HOW TO CHOOSE A GOOD RESEARCH TOPIC?

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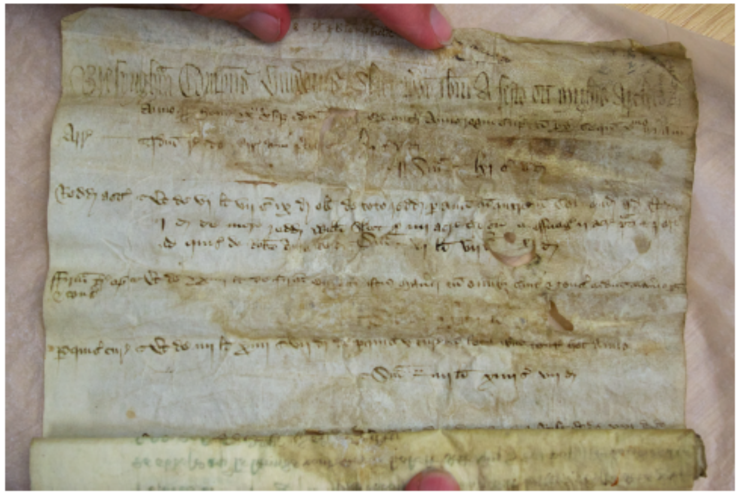
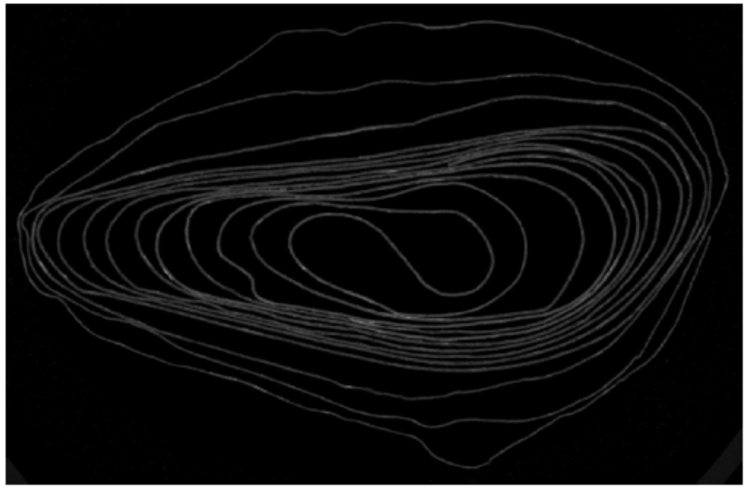
WHY DO YOU WANT TO DO RESEARCH?

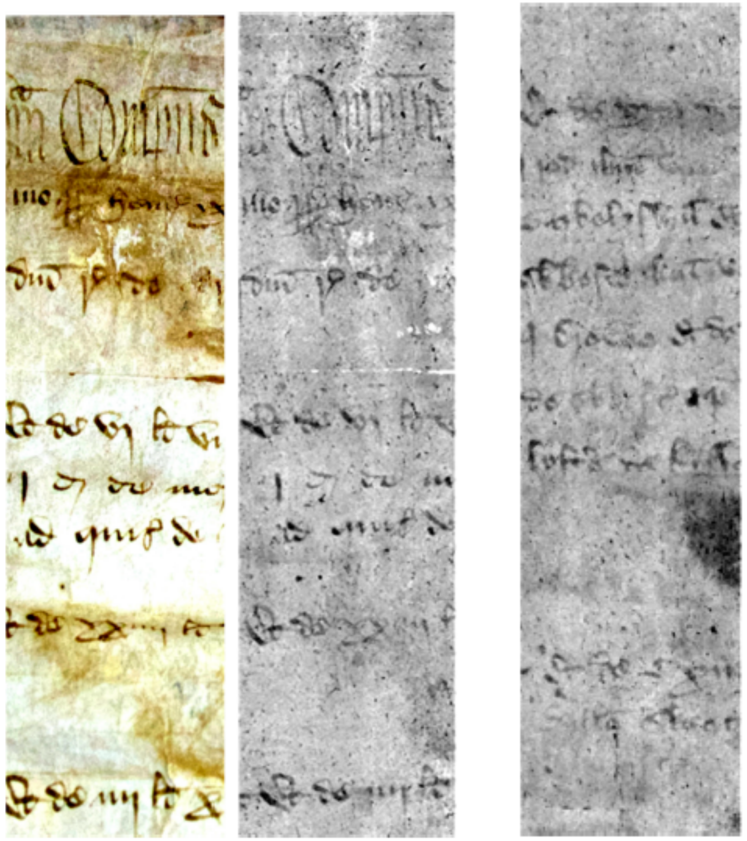
- It is great to do research, because we as humans would like to
  - Know the unknowns
  - Develop technology that makes our life better
- Research is not an easy job
  - Like athletics, only the best that really matters
  - The way to research success can be long, challenging, full of barriers
- Personal reasons
  - Interested in a research career (universities, research labs, or industrial research)
  - Curiosity, enjoying the experience of research, taking challenges
  - Trained in logical thinking & arguments, to solve practical problems
  - A PhD degree

