Human-Centered Evaluation of Software Artefacts in Computer Science: Introduction, State-of-the-Art, and Perspectives

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What is this talk about?

- Tries to argue that human-centered / empirical studies are necessary
- Introduces into some basic terms
- Gives an overview of techniques required to perform experiments
- Shows pitfalls of experiments
- Gives an example of an experiment

Motivation

- Two different targets for research in CS
  - Machines
    - Execution speed, memory consumption, etc.
  - Human
    - Development speed, development errors, etc.
- Nowadays research methods mainly address machines
- Human plays rather minor role
- Usability (human interaction) rarely tested

Why should we care about humans?

- Humans are one of the main audience for CS constructs
- Usability of
  - Programming languages
  - APIs
  - User interfaces
  - ...
- Extensibility
- Maintainability

Current situation

- Example: Programming Language
  - Typical statement from the community:
    - „*If a language is good, people will use it*“
  - Questions:
    - „*How many people must use a language so that it becomes good*?“
    - „*What about the moment when a language was initially developed?“*
    - „*What about marketing effects?“*
    - „*What should be the motivation of the first developer using a new PL?“*
- Strange
  - Later on hardly tested whether PL was being used
  - „*There is a community...so the language must be good*“
- Example: well.....many, many

Typical situation: anecdotes instead of applied research method

Claim

- Artifact design is (often) about developers
- Current dominating approach

  (1) Find example

  (2) Build construct

  (3) Claim that construct helps developers

This leads to nowhere

- Research methods needed that consider developers / users … involved humans

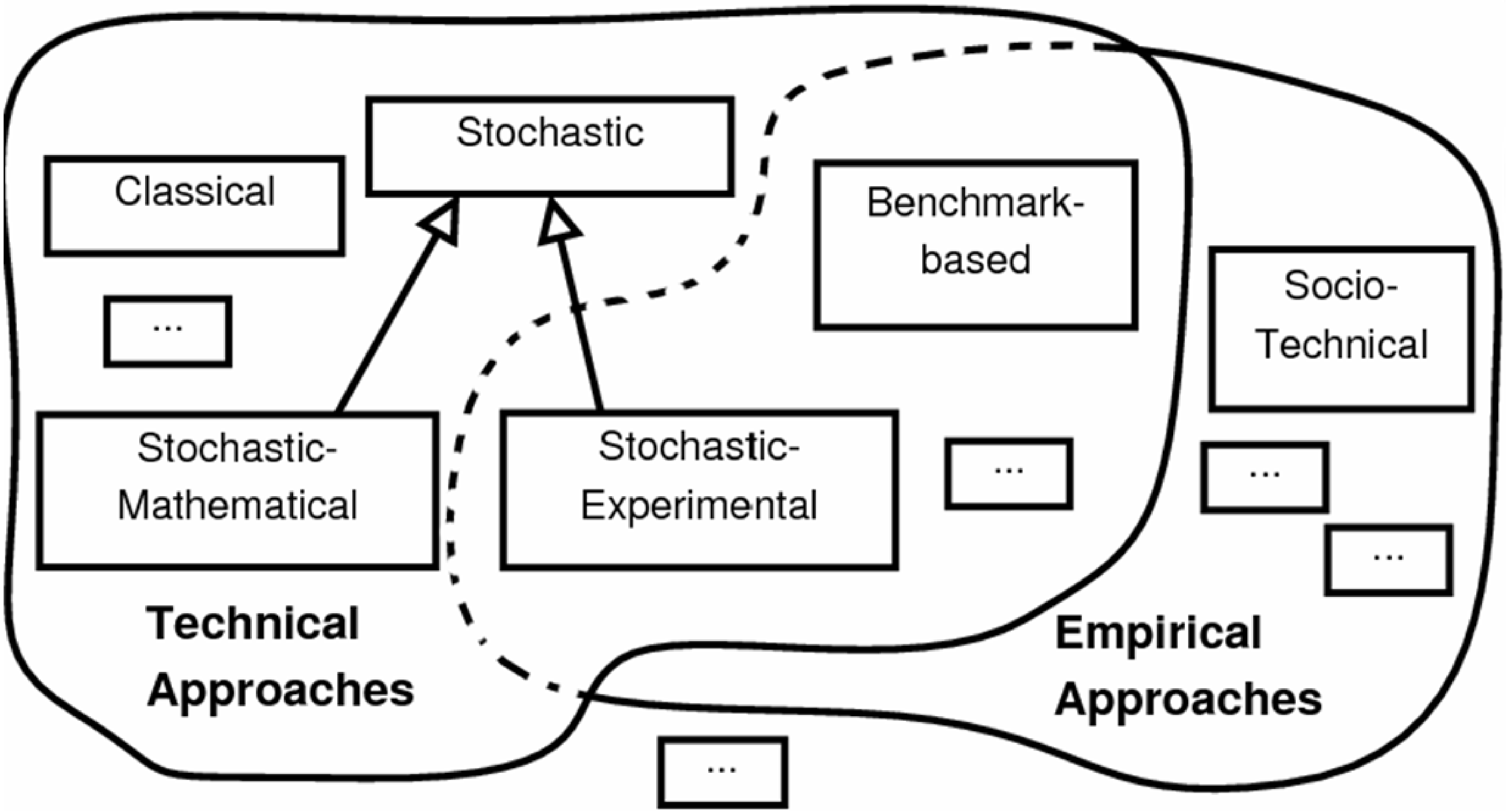
Why not the traditional way?

- Machine / algorithm / etc.
  - Formal models, formal proofs, etc.

- Human
  - No formal model
    => no formal reasoning
    => traditional approaches cannot be applied

Overview of CS Research Methods

Taken from [Hanenberg, Onward'10]



Structure

- Need for experimentation
  (here: controlled experiments with humans)
  - What means experimentation?
  - What is required to run experiments?
- State-of-the-art
- Challenges in experimentation
- Example: Experiment on type systems
- Conclusion

Why experiments?

