Testing the Rationality of Price Forecasts: New Evidence from Panel Data

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This paper tests the rationality of individual price forecasts in a panel of professional forecasters. Here, unlike in most previous studies, rationality is not rejected. The results here differ because (1) using individual forecasts avoids aggregation bias, (2) comparison of forecasts to initial data avoids bias due to data revision, (3) the professional forecasters have economic incentives to state their expectations accurately, (4) a new covariance matrix estimator consistent when forecast errors are correlated across individuals is used. (JEL 132)

How people form their expectations of future economic events has been an important issue in macroeconomics for many years. Businessmen's expectations play a central role in the business-cycle theories of both Arthur Pigou (1927) and John Maynard Keynes (1936). Since the way in which expectations are formed has important implications for economic behavior, many economists have used survey data to test hypotheses about expectation formation. In particular, several researchers have tested the hypothesis that expectations are rational in John Muth's sense (Muth, 1961). In this paper, we provide a further test of the rational expectations hypothesis. Specifically, we test the rationality of price-level forecasts. We use price-forecast data from the ASA-NBER survey of professional forecasters, initiated by Victor Zarnowitz (1969, 1974, 1984, 1985). Zarnowitz, like most other researchers in this literature, finds that price forecasts are not rational.[1] However, using different statistical methods, we find strong evidence that price forecasts are rational.

We believe that our further testing of price-forecast rationality is warranted because of severe problems in almost all existing tests, which suffer from one or more of the following four flaws. First, some use average survey response data rather than individual data. This can bias tests in two ways. It can lead to false rejection of rational expectations because average forecasts that are conditional on *different* information sets are not rational forecasts conditional on any *particular* information set. Further,

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The authors' previously published paper, Keane and Runkle (1989), also deals with the subject of price forecast rationality. The main object of that paper is to explain for a general audience the basic issues involved in testing rationality. It presents several of the rationality test results presented here. However, that paper does not describe the statistical methods we develop to deal with aggregate shocks and moving-average errors, or discuss the issues of aggregation bias or the problems involved in measuring expectations. Nor does it contain the empirical results on the importance of aggregate shocks and moving-average errors, the results on the importance of data revisions, or the results on private information.

[1] Zarnowitz (1985) found that the forecasts of other variables, besides prices, were rational. We explain his finding of price-forecast irrationality. Lovell (1986) reviews the literature on tests of rational expectations.

it can lead to false acceptance of rational expectations by masking systematic individual bias that may be randomly distributed in the population. Second, many tests fail to deal properly with the pervasive problem of systematic data revision. Tests of forecast rationality depend upon correct assumptions about what the forecasters tried to predict and what they knew when they made their predictions. Much work has implicitly tested whether forecasters rationally forecast revised data conditional on other revised data, none of which was available until long after the forecasts were made. Third, most studies of forecast rationality use predictions from individuals who are not professional forecasters; these people have few economic incentives to report their expectations precisely. Finally, many studies that use micro data fail to account properly for the covariance structure of the forecast errors. This failure can take two forms. First, some studies assume that forecast errors must be white noise. In fact, lags in the availability of relevant data can produce serially correlated errors even when agents are rational. Second, most studies fail to account for the fact that shocks to the aggregate economy produce forecast errors that are correlated across individuals. In either case, improperly assuming independent, identically distributed errors can produce severely biased results.

In this paper we avoid these problems. We use the individual data on quarterly GNP deflator forecasts from the ASA-NBER survey of forecasters to test the rational expectations hypothesis. We treat the data as a panel to avoid aggregation biases, and we reconstruct the actual information sets available to agents when they made their forecasts. We compare these forecasts only to the GNP deflator data announced 45 days after the end of the quarter, before systematic data revisions. The survey data include only forecasts from professional forecasters, who have an economic incentive to be accurate. Because these professionals report to the survey the same forecasts that they sell on the market, their survey responses provide a reasonably accurate measure of their expectations. Thus, these data are less subject to the criticism made by opponents of survey forecast rationality tests that the respondents had nothing to lose if they made bad forecasts.[2] Finally, we develop a new covariance matrix that is consistent both in the presence of aggregate shocks and in the presence of serial correlation resulting from delays in data availability. When we use all these procedures, we find that the rational expectations hypothesis cannot be rejected, and we demonstrate how the hypothesis is rejected when some of these procedures are not followed. We also report some findings that extend beyond the existing literature. We test the hypothesis that differences in individual forecasts are due solely to asymmetric information and cannot be explained by any publicly available information. To our knowledge, this strong implication of the rational expectations hypothesis has not been tested previously. We find that it cannot be rejected. Finally, we find that, because of lagged data availability, forecast errors are MA(1) despite the rationality of the forecasters. This error structure implies that the class of rational expectations models in which output varies only because of monetary surprises can produce persistent movement in output.

The outline of the paper is as follows. Section I addresses the argument, made by some economists, that the rational expecta-

[2]Note that some authors (for example, Cukierman, 1986) have called this inaccuracy "measurement error." But what these authors are discussing is different from the usual interpretations of measurement error. An example will make quite clear what they call "measurement error." Suppose someone calls Keane and asks for his forecast of the three-month T-bill rate for the next quarter. He is busy writing a paper for a conference—the activity for which he receives monetary reward—that is due in three days. Quickly, Keane tells the caller 8 percent. While reading the *Wall Street Journal* later in the day, Keane sees that the forward rate on three-month T-bills is 9 percent. He does not run out to buy bonds in the expectation that rates will fall to 8 percent because, when he thinks about it, 9 percent seems reasonable. Thus, 8 percent is an erroneous measure of his true expectation because he does not act in the market as if that were his expectation. But that is not measurement error as it is usually understood, so we prefer to call the problem "lack of an economic incentive to accurately state expectations."

tions hypothesis cannot be tested using survey data. Section II reviews the literature and describes the problems with previous research that have motivated our work. Section III discusses our econometric methods, including a new covariance matrix estimator for panel data that is consistent in the presence of aggregate shocks. Section IV describes the ASA-NBER survey data and discusses the complex data timing and revision issues. Section V presents our empirical results. Section VI concludes.

I. On the Use of Survey Data to Test Theories of Expectation Formation

Some economists have questioned whether it is valid to use survey data on agents' expectations to test among various mechanisms of expectation formation. For example, Edward Prescott (1977, p. 30) has argued that "surveys cannot be used to test the rational expectations hypothesis. One can only test if some [economic] theory, whether it incorporates rational expectations or, for ... [that] matter, irrational expectations, is or is not consistent with observations." Others, such as Zarnowitz (1984, p. 15), have argued that "it is not good 'positive economics' to dismiss it [evidence from survey data] on the ground that only theories, not their assumptions, can be tested."

Economists do agree that the ability of economic models to explain behavior depends on the assumed expectation-formation mechanism. For example, the intertemporal substitution model proposed by Robert Lucas and Leonard Rapping (1969) provides a far better explanation of employment fluctuations if expectations are "adaptive" rather than rational. Since any test of a model involves a joint test of the behavioral equations and the assumed expectation-formation mechanism, researchers face an identification problem. First, if the model is rejected, we do not know whether the behavioral equations or the expectations mechanism is being rejected. Second, if two different models (different in both the behavioral equations and expectation-formation mechanism) explain data equally well, we cannot identify the proper behavioral model without knowing the expectation-formation mechanism.[3]

Many economists have assumed the validity of the rational expectations hypothesis and regarded joint tests of behavioral equations and this particular expectation-formation mechanism as identified tests of behavioral equations. This willingness to assume the validity of Muth's rational expectations hypothesis perhaps results from a widespread view that forming expectations rationally is simply the logical consequence of optimizing behavior. Yet there are many reasons why the rational expectations hypothesis *does not* follow directly from the assumption of optimizing behavior. For example, agents' expectations in stochastic equilibrium may be rational in Muth's sense, while the expectations of Bayesian learners in a nonstationary environment are not (see John Caskey, 1985). Or, perhaps, information necessary to form expectations on the basis of the true economic model may be impossible or too costly to obtain (see Kenneth Arrow, 1978). In this paper we adopt Zarnowitz's view that the hypothesis of rational expectations should be tested and not simply assumed valid. For further discussion of the philosophical issues, we refer the reader to Michael Lovell (1986) or Zarnowitz (1984).

II. A Critique of the Literature

Given the large number of empirical tests of the rational expectations hypothesis in the literature, one might wonder whether yet another test is necessary. We contend that almost all existing tests are either incorrect or inadequate for four reasons. First, most tests use sample mean forecasts rather than individual forecasts. Second, respondents in most surveys have little incentive to make accurate forecasts. Third, most tests compare survey forecasts to revised rather than initial data. Fourth, many tests are based on incorrect assumptions about the

[3]This is the well-known problem of observational equivalence (see Sargent, 1976).

covariance structure of forecast errors. We discuss these problems with the existing literature in this section.

Many tests of the rational expectations hypothesis that use survey data examine the rationality of the sample mean or "consensus" forecast constructed from surveys of individual forecasters. These include most tests using the Livingston data,[4] as well as the work by Jonathan Leonard (1982) on the Endicott survey of employers' wage expectations, that by Frank de Leeuw and Michael McKelvey (1981) on the price expectations of business firms, that by Jeffrey Frankel and Kenneth Froot (1987) on various surveys of exchange rate expectations, and that by Benjamin Friedman (1980) on the Goldsmith-Nagen survey of interest rate expectations.

