IEEEVR2014
MINNESOTA

Quantitative and Qualitative Methods for Human-Subject Experiments in Augmented Reality

IEEE VR 2014 Tutorial

J. Edward Swan II, Mississippi State University (organizer)
Joseph L. Gabbard, Virginia Tech (co-organizer)

Schedule

9:00–10:30	1.5 hrs	Experimental Design and Analysis	Ed
10:30–11:00	0.5 hrs	Coffee Break	
11:00–12:30	1.5 hrs	Experimental Design and Analysis / Formative Usability Evaluation	Ed / Joe
12:30–2:00	1.5 hrs	Lunch Break	
2:00–3:30	1.5 hrs	Formative Usability Evaluation	Joe
3:30–4:00	0.5 hrs	Coffee Break	
4:00–5:30	1.5 hrs	Formative Usability Evaluation / Panel Discussion	Joe / Ed

Experimental Design and Analysis

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Motivation and Goals

- **Course attendee backgrounds?**

- **Studying experimental design and analysis at Mississippi State University:**
  - PSY 3103 Introduction to Psychological Statistics
  - PSY 3314 Experimental Psychology
  - PSY 6103 Psychometrics
  - PSY 8214 Quantitative Methods In Psychology II
  - PSY 8803 Advanced Quantitative Methods
  - IE 6613 Engineering Statistics I
  - IE 6623 Engineering Statistics II
  - ST 8114 Statistical Methods
  - ST 8214 Design & Analysis Of Experiments
  - ST 8853 Advanced Design of Experiments I
  - ST 8863 Advanced Design of Experiments II

- **7 undergrad hours; 30 grad hours; 3 departments!**

Motivation and Goals

- What can we accomplish in one day?
- Study subset of basic techniques
  - Presenters have found these to be the most applicable to VR, AR systems
- Focus on intuition behind basic techniques
- Become familiar with basic concepts and terms
  - Facilitate working with collaborators from psychology, industrial engineering, statistics, etc.

Outline

- *Experimental Validity*
- **Experimental Design**
- **Describing Data**
  - **Graphing Data**
  - **Descriptive Statistics**
- **Inferential Statistics**
  - **Hypothesis Testing**
  - **Power**
- **Graphical Data Analysis**

The Empirical Method

- The *Empirical Method*:
  - Develop a hypothesis, perhaps based on a theory
  - Make the hypothesis testable
  - Develop an empirical experiment
  - Collect and analyze data
  - Accept or refute the hypothesis
  - Relate the results back to the theory
  - If worthy, communicate the results to scientific community
- Statistics:
  - For empirical work, necessary but not sufficient
  - Often not useful for managing problems of gathering, interpreting, and communicating empirical information.

Designing Valid Empirical Experiments

- Experimental Validity
  - Does experiment really measure what we want it to measure?
  - Do our results really mean what we think (and hope) they mean?
  - Are our results reliable?
    - If we run the experiment again, will we get the same results?
    - Will others get the same results?

- Validity is a large topic in empirical inquiry

Experimental Variables

- Independent Variables
  - What the experiment is studying
  - Occur at different levels
    - Example: stereopsis, at the levels of stereo, mono
  - Systematically varied by experiment

- Dependent Variables
  - What the experiment measures
  - Assume dependent variables will be effected by independent variables
  - Must be measurable quantities
    - Time, task completion counts, error counts, survey answers, scores, etc.
    - Example: VR navigation performance, in total time

Experimental Variables

- Independent variables can vary in two ways
  - Between-subjects: each subject sees a different level of the variable
    - Example: ½ of subjects see stereo, ½ see mono
  - Within-subjects: each subject sees all levels of the variable
    - Example: each subject sees both stereo and mono
- Confounding factors (or confounding variables)
  - Factors that are not being studied, but will still affect experiment
    - Example: stereo condition less bright than mono condition
  - Important to predict and control confounding factors, or experimental validity will suffer

Experimental Design

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- *Experimental Design*
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Experimental Designs

- 2 x 1 is simplest possible design, with one independent variable at two levels:

Variable
level 1
level 2

Stereopsis
stereo
mono

- Important confounding factors for within subject variables:
  - Learning effects
  - Fatigue effects
- Control these by counterbalancing the design
  - Ensure no systematic variation between levels and the order they are presented to subjects

Subjects	1st condition	2nd condition
1, 3, 5, 7	stereo	mono
2, 4, 6, 8	mono	stereo

Factorial Designs

- *n* x 1 designs generalize the number of levels:

VE terrain type
flat
hilly
mountainous

- Factorial designs generalize number of independent variables and the number of levels of each variable
- Examples: *n* x *m* design, *n* x *m* x *p* design, etc.
- Must watch for factorial explosion of design size!

3 x 2 design:

	Stereopsis	
VE terrain type	stereo	mono
flat		
hilly		
mountainous		

Cells and Repetitions

- **Cell**: each combination of levels
- **Repetitions**: typically, the combination of levels at each cell is repeated a number of times

	Stereopsis	
VE terrain type	stereo	mono
flat		
hilly		
mountainous		

cell

- Example of how this design might be described:
  - "A 3 (VE terrain type) by 2 (stereopsis) within-subjects design, with 4 repetitions of each cell."
  - This means each subject would see 3 x 2 x 4 = 24 total conditions
  - The presentation order would be counterbalanced

Counterbalancing

- Addresses time-based confounding factors:
  - Within-subjects variables: control learning and fatigue effects
  - Between-subjects variables: control calibration drift, weather, other factors that vary with time

- There are two counterbalancing methods:
  - Random permutations
  - Systematic variation
    - Latin squares are a very useful and popular technique

$$\begin{bmatrix} 1 & 2 \\ 2 & 1 \end{bmatrix}$$

2 x 2

$$\begin{bmatrix} 1 & 2 & 3 \\ 2 & 3 & 1 \\ 3 & 1 & 2 \end{bmatrix}$$

$$\begin{bmatrix} 1 & 3 & 2 \\ 2 & 1 & 3 \\ 3 & 2 & 1 \end{bmatrix}$$

6 x 3 (there is no 3 x 3 that has all 3 properties)

$$\begin{bmatrix} 1 & 2 & 3 & 4 \\ 2 & 4 & 1 & 3 \\ 3 & 1 & 4 & 2 \\ 4 & 3 & 2 & 1 \end{bmatrix}$$

4 x 4

- Latin square properties:
  - Every level appears in every position the same number of times
  - Every level is followed by every other level
  - Every level is preceded by every other level

Counterbalancing Example

- "A 3 (VE terrain type) by 2 (stereopsis) within-subjects design, with 4 repetitions of each cell."
- Form Cartesian product of Latin squares {6 x 3} (VE Terrain Type) ⊗ {2 x 2} (Stereopsis)
- Perfectly counterbalances groups of 12 subjects

Subject	Presentation Order
1	1A, 1B, 2A, 2B, 3A, 3B
2	1B, 1A, 2B, 2A, 3B, 3A
3	2A, 2B, 3A, 3B, 1A, 1B
4	2B, 2A, 3B, 3A, 1B, 1A
5	3A, 3B, 1A, 1B, 2A, 2B
6	3B, 3A, 1B, 1A, 2B, 2A
7	1A, 1B, 3A, 3B, 2A, 2B
8	1B, 1A, 3B, 3A, 2B, 2A
9	2A, 2B, 1A, 1B, 3A, 3B
10	2B, 2A, 1B, 1A, 3B, 3A
11	3A, 3B, 2A, 2B, 1A, 1B
12	3B, 3A, 2B, 2A, 1B, 1A

$$\begin{bmatrix} 1 & 2 & 3 \\ 2 & 3 & 1 \\ 3 & 1 & 2 \end{bmatrix}$$

$$\begin{bmatrix} 1 & 3 & 2 \\ 2 & 1 & 3 \\ 3 & 2 & 1 \end{bmatrix}$$

$$\begin{bmatrix} A & B \\ B & A \end{bmatrix}$$

Types of Statistics

- Descriptive Statistics:
  - Describe and explore data
  - All types of graphs and visual representations
  - Summary statistics:
    many numbers → few numbers
  - Data analysis begins with descriptive stats
    - Understand data distribution
    - Test assumptions of significance tests

- Inferential Statistics:
  - Detect relationships in data
  - Significance tests
  - Infer population characteristics from sample characteristics

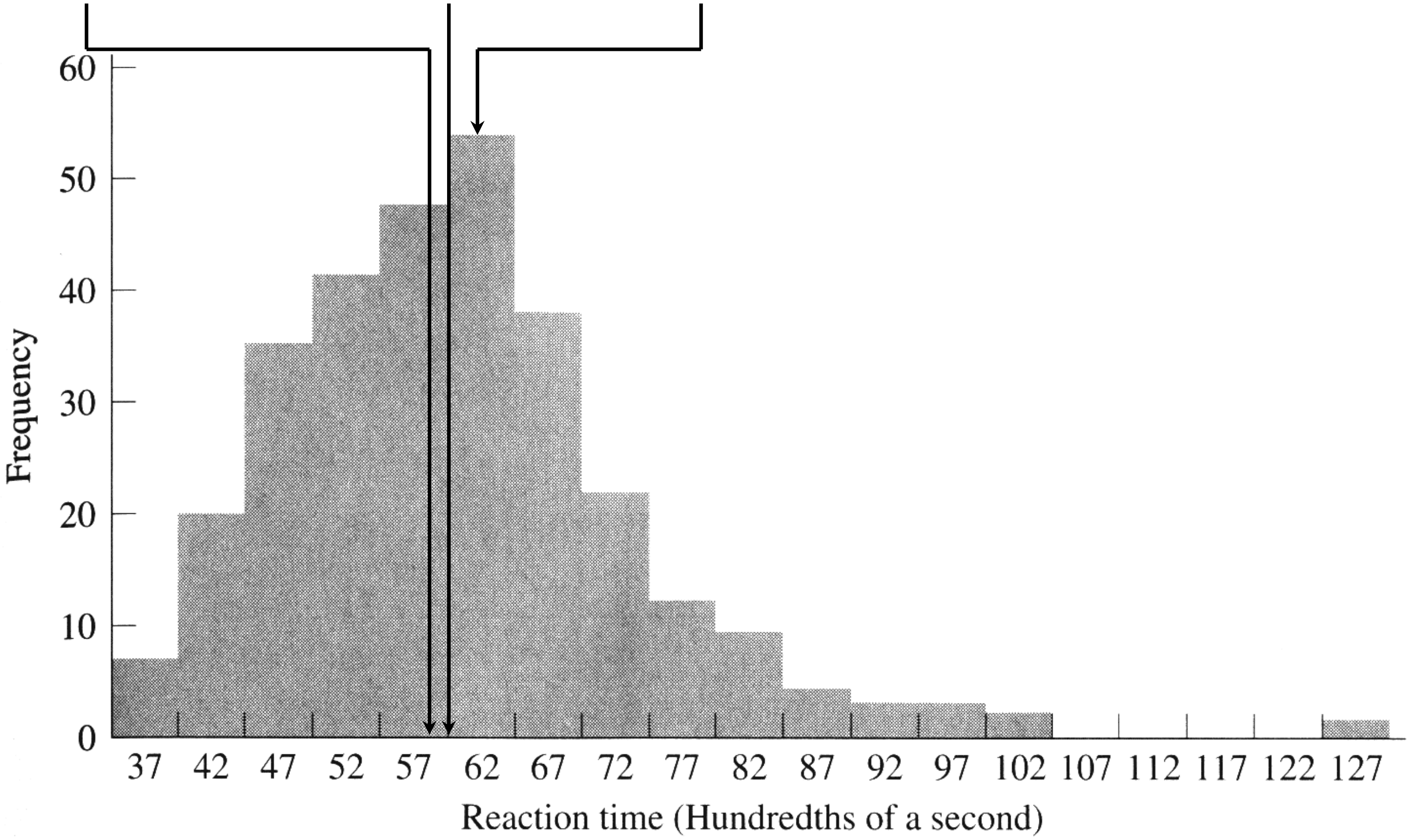
Graphing Data

- Experimental Validity
- Experimental Design
- *Describing Data*
  - *Graphing Data*
  - Descriptive Statistics
- Inferential Statistics
  - Hypothesis Testing
  - Power
- Graphical Data Analysis

