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Commentary

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Stephen G. Cecchetti's article, "Measuring Short-Run Inflation for Central Bankers," develops and extends several themes on the problem of how best to measure inflation. He and Michael F. Bryan of the Cleveland Fed have explored these issues during the past few years in a series of papers (Bryan and Cecchetti, 1993, 1994, 1995, 1996). The specific objective of the current article is to examine the severity of the noise and bias problems in price measurement and to suggest some solutions. The bulk of the article (and most of the interesting results in it) concerns the issue of reducing noise. Three novel results are presented:

- First, given Bryan and Cecchetti's (1996) documentation of substantial kurtosis in the cross-section distribution of price changes, it is no longer obvious that the sample mean is the best (most efficient) estimator of the population mean rate of inflation. Rather, limited-influence estimators, such as trimmed means or the weighted median, may be more efficient estimators of the population mean. Cecchetti shows this by Monte Carlo experimentation, demonstrating in the process that the usual noise-reducing strategy of simply excluding food and energy prices is inferior to the limited-influence alternatives.

- The second key result is that, of the various limited-influence estimators of the mean that are considered, the 10 percent trimmed mean seems to do best at tracking "core" inflation [defined either as a 36-month moving average or as the Bryan-Cecchetti (1993) Dynamic Factor Index (DFI) measure of inflation].

- The third major finding, and perhaps the most surprising, is how little noise is eliminated by seasonal adjustment.

The bottom line for central bankers interested in measuring short-run inflation is that looking at month-to-month changes in the seasonally adjusted consumer price index (CPI) excluding food and energy does not give a very accurate picture of how trend inflation is changing over time.

As this article is very much the latest installment in an ongoing research program, I will take some liberties and direct many of my comments and questions at the overall program rather than confining myself to the specifics of this one article. The Bryan-Cecchetti research program on the measurement of inflation is part of a broader revival of interest in alternative statistical or stochastic approaches to the measurement of inflation. This revival dates back about 15 years or so and seems to have been motivated by at least two issues. The first issue is the desire to be able to associate some sort of confidence interval with measures of change in the aggregate price level. This is usefully summarized in the recent book by Selvanathan and Prasada Rao (1994). The second issue is the reemergence of a "macro" or "quantity-theoretic" perspective on inflation measurement that views the construction of inflation estimates as a signal-extraction problem, motivated at least in part by Lucas (1973). Bryan and Cecchetti's work on this issue has received the most attention by far in policymaking circles.

My objectives will be to

- try to relate Cecchetti's analysis to some of the older and newer stochastic approaches to price-level measurement,

- question the legitimacy of interpreting these alternative measures as better estimates of what some economists

refer to as monetary inflation, and

- give an example of the performance of an alternative inflation estimate over the past 20 months.

ISOLATING THE COMMON COMPONENT OF PRICE CHANGES

The common point of departure for the new stochastic approach to inflation measurement is the following simple model of individual price changes:

$$(1) \qquad \dot{p}_{i,t} = \dot{P}_t + \dot{x}_{i,t},$$

where we define $\dot{p}_{i,t} = \ln(p_{i,t}) - \ln(p_{i,t-1})$. This model defines the rate of change in the price of an individual commodity as consisting of an aggregate inflation component, $\dot{P}_t$, and a relative price-change component, x_{it}. The object we are interested in is $\dot{P}_t$—the common trend in all prices and what Cecchetti describes as the proper analog to changes in the aggregate price level of macroeconomic theory.

If we are willing to assume that the "error" or relative price term in Equation 1 is normally distributed, with mean and variance given by

$$(2) \qquad E\left(\dot{x}_{i,t}\right)=0, \quad Var\left(\dot{x}_{i,t}\right)=\sigma_t^2,$$

it is straightforward to show that the maximum-likelihood estimator of the inflation rate, $\hat{\dot{P}}_t$, is a simple unweighted average of the rates of change of the individual price series:

$$(3) \qquad \hat{\dot{P}}_t = \frac{1}{N}\sum_{i=1}^{N} \dot{p}_{i,t}.$$

We can interpret this model (Equations 1 and 2) as a simple static factor model where the unobserved component or factor, $\hat{\dot{P}}_t$, is identified by assuming that "inflation" is orthogonal to relative price changes *contemporaneously*. By construction, the estimated rate of inflation $\hat{\dot{P}}_t$, will be orthogonal to the estimated relative price changes, $\hat{\dot{x}}_{i,t}$. Such a measure of inflation was proposed by Jevons and Edgeworth more than 100 years ago. Exponentiating both sides of the above expression gives us

$$(4) \qquad \exp\left(\hat{\dot{P}}_t\right)=\prod_{i=1}^{N}\left(\dot{p}_{i,t}\right)^{1/N},$$

where the term on the right is generally known as Jevons' geometric-mean price index.