- Problem (again)
  - No formal model available how humans work
- Experiments
  - Observations as <u>tests</u> what really happens
  - Approximation (examples) of actual behavior
- What is a test?
  - There must be a statement which says when a test fails (<u>hypothesis</u>)
  - There must be a objective way to check, whether test has failed (<u>falsification</u>)

Logic of experimentation

- An experiment...
  - does not provide a proof for a theory
  - can NEVER consider all existing variables
  - can hardly reflect on real world situations
  - can only provide some evidence that a new construct helps (apart from developer's subjective impression)

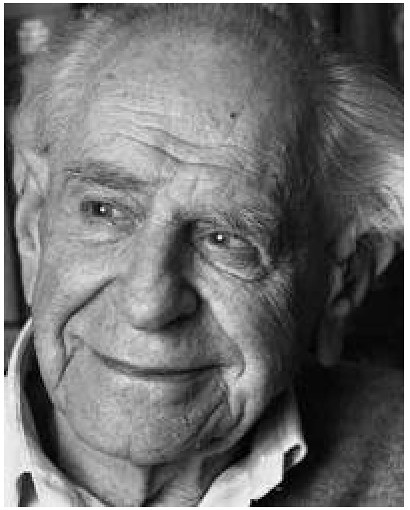
- Why should it be useful?
  - Test: „Does the artifact really help in situations the inventor had in mind“?
  - Result: „Uselessness of artifact can be shown!“

Structure of Experiment

- Measurement of impact of
  - <u>Independent variable</u> (e.g. PL) on
  - <u>Dependent variable</u> (e.g. development time)
- A variable has a number of different <u>treatments</u>
  - Example:
    Comparison between Java, C++, and C
    => Indep. Variable PL with three treatments
- Experiment typically suffers from <u>confounding factors</u> (variable which are not controlled)

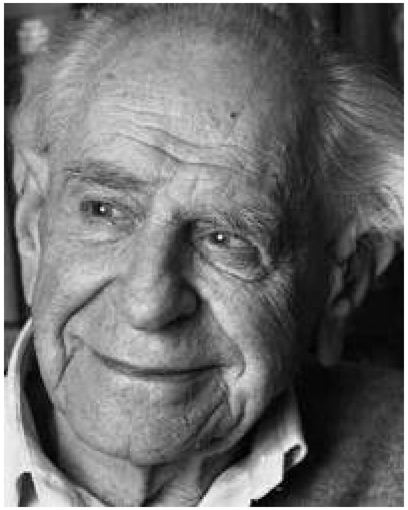
Background of Experiments (Karl Popper)

- Scientific argumentation
  - Falsification of hypothesis
    (use of statically typed language decreases development time)
  - More often
    - Exploratory analysis (let‘s see what happens if…)



  - **NO PROOFS / NO GENERALIZABILITY**
    - But always the hope that repeated observations reveal some truth

Background of Experiments (Karl Popper)



- Validity of hypotheses
  - Evidence for hypotheses increases the more often they could not be rejected
- Assumption
  - Massive execution of experiments
- Hope...(as practical researcher)
  - the more data available, the more probable it is, that we finally „see some rules“

Single vs. Multiple Runs

- General idea of experimentation
  - It shows, that hypothesis does not hold

- Single run experiments (in physics)
  - Example: Galilei's Pisa experiment

    => Single run falsified existing theory
    => Boolean statement from single run => **Boolean logic**

- With humans: Multiple runs
  - Humans differ too much

    => Multiple runs required
    => How often do runs need to falsify theory?
    => Argumentation based on analysis of sample => **Statistics**

Remaining questions

- How to design / perform experiments?

- How to analyse experiments?

...let's discuss it the other way around

Statistics in 5 minutes....

- Descriptive Statistics
  - Arithmetic mean, medians, variance, etc.
  - Relatively easy to understand, but <u>inappropriate</u>

- Inductive Statistics
  - Consideration of probabilities
  - Not that intuitive to understand, but <u>state-of-the-art</u>

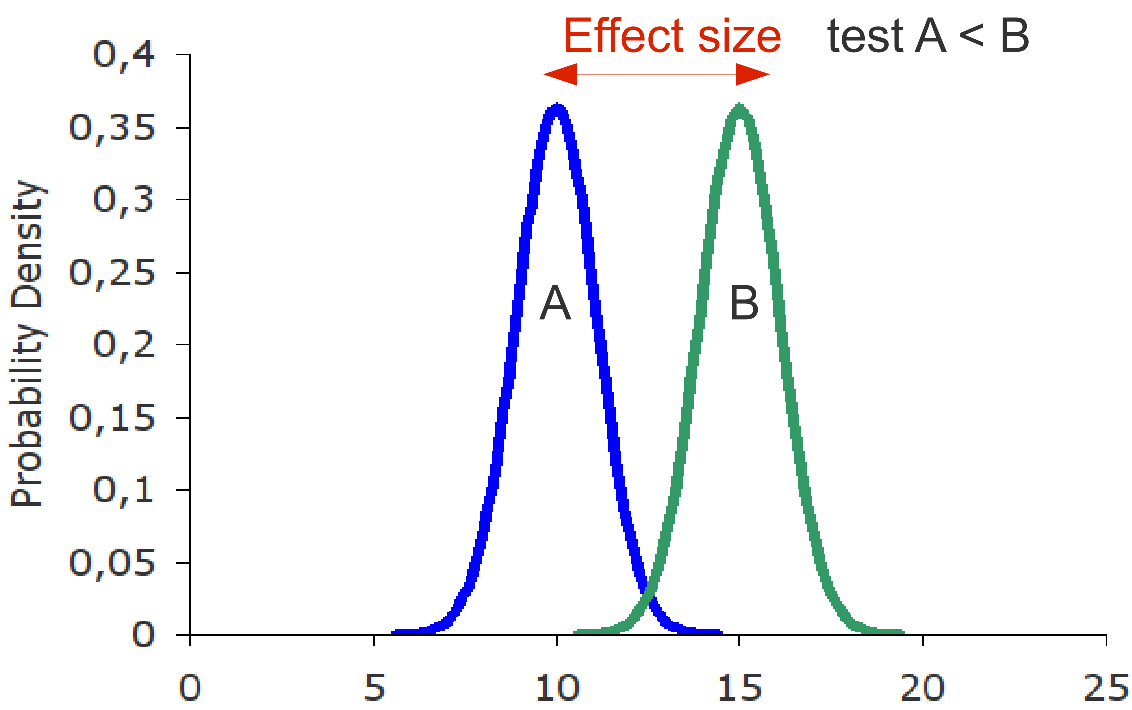
Example: Descriptive Statistics

- Software development times with techniques A and B (in hours), 10 subjects
  - A: 1, 2, 3, 4, 1000 (mean: > 200, median: 3)
  - B: 10, 20, 30, 40, 50 (mean: 30, median 30)

- Problem
  - Argumentation based on mean or median?
  - Is 1000 an outlier that should not be considered?
  - Problems of descriptive statistics well known...

