

# NCCC-134

APPLIED COMMODITY PRICE ANALYSIS, FORECASTING AND MARKET RISK MANAGEMENT

## **Research on Price Forecasting and Marketing Strategies: Improving Our Relevance**

by

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Suggested citation format:

Brorsen, B. W., and S. H. Irwin. 1994. "Research on Price Forecasting and Marketing Strategies: Improving Our Relevance." Proceedings of the NCR-134 Conference on Applied Commodity Price Analysis, Forecasting, and Market Risk Management. Chicago, IL.  
[<http://www.farmdoc.uiuc.edu/nccc134>].

## Research on Price Forecasting and Marketing Strategies: Improving Our Relevance

B. Wade Brorsen and Scott H. Irwin\*

### Introduction

There is a growing perception that agricultural economists' research on price forecasting and marketing strategies is of limited relevance to real-world applications. This concern has been voiced repeatedly by industry representatives attending previous NCR-134 Conferences. Similar concerns have been raised in numerous other forums with respect to agricultural economics research in general (e.g. Armbruster, 1993; Robison and Colyer, 1994).

The purpose of price forecasting and marketing strategies research should be to increase social welfare through improved resource allocation. There is little direct evidence whether our research does or does not increase welfare. The only direct evidence favoring price forecasting is found in Freebairn (1976) and Antonovitz and Roe (1984). Both studies report substantial welfare gains to improved price forecasts, but as Irwin (1994) notes, the analysis likely overstates the improvement because private information is ignored.

Most importantly, if our research is to increase social welfare, then it needs to be used. The available evidence is profoundly discouraging. Batte, Schnitkey, and Jones (1990) found the Cooperative Extension Service ranked 12th out of 19 information sources used by producers for marketing decisions; behind farm magazines, commercial newsletters, and marketing consultants. Other studies report similar findings (e.g. Smith, 1989). This suggests a general irrelevance of the price forecasting and marketing strategies information that researchers provide to extension specialists.

A number of explanations for the apparent lack of practical relevance have been offered. Bromley (1993) suggests an agency problem exists between society and agricultural economics researchers. That is, the incentives facing agricultural economics researchers fail to elicit the types of research that society desires. In a similar vein, Robison and Colyer (1994) suggest that professional certification through peer-reviewed publication is overemphasized. Bonnen (1986) argues that our profession has drifted towards "anti-empiricism." Finally, Robison and Colyer (1994) suggest that publication pressures and cost efficiencies contribute to an over-emphasis on testing secondary data in a standardized format.

In sum, the evidence is persuasive that a real and significant problem exists with respect to the relevance of price forecasting and marketing strategies research. In this paper, we focus on ways of improving the relevance of this research. First, we suggest a new theoretical framework for valuing price forecasting and marketing strategies research. Then,

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we discuss key research application issues. Next, we offer some thoughts on the role of the NCR-134 Conference in the process of improving the relevance of our research. Finally, we approach (cautiously) the implications of our analysis for extension programs.

We recognize that any paper like this will focus on what we are doing wrong, not what we are doing right. Undoubtedly, there are many examples of practical research on price forecasting and marketing strategies. We intentionally focus on the problems and take firm positions in order to provoke discussion. It is our deeply-held view that the problems cannot be solved individually, but only through joint action. We hope this paper contributes in some small way to moving forward.

### **A New Theoretical Framework**

As noted earlier, the purpose of price forecasting and marketing strategies research should be to improve social welfare through improved resource allocations. However, expectational and information assumptions greatly influence your view of whether we can in fact accomplish this goal. We believe agricultural economists have been trapped between two opposing models, one that assumes too little on the part of producers and one that assumes too much. We suggest a new theoretical framework that lands someplace in between these two alternatives.

Irwin (1994) argues that the traditional justification for public programs in this area is based on the assumption of naive, backward-looking (cobweb) price expectations on the part of producers. Because of the naive expectations, producers make systematic forecasting errors, which in turn results in mis-allocations of resources. Hence, social welfare can be increased by providing producers with more forward-looking forecasts and marketing strategies.

