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Improving the Relevance of Research on Price Forecasting and Marketing Strategies

B. Wade Brorsen and Scott H. Irwin

Agricultural economists' research on price forecasting and marketing strategies has been used little by those in the real world. We argue that fresh approaches to research are needed. First, we argue that we need to adopt a new theoretical paradigm, noisy rational expectations. This paradigm suggests that gains from using price forecasting models with public data or from using a marketing strategy are not impossible, but any gains are likely to be small. We need to conduct falsification tests; to perform confirmation and replication; to adjust research to reflect structural changes, such as increased contracting; and always to conduct statistical tests. We also provide a modest agenda for changing our research and extension programs.

There is a growing perception that agricultural economists' research on price forecasting and marketing strategies is of limited relevance to real-world applications.¹ For example, this concern has been voiced repeatedly by industry representatives attending the NCR-134 Conference on Applied Commodity Price Analysis, Forecasting, and Market Risk Management. Similar concerns have been raised in numerous other forums with respect to agricultural economics research in general (e.g., Armbruster; Robison and Colyer).

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. We are aware of only two studies that find direct evidence favoring price forecasting. Freebairn and Antonovitz and Roe report substantial welfare gains to improved price forecasts, but as Irwin notes, the analyses likely overstate 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 quite troubling. Batte, Schnitkey, and Jones found the Cooperative Extension Ser-

vice ranked twelfth out of nineteen information sources used by producers for marketing decisions, behind farm magazines, commercial newsletters, and marketing consultants. Other studies report similar findings (e.g., Smith). 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 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 suggest that professional certification through peer-reviewed publication is overemphasized. Bonnen argues that our profession has drifted towards "anti-empiricism." Finally, Robison and Colyer suggest that publication pressures and cost efficiencies contribute to an overemphasis on tests with secondary data in a standardized format.

In sum, the evidence is persuasive that a 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, we discuss key research application issues. 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

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¹ We define a marketing strategy as a means of deciding when to sell a commodity whether sales are made with futures, forward contracts, basis contracts, or cash only.

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 thought. It is our view that the problems can be solved, not individually, but only through joint action. We hope this paper contributes in some small way to moving forward.

An Alternative 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 informational assumptions greatly influence our 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 an alternative theoretical framework that lands someplace in between the two traditional methods.

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 (Irwin). Because of the naive expectations, producers make systematic forecasting errors, which in turn result in misallocations 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 has been that producers form rational expectations (Muth). 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 research to improve forecasts is unnecessary, 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).

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), the rational expectations assumption is the most logical expectations assumption. Numerous deviations from market efficiency have been reported in the literature,

yet none of the deviations seems 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.

A "noisy" rational expectations model 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 do traditional cobweb models.² In a noisy rational expectations equilibrium, social welfare depends on the speed of convergence toward the rational expectations equilibrium. All else constant, total social loss will be smaller the faster a rational expectations equilibrium is achieved (Stein 1992a, 1992b). To determine whether 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. This is consistent with the long-held view that a vital aspect of public situation and outlook programs is economic education. Further, public research may be more valuable where active futures and options markets do not exist. Stein argues that the existence of these markets substantially lowers the cost of trading, which allows firms to profit more readily from their private information. This in turn speeds convergence to a rational expectations equilibrium. Hence, it can be hypothesized that public research is more valuable in markets without futures and options trading.

To summarize, the benchmark for public support of our research should be whether the research increases the speed of convergence toward a ratio-

² Bray provides an excellent introduction to the noisy rational expectations literature.

nal expectations equilibrium. This new benchmark presents a high, yet realistic, standard for 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. The individual researcher must provide the detailed empirical evidence. The empirical evidence, however, requires that four critical issues be addressed: (1) confronting models with data on actual producer behavior, (2) confirming applied research results, (3) carefully studying the underlying structure of agricultural markets, and (4) carefully conducting significance tests.

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 are undoubtedly rational reasons for our behavior. As Robison and Colyer point out, such confrontation is expensive because it requires careful collection of primary data.

An example of sterile ("nonconfrontational") modeling is optimal hedging modeling. The gap between actual practice and research results is large by any measure. Two recent studies illustrate the problem. Table 1 presents findings from a study by Schroeder and Goodwin that shows producers tend to forward price very little (less than 20%) and when they do, they usually use forward contracts rather than futures contracts. Figure 1

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 (sample of 539 Kansas farms).

summarizes data from a study by Martines and Irwin. The recommended corn hedge ratios for the 1993 preharvest period are plotted for six private market advisory services. These hedge ratios are typically less than 0.50. There is substantial variation across time in the individual recommendations, and there is significant variation across advisors.

