CSC 595 - Research Skills

Revolutionaries
(A Case Study in Great Researchers)

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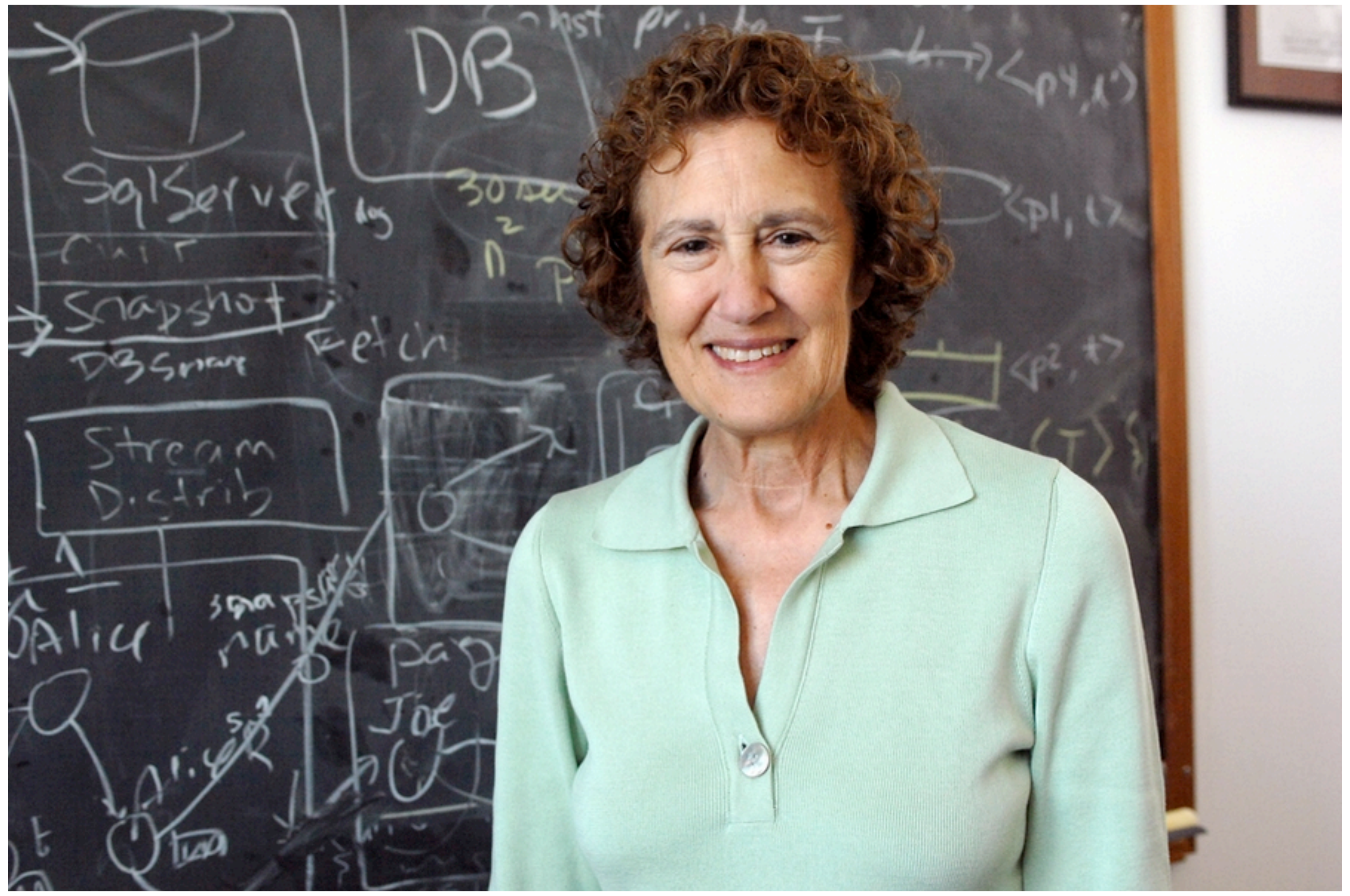
*For some structure and content, credit goes to
Nick Feamster and Alex Gray: https://noise-lab.net/research-course/

Barbara Liskov



- 2008 Turing Award
- Known for:
    - Abstract data types
    - work that led to object-oriented programming
    - Byzantine fault tolerance

[Talk: Finding the Great Problems]

Research Path

"I didn't have a plan for where I was going. Instead I reacted to obstacles and opportunities. I believe that some of this was due to being a woman. I focused on work that was interesting, but expected to stop working when I had a family."

"I got into research in software systems and realized that **I was really committed to my work and would not give it up**. Later, when my husband and I had a family, I continued to work full time."

How to Select Problems

“When I saw that programming methodology problem, it was clear that was a hugely important problem. When you can see a problem that is hugely important, that’s a great thing to work on.”

“don’t do incremental work”

“I've changed research areas over time. The common thread has been **my interest in working on problems whose solutions were needed at the time**.”

