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The Dos and Don'ts of Benchmarking

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… or how not to lie with benchmarks

Benchmarking in Research

- Generally one of two objectives:
  - Show new approach *improves* performance
  - Show otherwise attractive approach *does not undermine* performance

- Requirement: objectivity/fairness
  - Selection of baseline
  - Inclusion of relevant alternatives
  - Fair evaluation of alternatives

- Requirement: analysis/explanation of results
  - Model of system, incorporating relevant parameters
  - Hypothesis of behaviour
  - Results must support hypothesis

Lies, Damned Lies, Benchmarks

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- Micro- vs macro-benchmarks
- Standard vs ad-hoc
- Benchmark suites, use of subsets
- Completeness of results
- Significance of results
- Baseline for comparison
- Benchmarking ethics
- What is good — analysing the results

Micro- vs Macro-Benchmarks

- Macro-benchmarks
  - Use realistic workloads
  - Measure real-life system performance (hopefully)
- Micro-benchmarks
  - Exercise particular operation, e.g. single system call
  - Good for analysing performance / narrowing down down performance bottlenecks
    - critical operation is slower than expected
    - critical operation performed more frequently than expected
    - operation is unexpectedly critical (because it's too slow)

Micro- vs Macro-Benchmarks

Benchmarking Crime: Micro-benchmarks only

- Pretend micro-benchmarks represent overall system performance

- Real performance can generally not be assessed with micro-benchmarks
- Exceptions:
  - Focus is on improving particular operation known to be critical
  - There is an established base line

Note: My macro-benchmark is your micro-benchmark

- Depends on the level on which you are operating
- Eg: lmbench
  - … is a Linux micro-benchmark suite
  - … is a hypervsior macro-benchmark

Synthetic vs "Real-world" Benchmarks

- Real-world benchmarks:
  - real code taken from real problems
    - Livermore loops, SPEC, EEMBC, …
  - execution traces taken from real problems
  - distributions taken from real use
    - file sizes, network packet arrivals and sizes
  - Caution: representative for one scenario doesn't mean for *every* scenario!
    - may not provide complete coverage of relevant data space
    - may be biased
- Synthetic benchmarks
  - created to simulate certain scenarios
  - tend to use random data, or extreme data
  - may represent unrealistic workloads
  - may stress or omit pathological cases

Standard vs Ad-Hoc Benchmarks

Why use ad-hoc benchmarks?

- There may not be a suitable standard benchmark
  - Example: lack of standardised multi-tasking workloads
- Cannot run standard benchmarks
  - Limitations of experimental system

Why not use ad-hoc benchmarks?

- Not comparable to other work (unless they use the same)
- Poor reproducibility

Facit: Only use ad-hoc benchmarks if you have no other choice

- Justify well what you're doing

Benchmark Suites

- Widely used (and abused!)
- Collection of individual benchmarks, aiming to cover all of relevant data space
- Examples: SPEC CPU{92|95|2000|2006}
  - Originally aimed at evaluating processor performance
  - Heavily used by computer architects
  - Widely (ab)used for other purposes
  - Integer and floating-point suite
  - Some short, some long-running
  - Range of behaviours from memory-intensive to CPU-intensive
    - behaviour changes over time, as memory systems change
    - need to keep increasing working sets to ensure significant memory loads

Obtaining an Overall Score for a BM Suite

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- How can we get a single figure of merit for the whole suite?
- Example: comparing 3 systems on suite of 2 BMs

Normalise to System Y

Normalise to System X

Geometric mean?

Benchmark	System X		System Y		System Z	
	Abs	Rel	Abs	Rel	Abs	Rel
1	20	2.00	10	1.00	40	4.00
2	40	0.50	80	1.00	20	0.25
Geom. mean		**1.00**		**1.00**		**1.00**

Invariant under normalisation!

...e sense?

Arithmetic mean is meaningless for relative numbers

Rule: *arithmetic* mean for *raw* numbers,
geometric mean for *normalised!* [Fleming & Wallace, '86]

Benchmark Suite Abuse

Benchmarking Crime: Select subset of suite

- Introduces bias
  - Point of suite is to cover a range of behaviour
  - Be wary of “typical results”, “representative subset”
- Sometimes unavoidable
  - some don't build on non-standard system or fail at run time
  - some may be too big for a particular system
    - eg, don't have file system and run from RAM disk...
- Treat with extreme care!
  - can only draw limited conclusion from results
  - cannot compare with (complete) published results
  - need to provide convincing explanation why only subset

Other SPEC crimes include use for multiprocessor scalability
  - run multiple SPECs on different CPUs
  - what does this prove?