In recent years, a popular assumption is that producers form rational expectations (Muth, 1961). Rational expectations imply that producers use all available information when making forecasts and do not make systematic mistakes. If producers have rational expectations, price forecasting and marketing strategies research *cannot* improve social welfare. Resources spent on public research represent a net social loss. Hence, if producers have rational expectations then we should not even do any research, as producers already make the best possible forecasts and marketing decisions. The rational expectations model is often referred to in the finance literature as the efficient market hypothesis (Fama, 1970).

Clearly, the rational expectations model provides a strong theoretical challenge to continued funding of public research on price forecasting and marketing strategies. Despite mixed empirical evidence (e.g. Irwin and Thraen, 1994), rational expectations is the most logical expectations assumption. Numerous deviations from market efficiency have been reported in the literature, yet none of the deviations seem large enough for a producer to exploit profitably.

The rational expectations model, however, assumes too much knowledge on the part of producers. First, the rational expectations model requires that producers know the true underlying parameters of the supply and demand functions. The mechanism by which they

learn these parameters is not specified. Second, the rational expectations model assumes that information is costless. Hence, producers incur no costs as they gather and analyze information in the process of forming expectations.

The "noisy" rational expectations literature provides a framework with more realistic assumptions about learning and the cost of information. Models in this literature assume producers have rational expectations, but must learn model parameters and purchase information. This opens the door again to a theoretical justification of public research on price forecasting and marketing strategies. However, a noisy rational expectations model provides a higher "hurdle" for justifying public expenditures than traditional cobweb models.<sup>1</sup>

In a noisy rational expectations equilibrium, social welfare depends on the speed of convergence towards the rational expectation equilibrium. All else constant, total social loss will be smaller, the faster that a rational expectations equilibrium is achieved (Stein, 1992a, 1992b). To determine whether our price forecasting and marketing strategies research has social value, we must ascertain whether the research significantly speeds convergence to equilibrium in agricultural markets.

Public research may increase the speed of convergence to a rational expectations equilibrium by educating producers regarding the structure and parameters of the underlying economic model and prospective economic conditions. But, public research may be less valuable where active futures and options markets exist. Stein argues that the existence of these markets substantially lowers the cost of trading, which allows firms to more readily profit from their private information. This in turn speeds convergence to a rational expectations equilibrium. Hence, public research is less valuable in commodity markets with futures and options trading than those without them. Therefore, we may be over-investing in outlook on corn, soybeans, wheat, cattle, and hogs relative to outlook on such things as land, sheep, and ostriches.

To summarize, the benchmark for public support of our research should be whether it increases the speed of convergence towards a rational expectations equilibrium. This new benchmark presents a high, yet realistic, standard for our research on price forecasting and marketing strategies.

### **Critical Applied Research Issues**

The noisy rational expectations model provides only a broad framework for valuing and conceptualizing price forecasting and marketing strategies research. It is left to the individual researcher to provide the rich detail of empirical evidence. In order to produce relevant empirical evidence, however, we think there are three critical issues that must be addressed. These are: 1) confronting models with data on actual producer behavior,

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<sup>1</sup> Bray (1985) provides an excellent introduction to the noisy rational expectations literature.

2) confirming applied research results, and 3) careful attention to the underlying structure of agricultural markets.

### Confronting Models

To produce relevant applied research on price forecasting and marketing strategies, we must first confront our models with micro-level data on the behavior of participants in agricultural markets. Unfortunately, in many instances, we have almost totally abandoned this practice. Instead, we build more sophisticated and complex models and "test" them using secondary data. There is undoubtedly rational reasons for our behavior. As Robison and Colyer point out, such confrontation is expensive because it requires careful collection of primary data.

One of the best, and most topical, examples of sterile ("non-confrontational") modeling is in the area of optimal hedging. The gap between actual practice and our research is large by any measure. To demonstrate the problem, we examine evidence from two recent studies. Table 1 presents findings from a study by Schroeder and Goodwin (1993) that shows producers tend to hedge very little (less than 20%) and when they do, they usually use forward contracts rather than futures contracts. Figure 1 shows data from a study by Martines and Irwin (1994). The optimal corn hedging recommendations for the 1992 pre-harvest period are plotted for six private market advisory services. The data show that the recommended hedge ratios are typically less than 0.50, and there is substantial variation across time in the individual recommendations. Also, there is significant variation across advisors.

