An empirical analysis of the Canadian budget process

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Abstract. This paper provides a statistical analysis of the forecasts of a significant number of expenditure and revenue components of the federal budget provided each year by the Department of Finance. The sample available for such an investigation is limited, and we describe an easily applied non-parametric testing methodology that is more appropriate than the usual regression-based approach in small samples. The reliability and relative power of the various non-parametric tests are illustrated in a series of simulations. Applying these tests to the fiscal forecasts, we find that there is little cause to be concerned with the forecast performance of the Department of Finance over the last seventeen years.

Une analyse empirique du processus budgétaire Canadian. Ce texte présente une analyse statistique des dépenses et des revenus du budget fédéral qui sont faites par le Ministère des Finances. L'échantillon est restreint et les auteurs présentent la description d'une méthode non-paramétrique conviviale qui est beaucoup plus appropriée que l'approche traditionelle utilisant la régression quand les échantillons sont petits. La fiabilité et la puissance relative des tests non-paramétriques sont illustrées à l'aide de simulations. Après avoir appliqué ces tests aux prévisions fiscales, les auteurs concluent que les prévisions du Ministère des Finances au cours des dix-sept dernières années sont satisfaisantes.

I. INTRODUCTION

The announcement in the spring of 1994 that the federal budget deficit was not $35 billion, as predicted the previous fall during the election campaign, but a forbidding $45 billion has focused attention on the reliability of fiscal forecasts produced by the Department of Finance, which form the basis of the federal government's

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budgetary predictions. The political and strategic context of this revelation aside, two more general issues come immediately to mind. With what frequency have forecast errors of this magnitude occurred in the past? What was the provenance of the error: did the error originate from the expenditure or from the revenue side of the budget, and which specific budgetary forecast components contributed most to the error?

These concerns suggest that a statistical study of the specific fiscal forecasts used in the budgetary process on both the expenditure and the revenue sides would be a relevant first step in the assessment of the federal budgetary process. Whereas there is a considerable American literature on the reliability of government forecasts (see, e.g., Shkurti and Winefordner 1989; Gentry 1989; Feenberg et al. 1989; and Plesko 1988), there is surprisingly little work addressing the forecast performance of the Department of Finance in the Canadian budgetary context. A first step was taken by David and Ghysels (1989), but their focus was on forecasts of expenditure and revenue aggregates. In this paper we are concerned as well with the specific components of forecast expenditures, such as income security or debt servicing, and of revenues, such as income tax or excise tax receipts. Our aim is to assess the fiscal performance of the Department of Finance involving the key components of the budget over as long a time horizon as the coherence of the series considered can be maintained. The analysis pursues the traditional issues: are budget projections systematically biased or not? Are any of the budgetary components prone to bias? Is there any evidence that past errors are overlooked in budget forecasts? Could other information in the form of important macroeconomic aggregates have improved forecast performance? We should emphasize at the outset that the paper does not attempt to determine the extent to which forecast errors reflect mistaken macroeconomic prediction or inaccurate predictions of actual policy or are examples of technical errors; for a discussion of these matters in the U.S. context, see Auerbach (1994).

The data available to address these issues are limited. As will be seen, it is difficult to extend the analysis prior to 1976, a limitation that imposes a small sample for statistical inference. Our position with regard to the appropriate statistical methodology to adopt is that regression-based procedures may be misleading in this context. The second contribution of this paper is to present a non-parametric methodology that incorporates exact tests for evaluating the unbiasedness and efficiency of forecasts and to give some sense of the performance of these tests relative to the regression-based procedures in a series of simulation exercises. The theoretical basis for these tests has been established in Dufour (1981) and Campbell and Dufour (1991, 1995) and have been applied in a forecasting study by Campbell and Ghysels (1995) to U.S. federal government forecasts. The tests have good finite sample properties, are robust against departures from assumptions such as normality and homoscedasticity, and, as will be indicated, display good power relative to regression-based procedures, even in circumstances favourable to these traditional procedures. It should be emphasized that the non-parametric approach is straightforward to apply and is of potential interest in many other applications.

The paper is organized as follows. In the second section of the paper we introduce the tests to be used in the applied work and through simulation studies contrast the performance of the non-parametric approach with that obtained with the more usual regression methods for a sample comparable in size to the sample of forecasts considered in the paper. In the third section the Canadian budget process is discussed and the twelve series to be investigated in the subsequent statistical analysis are introduced. The empirical work is presented in the fourth section. Here, the non-parametric results are compared with regression-based results. Some conclusions are offered in the final section of the paper.

II. A NON-PARAMETRIC METHODOLOGY FOR ASSESSING FORECAST PERFORMANCE

Over the last three decades a regression methodology has been developed to test various implications of the rational expectations hypothesis; this material is surveyed, for instance, in Pesaran (1987). In the particular context where expectations are observable or generated by some forecasting procedure, one has been interested in testing whether the expectation is an unbiased predictor of the realized value and whether the forecast efficiently exploits all information available to the forecaster. More precisely, let the one-period forecast error be $(Y_t - Y^e_{t-1})$, with Y^e_{t-j} denoting the expectation or forecast of the variable Y_t made at time $t-j$. The claim that expectations are unbiased can be assessed by considering the regression of the error on a constant. Broader orthogonality or conditional independence claims that forecast errors are uncorrelated with the entire set of information that is costlessly available to the forecaster may be readily tested via regressions of the error on relevant past information. This regression-based testing methodology has been widely used; for representative examples see McNees (1978) and Friedman (1980) and, in the context of studies of government forecasts, Plesko (1988), David and Ghysels (1989), and Gentry (1989).

However convenient it is to apply the methodology, the results must be interpreted with considerable caution. On the one hand, deviations from the assumption that the forecast errors are normally distributed with constant variance throughout the sample may compromise the efficiency of the regression statistics, particularly in small samples. Tests for bias, for example, may have little power in the presence of outliers. By contrast, as illustrated in a simulation study by Mankiw and Shapiro (1986), regression procedures used to test the efficiency of forecasts may reject too often when disturbances affecting the magnitude of the forecast error themselves are correlated with future values of the regressors. Moreover, it should be emphasized that such departures from standard assumptions such as heteroscedasticity and feedback are entirely consistent with the rationality hypothesis.

Against this backdrop, we now describe a classic finite-sample non-parametric testing methodology to assess the unbiasedness and efficiency of forecasts. The test statistics considered are based on signs. These are the only statistics that can produce valid tests about a median under sufficiently general distribution assumptions;

this point is emphasized by Dufour and Hallin (1991); for a general discussion see Pratt and Gibbons (1981, 233–34). The sign-based testing procedures introduced below are known to be robust to problems of non-normality and heteroscedasticity and are valid under conditions of feedback, including the paradigm considered by Mankiw and Shapiro (1986). Moreover, the power of these tests can be considerably superior to parametric procedures in such situations. These issues are discussed in Campbell and Dufour (1991, 1995). A related feature of non-parametric testing procedures that may not be widely appreciated is that, relative to regressions tests applied in situations favourable to parametric procedures, the power lost in applying non-parametric tests is not particularly pronounced; a more thorough discussion of relative efficiency can be found in Hettmansperger (1984). This point will be illustrated in the simulation studies that are presented in this section. Finally, at the outset we should mention that the non-parametric approach focuses on the median of the forecast error rather than the mean. It is clear that for symmetric distributions with finite mean, median-unbiasedness and mean-unbiasedness are equivalent. The issue of whether one should test median-unbiasedness or mean-unbiasedness in the situation of asymmetric disturbances is certainly debatable. Whatever one's position on this issue, the rational expectations hypothesis does entail median-unbiasedness when the mean absolute forecast error rather than the mean square forecast error is minimized.

