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The Reliability of Inflation Forecasts Based on Output Gap Estimates in Real Time*

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Résumé / *Abstract*

Dans ce papier, on jauge l'utilité de plusieurs estimations (univariées autant que multivariées) de l'écart de production pour prévoir le taux d'inflation. Une analyse *ex post* suggère que plusieurs de ces estimations aident à prédire l'inflation. Néanmoins, les erreurs de prédictions hors de l'enchantillon qui se sont construites avec les écarts de production estimés en temps réel indiquent que cette amélioration de prédiction est illusoire. On trouve que l'utilité des écarts de production pour prédire l'inflation a été exagérée et que les prédictions faites avec l'écart de production sont souvent moins précises que celles qui ignorent le concept d'un écart de production.

A stable predictive relationship between inflation and the output gap, often referred to as a Phillips curve, provides the basis for empirical formulations of countercyclical monetary policy in many models. In this paper, we provide an empirical evaluation of the usefulness of alternative univariate and multivariate estimates of the output gap for predicting inflation. In-sample analysis based on ex post output gap measures suggests that many of the alternative estimates we examine appear to be quite useful for predicting inflation. However, examination of out-of-sample forecasts using real-time estimates of the same measures suggests that this predictive ability is mostly illusory. We find that the usefulness of output gaps as predictors of inflation has been severely overstated and that real-time forecasts using the output gap are often less accurate than forecasts that abstract from the output gap concept altogether.

Mots clés : la courbe de Phillips, l'écart de production, des prévisions d'inflation, des données en temps réel.

Keywords: Phillips curve, output gap, inflation forecasts, real-time data.

Codes JEL : E37, C53

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1 Introduction

A stable predictive relationship between inflation and a measure of deviations of aggregate demand from the economy's potential supply—the "output gap"—provides the basis for many formulations of activist countercyclical stabilization policy. Such a relationship, referred to as a Phillips curve, is often seen as a helpful guide for policymakers aiming to maintain low inflation and stable economic growth.[1] According to this paradigm, when aggregate demand exceeds potential output, the economy is subject to inflationary pressures and inflation should be expected to rise. Under these circumstances, policymakers might wish to adopt policies restricting aggregate demand aiming to contain the acceleration in prices. Similarly, when aggregate demand falls short of potential supply, inflation should be expected to fall, prompting policymakers to consider adoption of expansionary policies to restore stability.

Regardless of the analytical usefulness or the theoretical validity of a presumed predictive relationship between a concept of the output gap and inflation, however, the *practical* usefulness of such a relationship is largely an empirical matter. Even under the presumption that a stable predictive relationship is present in the data, a number of issues may complicate its use in practice. The appropriate empirical definition of "potential output"—and the accompanying "output gap"—that might be useful in practice is far from clear. For any given empirical definition of the gap, the exact form of its empirical relationship with inflation cannot be known a priori and would need to be determined from the data. Further, even if we were to assume that the proper concept and empirical relationship are identified, the operational usefulness of this predictive relationship would be subject to the availability of reliable estimates of the relevant gap concept in real time, when the desired inflation forecasts are required. But as is well known, real-time estimates of the output gap are generally subject to significant revisions. The subsequent evolution of the economy provides

[1] The appeal of this paradigm is evidenced by the fact that many estimated models employed for monetary policy analysis, including at numerous central banks, feature estimated "Phillips curves"of various forms. See Bryant, Hooper and Mann (1993) and Taylor (1999) for collections of monetary policy evaluation studies that feature such estimated models.

useful information for determining which part of the business cycle the economy was in at a particular point in time—information which leads to improved estimates of the gap. As a result, considerable uncertainty regarding the value of the gap remains even long after it would be needed for forecasting inflation, which renders real-time estimates unreliable.[2] This, in turn, raises questions regarding the empirical usefulness of the output gap for forecasting inflation in real time.

In this paper we assess the usefulness of alternative univariate and multivariate methods for estimation of the output gap for predicting inflation, paying particular attention to the distinction between *suggested usefulness*—based on in-sample historical analysis— and *operational usefulness*—based on simulated real-time out-of-sample analysis. First, using in-sample analysis based on ex post estimates of the output gap, we confirm that some appear to be useful for predicting inflation. This is as would be expected since the implicit Phillips curve relationships recovered in this manner are similar to the relationships commonly found in empirical macroeconometric models. However, the ability to explain inflation ex post does not imply an operational ability to forecast inflation. To assess the latter, we generate out-of-sample forecasts based on *real-time* output gap measures; those constructed using only data (and parameter estimates) available at the time forecasts are generated. For this exercise, we rely on the real-time dataset for macroeconomists which was created and is maintained by the Federal Reserve Bank of Philadelphia.[3]

Our findings based on this real-time analysis suggest that the predictive ability of output gap measures is mostly illusory. Ex post estimates of the relationship between inflation and the output gap severely overstate the gap's usefulness for predicting inflation. Further, real-time forecasts using the output gap are often less accurate than forecasts that abstract from the output gap concept altogether. These pessimistic findings mirror earlier investigations regarding the predictive power for forecasting inflation of "unemployment gaps," that is the difference between the rate of unemployment and estimates of the NAIRU. As demonstrated

[2] Orphanides and van Norden (2002) document the extent of this unreliability.

[3] See Croushore and Stark (2001) for background information regarding this database.

by Staiger, Stock and Watson (1997a,b) and Stock and Watson (1999), estimates of the NAIRU are inherently unreliable, and using information about unemployment does not lead to a robust improvement in inflation forecasts. There are at least two possible explanations for these results. One is simply that the existing underlying theoretical framework of the "Phillips curve" relationships that motivate the use of output and unemployment gaps for forecasting inflation may be seriously incomplete. Another, suggested by the findings of pervasive instability in macroeconomic relationships documented by Stock and Watson (1996), is that such relationships are simply not sufficiently stable over time to be useful in practice. Regardless, of the explanation, the questionable practical usefulness of output gaps for forecasting inflation brings into question their reliability for real-time policy analysis.

2 Trends and Cycles Ex Post and in Real Time

One way to define the output gap is as the difference between actual output and an underlying unobserved trend towards which output would tend to revert in the absence of business cycle fluctuations. Let q_t denote the (natural logarithm of) actual output during quarter t, and μ_t its trend. Then, the output gap, y_t can be defined as the cycle component resulting from the decomposition of output into a trend and cycle component:

$$q_t = \mu_t + y_t$$

Since the underlying trend is unobserved, its measurement, and the resulting measurement of the output gap, very much depends on the choice of estimation method, underlying assumptions and available data that are brought to bear on the measurement problem. For any given method, simple changes in historical data and the availability of additional data can change, sometimes drastically, the resulting estimates of the cycle for a given quarter. As a result, examination and interpretation of statistical relationships between the "output gap" and other variables, such as inflation, requires additional specificity regarding the temporal perspective from which the relationship is examined.

To illustrate this issue figures 1 and 2 provide some comparisons of output gap measures obtained using the Hodrick-Prescott (HP) filter using alternative information sets.[4] The solid line in the top panel of figure 1 denotes the output gap obtained with our “final” dataset with data ending in 1999Q4 as published in 2000Q1. The dotted line, instead, shows real-time estimates of the gap, as could be estimated with the historical data available at the time data first became available for that quarter. Thus, the real-time estimate for 1969Q1 was obtained by applying the Hodrick-Prescott filter to the data available in 1969Q2, when output figures for 1969Q1 were first released. Similarly, the real-time estimate for 1995Q4 was obtained by applying the Hodrick-Prescott filter to the data available in 1996Q1. The bottom panel provides a similar comparison of the four-quarter moving average of the output gap, as estimated over history and in real-time. Comparison of the series in either panel indicates that the resulting real-time and final series for the output gap exhibit significant differences. The series roughly agree on the timing of periods when output was significantly above or below its trend—as defined by the filter. But, as is also apparent from the figure, the real-time and final series frequently do not even agree on whether the output gap is positive or negative.

Figure 2 illustrates this difficulty in greater detail for two specific episodes. The top panel compares the historical estimates of the output gap as could be constructed in 1969Q1 with the final estimates. As can be seen, the real-time estimates as could have been constructed at the beginning of 1969 based on this method would have suggested that the economy was operating below its trend for the previous two years. But based on the ex post estimates, the output gap during the previous year was positive. The implications of this difference for a forecasting exercise are quite clear. Presuming the presence of a positive predictive relationship between the output gap and inflation, the ex post estimates would have suggested inflationary pressures. But the real-time estimates would have suggested the opposite, instead. The bottom panel provides a similar comparison where the oppo-

[4] We selected the HP filter (Hodrick and Prescott, 1997) for this illustration because of its popularity and simplicity which have made it a focus of extensive analysis and a benchmark for comparisons with alternative detrending techniques, both univariate and multivariate. See, for example, Harvey and Jaeger (1993), King and Rebelo (1993), Cogley and Nason (1995), Kozicki (1999), and Christiano and Fitzgerald (1999).

site conflict is apparent. Historical estimates of the output gap as could be constructed in 1996Q1 would have indicated an overheated economy during the previous year, whereas the final estimates suggest output was below its trend, instead. Although these examples are merely illustrative, the suggested errors and their likely influence on the policy process are not merely hypothetical. Indeed, the historical evidence presented in Orphanides (2003a,b) suggests that incorrect estimates of the output gap and associated incorrect inflation forecasts, were important factors in the monetary policy mistakes that led to the Great Inflation in the United States.

Further evidence of the difference between historical and real-time estimates of output gaps has been presented by Orphanides and van Norden (1999, 2002). In Table 1, we present some of the summary reliability indicators they examine for twelve alternative measures of the output gap, which we employ in our analysis. (These are described in greater detail below.) These results show that revisions in real-time estimates are often of the same magnitude as the historical estimates themselves and confirm that historical and real-time estimates frequently have opposite signs for many of the alternative methods.

As these examples illustrate, the presence of a predictive relationship between the output gap and inflation based on ex post estimated output gap measures might not be sufficient to assess whether the output gap could provide useful information for forecasting inflation in real time. Importantly, this is a difficulty that would apply even if such a predictive relationship were precisely estimated and known to be quite useful in-sample. Of course, if this relationship were not known exactly, its estimation—which would also need to be performed in real time—would present additional some difficulties. Econometric estimates would obviously also change with the evolving renditions of historical output gaps, even for a relationship estimated over a fixed sample.

3 A Forecasting Experiment

The results above suggest that ex post estimates of output gaps at a point in time may differ substantially from estimates which could be made without the benefit of hindsight.

This in turn could affect their ability to forecast inflation. The remainder of this section discusses the methodology used to investigate this question. We begin by describing the data sources used, and we then discuss the measurement of the output gap in more detail. Thereafter, we detail how the forecasting power of these output gap estimates is gauged.

3.1 Data Sources and Vintages

We use the term *vintage* to describe the values for data series as published at a particular point in time. Most of our data is taken from the real-time data set compiled by Croushore and Stark (2001); we use the quarterly vintages from 1965Q1 to 1999Q4 for real output. Construction of the output series and its revision over time is further described in Orphanides and van Norden (1999, 2002). We use 2000Q1 data as "final data" recognizing, of course, that "final" is very much an ephemeral concept in the measurement of output.

To measure inflation, we use the quarterly rate of inflation in the consumer price index (CPI). We use this both for our forecasting experiments and also to estimate measures of the output gap based on multivariate models that include inflation. For all of our analysis, we rely on the consumer price index (CPI) as available in 2000Q1. CPI data do not generally undergo a similar revision process as the output data. The major source of revision is changes in seasonal factors most noticeable at a monthly frequency. We therefore use the 2000Q1 vintage of CPI data for all the analysis which allows us to focus our attention on the effects of revisions in the output data and the estimation of the output gap in our analysis. One of our models (Structural VAR) also uses data on interest rates which are never revised.

3.2 Measuring Output Gaps

We construct ensembles of output gaps estimates of varying vintages. Each output gap vintage uses precisely one vintage of the output data. An estimated output gap is called a final estimate if it uses the final data vintage.