This evidence can be compared to the optimal hedge ratios typically estimated by agricultural economists. For example, at the 1992 NCR-134 Conference, papers were presented estimating optimal or minimum variance hedge ratios of 0.75 - 1.02 (Norvell and Leuthold, 1992; Lence, Kimle, and Hayenga, 1992). Similar ratios can be found in a number of other studies. Tomek (1987) argues this gap can be explained by considering hedging costs and yield risk, but we really do not know. Peck and Nahmias (1989) have shown that minimum variance hedge ratios cannot explain the changes in the level of hedging over time of flour mills. Since our current theory of hedging does not match what people really do, philosophy of science such as Friedman's positivism or Popper's falsificationism (see Blaug, 1980) would say that our current theory of hedging can be rejected.<sup>2</sup>

Optimal hedging represents just one area where we need to confront models with actual data on producer behavior. Another area is price expectations. Irwin and Thraen (1994) note that there is little direct evidence on the way producers form price expectations. We know even less about how producers process and evaluate information. Clearly, there are many challenges to be faced.

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<sup>2</sup> As Bessler (1993) noted, when a paradigm shift is underway, authors sometimes resort to unconventional methods such as writing essays. We hope this essay is part of a small scientific revolution because a revolution is needed.



### **Confirming Results**

If applied research results are to be practically relevant, they must be reliable. But how can we establish reliability? There are several approaches to this issue, but we concur with Tomek (1994) that the best approach is independent confirmation by other researchers. This issue is thoroughly (and we might add, persuasively) argued in Tomek's article. We can only repeat his arguments and suggest its importance to price forecasting and marketing strategies research.

Tomek defines confirmation to mean, "...attempts to fit the original model with the original data." (p.6.) He also defines the closely related concept of replication as, "... the fitting of the original specification to new data." (p.6) Divergent results are often found as a result of confirmation or replication efforts. Tomek suggests four reasons for the divergent results: 1) differences in models, 2) differences in data, 3) use of alternative estimators, and 4) variations in the way results are used and analyzed. Substantial knowledge about reliability can be gained by understanding sensitivities to the above four factors.

Surely there are few areas where reliability is more important than in the formulation of price forecasts and marketing strategies. Unfortunately, the limited available evidence suggests our research results are highly fragile. Tomek demonstrates this for two models of meat demand. Irwin and Thraen (1994) review a number of econometric rational expectations models and find a tremendous variation in results. For example, depending on the study examined, soybean producers are implied to have adaptive expectations, naive expectations, perfect foresight, or rational expectations.

We currently devote almost zero resources to independent confirmation and replication of previous research results. It is possible to reach the depressing conclusion that we don't attempt to do so because we know no one actually uses our research results! Otherwise, we would be much more concerned with the issue. Confirmation and replication needs to be a standard part of our research on price forecasting and marketing strategies.

### **Structural Realities**

Another important applied issue is the structural assumptions that underlie our research. Relevant and reliable research has to reflect to a reasonable degree the underlying structure of agricultural markets. The structure of these markets has been evolving over the entire time-span that agricultural economists have been examining them. However, the pace of structural change appears to be quickening in many markets, especially for livestock. Many observers argue that even faster change is in store.

Much of our research, particularly on marketing strategies, ignores the large structural changes that have occurred and will likely continue to occur in the future. We simply have not recognized the changing reality of agricultural marketing, and seemed to be trapped in a 1950s view of agricultural markets. Agricultural markets are becoming much more vertically-integrated.

With the evolution to more vertically-integrated markets, trade in centralized, terminal markets is replaced by individually-negotiated contracts. Hence, the economics of contracts takes center stage. Research in this area traditionally has been conducted by agricultural economists with an interest in industrial organization. However, this need not and should not be the exclusive domain of these individuals. Both Mark Powers in his 1985 address to the NCR-134 Conference and James Moser at a recent CBOT Financial Educator Seminar have called for more research on financial institutions and regulations. Agricultural economists interested in marketing strategies must take an active interest in this area, or risk even further irrelevance to a significant group of participants in agricultural markets.

A substantial body of theory is available to guide new empirical research into agricultural contract economics. One strand of this literature deals with information asymmetries and principal agent problems. Bhattacharya provides an excellent survey of theory in this area. Other strands deal explicitly with the bargaining process and the outcome in terms of contract terms. This is in reality a substantial portion of the new theoretical work done in economics over the last twenty years. To date, it has been largely ignored by agricultural economists.

