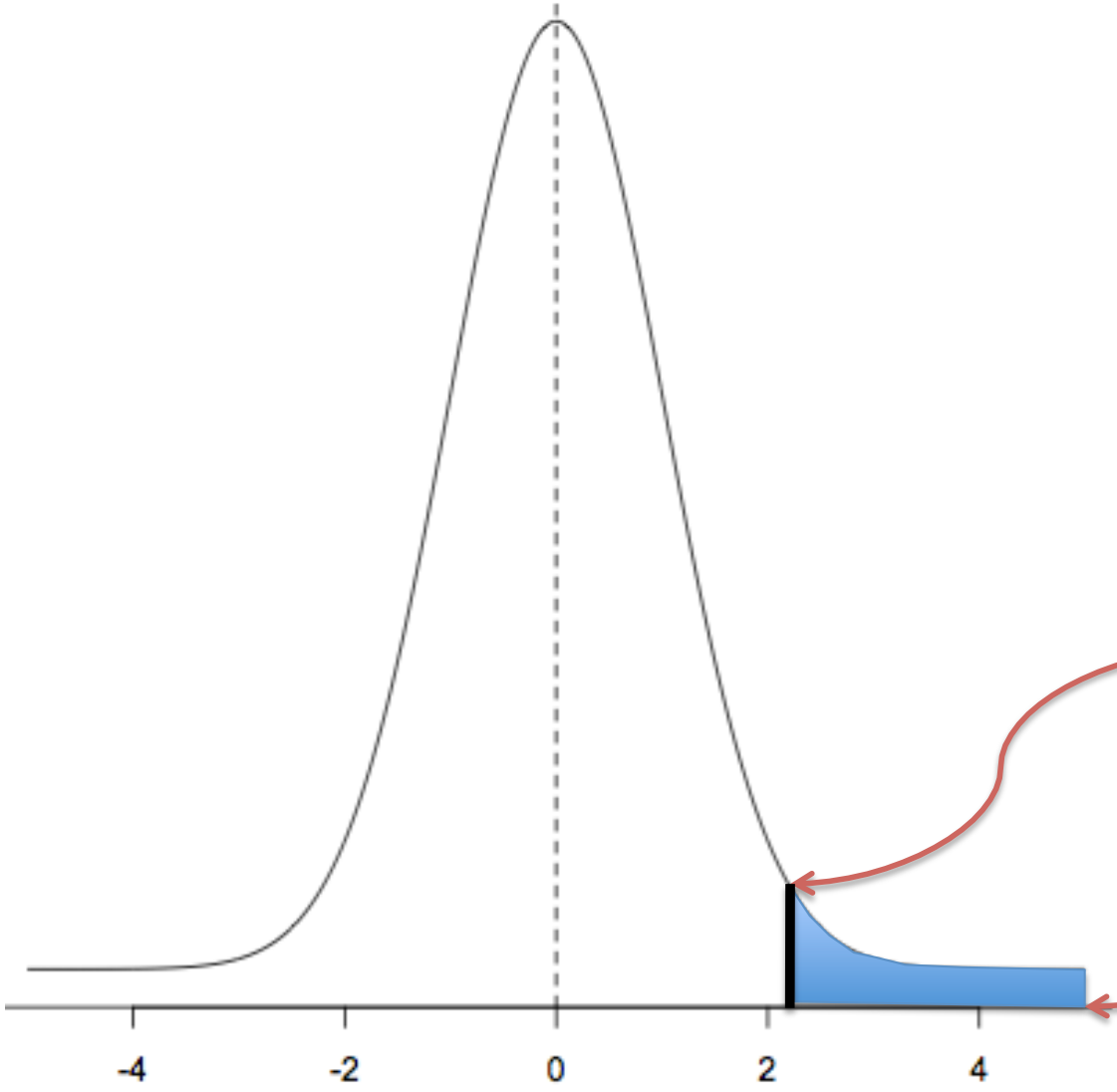
Query	A	B	B-A
1	.25	.35	+.10
2	.43	.84	+.41
3	.39	.15	-.24
4	.75	.75	0
5	.43	.68	+.25
6	.15	.85	+.70
7	.20	.80	+.60
8	.52	.50	-.02
9	.49	.58	+.09
10	.50	.75	+.25

$$\overline{B-A} = 0.214$$

$$\sigma_{B-A} = 0.291$$

$$t = \frac{B-A}{\sigma_{B-A}}\sqrt{n} = 2.33$$

Student's t-test



$$\overline{B-A} = 0.214$$

$$\sigma_{B-A} = 0.291$$

$$t = \frac{B-A}{\sigma_{B-A}}\sqrt{n} = 2.33$$

$$p - value = 0.02$$

Sign Test

- Non-parametric test

- Ignores magnitude of differences
- Null hypothesis for this test is that
  - $P(B > A) = P(A > B) = ½$

- Statistic : number of pairs where B>A

Wilcoxon Signed-Ranks Test

- Non-parametric test
- Statistic: $w = \sum_{i=1}^{N} R_i$
  - R_i is a signed rank of absolute differences
  - N is the number of differences ≠ 0

Wilcoxon Signed-Ranks Test

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9	.49	.58	+.09
10	.50	.75	+.25

Sorted
-.02
+.09
+.10
-.24
+.25
+.25
+.41
+.60
+.70

Signed-rank
-1
+2
+3
-4
+5
+6
+7
+8
+9

$$w = \sum_{i=1}^{N} R_i$$

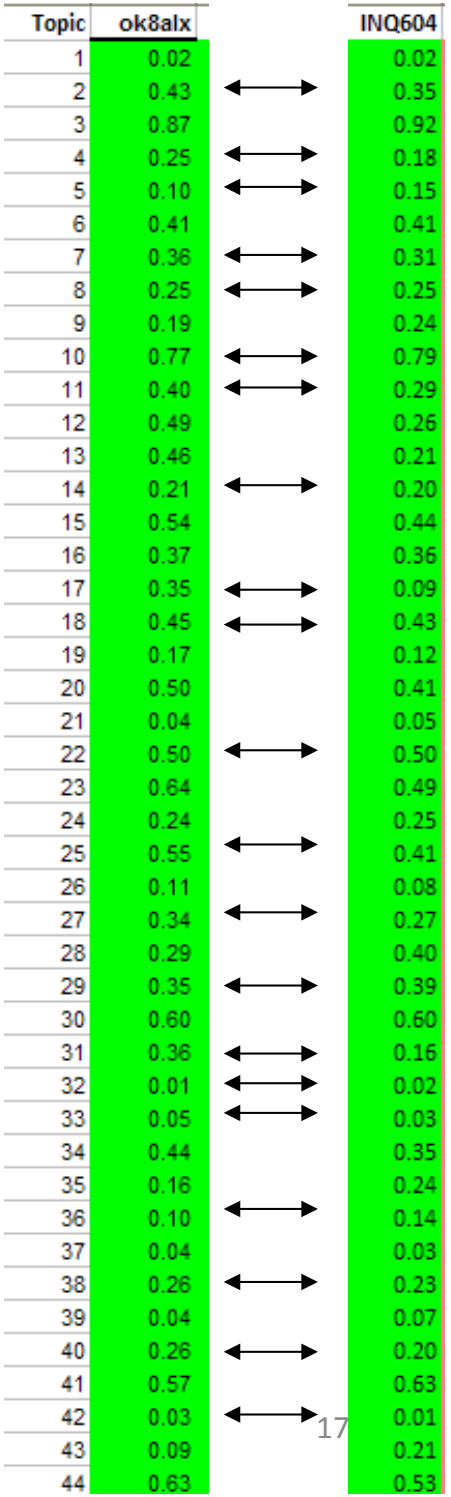
w = 35 =>

p = .025

Randomisation test

- Loop for many times {
  1. Load topic scores for 2 systems
  2. Randomly swap values per topic
  3. Compute average for each system
  4. Compute difference between averages
  5. Add difference to array

