HOUSE OF COMMONS

Science and Technology Committee

Oral evidence: A new UK research funding agency, HC 778

Wednesday 23 September 2020

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Watch the meeting

Members present: Greg Clark (Chair); Aaron Bell; Katherine Fletcher; Andrew Griffith; Mark Logan; Graham Stringer.

Questions 1 - 49

Witnesses

I: Dr Peter Highnam, Deputy Director, Defense Advanced Research Projects Agency; and William Bonvillian, Lecturer, Massachusetts Institute of Technology.

II: Dr Regina Dugan, Chief Executive Officer, Wellcome Leap; and Dr Antoine Petit, Chairman and CEO, French National Centre for Scientific Research (CNRS).

Examination of witnesses

Witnesses: Dr Highnam and William Bonvillian.

Q1 **Chair:** This is the first evidence session in a short inquiry that the Science and Technology Committee is holding into a new scientific funding agency that the Government have said they will introduce "broadly modelled on the US Advanced Research Projects Agency, or ARPA and its successor DARPA."

This afternoon we are taking evidence from the deputy director of DARPA and the director of the Office of Digital Learning of the Massachusetts Institute of Technology, followed by evidence from the chief executive of the French National Centre for Scientific Research, and the chief executive of Wellcome Leap, who is also the 19th director of DARPA. We thank our witnesses very much indeed for joining us.

I am delighted to welcome from the United States: Dr Peter Highnam, deputy director of DARPA, and William Bonvillian, lecturer at MIT and senior director at MIT's Office for Digital Learning.

Mr Bonvillian, you have written extensively on the history of the institution and how it has evolved over time. Will you share with the Committee your view on the problem to which ARPA and then DARPA was the solution?

William Bonvillian: A lot lies in the name itself. It is advanced research; it is not normal, basic research but advanced research. It is not just doing research; it is doing projects. It is going a whole stage beyond the research level and into actual technology projects.

It is an agency with stand-alone status, but in the front of the name is the word "Defense", and it is and must be very oriented towards the defence mission. The name tells us a lot about DARPA.

Historically, it is focused on what can be called a right-left model. If you envision innovation as a pipeline, DARPA has been interested in the right side of the pipeline first. What needs to come out of that innovation pipeline? What do we need? What breakthrough technology advances do we need to get? Then it goes back to the left side of the pipeline to think about the research that could lead to these breakthrough technologies. In a nutshell, that is a quick summary of what it is about.

Q2 **Chair:** That is a very precise and useful outline.

Dr Highnam: DARPA's mission began back in 1957-58 when, from the defence perspective, there was a severe national shock: Sputnik. Therefore, DARPA's very clear mission is to avoid and impose technological surprise. The mechanisms behind that, which I hope to get into in the question-and-answer discussion today, are about how it does that and has been able to do it for over 60 years.

Q3 **Chair:** Before we go into some of the detail, will you, Dr Highnam, give us a summary of the key organisational features of DARPA? What are the main heads of its organisation?

Dr Highnam: I am a big admirer of this model and have worked in it. I ran and was there near the beginning of the intelligence version of ARPA in the US and my second tour at DARPA. I have also tried to put it in place in other organisations.

First, as for the DARPA secrets, at least from my perspective we have temporary people. On the technical management side of DARPA, which is about 100 people at any given time, we all do limited-term tours. We know that when we show up; our dates are on our badges, and then we leave. We maintain about 25% turnover. That has varied a little bit over DARPA's history, but that is a key element of the technical health of the agency.

Secondly, everything begins and ends; it has metrics. Unlike other places where something might continue for a long time because it is a good idea, we do not do that. We frame a project, pitch it and work it.

Thirdly, we have no in-house capabilities of our own. For everything that we do, to the greatest extent possible, we have a full and open competition to find the best teams who can work that project. We do not do any research in-house; we have no in-house labs.

We have temporary people; it is project based; it has no fixed assets; there are no careers to manage; and there are no particular lines of technical approaches to pursue because the place was designed in this way back in 1957, and it is really good to watch.

People come to do tours at DARPA not so much for it being on a resumé but because they get to change their technical field. We can give you the autonomy, assets and rigour around you to make it possible. There is a real honour in public service to come here to do that.

Q4 **Chair:** Again, that is very clear and I am grateful for that.

Mr Bonvillian, is there anything you would reflect on as the salient features of the agency and perhaps how it has changed over the years?

William Bonvillian: DARPA has changed over the years and modified a bit here and there, but the same fundamentals Peter described are still very much with us and have been from the beginning.

It is a small and flexible organisation. The core is about 100 programme managers. It is flat and very non-hierarchical. It is entrepreneurial—in other words, it is after programme managers who will make things happen. A phrase ARPA uses is that its programme managers have to have religion. In other words, they have to believe in the projects they are attempting to do and move them into implementation.

Its focus is on impact, not risk, so it is charged with taking risk, and in order to do breakthroughs you have to take high risk. It is quite autonomous and free of bureaucratic impediments. It is embedded in one of the most notorious bureaucracies, our military. None the less, it is given a lot of freedom from that bureaucracy. It has special kinds of hiring authority that enables it to hire quickly very good talent.

It is a hybrid model, so typically DARPA will engage a mix of actors on the scene. It will engage companies, particularly smaller companies and start-ups, but it will also engage academic researchers and put them together so that the company and researcher skills and breakthroughs in bringing about a technology will go together on a portfolio of projects.

It is very team oriented. There is wide acceptance of tolerance of failure. It is not so much that there is always failure; it is often that things have to be reorganised in the process. It thinks about putting together great groups of innovators. How do you find those great innovators around the country and elsewhere who will get the true breakthroughs and enable them to go off and form technology visions? This is not an industry stage-gate evaluation at every stage; this is forming a technology vision and putting assets on the table to get there.

It is very much an island bridge. DARPA is created as a kind of protected island isolated in a different place from the Pentagon, but it has a strong bridge back, because typically it reports theoretically through an Under-Secretary to the Secretary, but often directly to the Secretary, so that the things DARPA is coming out with can be put in place.

Those are some of the rules in the complex DARPA rule set.

Chair: That is extremely helpful.

Q5 **Katherine Fletcher:** I was particularly struck by your description of the right-left approach to things. When projects or areas of research are being identified, how specific are the goals on the right-hand side of the Gantt chart?

Dr Highnam: Very specific. I served in DARPA as a PM and office director, which is intermediate level, and twice in the front office of these places. The creative act in the DARPA model is to frame a programme and get it approved. We use things called the Heilmeier questions to frame very simple constructs, but answering them well could take a long time. To be allowed to spend taxpayer funds on the work is hard, but absolutely critical.

Within that set, the first Heilmeier question is: the elevator pitch. The second is: do you know a state of the art to solve this problem—do you know what you are talking about? The third is: why DARPA, and why now? If it is just engineering, anybody could do it. If it is just a procurement, anybody could do it. What makes it DARPA-worthy or DARPA hard? The fourth Heilmeier question is: if you succeed, who cares?

That is where the metrics start to come in, because it is driven by use cases. We are defence, so it is driven by national defence military use cases. DARPA is not a blue-sky research place; we do not do that. Even with our fundamental research we know where it will be applied if we can make the science possible, all the way through to the higher technology systems programmes. The fifth Heilmeier question is: how do you measure success? You go back to the use cases. What are the end goals to the far right of the Gantt chart, as you say, that we need to achieve? Getting those metrics right, not just their quantitative aspects but which metrics, is absolutely important. That is all the homework that goes into building the story, narrative or case using these Heilmeier questions as the construct.

I have seen programmes that have done really well, not in technical results—maybe papers coming out of university and systems being built—but they have solved the wrong problem and a problem not of use in the end. There are not many of those because the process internally for identifying and launching programmes is very rigorous.

Q6 **Katherine Fletcher:** Mr Bonvillian, would you add anything to that? It strikes me that getting the right to-do list is the key to success.

William Bonvillian: I certainly think it is one of the keys. The rigorous process that George Heilmeier, a DARPA director back in the 1970s, put in place remains in place today and is very much followed in the process of setting up these research projects.