There are two problems with using consensus forecasts to test rationality. First, doing so causes serious specification bias. If forecasters are rational, their forecasts will differ only because of differences in their information sets.[5] The mean of many individual rational forecasts, each conditional on a private information set, is not itself a rational forecast conditional on any particular information set (see Stephen Figlewski and Paul Wachtel, 1983). This seemingly minor issue can produce severe bias. We discuss this problem further in Section III, Part B.

A second problem with using consensus forecasts is that this approach can mask individual deviations from rationality. Albert Hirsch and Michael Lovell (1969), looking at data from individual firms in the Commerce Department Manufacturers' Inventory and Sales Expectations survey, found (p. 71) that some firms are consistently optimistic about future sales while others are consistently pessimistic. Averaging expectations, however, can cancel these biases across firms so that industry mean expectations show no bias. Muth (1985) looked at anticipated production for individual Pittsburgh steel firms and found the same phenomenon.

For both of these reasons we argue that researchers must use *individual* data in order to test hypotheses about how people form expectations. These data can be used to test rationality either on an individual-by-individual basis or by running pooled time-series cross-section regressions. Both individual and pooled regressions are represented in the work of Hirsch and Lovell (1969), Muth (1985), Figlewski and Wachtel (1981), Thomas Urich and Paul Wachtel (1984), de Leeuw and McKelvey (1981), and Zarnowitz (1984, 1985).

A second, equally severe problem with most tests of forecast rationality is that survey data on expectations do not necessarily reflect the true expectations of the forecasters. But in order to test the rational expectations hypothesis, one needs data that can be reasonably assumed to reflect the forecasters' expectations. As de Leeuw and McKelvey (1984) found, this is certainly not the case with surveys, such as the BEA data on sales price expectations of individual firms, that ask individuals or firms to give "rough estimates" of expected future quantities. As mentioned earlier, Hirsch and Lovell (1969) and Muth (1985) found evidence against rationality on the firm level. However, the problem found by de Leeuw and McKelvey may well plague the Hirsch-Lovell and Muth studies as well. As we discussed in the introduction, it is reasonable to assume that this problem is limited when dealing with the forecasts of professional forecasters who receive a monetary reward for producing accurate forecasts and who report to the survey the same forecasts they sell on the market. This is not true of the Livingston price forecast data, because the economists polled by Livingston were not all professional forecasters. Hence we cannot be sure that the rejection of rationality found by Figlewski and Wachtel (1981), who ran a pooled regression using the

[4]See Brown and Maital (1981), Carlson (1977), Gramlich (1983), Mullineaux (1978), Pearce (1979), Pesando (1975), and Rich (1987).

[5]This is a very important point. If each forecaster had exactly the same information, all forecasts would have to be the same, if they were rational. Forecasters can make different rational forecasts if they each have some different private information. We discuss this issue more fully in Section III, Part C.

Livingston data, is not the result of improperly measured expectations.

Since 1968 Victor Zarnowitz has worked with the NBER and the American Statistical Association on the ASA-NBER Survey of Forecasts by Economic Statisticians.[6] This survey is well suited for testing because it is limited to professional producers of quarterly forecasts of GNP and its major components and other economic indicators for each of several quarters ahead. Since respondents report forecasts that they produce professionally, the problem of inaccurate reporting of expectations is probably small. Interestingly, the ASA-NBER also conducted a few annual surveys, in the early years of the period covered, of a much larger group of economists of whom three-quarters are "occasional" rather than "professional" forecasters. In this survey, with method similar to that of Livingston, Zarnowitz (1969) finds that "a number of the occasional forecasters submitted extreme and rather unreasonable predictions." This finding adds to our concern about inaccuracies due to lack of proper economic incentives in the Livingston data.

Using the ASA-NBER survey, Zarnowitz (1984, 1985) has performed tests of the rationality of the respondents' forecasts of inflation and other macroeconomic variables. Using both survey means and pooled data, he rejects the rational expectations hypothesis for inflation forecasts. We consider these results questionable, however, because Zarnowitz uses revised rather than initial price data in his tests. Using revised data does not necessarily invalidate tests of unbiasedness, but their use is suspect in this case because of the nature of the data revisions.[7] We show in Section V, Part B that these data revisions introduce a systematic bias that may invalidate Zarnowitz's tests of unbiasedness.

A final problem with much of the existing literature is incorrect assumptions about the covariance of forecast errors across forecasters. When they pool individual data, Figlewski and Wachtel (1981) assume that forecast errors are independent across forecasters.[8] Since aggregate shocks affect the price level, this is certainly not true. Falsely assuming independent errors creates a severe downward bias in estimated standard errors, tending to cause false rejection of the rational expectations hypothesis. In Section III, Part D we discuss how to account correctly for the effects of aggregate shocks on inference in tests of forecast rationality. In Section V, Part A we show the empirical importance of these effects.

III. Econometric Issues

A. *Definitions*

Expectations are rational in Muth's sense (Muth, 1961) if they are equal to mathematical expectations conditional on the set of all information relevant for forecasting. For an individual forecaster, we can express this relationship as

$$(1) \qquad {}_tP_{i,t+k} = E(P_{t+k} \mid I_{i,t}),$$

where P_{t+k} is the realized value of the time series P at time $t+k$, ${}_tP_{i,t+k}$ is a k-step-ahead prediction of P made at time t by forecaster i, $I_{i,t}$ is the information available at time t to forecaster i, and E is the mathematical expectation operator. This is equivalent to the statement: $E(\varepsilon^i_{t,k} \mid I_{i,t}) = 0$, where $\varepsilon^i_{t,k} = P_{t+k} - {}_tP_{i,t+k}$. This statement can be broken down into the separate hypotheses that forecasts are unbiased and efficient.

For an individual forecaster, a test of rationality can be performed by running the

[6]For a description of the survey, see Zarnowitz (1969, 1974).

[7]In Section IV we discuss the circumstances under which it is appropriate to use revised data to test for unbiasedness.

[8]Zarnowitz argues, correctly, that it is invalid to pool individual data while assuming independent forecast errors within each cross section if there are aggregate shocks.

regression

$$(2)\quad P_{t+k} = \alpha_0 + \alpha_1 {}_tP_{i,t+k} + \alpha_2 X_{i,t} + \varepsilon^i_{t,k},$$

where $X_{i,t}$ is any variable in forecaster i's information set at time t. Unbiasedness requires that, in a regression without $X_{i,t}$ variables, the coefficients in equation (2) may be restricted to $\alpha_0 = 0$ and $\alpha_1 = 1$. Efficiency requires that any variable known at time t or before be orthogonal to $\varepsilon^i_{t,k}$; that is, $\alpha_2 = 0$ for any $X_{i,t} \in I_{i,t}$.[9]

B. *The Aggregation Problem and the Advantages of Panel Data*

As we discussed in Section II, using survey means to test the rational expectations hypothesis leads to serious specification error. This bias can be illustrated by comparing the results of three different estimation methods. If we were to run three separate tests of unbiasedness using equation (2), first doing a separate regression for each individual and taking the mean of the estimated coefficients, second using the pooled data, and third using survey means, we could call the α_i estimates $\hat{\alpha}_{1i}$, $\hat{\alpha}_{1p}$, and $\hat{\alpha}_{1m}$, respectively, and we would have

$$(3)\quad \hat{\alpha}_{1i} = \frac{1}{N}\sum_{i=1}^{N} \frac{\sum_{t=1}^{T} P_{t+k}\, {}_tP_{i,t+k}}{\sum_{t=1}^{T} {}_tP^2_{i,t+k}}$$

$$(4)\quad \hat{\alpha}_{1p} = \frac{\sum_{t=1}^{T}\sum_{i=1}^{N} P_{t+k}\, {}_tP_{i,t+k}}{\sum_{t=1}^{T}\sum_{i=1}^{N} {}_tP^2_{i,t+k}}$$

$$(5)\quad \hat{\alpha}_{1m} = \frac{\sum_{t=1}^{T} P_{t+k}\left(\sum_{i=1}^{N} {}_tP_{i,t+k}\right)}{\sum_{t=1}^{T}\left(\sum_{i=1}^{N} {}_tP^2_{i,t+k}\right)},$$

where i indexes individuals from 1 to N. We see that $\text{plim}_T\, \hat{\alpha}_{1i} = \text{plim}_T\, \hat{\alpha}_{1p}$ but that

$$(6)\quad \frac{\hat{\alpha}_{1p}}{\hat{\alpha}_{1m}} = \frac{\text{Var}({}_t\bar{P}_{i,t+k})}{\text{Var}({}_t\bar{P}_{i,t+k}) + \text{Var}({}_tP_{i,t+k} - {}_t\bar{P}_{i,t+k})},$$

where ${}_t\bar{P}_{i,t+k} \equiv (1/N)\sum_{i=1}^{N} {}_tP_{i,t+k}$ and the grand mean of ${}_tP_{i,t+k}$ is assumed to be 0. Thus, as long as individual forecasts differ, $\hat{\alpha}_{1m}$ will be biased upward. The importance of this bias can be seen in the work of Urich and Wachtel (1984) who, using 20 individuals and 95 time periods to test the rationality of money supply forecasts, obtained the following estimates:

Estimator	α_0	α_1
Individual Means	−0.13	0.78
Pooled Data	−0.12	0.77
Sample Means	−0.29	1.06

The upward bias in the sample means estimator $\hat{\alpha}_{1m}$ is seen here to be quite severe (about 40 percent). We avoid this aggregation problem by using only individual data in a pooled time-series cross-section regression.

