CASE STUDIES:

HOW TO DO MEANINGFUL AND INSPIRED WORK

GENIUS!

Elodie Ghedin, assistant professor of computational systems biology and member of the Center for Vaccine Research at the University of Pittsburgh, runs a pretty small shop. The way she sees it, the most interesting projects are not confined to the four walls of her laboratory. "I used to work on more 'focused' projects—'my own' projects, more 'singular' research," she says, the quotation marks clearly audible in her voice. But since she discovered the power of genomics, the most exciting way forward has been through ambitious collaborations.

Something about her approach seems to be working. In September, Ghedin, 44, was awarded a MacArthur Foundation fellowship–the so-called genius prize bestowed on individuals who show exceptional creativity and self-direction in their field. She initially mistook the cryptic e-mail message from the foundation as spam—she'd been getting a lot in recent weeks—and quipped to a friend that unless there was money involved she wasn't calling this Robert Gallucci guy back. Good thing she Googled him and found out he was the foundation's president.

There was, of course, money involved—$500,000, to be exact, no strings attached. But what made her feel most honored was how recipients are chosen. "It's an anonymous nomination, and then they require multiple letters of support," she says. The recognition from people in her field "was incredibly flattering."

This September, Elodie Ghedin received a MacArthur fellowship, the so-called genius award. What better occasion to delve into the nature of inspiration?

DRAWINGS | LEONARDO DA VINCI

ILLUSTRATIONS OF SCIENTISTS | ROB KELLY

GEOMETRICAL DRAWINGS ABOUT TRANSFORMATION FROM CURVED TO RECTILINEAR SURFACES AND VICE VERSA. FROM *CODEX ATLANTICUS*, LEONARDO DA VINCI (1452–1519). © VENERANDA BIBLIOTECA.

Ghedin

Her colleagues say the award is richly deserved. "Technically, she's outstanding," says Eddie Holmes, a molecular evolutionist at Pennsylvania State University who has worked with Ghedin on sequencing influenza genomes for the past seven years. And yet, he says, "there are many people who work in the technical stuff, but Elodie also has an amazing ability to understand the biology and the evolution and the bigger-picture stuff, as well."

In the early 1990s, for her master's degree in environmental studies at the University of Quebec in Montreal, Ghedin traveled through villages in rural West Africa measuring the bacterial and chemical content of drinking water. "There were tons of parasitic diseases," she recalls. "I saw cases of elephantiasis, leishmaniasis, schistosomiasis." She came back resolved to study the biology behind the ravaging force of these pathogens.

For her PhD research, she developed a potential diagnostic for leishmaniasis—a project that she jump-started in the lab herself, says McGill parasitologist Greg Matlashewski, Ghedin's PhD supervisor. "She would develop these DNA constructs to express things in *Leishmania* and control their expression," he says, and often he doubted it would work. Almost invariably, though, it did.

As a postdoc at the National Institute of Allergy and Infectious Diseases in Bethesda, Md., Ghedin began to delve more deeply into genomics. But her first real taste of leading a genomics-based collaboration came in 2005, when as a research scientist at The Institute for Genomic Research in Rockville, Md. (now part of the J. Craig Venter Institute, or JCVI), she led the effort to analyze the genome of *Brugia malayi*, a parasitic worm that causes elephantiasis. She assembled the world's experts on the worm to do so—about 50 scientists, each spending a week during a two-week period lodged in front of a computer tussling with their favorite genes. "Every day, morning to night, we were just sitting there," says Sara Lustigman, a molecular parasitologist at the New York Blood Center who got to know Ghedin through the experience and remains a frequent collaborator. Surprisingly, says Lustigman, it was really fun. "It was really an example of how she extracts the best from people," she says.

Ghedin's six-member lab at Pitt applies innovative genomic techniques to an array of very small troublemakers, including *B. malayi*, viruses like influenza, and microbes. Ghedin, who maintains a joint appointment at JCVI, credits this mix of systems as a major source of inspiration. In lab meetings, everyone in the group weighs in on the projects under way, and often the best ideas come from people who are working on a different organism. "I think that's creativity, when you see connections that are not obvious," she says. "And for that, it helps to cast a very wide net in your research."

Ghedin plans to use her MacArthur award money to advance her work with *B. malayi*. Finding ways to kill the worm has proven very difficult. It is itself infected with a parasite—an intracellular bacterium that is necessary for its survival—and these symbiotic partners trick the host immune system into overlooking them both. Ghedin aims to identify proteins secreted by the worm and start to dissect their immunomodulatory talents. Because parasitic diseases like elephantiasis don't affect many people outside the developing world, she says, "that's where I always have the most trouble getting funding."

On the flu-virus front, Ghedin and collaborators have most recently been tracking how the composition of strains in a viral population mutates and how the virus is transmitted between hosts. In a virus like HIV, that change is dramatic throughout the many years it inhabits a human host. Influenza, though, generally infects individuals for a week, tops, so much less variability would be expected, but no one knows for sure.

