MONITORING BUDGET DEFICITS WITH TIME SERIES MODELS: SOME OBSERVATIONS

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Abstract

The Ministry of Finance had come up last year with a method of determining normative levels of budget deficit at the end of each month of the fiscal year, using time series models of the Box-Jenkins type. Recently, Madhur and Wadhwa have contested the validity of the model specification. This note attempts to clear some of the misunderstandings expressed by Madhur and Wadhwa regarding the model used in the Technical Note. In brief, it shows that the criticism levelled against the model is not quite warranted and the points raised are inconsequential.

MONITORING BUDGET DEFICITS WITH TIME SERIES MODELS SOME OBSERVATIONS.

I. Introduction.

In an attempt to closely monitoring the Central Government Budget deficit, the Ministry of Finance had come up last year, with a method of determining normative levels of budget deficit at the end of each month of the fiscal year, 1990-91 against which the actual deficit can be assessed (Government of India, 1990). The Technical Note (TN) considered several alternative methods of deriving the monthly norms and proposed the use of time series models of the Box-Jenkins type, for the purpose. As an illustration, the authors of the TN fitted a seasonal Auto-Regressive Integrated Moving Average (ARIMA) model to monthly deficit data over a ten-year period (April, 1980 to March, 1989), and found that within this class of models, a particular specification comprising a secular component and a seasonal component, was more suited for the purpose than other models considered.

Prior to the fitting of the model, the data were subject to two transformations: The first one, although not explicitly stated in the TN, follows from the non-uniform nature of data series collected by the Ministry, which were in the form of month-end positions of the yearly budget deficits. Thus, although the March-end figure might be non-zero for a year, the starting figure for the next year was set at zero, which is consistent with the way the yearly budget deficit is commonly viewed. For the empirical exercise, these month-end series were converted into a uniformly defined monthly change series so that each data point denoted the deficit incurred during the corresponding month. Further, the data series were transformed into a 12-month differenced series as they were found to be yearly non-stationary as revealed by their plot and also confirmed by the sample autocorrelation function (SACF). The fitted model explained over a half of the past movements in terms of two yearly, and one monthly, autoregressive lags.

Recently, Madhur and Wadhwa (1991) have contested the validity of our model specification used in the TN. Their criticism pertains mainly to three aspects of the TN: (a) the choice of the Box-Jenkins models in preference to other types of models, (b) the forecasting ability of the model used, and, (c) the exact specification of the model. In particular, they have questioned the second stage (12-month) differencing of the series, alleging that it was done without conducting any rigorous scientific tests for the existence of stationarity of the monthly deficit series. In an attempt to re-do the empirical exercise, they have conducted certain statistical tests for the existence of stationarity in the series. However, these tests are recommended in the literature in the context of non-multiplicative autoregressive linear models, and therefore, their validity for multiplicative models such as the one used in the TN is debatable. On the basis of these misplaced tests they have wrongly concluded that the yearly differencing of the deficit series as done in the TN was not required. Accordingly, they have re-specified the model without differencing in terms of one yearly lag and one monthly lag, and claimed that the modified version fitted (on fewer than available observations) and forecasted better than the one used in the TN.

The objective of this note is to clear out some of the misunderstandings expressed by Madhur and Wadhwa (MW) regarding the model used in the TN. In brief, the criticism levelled against the TN by MW is not quite warranted and the points raised are inconsequential. Their modification of the model is based on misspecified stationarity tests, and as such, does not connote substantive modification, let alone improvement, of the model used in the TN. In what follows, I shall elaborate on these points.

The study plan is as follows. Since it is the third point of criticism that led MW to look for an alternative specification, we consider it as a substantive point, and devote the next two sections, 2 and 3, to dwell on it. Section 2 gives reasons as to why we resisted from conducting these `formal´ tests for the TN. An important reason was that these tests cannot be applied to multiplicative models without substantial modifications. This point is elaborated in section 3 and the possible modifications needed are indicated. Section 4 takes a critical look at the alternative specification proposed by MW, where its validity is questioned by arguing that the tests leading to the modified specification are misplaced and therefore, it does not stand to reason. Then, the remaining two points of criticism are taken up for clarification in section 5. And section 6 contains the concluding remarks.

