

Policy Perspectives from Members of the Scientific Community
Richard B. Marchase, Ph.D.

DR. WILLARD: Our next speaker is Richard Marchase. He is vice president for research and the senior associate dean for research at the School of Medicine at the University of Alabama at Birmingham, but today he is here representing FASEB, the Federation of American Societies for Experimental Biology. His presentation will be followed by a specific Q&A to him, and then we'll invite Sharon back for a broader discussion involving everyone.

Dr. Marchase?

DR. MARCHASE: Thank you very much. The Federation of American Societies for Experimental Biology is a coalition of 23 member societies representing over 70,000 scientists in diverse areas of life science and medical research. Prior to a decision about undertaking a large population study in the U.S., we at FASEB agree that the broader scientific community should be given an opportunity to comment, and I thank you for allowing us this opportunity today. Such consultation will surely be important for the technical and design considerations that will be inherent in this study, but these are not the issues that I will be addressing or focusing on primarily today.

In developing a response to the questions posed by the organizers of this session, discussions were held with FASEB's Clinical Research Subcommittee, or NIH Issues Subcommittee, and member societies, including the American Society of Human Genetics.

I'd like to begin by saying that FASEB recognizes the potential of such a study to improve people's health. The policy issues raised by the committee's task force, described in the background information that Dr. Willard already has described, are all important issues to address. When we at FASEB looked at what the policy issues were that were most critical to us as the broad representative of the scientific community, we focused on three: the prioritization of this study relative to other large-scale studies; the study goals, how well the study is designed so that useful data can be produced; and the cost and possible effects on research project grants, investigator-initiated studies, and other initiatives at NIH.

Relative to the first point, the prioritization of this study relative to large-scale studies, we are interested in the dialogue that will allow us to put this study into perspective relative to the other large-scale initiatives that are currently being undertaken. This includes things such as the Children's Health Study and recent initiatives toward increasing NIH's presence in clinical and translational initiatives. Dr. Zerhouni's roadmap initiatives are already on the table as important ways for the NIH to expand the relevance of its mission, and we are interested in seeing how this study will shape up, how it will be prioritized relative to the studies that are already on the books at NIH.

The other point I would like to make here is are we sure before we initiate this study that the other long-term studies that have been referred to before have been mined as much as they could be to allow the appropriate data that would set the stage for such a study as the one being described here?

The second point has to do with study goals and outcomes. A major challenge to the usefulness would be how well will the outcomes of such a study be used by the scientific committee. Clearly, there's been a lot of thought to the way the study would be designed, and we are not going to in any way doubt that this study would go forward in as efficient a way as possible. But

there are some questions -- for instance, those raised by Dr. Kardia -- that we think do need to be considered in much more detail than they have at this point.

How will the data be collected, stored and made available? The lack of appropriate electronic medical records has already been referred to. There are questions about how environmental data would actually be collected, and there was a lot of discussion in the background information about the necessity to develop new techniques to, in fact, make sure that environmental data were going to be appropriately handled by these studies. How will the genetic and other personal information be protected? Again, an issue that Dr. Kardia has addressed very well. And does our current health care system have sufficient technology and infrastructure to support the data collection and the data sharing that would be necessary to make this study a success?

Lastly, there is this idea that a need might be found to restrict or focus the study more. We've talked about pilot studies and what advantages pilot studies might have, and this is going to plan for the last point that we're really going to focus on, and that is the skepticism that Dr. Fink referred to that was characteristic of the scientific community at the beginning of the Genome Project and which we are concerned would also be the first stage of recognition of this project by the broader scientific community, not just those who are geneticists and not just those who might have biases against geneticists, the social and behavioral scientists, but rather the broader range of wet lab and scientists that FASEB to a large extent represents.

