Experimental Research in Evolutionary Computation

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Overview

Scientific Goals?

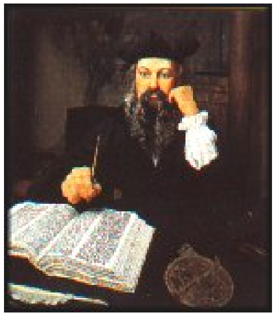

Figure: Nostradamus

- Why is astronomy considered scientific—and astrology not?
- And what about experimental research in EC?

Goals in Evolutionary Computation

(RG-1) *Investigation.* Specifying optimization problems, analyzing algorithms. Important parameters; what should be optimized?

(RG-2) *Comparison.* Comparing the performance of heuristics

(RG-3) *Conjecture.* Good: demonstrate performance. Better: explain and understand performance

(RG-4) *Quality.* Robustness (includes insensitivity to exogenous factors, minimization of the variability) [Mon01]

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Goals in Evolutionary Computation

- Given: Hard real world optimization problems, e.g., chemical engineering, airfoil optimization, bioinformatics
- Many theoretical results are too abstract, do not match with reality
- Real programs, not algorithms
- Develop problem specific algorithms, experimentation is necessary
- Experimentation requires statistics

A Totally Subjective History of Experimentation in Evolutionary Computation

- Palaeolithic
- Yesterday
- Today
- Tomorrow

Stone Age: Experimentation Based on Mean Values

- First phase (foundation and development, before 1980)
- Comparison based on mean values, no statistics
- Development of standard benchmark sets (sphere function etc.)
- Today: Everybody knows that mean values are not sufficient

Stone Age Example: Comparison Based on Mean Values

Example (PSO swarm size)

- Experimental setup:
  - 4 test functions: Sphere, Rosenbrock, Rastrigin, Griewangk
  - Initialization: asymmetrically
  - Termination: maximum number of generations
  - PSO parameter: default
- Results: Table form, e.g.,

Table: Mean fitness values for the Rosenbrock function

Population	Dimension	Generation	Fitness
20	10	1000	96,1725
20	20	1500	214,6764

- Conclusion: “Under all the testing cases, the PSO always converges very quickly”

Yesterday: Mean Values and Simple Statistics

- Second phase (move to mainstream, 1980-2000)
- Statistical methods introduced, mean values, standard deviations, tutorials
- *t* test, *p* value, . . .
- Comparisons mainly on standard benchmark sets
- Questionable assumptions (NFL)

Yesterday: Mean Values and Simple Statistics

Example (GAs are better than other algorithms (on average))

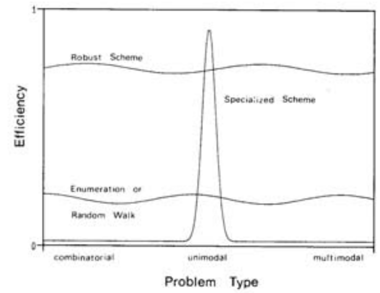

Figure: [Gol89]

Theorem (NFL)

There is no algorithm that is better than another over all possible instances of optimization problems

Today: Based on Correct Statistics

- Third phase (Correct statistics, since 2000)
  - Statistical tools for EC
  - Conferences, tutorials, workshops, e.g., Workshop On Empirical Methods for the Analysis of Algorithms (EMAA) (`http://www.imada.sdu.dk/~marco/EMAA`)
  - New disciplines such as algorithm engineering
- But: There are three kinds of lies: lies, damned lies, and statistics (Mark Twain or Benjamin Disraeli), why should we care?
- Because it is the only tool we can rely on (at the moment,i.e., 2006)

Today: Based on Correct Statistics

Example (Good practice)

Table 3: Results of the algorithms with population of 20

Test functions	SGA mean best (std. dev.)	FDGA			t-value between SGA to the best FDGA	Best algorithm
		OGA mean best (std. dev.)	MGA mean best (std. dev.)	IGA mean best (std. dev.)		
f_1	8.0607e+000 1.5537e+000	8.5689e+000 1.6671e+000	8.6545e+000 1.5069e+000	8.2722e+000 1.5728e+000	-6.76 *	SGA
f_2	7.8010e-001 4.5832e+000	4.2479e+000 1.3211e+000	3.5444e+000 2.0873e+000	3.5093e+000 1.4978e+000	-4.00 *	SGA
f_3	6.4957e+000 1.8003e+000	9.2728e+000 1.8037e+000	8.6608e+000 1.8614e+000	8.6378e+000 1.9866e+000	-5.65 *	SGA
f_4	1.3506e+002 3.3349e+002	9.2200e+002 2.8070e+002	8.2073e+002 2.5999e+002	8.2272e+002 2.4858e+002	-11.69 *	SGA
f_5	2.7476e-002 3.0828e-002	6.8234e-002 5.4773e-002	8.2052e-002 5.2042e-002	6.2478e-002 5.5991e-002	-3.87 *	SGA
f_6	2.0791e-003 9.1846e-004	2.7050e-006 3.5287e-006	2.5915e-006 3.3219e-006	2.5830e-005 2.7375e-006	15.81 *	FDGA
f_7	2.0791e-003 9.1846e-004	4.3338e-011 7.5496e-012	4.0195e-011 8.0494e-012	4.0062e-011 8.3297e-012	1.91 *	FDGA
f_8	7.1211e+001 7.1211e+001	5.0154e+001 4.1123e+001	5.1774e+001 3.7574e+001	4.0649e+001 4.1068e+001	3.13 *	FDGA
f_9	1.4856e-001 6.2373e-002	5.1283e-002 4.1936e-003	4.6518e-002 1.6727e-002	4.6506e-002 1.2852e-002	11.33 *	FDGA
f_{10}	9.2123e-002 6.1055e-002	7.2324e-002 2.1381e-002	6.4803e-002 2.1804e-002	6.4846e-002 2.4023e-002	2.94 *	FDGA

* The value of t with 49 degree of freedom is significant at $\alpha = 0.05$ by a one tailed test.

