Examining Trends of Criteria Air Pollutants: Are the Effects of Governmental Intervention Transitory?*

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Abstract

Understanding time paths of variables is critical in determining optimal regulatory policy as well as developing empirical verification of theories put forth to explain economic phenomena. To provide initial evidence into the temporal behavior of pollutants, we make use of U.S. nitrogen oxide (NO_x) emission data over the period 1900-1994. We find several interesting results that are robust to alternative empirical specifications. First, we find that the emissions series contains both a permanent and random component. Second, intervention analysis suggests that the 1970 Clean Air Act (CAA) did not merely have transitory effects, but permanently influenced the NO_x emission path. In terms of total regulatory impact, we estimate the emissions saved due to the 1970 CAA to be in the range of 27%-48%; EPA's estimated savings figures are *slightly lower* than our preferred estimate.

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1. *Introduction*

Examining the time-series properties of real variables, such as GDP, to determine whether they follow deterministic or stochastic trends has become commonplace amongst macroeconomists. Yet, nearly two decades after Nelson and Plosser's (1982) seminal study regarding stationarity of macroeconomic variables, analysis of environmental time-series data has been sparse.[1] Although data availability represents a serious roadblock, a better understanding of the time paths of pollutants can help shape the development and empirical verification of theories put forth to explain the temporal behavior of emissions. Accuracy of predicting pollution paths has far reaching implications such as aiding in the development of optimal abatement strategies and multilateral bargaining strategies.

Our goal in this paper is to present a dynamic characterization of U.S. emissions for one criteria air pollutant–nitrogen oxide (NO_x)–while simultaneously providing an external validity check of the recent analytically intensive self-evaluation carried out by the Environmental Protection Agency (EPA). Intuition implies that NO_x emissions may have both a permanent and random component. In such a circumstance, EPA mandates can have a permanent and/or a temporary effect on emission levels. If governmental mandates influence only the temporary component, they are futile in shaping long-run pollution trends since pollution flows will revert to their mean. Conversely, if policies affect the permanent component, they are influencing long-term emission growth paths.

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[1] For exceptions, see recent studies of metals use and resource commodity prices (e.g., Labson and Crompton (1993), Labson (1995), and Ahrens and Sharma (1997)).

We use a unique data set that is a compilation of U.S. emission data from 1900-1994 to perform a battery of tests to characterize the stochastic nature of NO_x emission flows. Our empirical findings support the hypothesis that emissions of NO_x follow a unit root process. As discussed in more detail below, this finding has significant implications for the large number of studies that have found a Kuznets curve relationship between environmental quality and income. We also find that EPA mandates have had a permanent effect on emission flows–turning the difference stationary process into a trend stationary process. We conclude by estimating the emissions saved due to the Clean Air Act of 1970. Although our empirical techniques are much less labor intensive than EPA's, our estimates, in the range of 27%-49%, are quite close to those of EPA's (1997) recent self-assessment.

2. Data and econometric methods

As an indicator of previous domestic emission flows, we analyze U.S. per capita NO_x emissions over the period 1900-94. We selected NO_x emissions for our exploratory probe for two reasons. First, NO_x was one of the original five criteria air pollutants targeted for reduction by the Federal government in 1970. NO_x emissions were targeted due to the numerous deleterious effects they have on the general public–including respiratory illness, decreased pulmonary functions, immunological changes, and decreased visibility. Besides many primary effects, NO_x also has a plethora of secondary effects. For example, the primary emission NO_x is oxidized in the air or in cloud-water to form new, secondary compounds, which are acidic (particularly nitric acid) or which add to the ambient levels of oxidants, such as ozone. Emissions of NO_x therefore present an acute problem, as they are a major precursor to groundlevel ozone. Ozone has attracted a large amount of regulatory attention of late due to the limited progress that has been made to reduce ozone concentration levels. A second reason to analyze NO_x emissions relates to data availability. Because of the numerous harmful effects of NO_x, EPA has compiled estimates of domestic emissions of NO_x over the relatively lengthy period 1900-1994. A time-series of this length allows flexibility in the modeling approach and permits an examination of the exogenous factors that induced a change in the emission path, such as EPA mandates.