  }
- Sort array
- If actual difference outside 95% differences in array
  - Two systems are significantly different



Topic	ok8alx		INQ604
1	0.02		0.02
2	0.43	↔	0.35
3	0.87		0.92
4	0.25	↔	0.18
5	0.10	↔	0.15
6	0.41		0.41
7	0.36	↔	0.31
8	0.25	↔	0.25
9	0.19		0.24
10	0.77	↔	0.79
11	0.40	↔	0.29
12	0.49		0.26
13	0.46		0.21
14	0.21	↔	0.20
15	0.54		0.44
16	0.37		0.36
17	0.35	↔	0.09
18	0.45	↔	0.43
19	0.17		0.12
20	0.50		0.41
21	0.04		0.05
22	0.50	↔	0.50
23	0.64		0.49
24	0.24		0.25
25	0.55	↔	0.41
26	0.11		0.08
27	0.34	↔	0.27
28	0.29		0.40
29	0.35	↔	0.39
30	0.60		0.60
31	0.36	↔	0.16
32	0.01	↔	0.02
33	0.05	↔	0.03
34	0.44		0.35
35	0.16		0.24
36	0.10	↔	0.14
37	0.04		0.03
38	0.26	↔	0.23
39	0.04		0.07
40	0.26	↔	0.20
41	0.57		0.63
42	0.03	↔	0.01
43	0.09		0.21
44	0.63		0.53

Inference from Hypothesis Tests

- We often uses tests to make an inference about the population based on a sample
  - e.g. infer that H_0 is false in the population of topics
- That inference can be wrong for various reasons:
  - Sampling bias
  - Measurement error
  - Random chance
- There are two classes of errors:
  - Type I, or false positives: we reject H_0 even though it is true
  - Type II, or false negatives: we fail to reject H_0 even though it is false

Errors in Inference

- A significance test is basically a classifier

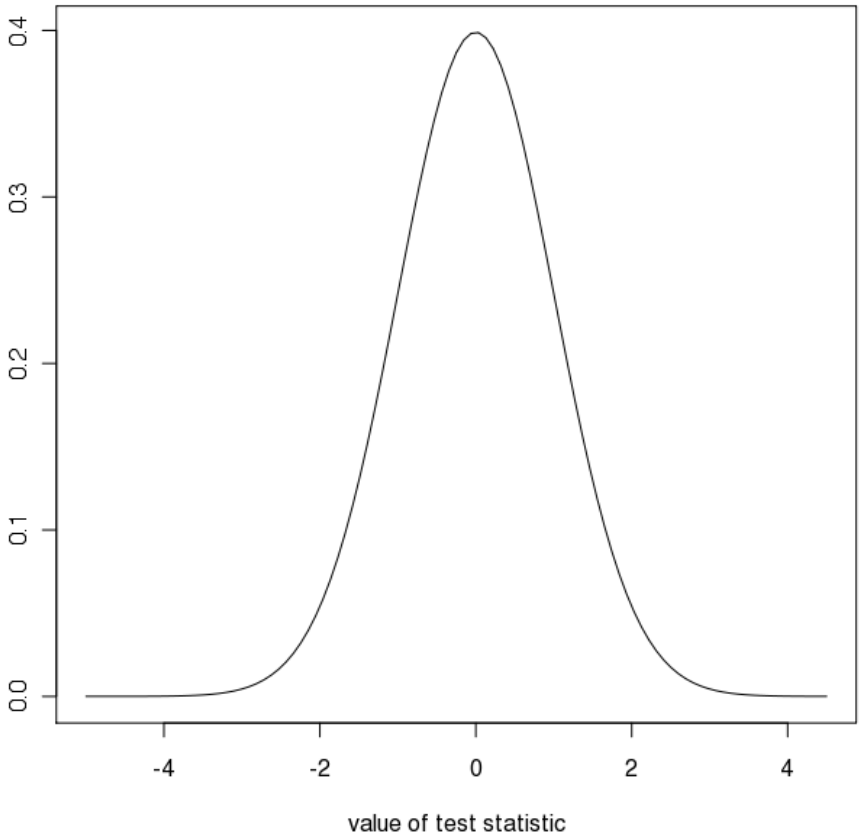
	H_0 false	H_0 true
$p < 0.05$ (reject H_0)	correct	Type I error
$p \geq 0.05$ (do not reject H_0)	Type II error	correct

- We can't actually know whether H_0 is true or not
  - If we could, we wouldn't need the test
- But we can set up the test to control the expected Type I and Type II error rates

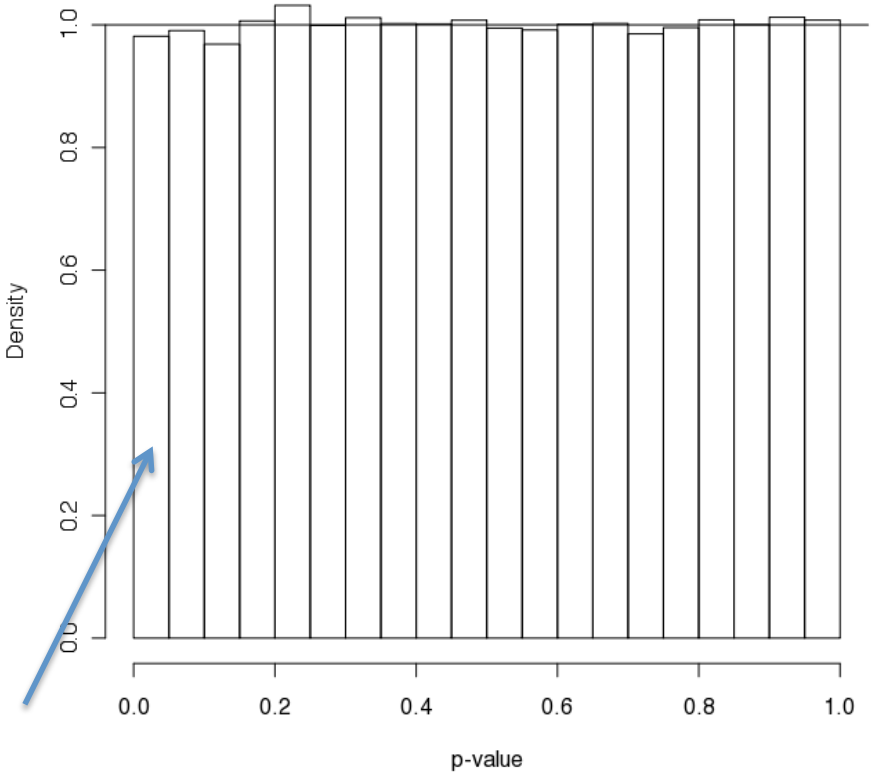
Expected Type I Error Rate

- Test parameter α is used to decide whether to reject H0 or not—if $p < \alpha$, then reject H_0
- Choosing α is equivalent to stating an expected Type I error rate
  - e.g. if $p < 0.05$ is considered significant, we are saying that we expect that we will incorrectly reject H_0 5% of the time
- Why?
  - Because when H_0 is true, every p-value is equally likely to be observed
  - 5% of the time we will observe a p-value less than 0.05… and therefore there is a 5% Type I error rate