2. Choice of Stationarity Testing Methods in the TN.

The critics felt that the use of the twelfth differenced series of the monthly deficit as the dependent variable without conducting stationarity tests such as the Dickey-Fuller test led to overdifferencing and, as a result, misspecification of the TN model.

As the critics are well aware, the need to include a differencing factor in Box-Jenkins models arises if the autoregressive filters in the model are non-stationary, (nevertheless, homogenious). It is true that occasionally, in *borderline* cases, the `traditional' or `informal' methods employed for detecting non-stationary behaviour, such as examination of the plot of the series or its differences, inspection of the sample autocorrelation function (SACF), or informed inspection of a fitted model of a pre-determined specification for a unit root, might make researchers face certain difficulties. These ambiguities, *albeit* rare, necessitated the search for alternative test procedures, generating sizeable literature.

Yet, for the empirical exercise in the TN, we had relied upon the SACF of the monthly (redefined) budget deficit series, rather than the more formal tests mentioned by the critics, for the following reasons.

First, recourse to such tests was advocated in the literature mainly to such occasions where conventional tools cannot resolve the question. However, in the case of the TN, the plot of the SACF (Fig. 1) left little doubt regarding the non-stationary nature of the series, and clearly indicated significant autocorrelations for 12th, 24th and 36th degrees, while the intra-year autocorrelations are not significant. The failure of the yearly autocorrelations to dampen quickly, prompted us to suspect non-stationary behaviour of the budget deficit series between years.

Second, the non-traditional methods are not free from statistical ambiguities. Their precarious nature is clear from the caveats put on their use by those who developed them. Most part of the theory underlying these tests applies for deciding the last degree of differencing. While reviewing these tests, Dickey, Bell and Miller (1986) make it amply clear - "Before proceeding, we need to make an important qualification. The theory we shall discuss applies only to the case $d=1$ (first differencing), that is between one and no differences. Y_t (the series under study) could be an already transformed series including the important case of seasonally differenced series.... Thus, we assume that the other techniques mentioned (informal examination of plots, ACF's, and finite models) or other knowledge about the series can be used to discover all differencing factors except the last ∇ (difference)" (Dickey, Bell and Miller, 1986, p.12).

To be more specific, an important limitation of the Dickey and Fuller (1979) test is that the order of the AR and MA polynomials, even for a non-multiplicative BJ model, should be known prior to determining the degree of differencing, which is usually not the case. Later, although Said and Dickey (1984) developed a test by showing that it is possible to approximate an ARIMA (p,1,q) by an autoregression whose order is a function of the number of observations, the disadvantage, however, is that the approximate autoregression contains a number of parameters which are of `nuisance' value, yet, are to be estimated along with the first order coefficient. The alternatives suggested by Phillips (1987), and Phillips and Perron (1988) that do not need prior knowledge of p and q, and also avoid using a long autoregression recommended by Said and Dickey, still require approximations for truncation of lags in estimating the population error variance, that is essential for computing the z-statistic.

Third, in most cases, the test statistics are obtained by OLS regressions of specifications involving lagged dependent variables. If the model also consists of a moving average process, OLS might yield biased coefficient estimates.

Further, economic reasoning also makes it sensible to suspect non-stationarity in the yearly budget deficits atleast due to the price effect, which cannot always be taken care of by incorporating time trend variable in the model.

Thus, the alternative methods of determining the degree of differencing are not without limitations, and a foolproof method still eludes researchers.

The implications of wrongly testing for the unit root models were studied by Nelson and Kang (1984) and Plosser and Schwert (1978). It was shown that out of the three distinct possibilities *viz.*, differencing, removal of linear trend, and doing nothing, the last alternative, namely, doing nothing when differencing is needed can have dire consequences, frequently leading to falsely significant regresssions of non-stationary series on time and on other independent non-stationary time series. On the other hand, the consequences of unnecessary differencing were shown to be far less serious: inefficient, though unbiased and consistent, parameter estimates.