Exploring Data with Graphs

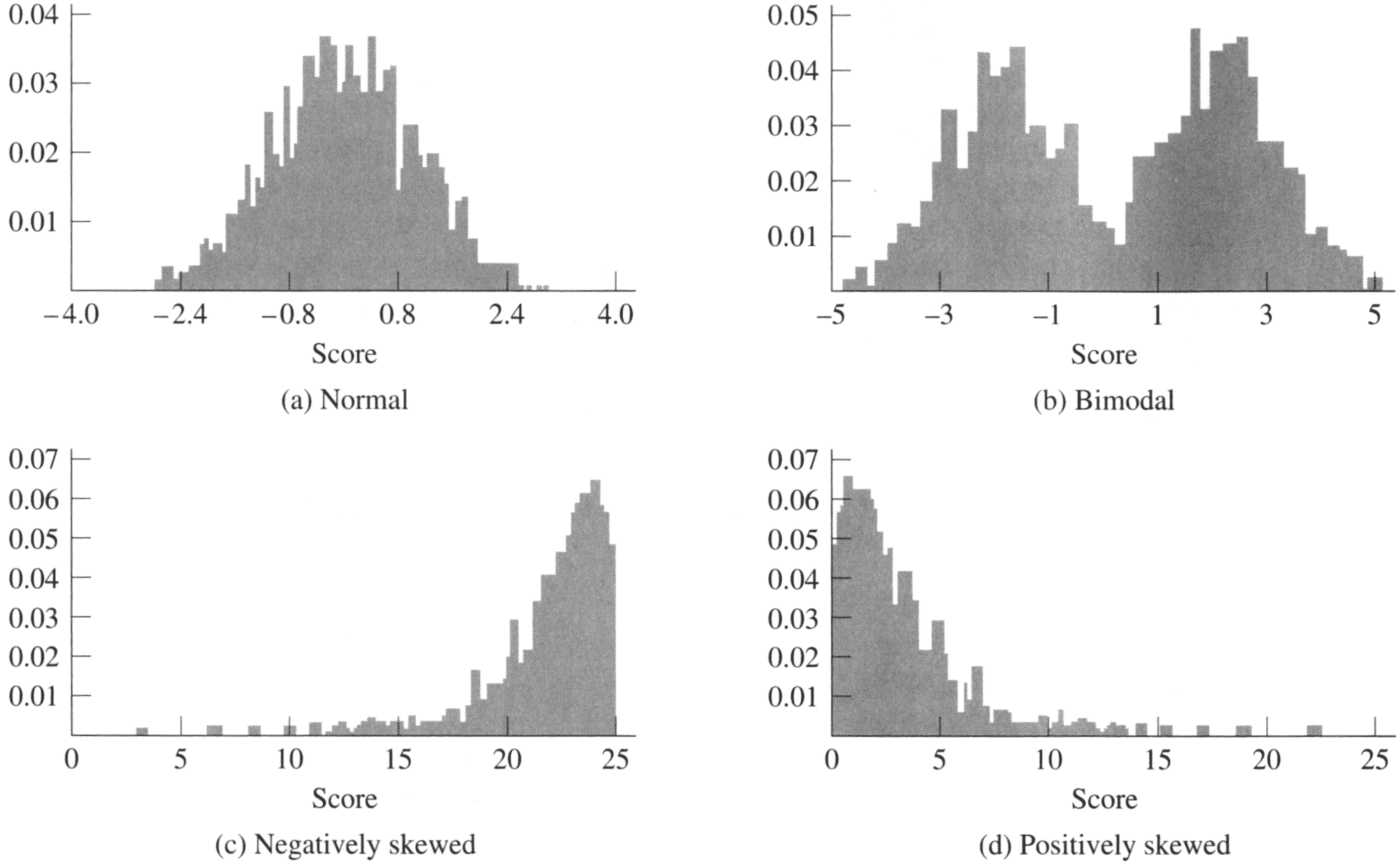
- **Histogram common data overview method**

median = 59.5　mean = 60.26　mode = 62



From [Howell 02] p 21

Classifying Data with Histograms



(a) Normal

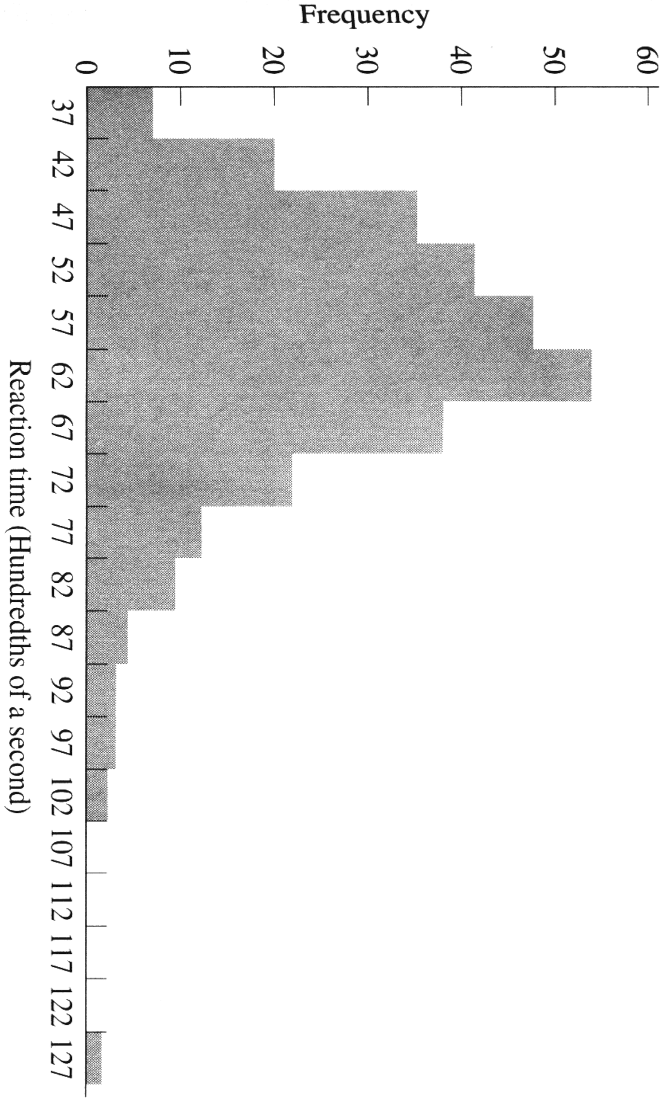
(b) Bimodal

(c) Negatively skewed

(d) Positively skewed

From [Howell 02] p 28

Stem-and-Leaf: Histogram From Actual Data

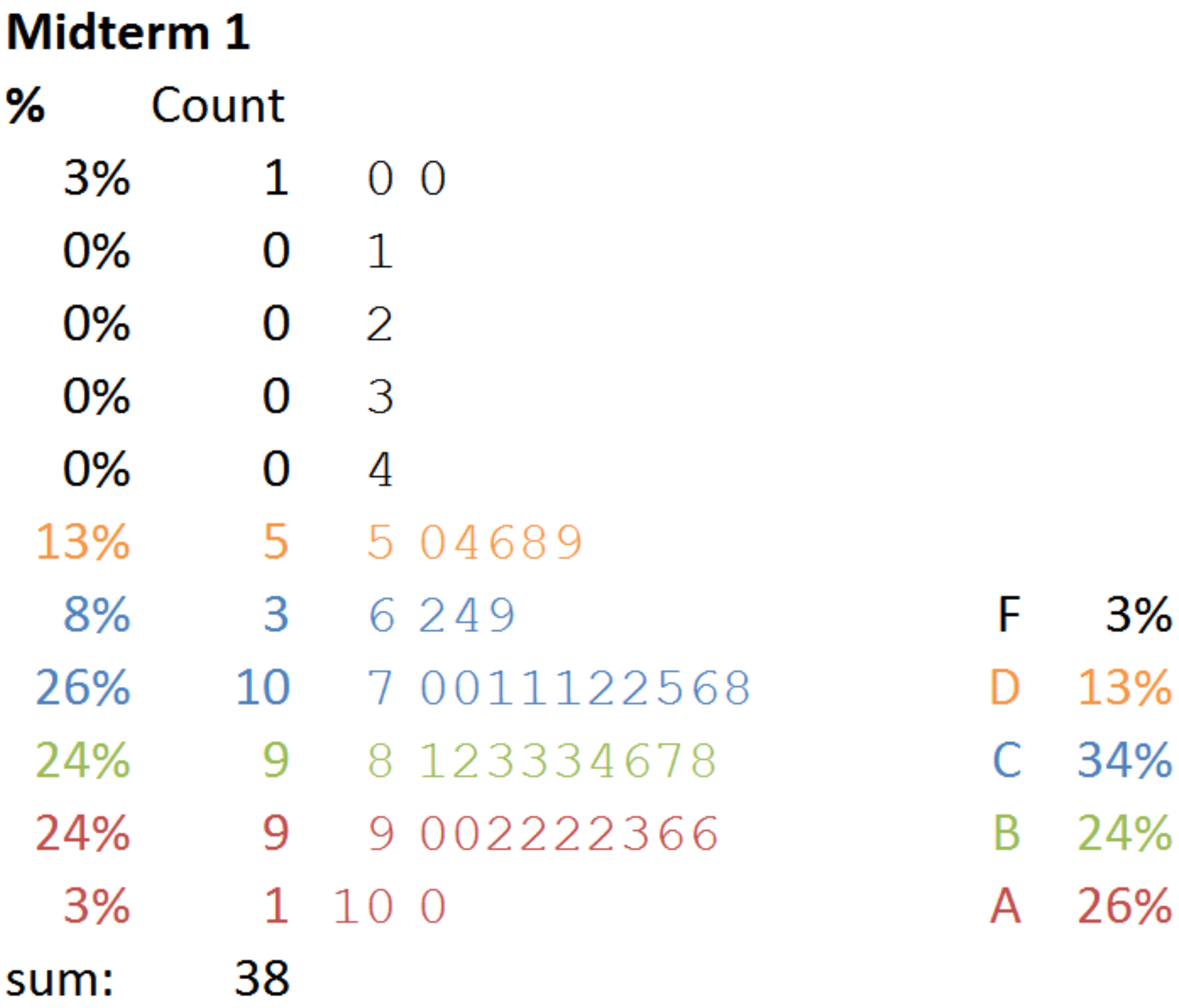


Raw Data	Stem	Leaf
36 37 38 38 39 39 39 40	3s	67
40 40 40 41 41 41 42 42	3.	88999
42 43 43 43 43 43 44 44	4*	0000111
44 44 44 45 45 45 45 45	4t	22233333
45 46 46 46 46 46 46 46	4f	44444555555
46 46 46 46 47 47 47 47	4s	66666666666777777777
47 47 47 47 47 48 48 48	4.	888899999
48 49 49 49 49 49 50 50	5*	00000111111111111
50 50 50 51 51 51 51 51	5t	222222222233333333
51 51 51 51 51 51 51 52	5f	4444445555555
52 52 52 52 52 52 52 52	5s	66666666667777777
52 53 53 53 53 53 53 53	5.	88888888888899999999999
53 54 54 54 54 54 54 55	6*	00000000000011111111111
55 55 55 55 55 55	6t	222222222222223333333333
	6f	444444455555555
	6s	6666666677777777777777
	6.	889999999
	7*	01111
	7t	22222222333
	7f	44444455
	7s	666677
	7.	88899
	8*	00011
	8t	2333
	8f	5
	8s	67
	8.	8
	9*	0
	9t	
	9f	4455
	9s	
	9.	8
	High	104; 104; 125

FIGURE 2.4 *Stem-and-leaf display for reaction time data*

From [Howell 02] p 21, 23

Stem-and-Leaf: Histogram From Actual Data



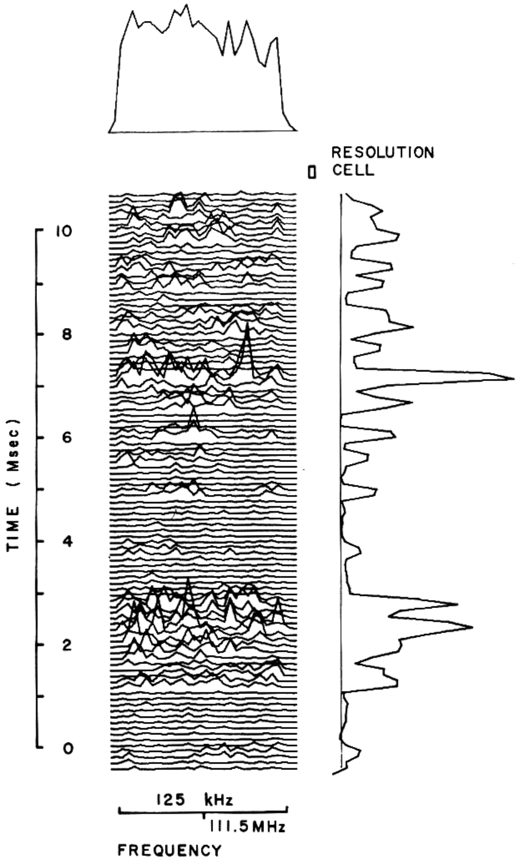
Midterm 1

%	Count				
3%	1	0	0		
0%	0	1			
0%	0	2			
0%	0	3			
0%	0	4			
13%	5	5	04689		
8%	3	6	249	F	3%
26%	10	7	0011122568	D	13%
24%	9	8	123334678	C	34%
24%	9	9	002222366	B	24%
3%	1	10	0	A	26%
sum:	38				

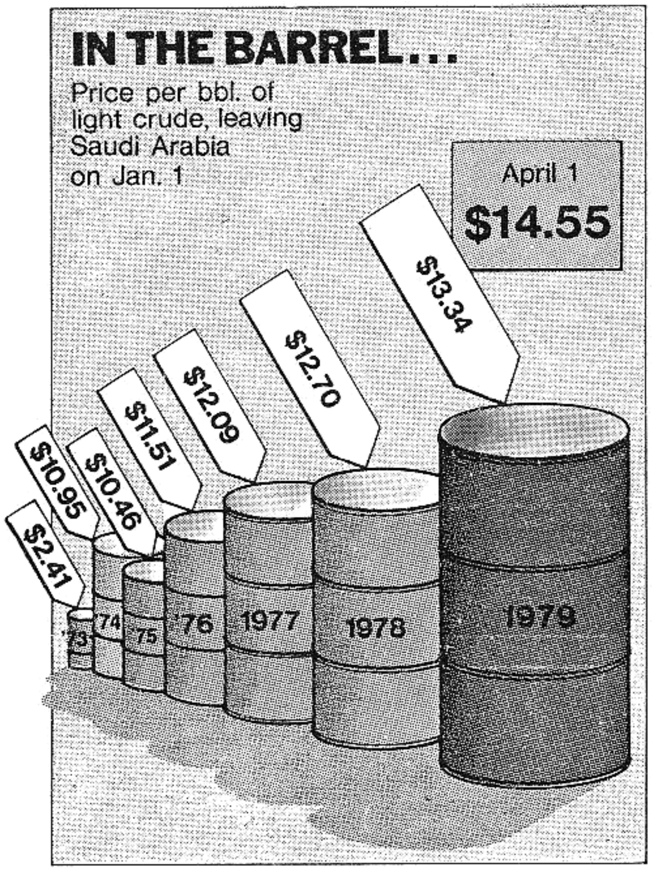
Grades from my fall 2011 Formal Languages class; first midterm

We Have Only Scratched the Surface…

- There are a vary large number of graphing techniques
- Tufte's [83, 90] works are classic, and stat books show many more examples (e.g. Howell [03]).



Lots of good examples…



And plenty of bad examples!