By using panel data to test the rational expectations hypothesis, we avoid the aggregation problem. We also achieve two other important advantages over time-series data. First, we can detect systematic bias of individual forecasters by testing for the presence of individual effects. Second, using panel data increases the degrees of freedom available to test hypotheses, making our tests of rationality more powerful.

It is not immediately clear that using panel data makes tests of rationality more powerful. Since the dependent variable is the same for all individuals, forecast errors will be highly correlated across individuals and the effective number of degrees of freedom will not equal the number of observations minus the number of parameters. OLS standard errors will therefore be severely biased. Other authors who have used panel data (for example, Figlewski and Wachtel, 1981; Urich and Wachtel, 1984; and de Leeuw and McKelvey, 1984) have not adjusted the OLS standard errors. We discuss our

[9]If forecasters have differential costs of over- and underprediction, it could be rational for them to produce biased forecasts. If we were to find that forecasts are biased, it could still be claimed that forecasters were rational if it could be shown that they had such differential costs.

method of consistently estimating the covariance matrix in Section III, Part D.

C. *The Forecasters' Information Sets*

We now address two issues concerning the covariance structure of the errors in (2). First, what is the information structure implied by (2)? Second, what are the effects of aggregate shocks on inference in (2)? Certainly any public information known at time t or before should be orthogonal to $\varepsilon^i_{t,k}$. But an additional restriction requires that any private information known by person i at time t or before also be orthogonal to $\varepsilon^i_{t,k}$. One's own prior forecasts and prior forecast errors are examples of such private information. Further, if forecasts are publicly announced, then other people's prior forecasts and prior forecast errors should be orthogonal to $\varepsilon^i_{t,k}$ as well. Finally, the rational expectations hypothesis implies that forecasts should only differ across individuals if individuals have different private information (i.e., if the conditioning set $I_{i,t}$ differs among forecasters). This implies that no readily available data should be able to explain the differences between individuals' forecasts. This proposition is implied by rational expectations but has not been tested previously.

Unfortunately, merely stating these constraints does not tell us what forecasters should know for a particular survey. Certainly, forecasters should know their own forecasts in this and previous periods, so ${}_tP_{i,t+k-1}$, and ${}_{t-1}P_{i,t+k-1}$ should both be orthogonal to $\varepsilon^i_{t,k}$. The most difficult informational issue is whether P_t itself is known when the forecast ${}_tP_{i,t+k}$ is made. In many surveys, P_t is not released until after forecasters have predicted ${}_tP_{i,t+k}$. In those surveys, neither P_t nor the lagged one-step-ahead forecast error $\varepsilon^i_{t-1,1} \equiv P_t - {}_{t-1}P_{i,t}$ should be orthogonal to $\varepsilon^i_{t,k}$. Therefore, the forecast errors will be MA(k), rather than MA($k-1$), as they would be if the forecasters knew P_t when they made their forecasts.[10] This serial correlation does not refute rationality. However, rationality cannot hold if higher-order serial correlation exists, because such serial correlation would imply that individual forecasters do not learn from their own past errors.

We also note that if we observe ${}_tP_{i,t}$, both it and forecasters' perceptions of their own lagged one-step-ahead forecast errors $({}_tP_{i,t} - {}_{t-1}P_{i,t})$ should be orthogonal to $\varepsilon^i_{t,k}$.[11] Of course, information that forecasters did not know when they predicted ${}_tP_{i,t+k}$ should not be orthogonal to $\varepsilon^i_{t,k}$. That would include, for example, such quantities as P_t, ${}_{t+1}P_{i,t+k}$, ${}_{t+1}P_{i,t+k+1}$, $P_t - {}_{t-1}P_{i,t}$, and $P_t - {}_tP_{i,t}$. Additionally, if the past forecast of another forecaster $({}_{t-1}P_{j,t})$ is publicly announced, then a forecaster's perception of the other forecaster's lagged error $({}_tP_{i,t} - {}_{t-1}P_{j,t})$ should not improve price forecasts.

D. *Aggregate Shocks and Tests of Rationality*

At first glance, (2) seems trivial to estimate. It would seem that we could use OLS and estimate a covariance matrix that is consistent in the presence of serial correlation. However, to use these estimators is to assume that forecast errors are uncorrelated across forecasters. But such correlation is likely because of aggregate shocks to the economy. If the average forecast error is not zero for each period, then OLS will yield inconsistent parameter estimates. Chamberlain (1984) first noted the potential effects of aggregate shocks on rational expectations models estimated with panel data. Chamberlain suggests that one result of aggregate shocks in a rational expectations model is that the sample version of the orthogonality condition $E(\varepsilon^i_{t,k} \mid I_{i,t})$ converges to zero as the number of time peri-

[10] In a different context, Watson (1983) made this observation. Note also that we cannot use a GLS transformation on (2) in this case because the regressors are not strictly exogenous.

[11] ${}_tP_{i,t}$ is the guess that person i made at the end of time t about the value of P_t. Since data on P_t may not be released until after that guess is made, rationality does not imply that ${}_tP_{i,t} = P_t$. Note also that data revisions made after the forecast are not in the forecaster's information set when he makes the prediction, and therefore functions of those revisions cannot be used as independent variables in efficiency tests.

ods increases, but not as the number of individuals increases, if the number of time periods is held fixed. He also notes that most panel data models rely on a large number of individuals to achieve consistency. Thus, in a panel with a large N and a small T, coefficients in rational expectations models may be inconsistent.[12] This problem can be addressed in two ways. One can use either a long panel so that aggregate shocks do not affect consistency or time dummies to eliminate the effects of aggregate shocks. In this study, we use a long panel to achieve consistency.[13]

OLS also yields inconsistent standard errors in the presence of aggregate shocks. In this paper, we present an important innovation: In order to achieve efficiency, we use a covariance matrix estimator that remains consistent in the presence of aggregate shocks.

We now describe the structure of the covariance matrix estimator. Recall that the GMM estimator in a linear model is

$$(7)\quad \hat{\beta}_{GMM} = \left(\frac{X'Z}{NT}\left(\frac{Z'\Omega Z}{NT}\right)^{-1}\frac{Z'X}{NT}\right)^{-1} \times \left(\frac{X'Z}{NT}\left(\frac{Z'\Omega Z}{NT}\right)^{-1}\frac{Z'Y}{NT}\right).$$

In this case, Y is P_{t+k}, X is a constant and ${}_tP_{i,t+k}$, and Z is $Z_{i,t}$. (The $Z_{i,t}$'s are instruments chosen from information available to forecaster i at time t.) Here the Ω matrix will have off-diagonal elements because of aggregate shocks and MA errors (for OLS, $\Omega = I$). To consistently estimate the covariance matrix we need to consistently estimate $(Z'\Omega Z/NT)$. If the errors in this model were i.n.i.d., then $(1/NT)\times \sum_{i=1}^{N}\sum_{t=1}^{T} Z_{i,t}'\hat{\varepsilon}_{t,k}^{i}\hat{\varepsilon}_{t,k}^{i\prime}Z_{i,t}$ would converge to a fixed matrix M, which is a consistent estimator of $(Z'\Omega Z/NT)$. If we merely had moving average errors, with no correlation of the errors across people, then with an MA(k) error, $(1/NT)\sum_{j=-k}^{k}\sum_{i=1}^{N}\sum_{t=1}^{T} Z_{i,t}'\hat{\varepsilon}_{t,k}^{i}\hat{\varepsilon}_{t+j,k}^{i\prime}Z_{i,t+j}$ would be a consistent estimator. Note that as we increase the order of the MA error, the number of terms that we compute increases very slowly, so computation of this matrix is feasible.[14]

However, if errors are correlated across people, $(2k+1)N^2$ terms must be estimated to compute a general covariance matrix based on the orthogonality conditions from (2). Unfortunately, this calculation would exhaust the number of degrees of freedom in the data. However, we can construct a covariance matrix estimator that is consistent even in the presence of aggregate shocks if we make the following assumptions:

$$(8)\quad E\left(\varepsilon_{t+l,k}^{i}\varepsilon_{t+m,k}^{i}\right) = \begin{cases}\sigma_{|l-m|}, & \forall\, i,t,l,m, \\ & \text{s.t. } |l-m|\le k; \\ 0, & \text{otherwise.}\end{cases}$$

$$(9)\quad E\left(\varepsilon_{t+l,k}^{i}\varepsilon_{t+m,k}^{j}\right) = \begin{cases}\delta_{|l-m|}, & \forall\, i,j,t,l,m, \\ & \text{s.t. } |l-m|\le k; \\ 0, & \text{otherwise.}\end{cases}$$

This amounts to assuming that the data are not conditionally heteroskedastic and that no forecaster is systematically better than any other (i.e., σ_i's are the same for each individual).[15]

[12] We ignore problems with unit roots in this paper. Results of West (1988) show that in time-series like the deflator, in which trend terms dominate, standard asymptotic results for GMM estimators are correct.

[13] Zarnowitz (1985), arguing that pooled time-series cross-section regressions are misspecified, has not recognized that such regressions are $\sqrt{T}$ consistent. He does, however, recognize that OLS standard errors are inconsistent for these models.

