Endless frontier of plasmonics: a conversation with Naomi Halas

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Advanced Photonics associate editor Dr. Jia Zhu (left) interviewed Dr. Naomi J. Halas, the Stanley C. Moore Professor of Electrical and Computer Engineering at Rice University and winner of the 2022 Eni Energy Transition Award for developing light-powered "antenna-reactor" catalysts for industrial-scale hydrogen production. Readers are invited to enjoy the interview also in video format, https://doi.org/10.1117/1.AP.4.6.060501.s1.

Halas: Hi, Jia.
Zhu: Hi, Naomi. Congratulations on winning the ENI Energy Transition award. It's a really big deal, very prestigious award. Congratulations on that!

Halas: Thank you very much! I really appreciate it. It was a tremendous honor to shake the hand of the president of Italy and to have this great energy company honor the hard work we've been doing for the last several years using plasmonics for doing photochemistry. It's really groundbreaking work.
Zhu: Yes, it's really pushing the field towards addressing big problems, I think. Also sustainability requires a lot of work from various disciplines – great deals.

Halas: One of the really exciting things about plasmonic photochemistry that we've been doing with the antenna-reactor particles is that this concept started a company in 2018 and the company now has over 70 employees, an enormous – 45,000 square foot development space – and they're going to have their first commercial hydrogen generator out next year, 2023. They just signed a major deal to decarbonize the Southern part of the Korean Peninsula with Lotte Chemicals and Sumitomo. So we're very excited about the fact that this has very rapidly moved into commercialization. It shows how well it works –
Zhu: That's really exciting! Really exciting. Let's start with the first question. You received your bachelor's in chemistry and your PhD in physics. Was it a big decision or a natural choice for you?

Halas: That's a great question. And it actually was a big decision, it was not a natural choice. But I began to get interested in light, and I had – my background was a little strange. I was actually a musician and a music major, and I decided, after two years of college –
Zhu: Ah, I didn't know that!

Halas: But I was, and I decided that I was actually – really, my heart was in science. And so I jumped into science full force. Because I'd already finished half of my college degree, I had to double up on all my classes and not take some classes that may have been more interesting to me. So once I got my bachelor's degree, I began to think about other aspects that I was very – other parts of the physical world – I was very interested in, and light–matter interactions were something I had always found very interesting, very fascinating on many different levels.

And so that steered me more towards physics or applied physics, but mostly optics and photonics.

So I was a little weird person with this optics and photonics expertise developed at a graduate level, but then I had a lot of chemistry knowledge. And so when I first started, I thought this was such a liability to not have gone through physics for my entire life, but to have this other knowledge. But then after nano began to happen, and certainly C60 and fullerenes and so on at Rice University, I began to realize that having this unusually broad background actually was an asset and not a liability. And so I've always seen it as – yes, it's weird, it's unique, but I can really take advantage of the fact that I have this foundational expertise in chemistry. It's helped me very, very much.

Zhu: Oh yeah, I cannot agree more. As Steve Jobs said, you can only connect all the dots backwards, right? You never know what's coming towards you.

Halas: That's true!

Zhu: That's right. Interesting story, yes. What motivates you creatively and professionally?

Halas: Really good question, again! What motivates me professionally? I would say two things. I mean, the central motivation is discovery science. There's nothing more wonderful than working on something – say, exploring a new material. You certainly know this yourself – you began to work on – you were working on sodium – we were working on aluminum, exploring a new material. But then when you really start to make this and you start to study it, you have all sorts of very interesting discoveries that you would not have anticipated. And even the best theorists in the world can't predict the sorts of things that you can discover. And so that's one thing that I think really is so fascinating. It certainly motivates students, it motivates the passion of our work to realize that we are explorers and we are discovering new things – sometimes about materials that people thought were old and "there's nothing new there" – but aha! There can be new things there!