From [Tufte 83], p 134, 62

Descriptive Statistics

- Experimental Validity
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Summary Statistics

- Many numbers → few numbers

- Measures of central tendency:
  - Mean: average
  - Median: middle data value
  - Mode: most common data value

- Measures of variability / dispersion:
  - Mean absolute deviation
  - Variance
  - Standard Deviation
  - Standard Error

Populations and Samples

- **Population:**
  - Set containing every possible element that we want to measure
  - Usually a Platonic, theoretical construct
  - Mean: μ Variance: σ^2 Standard deviation: σ

- **Sample:**
  - Set containing the elements we actually measure (our subjects)
  - Subset of related population
  - Mean: $\bar{X}$ Variance: s^2 Standard deviation: s
    Standard error: e Number of samples: N

Hypothesis Testing

- Experimental Validity
- Experimental Design
- Describing Data
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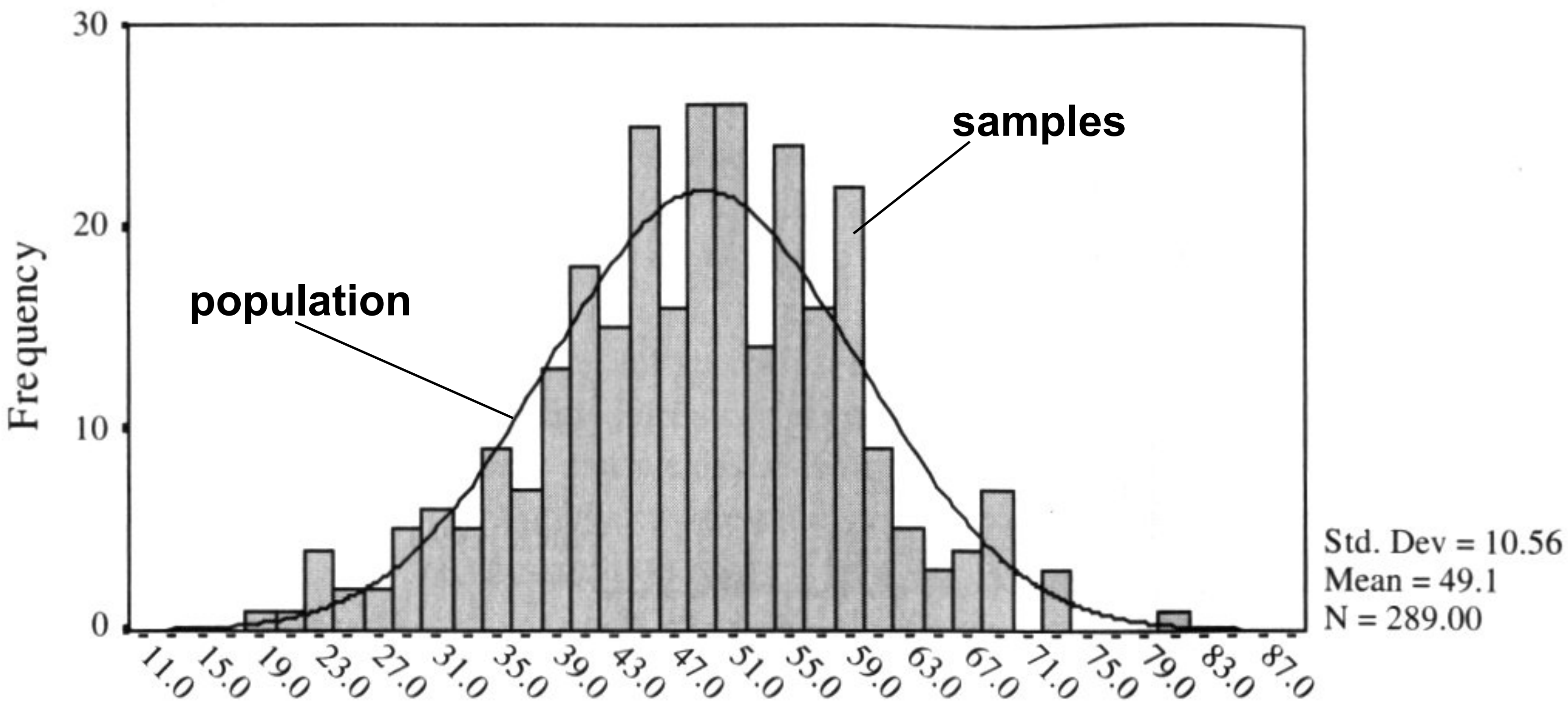
Hypothesis Testing

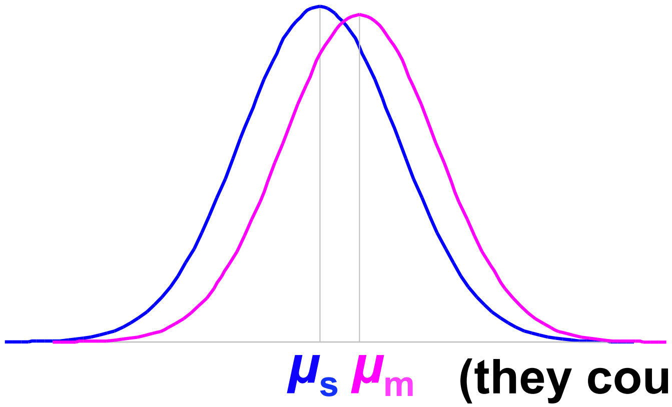
- Goal is to infer population characteristics from sample characteristics



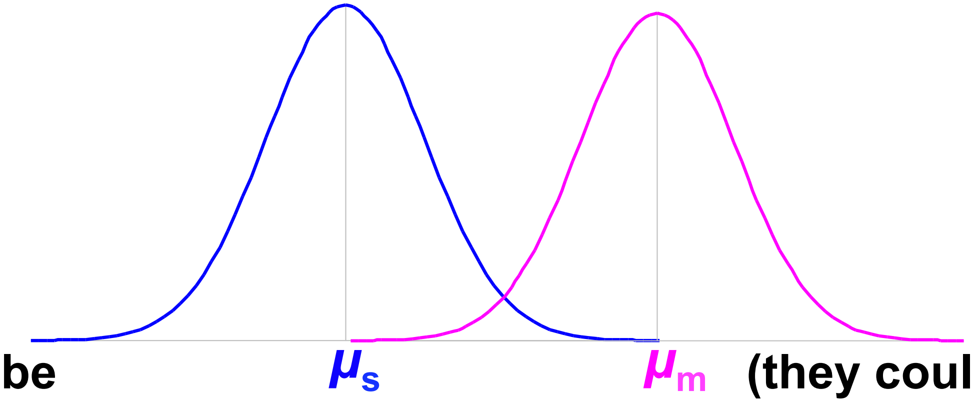
From [Howell 02], p 78

What Are the Possible Alternatives?

- Let time to navigate be μ_s: stereo time; μ_m: mono time
  - Perhaps there are two populations: $\mu_s - \mu_m = d$

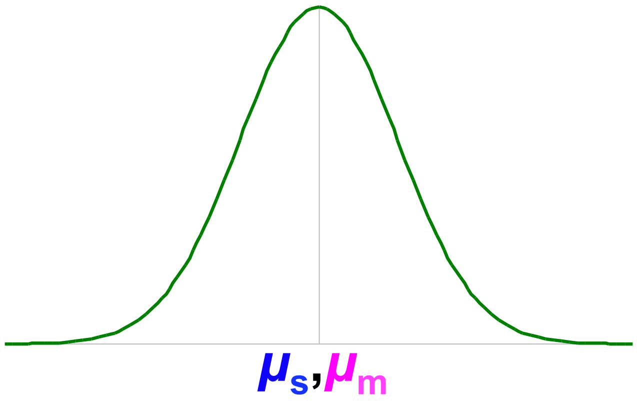


(they could be close together)



(they could be far apart)

  - Perhaps there is one population: $\mu_s - \mu_m = 0$



Hypothesis Testing Procedure

1. Develop testable hypothesis H_1: $\mu_s - \mu_m = d$
   - (E.g., subjects faster under stereo viewing)

2. Develop null hypothesis H_0: $\mu_s - \mu_m = 0$
   - Logical opposite of testable hypothesis

3. Construct sampling distribution assuming H_0 is true.

4. Run an experiment and collect samples; yielding sampling statistic X.
   - (E.g., measure subjects under stereo and mono conditions)

5. Referring to sampling distribution, calculate conditional probability of seeing X given H_0: $p(X \mid H_0)$.
   - If probability is low ($p \leq 0.05$, $p \leq 0.01$), we are unlikely to see X when H_0 is true. We reject H_0, and embrace H_1.
   - If probability is not low ($p > 0.05$), we are likely to see X when H_0 is true. We do not reject H_0.

Example 1: VE Navigation with Stereo Viewing

1. Hypothesis H_1: $\mu_s - \mu_m = d$
   - Subjects faster under stereo viewing.
2. Null hypothesis H_0: $\mu_s - \mu_m = 0$
   - Subjects same speed whether stereo or mono viewing.
3. Constructed sampling distribution assuming H_0 is true.
4. Ran an experiment and collected samples:
   - 32 subjects, collected 128 samples
   - $X_s = 36.431$ sec; $X_m = 34.449$ sec; $X_s - X_m = 1.983$ sec
5. Calculated conditional probability of seeing 1.983 sec given H_0: $p(\ 1.983 \text{ sec} \mid H_0\ ) = 0.445$.
   - $p = 0.445$ not low, we are likely to see 1.983 sec when H_0 is true. We do not reject H_0.
   - This experiment did not tell us that subjects were faster under stereo viewing.

Data from [Swan et al. 03]

Example 2: Effect of Intensity on AR Occluded Layer Perception

1. Hypothesis H_1: $\mu_c - \mu_d = d$
   - Tested constant and decreasing intensity. Subjects faster under decreasing intensity.

2. Null hypothesis H_0: $\mu_c - \mu_d = 0$
   - Subjects same speed whether constant or decreasing intensity.

3. Constructed sampling distribution assuming H_0 is true.

4. Ran an experiment and collected samples:
   - 8 subjects, collected 1728 samples
   - X_c = 2592.4 msec; X_d = 2339.9 msec; $X_c - X_d$ = 252.5 msec

5. Calculated conditional probability of seeing 252.5 msec given H_0: p(252.5 msec | H_0) = 0.008.
   - $p = 0.008$ is low ($p \leq 0.01$); we are unlikely to see 252.5 msec when H_0 is true. We reject H_0, and embrace H_1.
   - This experiment suggests that subjects are faster under decreasing intensity.

Data from [Living Swan et al. 03]

Some Considerations…

- The conditional probability $p(X \mid H_0)$
  - Much of statistics involves how to calculate this probability; source of most of statistic’s complexity
  - Logic of hypothesis testing the same regardless of how $p(X \mid H_0)$ is calculated
  - If you can calculate $p(X \mid H_0)$, you can test a hypothesis

- The null hypothesis H_0
  - H_0 usually in form $f(\mu_1, \mu_2,\ldots) = 0$
  - Gives hypothesis testing a double-negative logic: assume H_0 as the opposite of H_1, then reject H_0
  - Philosophy is that can never prove $f = 0$, because 0 is point value in domain of real numbers
  - H_1 usually in form $f(\mu_1, \mu_2,\ldots) \neq 0$; we don’t know what value it will take, but main interest is that it is not 0

When We Reject H_0

- Calculate $\alpha = p(X \mid H_0)$, when do we reject H_0?
  - In psychology, two levels: $\alpha \leq 0.05$; $\alpha \leq 0.01$
  - Other fields have different values

- What can we say when we reject H_0 at $\alpha = 0.008$?
  - “If H_0 is true, there is only an 0.008 probability of getting our results, and this is unlikely.”
    - Correct!
  - “There is only a 0.008 probability that our result is in error.”
    - Wrong, this statement refers to $p(H_0)$, but that’s not what we calculated.
  - “There is only a 0.008 probability that H_0 could have been true in this experiment.”
    - Wrong, this statement refers to $p(H_0 \mid X)$, but that’s not what we calculated.

[Cohen 94]

When We Don’t Reject H_0

- What can we say when we don’t reject H_0 at $\alpha = 0.445$?
  - “We have proved that H_0 is true.”
  - “Our experiment indicates that H_0 is true.”
    - Wrong, statisticians agree that hypothesis testing cannot prove H_0 is true.

- Statisticians do not agree on what failing to reject H_0 means.
  - Conservative viewpoint (Fisher):
    - We must suspend judgment, and cannot say anything about the truth of H_0.
  - Alternative viewpoint (Neyman & Pearson):
    - We can accept H_0 if we have sufficient experimental power, and therefore a low probability of type II error.

From [Howell 02], p 99

Probabilistic Reasoning

- If hypothesis testing was absolute:
  - If H_0 is true, then X cannot occur…however, X has occurred…therefore H_0 is false.
  - e.g.: If a person is a Martian, then they are not a member of Congress (true)…this person is a member of Congress…therefore they are not a Martian. (correct result)
  - e.g.: If a person is an American, then they are not a member of Congress (false)…this person is a member of Congress…therefore they are not an American. (incorrect result, but correct logical reasoning)

p	q	$p \rightarrow q$	$\neg q \rightarrow \neg p$
T	T	T	T
T	F	F	F
F	T	T	T
F	F	T	T

$$\left.\begin{array}{l} p \rightarrow q \\ \neg q \\ \hline \rightarrow \neg p \end{array}\right\} \text{modus tollens}$$

From [Cohen 94]

Probabilistic Reasoning

- However, hypothesis testing is probabilistic:
  - If H_0 is true, then X is highly unlikely…however, X has occurred…therefore H_0 is highly unlikely.
  - e.g.: If a person is an American, then they are probably not a member of Congress (true, right?)…this person is a member of Congress…therefore they are probably not an American.
    (incorrect result, but correct hypothesis testing reasoning)

p	q	$p \rightarrow q$	$\neg q \rightarrow \neg p$
T	T	T	T
T	F	F	F
F	T	T	T
F	F	T	T

$$\left.\begin{array}{l} p \rightarrow q \\ \neg q \\ \hline \rightarrow \neg p \end{array}\right\} \text{modus tollens}$$

From [Cohen 94]

Hypothesis Testing Outcomes

		Decision	
		Reject H_0	Don’t reject H_0
True state of the world	H_0 false	correct a result! $p = 1 – \beta$ = power	wrong type II error $p = \beta$
	H_0 true	wrong type I error $p = \alpha$	correct (but arguing H_0) $p = 1 – \alpha$

- $p(X \mid H_0)$ compared to α, so hypothesis testing involves setting α (typically 0.05 or 0.01)
- Two ways to be right:
  - Find a result
  - Fail to find a result and possibly argue null hypothesis
- Two ways to be wrong:
  - Type I error: we think we have a result, but we are wrong
  - Type II error: a result was there, but we missed it

When Do We *Really* Believe a Result?

