

consist of current problems and many would be short term. Any good scientist knows that an overemphasis of short-term issues will kill positive scientific inquiry that is necessary for the future.

Summary

In conclusion, the setting of meat research priorities will become increasingly necessary because of available human and financial resources, and the unresolved aspects of many current problems or concerns are very complex. Since real world problems usually require the science from numerous disciplines, there is a need for multi-disciplinary inputs and

planning for resolving these problems or concerns. The development and acceptance of both short- and long-term priorities should be developed for specific areas of meat research. Once these have been developed, they can then be used by administrators of broader research programs to assess meat research priorities in relation to other research priorities. Without appropriate justification of a few meat research priorities, the importance of meat research in the larger context of research will be less well articulated. It is also important in establishing any kind of priorities to recognize that priorities are not the summation of everyone's wishes and the justification of everyone's program!

Discussion

First Session

J. Crouse: Could you restate your definition of meat science?

E. Allen: An interdisciplinary area concerned with the conversion of meat into food.

Crouse: Then you omit the growth and development of animal production?

Allen: I believe at this time that meat research has to be separated from muscle biology and from growth biology. I believe there are enough differences that we should not use meat research as a generic label for these. I think growth biology is an area of its own. I understand, John, your point that the growth of the animal up to that point is certainly important to the meat properties; but I think that when it comes to talking about research, we need to talk about growth biology, muscle biology and meat research. Then we'll have less confusion about research priorities. You cannot develop research priorities if you include all three of those. You are going to have an absolute mess, in my opinion. That's why I separate them.

M. Dikeman: It seems to me that your point about your providing a justification for priorities should include adherence to previously-established priorities. In other words, how were priorities discussed at the Reciprocal Meat Conference at South Dakota developed and utilized? In order to sell the idea of priorities, it seems that there should be some measure of how those previously established priorities were utilized and adhered to. Do you have any indication of how those previously-established priorities were utilized or followed, or did they really have an impact on the direction that researchers are headed?

Allen: My impression is that, for the individual researcher, it perhaps provided some insights and directions on ways to go. Jumping to the national level, I think it brought an awareness to politicians and others of some of the kinds of problems that were important for animal agriculture. My personal feeling is that the First Boyne Mountain Conference had too many things in it and was not focused enough to really create a major initiative nationally. If you're going to create a major initiative nationally, you had better have very few things and you must have a good justification for them; then you must be able to sell that.

Let's think about what those have been. The first one in agriculture, of course, related to plant sciences, where they really focused on nitrogen fixation, genetic engineering and photosynthesis. The next one was biotechnology. The next one that I'm aware of that's coming up is water quality. I would share with you that the special initiatives committee of the Experiment Station directors are making a push for two more areas. One is in expert systems and the other one is in ag policy. In other words, those would be major initiatives that would have some focused initiatives underneath them. You might think initially that these are as broad as animal agriculture, and in some ways they are, but in other ways they are not. They really focus on a major problem. Animal agriculture per se is not a major problem. It has its problems, but in my opinion, it is not a major problem like water quality is or a major opportunity like biotechnology is. I think that the First Boyne Mountain Conference definitely had an effect; I think it's where you want to target your effect to. But I agree with you that when a smaller group begins to take on and turn down the priorities that have been set by a larger group, they had better be very careful about doing that. They probably need to focus more. I think the greatest focus needs to come from the smallest group, that is closer to the problems, and then they need to broaden and grow from that.

I would ask Glenn and Marv if they have any comments.

G. Schmidt: I would just add to what Gene said and raise some scenarios that could happen. I agree very much with identifying a problem and working across disciplines. Let's just pick an area. We want to automate the assurance of a safe meat supply to the nation. Let's start with that. You want whatever that entails. You sample the meat supply to be able to assure that it is residue-free, free of contaminants, etc. You start looking at disciplines. You listed some of them; but you can go on and on, and many of the disciplines won't be at the RMC. We're talking physics, chemical engineers, physicists, etc. So when you start setting priorities and giving grants, sometimes a person is too narrow-minded for what you consider to have quality input into problem solving. I think that's one of the things we have to watch; once priorities are established, to make the bidding to solve the problem truly available to all disciplines and not limited to us and our friends. The other way that scientists can get into trouble, including myself, is that if you get good at doing a certain type

of science, you want to find the solution in the area you're good at, in effect in the light. You never want to look in the dark where you don't know anything; that is another shortcoming that I see in our present method of granting. We take what we know, and we try to find the solution there. Somehow, I think we have to change our thinking as scientists; really look at a problem and be willing to use whatever we can find to solve the problem.

M. Stromer: I particularly like the overall organization of Gene's comments. I think they certainly provide a mechanism in which we can have a productive discussion. As I suspect many of you who know me might be thinking, I would disagree with Gene in his separation between muscle biology and meat science. I believe those two have perhaps been separate to the detriment of both groups. The muscle biologists tend to use a multi-disciplinary approach involving biochemistry, physiology, biophysics; by working more closely together, I think that a broader, deeper approach to some of the problems we're interested in solving can, in fact, be generated.