These ensembles of varying vintages of output gap estimates were constructed for each of a number of different output gap estimation techniques The alternative methods are detailed in the Appendix. Some, such as the linear or the quadratic trend, are based on purely deterministic detrending methods. Some, such as the Hodrick-Prescott filter, do not directly rely on statistical model-fitting. Five are estimated unobserved components, of which three (Watson, Harvey-Clark and Harvey-Jaeger) are univariate models and two (Kuttner and Gerlach-Smets) are bivariate models, using data on both output and prices. The remaining models are all univariate with the exception of the Structural VAR method, which uses a trivariate VAR with long-run restrictions as proposed by Blanchard and Quah (1989).

Note that all the output gap estimation techniques (aside from the Hodrick-Prescott filter) require that one or more parameters be estimated to fit the data. Such estimation was repeated for every combination of technique and vintage. This means, for example, that in constructing output gap vintages from an unobserved components model spanning the thirty year period 1969Q1-1998Q4 (120 quarters), we reestimate the model's parameters 120 times, and then store 120 series of filtered estimates.

3.3 Forecasting Specification

We restrict attention to linear specifications. Let $\pi_t^h = (400/h)(log(P_t) - log(P_{t-h}))$ denote inflation over h quarters ending in quarter t, at an annual rate. (The quarterly rate of inflation is simply $\pi_t \equiv \pi_t^1 = (400)(log(P_t) - log(P_{t-1}))$.) We examine forecasts of inflation at various horizons but are mainly interested in forecasts over a one year horizon and use that horizon as our baseline. Thus, given data for quarter t and earlier periods, our objective is to forecast $\pi_{t+4}^4 = (\pi_{t+4} + \pi_{t+3} + \pi_{t+2} + \pi_{t+1})/4$. We note that because of reporting lags, information for quarter t is not available before quarter $t+1$. Thus, a four-quarter ahead forecast is a forecast five quarters ahead of the last quarter for which actual data are available. The forecasting relationship we examine is thus:

$$\pi_{t+4}^4 = \alpha + \sum_{i=0}^{n} \beta_i \pi_{t-i} + \sum_{i=0}^{m} \gamma_i y_{t-i} + e_{t+4} \tag{1}$$

where n and m denote the number of lags of inflation and the output gap in the equation.

Given a concept of the output gap, two issues complicate the interpretation of how we could obtain inflation forecasts using equation (1). First, since the most suitable number of lags of inflation and the output gap n and m, and the parameters of the equation are not known a priori, these need to be estimated with available data. As our sample increases and additional data become available we would expect, of course, that these estimates would change.[5] Second, the estimates of the historical output gap available up to some specific period are revised and also change over time. This in turn, has two possible effects. First, this alone can influence the determination of the most suitable number of lags and the estimated parameters of equation (1)—for any fixed estimation sample. Second, given some fixed values of the parameters of the equation, the implied forecasts corresponding to the revised estimates of the output gap would be different as well.

In examining the usefulness of the output gap for predicting inflation using equation (1), we thus perform two different experiments for every output gap estimation technique we examine. First, we examine the in-sample fit of the the data, using final estimates of the output gap to both estimate (1) and compute its fitted values. Second, we simulate a real-time out-of-sample forecasting exercise. In this case, in each quarter, t we use the tth vintage of the output gap series to estimate (1) over the full sample (which includes determining its lag lengths m and n) and to generate its implied forecast.

To provide a benchmark for comparison, we estimate a univariate forecasting model of inflation based on equation (1) but omitting the output gaps. Again, we do this twice, first in-sample and second in simulated real-time, re-estimating the model with each additional observation. Figure 3 shows the resulting in-sample and out-of-sample forecasts, and corresponding forecast errors generated from our univariate and, for comparison, Figure 4 shows the corresponding forecasts using output gaps generated with the Hodrick-Prescott filter.

This experiment is designed to mimic in a simple way the problem facing a policymaker who wishes to forecast inflation in real time. Of course, the forecasting problem faced

[5] The baseline results we report in subsequent sections use the Ng-Perron approach of determining lag length within a general-to-specific testing framework and using t-ratio tests to determine the last significant lag. We also report results using the AIC and BIC model selection criteria.

by policymakers in practice is more complex than the one we consider. One obvious and important difference is that the information set available to policymakers is much richer. It is therefore possible that output gaps might improve on simple univariate forecasts of inflation but not on forecasts using a broader range of inputs. For this reason, we feel that the experiment we perform may actually overstate the utility of empirical output gap models.

In addition, we examine two sets of forecasts obtained using equation (1) but replacing the output gaps with either real or nominal output growth not subjected to any prior filtering and/or smoothing. As van Norden (1995) explains, using output growth in this way can be interpreted as implicitly defining an estimated output gap as a one-sided filter of output growth with weights based on the estimated coefficients of equation (1).[6] On the other hand, this approach does not rely nor require prior estimation of an output gap measure per se and is therefore simpler. The resulting forecasts should give the best linear unbiased predictors of future inflation. Since our output gap measures use the same information set (past prices and the current vintage of output) as these unrestricted forecasting equations, comparing their forecast performance aids in isolating the usefulness of the economic structure (or other restrictions) embedded in our output gap measures.

4 Baseline Results

4.1 In sample

To examine the in-sample performance of alternative measures of the gap, we estimated equation (1) using observations from 1955:1 to 1998:3. To allow for direct comparison with the simulated real-time forecasts, we examine its fit only over the period starting in 1969:1—the first quarter for which we calculate a simulated real-time forecast. Table 2 presents the root-mean-square errors (RMSE) of the resulting in-sample forecasts (i.e. the fitted values) of inflation. The first 12 rows in the table reflect alternative detrending methods, and the last three show statistics for the autoregressive forecast benchmark and

[6] van Norden refers to these estimates as TOFU (Trivial Optimal Filter - Unrestricted).

the forecasts based on real or nominal income growth instead of a pre-defined output gap measure. For each method, the first column shows the resulting forecast RMSE for the whole evaluation period, 1969-1998 and the remaining two columns show the same statistic for two subsamples, 1969-1983 and 1984-1998. The break follows the one examined by Stock and Watson (1999) in their study of inflation forecasts and splits the evaluation sample in two parts with roughly equal observations. The two subsamples also correspond, respectively, to a period of relatively high and relatively low variability in inflation. As can be seen, the autoregressive forecast (AR) has a RMSE of about 1.9 percent for the whole period but much higher (2.3 percent) during the first part of the sample and much lower (1.3 percent) during the second half. A quick comparison of the RMSE of alternative methods with the AR in column 1 indicates that (of course) all 12 of the alternative detrending techniques have lower RMS errors for the complete sample and 10 of the 12 also suggest improvements for both subsamples. Use of real or nominal growth also appears to improve the inflation forecasts relative to the autoregression in both subsamples.

To assess whether these improvements are statistically significant, we computed a modified Diebold-Mariano statistic for the alternative forecasts. Table 3 shows the ratio of the RMSEs shown in Table 2 to the RMSE of the AR forecasts, and notes when the modified Diebold-Mariano statistic rejected the null hypothesis of no improvement in RMSE at the 5% percent level. As can be seen, over the whoel sample only the Harvey-Jaeger technique failed to reject the null. An interesting aspect of the evaluation for the two sub-samples, however, indicates that most of the improvement appears to be concentrated in the first half of the sample, when inflation was more volatile. As judged by the modified Diebold-Mariano statistic, the improvement in forecasts is statistically significant at the 5% level in both subsamples for only three of the twelve methods.

4.2 Real Time

Next, we ran the simulated out-of-sample forecasting experiment. In each quarter t starting with 1969:1, we re-estimated equation (1) with data vintage t starting from 1955:1.[7] We then used equation (1) to obtain the inflation forecast corresponding to that method for that quarter. We repeated the procedure for each quarter up to 1998:3 and for each method.

The results are presented in Tables 4 and 5. These are directly comparable to tables 2 and 3, respectively. Comparison of the entries in Table 4 with respective entries in Table 2 indicates that the forecast performance of all methods appears markedly worse in real-time than in-sample. For the AR forecast benchmark, for example, the RMSE in real-time for the 1969-1998 evaluation sample is 2.3 percent, compared to the in-sample value of 1.9 percent. The forecast deterioration when we compare in-sample and real-time results, however, appears more severe for output gap based forecasts. Looking at the ratios of the RMSE relative to the AR forecast shown in Table 5 for the 1969-1998 period, we note that six of the twelve output gap methods indicate that the output-gap-based forecasts are worse, on average, than the autoregressive forecasts. Of the six that indicate some improvement, none indicates that this improvement is statistically significant at the 5% level, based on the modified Diebold-Marianno statistic. Interestingly, forecasts based on real and nominal output growth appear to deteriorate somewhat less than those based on output gaps and only forecasts based on nominal output growth appear to significantly improve on the autoregressive forecasts for the full sample. However, examining subsamples, we note that this improvement is only evident in the first half of the sample and is not apparent in the second.

The results in Table 5 give grounds for doubting the usefulness of output gaps, at the margin, for improving real-time forecasts of inflation. None are consistently significantly useful. None appear to improve the forecasts very much. This despite the fact that the benchmark (AR) forecast is trivially simple and uses unrealistically little information.

It is also interesting to compare the forecasting performance of the output gaps to that

[7] Below, we consider sensitivity to the choice of starting date.

of real or nominal output growth rates, as shown in Table 6. The first column is replicated from the first column of table 5, using the AR forecast as a benchmark. The second and third column use, instead, the real and nominal growth based forecasts as benchmarks. As can be seen, using the real or nominal growth-based forecasts as benchmarks suggests even more disappointing results regarding the usefulness of output gaps for predicting inflation. Only one of the twelve output gap measures suggests any overall improvement in the forecasts over those based on real growth and none suggests any improvement over forecasts based on nominal growth.

5 Sensitivity to Forecasting Experimental Design

In designing our forecasting experiment, we have tried to mimic the way in which professional forecasters may use the output gap to forecast inflation. However, several features of the experimental design require judgement; the horizon over which forecasts are evaluated, the period over which the forecasting equation is estimated, the specification of the forecasting equation, etc. To determine the robustness of our baseline results, in this section we examine the sensitivity of these results to several variations in the precise specification of the forecasting experiment and as well as the techniques used for forecast evaluation. We begin by considering whether the results differ if we are interested in inflation forecasts over different horizons, if we vary our estimation sample, or if we estimate and employ restricted versions of our forecasting equation for generating forecasts.

5.1 Forecasting Horizon

Our base case results (Table 5) use an effective forecast horizon of 5 quarters (i.e. forecasting 4 periods into the future plus a 1 quarter reporting lag.) Table 7 shows how these results vary as we vary the forcast horizon from 3 to 5, 7, and 9 quarters. On the one hand, we might expect the relationship between inflation and the output gap to become clearer as we increase the forecast horizon and thereby reduce the effects of other transitory shocks on inflation. On the other hand, we would expect the effects of monetary policy to become more apparent and weaken the empirical relationship as the horizon increases. In the extreme case, where the central bank strictly and successfully targets inflation at an intermediate horizon, the effect of policy should be to eliminate any role for output gaps in forecasting inflation at that horizon (Goodhart's Law). Because the Federal Reserve has been more concerned with inflation control over the medium rather than the short term, we would expect this effect to become more pronounced as we increase the forecast horizon.

Table 7 shows that there is a tendency for output gaps to become more informative in forecasting inflation as we increase the forecast horizon. For example, over the full sample the number of models which improve forecast accuracy relative to the AR rises from 9 of 14 at the 3 quarter horizon to 12 of 14 at the 9 quarter horizon. Similar improvements may be found in the high and low inflation subsamples. However, on the whole these improvements remain insignificant. We see from the table that evidence of a significant improvement in forecast performance is never stronger than at the 5 quarter horizon, regardless of the sample we examine. The only model to improve inflation forecasts significantly over the full sample at any horizon is the nominal income growth model. In addition, none of the models show a significant improvement in forecast performance at any horizon in the most recent subsample. This confirms that our baseline results, presented in Table 5, are robust to reasonable variations in the choice of forecast horizon.