Figure: [CAF04]

Today: Based on Correct Statistics

Example (Good practice?)

- Authors used
  - Pre-defined number of evaluations set to 200,000
  - 50 runs for each algorithm
  - Population sizes 20 and 200
  - Crossover rate 0.1 in algorithm *A*, but 1.0 in *B*
  - *A* outperforms *B* significantly in f_6 to f_{10}

- We need tools to
  - Determine adequate number of function evaluations to avoid floor or ceiling effects
  - Determine the correct number of repeats
  - Determine suitable parameter settings for comparison
  - Determine suitable parameter settings to get working algorithms
  - Draw meaningful conclusions

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Today: Based on Correct Statistics

- We claim: Fundamental ideas from statistics are misunderstood!
- For example: What is the *p* value?

Definition (*p* value)

The *p* value is the probability that the null hypothesis is true

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Definition (*p* value)

The *p* value is the probability that the null hypothesis is true. No!

Today: Based on Correct Statistics

- We claim: Fundamental ideas from statistics are misunderstood!
- For example: What is the p value?

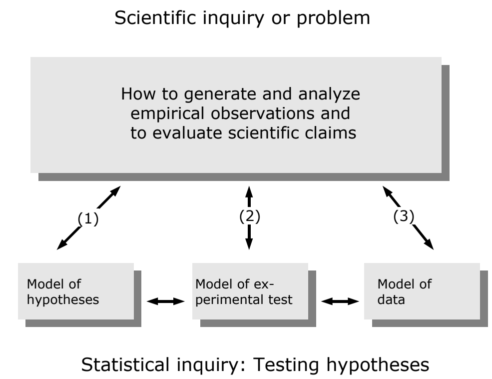
Definition (p value)

The p value is $p = P\{$ result from test statistic, or greater $|$ null model is true $\}$

- $\Rightarrow$ The p value is not related to any probability whether the null hypothesis is true or false

Tomorrow: Correct Statistics and Correct Conclusions

- Problems of today:
  Adequate statistical methods, but wrong scientific conclusions
- Tomorrow:
  - Consider scientific meaning
  - Severe testing as a basic concept
  - First Symposium on Philosophy, History, and Methodology of Error, June 2006



Tomorrow: Correct Statistics and Correct Conclusions

- Generally: Statistical tools to decide whether a is better than b are necessary
- Today: Sequential parameter optimization (SPO)
  - Heuristic, but implementable approach
  - Extension of classical approaches from statistical design of experiments (DOE)
  - Other (better) approaches possible
  - SPO uses plots of the observed significance

Tests and Significance

- Plots of the observed significance level based on [May83]
- Rejection of the null hypothesis $H : \theta = \theta_0$ by a test T^+ based on an observed average $\overline{x}$
- Alternative hypothesis $J : \theta > \theta_0$

Definition (Observed significance level)

The observed significance level is defined as

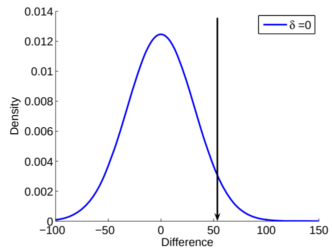
$$\alpha(\overline{x}, \theta) = \hat{\alpha}(\theta) = P(\overline{X} \geq \overline{x} | \theta) \tag{1}$$

Plots of the Observed Significance

- Observed significance level

$$\alpha(\overline{x}, \theta) = \hat{\alpha}(\theta) = P(\overline{X} \geq \overline{x}|\theta)$$

- Observed average $\overline{x} = 51.73$



- Rejection of the null hypothesis

$$H : \theta = \theta_0 = 0$$

by a test T^+ in favor of an alternative

$$J : \theta > \theta_0$$

Then $\hat{\alpha}(\theta) = 0.0530$

- Interpretation: Frequency of erroneously rejecting H ("there is a difference in means as large as θ_0 or larger") with such an $\overline{x}$

Plots of the Observed Significance

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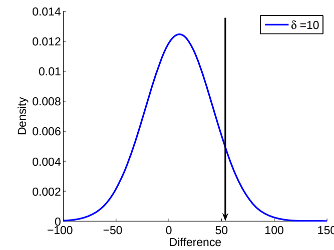


- Rejection of the null hypothesis

$$H : \theta = \theta_0 = 10$$

by a test T^+ in favor of an alternative

$$J : \theta > \theta_0$$

Then $\hat{\alpha}(\theta) = 0.0961$

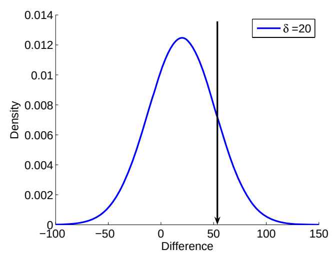
- Interpretation: Frequency of erroneously rejecting H ("there is a difference in means as large as θ_0 or larger") with such an $\overline{x}$

Plots of the Observed Significance

- Observed significance level

$$\alpha(\overline{x}, \theta) = \hat{\alpha}(\theta) = P(\overline{X} \geq \overline{x}|\theta)$$