R. Benedict: Do you feel we should be giving people Ph.D.'s in meat science, or should we have a broad area for Masters' students and have students involved in support areas for Ph.D. like food science, biochemistry, histochemistry, etc.?

Allen: I don't think we ought to give doctorates in meat science. I think we ought to give doctorates in something that is more disciplinary. For those individuals who are interested in working on meat as a food, they ought to be getting a doctorate in food science at the minimum, but not as specific as meat. On the other side of the coin, they can do that in a department of animal science. When we narrow the Ph.D. down to a problem as I have defined it (meat science), we have narrowed it in the wrong direction. That's my opinion. Would anybody like to offer the other side?

R. Cassens: I'd like to first say that I disagree on your point that muscle biology should not be considered when establishing meat science priorities. Would you comment further about why you feel they should be separated?

Allen: The reason, Bob, is because as I've tried to think about the priorities for meat research per se, I see too much of a mixed bag to be effective at this point by keeping all of this together in one package. Now, if meat research priorities can be put together in a logical manner that goes from animal to consumption, I have no objections. I don't see, beyond a couple of areas, the same integration that we needed a few years ago. I think, for example, that in the calorie reduction of animal products, we have to have them together. But in some of the other areas, I think we have made great progress because they were together. Right now, I don't think, for example, that it was the meat scientists who separated themselves from the muscle biologists. I think the muscle biologists for the most part separated themselves from meat scientists. That's my opinion.

Schmidt: I really think this argument or definition of growth biologist, meats scientist, and muscle biologist isn't worth anything because if you list problems, whoever can solve the problem should be called in on it. Whatever you call them doesn't make any difference.

Allen: I couldn't agree with that more. If you orient yourself towards problems, then what those problems are categorized

under is unimportant. That's what you are saying, and I would agree. If the problem is reducing calories in animal products, you must put the animal components in there and not worry about what it's titled. But I think if you attempted to bring together a group to establish priorities for meat research and growth biology, I can't see it. To me, you need to identify the problems that are under growth biology and the problems that are under meat research, and then decide on the priorities. There will be some overlap between the animal parts and the postmortem parts, but I don't think you ought to throw them together to begin with. I think you ought to identify what the problems are and establish the priorities from the problems.

I'd like to add one more thing to what Glenn has said; one of the things that I have been pleasantly surprised with is how well large, multi-disciplinary and widely divergent committees can function when no one group dominates the committee. When he was talking about research priorities, that would deal with assuring a safe supply of meat in the program. Another example we could use is determining body composition of a living animal. We go beyond the expertise of the American Meat Science Association. To do this, you need a multi-disciplinary committee that might be put together by the National Research Council or the national Experiment Station directors; something that brings together the very best people that can be identified. When that happens, and there is basically a spokesperson per area, my experience has been that those committees really function quite well. They do look at resolving the problem, rather than carrying only the banner of a discipline. So, Glenn, I would strongly endorse your concept on that.

Schmidt: Once you've decided on a research priority and you get grants, how long do you allow before you see positive results?

Allen: I guess that comes back to what I believe we ought to be doing more frequently; and that's saying in our proposals what the limitations are to achieving that goal and when we think we will achieve it. As we think of research priorities, we need to think of it more like an industry product development protocol. What is the product we want? What are our limitations to getting there? What is the cost and time frame of doing it? When we do that in some of the research priority areas, I think we will also see that there are some times when a piece of information from one discipline is more critical to getting to the second stage of resolving the problem than information from another area of the discipline. Once that's resolved, then we can take off, but there's probably something coming from that first piece that feeds into the second. That's why I think we need to think in problem-solving terms; whether there is a natural sequence, or whether we can approach all aspects of the problem at the same time, given the resources to do it. I suspect that in many of them, there is a sequencing that makes more logic than just approaching it randomly.

W. Moody: I'd like to address your point of view regarding disciplinary science. I agree with what you said regarding problem solving, but a caution that you have to think about is that a lot of the ability to solve problems comes out of basic information we have developed over the years from fundamental and not problem solving-oriented research. We want to be careful. I don't think anyone can see years into the

future. Look at biotechnology, for example. Fifteen years ago, I suspect findings that came out of nonproblem-solving research led to the development of the whole biotechnology field. I think we need the basic fundamental research that problem-solving research will use later on for solutions.

Allen: As you probably know, I'm not going to say we should cancel all disciplinary research. What I am trying to make a case for is that I think some disciplinary research ought to be driven by problem solving, rather than by the biologist who is only doing disciplinary research because of interest in the science. In other words, let's identify what disciplinary pieces of research are necessary; and then let's give some money to the disciplinary scientists and let them work on that, because it's going to help us resolve the problem. If we do everything where we are working at it from the standpoint of interdisciplinary scientists, I think we will make a mistake because I think we're leaving out some very good scientists who could contribute to resolving the problem. So, I'm saying: Let's identify the problem, where the disciplinary needs are; let's give some money to those disciplinary scientists to help get the information from them, rather than doing it all in the other system. That can't drive the whole process, no. And, likewise, you can't change all disciplinary scientists. Some of them are going to do their thing forever.