The emission data come from the *National Air Pollutant Emission Trends* (NAPET), 1900-1994, published by the EPA. The emission estimating methodologies for this time period fall into two major categories: 1900-1984 methodology and 1985-1994 methodology. Emission estimates from 1900-1984 are based on national estimates of economic activity, material flows, consumption of fuel, and in the case of combustion sources, fuel type used. Emissions for the years 1985-1994 are estimated using a "bottom-up" methodology where emissions are derived at the plant or county level and aggregated to the national level. Although a potential shortcoming regarding these data relates to combining indirect estimates with direct estimates, national emission estimates are largely unaffected by this methodological change.[2]

In light of our twin goal of analyzing the effects of the Clean Air Act of 1970 (denoted EPA mandates) and simultaneously examining the time-series properties of emissions, we use two methodologies to examine our time-series. First, we examine whether emissions still contain the mean-reverting property after EPA mandates which took effect in 1970 is taken into

[2] See List (1999) for a more thorough discussion of the NO_x data.

consideration.[3] If emissions are mean reverting, implications are that EPA policies did not permanently change the long-run properties of NO_x emissions. For this purpose, we employ unit root tests by allowing and not allowing for an intervention dummy variable. We then compare results from the unit root tests across these two specifications. The argument in the literature concerns the potential bias of unit root tests in the presence of a structural break–the unit root test is biased toward accepting the false unit root null when an existing structural break is ignored (see, e.g., Perron, 1989 and Amsler and Lee, 1995).

In our second approach, we employ the intervention analysis of Box and Tiao (1975) to determine whether EPA policies have had permanent and/or temporary effects on the growth rates, or levels, of emissions. Intervention analysis has many important advantages. For example, upon EPA intervention it may be difficult to disentangle short- and long-term effects of EPA policies. This problem is handled naturally with the Box and Tiao approach, as the issue of whether the effects of EPA mandates are permanent, gradual, or temporary is self-contained in the model. We conclude by making use of our time-series models to calculate the amount of emissions saved due to the Clean Air Act of 1970.

3. Empirical results

We begin the empirical investigation by testing for a unit-root in log(NOX1).[4] This preliminary analysis is necessary for two important reasons. First, standard inference of the time series model is potentially incorrect in the presence of a unit root–a relevant transformation of

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[3] We follow EPA's (1997) self-assessment and analyze how EPA mandates affected emission flows after 1970.

[4] We transformed the data by taking the logarithms of per-capita emissions: log(NOX1) = log(NO_x/Population).

the time series is necessary to achieve a correct statistical inference if a unit root is present. Second, the existence of a unit root implies that the time series is difference stationary (DS). Whether a time series is DS or not has important implications. A time series that follows a DS process has tendencies to drift (upward or downward) over time exhibiting no trend-reversing properties. Thus, shocks to a variable following a DS process will have a permanent effect on the level of the variable. In contrast, if a time series follows a trend stationary (TS) process, it has tendencies to drift (upward or downward) over time exhibiting trend-reversing properties. The TS process would return to trend after shocks.

Our first two empirical tests for stationarity are the augmented Dickey Fuller (ADF) unit root tests of Said and Dickey (1984) and the Phillips-Perron (PP) unit root tests, which include a drift and/or a trend function. The ADF unit root tests are denoted as follows:

$$y_t = \alpha + \gamma t + \beta y_{t-1} + \sum_{j=1}^{k} c_j \Delta y_{t-j} + e_t \,, \qquad (1)$$

where y_t is the log of NO_x per capita emissions at time t; *a* and *t* are drift and trend components; y_{t-j} is the lagged value of y_t; Δy_{t-j} is the lagged change in the log of NO_x per capita emissions; e_t is the well-behaved error term; and γ, β, and c_j are coefficients to be estimated. Equation 1 is the standard ADF test for a unit root and therefore if $\beta = 1$, shocks to emissions are permanent and emissions have a unit root.