“There were points in my career where I had to choose **whether to go on to a new research area** or spend time solidifying and selling earlier results. **I always chose to go on, and I don't regret that**.”

“It's good to question the assumptions that others make. This can often lead insights into better ways of doing things.”

How to Solve Problems

Thinking without thinking: “the way I have always done my research is to focus very hard during the day, and then I stop and go home in the evening and I don’t work... the thought I was doing during the day is in my subconscious”

Liskov would often see a solution in the morning the next day when driving to work

Paying Attention to Your Knowledge and Your Environment

Quoting advice from Nobel laureates:

"You pay a lot of attention to what's going on around you. Every time you read a paper or listen to a talk, you **ask questions: what didn't they do? what's wrong? what could be better? That way you find things that are directions to go on**."

"I also got **very good at understanding what I didn't know**, which is almost more important as knowing what you do know because then you can see where the holes are in your reasoning"

Ken Thompson

- 1983 Turing Award
- Known for:
  - UNIX
  - C programming language
  - Belle
    - first machine to achieve master level play
    - won 1980 World Computer Chess Championship

Style of Thinking

“I am **a very bottom-up thinker. If you give me the right kind of Tinker Toys, I can imagine the building**. I can sit there and see primitives and recognize their power to build structures a half mile high, if only I had just one more to make it functionally complete. I can see those kinds of things.”

“The converse is true, too, I think. **I can’t—from the building—imagine the Tinker Toys. When I see a top-down description of a system or language that has infinite libraries described by layers and layers, all I just see is a morass. I can’t get a feel for it.** I can’t understand how the pieces fit; I can’t understand something presented to me that’s very complex. Maybe I do what I do because if I built anything more complicated, I couldn’t understand it. I really must break it down into little pieces.”

Who to Seek Out

"Occasionally—maybe once every five years—**I will read a paper and I'll say, 'Boy, this person just doesn't think like normal people**. This person thinks at an orthogonal angle.' When I see people like that, **my impulse is to try to meet them, read their work, hire them**. It's always good to take an orthogonal view of something. **It develops ideas**."

On Arguments and Ideas

"When you know something is systemically wrong despite all the parts being correct, you say there has to be something better. You argue back and forth. You may sway or not sway, but mostly what you do is come up with an alternative. Try it. Many of the arguments end up that way. You say, "I am right, the hell with you." And, of course the person who has been "to helled with" wants to prove his point, and so he goes off and does it. That's ultimately the way you prove a point. So that is the way most of the arguments are done—simply by trying them."

"I have certainly generated as many bad ideas as I have good ones."

The UNIX Revolution

What accounted for the success of UNIX, ultimately?

“I mostly view it as serendipitous. **It was a massive change in the way people used computers**, from mainframes to minis; we crossed a monetary threshold where computers became cheaper. People used them in smaller groups, and it was the beginning of the demise of the monster comp center, where the bureaucracy hidden behind the guise of a multimillion dollar machine would dictate the way computing ran. People rejected the idea of accepting the OS from the manufacturer…"