Moody: Gene, I'd like for you to relate the problem we have with nitrites. Could you put this into your scenario and deal with that? Then look at the needs of the future and some of the things we need to deal with.

Allen: Looking back on nitrite and where we came to its resolution, how did that fit into the process? The first thing we did was to have a good historical review put together of the use of nitrite. That was done in a number of situations. Secondly, after the initial press reports, some toxicological studies were initiated. Some we agreed with; some I'm sure we didn't. Thirdly, we began to look at what I think is more directly related to the interest of this group. What is it that happens during processing and cooking that leads to the production of N-nitrosopyrrolidine? In other words, we found out that all bacon, when cooked to a high degree of doneness, did not produce N-nitrosopyrrolidine. From that, we then started looking back to sort out what was different between one belly and another. I think some of the things we found were fat-to-lean ratio; we found some things relative to the levels of nitrite that were remaining, a whole variety of things. Many of you know much better than I do. From that we could say, "Well, we can still produce bacon in the way we think it should be produced, but these are the critical points in the processing stages that we must keep track of and be careful of to minimize N-nitrosopyrrolidine during cooking." Along the way, there were many disciplines involved in resolving this problem. I think that is a good problem where you might have been able to sit down and say, "This is the most critical piece of information that we need; and if we have x number of dollars, that's where we ought to put our money on it. Once we get that, then we can go to this."

You're asking me to project into the future relative to meat science research. I think one of the big problems of the future is going to be in residues. How do we keep them out, and how do we monitor them? I wouldn't say it's a future problem, I'd say it's a current problem. This is what has come through

loud and clear from a variety of groups, the majority of which have testified to the National Research Council Committee. We've got to do what some people said we should be doing 20 years ago; getting some calories off our animals, keeping those calories out of products and giving consumers a greater choice in the market place. As we do that, how can we then produce manufactured or nonmanufactured products that are still acceptable? I just heard from the National Restaurant Association the other day. They are absolutely determined that no one is going to change the Choice quality grade. A modest degree of marbling is what is preferred by a number of them. I don't happen to believe that's necessary, but they're not going to be easy to convince. Likewise, in that total realm of those problems, how will marketing change? How will processing change? If we're still producing fat, and the lean becomes preferred and can be marketed that way, what will we do with the fat we're still producing? One of the scary problems coming up is in the area of residues and residue monitoring. I put that in the nitrite category, quite frankly.

Schmidt: I am sure all these meetings would love to start off with a list of our successes. What have we done in the past that's made things wonderful today? I had a call from a producer who happens to be a Harvard graduate, a rancher in Colorado, and he's interested in going into private-label marketing beef. He wanted two tests. One, what can I put these carcasses through to give me fat, moisture and protein? Two, what can I do to guarantee them residue-free? Residue-free, I could say "We haven't worked on that a whole lot and there's tests on the way." We worked on body composition for 30 years, why can't we give that producer a test that works?

Allen: That's a great need. I would add one to that list that came from Rod Bowling a couple weeks ago, a member of this association and now Vice President with Monfort. His comment was, "I want to be able to guarantee that every carcass that goes out of my plant is going to be tender, and I'm down to the point of where it's a very small minority of the total carcasses that I have a problem with. I'm not interested in you finding it, I just want to guarantee that every one that goes out is tender." Until he's able to do that, we'll have a problem with certain groups relative to grading standards and a whole variety of things. That's a perspective from the other end, but your point on composition, Glenn, is very real relative to how we market livestock and what we go through to get them to where they finally go to market.

Schmidt: Do you have any good reasons as to why we haven't been successful?

Allen: I have one. That problem cannot be resolved by the American Meat Science Association and the American Society of Animal Science. It requires a great input from engineering. In this day and age, when cat scans and nuclear magnetic resonance and all these other things are being used, we in animal agriculture had better reach out and get those people interested in our problem. Because if they can do it with a degree of sophistication (they're doing it for medical purposes), I can't understand why in the world they can't just tell us how much fat and moisture is in this animal. I think that's a result of us not having the talent to resolve it and not being able to get the other people who have that talent to work on our problems. This would be a good example of Bill's point,

"How do we reach out and get those very good disciplinary scientists involved in this problem?"

J. Sofos: Should a Ph.D. in meat science have a minor in other disciplines, like microbiology, statistics, biochemistry, etc.?

Allen: I would hope they have some strength in at least one discipline, yes. I know requirements and options vary among institutions. But I believe everybody ought to be able to hang their hat on one discipline. If not, have a good knowledge of two. If they can't, I don't think they're going to be on the forefront of their science ten years after they graduate in whatever they choose to do.