This set of equations (1 and 2) is all we need to motivate the use of limited-influence estimators of the inflation rate. The evidence in Bryan and Cecchetti (1996) suggests that the distribution of price changes for the components of the CPI is fat-tailed, so the distribution is not adequately characterized by its first two moments as in Equation 2.

Now a potential shortcoming of the above estimate of the inflation rate is that the prices of all goods are treated as being equally important in estimating the overall inflation rate. A more appealing approach would be to weight the individual price changes according to their importance, somehow defined. Thus it might be argued that an estimator of the form

$$(5) \qquad \hat{\dot{P}}_t = \sum_{i=1}^{N} w_i \dot{p}_{i,t},$$

is more appropriate, where the w_i denotes some set of weights to be attached to the individual price observations in estimating the overall inflation rate. To this end, Clements and Izan (1981) showed that if we replace the assumptions about the relative price terms in Equation 2 with

$$(6) \qquad E\left(\dot{x}_{i,t}\right)=0, \; Var\left(\dot{x}_{i,t}\right)=\sigma_t^2 / w_i,$$

we obtain Equation 5 as the maximum-likelihood estimator of the common trend in prices, where the w_is are budget shares of some sort. Clements and Izan are very explicit about interpreting $\dot{P}_t$ as "the common trend in all prices (due to, e.g.,

monetary expansion)" (Clements and Izan, 1981, p. 745), while the x_{it}s represent everything else. The key difference with the first model is the assumption that the variance of the ith relative price is inversely proportional to its budget share or weight w_i. Clements and Izan motivate this assumption by arguing that "...it seems reasonable to postulate that the collection agency invests more resources in sampling the prices of those goods more important in the budget" (Clements and Izan, 1981, p. 745). This is a rather odd motivation for the distributional assumption. Later, Clements and Izan (1987) justified this variance assumption on the grounds that, as a commodity becomes more important in consumers' budgets, there is less scope for relative price changes. Exponentiating both sides of Equation 5 gives us

$$\exp\left(\hat{P}_t\right) = \prod_{i=1}^{N} \dot{p}_{i,t}^{w_i}, \tag{7}$$

which reduces to a Tornqvist index if we assume that $w_i = (\frac{1}{2})w_{i,t-1} + (\frac{1}{2})w_{i,t}$.

The assumption in this second model (that the variance of the ith price change is inversely proportional to its weight) is unpalatable and does not match what we observe in reality. If we simply look at the 36 components of the CPI that Cecchetti uses as raw price data in his article, we see essentially no relationship between the variance of the individual price changes and their expenditure weights.[1]

But why confine ourselves to using budget shares for the weights? The use of budget shares as the weighting scheme has some basis in the theory of the (atemporal) cost-of-living index. However, if we are going to take seriously the idea that the price level of monetary theory differs from that of the cost-of-living index, it is no longer obvious that we would want to use similar weighting schemes. A weighting scheme that might appeal to a monetary economist is one that weights prices on the strength of their inflation "signal." The "excluding food and energy" approach to estimating "core" inflation is in this spirit, where we attach zero weight to food and energy prices on the assumption that they are so volatile they convey no information about the underlying inflation rate. A more general scheme for operationalizing this idea would be to set the weights as follows:

$$w_i = \frac{\frac{1}{\sigma_i^2}}{\sum_{i=1}^{N} \frac{1}{\sigma_i^2}}. \tag{8}$$

In other words, choose weights for the various individual prices that are inversely proportional to the volatility of those prices. A weighting scheme along these lines was suggested by Dow (1994) (who termed the resulting measure of inflation a Variance Weighted Price Index) and Diewert (1995) (who termed the resulting index neo-Edgeworthian).

