Towards an Applied Science of Research
Thinking More Deeply About the Nature and Nurture of Research

1 Humanity & Intellect
2 Nature of Research
3 Nurture of Research

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THANKS. Good afternoon. A big thanks to Jeff Osborn for inviting me to give this talk. I have been working on this topic as a hobby for almost a decade now, and as a serious effort for about two years. But only in the last few months have I felt that enough progress has been made on this topic to be able to give a full colloquium. So I’m very excited that Jeff gave me this opportunity. Still, it’s early stages and there are lots of missing pieces, so I really welcome your questions and comments – please don’t hesitate to interrupt during the talk.

TOPIC. The topic is “Towards an Applied Science of Research: Thinking More Deeply About the Nature and Nurture of Research.” Throughout my career I’ve been very respectful of the importance of research as society’s formal way of creating new scientific and engineering knowledge. But at this later stage of my career, I’ve also become respectful of how complex a human and intellectual enterprise research is, with lots of variables and moving parts. As managers, policy-makers, and researchers ourselves, we nurture that enterprise as best we can, but we don’t understand the nature of research well enough to be able to nurture it as productively as we would like. This work is an attempt to help remedy that: to begin thinking more deeply about the nature of research, so that we can nurture it better.

OUTLINE. At the left in red is an outline of what I’d like to cover. I’ll spend the first third of my talk on its overarching theme: that knowledge resides in our humanity and our intellects, and that knowledge creation is both a human and intellectual endeavor. Then I’ll spend the middle third diving into the nature of research, and the last third diving into the nurturing of research.

ACKNOWLEDGEMENTS. Before I start, though, let me acknowledge a number of collaborators. My most important recent collaborators have been Venky Narayanamurti at Harvard, and Curtis Johnson and Christina Ting at Sandia. But many others have been influential in my thinking, and have provided support in various ways.
• OVERARCHING THEME. Let me start with the overarching theme that I already mentioned, the theme that permeates my thinking. And that is that research is a complex human and intellectual enterprise, not just an intellectual enterprise. Its motivations are human motivations, and its purposes are human purposes. That doesn’t mean the heart of research isn’t the creation of useful scientific and engineering, i.e., intellectual, knowledge, but humans are at the beginning and humans are at the end.

• TIMELINE. Many of you are familiar with Charles Percy Snow, who in the 1960’s coined the phrase “the two cultures”: the culture of humanists, and the culture of scientists and engineers. He pointed out how different their cultures were, how difficult for even Cupid to get them to appreciate each together. Moreover, each culture has had periods of ascendancy.

• Before the 1950s, humanists were king: if you hadn’t read a work of Shakespeare, you couldn’t be thought to be an educated person. There was a snobbery of humanists looking down on scientists and engineers.

• After the 1950s, especially after the invention and use of the atom bomb, scientists and engineers became king: if you didn’t know what mass and energy were, and who Einstein was, you couldn’t be thought to be an educated person. In the 2000s, with human society embedded in technology, scientists and engineers have all the more become king. Most of our multi-billionaires are from the tech world, and the most sought-after degrees in many schools are in science and engineering. The snobbery is still there, but reversed.

• But, as you can see on the right, I think we may be poised for a reconciliation. Why do I think that? I think that because humanity bookends science and engineering.

On the back end, humanity gives science and engineering purpose, and is its ultimate user; on the front end, humanity motivates science and engineering, and is its ultimate creator. So, in order to understand research, the creation of useful scientific and engineering knowledge, we have to understand it as a human and intellectual enterprise, not, as many do, as a purely intellectual enterprise.
FRAMING. To dig more deeply into the relationships between humanity, science and engineering, let’s depict those relationships within a general framing of human knowledge. In this framing, there are repositories of knowledge, the nouns of the framing, and processes that build those repositories, the verbs of the framing.

R&D. Let’s focus first on the left side of this slide. The two basic entities are the ones in grey. At the bottom there’s humanity, us, and at the top there’s the natural environment. This brings us to the repositories of knowledge, or the nouns of our framing. There are three of these.

