A Conversation with Prof. George Whitesides: Pioneer in Soft Nanolithography

George Whitesides and I spoke during the Fall 2007 ACS National Meeting & Exhibition in Boston, Massachusetts, where he gave presentations in the Presidential symposium, "Material Innovations: From Nanotech to Biotech and Beyond",[1] and other sessions and where *ACS Nano* debuted.

PSW: Let's start with the topic of your lecture in ACS President [Catherine] Hunt's symposium on material innovations. What are your thoughts on the commercial potential of nanoscience and nanotechnology?

George Whitesides: I've become an admirer of nanotechnology as an integrating discipline. If you say to me, staggering amounts of the gross national product will be coming from nanotechnology, if you say that this is revolutionary nanotechnology in the sense that information technology truly was revolutionary, it was a technology that supplied a capability that was not there before, I don't see nanotechnology doing that at the moment. On the other hand, I think that if you look at the economy, and you look at all the places that nanoscale science, or science involving nanoscale objects—in which the dimensions and the characteristics and the surface chemistry and the support interactions and all the rest of that are really built on nanoscale scientific concepts—it's a big, big deal.

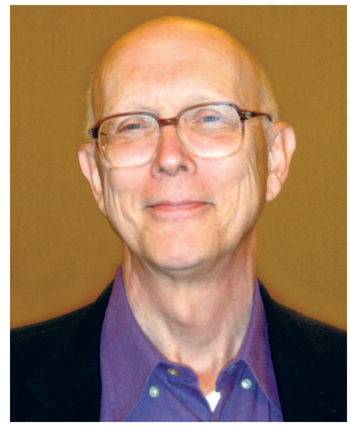
Catalysis, information technology, materials, aerosols...you can go through a long list of various technologies. I think it is a very important area. The mere fact that one has a name for it, which provides an umbrella under which scientists from a wide variety of areas can talk to one another, to me, is a very attractive feature.

Now, is there going to be somewhere in that a revolutionary nanotechnology? It's a little hard to say. I would say that's what's happening in electronics right now, the mere fact that we're getting to the point where it's very practical to talk about consumer products with 20- to 40-nm resolution design rules. I mean, that is just amazing to me because the amount of stuff that can be packaged in a very small space does have the characteristic that it could move things that used to occupy a desktop or maybe even a room onto your belt—that has the potential for a very large impact.

But, what's interesting about that is that it's *not* revolutionary. I don't actually think that we're ever going to see single buckytube transistors. But, clever engineers at Intel working with phase-shift masks and immersion optics and frequencies that have really been hard to work with in vacuum—all the rest of that, they've done a fantastic job of grinding this forward.

To me, a really interesting question is, "where is there something that really might be revolutionary?" In the distant future, I would argue that nano is the natural home of things that are quantum at room temperature. We know that molecules are quantum objects, but the circuits that are being used right now are not quantum objects. They're pretty classical circuits. There are some funny things about tunneling through thin-film dielectrics, but generally you try to keep them classical insomuch as you can. But, as you begin to talk about things that are just at domain edges, and magnetic particles, and quantum dots—these are the beginnings of trying to look at quantum objects.

Since I don't understand quantum mechanics—I think that any physicist or cryptophysicist will say that quantum mechanics is a set of rules that operates with great predictability, but nobody really understands it. How do we understand something about which we have no intuition? There is a real potential for having important things happen there. And so, we talk about quantum computation, and quantum entanglement, and quantum communications, and the concepts are there, but the



Prof. George Whitesides at the Fall 2007 ACS National Meeting & Exhibition.

To hear Prof. Whitesides's audio greeting and advice to young scientists, please visit us at http://www.acsnano.org/. Look for Perspectives from Prof. Whitesides and others on these topics in upcoming issues.

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realization is going to require nanotechnology to make it work. If there is something there (I don't know whether there is), what we're seeing now is the beginning of the materials base that will lead to that, and that could be revolutionary in some major way.

PSW: Are you trying to play a role in that?