There is an analogous organisation. There are a number of DARPA clones. ARPA-E is the energy attempt to create a DARPA-like model in our Department of Energy. It varies the model somewhat and adopts some additional features that you might be thinking about. DARPA is a $3 billion agency, so it is a much larger entity than the $1 billion entity over five years that you are contemplating.

In some ways, the ARPA-E analogy may be one that you want to look at carefully in addition to the DARPA analogy. Like your organisation, ARPA-E was not attached to a procurement agency. The Department of Defense is the home agency for DARPA, which can rely on that Department to carry through its technology advances, at least on a good day. ARPA-E does not have a procurement agency in the Department of Energy; it does not buy things.

I think it is well worth looking at the ARPA-E model. ARPA-E goes through a similar but different process from that of DARPA in selecting its projects. It goes through a very rigorous evaluation process. Unless there is a way in which the research can move into technology and be implemented, it is not even considered for a research project, so it is the thinking about what the end goal would be and an affordable pathway to get there.

Q7 **Katherine Fletcher:** I can see that Dr Highnam wants to come in.

Dr Highnam: Ms Fletcher, the question you asked was: what are the end points that you aim for? How do you assess the success of an individual project? How do you select projects? I described the Heilmeier process, which is how you get approval to launch and how you monitor progress as you move through it. We hold people to account with rigorous independent testing all the way through.

The other metric of success—we are an agency in defence—is transitions to use: transitions in moving to other types of procurements and, because we now know something can or cannot be done, moving that into play, and we track those. That is a macro form, if you like, of the metrics, and each programme has its own individual metrics.

Q8 **Katherine Fletcher:** For me, getting that right is the key.

The final area I want to touch on—perhaps Mr Bonvillian will respond first—is that some of the defence targets on the right-hand side are highly classified at this stage. How do you manage the transition to commercial development and delivery, perhaps from an ARPA-E rather than a DARPA perspective? Surely, that idea of setting a goal and getting academics and businesses together ultimately has to end up with a commercial spin-off, or is that not something DARPA and ARPA-E touch?

William Bonvillian: A very important issue for your Committee and the Government to consider is what to do on implementation. DARPA is not a stand-alone entity; it is in the business of technology development and implementation and does not do just research. It has connections to the Department of Defense. I mentioned the phrase "island bridge". It is an island protected from the bureaucracy, but it has a very strong bridge back, often through the Secretary, to technology implementation. Our Department of Defense does the whole spectrum of innovation activity: it does research but it does early and late stage development, testing, prototyping, demonstration and often it will create an initial market for a new technology or product.

Q9 **Katherine Fletcher:** Does it do seed capital for the commercial venture as well?

William Bonvillian: In effect, because it will buy the product, it creates an initial market. In other words, there is a strong system in place at DOD to move its technologies once a decision is made to do so.

That is not the case with ARPA-E. It is not embedded in a procurement entity. The Department of Energy is not going to buy whatever it is—solar or battery advances that come out of ARPA-E—so ARPA-E has had to organise in a different way to get its technologies out. I think that is worth considering when you look at your model. It cannot be just stand-alone; it cannot just be an isolated group of merry, happy technologists racing around. They have to be connected to systems that will do implementation. ARPA-E has tried to deal with that.

I was involved in the early days in trying to stand up the ARPA-E model. We had assumed that the American venture capital system would pick up these new energy technologies, and that had been happening in the early 2000s. Venture funding had been scaling up. In 2009, venture, frankly, pulled out of energy technologies because it was too long term and too high risk. The possibility of a carbon price had failed in Congress, so that model disappeared. That was the model we assumed would pick up this stuff, but it did not. ARPA-E had to get very creative about how it was going to do things. Interestingly, it developed a whole commercialisation team within ARPA-E, which DARPA previously did not have. It is called the tech-to-market team. These people have experience on the commercialisation side. They get assigned to each project and develop a commercialisation plan with the group, and help the programme manager in ARPA-E, called the project director, to implement that plan. They will use other agencies within the Department of Energy to do applied work, and often those organisations will work further along the spectrum than the breakthrough work ARPA-E is doing.

ARPA-E can do hand-offs to organisations like our energy efficiency and renewable energy agencies within DOE. Using that in-reach within DOE educates its researchers. Often, a company will need to pick this up. A salvage company will educate its research teams in the stage gate process, which they are often not familiar with, of how industry implements technologies. They know how to get through that kind of incremental evaluation process that industry uses. It is not familiar to them. Therefore, it has a series of steps that it uses to try to work on commercialisation.

Q10 **Katherine Fletcher:** That is understood. Would you add anything, Dr Highnam, if we are looking at achieving commercial success from our version of this, should it come forward?

Dr Highnam: The luxury of DARPA and IARPA, which is a very close model or copy of DARPA, is that our mission is national security writ large. Purely achieving commercial success with any project is not the primary objective, which is to avoid and impose technological surprise. Can it be done? If so, how well can it be done? If somebody else is doing it, can we detect it, and how will we stop it? It is very clear.

Some of the routes to use turn out to be commercial. About 12 years ago, DARPA ran some programmes in a pandemic response. When you look at the companies now that are doing rapid discovery characterisation of antibodies from recovered patients, or some of the nucleic acid vaccine approaches, those were investments DARPA made, because DARPA, with the Department of Defense as the customer, needs to be able to deploy its troops and forces anywhere in the world now. If you have an endemic disease, how do you prepare for that quickly, looking at the long poles in the tent to do so, as we are all seeing now in public?

DARPA investments back then are now commercial and some of the leading candidates for vaccines are absolutely at the front in antibody discovery characterisation and production.

It depends on other things. Those that are highly classified will go straight into programmes of record, which in US-speak are major systems; for others, the route is through commercial activities, bringing it back to us as a relatively small player in terms of market volume, and we need it.

Chair: May I bring in my colleague Graham Stringer? Mr Bonvillian, you might bring the point you were going to make into one of Graham's questions, because it is very much continuing this theme.

Q11 **Graham Stringer:** Dr Highnam, listening to what you said, the relationship with the Ministry of Defence, as we would call it, is the most important feature in DARPA's success. What are the second and third most important features? Does the relatively short-term nature of the contracts offered to scientists mean that you tend to recruit at the younger and older end of the age range, because younger scientists can take a risk and older scientists are coming to the end of their careers? Is that a factor and, if so, is it a positive one?

Dr Highnam: To reiterate my points, having national security as the mission frames everything. It is not wide open; there is always context and use cases. We have temporary people; it is project based; and there are no fixed assets or labs to keep busy. This place was designed 50-odd years ago always to drive the leading edge of technology and explore places of importance to the Department. Those are the other elements.

Over those 62 years, DARPA has built a reputation not always for getting it right but for delivering again and again. One example of a long-haul piece of work would be AI. The first investments in artificial intelligence were made with a very clear vision back in 1960, and DARPA's investment has gone up and down over the decades in between, but the fact that we have expert systems, neural networks, the high-end hardware to do that work, and the internet to provide the data and move it to do so, is the result of DARPA investments over the decades.

We have lots of quotes from people who have been at DARPA and moved on, or are coming to DARPA. One example is, "If you're not taking enough risk, you don't belong at DARPA." Another is, "Mission oriented, not process-oriented." Another is, "If you don't invent the internet you get a B," and, "Ordinary people think of spending time; DARPA people think of using it." This gives you a sense of the culture.

To answer your question specifically, I came to DARPA as a programme manager from an oil and gas exploration company—a non-defence company—early in my technical career. I had run some nice technical projects with a lot of people involved and I had the right mix. Learning

how to work in government was hard, but I got over it. If you learn how to do things you can do a lot.

Looking at the folk we have at any given time, there is not a particular point in a career—certainly not the young or old. It is absolutely not the case. Academics and university folk tend to come probably just when they have achieved tenure and have security in the system. They can work away for a couple of years at DARPA as a programme manager. Others from different types of industry come to DARPA to change their field; they almost in a sense do a refresh of their field to drive things to a new place. There is nowhere else you can do that of which I am aware, and we get great people because of that.

There is a quote from a science and technology lead. He is not a technologist but he has worked in this area in the US. He said that the DARPA people who came as PMs were not necessarily the best in their field, but they know them and work with them. When we talk about the second Heilmeier question—what is the state of the art?—we can make the arguments or drive what should come next. That is a well-informed bet that we take. It is high risk, but well informed. There is no easy characterisation of our people.