Typical distribution of test statistic assuming H_0 true



Distribution of p-values assuming H_0 true

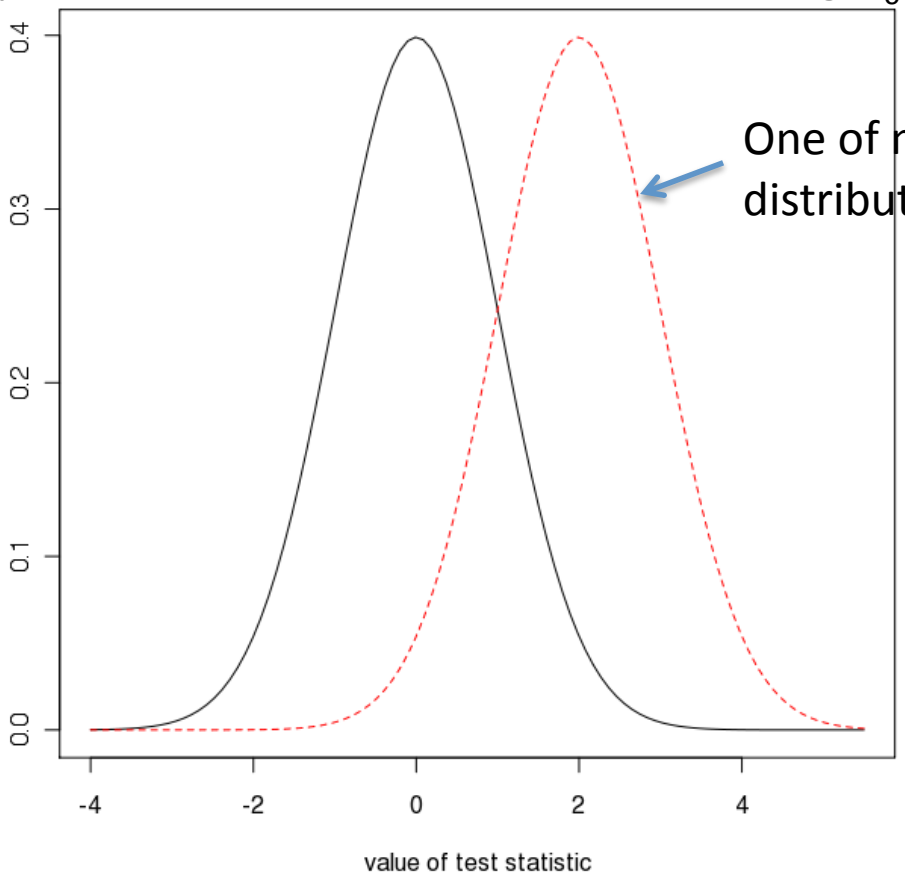


5% chance of incorrectly concluding H_0 false

Expected Type II Error Rate

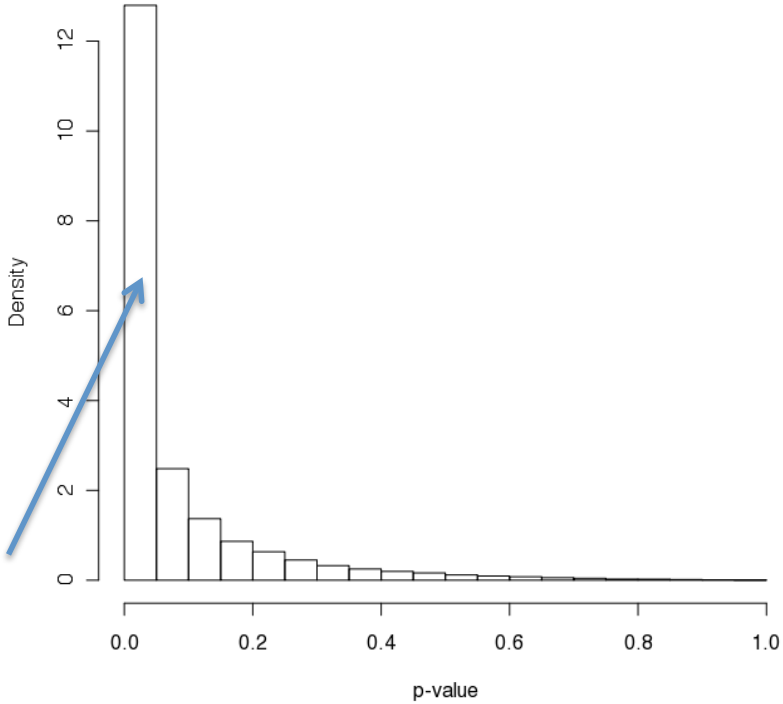
- What about Type II errors?
  - False negatives are bad: if we can't reject H_0 when it's false, we may miss out on interesting results
- What is the distribution of p-values when H_0 is false?
  - Problem: there is only one way H_0 can be true, but there are many ways it can be false

Typical distribution of test statistic assuming H_0 true



One of many possible distributions if H_0 is false

Distribution of p-values for that alternative



0.64 probability of rejecting H_0

⇒ 0.36 Type II error rate

Power and Expected Type II Error Rate

- Power is the probability of rejecting H_0 when it is false
  - Equivalent to (1 – Type II error rate)
- Power parameter is β
  - Choosing a value of β therefore entails setting an expected Type II error rate
- But what does it mean to "choose a value of β"?
  - In the previous slide, β was calculated post hoc, not chosen a priori

Effect Size

- A measure of the magnitude of the difference between two systems
  - Effect size is dimensionless; intuitively similar to % change in performance
  - Bigger population effect size → more likely to find a significant difference in a sample
- Before starting to test, we can say "I want to be able to detect an effect size of h with probability β"
  - e.g. "If there is at least a 5% difference, the test should say the difference is significant with 80% probability"

    ⇒ $h = 0.05$, $\beta = 0.8$

Sample Size

- Once we have chosen α, β, h (Type I error rate, power, and desired effect size), we can determine the sample size needed to make the error rates come out as desired
  - n = f(α, β, h)
  - Usually involves a linear search
  - There are software tools to do this
- Basically:
  - Sample size n increases with β if other parameters held constant
  - If you want more power, you need more samples

