From:	████████████████████████████████████ >
Sent:	Tuesday, September 4, 2012 8:22 PM
To:	jeevacation@gmail.com
Subject:	FW: George Whitesides Is An Unusually Bright-&-Thoughtful(-&-Accomplished) Guy….

FYI

George thinks my idea =f sub-contracting and "taking money to do the science I want someone=to do so I don't have to" (as he puts it) is pretty neat.=/o:p>

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From: Lowell Wood [mailto ███████████████████] <mailto:[mailto ████████████████]>
Sent: Tuesday, September 04, 2012 11:42 AM
To: 'Lowell Wood'
Subject: George Whitesides Is An Unusually Bright-&-Thoughtful(-=amp;-Accomplished) Guy….

….and thus his charact=ristically-crisp responses to Conor's queries are worth musing over,=I suggest.

Lowell

<http://www.te=hnologyreview.com/> Published by MIT

Three Questions for George Whitesides<=b>

The Harvard chemist =alks about the problem of solving problems.

<http://www.te=hnologyreview.com/contributor/conor-myhrvold/>

Conor Myhrvold <http://www.technologyreview.com/contributor/cono=-myhrvold/>

Monday, September 3, 2012

Conor L. Myhrvold

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Harvard professor George Whitesides <http://gmwgroup.harvard.edu/content.php?page=gwhitesides> has spent his career solving problems in science and =ndustry—he cofounded the pharmaceutical giant Genzyme, and he's the =orld's most cited living chemist. And yet Whitesides often reflects as muc= on problems that haven't been solved as on his successes. He recently spoke to Technology Review's Conor=Myhrvold on the challenge of problem solving, and on how better communication between s=ientists and the general public could help address some of the many problems worth solving.

Technology Review: =hat's the problem you have most wanted to solve and haven't been able to?<=span>

Whitesides: There's an intellectual problem, which is the o=igin of life. The origin of life has the characteristic that there's somet=ing in there as a chemist, which I just don't understand. I don't understand how you go from a system that's random chemicals to som=thing that becomes, in a sense, a Darwinian set of reactions that are gett=ng more complicated spontaneously. I just don't understand how that works.=So that's a scientific problem.

There's a second class of probl=m, which is how do you do university-based research in a such a way and in=such an environment that it eventually gets to be real? And that's the problem that we're working on with paper diagnostics (see &=uot;TR10: Paper Diagnostics <http://www.technologyreview.com/article/412187/tr10-paper-d=agnostics/> ") and diagnostics for all and=the entire low-cost diagnostics area (see "Lab-on-a-Chip Made of Paper <http://www.tech=ologyreview.com/news/410126/lab-on-a-chip-made-of-paper/> " and "Diagnosing Disease =ith Paper and Tape <http://www.technologyreview.c=m/news/411324/diagnosing-disease-with-paper-and-tape/> "). And it's not that we don't know how to do i=, but we haven't done it yet. And universities in the United States are incredibly strong, developed industry is incredibly =trong, but there's really a gap between the two. And understanding how to =et over that is a separate class of problem, which is important and to whi=h we don't know the answer.

And then there's a third class of problem which is local an= personal, which is that I'm in the business of teaching. And the question=is, how do you find the connection between individual students and what they want to do in such a fashion that they end up being=good scientists. What's necessary to do that?

What problems are being neglected?

I don't have strong feelings about that. There are so many =roblems in the world, from the origin of life or why proteins fold and how=life works or how the mind works, those are intellectual problems. And then there are problems like, how can you make lots of clean=water? And how can you store energy? And how do we get an energy balance f=r things and how do we deal with the development of an economic base for t=e developing world? And how do you think about global health? There are so many interesting problems that=there's no shortage of problems of every possible sort.

Often the problems that are least worked on are the problem= that have the characteristic that they're important, but they don't seem =o be something which is the biggest or fastest or most profitable. And if it's really important but it's none of the above, then =ho does it?

So water, for example. The number of people who really work=creatively on new sources of water isn't enormously large for the reason t=at I don't think people have very many ideas on how to get fundamentally new sources of water. We sort of think we've thought tha= problem through. I hope that's not true. But in every area where there's =omething that looks like a major change, it's usually because the field co=lectively had thought that it had thought about all the answers and had sort of given up, and then somebody =ame along with a new approach. And we certainly are going to need it for w=ter.

Could there be an incentive structure, besides profitabi=ity, that would encourage researchers to address some of these problems? &=bsp;

You could certainly imagine structures that might do that, =ut it's always tricky to predict outcomes because any incentive that you h=ve will be gamed in various ways. Universities are supposedly set up to incentivize, that is to

reward, creativity, good teaching, darin=—intellectual daring running counter to convention. Corporations are=basically set up to, legally, to return. Stockholders put in money, and th= corporation is supposed to give back more money.

Is there some way of putting together a structure that woul= put all of that together and for sure achieve a socially desirable object=ve—would you even know what a socially desirable objective was once you saw one? I'm not smart enough to be able to answer that quest=on.

But the idea of having people who are taking money being ab=e to explain why they're doing what they're doing is not a bad idea. The i=ea that publically funded science should have some measure of "I'm doing it because, and this is where it might end up being use=ul," strikes me as being perfectly reasonable. I think it'll actually=make for better science, too, because it's very easy in academic science t= end up working on projects that are just little extensions of previously known stuff, and that's sort of a waste of=time.

<http://www.te=hnologyreview.com/contributor/conor-myhrvold/>

Conor Myhrvold <http://www.technologyreview.com/contributor/c=nor-myhrvold/>

Contributor

I'm an editorial intern at MIT Technology Review, reporting on science and technology from Cambridg=, Massachusetts. Topics I am currently covering include the future of...
See=full bio » <http://www.technologyreview.com/contributor/conor-myhrvold/>