Partial Data

- Frequently seen in I/O benchmarks:
  - Throughput is degraded by 10%
    - "Our super-reliable stack only adds 10% overhead"

Almost certainly not true!

  - Why is throughput degraded?
    - latency too high
    - CPU saturated?
  - Also, changes to drivers or I/O subsystem may affect scheduling
    - interrupt coalescence: do more with fewer interrupts
  - *Throughput on its own is useless!*

Throughput Degradation

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- Scenario: Network driver or protocol stack
  - New driver reduces throughput by 10% — why?
  - Compare:
    - 100 Mb/s, 100% CPU vs 90 Mb/s, 100% CPU
    - 100 Mb/s, 20% CPU vs 90 Mb/s, 40% CPU
  - Correct figure of merit is *processing cost per unit of data*
    - Proportional to *CPU load divided by throughput*
  - Correct overhead calculation:
    - 10 µs/kb vs 11 µs/kb: *10% overhead*
    - 2 µs/kb vs 4.4 µs/kb: *120% overhead*

CPU limited

Latency limited

Benchmarking crime: Show throughput degradation only

- … and pretend this represents total overhead

Significance of Measurements

All measurements are subject to random errors

- Standard scientific approach: Many iterations, *collect statistics*
- Rarely done in systems work — why?
- Computer systems tend to be *highly deterministic*
  - Repeated measurements often give identical results
  - Main exception are experiments involving WANs
- However, it is dangerous to rely on this without checking!
  - Sometimes “random” fluctuations indicate *hidden parameters*

Benchmarking crime: results with no indication of significance

Non-criminal approach:

- Show at least standard deviation of your measurements
- … or state explicitly it was below a certain value throughout
- Admit results are insignificant unless well-separated std deviations

How to Measure and Compare Performance

Bare-minimum statistics:

- At minimum report the mean (μ) and standard deviation (σ)
  - Don't believe any effect that is less than a standard deviation
    - 10.2±1.5 is not significantly different from 11.5
  - Be highly suspicious if it is less than two standard deviations
    - 10.2±0.8 may not be different from 11.5
- Be *very suspicious* if reproducibility is poor (i.e. σ is *not* small)
  Distrust standard deviations of small iteration counts
  - standard deviations are meaningless for small number of runs
  - … but ok if effect ≫ σ
  - The proper way to check significance of differences is Student's t-test!

How to Measure and Compare Performance

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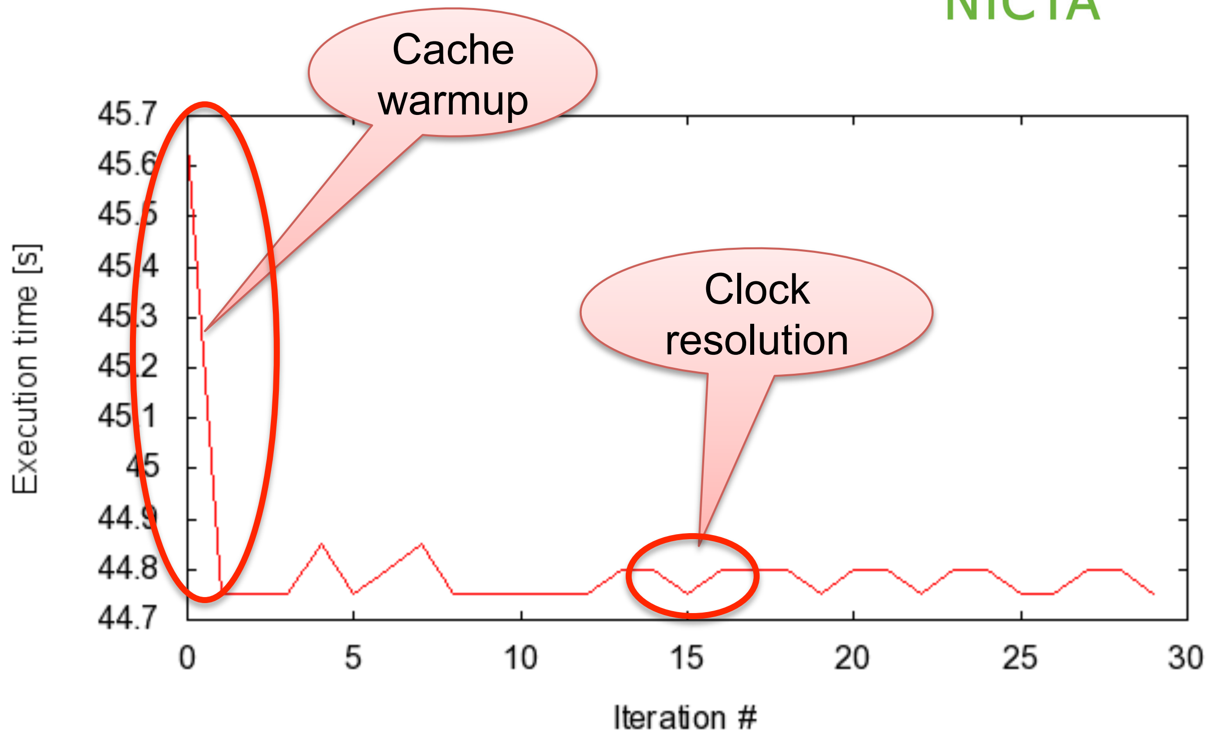
Obtaining meaningful execution times:

- Make sure execution times are long enough
  - What is the granularity of your time measurements?
  - make sure the effect you're looking for is much bigger
  - many repetitions won't help if your effect is dominated by clock resolution
  - do many repetitions in a tight loop if necessary

Example: gzip from SPEC CPU2000

Observations?

- First iteration is special
- 20 Hz clock
  - will not be able to observe any effects that account for less than 0.1 sec



Lesson?

- Need a mental model of the system
  - Here: repeated runs should give the same result
- Find reason (hidden parameters) if results do not comply!

How to Measure and Compare Performance

Noisy data:

- sometimes it isn't feasible to get a “clean” system
  - e.g. running apps on a “standard configuration”
  - this can lead to very noisy results, large standard deviations

Possible ways out:

- ignoring lowest and highest result
- taking the floor of results
  - makes only sense if you're looking for minimum
    - but beware of difference-taking!

Both of these are dangerous, use with great care!

- Only if you know what you are doing
  - need to give a convincing explanation of why this is justified
- Only if you explicitly state what you've done in your paper/report

Real-World Example

Benchmark:

- `300.twolf` from SPEC CPU2000 suite

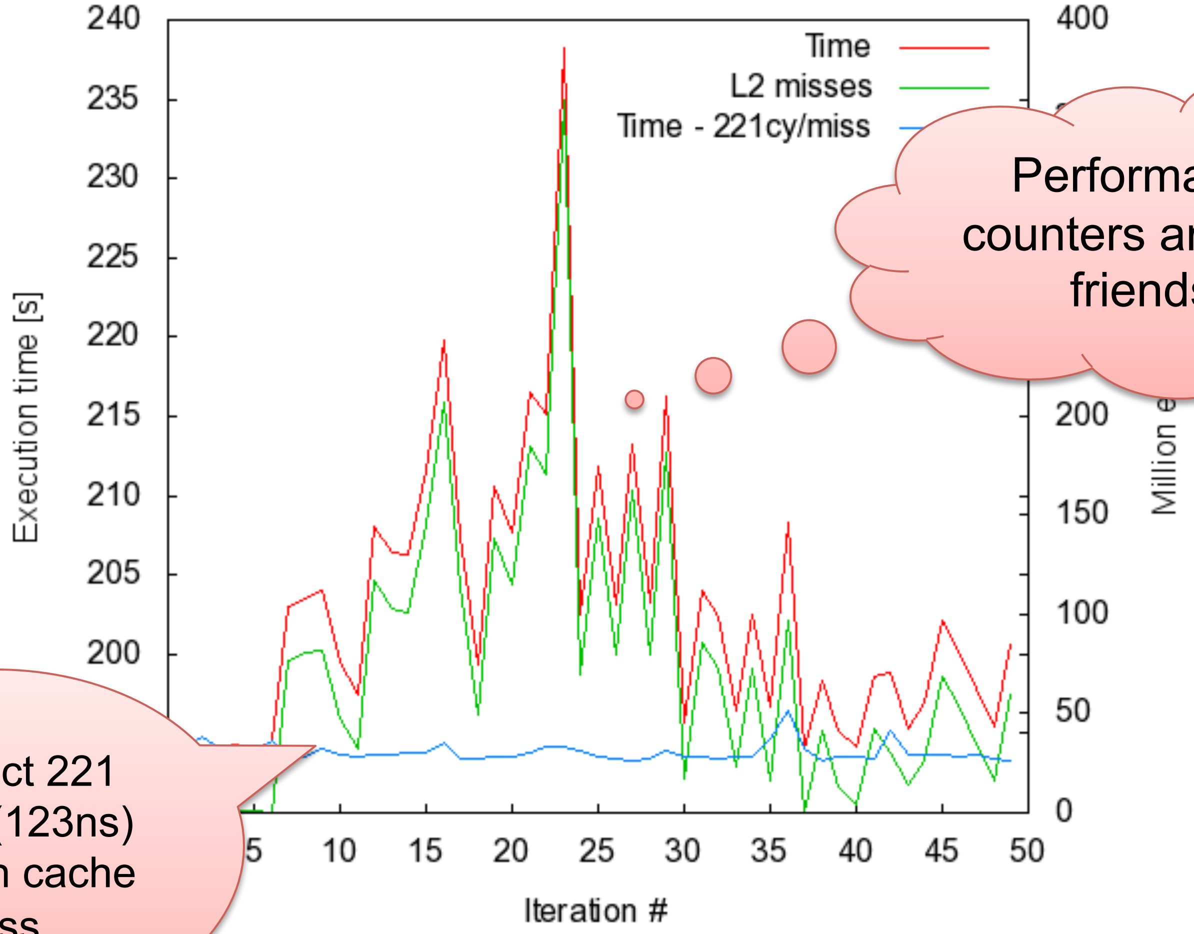
Platform:

- Dell Latitude D600
  - Pentium M @ 1.8GHz
  - 32KiB L1 cache, 8-way
  - 1MiB L2 cache, 8-way
  - DDR memory @ effective 266MHz
- Linux kernel version 2.6.24

Methodology:

- Multiple identical runs for statistics...

`twolf` on Linux: What's going on?



Performance counters are your friends!

Subtract 221 cycles (123ns) for each cache miss

`twolf` on Linux: Lessons?

- Pointer to problem was standard deviation
  - σ for "twolf" was much higher than normal for SPEC programs
- Standard deviation did not conform to mental model
  - Shows the value of verifying that model holds
  - Correcting model improved results dramatically
- Shows danger of assuming reproducibility without checking!

Conclusion: *Always* collect and analyse standard deviations!

How to Measure and Compare Performance

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Avoid incorrect conclusions from pathological cases

- Typical cases:
  - sequential access optimised by underlying hardware/disk controller...
  - potentially massive differences between sequentially up/down
    - pre-fetching by processor, disk cache
  - random access may be an unrealistic scenario that destroys performance
    - for file systems
  - powers of two may be particularly good or particularly bad for strides
    - often good for cache utilisation
      - minimise number of cache lines used
    - often bad for cache utilisation
      - maximise cache conflicts
  - similarly just-off powers (2^n-1, 2^n+1)
- What is "pathological" depends a **lot** on what you're measuring
  - e.g. caching in underlying hardware

