ARIA and UK Mathematical Sciences
Event 2 – May 2022
Panel Discussion and Q&A - "Experiences of DARPA-esque programmes"

Panel Members:

Chair: Robert MacKay (Warwick)

Ronni Bowman (Dstl)

Tim Lenton (Exeter)

Marina Knight (York)

Richard O'Neill (ARPA-E)

Jared Tanner (Oxford)

Robert MacKay provided an overview of the ARIA programme (as understood) and asked the panel to share their experience of working on/bidding for 'DARPA-esque programmes'

Key points from Introduction:

- ARIA being set up in the UK and is loosely based on the DARPA/ARPA model.
- DARPA and other similar agencies focus on grand challenges that are set by programme managers.
- light bureaucracy coupled with 'large funding'. The funding can be 'cut' at any time. [He should have explained that by 'light bureaucracy' he meant at the application stage; as pointed out by some of the panel later, once funded there tends to be close monitoring.]

Whilst DARPA is explicitly for Defence it began with the Internet and has funded things like topological data analysis and fundamentals of biology. The ARPA brand in the USA is larger than DARPA.

Note: The new CEO of ARIA has not been announced. Once the position is announced, set up then our plan is to invite them to join 'us' at a future meeting, to share their thoughts and vision and answer questions from the mathematical sciences community

Panel Intros

Please Note: the panel members are expressing personal views, which do NOT necessarily reflect the views of their host institutions.

Jared Tanner (Oxford)

Jared has worked in a **DARPA** project and shared his experience, stating he was unable to comment how typical this was.

It began with a programme manager from DARPA reaching out to the Community. There was a new area, a new idea of being developed in 2006 called compressed sensing.

The programme manager for DARPA was up to speed with innovations going on in mathematics/engineering/computer science/statistics around signal processing. He was working in signal processing application areas for Defence applications. When he heard about Compressed Sensing he reached out to us.

He asked those of us with expertise in this area to partner up with industrial Primes, e.g. Lockheed Martin, typically large Defence companies and to write proposals to him, working on this topic, both from the academic point of view, developing foundational theory AND working with the company and helping them develop early test systems for the particular application that he had in mind.

Once applications were successful, there were around six or seven different groups involved. We were then given briefings on what the application, they had in mind what was what they expected us to try to achieve.

They gave us data and they ran something which, at the time was, I think, relatively uncommon but which nowadays, we would know as being analogous to a Kaggle. They would give us data and a task and we would compete with each other to see how we would get on.

We would also do foundational papers and we would meet about every six months to a year to report and to compare and see how our different methods were working. Part of the project I was in was also to help develop the expertise in the companies. Part of the time you would go to the companies and they would explain to us the details of the problems, beyond the academic setting we would explain to them the academic side, and we would help them to develop the early prototypes.

As time went on and we had successes, different groups would stay on and different groups would leave the project. As the project transitioned towards applications, groups with more engineering expertise would join the project and foundational mathematical expertise would step out.

It was really focused on trying to achieve this mission to help the US Defence department, achieve a particular aim.

It was NOT like applying for our normal grant where your aim is to tell them what you want to do, and ask them to fund it. This was really more like a request from them to help achieve a particular project objective and then we were going to do the best we could with that, and when we were done with that we would step out.

Tim Lenton (Exeter)

My experience with **DARPA** is ongoing. We are several months into an ongoing project as part of a programme called artificial intelligence assisted climate tipping point modelling. This is championed at DARPA by a programme manager.

I had met, albeit briefly, with the programme manager before the launch of this call. The call is on my favourite subject of identifying climate tipping points, and an area of work I've been interested in for some time, e.g. how can we use some form of machine learning/deep learning to advance modelling capability.

I was already collaborating with a group in Canada and we'd been training a deep learning tool for Early Warning of tipping points in complex dynamical systems. It was an easy decision to apply to this programme. The subsequent programme consists of about eight successful projects. Each project is working in slightly different areas.

DARPA has been very effective at 'forcing' interdisciplinary collaboration, in the best possible way, between the projects.

The project teams interact and try to share progress and learning and find ways of.: interacting and getting more out of synergies between the projects.