To parallel the regression-based methodology in forecast evaluation, we introduce, in turn, sign and signed rank tests for unbiasedness of forecasts and for the orthogonality of forecast errors both to past forecast errors and to available macroeconomic information. The performance of the non-parametric statistics relative to the analogous regression procedures are investigated via simulation studies as the tests are introduced. These results are presented using the graphical methods described in Davidson and Mackinnon (1994).

Let the one-period forecast errors be written as $E_{1t} = (Y_t - Y^e_{t-1})$. Let also $u(z) = 1$ if $z \geqq 0$ and $u(z) = 0$ otherwise; the role of the function $u(\cdot)$ is simply to indicate whether the forecast error is positive or negative. To test the unbiasedness of forecast errors, consider first the statistics:

$$S_1 = \sum_{t=1}^{T} u(E_{1t}) \text{ and } W_1 = \sum_{t=1}^{T} u(E_{1t})R^+_{1t}, \tag{1}$$

with R^+_{1t} the rank of $|E_{1t}||E_{11}|$ when $|E_{1t}|, \ldots, |E_{1T}|$ are placed in ascending order and T is the sample size. These traditional non-parametric statistics are used in tests of location in very general circumstances; see Hettmansperger (1984) for a systematic presentation. Under the general null hypothesis that the forecast errors are independent with 0 median, the sign statistic S_1 is distributed Bin $(T, 0.5)$; that is, as the binomial distribution with number of trials T and probability of success 0.5. Under the additional assumption that the forecast errors are symmetric about 0, the statistic W_1 has the Wilcoxon signed rank distribution (i.e., like the weighted sum of T independent Bin $(1, 0.5)$ variates); for a general discussion see Lehmann

(1975). The two statistics S_1 and W_1 can thus be used to test the hypothesis that the one-period forecast errors are centred at 0. In passing, it should be remarked that W_1 under the null has been tabled for sample sizes up to fifty (see, e.g., Wilcoxon, Katti, and Wilcox 1970) and that the normal approximation with $E(W_1) = T(T+1)/4$ and Var $(W_1) = T(T+1)(2T+1)/24$ works well even for small values of T.

To assess the power of S_1 and W_1 relative to the t-statistic, the usual parametric procedure to use in location tests, we considered a simulation involving twenty random draws from a distribution perhaps with non-zero centre; for example, in figure 1 the mean is 0.4 when the disturbances are normal and homoscedastic. The sample size corresponds roughly to the length of the forecast samples considered in the empirical study in the next section. For each of 5,000 replications, we computed the probability values associated with each of the sign, Wilcoxon, and t-statistics under the null hypothesis that the centre of the distribution is 0. Three empirical distribution functions of the probability values corresponding to the statistics are then estimated:

$$\hat{F}_k(x_i) = \frac{1}{N}\sum_{j=1}^{N} I(p_j^k \leqq x_i), \tag{2}$$

where p_j^k is a p-value; $I(p_j^k \leqq x_i)$ is 1 if the argument is true and 0 otherwise; and $N = 5000$ and $k = 1, 2, 3$, corresponding to the three statistics. The values x_i, $i = 1, \ldots, m$ correspond to a grid of the $[0, 1]$ interval; we follow Davidson and Mackinnon (1994) and consider $m = 215$ with $x_i = 0.001$, $0.002, \ldots, 0.010, 0.015, \ldots, 0.990, 0.991, \ldots, 0.999$.

In this section two types of graphs based $\hat{F}_k(x_i)$ on will be considered. The direct plot of $\hat{F}_k(x_i)$ against x_i, or what is called a P-value plot, is a measure of how the underlying statistics perform for various nominal sizes. If the simulated distribution is $N(0, 1)$, for example, then it is clear that all the distributions used to compute the probability values of the three statistics are correct and the resulting P-value plots should be close to the 45° line. On the other hand, if the simulations were based on $N(0.4, 1)$, the distance from the 45° line in the P-value plots is an indication of the power of the test. The presentation of the empirical distribution functions $\hat{F}_k(x_i)(k = 1, 2, 3)$ within the same graph gives immediate insight into the relative power of the three underlying tests. To investigate the power of a test in situations where the true size does not correspond to the nominal size, it is more reasonable to consider a size-power curve that traces the points $(\hat{F}_k(x_i), \hat{F}_k^*(x_i))$, where $\hat{F}_k(x)$ and $\hat{F}_k^*(x)$ are the empirical distribution functions under the null and alternative, respectively. In this way, the power of the test is size corrected. Davidson and Mackinnon (1994) should be consulted for further details and an illustration.

Figure 1 presents four P-value plots to assess the relative power of the sign, Wilcoxon, and t-statistics in detecting bias for various simulated distributions when the sample size is twenty. Since the non-parametric statistics can assume only a finite number of values that are associated with a corresponding number of distinct p-values, the indicator function in (2) will yield a step function for both the sign