- Observed average $\overline{x} = 51.73$



- Rejection of the null hypothesis

$$H : \theta = \theta_0 = 20$$

by a test T^+ in favor of an alternative

$$J : \theta > \theta_0$$

Then $\hat{\alpha}(\theta) = 0.1607$

- Interpretation: Frequency of erroneously rejecting H ("there is a difference in means as large as θ_0 or larger") with such an $\overline{x}$

Plots of the Observed Significance

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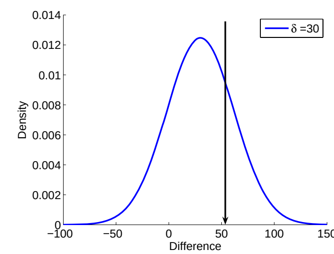
$$\alpha(\overline{x}, \theta) = \hat{\alpha}(\theta) = P(\overline{X} \geq \overline{x}|\theta)$$

- Observed average $\overline{x} = 51.73$



- Rejection of the null hypothesis

$$H : \theta = \theta_0 = 30$$

by a test T^+ in favor of an alternative

$$J : \theta > \theta_0$$

Then $\hat{\alpha}(\theta) = 0.2485$

- Interpretation: Frequency of erroneously rejecting H ("there is a difference in means as large as θ_0 or larger") with such an $\overline{x}$

Plots of the Observed Significance

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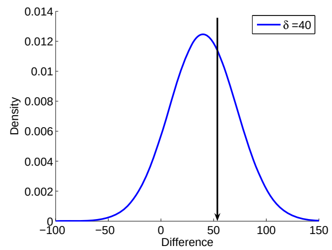
$$\alpha(\overline{x}, \theta) = \hat{\alpha}(\theta) = P(\overline{X} \geq \overline{x} | \theta)$$

- Observed average $\overline{x} = 51.73$



- Rejection of the null hypothesis

$$H : \theta = \theta_0 = 40$$

by a test T^+ in favor of an alternative

$$J : \theta > \theta_0$$

Then $\hat{\alpha}(\theta) = 0.3570$

- Interpretation: Frequency of erroneously rejecting H ("there is a difference in means as large as θ_0 or larger") with such an $\overline{x}$

Plots of the Observed Significance

- Observed significance level

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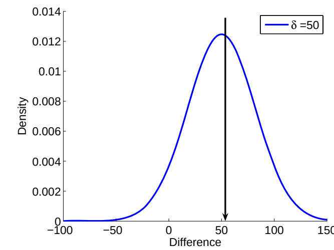
- Observed average $\overline{x} = 51.73$



- Rejection of the null hypothesis

$$H : \theta = \theta_0 = 50$$

by a test T^+ in favor of an alternative

$$J : \theta > \theta_0$$

Then $\hat{\alpha}(\theta) = 0.4784$

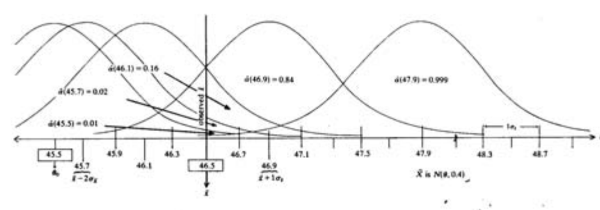
- Interpretation: Frequency of erroneously rejecting H ("there is a difference in means as large as θ_0 or larger") with such an $\overline{x}$

Small α Values

- Rejecting H with a T^+ test with a small size α indicates that $J : \theta > \theta_0$
- If any and all positive discrepancies from θ_0 are scientifically important $\Rightarrow$ small size α ensures that construing such a rejection as indicating a scientifically important θ would rarely be erroneous
- Problems if some θ values in excess of θ_0 are not considered scientifically important
- Small size α does not prevent a T^+ rejection of H from often being misconstrued when relating it to the scientific claim
- $\Rightarrow$ Small α values alone are not sufficient

Largest Scientifically Unimportant Values

- [May83] defines θ_{un} the largest scientifically unimportant θ value in excess of θ_0
- But what if we do not know θ_{un}?
- Discriminate between legitimate and illegitimate construals of statistical results by considering the values of $\hat{\alpha}(\theta')$ for several θ' values



OSL Plots

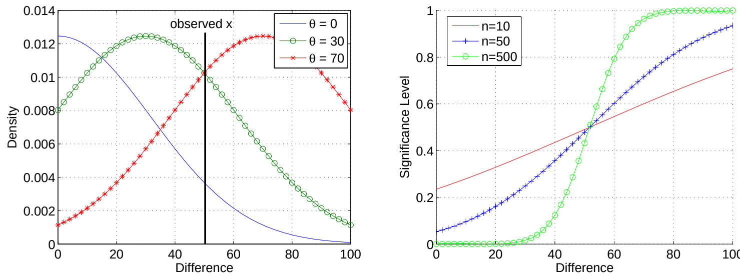


Figure: Plots of the observed difference. *Left*: This is similar to Fig. 4.3 in [May83]. Based on $n = 50$ experiments, a difference $\overline{x} = 51.3$ has been observed, $\hat{\alpha}(\theta)$ is the area to the right of the observed difference $\overline{x}$. *Right*: The $\hat{\alpha}(\theta)$ value is plotted for different n values.

