THE CANCER LETTER

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Two "Optimists" Present Different Visions For Rapid Progress In Cancer Research

By Kirsten Boyd Goldberg

"Are you optimistic or pessimistic about improving cancer outcomes in the next decade?"

With this opening to his lecture at the American Association for Cancer Research annual meeting in Orlando last week, Leland Hartwell, president and director of the Fred Hutchinson Cancer Research Center, provided a counterpoint to the boundless optimism of NCI Director Andrew von Eschenbach, who preceded him at the podium.

"As you heard, Andy von Eschenbach is optimistic," Hartwell said in

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NCI Director: Imagine Einstein With A Laptop Einstein Experts: It Would Have Slowed Him

By Kirsten Boyd Goldberg

Gigantic screens at the front of the conference hall flashed the iconic black-and-white photograph of the mushroom cloud erupting from the small island at the moment of devastation.

Above the photograph, a headline: "20th Century Goal of Science and Technology." Below the mushroom cloud, the text: "Einstein and a Lap Top Computer??"

"When science at the turn of the last century set out on its quest to understand the fundamental nature of matter and split the atom, they needed to know just enough to be able to release that energy," said NCI Director Andrew von Eschenbach, speaking at the American Association for Cancer Research annual meeting in Orlando, Fla., last week.

"They didn't know everything there was to know," von Eschenbach continued. "But although their pace of progress was relatively accelerated, our pace of progress is even more extraordinary. Could you imagine what Einstein could have done if he had some of the tools that we have available to us today?"

Albert Einstein figures prominently in von Eschenbach's iconography as the NCI director struggles to justify his goal to "eliminate suffering and death due to cancer" by the year 2015:

--At a meeting of the Association of American Cancer Institutes, von Eschenbach said: "Our ability to understand cancer and its fundamental mechanisms is critically linked to our development of technology. Could you imagine what Einstein could have done with a laptop? If we are going to move forward in our agenda, we must move forward in the development, creation, and utilization of enabling technologies" (**The Cancer Letter**,

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Von Eschenbach Describes "Fantasies, Dreams, Visions"

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his lecture March 29. "He's challenged us to eliminate the suffering and death due to cancer by 2015. Is such optimism warranted?"

Hartwell, who received the 2001 Nobel Prize in physiology or medicine for his work in yeast genetics, said he, too, is optimistic that cancer research is "at the forefront of a major breakthrough."

The contrast between the lectures delivered by these self-described optimists was striking. Delivering his stump speech, von Eschenbach made repeated references to God, dreams, magic, and blessings.

"Fantasies, and dreams, and visions can become a reality," von Eschenbach said. "We are at this magic moment in the trajectory of cancer research that has led us to this point where we have the opportunity to seize upon the explosion in the knowledge that's occurring and rapidly accelerate the pace of that progress, such that instead of a linear journey, we actually are embarking on an exponential journey that leads us to 2015.

"There should be no doubt in this audience's mind…that we have within our power to create the dream of a time when no one suffers and dies as a result of cancer, into a reality," von Eschenbach continued. "You are transforming the world. You are saving lives. God bless you for it and God continue to bless you in your work."

Hartwell said his optimism is "tempered" by the organizational and cultural changes required for making a significant impact on cancer mortality. Most of the resources for cancer research are spent on "producing ineffective drugs" for late-stage disease, he said.

What's needed is a massive publicly-funded program--like the Human Genome Project--for molecular diagnostics, Hartwell said.

"Improved diagnostics will require a highly coordinated effort that is very different than the way we normally carry out our research," he said.

Losing the War?

As AACR attendees made their way to Orlando last month, airport newsstands held the March issue of Fortune magazine, with a headline in capital letters, "WHY WE'RE LOSING THE WAR ON CANCER (AND HOW TO WIN IT)."

Inside the magazine, an article by Clifton Leaf, an executive editor of Fortune who survived Hodgkin's disease as a teenager in the late 1970s, asked, "Why have we made so little progress in the War on Cancer?" As one who considered himself lucky to survive, asking that question seemed "particularly ungrateful," wrote Leaf, who credited his survival to treatment with MOPP plus radiation in a clinical trial at NCI.

Yet, the myth that "the cure is within our grasp" is one of the problems with cancer research today, Leaf maintained.

"Hope and optimism, so essential to the fight, have masked some very real systemic problems that have made this complex, elusive, relentless foe even harder to defeat," he wrote. "The result is that while there have been substantial achievements since the crusade began with the National Cancer Act in 1971, we are far from winning the war."