How to Measure and Compare Performance

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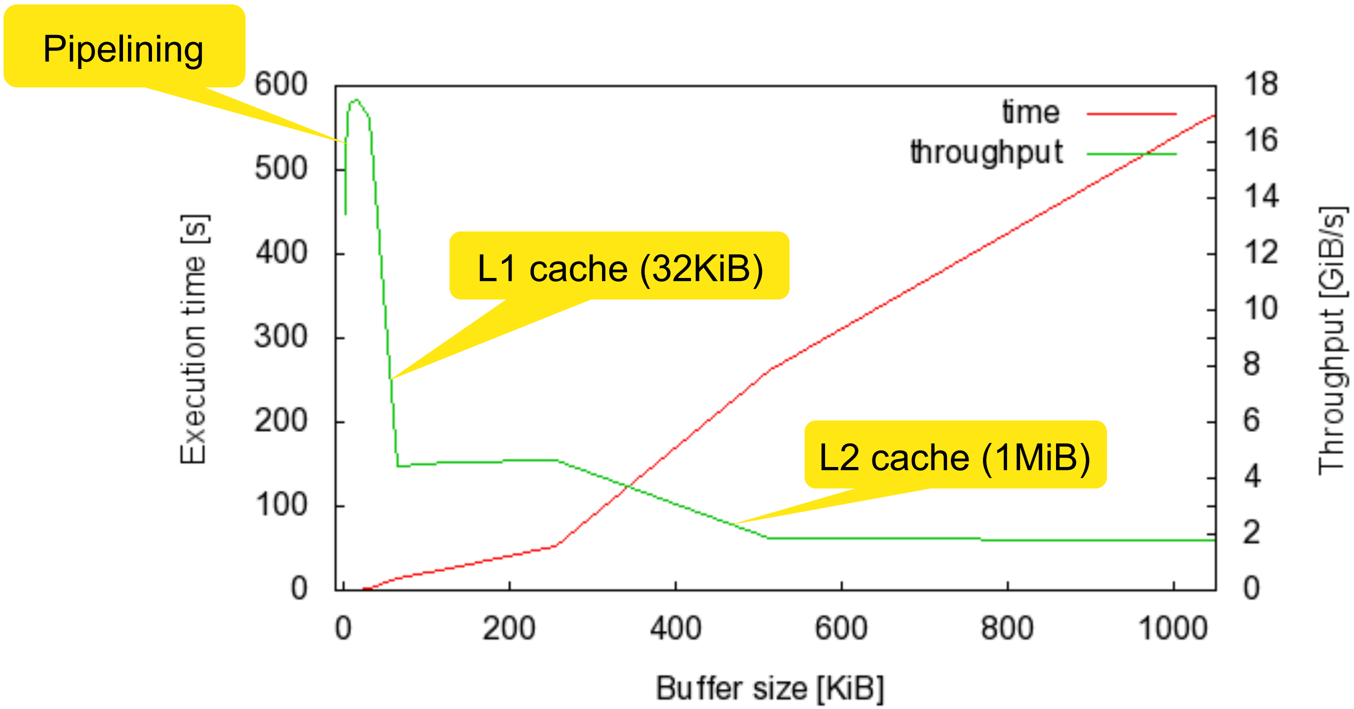
Use a model

- You need a (mental or explicit) model of the behaviour of your system
  - benchmarking should aim to support or disprove that model
  - need to think about this in selecting data, evaluating results
  - eg: I/O performance dependent on FS layout, caching in controller...
  - cache sizes (HW & SW caches)
  - buffer sizes vs cache size
- Should tell you the size of what to expect
  - you should understand that a 2ns cache miss penalty can't be right

Example: Memory Copy



How to Measure and Compare Performance

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Understand your results!

- Results you don't understand will almost certainly hide a problem
  - Never publish results you don't understand
    - chances are the reviewers understand them, and will reject the paper
    - maybe worse: someone at the conference does it
      - this will make you look like an idiot

Of course, if this happens you ***are*** an idiot!

Relative vs Absolute Data

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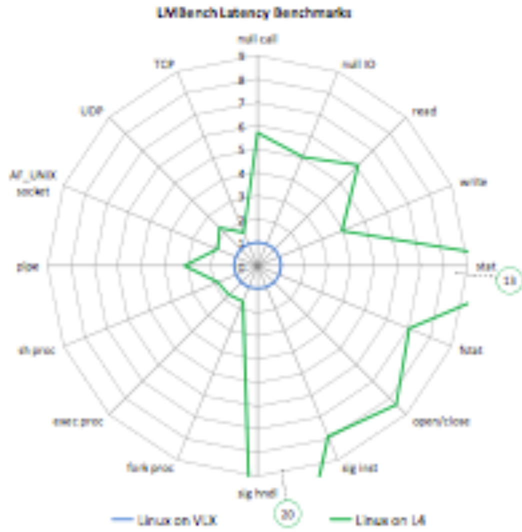
From a real paper (IEEE CCNC'09):

- No data other than this figure
- No figure caption
- Only explanation in text:
  - "The L4 overhead compared to VLX ranges from a 2x to 20x factor depending on the Linux system call benchmark"
- No definition of "overhead factor"
- No native Linux data



Benchmarking crime: Relative numbers only

- Makes it impossible to check whether results make sense
- How hard did they try to get the competitor system to perform?
  - Eg, did they run it with default build parameters (debugging enabled)?