D. Beermann: From the teaching aspect, what would you predict the effects would be of moving priority listing by discipline areas like food science instead of narrower areas such as meat science or teaching? Who will teach those courses that are not now taught in food science or physiology departments, and how will these multi-disciplinary instructors be trained?

Allen: I think he has just reminded me of one of this association's problems over the years. Many of us have been in food science or animal science departments, and we're sort of the minority. We are not primarily discipline-oriented. We're multi-disciplinary. As such, when we talk to a nutritionist or a geneticist or a physiologist, we are talking to a discipline-oriented person. I never thought about it that way, but maybe that's been one of our problems over the years: We have come at it from an interdisciplinary standpoint when they were coming at their particular work from pretty much a disciplinary point of view. I think our mix on that is definitely changing; but in the process, we have to be careful we do not lose people who know meat. Not the biochemistry or the histology or the microbiology of meat, but *people* who know meat. I sense from some discussions that have taken place and recent hirings, that there's great risk in that.

A. Booren: Do you think you can have the best of both worlds?

Allen: It will be increasingly difficult because in order to be good in discipline, you have to have some depth in it. There are some people who achieve that; but I must tell you that I'm not optimistic that, with the nature of the problems we have today and the driving forces to become narrower and deeper, it will be as easy to have as many people who understand meat in the total context in the future as it has been in the past. What does that leave us with? It leaves us with the fact that, in order to understand meat, we'll have to do it in a team approach rather than in the single individual approach.

Think of where we've come from. Let's go back to the beginning of the Reciprocal Meat Conference. Think of what those individuals in the classroom and their research programs were doing in their day. I've used the example many times of Andrew Boss, who was one of the founders of this particular area. I would remind you that Andrew Boss was not just a meat scientist. He was the founder of farm management, he was a plant breeder, and he was a sheep specialist as well as a meat scientist. This was in 1890. From where we have come at that time to where we are today is very different; and I think where we were 10 or 15 years ago and where we are today is very different. If we're going to continue to attack the problems, we have to have a strong knowledge of a disciplinary science, but I think it's placing

more emphasis upon a team approach to resolving those total problems than perhaps it ever was in the past.

A. Foegeding: It seems that, for example, the American Cancer Society have one problem to address; it's funded in that direction, so it's easy to get priorities. It seems that our priorities tend to be set according to the priorities of the funding agency, and those are priorities that come out of their particular problems. Therefore, as a non-funding group of meat scientists, how do we, by establishing meat research priorities, fit into this system?

Allen: That's the point that I was hoping to make at the end when I said that what I think we need to do is identify what our major problems are related to meat, then take each of those problems and develop them in great detail and depth. But not more than perhaps two, three or four. You can't take everything that everyone in this room wants to work on and call those "research priorities." If you can't get them down to probably three, you don't have any priorities. I think that we need to do that; if these are major problems, the priorities are not going to change from year to year.

That's the other thing that has bothered me frequently about establishing priorities. One year you have priorities established; the next year, you have a committee to look at priorities again. If they're good priorities in the beginning, they're not going to change from year to year. Probably every three to five years, and then you'll have the outside meteorites that hit you; problems which must be addressed that become high-priority. But if you're doing a good job, you should be able to predict some of those. Get the right people and the right groups together, then you have an offense rather than a defense. A non-funding group like this, I think their only chance is to demonstrate that they've put a lot of thought into what they're putting forward and they put relatively few things forward. But they do it better than anyone else.

I. Peng: You have said that problem solving should often be from a multi-disciplinary approach. How do we get these other people incorporated, and secondly, why are many people in poultry meats associated only with the Poultry Science Association and not AMSA as well?

Allen: There may be good reasons why they would do that, but I think it's very unfortunate for this group that they do. I believe, in the long run, it will be unfortunate for them as well. I guess that's their culture, that's their brotherhood, and this is not it. Things like that don't change easily, but when this organization calls itself the American Meat Science Association, everything that's meat ought to be under this umbrella. No one should have reservations about having a portion of the program more oriented toward one species or the other. If it happens to be fish or poultry, that's the way it should be. That's where our people are going to go, some of them for employment, and it's part of the total ballgame that we're in out there. Since they're beating us right now, in terms of some of the areas they're in, we might also want to join them for that reason.

B. Breidenstein: Would you discuss your involvement in the Technological Options for Altering Animal Products of NAS and discuss how these fit into the priority schedule?

Allen: This committee, called "Technological Options for Altering Animal Products," was initiated last fall. So far, I think I can honestly say that all we've done is listen. I think we have

listened to somewhere between 16 and 20 individuals representing commodity groups, National Restaurant Association, consumer groups, etc., and this has involved a fair amount of time. The time goal for beginning to draft this report is next fall. In our meeting in September, a good portion of the time in that meeting is going to be devoted to writing. Not that there won't be some writing done before, but we're supposed to have some drafts done to work on at that meeting. The other thing is that there are a few small groups that are being called together this summer to further explore the specific areas related to technological options in altering animal products. When we're talking about animal products, it is complicated by the fact that it is not just meat; it's also eggs and dairy products, and the meat includes poultry, fish and red meat items. The most common theme that has come through is the need to do something about calories and options in the counter.

