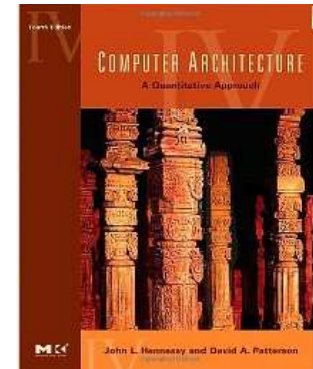


How to Have a Bad Career in Research/Academia

Professor David A. Patterson
University of California at Berkeley
(Spring 2001)



www.cs.berkeley.edu/~pattrsn/talks/nontech.html

Presented and **Revised** by
Chakchai So-In, Ph.D.

Agenda

- ❑ Part I: Key Advice for a Bad Career while a Grad Student
- ❑ Part II: Key Advice for a Bad Career
- ❑ Part III: Key Advice on Alternatives to a Bad Career
- ❑ Conclusions

Part I:
**Key Advice for a Bad Career while a
Grad Student**

Part I: How to Have a Bad Graduate Career

- ❑ Concentrate on getting good grades:
 - Postpone research involvement: might lower GPA
- ❑ Minimize number and flavors of courses
 - Why take advantage of 1 of the top departments with an emphasis on excellent grad courses?
 - May affect GPA
- ❑ Don't trust your advisor
 - Advisor is only interested in his or her own career, not your's
 - Advisor may try to mentor you, use up time, interfering with GPA
- ❑ Only work the number of hours per week you are paid!
 - Don't let master class exploit the workers!

Part I: How to Have a Bad Graduate Career (cont.)

- ❑ Concentrate on graduating as fast as possible
 - Winner is first in class to PhD.
 - People only care about that you have a PhD and your GPA, not on what you know.
 - Don't spend a summer in industry: takes longer
 - Don't work on large projects: takes longer
 - Don't do a systems PhD: takes longer
- ❑ Don't go to conferences
 - It costs money and takes time; you'll have plenty of time to learn the field after graduating.
- ❑ Don't waste time polishing writing or talks (that also takes time.)

Part I: Better Way to go (DO IT!)

- ❑ Concentrate on getting good grades?
 - Reality: need to maintain reasonable grades
 - ❑ 3 prelim courses only real grades that count
 - What matters on graduation is letters of recommendation from 3-4 faculty/PhDs who have known you for 5+ years
- ❑ Minimize number and flavors of courses?
 - Your last chance to be exposed to new ideas before have to learn them on your own
 - Get a real outside minor from a campus with great departments in all fields; e.g., Management of Technology certificate, Copyright Law

Part I: Better Way to go (DO IT!) (cont.)

- ❑ Don't trust your advisor?
 - Primary attraction of campus vs. research lab is getting to work with grad students
 - Faculty career is judged in large part by success of his or her students
 - try taking advice of advisor?
- ❑ Concentrate on graduating as fast as possible?
 - Your last chance to learn; most learning will be outside the classroom
 - Considered newly “minted” when finish PhD
 - ❑ Judged on year of PhD vs. year of birth

Part I: Better Way to go (DO IT!) (cont.)

□ Don't go to conferences?

- Chance to see firsthand what the field is like, where its going
- There are student rates, you can share a room
- Talk to people in the field in the halls
- If your faculty advisor won't pay, then pay it yourself; almost always offer student rates, can often share rooms

□ Don't waste time polishing writing or talks?

- In the marketplace of ideas, the more polish the more likely people will pay attention to your ideas
- Practice presentation AND answering tough questions

Part I: Better Way to go (DO IT!) (cont.)

- ❑ Only work the number of hours per week you are paid?
 - Campus Faculty average is 65-70 hours/work.
 - Students should be in that range
 - Organize each day: when most alert? nap? exercise? sleep?
 - When/how often/how long: write, read, program, email?
 - To do lists: daily, weekly, semester
- ❑ Industrial Experience?
 - 1st or 2nd summer get work experience, or 1 semester off
- ❑ Sutherland's advice (Father of Computer Graphics)
 - Be bold; Take chances on hard topics

How to be a Success in Graduate School

1) “Swim or Sink”

- “Success is determined by me (student) primarily”
- Faculty will set up the opportunity, but its up to me leverage it

2) “Read/learn on your own”

- “Related to 1), I think you told me this as you handed me a stack of about 20 papers”

3) “Teach your advisor”

- “I really liked this concept; go out and learn about something and then teach the professor”
- Fast moving field, don’t expect Prof. to be at forefront everywhere

Part II:

Key Advice for a Bad Career

➤7 Bad Career Moves

➤5 Writing Commandments for a Bad Career

➤7 Talk Commandments for a Bad Career

Bad Career Move #1: Be THE leading expert

- ❑ Invent a new field!
 - Make sure its slightly different
- ❑ Be the real Lone Ranger: Don't work with others
 - No ambiguity in credit
- ❑ Research Horizons
 - Never define success
 - Stick to one topic for whole career
 - Even if technology appears to leave you behind, stand by your problem

Bad Career Move #2: Let Complexity Be Your Guide

- ❑ Best compliment:
 - “Its so complicated, I can’t understand the ideas”
 - If no one understands, how can they contradict your claim?
- ❑ It’s easier to be complicated
 - Also: to publish it must be different; N+1st incremental change
- ❑ If it were not unsimple then
 - How could distinguished colleagues in departments around the world be positively appreciative of both your extraordinary skills and talents

Bad Career Move #3: Never be Proven Wrong

- ❑ Avoid Implementing
- ❑ Avoid Quantitative Experiments
 - If you've got good intuition, who needs experiments?
 - Why give grist for critics' mill?
 - Takes too long to measure
- ❑ Avoid Benchmarks

Bad Career Move #4: Use the Computer Scientific Method

Obsolete Scientific Method	Computer Scientific Method
<ul style="list-style-type: none">➤ Hypothesis➤ Sequence of experiments➤ Change 1 parameter/exp.➤ Prove/Disprove Hypothesis➤ Document for others to reproduce results	<ul style="list-style-type: none">➤ Hunch➤ 1 experiment & change all parameters➤ Discard if doesn't support hunch➤ Why waste time? We know this

Bad Career Move #5: Don't be Distracted by Others -Avoid Feedback

- ❑ Always dominate conversations: Silence is ignorance
 - Corollary: Loud is smart
- ❑ Don't read
- ❑ Don't be tainted by interaction with users and industry
- ❑ Reviews
 - If it's simple and obvious in retrospect → Reject
 - Quantitative results don't matter if they just show you what you already know → Reject
 - Everything else → Reject

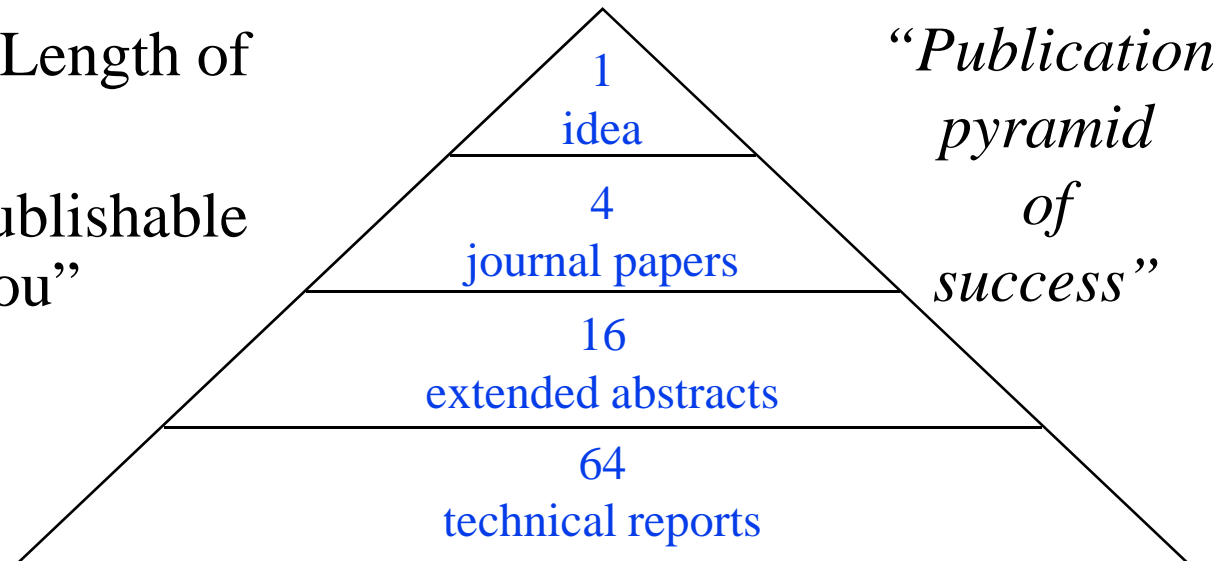
Bad Career Move #6: Publishing Journal Papers IS Technology Transfer

- ❑ Target Archival Journals: the Coin of the Academic Realm
 - It takes 2 to 3 years from submission to publication
→ timeless
- ❑ As the leading scientist, your job is to publish in journals *not* your job to make you the ideas palatable; wastes valuable research time
 - Travel time, having to interact with others, serve on program committees, ...