OSL Plots

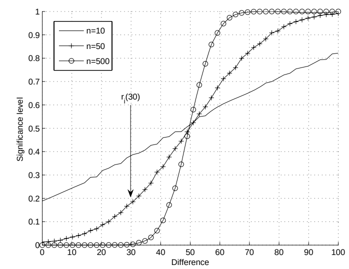


Figure: Same situation as above, bootstrap approach

- Bootstrap procedure ⇒ no assumptions on the underlying distribution necessary
- Summary:
  - p value is not sufficient
  - OSL plots one tool to derive meta-statistical rules
  - Other tools needed

The Art of Comparison

Orientation

The NFL[1] told us things we already suspected:

- We cannot hope for the one-beats-all algorithm (solving the general nonlinear programming problem)
- Efficiency of an algorithm heavily depends on the problem(s) to solve and the exogenous conditions (termination etc.)

In consequence, this means:

- The posed question is of extreme importance for the relevance of obtained results
- The focus of comparisons has to change from:

  Which algorithm is better?

  to

  What exactly is the algorithm good for?

[1] no free lunch theorem

The Art of Comparison

Efficiency vs. Adaptability

Most existing experimental studies focus on the efficiency of optimization algorithms, but:

- Adaptability to a problem is not measured, although
- It is known as one of the important advantages of EAs

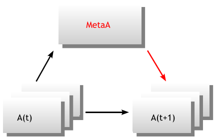
Interesting, previously neglected aspects:

- Interplay between adaptability and efficiency?
- How much effort does adaptation to a problem take for different algorithms?
- What is the problem spectrum an algorithm performs well on?
- Systematic investigation may reveal inner logic of algorithm parts (operators, parameters, etc.)

Similarities and Differences to Existing Approaches

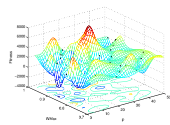
- Agriculture, industry: Design of Experiments (DoE)


- Evolutionary algorithms: Meta-algorithms



- Algorithm engineering: Rosenberg Study (ANOVA)

- Statistics: Design and Analysis of Computer Experiments (DACE)

Empirical Analysis: Algorithms for Scheduling Problems

- Problem:
  - Jobs build binary tree
  - Parallel computer with ring topology
- 2 algorithms:
  - Keep One, Send One (KOSO) to my right neighbor
  - Balanced strategy KOSO*: Send to neighbor with lower load only
- Is KOSO* better than KOSO?

spo Rosenberg Study: Problem Definition and Scientific Hypothesis

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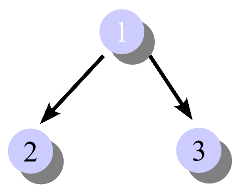

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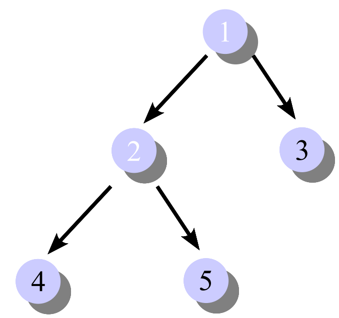

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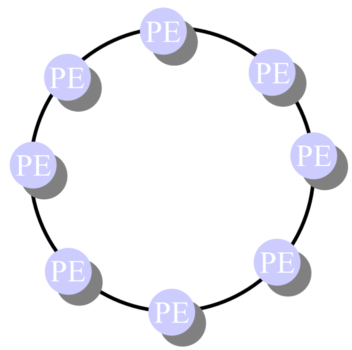

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Empirical Analysis: Algorithms for Scheduling Problems

- Hypothesis: Algorithms influence running time
- But: Analysis reveals

  # Processors und # Jobs explain 74 % of the variance of the running time

  Algorithms explain nearly nothing
- Why?

  Load balancing has no effect, as long as no processor starves.

  But: Experimental setup produces many situations in which processors do not starve
- Furthermore: Comparison based on the optimal running time (not the average) makes differences between KOSO und KOSO*.
- Summary: Problem definitions and performance measures (specified as algorithm and problem design) have significant impact on the result of experimental studies

Designs

- Sequential Parameter Optimization based on
  - Design of Experiments (DOE)
  - Design and Analysis of Computer Experiments (DACE)
- Optimization run = experiment
- Parameters = design variables or factors
- Endogenous factors: modified during the algorithm run
- Exogenous factors: kept constant during the algorithm run
  - Problem specific
  - Algorithm specific

Algorithm Designs

Example (Algorithm design)

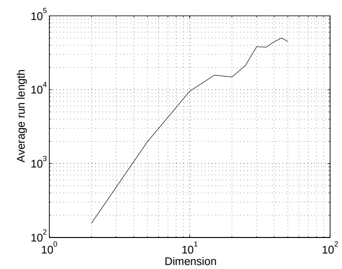
Particle swarm optimization. Set of exogenous strategy parameters

- Swarm size s
- Cognitive parameter c_1
- Social parameter c_2
- Starting value of the inertia weight $w_{\max}$
- Final value of the inertia weight w_{scale}
- Percentage of iterations for which $w_{\max}$ is reduced
- Maximum value of the step size $v_{\max}$

Problem Designs

Example (Problem design)

Sphere function $\sum_{i=1}^{d} x_i^2$ and a set of d-dimensional starting points, performance measure, termination criterion



- Tuning (efficiency):
  - Given one problem instance $\Rightarrow$ determine improved algorithm parameters
- Robustness (effectivity):
  - Given one algorithm $\Rightarrow$ test several problem instances

SPO Overview

1. Pre-experimental planning
2. Scientific thesis
3. Statistical hypothesis
4. Experimental design: Problem, constraints, start-/termination criteria, performance measure, algorithm parameters
5. Experiments
6. Statistical model and prediction (DACE). Evaluation and visualization
7. Solution good enough?
   Yes: Goto step 8
   No: Improve the design (optimization). Goto step 5
8. Acceptance/rejection of the statistical hypothesis
9. Objective interpretation of the results from the previous step