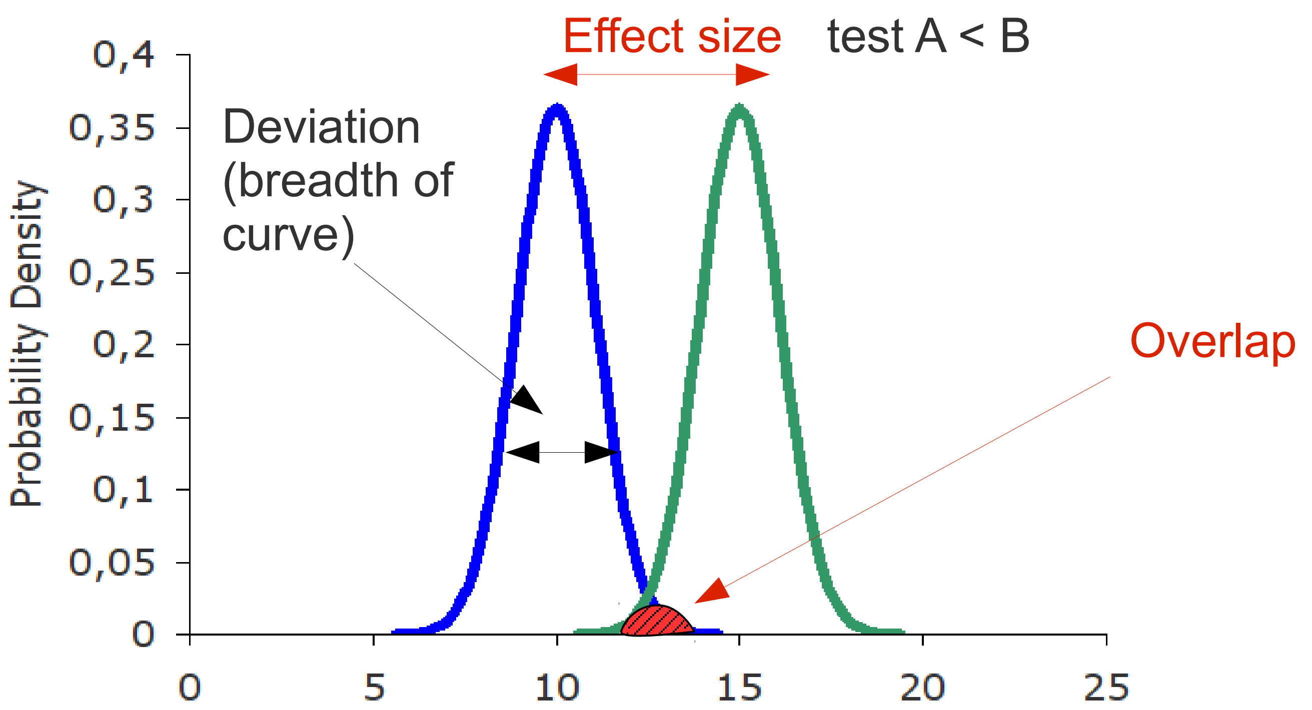
Inductive Statistics(1)

- General idea: compare distribution / density functions of samples A and B



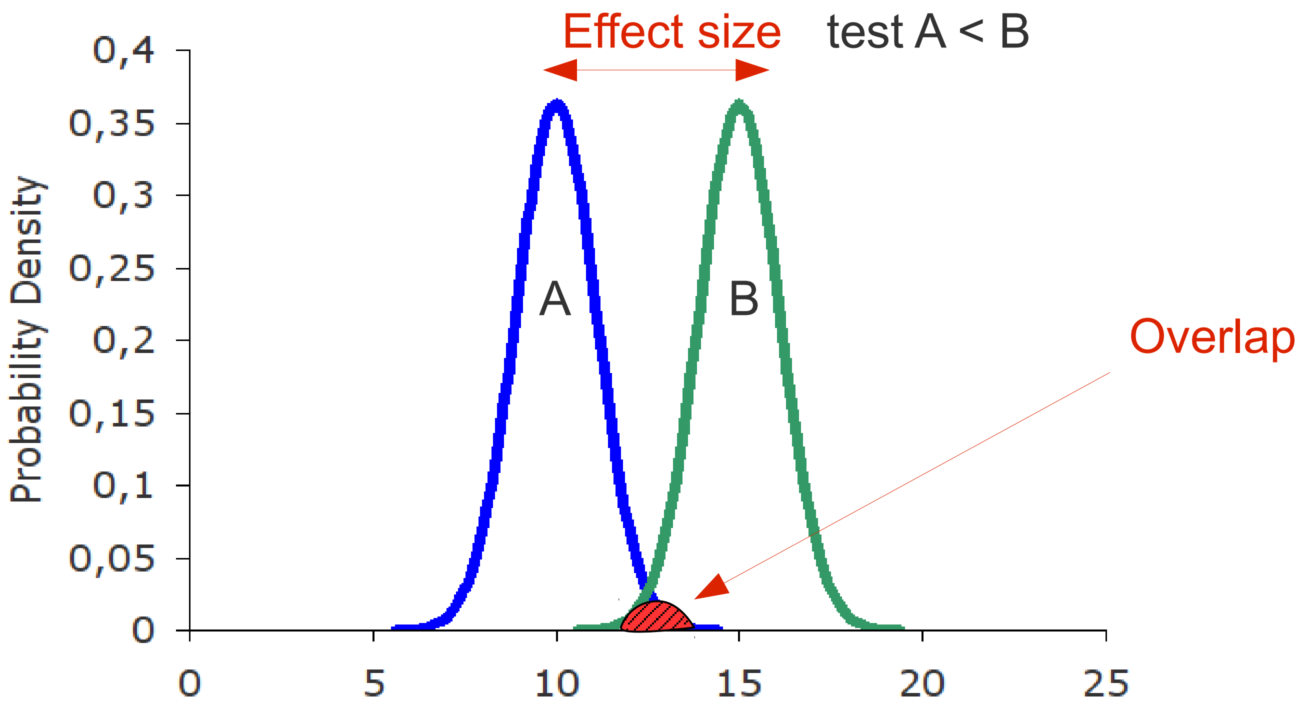
Inductive Statistics(1)

- General idea: compare density functions



Inductive Statistics (1)

- General idea: compare density functions



- Computation of overlap between density function

Inductive Statistics (2)

- <u>P-value:</u> (Error-) Probability that a sample does NOT show A<B

- Arbitrarily(!) chosen alpha-level as „significance level“ (typically: 0.05, 0.01, …)

- Example:
  - „*The difference turned out to be significant under an alpha-level of 0.05*“
    => $p<0.05$

Inductive Statistics (3)

- Sample typically does not show perfect curve
  => approximation of density function required
  => sometimes, not even the kind of density function is known