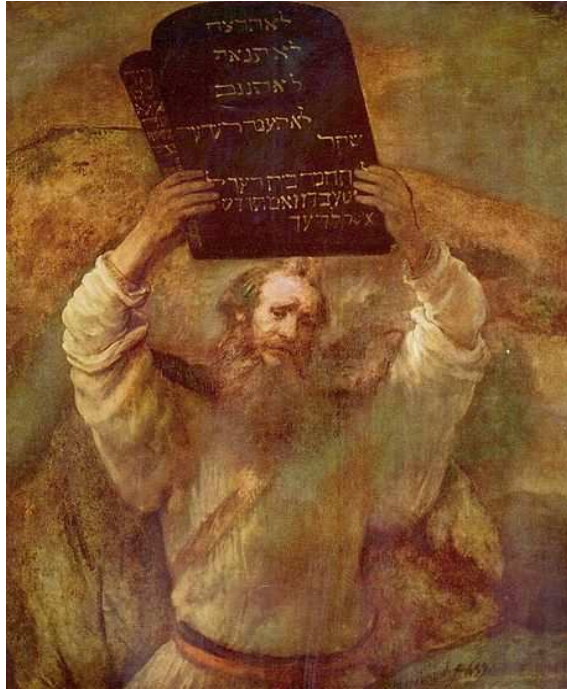
Bad Career Move #7: Writing Tactics for a Bad Career

Papers: It's Quantity, not Quality

- Personal Success = Length of Publication List
- “The LPU (Least Publishable Unit) is Good for You”



- ❑ Student productivity = number of papers
 - Number of students: big is beautiful
 - Never ask students to implement: reduces papers
- ❑ Legally change your name to “Chaaaakkkkkccccchaaaaai”



5 Writing Commandments for a Bad Career

Ref: [wikipedia](#)

- I. Thou shalt not define terms, nor explain anything.
- II. Thou shalt replace “will do” with “have done”.
- III. Thou shalt not mention drawbacks to your approach.
- IV. Thou shalt not reference any papers.
- V. Thou shalt publish before implementing.

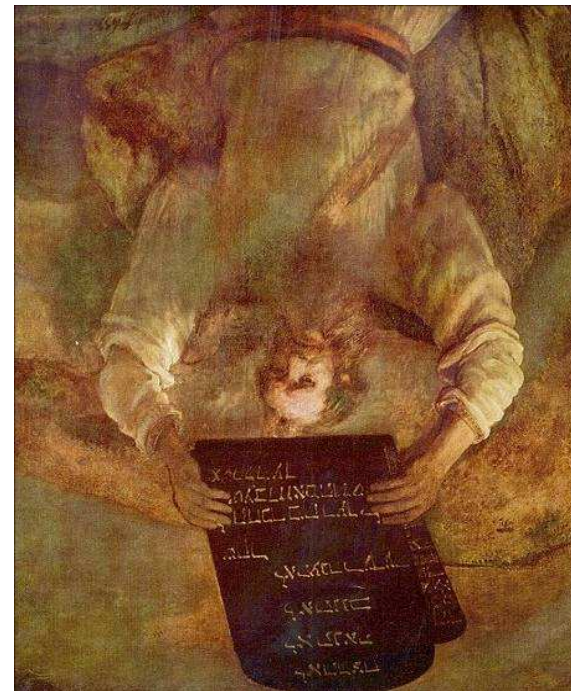
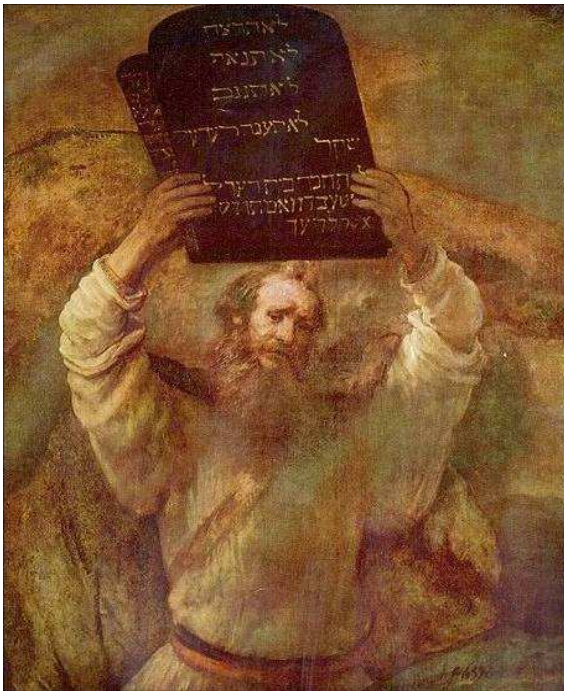