In any case, over-differencing is not as dangerous as under-differencing as brought out clearly by the past studies. In fact, the designers of the stationarity tests over the years have sought to build a bias in favour of over-differencing. While surveying the literature on unit root testing, Dickey, Bell and Miller makes it clear in no unambiguous terms: "We do this because of what we perceive to be the relative importance of the two possible errors in deciding on differencing. Failure to include a differencing operator when it is needed results in bounded forecast intervals that must eventually be too narrow, giving unreasonable confidence in the forecasts, especially the long-term forecasts. This can be especially true if a polynomial trend plus stationary error model is used when diferencing is needed. Even if the polynomial trend fits well over the span of the observed data, extrapolating it implies a strong assumption about the future, and this may well produce highly unrealistic forecasts and forecast intervals. It is also quite possible for the polynomial to fit poorly at the last data points, resulting in poor short term forecasts. On the other hand, differencing when a ∇ is not needed is unlikely to have serious consequences. Such overdifferencing can even produce forecast results equivalent to those from a model without a ∇ (Harvey, 1981). Overdifferencing can also sometimes be detected and corrected at the modelling stage (Abraham and Box, 1978). At worst, use of ∇ when $1-\rho B$ with $\rho<1$ is more appropriate will tend to produce more conservative forecast intervals." (Dickey, Bell and Miller, 1986, p.16).

The single most important reason as to why we did not resort to these formal statistical tests was the ambiguities posed by them for multiplicative models. The difficulty of applying these tests to multiplicative models will be clear from the very design of such models as can be seen in the next section.

3. Stationarity Testing in Multiplicative Models.

In general, a seasonal multiplicative model ARIMA (P,D,Q)*(p,d,q) represents the process

$$a_P(l).\phi_p(\lambda)\ (1-l)^D.(1-\lambda)^d\ x_t=b_Q(l).\theta_q(\lambda).\varepsilon_t \qquad (4)$$

where $a_P(l)$, $\phi_p(\lambda)$, $b_Q(l)$, and $\theta_q(\lambda)$, are polynomials in the backshift operators, λ and l (where $l=\lambda^s$, s being the width of the seasonality), and, ε_t is the white noise.

The model being multiplicative the relevant characteristic equation is a product of two polynomials, one for the inter-year variation and the other for intra-year variations. As such there could be four possibilities. The series could be (a) stationary with respect to both the inter- as well as intra-year variations; (b) stationary only with respect to intra-year movements and homogeneous with respect to inter-year movements; (c) stationary with respect to inter-year movements and homogeneous with respect to intra-year movements; and (d) series are non-stationary but homogenious. Cases (b) and (c) denote a kind of partial stationarity, while (a) refers to the full stationarity for multiplicative series, and case(d) would imply non-stationarity.

Thus while the situation of full stationarity implies that both the AR polynomials, $a_P(l)$ and $\phi_p(\lambda)$ should satisfy the stationarity conditions separately, the disadvantage with testing for stationarity in the combined polynomial,

$[\alpha_p(l).\phi_p(\lambda)]$, is that even when the roots of the combined polynomial lie out side the unit circle, it does not ensure that the roots of both the component polynomials also do so. This can be shown by extending the derivation of the testing equation. While a comprehensive discussion of all the problems involved in such an extension would require a separate study, a tentative version of the testing equation for seasonal models can be derived by proceeding as follows.

The problem now is to decide about the last differencing for both the non-seasonal (yearly) as well as the seasonal (monthly) component processes. As in the case of non-seasonal models, to simplify let us consider the series as already transformed upto the last but one differencing for each of the component processes. Accordingly, consider the process,

$$(1-rl)\ y_t = c_l + u_t \qquad (1)$$
$$(1-\rho\lambda)\ u_t = c_\lambda + e_t \qquad (2)$$

where (1) and (2) represent the yearly and monthly component processes respectively, y_t and e_t denote the respective transformed series upto the last differencing such that $y_t = \nabla_s^{D-1} a_p(l) b_Q^{-1}(l) x_t$, and $e_t = \vartheta_q(\lambda)[\phi_p(\lambda)\nabla^{d-1}]^{-1}\varepsilon_t$, r and ρ, the first order coefficients to be tested, and, c_l and c_λ constants (indicating possible mean 'drift' of the series).

