

Strategic Thinking for Researchers

Andy Gordon

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Based in part on the MSRC Impact Workshop 2010,
co-organised by Thore Graepel,
with material by Bob Williamson

Strategic Thinking for Researchers

- You can write great papers, give great talks – what next?
- This talk: some thoughts on long-term goals and strategies
- If research is your life, learn strategy as well as execution
- No claim of originality; compendium of various sources
- No rigorous theory; no empirical evaluation; no usability study
- There is no one correct strategy, but there are commonalities
- I don't even claim anyone follows all this advice all of the time
- I don't feel especially qualified, except...

Background: Impact Workshop 2010



Making a Difference, by Research

Impact Workshop 2010

Microsoft Research Cambridge, 8 November, 1-6pm

Organised by Andy Gordon and Thore Graepel,
with Louise de Caux

Goal

To gather as a community to consider the question:

How to have impact, through research?

We hope that the afternoon will:

- Make us stop and think how our work can change the world.
- Get all of us talking about how to do that.
- Generate tips for how we can set bigger goals, be more effective.

Organising it was so much fun it almost didn't feel like work...



**MSR Speed
Dating Society
5.5.11**

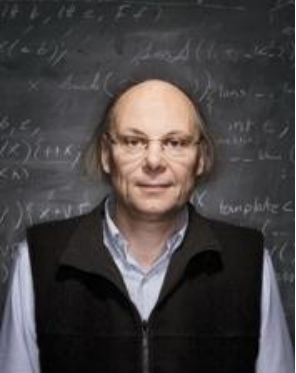
MSR Speed Dating Society 5.5.11

We believe social links lay foundations for eventual transfer of expertise and for serendipitous collaborations between groups, but the immediate goal is simply to spend 90 minutes surprising each other!

- **24 attendees: each had one minute “surprise on a slide”**
- **8 “speed dates” 5 mins each**
- **Followed by wine and nibbles**



Know what you are trying to do

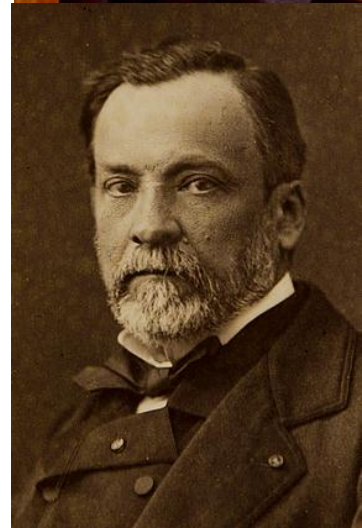


The most important single aspect of software development is to be clear about what you are trying to build.

– Bjarne Stroustrup

Research is what I'm doing when I don't know what I'm doing.

- Wernher Von Braun



- But you can know what you are *trying* to do
- A job interview question (2 parts):
 - What's the most important problem in your field?
 - *What are you working on?*
- Serendipity is real, *but it is not an excuse for not knowing what you are trying to do*
 - *Chance favours the prepared mind*

You've got to work on important problems



- I committed 10% of my time trying to understand the bigger problems in the field, what was important.
- If I really believe the action is over there, why do I march in this direction? So I changed something I did and I marched in the direction I thought was important. It's that easy.

Richard Hamming, “You and Your Research”

- Corollary: don't work on unimportant problems
- Be opportunistic, but don't get carried away

Going All In



- Don Syme led successful MSRC tech transfer
 - .NET Generics (research 1999-2003, “all in” 2002-2004)
 - F# 2.0-3.0 (research 1999-2007, “all in” 2007 on)
 - Required deep trust, total dedication
 - Show respect at all times, win respect by going deep
 - Spot problems, fix them – apply research at the base (language, runtime)
 - Get your hands dirty, treat industry problems as real and serious
 - Align with a shared vision, eg, .NET
- Consider going “all in” with a product team partnership; lots of upside, lots of risks, a guaranteed wild ride

Use the Heilmeier checklist

- **What are you trying to do? [GOAL]**
Articulate your objectives using absolutely no jargon.
- How is it done today,
and what are the limits of current practice?
- What's new in your approach,
and why do you think it will be successful?
- **Who cares?**
- **If you're successful, what difference will it make? [IMPACT]**
- What are the risks and the payoffs?
- How much will it cost?
- How long will it take?
- **What are the midterm and final exams to check [REVIEW]**
(plans, *not* guarantees)



Know Tactics for Creative Ideas

- Go for a walk
- Cultivate hunches
- Write everything down, but keep your folders messy
- Embrace serendipity
- Make generative mistakes
- Take on multiple hobbies
- Frequent coffeehouses and other liquid networks
- Follow the links
- Let others build on your ideas
- Borrow, recycle, re-invent
- Build a tangled bank



http://www.ted.com/talks/steven_johnson_where_good_ideas_come_from.html

Seek criticism



- Better to seek than have imposed!
- Proposals
 - Force you to decide what you want to do
 - Force you to articulate the path to impact
 - Criticism can make a better project

The fundamental problem is to make the mistakes as fast as possible

- Reviews

- Easy to fool yourself; harder to fool peers
- Some failures are good; and plans never survive contact with reality anyway
- Any researcher who has never failed is not pushing the envelope... is not living on the edge
- Researchers *a priori* defensive; *a posteriori* grateful



If all your researchers' projects succeed, you have failed!

Don't be seduced by proxies

- Numbers of papers
- Memberships of program committees
- Citations
- Downloads
- Most people can find measures by which they look good
- *It is harder to fix the proxy up front!*
- Do less, but do it well



Work with the System

- Exploit resources at your disposal
 - Funding bodies, but also “cognitive surplus”
eg the open source community, specialist interest groups, MS Garage
- If you fight the system, pick your battles carefully
 - Hamming: Which do you want to be? The person who changes the system or the person who does first-class science?
- Invite yourself places
 - Boston -> Newhaven -> DC -> Chicago -> Calgary
-> Vancouver -> Berkeley -> New Jersey



Maintaining Balance

- Research jobs are amazing, but demanding, anxious-making
 - Too much to do? Doing well? Nobody tells you what to do



Get out of the lab



*When it started out it was
an awful lot of fun - keep
the fun in the house*



*Identify your principles:
work, family, community*

Five Regrets of the Dying



- I wish I'd had the courage to live a life true to myself, not the life others expected of me.
- **I wish I hadn't worked so hard.**
- I wish I'd had the courage to express my feelings
- I wish I had stayed in touch with my friends
- I wish that I had let myself be happier

- Know what you are trying to do
- You've got to work on important problems
- Go all in
- Use the Heilmeier checklist
- Know tactics for creativity
- Seek criticism
- Don't be seduced by proxies
- Work with the system
- Maintain balance and keep the fun in the house
- Finish things and give them to people!

Resources

- Richard Hamming (Bell Labs, 1986): *You and Your Research*
<http://www.paulgraham.com/hamming.html>
- David Patterson (UCB, 2010): *How to Have a Bad Career in Research/Academia*
<http://www.cra.org/uploads/documents/events/cmw/2010/CMW2010.Patterson.pdf>
- Kathleen Fisher (AT&T Labs, 2010): *Finding Balance*
<http://www.cra.org/uploads/documents/events/cmw/2010/CMW2010.Fisher.pdf>