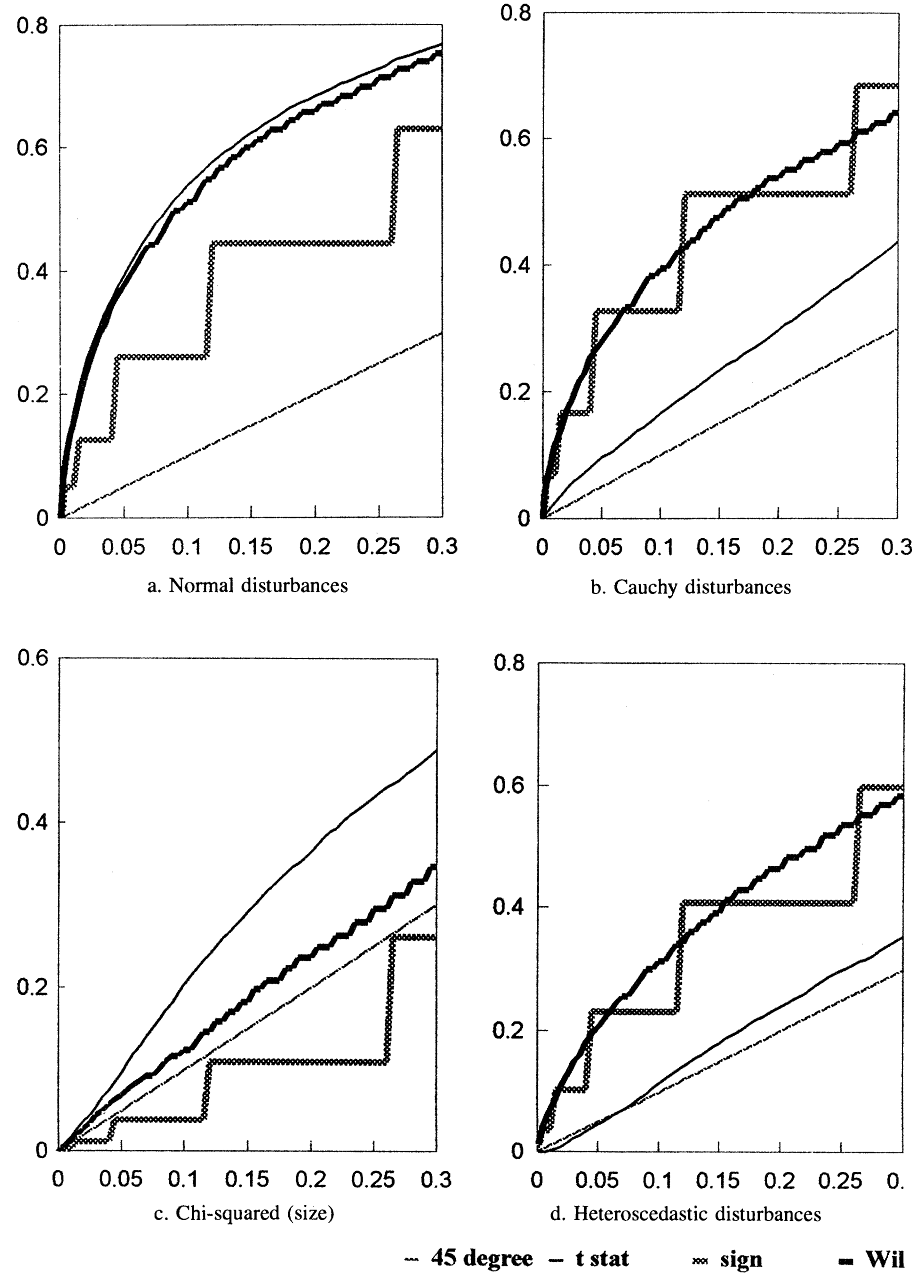


FIGURE 1 Power in the presence of bias, $N = 20$

NOTES

Each of figures 1a–1d is a p-value plot based on the empirical distribution functions given by (2). Within a figure, the distances from the 45° line are indicative of the relative power of the three tests in rejecting the null hypothesis that the disturbances are centred at zero; see the text for details concerning the test statistics.

and the Wilcoxon statistics. The steps will be further apart in the case of the sign statistics (as is revealed in figures 1–3), which assume fewer values than their Wilcoxon counterparts. In the case of normal disturbances with mean 0.4, the striking message is that there is very little loss of power in using the Wilcoxon test and relatively little in applying the sign test. When the disturbances are Cauchy with non-zero median, a distribution that has fat tails, the two non-parametric procedures have equivalent power and clearly dominate the t-test. In the third example the asymmetric chi-squared distribution with four degrees of freedom that has been centred at its median is considered. The graphical results reveal at a glance that both the Wilcoxon and the t-statistics over-reject the true null in this environment and underscore the necessity of the symmetry assumption in applying these tests. To assess the performance of the statistics in the presence of heteroscedasticity, samples were drawn from a normal distribution with non-zero mean with unit variance for the first fifteen points and with variance 16 for the last five. Figure 1(d) reveals that the t-test has little power to detect the alternative in the presence of such a systematic break in the variance, while the two non-parametric tests perform considerably better with comparable power. In sum, the message conveyed by these pictures is clear: there are very good reasons to use the non-parametric procedures in a sample of such size with very little cost in power in the one circumstance that favours the use of the t-statistic.

To test for serial correlation in the forecast errors let $Z_{1t}^k = E_{1t}E_{1(t-k)}$ and consider the statistics:

$$\mathrm{sc}_k = \sum_{t=k+1}^{T} u(Z_{1t}^k) \text{ and } \mathrm{wc}_k = \sum_{t=k+1}^{T} u(Z_{1t}^k)R_{2t}^+, \tag{3}$$

where R_{2t}^+ is the signed rank of the product Z_{1t}^k, $t = 1, \ldots, T$. These tests, introduced by Dufour (1981), can be interpreted as location tests: correlation between E_{1t} and $E_{1(t-k)}$ will tend to move the centre of their product away from 0. More formally, if the E_{1t} have 0 median and are uncorrelated at length k, then the statistic sc_k is distributed Bin $(T-k, 0.5)$, and, on the additional assumption of the symmetry of the forecast errors about 0, the wc_k statistics are distributed Wilcoxon signed rank of size $T-k$. Again it is important to stress that the validity of tests based on sc_k and wc_k is not compromised by non-normal or heteroscedastic forecast errors.

Figure 2 presents the results of simulations similar to the previous study, but in the context of first-order correlation defined by

$$E_{1t} = \rho E_{1(t-1)} + \epsilon_t, \tag{4}$$

$t = 1, \ldots, 20$, where the disturbances ϵ_t are either standard normal, Cauchy, or heteroscedastic. Along with the non-parametric statistics defined as in (3), we also consider the t-statistics based on a regression without a constant term of the forecast error on its own lag. The Wilcoxon test displays credible power with regard to the t-statistic as shown in figure 2(a), where the disturbances are normal, and it completely outperforms its parametric alternative in the case of Cauchy disturbances, as is evident in figure 2(b). The unreliability of the t-statistic based on a regression without a constant where the disturbances are heteroscedastic is illustrated in figure 2(c), which presents as well the expected empirical confirmation that the non-parametric statistics are reliable in such a context. In figure 2(d), which is a size-power plot of the performance of the t-statistic, the sign statistic is seen to perform as well as the size-corrected t-statistic. Clearly, the results of figure 2 support the theme struck in the previous simulations regarding bias: there is no compelling reason to use the t-test over the sign and Wilcoxon tests.

To show that forecast errors are independent of previous information available to the forecaster, denoted X_t, we first introduce the series X_t^c, which represents an attempt to centre X_t around 0 using only information available at time t. For one example, consider

$$X_t^c = X_t - \text{median}\,(X_1, X_2, \ldots, X_t).$$

For one-period forecasts, the efficiency or orthogonality tests to assess whether the forecaster has made efficient use of available information represented by the series X up to time t are based on statistics of the form $Z_t^k = E_{1t}X_{t-k}^c$, with E_{1t} the one-period forecast and $k \geqq 1$. Let the sign and signed rank statistics be respectively defined as

$$\mathrm{so}_k = \sum_{t=k+1}^{T} u(Z_t^k) \text{ and } \mathrm{wo}_k = \sum_{t=k+1}^{T} u(Z_t^k)R_{1t}^+, \tag{5}$$

where R_{1t}^+ is the signed rank of E_{1t}, $t = k+1, \ldots, T$, and $k \geqq 1$. Under the null that the forecast errors have median zero and are mutually independent, so_k and wo_k are distributed binomial and Wilcoxon sign rank of size $T-k$, respectively. These procedures, which have been introduced in Campbell and Dufour (1991, 1995), not only are robust to the presence of non-normal and/or heteroscedastic disturbances, but are valid in the presence of feedback of the sort studied by Mankiw and Shapiro (1986).

Several points must be added by way of clarification. First, the non-parametric tests check whether the location of Z_t^k is 0; a non-zero centre indicates that there is some correlation between the forecast error at t and past information X_{t-k}. Since the forecast errors themselves may be centred at 0, it is necessary to centre X_t for each t around 0 if the test is to have any power. It must be emphasized that the centring procedure should use only information available at the time of the forecast. The second observation concerns the Wilcoxon statistic: to preserve results that establish the small sample distribution, the signed ranks of the forecast errors, not of Z_t^k, must be used; this innovation is introduced in Campbell and Dufour (1995). Finally, the above tests are defined relative to a single fixed k. To test efficiency for $k = 1$, 2, 3, for example, it is necessary to carry out three non-parametric tests based on