The first repository of knowledge, in red, is our human motivations. At a basic level we are of course motivated to exploit our environment to find food and to reproduce, and because our environment is a changing one we are also motivated to explore and learn about our environment so that we can adapt to that changing environment. But at a less basic level we have a complex web of motivations and sub-motivations – to feel psychologically safe, to be liked and respected by others – an evolving repository of knowledge that we might call our “humanness.”

The second repository of knowledge, in green, is our understanding of our natural environment. At a basic level we need to understand how to find food and reproduce. But at a less basic level we have developed a complex web of understanding of our natural environment – where is there food to be found, where is there shelter to raise children, how does mass mediate acceleration in the presence of force – an evolving repository of knowledge we might call science and understanding.

In other words, how humanity interacts with the world reflects its motivations and its understanding of its environment. And if we weren’t also a tool-building animal, that’s all there would be. We would interact with our environment with whatever biology we were born with: arms, legs, eyes, ears. But we are a tool-building animal, so we have a third repository of knowledge, in blue: engineered tools and technologies via which we enhance the productivity of our interactions with our natural environment.

TWO FACES OF T&T. Note that this repository of tools-and-technology knowledge is an interesting one because it has two faces.

The first face of tools and technologies, illustrated on the left, is as an extension of us, serving as artificial arms, legs, eyes, ears through which we interact with our natural environment. In fact, tools are such an integral part of us, we often aren’t aware of when our biology ends and our engineering begins. Roger Federer’s tennis racket is to him like an extension of his biological arm. My eyeglasses are to me like an extension of my eyes. Google is like an extension of your brain.

The second face of tools and technologies, illustrated on the right, is as an extension of the natural environment, serving as a “built,” artificial environment that we interact with as if it were our natural environment. This artificial environment becomes itself something to be explored, something to build a science and understanding of just as we build a science and understanding of our natural environment.

In other words, tools and technologies are on the one hand simply improved ways of exploring and exploiting our natural environment, and on the other hand are themselves an artificial environment that we can explore and exploit.

OUR FOCUS: RESEARCH. OK, we now have our three nouns, or repositories of human knowledge: science, engineering and humanness. And we have our two verbs, or processes for building those repositories of knowledge: research, or exploration, and development, or exploitation. Let’s now zero in on research. And let me be clear that I’m not zeroing in on it because it is more important than development; it’s actually less important. But it is research that I worry about, perhaps because it is less important and easier to let go, but also perhaps because it is longer term and less immediate than development, and so more difficult to understand.
Nature of Successful Research: Implausible Usefulness