A fundamental objection to the foregoing models (and the various trimmed-mean estimators proposed by Cecchetti in his article) is that levied by Keynes against Jevons' and Edgeworth's attempts at measuring inflation. Namely, they require that the systematic component of each price change be the same, precluding any long-term changes in relative prices. As we know from recent experience with computer prices, this assumption or requirement is somewhat at odds with reality.

In response to Keynes's criticism, Clements and Izan (1987) proposed the following model:

$$\dot{p}_{i,t} = \dot{P}_t + \dot{x}_{i,t} = \dot{P}_t + r_i + \varepsilon_{i,t}, \tag{9}$$

with the assumptions

$$E\left(\varepsilon_{i,t}\right) = 0 \quad Var\left(\varepsilon_{i,t}\right) = \sigma_t^2 / w_i, \tag{10}$$

along with the identifying assumption that

$$\sum_{i=1}^{N} w_i r_i = 0. \tag{11}$$

[1] The correlation is -0.15 for the 36 components of the CPI used by Cecchetti when we correlate the 1985 expenditure weights with the standard deviations of the 36 series during the 1967:02-1996:08 period.

In this model the expected change in the ith relative price, $E(\dot{p}_{i,t} - \dot{P}_t)$, is simply equal to r_i. The maximum-likelihood estimator of the inflation rate is simply

$$\hat{P}_t = \sum_{i=1}^{N} w_i \dot{p}_{i,t}, \tag{12}$$

the same as Equation 7.

Diewert (1995) criticizes this approach on the grounds that the identifying assumption is incredible: Specifically, it is unlikely that the weights appearing in Equation 10 would be the same as the weights appearing in Equation 11. Diewert also challenges the reasonableness of assuming that the systematic component of relative price changes, r_i, is constant over time.

The preceding can be considered essentially static approaches to the problem of inflation measurement. The DFI proposed by Bryan and Cecchetti is readily seen to be an extension of these simpler (static) models.[2] The DFI model starts with Equation 1, written in vector form:

$$\dot{p}_t = \dot{P}_t + \dot{x}_t, \tag{13}$$

where $\dot{p}_t = [\dot{p}_{1,t}, \dot{p}_{2,t}, \ldots, \dot{p}_{N,t}]'$, $\dot{x}_t = [\dot{x}_{1,t}, \dot{x}_{2,t}, \ldots, \dot{x}_{N,t}]'$.

Identification of the common inflation component is achieved by postulating time-series processes for the inflation and relative price-change components of individual price changes as follows:

$$\psi(L)\dot{P}_t = \delta + \xi_t, \tag{14}$$

$$\theta(L)\dot{x}_t = \eta_t, \tag{15}$$

where $\psi(L)$ and $\theta(L)$ are matrix polynomials in the lag operator—L— and ξ_t and η_t are scalar and vector *i.i.d.* processes, respectively.[3] Now the common element $\dot{P}_t$ is identified by assuming that it is uncorrelated with the relative price disturbances at *all* leads and lags (or rather the common element is defined to be that component of overall inflation which is uncorrelated with relative price disturbances at all leads and lags). This is clearly a much more restrictive assumption than that required to identify the simple static models above. It raises the question of what (if anything) do we get in return. Estimation yields

$$\hat{P}_t = \hat{W}(L)\dot{p}_t, \tag{16}$$

where $\hat{W}(L)$ is a matrix polynomial in the lag operator—L. Again, this model is susceptible to Keynes's criticism about requiring there be no relative price changes. It is possible to extend the model along the lines suggested by Clements and Izan (1987) by following Stock and Watson's (1991) modification of their coincident indicator model to allow for different trends in the various indicators. But this is as far as it is possible to go: The trends in relative prices cannot be time varying.

CRITIQUE

We return to the question, What is the estimated inflation rate $\hat{p}_t$ (either static or dynamic) supposed to be measuring? I believe that what Cecchetti and the other recent contributors to this literature (especially Dow, 1994 and Clements and Izan, 1987) have in mind is some concept of "monetary" inflation that ought to be of concern to monetary policymakers and that, in principle, is not necessarily the same thing as changes in the cost of living. Cecchetti explicitly distinguishes between two concepts of bias: the deviation of a measure of the price level from a (static, value-theory based) true cost-of-living index (the way we usually think about bias) and the deviation of a price statistic from the true monetary rate of inflation. Implicitly there is some notion here that the price level of monetary theory differs from the cost-of-living index of value theory and that it is the rate of change of the former that ought to be of concern to monetary policymakers. I see no problem with this in principle. However, it is difficult to make a rigorous formal argument.