Our second unit root test is due to Phillips and Perron (1989), who make use of transformed statistics using the estimates of two error variances from the regression:

$$y_t = a + gt + b\, y_{t-1} + e_t \,. \qquad (2)$$

The innovation variance is estimated as the error sum of squares from the above regression. The long-run variance is estimated by choosing a truncation lag parameter *l* and a set of weights w_j, j = 1,..,*l*:

$$\hat{\sigma}^2 = \hat{\gamma}_0 + 2\Sigma\, w_j\, \hat{\gamma}_j \qquad (3)$$

where $\hat{\gamma}_j$ is the jth sample autocovariance of the residuals from (2). Using these estimates of nuisance parameters, PP provide transformed tests of the Dickey-Fuller statistics. Precise definitions of the transformed PP statistics, $Z(\tau)$ and $Z(\alpha)$, are provided in PP (1989, p. 1382).

The plot of the data in Figures 1a and 1b indicate a trend function for log(NOX1). Nonetheless, we consider testing procedures with both types of models, one with trend, and the other with drift. Table 1 presents results of our initial unit root tests. Because results of the ADF tests depend on the number of augmentations, we consider various lags from 0 to 12, and report results using 4, 8, and 12 augmentation lags. In addition, we compute the Akaike and Schwarz information criteria (AIC and BIC), and report these statistics for each model type. Overall, at the $p < .05$ level, our results suggest we should not reject the null hypothesis of a unit root in all of the ADF tests.

The bottom panel of Table 1 presents results from the Phillips and Perron (1989) unit root tests. We employ 4, 8, and 12 truncation lags for the PP tests, and use the optimal bandwidth lags of Andrews (1991), and the Fejer kernel in estimating the long-run variance for the PP tests. The results are essentially unchanged. Overall, the empirical findings suggest quite strongly that the NO_x emission series is non-stationary, or contains a unit root. These results, however, were obtained by not taking into account of the apparent structural change in 1970.

Have EPA mandates been effective?

As previously mentioned, one test of EPA effectiveness is to examine whether the 1970 EPA mandates have permanent impacts on emissions. One way to achieve this goal is to analyze if the series is difference stationary after allowing for the policy dummy variables. We follow Park (1995), and consider the breaking slope and crash model which includes dummy variables D_t and B_t ($=\Delta D_t$), and DT_t^* ($=t \cdot D_t$), in addition to a constant and a trend (t) term in the ADF unit root testing regression (see Perron 1989).[6] Park's (1995) test is similar in spirit to PP, except additional dummy variables are included in the testing regressions. As suggested by Park (1995), the testing equations become:

$$y_t = a + gt + d_0 B_t + d_1 D_t + d_3 DT_t^* + b\, y_{t-1} + e_t\ , \qquad (4)$$

Table 2 presents estimation results for the unit root tests with intervention. While we report empirical results using different lags, we focus on results using the optimal bandwidth lag. In Table 2, we see that we can reject the unit root hypothesis at the conventional $p < .05$ level for each of the augmentation levels as well as the model using the optimal bandwidth lag at the $p < .10$ level.[7] Therefore, our findings show that while the emission series could be seen as non-stationary, it is now seen as trend-stationary when the structural break in 1970 is accounted for.

These findings may have important implications on model specifications for the large number of empirical studies that have regressed a measure of environmental quality on wealth

[6] Following EPA (1997), we set $D_t = 1$, for $t \geq 1970$, and $= 0$ otherwise.

[7] We also considered Perron's ADF type test with 4, 8, and 12 augmentations; results are similar. Given that the break year is known, and has been exogenously imposed, we do not have to consider the so-called endogenous unit root tests in which break points are determined from the data.