In his lecture, Hartwell said Leaf's assessment is correct, "to some extent." While mortality from heart disease, and stroke have declined over the past five decades, cancer mortality has remained almost stable. "Under those dismal statistics are success stories that we are rightfully proud of, great advances in curing childhood leukemia and testicular cancer," Hartwell said. "But, overall, this is not a great scorecard.

"As cancer scientists, it has been difficult for us to approach cancer with a sense of urgency," Hartwell said. "Our role has been seen largely as early-stage identification of potential therapeutic targets. We might consider ourselves lucky if, someday, pharma will use the knowledge we helped create to make a useful drug, but we are not likely to be directly involved in that process and it's not likely to be soon.

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"As a basic scientist for the last 40 years, I was often satisfied if I could provide an answer to the question put to me by my program administrator at NIH, my peer review study section, and my promotion committee: 'What are you learning about cancer?'" Hartwell said.

"Collectively, we have learned an enormous amount about cancer in last 40 years," he said. "But that knowledge has had surprisingly little impact on cancer outcomes.

"Now, as a cancer center director, I am asked a different question, by board members by patients and by philanthropists which is, 'What are you doing to cure cancer?'

"It's a fair question," Hartwell said. "Congress and the public are not paying $4.7 billion a year just to learn about cancer. They are paying to cure the disease."

Cancer researchers "can have a direct and rapid impact on cancer outcomes," Hartwell said.

"My optimism arises from my belief that we are at the forefront of a major breakthrough," Hartwell said. "I believe it will come from improvements in molecular diagnostics that will better inform our understanding of cancer in the patient and will impact every step of cancer intervention: prevention, diagnosis, and therapy.

"That said, my optimism is tempered. Why? Because seizing the opportunity depends not just on the science that we know how to do, but also upon reorganizing our activities in a highly coordinated fashion that most of us do not know how to do."

Hartwell said molecular diagnostics could improve cancer survival through improvements in prevention, early detection, and therapeutics. For these three areas, he described the knowledge base, the state-of-the art in application of that knowledge, and opportunities to improve outcomes through molecular diagnostics.

Following are excerpts of his lecture:

Prevention

What do we know that can guide progress in prevention? First, we know that cancer likely arises from a genetic predisposition. We know from many studies in rare families that single gene defects predispose to a high risk of cancer and this is likely to be true in others that are susceptible to cancer as well, where the genetic predisposition is more complex. We also know that most of this susceptibility is due to defects in DNA repair. That is an extremely important clue.

Second, we know that environmental influences act on this genetic predisposition. We know that from geographical differences in cancer incidence, which can vary more than 10-fold for each of the cancer sites, and from migration studies, which show that these variations in incidence change over decades when people migrate to a new environment. We know that these environmental influences involve mutagens as carcinogens and promoters that stimulate cell proliferation. This is the fundamental knowledge on which we can plan prevention strategies.

How have we been able to use that knowledge? Bruce Ames provided us an enormous breakthrough a couple of decades ago by developing simple tests for mutagens and demonstrating that most carcinogens are in fact mutagens. Other tests have been designed for other forms of DNA damage and for chromosome missegregation errors, and these tests are currently keeping mutagens and carcinogens out of our food and out of the workplace. We will probably never know the enormous benefit that has occurred from this form of screening.

From population-based association studies, we have been able to identify a few strong risk factors: viruses that cause cancer, radiation, smoking, asbestos. Many association studies demonstrate relatively weak effects, and many of the environmental causes of cancer probably act in complex combinations that will be difficult to sort out.

What could we do with improved molecular diagnostics? For one, we need to supplement our routine assays for DNA damage and chromosome missegregation with equally routine assays for cancer promoters, things like hormones and things that cause inflammation and cell division.

We need better assays for DNA methylation, probably equally important to DNA mutation in silencing tumor suppressor genes.

There is probably also an enormous opportunity in looking for new infective agents. Only recently was H. pylori, the bacterium, discovered as the causative agent in gastric cancer. Like Epstein-Bar virus, HPV, hepatitis virus, HIV—there are probably other human pathogens that contribute importantly to cancer incidence that we do not yet know about. We do not even have an inventory of the normal human flora.

These are things where I think there are enormous opportunities where cancer scientists can provide help for, assays for, in the near term, that I think could have enormous impact on reducing cancer incidence quickly.