[2] Bryan and Cecchetti's DFI model is itself based on the Stock and Watson (1991) model of coincident economic indicators.

[3] Bryan and Cecchetti's DFI model includes as a special case the common factor model of Dow (1994), obtained by restricting $\psi(L) = 1 - \psi_1 < L$ *and* $\theta(L) = 1$.

Perhaps the easiest way to start thinking about the possibility of there being two such inflation concepts is to imagine a world as that envisaged by Black (1970). There, the financial system has developed to the point (and is sufficiently deregulated) that no well-defined demand for base money exists. In such a world, the price level of monetary theory would be indeterminate, but arguably there would still be such an object, as a well-defined cost-of-living index, and it would be possible to measure year-to-year (or month-to-month) changes in the latter.

The monetary theory of price-level determination most advocates of the alternative approaches to price level measurement have in mind is some sort of quantity theory of money. Perhaps it is no surprise, therefore, that one of the most important twentieth-century advocates of the quantity theory of money, Irving Fisher, was also a prominent contributor to index number theory. In *The Purchasing Power of Money*, Fisher (1920) argued that we ought to look at a wide range of prices in measuring the aggregate price level. Fisher was quite explicit in stating that it is not just the price of consumption goods that should be considered when trying to determine whether money is losing its value, but rather the average price of *all* transactions conducted through the medium of money:

> Perhaps the best and most practical scheme [for the construction of an index number] is that which has been used in the explanation of *P* in our equation of exchange, an index number in which every article and service is weighted according to the value of it *exchanged* at base prices in the year whose level of prices it is desired to find. By this means, goods bought for immediate consumption are included in the weighting, as are also all durable capital goods exchanged during the period covered by the index number. What is repaid in contracts so measured is the same *general purchasing power*. This includes purchasing power over everything purchased and purchasable, including real estate, securities, labor, other services, such as the services rendered by corporations, and commodities (pp. 217-18).

Fisher noted, however, the practical difficulty of collecting data on all relevant prices and concluded that:

> It is, of course, utterly impossible to secure data for all exchanges, nor would this be advisable. Only articles which are standardized, and only those the use of which remains through many years, are available and important enough to include. These specifications exclude real estate, and to some extent wages, retail prices, and securities, thus leaving practically nothing but wholesale prices of commodities to be included in the list of goods, the prices of which are to be compounded into an index number (pp. 225-26).

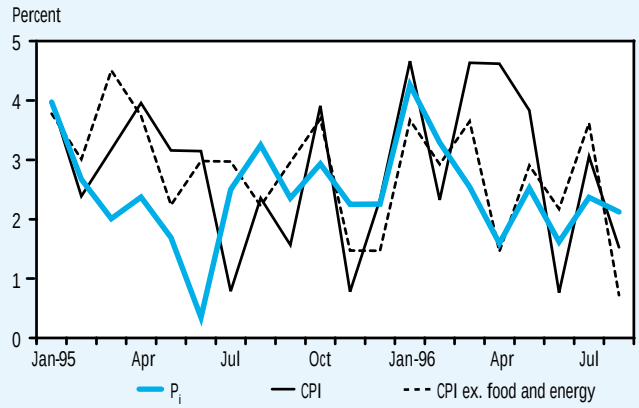
Fisher's conclusion that the wholesale price index might be an adequate indicator of movements in the general price level thus seems to have been motivated in large part by the problem of a constant-quality basket of goods to include in an index.[4]

I am sympathetic to the idea that the price level of monetary theory need not be the same as the price index, which tracks movements in the cost of living. Yet, if we are to accept this as the motivation for the framework employed by Cecchetti, and if we are willing to accept the transactions version of the quantity theory as the appropriate alternative theoretical framework, a number of questions are raised. First, the quantity-theoretic approach suggests that all prices be included in the index and not just the prices of final goods. Thus Dow's (1994) inclusion of a number of the components of the producer price index (specifically, the prices of intermediate and crude goods) in the observation equations of his Kalman filtering problem is more in the spirit of the quantity-theory approach than Bryan

[4] Parenthetically, we might note that Fisher (1920) was also quite enthusiastic about the use of limited-influence estimators or trimmed means as a solution to the index number problem as the following quote indicates: "For practical purposes, therefore, unless the expense and labor of computation can be disregarded, the median (with its two neighboring quartiles) is recommended, with a simple system of weights (whole numbers) based on expenditures, and changing from time to time for the sake of making better year-to-year comparisons" (p. 429). Fisher also notes that the median was earlier endorsed by Edgeworth, whose endorsement was based on the asymmetry in the distribution of price changes.