George Whitesides: Our primary interest in nano, right now, is in fabrication. My argument for focusing on fabrication is that since I don't know what nano is going to be, to the extent that our efforts can open nanostructures, make nanostructures of whatever sort accessible to the widest possible variety of scientists in the easiest possible way, this is, to me, a good contribution to make.

I don't know what "it" is, so there's no way for me to build it. But soft lithography,[2,3] which was really a micron-scale technology, has really been very useful. The practical outcomes have been most important in microfluidics[4] and biology.[5] But, there are a lot of other things. I think it will end up being an important part of consumer electronics, where you can think of it being very high-resolution silk-screen printing rather than low-resolution photolithography. And, in other areas, this is turning out to be just right for optical structures, for plasmonics. I'm very enthusiastic about that.

We can make things in the open laboratory that have the characteristic that you really can't even make them with an e-beam writer. I mean, it's new stuff; it's pretty simple stuff, but simple is not bad. I'm happy with simple. I like simple things.

PSW: So, you are trying to build the "nano-infrastructure"?

George Whitesides: We are trying to do picks and shovels, essentially, for the California gold rush. Now, whether there is a California gold rush or not was sort of immaterial to the people that built picks and shovels, because people bought picks and shovels anyway.

PSW: One of the intriguing aspects of your operation is your "open laboratory" policy.

George Whitesides: I think there are two interesting philosophies in doing this kind of research. [Some of] my friends like to do experiments in which they build very complicated instruments, get them to work, and then, for a significant period of time, they're really the only ones that can do those kinds of experiments for reasons of the length of time required to build them and the sophistication.

I actually like to do it exactly the opposite, which is to make stuff as simple as we can possibly make it, try to get people in to learn how to use it, propagate it as rapidly as we can possibly propagate it in the community, and go on and do something else. It's just a different way of doing it.

If you're a tool-maker and people aren't using your tools, what are you doing it for? And, we learn all kinds of things every time we invite a new group in, or come up with a new thing. We learn stuff ourselves. We're working with a number of people, particularly Federico Capasso at Harvard in optics. And, we are learning all kinds of stuff about optics. We've gotten involved in the soft lithography area with the worm community, *C. elegans*. It's just a wonderful organism and this is a very smart, interesting group of people and soft lithography turns out to be exactly right for many of the problems in that community. If you didn't bring people in and talk to them, you'd never learn about it.

I tell students that there are three phases to a research project: there is the phase in which you define the problem and design the experiments; then, there's the phase in which you solve the problem; and there's the phase in which you sell the solution. As a graduate student, you learn how to solve the problem, and that's the easiest one. And then, when you are beginning your career, you learn how to identify the field and define the problem, and that's the next easiest one. And, what's really hard turns out to be to sell something new, because it requires people to do something that is an unnatural act—that is, to give up what already works well for them in order to do something else. They don't like to do that; none of us do. You do it only if you have to do it because it opens some major door.

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Getting to that point is to begin to have a community of users begin to work, develop, make things happen; then, there's a kind of infrastructure. The cost of entry, the first-user cost, goes way down under those circumstances.

So, yes, we very much like to have people come to the laboratory and spend some time. Many of these things are easy enough that they can learn the techniques in a couple of days, and they come, spend a couple of days, and they go away. Everybody's happy.

PSW: How did the original ideas for soft lithography come about?

George Whitesides: The original ideas for soft lithography really came from the misperception in the mid-90s that Moore's law[6] and diffraction limits were going to limit photolithography to a size range that was—pick a number, but let's say, 100 nm, or something like that. And that was obviously wrong, but it was sufficiently problematic at that time that there seemed to be a real reason for looking for alternatives, and the fundamental issue in soft lithography[2,3] is it that the physics, if you want to think about it that way, is limited by van der Waals contact, which is a 0.1 Å phenomenon, as opposed to diffraction, which is a $\lambda/2$ phenomenon. So, it was clear that the same kinds of limits would not apply.