Statistical Model Building and Prediction

Design and Analysis of Computer Experiments (DACE)

- Response Y: Regression model and random process
- Model:

$$Y(x) = \sum_h \beta_h f_h(x) + Z(x)$$

  - $Z(\cdot)$ correlated random variable
  - Stochastic process.
  - DACE stochastic process model
- Until now: DACE for deterministic functions, e.g. [SWN03]
- New: DACE for stochastic functions

Expected Model Improvement

Design and Analysis of Computer Experiments (DACE)

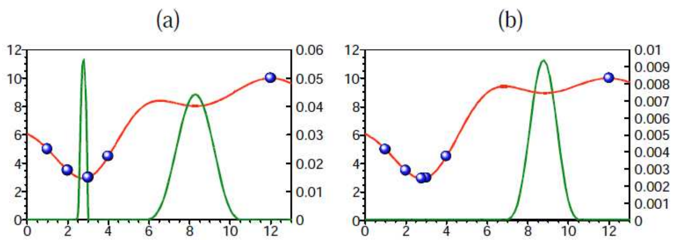


Figure: Axis labels left: function value, right: expected improvement. Source: [JSW98]

(a) Expected improvement: 5 sample points

(b) Another sample point $x = 2.8$ was added

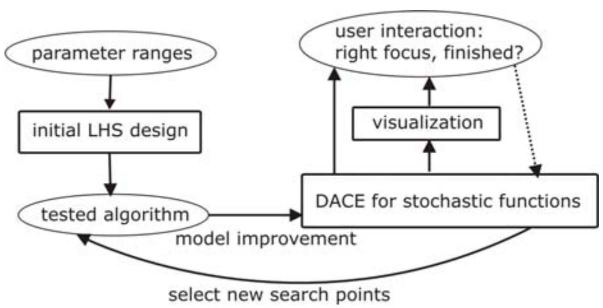
Heuristic for Stochastically Disturbed Function Values

- Latin hypercube sampling (LHS) design: Maximum spread of starting points, small number of evaluations
- Sequential enhancement, guided by DACE model
- Expected improvement: Compromise between optimization (min Y) and model exactness (min MSE)
- Budget-concept: Best search points are re-evaluated
- Fairness: Evaluate new candidates as often as the best one

Table: SPO. Algorithm design of the best search points

Y	s	c_1	c_2	$w_{\max}$	w_{scale}	w_{iter}	$v_{\max}$	Conf.	n
0.055	32	1.8	2.1	0.8	0.4	0.5	9.6	41	2
0.063	24	1.4	2.5	0.9	0.4	0.7	481.9	67	4
0.061	32	1.8	2.1	0.8	0.4	0.5	9.6	41	4
0.058	32	1.8	2.1	0.8	0.4	0.5	9.6	41	8

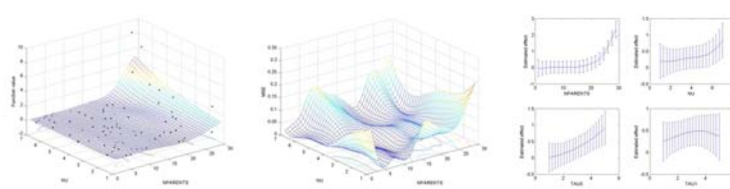
Data Flow and User Interaction



- User provides parameter ranges and tested algorithm
- Results from an LHS design are used to build model
- Model is improved incrementally with new search points
- User decides if parameter/model quality is sufficient to stop

SPO in Action

- Sequential Parameter Optimization Toolbox (SPOT)
- Introduced in [BB06]



- Software can be downloaded from `http://ls11-www.cs.uni-dortmund.de/people/tom/ExperimentalResearchPrograms.html`

SPO Installation

- Create a new directory, e.g., `g:\myspot`
- Unzip SPO toolbox: `http://ls11-www.cs.uni-dortmund.de/people/tom/spot03.zip`
- Unzip MATLAB DACE toolbox: `http://www2.imm.dtu.dk/~hbn/dace/`
- Unzip ES package: `http://ls11-www.cs.uni-dortmund.de/people/tom/esmatlab03.zip`
- Start MATLAB
- Add `g:\myspot` to MATLAB path
- Run `demoSpotMatlab.m`

SPO Region of Interest (ROI)

- *Region of interest* (ROI) files specify the region, over which the algorithm parameters are tuned

```
name low high isint pretty
NPARENTS 1 10 TRUE 'NPARENTS'
NU 1 5 FALSE 'NU'
TAU1 1 3 FALSE 'TAU1'
```

Figure: demo4.roi

SPO Configuration file

- *Configuration* files (CONF) specify SPO specific parameters, such as the regression model

```
new=0
defaulttheta=1
loval=1E-3
upval=100
spotrmodel='regpoly2'
spotcmodel='corrgauss'
isotropic=0
repeats=3
...
```

Figure: demo4.m

SPO Output file

- *Design* files (DES) specify algorithm designs
- Generated by SPO
- Read by optimization algorithms

```
TAU1 NPARENTS NU TAU0 REPEATS CONFIG SEED STEP
0.210507 4.19275 1.65448 1.81056 3 1 0 1
0.416435 7.61259 2.91134 1.60112 3 2 0 1
0.130897 9.01273 3.62871 2.69631 3 3 0 1
1.65084 2.99562 3.52128 1.67204 3 4 0 1
0.621441 5.18102 2.69873 1.01597 3 5 0 1
1.42469 4.83822 1.72017 2.17814 3 6 0 1
1.87235 6.78741 1.17863 1.90036 3 7 0 1
0.372586 3.08746 3.12703 1.76648 3 8 0 1
2.8292 5.85851 2.29289 2.28194 3 9 0 1
...
```