The primary problem that we foresee here is that this is a very expensive endeavor, and it is being proposed at a time when NIH funding is not increasing and when success rates and paylines for all grants, including R01s, are at a very low ebb. If I could advance to the next slide, I'd just like to show you some data that I think most of you are familiar with, but this has to do with the percent change in the NIH budget. Those numbers appear a little small, but what you can see is that in the mid-90s there were percent changes that were on the order of 5 to 7 percent. During a doubling period, the changes went up to 14.4, 15.9 percent. For 2004, there was a 3.2 percent increase in the NIH budget. The 2005 budget is not set but it is likely to be 0 to 1 percent in terms of where it will be relative to the 2004 budget.

Now, these low increases in the NIH budget put a very significant burden on investigators who are submitting their own ideas for funding at the NIH. Much of the buildup that occurred in the Genome Project and much of the overcoming of the skepticism that Dr. Fink referred to took place during times when success rates at NIH were not being challenged by the lack of discretionary income that was available.

The next slide, in fact, shows those success rates from 1995 until 2004, and you can see that during the very largest buildup and the completion of the genome study, success rates ranged from 27 to 32 percent. During the period of the doubling, these success rates were very high. This allowed a third of the grants that were submitted to be funded. That's still not a very large number, but a lot of meritorious research was, in fact, included in that one-third. If we look at the success rate for 2004, you can see that there's a significant drop, about a five-point drop, as we are suffering through what's called the hard landing at NIH that's following the doubling. We expect that success rates in 2005 will drop even further.

Now, as I said earlier, FASEB believes that the funding for investigator-initiated research projects should remain a high priority at NIH. Therefore, an important question to our community is what would happen to success rates if R01 funds were cut in order to fund this study? We've gone through a hypothetical example that's shown in this next slide.

No one knows exactly what this study would cost. The estimated cost could be as much as \$3 billion, perhaps even more. If we were to take roughly a tenth of that, \$350 million taken out of the R01 budget, that would be approximately 1,000 fewer grants that would be awarded. Based on 2004 data, the success rate for R01s would drop from 24.9 to 21.3 percent. We are very concerned that the allocation of this size of a pot to this project at this time during flat funding periods would be highly detrimental both to this generation of biological scientists, as well as to the next generation. It's already very difficult for a young investigator to think that as he submits a grant, he has a 24 percent chance of success. When that success rate goes down to, say, 20 or even below, it can be a very discouraging thing. In the late '80s and early '90s, we saw how discouraging such success rates were to the influx of new investigators and to academic research careers. We would just not want to see this study be funded in a manner that would both hurt the entry of scientists into our research pool, as well as the human cost to our scientists who are already working. If 1,000 fewer investigators are funded per year because of this allocation, what does that do to the faculty in our biology departments and our medical schools that currently are already there and struggling in many cases to assure that their research careers are going to continue to flourish?

This isn't a welfare program in any way. These are scientists who have been selected through a very highly selective process, and they're talented. They are contributing to the kinds of advances that are going to allow the next generation of medical discovery to lead to real cures.

FASEB's longstanding principle has been that investigator-initiated, competitive, peer-reviewed grants should remain the core mechanism for distributing research funding. This mechanism does allow highly skilled scientists to propose a direction and priorities for future research based on their own expertise and preliminary data. Funding of these proposals occurs only after very rigorous peer review. These grants have been the foundation for much of the progress to date in biomedical science, and by placing most of the resources in investigator-initiated peer-reviewed research, NIH ensures that federal taxpayer dollars will support the best science.

Therefore, this study should be undertaken only if funded through sources that do not compromise investigator-initiated projects.

In conclusion, we recognize the numerous potential benefits of such a study for public health. We are not in any way disputing that. This is also a visionary type of study that, in fact, could help to break the flat-level funding that we are experiencing. It could perhaps be the kind of vision that Congress would get behind and new monies might be allocated. We are concerned, however, that in a time when discretionary spending is very limited, with the Iraq war and the response to our hurricanes, that there may not be new funds available in addition to the existing monies that are already at NIH.

I commend the committee for grappling with these issues now and thank you for the opportunity to bring these concerns of our bench scientists to you today.

DR. WILLARD: Thank you, Dr. Marchase.