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OP-ED CONTRIBUTOR

To Break the Disease, Break the Mold

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WITH the cancer recurrences of Elizabeth Edwards and Tony Snow the question arises: Why does this still happen? As is often the case, the answer isn't very satisfying: not all cancers are alike, early detection doesn't always work and treatments are still far from perfect.

But there's another problem: we keep focusing on doing the same thing better rather than trying something new. It is as if we are wearing blinders that let us see only one path and not the alternatives.

If you look at most cancer research journals you will see that our focus remains on finding smaller cancers, doing less surgery and radiation and developing new drugs to add to the old ones in an attempt to treat the cancers we detect. This approach — finding the enemy, and then slashing, burning and poisoning it — hasn't changed since I was a resident in training 30 years ago. We have certainly refined it over the years — two publications just came out that recommended expanding the use of M.R.I. scans in women who have breast cancer or are at risk for it — but, as in this situation where the additional exam only identified 3 percent more cancers, each progressive development leads to a smaller increment in benefit.

Why do we lack new approaches? One of the key problems is the way research on cancer is carried out. In the past it was common for clinicians to observe their patients, come up with a hypothesis regarding diagnosis or treatment and then head to the lab to test it out. For instance, in 1983, two Australian clinicians — one was a pathologist, the other a gastroenterologist — observed bacteria in stomach biopsies and went on to prove that ulcers were caused not by acid, as had been assumed, but by a bacterial infection. Ulcer researchers, who had spent their careers studying gastric acid, thought the idea was absurd but much to their amazement it turned out to be true.

The curious clinician is becoming increasingly rare. Medicine and science have become so complicated that it is almost impossible for one person to be an expert at both. Researchers tend to take a discovery from the lab and apply it to patients; the reverse trip is more and more uncommon. More often than not, someone makes an interesting discovery in the lab and then tries to find a clinical application. There is little chance, much less financing, for the wild idea that might prove revolutionary.

This situation is not helped by the incentives we give to young cancer researchers but not to experienced clinicians who want to test a hypothesis developed over years of treating patients. It is difficult indeed to obtain a grant to do research if you haven't spent your career in the laboratory. As the baby boomer generation of doctors approaches retirement, we should harness their experience and wild ideas by offering training in science or partnering them with younger research colleagues. Otherwise we risk inventing and discovering without reference to actually helping cancer patients.

Another aspect of the problem is our peer review system for financing research. It works well at eliminating poor investments, but it squelches innovation and fosters the old boy network. Organizations that give out "innovator" and "pioneer" awards claim to want to support new ideas but end up giving money to better ways of doing the same thing. And our academic and research institutions reward projects with clearly defined objectives that have a good chance of quickly leading to publications and tenure. If you have a wild idea or a completely new paradigm, forget about it.

Cancer of the cervix is one of the few cancers where we have been able to break the mold. We have moved from the Pap smear, which merely discovers abnormal cells, to a vaccine that can prevent the resulting cancer by protecting women against the virus strains that cause it.

At a breast cancer conference in San Antonio last December, a leading cancer researcher, James Holland, presented evidence suggesting that breast cancer may also have viral associations. A wild idea indeed; however, rather than being greeted with enthusiasm by the attending scientists and members of the press it was dismissed. Might there be something to it? We'll probably never know.

We need a new approach to fight this war and we need the money to do it, but, most of all, we need wild ideas to get us out of the rut of doing the same thing better.

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