Is Computer Science Dying?

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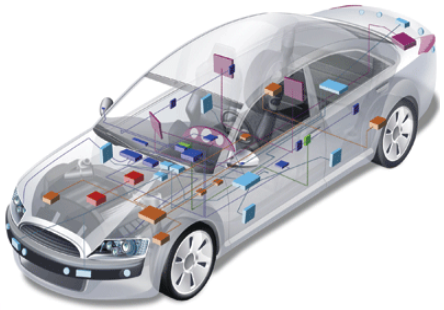
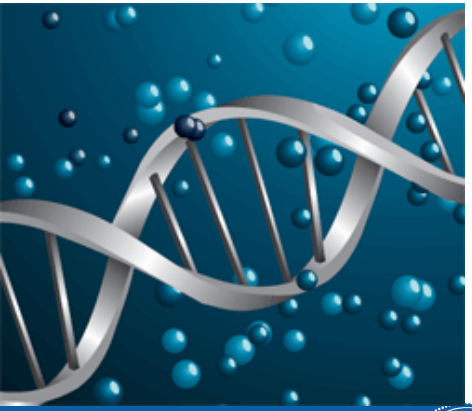
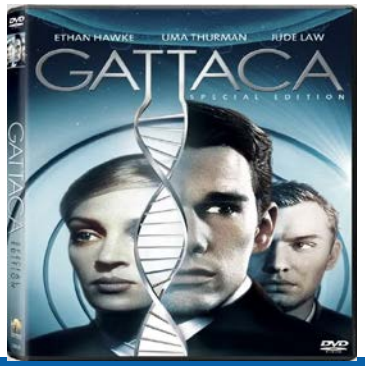