EXAMPLES

- General curiosity: reading unopenable parchments

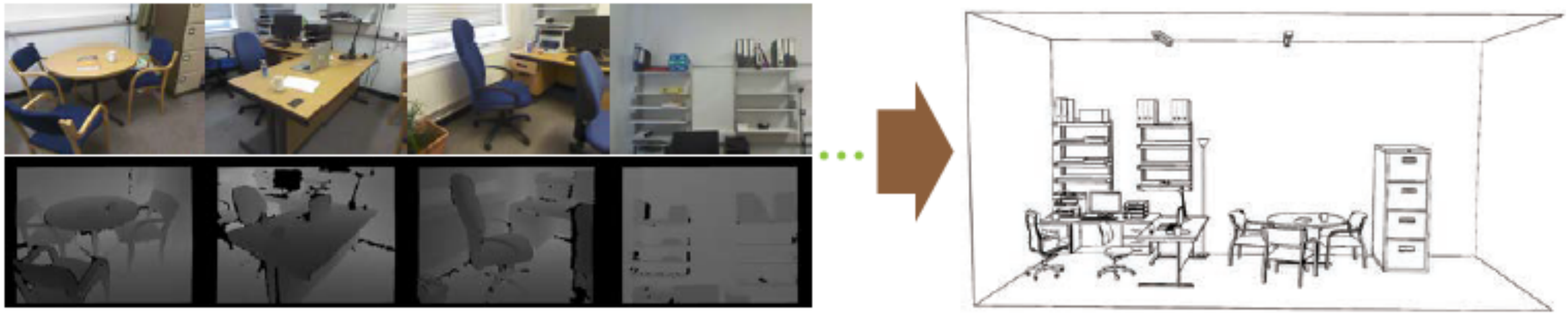






EXAMPLES

- Academic curiosity – anyway to make it better?
  - Can we obtain a semantically meaningful representation for indoor scenes using cheap depth cameras?
  - Existing solutions: require user interaction, or high quality laser scanner
  - Next question: how to achieve this?



CHOOSING A RESEARCH DIRECTION

- To begin with, choose a research direction
  - Why?
    - Limited time & energy
    - Coherent theme for PhD thesis
  - How?
    - PhD supervisor
    - Friends, more experienced students in the lab
    - Discussion with experienced researchers (at conferences etc.)
    - Read surveys on the state of the art
  - What?
    - Your interest
    - Something you are good at (e.g. programming, mathematics)
    - How likely to get a good job
  - Having a good supervisor is important!

KNOWING STATE-OF-THE-ART

- Why?
  - Reinventing wheels won't get a PhD
  - A way of knowing potential applications (why this research is useful)
  - The way of solving problems
  - Useful techniques which can be adapted to new problems
  - Even learning how to write technical papers
- How?
  - Attending conferences/workshops etc.
  - Reading papers
  - Discussion with experts

READING PAPERS

- So many papers published in a year
  - Not practical to read all the papers in detail
  - Papers are of varying importance.
- Knowing the top conferences/journals of the subject
  - Computer Graphics: ACM TOG/IEEE TVCG/SIGGRAPH/SigAsia etc.
  - Computer Vision: ICCV/CVPR/ECCV/IEEE TPAMI/IJCV/etc.
- Good venue ≠ good research
  - Citation numbers
  - Not comparable across subjects
- Google Scholar
  - Finding papers & authors
  - Citations & Metrics

READING PAPERS

- Reading papers in varying levels of details
  - Reading carefully in detail papers that
    - introduce fundamental ideas
    - important techniques
    - directly related to what you would like to do
  - Quick reading
    - getting the main ideas, rather than technical details
    - novel ideas, improvements of results, limitations etc.
    - allows reading 10x more papers

READING PAPERS

- Reading different sections
  - Introduction: motivations, potential applications
  - Related work: overview of research in the field
  - Abstract + Conclusions: summary
- Survey Papers
  - Good for getting a big picture
  - Tell you which are the key papers
  - Can be tricky to understand fully

READING PAPERS CRITICALLY (1)

- Reading papers is not just about understanding what they propose to do
- Reading critically
  - Find out the main novelty, technical advances, limitations and assumptions
  - Are there any concepts/ideas/techniques useful for other problems (with adaptation)?
  - Are there any limitations of the work?
  - Is there any way to circumvent the limitations?