Second Session

Stromer: I think that one of the concerns I have in research priorities is the fact there's been a tendency, particularly of late, toward the short-term emphasis on research. I am concerned about that for a couple of reasons. One is that sometimes this has led us into embarrassing situations where we've attempted to provide quick fixes that were somewhat unrealistic. The other term that concerns me in that formula is the fact that I think this has tended to short-change us insofar as developing the information base that we really need to continue in order to solve problems effectively. My feeling in setting research priorities is that we should, if at all possible, continue to maintain a balance between the longer-term types of research that attempt to get fundamental information and use that information to apply it to problems.

Schmidt: I would add that I think we have to clear our minds a bit; once a person had chosen a discipline or area to work in, restudying a given approach to a given problem doesn't especially solve the problem. This is aesthetically true if one talks to the industry or producer organization; the last thing they want to hear from a scientist giving a speech is that it needs more work. They've had it with that approach. The next time that speech is given, they may be out of business; and their patience is running thin. That's something we have to remove from our way of thinking.

In order to approach a problem, the other thing we need to think about is that it may be necessary for us to really change our discipline thinking; actually retrain ourselves in a new area to approach a problem, and to seek out methodology from other colleagues that are fundamentally trained. It may be in an area that we don't know, and we're going to have to learn it. Whether it's chemistry, technology, physics, mechanical engineering, electrical engineering or even psychology in some cases. Just to bring to mind some of the problems, one thing we got involved in is that we thought we were protein chemists. We studied protein, we studied fat and we studied water. We tried to solve the problem by doing that. Well, it turns out that the solution to the problem was carbohydrates. We didn't know anything about carbohydrates. The solution lay there, and we should have sought out that expertise. We didn't; we're lucky. How does one

carry out what Gene has said? What steps should be taken? First, prioritize problems. What do you do next? Review proposals for the approach. That means you should advertise to all these other competing disciplines for possible approaches to solutions. We don't like that idea, do we? That's really what we should do. Then we should reward stepwise innovation short-term i.e., a year. When you see proposals, it's amazing what can happen when you bring a physicist in to solve chemical problems. They have some approaches, and they know some things about instrumentation that the technologist doesn't know. That kind of innovation should be rewarded short-term. When they can show some initial success, then they are rewarded with greater funds and a longer-term fund commitment to really get into depth and solve that problem with what's shown as initial success. This doesn't disagree with anything that was said, but I think this is the way we're going to have to think more and more; not just say that we're meat technologists and we really need to study this one problem again since we didn't find the answer and keep the same approach. That is simply not going to lead to success.

Allen: I would like to comment about the point that has been raised here. Coming back to this short-term, long-term, as we look down the road the next decade or so, I think one of our opportunities for some money coming into the research program is going to be from some check-off dollars, as well as perhaps other money that is non-government. The real risk I see in this is that if that money is directed toward product development at universities, for example, I think it will be money down the drain, because this money is going to be used to develop products that then someone else has to adopt. I can tell you right now that the corn producers are very much on this kick. We want the Ag Experiment Stations to do work on new uses of corn. There happens to be a very, very long list of the uses of corn right now, so we're working at the real margin to begin with. Then, we're not very bright and good at marketing in academic circles, so we don't have a chance of succeeding in that. Yet, I believe that there are certain components of those things that deal with disciplinary science, perhaps associated with what corn is made of and things of this nature, that could make a contribution. But it's not going to be oriented towards a specific product. I see that as one of the things that could happen. About Marv's point about the well being dry in terms of the background of scientific information that's available, what is going to replenish the well? We can't have all the money going into the applied areas, we need a balance. Just as we need a balance between disciplinary and problem-solving orientations in research.

H. Mersmann: I see some kind of a dichotomy in regard to your discussion of different kinds of persons and their backgrounds in regard to setting priorities. I totally agree with you that the narrowly-developed single-disciplinary scientist is probably not very useful on a planning committee. Yet if we have a person who is a total generalist, I'm not sure that they can come up with the kinds of things that you talked about when you discussed setting justifications for the investigation of this problem and really being able to see the problems. I think that generals can see the need for research in an area, but I don't know that they can see the holes that are in the research that has already been done leading up to the

solution to those problems. So there's some kind of dichotomy there with regard to two extremes. Maybe the really good people on these kinds of committees are persons who are in an old tradition that I think we're losing in this country; we don't train renaissance persons any more and it's very unfortunate.