Programme managers, office directors and the front office at DARPA are all in the same mix; we all have expiration dates on our badges when we show up. That is very important.

Q12 **Graham Stringer:** Mr Bonvillian, we have heard the success story. What are the weaknesses of the DARPA model? I do not know whether you are familiar with the Janelia centre of the Howard Hughes Medical Institute. If you are, is it possible for you very briefly to compare the strengths of that system, which has some similarities with the DARPA model?

William Bonvillian: I am not as familiar with that model, but I am generally informed about it.

Let me make a couple of points in reaction to your questions about the success story. A key element of DARPA is that it creates thinking communities around problems. When you think about DARPA you do not think just about an office director and the programme managers; you need to think about the researchers who are involved with DARPA over time, not just on a single project. In effect, this is a community of former programme managers and alumni of DARPA who are focused on particular problems.

These thinking communities lead to a broad base of thinking and ideas. For example, how do we think about personal computing? A lot of personal computing was developed by a former DARPA programme manager and office director who went through Xerox Park back in the 1970s and developed lots of things. His team, which often had experience as DARPA awardees, developed a remarkable number of the elements that led to personal computing, just because they were no longer

programme managers or awardees from DARPA; they were part of the thinking community engaged in projects for DARPA.

An issue for DARPA, whether or not it is going to be successful, is whether it can create these resilient-thinking communities that will be enduring and stay on the problem to move it towards fruition. When you think of DARPA, not just as the existing formal organisation but beyond it, it becomes more of a network. How strong is that network, and will it be strong enough to see these problems through?

Another issue for DARPA is trying to persuade the military to pick up its projects. That is not always easy. The last thing the United States Air Force wanted were Stealth aircraft. It did not want an aircraft that could barely fly and that the minute computers were turned off it would fall like a rock. That was not what it was after; it wanted an agile, manoeuvrable aircraft— and the Stealth was not that. None the less, DARPA was able to use its island bridge connection with the Secretary in his office to push this technology ahead. That is often not an easy process. A lot of DARPA advances can die in that implementation stage, and that is even more the case for organisations that do not have that defence capability to fall back on.

As you think about a DARPA model, I keep pushing the notion that you need to think about the implementation process and how effectively to protect that island model, but make sure it has connections to industry and other Government agencies that can see its work through. How are you going to organise that network in effect?

Q13 **Graham Stringer:** Again, you seem to be focusing on the strengths and threats, but what are the weaknesses in the system?

William Bonvillian: There is a real issue with the multigenerational transfer of projects. A strength of DARPA, as Peter has emphasised, is that it is getting new ideas and talent all the time, with a 20% to 25% turnover annually, which means that new thinking is constantly coming in, so any strong organisation has to get a strong incoming dose of new ideas. DARPA has created a very interesting model to do it, as have the successful DARPA clones following that model, but you also need experience in the system.

Typically, DARPA solves this by having office directors with prior experience with DARPA or the Department of Defense who have worked on the implementation side. That process is not always perfect.

The life of a project at DARPA is the life of the programme manager. The terms they fill are three to five years. When they leave, what happens? Will that get picked up and will it be continued? That is a complicated process. How do the successors come in, pick up the interesting pieces and keep advancing those? Peter will have insights into this, too.

Q14 **Graham Stringer:** There is always a conflict between freedom to work

on projects and to think difficult, out-of-the-box thoughts and accountability. How do you deal with that at DARPA?

Dr Highnam: This is normal business; this is management and leadership for each of the programmes we launch. We have over 250 programmes running at any time. There are evaluations at least once a year according to the metrics we discussed and that were agreed to at the launch of the project. The balance is more to provide the programme managers with the autonomy to make local decisions at speed, while holding them accountable to the slightly larger metrics and performance aspects, which include expenditure of funds as well as the technical and transition-related aspects. We do that internally all the time—that is normal business.

Q15 **Mark Logan:** Dr Highnam, does DARPA's key difference from other research and innovation bodies lie in its defence capability, or are there a couple of other things that you think make it stand out?

Dr Highnam: It is a great question, and I do not duck and weave when I answer it, but you will be the judge of that. To have national security as the mission helps; it frames everything. Behind that, there are people; projects designed to move quickly; a certain level of autonomy; a minimum structure inside; and a heavy process where needed, but not in other places.

The organisations I know other than DARPA do R&D funding. We do not do R&D in-house; it is all done by others in thousands and thousands of companies and universities that work with us at any time. We pay a lot of attention to the metrics and performance.

I was with IARPA, the intelligence arm, shortly after it was stood up, helping to grow it, and it was as much a clone of DARPA as we could make it. The internal processes and rigour were the same, and the intelligence community was the customer. Again, it was unique and focused. That helps a lot.

The operating roles of other organisations that have ARPA or "advanced research" somewhere in their titles are different. It could be that for that context it is better, but what you are getting is not an ARPA, DARPA or IARPA but something else. I do not feel I am in a position to judge that aspect. What I can say is that, having worked three times in the DARPA system, including IARPA, I know what it is and how to make it work and how to work within it to achieve results.

As for others, I see different things being added to it, or mission spread to include, perhaps explicitly, commercialisation as a goal. That is not what we do; that is a piece of what we do, depending on the route and transition of technology into the Department's uses, but it is not the focus.

You will be the judge of whether I ducked and weaved on that one. I have thought a lot about this, too. That is the way I think about it. If you get something different, it could be better.

Q16 **Mark Logan:** Dr Highnam, that is a nice segue into the next section, when we will talk about clones. Before today's session, I did a bit of nascent research about developments in China. I read that from 2017 there has been an idea called the scientific research steering committee. Do we know how that has developed in the past three years? To what extent does it differ from what has been done in the United States?

Dr Highnam: I have read a lot about that as well. Without being on the inside, it is hard to tell. You and I can look at the description of a funding agency in the US, UK or EU and say, "I see what you are saying, but I don't know how it is actually working." That is the way I feel about the Chinese description.

Q17 **Mark Logan:** Mr Bonvillian, do you have any insights into that question?

William Bonvillian: No. I very much hear what Peter said. You cannot really understand how an organisation functions and operates until you are inside it, and we are not. Therefore, the description in many ways does not help you.

DARPA is a culture and its rules largely are not written down. This culture is passed to programme managers, office directors and directors. Getting that culture right at the outset is absolutely critical.

I have had experience in some of the DARPA clones. Early on, when I was working for the United States Senate, I worked on legislation to create a Homeland Security ARPA. We thought, "We need a science and technology capability there," as obviously we did, "so let's put an ARPA clone into this." Yet, when that organisation was stood up and it had a promising initial talent base—it had no fewer than 10 people with DARPA experience at the outset—it was placed within a bureaucracy and never allowed to develop its own budget and independence. Everything it did was very closely overseen by another directorate and it never developed any kind of independence, frankly.

Therefore, the talent was wasted at the outset. There was an attempt later to stand it up. It taught me a tremendous lesson. Regardless of what the description of the legislation might be, the early culture that gets into an organisation will lock it in.

DARPA had great early leadership from people like Herbie York, Jack Ruina and others—great technology thinkers and leaders. That was absolutely critical.

ARPA-E in the United States, likewise, had brilliant leadership from Arun Majumdar, now a professor at Stanford who is leading the energy initiative. I think the Committee will find him well worth talking to. Arun created this early dynamic culture of great talent and skill, applied that

DARPA rule set and added some new rules that ARPA-E itself has needed, but made it work.

Getting that early leadership and culture will lock in and determine the future of that organisation. Absolutely key to your endeavour is making sure you have a very talented initial team to do the start-up.

Chair: You have made that point very clearly.

Dr Highnam, you gave a full answer to Mark Logan's question. When it comes to ducking and weaving, many witnesses who have come before the Committee have practised that. That was not an example of it, so thank you for that reply.

Q18 **Andrew Griffith:** Please do not take that as a challenge. I am sure you are more than equal to that.