"It's such a basic thing to understand about the dynamics of that virus," she says. "We are designing vaccines with no idea of what's going on." —*Alla Katsnelson*

In 2005, Ghedin led the effort to analyze the genome of *Brugia malayi*, a parasitic worm that causes elephantiasis, the first of her many genomics-based collaborations.

CREATIVE FEARLESSNESS

AND OTHER SIGNS OF LIFE

A CONVERSATION WITH

JEREMY BERG AND GEORGE WHITESIDES

During the University of Pittsburgh's Science2011, we pulled aside George Whitesides and Jeremy Berg, plied them with a couple of beers (courtesy of Pitt's N. John Cooper, dean of the Kenneth P. Dietrich School of Arts and Sciences), and asked for their perspectives on what makes genius happen. Whitesides, who gave the Provost Lecture at the science festival, is the Woodford L. and Ann A. Flowers University Professor at Harvard University. The chemist by training is known for his astonishing breadth of inquiry and ability to contribute to many fields, including nanotechnology, microfabrication, and microfluidics. (He has authored more than 1,100 publications.) Berg, Pitt's associate senior vice chancellor for science strategy and planning and visiting professor of computational and systems biology, is highly regarded for his work in molecular recognition processes and for his scientific leadership. Until earlier this year, he directed the National Institute of General Medical Sciences. (To read more about Berg, see our profile on p. 22.)

Berg

JEREMY BERG: So one thing that we'd like to talk about is creativity, and what leads to creativity. And one thing that I have always admired about your work is the breadth of fields where you've made creative contributions. Clearly it's not the sort of genius where a person has a flash of insight, [but rather where someone] does the same thing over and over again in a clearly intentional, systematic way. So I'd be interested in your strategies and what you think has led to your successes.

GEORGE WHITESIDES: I think there are two thoughtful, well-considered strategies in this, and I would say the first is simply laziness. So, one definition of creativity is doing something that other people don't do. The nice thing about working in areas where other people haven't worked is you don't have to read the literature, … and you can do it according to your own pace. …

I'm a big believer in the notion [of starting from] something that you think is interesting and emotionally engaging and something, ideally, that other people aren't doing. So if you have a problem that has that characteristic—if you look around, and you see evidence everywhere, you can pick problems in health, you can pick problems in the environment, you can pick problems in just phenomenology of nature, and start with something—Where does lightning come from? How do we actually increase the lifespan? What do you do to make pure water? Those are all interesting, good problems that people care about. And the idea of starting from something that's already in the literature strikes me as just an intrinsically bad idea. If it's already in the literature, why are we wasting taxpayers' money doing it?

PITT MED: How do you develop an idea without relying on the past as a foundation?

WHITESIDES: The thing about science that's so wonderful is you don't have to be particularly smart to do good science. If you pick a good problem, nature does it for you. … There are other fields of science where that's not true. You can't be a [standout] mathematician without being a really, really good mathematician. You could be a very good chemist or biologist without being breathtakingly smart.

Whitesides

BERG: Just to push back on what you said: Obviously what you talked about today—protein/ligand interactions and drug design—is something a lot of people have thought a lot about. But what's certainly one of the messages I got from your lecture is "Don't believe everything you think." That, you know, questioning the sort of underlying assumptions that have been made and digging into the fundamentals can lead you in interesting directions.

WHITESIDES: Yes, but there's also another thing about that problem, and that is we know that people have been trying to design ligands to fit proteins for as long as you and I have been in the business. And, you know, it doesn't work. Basically, retrospectively, people will claim success, but it basically doesn't work. And when smart people well equipped with the best tools available try at something for years or decades and it doesn't work, you begin to get the idea that there's something underlying that's wrong. So in that particular area, it's a little bit of a special circumstance because the relation of water and biology is actually a big, important problem, and I don't think that anybody would argue that. And there's been something wrong with our ability to understand how molecules interact in biology—which means in water. And we've tended, because we didn't know how to do

it, to neglect the solvent part of it. ... So, we were, in the [Thomas] Kuhn sense, that is "the nature of scientific revolutions," we were forced to look at water. But not smart in looking at water.

And Kuhn has a notion, which is that revolutions in science occur only in special circumstances, and those circumstances come from the fact that scientists are just as lazy as anybody else. And most scientists … basically make sausage. They repeat work, or extend work, or do whatever they're doing, which is fine. But every once in a while, the theory that's available and the importance of the problem [are] such that you find the theory simply does not explain what's there. You can't get it to work. It won't work. Then somebody has to sit down and try to figure it out. If you can figure it out, if it is figure-outable, and there is a new direction, then that becomes a revolution.