The Dickey-Fuller type of equation can be obtained as follows: From (1), substituting the value of $u_t=(1-rl)y_t-c_l$ in (2) we obtain,

$$(1-\rho\lambda)(1-rl)y_t=(1-\rho\lambda)c_l+c_\lambda+e_t \qquad (3).$$

And, using the decompositions,

$(1-rl)=(1-l)+(1-r)l$, and $(1-\rho\lambda)=(1-\lambda)+(1-\rho)\lambda$, the final testing equation can be obtained as

$$\nabla\nabla_s y_t = (\rho-1)y_{t-1} + (r-1)y_{t-s} + (1-\rho r)y_{t-s-1} + \mu + e_t \qquad (4).$$

where $\mu=(1-\rho\lambda)c_l+c_\lambda$. Similarly, assuming that e_t is weakly dependent and heteroskedastic, the Phillips-Perron type of testing equation can be derived for the multiplicative models as

$$y_t=\rho y_{t-1}+ry_{t-s}-\rho r y_{t-s-1}+\mu+e_t \qquad (5).$$

The equations (4) and (5) imply that the tests for seasonal and non-seasonal differencing have to be conducted simultaneously, and separate testing will lead to specification bias. On the practical side, while the of Dickey-Fuller version of the testing equation is conditional on the prior knowledge of the orders of the polynomials involved, the Said-Dickey version would involve truncation of two autoregressive polynomials resulting in even longer testing equation with more nuisance parameters. As for the Phillips-Perron version, the z-transformations suggested for non-multiplicative models may no longer be valid as the assumption of the composite error being `weakly dependendent and heteroskedastic´ may not hold. Also the z-transformations involve double truncations of the lag order for computing the population variance.

In an attempt to check the correctness of the yearly differencing decision in the TN model, we have fitted the testing equation (4) which is as follows.

Data from June, 1983 to March, 1989.

$$\nabla\nabla_s y_t = -1.01 y_{t-1} - 0.015 y_{t-12} + 0.964 y_{t-13} + 204.86 + e_t \qquad -(6)$$

$$\tau_{\mu} \quad (-8.23) \quad (-0.29) \quad (7.37) \quad (1.45)$$

$R^2 = 0.51 \quad \sigma = 994.8 \quad DW = 2.009$

The regression results show that the hypothesis of unit root at 12th lag cannot be rejected. Although this result appears to confirm the pattern of the SACF and justifies the 12th differencing of the series in the TN model, I would not recommend the test as a pre-specification tool for determining the order of differencing.

4. Testing and Re-estimation by the Critics - A critique.

The study by MW is an attempt to re-do the empirical exercise, ostensibly with a view to improve upon the model used in the TN. Much of the empirical work has been on the same lines as in the TN. They also started by converting the cumulative month-end series of the budget deficit into monthly change series. Further, despite their criticism regarding the choice of the Box-Jenkins model in the TN, they did not seem to be averse to using the seasonal ARIMA models. The crucial difference between the TN and their study, however, stems from the decision as to whether or not the seasonal (12-month) differencing is needed.

MW, in their eagerness to conduct formal tests, have mis-specified the testing equations. For example, to conduct the Dicky-Fuller type of testing, two separate equations were specified, one for monthly and the other for yearly differencing (of course, with and without the time trend in each case). The equations are.

$$\nabla_i x_t = \mu + (\alpha-1).x_{t-i} + \varepsilon \qquad (6),$$

where x_t is monthly deficiti=1,12; t=1...T, and, with the null hypothesis, $\alpha=1$. Similarly, the Phillips-Perron tests are based on the equations of the form

$$x_t = \mu + \alpha.x_{t-i} + \varepsilon \qquad (7),$$

with the hypothesis that $\alpha=1$. Two alternative hypothesis were considered: (i) unit root exists at lag 1, and (ii) unit root exists at lag 12. For each of these two variants were investigated, namely, with and without a time trend.

The main flaw in the testing for the unit roots by MW lies in the specification of the test equations. As we have seen in the previous section, unit root testing in multiplicative models involves simultaneous estimation and testing of hypothesis for both seasonal as well as non-seasonal differencing. Accordingly, the test equation to be estimated should be either in the form of equation (4) or equation (5). Instead, MW have sought testing for the stationarity of the combined polynomial, which can be achieved even when one of the component polynomials is non-stationary. Even if their separate testing procedures showed that the series are stationary it does not ensure stationarity of the component series.