- When we reject H_0, we have a result, but:
  - It’s possible we made a type I error
  - It’s possible our finding is not reliable
    - Just an artifact of our particular experiment

- So when do we *really* believe a result?
  - Statistical evidence
    - α level: ($p < .05$, $p < .01$, $p < .001$)
    - Power
  - Meta-statistical evidence
    - Plausible explanation of observed phenomena
      - Based on theories of human behavior: perceptual, cognitive psychology; control theory, etc.
    - Repeated results
      - Especially by others

Power

- Experimental Validity
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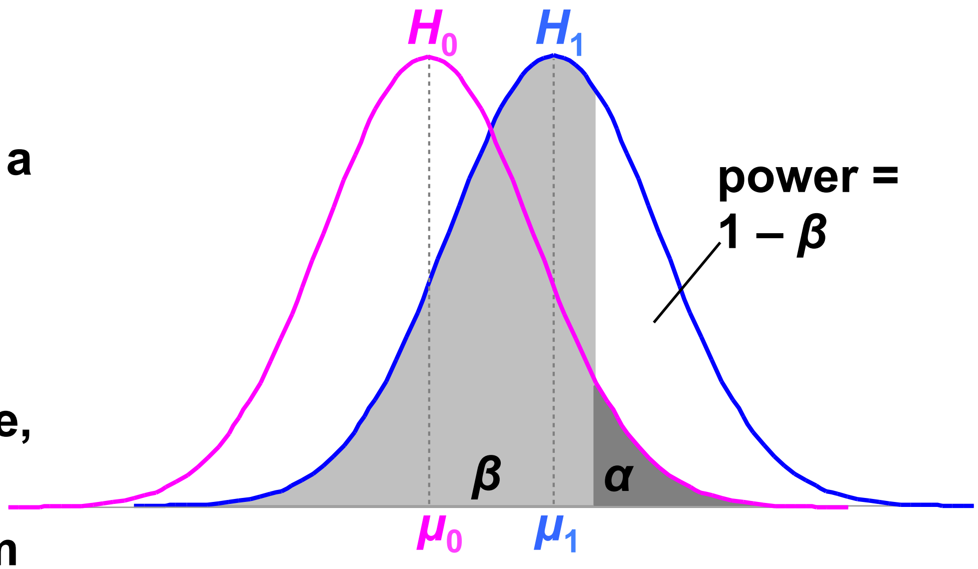
Interpreting α, β, and Power

		Decision	
		Reject H_0	Don’t reject H_0
True state of the world	H_0 false	a result! $p = 1 – \beta$ = power	type II error $p = \beta$
	H_0 true	type I error $p = \alpha$	argue H_0? $p = 1 – \alpha$

- If H_0 is true:
  - α is probability we make a **type I error**: we think we have a result, but we are wrong
- If H_1 is true:
  - β is probability we make a **type II error**: a result was there, but we missed it
  - Power is a more common term than β

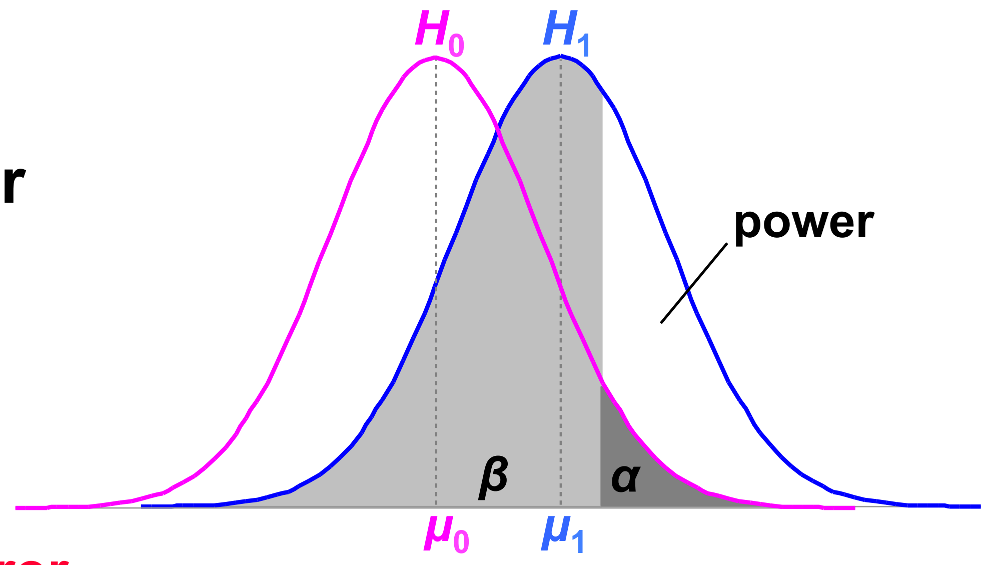


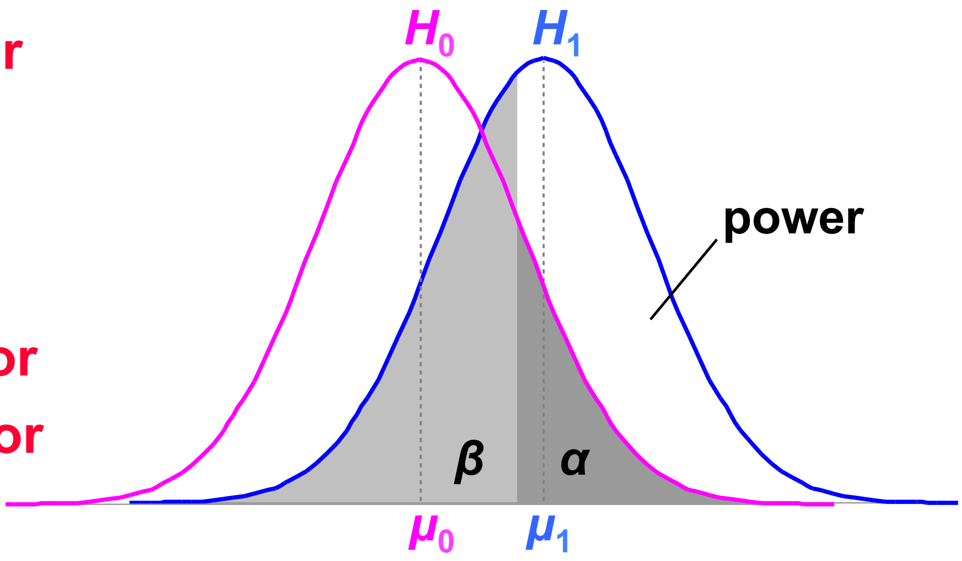
Increasing Power by Increasing *α*

- **Illustrates *α* / power tradeoff**

- **Increasing *α*:**
  - **Increases power**
  - **Decreases type II error**
  - **Increases type I error**

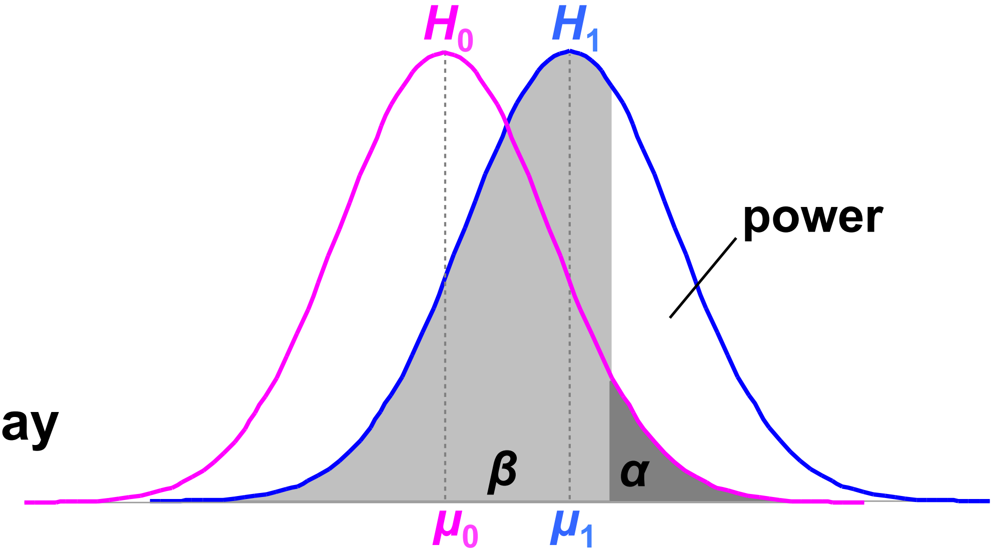
- **Decreasing *α*:**
  - **Decreases power**
  - **Increases type II error**
  - **Decreases type I error**



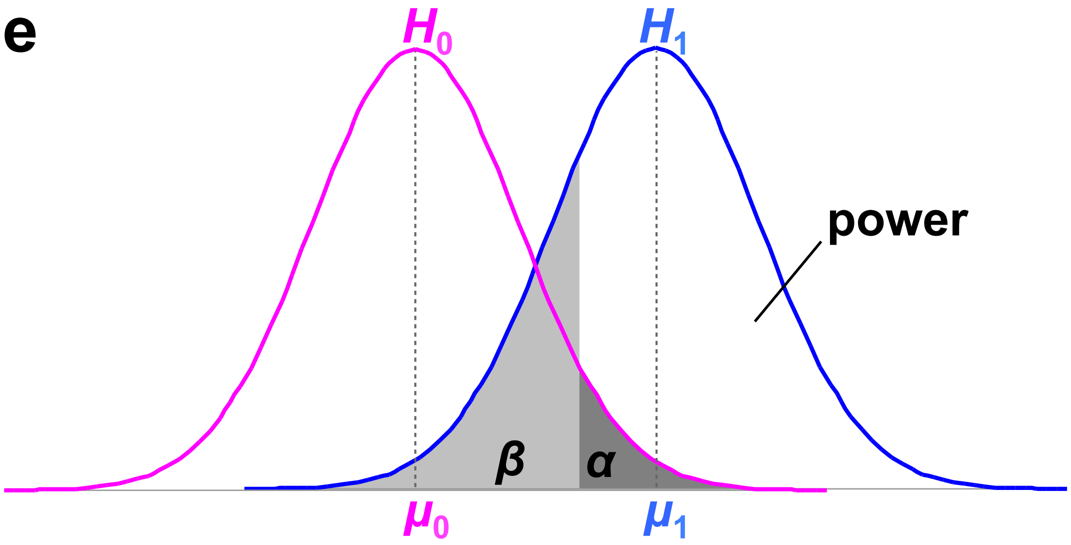


Increasing Power by Measuring a Bigger Effect

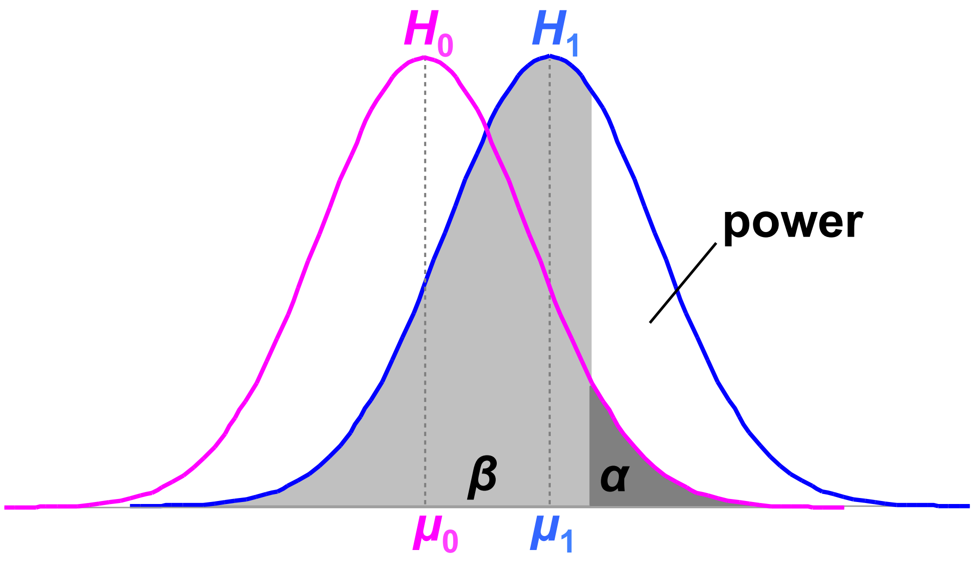
- **If the effect size is large:**
  - Power increases
  - Type II error decreases
  - α and type I error stay the same