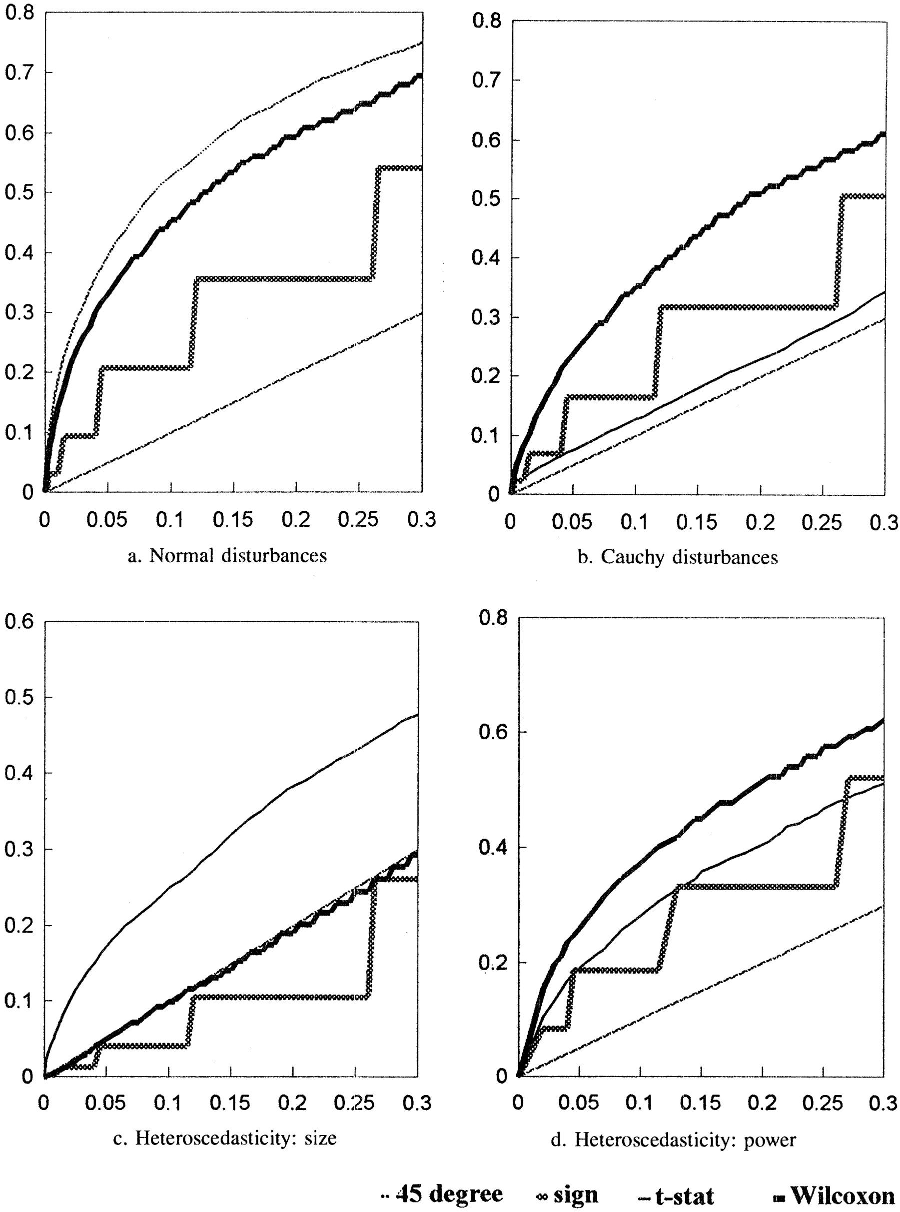


FIGURE 2 Power in the presence of serial correlation, $N = 20$

NOTES

Each of figures 2a–2d is a p-value plot based on the empirical distribution functions given by (2). Within a figure, the distances from the 45° line are indicative of the relative power of the three tests in rejecting the null hypothesis that the disturbances are centred at zero; see the text for details concerning the test statistics.

Z_t^k corresponding to each k with levels $\alpha/3$ in order to test the null with level bounded by α. The null is rejected if one of the tests is significant.

To assess the performance of the statistics defined by (5), we consider the following variant of the model investigated by Mankiw and Shapiro (1986):

$$E_{1t} = \beta_1(X_{t-1} - \mu) + \epsilon_t, \qquad t = 1, \ldots, 20 \tag{6}$$

$$X_t = \theta_0 + \theta_1 X_{t-1} + \eta_t, \qquad t = 0, \ldots, 19, \tag{7}$$

where μ is the mean of X_t. For each of the experiments, data were generated for this model by setting $\theta_0 = 0.1$, $\theta_1 = 0.9$, and $\eta_t = \rho\epsilon_t + \sqrt{1-\rho^2}\,\eta_t'$, where $\rho = 0.9$, and ϵ_t and η_t' are independent with the same distribution either normal, Cauchy, or heteroscedastic, as in the previous simulations. To test whether the forecast errors E_{1t} are independent of past movements of X_t, it is standard procedure to apply the t-test associated with the slope coefficient of the regression model

$$E_{1t} = \alpha_0 + \alpha_1 X_{t-1} + \epsilon_t. \tag{8}$$

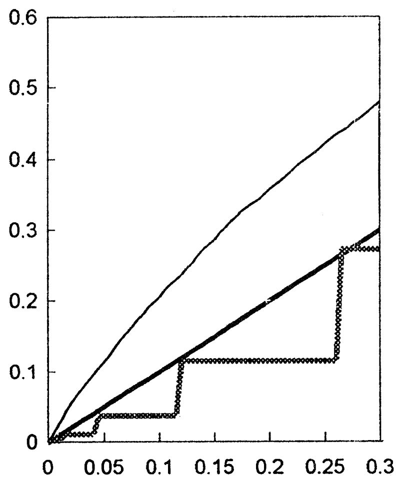
Finally, in defining the non-parametric statistics (5), we arbitrarily take

$$X_t^c = (X_t - X_{t-1}) - \sum_{s=1}^{t}(X_s - X_{s-1})/t, \tag{9}$$

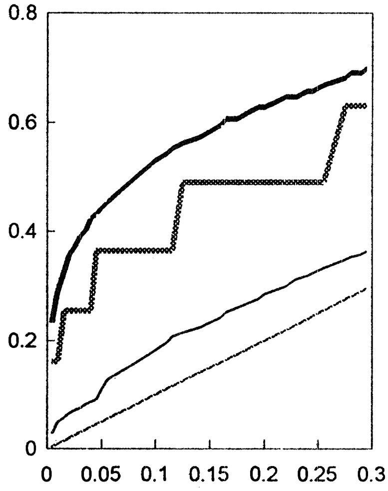
which is the centring procedure used in the empirical analysis in the following section. The results of this simulation exercise are presented in figure 3.

Figure 3(a) underscores the finding of Mankiw and Shapiro (1986) that the t-statistic rejects too often when θ and ρ are close to 1; as expected, the non-parametric statistics both reject at the nominal level. In Figure 3(b), a size-power analysis that corrects the over-rejection of the t-statistic under the null, it is evident that both non-parametric statistics considerably outperform the size-corrected t-test, with the Wilcoxon test's having a slight edge in power. As in the previous simulations, Figure 3(c), which is a P-value plot, confirms the superior power performance of the non-parametric statistics relative to the t-test when the disturbances are Cauchy. For the final simulation involving the type of heteroscedasticity previously considered, we let $\rho = 0$, so that there would be no over-rejection under the null if the disturbances were identically normal. But the t-statistic none the less over-rejects in the presence of heteroscedasticity, and we present a power-size curve in figure 3d. For this specification, the Wilcoxon test is the most powerful, with the sign test's exhibiting as much power as the size-corrected t-test. It should be emphasized here that in practice it is difficult to correct as precisely for such over-rejection, since the relevant critical values for the correct application of the t-test depend on the sample size, the unknown specification of the model, and the unknown parameters of the model.