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\text{Successful Research} = \text{Useful Learning} = \bar{u}_{\text{post}} \cdot I (\text{Kullback-Leibler divergence})
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\[
= \bar{u}_{\text{post}} \cdot \left[ \ln (1 + \Delta b) - \frac{1}{2} \frac{1}{2(1 + \Delta b)^2} + \sigma_{\text{post}}^2 \right]
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= \text{Implausible Usefulness} = \text{Creativity}
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- NATURE OF SUCCESSFUL RESEARCH. OK, let’s zoom in now on research, in particular on the nature of research, and even more in particular on the nature of successful research (this slide) and the nature of successful research processes (the next slide).
- HEAD OF RESEARCH INSTITUTE. Let’s start with the nature of successful research.
  - Imagine you’re the head of a research institute, and you want to evaluate the success of a piece of research. You don’t want to wait for citations to accumulate, and besides you’re not really sure what citations actually mean. You want to use your own human judgment, because that’s immediate, but you’re wary that human judgment can be fooled. In fact, the most common way for human judgment to be fooled is to judge research as if it’s development, to judge the success of research according to how useful the knowledge gained was, rather than also according to how much knowledge was learned.
  - The reason? My guess is that it is because we are in general biased towards discounting, and misjudging, how much we have learned. It is our common human experience that, once we have learned something, we have difficulty imagining what was like before we learned it. It is a common difficulty in teaching, not to be able to understand why the person you’re teaching doesn’t get something that seems obvious to you. That makes sense evolutionarily – once we’ve learned something, better that it be second nature to us than that we keep remembering what is was like not to have learned it.
- USEFULNESS-LEARNING MATRIX. But if we’re going to evaluate the success of research, we have to keep track of how much we have learned, because research is learning, and successful research is useful learning. To see what I mean by useful learning, imagine that you are searching for something that is in one of these six bins. There are four possibilities that I’ve sketched in this 2x2 matrix, organized according to whether the result of your search was useful or not useful, and whether you learned from your search or not.
  - In the lower left quadrant, you don’t think it is in this red bin, and after doing some research, you find that indeed it isn’t in this red bin. This result is not useful, and on top of that you haven’t learned because all you’ve done is confirm what you thought all along, that it was not in the red bin.
  - In the lower right quadrant, you think it is in this blue bin, but, after doing some research, you find that it isn’t in that bin. This result isn’t useful either, but at least you’ve learned something – what you thought previously has been overturned.
  - In the upper left quadrant, you think it is in this blue bin, and, after doing some research, you find that it is in that bin. The result is useful, but you haven’t learned because all you’ve done is confirm what you thought all along, that it was in that bin.
  - It is the upper right quadrant, outlined in green, where both usefulness and learning come together. You don’t think it is in the red bin, but, after doing some research, you find that it is in that bin. The result is useful, and on top of that you’ve learned – what you thought previously has been overturned.
- IDEA GST. In fact, one can also quantify useful learning using an analysis like the one in the upper left of probabilistic idea generation and test. Your idea, before you’ve done the research, has some prior probability distribution over utility. Your idea, after you’ve done your research, has some posterior probability distribution over utility. If research success is useful learning, then it’s the product of the mean utility of the posterior probability distribution, times how much you’ve learned. How much you’ve learned is the so-called Kullback-Leibler divergence – an information-theoretic measure of how different the posterior and the prior probability distributions are from each other. And the Kullback-Leibler divergence, in turn, is this somewhat complicated expression. I won’t go into the details, except to point out that the expression is dominated by this term \( \sigma_{\text{post}}^2 \), which is a measure of how different the mean posterior utility is from the mean prior utility – that is, how surprised you were that the idea has the utility it turns out to have. In other words, useful learning is mathematically equivalent to implausible usefulness, which you can see now is the same as the green-outlined box we discussed earlier. And, finally, in another interesting twist, this is exactly the criteria that creativity theorists have recently begun to think of as creativity. So: successful research is useful research, which is implausible usefulness, which is creativity!
- HEAD OF RESEARCH INSTITUTE. Let’s come back now to you as the head of a research institute, trying to evaluate the success of a piece of research. It is still your human judgment, but now you know not to fool yourself into overweighting usefulness. You must also properly weight learning and surprise. This isn’t necessarily easy to do, because, again, our bias is to discount learning. So the challenge is to figure out how to watch out for it, now that we know to watch out for it.
SUCCESSFUL RESEARCH PROCESS. OK, we’ve just discussed the nature of successful research. Let’s now zoom in on what the nature of a successful research process is; one that has as its end point successful research. I’ve sketched the process as the birth and death of research ideas, where death occurs in these bins defined by a combination of whether society believes the idea is useful or not, and whether the researcher thinks the idea is useful or not. The process has three steps: the generation of research ideas, the selection of which idea to go forward with, and the testing of that idea. This is a simplification, of course: research ideas don’t arise cleanly like this, but are nested in messy ways with ideas. But these three steps are the underlying engine, and by breaking them apart this way we can see more clearly why they are sometimes executed well and why sometimes not so well. Let’s examine each of these three steps in turn.

- **IDEA G.** First, in blue, there’s generation of research ideas. This is of course a divergent thinking step. You want to come up with lots of ideas. This is where intellectual breadth comes in. The first possibility is the most likely one. It turns out you are wrong after all, and your peers were right. So you give up, go back to the start, and try again. Or do you?

