CIG 2011 Tutorial:
August 31, 2011

Experimentation in CI-Affected Games Research

Mike Preuss

Technische Universität Dortmund
Computational Intelligence Group, Computer Science 11

Contents

CI in Games: What is this good for?

With respect to experimentation

last year's CIG (Alex Champandard):

- some researchers interested in industry problems, many not interested
- industry not too interested in us
- I would argue: normal situation, even worse in other academic fields
- Alex working on this at the Paris Game/AI conference
- we are not the slaves of industry (at least not all of us)
- which enables us to do some *fundamental research*

We believe

what do we believe?

but of course, our research shall *somehow* make sense:

- we are here because we believe it does
- we are not alone, e.g. AIIDE: different methods/approaches, but similar focus

back to Lucas/Kendall 2006 "Evolutionary Computation and Games" (IEEE Computational Intelligence Magazine):

1. good testbed to apply our methods
2. do things in a better way
3. do things we (or they) could not do before

Testbed

the testbed argument seems to loose importance:

- test problem collections (benchmarks) and competitions are getting popular in many fields
- not really simple to transfer back obtained knowledge (games research partly engineering)
- the need to *defend* games research is shrinking

Improvement

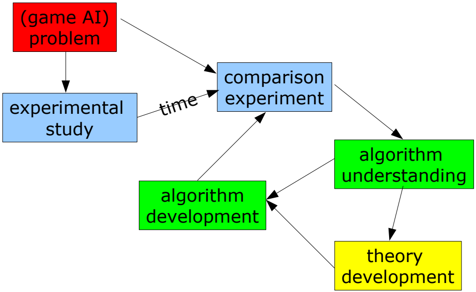
the doing things better argument is (still) important:

- can involve theory, but usually based on experimentation
- question: what does *better* mean?
- measurement sometimes fully automated, sometimes requires user interaction (no fun formula)
- required: being open to other methods (to achieve meaningful comparisons)
- ideal situation: competition as joint effort experiment (fair)

To boldly go...

we may encounter problems not solved or not even realized by others:

- interesting features of CI techniques: coping with noise, black-box approach, realtime ability, multiple objectives
- show that our approach indeed *does* fulfill some minimal requirements by experiment



Algorithm development and theory

- of course we can improve our *methods* while applying them
- but this is usually not restricted to games problems
- improvement/improved understanding may result in better theory
- discrete state games: algorithm engineering cycle applicable
- more complex games (e.g. RTS): theory connection very difficult
- solving games problems is to a large extent *engineering*
- *we have to rely on good experimentation in most cases*

What is an experiment?

we ask Wikipedia (not that this is always right):

> *An experiment is a method of testing - with the goal of explaining - the nature of reality. [...]*
> *More formally, an experiment is a methodical procedure carried out with the goal of verifying, falsifying, or establishing the accuracy of a hypothesis.*

important words: goal, reality, methodical procedure, hypothesis

Why do we need experimentation?

- practitioners need so solve problems, even if theory is not developed far enough
- counterargument of practitioners: Tried that once, didn't work (expertise needed to apply convincingly)
- we need to establish guidelines how to adapt the algorithms to practical problems
- helps theoreticians to find exploitable (problem/method) relations

experimental methodology is improving, leaving the phase of

a) funny but useless performance figures
b) lots of better and better algorithms that soon disappear again

Why do we need experimentation?

- practitioners need so solve problems, even if theory is not developed far enough
- counterargument of practitioners: Tried that once, didn't work (expertise needed to apply convincingly)
- we need to establish guidelines how to adapt the algorithms to practical problems
- helps theoreticians to find exploitable (problem/method) relations

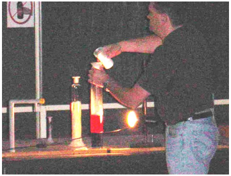
instead, we converge to

a) deliberate and justified choice of parameters, problems, performance criteria—much less arbitrariness
b) better generalizability (not quite resolved, but targetted)

Are we alone (with this problem)?

in natural sciences, experimentation is not in question

- many inventions (batteries, x-rays) made by experimentation, often unintentional
- experimentation leads to theory, theory has to be *useful* (can we do predictions?)



this is an experiment

different situation in computer science

- 2 widespread stereotypes influence our view of computer experiments:

a) programs do (exactly) what algorithms specify
b) computers (programs) are deterministic, so why statistics?

is this an experiment?