A classic example is quantum mechanics—where, in 1900, physics was regarded as dead because Newton's laws explain mechanics, and the laws of Maxwell, Maxwell's equations, basically explained electricity and magnetism. The only problem was there was a little phenomenon called the ultraviolet catastrophe, where distribution of power radiated by something didn't fit with what was predicted. [So when you did] the simple experiment in which you took a prism and a slit, and you took the solar light and spread it out on a wall and instead of being perfectly continuous, there were these funny black lines. There was nothing in the theory at that point that explained the black lines. And try as you might, you couldn't get a consistent theory to explain the black lines. So what happened in 1925 was this flurry that led to quantum mechanics. Quantum mechanics [didn't say] that Newtonian mechanics and Maxwell's equations were wrong—in fact, they're right—but that there's an underlying story which shows that [there are] other things going on underneath that we hadn't known. And I think there's the same kind of thing in biology. I think we don't have all the tricks, all the basics, of understanding biology.

PITT MED: Is there a way to prepare a mind to notice these things?

WHITESIDES: One of my views is that the way you prepare people to do that is you encourage them when they're in the stage where they can be encouraged not to be timid.

That is, find something that you think is important where the answer isn't actually known. Then the encouragement you can give as a research director is, "Go try it. If it's possible—you're as smart as anybody—then it'll work. …" It won't work every time. But for good people, it basically always works.

BERG: One issue, which I think is really important in thinking about education, is many problems that are interesting and important are at the interface between traditionally separate fields. So the temptation is to work at an interface, but that only works if you actually know a lot of the fundamentals about one side of the fence or the other. In my case, I had just the luck of being completely convinced I wanted to be a chemist then [discovered] biology later on. So I learned a lot of fundamental chemistry, including from people around here, then moved into biology. … So that gets you a leg up on a problem where you can apply a new tool. So the ideal training environment: You don't let people know where they're going to go, train them in one field, and then say, "Now you can look behind the curtain and find out what the new direction is." The danger with [being too] interdisciplinary is teaching people a lot about the border between two fields … is great for the next three or four or five years where that's an interesting frontier, but when that problem gets solved, then they don't know enough to go find a new one.

WHITESIDES: There are things that as a scientist you actually have to know. You have to know thermodynamics. You have to know something about descriptive metabolism and related things. You have to know how catalysis actually works. You have to know the fundamentals of statistical mechanics and quantum mechanics. You have to know something about electromagnetism. These are all hard subjects that require bending your mind. But the laws of electromagnetism in biology are exactly the same as the laws of electromagnetism in physics or in circuit theory or anything else. You get it early, you're then prepared to talk to anybody, for one thing. And [you're able] to do research without doing truly stupid things. My entire career as a consultant has been spent doing one thing, to ask the question—basically, *Does the proposed project violate the second law of thermodynamics?* And, frequently, it does. When it does, you can say with perfect confidence, *You should not do this because it will not work.*

"I think we don't have all the tricks, all the basics, of understanding biology."

BERG: In physics and chemistry, what you need to know is well defined. One thing I learned moving into biology and medicine is that … the same level of information is not quite as fundamental in terms of being able to write it down as facts or equations. … I would come up with these wonderful theories and would talk to a real developmental biologist who would say, "That can't possibly be right because, you know, it wouldn't predict this and this and this—and those are all known to be absolutely true." So there's a base of knowledge in lots of fields you need to master in order to avoid doing things that are just silly.

WHITESIDES: There is another side of this, though. The word that I've come to be very unhappy with is the word "apprentice." We hear this often in science—that a graduate student is an apprentice in the research group, and that he's learning the techniques of the master. It's exactly the wrong way to do it, because the issue is that the master is the master of whatever—the master is master in that time and place—and five years later, it's going to be something else.

I think the master class environment in which [a young person does] something with [a senior person] who really knows how to do it well, and [the senior person] helps you to do it rather than teaches you how to do it, is the right way of doing things. And the great thing about the U.S. system, at least in the past, has been that as a young, independent investigator, as an assistant professor,

you could go do what you wanted to do. I have to say that I'm a little worried right now that there's so much emphasis on getting money that people begin to try to game the system as opposed to doing what they really want to do. That's pretty troublesome to me.

BERG: I would second that. I was also blessed with supervisors and mentors who would give me the freedom to go off and do things and make mistakes. It's a lot like parenting. … You know the mistakes to be made, but you want somebody to feel comfortable exploring … and if they're heading … onto the freeway, you go over and sort of nudge them back away from it.

WHITESIDES: Either that, or if you note they're having problems staying off the freeway, maybe the freeway is the right place.