That's Absurd – Computers are everywhere!







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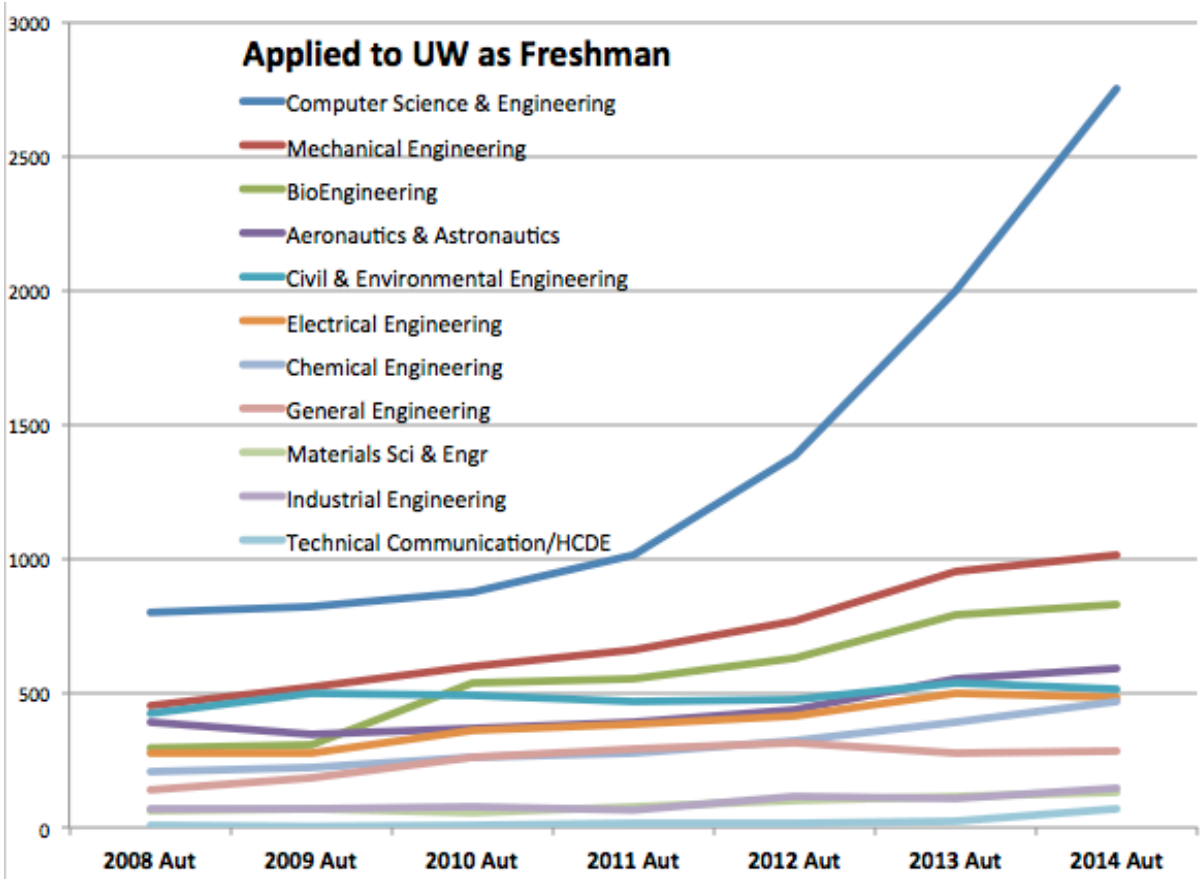
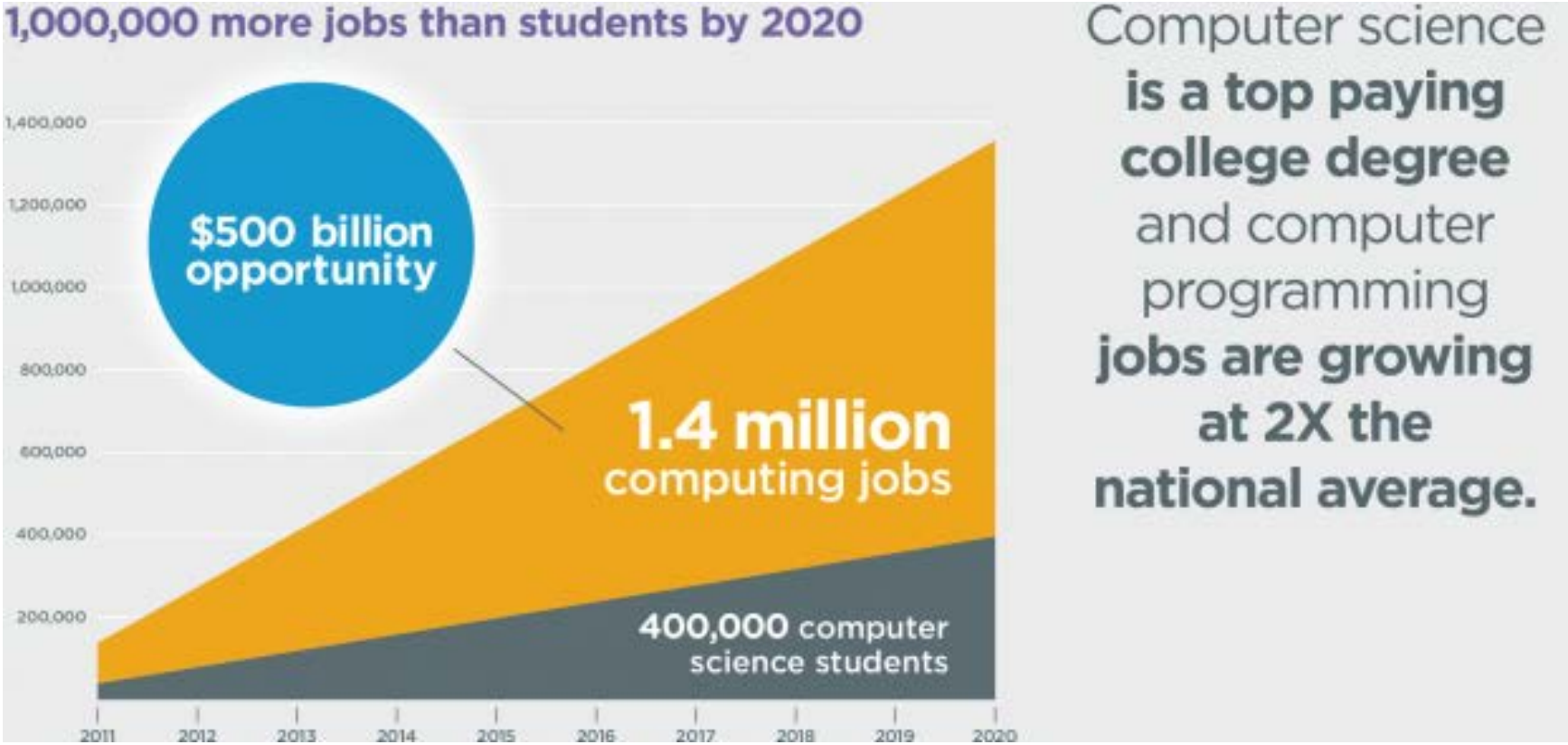
That's Absurd – enrollments are absurdly high and growing!