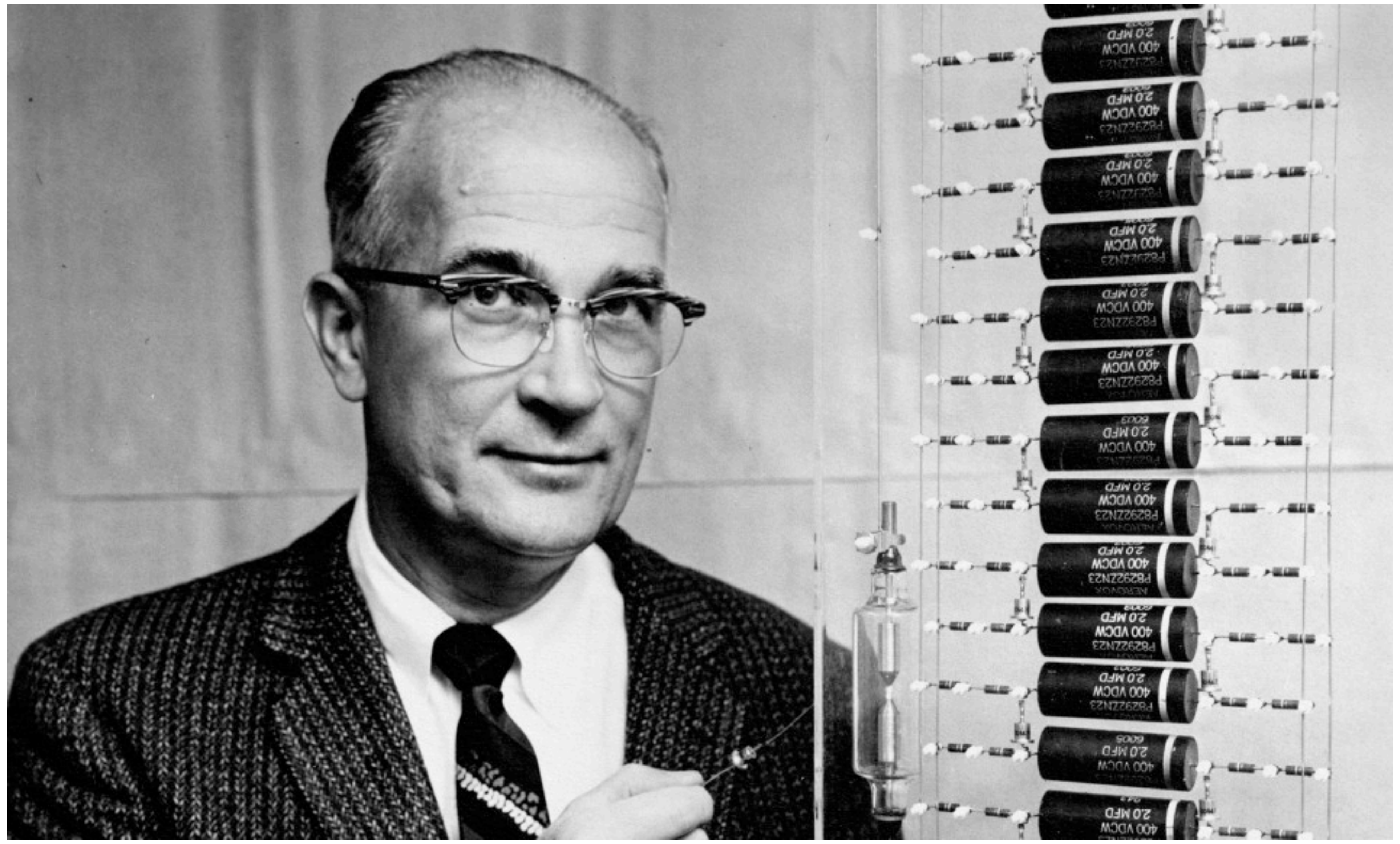
Perseverance… and Luck

"It's hard to give advice in a product kind of world when what I do, I guess, is some form of computer Darwinism: **Try it, and if it doesn't work throw it out and do it again.**"

"Plus I am not sure there are real principles involved as opposed to serendipity: You happened to require this as a function before someone else saw the need for it. The way you happen upon what you think about is just very lucky. **My advice to you is just be lucky.** Go out there and buy low and sell high, and everything will be fine."

William Shockley

- 1956 Nobel Prize in Physics
- Known for:
    - Transistor
    - (also some bad things later in life, sadly)



On Practically-Motivated Research

“**The objective of producing useful devices has strongly influenced the choice of the research projects** with which I have been associated. It is frequently said that having a more-or-less specific practical goal in mind will degrade the quality of research. I do not believe that this is necessarily the case and to make my point in this lecture I have chosen my examples of the new physics of semiconductors from research projects which were very definitely motivated by practical considerations.”

On the Nature of Fundamental Research

"…I would like to express some viewpoints about words often used to classify types of research in physics; for example, **pure, applied, unrestricted, fundamental, basic, academic, industrial, practical, etc**. It seems to me that **all too frequently some of these words are used in a derogatory sense**, on the one hand to belittle the practical objectives of producing something useful and, on the other hand, to brush off the possible long-range value of explorations into new areas where a useful outcome cannot be foreseen.

Frequently, **I have been asked if an experiment I have planned is pure or applied research; to me it is more important to know if the experiment will yield new and probably enduring knowledge about nature**. If it is likely to yield such knowledge, it is, in my opinion, good fundamental research; and this is much more important than whether the motivation is purely esthetic satisfaction on the part of the experimenter on the one hand or the improvement of the stability of a high-power transistor on the other."

On Fostering Creativity

“least effective in encouraging creativity is to give the impression — that the student so often gets in school — that all of the nice things, the important things, are found out nicely and neatly…
…all of the more difficult inventions I’ve made… require many failures to accomplish… **to do creative work, one must over-extend oneself, one must count on falling on his face, on getting into difficulties, one must learn from these failures and not be stopped by them**.

“But if one is taught there everything is neat and orderly and one never gets into a mess when trying to do anything new, then he will be so conservative that I don’t think he will break new ground”

“**the biggest educational contribution that can be made to the creativity of people is to persuade them that they shouldn’t worry about making mistakes**”

Richard Hamming

- 1968 Turing Award
- Known for:
  - Error-correcting codes (first one: Hamming code)
  - Hamming distance
  - Hamming window

Compounding Gains from Hard Work

Hamming’s description of explanation from his boss Hendrik Bode:

“What Bode was saying was this:
‘Knowledge and productivity are like compound interest.’

Given two people of approximately the same ability and one person who works ten percent more than the other, the latter will more than twice outproduce the former.

The more you know, the more you learn; the more you learn, the more you can do; the more you can do, the more the opportunity - it is very much like compound interest.