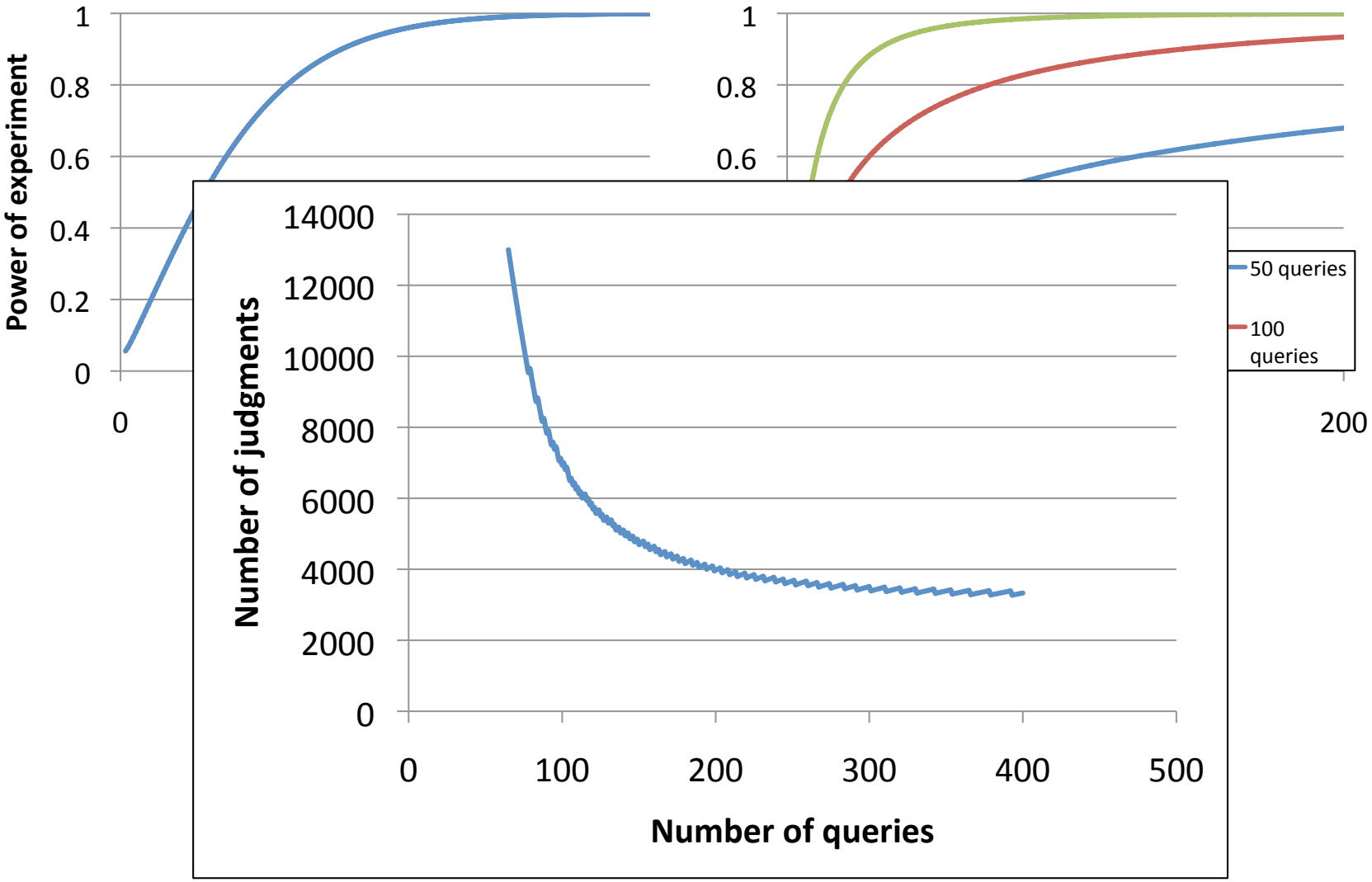
Implications for Low-Cost Evaluation

- First consider how Type I and Type II errors can happen due to experimental design rather than randomness
  - Sampling bias: usually increases Type I error
  - Measurement error: usually increases Type II error
- When judgments are missing, measurements are more errorful
  - And therefore Type II error is higher than expected

Implications for Low-Cost Evaluation

- What is the solution?
  - If Type II error increases, power decreases
  - To get power back up to the desired level, sample size must increase
  - Therefore: deal with reduced numbers of judgments by increasing the number of topics
- Of course, each new topic requires its own judgments
  - Cost-benefit analysis finds the "sweet spot" where power is as desired within available budget for judging

Tradeoffs in Experimental Design



Criticism on Significance Tests

- Significance testing
  - Note: The probability *p* is not the probability that H_0 is true.
    - $p = P(\text{Data} \mid H_0) \neq P(H_0 \mid \text{Data})$

Criticism on Significance Tests

- Are significance tests appropriate for IR evaluation?
- Is there any single best to be used?

  [Saracevic CSL68, Van Rijsbergen 79, Robertson IPM90, Hull SIGIR93, Zobel SIGIR98, Sanderson and Zobel SIGIR05, Cormac and Lynam SIGIR06, Smucker et al SIGIR09 …]
- Are they any useful?
  - With a large number of samples any difference in effectiveness will be statistically significant.
    - e.g. 30,000 queries in new Yahoo! and MSR collections
  - "Strong form" of hypothesis testing [Meehl Phil.Sci.67, Popper 59, Cohen Amer.Psy.94]

Criticism on Significance Tests

- What to do?
  - Improve our data (make them as representative as possible)
  - Report confidence intervals [Cormack and Lynam SIGIR06, Yilmaz et al. SIGIR08, Carterette et al. SIGIR08]
  - Examine whether results are "practically significant" [Allan et al SIGIR05, Thomas and Hawking CIKM06, Turpin and Scholer SIGIR06, Joachims SIGIR02, Radlinski et al CIKM08, Radlinski and Craswell SIGIR10, Sanderson et al. SIGIR10]

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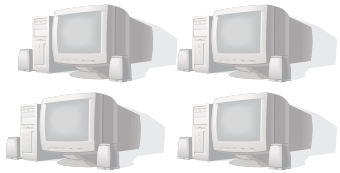
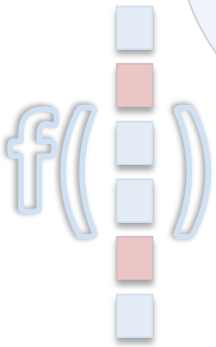
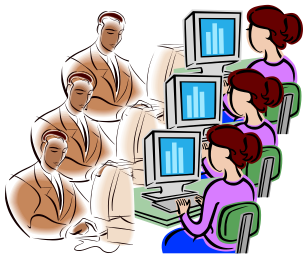




Judges



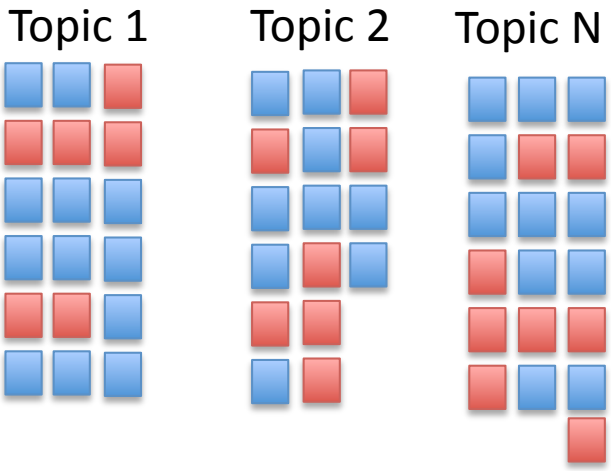
Reusability

- Why reusable test collections?
  - High cost of constructing test collections
    - Amortize cost by reusing test collections
  - Make retrieval system results comparable
    - Test collections used by systems that did not participate while collections were constructed

- Low-cost vs. Reusability
  - If not all relevant documents are identified the effectiveness of a system that did not contribute to the pool may be underestimated