Therapy

Basically, cancer cells are genetically and

epigenetically different than normal cells. The few effective drugs and immunotherapies that we have work through those differences. We are still dependent primarily on the paradigm of agents that are toxic to all dividing cells. But we are trying to get more specific to focus on molecular pathways involved in cancer, and, of course, Gleevec is the poster child for success in that area.

We mustn't forget that cancer cells are genetically unstable, like many pathogens, and it's very likely that it will require more than one targeted drug to cure any particular form of cancer, much as we use multiple drugs in treating HIV.

What's the state-of-the-art in developing new therapies? It takes about a billion dollars and 10 years of research and clinical trials to bring a new drug to patients. The time line for effective combinations of targeted drugs will inevitably be decades. Most of the new drugs that do even get approved are not effective in curing cancer. Some of the reasons for this are the following:

New drugs are tested on terminal-stage disease, whereas we know that many important targets function early in the cancer process, like angiogenesis and metastasis, and will not be revealed in tests on late-stage disease.

Drugs must have efficacy in isolation to be approved, currently; whereas, most of the protein targets, that is, most of the genes in the cell, have redundant functions and must be knocked out in combination. For example, in yeast, my favorite organism, about seven percent of the genes are nonessential. This is not because they are not used, but because there is so much redundancy in the cell. If you took one of those nonessential genes and knocked it out, the cell grows fine. You can find about 30 other targets for every gene, which, when knocked out at the same time, will kill the cell. So there is a huge redundancy and a huge number of potential secondary targets for each primary target.

Finally, preclinical validation of drug targets has been nearly impossible. Mouse studies have not been predictive. Here, there is tremendous new hope. In just the last few years, RNAi technology, small RNA technology, permits us to do what is essentially genetics in diploid somatic human cells. This is an enormous advance, which is going to rapidly allow us to do drug validation in human cells, which should be much more predictive.

A neat example of this is that shortly after the tremendous hype about the discovery of a fat-related gene in humans, studies on nematodes using RNAi discovered 350 genes that are related to fat metabolism.

Let me talk about the role for diagnostics in therapeutics. What about testing drugs on earlier-stage disease? What about following responses in real time by improvements in biomarkers or molecularly-targeted imaging? What about dosing up to efficacy through these real-time monitors of therapeutic response, rather than dosing up to the maximally tolerated dose?

With that sort of approach, it might be possible to individualize the testing of drugs, or a series of drugs could be tested in the same patient in succession.

What about testing combinations of drugs that have been validated together, but do not have efficacy in isolation? For example, RNAi technology could find combination of targets, which, when knocked out together, are effective in killing cancer cells.

What about taking advantage of what is universally true about tumor cells. It's true that they divide, but some of our normal cells divide as well. What's unique about cancer cells is their genetic instability, and we have not, despite the fact that we have known this for decades, really taken advantage of that fact.

Why do some of the chemotherapeutic agents now work? It's likely because we happened by chance to be matching the DNA damage these agents cause with the vulnerability that a particular cancer cell happens to have. For example, cisplatin, which is effective for disseminated testicular cancer, in yeast cells acts very effectively, 100-fold more effectively, on cells that are defective in post-replication repair, one of the roughly 10 different repair pathways. Could it be that testicular cancer cells are uniformly defective for post-replication repair? It would certainly be worth looking at.

We should generate a catalogue of DNA repair defects in cancer and rapidly employable assays to type each pathway.

Many of the things I just mentioned are things we as cancer scientists can be doing right now to improve cancer outcomes.

Early Detection

Finally, let me turn to the area of early detection. What's our knowledge base, what's the state-of-the-art, and what's the opportunity for improving outcomes through molecular diagnostics?

The most important fact that I know about curing cancer is that we do cure early-stage disease.

Not only is it true that we now cure 90 percent of the colon cancer patients that present with early-stage disease and less than 10 percent of patients that present with late-stage disease, but this has been true since 1975.

Shouldn't we be taking advantage of the fact that we already know how to cure cancer?

Also, we know that screening for early-stage disease can work. We do it for cervical cancer, and it dramatically reduced cervical cancer deaths in this country. We know its true for colon cancer, for individuals who get regular colonoscopy. It's undoubtedly true for melanoma and for esophageal cancer.

Why can't we extend this to other cancers?

It's not easy to detect early-stage cancer in a valid way. It must be validated that early detection actually improves cancer mortality. If it's not, we can be fooled with things like lead time, and benign lesions that don't need to be treated.

Population screening demands high standards for specificity and sensitivity. Those high demands can be considerably relaxed if we can identify high-risk groups, because then there are fewer false positives.