BERG: I think the fear, and I share [your] concerns, is that if you build cautiousness and timidity into the system, you're going to seriously limit what comes out the other way. … When I was in the NIH, we were involved with developing a couple of programs. And one of the things that was most satisfying about the programs—they were intended for young investigators and highly innovative projects—was I got several e-mails and phone calls from people who didn't get [one] award, saying, *It was so much fun to write about what I really want to do as opposed to what I thought I could get funded to do. And it really helped.*

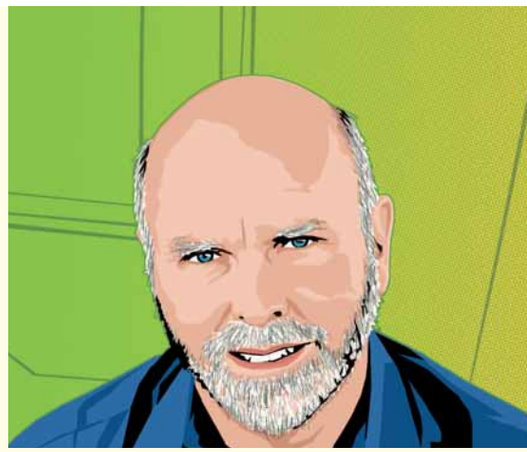
WHITESIDES: What I tell my students is that what I want them to do is to come to me and astonish me. To come with an idea I just never would have thought of myself. And they do it regularly.

—Interview with Joe Miksch

WATCH FOR THE PITTMEDCAST ON ZINIO'S ONLINE NATIONAL NEWSTAND.

ON INSPIRATION

WITH J. CRAIG VENTER



Venter

J. Craig Venter *received the Dickson Prize in Medicine this year at the University of Pittsburgh's Science2011. Venter is founder of the J. Craig Venter Institute (JCVI) and is arguably one of the nation's most productive scientists. His teams at JCVI and elsewhere have developed genomic tools that are transforming medical science by taking on ambitious projects like decoding the human genome, the first human diploid genome (Venter's), and 165 genomes from microbes in the world's oceans. One of his teams has also created organisms from synthetic genomes—he foresees a future in which scientists can "write the computer code of life." Venter set aside some time for* Pitt Med *to share his perspective on the nature of inspiration and how to support inspired people. Edited excerpts follow.*

PITT MED: What do you look for in people you recruit whom you hope will do meaningful and inspired work—will make significant contributions? I suppose we're talking about the scientific level, but if you want to talk about other realms, you can do that, too.

J. CRAIG VENTER: I think the rules are pretty applicable [for all], as far as I can see. Obviously, we start with people who are generally bright people. That shows up in all kinds of ways. Genius is such a relative term. Malcolm Gladwell has looked at all different kinds of intelligence. ...

I've not known too many, if any, really brilliant people who were lazy. So somehow the energy of doing things plays a big role. I know I do much of my learning by physically doing things and by trial and error.

I think [with] people that exhibit genius—other than the kind of genius that shows up with mathematical prodigies or physics prodigies who make their major breakthroughs out of sheer brain horsepower, usually in their early 20s, or 30s at the latest—inspiration comes from a variety of sources. Mainly from people who, you can tell, look at the world a little bit differently than others. I never con-

"I've not known too many, if any, really brilliant people who were lazy."

sidered being an outsider when you come into a new field to be a disadvantage.

If you've read any Sherlock Holmes, you know that he didn't keep a lot of trivia in his head, because he didn't want to clutter up his brain with things. To some extent, that's what happens when going through the school system. We learn how to memorize things, and we clutter things up with lots of memorization versus understanding systems and asking fundamental questions about them.

[He then talks about the fresh perspectives of young scientists like Pitt's Elodie Ghedin (who holds a joint appointment with the Venter Institute) and the Venter Institute's Dan Gibson (who figured out how to assemble and synthesize DNA), and how they used their gifts to contribute to the new field of genomics, a field that "didn't exist long enough for anyone to have any preconceived notions."]

In Elodie's case, she applied [her gifts] to making a big difference in what we're doing in viral analysis. I think just a different perspective has a huge impact.

PITT MED: What about the working environment? Do you put a lot of thought into how to set up things so you don't dampen creativity or inspiration?

VENTER: I put a lot of thought into that.

In part, first off, trying to make it an environment that I find healthy for me. I had this great teacher in high school, Bruce Cameron, who was—when I got back from Vietnam and enrolled in community college—talking about the creative process. Even in writing it's contrary to what people think—[that writers are] inspired by misery, living in difficult conditions… His argument was that people are at their creative best when their pleasure tanks are full. It's hard to think about solving the world's problems if you are hungry or sick or tired or constantly worried about other things.

So I try to keep my pleasure tanks full. [Laughter.]

PITT MED: Do you see missed opportunities where organizations may have had well-intentioned ideas and instead dampen and quash creativity? You don't need to name names.

VENTER: Sure. It's our entire education system. Our university system. And how we construct most businesses. …

The stovepipe academic model … doesn't work very well. I think a lot of my success has been from getting rid of those constraints.

I find most people really like working on teams and on projects that are much bigger than anybody but where their unique expertise is actually required and makes a difference.