Reusability

Relevance Judgments



New System S'
Ranked List
on Topic1

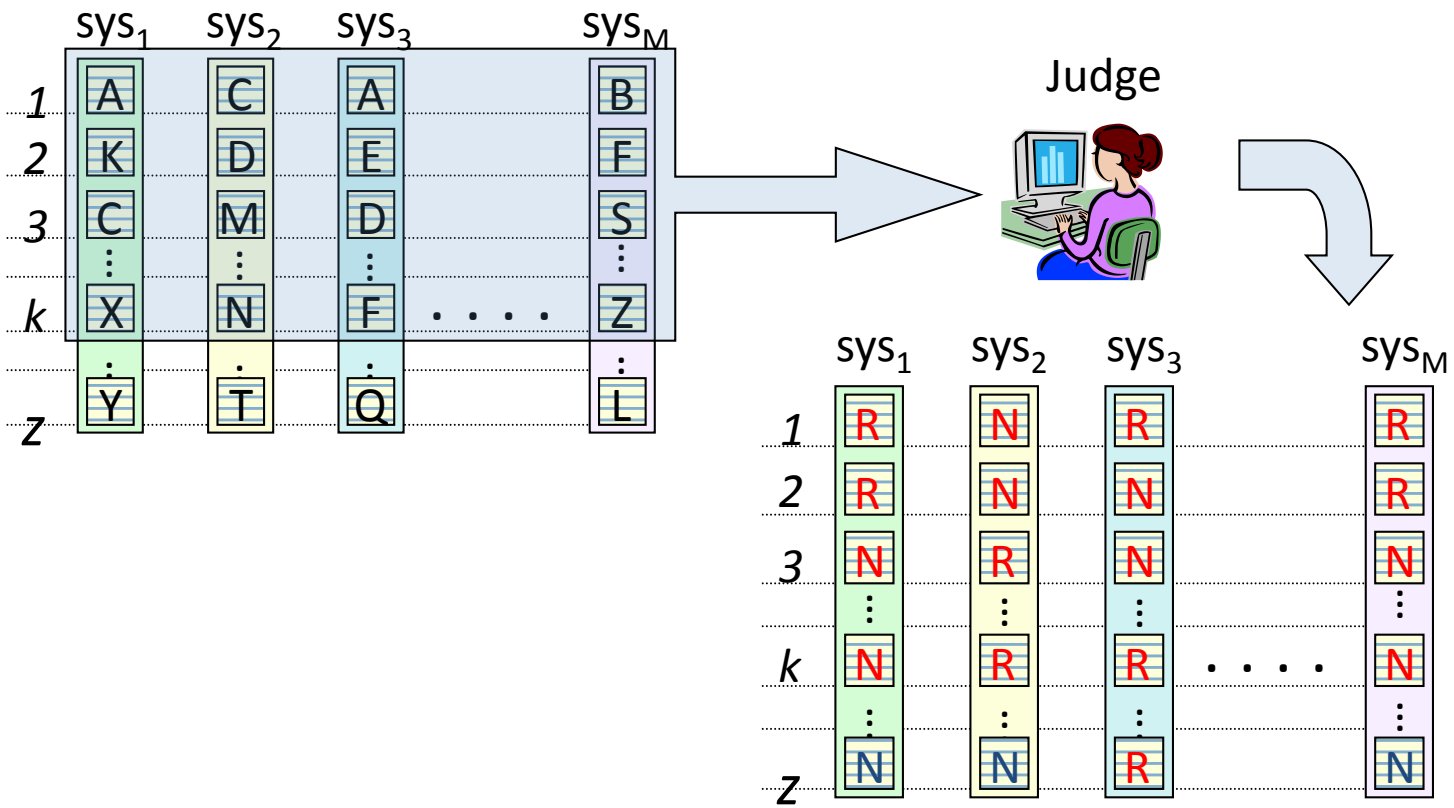
Reusability

- Reusability is hard to guarantee

- Can we test for reusability?
  - Leave-one-out

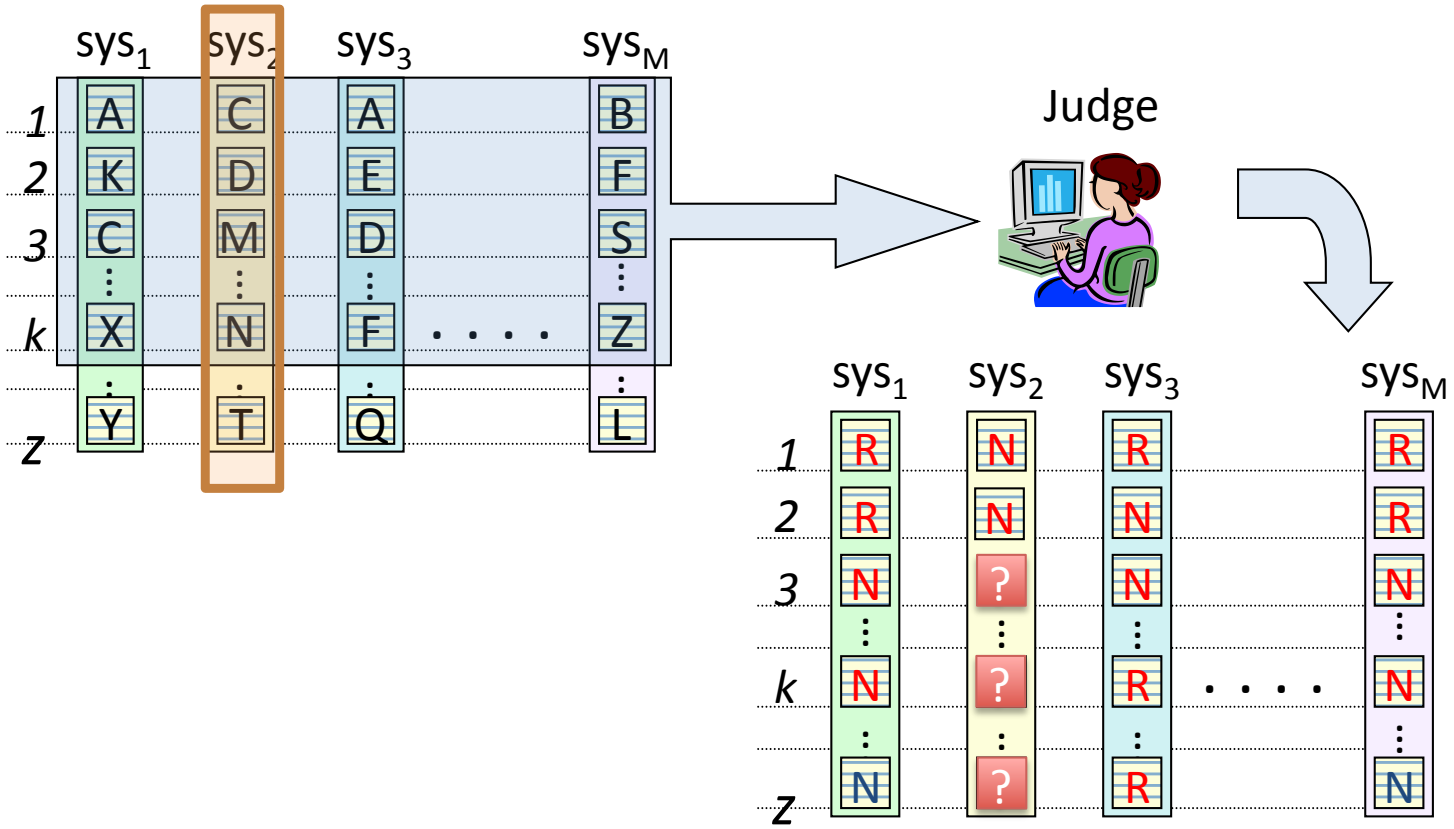
    [Zobel SIGIR98, Voorhees CLEF01, Sanderson and Joho SIGIR04, Sakai CIKM08]
  - Through Experimental Design
    - Carterette et al. SIGIR10

Reusability - Leave-one-out



Reusability - Leave-one-out

- Assume that a systems did not participate
- Evaluate the non-participating system with the rest of the judgments



Reusability

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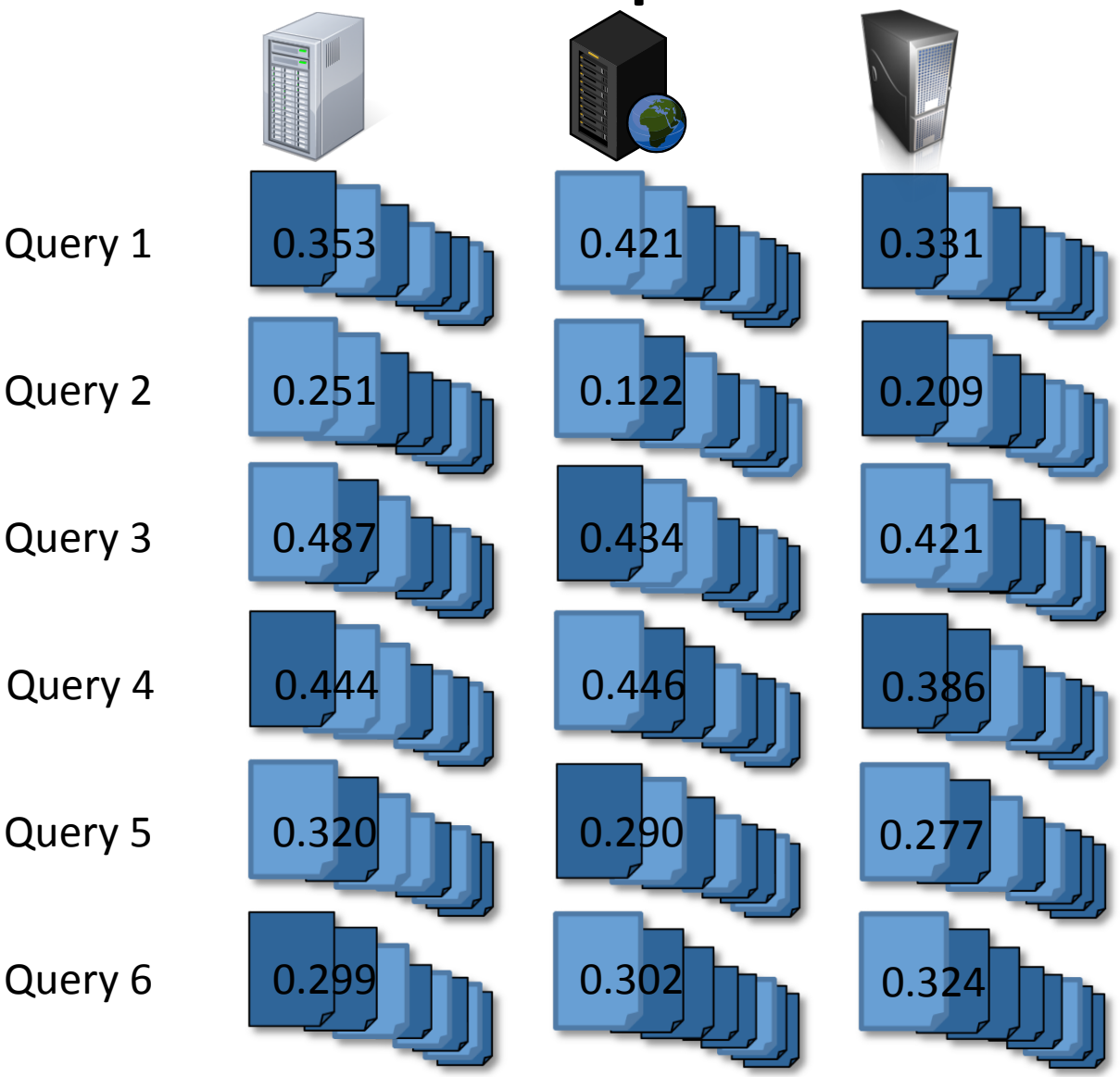
Low-Cost Experimental Design



