

Establishing Meat Research Priorities

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Introduction

Research priorities are necessary as guidelines for directing human and financial resources into the most needed areas. In times of fiscal constraint, such priorities become increasingly necessary, and if properly established, can be useful in bringing new resources to high priority needs. However, the process of setting priorities is one that can be done in a variety of ways at numerous levels of research management. The purpose of this presentation is to provide some insights into the variety of considerations that become important during the establishment of priorities for an area such as meat research. The ideas presented by the author in this brief paper are ones that have come from the following experiences:

Directing my own research program and working on a variety of interdisciplinary projects.

Working with AMSA and Intersociety Research Priority Committees.

Serving as a chairperson or committee member of three different multidisciplinary National Research Council Committees concerned with research priorities.

Serving on a number of review teams for departments of Animal Science or Food Science.

Serving as a member of the Special Initiatives Committee for Experiment Station Directors.

Responsibility for University of Minnesota Experiment Station Projects in the College of Agriculture.

These experiences have taught me that establishing sound research priorities is not easy. Furthermore, I have

also had the pleasure of working with some individuals who have had the professional background, an integrative mind and a visionary perspective which was invaluable to the development of sound research priorities. However, in my opinion, some of the best scientists have too narrow a perspective to effectively contribute to the delineation of research priorities in a multidisciplinary area such as meat research.

What Are Meat Research Priorities and Who Establishes Them?

A primary objective of meat research priorities should be to resolve problems through appropriate multidisciplinary research. The converse of this is to expand disciplinary science and hope that the appropriate scientific information is available and can be integrated to solve a specific problem.

However, before discussing this any further, I believe it is necessary to consider the potential kinds of individuals or groups that would have an interest in meat research priorities and therefore could be involved in establishing the priorities. In this regard, a key question is "whose meat research priority is it and how do the priorities differ among groups?" The following are some of the kinds of groups that are involved in determining a variety of research priorities, including those for meat.

- Disciplinary scientists.
- Interdisciplinary scientists.
- Industry research, development of marketing groups.
- Government agencies ranging from pure research to regulatory concerns.
- Consumer-oriented groups.
- Department heads or primary unit administrators or committees of such.
- Directors or secondary unit administrators or committees of such.
- Local, regional or national disciplinary or multidisciplinary committees.

From this listing, it should not be surprising that there would be some differences in the research priorities that are established by the different groups. A key question in comparing priorities among groups is whether the priorities that were developed were based upon an "interest," a "problem" or a real "need" in relation to further developments. I suspect that some of the above groups would tend to more clearly

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focus on single rather than multiple considerations (interest, problem or need) for establishing meat research priorities. For example, it would not be surprising if the research priorities of disciplinary scientists, regulatory agencies and consumer groups were markedly different because of a tendency to focus on interests, needs and problems, respectively.

Figure 1 is an attempt to summarize with some specific examples how the variety of orientations are related to meat research interests of different groups. Figure 1 assumes that there are two basic orientations in a research area such as for meat. These are "disciplinary" and "problem areas." Any "needs" are related to one of these two, whether it be for science, product development, regulatory or human concerns. Thus, disciplinary scientists, such as biochemists, will tend to view the needs in any of the problem area examples in Figure 1 from a point of view of how biochemistry relates to these problems. In contrast, inter-disciplinary scientists or a group of multidisciplinary scientists will tend to give primary focus to the problem and what disciplinary science is needed to resolve the problem or address needs related to product development, regulatory issues or consumer concerns.

Thus, as illustrated in Figure 1, a disciplinary view is vertical, whereas a problem view is horizontal. In my experience, the identification of single-disciplinary research priorities tends to be more straight forward and more easily accomplished for a given aspect of a problem than the necessary integrated disciplinary needs to address the whole of the problem. This should not be surprising; but if the problem is to be resolved, the whole of the problem rather than only a single component of it must be addressed.

Figure 1					
— DISCIPLINES —					
B	P	H	M	N	
I	H	I	I	U	
O	Y	S	C	T	
C	S	T	R	R	
H	I	O	O	I	
E	O	L	B	T	
M	L	O	I	I	
I	O	G	O	O	
S	G	Y	L	N	
T	Y		O		
R			G		
Y			Y		
PROBLEM AREAS					
Meat Tenderness	X	X	X		
Product Development	X		X	X	X
Warmed-Over Flavor	X				X
Reformed Meat	X		X	X	X
Meat Color	X	X		X	
Diet-Health Issues	X	X	X	X	X

A two-dimensional organization of research interests.

Table 1 provides a list of concepts or guidelines that should be useful in establishing and using meat research priorities. The establishment of research priorities occurs at all levels, for a variety of areas and purposes. The same is true of meat research priorities. Therefore, the information in item 1 of Table 1 is essential to the priority-establishing process because priorities established for one purpose may be inappropriate for another. For example, the establishment of local or regional meat research priorities may have some major differences from nationally-developed priorities. In considering the establishment of research priorities, it is also ideal to establish the maximum number of items to be included in the list of priorities. In general, the longer the list, the less impact the list of priorities will have on decision-makers. By definition, research priorities are not what everyone is doing or would like to do!