5.2 Estimation Period

Another feature of our forecasting experiment which might reasonably be varied is the sample period over which the forecasting equation is estimated. Since the end of period is determined by data availability, this is question of when to start the estimation. The results in Table 5 start the estimation in 1955Q1. Table 8 shows how the results change as we consider three other possible starting dates; 1947Q1, 1960Q1 and 1965Q1.[8]

As can be seen, the 1955Q1 start date used in Table 5 provides the largest number of

[8] Although the real-time output series start in 1947Q1, some of the estimated output gaps (such as the structural VAR) require several lags of output and so their estimated gap series start somewhat later. When using the 1947Q1 start date, we simply used the first available estimate of the output gap in such cases.

cases in which the output gap significantly improves inflation forecasts. No other start date produces a significant improvement over the full sample. In only one other case, the Gerlach-Smets model over the high-inflation subsample with the 1947 start, does there appear to be a significant improvement over the univariate model. Improvements never appear to be significant for the 1960 or 1965 start dates. With the 1965 date, forecasting in our first subsample is difficult, of course, as models are estimated with few observations. But evaluation over the second subsample, where models are estimated with a much larger number of observations, does not indicate that starting the estimation later results in significantly improved forecasts.

We also note that the Beveridge-Nelson, Gerlach-Smets and Kuttner models give quite a bit (but not significantly) better RMSEs with the 1947 start; this is apparently due to their improved performance in the high inflation subsample; the earlier start makes all three do quite a bit worse than than before in the post-1983 period.

We conclude that our baseline results appear to be robust to a substantial range of starting dates.

5.3 Restricting the Forecasting Model

In our forcasting equation, the level of inflation is regressed on its own lags as well as those of the output gap. An important special case is one in which the output gap affects the change rather than the level of inflation, that is the specification that imposes the so called "accelerationist restriction" on the data. Our general specification nests this special case as one in which the sum of the coefficients on lagged inflation sum to one. However, if the special restricted case reflects the true relationship between inflation and the output gap more accurately, we may be able to improve the accuracy of our forecasts by imposing this restriction.

Table 9 compares the base case results from Table 5 to those produced by imposing the accelerationist restriction (Δpi). Imposing the accelerationist restriction (Δpi columns in the table) does not appear to change the baseline results; the number of models which forecast significantly better than the AR falls by one in the full sample, rises to two in the second subsample and is unchanged in the first subsample. No single model improves significantly on the AR model over any two of these three samples. However, both the Watson and the Kuttner models give significant improvements in the most recent period and improve the RMSE by 10 to 15% in all three periods.

Table 9 also considers whether using lags of *changes* in the gap (Δy) rather than its level produces better forecasts of inflation. Again, while this possibility is nested within the specification we consider as our Base Case, imposing a valid restriction should improve the accuracy of our forecasts. Table 9 shows that this restriction produces mixed results. Overall forecast performance is worse, with none of the models now forecasting significantly better than the AR model over the full sample, and every model forecasting worse than the AR in the most recent subsample. However, performance over the first subsample improves noticeably, with 13 of 14 models resulting in lower RMSEs and six of those forecasting significantly better than the AR.

5.4 Final versus Real-Time Gaps

It is of interest to explore whether the weak forecasting performance we find for real-time gaps is primarily due to the subsequent revision of these gaps (documented in Orphanides and van Norden, 2002) or the inherent difficulty in forecasting US inflation over this period (documented in Stock and Watson, 1999), To isolate these effects, we compared the performance of the real-time and final output gaps in explaining inflation, both in-sample and in simulated out-of-sample experiments.

First we compare the performance of the real-time and final output gaps in explaining inflation in-sample in a manner analogous to that of Table 2. The results are shown in Table 10. We see that over the full sample, use of real-time output gaps provide a better fit for inflation (in terms of lower RMSE in-sample) than do final gaps in the majority of cases (although the improvement over the AR model is more frequently significant for the final gaps). In the pre-1984 sample, the real-time gaps fit better than the corresponding final gap estimates in all but two cases and are always nominally significant, while in the post-1983 sample real-time gaps provide a worse fit in all but one case and neither ever appears to offer a significant improvement over the AR model. These results provide no evidence to suggest that the relative inaccurracy of the real-time gaps is responsible for their poor forecasting performance. Instead, the marked deterioration of the in-sample fit in the latter portion of the sample period suggests quantitatively important instability in the relationship between output gaps (however measured) and inflation.

In Table 11, we present a similar comparison of real-time and final output gaps, but now for out-of-sample forecast performance. Over the full sample, real-time gap estimates provide lower RMSEs slightly more often than final estimates, and provide the only instance of a significant forecast improvement. During the high-inflation sub-sample, real-time estimates again provide the only nominally significant evidence of forecast improvement (in two of the fourteen models tested); during the low-inflation subperiod no models forecast significantly better than the AR model. In both subsamples, real-time output gap estimates forecast better than the final estimates for 8 of the 14 models examined. This reinforces the results from the comparison of in-sample fits; it appears that instability in the inflation-output gap relationship, rather than the relative imprecision of real-time estimates is the primary cause of the poor forecasting performance of the real-time gaps.

6 Sensitivity to the Econometric Methodology

The design of our forecast experiment has also required some choices regarding econometric techniques. For example, we must select the lag structure used in the forecasting equation, choose a way in which to evaluate the significance of a particular change in RMSE, etc. As check of robustness against alternative choice, in this section we consider the impact of several of these choices on the interpretation of our baseline results.

6.1 Lag Selection

To determine the number of lags used in our forecasting equation, we used a simple general-to-specific testing procedure. We began with large number of lags (12) of both output and inflation, then discarded the highest lag if its t-statistic indicated its coefficient was insignficantly different from zero.[9] Of course, if this method does a poor job of identifying the correct lag structure, then we may expect our forecasting models to perform poorly (although this by itself should not cause our bivariate models to perform poorly *relative* to our univariate model.) Therefore, to check the robustness of our results, we repeated the forecasting experiment using an information criterion (AIC or BIC) to determine the number of lags of each variable used in the forecasting equation. Results are shown in Table 12.

Using BIC seems to produce some modest improvements in relative forecast accuracy overall while using AIC gives results roughly comparable to those in Table 5. In all three cases we find exactly one model which improves significantly on the AR model in the full sample; the biggest change perhaps being the improvement of the forecast accuracy of the Band-Pass model with BIC-determined lags, coming up to roughly the level of the real and nominal output growth models. We continue to find that none of the models forecasts significantly better than the AR model in the post-1983 period. However, the use of BIC improves relative forecast performance in the high-inflation period, with 3 of 12 models now forecasting significantly better than the AR model. However, two of these (Hodrick-Prescott and Harvey-Jaeger) become the worst-performing models in the post-1983 period. We conclude that apparent reliablity of the forecasts documented in Table 5 is not very sensitive to the choice of lag selection technique.

6.2 Alternative use of Data Vintages

In a recent paper examining the problem of forecasting with data subject to revision, Koenig et al. (2000) argue that we may be able to improve the accuracy of our forecasts by

[9] We experimented briefly with the use of heteroscedasticity and autocorrelation consistent standard errors in this test but found they made little or no difference.

combining different vintages of data in our estimated forecasting equation. Specifically, they begin by considering relationships of the form

$$Y(t) = X(t)_t \cdot a + w(t) \tag{2}$$

where $Y(t)$ and $X(t)$ are the "true" values of Y and X in period t. They consider the use of alternative vintages for estimation when the official estimates of $Y(t)$ and $X(t)$ available in period s, $Y(t)_s$ and $X(t)_s$, may differ from the "true" values. When lags of $X(t)$ enter the above relationship, forecasters generally estimate:

$$Y(t)_T = \sum_{j=0}^{p} X(t-j)_T \cdot a_j + w(t) \quad \text{for } t = \{1, ..., T\} \tag{3}$$

Koenig et al. point out that since this uses extensively revised estimates of X (i.e. $t - j << T$) for most of the sample, whereas forecasts are generated using the last vintage available, $X(T)_T$, and neighbouring observations which are little revised, better forecast performance may be produced by estimating:

$$Y(t)_T = \sum_{j=0}^{p} X(t-j)_t \cdot a_j + w(t) \quad \text{for } t = \{1, ..., T\} \tag{4}$$

Here, X and its lags are no longer taken from the same vintage of data for each observation; rather, $X(t)_T$ is replaced by unrevised estimates $X(t)_t$, the first lag $X(t-1)_T$ is replaced by the first revision $X(t-1)_t$ and so on. (Again, it is not clear that this by itself should cause our bivariate models to perform poorly *relative* to our univariate model.)

On the other hand, van Norden (2002b) argues that Koenig et al.'s procedure does not always result in improved forecasts. In particular, he suggests that when Y is not subject to revisions, so $Y(t)_t = Y(t)$, and the underlying relationship is of the form

$$Y(t) = X(t) \cdot a + w(t) \tag{5}$$

rather than (2), then (3) should forecast better than (4). However, since neither (2) nor (5) can be directly observed, only a direct comparison of forecasts from (3) and (4) can discern the best method.

To investigate the possibility that respecification of the use of vintages in our our forecasting equation might result in better forecasts, we performed a comparison using the combination of different data vintages suggested by Koenig et al. (2000). Because the first data vintage in our sample was published in 1965Q2, the period over which we could estimate our forecasting models is considerably restricted. We therefore considered only the forecast performance in the 1984-98 period using forecasting models estimated starting in 1965Q1 for this comparison. For computational reasons we also restricted attention to two of our three lag-selection criteria, the AIC and BIC and did not implement the Ng-Perron selection criterion in this case.

Table 13 presents the results results using the conventional data structure (3), with those using the Koenig et al. structure (4), (labelled KDP in the Table.) The table does not suggest that the alternative use of revised data changes the conclusions. The new lag structure improves the relative RMSE in only 7 of the 28 cases examined (3 using AIC, 4 using BIC). Furthermore, although the improvements are often small, the changed lag structue also frequently causes sharp deteriorations in forecast performance. In no case did any of these models appear to improve significantly on those of the simple AR model, and all but 8 of the 56 appeared to have larger RMSEs.

6.3 Bootstrap Inference and Test Power

As a last check on our results, we examine the reliability of our use of RMSE as a measure of forecast accuracy as well as on the use of modified Diebold-Mariano tests for the significance of measured differences in RMSE.

If a forecaster or policy-maker simply wishes to minimize expected RMSE, and can trust historic rankings of the alternative methods to be unbiased indicators of future relative performance, the he/she should select the model which reports the lowest such errors, regardless of how small or large its edge over competing models. The significance of the differences in measured performance arises only if we ask how certain they can be about this choice of model.

This question in turn forces us to consider both the power and size of our statistical tests. If our tests lack power, we might fail to detect a model with superior forecasting ability. There are reasonable grounds to worry about the power of Diebold-Mariano and related tests. Kilian and Inoue (2002) argue that tests of forecasting ability will tend to be less powerful in detecting structural changes than in-sample break tests, while Clark and McCracken (2002) argue that forecast encompassing tests offer more power against the null of equivalent forecast performance. Despite this, we think the inferences we draw from modified Diebold-Mariano tests are interesting for a number of reasons.

First, our baseline results clearly show that the tests we use have the power to detect at least some interesting differences in forecast performance; in particular they suggest that differences in RMSE of more than 15-20% will typically be statistically significant. Therefore, while our tests may well fail to detect some significant differences in forecast performance, such differences are likely to be limited in size and economic importance.

Second, while in-sample tests may be a more powerful way of detecting structural breaks, the hypothesis of no structural breaks seems to us to be of less interest than that of no improvement in forecast accuracy. For example, it is possible that a test may detect significant evidence of a structural break, but that the shift is small enough to be of little economic importance.

Third, although we experimented with the use of forecast encompassing tests (along the lines of Harvey, Leybourne and Newbold (1998) and Clark and McCracken (2002)), we decided against their use. To be sure, they permit a direct test of an interesting hypothesis; that a model's forecasts cannot be improved by incorporating estimates of the output gap. However, they allow for the weights on the output gap to be determined ex post, whereas our testing procedure requires such weights be determined ex ante. We feel that the latter better reflects the operational needs of forecasters. Furthermore, our bootstrap experiments (discussed below) indicated that nominal critical values for the encompassing tests were much less accurate than those for tests of forecast accuracy. Encompassing tests seemed to suffer from much more severe size distortion in our setting, greatly increasing the frequency of spurious findings of potential improvements in forecast performance.