7 Talk Commandments for a Bad Career

Ref: [wikipedia](#)

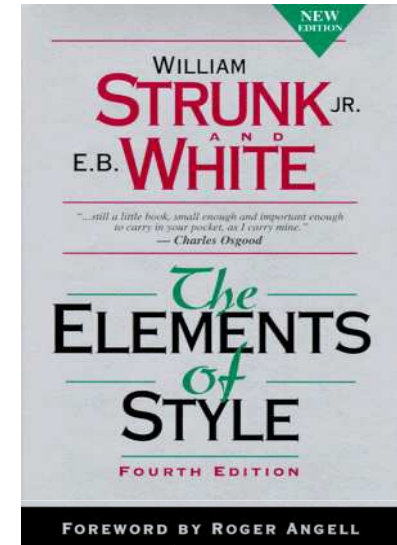
- I. Thou shalt not illustrate.
- II. Thou shalt not covet brevity.
- III. Thou shalt not print large.
- IV. Thou shalt not use **color**.
- V. Thou shalt cover thy naked slides.
- VI. Thou shalt not skip slides in a long talk.
- VII. Thou shalt not practice.

Real Way to DO = DO OPPOSITE!



Part II: Bad Paper = Better Way to go (DO IT!)

- ❑ Do **opposite** of Bad Paper commandments
 - Define terms, distinguish “will do” vs. “have done”
 - Mention drawbacks, real performance, and reference other papers
- ❑ Find related work via IEEE/ACM/INSPEC online search/paper retrieval vs. WWW only
- ❑ First read Strunk and White, then follow these steps;
 1. 1-page paper outline, with tentative page budget/section
 2. Paragraph map
 - ❑ 1 topic phrase/sentence per paragraph, handdrawn figures with captions



Part II: Bad Paper = Better Way to go (DO IT!)

3. (Re)Write draft

- Long captions/figure can contain details ~ Scientific American
- Uses Tables to contain facts that make dreary prose

4. Read aloud, spell check & grammar check

(MS Word; Under Tools, select Grammar, select Options, select “technical” for writing style vs. “standard”; select Settings and select)

5. Get feedback from friends and critics on draft; go to 3.

Part II: Bad Talk = Better Way to go (DO IT!)

- ❑ Do opposite of Bad Talk commandments
 - I. Thou shalt not illustrate, II. Thou shalt not covet brevity.
 - III. Thou shalt not print large, IV. Thou shalt not use color.
 - V. Thou shalt cover thy naked slides, VI. Thou shalt not skip slides in a long talk, VII. Thou shalt not practice.
- ❑ Explanation also required with detailed figures
- ❑ Allocate 2 minutes per slide, leave time for questions
- ❑ Don't over animate
- ❑ Do dry runs with critics for feedback, including tough questions
- ❑ Tape a practice talk (audio tape or video tape)

Part III: Key Advice on Alternatives to a Bad Career

6 Step to Success in Academic Research

- Goal is to have impact:

Change way people do Computer Science & Engineering

BUT Academics have bad benchmarks: **published papers**

- 1) Selecting a problem
- 2) Picking a solution
- 3) Running a project
- 4) Finishing a project
- 5) Quantitative Evaluation
- 6) Transferring Technology

1) Selecting a Problem



Invent a new field & stick to it?

- ❑ **No!** Do “Real Stuff”: solve problem that someone cares about
- ❑ **No!** Use separate, short projects
 - Always takes longer than expected
 - Matches student lifetimes
 - Long effort in fast changing field?
 - Learning: Number of projects vs. calendar time; I’m on 9th project?
 - If going to fail, better to know soon

1) Selecting a Problem (cont.)



- ❑ Strive for multi-disciplinary, multiple investigator projects
 - 1 expert/area is ideal (no arguments)
- ❑ Match the strengths and weaknesses of local environment
- ❑ Make sure you are excited enough to work on it for 3-5 years
 - Prototypes can be exciting