#### **A Modest Agenda for the NCR-134 Conference**

A reasonable response to the previous discussion is, "If these issues are so important, why aren't we researching them?" This is a particularly good question, given that we are both favorably disposed to an efficient market view of the world. We are convinced there is a "rational" explanation for our behavior. A number of writers (e.g. Leontief, 1993; Tomek, 1993; Robison and Colyer, 1994) point to two key factors: costs and incentives.

First, solving each of the applied research issues is costly, both in terms of time and direct costs. Collection of primary data generally is costly, as is the process of confirming the results of previous studies. Second, the incentives faced by agricultural economists at land-grant universities typically work against solving the applied research issues. Our current incentive structure rewards publication quantity, which is most easily accomplished with sophisticated and "innovative" models tested on secondary data.

For those of you waiting for the punch-line, you will be disappointed; we don't really have any profound suggestions to remedy the situation. But, we do think there are useful incremental actions that we can take collectively. As reviewers, we have the power to modify incentives. Also, the NCR-134 Conference can play a useful role in the process of change.

Consider the issue of collecting primary data on micro-level behavior. We think there are some underutilized sources of primary data. For example, consider the case of private market advisory services. These services are now the most important source of marketing information to producers (survey after survey documents this). Many have records going back a number of years. While it will not replace direct evidence on hedging behavior, their recommendations represent a rich source of data on marketing behavior. Presumably, since producers pay for the information, they use it.

These services have almost no connection to agricultural economists and their research. Agricultural economists have likely ignored them because of the perceived difficulty in gathering primary data. Maybe the services ignore us because they find our research completely irrelevant.

A new and useful relationship could be formed between agricultural economists interested in price forecasting and marketing strategies research and private market advisory firms. How to get such a relationship started is not an easy matter. We have discussed the matter with a few individuals in the advisory business, and they seem enthusiastic about the possibilities. We suggest the NCR-134 Conference plan a special session, in cooperation with the advisory services, that would explore the possible linkages. Perhaps this could become an on-going component of the Conference.

In some ways the incentive structure is the most difficult to overcome. But, we have to start someplace, and we think the NCR-134 Conference is as good of place as any to start. The papers presented at the conference do often end up getting published in the agricultural economics literature. Hence, presentation at the conference, and subsequent publication of the proceedings, does appear to have a revealed value to agricultural economists. This suggests that some influence on the research process can be achieved through the organization and structure of the NCR-134 Conference.

One possibility is to reserve space on the program for papers in high priority areas. For example, we could declare a year in advance that one session will be devoted solely to primary data studies or confirmation studies. Researchers would still need to submit abstracts in the usual fashion, but there would in all likelihood be a much higher chance of acceptance with this procedure. If no abstracts are submitted, we could simply revert to an open session. Even more structured alternatives could be considered. Specific papers could be commissioned, similar to the paper we are presenting today.

In sum, we have more ability to influence the research process than is commonly believed. Let us also be clear about an important point. We are not suggesting the NCR-134 Conference become an invitation only affair. The conference serves as an especially useful forum for young faculty members and graduate students, and this aspect should be preserved. We simply suggest some alteration of the portfolio of papers presented. This will require discussion, debate, and imagination to ensure a fair and open forum.

### **Extension Programs**

Before concluding, we offer some observations on extension programs in price forecasting and marketing strategies. It is obvious that one cannot entirely separate research and extension programs. So, we cautiously make a few observations.

We believe agricultural economists working in extension provide inconsistent information because of the inconsistency of their underlying models. Some rely on conceptual models with naive expectations, while others clearly employ models with rational expectations. We argued earlier that both of these models are too extreme. If noisy rational expectations theory becomes the foundation of our theoretical models, then it should also be



the foundation of our marketing education programs. Therefore we will speculate on the implications of noisy rational expectations theory for marketing education programs.

To begin, producers should be considered uninformed traders since they do not have access to any special source of information. Producers are attempting to become informed traders by purchasing information. In a noisy rational expectations equilibrium, the returns to information equal the cost of the information. Further, producers receive information with a lag and have little economies of size in gathering information. These two difficulties can be overcome by hiring a marketing advisory service, but still theory suggests it is a break-even proposition (after adjusting for costs and risk).