… I think the environment is a very, very key part of creativity. I think people probably have even more creative ideas than even they realize, [but the environment might not be] conducive for their expression.

PITT MED: What sorts of ways does the academic environment quash creativity?

VENTER: Well, rote memorization versus comprehensive understanding. Why? Because you can quantitate it. The same way universities, for faculty promotions, want to count publications and citations. …

I've often joked that people prefer those systems because they can count and they don't have to read. So if you have to actually read somebody's study and understand it and decide whether it has value, that's totally different from just saying, "Well, 300 other people have cited it, [so] it must have value." Even if 300 other people are citing it and saying, "This is a great example of crap."

[Also], this is a nonscientific notion for someone who is a geneticist and who sequenced the human genome—but bright people have bright eyes. It looks like there's a light on in there. [Laughter.]

… I think being in the Vietnam War from a very young age had a huge impact on the rest of my life. Because you learn in war, certainly the biggest thing you have to lose is your life. And once you get past that … It certainly changed my risk outlook on things. I've not been afraid to fail or walk away from things. I've rebuilt my career a few times. [Laughter.] I think that it's really amazing in science the number of people who are actually afraid to do the experiment. Whether it's fear of failure or fear of success, a lot of people in science can't bring things to closure. They will drag on a six-month study for 10 or 20 years.

PITT MED: Think about what it must have been like for someone like Thomas Starzl, a surgeon who cared about his patients, to take the risk. Of course, these [patients consenting to experimental procedures] are usually people who have no other alternatives. But it must be scary to be the one who could immediately end a life because of trying something new.

VENTER: But how much worse is it to not try?

It looks like optimism and pessimism are probably genetic traits. I think I've been quoted on this before: It's usually the optimists that accomplish things. You have to have that life-affirming energy.