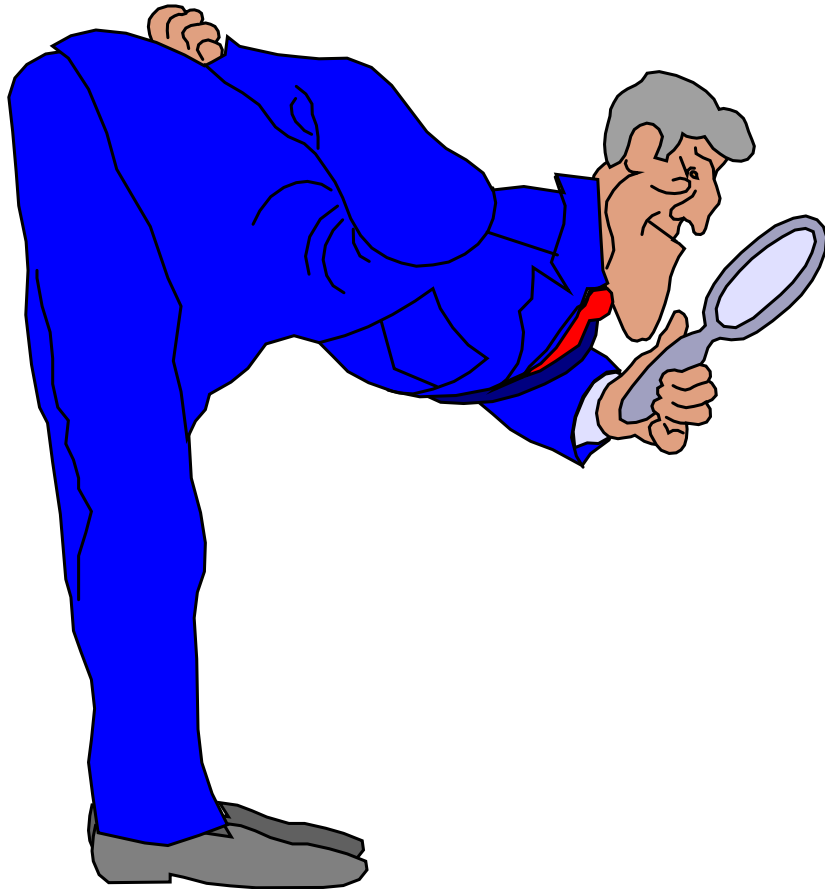
2) Picking a solution



Let Complexity Be Your Guide?

- ❑ **No!** Keep things simple unless a very good reason not to
 - Pick innovation points carefully, and be compatible everywhere else
 - Best results are obvious in retrospect (simple <> easy)
“Anyone could have thought of that”
- ❑ Complexity cost is in longer design, construction, test, and debug
 - Fast changing field + delays
→ less impressive results

2) Picking a solution



Use the Computer Scientific Method?

- ❑ **No!** Run experiments to discover real problems
- ❑ Use intuition to ask questions, not answer them

(And Pick A Good Name!)

Reduced

Instruction

Set

Computers

Redundant

Array of

Inexpensive

Disks

Intelligent

Random

Access

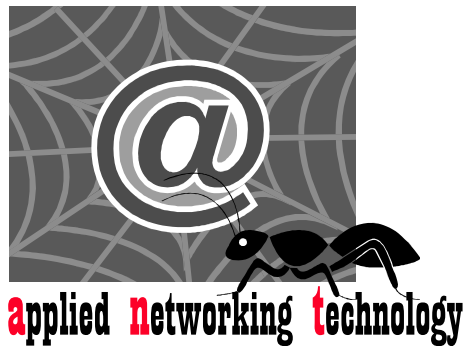
Memory

(And Pick A Good Name!)

Reduced
Instruction
Set
Computers

Redundant
Array of
Inexpensive
Disks

Intelligent
Random
Access
Memory



Appled **N**etworking **T**echnology **LAB** @CSKKU

3) Running a project



Avoid Feedback?

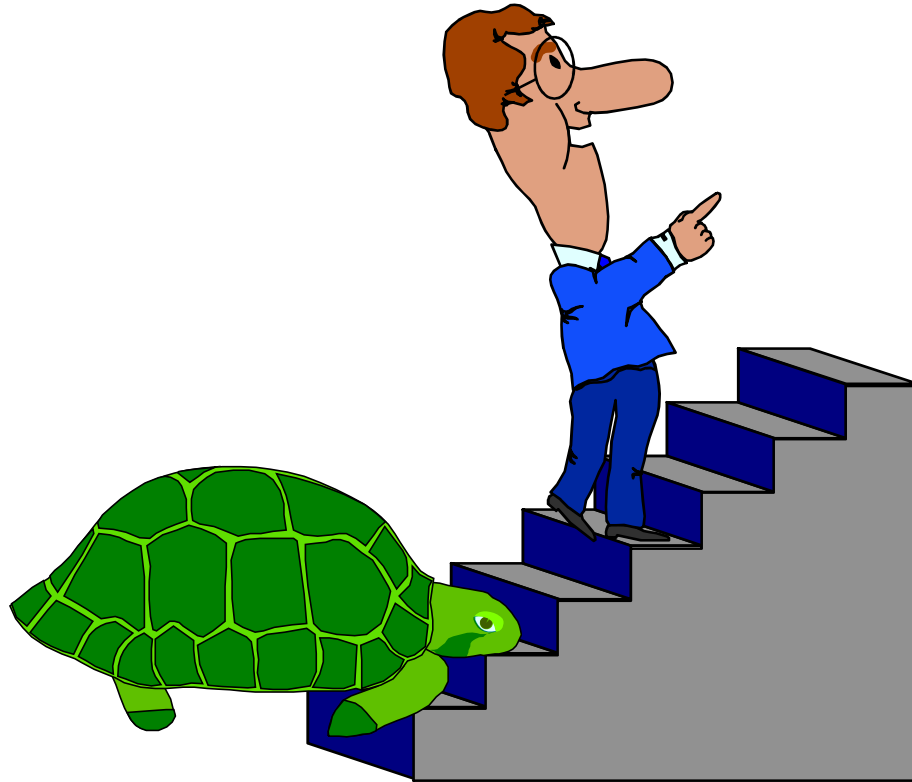
- ❑ **No!** Periodic Project Reviews with Outsiders
 - Twice a year: 3-day retreat faculty, students, staff + [guests](#)
 - Key piece is feedback at end
 - Helps create deadlines
 - Give students chance to give many talks, interact with others industry