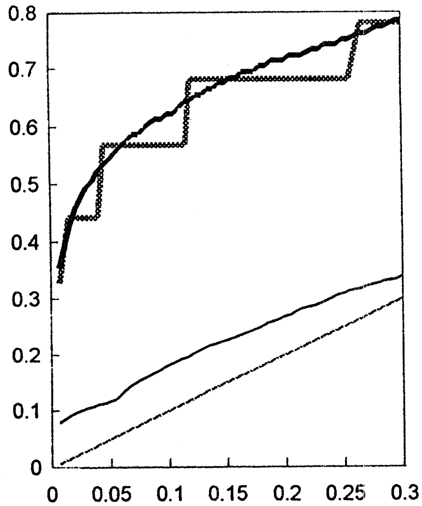
To summarize: non-parametric statistics based on signs and signed ranks have been introduced in (1), (3), and (5), which can be used to test for bias and efficiency



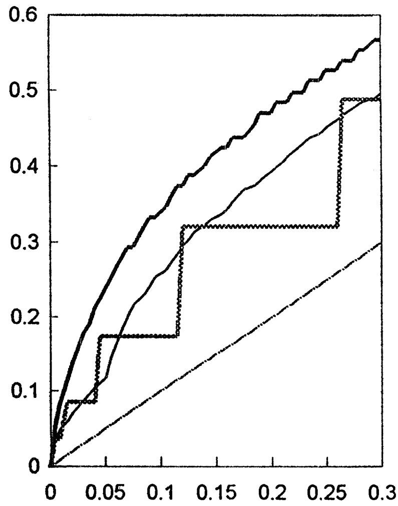
a. Normal disturbances: size



b. Normal disturbances: power



c. Cauchy disturbances



d. Heteroscedastic disturbances

FIGURE 3 Efficiency tests: analysis of power, $N = 20$
NOTES
Each of figures 3a, 3c, and 3d is a p-value plot based on the empirical distribution functions given by (2); Figure 3b has corrected the t-test for the size distortion exhibited in 3a. Within figure 3b the distances from the 45° line are indicative of the relative power of the three tests in rejecting the null hypothesis of efficiency; see the text for details concerning the model and the test statistics.

under minimal distributional assumptions. These non-parametric tests will have no size distortions, since they are exact. By contrast, regression-based methods generally will not be exact unless some strong auxiliary assumptions are met. Moreover, the non-parametric tests have shown superior power properties relative to parametric tests in the simulation studies presented in this section for a sample size frequently encountered in practice; it should be added that the tenor of the simulation results is maintained when the sample size is increased to fifty and, in certain situations involving for example Cauchy disturbances, for much larger sample sizes; for further details see Campbell and Dufour (1995). We can conclude only that the appropriate testing methodology to adopt when assessing certain aspects of forecast performance is non-parametric based on signs and signed ranks.

III. THE CANADIAN BUDGET PROCESS

The federal government's fiscal year begins on 1 April. By law, spending estimates must be tabled in the House of Commons in March. These estimates reflect the government's view of the costs of maintaining and developing existing programmes and may incorporate as well the estimated costs of whatever policy initiatives the government may have in mind. Such initiatives are generally outlined in a budget that is presented to Parliament around this time, although the precise timing of the budget can be determined by political exigency. The budget document also contains the government's specific revenue forecasts, which are presented as part of a more general picture of the course of the economy in the upcoming year. In recent years, forecasts of key macroeconomic variables, such as GDP growth, unemployment rate, and so on, are given as well. The spending estimates are now also reported in the *Public Accounts*, which are published each year in the fall as a record of the government's fiscal position the previous year.

The *Public Accounts* present the realizations of the previous year's expenditures and revenues on a detailed basis. It should be emphasized that they are published some six months after the start of the new fiscal year. Given this lag, it may be argued that the information set available in framing the upcoming year's fiscal forecast does not contain the previous year's forecast errors. The Department of Finance, however, is hardly kept in suspense regarding the realizations of the different components of the budget, as information relating to program expenditures and various tax revenues is collected on a regular (even weekly for some variables) basis. As a consequence, we do not find it unreasonable to assume that the previous year's forecast errors are known when the current forecast is determined. In this regard, forecast procedures in Canada certainly appear to be less ragged than in the United States, where the Executive Branch presents its forecasts to Congress some nine months before the beginning of the fiscal year as an initial step in the budgetary process; for further discussion of the implications of this long lag for forecast evaluation see Campbell and Ghysels (1995).

To obtain some insight into the structure of the Canadian budget, in table 1 we present some of the big-ticket expenditure items as a percentage of total expenditures and the major revenue items as a percentage of total revenue; the deficit is

TABLE 1
Components of the federal budget

	1976	1982	1987	1992
Expenditures				
Program	88.1	81.0	76.9	75.7
Income Security	15.1	13.3	13.5	13.2
Transfers to Gov.	15.9	15.3	15.4	15.6
U.I. Benefits	8.2	10.8	8.4	11.8
Defence	7.9	7.8	8.5	7.3
Debt Service	11.1	19.0	23.1	24.3
Revenues				
Income Tax	41.6	43.8	46.3	48.0
Corporate Tax	15.3	11.9	11.1	6.8
Excise Tax	24.6	24.4	23.5	21.5
U.I. Contributions	7.2	8.4	10.7	14.4
Deficit	17.9	40.1	28.8	32.1

NOTES: All figures are in percentages. Program Expenditure series are computed relative to Total Expenditures, while Revenue and the Deficit series are taken relative to total revenues.
SOURCE: Public Accounts of Canada

given as a percentage of total revenue. The starting point for the analysis is 1976 for reasons to be discussed below. In this presentation, total expenditures are divided into Program Expenditures and Debt Service. Income Security covers Family Allowance, Old Age Security, Guaranteed Income Supplement, and Spouse's Allowance, but not U.I. Benefits, which is presented as a separate category. Transfers to Governments is defined here as including Fiscal Arrangements (payments to provinces under the BNA Act and other statutory authority), Health Insurance (including the Insured Health Services Program and Extended Health Care Services), Education Support (containing Post-Secondary Education Payments but not including payments under the Canada Student Loans Act), and the Canada Assistance Plan. It is relatively straightforward to track spending on these items through the Public Accounts over the period indicated. The four series listed under Program Expenditures in table 1 account for roughly 65 per cent of such expenditures in 1992. The other 35 per cent covers other transfers to persons and governments, net expenditures by Crown corporations, and spending by departments and agencies other than National Defence, which is difficult to disaggregate into interesting components that can be followed from year to year in a coherent way.

The issue of the coherence of a spending series is closely tied to the problem of the determination of the most appropriate sample size to investigate. The analysis of the U.I. accounts is an important case in point. U.I. Benefits and Contributions were off-budget items until the fiscal year 1985–6; in other words, total budgetary expenditures as reported before 1985 did not report unemployment benefits, which comprise some 10 per cent of total expenditures. Accordingly, expenditure errors before 1985 did not include errors in the estimation of U.I. payments and would be relatively lower than in subsequent years. It would be incorrect to presume that forecasts during the earlier period were more accurate. Some adjustment to the calculation of total expenditures is necessary to establish the coherence of the series. It would be natural to add the U.I. figures to the earlier spending and revenue estimates and realizations. The problem, however, is that forecasts for the U.I. accounts are not available before 1981. To extend the sample size before 1981, we were forced to exclude the Unemployment Insurance accounts from the total spending and revenue aggregate series.

Prior to 1976 the Old Age Security account was not included in the budget. Since part of tax revenues were earmarked to support the expenditures from this account, these tax receipts were not included on the revenue side of the budget. It would be natural simply to include OAS with the Income Security series before 1976 but for the problem that forecasts for these expenditures and for those revenues reserved for this account were not included as a matter of course in the budget documents of the period. We resolved this problem by fixing on 1976 as the beginning of the sample.