From the preceding discussion, it should be apparent that the selection of people who will establish the priorities is extremely critical and should be given more serious attention than is sometimes apparent. This is probably the most critical part of the process. For meat research, I believe the primary orientation should be toward addressing real world problems rather than just expanding scientific information. Since meat research encompasses many problem areas and requires the inputs of many disciplines, the membership of a commit-

Table 1. Concepts for Developing and Using Meat Research Priorities

Item Concept

1. Clearly define the kind of priorities that are to be established and how they will be used when completed.
2. Utilize respected scientists who have a background and expertise broader than a single discipline. Ideally, these individuals should have a historical perspective of the problem and a vision for problem solving.
3. Seek the input of non-scientists who can contribute information about the problem from a variety of perspectives.
4. Define short- and long-term research priorities that relate to a given problem. This will help to establish any sequencing of information that would be helpful.
5. Review what is known about the problem, why it still persists and then establish the major scientific limitations that would help resolve the problem.
6. Estimate what resources and timeframe are believed necessary in addressing each researchable aspect of the problem.
7. Justify each of the priorities and obtain a review of the priorities from a second group not involved in their original development.
8. Following the establishment of the priorities, the priorities should be printed, distributed and officially endorsed by the group that will make use of them.
9. Once the priorities are established, they need to be "championed" by an individual or small group if it is intended that they be used to generate additional research funds.
10. Good priorities should not necessarily be changed each year, but periodically reviewed and revised as problems are resolved and new priorities or problems arise.

tee and the process used to develop meat research priorities must be carefully considered. It should not be surprising that scientists with their primary interest in fresh meat properties, meat processing, muscle biology and growth biology would have a difficult time deciding on a single and restricted list of meat research priorities. Although there are some natural areas of interfacing among these groups, the diversity of specific research interests will frequently be too great for such a committee to work most effectively. The ability of the committee to establish appropriate meat research priorities will be assisted by selecting scientists who not only know good science, but who also have an inquisitive interest in a variety of research areas, appreciate multi-disciplinary research needs and can relate good science to problem-solving needs. Thus, item 2 in Table 1 is meant to specifically address meat research or areas that clearly involve the inputs of many disciplinary areas of science.

In addition to the scientists who are involved in setting priorities, it may also be preferable to solicit or involve the views of non-scientists for many of the meat research areas. These could include consumers, dietitians, regulators, salespeople and producers. These individuals can provide useful perspectives and soliciting their input can broaden the base of support once the priorities are finalized.

Item 4 in Table 1 is concerned with properly defining both short- and long-term objectives and priorities. Without this consideration, the priorities will be less coherent because of a mixture of urgencies attached to the list of priorities. This mixture is almost inevitable because different individuals and groups tend to attach a different priority to short- and long-term research needs when in fact both are necessary. Once a problem is well defined and the sequence of information necessary to resolve a problem can be delineated, then a time frame for achieving each of the research priorities should be established. The problem then becomes one of funding and coordinating the research inputs necessary to address the problem.

Priorities cannot be properly established until one considers what is known about the problem, why it still exists and the possible avenues of research for resolving the problem. Thus, the reason for item 5 in Table 1. After completing this exercise, it may be determined that no additional information is necessary for a given aspect of the problem, that an adaptation or new technique is necessary to advance the area, or that a major emphasis of the research should be targeted to a very specific area because without its resolution further progress would most likely be very limited. This exercise would also be helpful in directing resources to the most fruitful areas for problem resolution and minimize the duplication of effort about what is already known. There is a need to more clearly delineate the minor and major questions related to a problem. In other words, is the suggested research a fine tuning or is it directed toward major unmet disciplinary and problem needs? Another important question is as follows: "Other than money, what is the most limiting factor in resolving the problem associated with each research priority?"

Once the priorities for a given problem area have been defined, it would be ideal to know the resources and timeframe needed to address specific questions and the probability of succeeding. Although it is recognized that this

may be difficult to achieve for all problems, it needs to be more frequently considered. In this regard, it is increasingly important to consider how the research might be coordinated among scientists and the incentives for them to do it. This would be helpful in reducing unnecessary duplication of effort and would encourage team work that can accelerate the resolution of the problem.

Once research priorities have been established, the justification for each should be given. Whenever possible, the justification should include the scientific and practical importance of the priority. For example, the economic aspects of the problem can be a very useful part of the justification. At this point in development, it may be ideal to get a reaction to the priorities from individuals or groups not involved in their development. The purpose of this exercise would be for fine tuning, additional justifications or expression of major concerns rather than a major redoing of the priorities. Obviously this will extend the process, but there are times when it may be very useful.

Following the completion of the development of the priorities, it is necessary to broaden their base of support to the group or groups identified at the beginning of the process. Items 8 and 9 in Table 1 relate to this aspect of the process. The priorities should not only be printed and distributed as appropriate, but someone needs to speak for or "champion" the priorities in a vigorous way if they are to have meaning and impact beyond the primary group or organization that asked for their development. This is absolutely essential for national priorities where getting the attention and commitment of key decision-makers is not easily accomplished.

The last item in Table 1 is one that needs careful consideration. It has to do with the lifespan of research priorities and the individuals or committees charged with their drafting and review. For example, the highly specific research priorities of the individual laboratory scientist are frequently very dynamic and change weekly or monthly. However, at the other end of the spectrum are research priorities which should be less specific and which for many will be viable for more than one year. Since this last category is the one of most concern to organizations such as AMSA and the Intersociety Research Committee, I believe some caution needs to be exercised about how often national research priorities are changed. If a significant number of priorities have to be changed each year because they are out of date, they are probably too narrow. National research priorities should focus on a few research themes such as meat residues, meat microbiology, diet-health issues, processed meats, meat flavor, etc. In contrast, more specific research priorities are necessary for project proposals, regional research projects and for use within a group or organization that is responsible for doing the research. If the thematic research in national priorities is changed too often, the majority of them will probably have little or no impact on key decision-makers because the process of gaining such support frequently requires more than a year. Therefore, it may be undesirable to have a standing committee concerned with developing national research priorities unless the writing of new research priorities is clearly defined in relation to current national research priorities for the area. In this regard, it should also be noted that many non-scientific groups would establish some new national priorities each year. These would almost always

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