Now, there were other issues that have come up that make soft lithography not a particularly attractive thing to think about for very high resolution patterning of transistor integrated circuits—primarily, the issue of maintaining the in-plane registration sufficiently so that you can stack things. But, it's really good when you don't have to do that. So, either sloppier features, or curved surfaces, or unusual materials, or cells, or whatever, you know, it's terrific for that kind of thing. And, you don't know until you get into it, you find out what works. But, the original motivation was the Moore's law problem.

PSW: How did you first hear of self-assembly? What was the background that led to soft lithography?

George Whitesides: I was raised as an organic chemist, and organic chemists when I was raised made covalent bonds, and you made molecules. And yet, as soon as you learn a little about biochemistry, you recognize that everything in biochemistry doesn't come that way. It's all noncovalent bonds, so it's cooperative interactions that together add up to something that's on the order of a few times kT, or maybe its more than that. And the question of how one engineered that was really not all that evident. So self-assembly is an idea that ranges from how a protein folds and how a ligand fits into a protein, to self-assembled monolayers (SAMs), to crystallization, to colloids coming together in photonic crystals, to some of these things we and others continue to work on, which have to do with the idea of, can you take mesoscale structures and build things that are electronically functional or have some much more sophisticated function? It's a very broad concept. It's actually, I think, a broader concept than a covalent bond.

But, getting chemists to extend their enthusiasm beyond the very sophisticated kind of technical artwork that goes into covalent bond synthesis has been a—it's taken awhile. Biology has really driven the interest in self-assembly in a molecular sense, and then SAMs have been very important in driving it in a surface sense.[7–11] And SAMs, after all, do have a covalent bond in there, or at least a strong bond. But then, these mesoscale systems and the ones that are larger than that—there, the interest is primarily in electrical engineering and materials science. It's a nice integrating area; it goes all the way across.

PSW: What would you like most to accomplish next?

George Whitesides: Well, we've always done research in a way that has a strong element of curiosity in it, and from the curiosity, with luck, comes something that is interesting, and from the something that's interesting comes something that works, and when the something works, it is perhaps that case that you can actually make something useful out of it.

The whole cycle from discovery to commercialization is interesting. I think the group has two strengths right now. One of them is in discovering new stuff, and the second is in training people to discover new stuff.

We also are interested in commercialization; I think as a group we do that pretty skillfully. So the question is, what are the areas that we're working on now that we're excited about that have us discovering new stuff?

One of them is the business of looking for new ways of making small structures; that's good. Another is ways of trying to solve the problem of rational design of protein–ligand interactions. Another, which we're very, very interested in, is dissipative and out-of-equilibrium structures, so complexity and emergence, this is a big part of the group, in fact.

We're interested in new materials, and we finally understand how electrets work, sort of. I mean, this has been a 10-year project and it's finally worked and it's going very well.

We've finally gotten these soft contact junctions, the trans-SAM tunneling junctions to work. We started on that in the mid-90s with mercury as the contacting electrode,[12] and it's always been unsatisfactory. It sort of worked, but it was really problematic. But, what we found recently is that if you use ultraflat gold, template-stripped gold, that gets rid of a lot of the problems, and then the other thing that you do is to use a eutectic gallium–indium alloy as the soft contacting electrode. And all the problems in that dissipate. We can finally get really good, easy, reproducible things so that we can go on—we and others can go and start doing what we hoped to do at the beginning, which was to do the physical–organic chemistry of electron transport across ultrathin films where, without too much trouble, [one can] look at electron transport. Well, we can just make it and try it, see what happens, and it's not going to be such a horrendous deal.

We're working on origin of life, and that's extremely interesting. It leads into a series of questions like autoamplification and how is it that you possibly get a set of reactions to somehow come together in a way that that set of reactions begins to dominate other reactions that are going on in those conditions? And, the answer there is, to me, completely unknown. I do not understand how a lot of that happens.

There are different views of how life started. [One] says they started in compartments. Then, there's a view that says that it's really catalytic. And there's a view that says that you look at parts of metabolism and ask where those might have come from.