So that's one thing, and the other thing is actually the thrill that you get when the work that you've done in your laboratory begins to actually go out into the real world. And you see people who are not scientists, and you see that maybe the work that you've done satisfies some type of critical need. And that's something that's really wonderful. We're in the 21st century – the 20th century was the century of quantum and the genome. Those were the two big thing – of course, many, many great things happened in the 20th century scientifically – but those were the two big scientific breakthroughs. And so in contrast, the 21st century is really the century of critical challenges. We have climate change, which is a great example of all the other more complex problems that you can't solve in a single field, you can't solve by yourself. But the idea that you can work with people from other fields, learn from them and come up with all together new solutions is a really great aspect of – that's what we do – in many ways, this is the engineering century. Because we can take all of the breakthroughs and the discoveries, we can keep doing discovery, but we also can use the discoveries to really address some of these critical needs.

Zhu: Yeah, I cannot agree more. In terms of sustainability, I think we really need the multidisciplinary approaches to really address that, using what we know to address all these big challenges.

Halas: Exactly. And one of the interesting things – John Kerry, who has been sort of the tsar of climate change, he said something very wise. And that is that if we really want to be addressing sustainability and climate change, half of the technologies, half of the things that we need for this have not been invented yet. So I'm not exactly sure if 50% – nobody knows if that 50% metric is correct or not – but that's why discovery and invention are both so incredibly important. Because – I agree, electric vehicles are not the solution. We all know they're not the long-term solution. They're all going to cause a whole host of new problems. But we can see just how complex, just how difficult it is to actually begin to address some of these really, really big global issues. And so that's why we need discovery and invention more than ever right now.

Zhu: I agree. I fully agree. What was the influence of your postdoc experience in terms of your view of scientific work and your choice of research interest?

Halas: That's a really great question. So I was at Bell Labs and of course it was such a legendary place. It's referred to as the "Idea Factory," but it was also a place where people were very interactive and people were really very passionate about their work and really striving to do the best newest thing. And it was a really great atmosphere. You would go to lunch with other scientists and they would ask you questions you'd never thought about. It was very stimulating and it was very interesting to be a part of the Bell Labs culture. I think it's very important – you see ex-Bell Labs people, they are older now, but they carry with them the idea that to succeed when you have a goal – it can be research goal or an applied applications goal – you need teamwork. You're not going to be an island and learn everything. The best way to do is learn from other world experts and come together – and the interface between what you know and what they know is really where tremendously exciting things can happen.

Zhu: Yeah, I fully agree. So I was actually very curious. Your work converges the chemical synthesis fabrication with optics, giving rise to the field of plasmonics. How did that happen? Was that a serendipity moment, or is something else?

Halas: No, I think – so it's a very interesting quote. People talk about "serendipity science" a lot, "serendipity in science" a lot. One important thing to think about is a great quote from Louis Pasteur, and that is that "luck favors the prepared mind." Many people – there are many discoveries that people – you find out, oh, somebody actually saw it but they ignored it in the laboratory. Rick Smalley used to tell a story about how that funny little signal that you don't understand… You know, some people, they go away from the things they don't understand and they go to the things they do understand because it's safer. But then some people go to that thing that they don't understand, or that they'll go to – they'll begin to look at a type of material that is not what everybody else is interested in. The world is chasing carbon, some people they chase nitrogen. And we see this also with metals – we see this, also, with lots of different material systems. It's important to have that sense of discovery and also the ability to, when you see something unusual, to follow up and really say, "why does it do that?" Asking those questions and not ignoring the things that are unusual – really that can open doors. And I think that's really where serendipity happens. So it's two things: it's being able to observe and then understand that this is unusual, but also not to run away from it.

Zhu: I agree, I agree. A really, truly amazing thing happens when you got something unexpected, right? If you ignore it, then you miss a lot of good things.

Halas: Yes, you see many papers, for example, where people will say, "oh, the theory agrees perfectly with the experiment" or "the agreement between theory and experiment is so outstanding," you know? And you say –

Zhu: Perfect story, yeah.

Halas: Yeah. Maybe you don't want a perfect story.
Zhu: So, go back to the very beginning, during the stage of infancy of plasmonics. How did everything start? I think that we'll be very curious to know.

Halas: We were studying a completely different material. And my graduate student at the time was Richard Averitt. He's now a physics professor at UC San Diego. We were complaining that we were getting really, really weak signals in our optical measurements. And he was just sort of – it was late in the day and he was complaining these are so weak, these signals are so weak. And I said, well, if we want something strong, we should study metal particles, since they have huge signals of the type we were looking for.