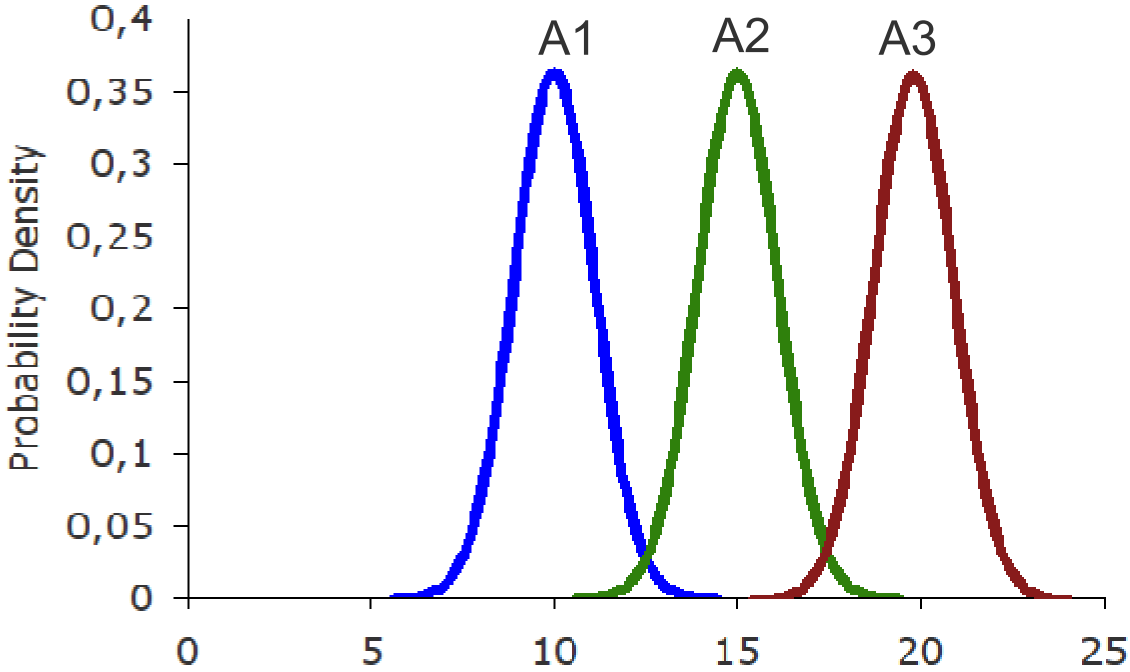
- Standard mechanisms (significance tests) to compute <u>p-values</u> for different <u>scales</u> and <u>sample sizes</u>
  - T-Test, Wilcoxon-Test, Mann-Whitney-U-Test,

- Standard mechanisms to determine, whether a <u>certain distribution</u> can be assumed
  - Shapiro-Wilk-Test, K-S-Test, etc.

- All these tests are implemented in standard statistic software (R, SPSS, S, MiniStat, SAS, …)

Inductive Statistics (4)

- Comparison of multiple curves (ANOVA): Impact of 1, 2, 3 on measurement



- Again: p-value (error probability that difference does not depend on 1-3)
- Partial-Eta-Square: How much of the variation can be explained by the variable (with the treatments 1-3)

Inductive Statistics (5)

- Quasi-endless different kinds of tests for different number of treatmeants and variables

- Take away:
  - Determination of error-probability p
    - Different standard significance tests
  - Value of p depends on
    - Effect size
    - Sample size
    - Scale
    - Applied significance test
    - Deviation (breadth of curve)

Remaining question

- How to design / perform experiments?
  - What kinds of experimental design are possible / desirable?
  - What is the impact of a certain design on the results?
  - What kinds of measurements can be applied?
  - ...

Experiment Design (1)

- Two-group between-subject design
  - One independent variable with two treatments
  - One subject tested under one treatment
  - Two different groups, each contains subjects with same treatment
- Example (Language A, B):
  - A: 1, 2, 3, 4, 1000
  - B: 10, 20, 30, 40, 50

Lang. A	Lang. B
Group A	Group B

- Problem
  - Both groups require subjects with „the same characteristics“
  - Problem: requires „very large“ effect size in order to measure difference (for small sample sizes)

Experiment Design (2)

- Four-group between-subject design
  - Two independent variables with two treatments
  - One subject tested under one treatment
  - Four different groups, each subject assigned to treatment pair
- Example (Language A, B; Programming Task 1, 2)
  - G1 (Language A, Task 1): 1, 2, 3, 4, 1000
  - G2 (Language A, Task 2): ...
  - G3 (Language A, Task 3): ...
  - G4 (Language A, Task 3): ...

	Lang. A	Lang. B
Task 1	Group 1	Group 2
Task 2	Group 3	Group 4

- Problem
  - Groups still require subjects with „the same characteristics“
  - Still: requires „very large“ effect size in order to measure difference (for small sample sizes)

Experiment Design (3)

- Large variety of further designs
  - Repeated measures designs, factorial designs, block designs, ...
  - Between vs. within-subject designs, …

- General problems / considerations
  - Does design match hypotheses?
    - Difference hypotheses, correlation hypotheses, ...
  - Does design permit to determine effect?
    - Effect size, deviation, sample size, statistical power of required significance tests, ...

Experiment Design (4)

- General problem: No measured effect
  - Possible interpretations:
    - Sample size too small
    - Deviation too high
    - Inappropriate design
    - Non-exact measurement
    - ....
  - Alternative interpretation
    - Well, maybe the effect does not exist

- Pure technical problems
- Easy to run into these problems!!!
- NO (!) indicator that main effect does not exist

Experiment Design Example

- Example
  - 2 group experiment, 10 subjects, comparison of Java and Assembler
  - Subjects: First year students
  - Task:
    - Write an algorithm that computes a strongly connected component with O(n^3)
    - ...without using a book on algorithms
  - Assumed result:
    - Average solution requires more than a year development time
    - No measured difference between Java and Assembler
      => very large deviation, small sample size, unbalanced groups,...

=> actual task has a huge impact on measurements

=> be careful when having an experiment without measured effect (p > alpha-level)

Experiment Design: $p > 0.05$

- But
  - if the significant effect of variable is „obvious“ (common community believe)
  - if the number of subjects is „high“ (whatever that means)
  - chosen tasks are the „killer-examples“ for the measured technique
  - …then...

=> Non-significant results are still interesting

(but <u>only!</u> then)

Experiment Design (6)

- Take away: Experiment design
  - ...must match research question
  - ...influences the final result (p-value)
  - ...requires appropriate analysis (t-Test, ANOVA, …)
  - ...results highly depend on actual task
  - <u>...be careful when no effect has been measured</u>

Ok, let's do experiments

… but where and how to start?