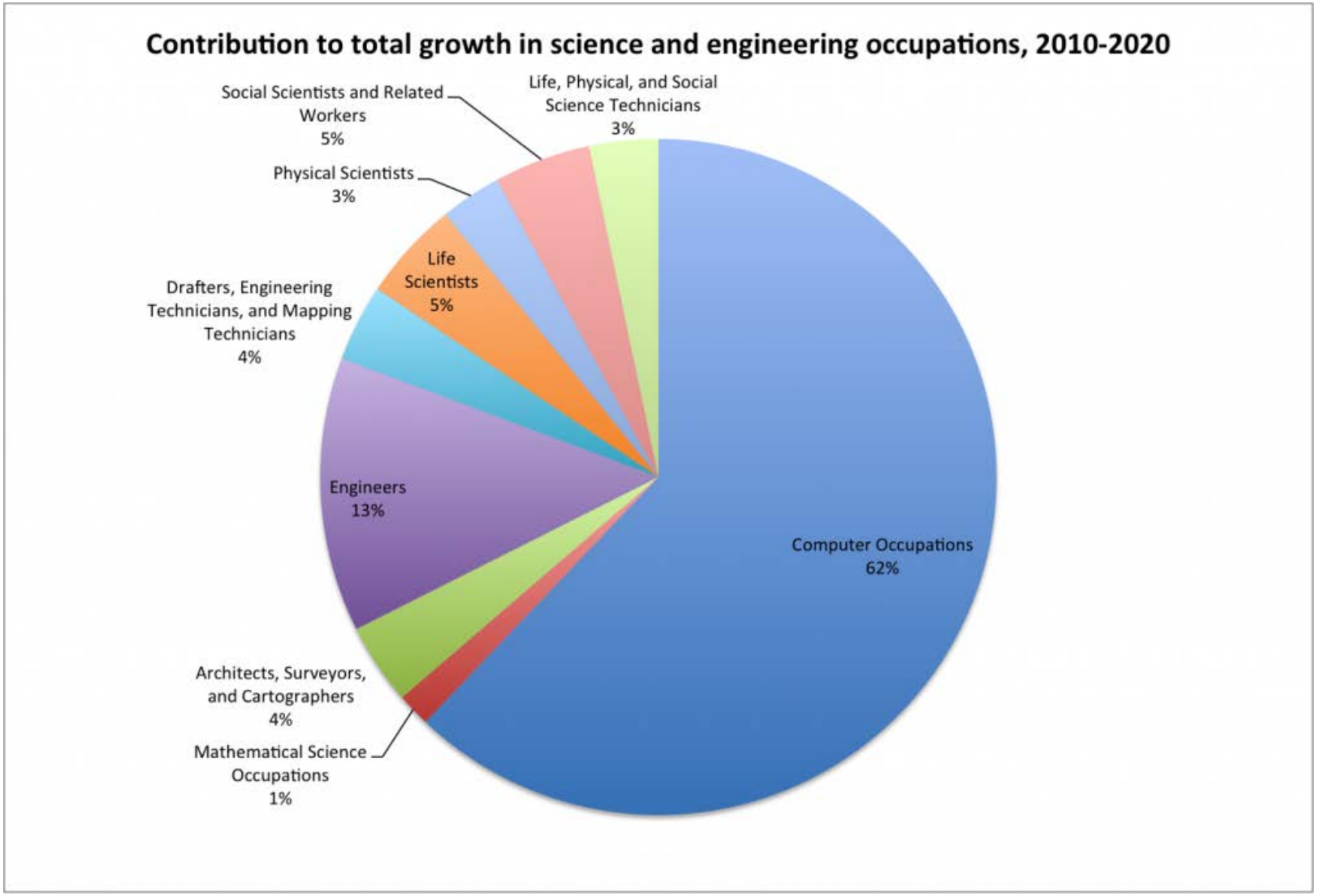
Credit Ed Lazowska

That's Absurd – Jobs Growth !!



Credit Ed Lazowska

That's Absurd – Demand in Context!!!

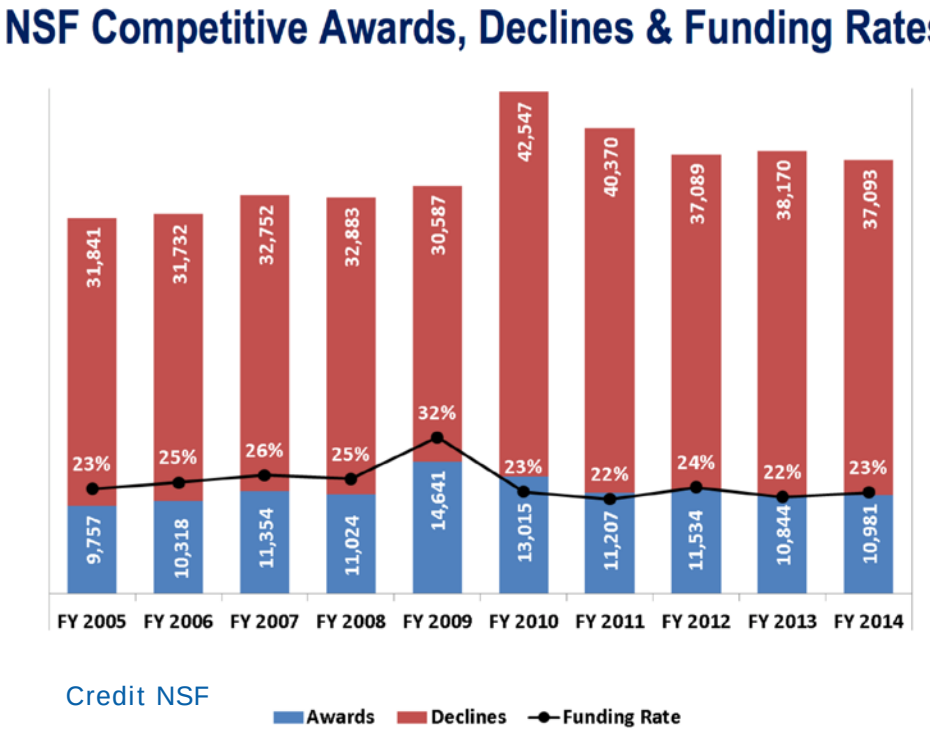


Computer Science is all over the media daily!!! Computer Industry leaders are ROCK STARS!!!So premise is

Or Is it?

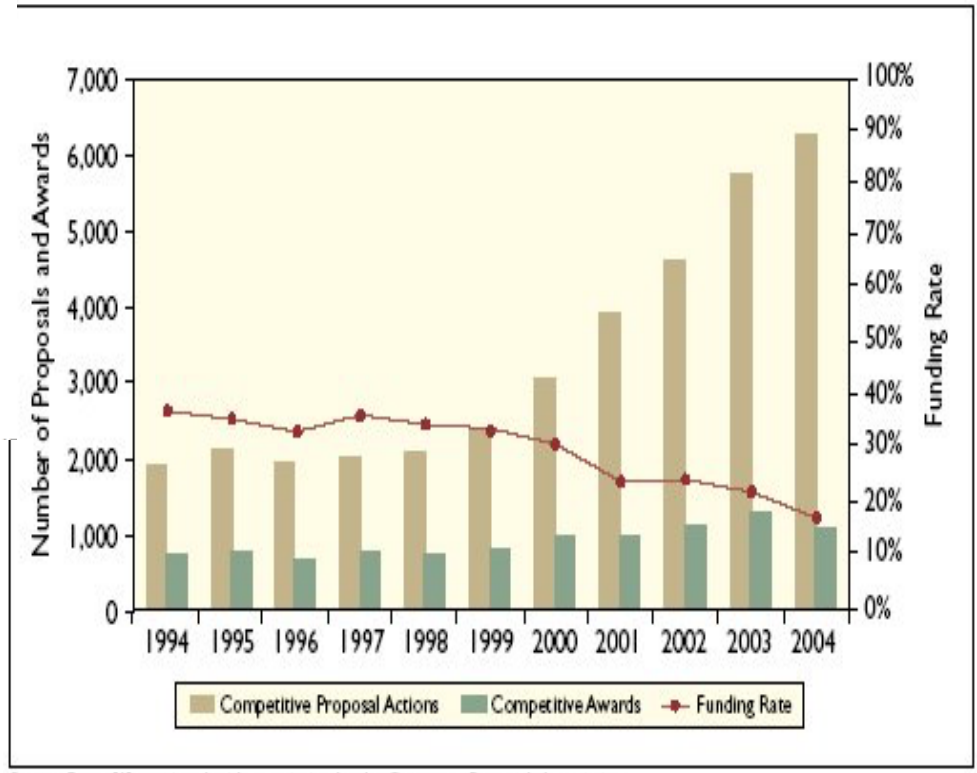
The Question was:
IS COMPUTER *SCIENCE* Dying?