I don't want to give you a rate, but it is a very high rate. **Given two people with exactly the same ability, the one person who manages day in and day out to get in one more hour of thinking will be tremendously more productive over a lifetime.**”

On Luck

"…**people think great science is done by luck.** It's all a matter of luck. **Well, consider Einstein. Note how many different things he did that were good. Was it all luck? Wasn't it a little too repetitive? Consider Shannon.** He didn't do just information theory. Several years before, he did some other good things and some which are still locked up in the security of cryptography. **He did many good things.**"

"You see **again and again, that it is more than one thing from a good person.** I claim that luck will not cover everything. And I will cite **Pasteur who said, 'Luck favors the prepared mind.'** … The prepared mind sooner or later finds something important and does it. So yes, it is luck. The particular thing you do is luck, but that you do something is not."

A Treasure Trove of Advice on How to do Great Research

Richard Hamming

``You and Your Research''

Transcription of the
Bell Communications Research Colloquium Seminar
7 March 1986

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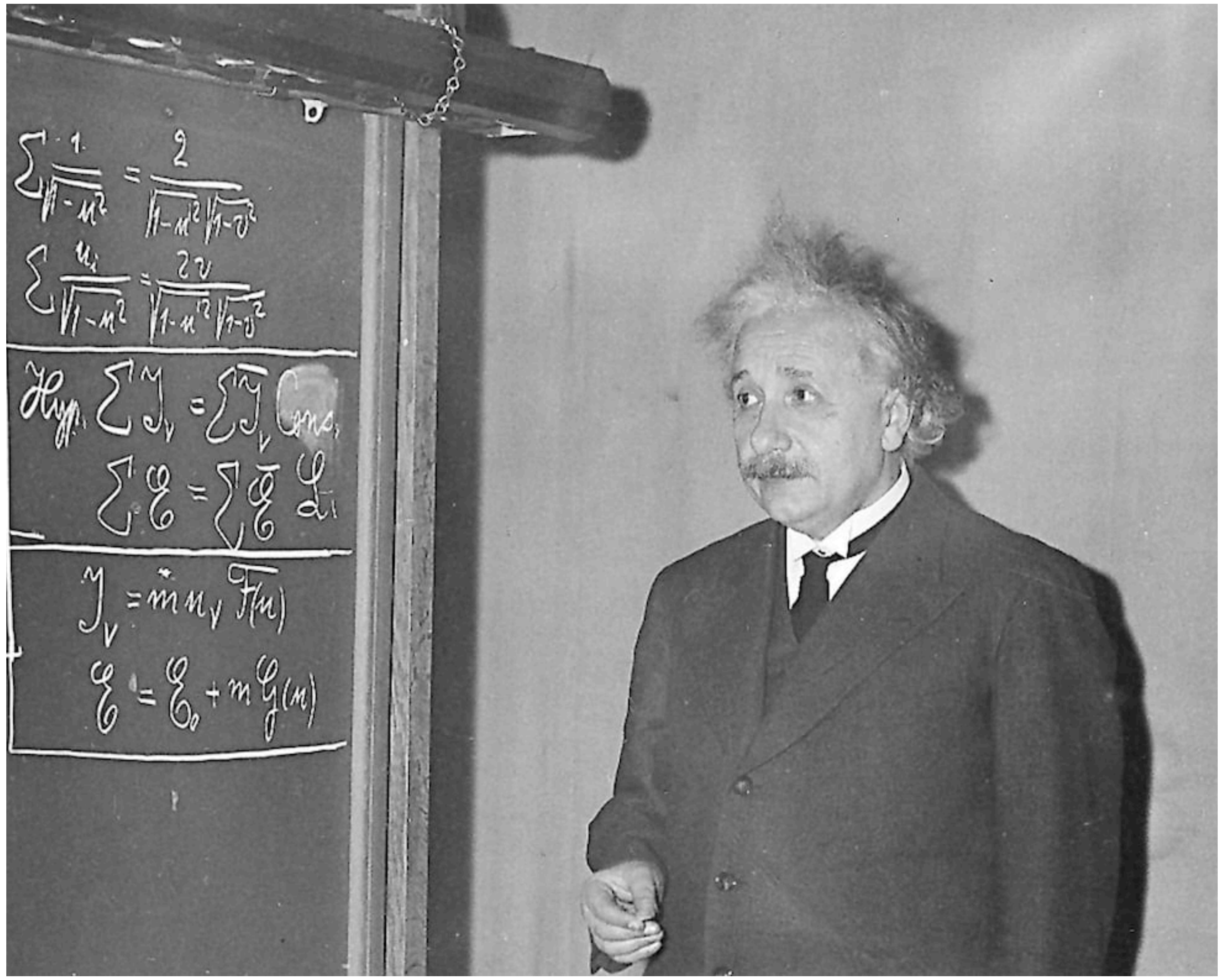
Albert Einstein

