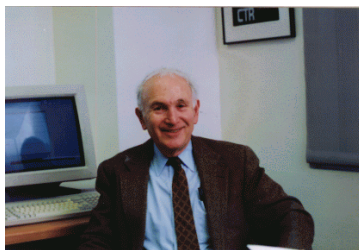


~~1~~
~~2~~
~~3~~
~~4~~
~~5~~
~~6~~
~~7~~
~~8~~
~~9~~
~~10~~ pieces of advice I wish my
PhD advisor had given me

Jim Kurose
Department of Computer Science
University of Massachusetts
Amherst, MA USA
<http://www.cs.umass.edu/~kurose>



With great affection and apologies
to my advisors

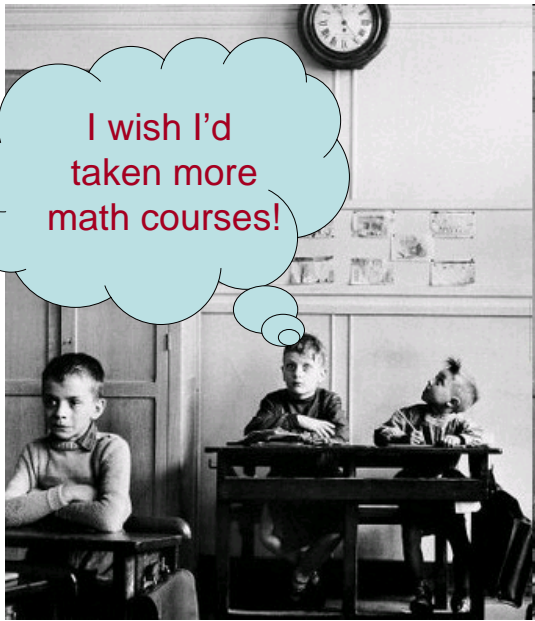


Mischa Schwartz, EE Department,
Columbia University



Yechiam Yemini
CS Department
Columbia University

and a disclaimer.....

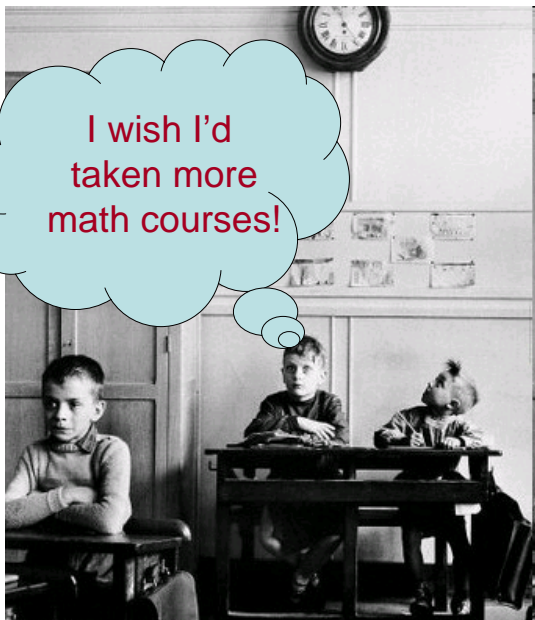


I wish I'd
taken more
math courses!

study broadly #1

Take math courses!

- ❑ every math course I've taken has been valuable
- ❑ won't have time later
- ❑ research fields draw increasingly on math as they mature
- ❑ theory is timeless!

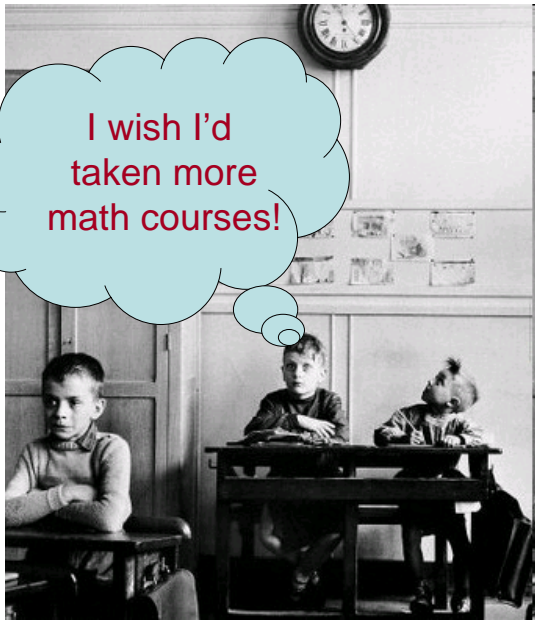


I wish I'd
taken more
math courses!

study broadly #1

Important courses outside CS

- ❑ signal processing
- ❑ control theory
- ❑ information theory
- ❑ nonlinear optimization
- ❑ stochastic processes
- ❑ game theory
- ❑ domains: biology, economics,...



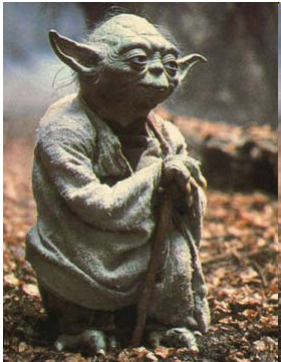
#1

study broadly

- ❑ you will never again have so much “relaxed” time to study, learn, think

Sorry for that piece of bad news... but it's true

Choosing, defining a research problem



A fool can ask more questions in a minute than a wise man/woman (or a Yoda) can answer in a lifetime

pick your problems carefully!