(the environmental Kuznets curve (EKC) literature; see, e.g., Selden and Song (1994), Grossman and Krueger (1995), and Hilton and Levinson (1998)). A brief review of the burgeoning EKC literature reveals that stationarity is assumed in the majority of studies. Our findings suggest that this assumption may be erroneous, and their conclusions are potentially misleading–the inverted-U shape association between pollution and wealth may be due to spurious correlation, given that pollution is seen as non-stationary if the structural change in 1970 is not accounted for. Thus, variables of interest may be seen as being neither purely stationary nor purely non-stationary. The inference hinges on whether structural changes are allowed for. Therefore, our results imply that if the environmental time series used in these studies have similar properties, a similar intervention term should be allowed for.

Most importantly, in light of our conjecture—if the data can be viewed as a mean-reverting process after allowing for policy dummy variables, then our empirical results strongly support the perceived effects that EPA had on emissions trends.[8] Thus, we conclude that 1970 EPA mandates have permanent impacts on emissions and that they have been effective.

Have EPA mandates had a permanent effect on emission levels?

Our second empirical method extrapolates information from the time series by using Box and Tiao's (1975) intervention analysis approach. One important advantage of this method is that we can examine whether EPA mandates had a permanent, abrupt, gradual, or temporary impact on emissions. As our above results indicate, log(NOX1) is a difference-stationary time-

[8] Our result is reinforced by the unit root test results applied to the pre-1970 data–test results using the 1901-1970 (or 1901-1960) data, not shown here to conserve space, indicate the presence of a unit root.

series, leading us to use the first difference data Δlog(NOX1) to achieve stationarity for the intervention analysis. Despite this obvious choice of the first difference data, we also consider data in levels since log(NOX1) may be a trend stationary process after allowing policy dummy variables. We therefore consider two different models for the intervention analysis.

Our first model type is the ARIMA(p,1,q) model where the first differences of the data, Δlog(NOX1), are used. Our second model type is the ARIMA(p,0,q) model where the level data, log(NOX1), are used. To facilitate estimation of the ARIMA models, we eliminate the deterministic portion of the time series by subtracting the mean of the first difference data, and de-trending the level data. The dummy variable coefficients in the analysis are therefore interpreted as deviations from the mean for the first differenced data and deviation from the trend function for the level data.

To estimate the intervention models, we consider the usual approach[9]: identify the ARIMA models without intervention terms, and estimate the intervention models based on *a priori* identified models. For this task, we use the *ITSM (Interactive Time Series Modelling)* 6.0 software developed by Brockwell and Davis (1991) to identify and estimate the ARIMA models. For model identification purposes, we use the Akaike Information Criterion (AIC) to identify appropriate values of p and q in our ARMA (p,q) models. Using maximum likelihood estimates, we allow p, q = 0,1,.,4, and choose p and q that minimize Akaike's AIC. We then use numerical optimization procedures to obtain maximum likelihood (ML) estimates of the parameters of our ARMA models, with selected orders p and q along with preliminary ML

[9] We also considered joint model identification by determining the orders of the ARMA and intervention terms. This method appears a superior choice, but the ARIMA model estimations from this method often

estimates. If fitted models are satisfactory, then the residuals should have the appearance of a realization of white noise. As a diagnostic check, we use the Portmanteau test of Box and Pierce (1970), and other nonparametric tests for randomness of residuals.[10] We find that the ARIMA(1,1,0) model provides a best fit for the logged, first difference data. For the level data, however, we find the ARIMA(2,0,0) model is the superior choice.