Lessons from other sciences

in economics, experimentation was established quite recently (compared to its age)

- modeling human behavior as rationality assumption (of former theories) had failed
- no accepted new model available: experimentation came in as substitute

nonlinear behavior

in (evolutionary) biology, experimentation and theory building both have problems

- active experimentation only possible in special cases, otherwise only observation
- mainly concepts (working principles) instead of theories: always exceptions

⇒ stochastical distributions, population thinking

Ernst Mayr

Experimentation at unexpected places

since ≈1960s: Experimental Archaeology

- gather (e.g. performance) data that is not available otherwise
- task: concept validation, fill conceptual holes

Viking bread baking (Lejre, Denmark)

experimentation in management of technology and product innovation

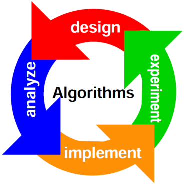
- product cycles are sped up by 'fail-fast', 'fail-often' experimentation
- what-if questions may be asked by using improved computational ressources
- innovation processes have to be tailored towards experimentation

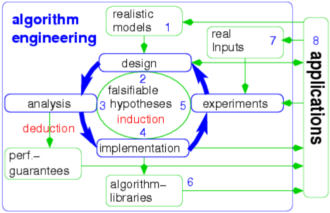
Stefan H. Thomke

Algorithm Engineering

How theoreticians handle it...(recently)

- Algorithm Engineering is *theory* + real data + concrete implementations + experiments
- principal reason for experiments: test validity of theoretical claims
- are there important factors in practice that did not go into theory?
- approach also makes sense for CI methods, but we start with no or little theory
- performance measuring usually no problem for us, but user interaction

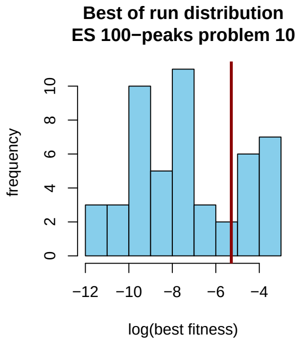




So what about statistics?

are the methods all there? some are, but:

- our data is usually not normal
- we can most often have lots of data
- this holds for algorithmics, also!
- these are not the conditions statisticians are used to
- in some situations, there is just no suitable test procedure



⇒ there is a need for more statistics and more statistical methods.

Catherine McGeogh:
our problems are unfortunately not sexy enough for the statisticians...

What experiments are all about

good experiments all have in common:

- fairness (even if we want to show that our method is better)
- openness, enable the system to come up with surprises
- defined goals
  - how is the winning method identified (comparison)
  - how is reaching minimal requirements defined (study)
- defined procedure (not ad-hoc during experiment)
- documentation (enables others to rebuild experiment)
- iteration (the first question is almost never very good)

Openness example: clutch control in TORCS

EvoStar 2011: Mr Racer bot (Jan Quadflieg, Tim Delbrügger, Mike Preuss) shows potential but has bad clutch control

- first approach similar to Autopia clutch control: speed based
- Autopia closes clutch at below 70 km/h
- we adapt closing (generalized logistic) *function* with a bit more freedom
- result: using the clutch until 180 km/h is profitable!
- we would be much worse with restriction to 70 km/h

(see the videos)

The hypothesis issue

we remember: *hypothesis* and *goals* important words in experiment definition

Cohens investigation of 1990 (all papers of the AAAI conference):

- almost no relationship between experiments and theory
- 60% testing only on one problem instance
- 80% did not explain the measured result in any way
- 16% provided a hypothesis or defined objectives

Paul R. Cohen

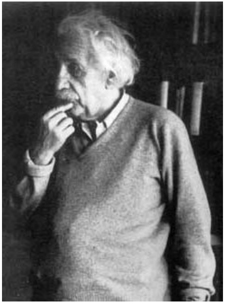
Research question

- not trivial $\Rightarrow$ many papers are not focused
- the (real) question is not: is my algorithm faster than others on a set of benchmark problems?
- final task is *reality*, not benchmarking

  horse racing: set up, run, comment...

explaining observations leads to new questions:

- explanations we give for a result can often also be tested experimentally
- range of validity shall be explored (problems, timing, parameters, etc.)