This new theory, unlike pure rational expectations, does not say that trying to forecast prices is a waste of time. It says that forecasting prices is only possible when superior information is obtained. The theory also says that information must be obtained and acted upon before other traders if it is to be useful. Extension cannot match the speed of a marketing consultant that does the actual trading. Therefore, extension should move away from predicting price levels. Many extension programs have already done this to varying degrees.

Extension definitely has a role in evaluating market advisory services. There is also still a potential place for an extension outlook program that transforms a naive producer into a rational one. Such an outlook program might rely on the futures market to obtain price forecasts. The program would emphasize education, e.g. how to derive the price forecasts implicit in futures prices and why these implicit price forecasts are rational. It could also inform producers of the known tendencies to deviate from a random walk such as the tendency of daily and weekly cash prices to exhibit positive autocorrelation. Such an outlook program might increase the economic efficiency of production decisions.

Extension should also put greater emphasis on managing risk. As noted in many places in this paper, the problem is that much of our risk management research seems to be of little use to producers. We must first develop models which are relevant. Eales, Engel, Hauser, and Thompson (1990) found that producers consistently underestimate the variance of their price forecasts. We can help them better determine risk levels. Such information could help producers determine when to hedge. Our current models which assume hedging is costless are obviously inadequate. We must inform producers about the costs of using forward contracts, futures, and options. If producers are indeed naive traders, then producers should not hedge unless they are willing to accept a smaller profit per unit in order to reduce risk. They could still increase total profit if the reduction in risk allows them to take a larger position. We must develop models which view marketing risk as just one part of the risk faced by producers. This will lead to models in which the recommendations vary depending on the farmer's individual situation.

### Conclusions

Our research portfolio is out of balance. We have too much supposedly applied work which is never used, too many applications or refinements of existing methods which have little connection to the real world, and not enough of what Leontief (1993) calls fact-finding research. Most of our research uses publicly available data which is easy to obtain. Such research is useful, but some important questions cannot be answered with public data.<sup>3</sup> Research that addresses big questions is often easy to criticize. As reviewers, we must give greater value to research which addresses the important questions, but uses imperfect data and simple statistics. We must give lower value to research which uses established approaches and addresses minor questions. Such research is difficult to criticize, but may be of low value.

We stand at a threshold regarding research price forecasting and marketing strategies. We can take up the mantle, adopt new theories, and collect new data, or we can ignore the changing reality and become more irrelevant. If we do not make some reallocations to increase the value of the information we provide, then continued funding of price analysis in research and extension is at risk.

We want to end on a positive note. We believe this is an exciting time to be a price analysis and marketing researcher. There are new and important questions that need to be addressed. There are new theories that can be used to guide the research. We don't have to simply apply traditional theory and methods. Let's get started!

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<sup>3</sup> Powers (1994) expressed a similar dismay about the portfolio of research published in the *Journal of Futures Markets*.