- **IDEA F.** Second, in red, there’s selection of research ideas. This is of course a convergent thinking step. You want to test the idea rigorously, and where intellectual depth and skepticism come in. Great researchers are deep thinkers and also deeply skeptical. They are constantly trying to beat down their own ideas. The second possibility is the most important. Your idea withstands the test. It really is useful, but society doesn’t know that yet and if the idea is surprising enough they are likely to be a tough audience. If you have deep conviction and tough skin, then you forge ahead and begin the arduous task of convincing your peers. But if you fear ridicule, if you fear for your reputation, then you might not forge ahead. You might publish, but in an obscure journal, so as not to attract attention if the community persists in thinking you’re wrong. The third possibility is in the upper right: you think the idea will be useful, but your technical peers disagree. Of course, your technical peers aren’t stupid; they are more often right than wrong, and so there is a good chance the idea really isn’t going to be useful. The only reason you might be right instead is if you have what I like to call “inside knowledge.” From your own deeper analysis, or because you have some new tool that they don’t have, or because you just know something that they don’t, you think they’re wrong and you’re right. If you don’t have inside knowledge, then you’re likely to be making a mistake by going for it, and many make this mistake just because they get enamored of their own ideas. But if you do have inside knowledge, that’s when you go for it.

- **IDEA S.** Second, in red, there’s selection of research ideas. This is of course a convergent thinking step. You want to weed out the ideas not worth exploring further. This is where intellectual depth and skepticism come in. Great researchers are deep thinkers and also deeply skeptical. They are constantly trying to beat down their own ideas with deep not superficial thinking. It’s only when they’ve given their ideas a sufficient beating, and the ideas have survived, that they are willing to move forward. But what exactly does it mean to try to beat down a research idea? There are four possible outcomes of this beating-down process: three of them should lead to death, while one of them should lead to survival, or at least temporary survival until the next step.

  - A first possibility is in the lower left corner: you don’t think the research idea will be useful, and your technical peers agree that the idea won’t be useful. Here’s where it is easy to make the right choice, to kill a bad idea, because everyone agrees that this is a bad idea.
  - A second possibility is in the upper left corner: you don’t think the research idea is a very good one, but your technical peers in the community do think it’s a good one. Maybe it’s in a fad area, so, well, of course it must be a good idea. Here’s where it is so easy to make the wrong choice, to go along with the fad, instead of being skeptical of others, going with your own deeper thinking, and killing the idea. This is a common way of going wrong, because it is typically the easy way to get your research funded, if peer review is at all involved in the funding process.
  - A third possibility is in the upper right: you think the research idea will be useful, and your technical peers agree that the idea will be useful. Here’s also where it is easy to make the wrong choice, to go along with the idea because everyone, including yourself, thinks it will be useful. Indeed, this would be the slam dunk choice if you were doing development, if you were interested in exploiting what you already know will be useful. But this is the choice you have to resist if you’re doing research, if you’re interested in exploration, learning, and surprise.

- The final, fourth, possibility is in the lower right: you think the research idea will be useful, but your technical peers disagree. Of course, your technical peers aren’t stupid; they are more often right than wrong, and so there is a good chance the idea really isn’t going to be useful. The only reason you might be right instead is if you have what I like to call “inside knowledge.” From your own deeper analysis, or because you have some new tool that they don’t have, or because you just know something that they don’t, you think they’re wrong and you’re right. If you don’t have inside knowledge, then you’re likely to be making a mistake by going for it, and many make this mistake just because they get enamored of their own ideas. But if you do have inside knowledge, that’s when you go for it.

- **IDEA T.** OK, now you’ve generated ideas, and you’ve selected this one to go forward with. Now you do the research, you test the idea, in green. This is also a convergent thinking step, where you want to test the idea rigorously, and where intellectual depth and skepticism come in. Here there are three possibilities.

  - The first possibility is the most likely one. It turns out you are wrong after all, and your peers were right. So you give up, go back to the start, and try again. Or do you? Maybe you’re stubborn, and maybe you’ve had some success in the past overturning conventional wisdom. You’re full enough of yourself that you convince yourself you’re right even when you are wrong. This is of course one road to scientific fraud, and is a dangerous one.
  - The second possibility is the most important. Your idea withstands the test. It really is useful, but society doesn’t know that yet and if the idea is surprising enough they are likely to be a tough audience. If you have deep conviction and tough skin, then you forge ahead and begin the arduous task of convincing your peers. But if you fear ridicule, if you fear for your reputation, then you might not forge ahead. You might publish, but in an obscure journal, so as not to attract attention if the community persists in thinking you’re wrong, but to cover your bases if someday the community changes its mind.
  - The third possibility, though, is that you do forge ahead, you convince your peers, and finally you have research success!