Einstein thinking

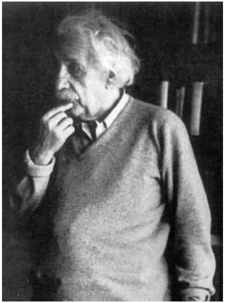
Research question

- not trivial $\Rightarrow$ many papers are not focused
- the (real) question is not: is my algorithm faster than others on a set of benchmark problems?
- final task is *reality*, not benchmarking

horse racing: set up, run, comment...**NO!**

explaining observations leads to new questions:

- explanations we give for a result can often also be tested experimentally
- range of validity shall be explored (problems, timing, parameters, etc.)



Einstein thinking

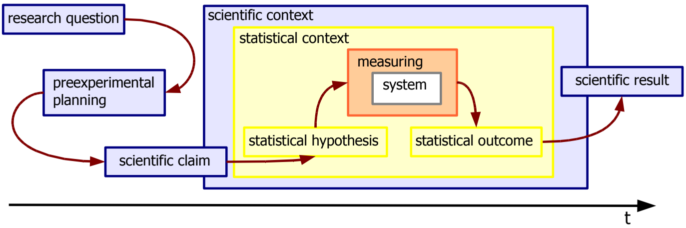
How to set up research questions?

- when comparing, always ask if a difference is meaningful in practice
- same for experimental studys: what quality level is required to make the approach useful?
- usually, we do not know the 'perfect question' from the start, this requires some experimentation...
- an inherent problem with experimentation is that we do (should) not know the outcome in advance
- but it may lead to new, better questions
- try small steps, expect the unexpected

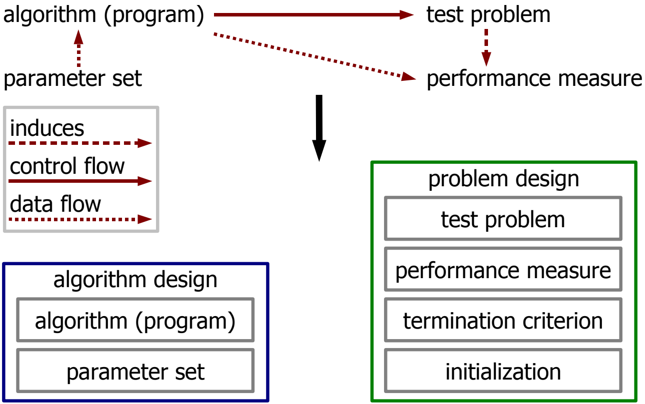
Sample flow chart for experiments

how do we obtain decision criteria from an initial *research question*?

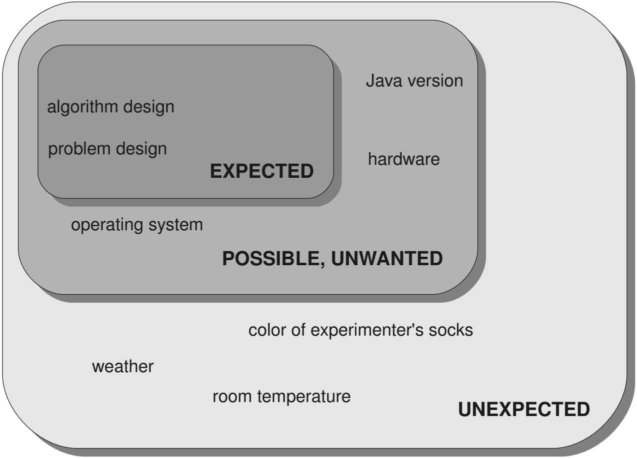
- set up scientific claims
- formulate as statistical hypotheses
- experiment, and then the same way back



Components of an (optimization type) experiment



Factors: overview

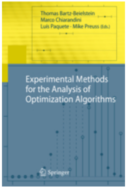


Tuning: when there are too many factors

- comparison of methods without suitable parameter settings is comparing unsuitable algorithms
- not looking at parameters often means to give away good performance
- tuning reveals parameter relevance and interactions

even if time is critical: small design with 10 points in parameter space already reveals a lot

Tuning: available methods



recent compilation of methods and more general considerations concerning experimental approaches

methods:

- SPO (Bartz-Beielstein): sequential model-based improvement
- F-Race (Birattari, Stützle): iterative bad parameter elimination
- REVAC (Nannen, Eiben, Smit): meta-EDA
- ParamILS (Stützle, Hoos, Hutter): iterative local search
- probably more to come, active research area...