This evidence can be compared with 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; Lence, Kimle, and Hayenga). Similar ratios can be found in a number of other studies. Tomek (1987) argues that this gap can be explained by considering hedging costs and yield risk, but we really do not know. Peck and Nahmias have shown that minimum variance hedge ratios cannot explain the changes in the level of hedging over time of flour mills. Since recommendations of the current theory of hedging do not match what people really do, a philosophy of science such as Friedman's positivism or Popper's falsificationism (see Blaug) would say that our current theory of hedging can be rejected.³

Another area where models need to be confronted with actual data on producer behavior is price expectations. Irwin and Thraen 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.

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 (1993) 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 the importance of confirmation to price forecasting and marketing strategies research.

Tomek (p. 6) defines confirmation to mean "attempts to fit the original model with the original data." He also defines the closely related concept of replication as "the fitting of the original specification to new data." Divergent results are often

³ As Bessler 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.

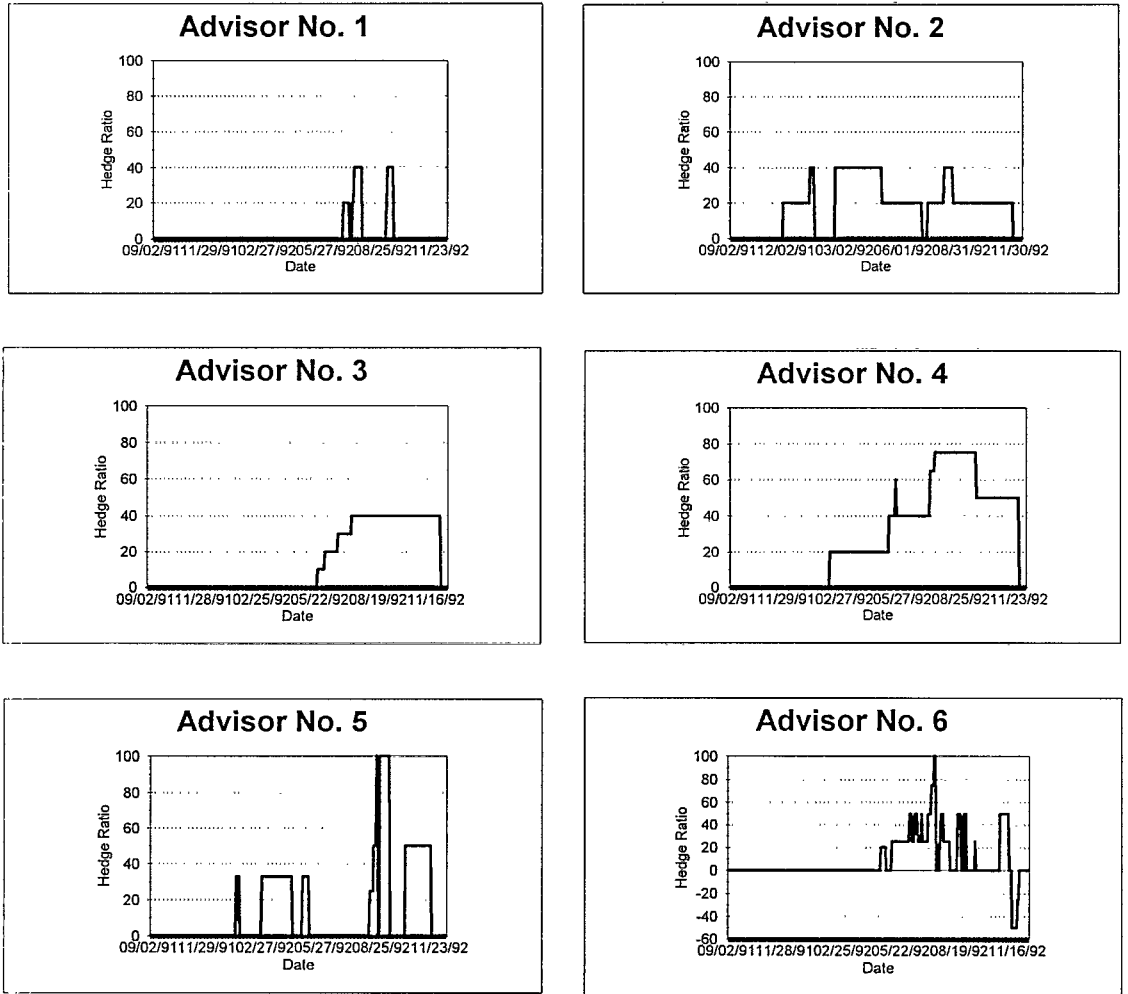


Figure 1. 1992 Preharvest Optimal Hedging Recommendations by Market Advisory Services
 Source: Martines and Irwin.

found as a result of confirmation or replication efforts. Tomek suggests four reasons for 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, few areas exist where reliability is more important than in formulating 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 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 do not attempt such tasks because we know that 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 resides in 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 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 seem to be trapped in a 1950s view of agricultural markets. Today 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 area need not and should not be the exclusive domain of these individuals. 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. Other strands deal explicitly with the bargaining process and the outcome in terms of contract terms. This area is in reality a substantial portion of the new theoretical work done in economics over the last twenty years. Sheldon provides an excellent introduction to the theory in this area and potential applications in agriculture.