- **POSSIBLE MISTAKES EVERYWHERE.** Now, you see that this has been a long and arduous process, and there were many points at which ideas should either live or die, but don’t. Understanding why these mistakes are made, and figuring out interventions that enable the researcher or research team to avoid them, is the essence of nurturing research. Some of these mistakes have to do with human motivations – like this one where one is seduced to exploit rather than to explore, or the one where you are insufficiently skeptical of others and want to follow the crowd. Some of the mistakes have to do with the intellectual environment – like if one doesn’t generate a sufficient number of good ideas in the initial divergent thinking step or doesn’t think sufficiently deeply in the subsequent convergent thinking steps. So in the next couple of viewgraphs, let me discuss the nurturing of research, first from the human side, then from the intellectual side.
Researchers are Humans 1st, Intellects 2nd:
nurturing human motivations and biases

- **HUMAN SIDE.** Let's start with the human side. I start with this side because ultimately I think it is the more important side. Researchers are humans 1st, intellects 2nd, not the other way around. So nurturing our human side is most important. Here I give two examples.
- **MOTIVATION TO EXPLORE.** A first example of nurturing our human side, illustrated on the left, has to do with our human motivation to explore. This motivation many of us have. But we have them to various degrees, and we are influenced by organizational culture, and organizational culture in turn is influenced by organizational architecture. So consider the two organizational architectures illustrated in the grey rectangles.
  - In the organizational architecture at the bottom, suppose we have two R&D departments, one software and one hardware. The software staff are all in sync intellectually, with a department manager who really knows software. The hardware staff are all in sync intellectually, with a department manager who really knows hardware. Suppose now we ask these three blue staff in software to do research, and these two blue staff in hardware to do research, but ask the rest of the staff in red to do development. We try to set up a culture that rewards the blue staff for exploration and taking risk but that rewards the red staff for exploitation and not taking risk. How well will research fare? Not well, in my experience. The culture of development and reducing risk will dominate not just because they are in the majority, but because of the inherent human bias against risk. If our close colleagues in our same department can succeed without taking risk, why would we want to take risk?
  - In the organizational architecture at the top, suppose instead we have three departments. Two departments are development, software and hardware; and one department is research that mixes software and hardware. You ask all the blue staff in the research department to do research, and you bring in a department manager who is keen to build a culture of exploration and taking risk. You ask all the red staff in the two development departments to do development, and you bring in department managers who are keen to build cultures of exploitation and reducing risk. Now how well will research fare? Much better, in my experience. Again, we are humans first, intellects second, and respond first and foremost to our local peers and local culture. The researchers may have less intellectual synchrony amongst themselves, because they span both hardware and software, and they may need to reach out to the development departments to discuss and vet ideas. But they will be in sync motivationally to explore and learn.
- **MOTIVATION TO BE SAFE AND TO EXCEL.** A second example of nurturing our human side, illustrated on the right, actually has to do with two closely related human motivations: first, to be safe, and second, to excel. To illustrate this kind of nurturing, I draw at the bottom a schematic of organizational reward, either monetary or reputational, versus the success of a researcher or research team. I’ve also drawn a lot of noise, because research success is highly uncertain and full of historical accident. In any given year, your worst researcher could have had the most success, and your best researcher could have had the least success. You can see two key features.
  - First, you see that there is a floor to the rewards. Regardless of what research success one has had, there should be a safety net, with reward not decreasing below some minimum. In fact, nowadays most research organizations are moving towards soft money. If you aren’t having short-term research success, and aren’t bringing in outside research contracts, your own organization won’t support you, at least not to do research. Very quickly you are moved to non-research projects. This might in fact be the best, as of course research isn’t for everyone. But it is often not the best, it is often done simply because there is noise and the organization hasn’t been patient enough.
  - Second, you see that there is a steady increase in reward the more successful the research – there must be some kind of encouragement to take big risks and climb the peaks of research success. But there should also be some kind of saturation as, again, there is so much noise and historical accident associated with research success. In fact, most research organizations provide very little slope to this reward curve – effectively the saturation sets in extremely early, much earlier than depicted here. The reason? Most research organizations don’t feel capable of judging the success of their researchers, and so don’t feel able to construct a reward system that reflects that success. That brings us back to the question of research success – if we can get a better handle on that, as we tried to do a couple of viewgraphs ago, then we can construct a reward system that is consistent with our human motivations to be safe and to excel.
But Researchers are also Intellects: 