Sticking with the same theme, how hard would it be to create a DARPA clone, or subsequent deployment of DARPA, without the original DARPA DNA? I noticed in the reading materials that ARPA-E came into existence more than 30 years after the original success and creation of DARPA. As we embark upon this, could you draw out some of the challenge that we will face in not having started with a DARPA but intending to create something with a similar culture, but for more general science and without a single captive Department?

William Bonvillian: I had the pleasure of working on that original ARPA-E model as well as legislation regarding the ARPA model in several different circumstances, so I have done some thinking about how you get the clones right. Believe me, it is not an easy process.

A minute ago in response to Mr Logan's question I mentioned the importance of locking in that culture early. That is key. As I noted about ARPA-E, it was not attached to a procurement agency. Our Department of Energy does not do procurement. None the less, it was an agency from which it could utilise resources, so it was an island bridge; it was parked on an island away from the Department of Energy blocks away, but it had strong reach-back into the Department through reporting to the director, who in turn could enable and require the other organisations within DOE—labs, applied agencies or basic research agencies—to co-operate and work with this new rival that had suddenly arrived on the block.

First, you have to think about what your connectivity is going to be with other governmental agencies and how to utilise it and make it serve the mission of your ARPA-like entity. Secondly, as you are probably not going to do military technologies, you have to think about the commercial routes and what connections you need to link up with industry—your Catapult centres, for example—or your basic research operations. I think that will be significant.

The size of the organisation will be important. As I understand it, you are contemplating a $1 billion agency, with that funding spread over five

years—about $200 million a year. That is a small DARPA office. DARPA is a $3 billion organisation. ARPA-E is a $400 million organisation, so it is roughly twice the size with about 12 programme managers, a support team and tech-to-market group. That is the essence of the ARPA-E organisation. Frankly, I think that is a closer model in size for you to look at.

A very important feature of the ARPA group is that all the programme managers are working in energy; they are all on the same broad project and bring in a very collaborative effort. Projects do not go ahead in ARPA-E unless the other project managers—roughly, 10 of them—are participating in the decision making and critiquing the proposal that comes from the project manager that he or she wants to advance. It is a very collaborative team effort in many ways. That team constantly draws on and contributes to one another's expertise. A DARPA office works in a somewhat similar way.

If you have a scattered portfolio of technologies that you are going to dump on your ARPA, you will not have a lot of that collaborative teamwork; you will be in fields that are just too different to be able to collaborate much. You need to think about having a focused agenda given the budget and size of your ARPA.

Q19 **Andrew Griffith:** Dr Highnam, develop for me how challenging it would be if you had not already got within the gene pool the DARPA gene floating around and individuals experienced in that methodology and understanding of what good looks like.

Dr Highnam: If the goal is to create something that is close to DARPA, I do not see another way of doing it, except having people there who have been through it and believe in it. I watch other organisations attempt to copy even with DARPA people there, but they add a new speed stripe; they add bells and whistles and do something different. They change a process because they did not like that process when they were in DARPA and you get something different. If you want a DARPA clone, or very focused DARPA system, that is necessary but not sufficient.

Do you have a specific customer? The intelligence community for IARPA is a much broader remit than defence in some ways. It is smaller, but with very different types of missions. A few of us copied that across and did something that internally looked and felt very much like DARPA. Therefore, it is possible to do it. As a scientist, reproducibility is a good thing—I have seen it done and have participated in doing it.

It would be hard, if you want it to be like DARPA, to do a general-purpose thing that did not have the DARPA culture gene pool—culture and process combined, not a passing experience but something that people have lived, done and understand. It is about making sure you have all that tacit knowledge about how things get done, not just what the policy and papers say, if you want to make it work. It is a great question and it is one I have wrestled with from the inside in different jobs.

Q20 **Andrew Griffith:** Over the course of this session we will probably come back to this. You have seen people trying to create clones. You have seen that domestically, and I am sure you have had more than a passing interest in some international ventures. For you, what would be the signs to look out for that say this is not going to be a success?

Dr Highnam: It is a different question from whether you are going to create a DARPA. This is where I do the duck and weave again because, your needs, the context and your Government structure are going to be different from here. There may be some design principles that you have to have that we do not, and I cannot tell that from the outside. I am sorry I cannot be more definitive.

William Bonvillian: Mr Griffith, I do think it could be done. An international translation is even more complex than attempting to have some replication of the model in the United States. There has been a fair amount of discussion of additional ARPA models in the United States. That is an ongoing political discussion. One of the current candidates for President has proposed an ARPA for health, for example. This will be a continuing discussion in the US. How do you get that clone right so it is effective? Obviously, the mission that a clone is to be given will be different from what DARPA's or indeed IARPA's mission might be. Again, I point to ARPA-E as a possible area for you to explore further.

I do think it can be done, however. It would be useful to understand in real depth the DARPA, ARPA-E culture and IARPA culture in the US. I am confident that those organisations would be happy to share thinking, ideas and thoughts, if and when this might be attempted in the UK. You have talent on which you can draw from our resources, but in the end the design will be key.

From some of the points that Peter and I have mentioned, getting that island bridge right is going to be key. You just do not put them on an island to have fun; you have to have a bridge back to technology implementation. How you design that will be critical. Getting that early culture and leadership will be very critical. Attempting to put an ARPA entity within a thinking community, as we suggested earlier, so it can draw on lots of resources, will be an important organisational task. Getting an office structure designed so it has manageable tasks on which the full group can collaborate, given the size of the organisation you are going to be creating, rather than have it scattered around too many issues, will also be a very crucial element. Those are some initial thoughts.

Dr Highnam: There are other DARPA-motivated examples here. If we look at what became BARDA, the Biomedical Advanced Research and Development Authority, the original legislation and thinking around 2004 to 2006 was to do a biomedical DARPA. I served there for about three years as senior adviser to the director and it became something very different. The context was different. The emphasis and idea of something like a DARPA was there, but the actual facts inside the health and human

services arm of the US Government were very different. To get people to serve at DARPA, that notion of avoiding and imposing technological surprise is an exquisitely focusing concept. The absence of something like that, which in some sense is the lodestar, makes things harder.

Chair: That is very helpful. Let us go back to Mark Logan on the question of how we can apply these lessons in creating a British version or inspired agency.

Q21 **Mark Logan:** Dr Highnam, you mentioned that people come into the organisation, spend just four or five years there and then rotate out. As MPs we have the same problem. On our badge it says that in 2024 we are out. The Government want to make an impact. If we set up this organisation next year, how long will it take to have that manifestation of the impact?

Dr Highnam: When IARPA was stood up, for a longer-term R&D shot how long do you give it to start delivering results? Is it next year? Presumably not. Is it two years from now? You want to start seeing signs of things coming through. Is it three years? Yes. Then you need to start seeing that organisation impacting the thought processes of the end user agencies, or customers, if you like, as well as the external research teams and companies and universities that can work with you.

It is not a honeymoon period but a time of hands off and saying, "We have started up this thing. It will make mistakes at the beginning. We have told them this is where we want them to go and they have agreed to do it. Let's give them a bit of room to do so while keeping an eye on what is going on." It is akin to a DARPA project, but with metrics for success—three or four years at the beginning and thereafter every year—but it needs a little runway at the beginning to get its legs under it.

Q22 **Aaron Bell:** I want to focus on culture because I think that is what has come out of the hour. We really need to be thinking about culture and the focus of this agency. May I ask about the lack of peer review, which I believe is one of the key features of what you do at DARPA? Not having peer review gives programme managers more latitude to pursue transformative projects. Do you agree with that, Dr Highnam?

Dr Highnam: No. There is the acquisition aspect of DARPA where we go out and solicit the best teams to respond to proposals put together to attack a particular problem that we frame for them. There is a very robust and rigorous review process against those proposals.

For those programmes where basic science is involved there are publications. At this very moment we have a lot of Covid-19-related publications coming from DARPA performers. They absolutely go through peer review. Therefore, there are combinations. There is rigorous Government review of technical, programmatic health, financial realities, transition stories and business cases. That is against the proposals.

As for the science, where it can be published we treasure those best-paper-in-conference and top journal articles because that tells us from the outside that the scientific community is recognising the quality, rigour and importance of the work we are helping to fund.