The size of the tests we use is also a legitimate cause for concern. Most recently, Clark and McCracken (2002) show that in the case of multi-step forecasts from models with estimated parameters, Diebold-Mariano statistics are not pivotal even asymptotically, implying that appropriate critical values must be determined by bootstrap or other simulation methods on a case-by-case basis. We therefore checked the accuracy of our tests using such a bootstrap experiment.

Specifically, we estimated bivariate VAR systems of inflation and the output gap under the restriction that the output gap does not Granger-cause inflation. Residuals from the VAR were then used to bootstrap 2000 simulated data sets. The Granger-causal restriction implies that the output gap should not aid in forecasting inflation in the simulated data. The simulated data was then used to repeat our standard forecasting experiment, with recursive lag length determination, estimation and forecasting. The resulting distribution of our forecast test statistics under the null hypothesis was then analysed and tabulated. This was then repeated for each of the 14 different output gap measures examined in our study.[10]

Figure 5 presents a summary of the simulation results in the form of Davidson-MacKinnon p-value plots. These graphs compare the empirical cummulative distribution function (cdf) of the simulated Diebold-Mariano statistics to the standard normal cdf proposed by Diebold-Mariano (1995). The difference between the two is simply the degree of observed size distortion. The top-left panel in the figure is simply a scatterplot of the two cdfs; in the absence of any size distortion we should therefore find a straight line from the origin with a slope of 1 (shown by the green solid line.) The bottom-left panel shows the difference between

[10] Complete details on the simulation experiment are available from the authors. Note that only final data were used to estimate the VAR system; any effects of data revision are therefore ignored. The experiments were computationally demanding, requiring approximately 2 weeks of CPU time on an UltraSPARC 10 workstation. This was in large measure due to the need to select two optimal lag lengths for the forecasting equation at every period in every simulation for every model. Some experimentation showed that minor modifications to the determination of lag length produced only limited gains in speed but changed the distribution of test statistics. We therefore limited our analysis to that of the "base case" results presented in Table 5.

the cdfs on the vertical axis; in the absence of any size distortion we should therefore find a horizontal line at zero (shown by the green solid line.) The shaded region around the x-axis in this panel shows an approximate confidence interval for the difference between the theoretical and empirical cdf based on the 95% critical values for the Kolmogorov-Smirnov statistic. Although the two left panels show the entire cdfs over the interval $[0, 1]$, for hypothesis testing we are most interested in the region close to 0, which represents small p-values. This is more easily seen in the right panels which simply present a detail of the left tail of the distributions from the left panels, over the $[0, 0.1]$ interval.

The figure summarizes the distribution of the test statistics across all 14 output gap models, for a total of 28,000 simulated data sets.[11] Furthermore, it shows the distribution of both the full-sample and the two sub-sample (1st and 2nd half) forecast test statistics. Concentrating on the bottom-right panel, we see that all three statistics plot well above the horizontal axis, implying the presence of significant size distortion which varies considerably across the three statistics. For example, consider the case of the 5% nominal critical value typically used to test the null hypothesis of no improvement in forecast accuracy; this corresponds to the 0.05 theoretical p-value on the horizontal axis. The figure shows that the empirical p-value is higher than this by an amount which varies considerably with the sample considered; it is higher by about 3% for the 1st-half of the sample, 5% for the overall sample, or 13% for the 2nd-half of the sample. Using the standard 5% critical values, we should therefore expect to find spurious evidence of improvements in forecast performance with a probability of between 8% and 18%, depending on the sample period we consider.

This implies that our baseline results *overstate* the degree to which output gaps significantly improve the forecasts of inflation. Furthermore, this exaggeration is most severe in the most recent half of our sample. To correct for this, we redid the analysis presented in Table 5 with bootstrapped critical values specific to both the sample period and the model of the output gap. We also expanded the range of test statistics we consider to include

[11] These distributions varied somewhat across models. A complete set of p-value plots broken down by model is available from the authors by request. We take account of this variation when we tabulate model-specific critical values, below.

all four considered by Clark and McCracken (2002); the original Diebold-Mariano statistic (DM), a modified version proposed by Clark and McCracken (2002) which should be more powerful (MSE-F), the test for forecast encompassing (FE) and its similarly modified counterpart (MFE). Test statistics and their bootstrapped 95% critical values are presented in Table 14.

The conclusions from the two tests of forecast accuracy presented in Table 14 are similar to and somewhat more pessimistic than those in Table 5. None of the models are able to significantly improve on the univariate forecast over the full sample or over the more recent low-inflation period. In the high-inflation period, only the Harvey-Jaeger univariate estimate of the gap significantly improves forecast performance relative to the AR model of inflation. (Recall, however, that even this model does not forecast as well over this period as the nominal output growth model.)

These results change somewhat if we test for forecast encompassing rather than forecast accuracy. According to the MFE statistic, over the full sample, only the Harvey-Jaeger and nominal output growth models can significantly improve the accuracy of the univariate forecast, but no model does so over either subsample. According to the FE statistic, however, three models potentially allow us to improve on the univariate AR model in the most recent sub sample; the linear trend, Watson and Kuttner models.

We note that aside from the nominal output growth model, all these models share the assumption of a constant trend growth rate for potential output. This tends to cause large revisions; Table 1 shows that three of these have the worst noise-signal ratios. Furthermore, all three have forecast accuracy statistics which are far from significant; the best is the Watson model with a statistic roughly half of its critical and the worst is the linear trend which worsened forecast performance relative to the univariate model. For these reasons, we are sceptical that forecasters would have been able to exploit the potential benefits which the encompassing test here identifies ex post.

Overall, these results provide even less reason to expect a reliable improvement in forecasting performance by adding information from output gap in univariate inflation forecasts than our baseline findings.

7 Conclusion

Forecasting inflation is a difficult but essential task for the successful implementation of monetary policy. The hypothesis that a stable predictive relationship between inflation and the output gap—a Phillips curve—is present in the data, suggests that output gap measures could be useful for forecasting inflation. This has served as the basis for empirical formulations of countercyclical monetary policy. We find that many alternative measures of the output gap *appear* to be quite useful for forecasting inflation, on the basis of in-sample analysis. That is, a historical Phillips curve is suggested by the data, and ex post estimates of the output gap are useful for understanding historical movements in inflation. However, this suggested usefulness does not imply a similar operational usefulness. Our simulated real-time forecasting experiment suggests, instead, that this predictive ability is mostly illusory. These disappointing results bring into question the practical usefulness of output-gap-based Phillips curves for forecasting inflation and the monetary policy process.

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Appendix: Alternative Detrending Methods

A detrending method decomposes the log of real output, q_t, into a trend component, μ_t, and a cycle component, z_t.

$$q_t = \mu_t + z_t \tag{A.1}$$

Some methods use the data to estimate the trend, μ_t, and define the cyclical component as the residual. Others specify a dynamic structure for both the trend and cycle components and estimate them jointly. We examine detrending methods that fall into both categories.

A.1 Deterministic Trends

The first set of detrending methods we consider assume that the trend in (the logarithm of) output is well approximated as a simple deterministic function of time. The linear trend is the oldest and simplest of these models. The quadratic trend is a popular alternative.

Because of the noticeable downturn in GDP growth after 1973, another simple deterministic technique is a breaking linear trend that allows for the slowdown in that year. Our implementation of the breaking trend method incorporates the assumption that the location of the break is fixed and known. Specifically we assume that a break in the trend at the end of 1973 would have been incorporated in real time from 1977 on. As discussed in Orphanides and van Norden (1999) this conforms with the debate regarding the productivity slowdown during the 1970s.

A.2 Unobserved Component Models and the Hodrick–Prescott Filter

Unobserved component (UC) models offer a general framework for decomposing output into an unobserved trend and a cycle, allowing for an assumed dynamic structure for these components.

This framework can also nest smoothing splines, such the popular filter proposed by Hodrick and Prescott (1997) (the HP filter). We implement the HP filter, following Harvey and Jaeger (1993) and King and Rebelo (1993), by writing it in its unobserved components form. Assuming that the trend in (1) follows:

$$(1 - L)^2 \mu_t = \eta_t \tag{A.2}$$

the HP filter is obtained from (A.1) and (A.2) under the assumption that z_t and η_t are mutually uncorrelated white noise processes with a fixed relative variance q. We set q to correspond to the standard application of the HP filter with a smoothing parameter of 1600.

UC models also permit more complex dynamics to be estimated, and we examine two such alternatives, by Watson (1986) and by Harvey (1985) and Clark (1987). The Watson model modifies the linear level model to allow for greater business cycle persistence. Specifically, it models the trend as a random walk with drift and the cycle as an AR(2) process:

$$\mu_t = \delta + \mu_{t-1} + \eta_t \tag{A.3}$$

$$z_t = \rho_1 \cdot z_{t-1} + \rho_2 \cdot z_{t-2} + \varepsilon_t \tag{A.4}$$

Here ε_t and η_t are assumed to be i.i.d mean-zero Gaussian and mutually uncorrelated and δ, ρ_1 and ρ_2, and the variances of the two shocks are parameters to be estimated (5 in total).

The Harvey-Clark model similarly modifies the local linear trend model:

$$\mu_t = g_{t-1} + \mu_{t-1} + \eta_t \tag{A.5}$$

$$g_t = g_{t-1} + \nu_t \tag{A.6}$$

$$z_t = \rho_1 \cdot z_{t-1} + \rho_2 \cdot z_{t-2} + \varepsilon_t \tag{A.7}$$

Here η_t, ν_t, and ε_t are assumed to be i.i.d mean-zero Gaussian and mutually uncorrelated processes and ρ_1 and ρ_2 and the variances of the three shocks are parameters to be estimated (5 in total).

A.3 Unobserved Component Models with a Phillips Curve

Multivariate formulations of UC models attempt to refine estimates of the output gap by incorporating information from other variables linked to the gap. We consider two models which add a Phillips curve to the univariate formulations described above; those of Kuttner (1994) and Gerlach and Smets (1997).

Let π_t be the quarterly rate of inflation. The Kuttner model adds the following Phillips curve equation to the Watson model:

$$\Delta\pi_t = \xi_1 + \xi_2 \cdot \Delta q_t + \xi_3 \cdot z_{t-1} + e_t + \xi_4 \cdot e_{t-1} + \xi_5 \cdot e_{t-2} + \xi_6 \cdot e_{t-3} \tag{A.8}$$

The Gerlach-Smets model modifies the Harvey-Clark model by adding the similar Phillips curve:

$$\Delta\pi_t = \phi_1 + \phi_2 \cdot z_t + e_t + \phi_3 \cdot e_{t-1} + \phi_4 \cdot e_{t-2} + \phi_5 \cdot e_{t-3} \tag{A.9}$$

In each case the shock e_t is assumed i.i.d. mean zero and Gaussian. In the Gerlach-Smets model, e_t is also assumed uncorrelated with shocks driving the dynamics of the trend and cycle components of output in the model. Thus, by adding the Phillips curve, the Gerlach-Smets model introduces an additional six parameters that require estimation ($\{\phi_1, ..., \phi_5\}$ and the variance of e_t). The Kuttner model also allows for a non-zero correlation between e_t and the shock to the cycle, η_t. Thus, it introduces eight additional parameters that require estimation ($\{\xi_1, ..., \xi_6\}$, the variance of e_t and its covariance with η_t.)

A.4 The Band-Pass Filter

Another approach to cycle-trend decomposition is via the use of band-pass filters in the frequency domain. The clearest exponent of this approach is Baxter and King (1999), who suggest the use of truncated versions of the ideal (and therefore infinitely long) filter with a band passing fluctuations with durations between 6 and 32 quarters in length. Stock and Watson (1998) adapt this for use at the end of data samples by padding the available observations with forecasts from a low-order AR model fit to the data series. Following Stock and Watson, we use a filter 25 observations in length and pad using an AR(4) forecast.