3) Running a project (cont.)



- ❑ Consider mid-course correction
 - Fast changing field & 3-5 year projects → assumptions changed
- ❑ Pick size and members of team carefully
 - Tough personalities are hard for everyone.
 - Again, 1 faculty per area reduces chance of disagreement

4) Finishing a project

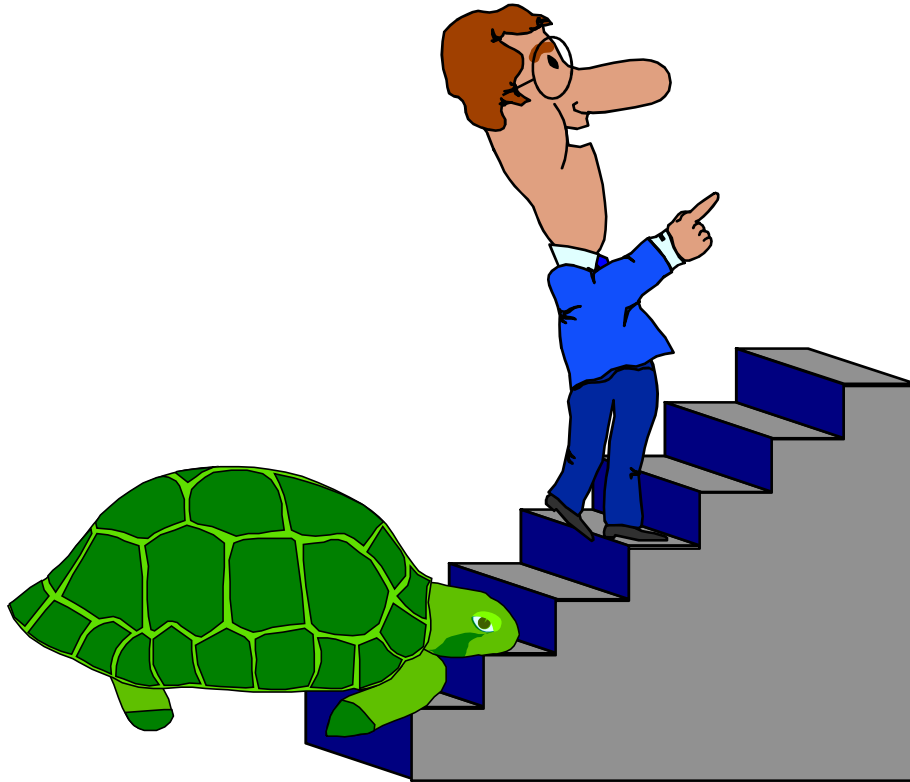


- ❑ People count projects you finish, not the ones you start
- ❑ Successful projects go through an unglamorous, hard phase
- ❑ Design is more fun than making it work.

“No winners on a losing team;
no losers on a winning team.”