Incidentally, an aspect of the estimation in MW that cannot go unnoticed by discerning readers is that the re-specified model is fitted on fewer than available observations. Thus, out of the total data span from April 1980 to March 1989, the specification in terms of one yearly, and one monthly lags leaves the researchers with as many as 95 observations (from May, 1981 to March 1989). Yet, MW have chosen to fit the model only on 59 observations from May 1984 to March 1989. Trivial may the decision seem, it has implications for long term forecasting with their specification. While their reason to drop as many as 36 initial observations is not known, re-estimation of their model on all the available 95 observations, gives a clue regarding its inconsistency. These regression results are as follows.

Data May,1981 through March,1989.

$$(1-0.47\lambda^{12})(1+0.16\lambda)x_t=379.6+\varepsilon_t \qquad (7)$$

R^2=0.23, SEE=1369.8, DW=1.98.

The regression results (7) are widely different from those obtained by fitting the specification on the 59 observations from May 1984 to March 1989. Thus, their model specification can, at best, be regarded as locally efficient and may not be useful for longer time forecasting.

5. Other Aspects of the Criticism.

Choice of the Model.

It is alleged that the TN abstained from any objective criteria while preferring Box-Jenkins models to other types of models for deriving the budget deficit norms. The critics feel that `the crucial issue is not whether the Box-Jenkins model is procedurally superior to the other methods but whether, on the average, it forecasts month-end budget deficits more accurately than the other methods.'

This criticism is based on lack of understanding of the general nature of the Box-Jenkins models. The main reason for preferring Box-Jenkins models in the TN was not due to their procedural superiority to others, but due to the elementary fact that 'this wider class of processes, provide a range of models, stationary or non-stationary, that adequately represent many of the time series met in practice.' (Box and Jenkins, 1976, p.8). As such, Box-Jenkins models encompass most of the other commonly used models such as the growth trend equation models, random-walk models, exponential smoothing models and so on, which can be derived as special cases by imposing suitable restrictions. The purpose of presenting these other models in the TN was only to bring out the maladies of restricted specifications. By nature, being more general, Box-Jenkins models suffer less from specification bias.

In fact, it is doubtful if one can regard Box-Jenkins models as *procedurally* superior to say, a restricted model such as the trend equation. Unlike these restricted models, Box-Jenkins methods involve cumbersome, and often not very objective procedures pertaining to, identification of the degree of differencing, determination of orders of polynomials involved, as also estimation by iterative procedures. Thus, procedural convenience is certainly not the main consideration for preferring these models. The main reason for the choice of the Box-Jenkins models is the amount of freedom they afford in identifying and capturing the trend components.

The Nature of Forecast Norms.

Regarding the nature of forecasts of month-end deficits from the model presented in the TN, it is pointed out that for all the twelve months of the forecasting horizon ie., fiscal year 1989-90, the forecasts were consistently lower than the actual month-end deficit which does not *speak well* for the particular Box-Jenkins specification.

To begin with, the critics have wrongly assessed the out-of-sample forecasts derived in the TN. Presumably, they have come to their stated observation after comparing the cumulated forecasts (table 2, column 5 of the Techinical Note) from the specified Box-Jenkins model, with the actual month-end deficits (table 2, column 2). Actually, as can be expected, since the model was fitted on monthly change series, the forecast inference results do not apply to the cumulative month-end budget deficits but to the monthly change series, whose forecasts were later converted into month-end deficits to be compatible with the original series. Thus, on the basis of these cumulated forecasts it is difficult to conclude if they are biased. The correct way of judging would have been to compare the non-cumulated forecasts and with corresponding actual monthly-differenced deficit series. Thus, a major reason for the observed 'consistently' different trend, is built in the way the deficit series is viewed. Once the transformation process is clear, it is not difficult to understand the biased trend of cumulated forecasts. Just one wide outlier data point could be sufficient to shift the subsequent trend of the accumulated forecasts. This shift could be even more prominent if forecasts for subsequent months of the year are based on previous months' forecasts, which is the case in the TN.