Bureaucracy or large funding. The funding is significant for what we're trying to do. There is monthly reporting and deliverables are due every month. I knew I was signing up for that and have no great problem with it because it's not onerous deliverables. It reflects that DARPA are really interested and want to stay in touch with the science. These deliverables are going to be read and used to engage scientifically with the projects. As a group we're really enjoying it, it keeps us on our toes.

The funding is significant, with a clear remit that you are under contract to interact strongly with DARPA and the other projects. They come to you on a virtual site visit, as we hosted recently and we talk. We talk science not just at a superficial level because you're dealing with people who really understand the sort of frontier methods and mathematics you're involved with.

Marina Knight (York)

I am working on this **Wellcome Leap** project that is called the "promoting healthy brain networks for the first 1000 days". The first three years in a child's life are crucial for forming the brain networks that subsequently helps us to evolve successfully in our individual environments.

I became involved when the interdisciplinary team (colleagues in psychology with well-defined expertise in this area) recognised that the call/project required a huge amount of signal processing. Additionally they realised that they required engineers AND statisticians on the team. From my perspective, this is a key challenge - making the interdisciplinary connection so that mathematics is part of the team. These challenges are typically led from disciplines outside of the mathematical science.

Wellcome Leap describe themselves as a "global ARPA for health".

Having observed this process is entirely different to anything else that I've experienced in terms of research funded projects.

It was a relatively standard research call, indeed. Once they announced the successful bids, the wider project team was brought together. The call acted as a way of pruning ideas for the $45 million investment. It is a very fast paced environment. Milestones are regularly reviewed. The team is highly interdisciplinary and spread across the globe.

There is an element of "internal competition", with the funding being very dynamically driven. In time, you can see the benefits of this approach as it starts to shape your own evolution and thinking. From the mathematical statistical point of view, the challenge we face is to optimise our behaviour in terms of appreciating the questions/challenges which arise from the posed questions across the disciplines involved.

There is a tension between providing 'proof of concept' and having time to provide a better solution. I think the mathematical sciences niche is to be patient and to wait for the opportunities where we can add value/benefit. This will also help us to identify opportunities to contribute novel mathematical research. I feel that this is where the real value of our contribution comes from.

However, this doesn't always come naturally/easily and is markedly different from a standard EPSRC environment.

Richard O'Neill (ARPA-E)

I am programme manager at **ARPA-E** and will give an overview of my role and some of the mathematics in the research that ARPA-E promotes.

ARPA-E does a lot of physical research. The programme that I lead is called the Grid Optimization (GO) Competition. The Grid Optimization (GO) Competition—managed by ARPA-E—is a series of challenges aimed at developing software management solutions to address challenging power grid problems. The competition's intent is to create a more reliable, resilient and secure American electricity grid. Challenges include theoretical mathematics, e.g. laplacian matrices and practicalities. To date, there have been 2 competitions.

It turns out that the winners come in all shapes and sizes , we don't prescribe how problems are solved. We say 'solve the problems in the best way possible', and then we reward the winners with either cash or research funding.

What we try to do is integrate. We use a high performance computer in one of the national labs or to conduct the competition. It requires an integration of how high performance computers work , an understanding of all the mathematics of the system and some understanding of AI.

We have a 'sandbox' where competitors can solve problems and try different strategies. It is very important to honour the physics of the system. The optimization problems keep coming. One of the more recent interesting issues is topology optimization.

We expect these problems to get larger and more complicated because, with the introduction of renewables, e.g. there is stochasticity to add into the problem

ARPA-E has a mandate to commercialise. We actually have a very strong emphasis on getting whatever technology gets developed into the marketplace. We try to establish close contact with the actual operating people in the system. DAPRA has one client (Department of Defense) whereas ARPA-E is interested in getting this technology into competitive markets to help our system operators optimise the system.

Ronni Bowman (Dstl)

I am going to talk predominantly about a bid to **DARPA**, which was unsuccessful and the lessons learned from that. I lead a project. This project involves Dstl and a team of UK universities. We bid into a DARPA open call around the theme of uncertainty. The feedback we received was "Interesting, but not adventurous enough"

This reflects the caution to commit to "impact" that could be achieved in the timescales rather than what we were confident would be achieved. For these big grand challenges this can be problematic. We need to be imaginative and sell the impact, rather than being specific about exactly what we can deliver

In the end, we secured funding from a different agency to fund the project. In terms of novelty, we integrated Epidemiological models and dispersion models and research from three different

universities across disciplines, we incorporated imagery, stats, pure maths and fundamental physics. This fed into the UK combined response for Covid. So it was a novel, challenging, cross discipline project. So to consider that this wasn't ambitious enough is interesting.