Defence Spending was also excluded from the Program Expenditure series on the grounds that these estimates of spending are less a forecast than a budget constraint. The Defence Spending series, however, is included in the Total Expenditure series.

Total Expenditure series from 1979–80 to 1983–4 (and beyond) contains realizations and estimates on a revised accounting basis. A footnote in the budget tables indicates that the forecasts were made in the light of the accounting changes. It should be admitted that there is a violation of coherence in the numbers before 1979–80 and after, since the former are given in the old accounting basis; but as some historical analysis indicates the differences are minor (*Public Accounts*, 1979–80, 1983–4, I.5, tables 1.1.1 and 1.1.2).

To sum up: the sample considered in this paper runs from 1976 to 1992 (seventeen data points), except for the U.I. accounts which cover 1981 to 1992 (eleven points). The expenditure series considered include Income Security (defined above), Transfers to Governments (defined above), Program Expenditures (excluding Defence and U.I. Benefits), U.I. Benefits and Total Expenditures (excluding U.I. Benefits). The revenue series include Income Tax, Corporate Tax, Excise Tax (including in recent years the GST) and U.I. Contributions. Finally, we also consider the Deficit series without the U.I. account.

In what follows, forecast errors are defined to be the realization of the series minus its estimate all divided by the realization two periods before. Errors are thus taken to be errors in growth-rate estimates rather than in nominal dollars. This procedure makes good economic sense and also permits us to avoid statistical problems associated with non-stationary series. A two-period rate of growth is chosen to reflect the fact that the level attained in the previous period is not known with precision when the forecast is made. The Deficit series is defined in the same way but relative to previous total revenues. The U.I. accounts are handled in levels. The forecast errors, so defined, are depicted in figures 4a to 4l, with the U.I. accounts given in billions of dollars.

As can be seen from figures 4a, 4h, 4i, and 4l, the Income Security, Corporate Tax, Excise Tax, and Total Revenue forecast error series seem to take mostly negative values whereby estimates overstate the realization. Such an error can be viewed more positively for expenditures than for revenues. Series with mostly positive forecast errors include Transfers to Government, U.I. Benefits, and the overall Deficit. The other series display a more balanced mixture of positive and negative errors. It should also be noted that some relative forecast errors are as high as 25 per cent and that large relative errors are not uncommon on the revenue side of the budget. Deficit errors relative to revenue range from modest values during the late 1980s to a 13 per cent underestimate in 1984 and 10 per cent in fiscal year 1992. Whereas in 1984 the sizeable error resulted from an unfortunate combination of underestimation regarding Program Expenditures and overestimation in Income and Corporate Tax revenue, the problem in 1992 can be traced entirely to the revenue side, where all the revenue series were significantly overestimated.

IV. RESULTS

In this section tests for unbiasedness, absence of serial correlation, and efficiency will be applied, in turn, to the budgetary forecast data introduced in the previous section; in each case, the non-parametric results will be contrasted with results obtained by the more traditional regression-based approach.

To begin, the results for parametric and non-parametric tests of bias and serial correlation are presented in tables 2 and 3. With regard to forecast bias on the expenditure side, the regression results indicate that Income Security, Transfers to Government, and Debt Service are biased at the 10 per cent significance level, and the Wilcoxon tests suggest that the forecast errors in these cases are not symmetric, confirming at least for Income Security and Transfers to Government the results of the parametric tests for skewness. The median tests, however, which are robust against asymmetric disturbances, find evidence of bias only in the case of Income Security forecasts. On the Revenue side, the two methodologies concur in finding that Corporate Tax forecasts are biased, but where the traditional regression approach would suggest biased Excise Tax, Total Revenue, and Deficit forecasts, the non-parametric results suggest that errors are asymmetric for these series, which, it should be noted, is not indicated by the parametric tests for skewness.

Both the sign and the regression approaches find evidence of first-order correlation among Income Security forecast errors, as is the case with Transfers to Government on the expenditure side. The non-parametric sign test suggests similar inefficiencies in Income Tax, Corporate Tax, and Total Revenue forecasts, results that are corroborated for Income Tax only by the parametric results. Both approaches find little evidence of second-order correlation among forecast errors in either the expenditure or the revenue side.

To test the external consistency of the forecasts, we used the annual (growth)

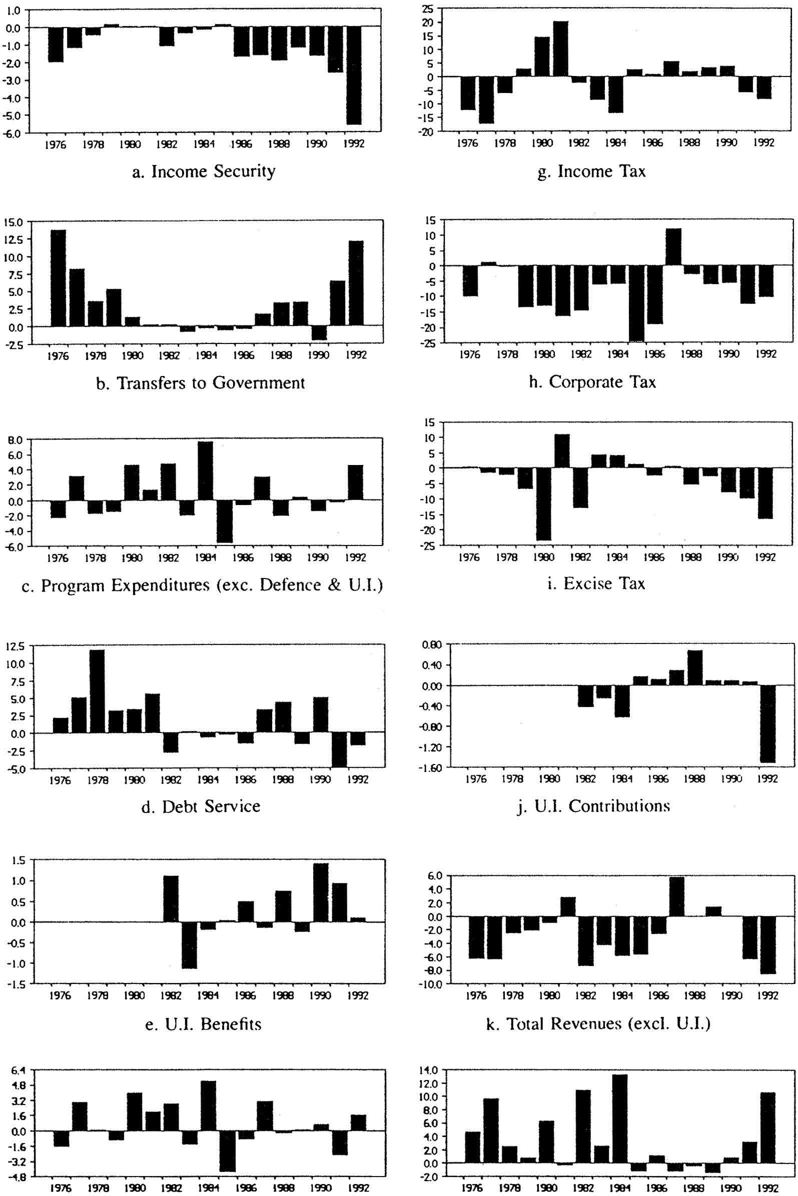


FIGURE 4 Budgetary forecast errors

NOTES

The errors for U.I. benefits (figure 4e) and U.I. Contributions (figure 4j) are given in billions of dollars. For the Deficit (figure 4l), the error is given relative to Total Revenues realized two periods earlier. For all other series the error is relative to the realization of the series itself two periods earlier.