A.5 The Beveridge-Nelson Decomposition

Beveridge and Nelson (1981) consider the case of an ARIMA(p,1,q) series, y, which is to be decomposed into a trend and a cyclical component. For simplicity, we can assume that all deterministic components belong to the trend component and have already been removed from the series. Since the first-difference of the series is stationary, it has an infinite-order MA representation of the form

$$\Delta y_t = \varepsilon_t + \beta_1 \cdot \varepsilon_{t-1} + \beta_2 \cdot \varepsilon_{t-2} + \cdots = e_t \tag{A.10}$$

where ε is assumed to be an innovations sequence. The change in the series over the next s periods is simply

$$y_{t+s} - y_t = \sum_{j=1}^{s} \Delta y_{t+j} = \sum_{j=1}^{s} e_{t+j} \tag{A.11}$$

The trend is defined to be

$$\lim_{s\to\infty} E_t(y_{t+s}) = y_t + \lim_{s\to\infty} E_t(\sum_{j=1}^{s} e_{t+j}) \tag{A.12}$$

From equation 6, we can see that

$$E_t(e_{t+j}) = E_t(\varepsilon_{t+j} + \beta_1 \cdot \varepsilon_{t+j-1} + \beta_2 \cdot \varepsilon_{t+j-2} + \cdots) = \sum_{i=0}^{\infty} \beta_{j+i} \cdot \varepsilon_{t-i} \tag{A.13}$$

Since changes in the trend are therefore unforecastable, this has the effect of decomposing the series into a random walk and a cyclical component, so that

$$y_t = \tau_t + c_t \tag{A.14}$$

where the trend is

$$\tau_t = \tau_{t-1} + e_t$$

and e_t is white noise.

To use the Beveridge-Nelson decomposition we must therefore: (1) Identify p and q in our ARIMA(p,1,q) model. (2) Identify the $\{\beta_j\}$ in equation 6. (3) Choose some large enough but finite value of s to approximate the limit in equation 8. (4) For all t and for $j = 1, \cdots, s$, calculate $E_t(e_{t+j})$ from equation 9. (5) Calculate the trend at time t as $y_t + E_t(\sum_{j=1}^{s} e_{t+j})$ and the cycle as y_t minus the trend.

Based on results for the full sample, we use an ARIMA(1,1,2), with parameters re-estimated by maximum likelihood methods before each recalculation of the trend.

A.6 The Structural VAR Approach

The Structural VAR measure of the output gap is based on a VAR identified via restrictions on the long-run effects of the structural shocks, as proposed by Blanchard and Quah (1989). Our implementation is identical to that of Cayen and van Norden (2002), who use a trivariate system including output, CPI and yields on 3-month treasury bills. Lag lengths for the VAR are selected using corrected LR tests and a general-to-specific approach.

Table 1

Reliability of Alternative Output Gap Measures

Method	COR	AR	NS	NSR	OPSIGN
Linear Trend	0.87	0.93	0.50	1.36	0.53
Quadratic Trend	0.61	0.95	0.95	0.98	0.31
Breaking Trend	0.78	0.85	0.80	0.81	0.21
Hodrick-Prescott	0.50	0.92	1.10	1.10	0.38
Band Pass	0.69	0.78	0.73	0.81	0.32
Beveridge-Nelson	0.82	0.02	0.60	0.62	0.22
Stuctural VAR	0.67	0.87	1.04	1.06	0.21
Watson	0.90	0.88	0.54	1.25	0.24
Harvey-Clark	0.88	0.88	0.61	0.64	0.13
Harvey-Jaeger	0.94	0.90	0.49	0.49	0.07
Kuttner	0.87	0.92	0.51	1.19	0.53
Gerlach-Smets	0.75	0.83	0.78	1.11	0.36

Notes: The table present summary measures of the reliability of real-time estimates of the output gap for 12 alternative methods of estimating the output gap. All statistics are for the 1969:1–1998:4 period. COR, denotes the correlation of the real-time and final estimates of the output gap. AR the first order serial correlation of the revision (the difference between the final and real-time series). NS indicates the ratio of the standard deviation of the revision and the standard deviation of the final estimate of the gap. NSR indicates the ratio of the root mean square of the revision and the standard deviation of the final estimate of the gap. OPSIGN indicates the frequency with which the real-time and final gap estimates have opposite signs.

Table 2

RMSE of Forecasts—In Sample

Method	1969-1998	1969-1983	1984-1998
Linear Trend	1.601	1.953	1.137
Quadratic Trend	1.629	1.971	1.183
Breaking Trend	1.741	2.127	1.230
Hodrick-Prescott	1.662	1.885	1.399
Band Pass	1.765	2.135	1.284
Beveridge-Nelson	1.746	2.085	1.315
Stuctural VAR	1.742	2.067	1.334
Watson	1.623	1.972	1.165
Harvey-Clark	1.728	2.077	1.279
Harvey-Jaeger	1.798	2.048	1.503
Kuttner	1.550	1.902	1.079
Gerlach-Smets	1.470	1.747	1.119
AR	1.912	2.340	1.344
Real Growth	1.750	2.078	1.337
Nominal Growth	1.550	1.737	1.332

Notes: The entries show the RMSE of the inflation forecast from equation (1). The first twelve rows show results using alternative output gaps. The AR forecast is univariate, and the last two rows show the forecasts based on real and nominal growth instead of the gaps.

Table 3

RMSE Relative to AR—In Sample

Method	1969-1998	1969-1983	1984-1998
Linear Trend	0.838*	0.835*	0.846*
Quadratic Trend	0.852*	0.843*	0.880
Breaking Trend	0.910*	0.909*	0.915*
Hodrick-Prescott	0.869*	0.805*	1.041
Band Pass	0.923*	0.913*	0.956
Beveridge-Nelson	0.913*	0.891*	0.978
Stuctural VAR	0.911*	0.883*	0.992
Watson	0.849*	0.843*	0.867
Harvey-Clark	0.904*	0.888*	0.952
Harvey-Jaeger	0.941	0.875*	1.119
Kuttner	0.810*	0.813*	0.803*
Gerlach-Smets	0.769*	0.747*	0.833
Real Growth	0.915*	0.888*	0.995
Nominal Growth	0.811*	0.743*	0.991

Notes: The entries show the ratio of the RMSE of the inflation forecast based on the method shown and the RMSE of the AR forecast. * indicates that the improvement in forecasts relative to the AR forecast, as measured by the modified Diebold-Marianno test statistic, is statistically significant at 5 percent level.

Table 4

RMSE of Forecasts—Real Time

Method	1969-1998	1969-1983	1984-1998
Linear Trend	2.341	2.821	1.719
Quadratic Trend	2.390	2.878	1.761
Breaking Trend	2.377	2.936	1.622
Hodrick-Prescott	2.333	2.622	1.998
Band Pass	2.244	2.634	1.763
Beveridge-Nelson	2.185	2.681	1.522
Stuctural VAR	2.483	3.007	1.798
Watson	2.214	2.764	1.453
Harvey-Clark	2.592	3.263	1.648
Harvey-Jaeger	2.111	2.316	1.880
Kuttner	2.254	2.807	1.495
Gerlach-Smets	2.242	2.810	1.451
AR	2.308	2.869	1.540
Real Growth	2.153	2.587	1.596
Nominal Growth	1.946	2.252	1.576

Notes: See notes to Tables 2 and 3.

Table 5

RMSE Relative to AR—Real Time

Method	1969-1998	1969-1983	1984-1998
Linear Trend	1.014	0.983	1.116
Quadratic Trend	1.036	1.003	1.144
Breaking Trend	1.030	1.023	1.053
Hodrick-Prescott	1.011	0.914	1.297
Band Pass	0.973	0.918	1.144
Beveridge-Nelson	0.947	0.935	0.988
Stuctural VAR	1.076	1.048	1.168
Watson	0.959	0.964	0.944
Harvey-Clark	1.123	1.138	1.070
Harvey-Jaeger	0.915	0.807*	1.221
Kuttner	0.977	0.979	0.971
Gerlach-Smets	0.971	0.980	0.942
Real Growth	0.933	0.902*	1.036
Nominal Growth	0.843*	0.785*	1.023

Notes: See notes to Tables 2 and 3.

Table 6

Relative RMSE: 1969-1998—Real Time

Method	AR	Real Growth	Nominal Growth
Linear Trend	1.014	1.087	1.203
Quadratic Trend	1.036	1.110	1.228
Breaking Trend	1.030	1.104	1.221
Hodrick-Prescott	1.011	1.084	1.199
Band Pass	0.973	1.042	1.153
Beveridge-Nelson	0.947	1.014	1.122
Stuctural VAR	1.076	1.153	1.276
Watson	0.959	1.028	1.137
Harvey-Clark	1.123	1.204	1.332
Harvey-Jaeger	0.915	0.980	1.085
Kuttner	0.977	1.047	1.158
Gerlach-Smets	0.971	1.041	1.152
AR		1.072	1.186
Real Growth	0.933		1.106
Nominal Growth	0.843	0.904	

Notes: Each entry denotes the ratio of the RMSE of the inflation forecast based on the methods shown in the corresponding row to the RMES based on the method shown in the corresponding column.

Table 7

RMSE Relative to AR for Alternative Horizon Forecasts

	1969-1998				1969-1983				1984-1998			
Horizon (quarters)	3	5	7	9	3	5	7	9	3	5	7	9
Linear Trend	1.006	1.014	0.924	0.921	0.974	0.983	0.862	0.871	1.088	1.116	1.224	1.142
Quadratic Trend	0.988	1.035	0.957	0.943	0.948	1.003	0.900	0.915	1.086	1.143	1.241	1.077
Breaking Trend	1.019	1.030	1.004	0.978	0.976	1.023	0.962	0.959	1.127	1.053	1.219	1.069
Hodrick-Prescott	0.965	1.011	0.964	0.903	0.875	0.913	0.905	0.888	1.173	1.297	1.259	0.978
Band Pass	0.942	0.972	0.935	1.002	0.914	0.918	0.880	0.974	1.015	1.144	1.212	1.134
Beveridge-Nelson	0.960	0.946	0.985	0.967	0.949	0.934	0.962	0.961	0.989	0.988	1.105	0.996
Structural VAR	1.052	1.075	0.989	0.942	1.013	1.048	0.924^*	0.899	1.151	1.167	1.311	1.135
Watson	1.000	0.959	0.891	0.908	0.972	0.963	0.879	0.896	1.071	0.943	0.962	0.970
Harvey-Clark	1.036	1.123	1.085	1.042	1.022	1.137	1.069	1.058	1.072	1.070	1.176	0.956
Harvey-Jaeger	0.939	0.914	0.876	0.935	0.814^*	0.807^*	0.807	0.920	1.210	1.220	1.204	1.008
Kuttner	0.970	0.976	0.902	0.871	0.953	0.978	0.875	0.868	1.016	0.970	1.044	0.887
Gerlach-Smets	0.953	0.971	0.920	0.900	0.941	0.979	0.910	0.915	0.983	0.942	0.978	0.822
Real Growth	0.954	0.933	0.941	0.992	0.913	0.901	0.899	0.985	1.054	1.036	1.160	1.027
Nominal Growth	0.916	0.843^*	0.857^*	0.899	0.873	0.785^*	0.803	0.885	1.022	1.023	1.121	0.968
AR	0.014	0.023	0.032	0.040	0.017	0.029	0.041	0.050	0.010	0.015	0.018	0.023

Notes: The horizons, $j \in \{3, 5, 7, 9\}$ indicate forecasts of the rate of inflation from quarter $t-1$ to quarter $t+j-1$. The last row shows the RMSE of the AR based forecast errors for the evaluation period shown. Entries in all other rows show the ratio of the RMSE of the forecasts using the output gap in that row relative to that of the corresponding AR forecast, for the same evaluation period.

