

# *Discussion*

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## **1. Øyvind Eitrheim**

The paper by Pierre Siklos provides a comprehensive overview of developments relevant to inflation expectations (based on survey and non-survey measures of inflation forecasts) in nine economies over the past two decades. In his presentation he made it clear that he would use the terms inflation expectations and inflation forecasts interchangeably. I will mainly use the latter term in this discussion.

### **Key research questions**

The paper can be seen as a contribution to better understanding the driving forces behind inflation (and inflation dynamics) and the role of monetary policy. Specifically, it addresses the following important questions. Can shocks to relative prices explain inflation forecast differentials (relative to the United States)? What is the role of commodity prices and asset prices? Is inflation driven by global factors? Or, are idiosyncratic factors the dominant forces? And, what role does the monetary policy regime play, notably the adoption of inflation targeting (IT)?

### **The strategy of the paper**

The paper examines inflation forecasts for nine economies, five of which are pioneering IT countries (Australia, Canada, New Zealand, Sweden and the United Kingdom) and four non-inflation targeters (the euro area, Japan, Switzerland and the United States). For each economy, inflation forecasts are collected from many different sources (survey- and non-survey-based). These are all considered relative to a benchmark forecast for the United States. Data span the period 1990:Q1 to 2008:Q4.

There is a comprehensive discussion of developments over the past two decades, supported by nice descriptive graphs of actual inflation, commodity and asset prices, (one-year-ahead) inflation forecast persistence (scatter plots), (one-year-ahead) forecast disagreement relative to the United States. The figures show converging inflation rates over the sample, volatile commodity and asset prices, an increasing concentration of (one-year-ahead) inflation forecast persistence and declining (one-year-ahead) forecast disagreements relative to the United States (for ITers as well as for non-ITers).

The econometric analysis includes analysis of the convergence properties and determinants of convergence for (one-year-ahead) inflation forecasts relative to the United States. This is based on tests of whether or not the forecast differentials are stationary using (panel) unit root tests, (threshold) co-integration tests (allowing for asymmetry) and regression models estimated on panel data (OLS), which try to pin down the main determinants of convergence.

The paper reports four main findings. First, the adoption of IT has contributed to a narrowing of forecast differentials (*vis-à-vis* the US inflation forecast benchmark). Second, there are signs of asymmetric convergence towards US expectations (that is, stronger convergence for economies deviating further from the United States), mostly in the early part of the sample. Third, one needs to look beyond the mean of the forecast distribution to learn about the effect of the adoption of IT as well as the persistence of forecast differentials. And fourth, (de-trended) commodity and asset prices have become more important determinants of inflation forecasts in the second half of the sample (that is, from 1999 onwards).

## Questions and comments

This is a nice paper with many interesting empirical results. My questions and comments concern their robustness and fall into three main categories. First, there is the problem of model uncertainty and unknown instabilities. The usual suspects are parameter non-constancies, invalid parameter restrictions, and omitted information. Second, there is the problem of potential heterogeneity among forecasters, who may differ with respect to the information they have available to make their forecasts and their objectives, as well as their abilities, particularly across different forecast horizons. Third, what is the effect of adopting IT? Are we able to separately identify this policy effect?

Before I get to the details of these, let me mention a few more specific comments. First, I commend the author for the compilation of a very interesting international dataset. I understand that they have data for additional countries within the European Union (both within and outside the euro area); one could ask whether there would be a gain from adding data for more countries to the analysis. Second, Table 1C (unit root tests) needs clarification; critical values and/or indications of significance would help. And finally, why not include inflation forecast data for Norway, which has been an inflation targeter over the second half of the sample?

## Model uncertainty and unknown instabilities

The paper's focus on (relative) forecast differentials is problematic for several reasons. First, there is a concern that the models considered may suffer from unknown parameter non-constancies. Sub-sample evidence (Table 4) indicates that there has been a shift in the mean forecast differential (relative to the United States) between the two sub-periods. This shift may need to be parameterised to avoid an upward bias in the estimates of (relative) persistence in the full sample.

Second, the modelling of relative forecast differentials imposes potentially invalid restrictions on model dynamics. In particular, short-run homogeneity restrictions are imposed, whereby shocks to inflation expectations in any given country are restricted to have the same effects as shocks to US inflation expectations.

Furthermore, in order to account for the effects of shocks to inflation, one could alternatively analyse (relative) forecast errors instead of (relative) forecast

differentials. This would allow shocks to inflation in each economy and the United States to be addressed more explicitly.