Benchmarking Ethics

- Do compare with published competitor data, but…
  - Ensure comparable setup
    - Same hardware (or *convincing* argument why it doesn't matter)
  - you may be looking at an aspect the competitor didn't focus on
    - eg: they designed for large NUMA, you optimise for embedded
- Be ultra-careful when benchmarking competitor's system yourself
  - Are you sure you're running the competitor system optimally?
    - you could have the system mis-configured (eg debugging enabled)
    - Do your results match their (published or else) data?
  - Make sure you understand exactly what is going on!
    - Eg use profiling/tracing to understand source of difference
    - Explain it!

Benchmarking crime: Unethical benchmarking of competitor

- Lack of care is unethical too!

What Is "Good"?

- Easy if there are established and published benchmarks
  - Eg your improved algorithm beats best published Linux data by *x*%
  - But are you sure that it doesn't lead to worse performance elsewhere?
    - important to run complete benchmark suites
    - think of everything that could be adversely effected, and *measure*!
- Tricky if no published standard
  - Can run competitor/incumbent
    - eg run lmbench, kernel compile etc on your modified Linux and standard Linux
    - but be *very careful* to avoid running the competitor sub-optimally!
  - Establish performance limits
    - ie compare against optimal scenario
    - micro-benchmarks or profiling can be highly valuable here!

Real-World Example: Virtualization Overhead

Good or bad?

- Symbian null-syscall microbenchmark:
  - native: 0.24μs, virtualized (on OKL4): 0.79μs
  - 230% overhead
- ARM11 processor runs at 368 MHz:
  - Native: 0.24μs = 93 cy
  - Virtualized: 0.79μs = 292 cy
  - Overhead: 0.55μs = 199 cy
  - Cache-miss penalty ≈ 20 cy
- Model:
  - native: 2 mode switches, 0 context switches, 1 x save+restore state
  - virtualized: 4 mode switches, 2 context switches, 3 x save+restore state

Expected overhead?

Performance Counters are Your Friends!

Good or bad?

Counter	Native	Virtualized	Difference
Branch miss-pred	1	1	0
D-cache miss	0	0	0
I-cache miss	0	1	1
D-µTLB miss	0	0	0
I-µTLB miss	0	0	0
Main-TLB miss	0	0	0
Instructions	**30**	**125**	**95**
D-stall cycles	0	27	27
I-stall cycles	0	45	45
Total Cycles	**93**	**292**	**199**

More of the Same...

First step: improve representation!

Benchmark	Native	Virtualized
Context switch [1/s]	615046	444504
Create/close [µs]	11	15
Suspend [10ns]	81	154

Further Analysis shows guest dis-&enables IRQs 22 times!

Second step: overheads in appropriate units!

Benchmark	Native	Virt.	Diff [µs]	Diff [cy]	# sysc	Cy/sysc
Context switch [µs]	1.63	2.25	0.62	230	1	230
Create/close [µs]	11	15	4	1472	2	736
Suspend [µs]	0.81	1.54	0.73	269	1	269

Yet Another One...

Good or bad?

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Benchmark	Native [µs]	Virt. [µs]	Overhead	Per tick
TDes16_Num0	1.2900	1.2936	0.28%	2.8 µs
TDes16_RadixHex1	0.7110	0.7129	0.27%	2.7 µs
TDes16_RadixDecimal2	1.2338	1.2373	0.28%	2.8 µs
TDes16_Num_RadixOctal3	0.6306	0.6324	0.28%	2.8 µs
TDes16_Num_RadixBinary4	1.0088	1.0116	0.27%	2.7 µs
TDesC16_Compare5	0.9621	0.9647	0.27%	2.7 µs
TDesC16_CompareF7	1.9392	1.9444	0.27%	2.7 µs
TdesC16_MatchF9	1.1060	1.1090	0.27%	2.7 µs

- Note: these are purely user-level operations!
  - What's going on?

Timer interrupt virtualization overhead!

Lessons Learned

- Ensure stable results
  - repeat for good statistics
  - investigate source of apparent randomness
- Have a model of what you expect
  - investigate if behaviour is different
  - unexplained effects are likely to indicate problems — don't ignore them!
- Tools are your friends
  - performance counters
  - simulators
  - traces
  - spreadsheets

Annotated list of benchmarking crimes: http://www.gernot-heiser.org/

Thank You!

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