At the risk of losing this whole group, I have to open with the apology that I am probably going to broaden out the discussion from the advertised topic. The reason is that I personally have a hard time separating research and economics. Let me explain.

In industry, especially forest products which is not a high margin industry, research capital requires a return on the investment just as any other capital investment. Therefore, very quickly in the evaluation of research projects we get to the position where all research projects or options have to have economics applied in order to come to meaningful decisions. By meaningful decisions I mean those that impact the P & L statement favorably.

With that backdrop, ya'll are probably thinking about what I'm saying in the same manner as Ronald Kluch, a researcher at the Oak Ridge National Laboratory. He speculates that creativity is stifled by our management systems. He goes on by speculating that under today's modern management systems, penicillin never would have been discovered because the technical manager would have rejected the research request!

Let me get it up front then that the forest industry deals basically in commodity markets and therefore research for new products is not a very successful adventure. Research of the general or theoretical type does not fit the forest products industry either. The forest product industry can best be served by applications research. By that I mean research which derives benefits through

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decisions which change the process or "doing aspects" of a particular business function. Fundamental or theoretical research for general, broad application and knowledge transfer is best suited to the federal agencies and the university sector.

Having said that, I have made the first step of prioritization and it surely baits the question of how industry deals with applications and knowledge transfer. Let me suggest how the process might work. In industry, we have the whole spectrum to consider and understand in order to be profitable.

If you start with the land and flow the process through biology, genetics, logistics, allocation, conversion, product lines, regeneration and energy, you have an example of broad categories for applications research. Under these broad headings one might expand the groupings into finer categories, such as natural stand management, natural stand harvest, wood flow from natural stands, value potential, raw material allocation, tree segmentation, transportation of trees and logs, storage yards, breakdown yards, and conversion facilities, which may be either solid wood or fiber, with subheadings under each of the product lines, such as machine type, machine repair, replace, reduce or inject new technology.

Now the question is (with this long list of processes or systems, we obviously can not do all at once), how do we set priorities? First, we need to ask ourselves, where in these processes do I see the greatest opportunity for economic leverage? Answering the question requires the application of economics to the whole spectrum of research alternatives. By asking ourselves about changes applied to any of the process areas, we focus our research on those decisions that leave the greatest impact on the economics.

Let me give an example to make this clear. Consider research for the purpose of estimating the base productivity of our land holdings. The output of this research is a table of acres by site class. Since the purpose is only to estimate site productivity, the trees which we plant and harvest will not grow any
differently as a result of the research. If our old system of estimating site index was in error and the new system corrects the error, we will not grow any more wood or derive any more value from our land -- unless, as a result of this new knowledge, we do something differently. For instance, with this new knowledge we may decide to change our planting level on a certain number of acres. The benefit of the research then is the increased value resulting from the changed stocking on those acres.

To illustrate further, say our policy is to plant site index 60 land at 400 trees per acre and site index 70 land at 450 trees per acre. Suppose that the NPV of planting 400 trees on site 70 is $300/AC, and the NPV of planting 450 trees on site 70 is $400/AC. As a result of the research, we might discover that 10,000 acres of land that we previously thought was site index 60 is now site index 70; so instead of planting 400, we plant 450 trees on it. The benefit of doing that research is 10,000 x ($400−$300) = $1,000,000 if we were to plant all 10,000 acres this year. This is simply the marginal value of our changed decision.

Now, it is a rather straightforward process to estimate the value of the research after the research was done. One just has to look at the old decision and the new decision and the contrast the difference in value between them on the acres that were impacted. However, the trick is to be able to estimate the value of research before it is done. This, in essence, must involve estimating what the likely outcome is and the probability of achieving each outcome. Again returning to our example, before we started this site productivity research we might have had a hypothesis which said that with probability 0.5, 15,000 acres of the land which is classified as index 60 should really be classified as site index 70. In this case the expected value of the research is 0.5 x 15,000 x ($400−$300) = $750,000. Obviously, in most cases the analysis will be more complicated, with several possible outcomes each with its associated probability.
Prioritization therefore is driven by potential gain. Business strategy is another very important ingredient. Now that we have our priorities, we get a research budget approved easily, guaranteed to pay big returns on the investment, and away we go. Right? Let's examine it. Suppose we determined there were large gains to be made by changing the tree segmentation into logs from a manual-visual process to an automatic electronic tree scanner-valuation-segmentation allocation process for log input to a manufacturing complex. Due to the large financial gains estimated to result from such a change, we made it a priority project. To complete the R & D on the scanners, process hardware, software, pilot test and retrofit hardware to the existing manual system, we estimate 3 years to complete the work and have it in an operational status. Not a bad time consumption for a high return project. Before we jump in, however, we need to know the strategy of the lumber business that we are going to apply this new process to. When we check with the lumber business and find that the manufacturing complex is changing to all small log machinery in five years due to the changing nature of the raw material supply, then this leaves a time period for payout of only 2 years. Such a short time period of course will change the return on the capital. In this example, the required capital would be substantial for both the R & D and the equipment retrofit; therefore, the return would probably be negated. The result, then, would change a high priority project to one to be scratched from the list.