Another aspect of model uncertainty stems from specification uncertainty. Other papers have cast a wider net and included more variables among the potential determinants of inflation; for examples, see Mishkin and Schmidt-Hebbel (2007) and Calderón and Schmidt-Hebbel (this volume). In both of these studies the authors argue that it is important to account for structural and institutional variables in an attempt to account for the determinants of inflation (forecasts). Pierre's paper finds that (de-trended) commodity prices and asset prices help to explain forecast differentials from 1999 onwards (and only for the mean of the forecast distribution, but I will come back to this in my discussion of forecaster heterogeneity). The HP-filter is used for de-trending but Pierre reports that the results for growth rates are similar. Further tests of robustness might consider using one-sided filters instead of the two-sided HP-filter.

## Heterogeneity among forecasters

Pierre aims to combine as many sources of inflation forecasts (survey- and non-survey-based) as possible. This is a strength since it allows for a wider information set but is also a potential weakness since there are more details to attend to. These issues are addressed by analysing the principal components of the inflation forecasts (Table 2). However, there are several potential problems with survey data (Figures 5 and 6). Some of the scatter plot clusters have a rather strange location which does not seem to have a straight forward interpretation. It would be worth discussing when the surveys contain news and when they may be contaminated by noisy observations. I would also like consideration to be given to the circumstances under which survey data are more informative (even superior) to data from other sources. In Table 1A it seems that the empirical results based on survey data tend to deviate from those based on non-survey data (and the joint dataset) and this could be discussed in the text.

Pierre applies the mean, MAX and MIN operators to represent the distribution of inflation forecast differentials. Interestingly, the statistical significance of some of the candidate determinants of forecast inflation differentials appears to depend critically on which operator is used. It seems reasonable, therefore, to analyse several measures extracted from the distribution of forecast differentials to take into account the heterogeneity among the forecasters and see how robust the results are. In future research, one might consider introducing a weighting scheme for forecasters, defining an *ensemble of forecasters* like in Ravazzolo and Vahey (this volume), or one could use *entropy measures* like the Kullback-Leibler information criterion to characterise the distribution of forecast differentials as suggested in Filardo and Genberg (2009).

Another issue is whether forecast disagreement is higher for longer horizons. This paper focuses solely on the one-year-ahead horizon but results in Lahiri and Sheng (2008), for example, indicate that forecast disagreement is higher for longer forecast horizons. Finally, one could also argue that forecast disagreement rises (for

a given horizon) in abnormal times and that there might be a need for extending the information set in this case.

## The role of IT adoption

I believe it is a useful empirical exercise to try to pin down the (partial) role of economic policy. This would indeed come in handy during periods of crisis since it would help debates of the type illustrated in Skånland (1989), where the ongoing banking crisis in Norway at the time was characterised as being the unfortunate consequence of ‘bad banking, bad policies, and bad luck’. Some authors argue that the role of ‘bad regulation’ should be added to this list in the aftermath of the current crisis. But how much weight should be put on each? There is a growing literature which examines the effects of adopting IT (for a range of different approaches, see Bernanke *et al* 1999; Ball and Sheridan 2004; Vega and Winkelried 2005; Mishkin and Schmidt-Hebbel 2007; and Filardo and Genberg 2009). Most of these studies find a significant and beneficial effect from adopting IT, although Ball and Sheridan find that adopting IT has an insignificant effect in a model which allows for mean reversion.

In Pierre’s paper, the significance of adopting IT seems to depend rather critically on how the information from the distribution of forecasts differentials is aggregated. The IT dummy variable is found to be not significantly different from zero when the regression is based on the mean of the forecast differentials, while it is negative and significantly different from zero for differentials based on the maximum and minimum inflation forecasts (taken to represent the most pessimistic and the most optimistic views among the forecasters, respectively). Two questions arise from this finding. Is this result robust to the choice of the control group, in this case the non-IT economies? Also, is this result robust to the removal of potential outliers among heterogeneous forecasters or robust to using alternative aggregators such as entropy measures (Filardo and Genberg 2009) or different weighting schemes (Ravazzolo and Vahey, this volume)?