[14] Newey and West (1987) discuss the limits on the growth of the number of terms that can be estimated.

[15] These assumptions are somewhat restrictive, but they seem reasonable. Although conditional heteroskedasticity exists in inflation, it is unlikely to exist with price-level forecasts. The findings of McNees (1975) and Zarnowitz (1967) that the accuracy of forecasters does not differ systematically supports assumption (9). Neither assumption can be formally tested because of the combination of aggregate shocks and moving average errors.

Given (8) and (9), we need only estimate the $(k+1)$ σ's and $(k+1)$ δ's, a feasible operation. Under these assumptions, our estimate of Ω will have the form

$$(10) \qquad \Omega = \begin{pmatrix} Q & R & \dots & R \\ R & Q & \dots & R \\ \vdots & \vdots & \ddots & \vdots \\ R & R & \dots & Q \end{pmatrix},$$

where

$$(11) \quad Q = \begin{pmatrix} \sigma_0 & \sigma_1 & \dots & \sigma_k & 0 & 0 & \dots & 0 & 0 \\ \sigma_1 & \sigma_0 & \dots & \sigma_{k-1} & \sigma_k & 0 & \dots & 0 & 0 \\ \vdots & \vdots & \ddots & \vdots & \vdots & \vdots & \ddots & \vdots & \vdots \\ \sigma_k & \sigma_{k-1} & \dots & \sigma_0 & \sigma_1 & \sigma_2 & \dots & 0 & 0 \\ 0 & \sigma_k & \dots & \sigma_1 & \sigma_0 & \sigma_1 & \dots & 0 & 0 \\ 0 & 0 & \dots & \sigma_2 & \sigma_1 & \sigma_0 & \dots & 0 & 0 \\ \vdots & \vdots & \ddots & \vdots & \vdots & \vdots & \ddots & \vdots & \vdots \\ 0 & 0 & \dots & 0 & 0 & 0 & \dots & \sigma_0 & \sigma_1 \\ 0 & 0 & \dots & 0 & 0 & 0 & \dots & \sigma_1 & \sigma_0 \end{pmatrix}$$

and

$$(12) \quad R = \begin{pmatrix} \delta_0 & \delta_1 & \dots & \delta_k & 0 & 0 & \dots & 0 & 0 \\ \delta_1 & \delta_0 & \dots & \delta_{k-1} & \delta_k & 0 & \dots & 0 & 0 \\ \vdots & \vdots & \ddots & \vdots & \vdots & \vdots & \ddots & \vdots & \vdots \\ \delta_k & \delta_{k-1} & \dots & \delta_0 & \delta_1 & \delta_2 & \dots & 0 & 0 \\ 0 & \delta_k & \dots & \delta_1 & \delta_0 & \delta_1 & \dots & 0 & 0 \\ 0 & 0 & \dots & \delta_2 & \delta_1 & \delta_0 & \dots & 0 & 0 \\ \vdots & \vdots & \ddots & \vdots & \vdots & \vdots & \ddots & \vdots & \vdots \\ 0 & 0 & \dots & 0 & 0 & 0 & \dots & \delta_0 & \delta_1 \\ 0 & 0 & \dots & 0 & 0 & 0 & \dots & \delta_1 & \delta_0 \end{pmatrix}$$

Given this estimate of Ω, we can compute $\hat{\beta}_{GMM}$ and its covariance matrix and test the restrictions implied by rationality.[16] No previous study has noted this form for $\hat{\beta}_{GMM}$. In fact, the limited panel-data work testing the rationality of forecasts has completely ignored this point.[17]

IV. Data

This study uses both panel survey data and aggregate time-series data. The survey data come from the ASA-NBER survey of economic forecasters who are members of the ASA Business and Economics Section. The panel is comprised of professional forecasters rather than research economists whose livelihood does not depend upon the accuracy of their forecasts.[18] Since they report to the survey the same forecasts that they sell on the market, these respondents have an economic incentive to make accurate predictions and we can assume that their survey responses are reasonably accurate measures of their expectations. Forecasters receive their surveys near the end of the first month of each quarter. They report their perceptions of the previous quarter's GNP deflator and their predictions for the current quarter and the following four quarters.[19] They respond by the end of the second month of the quarter.[20]

To test rationality, we compare individual quarterly forecasts with the deflator announcements released 45 days after the end of a given quarter. The deflator data used in

[16]Note that we must assume that $\varepsilon^i_{t+l,k}$ and $\varepsilon^j_{t+m,k}$ come from the same distribution. Thus we have symmetry rather than skew-symmetry in the R matrix.

[17]Since the dependent variable is the same for each forecaster, we cannot estimate a fixed time-effects model in this case. However, if there were cross-sectional variation in the dependent variable (as is true in the panel-data consumption literature), a fixed time-effects model could be estimated using the i.n.i.d. covariance estimator. With such a short panel, this is probably the preferred approach. For further discussion of this issue, see Runkle (1989).

[18]In fact, fewer than 5 percent of the respondents to the December 1970 survey were academics (Zarnowitz, 1974).

[19]They also have to regularly forecast all the major components of GNP in order to be included in the survey.

[20]It is not meaningless to ask about the deflator from the previous quarter since the 45-day NIPA release is made *after* the survey is mailed out. In fact, many respondents have already completed their forms when those numbers are released; we assume that those data are not in the forecasters' information set.

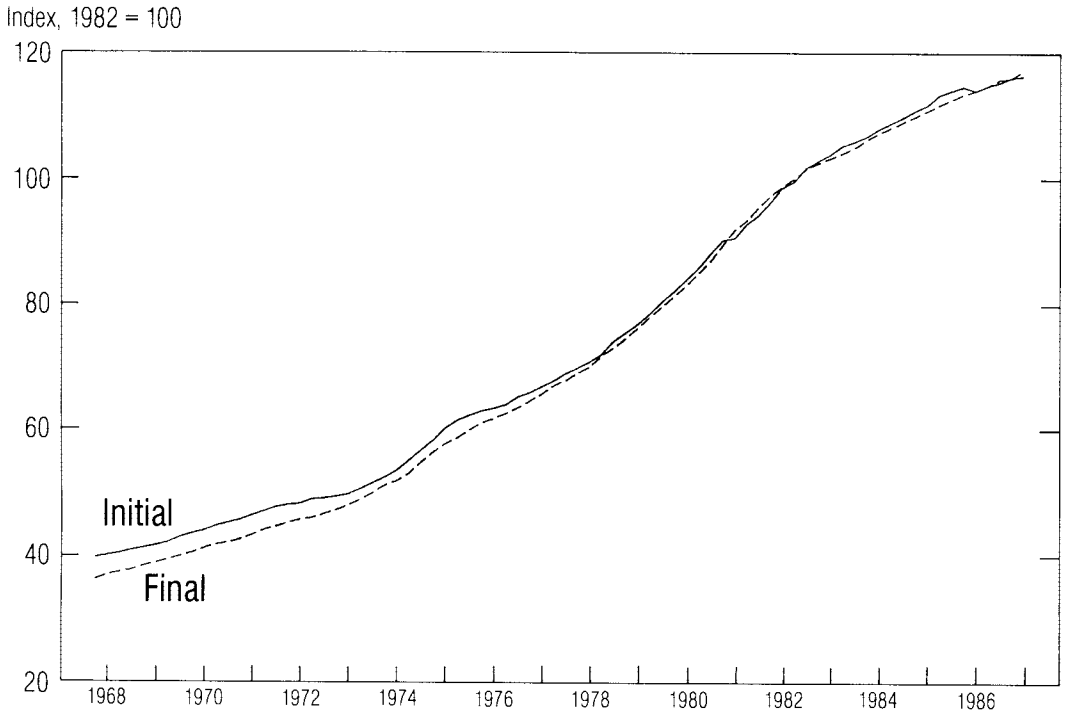

FIGURE 1. INITIAL AND FINAL GNP DEFLATOR DATA, 1968–1986

this study are often systematically revised. Forecasters generally do not know in advance (i.e., at time t) the nature of systematic data revisions occurring after the date on which they make their forecasts but before the date on which the NIPA announcements are made (i.e., time $t+1$). Often, the ASA-NBER survey asks forecasters for predictions that are further in the future than the next annual benchmark revision of the GNP deflator series. In these cases, forecasts differ systematically from announced values solely because of data revisions. We deal with this problem by excluding from the data all forecasts with horizons that extend beyond the date of systematic data revisions. We include only those observations for which there were no benchmark revisions from the time the survey was taken until the 45-day NIPA announcement was made. To do otherwise would severely bias our results against the hypothesis of rationality.

We assume that forecasters knew the GNP deflator announcements made in previous quarters. We allow for the fact that P_t, the current quarter's GNP deflator, is not necessarily known when ${}_tP_{t+1}$ is announced by assuming only that forecasters know their own perception of P_t, which is ${}_tP_t$. Note that the longer the forecast horizon, the more data we would lose because of our data screens. For this reason, we concentrate on one-step-ahead deflator forecasts and examine the rationality of price-level forecasts rather than inflation forecasts.