Q23 **Aaron Bell:** What proportion of the projects would you say do not work out in the end, whether you call it failure or something you accept as part of the project?

Dr Highnam: It varies. There are different ways of not making it. One is that we are too ambitious about something and the physics do not work out. Perhaps the management of a project was not as tight as it should have been; or, as I mentioned earlier, perhaps the metrics were wrong. I reviewed a few recently where maybe nine of the 10 key metrics were there. The 10th one, which we absolutely should have measured and was essential for transition to use, was not watched. It could have been energy storage for a particular type of device. You end up with a technical success against the metrics you posed, but a failure in terms of transition to serve and use by the Department of Defense. It is a mix.

We track and have broad classes of transition because, even for those failures, if we did it right and worked hard to do that, then we know why it failed, so it will inform subsequent projects. We will also know whether for a technical reason it could not be done, or done in a certain way, or with certain other parameters. That is important to know, too, because we and our partners throughout the Department will not make that mistake again, so a lot of knowledge accumulation and sharing takes place even from those that do not work out.

William Bonvillian: I want to reflect on a point made a little earlier about time. If you create one of these entities, you have to give it a significant amount of time to operate. Let us think about a famous fairly early DARPA success. DARPA was set up in 1958. In 1969 it did the ARPANET. It was not until the early 1990s that we understood the significance of the internet. That is a very long-term process, even for a technology that is considered to have advanced in a radically short period of time, so being patient and giving a certain amount of licence to your organisation over an extended period will be critical.

Peter's point is a particularly good one. One very good measure to understand the success of a new organisation is whether it is putting together the thinking community that is going to be necessary to achieve advances over time. It is not just the organisation itself. Is it creating the community around it that can push this stuff ahead? That might be very valuable.

Q24 **Aaron Bell:** I think you are absolutely right. We have heard loud and clear the point about giving some autonomy and freedom from political interference.

Purely on the money, the spec for UK ARPA is that it would back basic

science and breakthrough technology across long-term horizons. The figure that has been committed is £800 million, or about $1 billion, for the first few years of its operation. Given we are about a fifth of the size of the US in terms of population, is that sufficient?

William Bonvillian: I think you should consider some additional funding. If not, you really need to focus the task on probably one or two technology areas where there could be real collaboration within the organisation, rather than scattering it around too many different focus areas.

Q25 **Aaron Bell:** Dr Highnam, do you want to say anything about funding?

Dr Highnam: It depends on the context. Those kinds of numbers are big, serious ones, but, depending on not just political but Government-wide or industry-wide situations, I do not have a sense in your system about what it would take to have some long-lever arms to make things happen. I am sorry; I just do not have an idea.

Chair: Gentlemen, we are very grateful for your evidence today. It has been a fascinating hour. You have been very generous not just with your time but in giving us some very important and profound guidance on how this new proposed agency should be constituted from the outset and what can make it a success. We are very grateful indeed for your evidence today. Thank you for joining us.

Examination of witnesses

Witnesses: Dr Regina Dugan and Dr Antoine Petit.

Q26 **Chair:** We are pleased to welcome our second pair of witnesses in this session. Dr Regina Dugan is the chief executive officer of Wellcome Leap, an organisation set up by the Wellcome Foundation to accelerate discovery and innovation for the benefit of human health. She is a distinguished alumna of DARPA; she will draw on her experience of that as well. I do not know whether you caught some of the evidence in the earlier session.

Dr Dugan: I did, thank you.

Chair: Dr Antoine Petit is chairman and chief executive of the French National Centre for Scientific Research, the largest fundamental science agency in Europe.

Thank you very much, indeed, both of you.

Dr Dugan, we are very familiar with the Wellcome Foundation. Why was Wellcome Leap established? Perhaps you will speak about its structure and how it operates. To what extent, drawing on your distinguished service with DARPA, have you been influenced by that?

Dr Dugan: The Wellcome Trust identified the need to form Wellcome Leap back in 2018. You will recognise some of the themes as they were talked about in the previous panel. Mostly, they are motivated by the desire to create more breakthroughs in human health. Because of my time at DARPA and in Silicon Valley, I am most often asked about creating more breakthroughs in organisations, either because they are seeking them or because they have experienced some Sputnik-like event that changed their entire industry or the global landscape. Their work is often very good, even excellent. It is simply that they feel there is a gap or missed opportunities.

I found that mostly their approach is a linear progression from basic science to applied science to productisation. They have important and useful product and research road maps, but somehow they do not feel quite enough and there remains a gap.

In keeping with many of the comments about DARPA, what is different about DARPA and the style of work there? The most important thing from my perspective—the fundamental thing from which many of the other attributes flow—is that it is a flat rejection of the notion that all innovation must happen along this linear path: namely, this basic science to applied to productisation path.

DARPA instead folds the basically applied and product work on itself and into a single programme. This is what Bill talked about as the "right-left phenomenon", but Stokes described this as "use-inspired research"—so-called Pasteur's Quadrant work. It is work that occurs at the intersection of a bold or even audacious science objective and an application that matters.

Q27 **Chair:** Would you explain Pasteur's Quadrant? It is a very important concept and some of our viewers may not be familiar with it.

Dr Dugan: Indeed. Pasteur's Quadrant is so-called use-inspired research. Bill described it as this right-left phenomenon: the idea that you have a very specific problem or capability you are trying to create, but you cannot do it unless you have a breakthrough somehow in the science, the engineering or technical underpinnings. Stokes named it after Pasteur. You have a food safety problem but you cannot solve it unless you discover the underlying mechanism and its micro-organisms in food. If you raise the temperature, pasteurisation is the result. He called that "the breakthrough".

Stokes distinguished Pasteur's Quadrant work from basic science work—so-called curiosity-motivated work, which is your basic science foundation and which he called Bore's Quadrant—and from pure applied work, which is mostly done in organisations and companies that have particular products they are trying to advance.

If you think about Pasteur's Quadrant as your formative concept—if you believe that that gap is important to fill—a number of things flow from

that, because the problem and the science, that intersection, is perishable in time. The moment you understand that, then what you form are project-oriented activities that are themselves time-boxed. We have heard this as a theme many times. You time-box the project to capitalise on that intersection—that opportunity—and you create urgency in the solving of the problem.

When you do that, you get teams that demand independence—they self-edit off paths they have determined to be dead ends. It is a constant pressure against process growth and bureaucracy. If you imagine a four-year project, two weeks of delay is 1% of your time. So people gather that urgency and use it to push the projects and the organisation forward.

It also allows you to cap your risk so you can take more risks. You can cap your risk in people. There are no fixed assets, as Peter mentioned several times. Typically, with projects of three to five years in duration, you are turning them over. You get 20% to 30% of your resources back each year so you are constantly refreshing what the organisation is doing. That all requires that you optimise for speed and agility.

Those attributes are very important. They are at the centre of Wellcome Leap's activities focused on human health. The last point, which is very important, is focus.

I would encourage the Committee—you have heard this theme in the previous testimony—to think about the problem set you are embracing. Pasteur's Quadrant—use-inspired research—requires some understanding of the use or capability that you are trying to effect. This is notably a fine line to walk. You need to be close enough to the users to understand the problem set but not so close that you are not free to challenge conventional wisdom. This is a very important attribute of creating programmes that are successful as breakthroughs as opposed to evolutionary work.

Those attributes are at the centre of what we are doing at Wellcome Leap. You can see that its focus is already determined on human health.

Chair: Thank you. That is very clear. We are very grateful for that introduction. We are going to stick with Wellcome Leap, if we may, before we come on to Dr Petit.

Q28 **Aaron Bell:** Thank you, Chair. Thank you, Dr Dugan, for your time. The Committee welcomes what Wellcome has done in setting up Wellcome Leap. I congratulate you on your appointment.

What are your current and future plans? What are you looking at right now and what are your longer-term horizons? I think your horizons are a bit longer than DARPA's usually are.

Dr Dugan: Yes. Projects for us will run on the long side of DARPA projects, so three to four years in duration. It is important for the Committee to understand that, much like Peter talked about with AI, one can take a long-term objective—a 10-year type of story but divided into multiple chapters of execution that are shorter in duration. That is, again, part of capitalising on this Pasteur's Quadrant type of opportunity, because those opportunities shift and change over time.