Worrisome facts (I)



Credit NSF

Ah, but that's no doubt because NSF accounts for a small fraction of CS funding….

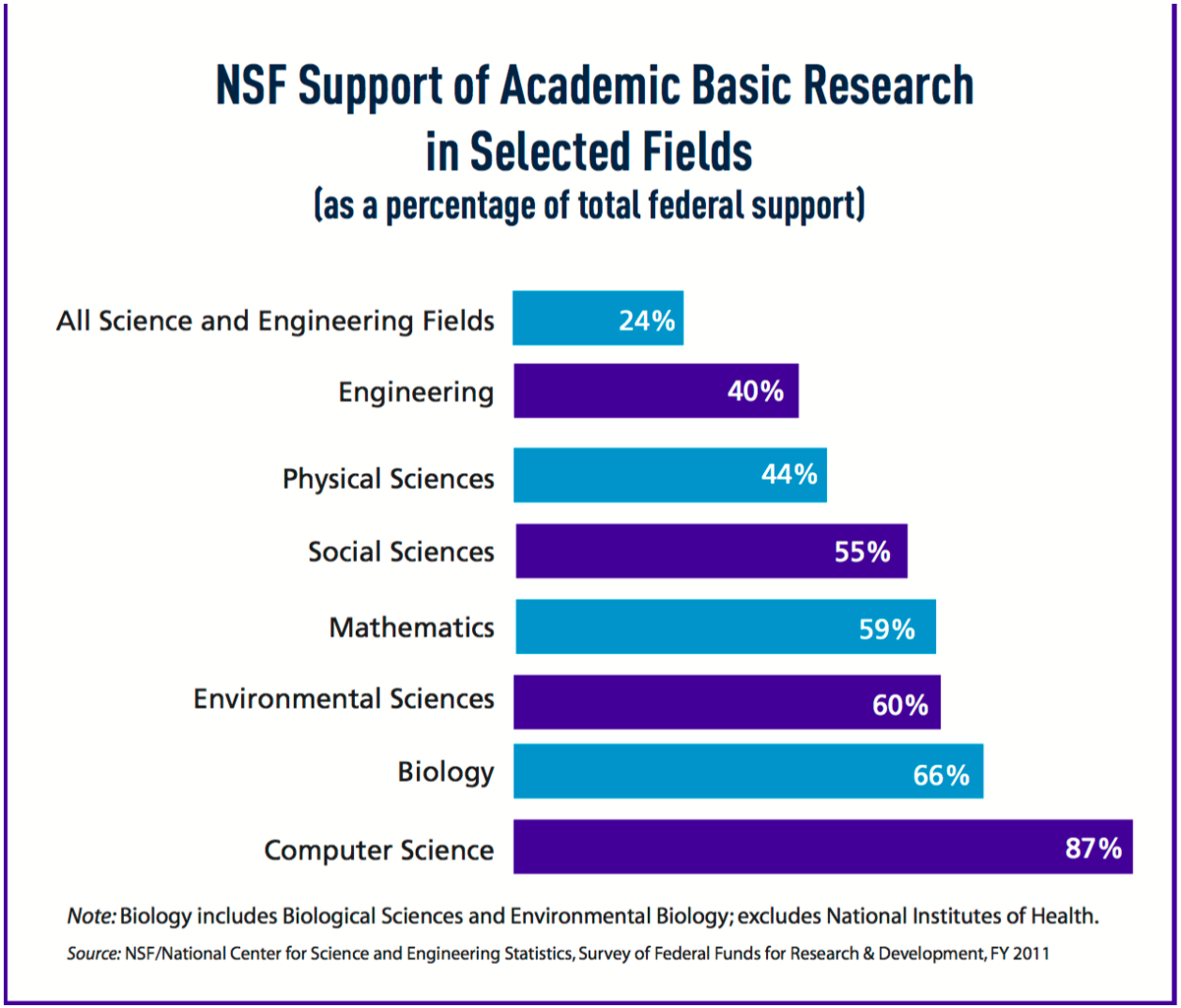


Source: From [1], reprinted with permission by the Computer Research Association.