- 1921 Nobel Prize in Physics
- Known for:
  - Relativity
  - Mass-energy equivalence
  - Planck–Einstein relation
  - fundamental work on Brownian motion
  - unified field theory (not successful…)
  - many more successes



Working Style

- Hard work
  - Over 300 publications
  - Patent clerk position gave him undistracted time
  - Collapsed from exhaustion at one point
- Very wide knowledge of physics
  - Connected many pieces of physics, resolved many anomalies of physics
- Discussion with others
  - Formed a weekly discussion group on science and philosophy, early in his career
  - Enlisted mathematicians to complete his theory

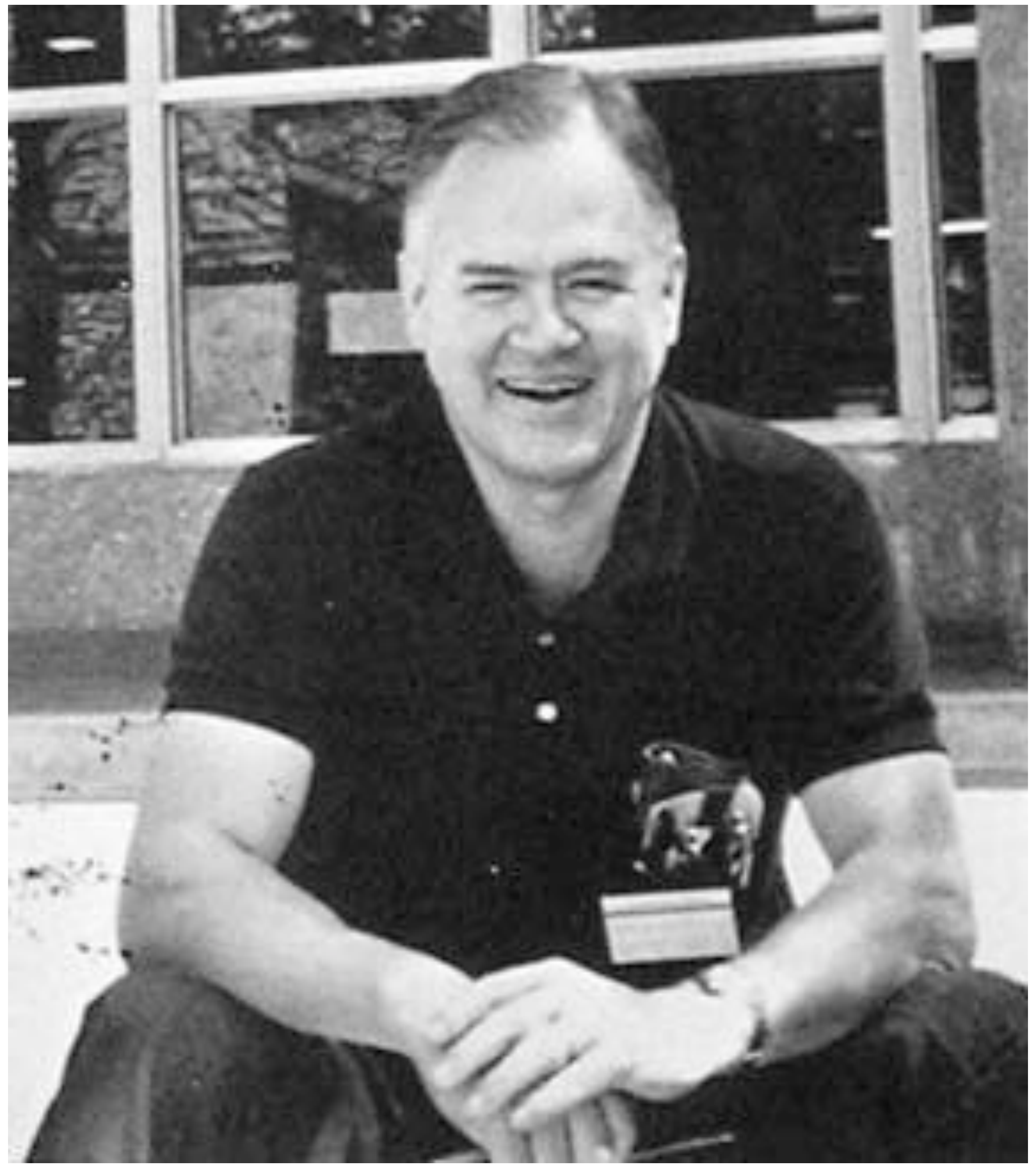
Working Style

- Not afraid to take risks
  - Was wrong a few times
- Believed in himself, not the establishment
  - Couldn’t get a job at first
  - Was initially met with great resistance, though a few top scientists supported him

Problem Selection

- Started with fundamental problems of the time
  - At first, wanted to show that atoms exist
- Was driven by explaining experimental results and anomalies, as well as theoretical inconsistencies
- Was not limited by sub-field boundaries
  - His quest led him to thermodynamics, statistical physics, specific heats of solids…