Projects need to be very cross discipline, and we need to consider carefully how we link with other disciplines and be more adventurous in how we present the impact.

A lot of these projects expect monthly, and sometimes weekly, reporting of progress. This can be quite daunting. However, the role of the mathematical sciences on these projects is vital. A mindset of "I do need to make progress, I might not be able to dot all the i's and cross all the t's. However, what I need to do is get something that works, and then I need to enhance that/improve it and go from there"

Panel Questions:

What advice would you give for how to influence ARIA's agenda, if possible, or to get involved in larger relevant projects?

Richard O'Neill: At ARPA-E the process basically is to take people through a lot of presentations and discussion about how to do things. Whether or not the project passes muster with not only management, but a significant number of different scientists and mathematicians.

There are some political sensitivities regarding international participation in the competition. Participation is largely open, funding may be controlled/limited in some way for non-US based applicants.

So it turns out that except in a few cases, you really do need an interdisciplinary team.

The GO competition is unique; it grew out of the fact that people could publish papers in the literature and make claims that nobody could validate. We have a controlled experiment, where we control the problems and the computer, and so we have a competition between whose code works better.

How critical have your existing links with programme managers been in getting involved with DARPA. (to Jared and Tim)

Tim Lenton: Although I mentioned that I might have slightly intersected with the programme manager at some point in the past, I don't think that had much to do with the success of our bid. It is certainly worth getting to know this programme manager role, but what you can offer the project is key.

The success of DARPA is they have really good programme managers, who are given autonomy. They not only manage the programme but are fully engrossed in the maths and science and the challenges they represent.

Jared Tanner: - My impression was the programme manager knew (or found out) everyone who was doing good work in the subject area.

The programme manager role would be one of the things I really hope happens with the UK version. They can get to know the community and spot exciting work. So if you do something that's

important they might come and knock on your door OR you could apply to be one of the programme managers.

How do you balance the need to excite non-specialists with the provision of enough technical detail to convince sceptics there's something new and feasible in the proposal, how much mathematics to include and then what form.

Ronni Bowman: It depends on who's going to review and, in my experience in general I would put NO mathematics in a proposal, particularly at the initial stages. I would then potentially put some mathematics into appendices in a slightly more detail. it's about knowing who you're submitting to. You are taking up space selling your idea, by putting a fundamental mathematics in unless there's a very good reason for doing it. It might be controversial, but I'm going to stick with none or very little.

Marina Knight: I was just going to see that I very much agree with Ronni. I included the mathematics that was required for the argument. I avoided expanding into interesting mathematics as there is plenty of scope subsequently to do that.

Jared Tanner: I would say that if this is NOT a regular grant application. You're telling them that there's an important problem which either they've identified or which you're proposing - which you can solve. You're going to pitch it to a programme manager and this programme manager is very, very smart in my experience.

Richard O'Neill: I am a big fan of putting the mathematics in the appendix. It's been my experience that mathematics doesn't help evaluate the project, the people involved and their track record is more useful.

A question about the life cycle of ideas, where do ideas for projects come from, how are they elaborated and refined. How long does the process take?

Richard O'Neill: At ARPA-E we have either

- approx. every two years, 'an open', where people just can submit projects that they think are valuable in one way or another.
- Internally the programme managers can pitch a programme that they think is valuable

High risk projects are our projects that are 'interesting' with the ethos 'high risk, high reward', and an acceptance of failure rates.

Note:

Graham Niblo (Southampton) gave a short update on the Tax Credit R&D action that was highlighted in the first meeting in this series. A small sub-group has been formed from Event 1. An event is planned for the Autumn – to provide the mathematical sciences community with the background, case studies and advice of how to unlock the opportunities the R&D tax credits for maths represents.

Questions not covered during session – but which fed into the general chat afterwards.....

- What advice would you give on being a mathematical scientist in a big project?
- How do we help maths community to take exciting ideas forward?