Another very important aspect of time in the prioritization process is the time gap between implementation of a change and utilization of the change. A simple example for illustration is one which we all need to be aware of, and that is how to grow and manage our plantations. We could apply current research and decide to grow plantations for fiber and process in our current paper mills 20 to 30 years from now. But 20 years from now we may find that paper has become obsolete due to electronic media and transmittal processess -- which would
obviously impact unfavorably on the bottom line. In time gap situations, we need to incorporate flexibility in order to cope with the changing scene and subsequent financial impact.

Capital constraints are, of course, additional considerations in the prioritization process. Each business has access to limited amounts of capital. Allocation of that capital must be soundly integrated into the R & D priority process, or we may find that some good research results have to sit on the shelf waiting for available funds to implement them.

Along with capital availability consideration comes cost of capital. Acceptable and clearly doable projects can become scratched at a certain point on an escalating cost of capital curve.

Not necessarily the last consideration in prioritizing research, but the last one I'll take time to discuss, is the technical complexity of process change resulting from research output. To reap the total benefits of the economic gain, the research has to be successfully implemented. Believe it or not, this sounds easier than it is. I believe several books could be presented on this subject alone. As a general rule, we are finding in our industry that the less complex process changes pay higher returns, simply because they are quicker and easier implemented. This varies, of course, by individual company, geographic location, and process consideration. For example, attracting and hiring competent electronic and computer repairmen and programmers is easier to do in Atlanta, Georgia, than it is is Dierks, Arkansas.

In summary, research priorities are set by evaluation of economic leverage, integration of business strategies, economic life of the research investment, time lag between change and economic returns, capital constraints and capital costs, as well as the technical complexity or estimated degree of implementation success.
Switching to what is being researched today, I'm back to my categorization of processes from the soil to product. I'm sure there is research going on in some facet in each of the categories somewhere in the industry. In lieu of going back to an itemized list, let me discuss an area that is probably of the most concern to the industry today -- plantations and the economic integration of management regimens in the plantation growing process.

We have a project going called SEER which uses computer modeling and encompasses the analysis of single acre forest management. SEER stands for "System for Economic Analysis for Regimens." This is a fairly large computer system designed to consider management alternatives on individual acres or individual forest stands.

There are three parts to the overall SEER system. The first part consists of a growth and yield model which predicts tree number, diameter, and volume as a function of site index, age and silvicultural prescriptions. The silvicultural prescriptions which SEER can handle include variable planting levels, fertilizer, and thinning (including the number and size of the trees removed, and the frequency of the thinnings). The second part in SEER is a raw materials cost and value module which predicts the associated cost for harvesting, hauling, yard and milling operations, and the value of the resulting products. These costs are sensitive to the particular setting configurations (slope, distance to mill, etc.) and the characteristics of the harvested trees. The final part of the SEER system is a financial module which performs two operations. First, it estimates the costs of the non-harvesting related silvicultural operations, such as planting, weed control, stocking control, fertilization, etc. Secondly, it discounts all the costs and values to compute the present net value and return on investment from the discounted cash flows.
The overall system can and has been used to derive normative prescriptions for various classes of bare land. These normative prescriptions are in essence forecasts of the actual stand management practices which will occur on our land as the timberland business and operating regions make those decisions in the future. In this sense, they provide a central tendency for the practices which we are likely to employ. These normative prescriptions, and the yield and values estimated from them, are used in a strategic sense for long-range planning. They are also used to make various tactical decisions concerning individual silvicultural operations, such as stocking control and fertilization. Using SEER in this tactical way, marginal returns on investment for various silvicultural operations can be estimated and compared with alternative investment opportunities.

A second major use of SEER is to use it to perform sensitivity analyses to discover which possible future technologies have large potential impacts on the wood volume and value produced from our land. For example, in the harvesting and handling area, scientists might envision a future harvesting machine with certain design and performance characteristics. The SEER code allows the performance of that machine to be simulated and the effects of that performance, in terms of bottom line cost savings, to be estimated. In this way we can scope the potential impact of future research before it is started. Other examples of this application of sensitivity analysis include new milling technologies, which would change the value of the products produced, and new biological technologies, such as in the area of fertilization.

Another application of SEER for the economic analysis of forestry opportunities involves determining the economic benefits of various generic biological impacts. One can determine the benefits of a ten percent increase in survival rate of outplanted seedlings, for instance, using SEER along with a long term forest simulation to estimate forest level impacts. Another example would be to determine both the single acre and forest management economic benefits associated with a ten
percent increase in harvested volume, or a two year shortening of rotation with no loss of volume. Using the tools in this way, one can discover the overall leverage points in our biological/financial system, and then direct research accordingly.

At this point I am back to leverage points and prioritization, and hope that I have left you with the impression that perhaps correct prioritization is almost more important than the research itself, because I have most certainly attempted to do that. Research results parked on the shelf for whatever reason ought to be avoided by the forest industry, especially in this day and time of disastrous solid wood markets, poor fiber related markets and the high cost of money. Successfully implemented R & D, on the other hand, can contribute to reducing inflation, improving margins, or just plain survival in these current hard times. Proper prioritization at the beginning is the key to successful R & D in industry, and economics is vital to the process.