Allen: I don't disagree with you. I've had to answer that question before because I've also used this chart to describe what I think is the land grant university in comparison to a private one. I believe that private universities do a great job in disciplinary science, but the majority of them do very little on problem solving. I believe that's the difference between a land grant university and a private one. The answer to your question is that you can take disciplinary scientists; and I have seen some of them on these committees, they are very good disciplinary scientists. In other words, I would say they are deep into their discipline; but there are a few people like that, they're not numerous, who are also very good in looking laterally. Lateral vision is needed rather than just vertical vision. Those are the ones that are difficult to find. But when you do find them, you want to make use of them in these activities because they appreciate the fact that what they're doing in their discipline is not the only thing. They are not numerous, but they can be very, very useful. That's what I was talking about when I was talking about formation of these committees.

Mersmann: Unfortunately, the good ones tend to be from the administration.

Allen: No, not always. This National Research Council Committee that I'm on right now has some of those people on it. I am only one of two administrators out of 14 people. There are some very good scientists there. The other thing about a committee like this is that there's basically one person representing a discipline or an area of knowledge. There's something magic that happens when we become a minority. We become much more willing to listen. I've seen that happen time and time again. We can put together some very large and complex committees, but if you have the right people, and you don't have any one group dominating the whole committee, there really is something that begins to happen that's difficult to explain. Whereas, if you go to a group that knows everything and already have their opinions formed, the priorities that they develop would be predicted before you start, then maybe that isn't the right committee. Sometimes it would be, but not always. The kind of people selected is very critical.

Schmidt: Continuing your corn story: One of the greatest technological developments of our lifetime, I feel, has been the development of high-fructose corn syrup. How did that come about? It would be interesting to know what was the approach taken. Was it fundamental research? Immobilize enzyme research? Were they trying to do it? That has to be one of the greatest inventions in the food technology area of our lifetime.

Allen: I don't know what the history of that is. Does anyone know how high-fructose corn syrup came about? I could include at least some of it. I know there's background disciplinary science in it, but I would say the outcome of that was because it was problem-driven and it resulted in a product in this case. I think if you saw the total development, you would call that product development work, at least in some segment

of it. I would share with you an experience that I went through recently in a conference where a committee was charged with looking at some of the frontiers of agricultural science's discovery. A subcommittee of about 10 people representing numerous backgrounds, interestingly shared by an immediate past director of a major commodity firm and a director of research in it, took a little different approach that I think we in academia and USDA are accustomed to, but is more typical in industry. That was to say that by the end of the next decade we're going to increase the export of beef by a million tons per years. In other words, that's the goal. It is a goal that is so large it's almost difficult to imagine. But that's what happened, this individual points out, when John Kennedy said, "We're going to go to the moon." What it does is that it causes you to jump clear out to the end and say, "What is the goal?" Then you back up and say, "What are all the reasons why we cannot do that right now, and what expertise do we have to bring to it?"

I think that is an approach that we sometimes need to use in problem solving. It kind of comes back to Glenn's point of view; to say this problem is so important we are going to resolve it, we think it's going to take us seven years and here's our plan for what we're going to do. Our problem is that we each work in an experiment station, or a company lab, or a USDA lab, and we can't get all of this coordinated. I think one of the challenges to groups like this is how we can do a better job coordinating what we're doing on a regional basis and what would really be great is if we could do it nationally. Because if we could, we could resolve a lot of problems quickly. But right now, it's uncoordinated; consequently, we can't bring the right pressures to bear upon solving some of these problems that are either minor or certainly the major ones.

Mersmann: How would you propose in a free-wheeling organization like a university or even a USDA lab to organize such a thing so as to get something done together as a unit, rather than everyone doing their own thing? I believe we need to rethink our whole current structure of research organizations.

Allen: I would answer that this way, and I know Marv is concerned about this issue: It has to do with money. I believe that in well-developed and well-justified research priorities which have consequences attached to them, they're not the summation of wishes and they are not just the justification of everybody's program; that when they are done well and we put some things into them that you frequently do not see, we have an opportunity to sell something. If we say, "We're going to turn the present resources that we have in this direction, what do you have to offer?" I think we'll begin to open up an opportunity for a variety of groups that we could go to for additional support. Especially when this is a research priority that is beyond an individual or a given institution. If it is a research priority that's been developed regionally or nationally or by an organization, and there's only two or three of these, it must be a high priority. But then go to the next step. Lay it out. What is it going to cost? There are certain parts of this that you as a producer or you as a consumer don't relate to. We understand that. But here's where we're trying to get: To resolve the problem as you and I see it; and it's going to require that we do "this at this time" and "this here," and it's going to take about "this long" and

it's going to cost about "this many dollars." If we don't have this time and this money, we don't have a chance of resolving it. I don't think we tell that kind of story often enough, so my answer to that would be to use research priorities as an opportunity to help create some new resources not previously tapped.

C. Kastner: In agriculture, how well have we done in establishing research priorities? How well have we done in meat science?