Many previous studies may be biased against the hypothesis of rationality because they assume that the final revisions of all variables contained in agents' information sets were known when forecasts were made. Figure 1 shows the effect of using revised rather than initial data. It shows the divergence between the 45-day announcement of the GNP deflator and the final data for the GNP deflator (as of September 1987), rescaled to reflect the benchmark revisions of 1976 and 1986. This figure shows that there are large systematic differences be-

tween these two price series.[21] Not only the level, but also the rate of growth of the series is affected.

Although we compare the forecasters' predictions to initial GNP deflator data in our study, it is important to note that using revised data in tests of unbiasedness is not necessarily inappropriate. Whether revised data should be used depends on two issues: First, do forecasters try to predict the initial or the revised data? Second, are there significant and predictable data revisions?

Obviously, if forecasters try to predict the revised data, those data should be the dependent variable in tests of unbiasedness. However, in the forecasts studied by Zarnowitz (1967) and McNees (1986), predictions were, on average, closer to the initial announcement than to the revision. This suggests that forecasters are, on average, trying to predict the initial announcement.

Regardless of which data the forecasters try to predict, data revisions should have little effect on tests of rationality unless they are significant and systematic. Since initial announcements of many variables, such as real GNP, are rational forecasts of the final data for those variables, the choice of dependent variable probably would have little effect on tests of the rationality of predictions of those variables.[22] But we show in Section V, Part C that the choice of data is very important in testing unbiasedness of forecasts of the GNP deflator, as one would expect from the evidence in Figure 1.

Although there are some circumstances in which it is permissible to use revised data for tests of unbiasedness, it is never permissible to use data unavailable to the forecasters as an independent variable in tests of efficiency. Thus, for example, if we wish to test whether forecasters properly adjust for their own past error $(P_{t-1} - {}_{t-2}P_{i,t-1})$, we should use the latest available revision of P_{t-1} before the forecaster made his prediction. We do this in our study. If we used final data for this test, the test would necessarily be incorrect because the final revision of P_{t-1} was not in the forecaster's information set when he made his prediction.

We use survey data from the fourth quarter of 1968 through the third quarter of 1986. We exclude periods containing the annual benchmark revisions, which usually occur in July, and forecasters who did not respond at least twenty times. Our sample contains 1613 observations. Some of our regressions use fewer observations because data are missing for other variables.

Like the survey data, our time-series data are reported as quarterly averages. The GNP deflator data come from the *Survey of Current Business*. Our M1 data come from the database of the Federal Reserve Board of Governors. Our nominal oil price statistics come from the Commerce Department's *Foreign Trade Statistics*.

V. Empirical Results

We address three questions in our empirical investigation. First, what is the covariance structure of the errors? Second, are the forecasters' predictions unbiased? Third, are the forecasters' predictions efficient?

All of the results presented are tests of rationality at the one-step-ahead horizon. Thus, our equation is

$$(13)\quad P_{t+1} = \alpha_0 + \alpha_{1t} P_{i,t+1} + \alpha_2 X_{i,t} + \varepsilon^i_{t,1}, \qquad E(\varepsilon^i_{t,1} \mid I_{i,t}) = 0.$$

To test for unbiasedness, we estimate regressions without $X_{i,t}$. Our null hypothesis in that case is that $\alpha_0 = 0$ and $\alpha_1 = 1$. To test for efficiency, we include some additional variable $X_{i,t}$ in the regressions. Our null hypothesis in that case is that $\alpha_0 = 0$, $\alpha_1 = 1$, $\alpha_2 = 0$.

A. *The Covariance Structure of Forecast Errors*

Two important questions arise about the covariance structure of the forecast errors.

[21] Note that Zarnowitz (1985) recognized that price-level data could not be compared across major benchmark revisions. No other author has recognized this. We discuss problems with Zarnowitz's data in Section V, Part B.

[22] See Mankiw and Shapiro (1986).

TABLE 1—TESTS FOR UNBIASEDNESS AND SERIAL INDEPENDENCE

Method	α_0	α_1	α_2	χ^2 for H_0	MA	Regressors	N
(1)	5.853	0.9967	–	21.66	0	$1, {}_tP_{i,t+1}$	1613
	(1.319)	(0.0008)	–	(0.00002)			
(2)	5.853	0.9967	–	1.226	0	$1, {}_tP_{i,t+1}$	1613
	(5.321)	(0.0031)	–	(0.5417)			
(3)	5.853	0.9967	–	0.900	1	$1, {}_tP_{i,t+1}$	1613
	(6.227)	(0.0036)	–	(0.6377)			
(4)	11.38	0.9943	0.3031	19.81	1	$1, {}_tP_{i,t+1}$	803
	(5.725)	(0.0032)	(0.0802)	(0.0002)		$P_t - {}_{t-1}P_{i,t}$	
(5)	7.078	0.9959	−0.0021	0.875	1	$1, {}_tP_{i,t+1}$	728
	(7.692)	(0.0044)	(0.1106)	(0.8315)		$P_{t-1} - {}_{t-2}P_{i,t-1}$	
(6)	7.756	0.9956	−0.0055	1.341	1	$1, {}_tP_{i,t+1}$	1111
	(6.720)	(0.0039)	(0.0695)	(0.7194)		${}_tP_{i,t} - {}_{t-1}P_{i,t}$	

Note: Standard errors are in parentheses under coefficients; significance levels are given under χ^2-statistics. H_0: ($\alpha_0 = 0$, $\alpha_1 = 1$, $\alpha_2 = 0$).

First, are aggregate shocks important? Second, are forecast errors serially correlated? We address both issues in Table 1. Row 1 of Table 1 reports the results of a simple OLS regression to test the rationality of one-step-ahead forecast errors.[23] The results indicate that the hypothesis of unbiasedness is strongly rejected. However, that rejection is misleading because of the magnitude of aggregate shocks. In this OLS regression, the average covariance of the forecast errors made in a given period by two different forecasters is 58 percent of the variance of the average forecast error. This large covariance implies that a large percentage of forecast error variance comes from aggregate shocks, and thus we cannot assume that these errors are independent across forecasters. Row 2 reveals the importance of aggregate shocks. If we correct the standard errors in the OLS regression to reflect the cross-forecaster error covariance, our test results change dramatically. We do not reject the hypothesis of unbiasedness using a GMM estimator with a covariance matrix that accounts for aggregate shocks. In fact, the estimated standard error for α_1 increases from 0.0008 to 0.0031 when we do so. (Note that $\hat{\beta}_{GMM} = \hat{\beta}_{OLS}$ in this case, because the model is exactly identified, and the instruments are also the regressors.) All subsequent results account for the cross-sectional correlation of forecast errors that is created by aggregate shocks.

For results presented in Rows 1 and 2, we assume that forecast errors are not serially correlated. If forecast errors are not serially correlated, then the regression coefficient on the lagged forecast errors should not be significant. In Row 4, we report the results of including the lagged error as a regressor. The χ^2-statistic for the null hypothesis ($\alpha_0 = 0$, $\alpha_1 = 1$, $\alpha_2 = 0$) clearly shows that that lagged error is correlated with the current forecast error. In fact, there is a very strong MA(1) correlation (the MA parameter is 0.3031).

Rejecting serial independence might cause us to question the rationality of the price-level forecasts.[24] However, we need to conduct additional tests to see whether this serial correlation implies that we could improve the forecasters' predictions using data they knew when they made their forecasts. We test whether two such pieces of data could improve our forecasts. First, as shown in Row 5, we test whether the second lagged error is significant in predicting future

[23]This test is similar to Figlewski and Wachtel's (1981).

[24]Zarnowitz (1985) draws this inference.

prices. The value for the χ^2-statistic is extremely low, indicating that this error has no power to predict future prices. This supports the hypothesis of rationality, since the second lagged errors were definitely known by forecasters when they made their predictions, ${}_tP_{i,t+1}$. Second, as reported in Row 6, we test whether the *perceived* lagged forecast error helps predict future prices. Since the survey elicits not only the predictions ${}_tP_{i,t+1}$ but also ${}_tP_{i,t}$ (the forecasters' perceptions of P_t), we can construct the forecasters' perceptions at time t of the one-step-ahead forecast errors they made from time $t-1$ to time t. Since these perceived errors are certainly in the forecasters' information sets, they should not help to predict future prices. The χ^2-statistic in Row 6 shows that the perceived forecast errors do not improve price forecasts. Thus, only the unperceived part of lagged forecast errors helps to predict future prices. (Remember that P_t is not known when the forecast ${}_tP_{i,t+1}$ is made.) The presence of MA(1) errors arising from unperceived lagged errors is compatible with the hypothesis of rationality. All subsequent regressions assume an MA(1) error structure.

B. *Unbiasedness Tests*

In Row 3, we present our OLS results assuming MA(1) errors and correcting standard errors for the effect of aggregate shocks. The estimate of α_0 is 5.853 (with a standard error of 6.227), and the estimate of α_1 is 0.9967 (with a standard error of 0.0036).[25] The value of the χ^2 test for unbiasedness is 0.900. Since this test statistic has a probability value of 0.6377, we cannot reject the null hypothesis of rationality.

Such strong support for rationality seems incompatible with Zarnowitz's (1985) rejection of unbiasedness using the same data. The only obvious differences between Zarnowitz's study and ours are that he looks at the unbiasedness of inflation forecasts and does not pool the data. But there is another difference that explains his rejection of unbiasedness: Zarnowitz uses revised data. He uses as his actual inflation series the final available data before the benchmark revisions of January 1976 and December 1980. We find that the annual July data revisions of the GNP deflator series by the Department of Commerce appear to have large systematic components. Therefore we use as *actual* data the final values available *before* each annual July revision.