- ❑ what's the fundamental issue you're solving?
- ❑ will the problem be of interest five, ten years from now?
- ❑ focus on fundamentals in a world with an increasingly short attention span

- QoS
- multicast
- congestion control
- P2P
- sensor networks
- energy

#2

Choosing, defining a research problem

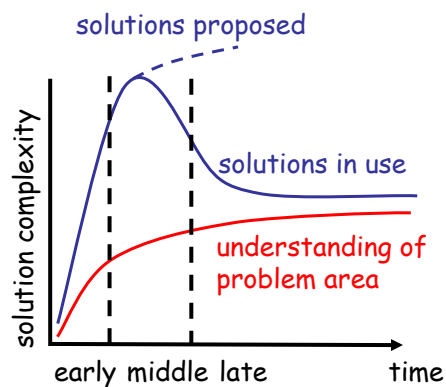


Wisdom of crowds?

There are lots of smart people out there!

- ❑ avoid crowded areas unless you have a unique talent, viewpoint
- ❑ low-hanging fruit has been picked
- ❑ researchers working on “next big thing” are not in the crowd.

Choosing, defining a research problem

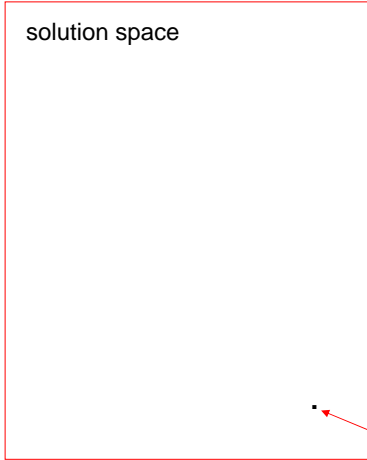


[adapted from Hluchj 2001]

- ❑ complexity, sophistication are themselves not of interest (except to purist)
 - ❖ means, not an end
 - ❖ how is the “story” changed in the end?

Choosing, defining a research problem

solution space



avoid point solutions

- ❑ insights that cut across solution space vs point solution
- ❑ what broader conclusions can be drawn from your work?



You are here (but shouldn't be)

Publishing

#3



- ❑ publish where you will get mindshare, impact
- ❑ there *is* life beyond sigcomm, infocom
- ❑ quality over quantity: avoid LPU's
- ❑ PhD dissertation != magnum opus
- ❑ don't be driven by conference deadlines

Time: your most precious resource #4



Time

- ❑ give yourself time to think
- ❑ manage your time carefully: consciously choose how you spend your time



Only "how to" book recommended by Bill Clinton

I've given away 50+

Learn how to write *really* well #5



- ❑ can *not* overstress importance of good writing
 - ❖ the most important course?
- ❑ "unfair advantage" in paper, proposal review
- ❑ Outstanding investment of your time
- ❑ study role models

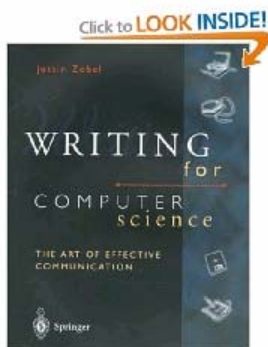
"No tale is so good that it can't be spoiled in the telling"
Proverb

<http://www-net.cs.umass.edu/kurose/writing/>

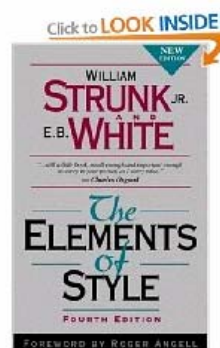
Top-10 tips for writing a paper

1. Every paper tells a story
2. Write top down
3. Introduction: crucial, formulaic
4. Master basics of organized writing
5. Put yourself in place of reader
6. Put yourself in place of the reader
7. No one (not even your mother) is as interested in this topic as you
8. State results carefully
9. Study the art of writing
10. Good writing takes time

Recommended reading:



Writing for Computer Science
by Justin Zobel



The Elements of Style
by William Strunk E. B. White
(50 years old – and still a classic!)

Learn how to speak *really* well #6



- ❑ can't overstress importance of good speaking
 - ❖ important course to teach/take?
- ❑ "unfair advantage" in mindshare
- ❑ convey exciting story/message
 - ❖ thoughtful
 - ❖ engaging
 - ❖ clear, concise
- ❑ practice, practice, practice
 - ❖ videotape, critique yourself
 - ❖ study role models

Learn the *process* of doing research #7



apprenticeship

- ❑ our field is a guild
 - ❖ grad student = apprentice
 - ❖ professor = master artisan
- ❑ it's about more than the results in your thesis
 - ❖ you'll be generating results for a lifetime
 - ❖ knowing process is what's most important
- ❑ why your advisor can't (and shouldn't) solve (or even define) the problem for you

#8

Think about what you want to do afterwards

academia:

- ❑ teaching schools
- ❑ research-1 schools
- ❑ big v small; public v private
- ❑ country?
- ❑ do you love (or at least like) to teach? students?

industry:

- ❑ many different types of industry settings
 - ❖ startup
 - ❖ “big industry”
 - ❖ research labs
- ❑ research institutes



#9

A community of scholars



- ❑ meet people, listen, collaborate
 - ❖ good students, colleagues, friends
- ❑ approach, talk with people
- ❑ interactions with peer students
 - ❖ research discussions
 - ❖ paper presentations
 - ❖ practice talks
 - ❖

#10

Identify role models



- ❑ who does something you care about *really* well?
 - ❖ how do they do it?
- ❑ many role models:
 - ❖ no one does everything
 - ❖ find your balance
- ❑ get a mentor
- ❑ be a mentor

#11

The last word



Have fun – enjoy
what you are doing

**Best piece of advice I
ever received (1984):**

*“Pick a place, job where you’ll
have fun, enjoy living, enjoy
your colleagues. Without that,
no level of success will make
you happy.” Jack K. Wolf*

Questions!

- ❑ What did I miss?
- ❑ What advice would you give to 1st year student?