In contrast to Ball and Sheridan (2003, 2004), Hyvonen (2004) finds that adopting IT leads to a significant decrease in inflation – based on an extension of the Ball and Sheridan dataset. But there are some caveats with respect to the robustness of this result. Mishkin and Schmidt-Hebbel (2007) found that the control group is critical for the statistical significance of IT. Vega and Winkelried (2005) propose using statistical methods like propensity score matching to define the most appropriate control group, and they found statistically significant and beneficial effects of adopting IT.

In summary, it would be of interest to see results for a larger group of countries. Pierre argues that IT adoption has led to a decline in forecast disagreement – but how can we be sure? The distinction between IT central banks and non-IT central banks may in practice be somewhat blurred. One can argue that central banks like the Swiss National Bank, the European Central Bank and the Federal Reserve all have a strong focus on controlling inflation, and that since both IT and non-IT economies have been reasonably successful in this regard, one may need to

go beyond the pioneers to analyse this. This would be in line with the results in Mishkin and Schmidt-Hebbel (2007), and also with the results in Calderón and Schmidt-Hebbel (this volume) who report results using inflation data (not inflation forecast data) from a much larger set of countries, and test for the (partial) effect of adopting IT using a wider information set including structural and institutional variables, among others.

In the empirical results in Table 3, Pierre allows for persistence by including lagged inflation differentials. Since the adoption of IT is potentially endogenous, it would of course be of interest to analyse whether the results hold when the model is estimated using IV methods (Mishkin and Schmidt-Hebbel 2007 are relevant in this respect), and the author might also want to apply robust standard errors as a safeguard against untested error heterogeneity.

## Conclusions

This is a nice paper with useful empirical evidence about topics highly relevant for policy-makers who need to understand the driving forces behind inflation (and inflation dynamics). Pierre has constructed an international dataset for inflation forecasts from a total of 16 economies (nine of which are included in the study) and from many different sources (both survey- and non-survey-based inflation forecasts). The results provide convincing evidence that there has indeed been convergence in inflation forecasts. But will that remain so? The question is: what drives this convergence and what role should be assigned to monetary policy? The adoption of IT seems to have worked quite well for many countries, but why? It seems that the jury is still out on many of these important research questions and I look forward to future research in this area, including from Pierre.

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## 2. General Discussion

The broad range of forecasts highlighted in the paper presented by Pierre Siklos generated much discussion among conference participants. A participant concurred that the paper's concern with disagreement over forecasts of inflation is a relevant policy consideration, and pointed to studies that find that the outliers for forecasts of inflation tend to be extreme relative to forecasts of other economic variables. The discussion went on to consider the present forecasts of inflation for the United States, which highlighted considerable disagreement regarding inflation over the next five years, though with the bulk of them implying some potential for deflation, whereas forecasts of inflation for the next ten years suggest inflation is still the longer-run concern. It was noted that it was difficult to interpret the implications of these observations for modelling and official forecasts. A subsequent comment suggested that the distribution of the forecasts might be skewed, which implied that it was worth examining non-normally-distributed forecast densities.

The role of the different motivations of forecasts received some attention. Some participants noted that some private-sector forecasters may want to gain notoriety by deviating from the general consensus – that is, their forecasts were essentially marketing tools for the financial institutions themselves. One participant thought that this was unfortunate to the extent that such forecasts might influence household and financial market expectations of inflation, potentially in an adverse way. Another participant added that it was important to understand the source of the inflation forecasts when evaluating their usefulness.

A participant suggested that the regression analysis of Pierre Siklos's paper should include a variable to account for differences in the variance in actual inflation across economies. Following on from this, a participant thought that current forecast dispersion is potentially useful conditioning information for forecast disagreements at longer horizons. In his response, Pierre Siklos mentioned that he uses the kurtosis of inflation in his specification to try to capture some of the variance in inflation forecasts.

A participant asked if there was evidence that the monetary policy regime influenced the forecasts, in particular, whether inflation targeting contributes to a narrowing of the distribution of forecasts. Related to this point, it was suggested that the paper could investigate whether external forecasts converge to published

central bank forecasts of inflation. This might also shed some light on issues related to the effectiveness of central bank communication.

The use of the inflation performance of the United States as the benchmark in Pierre Siklos's model was raised by a few participants. One participant wondered whether countries that peg their currencies to the US dollar also have inflation rates that are close to those of the United States. This line of reasoning suggested that the mix of countries in the dataset might be a relevant consideration. Pierre Siklos responded by saying that the comparison between the United States and other economies was designed to capture global factors that affect inflation forecasts, and that replicating the analysis with Europe as the benchmark yields the same results.