Allen: With respect to how well we have done, scientists usually relate to "what's the product, okay?" So let's think about that for a minute. Let's go back about eight years to the first new competitive grants in the plant science area, dealing with the 16 million dollars that came on top of the Hatch funding for nitrogen fixation and the other components of that. So that was one initiative. Another area is the biotechnology which had major components of agriculture in it. Without agriculture speaking for it; different societies having priorities that were developed to demonstrate that biotechnology would be important to them; and national leaders recognizing that about one-third of all the impact of biotechnology is going to be in the area of agriculture, we wouldn't have obtained the 46 million dollars placed in that area. Likewise, the same with the competitive monies that have gone into the food science and nutrition area and into animal agriculture. If you went back and you said, "What was it that caused this to come about?" I would challenge anyone to put their finger on one thing. It wasn't just the Boyne Mountain Conference, it wasn't just the efforts of the American Meat Science Association in conjunction with their sister associations and the rest of animal agriculture, but I think all those things had an effect.

I believe that what we have done in the past in research priorities is that we've addressed too many things. We need to boil the list down further. Remember that plant scientists initially had three priorities. That was a very astute move, in my mind; consequently, they scooped us by five or six years before we had any competitive grants. I think the reason they had only three is they had a focus that they could concentrate on. I think that agriculture in the last seven or eight years has done a much better job than it ever did before in bringing additional resources to our research scientists in the plant, animal and food area.

I have serious doubts as to whether you'll ever be able to bring something out of the national pool specifically for meat. I believe you can do it for food, but I don't think you can do it for meat. It is too small, it's too narrowly based. The best chance we will have for that is if the National Research Council Committee's report is well enough received by the Federal Government that the Federal Government decides that something really has to be done about taking calories out of animal products. Then they may put some money up for it. Without that kind of effort, I don't think you're going to see a targeting as narrowly focused as meat. I may be wrong, and I hope I am; but I wouldn't be too optimistic about that.

I'm not sure of what the role of research priorities from an organization like this at the national level would be in that regard; however, I think it's very important in another regard. That is to develop two, three, or four very specific priorities; state why these are really important, and feed them into the

total picture and the context of food, the industry and consumer well-being. Then I think we might make some progress on an integrated manner in that way. It has to be a pretty big issue and of major importance to make it, as I see it.

So as we referred to before, the plant science initiative, the biotechnology in the animal, I can tell you from serving on the Special Initiatives Committee of the Experiment Station Directors, the next two priorities that are on the agenda. The next one is water quality; and following that, I suspect the next one will be ag science policy. That's not firm, but that's what is being discussed. Another component that's being discussed is expert assistance. So compare those kinds of national initiatives, that people can really get behind, with meat research priorities and you'll see what I'm talking about. You must fit meat into some of these other things. You can't do that unless you have developed good priorities within this association or group.

Kastner: Do you think that we've got the meat research priorities fitted into some of these other areas, i.e., food science?

Allen: If they are, I'm unaware of it. I think the food science document that came out of IFT, between you and me, is much too lengthy, broad and not focused. I can just tell you as a food scientist, but also as an administrator, I haven't been in too many circles where that was positively received by outsiders. Remember that people who are making those decisions don't have time to read much. When you create a document like that, you completely overwhelm them. You've fed yourself, but you may starve in the process.

Kastner: How do you feel that we fared in the Farm Bill with respect to food in general?

Allen: Is this a political question, here?

Kastner: It looks like the recognition of the need for research in the food processing area was increased.

Allen: I believe that it was increased. I guess I would hold my judgment on that right now, though. Let's not get too optimistic. I think it was a step forward, as it appears to me. I think it could still drop through the slats very easily.

A. Mullins: I agree with your comments. I think we're going somewhat in an opposite direction. Let's take meat animal science, for example. Meat science has gone this way, geneticists have gone their way, physiologists have gone their way, nutritionists have gone their way; often, even on the same campus, they're not cross-communicating. None of us have included enough economics, so it seems as if we need to re-evaluate where we've been, where we want to go and how we're going to get there.

Allen: I didn't disagree with that, and I think what I've been trying to tell you is that I believe research priorities coming out of a group like this could be much more effective if they expanded the size of the group that it came from. I think this is too specific. You've just raised a very good point, and that's the economic part. If we had economists working with us on this, we'd be able to attach some dollars and cents to some of these things we think are important.

Mullins: We, as meat scientists, can't be too proud of where we've been for the last 30 years in establishing the priorities for the products we've used, and now where we're going back to.

Allen: I think one other thing we need to recognize, and I used this as an example in the last session. Shall I say "the majority of the easy problems have been solved?" The ones that remain are not easy. When you get to increasingly difficult problems, and if problems are really solved by multiple disciplines, the disciplines that are necessary to resolve some of the problems that remain are not a part of the American Meat Science Association. Consequently, if they're not part of our thinking when we go back home to do research, we need to reach out and create a new team. All the people are not around us who are necessary to resolve some of the problems that we want to work on. If we want to work on that, we need to reach out into some new areas for new expertise to assist us.

