Education and Career

- Master’s and PhD thesis at the University of Padua “Visions of a generalized probability theory”
- Visiting Scholar, ESSRL, Washington University in St. Louis (2000)
- “Giovane ricercatore” at Milan's Politecnico (03-04)
- Postdoc at the University of California at Los Angeles, UCLA Vision Lab (2004-2006)
- Marie Curie Fellow at INRIA Rhone-Alpes, France Perception Group (2006-08)
- Reader at CCT (2011-now)
- Head of Artificial Intelligence group (2012-now)
Research Interests

- **Computer vision**
  - Gesture recognition
  - Action and activity recognition
  - Identity recognition from gait
  - Example-based pose estimation

- **Machine Learning**
  - Spectral dimensionality reduction
  - Metric learning

- **Imprecise Probabilities**
  - Geometry of uncertainty measures
  - Decision making with Ips
  - Total probability
  - Mathematical foundations
Being a successful researcher
Being aware of what's what

- there really is not a recipe: people find different routes to success
- however, something can be learned from one's (and others’) experience

  first: be aware of the state of the art in the field

- people who are really successful in their field have a very extensive knowledge of what's what
  - scientific communities are cliques, to have other people interested in what you do you need to know what the best people are doing
  - only way to get citations, and through them positions/grants
- not enough to get acquainted, read some classic works and then chase after your own ideas, forgetting about the rest of the world
  - it is painful, and sometimes one does not feel like it, but crucial nevertheless
Find a hot topic!

- in my experience, you do not just follow your inclinations, or risk end up doing stuff nobody is interested in
- once you know what is going on, what are the “hot” problems, you can select one and think of a nice idea to bring forward
- two options: break new ground, into uncharted territory, or ..
  - case study later: geometry of imprecise probabilities
  - more difficult, but potentially rewarding in the long term
- .. solve a hot problem everybody is competing on
- other case study: action recognition from unconstrained videos
- potential for immediate recognition, much competition
Writing good papers

- once you have your great idea, what do you do?
- different styles: theoretical versus application-orientated (case studies)
  - theoretical: mathematical precision/application: state-of-the-art performance on benchmarks, reproducibility
- common elements:
  - know what you are talking about! Don't just pretend (see Week 1 slides)
  - this shows in the state of the art, referencing
  - make good case, why is the problem/work you propose important?
  - be clear: a good presentation puts the reviewer in a good mood
  - try to see things from an outsider's perspective, have someone give you an opinion on your paper
Being cited

- what is important, the number of papers of the quality of the papers?
- how do you measure “quality”, anyway?
- in the UK's REF, there is a panel who assesses the “quality” of an individual's best four papers, nobody really knows how nowadays, (Google) citations are considered a measure of the impact, if not the quality, of one's work
- distorted measure, function of the “popularity” of a topic, of the size of the academic community, etc, which are not really related to quality
- nevertheless, cannot be ignored. Strategies for improving citation:
  - firstly: work on hot topics, follow lead authors in the field
  - get orals/prizes are major conferences
  - disseminate your results via web sites, etc
  - make resources available, such as code, datasets, etc
Networking

- networking is another **means to get your work cited**
  - you can send your papers/theses to people that count
- not just that:
  - **fundamental to get a permanent position:** people will not hire you unless they know you
  - way to get **reference letters** from important players
  - way to establish collaborations
    - **writing papers together is easier** than writing them on your own!
  - way to initiate **joint projects**
    - hooking up with good people to ask for funding is a good idea
    - necessary for European projects (see Week 9)
Getting funding

- there are two stages in a researcher's career
- **early career** (PhD student, postdoc) → learn how to write papers under the direction of a senior researcher
- → you get a position, based on the quantity and quality of the papers you published
- second stage: you are a big boy/girl, need to fund your research on your own!
  - university only pays your salary
  - want to go to conferences? Need to find the money
  - too many ideas, too little time? Need collaborators? Need to find the money
- we shall see aspect in detail this next week
Case studies

Action Recognition
Geometry of Imprecise Probabilities
Action and gesture recognition

- problem: recognizing people's behavior and possibly guess their intentions from the way the act
- action and gesture recognition arise in many different contexts: human computer interaction, surveillance, smart rooms, games
- challenging problem, a number of approaches have been proposed
Why is action recognition hard?