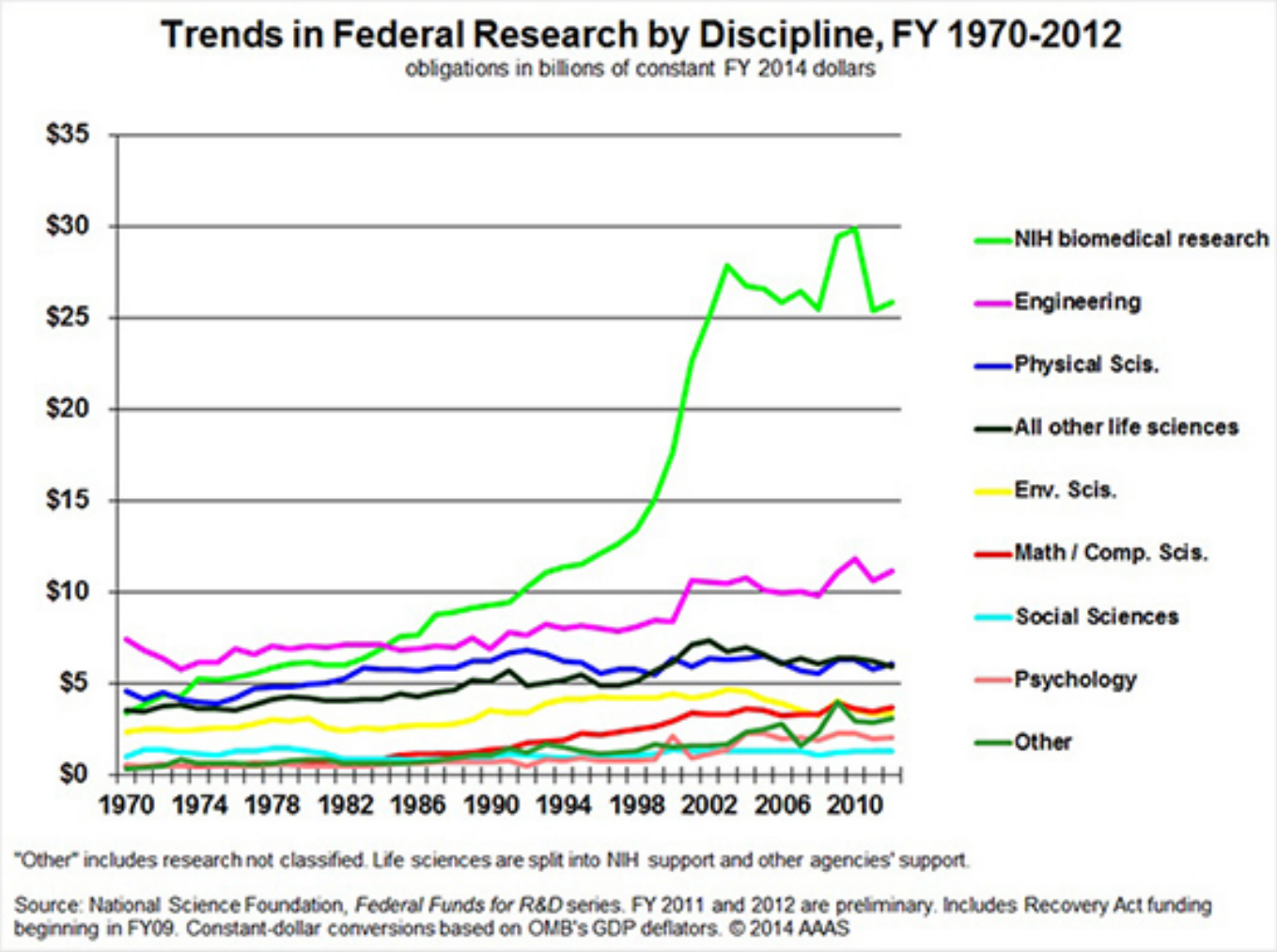
Worrisome Facts (II)

Well,

not really...