TABLE 2
Expenditure forecasts: tests of bias and correlation in errors

	Income Security	Transfers To Gov.	Program Spending	Debt Service	U.I. Benefits	Total Spending
Descriptive statistics						
Sample mean	−1.24	3.25	0.67	1.79	0.28	0.63
Sample median	−1.15	1.66	−0.27	2.21	0.09	0.06
Sample variance	2.02	20.86	11.56	16.40	0.53	6.25
Sample skewness	−1.85 [0.00]	1.18 [0.07]	0.32 [0.62]	0.65 [0.32]	−0.27 [0.75]	−0.00 [0.97]
Tests of bias						
t-test	0.00	0.01	0.42	0.09	0.23	0.32
Median test	0.05	0.14	0.99	0.63	0.55	0.63
Wilcoxon	0.00	0.01	0.61	0.11	0.37	0.35
Tests of serial correlation						
First order						
ρ_1	0.00	0.03	0.06	0.52	0.60	0.11
SC_1	0.07	0.08	0.21	0.80	0.75	0.21
WC_1	0.00	0.07	0.08	0.32	0.70	0.19
Second order						
ρ_2	0.18	0.86	0.57	0.48	0.83	0.97
SC_2	0.61	0.61	0.99	0.61	0.99	0.99
WC_2	0.08	0.30	0.98	0.19	0.82	0.98

NOTES
In tables 2 and 3, the forecast error for fiscal series Y_t is given by $(Y_t - Y_t^e)/Y_{t-2}$, except for the Deficit series, where the denominator is Total Revenues lagged twice, and for the U.I. accounts, where the error is given in levels (billions of dollars). P-values are given for all tests, including the test for skewness given in square brackets. The non-parametric tests are defined in (1), (3), and (5). The ρ_i tests ($i = 1, 2$) correspond to a t-test on the slope coefficient in the regression of the forecast error on the error lagged once or twice.

TABLE 3
Revenue forecasts: tests of bias and correlation in errors

	Income Tax	Corporate Tax	Excise Tax	U.I. Payments	Total Revenue	Deficit
Descriptive statistics						
Sample mean	−1.13	−8.70	−4.10	−0.124	−2.84	3.65
Sample median	0.59	−9.99	−2.50	0.83	−2.51	2.47
Sample variance	93.53	73.21	69.01	0.337	15.82	22.96
Sample skewness	0.44 [0.50]	0.53 [0.42]	−0.64 [0.33]	−1.39 [0.10]	0.56 [0.39]	0.85 [0.19]
Tests of bias						
t-test	0.64	0.00	0.06	0.50	0.01	0.01
Median test	0.99	0.00	0.33	0.55	0.14	0.14
Wilcoxon	0.61	0.00	0.06	0.97	0.02	0.01
Tests of serial correlation						
First order						
ρ_1	0.04	0.60	0.40	0.58	0.21	0.72
SC_1	0.08	0.08	0.80	0.11	0.08	0.45
WC_1	0.08	0.02	0.38	0.16	0.05	0.13
Second order						
ρ_2	0.55	0.12	0.29	0.75	0.84	0.20
SC_2	0.99	0.04	0.30	0.51	0.61	0.61
WC_2	0.42	0.06	0.14	0.57	0.30	0.05

NOTES
P-values are given for all tests. For details see table 2.

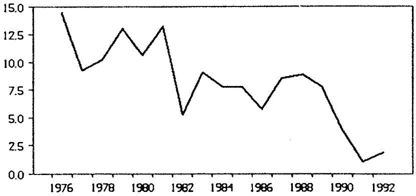
rates for five standard macroeconomic variables: nominal GDP, real GDP, unemployment, CPI, and the three-month T-Bill rate published by Statistics Canada in the spring as the first estimate of the variable for the preceding calendar year. These figures can be assumed to be in the Department of Finance's information set when the fiscal year forecasts are determined in March. The standard parametric procedure to test for forecast efficiency relative to a macroeconomic variable is to regress the forecast error on a constant and several lags of the variable, and to reject the null hypothesis of efficiency if the F-test that all the slope coefficients are zero is significant. In what follows, we take three lags of the variable.

It should be recalled from the previous section that in applying the non-parametric efficiency procedures the macroeconomic variables need to be centred around 0 for the tests to have any power. Here we have centred all the series by taking the distance of first differences from a cumulative moving average of first differences beginning in 1965 as defined in (9). Plots of the five macroeconomic series considered in this study along with their centred versions used in the calculation of the non-parametric statistics used in efficiency tests are given in figures 5a to 5e. The centring approach given by (9) appears to be effective.

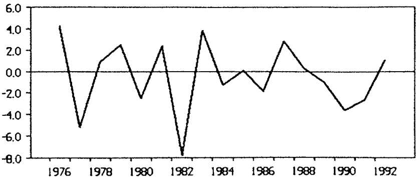
We test eleven of the twelve series for external consistency against the information contained in three lags of each of the macroeconomic variables considered in turn. The Corporate Tax series is omitted on the grounds of bias. In applying the non-parametric procedures, we considered only the sign test as reliable in the case of the Income Security, Transfers to Government, Total Revenue, and Deficit series, since the Wilcoxon tests previously applied to these series have found evidence that forecast errors are biased. Non-parametric statistics defined by (5) are calculated for $k = 1, 2, 3$; here, we reject the null of efficiency if the smallest p-value among the three tests is less than $\alpha/3$. Parametric and non-parametric results are presented in tables 4 and 5.

On the expenditure side, several contrasts between the non-parametric and the parametric results are noteworthy. According to the regression-based approach, Program Spending appears to be inefficiently forecasted with respect to three of the five macroeconomic series considered; these results are not corroborated by the non-parametric findings. It should be recalled that this is exactly the testing environment in which the F-statistic may be found to reject too often, as illustrated by the simulations of Mankiw and Shapiro (1986). On the other hand, the regression-based approach does not suggest as strongly as the sign test does that there exists some relationship between the Income Security forecast errors and the information contained in past movements of the Unemployment Rate. Similarly, the Wilcoxon test suggests that there may be some exploitable relationship between forecast errors associated with U.I. Benefits and nominal GDP.

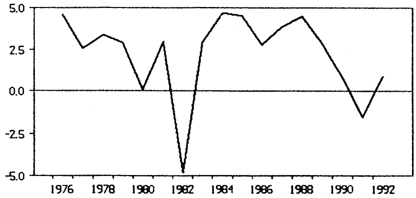
The results differ to some extent as well on the revenue side. The F-statistic is significant in four of the five cases involving the Deficit series; in none of these situations does the sign test reveal any evidence of inefficient forecasting. By contrast, the signed-rank procedures are significant in two cases in the analysis of U.I. Pay-