Significance Testing

Another problem is that much of our research on marketing strategies and to a lesser extent price forecasting is just not very good.⁴ Noisy rational expectations theory suggests that the advantages of one marketing strategy over another or one price forecasting model over another are likely small. Thus, detecting differences in models may require

a large number of observations. Yet, many studies have been and still are conducted with few observations. Only when significance tests are included does it become obvious that too few degrees of freedom are available. Even when the evaluation is based on economic criteria such as trading profits, significance tests are still needed.

The early 1980s saw a rush of literature on price forecasting models (e.g., Brandt and Bessler). This literature produced inconsistent conclusions with autoregressive integrated moving average models (ARIMA) sometimes outperforming structural econometric models and sometimes not. A common convention was to use twelve out-of-sample observations in evaluations. Significance tests such as those of Ashley, Granger, and Schmalensee or Henriksson and Merton have shown that twelve observations are not enough. Some of the recent research on price forecasting includes significance tests and more than twelve out-of-sample observations (e.g., Goodwin). Unfortunately, most recent studies also fail to find significant differences. Thus, even more degrees of freedom may be needed. Most price forecasting studies use prices of hogs or cattle where quarterly or monthly observations are available. Usually with grains, only one observation per year is available.

Anderson and Mapp report the frustration of extension economists who are asked to recommend marketing strategies when only a few annual observations are available. Current choices are to conduct no significance tests⁵ (Pfeiffer, Sandell, and Kendrick) or to make conclusions even when significant differences are not found (Chen, Elam, and Ethridge). The tests are right. The noisy rational expectations model says the effects are likely to be small and that large numbers of observations would be needed to find statistical significance. One way of obtaining more observations is pooling across commodities and locations. A universal strategy would need to reflect the realities described by Benirschka and Binkley, that firms close to the market should store grain for shorter time periods than those located farther away from the market. The failure to subject research on marketing strategies to significance testing has led to false conclusions. Fortunately, losses are not high, since under noisy rational expectations theory, one strategy should be almost as good as another.

⁴ More of the price forecasting work has been done by researchers and has been subjected to the journal peer review process. Less of the marketing strategy literature is peer reviewed which may explain its inadequate statistical tests.

⁵ An additional problem with many marketing strategy results is that they are in sample. In such cases, standard significance levels need to be adjusted with an approach such as Bonferroni's method (Milliken and Johnson, 33-34), or a joint test of significance used, or out-of-sample tests conducted.

A Modest Agenda

A reasonable response to the previous discussion is to ask, "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; Tomek 1993; Robison and Colyer) point to two key factors: costs and incentives.

First, solving each of the applied research issues is costly, in terms of both 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 research on some applied problems. The current incentive structure rewards publication quantity, which is most easily accomplished with sophisticated and "innovative" models tested on secondary data.

While easy solutions do not exist, we do think there are useful incremental actions that can be taken collectively. As reviewers, we have the power to modify incentives.

We also 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 they 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.

Extension Programs

Before concluding, we offer some observations on extension programs in price forecasting and marketing strategies. Extension economists may provide inconsistent information because of the inconsistency of their underlying models. Some rely on conceptual models with naive expectations, while others employ models with rational expectations. We argued earlier that both of these models are too extreme. If noisy rational expectations theory be-

comes 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 useful *ex ante* forecasting is possible only when superior information is obtained. The theory also says that a successful, informed trader must obtain and act upon information before other traders do 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 prices.⁶ Many extension programs have already done this to varying degrees.

Extension definitely has a role in evaluating market advisory services and, in some cases, providing information and analyses to these 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 above, the problem is that much of our risk management research seems to be of little use to producers. Anderson and Mapp ar-

⁶ The available empirical evidence supports this conclusion as well. Allen argues that outlook forecasts are more accurate than naive or simple trend forecasts, but generally worse than a range of other methods.

gue that most of our risk models are so complex that producers cannot use them. Eales et al. 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 allowed them to take a larger position or to obtain a reduced interest rate. We must develop models that 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 that is never used, too many applications or refinements of existing methods that have little connection to the real world, and not enough of what Leontief calls fact-finding research. Most of our research uses publicly available data that is easy to obtain. Such research is useful, but some important questions cannot be answered with public data.⁷ Research that addresses big questions is often easy to criticize. As reviewers, we must give greater value to research that addresses the important questions but uses imperfect data and simple statistics. We must give lower value to research that 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 on 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. Undoubtedly, we will have to do the hard work of collecting new primary data. 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 do not have to simply apply traditional theory and methods. Let's get started!

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⁷ Powers expressed a similar dismay about the portfolio of research published in the *Journal of Futures Markets*.

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