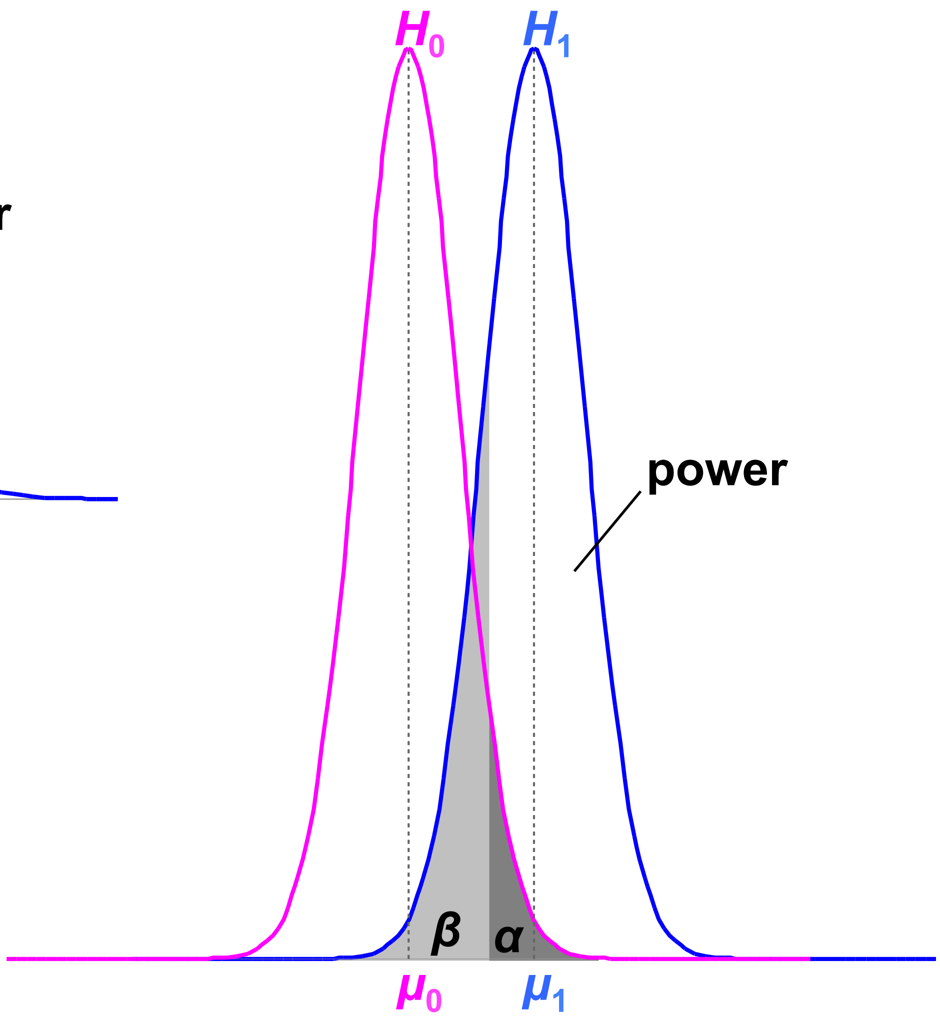
- **Unsurprisingly, large effects are easier to detect than small effects**



Increasing Power by Collecting More Data

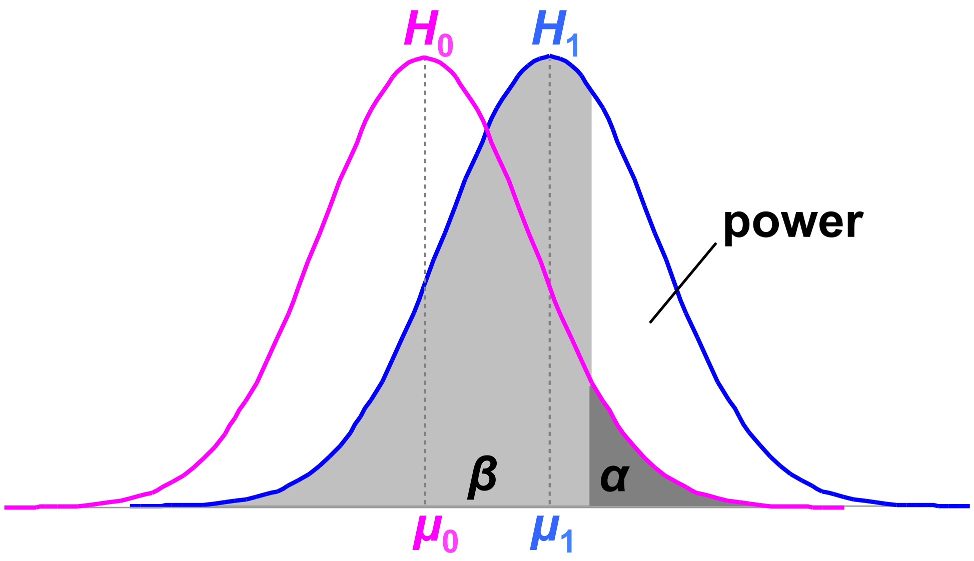


- Increasing sample size (*N*):
  - Decreases variance
  - Increases power
  - Decreases type II error
  - *α* and type I error stay the same
- There are techniques that give the value of *N* required for a certain power level.

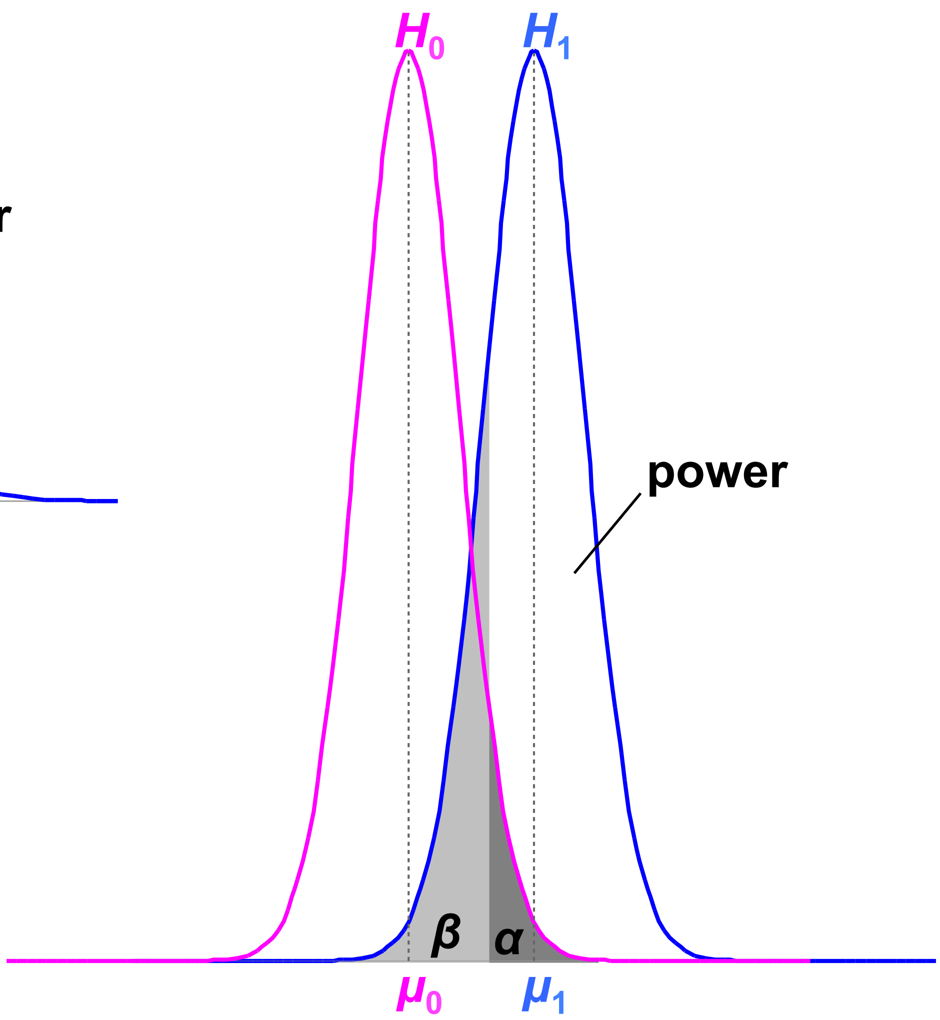


- Here, effect size remains the same, but variance drops by half.

Increasing Power by Decreasing Noise



- Decreasing experimental noise:
  - Decreases variance
  - Increases power
  - Decreases type II error
  - α and type I error stay the same
- More careful experimental results give lower noise.



- Here, effect size remains the same, but variance drops by half.

Using Power

- Need α, effect size, and sample size for power:

$$\text{power} = f(\alpha, |\mu_0 - \mu_1|, N)$$

- Problem for VR / AR:
  - Effect size $|\mu_0 - \mu_1|$ hard to know in our field
    - Population parameters estimated from prior studies
    - But our field is so new, not many prior studies
  - Can find effect sizes in more mature fields

- Post-hoc power analysis:

$$\text{effect size} = |X_0 - X_1|$$

  - Estimate from sample statistics
  - But this makes statisticians grumble (e.g. [Howell 02] [Cohen 88])
  - Same information as *p* value

Other Uses for Power

1. Number samples needed for certain power level:

$$N = f(\text{ power}, \alpha, |\mu_0 - \mu_1| \text{ or } |X_0 - X_1|)$$

   - Number extra samples needed for more powerful result
   - Gives "rational basis" for deciding *N* [Cohen 88]

2. Effect size that will be detectable:

$$|\mu_0 - \mu_1| = f(N, \text{ power}, \alpha)$$

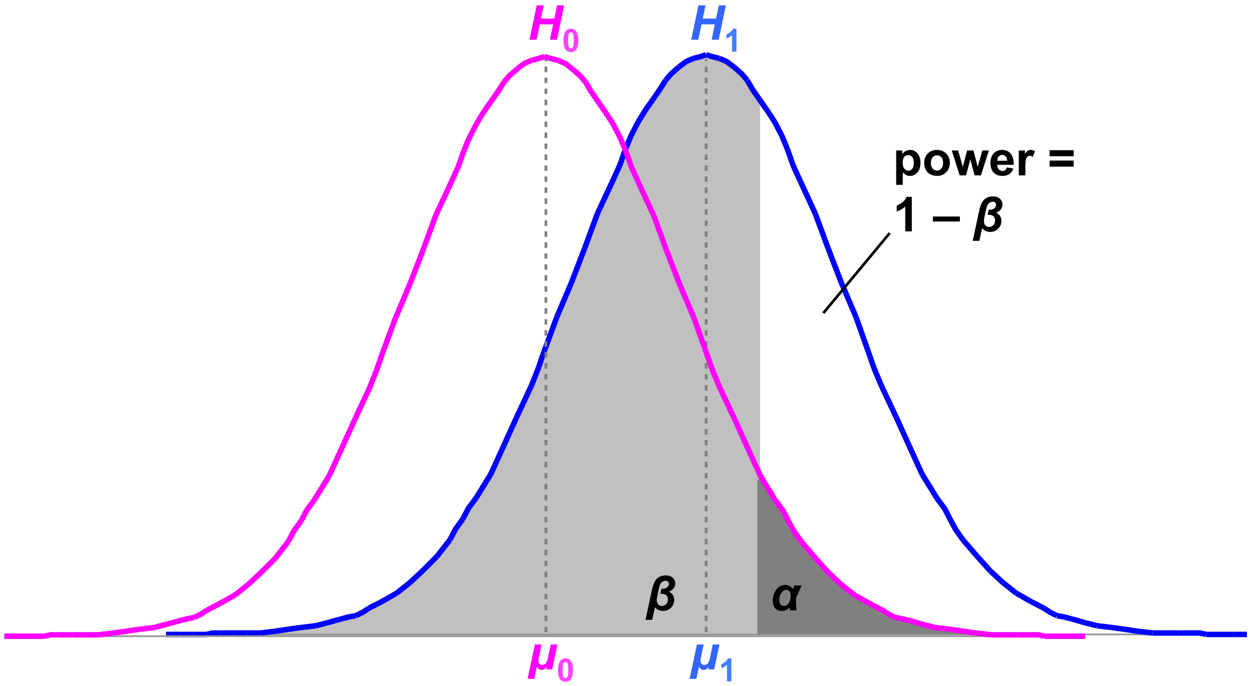
3. Significance level needed:

$$\alpha = f(|\mu_0 - \mu_1| \text{ or } |X_0 - X_1|, N, \text{ power})$$

(1) is the most common power usage

Arguing the Null Hypothesis

- Cannot directly argue H_0: $\mu_s - \mu_m = 0$. But we can argue that $|\mu_0 - \mu_1| < d$.
  - Thus, we have bound our effect size by *d*.
  - If *d* is *small*, effectively argued null hypothesis.