Sampling and Reusability

- Does sampling produce a reusable collection?
  - We don't know…
  - … and we can't simulate it
- Holding systems out would produce a different sample
  - Meaning we would need judgments that we don't have

Experimenting on Reusability

- Our goal is to define an experimental design that will allow us to simultaneously:
  - Acquire relevance judgments
  - Test hypotheses about differences between systems
  - Test reusability of the topics and judgments

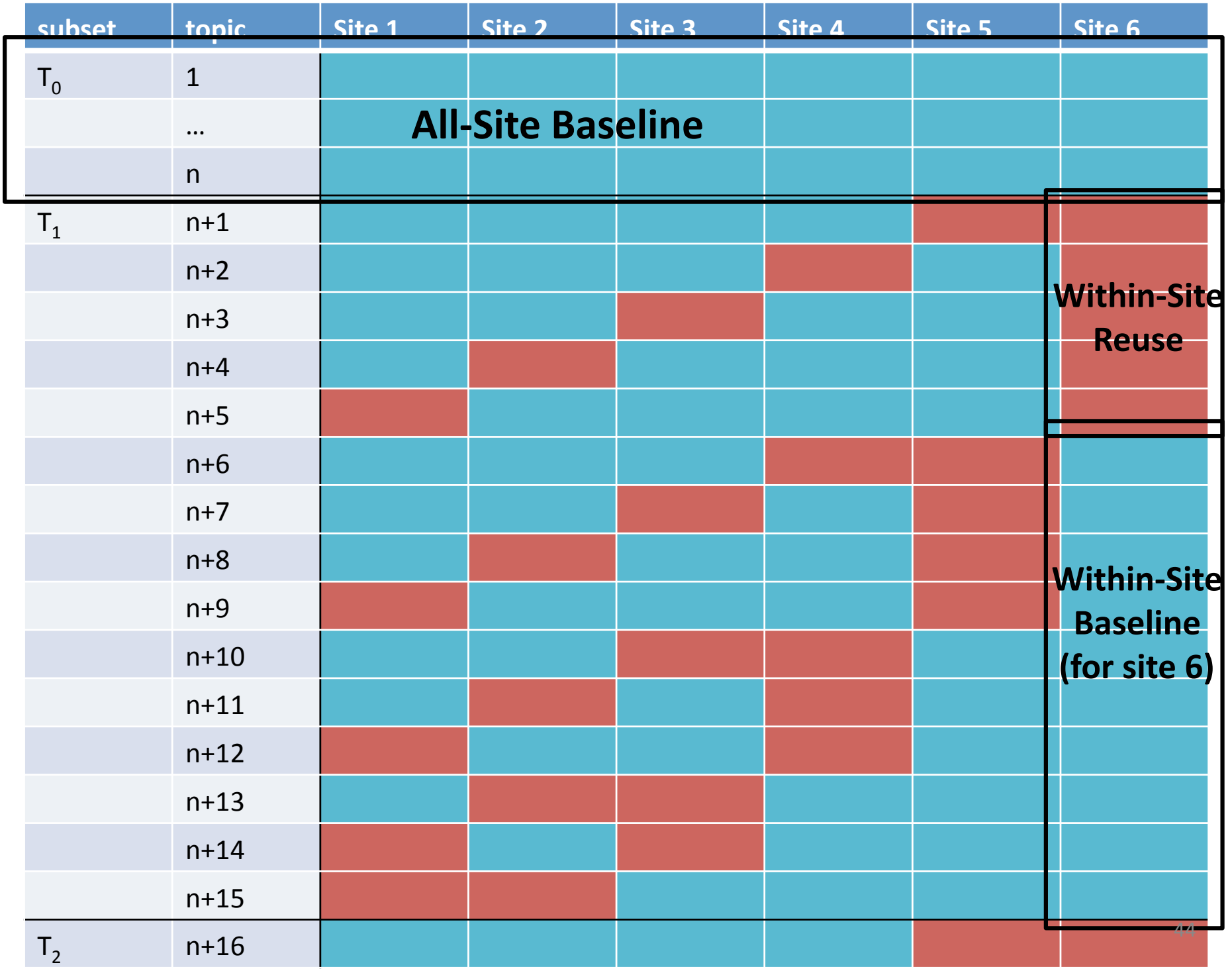
- What does it mean to "test reusability"?
  - Test a null hypothesis that the collection *is* reusable
  - Reject that hypothesis if the data demands it
  - Never *accept* that hypothesis