Using revised data, Zarnowitz finds the striking result that, for one-quarter-ahead inflation forecasts, the hypothesis of rational expectations can be rejected for 27 percent of the forecasters at the 5 percent level and for 69 percent of the forecasters at the 10 percent level. Over the 1968 to 1979 period, he finds a mean error of -0.16 percent for one-quarter-ahead inflation forecasts. Unfortunately, these results arise from Zarnowitz's use of revised rather than initial data.[26]

We reconstructed the data that Zarnowitz used for P_{t+1} from different issues of *Business Conditions Digest* and adjusted the sample so that all our regressions would include the same observations. Using Zarnowitz's approach, we then estimated individual regressions for each forecaster in our sample using both Zarnowitz's data and our unrevised data. The difference in the results is striking.[27] The following table summarizes our findings.

Number (and Percentage) of Individual Forecasters for Whom Unbiasedness Is Rejected

Data Set	5 Percent Level	1 Percent Level
Zarnowitz	27 (45.0 percent)	13 (21.7 percent)
Initial Data	8 (13.3 percent)	1 (1.6 percent)

This table shows that using initial data as the dependent variable results in test statistics that are far more favorable to the hy-

[25]The survey data are reported in 1000's, which is why our estimated constant is so large.

[26]Zarnowitz recognized that the use of revised data might affect his inference, but he believed the effect would not be large.

[27]We present a complete set of these individual regressions in the Appendix.

pothesis of unbiasedness. We believe that these data are a more accurate reflection of what the forecasters were trying to predict, and of what their information sets contained, than are the revised data used by Zarnowitz.

Our use of levels instead of inflation does not account for the difference in the results because rejections at the 5 percent level increase from 27 percent to 45 percent of the sample when we use the price level rather than the inflation rate in our regressions. Clearly our use of initial data accounts for the difference of our results from Zarnowitz's and our failure to reject rationality.

Testing whether the initial announcements of the deflator are rational forecasts of the revised data that Zarnowitz used further indicates the difference between our price data and Zarnowitz's. If there were no systematic revisions in the data, the 45-day GNP deflator announcement should be a rational forecast of the revised data. In that case, if we regress Zarnowitz's data on a constant and the 45-day announcement, the coefficient on the constant should be zero and the coefficient on the announcement should be one.[28] But when we computed that regression, the test statistic for the hypothesis that those coefficients had their assumed values was 15.92. Since this test statistic should be distributed as a χ_2^2 random variable under the null hypothesis, we must reject the notion that the 45-day deflator announcements are rational forecasts of the data used by Zarnowitz.[29]

As we mentioned before, using revised data may not have much of an effect on tests of unbiasedness if the initial announcement was a rational forecast of the final data, as was probably true with Zarnowitz's other data series. But for the GNP deflator, the choice of dependent variable is important. We cannot determine a priori whether forecasters are trying to forecast the initial or the revised data, but the fact that their forecasts are unbiased predictors of the initial data and biased predictors of the revised data suggests that they are trying to predict the initial announcement. Thus, it seems more appropriate to use the 45-day announcement as the dependent variable.

To the best of our knowledge, all of the other authors in the inflation-forecast literature have used revised data as the independent variable for their tests and as a conditioning set in their tests of rationality.[30] Since the revised data are not in the forecasters' information sets when they make their forecasts, other researchers' tests of rationality are biased toward rejection.

C. *Efficiency Tests*

Since we cannot reject unbiasedness, we must test the further implication of rational expectations that forecasts be efficient, that is, that no readily available information could have improved forecast accuracy. This involves testing the hypothesis that $\alpha_0 = 0$, $\alpha_1 = 1$, and $\alpha_2 = 0$ in equation (13). Tables 2, 3, and 4 give the results of our efficiency tests.

First note that none of the regressions reported in Table 2 rejects the coefficient restrictions implied by efficiency (H_0). In Rows 1 through 5, respectively, the following variables are shown not to improve price forecasts: the forecaster's perception of the price level at the time of his forecast (${}_tP_{i,t}$), the lagged price level (P_{t-1}), the forecaster's previous one-step-ahead prediction (${}_{t-1}P_{i,t}$), the lagged value of the forecaster's error in perceiving the price level ($P_{t-1} - {}_{t-1}P_{i,t-1}$), and the forecaster's perception of another forecaster's lagged error (${}_tP_{i,t} - {}_{t-1}P_{j,t}$). Remarkably, none of these implications of rationality is rejected.

[28]Mankiw, Runkle, and Shapiro (1984) devised this test.

[29]We also adopted the suggestion of an anonymous referee to test whether this result holds true in each half of the data. It does. We reject the hypothesis that the 45-day announcement is a rational forecast of Zarnowitz's data at the 5 percent level in each half-sample.

[30]Zarnowitz (1967) compares the annual forecasts of GNP and several other variables to their initial announcements for the years 1953–1963. He recognizes the importance of studying price forecasts (p. 138), but does not do so in that study.

TABLE 2—TESTS OF RESTRICTIONS IMPLIED BY EFFICIENCY

Method	α_0	α_1	α_2	χ^2 for H_0	MA	Regressors	N
(1)	4.542	0.8097	0.1906	3.847	1	$1, {}_tP_{i,t+1}, {}_tP_{i,t}$	1599
	(6.929)	(0.1012)	(0.1024)	(0.2785)			
(2)	7.243	0.8797	0.1200	4.012	1	$1, {}_tP_{i,t+1}, P_{t-1}$	1079
	(7.646)	(0.0645)	(0.0659)	(0.2602)			
(3)	7.471	0.9112	0.0859	2.331	1	$1, {}_tP_{i,t+1}, {}_{t-1}P_{i,t}$	1119
	(7.132)	(0.0676)	(0.0678)	(0.5066)			
(4)	7.876	0.9956	0.0200	1.259	1	$1, {}_tP_{i,t+1}, P_{t-1} - {}_{t-1}P_{i,t-1}$	797
	(7.034)	(0.0041)	(0.2546)	(0.7390)			
(5)	8.343	0.9953	−0.0823	2.478	1	$1, {}_tP_{i,t+1}, {}_tP_{i,t} - {}_{t-1}P_{j,t}$	1457
	(6.985)	(0.0040)	(0.0701)	(0.479)			

Note: Standard errors are in parentheses under coefficients; significance levels are given under χ^2-statistics. H_0: ($\alpha_0 = 0$, $\alpha_1 = 1$, $\alpha_2 = 0$).

TABLE 3—FURTHER TESTS OF EFFICIENCY

Method	α_0	α_1	α_2	χ^2 for H_0	MA	Regressors	N
(1)	3.092	0.9992	−0.0009	1.0567	1	$1, {}_tP_{i,t+1}, P_{O,t-1}$	1613
	(7.549)	(0.0056)	(0.0017)	(0.7875)			
(2)	0.7887	1.0012	−0.0016	1.6601	1	$1, {}_tP_{i,t+1}, P_{O,t-2}$	1569
	(7.523)	(0.0055)	(0.0016)	(0.6458)			
(3)	4.481	1.0003	−0.0014	1.964	1	$1, {}_tP_{i,t+1}, M1_{t-1}$	1613
	(6.011)	(0.0045)	(0.0012)	(0.5799)			
(4)	4.704	1.0003	−0.0014	1.868	1	$1, {}_tP_{i,t+1}, M1_{t-2}$	1569
	(6.214)	(0.0047)	(0.0013)	(0.6002)			

Note: Standard errors are in parentheses under coefficients; significance levels are given under χ^2-statistics. H_0: ($\alpha_0 = 0$, $\alpha_1 = 1$, $\alpha_2 = 0$).

TABLE 4—JOINT TEST OF EFFICIENCY

$$P_{t+1} = \alpha_0 + \alpha_1 {}_tP_{i,t+1} + \alpha_2 P_{t-1} + \alpha_3(P_{t-1} - {}_{t-2}P_{i,t-1}) + \alpha_4({}_tP_{i,t} - {}_{t-1}P_{i,t}) + \alpha_5 P_{O,t-1} + \alpha_6 M1_{t-1} + \varepsilon_{i,t+1}$$

α_0	21.882
	(11.451)
α_1	0.806
	(0.073)
α_2	0.191
	(0.073)
α_3	0.022
	(0.011)
α_4	0.005
	(0.082)
α_5	0.006
	(0.003)
α_6	−0.005
	(0.004)
No. of MA Terms	1
χ^2-Statistic	8.288
Significance Level	0.308
N	576

Note: Standard errors are in parentheses.

Two variables that varied greatly, and presumably had a large effect on the price level during the sample period, are the money supply and the price of oil. Economic forecasters were accused of systematically underestimating the effects of these variables on price behavior during the 1970s and 1980s. Table 3 examines whether forecasters fully adjusted their predictions to changes in the money supply and oil prices. This table presents regressions that include the previous two lagged values of nominal crude oil prices ($P_{O,t-1}, P_{O,t-2}$) and M1 ($M1_{t-1}, M1_{t-2}$) as dependent variables. The coefficient restrictions implied by rationality are not rejected in any of these regressions.