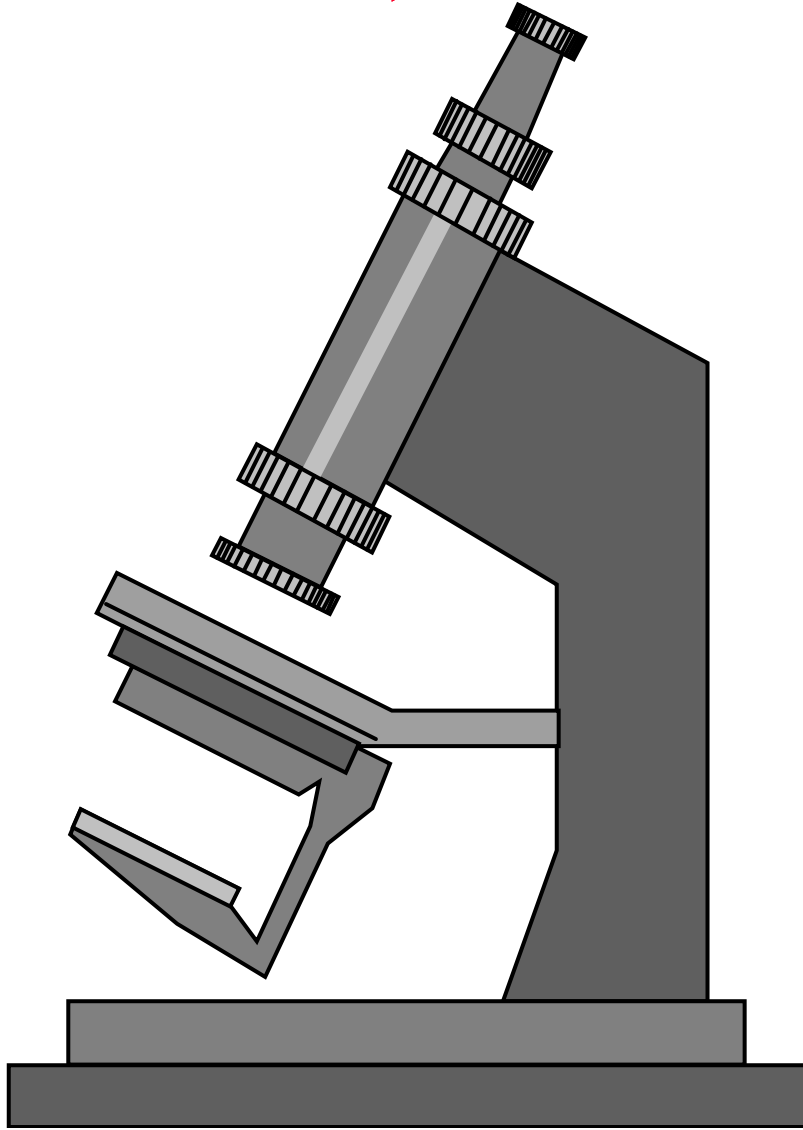
“You can quickly tell whether or
not the authors have ever built
something and made it work.”

4) Finishing a project (cont.)



- ❑ Reduce the project if its late
“Adding people to a late project makes it later.”
- ❑ Finishing a project is how people acquire taste in selecting good problems, finding simple solutions

5) Evaluating Quantitatively



Never be Proven Wrong?

- ❑ **No!** Keep things simple unless a very good reason not to
- ❑ **No!** If you can't be proven wrong, then you can't prove you're right
- ❑ Report in sufficient detail for others to reproduce results
 - Can't convince others if they can't get same results
- ❑ For better or for worse, benchmarks shape a field
- ❑ Good ones accelerate progress good target for development
- ❑ Bad benchmarks hurt progress