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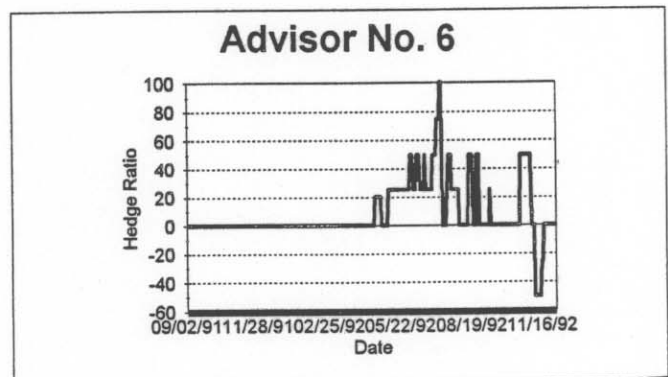
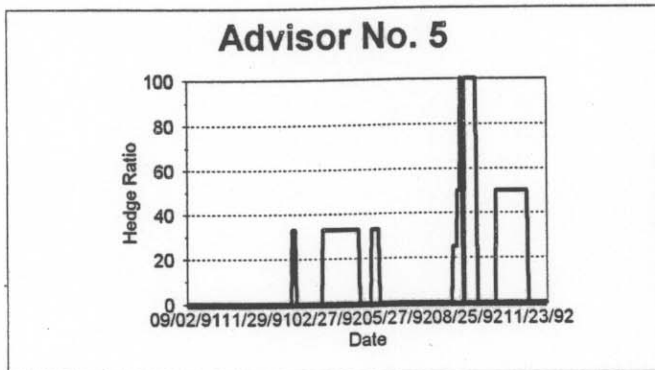
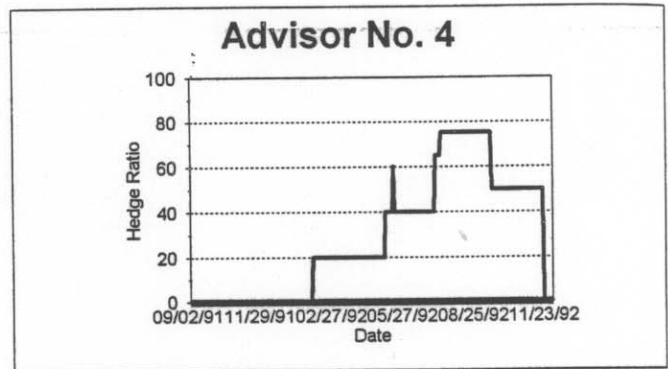
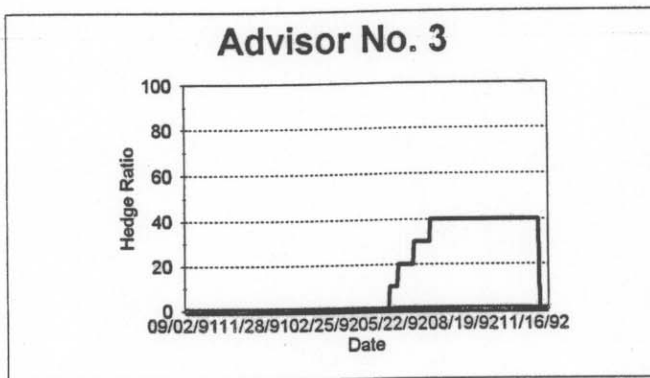
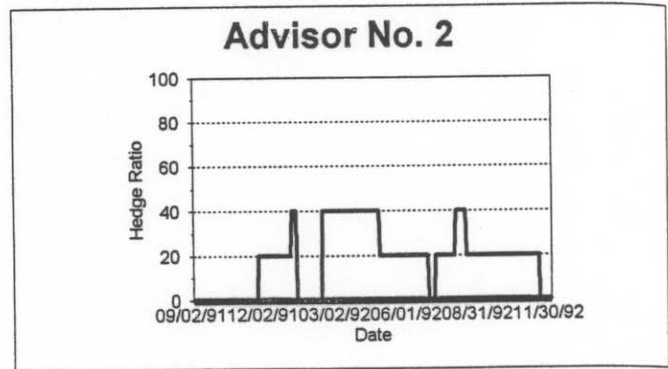
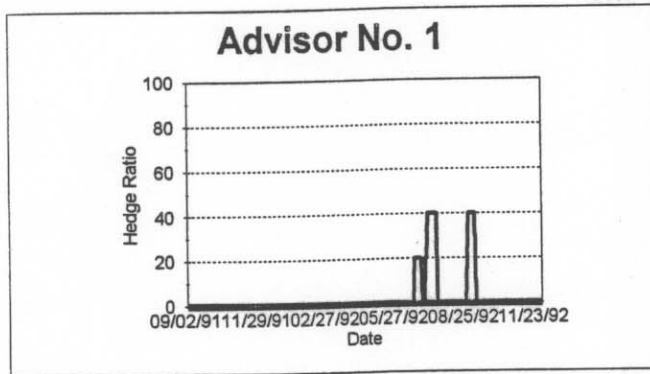
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**Table 1. Average Proportion of Crop Sold by Alternative Marketing Methods**

Crop	Forward Contracts	Futures Hedge	Futures Options
Wheat	9.7	1.3	5.0
Corn	12.8	3.5	2.9
Sorghum	6.2	0.4	1.0
Soybeans	10.4	1.5	1.6
Cattle	4.1	2.1	4.6
Hogs	0.2	0.4	0.5

Source: Schroeder and Goodwin (1994)



Source: Martines and Irwin (1994)

Figure 1. 1992 Pre-Harvest Optimal Hedging Recommendations by Market Advisory Services