Figure: demo4.des

Algorithm: Result File

- Algorithm run with settings from design file
- Algorithm writes *result file* (RES)
- RES files provide basis for many statistical evaluations/visualizations
- RES files read by SPO to generate stochastic process models

```
Y NPARENTS FNAME ITER NU TAU0 TAU1 KAPPA NSIGMA RHO DIM CONFIG SEED
3809.15 1 Sphere  500 1.19954 0 1.29436 Inf 1 2 2  1 1
0.00121541  1 Sphere  500 1.19954 0 1.29436 Inf 1 2 2 1 2
842.939 1 Sphere 500 1.19954 0 1.29436 Inf 1 2 2  1 3
2.0174e-005 4 Sphere 500 4.98664 0 1.75367 Inf 1 2 2  2 1
0.000234033 4 Sphere 500 4.98664 0 1.75367 Inf 1 2 2  2 2
1.20205e-007  4 Sphere 500 4.98664 0 1.75367 Inf 1 2 2  2 3
...
```

Figure: demo4.res

Summary: SPO Interfaces

- SPO requires CONF and ROI files
- SPO generates DES file
- Algorithm run with settings from DES
- Algorithm writes *result file* (RES)
- RES files read by SPO to generate stochastic process models
- RES files provide basis for many statistical evaluations/visualizations (EDA)

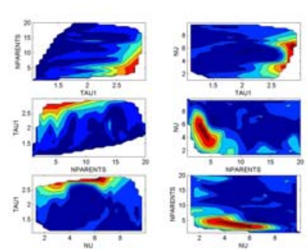
Figure: SPO Interfaces

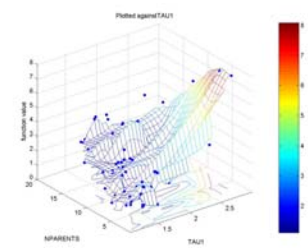
SPO live

- Tunig of an ES on the sphere (demo4)
- Compare best from initial LHD and tuned design (demo5)
- Include recommendations from literature (demo6)
- How do the results change if the dimension is increased? (demo8)
- Demos available from:
  `http://www.springer.com/3-540-32026-1` ($\geq$ August 2006)

SPO and EDA

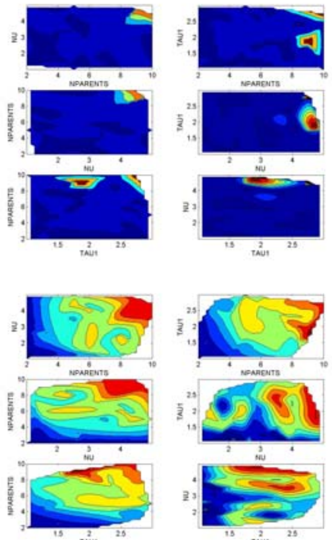
- Interaction plots
- Main effect plots
- Regression trees
- Scatter plots
- Box plots
- Trellis plots
- Design plots
- ...





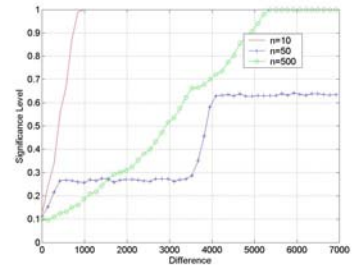
How to Perform an Experimental Analysis

- Scientific claim: “ES with small populations perform better than ES with larger ones on the sphere.”
- Statistical hypotheses:
  - ES with, say $\mu = 2$, performs better than ES with *mu* > 2 if compared on problem design $^{(1)}_{p}$
  - ES with, say $\mu = 2$, performs better than ES with *mu* > 2 if compared on problem design $^{(2)}_{p}$
  - ...
  - ES with, say $\mu = 2$, performs better than ES with *mu* > 2 if compared on problem design $^{(n)}_{p}$



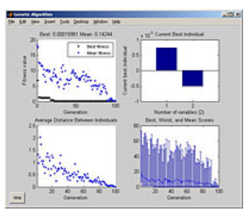
SPO Open Questions

- Models?
  - (Linear) Regression models
  - Stochastic process models
- Designs?
  - Space filling
  - Factorial
- Statistical tools
- Significance
- Standards



SPOT Community

- Provide SPOT interfaces for important optimization algorithms
- Simple and open specification
- Currently available (April 2006) for the following products:



Program	Language	
Evolution Strategy	JAVA, MATLAB	`http://www.springer.com/3-540-32026-1`
Genetic Algorithm and Direct Search Toolbox	MATLAB	`http://www.mathworks.com/products/gads`
Particle Swarm Optimization Toolbox	MATLAB	`http://psotoolbox.sourceforge.net`

Discussing SPO

- SPO is not the final solution—it is one possible (but not necessarily the best) solution
- Goal: continue a discussion in EC, transfer results from statistics and the philosophy of science to computer science

What is the Meaning of Parameters?

Are Parameters "Bad"?

Cons:

- Multitude of parameters dismays potential users
- It is often not trivial to understand parameter-problem or parameter-parameter interactions

  $\Rightarrow$ Parameters complicate evaluating algorithm performances

But:

- Parameters are simple handles to modify (adapt) algorithms
- Many of the most successful EAs have lots of parameters
- New theoretical approaches: Parametrized algorithms / parametrized complexity, ("two-dimensional" complexity theory)

Possible Alternatives?