Challenges of Empirical Studies

(remember: typically neither hypotheses nor concrete scenario available)

Challenges of Empirical Studies (1)

- Find / invent a hypothesis
- Find situations where hypotheses should be tested
- Find a good design

- Typical problem
  - „*Fighting the deviation / effect-size beast*“

Challenges of Empirical Studies (1)

- Scientific approach
  - Observation of singular events (sample)
    (e.g. developers using a dynamically/statically typed programming language)
    - Formulation of hypothesis
    - Identification of dependent / independent variables
      (e.g. development time depending on type system)
    - Construction of environment
      (IDEs, tasks, languages, machines, …)
  - Collection of subjects
  - Measurements (e.g. development time to solve a certain task)
  - Analysis (mainly inductive statistics)

Challenges of Empirical Studies (2)

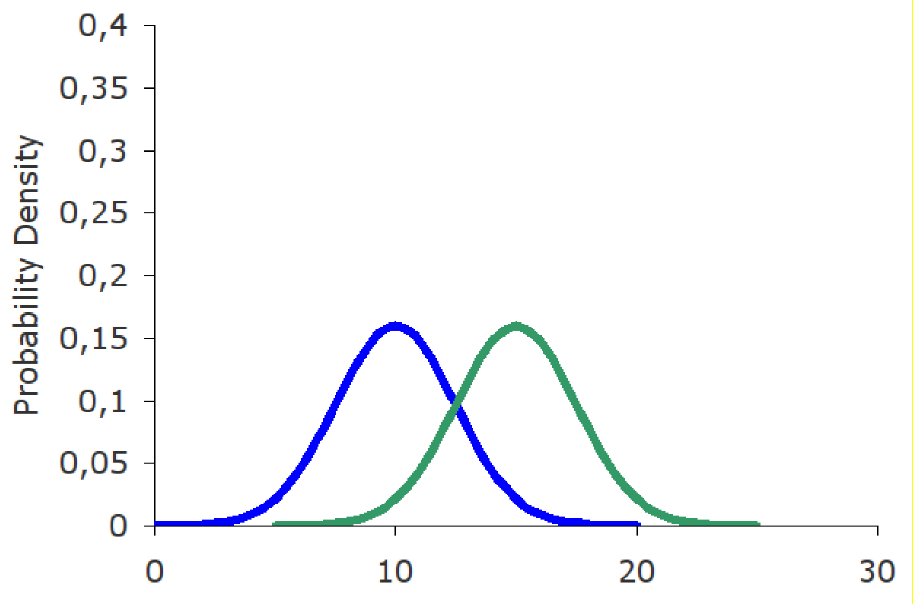
- Find / invent a hypothesis
- Find situations where hypotheses should be tested
- Find a good design

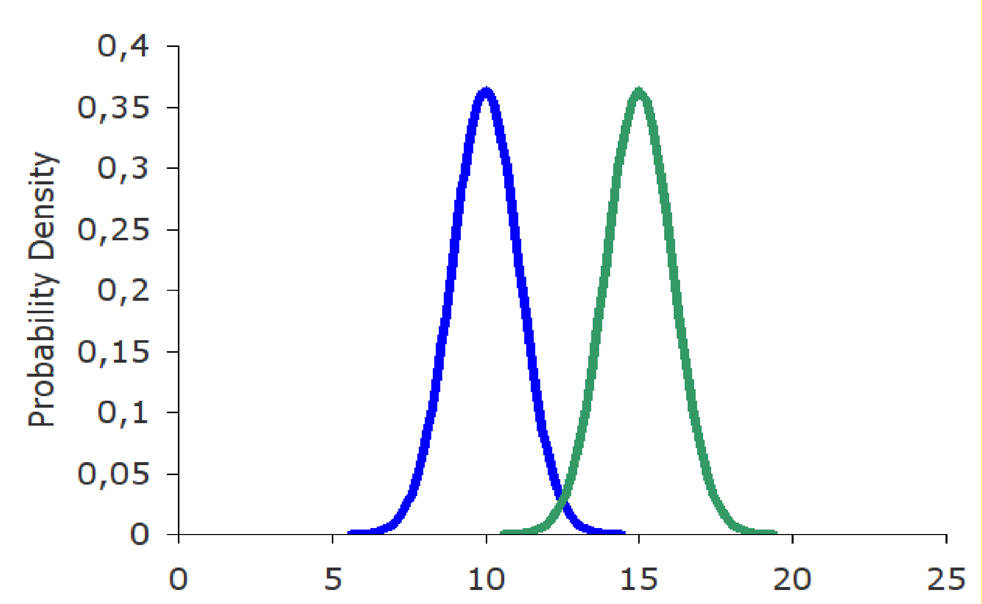
- Typical problem
  - „*Fighting the deviation / effect-size beast*“

Problems: Experiment Design

- Comparison between two samples

Example 1: Same effect size, different deviation





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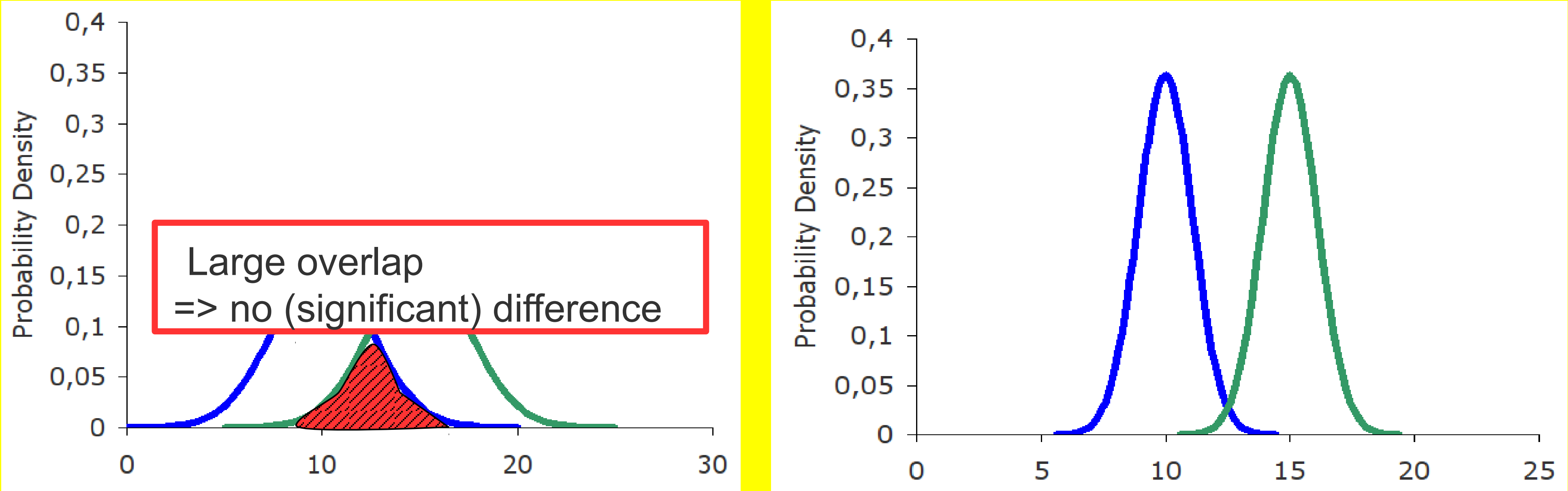