Reporting and keeping track of experiments

around 40 years of experimental tradition in CI, but:

- no standard scheme for reporting experiments (experimental protocols)
- instead: one ("Experiments") or two ("Experimental Setup" and "Results") sections in papers, often providing a bunch of largely unordered information
- affects readability and impairs reproducibility

keeping experimental journals helps:

- record context and rough idea
- report each experiment
- running where (machine)
- finished when (date/time), link to result file(s)

$\Rightarrow$ we suggest a 7-part reporting scheme

Suggested report structure

- ER-1: **research question** 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: **results/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** stat-test results and necessarily subjective interpretations for data and especially observations

Statistical testing

- many papers now employ statistical testing
- but 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

Statistical testing

- many papers now employ statistical testing
- but 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. **No!**

Statistical testing

- many papers now employ statistical testing
- but we claim: fundamental ideas from statistics are misunderstood!
- for example: what is the p value?

Definition (p value)

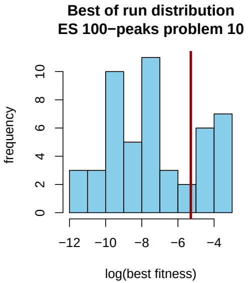
the p value is
$p = P\{$ obtain observed result, or greater $\mid$ null model is true $\}$

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

To test or not to test?

yes, but:

- we often have non-normal data
  $\Rightarrow$ non-parametric tests, permutation tests
- temptation to "make" tests valid by enlarging sample (not always helpful, e.g. if distribution bimodal)
  $\Rightarrow$ rule-of-thumb fixed size (e.g. 30)



Wilcoxon rank sum test

- also Mann-Whitney U-test or just U-test (equivalent)
- more robust than t-test, becoming standard test in Evolutionary Computation
- basic assumption: distribution functions G and F of X and Y only differ by a shift a, $G(x) = F(x - a)$
- this also means homogeneity of variances (may require F-test)!
- null hypothesis: $H_0 : a = 0,\ H_1 : a \neq 0$
- R-command:

```
wilcox.test(x, y, alternative = "two.sided",
conf.level = 0.95)
```

A simple example

(`rexp()` gives random numbers from an exponential distribution)

> N=10

> X=rexp(N)

> X
[1] 0.51762849 1.20825633 3.23399265 1.80257160 0.85732474
0.24931676 0.48776898 0.81129961 0.70829536 0.02036845

> Y=rexp(N)+0.2

> Y
[1] 0.457792 2.224912 1.095469 1.224541 5.392600 2.577539
2.334396 1.235689 7.564990 1.420925

> wilcox.test(X,Y)

Wilcoxon rank sum test

data: X and Y
W = 20, p-value = 0.02323
alternative hypothesis: true location shift is not equal to 0

Simple example part 2

the same with a t-test:

```
> t.test(X,Y)

Welch Two Sample t-test

data: X and Y
t = -2.0473, df = 12.066, p-value = 0.06305
alternative hypothesis: true difference in means is not equal to 0
95 percent confidence interval:
-3.22584888 0.09944275
sample estimates:
mean of x mean of y
0.9896823 2.5528854
```

But: much less difference in test results if distributions are a bit more normal

Earth is round ($p < 0.05$)

- paper of Jacob Cohen (in American Psychologist, 1994)
- summarizes criticism on 'unreflected' use of statistical testing
- be careful with small samples!
- first understand and improve data (EDA, Exploratory Data Analysis, after Tukey), then testing
- actually, one should test the other way around:
  postulate null hpyothesis and try to falsify it (very time-consuming procedure)
- providing confidence intervals gives important information!
- importance of reproducing a result

Correlations and correlation tests

- correlation coefficient: measure for (linear) relation of two measurements of same sample, between $+1$ (ideally correlated) and -1 (anti-correlated)
- example (work with Jan Quadflieg and Günter Rudolph): TORCS, 2-round times for 2 tracks in sec., different 'AI-drivers'
- question: are good drivers on track 1 also good on track 2?

in `R` (data in nd):

```
cor(nd$track1,nd$track2)
```

`>` $[1]$ `-0.075339`

looks uncorrelated. really?