Parameterless EAs:

- Easy to apply, but what about performance and robustness?
- Where did the parameters go?

Usually a mix of:

- Default values, sacrificing top performance for good robustness
- Heuristic rules, applicable to *many* but not *all* situations; probably not working well for completely new applications
- (Self-)Adaptation techniques, these cannot learn too many parameter values at once, and not necessarily reduce the number of parameters

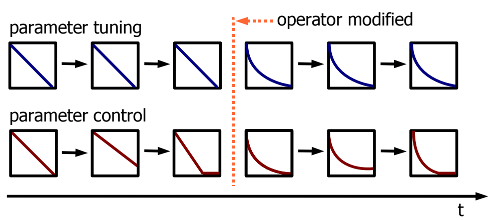
$\Rightarrow$ We can reduce number of parameters, but usually at the cost of either performance or robustness

Parameter Control or Parameter Tuning?

The time factor:

- Parameter control: during algorithm run
- Parameter tuning: before an algorithm is run

But: Recurring tasks, restarts, or adaptation (to a problem) blur this distinction

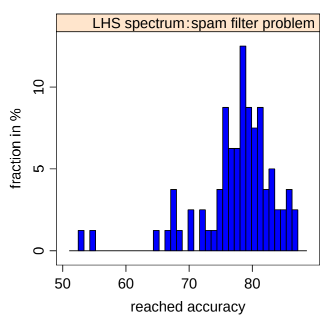


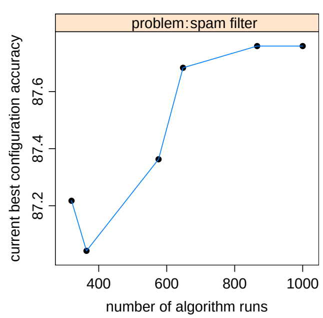
And: How to find meta-parameter values for parameter control?
$\Rightarrow$ Parameter control *and* parameter tuning

Tuning and Comparison

What do Tuning Methods (e.g. SPO) Deliver?

- A best configuration from $\{perf(alg(arg_t^{exo}))|1 \leq t \leq T\}$ for T tested configurations
- A spectrum of configurations, each containing a set of single run results
- A progression of current best tuning results





How do Tuning Results Help?

...or Hint to new Questions

What we get:

- A near optimal configuration, permitting top performance comparison
- An estimation of how good any (manually) found configuration is
- A (rough) idea how hard it is to get even better

No excuse: A first impression may be attained by simply doing an LHS

Yet unsolved problems:

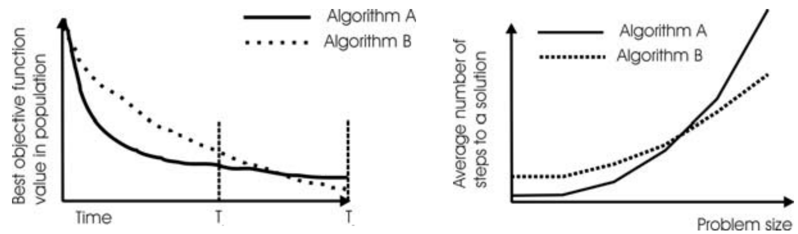
- How much amount to put into tuning (fixed budget, until stagnation)?
- Where shall we be on the spectrum when we compare?
- Can we compare spectra ($\Rightarrow$ adaptability)?

“Traditional” Measuring in EC

Simple Measures

- MBF: mean best fitness
- AES: average evaluations to solution
- SR: success rates, SR(t) $\Rightarrow$ run-length distributions (RLD)
- best-of-n: best fitness of *n* runs

But, even with all measures given: Which algorithm is better?



(figures provided by Gusz Eiben)

Aggregated Measures

Especially Useful for Restart Strategies

Success Performances:

- SP1 [HK04] for equal expected lengths of successful and unsuccessful runs $\mathbb{E}(T^s) = \mathbb{E}(T^{us})$:

$$SP1 = \frac{\mathbb{E}(T_A^s)}{p_s} \tag{2}$$

- SP2 [AH05] for different expected lengths, unsuccessful runs are stopped at FE_{max}:

$$SP2 = \frac{1-p_s}{p_s} FE_{max} + \mathbb{E}(T_A^s) \tag{3}$$

Probably still more aggregated measures needed (parameter tuning depends on the applied measure)

Choose the Appropriate Measure

- Design problem: Only best-of-n fitness values are of interest
- Recurring problem or problem class: Mean values hint to quality on a number of instances
- Cheap (scientific) evaluation functions: exploring limit behavior is tempting, but is not always related to real-world situations

In real-world optimization, 10^4 evaluations is a lot, sometimes only 10^3 or less is possible:

- We are relieved from choosing termination criteria
- Substitute models may help (Algorithm based validation)
- We encourage more research on short runs

Selecting a performance measure is a *very* important step

Current "State of the Art"

Around 40 years of empirical tradition in EC, but:

- No standard scheme for reporting experiments
- Instead: one ("Experiments") or two ("Experimental Setup" and "Results") sections in papers, providing a bunch of largely unordered information
- Affects readability and impairs reproducibility

Other sciences have more structured ways to report experiments, although usually not presented in full in papers. Why?