When we look at health and Wellcome Leap, we have identified a number of areas where we need to make 10 X-types of breakthrough. One of the things that has been increasingly important to us is to recognise that this pandemic is essentially a Sputnik-like event for us. Much in the same way as Sputnik catalysed the space age, this pandemic might inspire what we consider to be a health age. We are, perhaps, on the brink of the start of one of the greatest periods of advancement in science and medical history.

If we think about a health age, these axes on which we think we need to have 10 X breakthroughs include the following: a dramatic improvement in our mechanistic understanding of human health and disease; and a transformation of the economics and greater speed, scale and equity of delivery. We are setting up operations for Wellcome Leap or we are well on our way to achieving the speed and agility needed to form these programmes, very much inspired by DARPA and my time in Silicon Valley, but also in programme formulation against those objectives.

Q29 **Aaron Bell:** Fantastic. They sound like excellent goals for Wellcome Leap. Do you think that what we are trying to do in setting up a UK ARPA, given that we do not have the same single customer that DARPA has, means that we are better off trying to follow something more like your model that you are applying for Wellcome Leap, or should we be trying to find that customer? Would that be the better way for us to set up a UK ARPA?

Dr Dugan: First adopters can take many forms. I would encourage you to think about a space of focus for your activity where you have what we would call "a right to win". What I mean by that is looking for the place where you have a robust existing ecosystem, that you have good tradition in basic science, you have talent and you also have commercial and industrial activities that can create these transitions and pull-throughs.

One of the things that is intriguing and important about health, at least for the UK entity, is the presence of the NHS, and might there be an opportunity to create fluid transition pathways to breakthroughs in health across your health system? It is very important to think about both the foundations of basic science where you have strength—that is your source term—but also clarity of problem set and pull-through for transition, because the breakthrough itself is not impact. Impact is realised at scale and in the transition to scale.

Q30 **Aaron Bell:** You said you heard the evidence earlier from Dr Highnam and Mr Bonvillian. From what they said about aspects of the model, it seemed to me that focus and culture are going to be absolutely pivotal for us. I think that is going to be pivotal to what this Committee ends up recommending. Do you disagree with anything that they said about that, or did they hit the nail on the head regarding what we need to be concentrating on?

Dr Dugan: I agree with a lot of the comments that were made in previous testimony. I would encourage you again to think about what is fundamental to the organisation. This project-focusing, this time-boxing of programmes, is, probably, one of the most important things you can take on. The reason for that is that many of the other essential attributes flow from that. The project orientation in Pasteur's Quadrant—this use-inspired research—is critical. Then you also need to think about impact, and the impact has to be realised at transition to scale. Those are the two attributes that I think are most important. The culture flows very naturally from those things. You can begin to get a sense of how the culture has to align if those are your objectives.

To give you a sense of what it means to be lean and of operating leverage, as Peter mentioned, DARPA is a $3.5 billion-a-year agency with about 100 programme managers but about 250 full-time staff. That is about $15 million per FTE. That is extremely lean. There is no ability to have a lot of process and bureaucracy in that. There must be speed, it must be highly agile and it must operate independently so as to give you the breakthroughs that you desire.

Q31 **Aaron Bell:** You said that programme focus is absolutely key. Clearly, to do that we need to recruit the right programme managers. Where should we be looking to recruit these people from, in your opinion?

Dr Dugan: We have found great programme managers who come from academia and companies. What is important about these attributes is that they very typically have a PhD in one of the sciences. It need not be in the particular field of endeavour. You may find that someone who has a PhD in computer science contributes dramatically to a programme that is life sciences-focused, for example.

They very often are entrepreneurial in their ambitions. They are impact oriented. These are often mid-career types of individuals. They are not typically MBAs. They are very much focused on the advancement of the science and engineering but they have CEO-like qualities. In Silicon Valley we would say that they are the best in class scientists, engineers and CEOs.

Aaron Bell: Thank you. That was very helpful.

Q32 **Chair:** To pick up on one of the points that you made, we heard in the previous session, as you will know, that the defence focus of DARPA is foundational. You cited the coronavirus as a possible Sputnik moment for

this generation and perhaps for Wellcome Leap. Do you think that if we were to follow the advice of our previous witnesses and identify a particular field of inquiry—you pointed to having existing capability, experience and resource—health and life sciences ought to be the focus, if one has to choose one, of this new agency?

Dr Dugan: I think it is a natural candidate for you. You have a strong tradition in the life sciences at the basic level. You have a long history of investment and breakthroughs in contribution. You have the presence of the Wellcome Trust that operates globally and philanthropically, and you have the NHS. It is a natural candidate for you to consider.

Q33 **Chair:** Wellcome Leap has, as I understand it, a £250 million fund that the foundation has allocated to it.

Dr Dugan: Yes; seeded with that.

Q34 **Chair:** Did Wellcome Leap establish the fund because the Government were not doing it? If the Government were to have this as the focus of the UK ARPA, would that obviate the need for Wellcome Leap? Would you expect to align with it? How would that relationship be?

Dr Dugan: We can certainly talk about what that public-private partnership might look like in future if that is the direction that the UK ARPA determines to be appropriate. DARPA has a long history of public-private partnerships in catalysing advances, the most notable of which were the historical public-private partnerships when the US was trying to advance the field in the semiconductor industry. As a result, we had focused centres for research that were public-private partnerships. We also had the formation of Semitech, which was a public-private partnership.

Certain advantages can be realised in those types of partnerships. Of course, there is, with respect to human health, a need to think about a presence on the global stage because, at least in this particular area, we are certainly not going to solve the problem of global health only within an individual country's borders. There is a need to think about it broadly in that context as well.

Q35 **Chair:** So it would not break the DARPA model if there were to be some collaboration between the two. It is consistent with some aspects of the DARPA model.

Dr Dugan: Absolutely. There are good examples in the history of DARPA where public-private partnerships were foundational to advancing the efforts. In some respects, we are able to do some things in Wellcome Leap, informed by my time in industry, that we were not able to do in government. Certain elements of speed and agility that are achievable in a private organisation may be difficult to achieve in a Government organisation. Managing to occupy the best of both worlds is important to consider.

Q36 **Chair:** Thank you very much indeed.

Perhaps I may turn to Dr Petit now. You have been very patient, Dr Petit. Thank you very much, indeed. Would you describe to us CNRS's central mission?

Dr Petit: Thank you very much for the invitation. Let me say something about myself. I graduated in mathematics and then I moved to computer science. I was a professor of computer science for more than 20 years before taking on a more strategic position. I took up my position of CEO and chairman of CNRS two and a half years ago. It is a four-year mandate, renewable only once.

CNRS is not an agency but a huge public resource institution. It employs about 32,000 people. One particularity of CNRS is that all our labs—we have about 1,000 labs—are joint labs with French universities. That is a particularity of the French system.

CNRS covers all fields of sciences from mathematics, physics and chemistry to humanities and social sciences.

Our main job is to do basic research, but we try to apply that basic research. For us, it is important to say that we focus on basic research. That does not mean that we have no relationship with industry. On the contrary, we create every year between 80 to 100 start-ups. We have more than 150 joint structures with industry and partners throughout France.

We are also very international. About one third of our researchers are non-French citizens. If I look to the PhD students, it is more than 50% and to the post-docs it is more than 75%. We are based at the European level and are an institution that benefits the most from the European Framework Programme. If I consider the last programme, the current programme and the Horizon 2020 programme, more than €1 billion comes from this programme to CNRS.

CNRS probably seems strange to you because you have no equivalent in the UK and, probably, there is no equivalent throughout the world. I listened with interest to all the testimony that has been given, and I want, if you will allow me, to make one comment. Science is a worldwide game. We like to do comparisons. Something that has to be taken into account is the fact that it is complicated to make comparisons between countries because our histories, values and organisations are different. If you take one piece of the puzzle that could fit perfectly into one landscape, it is not clear how the same piece, if you copy it, will fit into a new landscape.

I think that DARPA is a fantastic example, but we do not have DARPA in France, although we probably could have a DARPA. From my point of view, it is very important to understand how DARPA will interact and take its place with the other tools that already exist. If you are a scientist, you

cannot have too many agencies or too many ways to get money because, if these agencies appear to be in competition among themselves, it is not an efficient system.