John Tukey



- 1982 IEEE Medal of Honor
- Claim to fame:
  - Fast Fourier Transform
  - Spectral analysis
  - Exploratory Data Analysis (subfield of statistics)
  - Jackknife (fundamental statistical method)
  - Projection pursuit
  - Box plot
  - “bit”

Working Style

- Hard work
  - Over 600 publications
  - Held three jobs at same time (prof, AT&T, govt)
- Highly collaborative
  - 127 co-authors
- Generous with his time and ideas

Working Style

- Communicated ideas well
  - Coined many memes: “bit”, “software”, …
- Had a habit of questioning assumptions and back-and-forth debate rather than giving answers

Problem selection

- Changed problems and even fields opportunistically
  - Chemistry → topology → statistics
- Driven by real problems (war-time, govt, etc) and also theoretical problems
- Worked successful on both large-scale and small-scale problems

Shafi Goldwasser

- 2021 Turing Award
- Known for:
  - Zero-knowledge proofs
  - Interactive proofs
  - Probabilistic encryption
  - Blum–Goldwasser cryptosystem
  - Goldwasser–Micali cryptosystem

The Importance of a Story

“And in general I think people have an easier time to read, especially in a new field where there it isn’t a mathematical problem that’s been defined for many years and that people are interested in and they don’t need any motivation. **In a new field, you need to compel people, and stories are helpful.**”

Do it because it’s interesting. Applications might only emerge later

“But some of the reactions they got is that “What is the application?” And they came to me and they asked me if they should work on it, they shouldn’t work on it, what’s my opinion, is it interesting? **I said, “It’s very interesting.” It’s intellectually interesting. They had a beautiful sort of approach to it. They had a beautiful proof. And at the end, that’s the nugget, right? It’s sort of something that captivates you, you have to use some ingenuity to solve it**, and you have insight. And **if it’s important, even for applications, it will emerge, but it’s not necessarily obvious in the moment that you start**. And sometimes if it is very obvious, first of all lots of people work on it, and you know competition is good but only to a certain extent. **If everybody’s working on the same problem**, there’s some kind of … I don’t know. **I don’t like to be in a space that’s very crowded**.”

Surviving grad school

"…crisis of becoming a graduate student. That was again a time which was extremely difficult, because you're trying to do something new, you're trying to do it on your own, **you are always comparing yourself to the people around you who are always brilliant**, and more brilliant than you are, and **you don't know that they're all feeling the same thing. You know this imposter feeling?** Apparently they're all feeling it. Some of them admit it, some of them don't admit it. But once you realize that this is the name of the game, I think again it's these moments of realization."

Surviving grad school

“You know, I decided to leave Carnegie-Mellon where I had lots of friends and just kind of conquer this new place totally on my own. I remember going through this cycle again and again and again, and then I had this realization that okay, maybe it’s all true. **Maybe I will amount to nothing and maybe I know nothing, and maybe I’m a failure. But if I’m going to be against myself and I’m not going to be my own friend, then who else? I’m going to have to like myself whatever I am. I got to accept that. And somehow that was like a very kind of deep, decisive moment, that from then on, everything became better.** Because I think it’s very important to realize that for graduate students especially, which have moments like this, I’m sure it’s universal, where you go, you’ve decided on this big adventure, and then it’s very unclear, right? Are you going to succeed? Are you not going to succeed? **There’s a lot of competition. Everybody seems better than you. And there’s a I think tendency for self-beating, at least for some people, and it’s very important to realize that it is what it is, you know you got to like yourself.**”

The role of serendipity

Decided to go to CMU for grad school

In between decision and starting, had a summer at RAND in Santa Monica.

Lived in Venice Beach, captivated by the scene: beach, roller skaters, bikers

All supervisors had PhDs, told her what to do. Thought “Why should they tell me what to do? I should get a PhD and I should tell somebody else what to do.” (jokingly)