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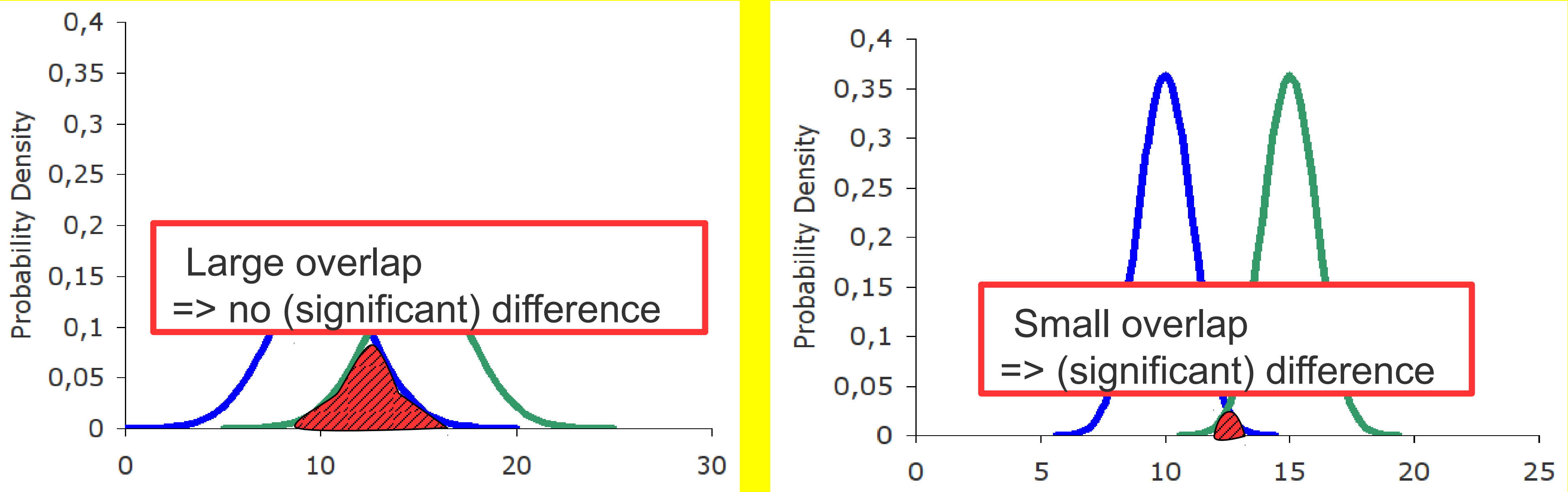
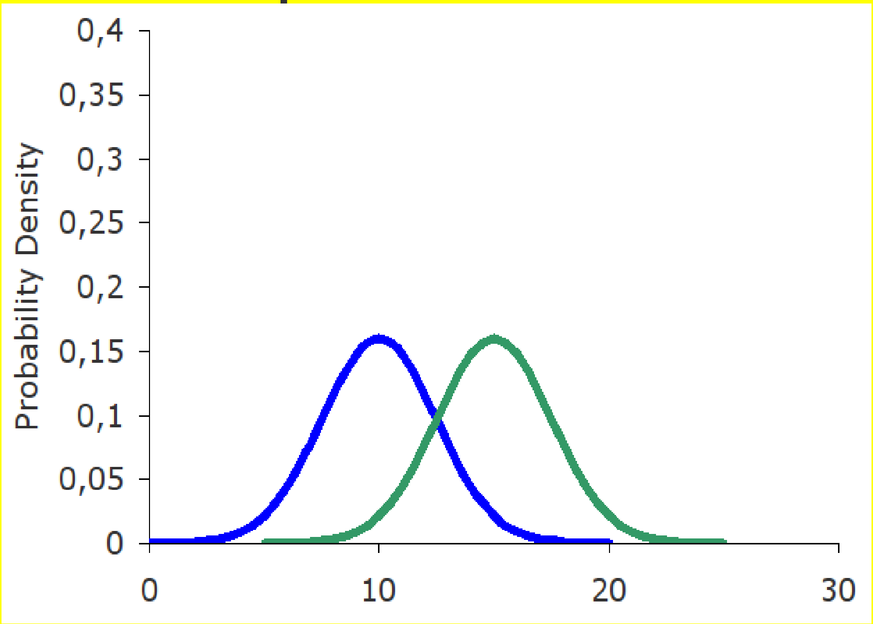
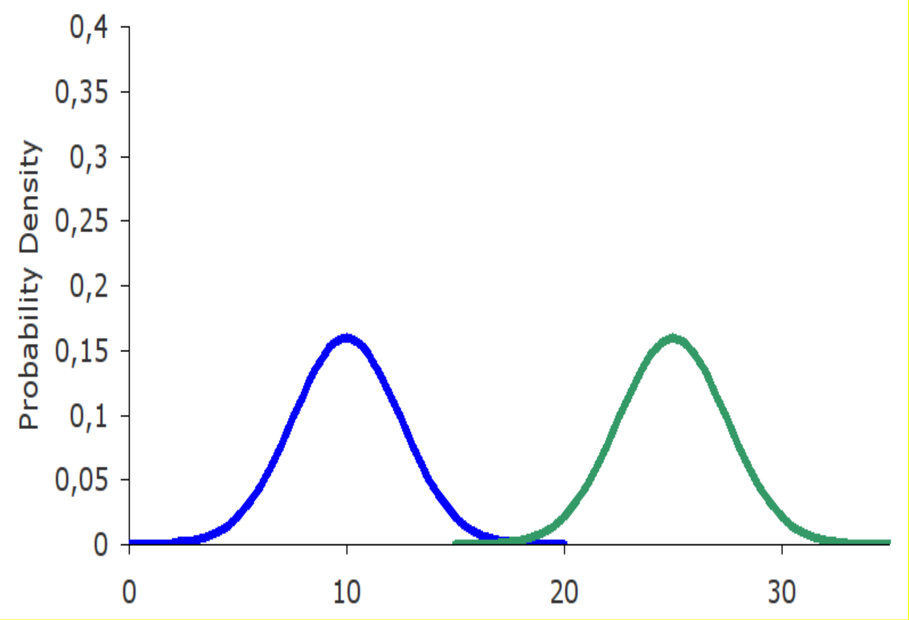