From [Cohen 88], p 16

Graphical Data Analysis

- Experimental Validity
- Experimental Design
- Describing Data
  - Graphing Data
  - Descriptive Statistics
- Inferential Statistics
  - Hypothesis Testing
  - Power
- *Graphical Data Analysis*

Exploratory Data Analysis (EDA)

- EDA is:
  - A set of data analysis tools and techniques
  - A philosophy of how to investigate data

- EDA philosophy: data should be explored, with an open mind
  - Contrary to then-popular view: statistical tests should be planned before data collected
  - Data may reveal more than anticipated, other than anticipated
  - Emphasizes images that yield rapid insight
  - Greatest value "when it forces us to notice what we never expected to see." [Tukey 77]

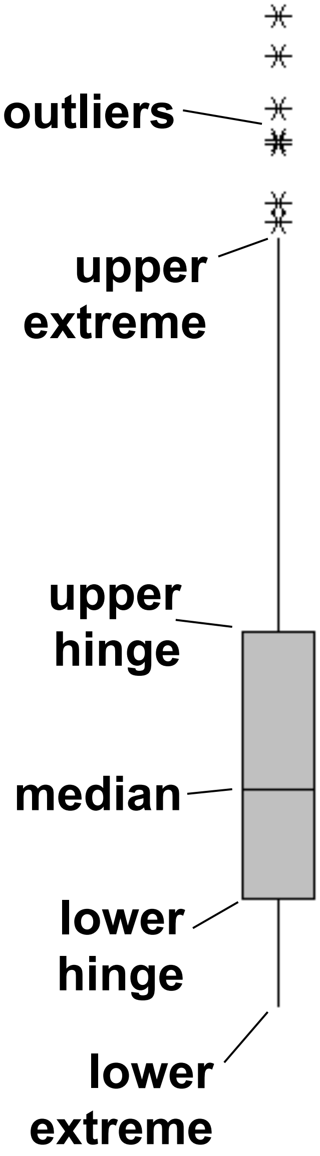
- EDA workflow:
  - 1st: explore the data (descriptive statistics)
  - 2nd: confirm the findings (hypothesis testing)

- EDA is visualization philosophy applied to data analysis

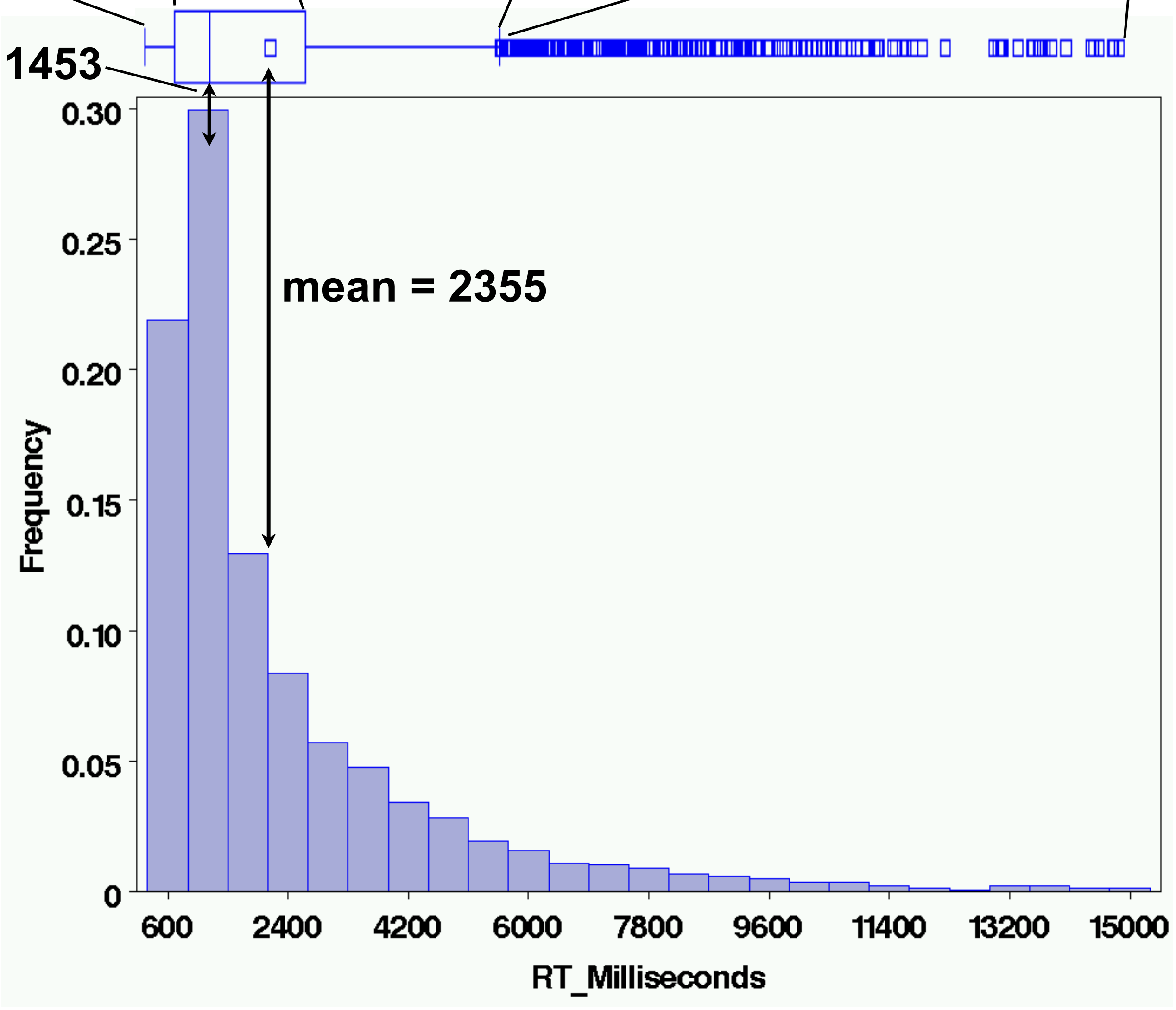
From [Smith Prentice 93]

EDA and Median Statistics

- **EDA** emphasizes **median statistics**:
  - median
  - upper hinge, lower hinge
  - upper extreme, lower extreme
- 5 values often drawn as a **boxplot**:
- Calculation of hinges and extremes depends on software
- Median statistics insensitive to
  - Data distribution
  - Outliers
- Use mean statistics once distribution is established and outliers removed



Example Histogram and Boxplot from Real Data



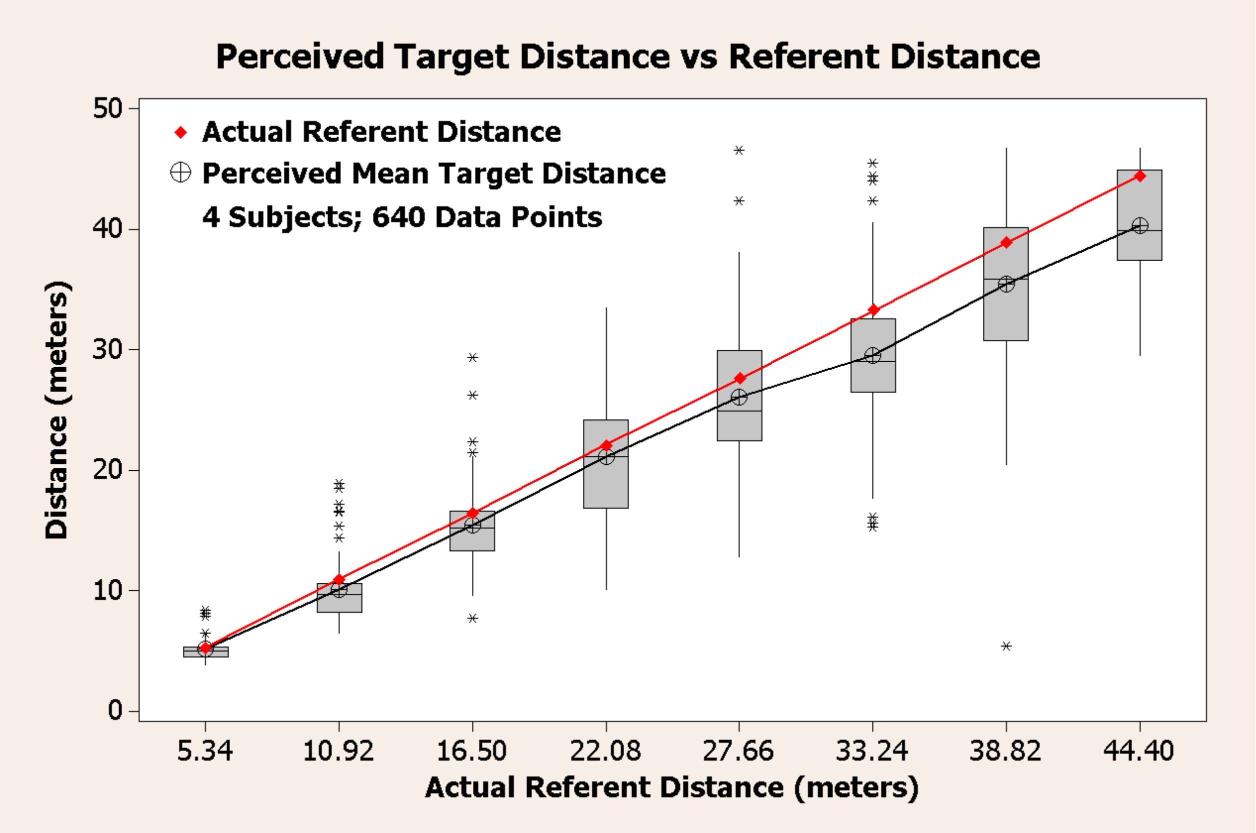
min value 25th 75th upper fence max values (outliers)

median = 1453

mean = 2355

Data from [Living Swan et al 03]

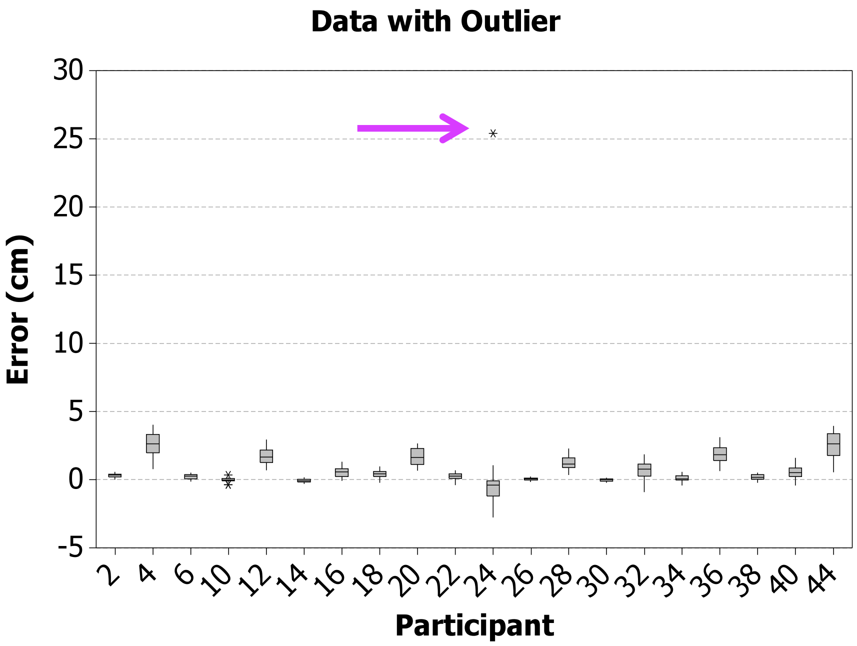
Boxplots Displaying Groups

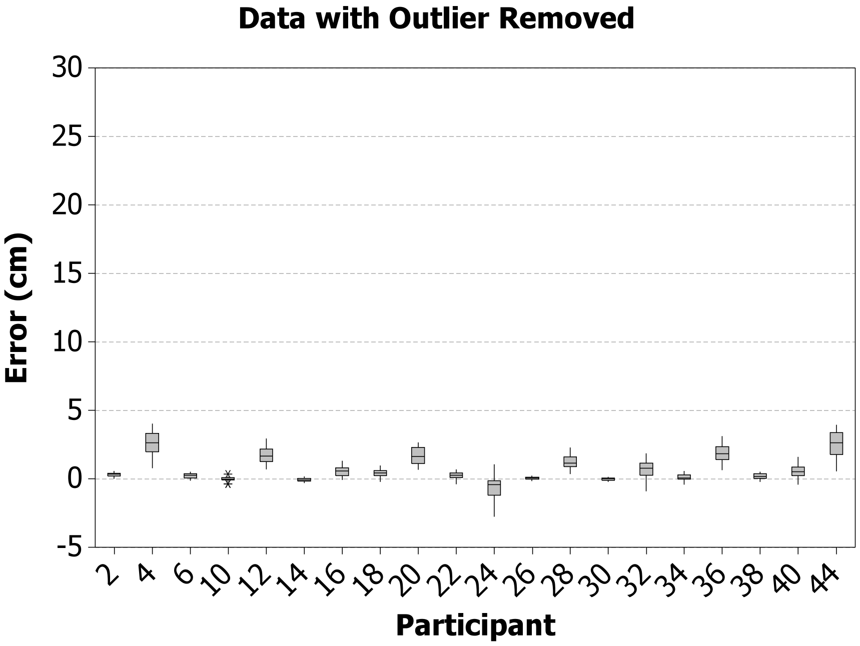


- **Emphasizes variation and relationship to mean**
- **Because narrow, can be used to display side-by-side groups**
- **EDA includes many other innovative graphical techniques…**

Data from [Swan et al 06]

Processing Outliers





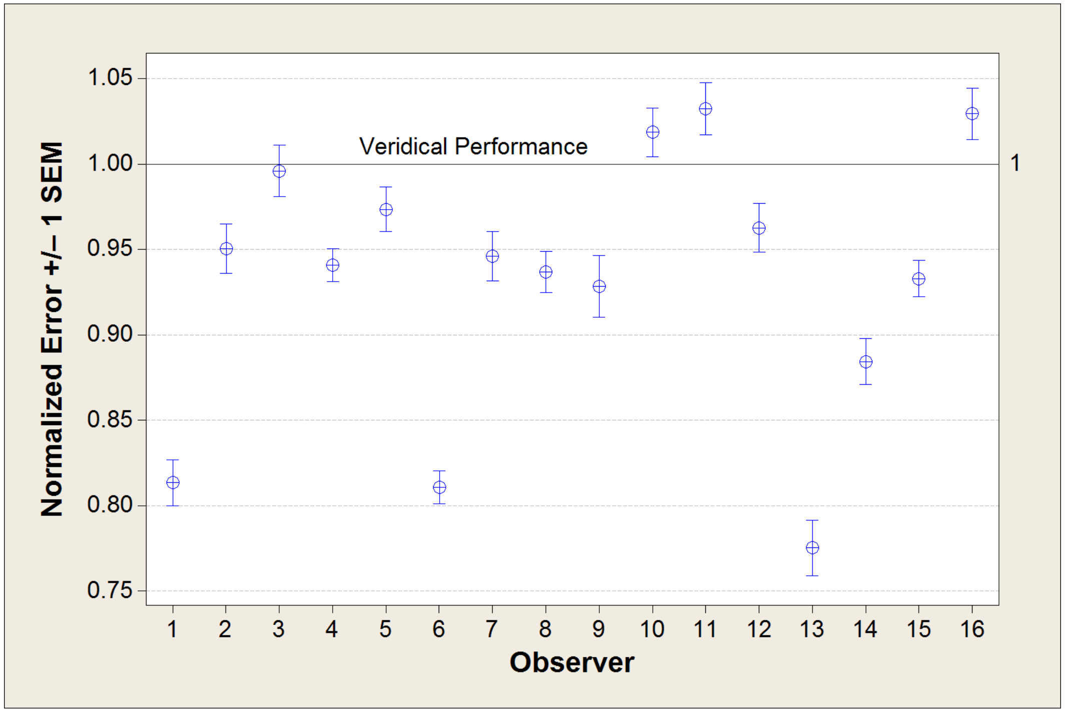
Participant	Environment	Judgment	Distance (cm)	Repetition	Error (cm)		Error (cm)	
24	AR	match	38	1	25.410	← outlier	-1.155	← replaced
24	AR	match	38	2	-0.782		-0.782	
24	AR	match	38	3	-1.155		-1.155	
24	AR	match	38	4	-0.205		-0.205	
24	AR	match	38	5	-1.582		-1.582	
24	AR	match	38	6	-1.624		-1.624	