We know that much of cancer is probably due to individuals with high susceptibility, if we could just identify them.

What is the state-of-the-art?

Many of the currently effective screening tests are just too expensive to apply broadly in the population—tests like imaging and endoscopy. But we know that molecular diagnostics can be cheap and effective.

At the present time, assays, molecular diagnostics for genomic changes, are revolutionizing treatments of leukemia, lymphoma, and sarcoma, where translocations are diagnostic of therapeutic response.

We are currently perfecting our analysis of transcript arrays in breast cancer and many other types of cancer where transcript profiles are also becoming diagnostic of therapeutic response.

What are the opportunities for improving these diagnostic capabilities?

First, we need better tests for susceptibility for those people who are at risk. One of the current hopes is that increased genomic surveillance, that is, analysis of SNPs—single nucleotide polymorphisms throughout our genome—will reveal that genetic susceptibility, even at birth.

My own assessment is that, while this SNP analysis or genomic analysis will be useful in some cases, it will not provide a general panacea of risk assessment. The reason for this comes from studies in inbred strains of mice, where it has been very difficult to map cancer susceptibility genes, even in crosses between two inbred strains of mice.

Much of the genetic complexity in out-bred populations like our own is probably too difficult to completely analyze in that way.

What I do think will work are assays for DNA repair defects that can be applied more comprehensively. There was, for example, a very nice study about a decade ago by Scott and Roberts and England, where they showed that a simple test for double-strand breaks in patients presenting with breast cancer revealed that about half of the patients, before any treatment, had about a two-fold worse ability to repair double-strand breaks than normal people.

As we know, double-strand break repair is central to many of the genes that confer familial risk to breast cancer.

We need more assays for DNA repair defects that can be applied to the normal population.

We need other cheap, noninvasive tests for early-stage cancer. We can't apply genomic translocation analysis or transcript array analysis before we know there is a cancer or before we know where it is.

In this arena, proteins will be very important diagnostic agents that can be accessed through peripheral body fluids like blood. Mutated DNA and methylated DNA are also promising diagnostic agents that need to be further investigated.

Is it possible to develop reliable biomarkers for diagnostics of bloods and other fluids? I think the answer is definitely yes.

First, we know that cancer cells lyse, releasing their DNA into the blood, and they must be releasing their proteins as well. We found some of those proteins, like PSA and CA-125, which are of intermediate utility, especially when used longitudinally in the same individual as diagnostics of cancer risk.

But think about the vast unknown in the proteome for diagnostics. Less than 1 percent of the proteins in our blood have been identified. Of that 1 percent, 20 percent have FDA-approved diagnostic utility. That's the richness of the diagnostic information in our blood, and we have 99 percent of it yet to examine.

The exiting thing that methods now exist to vastly improve that coverage. They come as a result of our knowledge of the genome content in humans, which provides a catalogue of what should be there, and advances in mass spectrometry, which permit us to go much deeper.

Organizational and Cultural Challenges

So let me return to the question I posed at the outset. Are you optimistic or pessimistic?

As I said, I am optimistic, but the optimism is tempered. I am optimistic that there are numerous

opportunities, as I just outlined, to improve cancer outcomes by improving the effectiveness of molecular diagnostics.

However, if we keep doing things as we are, hoping that Pharma will somehow create highly effective drugs for late-stage disease, it will take a very long time to make a difference.

If we implement new approaches to improve molecular diagnostics with a sense of urgency, we as cancer scientists, could make a big difference much sooner.

What would it take? Improved diagnostics for risk and especially for early detection. Diagnostics that will help guide clinical trials for new drugs.

Why is my optimism tempered? It's not because I think the scientific challenges are that difficult. I think it will be difficult to conquer the organizational and cultural challenges which are needed.

First, because the bulk of the resources for cancer are going toward producing ineffective drugs, and miniscule resources are going into molecular diagnostics.

Second, because improving diagnostics will require a highly coordinated effort that is very different than the way we normally carry out our research.

Consider the challenge in the area of proteomics. Current technologies can interrogate only about 1 percent of the proteome in a single analysis. To increase coverage so that we can look to a significant depth in the proteome and where the biomarkers are to be found, will require teams of scientists, agreeing on standards, sharing data, working with the same samples.

We will need new tools to be able to aggregate data across laboratories and instruments.

Fortunately, we do have one example in biomedicine where an enterprise-like approach has been successful: the genome project. We wouldn't have a genome sequence now if that hadn't been highly organized with quality standards.