ID	track1	track2
1137	441.4660	362.246
1069	438.6060	363.466
1059	437.8260	361.886
1027	438.3060	362.166
1162	439.7460	361.746

etc, 75 drivers

Different correlations

- default method is Pearson correlation (uses actual values)
- but: we assume *linear* relation of the measurements!
- alternatives: rank correlations (non-parametric)
- Spearman's rank correlation coefficient
  (as Pearson correlation, but using ranks)
- Kendall's tau (τ): uses no rank differences but only relative positions (less sensitive to outliers)

Correlated or not?

applying non-parametric correlation measures results in:

- > cor(nd$track1,nd$track2,method="spearman")
- > [1] -0.6390533
- > cor(nd$track1,nd$track2,method="kendall")
- > [1] -0.4619204

looks like a strong correlation
(rule of thumb from psychology: $|cor| > 0.4$ is strong)

good drivers of track 1 usually perform worse on track 2 and vice versa

Correlation test

same data as before, just `cor.test` instead of `cor`:

> cor.test(nd$track1,nd$track2,method="kendall")
> Kendall's rank correlation tau
> data: nd$track1 and nd$track2
> z = -5.6756, p-value = 1.382e-08
> alternative hypothesis: true tau is not equal to 0
> sample estimates:
> tau
> -0.4619204

Floor and ceiling effects

- floor effect: compared methods attain set task very rarely
  $\Rightarrow$ problem is too hard
- ceiling effect: methods nearly always reach given task
  $\Rightarrow$ problem is too easy

if problem is too hard or too easy, nothing is shown.

- pre-experimentation is necessary to obtain reasonable tasks
- if task is reasonable (e.g. practical requirements), then algorithms are unsuitable (floor) or all good enough (ceiling), statistical testing does not provide more information
- arguing on minimal differences is statistically unsupported and scientifically meaningless

Confounded effects

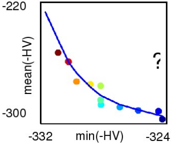
two or more effects or helper algorithms are merged into a new technique, which is improved

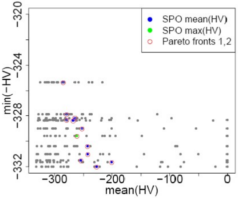
- where does the improvement come from?
- it is necessary to test both single effects/algorithms, too
- either the combination helps, or only one of them
- knowing that is useful for other researchers!

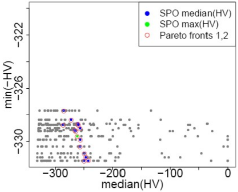
complex machinery

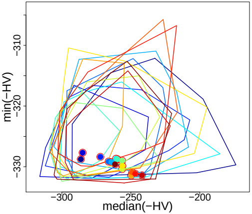
Underestimated randomness

- idea: find pareto front of two tuning criteria
- parameter changes not interpretable
- validation failed
- reason: deviations much too high!









more difficulties: see also papers of the GECCO'09 workshop *Learning from Failures in Evolutionary Computation (LFFEC)*

There is a problem with the experiment

after all data is in, we realize that something was wrong (code, parameters, environment?), what to do?

- current approach: either do not mention it, or redo everything
- if redoing is easy, nothing is lost
- if it is not, we must either:
  - let people know about it, explaining why it probably does not change results
  - or do validation on a smaller subset: how large is the difference (e.g. statistically significant)?
- do not worry, this situation is rather normal
- *Thomke*: there is nearly always a problem with an experiment
- early experimentation reduces the danger of something going completely wrong

What are the objectives?

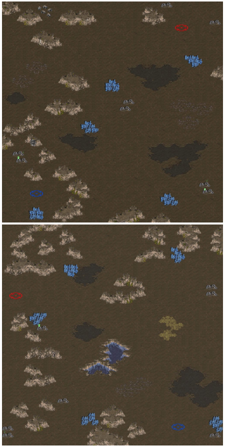
difficult to say, at some point user is involved
(fun: Georgios' and Julian's problem)

some approaches:

- completely interactive (user takes all decisions), usually preference based
- model-based, model is learned from user data and used for decisions
- some mix of the two (partially interactive)
- interesting: EvoMusart people have same problem (automated assessment of aesthetic criteria)