Table 8

Relative RMSE with Alternative Start of Estimation Period

	1969-1998				1969-1983				1984-1998			
Est. Start	1947	Base	1960	1965	1947	Base	1960	1965	1947	Base	1960	1965
Linear Trend	1.126	1.014	1.105	2.705	1.046	0.983	1.103	2.962	1.579	1.116	1.113	1.121
Quadratic Trend	0.996	1.035	1.123	2.706	0.992	1.003	1.106	2.965	1.029	1.143	1.198	1.110
Breaking Trend	1.000	1.030	1.119	2.717	0.996	1.023	1.117	2.961	1.030	1.053	1.128	1.262
Hodrick-Prescott	1.059	1.011	1.302	10.617	1.026	0.913	1.304	11.804	1.270	1.297	1.292	1.424
Band Pass	1.016	0.972	1.088	6.908	0.993	0.918	1.074	7.667	1.160	1.144	1.149	1.302
Beveridge-Nelson	0.879	0.946	1.041	6.410	0.818	0.934	1.052	7.119	1.225	0.988	0.991	1.073
Structural VAR	0.999	1.075	1.033	13.493	0.918	1.048	1.003	15.015	1.447	1.167	1.159	1.175
Watson	0.993	0.959	1.115	2.798	0.989	0.963	1.136	3.065	1.018	0.943	1.013	1.148
Harvey-Clark	1.070	1.123	1.157	3.452	1.062	1.137	1.172	3.777	1.126	1.070	1.089	1.455
Harvey-Jaeger	1.025	0.914	1.102	1.363	0.977	0.807^*	1.062	1.362	1.316	1.220	1.267	1.366
Kuttner	0.882	0.976	1.091	2.707	0.820	0.978	1.094	2.967	1.235	0.970	1.078	1.091
Gerlach-Smets	0.850	0.971	1.077	2.703	0.785^*	0.979	1.090	2.961	1.212	0.942	1.018	1.105
Real Growth	0.940	0.933	1.033	6.312	0.932	0.901	1.036	7.008	0.992	1.036	1.017	1.107
Nominal Growth	0.971	0.843^*	0.953	10.016	0.953	0.785^*	0.925	11.144	1.091	1.023	1.072	1.022
AR	0.025	0.023	0.029	0.028	0.033	0.029	0.037	0.035	0.012	0.015	0.017	0.017

Notes: The table compares evaluation results when the start of the estimation models used for forecastig varies from the baseline, 1955Q1, as shown. The last row shows the RMSE of the AR based forecast errors. Entries in all other rows show the ratio of the RMSE of the forecasts using the output gap in that row relative to that of the corresponding AR forecast.

Table 9

RMSE Relative to AR with Restricted Estimation Specifications

	1969-1998			1969-1983			1984-1998		
	Base	Δy	$\Delta \pi$	Base	Δy	$\Delta \pi$	Base	Δy	$\Delta \pi$
Linear Trend	1.014	0.933	0.919	0.983	0.901	0.901	1.116	1.036	0.954
Quadratic Trend	1.035	0.942	0.940	1.003	0.889*	0.909	1.143	1.108	1.000
Breaking Trend	1.030	0.941	0.975	1.023	0.885	0.951	1.053	1.117	1.022
Hodrick-Prescott	1.011	0.967	1.003	0.913	0.876*	0.936	1.297	1.235	1.130
Band Pass	0.972	0.946	0.973	0.918	0.891*	0.957	1.144	1.119	1.005
Beveridge-Nelson	0.946	1.029	0.930	0.934	1.017	0.945	0.988	1.070	0.898
Structural VAR	1.075	0.963	1.069	1.048	0.905	1.060	1.167	1.143	1.088
Watson	0.959	0.944	0.872	0.963	0.894*	0.878	0.943	1.101	0.860*
Harvey-Clark	1.123	0.941	0.880*	1.137	0.905	0.854	1.070	1.058	0.930
Harvey-Jaeger	0.914*	1.010	0.899	0.807	0.957	0.789*	1.220	1.178	1.093
Kuttner	0.976	0.933	0.895	0.978	0.901	0.899	0.970	1.036	0.888*
Gerlach-Smets	0.971	0.951	0.922	0.979	0.894	0.924	0.942	1.127	0.916
Real Growth	0.933	0.996	0.913	0.901	0.961	0.877	1.036	1.111	0.985
Nominal Growth	0.843*	0.996	1.026	0.785*	0.973*	0.998	1.023	1.076	1.083
AR	0.023	0.023	0.021	0.028	0.028	0.025	0.017	0.015	0.017

Notes: The column marked $\Delta \pi$ reports forecast evaluation results when the forecasting equation (1) is estimated with the accelerationist restriction, $\sum \beta_i = 1$, imposed. The column marked Δy reports results when the differencing restriction on the output gap variables, $\sum \gamma_i = 0$, is imposed on the equation.

Table 10

In-Sample Use of Final Versus Real-Time Output Gaps

	1965-1998		1965-1983		1984-1998	
	Final	Real-Time	Final	Real-Time	Final	Real-Time
Linear Trend	0.826	0.791*	0.779	0.732*	0.982	1.015
Quadratic Trend	0.870*	0.865	0.850*	0.790*	0.942	1.139
Breaking Trend	0.917*	0.846	0.909*	0.786*	0.946	1.085
Hodrick-Prescott	0.883	0.841	0.837*	0.713*	1.038	1.220
Band Pass	0.899	0.856	0.861*	0.755*	1.029	1.159
Beveridge-Nelson	0.873	0.840	0.808*	0.728*	1.081	1.184
Structural VAR	0.911	0.950	0.853*	0.884*	1.100	1.165
Watson	0.892*	0.827	0.870*	0.794*	0.972	0.958
Harvey-Clark	0.934*	0.894	0.924*	0.827*	0.969	1.122
Harvey-Jaeger	0.937	0.897	0.873	0.762*	1.147	1.281
Kuttner	0.778*	0.782*	0.728*	0.727*	0.941	0.995
Gerlach-Smets	0.795*	0.921	0.717*	0.872*	1.036	1.090
Real Growth	0.903*	0.765	0.877*	0.652*	0.996	1.100
Nominal Growth	0.836*	0.752	0.783*	0.622*	1.008	1.117
AR	0.019	0.019	0.024	0.024	0.012	0.012

Notes: The entries show the RMSE of the inflation forecast for the AR model, and the RMSE relative to that of the AR for all other models. See also notes to Table 2.

Table 11

Out-of-Sample Use of Final Versus Real-Time Output Gaps

	1965-1998		1965-1983		1984-1998	
	Final	Real-Time	Final	Real-Time	Final	Real-Time
Quadratic Trend	0.965	1.035	0.946	1.003	1.027	1.143
Breaking Trend	1.052	1.030	1.041	1.023	1.090	1.053
Hodrick-Prescott	1.045	1.011	0.980	0.913	1.247	1.297
Band Pass	1.052	0.972	1.011	0.918	1.183	1.144
Beveridge-Nelson	1.019	0.946	0.983	0.934	1.136	0.988
Structural VAR	1.005	1.075	0.909	1.048	1.287	1.167
Watson	1.035	0.959	1.039	0.963	1.021	0.943
Harvey-Clark	1.102	1.123	1.096	1.137	1.123	1.070
Harvey-Jaeger	1.028	0.914	0.941	0.807*	1.286	1.220
Kuttner	0.935	0.976	0.933	0.978	0.941	0.970
Gerlach-Smets	0.906	0.971	0.876	0.979	1.007	0.942
Real Growth	0.946	0.933	0.925	0.901	1.020	1.036
Nominal Growth	0.900	0.843*	0.858	0.785*	1.038	1.023
AR	0.023	0.023	0.028	0.028	0.015	0.015

Notes: The entries show the RMSE of the inflation forecast for the AR model, and the RMSE relative to that of the AR for all other models. See also notes to Table 2.

Table 12

RMSE Relative to AR with Alternative Lag Selection Criteria

	1969-1998			1969-1983			1984-1998		
	Base	AIC	BIC	Base	AIC	BIC	Base	AIC	BIC
Linear Trend	1.014	0.989	1.005	0.983	0.954	0.990	1.116	1.114	1.080
Quadratic Trend	1.035	0.981	0.993	1.003	0.932	0.965	1.143	1.147	1.121
Breaking Trend	1.030	1.006	0.929	1.023	0.968	0.897	1.053	1.140	1.071
Hodrick-Prescott	1.011	1.052	0.936	0.913	0.984	0.811*	1.297	1.276	1.404
Band Pass	0.972	1.016	0.875*	0.918	0.987	0.863*	1.144	1.120	0.936
Beveridge-Nelson	0.946	0.971	0.913	0.934	0.947	0.885	0.988	1.057	1.043
Structural VAR	1.075	1.104	1.120	1.048	1.108	1.13	1.167	1.088	1.072
Watson	0.959	0.991	0.956	0.963	1.001	0.971	0.943	0.950	0.881
Harvey-Clark	1.123	1.070	0.984	1.137	1.067	0.975	1.070	1.082	1.028
Harvey-Jaeger	0.914	0.901	0.926	0.807*	0.800*	0.786*	1.220	1.212	1.436
Kuttner	0.976	0.933	0.949	0.978	0.934	0.953	0.970	0.932	0.930
Gerlach-Smets	0.971	0.923	0.927	0.979	0.924	0.938	0.942	0.920	0.873
Real Growth	0.933	0.965	0.898	0.901	0.936	0.865*	1.036	1.069	1.050
Nominal Growth	0.843*	0.836*	0.882	0.785	0.785*	0.812*	1.023	1.009	1.171
AR	0.023	0.023	0.022	0.028	0.029	0.029	0.015	0.015	0.013

Notes: Base (Ng-Perron), AIC and BIC denote the alternative criteria for lag selection in the forecasting models. The last row shows the RMSE of the AR based forecast errors. Entries in all other rows show the ratio of the RMSE of the forecasts using the output gap concept in that row relative to that of the corresponding AR forecast.

Table 13

RMSE Relative to AR with Alternative Use of Data Vintages

	Conventional AIC	KDP AIC	Conventional BIC	KDP BIC
Linear Trend	1.114	1.643	0.981	1.646
Quadratic Trend	1.147	1.637	1.154	1.314
Breaking Trend	1.140	1.296	1.255	1.206
Hodrick-Prescott	1.276	1.456	1.239	1.427
Band Pass	1.120	1.107	0.989	0.962
Beveridge-Nelson	1.057	1.030	1.063	1.094
Structural VAR	1.088	1.163	1.118	1.183
Watson	0.950	1.571	1.193	1.299
Harvey-Clark	1.082	1.543	1.511	1.517
Harvey-Jaeger	1.212	1.638	1.544	1.509
Kuttner	0.932	1.762	1.016	1.482
Gerlach-Smets	0.920	1.257	1.101	1.250
Real Growth	1.069	0.966	1.044	0.996
Nominal Growth	1.009	1.056	0.953	1.046
AR	0.019	0.017	0.016	0.016

Notes: KDP indicates the Koenig-Dolmas-Piger usage of data vintages for model estimation. The forecast evaluation period is 1984—1998. AIC and BIC denote the lag selection criteria. The last row shows the RMSE of the AR based forecast errors. Entries in all other rows show the RMSE using the output gap concept in that row relative to the AR RMSE.