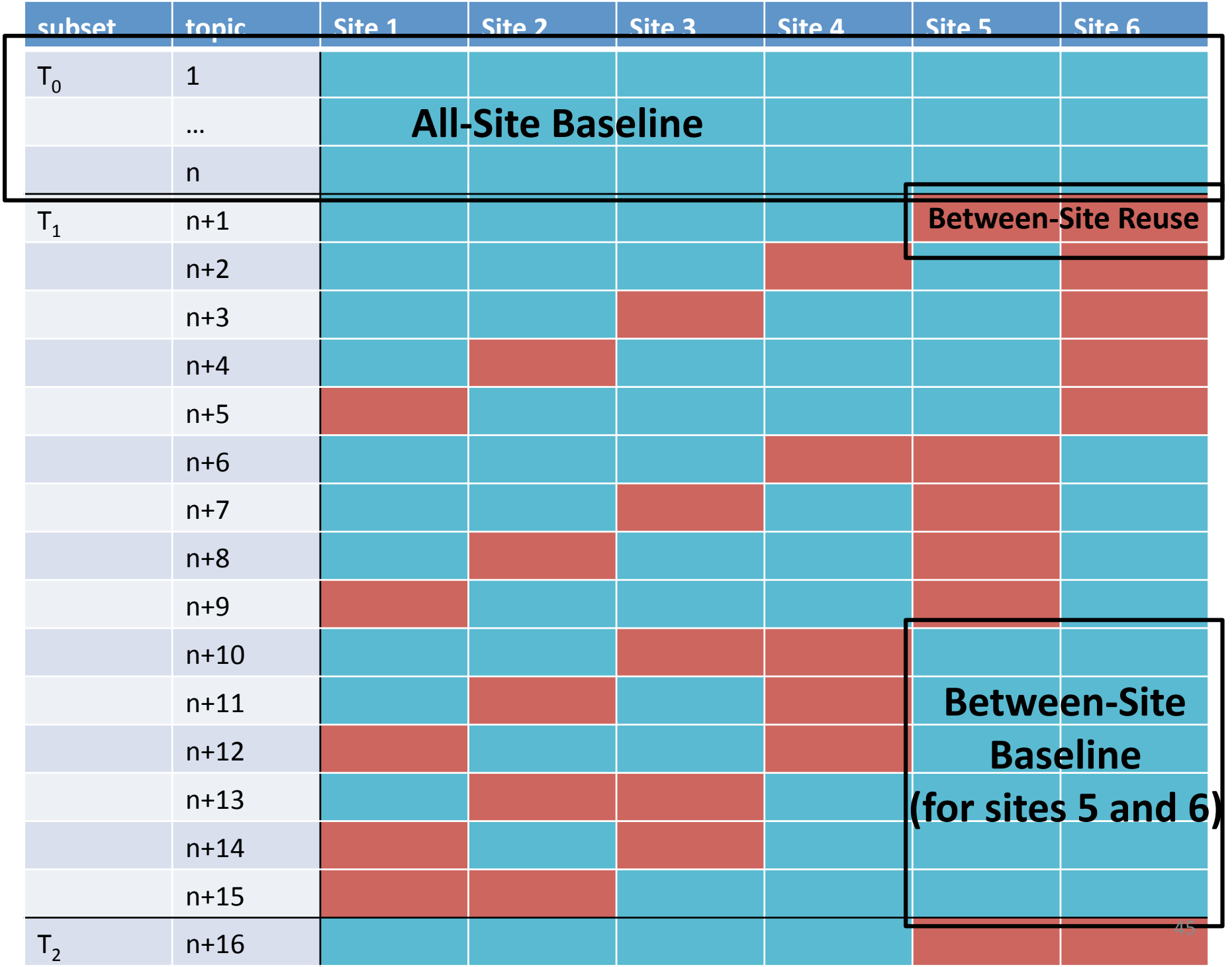
Reusability for Evaluation

- We focus on evaluation (rather than training, failure analysis, etc)

- Three types of evaluation:
  - **Within-site**: a group wants to internally evaluate their systems
  - **Between-site**: a group wants to compare their systems to those of another group
  - **Participant-comparison**: a group wants to compare their systems to those that participated in the original experiment (e.g. TREC)

- We want data for each of these


subset
topic
Site 1
Site 2
Site 3
Site 4
Site 5
Site 6
T_0
1
…
n
All-Site Baseline
T_1
n+1
n+2
n+3
n+4
n+5
n+6
n+7
n+8
n+9
n+10
n+11
n+12
n+13
n+14
n+15
T_2
n+16
Within-Site Reuse
Within-Site Baseline (for site 6)


subset
topic
Site 1
Site 2
Site 3
Site 4
Site 5
Site 6
T_0
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All-Site Baseline
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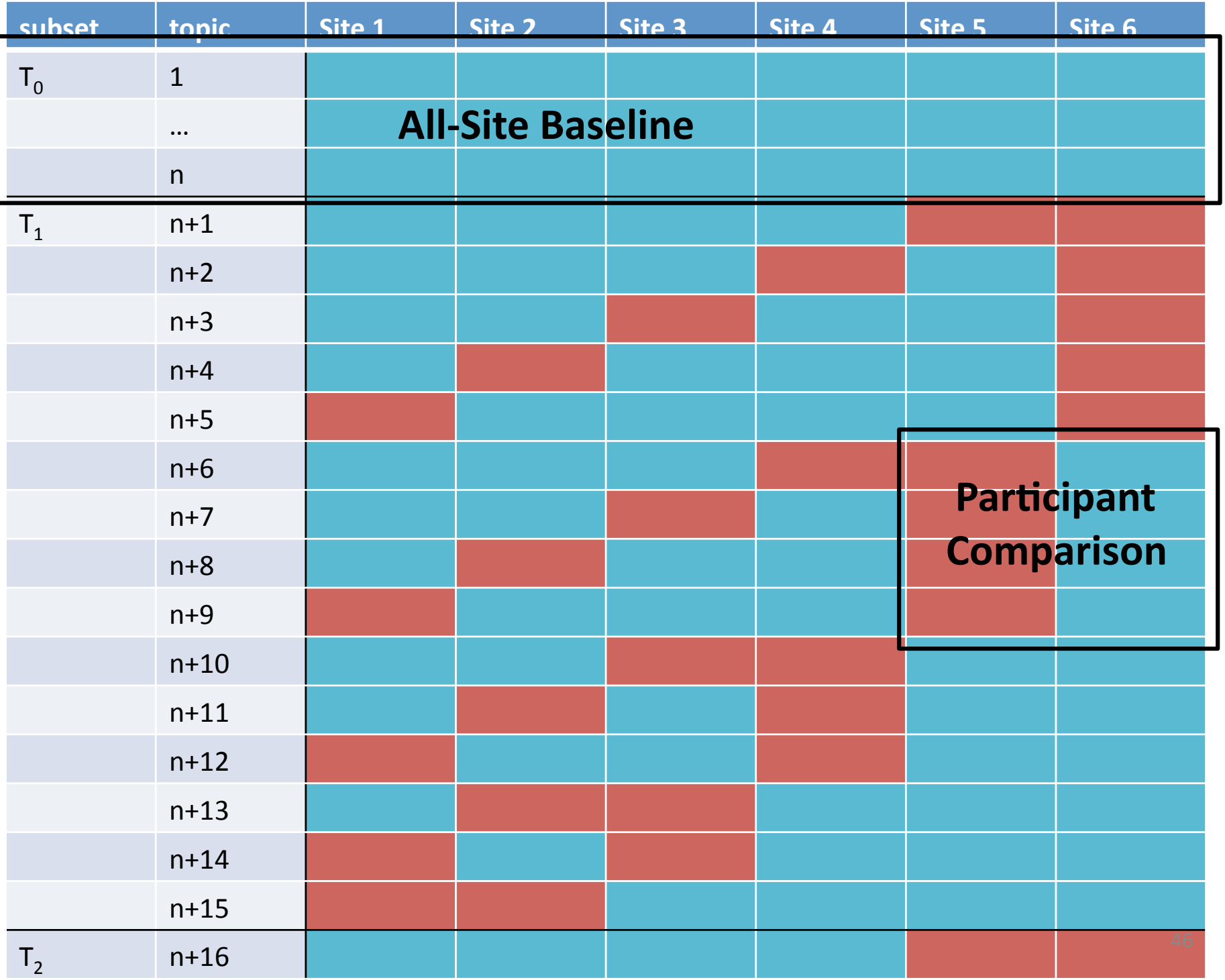
n+3
n+4
n+5
n+6
n+7
n+8
n+9
n+10
n+11
n+12
n+13
n+14
n+15
T_2
n+16
Between-Site Reuse
Between-Site Baseline (for sites 5 and 6)


subset
topic
Site 1
Site 2
Site 3
Site 4
Site 5
Site 6
T_0
1
…
n
All-Site Baseline
T_1
n+1
n+2
n+3
n+4
n+5
n+6
n+7
n+8
n+9
n+10
n+11
n+12
n+13
n+14
n+15
T_2
n+16
Participant Comparison

Statistical Analysis

- Goal of statistical analysis is to try to reject the hypothesis about reusability
  - Show that the judgments are *not* reusable
- Three approaches:
  - Show that measures such as average precision on the baseline sets do not match measures on the reuse sets
  - Show that significance tests in the baseline sets do not match significance tests in the reuse sets
  - Show that rankings in the baseline sets do not match rankings in the reuse sets
- Note: *within confidence intervals!*

Agreement in Significance

- Perform significance tests on:
  - all pairs of systems in a baseline set
  - all pairs of systems in a reuse set

- If the aggregate outcomes of the tests disagree significantly, reject reusability

Within-Site Example

- Some site submitted five runs to the TREC 2004 Robust track

- Within-site baseline: 210 topics
- Within-site reuse: 39 topics

- Perform 5*4/2 = 10 paired t-tests with each group of topics
- Aggregate agreement in a contingency table

Within-Site Example

	baseline tests	
reuse tests	**$p < 0.05$**	**$p \geq 0.05$**
$p' < 0.05$	6	0
$p' \geq 0.05$	3	1

- 3 significant differences in baseline set that are not significant in reuse set
  - ➔ 70% agreement
- … is that bad?