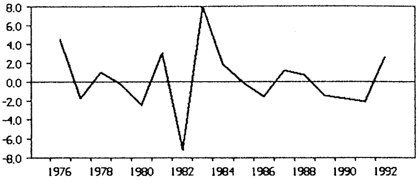
a. Nominal GNP



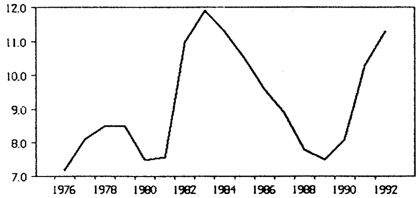
a. Nominal GNP, centred



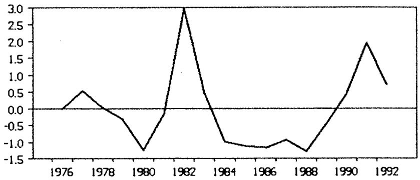
b. Real GNP



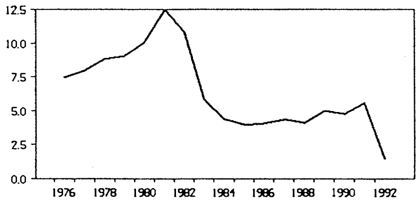
b. Real GNP, centred



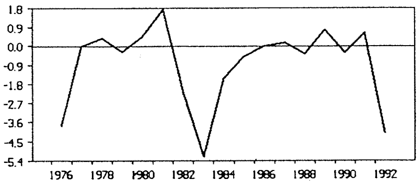
c. Unemployment rate



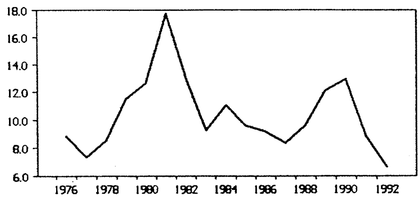
c. Unemployment rate, centred



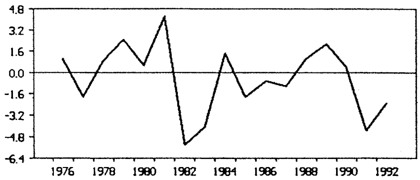
d. CPI



d. CPI, centred



e. Interest Rates



e. Interest Rates, centred

FIGURE 5 Recursively centred macroeconomic series

NOTES

Figures 5a and 5b are given in annual growth rates; all the other series represent rates. The centred representation of each is obtained by taking the distance of first differences from a moving average of first differences; see the text for details.

TABLE 4
Expenditure forecasts: efficiency tests

	Income Security	Transfers to Gov.	Program Spending	Debt Service	U.I. Benefits	Total Spending
Nominal GDP						
F-statistic	0.34	0.24	0.09	0.77	0.29	0.15
Sign statistic	0.63	0.99	0.33	0.14	0.23	0.63
Signed-rank statistic	n.a.	n.a.	0.15	0.12	0.02	0.28
Real GDP						
F-statistic	0.43	0.15	0.02	0.89	0.78	0.08
Sign statistic	0.63	0.05	0.33	0.33	0.99	0.63
Signed-rank statistic	n.a.	n.a.	0.10	0.46	0.52	0.22
Unemployment rate						
F-statistic	0.27	0.42	0.13	0.88	0.45	0.18
Sign statistic	0.01	0.33	0.33	0.63	0.55	0.33
Signed-rank statistic	n.a.	n.a.	0.28	0.28	0.21	0.26
CPI						
F-statistic	0.73	0.37	0.52	0.44	0.29	0.49
Sign statistic	0.99	0.33	0.14	0.33	0.23	0.14
Signed-rank statistic	n.a.	n.a.	0.22	0.19	0.15	0.12
Short interest rate						
F-statistic	0.59	0.56	0.03	0.98	0.19	0.09
Sign statistic	0.14	0.33	0.33	0.99	0.23	0.63
Signed-rank statistic	n.a.	n.a.	0.09	0.75	0.21	0.31

NOTES
P-values are given. The F-statistic is based on a regression of the forecast error on a constant and three lags of the indicated macroeconomic variable. Each non-parametric p-value is the minimum value associated with SO_k or WO_k, defined in (5), among $k = 1, 2, 3$.

ments, and in both cases the F-test does not reject the null of efficient forecasts. Both non-parametric and parametric procedures suggest inefficiency in the case of Income Tax forecasts relative to information contained in the Unemployment Rate.

The rejection of the null of efficiency, according to which there is significant correlation between forecast errors and past information, must be carefully interpreted. Such a result simply suggests an avenue whereby forecasts could possibly be improved. In this context, it strikes us that the reliability of the non-parametric results is critical in that unnecessary revisions of current forecasting procedures will tend to be avoided. Of course, it may turn out that the costs of isolating an economic relation that could be exploited in the forecasting process is prohibitive and that current forecasting practice cannot be improved in the direction suggested by the test result. We wish to emphasize that, as indicated in this empirical study, the non-parametric approach does exhibit power in tests of efficiency and may indicate useful directions for research towards the improvement of forecast performance.

TABLE 5
Revenue forecasts: efficiency tests

	Income Tax	Excise Tax	U.I. Payments	Total Revenue	Deficit
Nominal GDP					
F-statistic	0.03	0.65	0.18	0.18	0.04
Sign statistic	0.33	0.14	0.55	0.33	0.33
Signed-rank statistic	0.12	0.21	0.32	n.a.	n.a.
Real GDP					
F-statistic	0.34	0.98	0.49	0.42	0.01
Sign statistic	0.33	0.33	0.55	0.63	0.33
Signed-rank statistic	0.31	0.31	0.46	n.a.	n.a.
Unemployment rate					
F-statistic	0.03	0.84	0.16	0.04	0.05
Sign statistic	0.01	0.63	0.07	0.14	0.14
Signed-rank statistic	0.01	0.19	0.03	n.a.	n.a.
CPI					
F-statistic	0.12	0.46	0.57	0.65	0.16
Sign statistic	0.63	0.63	0.23	0.14	0.23
Signed-rank statistic	0.22	0.28	0.08	n.a.	n.a.
Short interest rate					
F-statistic	0.14	0.64	0.28	0.79	0.05
Sign statistic	0.05	0.33	0.06	0.33	0.05
Signed-rank statistic	0.08	0.31	0.02	n.a.	n.a.

NOTES
P-values are given. For details see table 4.

V. CONCLUSIONS

The expenditure and revenue forecasts provided each year by the Department of Finance to the federal government are an important part of the budgetary process, which itself figures largely in the public perception of overall government competence, particularly in the fall, when the extent of the past year's forecast errors is made public. Notwithstanding the significance of this annual process, there has been little statistical analysis of the actual fiscal forecasts, with perhaps the unfortunate consequence that the forecasts may be viewed more as strategic positions rather than as intelligent guides to the future.

In this paper we provide a statistical analysis of the forecasts of significant number of components of the federal budget. We found that for such a comprehensive analysis it is difficult to extend the analysis prior to 1979 and that the sample available for statistical analysis is, of necessity, limited. In response, we have described a non-parametric methodology based on signs and signed ranks for evaluating the bias and internal and external efficiency of forecasts. This approach is more appropriate than the standard regression-based procedures used in this context for two reasons. In contrast to the usual tests, the non-parametric tests are reliable in a wider number of circumstances and display comparable or superior power. These points are systematically illustrated in a series of simulation exercises for an appropriately sized sample. It is evident in particular that parametric procedures are unreliable in the presence of asymmetric disturbances and are dominated in power by the sign and signed rank tests. Accordingly, in applied work such as that undertaken in this paper, the regression results should be interpreted with caution and await corroboration from the non-parametric tests.

Applied to the forecasts of the different components of the budget considered in this paper, the non-parametric procedure found strong evidence of bias only in forecasts of the Corporate Tax revenue series. There is evidence that forecast errors are asymmetrically distributed in several of the series. The non-parametric approach finds little evidence that forecast errors are correlated and some evidence (generally in a different direction from that which the parametric tests would indicate) that the forecasts are inefficient with respect to some macroeconomic information. All in all, we conclude that there is little reason to be concerned with the forecast performance of the Department of Finance, at least from the perspective of these measures of forecast adequacy. To be sure, we could be more assertive if the results were based on a longer sample, but there is even less confidence to be had from a regression analysis.

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