We consider different specifications for permanent, gradual, and temporary impacts as follows:

$$\phi(L)y_t = \frac{\omega(L)}{\delta(L)} Z_{1t} + \frac{\varphi(L)}{c(L)} Z_{2t} + \theta(L)u_t \tag{5}$$

where $\phi(L) = 1 - \phi_1 L - .. - \phi_p L^p$, $\omega(L) = \omega_0 + \omega_1 L + .. + \omega_r L^r$, $\varphi(L) = \varphi_0 + \varphi_1 L + .. + \varphi_n L^n$, $\delta(L) = 1 - \delta_1 L - .. - \delta_m L^m$, $c(L) = 1 - \delta_1 L - .. - \delta_s L^s$, and $\theta(L) = 1 - \theta_1 L - .. - \theta_q L^q$; and L is a lag operator such that $L^k x_t = x_{t-k}$. Under this formulation, $\phi(L)$ and $\theta(L)$ denote the usual polynomials for the ARMA models, and $\omega(L)$ and $\varphi(L)$ are polynomials for the intervention terms. The polynomials $\delta(L)$ and $c(L)$ capture gradual impacts of EPA intervention. The variable y_t represents Δlog(NOX1) for the first difference data, and represents log(NOX1) for the level data. The first term on the right hand side includes the intervention terms, where Z_{1t} and Z_{2t} denote the exogenous policy variables. We define $D_t = 1$ for $t \geq 1970$, and 0 otherwise; and $B_t = 1$ for $t = 1970$, and zero otherwise. We also define $DT_t^* = t$ for $t \geq 1970$, and 0 otherwise.

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fail because the models are not causal. Thus, we adopt a two-step procedure: i) identify the orders of the ARMA terms; ii) identify orders of intervention terms based on *a priori* specified ARIMA models.

[10] These include the turning points test, the difference sign test, and the rank test (Brockwell and Davis (1991)).

For the ARIMA(p,1,q) model, the first difference data, Z_{1t}, includes the policy dummy variables such that $Z_{1t} = D_t$ for the permanent effects of interventions, while Z_{2t} is not included. To capture the temporary impacts, we set $Z_{1t} = B_t$, and Z_{2t} is absent; therefore ω_i and δ_i estimate the initial abrupt impact and the gradual dynamic response of an event of the observed time series. For the ARIMA(p,0,q) model, the level data, $Z_{1t} = D_t$, and $Z_{2t} = DT_t^*$, is used to examine the permanent effect of interventions. The temporary effects are not meaningful here, and therefore ignored. In this case, ω_i and δ_i estimate the initial abrupt impact and the gradual dynamic response of an event on the level of the observed time series, whereas φ_i and ς_i capture the same effects on the *trend* shift of the observed time series.

We first discuss the results of the intervention analysis with the ARIMA(p,1,q) specifications, for which the first difference data, Δlog(NOX1), are used. Table 3 presents these results. The results indicate that the estimated coefficients of ω_0 are insignificant at conventional levels in all four model types. In models (A) and (B), this finding suggests that abrupt impacts are not significant, while interpretations from models (C) and (D) are that gradual effects are insignificant. The coefficients of δ_1 and δ_2 are, however, significant at conventional levels for both permanent and temporary changes.[11] This result implies that EPA mandates had gradual effects on emission flows. Examining the magnitude of these coefficients, we observe that their sum is close to zero for the model with gradual and temporary effects, while it is significantly different from zero for the model with gradual and permanent effects.

[11] To identify the maximum orders for $\delta(L)$ we considered different orders and found two lags was most appropriate.

Combining these results, we conclude that EPA intervention had gradual and permanent changes on the *growth rates* of per capita NO_x emissions.[12]

Table 4 presents empirical results from models using the level data. In this model, the estimated coefficient of ω_0 is significant at conventional levels, implying abrupt and permanent effects in the NO_x time series. Yet, ω_0 is insignificant in the gradual and permanent models. In this case, however, the estimated coefficient of δ_1 is highly significant. Overall, these results indicate that EPA mandates have led to an abrupt change in the *level* of emissions of NO_x, as well as an abrupt change in its trend. Alternatively, these results can be viewed as implying that EPA mandates had gradual and permanent effects on emission *levels*, and abrupt and permanent changes on their trends.