I do not know whether you think you need an ARPA or not. You will know much better than me. One point that has not been mentioned, I understand, from the very beginning is the fact that you cannot consider it alone. You have to consider it as a whole system. How will the new ARPA take its place in the old system? From my point of view, it is very important to take this into account.

Q37 **Chair:** Thank you. That was very helpful. You anticipated some of the questions. DARPA has been very prominent and much discussed internationally in the science community and beyond in recent years. Has there been any time in France in recent years in which you have formally looked at the DARPA approach and considered whether France ought to have an equivalent?

Dr Petit: Not really. Perhaps you know this, but in France we are preparing a new law on research. Your colleagues in the French Parliament are examining the text of a new law. Much of this law is devoted to the development of the French National Agency for Research, which is not a DARPA-like agency but more an NSF model.

One very important point is that our science communities consider that the budget for this agency is too low. That is very important. You have already mentioned the point that, if the budget is too low, you have to focus on a limited number of projects.

We do not have in France a DARPA-like agency. Our priority until now was not to create a new agency but whether to have a better budget, because if the acceptance rate is too low the whole system is not efficient at all.

Chair: Thank you. That was very helpful.

Q38 **Mark Logan:** I have one question for Dr Dugan that concerns your previous experience with DARPA and the advice you can give to us in the UK Government. As we are starting from scratch, what suggestions would you have for us—the dos and don'ts, essentially?

Dr Dugan: We have talked a little bit about this. Focus is important in the beginning. The idea of committing to programmes with a fixed duration and no fixed facilities and having the power within the organisation to form the programmes and execute them with high agility are very important.

Here again, you want to think about the attributes that are necessary for that. You need fast-contracting mechanisms that allow you to reach multiple parts of your ecosystem from academia to companies. You want to be able to formulate programmes very much like Peter described in

previous testimony at this intersection between the scientific opportunity and the particular problem.

It is important to get the function and execution elements of the organisation right in addition to the programmes themselves. If those conditions are not different from what you currently have, you will not get different outcomes. Setting that foundation correctly from the beginning—independence of decision making, project focused, speed and agility, and that sense of urgency and moving forward—is very important.

Q39 **Mark Logan:** During your time and after, would you say that the DNA and/or culture of the organisation did not change that much, but it was more the projects that you would be working on that changed?

Dr Dugan: DARPA is a very flat organisation. It tends to take on the strategic initiatives and the style of the director at the time, but the core attributes remain the same.

We talked a little about benefits, strengths and weaknesses. My personal feeling is that the core strengths of an organisation are often also their Achilles heel. Here you have an organisation that moves extremely fast, and needs to. It is very flat. It takes on the strategic intent, the personality and the style of the individuals inside the organisation. That is a necessary attribute of that kind of speed and agility, that kind of independence.

Q40 **Graham Stringer:** Has DARPA been successfully replicated anywhere in the world? Dr Petit, you shake your head.

Dr Petit: I think that Dr Dugan is much more appropriate to answer that question. My feeling was not.

Dr Dugan: This is what we know. We will not get an organisation that replicates the 60-plus years' success of DARPA by creating an organisation that simply rhymes with DARPA. That is not sufficient. This idea of cloning DARPA is really about capturing the core attributes of it and the focus of the organisation.

I feel that we successfully executed the DARPA model when I was at Google in the advanced technology organisation and that we successfully executed it in other organisations. Those examples of success have not achieved the scale of a DARPA—the $3.5 billion a year that DARPA spends in this style of activity—but there are examples of successful execution. It is important to consider the issue of scale and the existence of, or lack of, the human resource to get it right. We simply do not have enough people globally who have executed on this model to do 23 different versions of DARPA globally. It is important to think about how to access the talent, to concentrate the efforts and then work together to make them successful.

Q41 **Graham Stringer:** Is DARPA the right answer to the question that the UK Government are asking? Can it work without it being so closely associated with one Department, one Ministry, in the way DARPA is, because they will be left with the problem of which projects to choose? In defence, it must be a huge number. If you are dealing right across Government, you are going from everything from AI and health to global warming. There is almost an infinite number of choices without the focus of one Department or one major area of science such as health.

Dr Dugan: My personal feeling is that you will need to focus. It is important with the resources available that you focus and use that as a way to launch successfully, to get the attributes right and build out from there into other areas.

As I mentioned, many organisations and Governments have felt this gap, this Pasteur's Quadrant-style gap. They have good fundamental and basic research activities as well as strong industry in certain areas. What is so interesting about the particular approach that DARPA takes is that it bridges that gap. It often facilitates the movement of breakthroughs in basic science and engineering intersected to scale and to impact.

I want to say a word about commercial, because many people have looked at DARPA in the US as defence focused and to the collection of advances that DARPA has contributed to the commercial sector as well. What has to be recognised there is that the Department of Defense is essentially a mini society. It is a mini society that has to be deployed anywhere in the world under very difficult conditions.

The focus of advance of capability for DARPA is on that mini society, but it is not coincidental that advancing things for that mini society has cascading implications for the broader society, whether we are talking about biomedical advances, communication systems, electronics and so on.

One can think of it in that context. It is not a competitive force with your commercial activity. If private capital can do it and it is for profit, private capital should do it.

Q42 **Graham Stringer:** Is the implication of what you are saying that the Government should narrow the focus of any UK DARPA before it is set up and say, "You should be looking at X,Y and Z," rather than leaving it up to the chief executive when the chief executive is appointed and a board is appointed to run it? In what order should those decisions be taken?

Dr Dugan: Some of it will depend on your objectives and your CEO's desires. The way the Wellcome Trust handled it is that they approved the formation of Wellcome Leap dependent on the recruitment of the right CEO and chair who would then shape and deliver the strategy. It is very important to understand that the concept when approved by the board was not in any way flimsy. It had a lot of thought dedicated to it from a talented team. The concept was not muddled. That clarity is partly what

allowed them to attract the talent they felt necessary to execute and be successful.

At least from my own point of view, that clarity of thought was an important part of my accepting the role. It is important to consider that you have both concerns. You want it to have a certain area of focus, you want there to be clarity about the mission of the organisation, which is necessary for getting the right talent, and that talent will help you further shape the strategy.

Q43 **Graham Stringer:** If you were looking to set up a breakthrough research institute, to use your phrase, there are many models around the world for spending research and development money. Would you choose a DARPA model, your own model in Wellcome, the French model or the model that the Howard Hughes Medical Institute has chosen? What model would you recommend to the UK Government?

Dr Dugan: I am a believer in the DARPA or ARPA model. I have spent an awful lot of my professional career executing it in government and in the private sector, now at Wellcome Leap.

The answer to that question lies in your own perception of your gap. Where is the gap that you have? Do you feel that your basic research activities are strong and foundational? Maybe they need to be amplified but they feel strong—you don't fill a gap there. Do you believe that private sector investments are robust? Do you have economic growth where you want? What you are missing is the opportunity to intersect these needs with the basic science and engineering breakthroughs necessary to realise new capability.

If that is what you seek, the DARPA model is a very good one.

Q44 **Chair:** Thank you, Graham. Let me turn to Dr Petit about some aspects of your institute. Part of the appetite to create a UK version of DARPA is to bring in, perhaps, a greater agility to respond to research and development opportunities and, with that, to be outside the disciplinary boundaries that can often be dominant in research institutions. That strikes me as a universal problem from my experience. What are your thoughts in France on how you create your very big institution, as you said, with over 30,000 people and more than 1,000 labs? How do you create the active agility to respond to new research challenges?

Dr Petit: That is a key point. The fact is even if we do basic research we always have in mind the idea of how to apply the basic research. If you consider questions coming from industry or from society, such as climate change or health, you need to have an interdisciplinary approach, because these questions are not questions of mathematics, biology or chemistry. You need to have joint research teams between people from basic sciences and people from humanities and social sciences.

In order to push the walls, we ask the people to have in mind a possible application of their research. To promote the interdisciplinary research is not an objective by itself. Why should we encourage relations between biology and mathematics? In fact, there is no objective reason, except that if you want to study genomics then precisely you have to put together people from biology, mathematics or computer science.