—Interview with Erica Lloyd

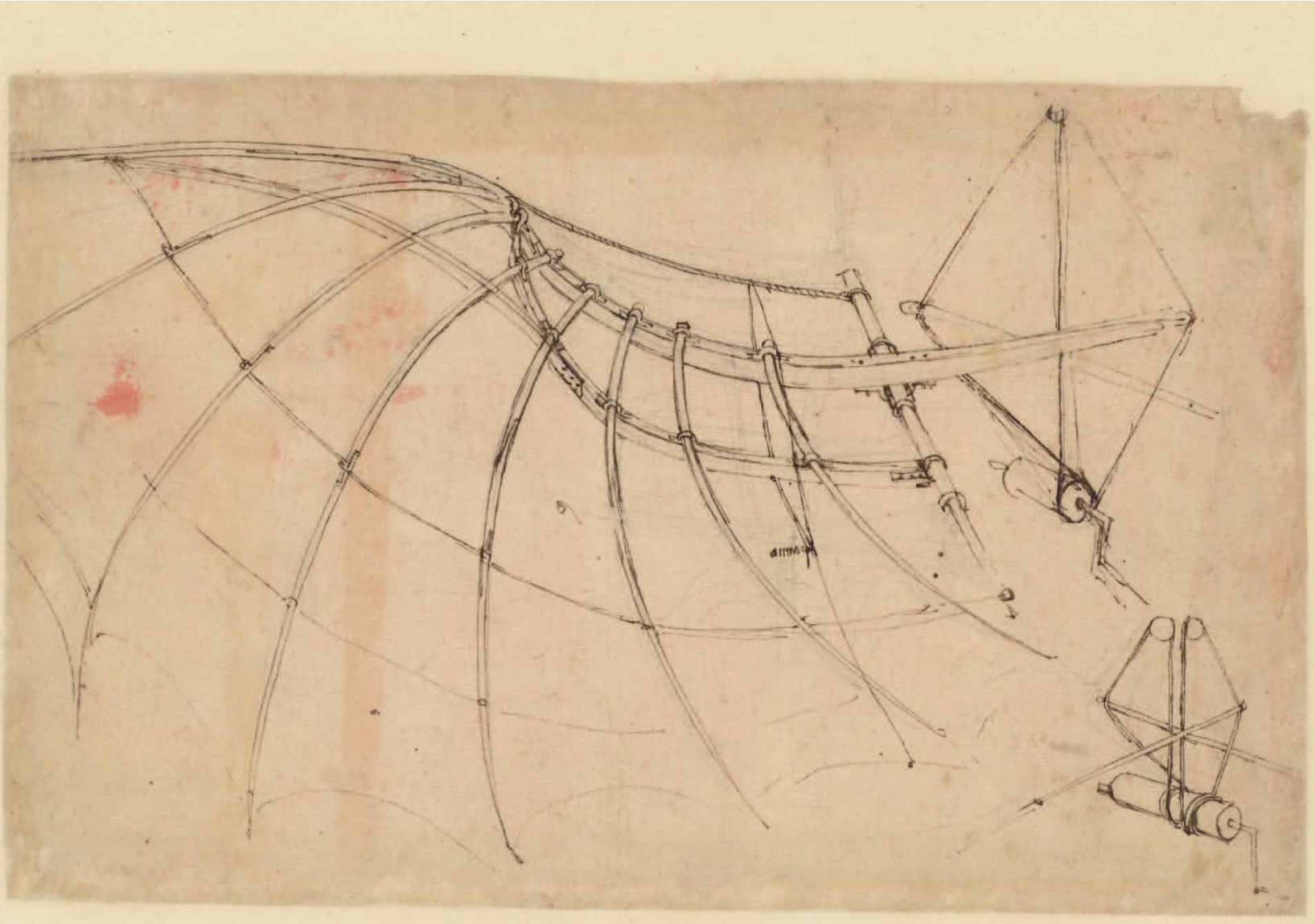
THE FIX FOR FIXATION

To boost creativity, beware the psychological phenomenon of fixation, warns innovation expert Christian Schunn, a PhD and Pitt professor of psychology, learning sciences, and intelligent systems who studies problem solving and creativity as a senior scientist in the University's Learning Research and Development Center. "The first idea that comes to mind can block your ability to come up with other ideas," he explains. "One answer can get stuck in your mind, even when you know it's a bad solution."

He offers these tips to avoid fixation:

■ **Put a twist on the classic brainstorming scenario where a group gathers in front of a whiteboard as a facilitator scrawls the ideas each person calls out. "That way is pretty much guaranteed to produce fewer ideas," says Schunn. "Once people see someone else's ideas, they get stuck on them." Instead, have everyone jot down suggestions first by themselves, then compile and review them together.**

■ **Likewise, when doing a literature review, instead of having everyone**



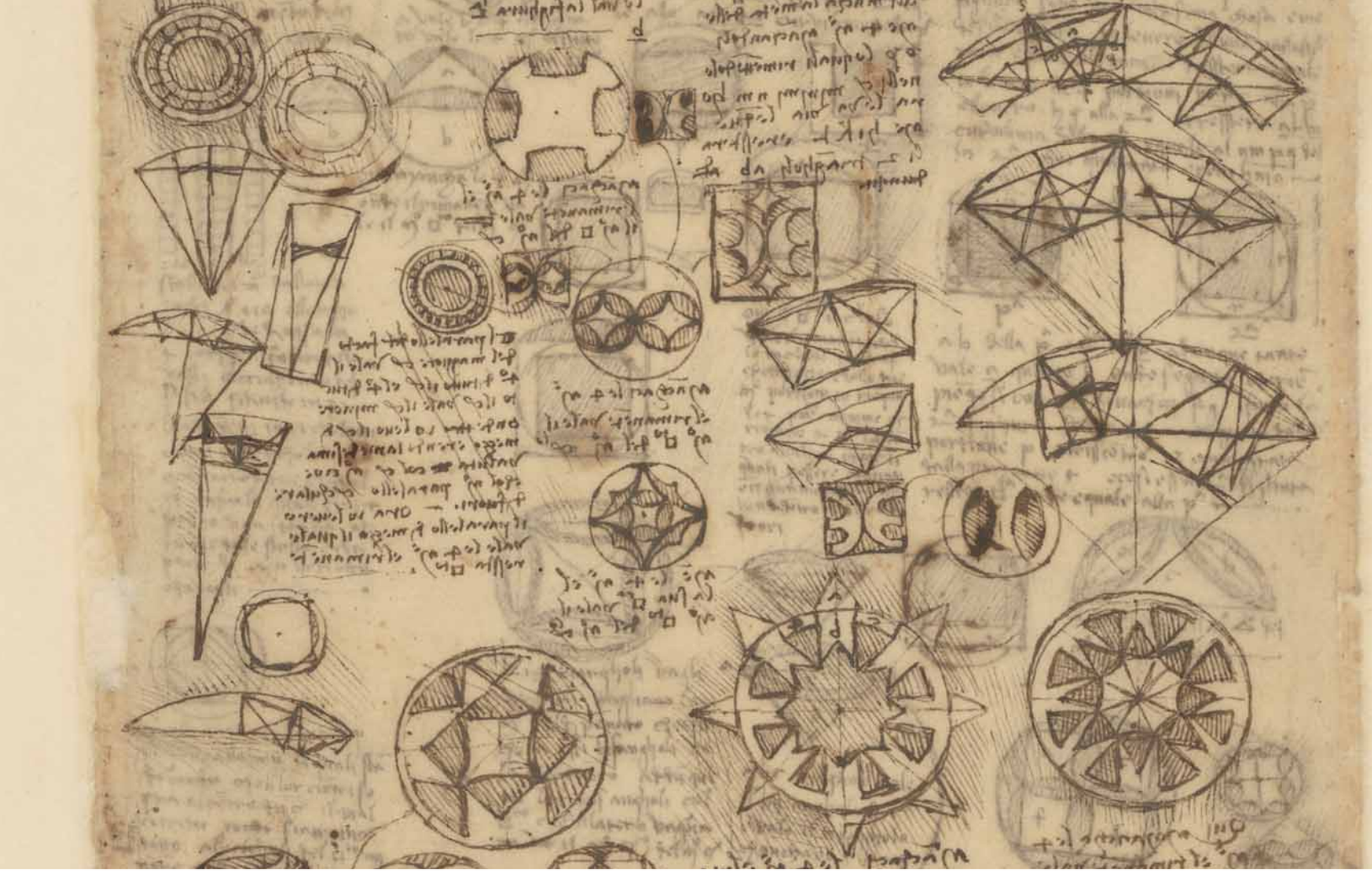
WING COVERED WITH A CLOTH AND MOVED BY MEANS OF A CRANK WINCH; BELOW RIGHT, DETAIL OF THE WINCH. *CODEX ATLANTICUS*, LEONARDO DA VINCI. © VENERANDA BIBLIOTECA.