Problems: Experiment Design

- Comparison between two samples

Example 2: Different effect size, same deviation





Problems: Experiment Design

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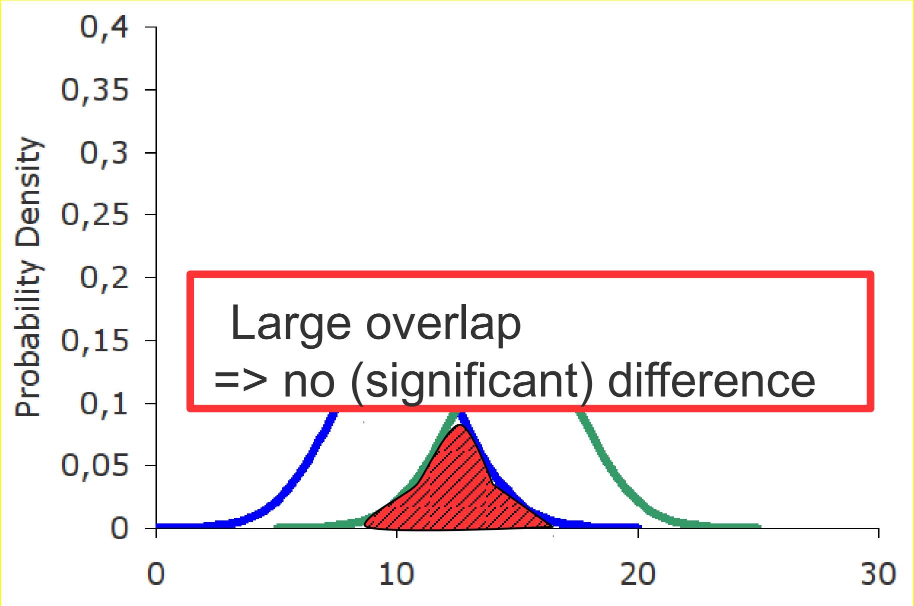
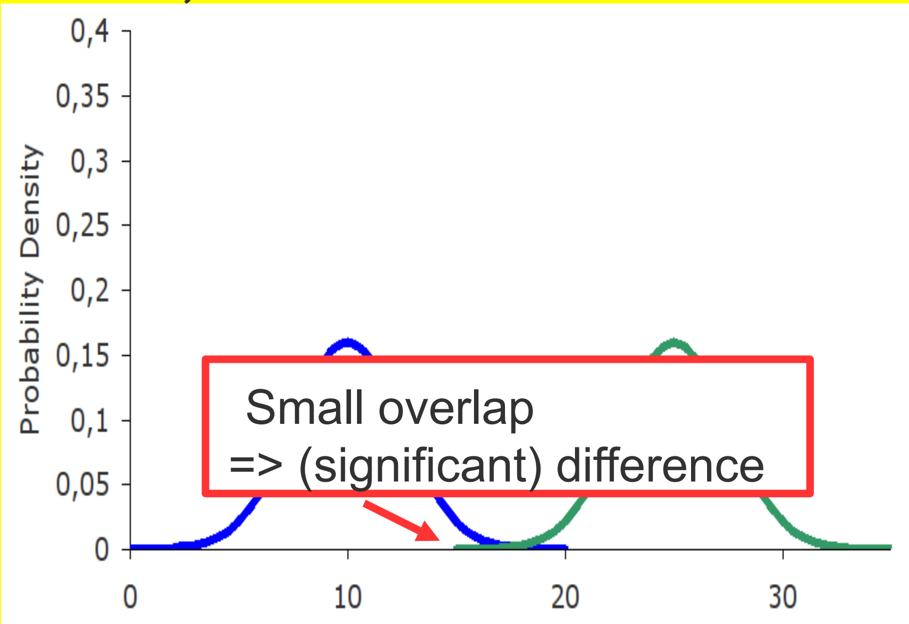




Problem(s) in Experimentation

Conclusion

Experimenter should try to

- **reduce deviation, and/or**
- **increase effect size**

- **Possible ways**
  - **Adaptation of experimental design**
    (e.g. within-subject design) => <u>**Reduction of deviation**</u>
  - **Adaptation of tasks**
    (no development „from scratch“) => <u>**Incease effect size**</u>