Replace outlier with median of remaining values in experimental cell

[Barnett Lewis 94]

Mean +/– SEM Plots

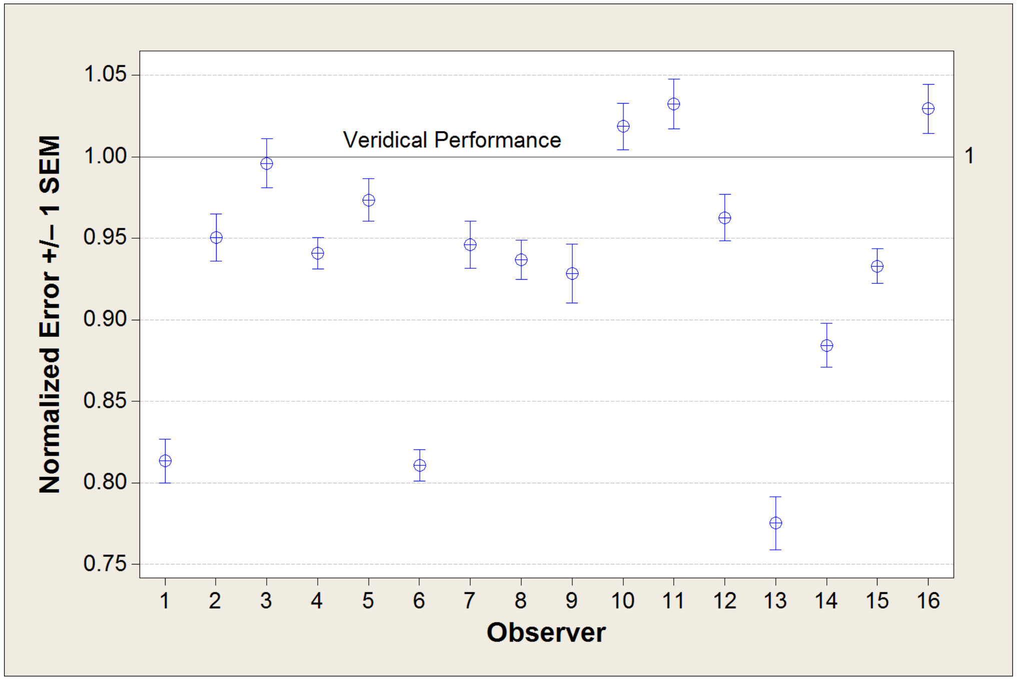
- Most important considerations:
  - Size of difference between means
  - Distance between error bars (separation / overlap)
    - Graphical indication of power
  - Size of smallest meaningful interval on *y*-axis
- Note that considerations are all graphical



Data from [Jones Swan et al 08]

What Kind of Error Bars?

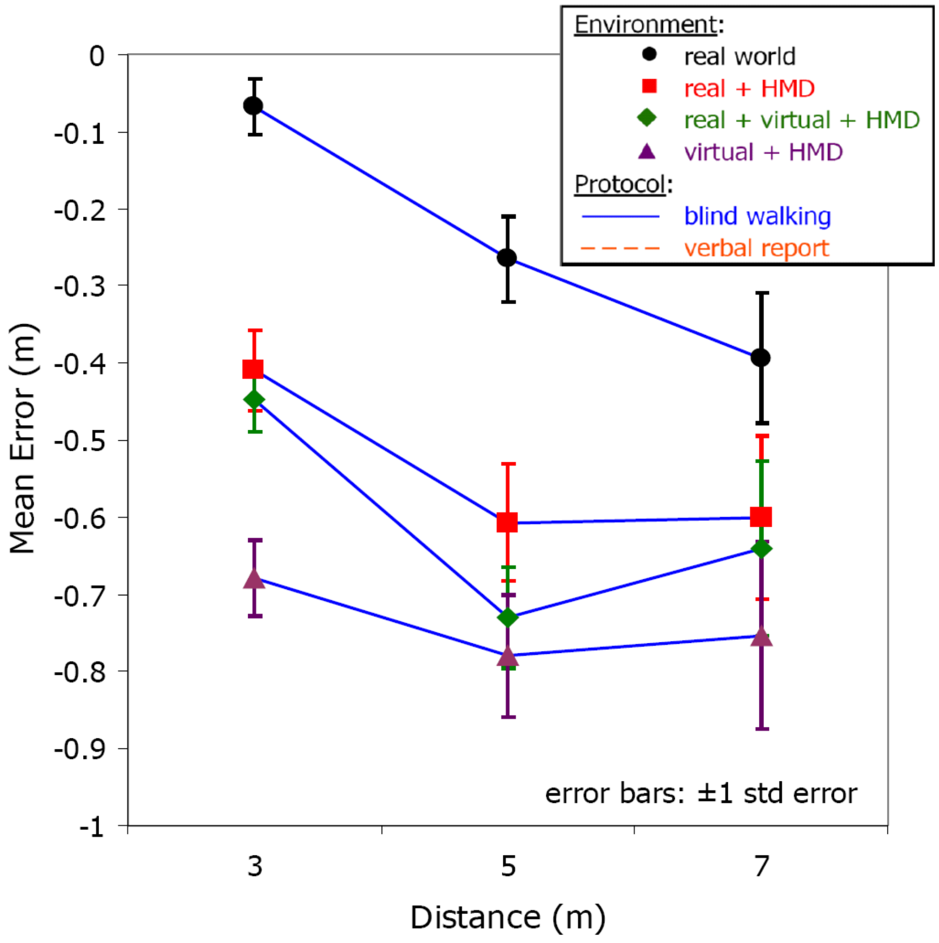
- 95% Confidence Interval (CI)
  - Between-subjects data
  - Graphical difference = inferential difference (same as *t*-test)



- Standard Deviation (SD)
  - Only interested in samples, not population
- Standard Error (SEM)
  - Inferring population from sample
- But:
  - Excel only draws 95% CI's and Standard Deviations!
  - Must calculate standard error separately
- Must label error bars!

Data from [Jones Swan et al 08]

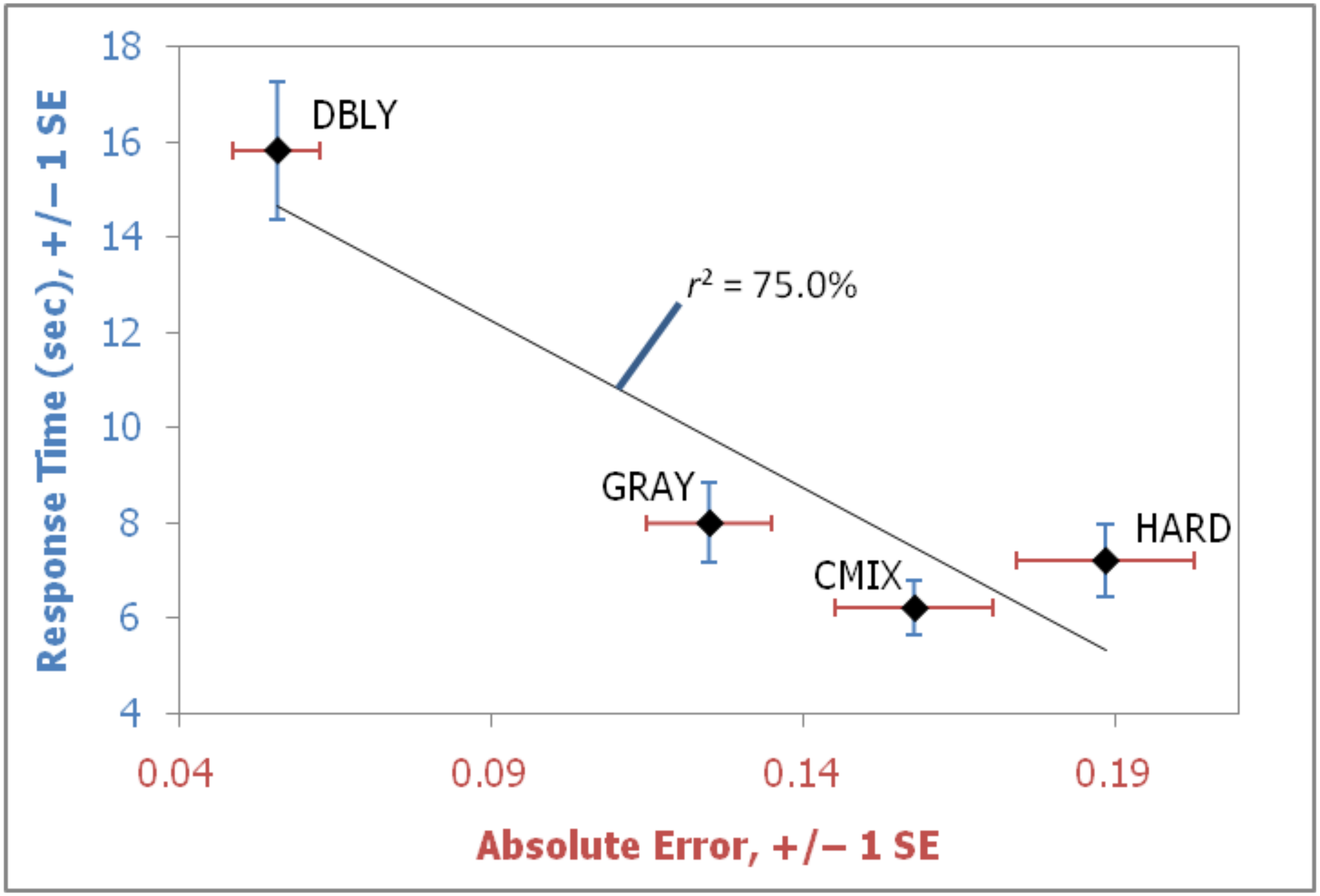
Mean +/– SEM Interaction Plots



Data from [Swan et al 07]

- Again, error bars give much more context to the results
- Here, error bars suggest where to group and separate the means

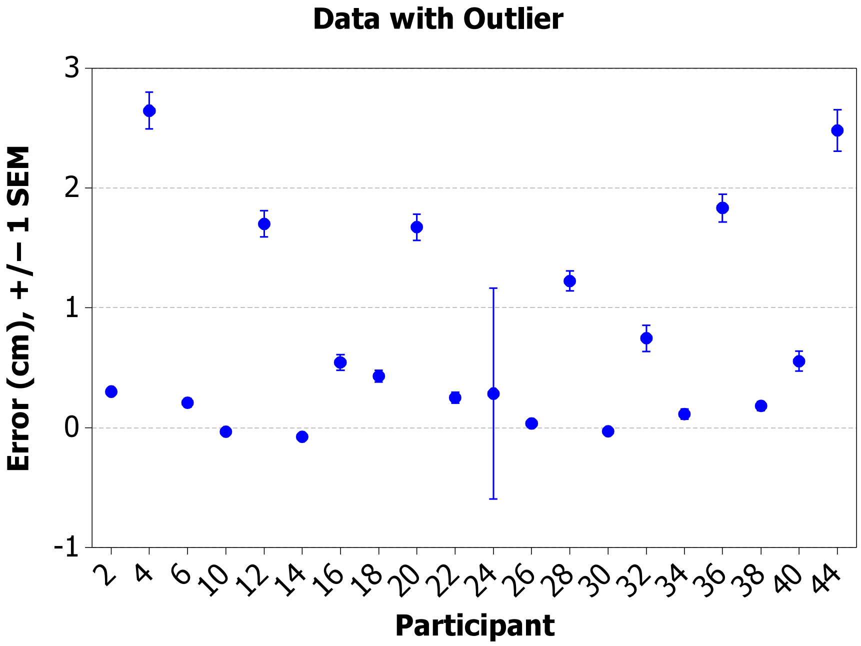
XY Mean +/– SEM Plots

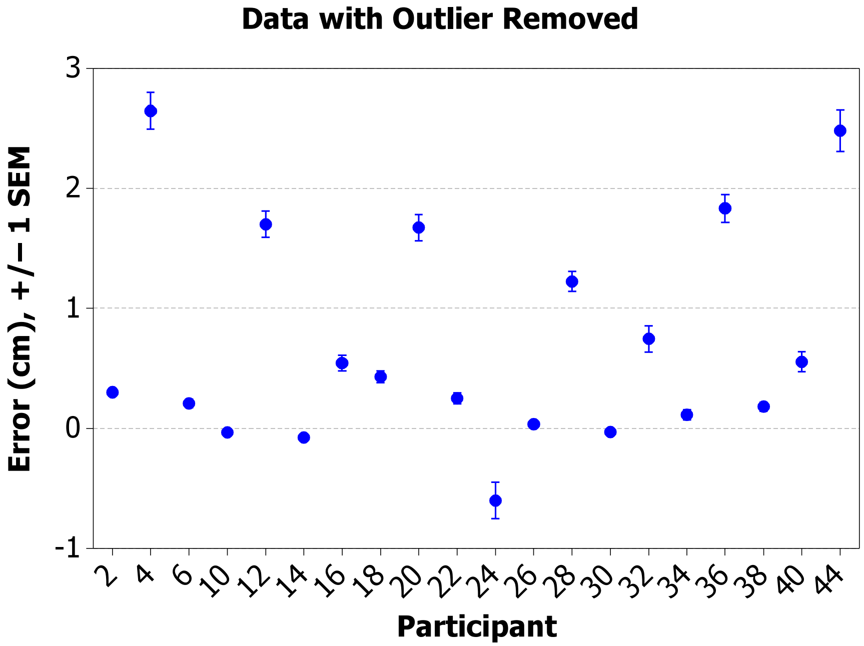


Data from [Cai Swan et al 09]