I've used the example many times of Andrew Boss, who was one of the founders of this particular area. I would remind you that Andrew Boss was not just a meat scientist. He was the founder of farm management, he was a plant breeder, and he was a sheep specialist as well as a meat scientist. How many in this room can do half what he did? What's happening to you?

What is happening to us is that we have to learn more and more about a narrower area in order to make an impact. If someone brought up lost time, meat science in an animal science department is really the only non-discipline other than management. Consequently, all of these years we've been working in rows and the rest of the department of animal science has been in columns. No wonder we didn't get along sometimes.

But the question is: "What about the future? Who will know about meat in the future?" If we have to know more and more about a specific area and not across numerous disciplines, the only thing we're left with is that we've got to be much better in forming interdisciplinary teams. In other words, if we want to get the best disciplinary science involved, we've got to link up to disciplinary scientists rather than being in one person "the meat scientist" or "the meats man" back in Andrew Boss's day. If we don't do that, we will not be as successful in the problem-solving mode in the future as we've been in the last 20 or 30 years. I think we need to look at that very, very seriously; and it comes to bear upon how we establish our priorities, who we involve in them, and the format for funding it and doing it.

Mullins: You mentioned something else in relationship to diet and health; perhaps we're taking an entirely too narrow viewpoint on that. Obviously we're interested in meat, but in the diet-and-health issue we have to be interested in a whole array of foods and the interaction of those foods.

W. Means: You also mentioned that we at the universities really should be doing product development. I understand that and respect that. I have a split extension/research appointment. I go out to what I consider aggressive people in industry who are on top of things; they're progressing and they're developing new products. What processors want from me is to help them develop new products from an extension standpoint. I have some problem with that. That's the main thing they want from me. Then when you talk to industry for research monies, they want results at the end of the year or three years or whatever. You are new on the scene; you're limited in resources, you're limited in training, and the people

you know; and that's kind of where it's at, product development. So what advice do you have for young scientists like myself in developing a program and working with this problem?

Allen: That's a very good question with no easy answer. We see the problem. The problem is that if everything is directed toward this product development with no backup and no learning about the principles that are involved in creating the next product, then we haven't educated anyone. I believe that's true.

Means: Do we then take advantage of those situations as far as getting money, work on developing product, and then have to use some of those funds in other resources to try to do the technical side of it, the basic research that goes hand in hand with that?

Allen: Well, I believe what many people do, some more successfully than others, is to try and create some bridging between expanding the science and also ending up with some product development. In the process of doing that, there's no doubt that we have compromised on one thing, but we've also created a positive benefit on the other. The concern is (and this is not just unique to meat science, it's a concern in agriculture), like Anson Bertram said four or five years ago, "In so many areas of agriculture, the well is nearly dry of additional science that we can tap to resolve problems." When the well goes dry and you want a drink, you're not going to get any. Consequently, when the science is not available to create another product, you can work a long time, but you're not going to get another product. I think that is a story we have to increasingly communicate to all people at whatever level and interest in the industry; to communicate this need that somebody must put up the dollars to put some more science back into this area.

Means: Unfortunately, they want short-term results. If they are willing to accept a long-term result, you can put more into the science of it.

Mullins: As a young scientist, you need to concentrate on developing the process and functionality of the materials utilized in the process.

Allen: Because then, the next time that person should be able (if they learned anything) to take the first few steps, at least in product development. You're pressured to do this because they want to sell it today.

T. Thornton: It seems to me that your comments are somewhat negative. I think agricultural scientists have done a good job; and in affluent countries like the US and Australia, have done an exceptional job. There are still great problems, but we've solved the number one problem and that's of production.

Allen: We've done a really great job, as you've said, Tim. But the reason why I want this to sound a little bit scary is because the US and a few areas in a number of other countries have been in the lead position for a long time, and it's difficult to imagine that you're not going to be in the lead again at all. We, in this country, face a great risk that some of our good science that could be converted into technology will not be converted; and that in other areas, we will not have the science to develop the technology.

We have situations where science is driving technology and other situations where technology is driving science.

This ranges all the way from the support for good scientific research coming out of discipline to what we do about regulatory requirements, and the area of biotechnology and gene insertions into plants. We've been trying for more than two years to get a tobacco seed released into the environment that has a gene for a protein from a bean inserted in it. I can't think of anything more absurd than the fact that it's taken us two years to get this tobacco out to where we could test it in the environment. That kind of thing will kill us in the international market, and we can't afford that if we're going to

continue to use our science and our technology and the resources we have in this country in agriculture.

I couldn't agree more, you know we need to have and we are deserving of a lot of credit for things that have been resolved. What I'm saying is that the challenge of the future, I think, is greater than anything we've seen in the past. We are in an international economy, we are dealing with an increasingly sensitive public to a whole variety of issues. To bring all this out the other end, it is sometimes difficult to imagine how we will do it.