I happen to be a catalysis person. I think life is a phenomenon that emerges from catalytic networks. But, as soon as you say that, you know it sounds terrific, but as soon as you say

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it, then immediately you have to begin asking questions of, how could you possibly have gotten to the catalytic networks that we see with just whatever was available in this violent environment of the prebiotic Earth? It's really not evident how that happens.

And then, we're also getting interested in other aspects of catalysis; for example, what does one do with CO_2? I'm interested in energy, but the atmosphere and water are such big deals for the future, and so little fundamental, imaginative stuff has gone on for the last period of time because chemists, recently, have been looking elsewhere. It's time to look at that.

And I should mention one other, which we're very excited about, which is this business of the third world–first-world science for the developing world. And the interest there is of course partially because it's a really interesting and important problem, but it also has this interesting characteristic that a successful technology for that has to be blindingly simple, robust, inexpensive. It's really quite interesting to work on those kinds of problems.

So, what do I most want to accomplish? The answer is, I'll take whatever comes out of this collection of stuff. I've never been smart enough to know what's going to work at any given time. I think they are all potentially very interesting, and what will make the difference is some combination of luck and a really smart student who will have a really great idea, and we're off and running.

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PSW: You have had tremendously broad "exposure" to problems through academics, companies, and federal advisory board service. Do you see that as a way to gain inspiration and ideas?

George Whitesides: It's a positive feedback kind of thing, because if you know several things—the government is always interested in people who will work for free. You've worked in Washington, you never get paid for it, but what you get paid in is *understanding*. You get to work with smart people who care about the country, who care about national security, who care about something. So, they teach you things, and they need people who are scientists. They need people who understand that this violates the second law of thermodynamics, and that's the way you can get through ceramic armor, and this is a kind of thing that you want to think about if you're thinking about aging explosives.

They sound like technical projects, but every one of them teaches you something. And then as you learn more about more stuff, more and more people have a reason to ask you for your advice because you can make connections that not everybody can make, which means, in turn, you get to work on more interesting problems and work with smarter people, which means, in turn, that you can learn more and more, and it just goes round and round and for anybody who's interested in science...

One of my operating rules is that if anyone comes to you with a proposition, particularly one that seems really loony, the right answer is "yes". You can always stop after awhile, if it doesn't work, you quit, but that basic process of just going and seeing what's there, sticking your nose under that rock and seeing what happens to be there, is unbelievably interesting.

You meet people that teach you about electronics, and they teach you about the immune system. They teach you about nuclear weapons, and they teach you about this and that and the other thing, and you don't remember all of it, but out of it, there are some themes that keep coming up over and over again, so you think about them over and over again. Some of the very simple things that scientists just take for granted—the second law of thermodynamics, the particle in a box, inert gases—it turns out that these really do frame an incredible set of useful questions for very complex problems in the world. And then, where this stuff fails, once you're convinced it's failed, that's a new problem. And that's the reason for being interested in complexity. For example, the generic answer to everything at some point is you look at it, and you look at it, and look at it...eventually, everyone just throws up their hands and they say "this is too complicated". Well, is there a science to that..."too complicated"? And if there is, then it changes the world. It really would change the world. I don't know whether there is, but that's how you find out about that kind of thing. So yes, I'm a big enthusiast of getting outside of the university and finding out what's out there and what their problems are and what they know and what technologies work for them, because often the technologies sit on top of rational science that people have not really thought about a lot. Sometimes engineering precedes science, but sometimes science precedes engineering. But, often engineering precedes science in terms of coming up with things that are worthwhile thinking about.

PSW: How do you manage your time between all the different things you do?

George Whitesides: I live a life of complete chaos and response to emergency.

So, it sort of varies. There are times where you just have to get papers out the door, and there are times when you have to raise money. I think we are spending probably too much time now raising money. It is certainly putting a strain on my life, and I suspect it's putting a strain on other people's lives as well. The country should be a little careful about that, because it is all well and good to try to have bureaucratic checks on how well we're doing our work, but I think it's not really designed to let people get on with the job of doing sci-

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ence, which is what we're supposed to do. There's an increasing frustration level with that, particularly for young people. They look at how hard some of us work, and they say, "I don't want to do that". And I sympathize.