In sum, our results suggest that the events surrounding 1970 led to gradual and permanent changes on the NO_x emission time path. This makes sense, as one would expect that NO_x emissions would take time to adjust to the new rules and regulations imposed in the 1970 Clean Air Act (CAA). The 1970 CAA contained a number of key provisions. Given that motor vehicle emissions and fuel combustion accounted for approximately 85% of all NO_x emissions in 1970, the main provisions for our purposes entail reductions from these types of sources. Concerning the former, the 1970 CAA called for "at least a 90 per centum [reduction] from the average of emissions of oxides of nitrogen actually measured from light duty vehicles manufactured during model year 1971" (CAAA, PL 91-604, p. 1690). To achieve this goal, the EPA stated that emissions standards must be met 5 years or 50,000 miles after

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[12] Note that we are using first differences of the logged data; thus it is appropriate to speak of changes in growth rates.

vehicle purchase. Given that highway vehicle emissions depend on fuel type, vehicle type, technology, and extent of travel, and that vehicle activity levels are related to changes in economic conditions, fuel prices, cost of regulations, and population characteristics, emissions are a function of vehicle activity levels and emission rates per unit activity. A gradual change in emissions from on-road vehicles is therefore expected since the turnover rate in the motor-vehicle fleet is not instantaneous.

The second major source of NO_x in the U.S. is fuel combustion by industrial and electric utilities. The 1970 CAA also allowed for a gradual decrease in emissions from these sources. In particular, EPA established National Ambient Air Quality Standards for each criteria air pollutant. For NO_x, the primary standard was initially .053 PPM annual mean (maximum human exposure). The EPA created emissions limits and monitoring for stationary sources to meet these criteria *within due time*, allowing NO_x emitters to gradually meet requirements.

How much has been saved due to the Clean Air Act of 1970?

Given that our results suggest that EPA mandates had a permanent effect on emission flows, an exploratory probe can be conducted to calculate how large an effect EPA has had on reductions of NO_x emissions. The EPA recently estimated the retrospective costs and benefits of the 1970 CAA between 1970-1990.[13] This analytically intensive self-evaluation found that Federal regulatory actions led to substantial savings in criteria air pollutants. This section

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[13] Section 812 of the CAAA of 1990 requires the EPA to assess periodically the effect of the Clean Air Act on the "public health, economy, and environment of the United States," and to report subsequent findings to the Congress (EPA (1997)).

reconsiders how Federal intervention affected estimated savings in nitrogen oxides during the regulatory era. We employ the ARIMA forecasting models to examine this issue. We estimate ARIMA models using emission data from pre-EPA mandate years–1900 to 1969–to obtain outside sample forecasts for 1970 to 1994. We compare these outside sample forecasts with actual values of emissions. Akin to EPA (1997), we assume that the emission time series would have followed the patterns of the outside sample forecasts absent EPA mandates. By doing so, we assume that the differences between the outside forecasts and actual values are due to EPA mandates.

The first important problem in such an exercise is to find the most appropriate ARIMA (p,d,q) model to represent the time series. As elaborated on above, since the time series involves both deterministic and stochastic trends, a transformation to produce a new stationary series with mean zero is obligatory. Since the series contains a unit root, we use first differences (d=1), $\Delta\log(NOX1_t)$ to eliminate any stochastic trends. For comparison purposes, we also use the level data (d=0), $\log(NOX1_t)$.[14] We follow a similar procedure to identify appropriate values of p and q in our ARMA (p,q) models. The ML estimates corresponding to the selected orders, and test statistics for randomness of residuals are in Table 5. Results in Table 5 provide vivid evidence of model appropriateness–all coefficients in the ARMA models are significant at conventional levels. Furthermore, for each test and for all residuals, we cannot reject the null

[14] Inspection of the graph of the series reveals that a linear trend is included in $\log(NOX1_t)$. Thus, we detrend the series with a linear trend function in modeling the logged data, and use the transformed series in estimating ARMA models. For the first difference series, a trend function may not be necessary; thus, we demean the series with a drift term before estimating ARMA models.

hypothesis of a white noise process. This is compelling evidence that the model is a "good fit" for our data.