In fact, the unbiased nature of the forecast errors is clear even from the regression diagnostics presented along with the results (see Appendix table A.3 of the TN). Given the adequacy of the model as represented by the R-bar and the SEE, the RMSE, the MAPE, the Q-statistic or the plot of residuals do not suggest any consistent bias in the residuals.

This leads us to believe that the phenomenon of the forecasts for the twelve-month period (1989-90) being consistently different from the actuals, could be due to certain 'shocks' in the form of policy changes occurred during the forecast period that might have generated a higher than the past trend in the actual monthly budget deficits.

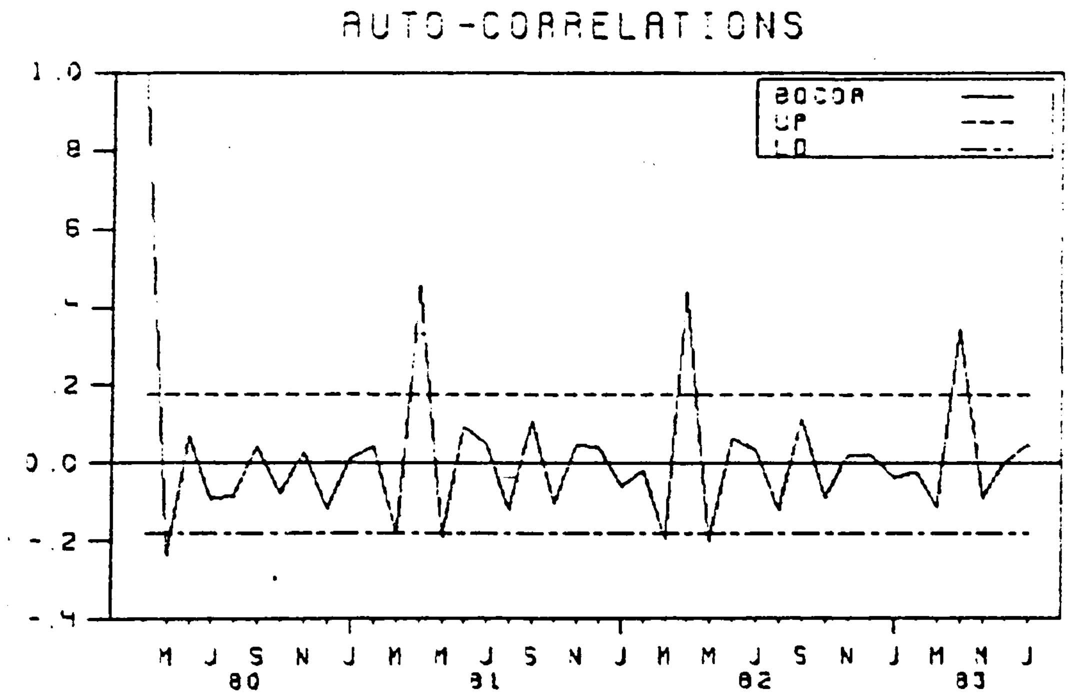
6. Conclusion.

Thus, in brief, the criticism levelled against the model is not quite warranted. Also, their alternative model specification can, at best, be regarded as locally efficient and may not be useful for longer time forecasting.

All this, however, is not to say that the model specified in the TN is final and there exists no scope for improvement in several aspects. As mentioned in the TN, there could be a host of factors other than seasonality and trend, which influence the deficit of the government at any point of time, and that it is necessary to take into account the impact of all relevant economic variables having a significant bearing on the level of the deficits. The respecified version by MW also suffers from these drawbacks, and to that extent, cannot be taken as methodologically much different from, let alone superior to, the original specification.

More importantly, the technical `innovations´ sugested by MW leading to the modified model, does not stand theoretical reasoning. In particular, it is doubtful if the so-called `rigorous´ tests of stationarity conducted by MW are valid in the context of multiplicative autoregressive models. In the absence of better test procedures, the conventional methods relied upon by the authors of the TN, based on the SACF, appear to be more relevant.

Fig 1. SACF OF MONTHLY BUDGET DEFICIT



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