We need a new genome project for molecular diagnostics. This will require strong leadership from the NCI, cooperation from other agencies like the FDA, and partnership with major industries like big Pharma, and a sense of team science where the goal is producing a diagnostic platform to revolutionize our approach to this disease.

You hear from Andy von Eschenbach words like 'coordinated,' 'integrative,' 'standardized,' 'team science,' 'collaborate,' 'cooperate,' and 'partnerships.'

I think these are the right concepts for us to be thinking about, and they are concepts that I hear over and over again from my colleagues in the field.

The vision I think we can look forward to, if we can accomplish these difficult sociological and organizational tasks, is a vision where blood tests for screening cancer will detect early-stage cancer for many cancer types, with thousands of diagnostic markers, not tens as we have today.

We will be able to link those biomarkers to molecularly-targeted imaging, where vast improvements are being made in resolution to localize the disease and its extent. We can take advantage of that knowledge with surgery, which we know to be able to cure early-stage disease, or even to link those same biomarkers with targeted therapeutics.

What If Einstein Had A Laptop?

(Continued from page 1)

Oct. 31, 2003).

--In a commentary in USA Today Jan. 21, von Eschenbach wrote: "The pace of scientific progress, fueled by the wonders of new technology, is accelerating. Today's Einsteins have laptop computers, not blackboards…. Well-funded, brilliant cancer researchers, armed with today's incredible technologies, will help us be ready by 2015."

--Announcing the launch of the Cancer Bioinformatics Grid March 9, von Eschenbach said: "The stark reality is the fact that as we sit here, one person in this country every minute is dying of cancer. That is the problem. There is also the promise, and it is based on the tremendous progress we are making, progress that we believe can be rapidly accelerated. To put the promise in perspective: Could you imagine what Einstein could have done, if, instead of having a blackboard and chalk, he had this laptop that's sitting before me? It's that kind of promise, to take the kind of intellectual talent that we have invested in cancer and biomedical research, and to give that talent the tools that were absolutely unimaginable even a few decades ago. "

Einstein's "Simple and Elegant Physical Pictures"

A laptop computer might have been a convenience for Einstein, but it would have been irrelevant to his work, physicists and historians say.

"I can't imagine what Einstein would have done with a laptop, except that perhaps he would have wasted so much time searching the Web that he wouldn't have gotten anywhere," said Robert Park, professor of physics at the University of Maryland, an expert on medical quackery, and editor of What's New, a weekly email

newsletter about physics and politics for the American Physical Society (http://www.aps.org/WN/index.cfm).

"In fact, one of the troubling issues within physics now is that there is a tendency, because of the enormous availability of computing power, to model the universe, instead of getting what would be called a closed solution, an equation that describes the universe," Park said.

"When you get an equation in a closed form, as he did, you have essentially solved all problems," Park said. "When you model the universe, you only solve one: how the model behaves as you have described it exactly. When you get a closed solution, it gives you a feeling for how the whole thing operates.

"In physics, the search is for a unified field theory, but you are not going to get that from a calculator," said Park. "So, I can't imagine a computer would have been much help to Einstein."

On arriving at Princeton University in 1933, Einstein was shown his office and asked what he needed, physicist Michio Kaku wrote in "Einstein's Cosmos: How Albert Einstein's Vision Transformed Our Understanding of Space and Time" (W.W Norton & Co., 2004).

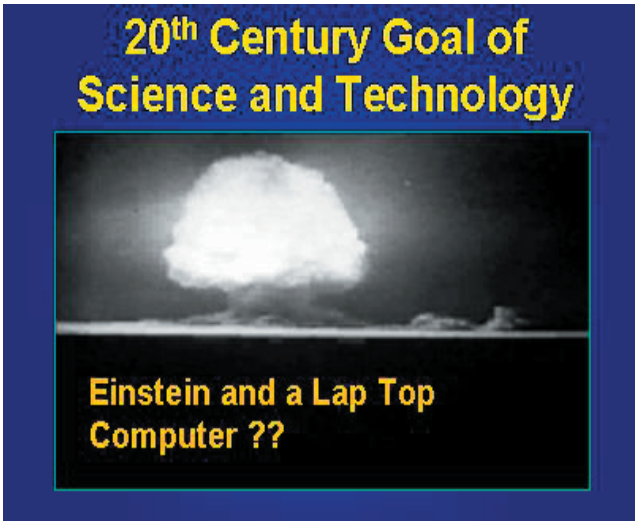
"Besides a desk and a chair, he said he needed a 'large wastebasket…so I can throw away all my mistakes," wrote Kaku, Henry Semat Professor in Theoretical Physics at the Graduate Center of the City University of New York and the City College of New York.