nurturing the crossing of intellectual boundaries

INTELLECTUAL SIDE. OK, the last slide was about the human side of nurturing research, and I gave that priority because of my belief that researchers are humans 1st, intellects 2nd, not the other way around. But intellects are a close second, so let us turn now to the intellectual side of nurturing research. In this case, one of the biggest impediments to great research is intellectual boundaries that have been imposed by the organization. There are many different kinds of boundaries, and they almost always have understandable reasons for being imposed, but all of them are damaging to the quality of research. Here I’ve depicted two common boundaries.

SCIENCE & ENGINEERING. The first boundary, on the left, is between science and engineering.

Why is there such a boundary? It’s because of two conflations that are very common to make. We conflate research with science, and we conflate development with engineering. Because of that, if we want someone to do research, we typically want them to do science but don’t want them to do engineering. We place an intellectual boundary between science and engineering.

In fact, these are conflations. Research is exploration, and exploration cuts across science and engineering. Engineering reveals new phenomena in the engineered instruments themselves, not to mention revealing new phenomena through new windows they give us into the natural environment. That’s illustrated in this “cycle of inventions and discovery” chart from Venky Narayanamurti and Tolu Odumosu. Science and engineering research don’t always go hand in hand, but they often do, and when they do they feed each other in powerful cycles of progress. Nobel Prizes associated with invention and engineering research include these up here for semiconductor heterostructures and the integrated circuit, and for fiber optics and charge-coupled devices. Nobel Prizes associated with discovery and scientific research include these down here for the quantized and fractional quantum Hall effects. And Nobel Prizes associated with both invention and discovery include these in the middle for the transistor and the transistor effect, and for quantum electronics, the maser and the laser.

So it’s important to disrespect this boundary between science and engineering. It doesn’t mean science and engineering have to be combined, but when it makes sense there should be no restrictions on combining them.

PROBLEM-SOLUTION SPACE. The second boundary, on the right, is between problem and solution spaces.

This boundary has its origin in the belief that research is the solving of difficult problems. And, for sure, many times researchers are given a problem and asked to explore a complex solution space for a solution to that problem. But if the problem is fixed too rigidly, you reduce your probability of research success drastically. Research breakthroughs very often involve searching for a solution to a particular problem, then finding a solution that doesn’t solve the particular problem, but solves a different problem. This is Louis Pasteur’s “chance favors the prepared mind,” the mind prepared to reframe the problem.

In our contemporary funding system, however, you write a proposal to solve a particular problem. You’re encouraged to be ingenious about exploring your solution space, but you are discouraged from reframing the problem and thus solving a different problem. Indeed, you can lose your funding if you switch to a different problem, even if the new problem is more impactful than the old problem. Nurturing research requires nurturing the simultaneous exploration of problem and solution spaces.
Towards an Applied Science of Research

Thinking More Deeply about the Nature and Nurture of Research

1 Humanity & Intellect
   Knowledge Repositories: Science, Engineering and Human Motivation
   Knowledge Processes: Research (Exploration) and Development (Exploitation)

2 Nature of Research
   Successful Research
   Successful Research Processes

3 Nurture of Research
   Harnessing Human Motivations
   Breaking Down Intellectual Boundaries

• THANKS. With that, let me end. I know there are many areas where the thinking is immature. Again, thank-you, Jeff, for the opportunity to expose my thinking to you all, and I really welcome your questions and thoughts.