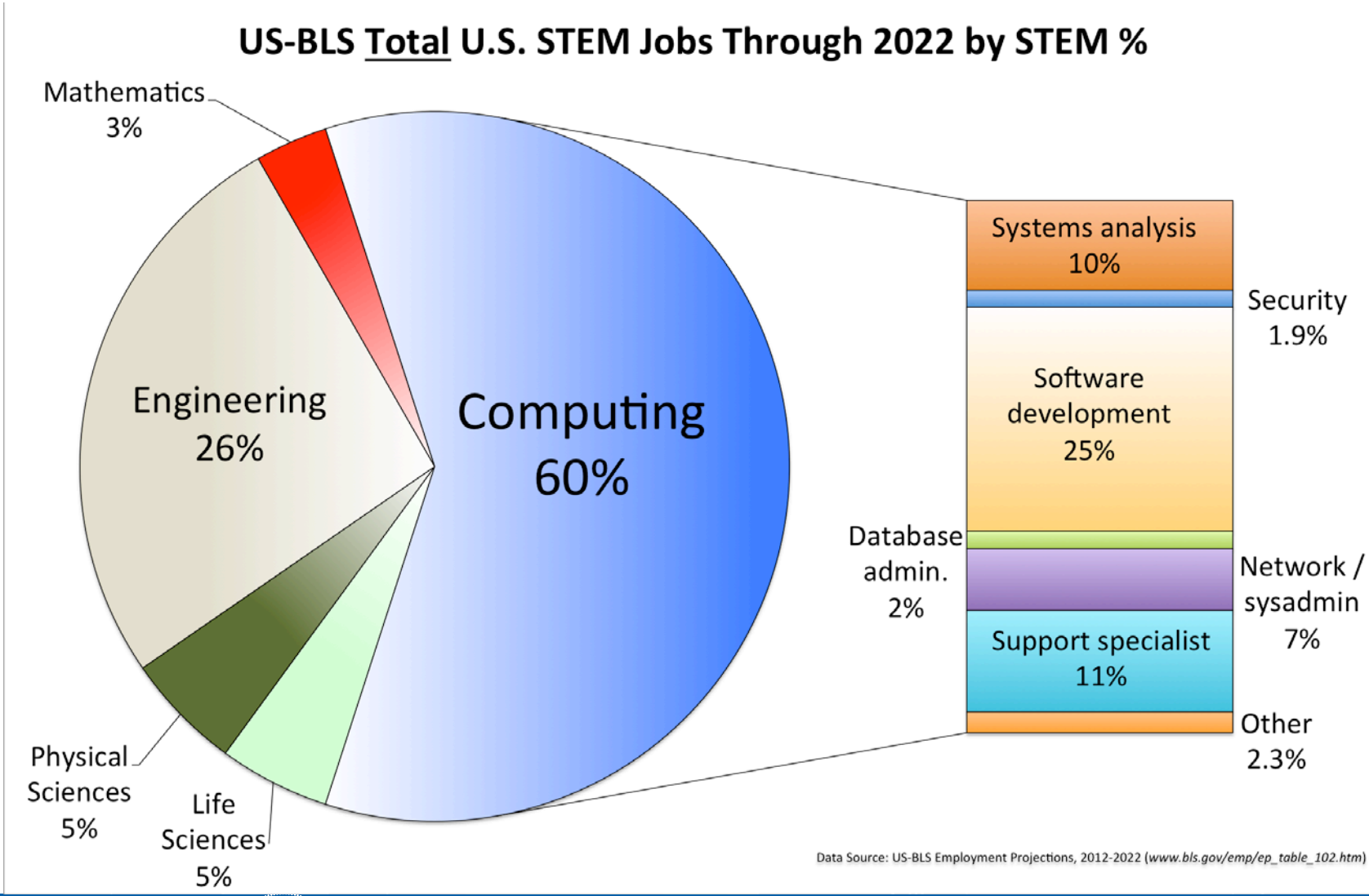


Maybe I'm misreading the previous slides? Well, here's another view…

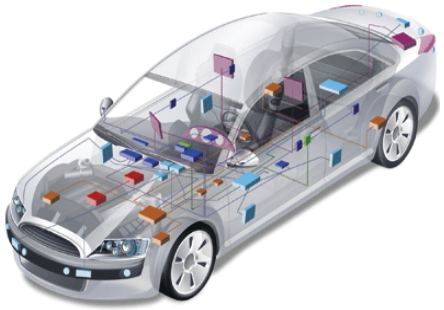


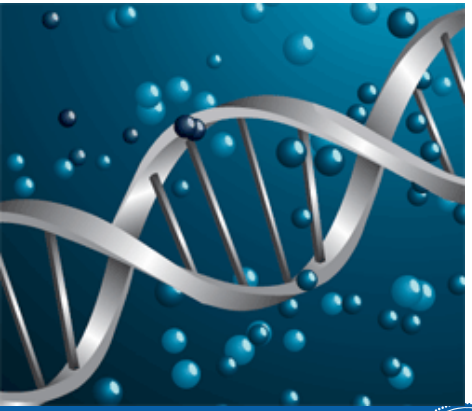
So why is Computer Science funding so puny?

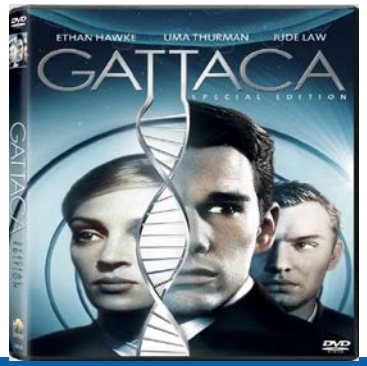
Perhaps even more worrisome (I)



Perhaps even more worrisome (II): we are the *hired help!*







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Here's perhaps a more interesting question: Is Computer Science a SCIENCE?

- Focused on/around a particular instrument, admittedly complex/important
  - "The invention of the Computer is, in its contribution to science, akin to Galileo's Microscope" – Ken Wilson, Nobel Prize Winner (Physics '82)
  - Driven by technological developments (maybe ok, better than Math…or is it?)
    - "Computer science is no more about computers than astronomy is about telescopes." - Edsger Dijkstra
    - "Djeksra's most cherished wish is to turn CS into a trivial branch of Math."
      – Alan Perlis
  - "The use of a program to prove the 4-color theorem will not change mathematics - it merely demonstrates that the theorem, a challenge for a century, is probably not important to mathematics." – Alan Perlis