on the team read everything, divide into subgroups that develop expertise in a particular realm of the problem. When the groups come back together, they'll enrich one another's thinking. "When you have people drawing from a broader pool of analogies, there's more you can draw on," he says. "Spreading the literature review around is a way of not getting the core thinking stuck on one similar path."

■ Try new metaphors. For example, if the problem is viral infection, consider re-framing the problem as "how things get in," opening your mind to such concepts as keys in locks, basketballs thrown through hoops, and even groundwater seeping into a well. "If you think about the problem in very specific ways," Schunn explains, "the problems that are already associated with it come to mind. By categorizing your work as a more general problem, it frees up associations to more general solutions."

■ Don't go in too deep too soon. For example, Schunn points out, computer modeling can demonstrate briskly whether a research approach is headed in the wrong direction, saving time and money. Yet simulations demand a level of detail that can pull a scientist deep into the weeds early in the process. "You can get stuck thinking about the details and prevented from thinking in more general ways," Schunn cautions. "Go back and forth between a very detailed model and just sketching it out on paper or some way that's purposefully sloppy so you can think in more general ways. People can get very attached to the specific thing they built on the computer and lose the forest, as it were."

—Sharon Tregaskis



GREATER THAN THE SUM

PITT PEOPLE: BETTER TOGETHER

Murat Can Cobanoglu, who trained as a computer scientist in Istanbul, applied to graduate school with the intention of one day upending the drug discovery process.

Now, as a PhD student in the joint University of Pittsburgh School of Medicine/Carnegie Mellon University computational biology program, he seems to be off to a good start. Cobanoglu is certainly energized by the weekly lab meeting in Room 3065 of Pitt's Biomedical Science Tower 3, where he muses over microscopic goings-on with a crew of disparate members that includes engineers, chemists, and physicists.

"When a chemist looks at a drug compound, he has insights someone else might not have," says Cobanoglu, who has been developing *in silico* models to predict drug-protein interactions. "[I] look at the computational methods. And the physicist looks at the interactions between the molecules in a molecular dynamics simulation and again has a valuable insight.

"When you combine each of these different people with their different backgrounds, it makes for an excellent and very fun environment and super-creative lab meetings."

The spirit of what's happening in Room 3065 can be found across campus. When speaking with new Pitt med recruits, as well as longtime faculty members, what often comes up is how extraordinarily welcoming the environment is to collaboration. Some have suggested it's a Pittsburgh thing—the task-oriented bent of people who choose to live and work in a rust belt/near-Midwest city.

"[In some academic towns], they don't even talk to each other in their own departments. They take it as a point of pride," says Christian Schunn, a PhD professor of psychology, learning systems, and intelligent systems, who studies problem-solving and creativity as a senior scientist in Pitt's Learning Research and Development Center. He says Pittsburgh thrives on a "help-your-neighbor, talk-to-people-on-the-street sensibility."

Schunn may be on to something, but there's also strategic thinking behind the culture that has arisen.

A collaborative ethos seems to have seeped into the soul of this medical school at least since the late Thomas Detre, who oversaw Pitt's health sciences from 1984 to 1998, began breaking down territorial strongholds by building institutes and centers. Observers say that the academic environment has become optimized in the last decade in a number of ways, from the "open lab"

design of new facilities to deciding who will lead programs. "There's a great deal of thought invested here to build the best teams and build an environment where there are no barriers," says Joan Lakoski, associate vice chancellor for science education outreach, health sciences, and professor of pharmacology and chemical biology.

"We never worry here at Pitt about where someone is housed. We just say so-and-so is the best, and we pick up the phone and talk to them."

She says that Arthur S. Levine, senior vice chancellor for the health sciences and dean of the medical school, delights in his work as a talent scout. "He gets the best minds and brings them together."