READING PAPERS CRITICALLY (2)

- It is generally more difficult to find out limitations than contributions
  - Most methods have some sort of limitations – which often inspire new research
  - This is not necessarily a problem: the method can still be useful/very useful
  - Analyzing theoretically
    - Is the method general enough? Does the method consider all the possibilities (topology, open boundaries, self-intersections, images with poor contrast, noisy input etc.)?
    - Does the method involve parameters that need tuning?
    - Is the method automatic? If not, is the amount of user interaction reasonable, or necessary?
    - Is the method slow/not fast enough?
  - Sometimes, it benefits from trying the method out, if code is available

READING PAPERS CRITICALLY (3)

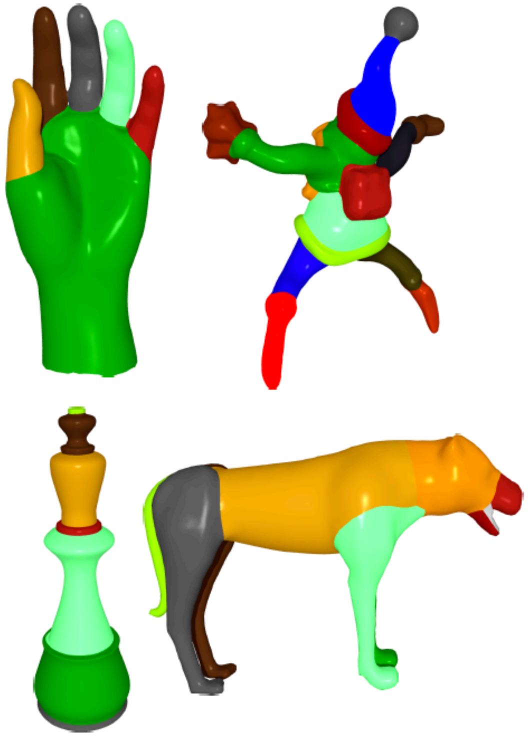
- Why?
  - Get the most out of reading: inspiration, new direction
  - Trained to think critically → critical analysis of potential ideas
- Writing Brief Summary
  - It is too easy to forget if you don't write it
  - Encourage critical reading
  - Help practise writing
  - Save time when writing papers and thesis

FINDING CANDIDATE TOPICS

- Not a linear process
  - Based on understanding of the subject (paper reading etc.)
  - Brainstorming
    - Open
    - Discuss and argue with other researchers
  - Don't worry too much about novelty or technical solutions at this stage
    - Topics sound too easy may not be that easy, when it comes to solve practical problems
    - Topics sound too difficult may be solvable when technique becomes a bit matured, or even when you learn some new techniques.
    - For difficult topics, solving a partial problem may be good research already.
    - Sometimes, it can be difficult to predict the quality of results entirely
  - Don't rule out a topic too early
- Keep your mind open for possible ideas 24/7 in the real world as well as in the lab

EXAMPLES

- Image-based random walk segmentation => mesh segmentation
- Image inpainting => image extrapolation