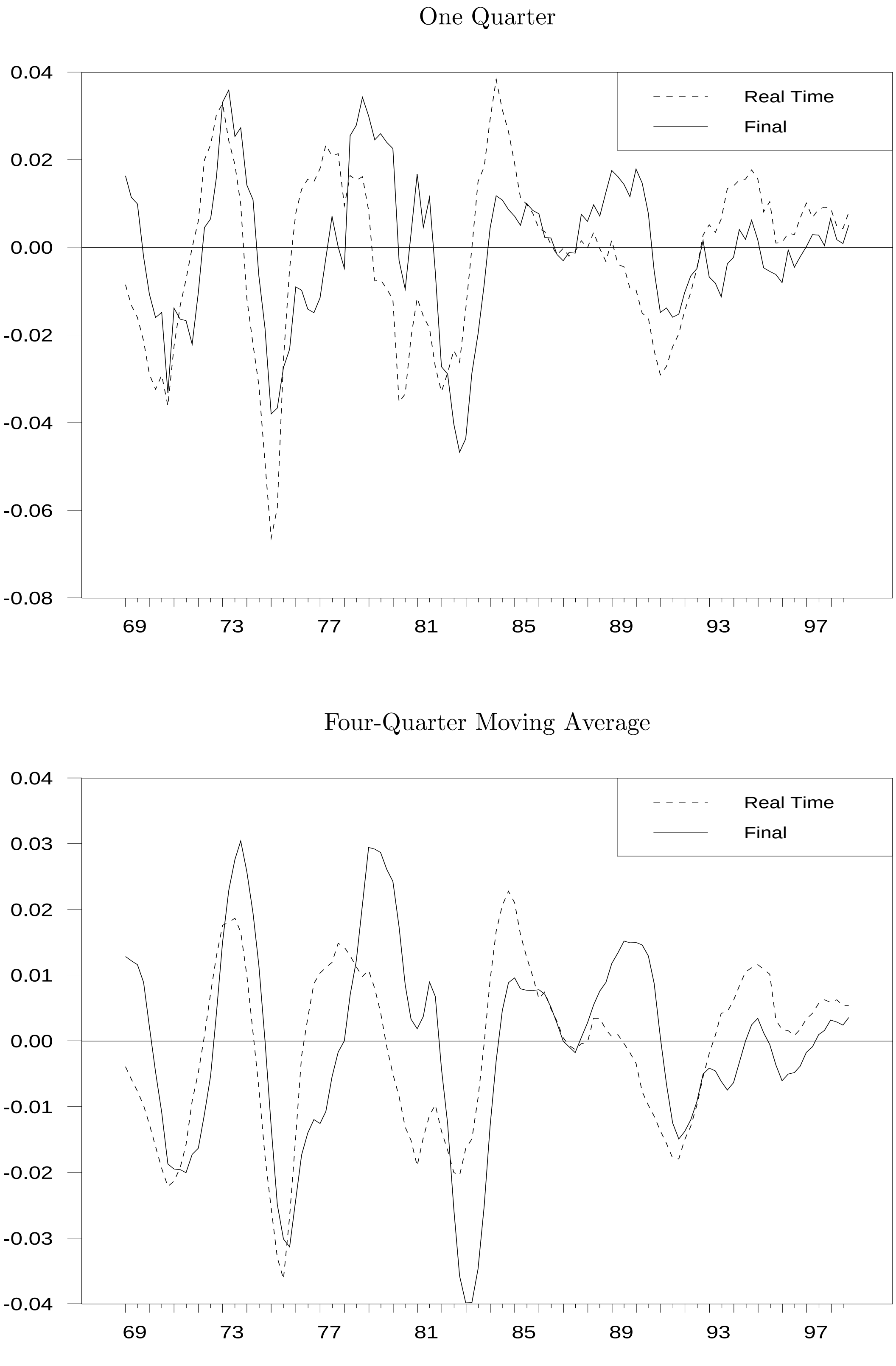
Table 14

Bootstrap Results for Forecast Evaluation

		1969-1998				1969-1983				1984-1998			
		DM	MSE-F	FE	MFE	DM	MSE-F	FE	MFE	DM	MSE-F	FE	MFE
Linear Trend	Statistic	−0.160	−0.305	2.165	2.396	0.146	0.264	1.279	1.361	−1.639	−1.532	6.503*	2.604
	95% CV	2.015	4.482	3.114	4.341	2.019	4.576	3.773	5.706	2.630	5.298	4.990	8.434
Quadratic Trend	Statistic	−0.474	−0.742	1.337	1.131	−0.033	−0.047	1.256	0.972	−1.179	−1.822	0.577	0.332
	95% CV	2.204	5.173	3.186	4.361	2.104	4.755	3.711	5.744	2.659	5.452	5.410	8.588
Breaking Trend	Statistic	−0.774	−0.631	1.925	1.132	−0.523	−0.351	1.927	0.923	−0.644	−0.765	0.873	0.392
	95% CV	1.860	3.999	3.006	3.502	1.928	3.966	3.576	4.634	2.366	4.273	4.935	7.320
Hodrick-Prescott	Statistic	−0.117	−0.239	1.778	1.577	1.352	1.527	2.665	1.871	−1.021	−3.142	−0.282	−0.208
	95% CV	2.039	3.051	3.010	2.592	1.968	2.741	3.604	3.312	2.375	3.060	5.014	5.209
Band Pass	Statistic	0.311	0.626	1.541	1.739	0.977	1.445	1.615	1.661	−0.611	−1.832	0.344	0.250
	95% CV	1.824	2.591	2.873	2.346	1.779	2.669	3.524	3.154	2.372	2.876	5.402	4.797
Beveridge-Nelson	Statistic	1.024	1.269	2.595	1.767	1.097	1.122	2.780	1.416	0.129	0.186	1.177	0.724
	95% CV	1.685	2.977	2.729	2.668	1.626	2.678	3.212	3.627	2.419	3.558	4.722	5.851
Stuctural VAR	Statistic	−0.906	−1.489	1.154	1.414	−0.464	−0.697	1.463	1.545	−2.302	−2.064	−1.406	−0.549
	95% CV	0.464	0.743	2.058	1.841	0.621	0.986	2.465	2.664	1.725	1.897	3.363	5.043
Watson	Statistic	0.504	0.949	2.096	2.488	0.351	0.595	1.435	1.510	1.382	0.950	6.184*	2.673
	95% CV	2.063	4.321	3.055	3.663	1.993	4.040	3.588	4.558	2.682	5.024	5.150	7.541
Harvey-Clark	Statistic	−0.748	−2.269	0.046	0.050	−0.676	−1.760	−0.032	−0.030	−0.469	−0.984	0.429	0.302
	95% CV	1.622	2.778	2.695	2.776	1.653	2.933	3.248	3.688	2.267	3.667	4.797	6.065
Harvey-Jaeger	Statistic	0.728	2.136	1.832	3.088*	1.937*	4.142*	2.043	3.375	−0.779	−2.549	0.369	0.346
	95% CV	1.519	2.808	2.672	2.735	1.636	2.865	3.196	3.631	2.161	3.521	4.786	5.960
Kuttner	Statistic	0.281	0.525	2.347	2.422	0.203	0.343	1.434	1.330	0.590	0.470	6.073*	3.083
	95% CV	1.718	4.027	2.937	3.951	1.729	3.911	3.499	5.208	2.508	4.835	4.650	8.556
Gerlach-Smets	Statistic	0.307	0.654	1.574	1.816	0.177	0.327	1.194	1.190	0.523	0.980	2.029	1.643
	95% CV	2.683	7.777	3.642	6.069	2.494	6.582	4.245	7.481	2.992	7.784	5.930	11.520
Real Growth	Statistic	1.199	1.626	1.611	1.710	1.605	1.779	1.769	1.617	−0.487	−0.532	0.225	0.117
	95% CV	2.259	3.862	3.292	3.159	2.227	3.619	3.941	4.085	2.617	3.926	5.744	6.129
Nominal Growth	Statistic	1.827	4.444	3.857*	5.400*	2.442	4.823	3.037	3.849	−0.191	−0.348	3.470	3.754
	95% CV	2.685	6.292	3.702	4.835	2.556	5.663	4.226	6.109	2.842	5.835	6.461	9.820

Notes: DM: Diebold-Mariano; MSE-F: Clark-McCracken modified statistic; FE: Forecast encompassing; MFE: Modified forecast encompassing.

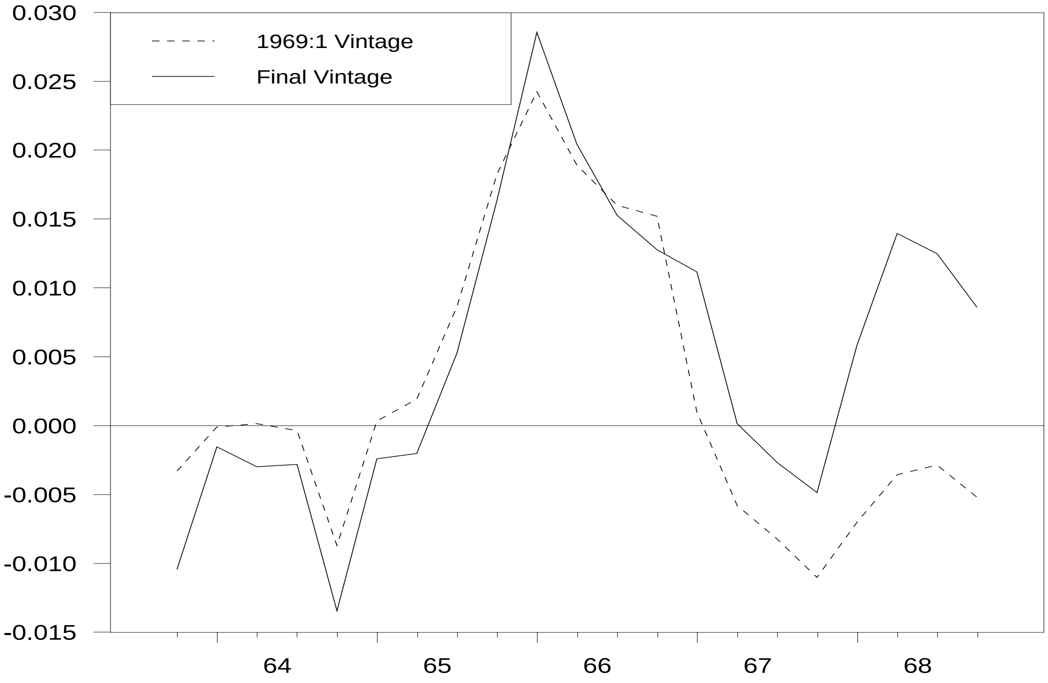
Figure 1
Real-Time and Final Hodrick-Prescott Output Gap