But the problem is, we have a laser with a wavelength at 820 nanometers and all of the metal particles that I know of have resonances up in the blue, the green region of the spectrum, silver and gold. And so we're kinda stuck, we don't have the right laser for that. We didn't have a very large laser laboratory, we had one laser. And so the next morning, I came in and he was waiting at my office door. And he said, "Not all metal particles have resonances there!" He had discovered this somewhat obscure paper about a core-shell nanoparticle that grows naturally. It was a gold sulfide core with a gold shell. And it was an interesting paper that didn't have, really, a very thorough physical interpretation of why the plasmon resonance would, as they grew the particle, move around spectrally. And really went across the red region of the spectrum and went down into the infrared and it came back up into the visible and we thought, this is really interesting. I'm not sure we understand it either, so let's study it!

And so he began studying it. At the time, I had another student who began to model this. It's just very simple concentric spheres, so he was modeling this using MATLAB. And I went into the student room to look at what they were doing one day, and his name was Dip Sarkar. He's in India, an inventor, very inventive person. And he said, "Look what happens: when I change the core in the shell, I can place the plasmon anywhere I want to place it." And I thought, oh my god! – we need to make those and control this.

So we began to study how this works. We realized that within that spectral information was really a very detailed way of looking at the growth of the nanoparticle. We ended up writing a really nice paper about that. Richard Averitt did that in his thesis, and then another student, Steve Oldenburg – we said we have to figure out how to build it. Start from one core and then make another, make a shell. And so that started it for us. And there was no field, there were not even conferences where I could talk about it.

I would get invited to conducting polymer conferences or to fullerene conferences. And I'd just say yes to everything because there was nobody working on metals. It was just crazy. And I was very fortunate because my colleague and spouse, Peter Nordlander, saw this work and he thought it was very interesting. He was doing single electron devices at the time. And I was so disgusted because I was working on this for two years, nobody was recognizing it. And he said: "Stick with it for five years, don't change your field. Don't change, jump again," and so on, because we had started to do some STM work and got a result published in *Science* three years before. And then I just got so discouraged going to meetings because I wasn't part of the "in" group in that field.
Zhu: Yeah, you feel lonely.

Halas: Yeah. And so then this happened. I said, this is really great and this has a huge future. And so he said, stick with it. Don't change. Pound your way into this and after five years, people will start to see this. And that was around the time – that was like two or three years before we convinced SPIE to really start to host nano-optics symposia, and that's where things happened. So SPIE asked me to chair a symposium on metal powders and I said yes, but only if we can call it "Plasmons and the Optics of Metal Nanoparticles." And they're like, OK. So I changed the title. All of a sudden, we started to attract young, interesting people that eventually became stars in the field.
Zhu: Oh, is that right?

Halas: There were so many people who started – we started having this meeting at the big SPIE annual meeting and all of a sudden, the room was packed. The SPIE people would come in and see, are there any empty chairs? No empty chairs. All of a sudden, more and more people were flocking into this conference. And so it was really – but it took five years. It actually took five years to really, really get – and after about the five year point, Harry Atwater came up with the name of "plasmonics." And so we thought, perfect, wonderful! Really great idea, perfect name. And that was a really great ride to see a field start, grow, and mature.
Zhu: Wow, what a beautiful story. What a beautiful story. We just need to stick with it for a little bit longer.

Halas: Yeah, stick with it. That's really important for a lot of people to do. And that's what – if you think about different inventions that people have come up with, and you say people made an invention, then how long did it take for people to really recognize it or then use it? For example, the laser. That had huge public relations as soon as it was invented but it took years for people to figure out what to do with it, other than science and so on. But really, the great – the big breakthrough, other than cutting and machining – those were some of the first applications. But it wasn't until around 1980 when people started to look at how they could use lasers for doing compact disks, data storage. There's data storage, telecommunications, optical fibers. If it wasn't for lasers, we wouldn't have optical fibers, of course. If it wasn't for optical fibers, we wouldn't have a lot of telecommunications and broadband and other things that that technology spawned. But these things can be, sometimes, remarkably slow.
Zhu: That's right. We just need to be persistent, yes.