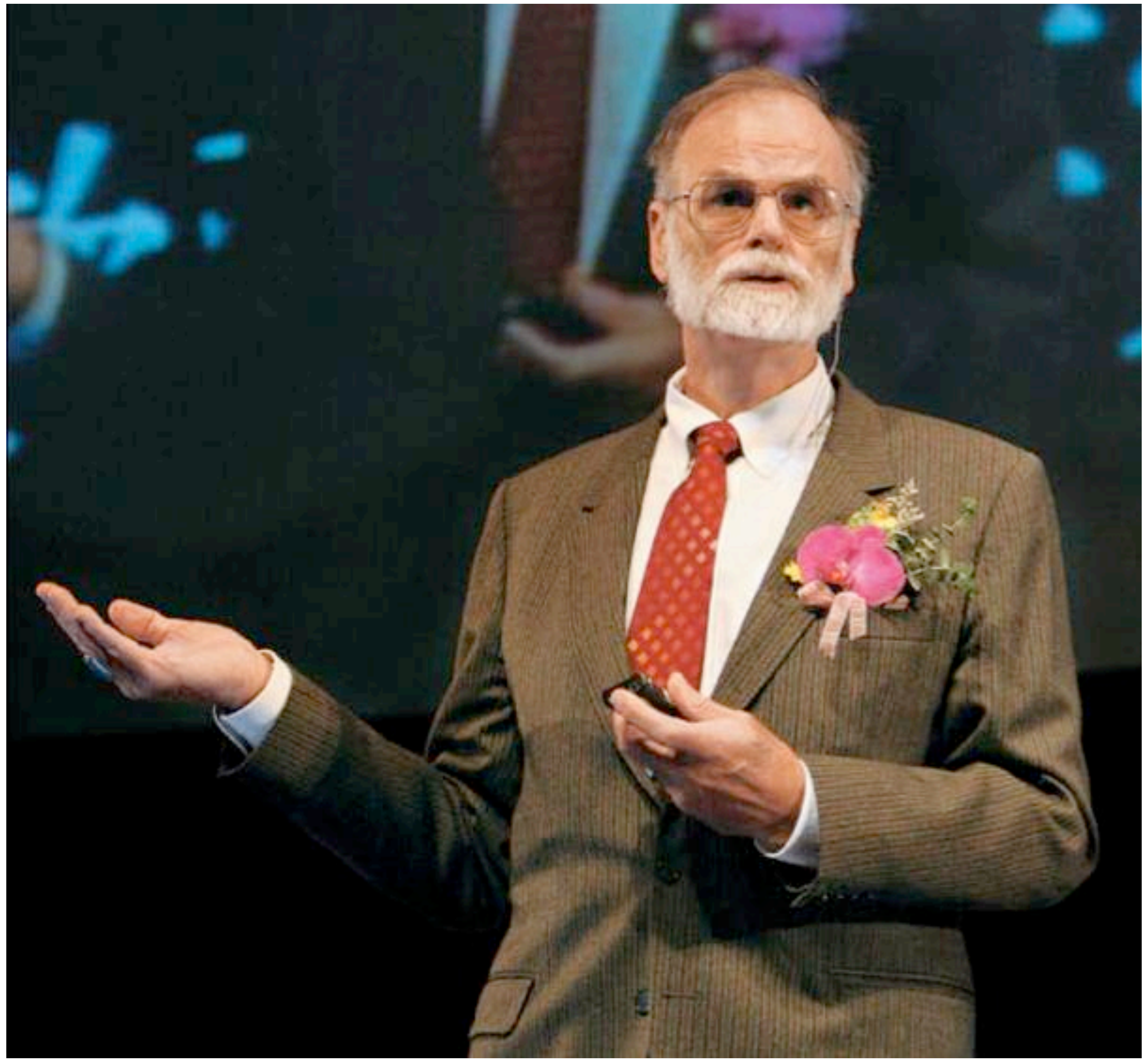
Captivated by scenery around Berkeley.

Switched grad school to decision to Berkeley

Was advised by Manuel Blum

Jim Gray

- 1998 Turing Award
- Known for:
  - Databases
  - Transaction processing
  - Putting astronomy data into database systems
    - Helped make Sloan Digital Sky Survey possible
- Tragically lost at sea in 2007



The Importance of Long Term View

“Some have lost sight of the fact that most of the cyberspace territory we are now exploiting was first explored by IT pioneers a few decades ago. Those prototypes are now transforming into products.”

“The gold rush mentality is casing many research scientists to work on near-term projects that might make them rich, rather than taking a longer term view. Where will the next generation get its prototypes if all the explorers go to startups? Where will the next generation of students come from if the faculty leave the universities for industry?”

What Makes a Good Long Range Research Goal?

Before presenting my list, it is important to describe the attributes of a good goal. A good long-range goal should have five key properties:

Understandable: The goal should be simple to state. A sentence, or at most a paragraph should suffice to explain the goal to intelligent people. Having a clear statement helps recruit colleagues and support. It is also great to be able to tell your friends and family what you actually do.

Challenging: It should not be obvious how to achieve the goal. Indeed, often the goal has been around for a long time. Most of the goals I am going to describe have been explicit or implicit goals for many years. Often, there is a camp who believe the goal is impossible.

Useful: If the goal is achieved, the resulting system should be clearly useful to many people -- I do not mean just computer scientists, I mean people at large.