When I started, I wrote an absolutely awful proposal to NSF and when I got to MIT there was $7,500 waiting there. I was off and running. It was almost effortless, and now it's just much more complicated at every stage. The peer review system is also not particularly effective now. I don't think the peer review system was ever intended to be democratic judgment by a community of average scientists. It was intended to be a distinction between judgment by scientists and judgment by bureaucrats, and its meaning has been changed to include all representative communities, including people who are not necessarily terrific about making strategic judgments about science. That's not necessarily what's supposed to go on.

I would like to say I'm well-ordered. The only thing I *can* say is that in chaos, I do a modestly good job of dropping only a modest number of balls, as opposed to a very large number of balls.

PSW: What advice do you have for young scientists? How would you raise another George Whitesides?

George Whitesides: I'm not sure that would be a good idea!

Well, many things come from undisciplined curiosity. But, it also comes from the fact that, in my view, the way to find interesting problems is to look for interesting or important phenomena. So you start as a natural scientist, or somebody looking at society, and, then, within a set of things that you're curious about, you'll find connections between that stuff and the world.

To take an example from the talk today,[13,14] I've always been curious about where lightning comes from and how a Van de Graff generator works and things like that. So that's a row or a column, the column or row is that materials science is in a stage right now where we need new materials. I mean, there's been a long period of very productive integration and composite-making from existing materials, but actually, the cupboard is getting a little bit bare. We'd like some stuff that's new. So that's an argument for looking at something and trying to control whatever it is that leads to charged matter. There's a fundamental question, which fits in with that, which is that chemistry has this deep assumption, that in a glass of water there's exactly the same number of plus charges and minus charges. And what happens when that's not true? You know, how do we think about it when it's not true? And it turns out that it's never true, but I never really thought about that. So, when enough things come together, and you find there's a place to get started, then that always seems to me like a good area. So, what's the background? Try to think about problems, realize that choosing the problem is actually a big deal. What one wants to do is to understand that nature's full of amazing things, and if you can find something that people have not worked on, you don't have to read the literature, it stands a chance of catching people's attention, and, if you are clever about it, it will connect to a problem that works in the future.

There was a guy named Fred Saalfeld at ONR [Office of Naval Research], who is a very smart guy. He was the civilian head and he got viewgraphed to death. And, he had this wonderful way of simplifying research problems, and his thing was "assume that I give you all the money you are asking for and more", which will happen in a good research project, "assume that the science works better than anyone could imagine", and then his question was, "Who cares?" I think it's a fair question. If you can't answer it, then maybe you shouldn't do it.

Let me just give you my little game-theoretic thing that I tell the graduate students, postdocs, and assistant professors. The question is, "when you're starting a project, is it more important to have the project succeed or to have the project be important?" And, I invite you to imagine a two-by-two matrix and you have columns which are "succeed" and "fail", and the rows are "important" and "not important". Obviously, if you have an important project and it succeeds, plus plus plus. If you have an unimportant project that fails, minus minus minus. So, that's easy. But, the question is, what about the off-diagonal terms? If you have an unimportant project and it succeeds, it's still minus, because nobody cares. If you have an important project and it fails, you almost always get credit for identifying an important project and taking a step. So, to me, it's a very clear argument for choosing important problems, rather than choosing problems that will succeed. And we've sort of trained people out of that, and I try to train people into that. You can't train people in that, but it requires a little bit of faith, you know that faith that—I don't know how to do this exactly, I don't even know how to do it at all, but it really is a neat problem and I have enough faith in my intuition that I'm going to go try it. That's what the university can still do. We can do it; industry cannot do that. And, that business of looking for something new and just going and trying it with the freedom you have is probably the most important thing for a young academic.

[Literature citations were added after our conversation to direct the reader to relevant publications.]
— Paul S. Weiss, Editor-in-Chief

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