Figure 1

Recent CPI Inflation



and Cecchetti's (1993, 1994, 1995, 1996) exclusive focus on the components of the CPI. Second, the quantity-theory perspective dictates that we should include only the prices of those goods exchanged via monetary transactions (however money is defined), raising a whole new class of thorny measurement and conceptual issues. Third, and perhaps most important, the quantity-theoretic perspective requires a treatment of durable goods prices very different to that employed in the traditional cost-of-living approach. For measuring the cost-of-living, theory dictates that we want to price the service flow from durable goods rather than the purchase price of the good itself.[5] However, from the quantity-theoretic perspective, just the opposite would be the case. We would want to include the purchase price of a durable asset and forget about the service flow. These points are relevant whether we adopt a static or dynamic approach to measuring the common inflation factor in individual price changes.

WHAT DOES THE SIGNAL-EXTRACTION APPROACH TELL US ABOUT RECENT INFLATION?

Given Cecchetti's article title and the topic of this conference, I believe it is worthwhile to determine what alternative approaches to inflation measurement are telling us about the recent behavior of inflation in the United States. This is doubly apt in light of the recent tensions that reportedly have surrounded discussions of monetary policy at the Federal Open Market Committee. Although I did not have time to replicate the DFI or limited-influence estimators Cecchetti discussed, I did review the variance-weighted or neo-Edgeworthian index suggested by Dow (1994) and Diewert (1995). Formally, this measure of the inflation rate is constructed as the solution to the following nonlinear system of equations:

$$\hat{P}_t = \frac{\sum_{i=1}^{N} \frac{\dot{p}_{i,t}}{\hat{\sigma}_i^2}}{\sum_{i=1}^{N} \frac{1}{\hat{\sigma}_i^2}} \tag{17}$$

$$\hat{\sigma}_i^2 = \frac{1}{T}\sum_{t=1}^{T}\left(\dot{p}_{i,t} - \hat{P}_t\right)^2 . \tag{18}$$

I estimated the neo-Edgeworthian index number using the 36 components of the CPI used by Cecchetti as raw price data, with a sample from January 1967 through August 1996. Figure 1 plots the behavior of the inflation rate as measured by the index during the past 20 months, along with the CPI and the CPI excluding food and energy.[6] All three indexes show the spike in inflation in January 1996. But the variance-weighted measure shows a fairly rapid recovery and has hovered in the 1.5 percent to 2.5 percent range for the past six months or so. It is also (not surprisingly) less volatile than the other two measures.

CONCLUSION

Cecchetti's article is the latest installment in an ongoing research program that Cecchetti has been conducting with Mike Bryan of the Cleveland Fed. I come away

[5] The shift to a rental equivalence treatment of housing in the CPI was thus deemed to be a major improvement in making that series better approximate the true cost of living.

[6] The standard deviations of the three series during the 1967:02-1996:08 sample period are 2.062E-03, 3.067E-03, and 2.637E-03, respectively.

with a sense that the traditional ("ex. food and energy") approach to measuring "core" inflation is not without pitfalls and that alternative estimates of the core inflation rate may give us more precise and timely information about shifts in the trend. This article, and the research program more generally, raises some important questions that need to be addressed at some point, but it holds substantial promise for deepening our understanding of the issues surrounding the measurement of inflation. Foremost among the questions is the establishment of a tighter link between these alternative methods of aggregating the rates of change into a measure of overall inflation (whether using weighted or unweighted averages, trimmed means, or the DFI) and the monetary theory of price-level determination that supposedly motivates the use of these methods. If the theory of price-level determination that underlies these approaches is a transactions version of the quantity theory, then we ought to be looking at a wider range of prices and treating the prices of durable goods very differently than they are treated in measuring the cost of living.

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