Or bottom up: make up many objectives

example: multiobjective StarCraft map-making
(Togelius, Preuss, Beume, Wessing, Hagelbäck, Yannakakis)

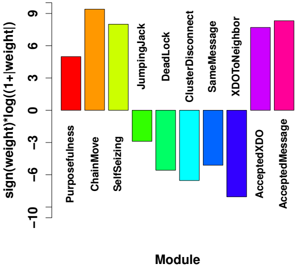


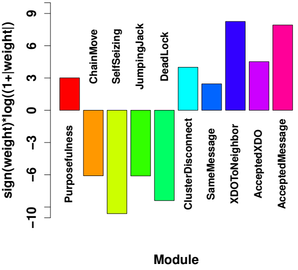
- 8 objectives related to base location, ressource fairness, path properties (e.g. choke points)
- unclear which objectives make sense
- but single objectives can be discussed with users
- users may be wrong (e.g. one *can* make fair *and* asymmetric maps)
- tool that helps exploring: multi-objective optimization

If fun is individual, so is believability

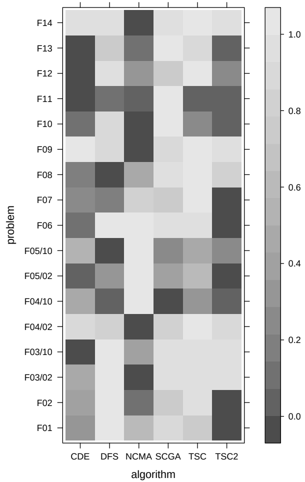
(from joint work with Markus Kemmerling and Niels Ackermann)

- indirect learning of player believability preferences in Diplomacy bots
- several test games per player, submitted believability ranking of 6 other players (bots)
- find best matching between rankings and measured features (minimizing rank errors by adjusting weights)





