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How to Find the Great Problems

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How do you find the most interesting and important problems to work on? Part of it, says MIT Institute Professor Barbara Liskov, boils down to simple luck, and being in the right place at the right time. But beyond that, she advises, you have to pay attention to what's going on around you, learn to read with a highly critical eye, recognize what you don't know, and avoid incremental work.

Transcript

- Barbara, you've been an incredible problem picker in figuring out sort of where to point your career, so how do you do that? - Oh, I have no idea. (audience laughing) I mean, look, (interviewer laughing) I feel like I was lucky in a way. So I got into the field early. I mean, of course, at that time, it was all very confusing 'cause nobody knew how to do anything, but when I saw that program methodology problem, it was clear that was a hugely important problem. And when you can see a problem that hugely important, that's a great thing to work on. I happened to be on a panel with some Nobel Prize winners a couple of years ago, and that question came up, "How do you find problems to work on?" And what they said is what you do. You pay a lot of attention to what's going on around you, and every time you read a paper or listen to a talk, you ask questions. You know, so you say, "What didn't they do? "What's wrong, what could be better?" And that way, you find things, somehow, that helps you find things that are directions to go in. I mean, actually, that doesn't match my story very well because, in fact, I saw Bob Cohn pose a problem. But still, that's another thing I do.

I'm a very critical reader. And I also got very good at understanding what I didn't know, which is almost more important than knowing what you do know because then you can see where the holes are in your reasoning, and when I read papers, I see the holes in their reasoning, and that often gives you an idea of which direction you should go in. But I don't know, you know, I can't really answer that question. I just, you know, look for good problems. Don't do incremental work, but look for things that look like they're gonna be important, and then try to be honest in your research and really think deeply and figure out what you understand and what has yet to be...