- Natural sciences: Long tradition, setup often relatively fast, experiment itself takes time
- Computer science: Short tradition, setup (implementation) takes time, experiment itself relatively fast

⇒ We suggest a 7-part reporting scheme

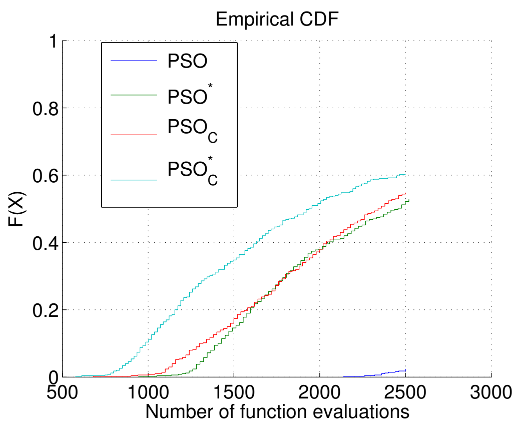
Suggested Report Structure

ER-1: **Focus/Title** the matter dealt with
ER-2: **Pre-experimental planning** first—possibly explorative—program runs, leading to task and setup
ER-3: **Task** main question and scientific and derived statistical hypotheses to test
ER-4: **Setup** problem and algorithm designs, sufficient to replicate an experiment
ER-5: **Experimentation/Visualization** raw or produced (filtered) data and basic visualizations
ER-6: **Observations** exceptions from the expected, or unusual patterns noticed, plus additional visualizations, no subjective assessment
ER-7: **Discussion** test results and necessarily subjective interpretations for data and especially observations

This scheme is well suited to report 12-step SPO experiments

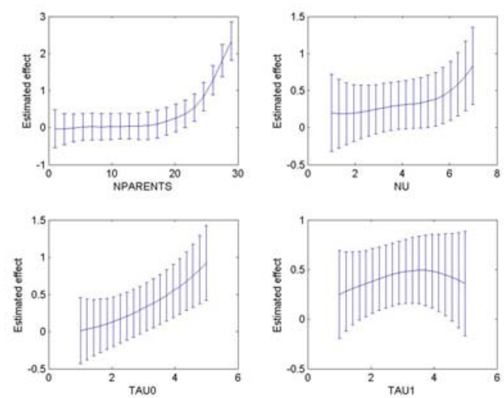
Objective Interpretation of the Results

Comparison. Run-length distribution



(Single) Effect Plots

Useful, but not Perfect

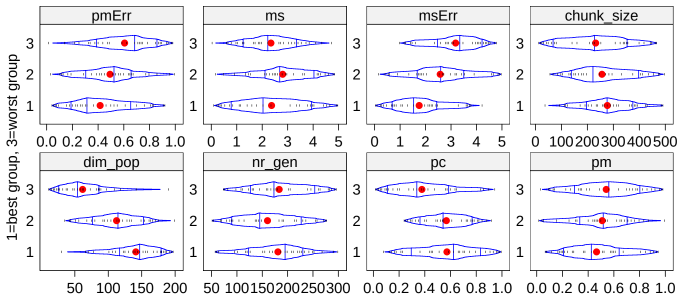


- Large variances originate from averaging
- The τ_0 and especially τ_1 plots show different behavior on extreme values (see error bars), probably distinct (averaged) effects/interactions

One-Parameter Effect Investigation

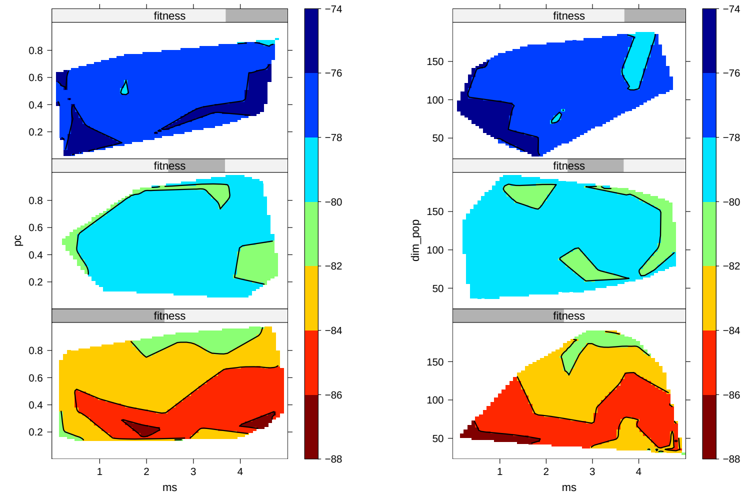
Effect Split Plots: Effect Strengths

- Sample set partitioned into 3 subsets (here of equal size)
- Enables detecting more important parameters visually
- Nonlinear progression 1–2–3 hints to interactions or multimodality



Two-Parameter Effect Investigation

Interaction Split Plots: Detect Leveled Effects



Updates

- Please check
  `http://ls11-www.cs.uni-dortmund.de/people/tom/ExperimentalResearchSlides.html`
  for updates, software, etc.

Discussion

- Standards for good experimental research
- Review process
- Research grants
- Meetings
- Building a community
- Teaching
- ...

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Performance Evaluation of an Advanced Local Search Evolutionary Algorithm.
In B. McKay et al., editors, *Proc. 2005 Congress on Evolutionary Computation (CEC'05)*, Piscataway NJ, 2005. IEEE Press.

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In X. Yao, H.-P. Schwefel, et al., editors, *Parallel Problem Solving from Nature – PPSN VIII, Proc. Eighth Int'l Conf., Birmingham*, pages 282–291, Berlin, 2004. Springer.

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The Design and Analysis of Computer Experiments.
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