Although the results in Tables 1, 2, and 3 show strong support for forecast rationality, a joint test of forecast rationality using the most important variables from those tables

TABLE 5—TESTS OF FALSE EFFICIENCY RESTRICTIONS

Method	α_0	α_1	α_2	χ^2 for H_0	MA	Regressors	N
(1)	8.602	0.7093	0.2908	10.76	1	$1, {}_tP_{i,t+1}, P_t$	1129
	(7.272)	(0.0970)	(0.0978)	(0.0131)			
(2)	0.6950	0.0567	0.9431	2353.7	1	$1, {}_tP_{i,t+1}, {}_{t+1}P_{i,t+1}$	1172
	(1.380)	(0.0195)	(0.0196)	(0.0000)			
(3)	4.268	0.5457	0.4457	110.2	1	$1, {}_tP_{i,t+1}, {}_{t+1}P_{i,t+2}$	1178
	(4.349)	(0.0441)	(0.0438)	(0.0000)			
(4)	10.12	0.9949	0.6891	19.64	1	$1, {}_tP_{i,t+1}, P_t - {}_tP_{i,t}$	1120
	(5.760)	(0.0033)	(0.1719)	(0.0002)			

Note: Standard errors are in parentheses under coefficients; significance levels are given under χ^2-statistics. H_0: ($\alpha_0 = 0$, $\alpha_1 = 1$, $\alpha_2 = 0$).

would provide even more convincing evidence that these forecasts are rational. Table 4 gives the results of a test of rationality in which the P_{t+1} was regressed on a constant, the one-step-ahead forecast ${}_tP_{i,t+1}$, the lagged price level P_{t-1}, the lagged perceived forecast error ${}_tP_{i,t} - {}_{t-1}P_{i,t}$, the second lagged forecast error $P_{t-1} - {}_{t-2}P_{i,t-1}$, the lagged nominal crude oil price $P_{O,t-1}$, and the lagged money supply $M1_{t-1}$. The χ^2 test statistic for this regression shows that the hypothesis of efficiency cannot be rejected.[31]

Given the strong support that the results in Tables 1–4 have provided for the hypothesis of forecast rationality, we might wonder whether these tests have any power. Table 5 addresses this issue. It displays the results of regressions that include variables that were not in a forecaster's information set when he made his price forecasts. In fact, the χ^2-statistics show that we can reject the test for forecast rationality for each of these variables. Row 1 of Table 5 shows regression results including the contemporaneous (but unknown) price level ($P_{i,t}$) as a regressor. Row 2 shows the results of including the forecaster's perception of the price level at the end of the period (${}_{t+1}P_{t+1}$). Row 3 results include the forecaster's next one-step-ahead prediction (${}_{t+1}P_{i,t+2}$). Results in Row 4 include the forecaster's contemporaneous error in perceiving the price level ($P_t - {}_tP_{i,t}$). The test of efficiency is rejected in every case. Thus, these tests have sufficient power to reject invalid restrictions.

The results in Row 4 are particularly instructive. The forecaster's perceived forecast error from the previous period (${}_tP_{i,t} - {}_{t-1}P_{i,t}$) is orthogonal to the current forecast error (see Table 1, Row 6. However, since his unperceived forecast error ($P_t - {}_tP_{i,t}$) is not orthogonal to his forecast error (see Table 5, Row 4), neither is the actual lagged forecast error, because it is the sum of the perceived and unperceived components (see Table 1, Row 4).[32]

The results in Tables 1–5 strongly confirm the hypothesis that survey forecasts are rational. The variables expected to improve price forecasts do so, and the variables not expected to improve price forecasts do not. The coefficient restrictions rationality implies cannot be rejected when the standard errors are correctly computed.

The rational expectations hypothesis implies that only differential private informa-

[31]Although the money supply and oil prices certainly do not exhaust the list of information available to the forecasters, the fact that forecasters seem to have efficiently used this information suggests that the forecasts are rational. Mullineaux (1980) found similar results in testing whether the mean Livingston forecaster efficiently incorporated data about money growth.

[32]Boschen and Grossman (1982) first made this distinction between perceived and unperceived forecast errors.

tion should explain differences among individual forecasts. Our final set of tests checks whether differences among the forecasts of different forecasters can be explained by publicly available information. We tested whether the lagged growth rate of M1 or the lagged growth rate of oil prices could explain the difference between any given forecaster's prediction error and the mean prediction error for that period. If either of those variables is correlated with idiosyncratic error components for some forecasters, then those forecasters did not fully account for that publicly available information in making their forecasts.[33] In only one of the 60 individual regressions is the rate of money growth significant at the 5 percent level in explaining the forecaster's idiosyncratic errors. In only 4 of the 60 individual regressions is the rate of oil price growth significant at the 5 percent level in explaining those errors. This suggests that heterogeneity in individual forecasts is not due to differential accuracy among forecasters in predicting the effects of money growth or oil price growth on future prices.

VI. Conclusion

Our results indicate that survey respondents' forecasts of the GNP deflator are both unbiased and efficient—and therefore rational. We demonstrate the importance of accounting for aggregate shocks in order to conduct correct statistical inference in panel data models. We find that failure to account for such shocks when estimating the covariance matrix for the regression estimates leads to false rejection of the hypothesis of rationality. We develop a covariance matrix consistent in the presence of aggregate shocks that leads to strong affirmation of rationality. We also demonstrate that although a strong moving average component is present in agents' one-step-ahead forecast error, it does not imply irrationality, because it arises from imperfect information regarding the current price level. Interestingly, this implies that the class of rational expectations models in which only unanticipated inflation can cause output to deviate from trend can explain persistence in output. Also, we show that using revised rather than initial price data to test the rationality of forecasts can greatly affect the results of those tests. In fact, Zarnowitz, who uses the same forecast data we do, rejects rationality for inflation forecasts solely because he uses revised data. Finally, we find that we cannot reject the strong implication of rational expectations that forecast differences should only result from asymmetric information. Readily available data do not explain the differences among individual forecasts.

Yet our results are not conclusive evidence for rational expectations. We find that expert forecasters are rational, but other people may not be. Still, our results and methodological discussion cast grave doubts upon the assumptions, methods, and conclusions in the existing literature. And, although the support we provide for the rational expectations hypothesis is limited, it takes on added importance when viewed from the perspective that almost the entire existing literature has rejected the rationality of price forecasts, even from professional forecasters. Hence, our results can be viewed as salvaging the possibility that the rational expectations hypothesis is empirically valid, and reopening the debate on this subject.

Finally, our finding of the importance of aggregate shocks should prompt other economists who explore macroeconomic questions with panel data to reexamine their statistical assumptions. It seems unlikely that the residuals in micro consumption and labor studies are independent within each cross section. Failure to adjust for this dependence could lead to incorrect statistical inference.[34]

[33]The idiosyncratic component is $\varepsilon^{i}_{t,1} - \bar{\varepsilon}_{t,1}$.

[34]Altug and Miller (1990) find aggregate shocks very important in their study of consumption and labor supply.