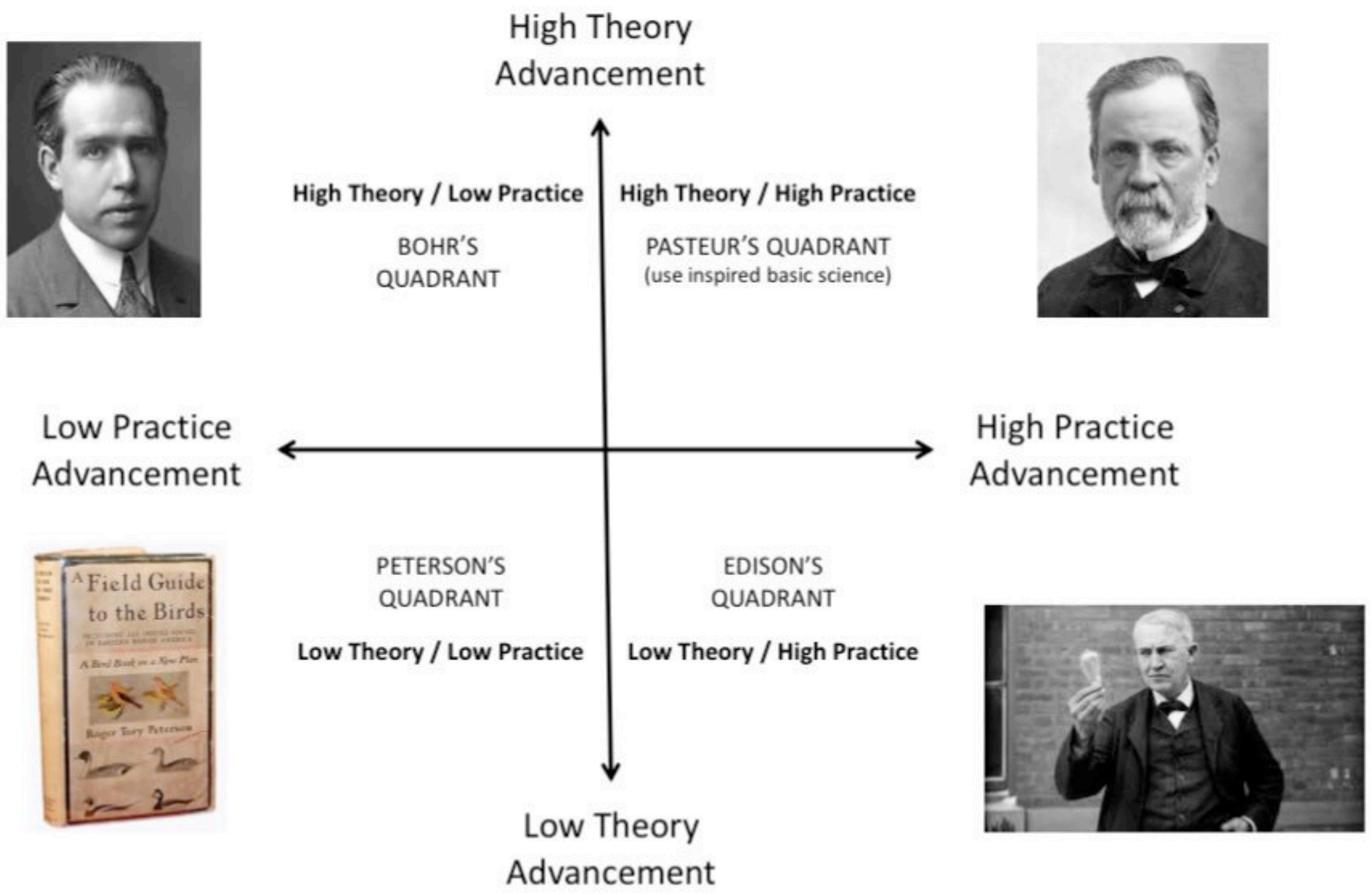
Testable: Solutions to the goal should have a simple test so that one can measure progress and one can tell when the goal is achieved.

Incremental: It is very desirable that the goal has intermediate milestones so that progress can be measured along the way. These small steps are what keep the researchers going.

Pasteur's Quadrant

Jim Gray claimed that a lot of fundamental research that led to billion dollar industries falls in Pasteur's Quadrant

Note: which quadrant research falls into depends on motivation rather (what drove the research) rather than eventual outcomes



In Memoriam

- Michael Stonebraker: “Jim was obviously brilliant, as anyone who talked to him quickly realized. However, he also read widely and knew a lot about a lot of things. In fact, he is one of the few people I have found to be intellectually intimidating. Moreover, he was always willing to read papers that other researchers sent him and offer insightful comments. I routinely sent him my work in draft form and was always amazed by the breadth of knowledge reflected in his comments. They usually took the form: "Have you looked at System XYZ?; the people behind it looked at the problem you are considering." XYZ would, of course, be an effort I had never heard of.”

- “He refused to conform to social norms; we never saw him wearing a coat and tie. He was an unmanageable free spirit in the workplace who could write prodigious amounts of code and even more prodigious research reports. It was reported at the Tribute that he had asked IBM to transfer him from its Thomas J. Watson Jr. Research Laboratory in Yorktown, NY, to its San Jose Research Laboratory in California to work on System R. When his boss refused, Jim quit on the spot and drove cross-country to be hired by the San Jose Lab.”

- Gordon Bell: “Jim is rare, ‘up and to the right,’ in the quadrant where research is inspired both by a quest for fundamental understanding and considerations for use.”