One Quarter

Four-Quarter Moving Average

Figure 2
Historical Vintages and Final Hodrick-Prescott Output Gap

1969Q1 Vintage



1996Q1 Vintage

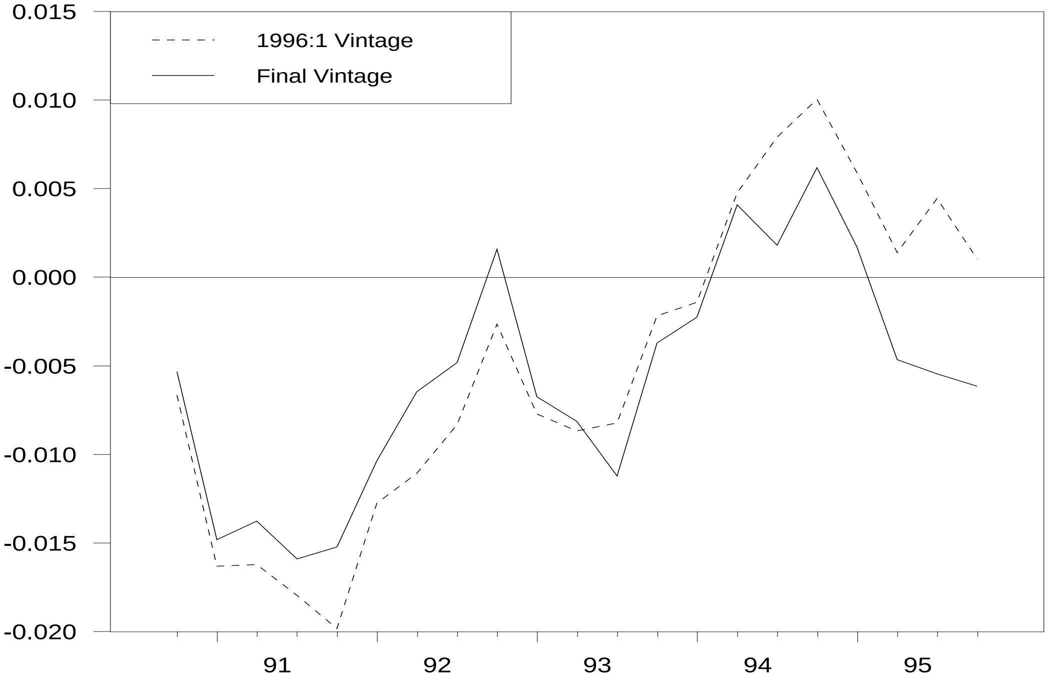
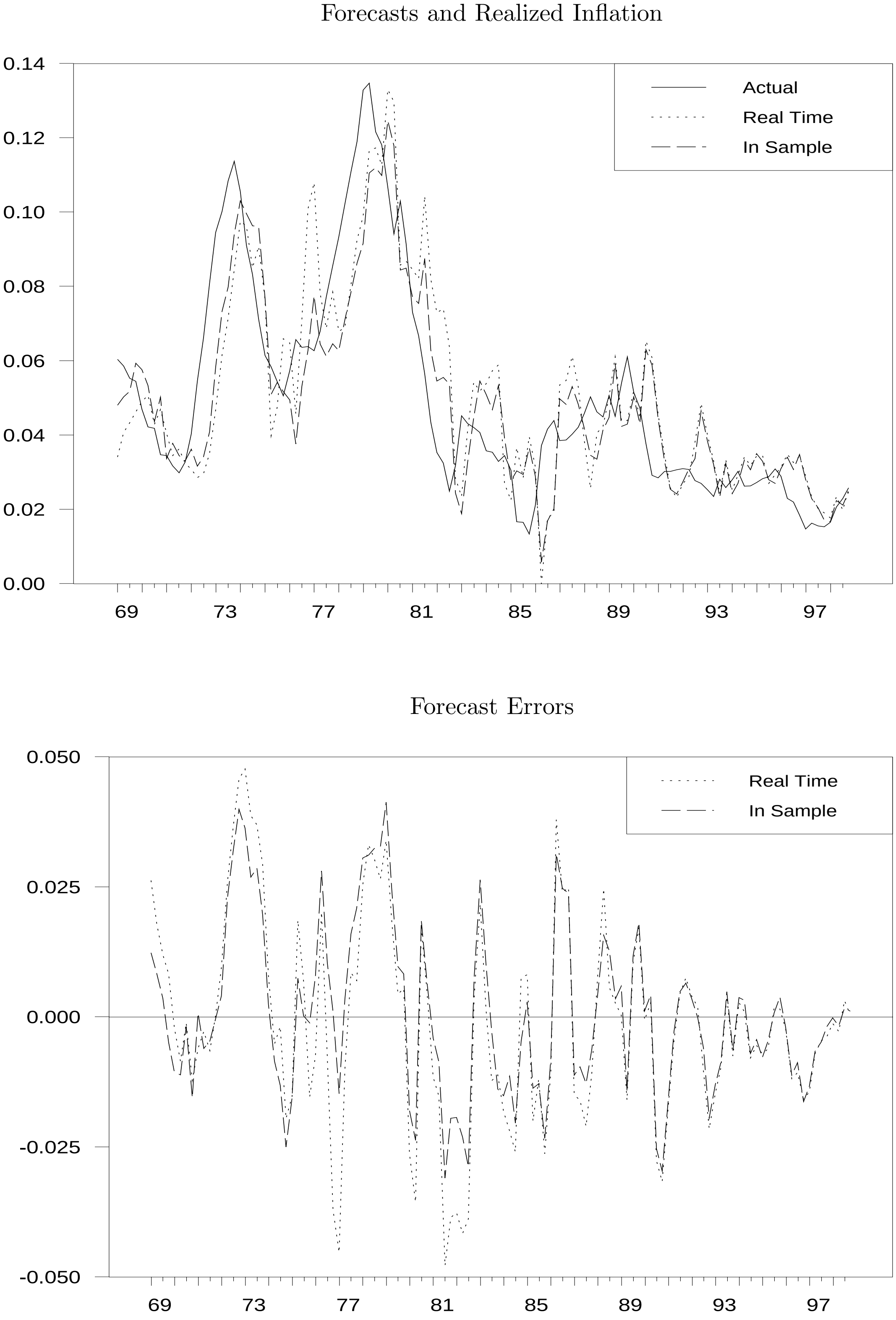


Figure 3

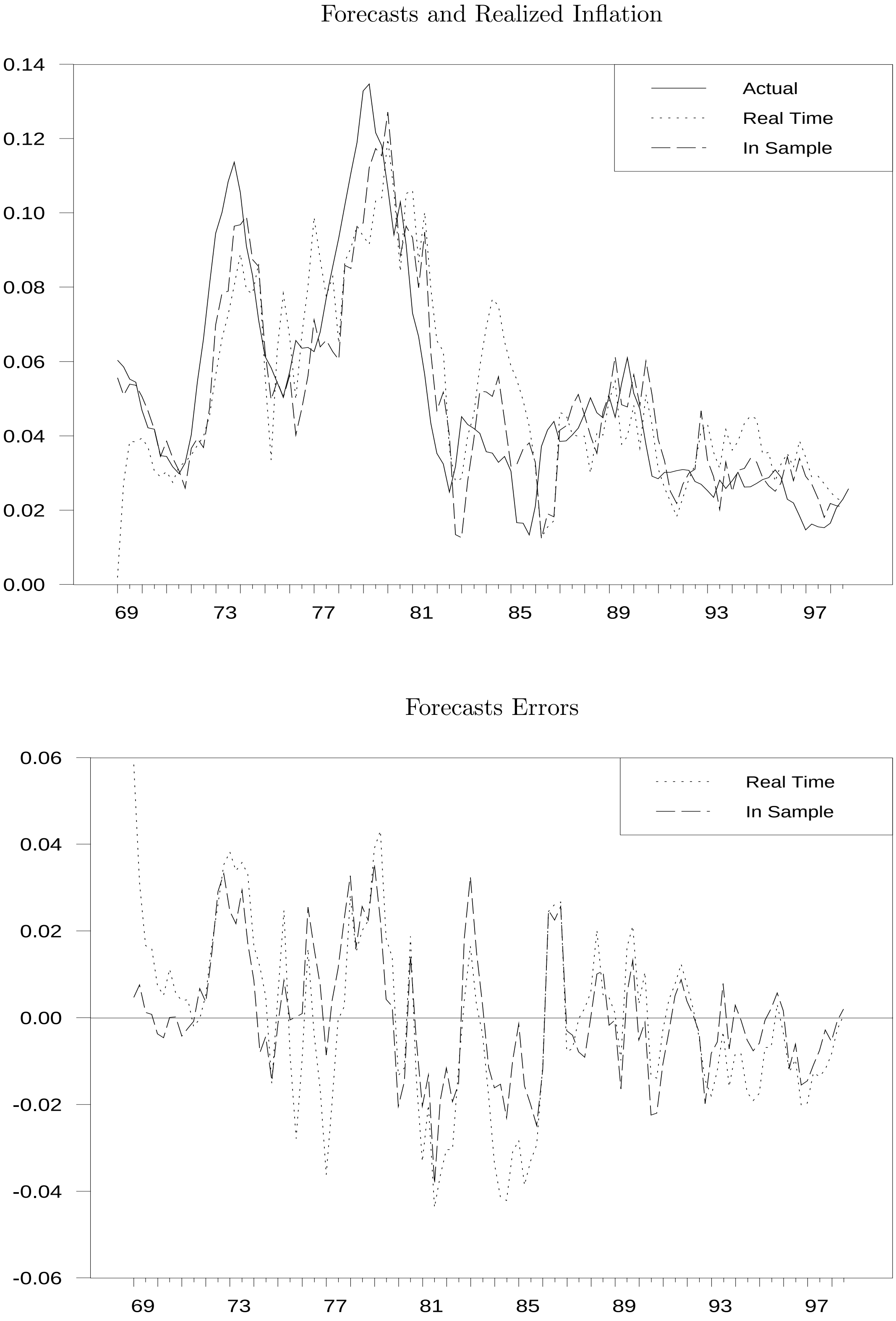
Univariate Inflation Forecasts



Forecasts and Realized Inflation

Forecast Errors

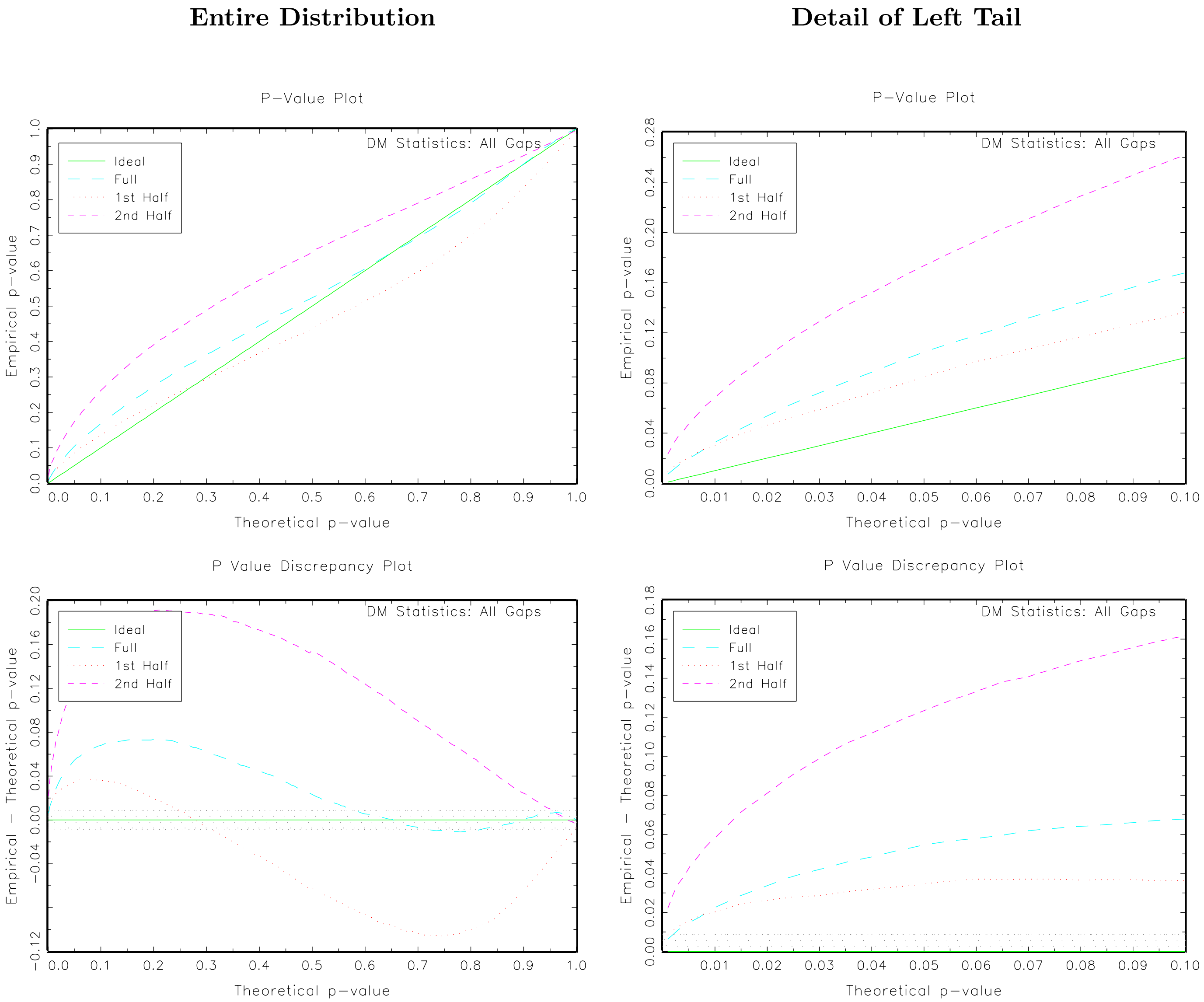
Figure 4
Inflation Forecasts and Errors with the Hodrick-Prescott Output Gap



Forecasts and Realized Inflation

Forecasts Errors

Figure 5
Boostrap p-values


Entire Distribution
Detail of Left Tail
P–Value Plot
DM Statistics: All Gaps
Ideal
Full
1st Half
2nd Half
Empirical p–value
Theoretical p–value
P Value Discrepancy Plot
Empirical – Theoretical p–value