When Simon Watkins joined Pitt's faculty in 1991, advanced imaging was considered technically demanding but not much more. The young scientist, now a professor of cell biology and physiology as well as of immunology, intended to establish the intellectual rigor of his field.

"I had this vision of building a center that would be at the edge of what you can do with optics and microscopes and computers," says Watkins, who founded the Center for Biologic Imaging shortly after he arrived and has overseen its growth into a 20-person research staff that works with scientists from across campus and around the world. "I was always given the resources to build that dream."

Early in his own tenure, Watkins was integrated into multiple investigations that continue to this day by senior faculty who already had established projects and funding streams. "We tend, generally, to cut the pie into thinner slices and get more people involved," he says.

Watkins is a scientific partner in an astonishing number of studies—he himself is "very active" in 60 or 70 at the moment, and his group is contributing to perhaps 250. His is a special case because other labs rely on Watkins for cellular imaging expertise, yet, Lakoski notes, the University has made the financial piece for him and others "virtually seamless, which offers tremendous flexibility."

"No one is bean counting," she says. If faculty get together to apply for a grant, the collaborators themselves agree on the division of labor and the money follows that plan.

"In fact, you are rewarded for taking risks," Lakoski says, pointing out pilot programs and bridge funding pools that reward collaborative activity. "A number of funds are designed to bring clinicians and basic scientists together."

Watkins says it's important to think about synergy when bringing in new people, too. "We look at how [prospective hires] fit or fill the needs of the larger medical campus," says Watkins, comparing the mindset to that of a landscaper choosing new plants to enhance an existing garden.

"Because we know where people will fit, we know whom they're going to collaborate with when they come here. There's nothing worse than bringing in [junior scientists] and then isolating them in their own lab."

As medicine and science get more complex, investigators and physicians need to be able to turn to sophisticated colleagues with differing expertise. And with federal funding getting sparser, they will have even more incentive to partner. That said, even with support from on high at Pitt, there's plenty to finesse on the ground.

Cobanoglu's mentor, PhD scientist Ivet Bahar, traveled from Turkey to Pittsburgh to establish a Center for Computational Biology and Bioinformatics in 2001. Her first few years here weren't exactly a walk in the park.

"When I joined this university, I was frustrated about not being able to speak the same scientific language with many people here," says Bahar. So she proposed, with the encouragement of Levine, that the school form what is now the Department of Computational and Systems Biology (systems biology being the field that uses modeling to investigate how the whole—whether a molecular system or an organism—is bigger than the sum of its parts). The department would bring together faculty with expertise in biology, chemistry, engineering, immunology, math, and physics. A year after the founding of the department, she campaigned successfully for the formation of the school's joint PhD program in computational biology with Carnegie Mellon (Cobanoglu's program)—another step in the process of building a common vocabulary among scientists from different fields.

Bahar requires that each member of her group develop a fluency in the language of computational biology: Imagine the United Nations conducting business exclusively in Klingon instead of using translators to bridge the chasm.

"It makes for super-efficient communication and collaboration," says Cobanoglu.

"What brings us together is the great opportunities in this field right now … in the post-genomic era," says Bahar, the John K. Vries Professor and Chair of Computational and Systems Biology. "We are all excited about our ability to solve some longstanding problems."

"The literature shows that teams composed of diverse individuals with different technical backgrounds—backgrounds in terms of where they trained, different outlooks—outperform individuals every time," says Lakoski.

Yet, the hazards of miscommunication among those trained in different fields are substantial.

"If you have a way of dealing with the process of getting to common ground, the overall diversity is helpful," says Schunn. "But a lot of smart teams go down in flames because they can't resolve their differences."

To head off probems before they start, Schunn advocates face-to-face meetings early in a collaboration. "You need to be able to draw, point, and follow up on quizzical looks in ways that the telephone or Skype just aren't great for," he says.

Such contact also builds trust. "If you hang out with people and get to know them, you can come to a different ability to understand why they did something differently from the way you might have done it."

Lakoski, who gives the first lecture in an annual course on team science (yes, there's a course) for clinical and research faculty from all of the health sciences schools, says, "Team science takes longer because you have arguments, people don't understand each other's perspectives, and, until recently, people haven't had training. It's not like you swallow a pill, and suddenly you're a team scientist. It takes practice."

Pitt is educating its future clinicians along these lines, as well.

Like research, caring for patients is a joint effort more than ever today. So the University's health sciences schools have been building awareness of the importance of cross-disciplinary communication as part of the curricula. In 2010, a team of students from the schools of pharmacy, nursing, and medicine trained together to compete in an interprofessional competition at the University of Minnesota. It was Pitt's first time sending a team. In the contest, the Pitt students spent hours on a fictional post-mortem. Then, before a panel of judges, they presented their analysis of what led to the death and a proposal for how to avoid similar outcomes. Groups from nine institutions competed.

Guess whose team took home the top prize.

—Sharon Tregaskis and Erica Lloyd

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