Halas: Yeah, absolutely.
Zhu: I agree. One thing I want to ask, particularly, is that your research is truly interdisciplinary: from physics, chemistry, to biomedicine, energy, environment, and largely, sustainability. And it's really fantastic, and science really has no boundaries. But how do you navigate the boundary of different disciplines in terms of not only just the knowledge, but also students, funding, all the other elements?

Halas: That's it. It is tough. I mean, it's because you're always out of your "comfort" zone, at least certainly to start. But partially, it is developing really good collaborators knowing that you are definitely not the smartest person in the room. So some people, they don't like that. They want to be the expert but when you go to somebody in a different field it doesn't matter if you know your field better than anyone in the world. When you interact with a biologist, a medical doctor, a synthetic chemist, you're going to learn from them and learn completely new ways of looking at things and solving problems and so on.

So, collaboration and teamwork. It is not easy to discover really good people who are equally committed, which is tough because it means it has to be interesting for you and interesting for them, not interesting for you and then trivial for them.
Zhu: That's right.

Halas: Maybe a little – they might have a transient collaboration that's not a good fit for both parties. So when you find something that is a

good fit for both parties, then it's really, really, exciting and then you can both sort of push off into new directions.
Zhu: I couldn't agree more. I think collaboration is very important, especially for all these interdisciplinary problems. It's really all sides need to be committed to this research. Then you can push it forward. Yeah, I agree with it. But then, when you're addressing those problems, do you find it extra challenging to get funding and to attract students? How do we – sometimes, I mean, students, they are afraid. Sometimes, especially if it's something new and we don't know the answers – how do we motivate students to do that?

Halas: Now that's – so that's a really, really interesting question. I noticed that when we first started doing plasmonics and metal particles, people were asking us about how we made them and they didn't understand why we weren't working on carbon nanotubes. They're very interested, and would tell them about what you're working on and show them some papers and things and they'd say, is there a book I could read about this? And I said, not yet!

So it can be – it's sort of a filter. I mean, there are some people who are very interested in new things and they realize the frontier is exciting. And I think it depends on their background. If they are very comfortable – two ways. If they're comfortable with their knowledge foundation, and so they realize that they do have the confidence to solve new problems. No matter how big a problem is, you can always break it down into smaller, manageable parts that one can actually solve and make progress. So that's why if they have that sort of confidence – but also if they have – a very individual thing about science is our taste, right? The taste for the types of projects we like to work on. We choose to work on some things, we choose not to work on other things. And so it's also a matter of a student's taste. If they see that there's – oh, the kinds of adventures that we're having in the laboratory, that's very, very exciting. So it's a very individual thing. I've had some of the most wonderful students: this work would not happen if it wasn't for really, really great students that are extremely committed to exploring in the directions where we're going.
Zhu: Yeah, I agree. What is your vision for the future of plasmonics and nanophotonics in terms of fundamental and applications?

Halas: That's a great question. We've seen some people introducing new types of materials, and combinations of materials – I think we already are in a very mature phase now. The simple things were done more than a decade ago, way more than a decade ago. So there's a foundation, but then you can always build on that foundation. There are so many new things that are happening with new materials. You get quantum effects, many-body effects, you get things that can blend into what you're doing.

But also using properties – I mean, these days, we're doing a lot of plasmonic photocatalysis and really understanding – how can plasmons drive chemistry? What are the new mechanisms that they give rise to? So it's new – it's fundamental science, but sort of this mixture of trying to do a chemical reaction using the properties of a plasmon that's excited by light. So that's a whole host of new activity, of new ideas, but there is a foundation that goes even before plasmonics. Surface science was an area where people really understood what happens to a molecule adsorbed on a surface. So there's a lot of very important foundational work that sort of feeds into our current research as well.
Zhu: Yeah, yeah. I think a lot –

Halas: I'm not sure I answered your question!
Zhu: I agree that a lot of things need to be done and could be done. There is always a lot of things down in the bottom.

Halas: Yeah, exactly. One of the biggest things right now that people, everybody, needs to be thinking about, are sustainable materials. How can we – you've thought about this yourself. I mean, great to be able to do something with a really rare expensive material, but actually, if we can figure out a way to do the same type of – whether it's chemistry or build the same material with physical properties or even better properties – with elements that are Earth-abundant, that will always be available, that won't be the topic of geopolitical conflict or just tremendously expensive, that is going to be a really important aspect for future research directions.