- many nuisance factors involved: illumination, moving background, camera(s) viewpoint, multiple agents, occlusions, locality ...
- inherent variability: different motions can carry the same meaning
- but also: semantic of motion distinct from dynamics of motion!
- finally: intrinsically difficult to attach labels to videos/motions
**Benchmark Datasets**

- **KTH dataset**: 6 actions, 25 subjects in 4 scenarios, constrained
- **YouTube dataset**: 11 actions, 1600 unconstrained videos from YouTube
Sample paper

Learning discriminative space-time actions from weakly labelled videos
International Journal of Computer Vision, 2013
Detecting discriminative action parts

- standard approaches use global representations (histograms of visual words)
- but then we incorporate irrelevant information!
  - from the common background
  - from partial motions in common with other actions/activities

(a) boxing  (b) running

(c) trampoline jumping  (d) volleyball spiking
Part-based deformable models

- discriminative parts can be used to classify the video directly [Sapienza, BMVC'12, Best Poster prize at INRIA ML summer school ]..
- .. or, they can be assembled in a coherent hierarchy.
- a tree-like structure joining the parts is learned too.
- the structure is deformable, as parts may have a slightly different configuration in the test videos.
- approach successful in object detection from images.
Part-based deformable models

- example of detecting a deformable model in a test video ->
- top and side view from a KTH video
- plots the best part configurations found in the test video
- red = root node
- blue and green = front and back parts
State of the art performances
[Sapienza et al, IJCV 2013]

ok, just trust me on this ;)

<table>
<thead>
<tr>
<th>KTH</th>
<th>Acc</th>
<th>mAP</th>
<th>mF1</th>
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<tbody>
<tr>
<td>State-of-the-art</td>
<td>96.76$^a$</td>
<td>97.02$^a$</td>
<td>96.04$^a$</td>
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<td>96.81</td>
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<td>mF1</td>
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<tr>
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<td>86.73 ± 5.43</td>
<td>82.43 ± 6.33</td>
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<td>Hollywood2 Acc</td>
<td>mAP</td>
<td>mF1</td>
<td></td>
</tr>
<tr>
<td>State-of-the-art</td>
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<td>59.5$^c$, 60.0$^d$</td>
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<td>HMDB51 Acc</td>
<td>mAP</td>
<td>mF1</td>
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<td>40.7$^a$</td>
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<td>37.21 ± 0.69</td>
<td>39.69 ± 0.47</td>
<td>38.14 ± 0.76</td>
</tr>
</tbody>
</table>

$^a$Sapienza et al. (2012), $^b$Wang et al. (2011), $^c$Jiang et al. (2012), $^d$Vig et al. (2012)
Links
[Sapienza et al, IJCV 2013]

- IJCV 2013 paper
  http://www.academia.edu/4919138/Learning_Discriminative_Space-Time
- BMVC 2012 paper:
  http://eprints.pascal-network.org/archive/00009593/01/sapienza_bmvc20
- YouTube videos
  http://www.youtube.com/watch?v=jtLfUfBVi5k
  http://www.youtube.com/watch?v=VmragBestpo
Doing research in Vision

- very large and fast growing community, tens of thousands of active researchers
- hierarchical one as well, 10-20 people at the core who set the hot topics for the rest of the community
  - importance of networking
  - focus on a few hot problems
  - benchmark testbeds, need to get state of the art results on those
  - lots of focus on the numbers
  - methodologies dominated by a few mathematical tools: support vector classification, kernels, numerical optimisation techniques
  - very difficult to introduce new methodologies, high entrance barrier + people do not have the background to understand
Writing a Vision paper

- crucial is total awareness of the state of the art!

structure:

- intro with review of state of the art
- concise description of problem and methodology
  - (not much space in top conference papers)
  - 2000+ papers to review, reviews are typically fast and not extremely accurate!
- lots of experiments with performance comparable or better than state of the art, on one or more (the more the better) benchmark testbeds
- if possible, upload additional material (e.g. videos, additional figures)
- if possible, make code available online
  - makes experiments reproducible, great impact if your code is used by other people!
Case studies

Action Recognition

Geometry of Imprecise Probabilities
Mathematics of uncertainty

- Decision making and estimation are central in most applied sciences.
- Need to make inferences about the state of the external world.
- The (uncertain) state of the world is often described by a probability distribution.
- Need to estimate such a distribution from the available data.
- Sometimes, however,
  - few statistics are available to drive the estimation (extremely rare events, e.g., a volcanic eruption, tsunami).
  - Part of the data can be missing (e.g., occlusions in computer vision).
  - Under the law of large numbers, probabilities are the outcome of an infinite process of evidence accumulation → utterly unrealistic!
Handling of uncertainty

- need to make inferences about the state of the external world, based on information which is at best limited, if not downright misleading.
- e.g. recognising human actions or estimating human body poses based on a limited training set of examples.
- e.g. modeling impact of developments on marine environment, with little cosystems etc.
- extreme cases: very rare events (e.g. volcanic eruptions, nuclear meltdowns).
- [http://en.wikipedia.org/wiki/Rare_Event_Sampling](http://en.wikipedia.org/wiki/Rare_Event_Sampling)
Uncertainty measures

- in all practical cases the available evidence only provides some sort of constraint on the unknown probabilities governing the process
- different kinds of constraints are associated with different generalizations of probabilities..
- .. which model uncertainty at the level of probability distributions:
  - put upper $u(x)$ and lower $l(x)$ bounds to its values $\rightarrow$ interval probability
  - set of linear constraints $\rightarrow$ convex sets of probabilities, or credal sets
  - (convexity is closely related to coherence [Walley's imprecise probability])
  - possibility measures and fuzzy sets [Zadeh, Dubois & Prade]
  - belief functions/random sets [Shafer & Dempster]
  - monotone capacities etc..
Belief functions

- probability on a finite set: function $p: 2^\Theta \to [0,1]$ with $p(A) = \sum_{x \in A} m(x)$, where $m: \Theta \to [0,1]$ is a mass function
- what if mass is assigned to sets rather than points? $\rightarrow$ random set
- belief functions $\rightarrow$ random sets as a mathematical description of uncertainty and partial knowledge

$$b(A) = m(B_1) + m(B_2)$$
Imprecise Probabilities and Belief

- geometry of imprecise probabilities
- focus on belief calculus, brought forward by Dempster and Shafer
- mathematical properties of these objects
- algebraic study of independence
- decision making with imprecise probs
- applications:
  - imprecise graphical models for activity recognition
  - pose estimation from examples
  - Cumulative Impact Assessment of offshore windfarms
Sample paper

A geometric approach to the theory of evidence
A geometric approach to the theory of evidence

- my seminal paper on the geometry of uncertainty measures
- has generated a flurry of follow-up papers
- All this work has ended up in a monograph I am publishing with Springer's Information Science and Statistics series
- List of follow-up journal papers (+ many more conference ones):
  - Geometry of Dempster’s rule of combination, IEEE SMC-B
  - Two new Bayesian approximations of belief functions based on convex geometry, IEEE SMC-B
  - Credal semantics of Bayesian transformations in terms of probability intervals, IEEE SMC-B
  - The geometry of consonant belief functions: simplicial complexes of necessity measures, FSS
  - Geometry of relative plausibility and relative belief of singletons, AMAI
  - On the relative belief transform, IJAR
  - On the fiber bundle structure of the space of belief functions, Annals of Combinatorics
  - Lp consonant approximations of belief functions, IEEE TFS
Geometric approach to uncertainty

- belief functions can be seen as **points of a Cartesian space**
- **belief space**: the simplex of all belief functions on a given domain

- imprecise probabilities can be handled via geometric methods
Belief functions as points
Case of a domain with two elements