Diagrams instead of tables



Method	Peak ratio		Basin ratio		Peak accuracy		Distance accuracy	
	Best	Avg.	Best	Avg.	Best	Avg.	Best	Avg.
***F*1, 1 global optimum, 9 local ones**								
TSC2	**0.99**	**0.84**	1	0.88	**0.13**	1.84	**0.04**	0.79
CDE	0.88	0.79	0.98	**0.93**	0.52	**1.59**	0.11	**0.41**
TSC [14]	0.85	0.64	0.83	0.66	4.52	7.7	1.29	3.26
NCMA-ES	0.8	0.49	0.9	0.59	1.85	8.89	0.88	3.87
SCGA	0.66	0.18	0.99	0.264	8.74	18.59	0.98	11.56
DFS	0.37	0.16	0.37	0.16	14.46	20.93	5.24	11.52
***F*2, 2 global, 4 local optima**								
TSC2	1	**0.77**	1	**0.77**	**6.93e-04**	**2.91**	0.02	2.09
NCMA-ES	1	0.59	1	0.61	1.72e-03	3.9	0.02	3.19
CDE	1	0.75	1	0.76	0.02	3.3	0.1	**1.99**
SCGA	0.96	0.32	1	0.35	0.39	6.37	0.44	7.02
DFS	0.67	0.26	0.67	0.26	4.64	7.27	2.73	6.22
TSC [14]	0.63	0.46	0.66	0.44	3.93	6.18	3.44	6.18
***F*3, 2 dimensions, 1 optimum**								
NCMA-ES	1	1	1	1	**4.6e-68**	3.92e-6	**6.48e-35**	5.84e-4
CDE	1	1	1	1	9.47e-40	4.48e-04	1.96e-20	5.25e-03
TSC2	1	1	1	1	5.85e-12	1.81e-07	1.61e-06	9.32e-05
SCGA	1	1	1	1	1.53e-11	2.86e-07	2.41e-06	1.65e-04
TSC [14]	1	1	1	1	2.48e-10	**1.75e-07**	4.9e-06	**9.08e-05**
DFS	1	1	1	1	2.55e-09	4.17e-06	4.23e-05	8.12e-04
***F*3, 10 dimensions, 1 optimum**								
CDE	1	**0.83**	1	1	**2.66e-25**	0.11	**4.07e-13**	**0.15**
NCMA-ES	1	0.73	1	1	1.28e-17	**0.08**	2.51e-09	0.19
TSC2	1	0.73	1	1	2.36e-06	0.15	0.001	0.23
TSC [14]	1	0.74	1	1	2.79e-06	0.12	0.003	0.51
SCGA	1	0.72	1	1	1.03e-05	1.43	0.003	0.45
DFS	1	0.72	1	1	3.12e-05	0.14	0.005	0.22
***F*4, 2 dimensions, 1 global optimum/ many local ones**								
NCMA-ES	1	0.86	1	0.88	**0**	0.19	**9.05e-9**	0.14
DFS	1	0.98	1	0.98	9.13e-08	0.02	7.24e-06	0.02
SCGA	1	**0.99**	1	**0.99**	1.4e-07	**0.01**	1.46e-05	**0.01**
CDE	1	0.88	1	0.98	4.29e-07	0.11	3.93e-05	0.03
TSC2	1	0.8	1	0.94	2.23e-06	1.63	8.23e-05	0.05
TSC [14]	1	0.74	1	0.93	5.04e-05	1.73	5.1e-04	0.07
***F*4, 10 dimensions, 1 global optimum/ many local ones**								
SCGA	1	**0.35**	1	**0.66**	0.002	18.42	0.003	1.71
TSC2	1	0.04	1	0.27	0.002	39.78	0.003	2.57
DFS	1	0.31	1	0.44	0.003	**8.93**	0.003	**1.44**
TSC [14]	0.97	0.03	1	0.28	0.03	51.46	0.03	6.08
CDE	0.9	0.12	0.97	0.19	0.09	18.68	0.04	1.68
NCMA-ES	0	0	0	0	26.9	23.6	2.46	3.32
***F*5, 2 dimensions, 1 global optimum/ many local ones**								
TSC2	**0.77**	**0.26**	**0.97**	**0.67**	14.74	369.93	**9.4e-04**	0.49
DFS	0.7	0.21	0.73	0.24	58.09	164.85	1.34	3.05
TSC [14]	0.63	0.19	0.73	0.29	273.64	934.45	0.96	1.07
SCGA	0.47	0.21	0.6	0.31	81.47	317.24	0.11	2.51
CDE	0	0.003	1	0.96	**20.65**	**134.64**	0.01	**0.07**
NCMA-ES	0	0	0	0	1700	1840	1.62	0.71
***F*5, 10 dimensions, 1 global optimum/ many local ones**								
NCMA-ES	0	0	0	0.01	1810	1900	**9.44**	**8.82**
TSC2	0	0	0	0	770.48	1234.14	9.64	11.24
DFS	0	0	0	0	**569.6**	**870.64**	11.08	12.85
CDE	0	0	0	0	1076.2	1301.02	11.99	9.32
SCGA	0	0	0	**0.3**	762.8	1311.85	12.95	11.49
TSC [14]	0	0	0	0	961.59	1151.36	33.33	32.37

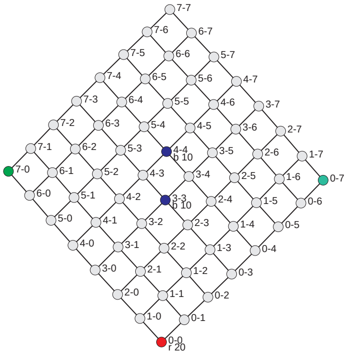
Visualizing high-dimensional data: MDS

- for more than 3D, we need a data reduction
- but we want to recognize the inner relation in the data
- one possibility: multi-dimensional scaling (MDS)
- uses only a distance matrix of the data
- more or less equivalent to principal component analysis (PCA)
- applying clustering to the results usually makes sense

(following example from joint work with Phil Hingston)

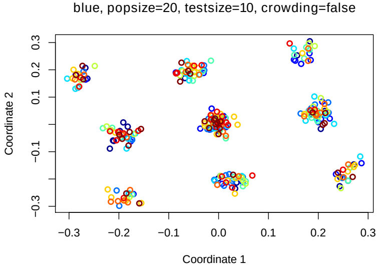
MDS example: Red Teaming strategies

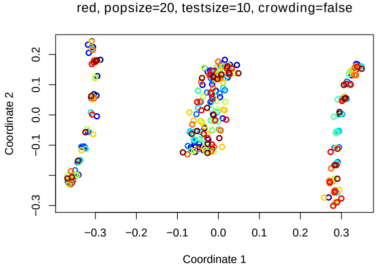
- Red Teaming: detect attack (red) strategies that prevail against an existing defense (blue) strategy
- RedTNet: simple (in this case grid) node structure, 2 teams (red, blue) with several agents
- Moves simultaneous (to neighbor node), majority in a node kills minority
- green nodes need to be conquered to win game
- strategies coded as node sequences, optimized via co-evolution