- Error bars are against both axes
- Suggests a clear speed / accuracy tradeoff

Outliers with Mean +/– SEM Plot





Participant	Environment	Judgment	Distance (cm)	Repetition	Error (cm)		Error (cm)	
24	AR	match	38	1	25.410	← outlier	-1.155	← replaced
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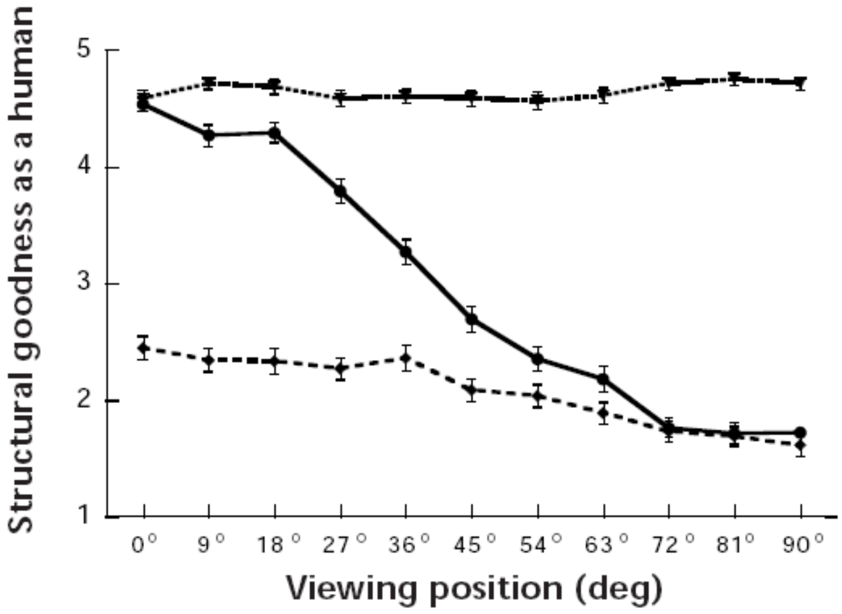
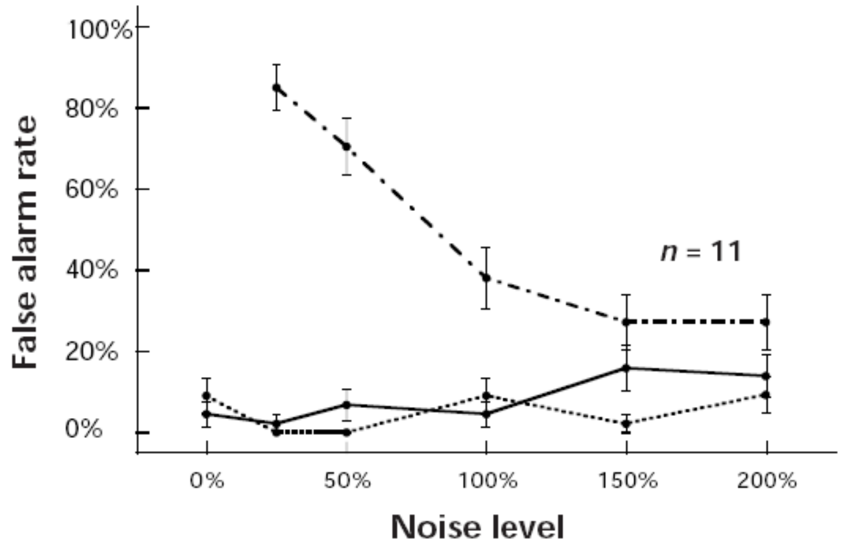
24	AR	match	38	3	-1.155		-1.155	
24	AR	match	38	4	-0.205		-0.205	
24	AR	match	38	5	-1.582		-1.582	
24	AR	match	38	6	-1.624		-1.624	

Replace outlier with median of remaining values in experimental cell

Are Plots All You Need?



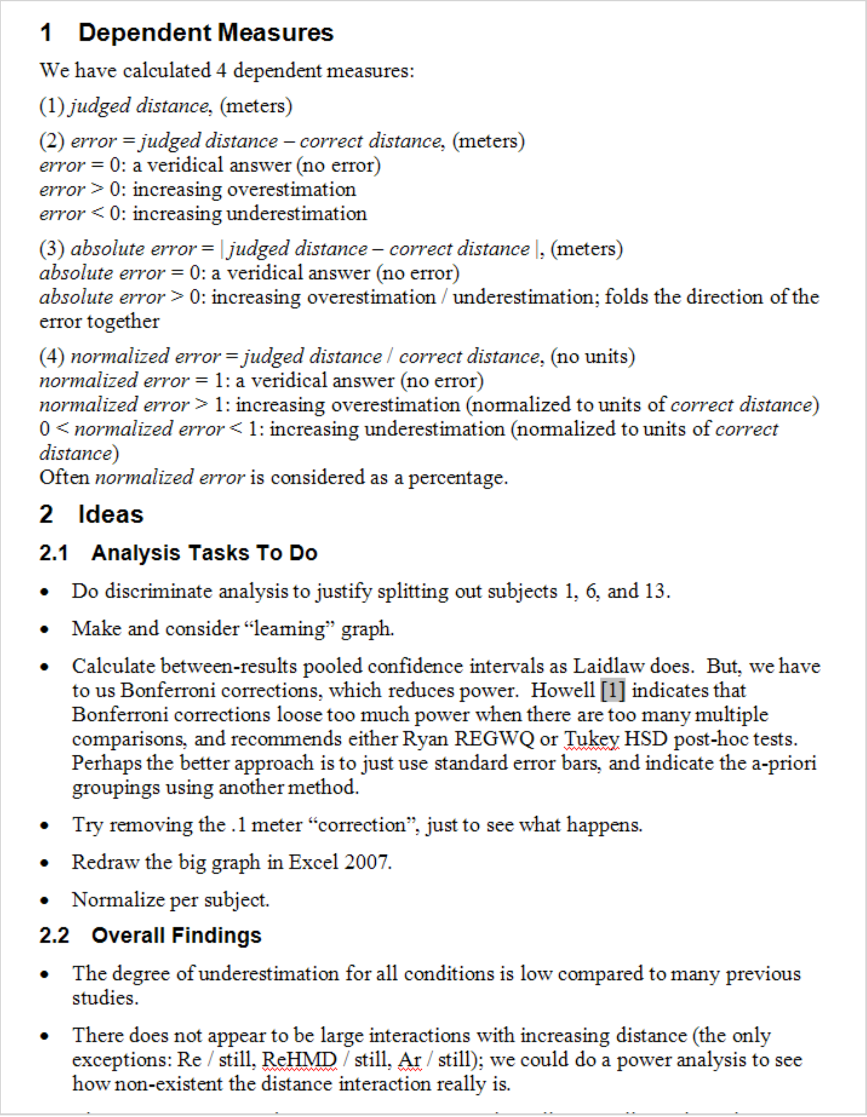


- Two plots from [Bülthoff et al 98], *Nature Neuroscience*
- Small error bars relative to
  (1) effect sizes,
  (2) smallest meaningful interval → large amount of power
- Paper contains no hypothesis testing!
- In some fields (e.g., psychophysics) hypothesis testing culturally unnecessary if plots convincingly show enough power

My Data Analysis Work Flow

- Create MS Word data analysis file
  - Can throw in text and graphics
  - Can organize using headings and outliner
- In a very non-linear fashion:
  - Draw histograms and boxplots; understand distributions
  - Remove outliers
  - Draw mean +/– SEM plots
  - Explain dependent measures calculations
  - Hypothesize as to what we (might have) found and why
  - Perform hypothesis testing on interesting results
  - Perhaps collect more data if results look promising but are not yet powerful
- Eventually determine what is the overall story of the data; what graphs to show

Example of My Analysis Document



1 Dependent Measures

We have calculated 4 dependent measures:

(1) *judged distance*, (meters)

(2) *error* = *judged distance* – *correct distance*, (meters)
error = 0: a veridical answer (no error)
error > 0: increasing overestimation
error < 0: increasing underestimation

(3) *absolute error* = | *judged distance* – *correct distance* |, (meters)
absolute error = 0: a veridical answer (no error)
absolute error > 0: increasing overestimation / underestimation; folds the direction of the error together

(4) *normalized error* = *judged distance* / *correct distance*, (no units)
normalized error = 1: a veridical answer (no error)
normalized error > 1: increasing overestimation (normalized to units of *correct distance*)
0 < *normalized error* < 1: increasing underestimation (normalized to units of *correct distance*)
Often *normalized error* is considered as a percentage.

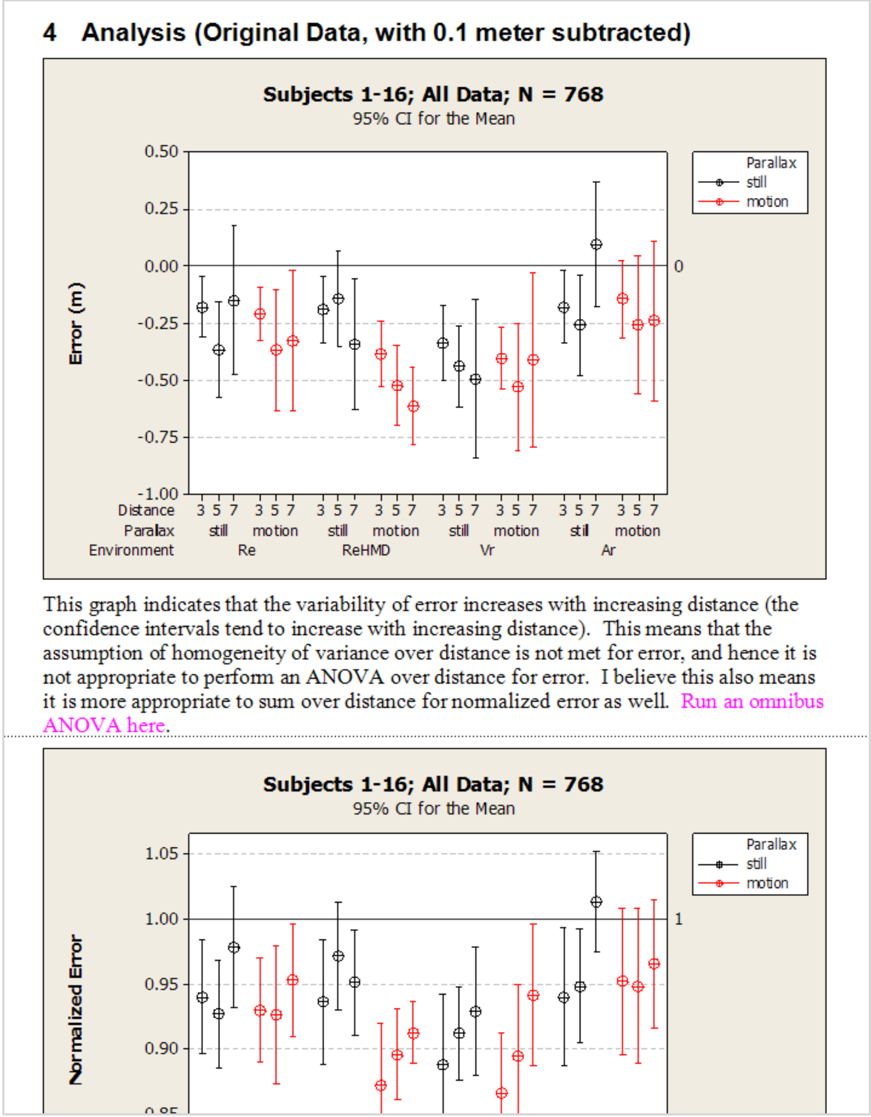
2 Ideas

2.1 Analysis Tasks To Do

- Do discriminate analysis to justify splitting out subjects 1, 6, and 13.
- Make and consider "learning" graph.
- Calculate between-results pooled confidence intervals as Laidlaw does. But, we have to us Bonferroni corrections, which reduces power. Howell [1] indicates that Bonferroni corrections loose too much power when there are too many multiple comparisons, and recommends either Ryan REGWQ or Tukey HSD post-hoc tests. Perhaps the better approach is to just use standard error bars, and indicate the a-priori groupings using another method.
- Try removing the .1 meter "correction", just to see what happens.
- Redraw the big graph in Excel 2007.
- Normalize per subject.

2.2 Overall Findings

- The degree of underestimation for all conditions is low compared to many previous studies.
- There does not appear to be large interactions with increasing distance (the only exceptions: Re / still, ReHMD / still, Ar / still); we could do a power analysis to see how non-existent the distance interaction really is.



4 Analysis (Original Data, with 0.1 meter subtracted)

This graph indicates that the variability of error increases with increasing distance (the confidence intervals tend to increase with increasing distance). This means that the assumption of homogeneity of variance over distance is not met for error, and hence it is not appropriate to perform an ANOVA over distance for error. I believe this also means it is more appropriate to sum over distance for normalized error as well. Run an omnibus ANOVA here.

Final Thoughts on Experimental Design and Data Analysis

- In the end, what matters are:
  (1) the results, and
  (2) how they relate to what's being studied.
- …not hypothesis testing (e.g., [Bülthoff et al 98])
- Paraphrased quote from many applied statistics texts:

  "Data analysis is an art, not a science"
- When applying data analysis to results:
  - There is no one way to be right
  - There is no one way to be wrong
- The best way to learn data analysis and experimental design: read and critique existing papers, both in VR / AR and in other fields.

  "A month in the lab will save you a day in the library"

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http://www.cse.msstate.edu/~swan/teaching/tutorials/Swan-VR2014-Tutorial.pdf