That is a key point—to push people to study. We are not so far from the DARPA model. If we consider the left-right model, which was described during the last testimony session, the right side could be a defence problem but, of course, it can also be a social problem. If you consider climate change, for instance, you need to have basic research but you need basic research in different areas and you need to put people together. That is how we try to do that.

The last word I would like to say on that is that even if you want plurality of research the disciplines are still very important. You can have good and outstanding plurality of research only if you put together people who are already outstanding researchers in their own field. You cannot create plurality of research by putting together people who are not so good in their own disciplines.

To summarise, my answer is that the idea is to try to focus on the right side of the pipeline, to have social questions and industry questions, and then to serve them you need to have plurality of research.

Q45 **Chair:** Referring back to the previous evidence session, one of the points that was made was the importance of the connection between DARPA and the Administration—in particular, the Secretary for Defense and the bridge that existed. In the UK there has been a tradition in scientific research of an arm's length relationship between research and Ministers. It is referred to as the Haldane principle, which means that the Government should determine how much money is available and a process of peer review should decide how it is spent. Will you give me an insight into what extent the French Government and perhaps the President take an interest and express that interest in your work as an organisation, and how any tension is managed?

Dr Petit: The main interest of the President and the Government is demonstrated by the new law that is before Parliament, which should lead to a quite huge increase in the budget devoted to research.

As you know, in France we like freedom and our researchers like freedom, too. The main point is that the Government have learned to say, from my point of view, "Do more chemistry," or, "Do more mathematics." Scientists cannot accept that. If your Government say, "Please put some effort into the climate change subject or on AI," after that you can do all the sciences that you want with all the techniques that you want, but you must have focus. There must be a good equilibrium between the freedom of the researchers and the people who put money

in—in this case, the Government—to have their say. Clearly, it is not so easy.

I think that scientists, at least in France, now accept the idea that to put money into health or climate change is a political decision. People who pay have the right to decide on what their money is spent. After that, the freedom of the researchers should be to imagine all the possible solutions, even involving all sciences, to try to solve the problems.

Q46 **Katherine Fletcher:** I am very struck by the fact that the British have quite a long history of putting a lot of very clever and disassociated fields into a room and coming out with really interesting innovations, Bletchley Park being the classic example with different humanities, scientists and computer scientists all oriented around a common goal.

I was particularly struck, Dr Dugan, by your point that, if your ultimate goal is profit rather than the more existential threat that was the lodestone that Bletchley Park was oriented around, then private capital should do the commercialisation process. I am interested in your views on how that practically translates from a collaborative Bletchley Park-style model, with many very clever people coming at a known problem and working right to left, and how any organisation should be set up to make that effective. The point is that Britain is historically brilliant at coming up with great ideas and then even our warmest supporters would say that we forget to commercialise them. An example is jet engines. If you were to offer us some advice when we start to develop our own, what would you say we need to watch out for?

Dr Dugan: One of the purposes of creating these project-oriented activities, and particularly building a discipline to do that, is that the project, in having a specific capability and goal, has to demonstrate that capability on what we would say is a convincing scale—namely, at a scale sufficient to convince those who would invest on the commercial side, that you resolve to the parts that people thought to be impossible and now you can actually see a path to scaling and commercialising.

That somewhat routinely happens at DARPA. We talk about the Defense Department as the first adopter, but the commercial opportunity is in part realised because you have done more than demonstrate the basic science. You have intersected the science with the technological, the engineering and the scaling-types of activities that are necessary to close that gap.

Even when we talk about the activities at Wellcome Leap, we say that a significant opportunity for us, in building something like a health age, is the intersection of biology and engineering. Those two things together are what creates so many of these commercial opportunities.

You still have to pay attention to how you deal with IP. One still has to make those kinds of decisions properly. In fact, in the investments we will make in Leap and in the investments that were made at DARPA, the

IP ownership belongs to the performer on the project, whether it be an academic institution or a commercial institution. Therein is an incentive for them to commercialise and realise some financial benefit.

You want those incentives to exist. In the US we have something called the Bayh-Dole Act, which allows universities, as an example, to commercialise the IP that they develop under Government funding. It is a very important part of commercialisation.

Q47 **Katherine Fletcher:** Thank you. That was really helpful. The implication of what you have just said is that the Government fund to the point where the idea is proven and at that point we can start to draw private capital in. Are there any examples where there is a taper, or is it always a cliff edge, to give the scientists, engineers, technologists and innovators the freedom to work outside of commercial pressure? Does it ever do that?

Dr Dugan: You can do it in certain public-private partnerships. A good example was the Semitech model in the 1980s in the US, where there was a desire to advance our core capacity in semiconductors. That was a joint activity between Government and industry. Industry contributed to the research. Government contributed to the research. Together they got to a critical mass of investment. The work was considered pre-competitive. All of the industrial partners gained access to the IP. The commercialisation was to them.

There are examples of those kinds of relationships, not that I am aware of a broad institutional design level, but certainly in very specific targeted areas where there is a desire to gain competitive edge through breakthroughs in research and engineering.

Dr Petit: Private-public relations are very important. They are very different if I compare the US and France. I do not want to compare with the UK. A key point in the success of DARPA is the quality of the relations between the public institutions and particular US universities and the private sector. We know that usually in Europe—for what it is worth, the UK has left Europe—these relations are not always so good. That is also a point to be taken into account.

Dr Dugan: I would echo that point. I think that Fraunhofers in Germany are an interesting example of the partnerships that have stood the test of time. I agree that the quality of the relationship between the funding organisation, universities and the private sector is very high and deeply collaborative. It comes from the belief that the breakthrough itself does not make impact. The impact has to be scaled and that happens outside your organisation. We need those partners to do that tough work.

Q48 **Katherine Fletcher:** Could you give me one example of how you would set up to make that relationship work? I address this question to both of you. Dr Dugan, can you give me one example of how we should look to

make sure that there is a fundamental characteristic in our organisation that allows that to happen?

Dr Dugan: I would argue a project focus and a fair competition among potential performers but not necessarily a consensus-driven process. Peer review and quality of proposal is absolutely essential in the sense that any organisation believes that the process is fair to them for the quality of the idea, but it need not be a consensus-driven process. That is very important.

Getting the IP structures set up properly is important so that the performers have a shared incentive in scaling and commercialising activity.

Dr Petit: One point that has already been mentioned is the excellence of research. That is a key point. You will not have undisrupted innovation without research at the highest level. The way DARPA works is to select the best teams putting their efforts into the ultimate goal. It is absolutely clear that the excellence of the academic teams is a crucial point.

Dr Dugan: Part of what happens in the programme design is that it is clear that no single performer is going to be able to go over the objective of the programme and succeed on their own. That is part of what facilitates that collaboration. It is an objective that each cannot achieve on their own but they can achieve together.

Katherine Fletcher: Thank you both for your answers. As a very out-of-date biologist, I will dig up the biologists, the mathematicians and the quantum tunnelling experts and move forward. Thank you both for your time.

Q49 **Chair:** Perhaps I could put to Dr Dugan the point that I discussed with Dr Petit about the link with political objectives and political directions. At Wellcome Leap you do not have that. You are independent of the Government and Governments. You have your trustees and your board. What do you think is the right relationship between the new agency that is promised and Ministers?

Dr Dugan: If what you seek are breakthroughs, it is important to recognise that in their earliest phases those breakthroughs or the activities associated with creating those breakthroughs can feel quite controversial. Having the independence to make those decisions separate from political influence is an important attribute. The agency must operate independently for its decisions on individual programmes.

Broad categories of focus and activity are appropriately set in collaboration. In the US, we do the broad programme elements as part of our budget submission, together with Congressional approval. Individual programmes within those broad categories are the purview of the agency. That is as it should be and that is a necessary attribute of success.

Chair: Thank you very much, indeed. You have both been very clear on that point and on every other point. We are very grateful to you for sharing your experience of two very different but very important and successful organisations—long-standing in the case of Dr Petit's work and relatively new in the case of Wellcome. We are very grateful for your advice. We will draw up some recommendations to Government from your evidence today.