"Einstein's theories are based not so much on arcane mathematics… but simple and elegant physical pictures," Kaku wrote. "Einstein would often comment that if a new theory was not based on a physical image simple enough for a child to understand, it was probably worthless."

Einstein's breakthrough in developing the special theory of relativity happened one day in May 1905 as he rode a streetcar in Bern and looked back at the city's clock tower. "He then imagined what would happen if his street car raced away from the clock tower at the speed of light," Kaku wrote. "He quickly realized that the clock would appear stopped, since light could not catch up to the street car, but his own clock in the street car would beat normally…. The answer was simple and elegant: time can beat at different rates throughout the universe, depending on how fast you moved."

Israeli Prime Minister David Ben-Gurion once told a friend that Einstein had the greatest mind of any living man, according to "Einstein: A Life" (John Wiley & Sons, 1996), by Denis Brian.

"Do you realize that Einstein is a scientist who needs no laboratory, no equipment, no tools of any kind? He just sits in an empty room with a pencil, and a piece of paper, and his brain, thinking!" Ben-Gurion said, according to Brian's book.



Von Eschenbach's slide. Einstein "felt terrible" about the bomb, historian Denis Brian said.

Einstein's lectures were often entirely extemporaneous, Brian said to **The Cancer Letter**. "Students said Einstein appeared with a very tiny scrap of paper in his hand for a lecture and very quickly discarded it, and then talked on, extemporizing, following trails that one particular student found fascinating and unusual," Brian said.

Someone once asked him, "Where is your office?" and he answered by tapping his head with his finger, Brian said.

"On another occasion, he had to wait for somebody, and he was told he might have to wait for a couple of hours. He said, 'I don't mind at all, because I can think anywhere,'" Brian said.

"I don't think he would have had as much use for a computer as many people do today," Brian said. "He might have used it for the mathematical side of his work. He was pretty good at math, but he didn't like math. At one time he said, 'Mathematics can prove anything.'"

Opposed Weapons of Mass Destruction

Laptop computers became available more than 80 years after Einstein's special theory of relativity was published in 1905, and more than 30 years after his death in 1955.

Had the Internet—born in the early 1990s—existed during his lifetime, Einstein might have used it to campaign for nuclear disarmament and the establishment of the State of Israel, biographer Brian said.

"Einstein was always very interested in politics,

so that I think he might very well have used [a laptop] to find out what people were thinking and saying about political issues," Brian said. "He read The New York Times pretty assiduously most mornings. He advocated nuclear disarmament and he would have used it to keep in touch with political organizations."

Einstein was chairman of the Emergency Committee of Atomic Scientists, an anti-nuclear group, in 1946.

"So, a laptop would have helped him tremendously, I suppose," Brian said.

After the U.S. dropped two atom bombs on Japan in 1945, the press commonly associated Einstein's equation $E=mc^2$ with the new weapon of mass destruction. Although Einstein hypothesized that tremendous energy could be released from matter, nuclear fission was demonstrated by others. "He didn't think it was feasible to split the nucleus of the atom," Brian said.

Einstein wasn't tapped by the federal government to work on the Manhattan Project, even though he had suggested the project in a letter to President Franklin Delano Roosevelt, because his pacifist and socialist beliefs made him a security risk, Brian said. After the war, Einstein said that had he known the German effort to build an atom bomb would fail, he wouldn't have sent the letter.

"He hated the fact that they used the bomb on people," Brian said. "He felt terrible about it."

"Perhaps It's Just Rhetorical"

"I wonder what answers the NCI director gets to his question?" Brian said. "Perhaps it's just rhetorical. If you take somebody who is considered one of the greatest minds of all time, and you take the latest, great invention for people who want to do a tremendous amount of research, and you put them together, you think you might get fabulous results."

The computer is "like magic, to me, as is the TV, not having grown up with them," Brian said.

Park was dismissive of von Eschenbach's question. "It's a clever line, but I don't think it gets you very far," he said. "Lines like that are never meant literally. But you wouldn't expect the head of Cancer Institute to know much about physics. It's a good throwaway line. It doesn't say much about anything.

"What he must mean by that is that we have things now that speed up our work enormously, and certainly the computer is one of them," Park said.

"I would hate to go back to writing without a laptop," Park said. "My life is in my laptop, but it will not help me figure out Einstein's theory."

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