MDS visualization in R

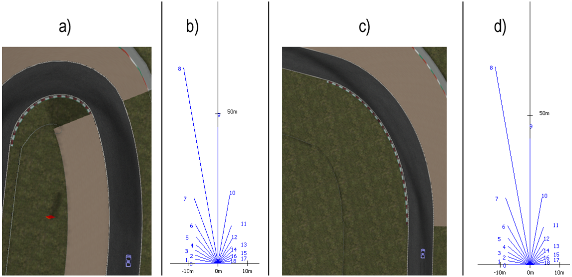
- distance matrices generated for red and blue final populations (in this case similar to edit distance)
- produce MDS in R (for 2D):
  `fit <- cmdscale((distmatrix), k=2)`
- plotting: `plot(fit$points[,1], fit$points[,2])`





Modeling away misleading sensor information

(joint work with Jan Quadflieg and Simon Wessing)

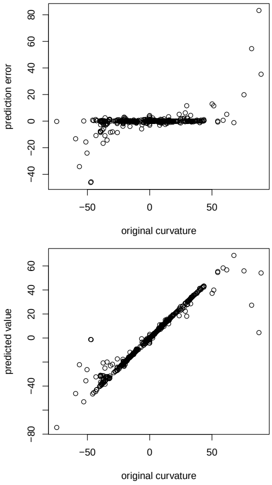


a) to d): approaching a hairpin corner (left), and a full speed corner (right), with sensory input

Jan developed a geometric method to extract a curvature value

but in 2010, competition organisers added noise (10%) to the sensor values, which broke our method

Kriging model of sensory data

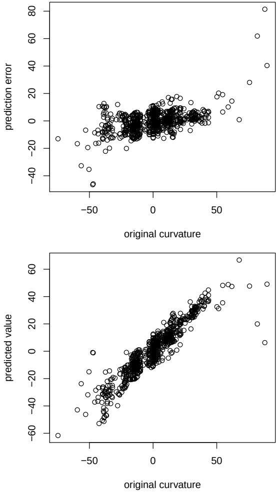


- 19 (noisy) 'forward' sensors between 0 and 200m
- data sampled every 20ms, some accumulation (up to 10 values) tolerable $\Rightarrow$ Noise drops to $\approx$ 3 %
- longer delay not tolerable, we need to act!

preliminary results:

- model input randomly drawn from one round $\approx$ 20,000 data
- less than 400 input points not sufficient for model that copes with 'some' noise
- we use the *DiceKriging* (R) package
- modeling the non-noisy data works well

And now with noise. . .



- validation with 1000 randomly selected of the 20,000 points
- works incredibly well for a naive approach (no expert knowledge)
- Kriging interesting tool for obtaining a good model quickly
- further improvement simple:
  pre-selection of learning data

Car setup optimization problem modeled

22 normalized real values for tuning car:

- gear ratios
- angles of front/rear wing
- brake system
- anti-roll bars
- wheels
- suspension (springs, ride height)

competition setup allows for 500 samples (with 2000 tics) max

Some methods compared

(joint work with David Ginsbourger and Tobias Wagner)

we distribute 500 points in 22D via Latin Hypercube Design (LHD), results sorted according to root mean squared error (RMSE), measured on (accurate) validation set:

method	repeats	RMSE	RMSEstd
kriging-gauss	10	0.0936	0.0000
gam	1	0.0976	0.0000
randomForest	10	0.1069	0.0008
rpart	1	0.1140	0.0000
svm-radial	1	0.1515	0.0000
lm	1	0.1699	0.0000
svm-linear	1	0.1718	0.0000

most interesting methods: kriging, gam, randomForest, rpart ok

Summary

- being aware of utilized experimental methodology is important
- more exploration possible (openness): let the system decide
- experiments are not perfect, iterate and improve
- statistics helpful, better do non-parametric
- modeling works even for 20D, but special tools needed for preference relation data
- visualization incredibly important