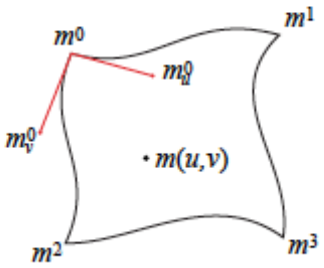


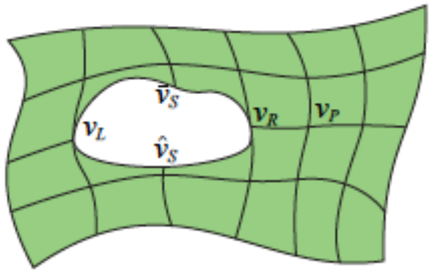
CROSSING SUBJECT BOUNDARIES

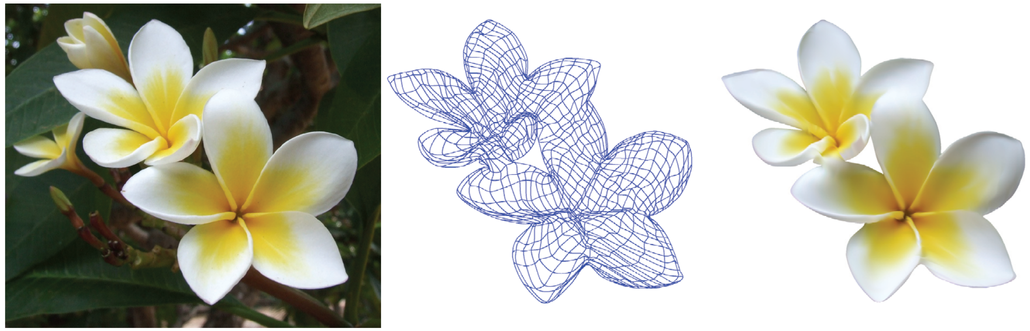
- One approach of finding research topics is adapting techniques from one field to problems of another
  - It can be more difficult to start with as you will need to be (reasonably) familiar with two fields
  - It can lead to useful techniques better than existing ones in the field, and easier than developing techniques from scratch
- Alternatively, getting problem inspiration from other subjects, leisure reading
  - Arts, e.g. NPR, caricature of human faces
  - Papercraft
  - Watch movies
  - Children's puzzles and toys
- Real world + generalisation
  - e.g. Inspired by artists, but automatic, or from images to videos

EXAMPLE

- Automatic gradient mesh generation
  - Standard format supported by commercial software
  - Existing methods are semi-automatic
  - Treat images as meshes (surfaces), and use geometric parameterisation for this purpose: topologically flexible, automatic, fast







WHAT IS A GOOD TOPIC

- What is good research?
- Judging research contributions:
  - Novelty
    - Concepts, methodology, techniques, or even problems
    - Improvement between state of the art and the proposed work
    - Hard to quantify and can be subjective → use commonsense and learn from examples
  - Results
    - How the method could be better than existing methods (if worked out as expected)
    - Better capability, better quality, better efficiency (one or more)
    - More important if the aspect being improved is critical
  - Usefulness (for practical topics)
    - Is the technique (if worked out OK) really useful
    - Or is it one step towards a useful technique
- It helps when you gain more experience
  - Reading more papers
  - Reviewing papers

WHAT IS A GOOD TOPIC FOR YOU

- Ambitious
  - The harder the topics are, the more rewarding solving them can be
  - Don’t worry about uncertainty – it is always there for real research
- Realistic
  - Hard topics can mean higher risks
  - Is technology ready? Or the problem can be too hard at the moment.
  - Does solving the problem involve many technical advances so is not realistic?
  - Does solving the problem involve building a system that takes too much time?
  - Is the problem too hard to solve based on the current knowledge and skills
- Useful for your lab etc.
  - It takes significant effort/teamwork to achieve something big
  - Does the research fit nicely with the lab so everyone can be better rewarded?

PRIORITISING TOPICS

- If you only have one good idea, go with it.
- If you have multiple (or many) ideas, prioritise them:
  - If you just start doing research, try some less challenging problems (or working with more senior students if you are facing difficult problems) to gain confidence and experience
  - Rank the topics based on the potential research output vs. risks
  - If manageable (skills/risks), try the project with more significant contributions
  - Keep remaining ideas
    - When you finish the current research
    - When it is necessary to change the topic

MAKING A PLAN

- Research is never fully planned
  - But making a plan helps to reduce the risk of going nowhere
  - Make sure you are working towards deadlines (conference, PhD dissertation etc.)
- The plan may contain multiple routes (Plan A and Plan B)
  - Expect to change the plan frequently (based on experimental results)
  - You know when you need to be and where
- Spend your time on the things that really matter
  - Don’t optimise too soon
  - e.g. until you know it is the right method before turning into GPU code

ITERATIVE REFINEMENT

- Research prototype often needs iterative refinement
  - Make it modular
  - Simple complete system first
  - Followed by sophisticated pieces
  - Experimental verification tells you what to do next
    - Focus on the steps that need most improvements
    - Or, if the topics are too difficult -> a scaled down version may be good enough

COLLABORATION

- The topic can be too difficult to do it on your own
  - It may involve expertise you don't have → working with others
  - Use libraries, publicly available code, or ask authors for code
  - Use existing data if possible

- "If I have seen further, it is by standing on the shoulders of giants."

REFINING/CHANGING TOPICS

- It is not uncommon that the original topic turns out to be not suitable
  - Technical challenges that cannot be easily overcome
  - The method does not produce results as expected
- Decision
  - Further improvements
  - Or loss stopping
- If you have to change topics later on
  - You still gain experience and knowledge, sometimes publishable
  - Try to reuse skills, ideas and code as much as possible

Q&A