APPENDIX

Individual Test Results Using Zarnowitz's Data

NBER I.D.	α_0	α_1	H_0	NBER I.D.	α_0	α_1	H_0
3	2.4898	1.0011	2.8829	7	−8.2951	1.0087	11.229
	(14.961)	(0.00974)	(0.23658)		(12.343)	(0.00796)	(0.00364)
8	4.4746	0.99956	1.8702	14	8.3869	0.99883	19.312
	(22.851)	(0.01478)	(0.39255)		(10.713)	(0.00709)	(0.00006)
15	11.962	0.99445	5.4328	21	−1.1597	1.0056	4.0226
	(7.9520)	(0.00496)	(0.06611)		(21.361)	(0.01251)	(0.01338)
22	−3.9781	1.0052	3.6053	23	20.768	0.99066	3.0006
	(18.836)	(0.01255)	(0.16486)		(14.039)	(0.00757)	(0.22307)
27	24.443	0.98641	5.6445	30	19.143	0.99200	1.3584
	(10.298)	(0.00590)	(0.05947)		(20.582)	(0.01011)	(0.50703)
31	20.897	0.99162	12.272	32	9.2309	0.9970	9.4499
	(11.312)	(0.00670)	(0.00216)		(13.606)	(0.00890)	(0.00887)
34	−0.72462	1.0037	7.5493	38	15.030	0.99281	7.2617
	(14.304)	(0.00909)	(0.02295)		(13.080)	(0.00853)	(0.02649)
39	14.890	0.99332	4.9309	40	18.354	0.99225	4.1992
	(11.189)	(0.00688)	(0.08497)		(13.503)	(0.00745)	(0.12251)
42	1.7853	1.0026	9.9783	43	30.891	0.98747	20.689
	(11.301)	(0.00716)	(0.00681)		(11.097)	(0.00605)	(0.00003)
44	−6.3401	1.0059	2.0942	46	8.4090	0.99806	5.4088
	(16.299)	(0.00979)	(0.35096)		(16.835)	(0.01120)	(0.06691)
47	2.5519	0.99761	0.23846	48	10.198	0.99800	12.409
	(20.115)	(0.01140)	(0.88760)		(16.415)	(0.01102)	(0.00202)
49	−6.8452	1.0079	28.547	51	26.053	0.98454	11.952
	(8.0293)	(0.00522)	(0.0000006)		(7.5482)	(0.00452)	(0.00254)
54	−13.488	1.0125	18.141	57	10.081	0.99654	5.8440
	(11.293)	(0.00725)	(0.00011)		(15.853)	(0.01026)	(0.05383)
59	9.6887	0.99712	6.6950	60	17.905	0.98964	4.9027
	(13.193)	(0.00866)	(0.03517)		(8.8023)	(0.00477)	(0.08618)
61	15.960	0.99276	4.8359	62	9.8688	0.99707	2.4453
	(9.3530)	(0.00537)	(0.08911)		(14.246)	(0.00728)	(0.29445)
64	4.8450	0.99910	6.8349	65	11.691	0.99563	6.5628
	(10.514)	(0.00692)	(0.03280)		(9.0874)	(0.00518)	(0.03758)
66	13.732	0.99389	2.6122	67	22.335	0.98815	6.3599
	(10.254)	(0.00553)	(0.27087)		(8.8707)	(0.00490)	(0.04159)
68	29.936	0.98663	6.8446	69	14.398	0.99324	2.5578
	(13.878)	(0.00732)	(0.03264)		(11.620)	(0.00678)	(0.27834)
70	8.5655	0.99709	7.1223	72	9.0130	0.99754	4.4448
	(7.0671)	(0.00398)	(0.02841)		(11.390)	(0.00663)	(0.10835)
73	20.168	0.99007	13.373	75	23.365	0.98999	7.3882
	(12.249)	(0.00801)	(0.00125)		(12.945)	(0.00751)	(0.02487)
77	−15.027	1.0112	1.5137	78	2.2050	1.0015	6.4330
	(18.470)	(0.01204)	(0.46913)		(11.446)	(0.00724)	(0.04010)
82	18.430	0.99119	6.5981	84	11.607	0.99486	1.8743
	(8.9620)	(0.00517)	(0.03692)		(11.200)	(0.00598)	(0.39174)
86	7.0478	0.99869	7.0460	87	8.8439	0.99406	1.7829
	(9.9970)	(0.00598)	(0.02951)		(10.752)	(0.00598)	(0.41007)
89	15.341	0.99269	4.2050	93	27.050	0.98559	3.7746
	(11.190)	(0.00666)	(0.12215)		(15.571)	(0.00925)	(0.15148)
96	2.4145	0.99318	0.28821	98	11.779	0.99478	8.6409
	(15.160)	(0.00915)	(0.86580)		(7.5815)	(0.00461)	(0.01329)
102	14.569	0.99180	0.77244	108	−31.562	1.0250	12.634
	(22.167)	(0.01409)	(0.67962)		(22.838)	(0.01432)	(0.00181)
109	−21.115	1.0176	11.217	112	−24.921	1.0160	2.0212
	(20.057)	(0.01248)	(0.00367)		(19.960)	(0.01215)	(0.36401)
125	−5.9854	1.0002	3.3509	138	−17.728	1.0132	5.8484
	(26.976)	(0.01684)	(0.18722)		(13.384)	(0.00845)	(0.05371)
144	−16.815	1.0114	2.1684	145	−6.3399	1.0055	1.5882
	(14.334)	(0.00898)	(0.33816)		(22.023)	(0.01328)	(0.45199)

NBER I.D.	α_0	α_1	H_0	NBER I.D.	α_0	α_1	H_0
148	−35.524	1.0289	6.4511	158	−3.1309	1.0053	5.9090
	(41.462)	(0.02586)	(0.03973)		(20.486)	(0.01267)	(0.05210)

Note: Standard errors are in parentheses under coefficients; significance levels are given under χ^2-statistics for H_0.

Individual Test Results Using Initially Released Data

NBER I.D.	α_0	α_1	H_0	NBER I.D.	α_0	α_1	H_0
3	9.1382	0.99450	0.47508	7	0.48552	1.0005	0.91773
	(14.126)	(0.00920)	(0.78857)		(10.056)	(0.00648)	(0.63200)
8	6.7419	0.99550	0.12371	14	11.003	0.99423	5.8201
	(21.129)	(0.01302)	(0.94002)		(7.8468)	(0.00519)	(0.05447)
15	3.3783	0.99820	0.28922	21	−0.38588	1.0013	0.37485
	(7.4004)	(0.00462)	(0.86536)		(15.913)	(0.00932)	(0.82909)
22	−13.857	1.0102	1.4730	23	12.695	0.99288	2.0525
	(16.682)	(0.01111)	(0.4788)		(9.2274)	(0.00497)	(0.35835)
27	17.579	0.98852	8.4831	30	1.5236	0.99892	0.08735
	(7.7145)	(0.00442)	(0.01438)		(14.447)	(0.00710)	(0.95726)
31	19.276	0.99011	6.4269	32	−3.4191	1.0037	2.4887
	(8.9184)	(0.00528)	(0.04022)		(12.352)	(0.00808)	(0.28812)
34	4.6144	0.99801	1.1179	38	3.3336	0.99895	1.5414
	(11.326)	(0.00720)	(0.57182)		(11.519)	(0.00751)	(0.46268)
39	8.80857	0.99580	1.3134	40	12.607	0.99394	2.0482
	(8.7244)	(0.00536)	(0.51857)		(10.164)	(0.00561)	(0.35912)
42	−5.4555	1.0048	2.2748	43	19.672	0.99235	11.113
	(9.8857)	(0.00626)	(0.32066)		(10.432)	(0.00569)	(0.00386)
44	−11.053	1.0070	0.70276	46	−4.6930	1.0045	1.1088
	(14.223)	(0.00854)	(0.70371)		(14.882)	(0.00990)	(0.57441)
47	−3.2375	0.99910	2.0598	48	−0.20145	1.0031	5.7632
	(18.572)	(0.01052)	(0.35705)		(14.478)	(0.00972)	(0.05604)
49	−5.6613	1.0048	6.0616	51	14.644	0.99033	7.3626
	(6.0361)	(0.00392)	(0.04828)		(6.2969)	(0.00377)	(0.02519)
54	−1.1542	1.0018	2.2354	57	−5.4562	1.0048	1.5203
	(8.6148)	(0.00553)	(0.32704)		(13.974)	(0.00904)	(0.46760)
59	−1.8680	1.0027	1.7603	60	9.2780	0.99340	6.7461
	(11.181)	(0.00734)	(0.41471)		(6.7636)	(0.00367)	(0.03428)
61	7.5849	0.99623	0.96154	62	−3.8786	1.0023	0.28313
	(8.5865)	(0.00493)	(0.61831)		(9.3386)	(0.00477)	(0.86801)
64	2.2385	0.99869	0.08927	65	4.3618	0.99774	0.47456
	(9.5383)	(0.00628)	(0.95635)		(6.7932)	(0.00387)	(0.78877)
66	5.1299	0.99726	0.55304	67	11.565	0.99224	4.8101
	(6.9177)	(0.00373)	(0.75842)		(7.6704)	(0.00424)	(0.09026)
68	19.760	0.98953	3.9391	69	12.249	0.99186	3.3008
	(10.024)	(0.00529)	(0.13952)		(8.4623)	(0.00494)	(0.19200)
70	3.4932	0.99804	0.41833	72	5.3485	0.99709	0.43405
	(5.4228)	(0.00305)	(0.81126)		(8.3018)	(0.00483)	(0.80491)
73	7.1518	0.99709	5.6098	75	23.011	0.98691	7.2293
	(9.7200)	(0.00636)	(0.06051)		(8.5596)	(0.00496)	(0.02693)
77	−4.7178	1.0019	0.94257	78	2.0963	0.99917	0.26855
	(14.526)	(0.00947)	(0.62420)		(10.034)	(0.00635)	(0.87435)
82	11.719	0.99338	3.2729	84	2.6116	0.99839	0.18663
	(6.4776)	(0.00374)	(0.19467)		(8.3672)	(0.00447)	(0.91091)
86	10.445	0.99364	1.9437	87	1.8489	0.99634	5.6542
	(7.6236)	(0.00456)	(0.37837)		(11.063)	(0.00611)	(0.05918)
89	7.2372	0.99551	0.73126	93	17.439	0.99021	1.2036
	(8.9314)	(0.00532)	(0.69376)		(16.295)	(0.00968)	(0.54782)
96	6.0837	0.99435	5.5642	98	6.4943	0.99638	1.2734
	(9.1806)	(0.00554)	(0.06191)		(6.2992)	(0.00383)	(0.52903)

NBER I.D.	α_0	α_1	H_0	NBER I.D.	α_0	α_1	H_0
102	15.938	0.98915	0.79484	108	−20.179	1.0159	5.3174
	(21.261)	(0.01352)	(0.67205)		(21.714)	(0.01362)	(0.07004)
109	−15.376	1.0120	4.8020	112	−9.5870	1.0046	1.3252
	(17.518)	(0.01090)	(0.09063)		(17.037)	(0.01037)	(0.51552)
125	0.11363	0.99449	9.0306	138	−9.1007	1.0054	0.80104
	(25.318)	(0.01580)	(0.01094)		(11.147)	(0.00704)	(0.66997)
144	−8.9352	1.0045	2.0037	145	−2.8352	1.0020	0.05842
	(11.625)	(0.00728)	(0.36719)		(20.514)	(0.01237)	(0.97121)
148	−34.150	1.0261	3.9021	158	2.4112	0.99915	0.31752
	(39.627)	(0.02471)	(0.14212)		(17.144)	(0.01060)	(0.85320)

Note: Standard errors are in parentheses under coefficients; significance levels are given under χ^2-statistics for H_0.

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