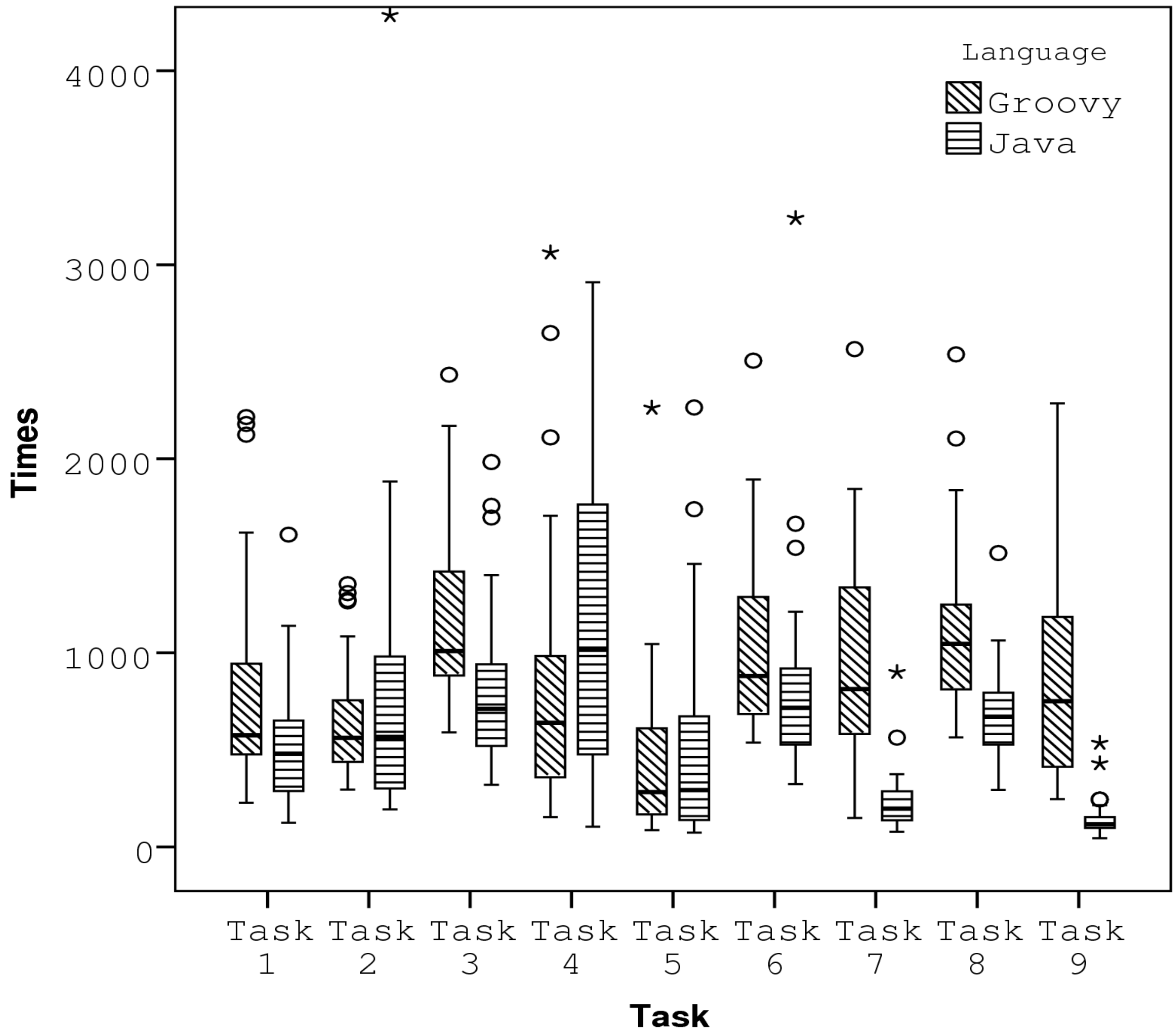
Example: Static Type System

[Kleinschmager, Hanenberg, Robbes, Tanter, Stefik; ICPC'12]

- Background: 4 experiments, „mixed results“
- Idea: Static type systems help when using an undocumented API
- Experiment
  - Java / Groovy as PLs
  - 9 programming tasks (designing tasks took about 2 month)
    - 2 tasks: fix semantic error / 2 tasks: fix type error / 5 tasks: use API classes
  - 33 subjects (mainly students)
  - Within-subject design (2 groups)
- Result
  - Positive effect for 6/9 tasks
    - No effect on fixing semantic error
    - Positive effect on fixing type error
    - Mostly (4/5) positive effect on using API classes

Example: Static Type System

- Task 4,5: Semantic errors
- 1,2,3,6,8: New class usage
- 7, 10: Type errors



Example: Static Type System

- Potential problems
  - Artificially constructed API
    - parameter names do not reflect on type names (but on names chosen from the domain)
    - Is it repesentative?
  - Artificially constructed environment
  - Artificial programming tasks
  - Java type system
- <u>Maybe we measured something else</u>
  - „*Existence of type annotations in the code help....no matter whether they are statically type checked or not*“
- <u>Maybe „in the wild“ positive effect of static type system „vanishs“</u>
  - There is no generalizability

Example: Static Type System

- How to go on?
  - Traditional way
    - „We have done an experiment on type systems and found differences, let's go to the next topic“
  - Alternative way
    - Go on with experimentation on type systems
      - Variations on type systems, IDE support, replication of experiments, etc.
    - Try to find correlation hypothesis that survives falsification trials

Where to Start?

- Relatively few textbooks available specific to software engineering

Where to Start?

- Huge bunch of textbooks outside the domain of software engineering

- Psychology
- Social Sciences
- Medicine
- …

- Why not just use these books?
- Problem: Different domains have different problems...

Problem of different domains

- What is the difference between measuring blood pressure and software development time?

Problem of different domains

- Blood pressure
  - You will hardly find two (living) human subjects on this planet whose blood pressure differs by factor 10 (even factor 5 is unlikely)

- Software development time
  - It is hard to find a sample of human subjects where development time between best and worst developer is less than factor 5

=> Large set of experimental designs / statistical methods from for example medicine cannot be (directly) used in software construction

State of the Art: Empirical SE

State of the Art: Empirical SE (1)

- Empirical approach typically not taught to students
  - ...how can students check whether a statement „static type systems are good for developer hold“?
  - ...how can students understand an empirical study they are reading about?
  - ...how can a student perform such a study?

- There are empirical studies / controlled experiments (ok, not that many)

State of the Art: Empirical SE (2)

- Typically, a large number of experiments suffer from general problems (experiment design as well as analysis)

- A lot of techniques come up without a hypothesis / proposed measurement
  - Example: „*Eclipse is quite a mature IDE and helps developer a lot*“

    => Experimenter becomes „inventor of hypothesis to be tested“

State of the Art: Empirical SE

- Theories mainly describe existence of a difference
  - ...“static type systems better than dynamic type systems“
  - ...empirical knowledge rather low
- Theories typically do not try to quantify differences (for some good reasons)
  - ...empirical knowledge rather low
- Experimenter currently have to „invent situations for language constructs on their own“
  - Example: Java vs. Assembler....

Empirical SE: Open issues

- Endless list of open issues
  - How can we distinguish good from bad developers upfront?
    - Fundamental question for certain experiment designs (factorial design, block design, etc.)
  - What kind of programming tasks are worth being studied?
    - What tasks do have small deviations, which represent „daily programming tasks“?
  - What tool support should be delivered in an experiment?
    - Most often, no data for tools is available...
  - ...

Long term goal of SE

- Theories
  - Descriptions of situations where certain constructs dominate others (size of difference part of theory)
  - Large number of experiments that try to falsify theories
  - Example (very first initial step):
    - „When using an undocumented API, ...
      ....static typing reduces development time“

- General kind of theory:
  - „*When the code is of kind X, ...*
    ...the use of construct A leads to C
    ...which differs to construct B by factor...“

Discussion & Conclusion

- Controlled experiments as a research method
  - Statistics, experiment designs
- Many, many problems
  - Missing experimentation in the past, basics, organizational issues
  - ...

Conclusion

- <u>We must teach experimentation</u>
  - Help people/students to understand what's going on
  - Students need to know <u>methods</u> which permit to identify techniques which are „bad, time consuming, error prone“

- <u>We need to integrate experimentation in our courses</u>
  - The SE course should not say „Pair programming is good“, it should also introduce the experiments which revealed that effect

- <u>We must do experimentation</u>
  - We want to improve the life of developers & users
  - <u>This does NOT mean that we should ignore the machines</u>

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