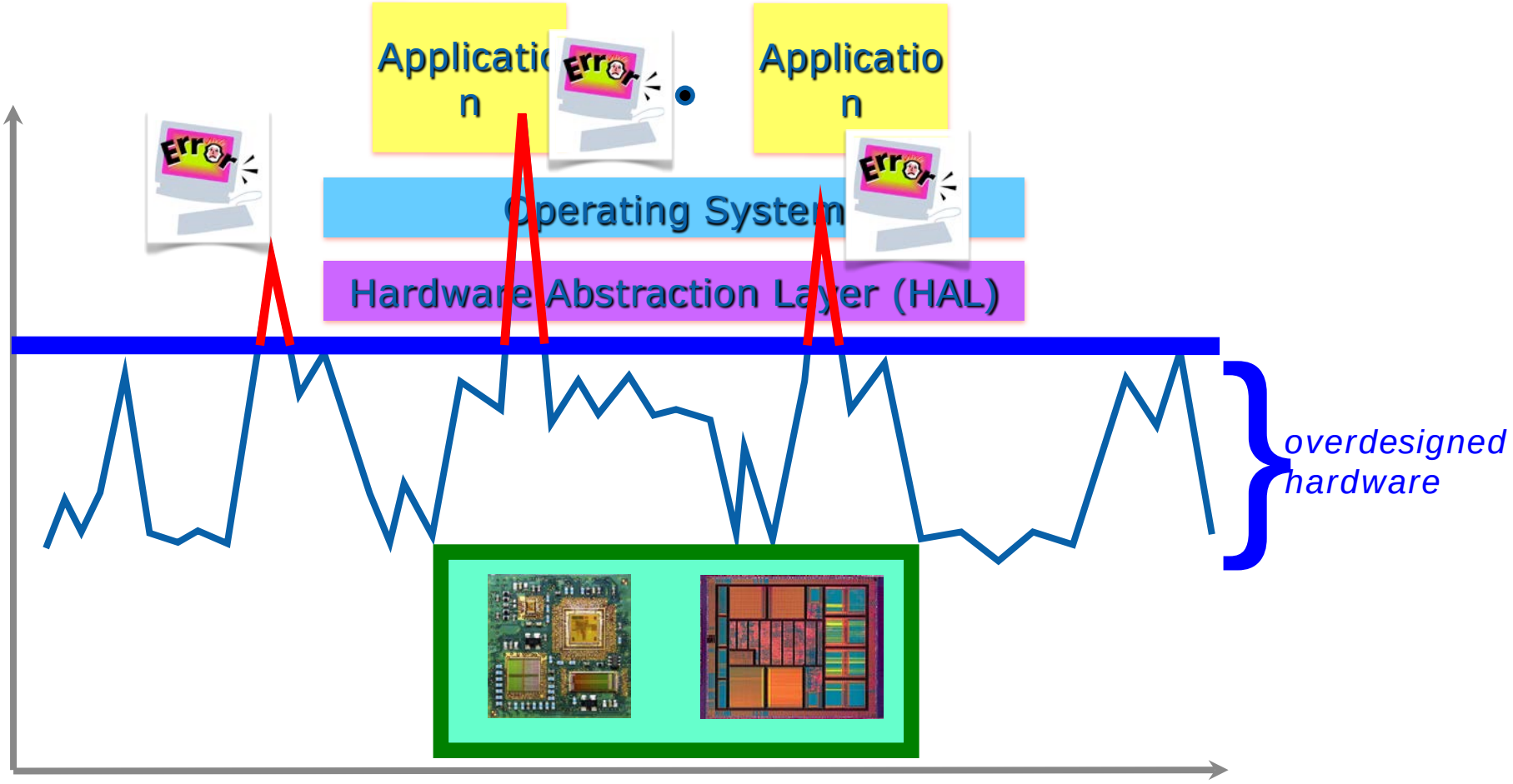
Computer Science is ???

- Driven by Technology and Applications (not ok):
  - In Academia, hard to track the developments in a rapidly evolving marketplace. Nor should we.
  - By tracking industrial developments, we end up with incremental, not very meaningful, short-sighted work that is often obsolete by the time it's published, instead of far-reaching, futuristic research that would drive industry.
- Theory part is often removed from practice, and the practice (systems) is removed from science.
  - Theoretical results are often too abstract, Systems' too applied.
- VERY poor scholarship
  - In citing prior work.
  - Poor peer-review (and getting worse).
  - Spin is king (paradoxically every paper claims to be a breakthrough)
  - No scientific validation process, repeating experiments is aggressively discouraged.
  - Publications mainly in conferences.

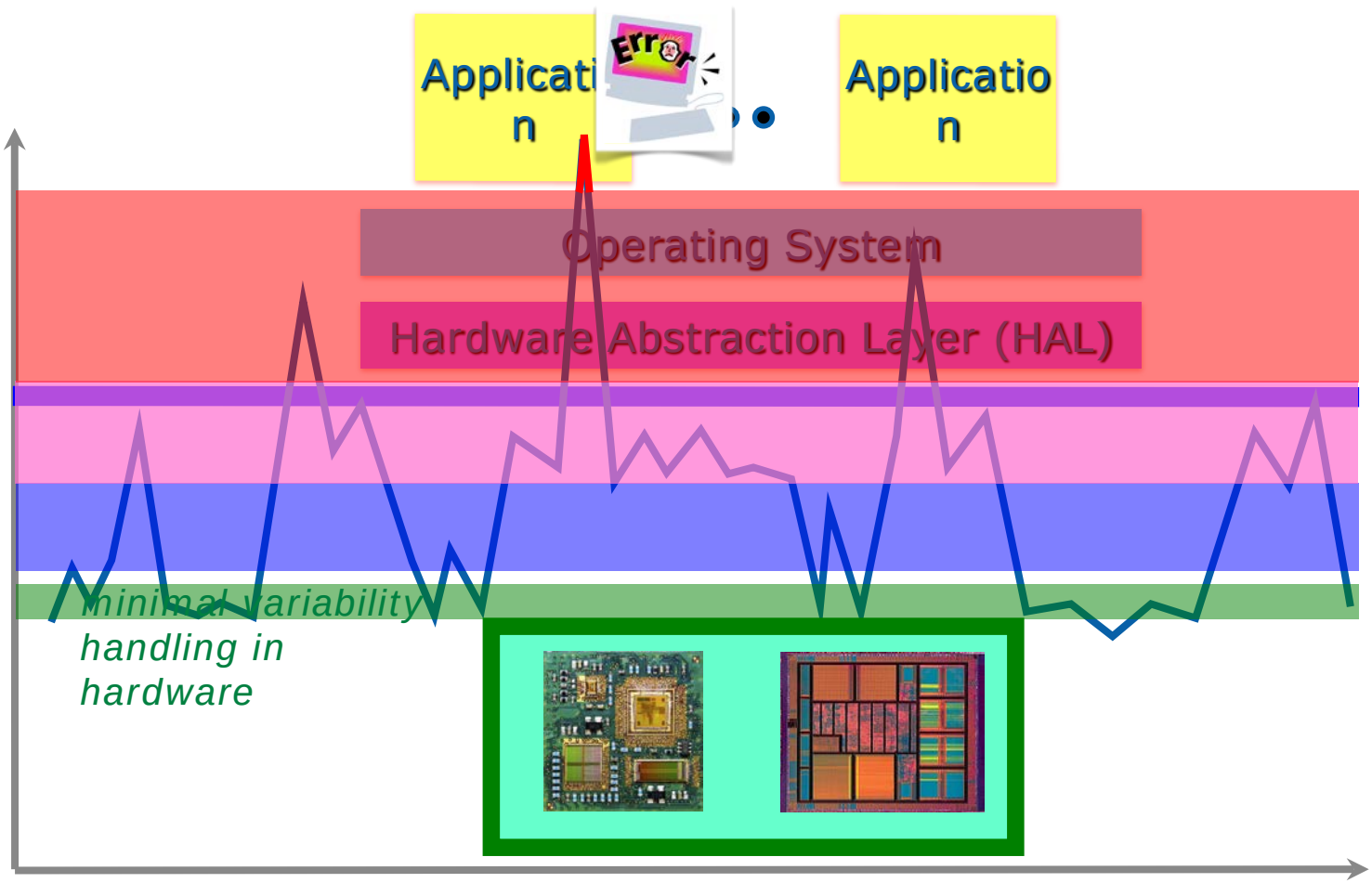
Case Study – Flagship Project, 14 PI's from 7 top schools. Five years duration, $10 million