Expected Errors

- Compare observed error rate to expected error rate

- To estimate expected error rate, use *power analysis* (Cohen, 1992)
  - What is the probability that the observed difference over 210 topics would be found significant?
  - What is the probability that the observed difference over 39 topics would be found significant?
  - Call these probabilities q_1, q_2

Expected Errors

- For each pair of runs:
  - q_1 = probability that observed difference is significant over 210 queries
  - q_2 = probability that observed difference is significant over 39 queries

  - Expected number of true positives += $q_1 * q_2$
  - Expected number of false positives += $q_1 * (1-q_2)$
  - Expected number of false negatives += $(1-q_1) * q_2$
  - Expected number of true negatives += $(1-q_1) * (1-q_2)$

Observed vs Expected Errors

- Observed:

	baseline tests	
reuse tests	p < 0.05	p ≥ 0.05
p' < 0.05	6	0
p' ≥ 0.05	3	1

- Expected:

	baseline tests	
reuse tests	p < 0.05	p ≥ 0.05
p' < 0.05	7.098	0.073
p' ≥ 0.05	2.043	0.786

- Perform a X^2 goodness-of-fit test to compare the tables
- p-value = 0.88
- Do not reject reusability (for new systems like these)

Validation of Design and Analysis

- Three tests:
  - Will we reject reusability when it is not true?
  - When reusability is “true”, will the design+analysis be robust to random differences in topic sets?
  - When reusability is “true”, will the design+analysis be robust to random differences in held-out sites?

Differences in Topic Samples

- Set-up: simulate design, but guarantee reusability
  - Randomly choose k sites to hold out
  - Use to define the baseline and reuse sets
  - Performance measure on each system/topic is simply the one calculated using the original judgments

- Reusability is true because all measures are exactly the same as when sites are held out

Observed vs Expected Errors (Within-Site)

- Observed:

	baseline tests	
reuse tests	$p < 0.05$	$p \geq 0.05$
$p' < 0.05$	196	2
$p' \geq 0.05$	57	45

- Expected:

	baseline tests	
reuse tests	$p < 0.05$	$p \geq 0.05$
$p' < 0.05$	189.5	4.3
$p' \geq 0.05$	62.1	44.1

- Perform a X^2 goodness-of-fit test to compare the tables
- p-value = 0.58
- Do not reject reusability (for new systems like these)

based on TREC 2008 Million Query track data

Differences in Held-Out Sites

- Set-up: simulation of design with TREC Robust data (249 topics, many judgments each)
  - Randomly hold two of 12 submitting sites out
  - Simulate pools of depth 10, 20, 50, 100
  - Calculate average precision over simulated pool
- Previous work suggests reusability is true
- (Only within-site analysis is possible)

Observed vs Expected Errors (Within-Site)

- Observed:

	baseline tests	
reuse tests	p < 0.05	p ≥ 0.05
p' < 0.05	130	17
p' ≥ 0.05	127	160

- Expected:

	baseline tests	
reuse tests	p < 0.05	p ≥ 0.05
p' < 0.05	135.4	13.9
p' ≥ 0.05	121.6	163.1

- Perform a X^2 goodness-of-fit test to compare the tables
- p-value = 0.74
- Do not reject reusability (for new systems like these)

based on TREC 2004 Robust track data

Course Outline

- Intro to evaluation
  - Evaluation methods, test collections, measures, comparable evaluation
- Low cost evaluation
- Advanced user models
  - Web search models, novelty & diversity, sessions
- Reliability
  - Significance tests, reusability
- Other evaluation setups

Evaluation using crowd-sourcing

- Sheng et al KDD 08, Bailey et al SIGIR08, Alonso and Mizarro SIGIR09, Kazai et al. SIGIR09, Yang et al WSDM09, Tang and Sanderson ECIR10, Sanderson et al SIGIR10

- Cheap but noisy judgments
- Large load under a single assessor per topic
  - Can you motivate an Mturker to judge ~1,500 documents
  - Multiple assessors (not MTurkers) per topics works fine [Trotman and Jenkinson ADCS07]
- Inconsistency across assessors
  - Malicious activity?
  - Noise?
  - Diversity in information needs (query aspects)?

Online Evaluation

- Joachims et al SIGIR05, Radlinski et al CIKM08, Wang et al KDD09, …

- Use clicks as indication of relevance?
- Rank bias
  - Users tend to click on documents at the top of the list independent of their relevance
- Quality bias
  - Users tend to click on less relevant documents if the overall quality of the search engine is poor

Online Evaluation

- Evaluate by watching user behaviour:
  - Real user enters a query
  - Record how the users respond
  - Measure statistics about these responses

- Common online evaluation metrics
  - Click-through rate
    - Assumes more clicks means better results
  - Queries per user
    - Assumes users come back more with better results
  - Probability user skips over results they have considered (pSkip)

Online Evaluation

- Interleaving [Radlinski et al CIKM08]
  - A way to compare rankers online
    - Given the two rankings produced by two methods
    - Present a combination of the rankings to users
    - Ranking providing more of the clicked results wins
  - Treat a flight as an active experiment

Team Draft Interleaving

Ranking A

1. Napa Valley – The authority for lodging...
   www.napavalley.com
2. Napa Valley Wineries - Plan your wine...
   www.napavalley.com/wineries
3. Napa Valley College
   www.napavalley.edu/homex.asp
4. Been There | Tips | Napa Valley
   www.ivebeenthere.co.u
5. Napa Valley Wineries an
   www.napavintners.com
6. Napa Country, California
   en.wikipedia.org/wiki/N

Ranking B

1. Napa Country, California – Wikipedia
   en.wikipedia.org/wiki/Napa_Valley
2. Napa Valley – The authority for lodging...
   www.napavalley.com
3. Napa: The Story of an American Eden...
   books.google.co.uk/books?isbn=...
4. Napa Valley Hotels – Bed and Breakfast...
   .com

Presented Ranking

1. Napa Valley – The authority for lodging...
   www.napavalley.com
2. Napa Country, California – Wikipedia
   en.wikipedia.org/wiki/Napa_Valley
3. Napa: The Story of an American Eden...
   books.google.co.uk/books?isbn=...
4. Napa Valley Wineries – Plan your wine...
   www.napavalley.com/wineries
5. Napa Valley Hotels – Bed and Breakfast...
   www.napalinks.com
6. Napa Valley College
   www.napavalley.edu/homex.asp
7. NapaValley.org
   www.napavalley.org

Click

Click

B wins!

Credit Assignment

- The "team" with more clicks wins
  - Randomization removes presentation order bias
- Each impression with clicks gives a preference for one of the rankings (unless there is a tie)
  - By design: If the input rankings are equally good, they have equal chance of winning
- Statistical test to run: ignoring ties, is the fraction of impressions where the new method wins statistically different from 50%?

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By the end of this course…

You will be able to evaluate your retrieval algorithms

A. At low cost
B. Reliably
C. Effectively

Many thanks to Mark Sanderson @RMIT,
for some of the significance testing slides

Many thanks to Filip Radlinski @MSR,
for many of the online evaluation slides