When I talk to graduate students, I always tell them about this. There's not enough of some of rare materials on our planet to really be addressing some mainstream problems using them. And that's hugely important in the world of catalysis, and important in many other fields. Optics is one of them. They're looking at – well, let's use hafnium. Hafnium is quite rare. And people don't look at the chart of elemental abundances as much as they really need to be doing early on, not after five or ten years of research. They need to be thinking about these things right away.
Zhu: I agree. Always need to be forward looking – to look for the next problems and to really address all the issues coming out of the real world, yeah. I agree. What is the – I guess you covered it a little bit – what is the most attractive aspect of your career as a professor and what do you do with your spare time?

Halas: Well definitely the most fun that I get is certainly, really, the discovery stage, working with students and discovering things. And those can be fundamental discoveries, those can also be discovering, "wow, this could be really good if we use it for X." And then sort of the discovery of invention –
Zhu: Very rewarding.

Halas: Yeah, a new way to do something that we only have much more difficult ways to do. So that's definitely the most exciting and satisfying aspect of what we do. I'm not a businessperson at all. The science and the laboratory is what's really, really exciting. So that's what really drives me.

And so, spare time: I have a ranch, which sounds very Texan but it's really like having your own park. I have a fruit orchard and I do gardening. So that's far outside of Houston. I do not live on a ranch, people say these things. But it's relaxing because it's beautiful, there's lots of wildlife and birds and so it's very nice, quiet and peaceful. That's something I do. And also, I do some gardening, growing interesting things that you can eat!
[LAUGHTER]

Jia Zhu studied physics at Nanjing University. He received his MS and PhD degrees in electrical engineering from Stanford University. He worked as a postdoctoral fellow at UC Berkeley and Lawrence Berkeley National Lab. In 2013, he returned to Nanjing University, as a professor at College of Engineering and Applied Sciences. Currently, he is a professor at Nanjing University's College of Engineering and Applied Science. He has received several prestigious awards, including Dupont Young Professor Award in 2016, MIT Tech Review TR35 award in 2016, OSA Young Investigator Award in 2017, and Tan Kah Kee Young Scientist Award in 2018. His research interest is in the area of nanomaterials, nanophotonics, and nanoscale heat transfer. He is a fellow of Royal Society of Chemistry. He is also an associate editor for *Advanced Photonics*, among many other services to the field.

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Nanoengineering. She is the founding director of the Laboratory for Nanophotonics at Rice University and the director of the Smalley-Curl Institute. She received her PhD in physics from Bryn Mawr College. She is pursuing her thesis research as a graduate fellow at IBM Yorktown. Then, she served as a postdoctoral researcher at AT&T Bell Laboratories, joining the rice faculty in 1990. She pursues fundamental studies of coupled plasmonic systems as well as applications of plasmonics in biomedicine, optoelectronics, chemical sensing, photocatalysis, and solar water treatment. She is the author of more than 350 refereed publications, has more than 15 issued patents, has presented more than 600 invited talks, and has been cited more than 98,000 times. She is the co-founder of Nanospectra Biosciences, a company offering ultralocalized photothermal ablation therapies for cancer based on her nanoparticles, and also the co-founder of Syzygy Plasmonics, developing a light-based chemical reactor for photocatalyst particles originally invented in her laboratory. She is a member of the National Academy of Engineering, the National Academy of Sciences, and the American Academy of Arts and Sciences. She is a recipient of numerous honors and awards, including the R.W. Wood Prize from the Optical Society of America, the Frank Isakson Prize for Optical Effects in Solids, and the Julius Lilienfeld Prize of the American Physical Society, and the American Chemical Society Award in Colloid Chemistry. She is a fellow of SPIE and several other professional societies: OSA, APS, IEEE, MRS, the Royal Society of Chemistry (UK), the National Academy of Inventors (United States), and the American Association for the Advancement of Science. She has been a National Security Science and Engineering Faculty Fellow of the US Department of Defense and an advisor to the Mathematical and Physical Sciences Directorate of the National Science Foundation.