- Premise
  - As feature size & power ↘ and speed ↗, more HW transient errors will occur, and an increasing % of chip area (~40% and growing) is dedicated to fixing these errors.
  - Aging and Temperature make this even worse.

Let's Revisit the Hardware-Software Interface...



Imagine a new hardware-software interface...



Bad news:

- At the coarse level, software already deals w/ faults (fault tolerance: via duplication/check-pointing) ➔ derivative research.
- At the fine-grain level, errors too numerous to handle w/ classic fault tolerance techniques, OR any SW.
- Goldilocks' level: errors numerous enough to make check-pointing impractical, but, not enough to make software approaches impractical.
  - Turns out the Goldilocks level is not as wide as the prior slide would imply.
  - Most of the researchers involved were hardware/CAD experts

More bad news:

- To get access to latest technology, work is done under NDA.
  - ➔ what can be published is partial and not that illuminating even when the work itself is very good.

- W/O access, academics are limited to old technology/information models (e.g., power models, simulators) strung together haphazardly, with dubious (if any) validation, and dubious test-beds
  - ➔ questionable results even when they appear very interesting.

Nevertheless, a big success…

- One hundred twenty four publications.
- Spawned workshops, special sessions, invited talks at major conferences on three continents.
- Spawned more derivative/incremental research in Europe and Japan with major funding.

Another example: Pointer Analysis

- Studied for over 40 years - still ongoing
- [Jones and Muchnick POPL'76, Horwitz et al. PLDI'89, Chase et al. PLDI'90, Hummel et al. PLDI'94, Andersen Ph.D. Thesis'94, Ghiya and Hendren POPL'96, Wilhelm et al. CC'00, Kastrinis & al PLDI '13] Focus: Precision, Analysis time

The bottom-line has been largely unaddressed!!

- How good is the coverage – not on SPEC, but at large.

- Fundamental alternatives to ambiguous references.

As for the future of the field…

- “plus ça change, plus c'est la même chose”

  [Jean-Baptiste Alphonse Karr, 1849]

- **“What has been is what will be, and what has been done is what will be done; there is nothing new under the sun”**

  [Ecclesiastes 1:9, cc. 200-195 BCE, Old Testament, New International Version]

Can we change this? HOW?

- Truly innovative research – Pie in the sky, NOT the next twist that could maybe make the newest Intel Processor (Which will be old by the time the work is published) save 3% power on a good day, on the right benchmark running on the bare machine….
- Reproducible & reproduced research
- Publish for intellectual stimulation/merit, not because it's useful to Intel, ARM, Nvidia, Micorsoft…or because it's fundable…
- Publish less, and only if one has something truly significant to say.
- Cite generously, and more thoughtfully, clearly, and *honestly*.
- Don't spin
- Don't run after the next big thing (trend): like in the stock market, it's already too late.

➔ Big Problem: This would require a major paradigm shift *in us AND our institutions*, away from bean counting.

There's more we can do:

Allow –require- publication of validation studies that duplicate important results independently, before taking them seriously.

Allow people to be promoted on the *quality* of their contributions and thinking, not on the *volume* of papers.

Reward reviewing whether monetarily or via promotions to improve quality of review (which is currently a disgrace).

Eliminate the third, fourth, and fly-by-night tier conferences, by simply refusing to publish in them and refusing to serve on their PCs.

Have stringent ethical standards/oversight along the lines of the medical/pharma fields for industrial supported work.

Have "kind but stringent" review of papers and proposals, with an eye on improving, not rejecting. [like the Medical field, again]

Use conferences for feedback/dissemination, journals for publications that count (don't worry about duplication between conferences/journals).

Tools for the community to establish baseline & enable *fair* comparisons.

It's not all bleak: some hopeful signs

BEE3: Revitalizing Computer Architecture

April 1, 2009

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Authors

John Davis

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Chen Chang

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MSR-TR-2009-45

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THANK YOU!