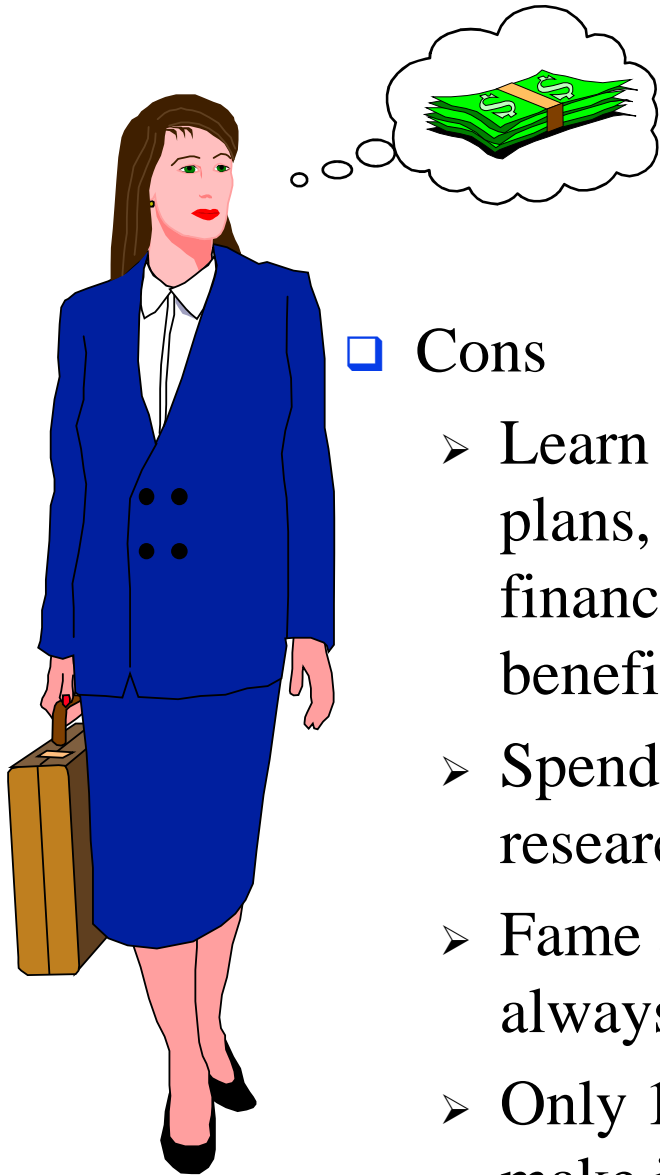
6) Transferring Technology

Publishing Journal Papers IS
Technology Transfer?



- ❑ **No!** Missionary work: “Sermons” first, then they read papers
- ❑ Selecting problem is key: “Real stuff”
 - Ideally, more interest as time passes
 - Change minds with believable results
- ❑ Need 1 bold company (often not no. 1) to take chance and be successful : RISC with Sun, Then rest of industry must follow

6) Transferring Technology (cont.)



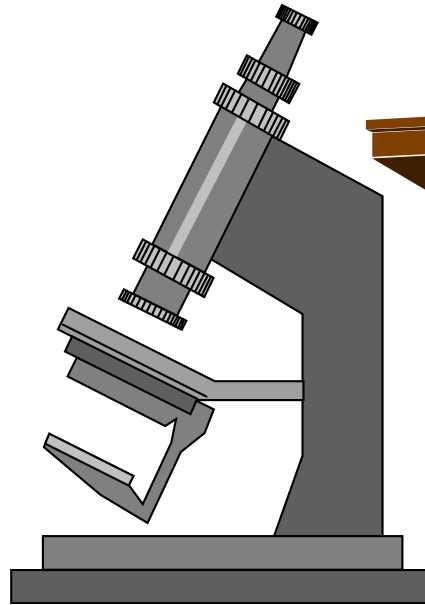
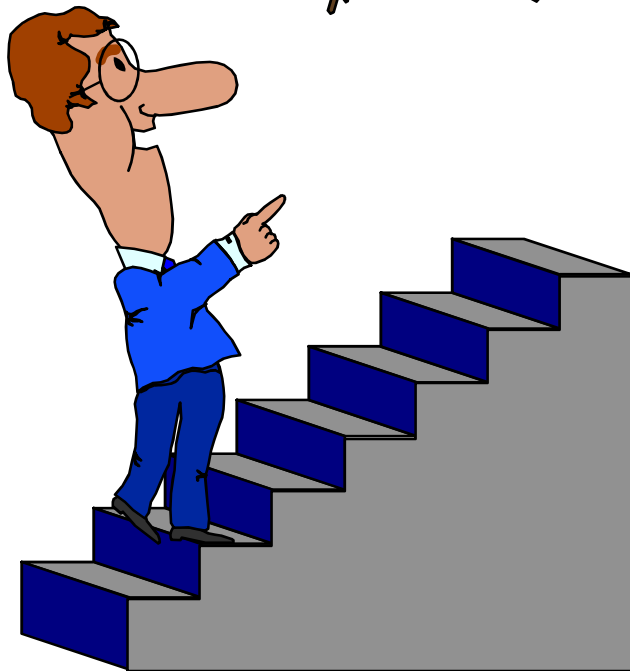
□ Cons

- Learn about business plans, sales vs. marketing, financing, personnel benefits, hiring, ...
- Spend time doing above vs. research/development
- Fame also if company not always successful
- Only 10% of startup really make it

□ Pros

- Personal satisfaction: seeing your product used by others
- Personal \$\$\$ (potentially)
- Fame

Summary: Leader's Role Changes during Project



Acknowledgments

- Many of these ideas were borrowed from (inspired by?) Tom Anderson, David Culler, Al Davis, John Hennessy, Steve Johnson, John Ousterhout, Bob Sproull, Carlo Séquin and many others

Conclusion: Alternatives to a Bad Career

- ❑ Goal is to have impact:
 - Change way people do Computer Science & Engineering*
 - Many 3 - 5 year projects gives more chances for impact
- ❑ Feedback is key: seek out & value critics
- ❑ Do “**Real Stuff**”: make sure you are solving some problem that someone cares about
- ❑ Taste is critical in selecting research problems, solutions, experiments, & communicating results; acquired by feedback
- ❑ Your real legacy is people, not paper:
 - Create environments that develop professionals of whom you are proud
- ❑ *Students* are the coin of the academic realm

Thank you and Questions?