- if \( n = |\Theta| = 2 \), a belief function \( b \) is specified by \( b(x) \) and \( b(y) \)
- as for all bfs, \( b(\emptyset) = 0 \) and \( b(\Theta) = 1 \)
- belief functions can be seen as points of a Cartesian space of dimension \( 2^n - 2 \)
Dempster's combination of belief functions

- In probability theory, evidence is fused via Bayes' rule.
- For belief functions, several combination operators been proposed.
- Original proposal: Dempster’s rule [Dempster 1968]
Example of Dempster's combination of two belief functions

\[ b_1: \]
\[ m(\{x_1\}) = 0.7, \quad m(\{x_1, x_2\}) = 0.3 \]

\[ b_2: \]
\[ m(\emptyset) = 0.1, \quad m(\{x_2, x_3, x_4\}) = 0.9 \]

\[ b_1 \oplus b_2: \]
\[ m(\{x_1\}) = 0.7 \times 0.1 / 0.37 = 0.19 \]
\[ m(\{x_2\}) = 0.3 \times 0.9 / 0.37 = 0.73 \]
\[ m(\{x_1, x_2\}) = 0.3 \times 0.1 / 0.37 = 0.08 \]
Geometry of Dempster’s rule

- Dempster’s sum <-> intersection of linear spaces! [IEEE SMC-B04]
- obtain the possible “futures” of \( b \), called its “conditional subspace”

- bottom line: both belief functions and their combination rule are geometric objects
- number of possible applications: decision making, conditioning, etc
Decision making via transforms

- **transforming a belief measure into a different uncertainty measure** → can be done geometrically
  - solves the computational issue
  - allows use of classical utility theory for decision making

- different families of transforms → **different decision frameworks** alternative to the popular Transferable Belief Model [Smets]

- [IEEE SMCB '07, IEEE SMCB '09, FSS'10, AMAI '10, BELIEF'12, IJAR'12, IEEE Fuzzy Systems'13]
Geometric conditioning

- unlike probabilities, there are several ways of defining conditional belief functions
- possible general framework: minimize distances (e.g. Lp) from conditioning region

right: example of conditioning on an event \( A = \{x,y\} \)

[Belief'10, ISIPTA'11, u/r Artificial Intelligence Journal]
Links, additional material

- my web page
  http://cms.brookes.ac.uk/staff/FabioCuzzolin/
- A geometric approach to the theory of evidence
  http://cms.brookes.ac.uk/staff/FabioCuzzolin/files/smcc08.pdf
- Belief Functions and Applications Society
  http://www.bfasociety.org/
Doing research in IPs

- small community, maybe a few hundred active researchers
- limited number of citations, relatively small (immediate) impact
- difficult to get funding
- on the other hand, more fundamental work which might have important consequences later on
- flat community, there is nobody who sets the research trends
- very orientated towards theoretical contributions
- more open to new ideas
- much stronger maths background, focus on mathematical precision of statements/proofs
- not much focus on applications, not good
Writing a theoretical paper

- still important **awareness of the state of the art**, there are research trends in maths too, although pace is quite more relaxed
- much more space for theoretical contributions
- precision of statements
- correctness of proofs
- normally there is an appendix with all proofs
- can be chains of ten or even twenty theorems, difficult to keep everything coherent!
- there are application papers in IP too, but normally experiments showing a proof of concept are sufficient
- there are no real benchmarks
- in other theoretical fields, such as **machine learning**, there are benchmarks normally made up of **synthetic datasets**
Some conclusions
Some conclusions

- The business of writing papers is different in different fields.
- Clear presentation, awareness of state of the art, significance, relative position in the current debate are important in any field.
- Theoretical papers focus on fundamental methodological contributions, chains of theorem, correctness of proofs, but also importance of problem, significance of results.
- Engineering papers focus on empirical results/experiments, typically people build benchmark datasets on which everybody has to compete.
- Numbers more important than ideas?
- Bigger crowds lead to conferences that are extremely competitive, much noise, extremely high citations counts, more funding (see week 9).