Our next step is to compute best linear *h*-step predictors by using the estimated model to predict values from 1970 to 1994. Since the prediction is established with transformed data, a transformation inversion is necessary to fit a zero-mean stationary model. Relevant inverted transformations include de-trending, de-differencing, and taking exponentials of the logged series. The inverted transformation allows us to obtain predicted values in original form, allowing a comparison between forecasted emissions and actual values. Since our data are measured as emissions per capita, we again multiply by US population to evaluate emissions and reductions in emissions.

Figures 1a and 1b display plots of forecasted emissions, actual emissions, and their differences, for each estimated model. An important finding is that we observe predicted values absent EPA mandates to be considerably above actual emissions levels after 1970. The difference between these two trends is considered the emissions saved due to the 1970 CAA. We summarize the mean emissions reductions for 1970-1990 in Table 6. Also in Table 6 are quinquennial estimates of saving figures from EPA's (1997) self-assessment. Comparison of empirical results shows that the estimate from Case 1 using the levels data, is very close to estimates from EPA's assessment. These figures suggest that by 1990 the CAA of 1970 had provided a total emission savings figure of 27% for NO_x.[15] Somewhat surprising is the fact that our relatively simple procedures produced estimates that closely resemble EPA's savings figures

[15] The Percentage of emissions saved for both the EPA and our estimates are calculated using the following rule: $(NC_{it} - C_{it})/NC_{it}$; where NC_{it} (C_{it}) represents the no-control (control) estimate of NO_x in time period t.

computed from very analytically intense methods. Meanwhile, the estimated emissions saved from Case 2 using the differenced data are nearly 48%. In this case, the estimated savings are much higher compared to estimates from the model based on the level data.

Accuracy of predicting pollution paths has far reaching implications, such as aiding in the development of optimal abatement strategies and multi-lateral bargaining strategies. In our analysis of NO_x emissions, two quite different predictions are provided from our models, which is not atypical (see, e.g., Diebold and Senhadji, 1996, and Diebold and Kilian, 1999). The results suggest that trend- and difference-stationary models of the same time series often provide different point forecasts, and that pre-testing improves forecasting accuracy relative to routinely differencing or using level data. Following Diebold and associates, we are inclined to adopt the estimates based on the model using the differenced data, since the pre-testing result indicates that the time path before 1970 EPA mandates is difference stationary. Consequently, EPA's estimated savings figures for NO_x are *slightly lower* than our preferred estimates.

4. Concluding remarks

Before commencing a rigorous assessment of theory or specifying an econometric model, a thorough investigation of the time series properties of the data has become almost second-hand amongst many economists. Yet, to date little has been done to further our understanding of our own time series data, where a proper understanding of certain time series is invaluable to forward optimal policy prescriptions. We present a battery of tests on U.S. NO_x emission data from 1900-1994 to provide a first examination of the time-series properties of air pollutants. Our analysis provides many new insights.

First, in certain respects, our empirical results suggest that NO_x emissions can be seen as being neither purely stationary nor purely non-stationary. The inference hinges on whether EPA mandates are accounted for or not. This finding is fundamentally important when developing appropriate modeling strategies to verify theories of optimal abatement strategies. Second, we find that the relationship between EPA mandates and NO_x emissions are of a deep nature, such that changes in EPA mandates have had a permanent effect on the path of NO_x emissions. This finding speaks well of recent EPA policies, and refutes the argument that governmental mandates have a transitory effect, or influence only the temporary component of long-run pollution trends. We conclude by estimating emission savings figures due to the 1970 Clean Air Act. Our estimates are consonant with EPA's recent estimates–verifying the premise that the U.S. has avoided about 48% of NO_x emissions due to the Clean Air Act of 1970.

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Table 1

Unit Root Tests Without Intervention

		Log(NOX1)	
ADF Tests	Augmentation Lags	With trend	With drift
	0	-2.84	-1.92
	4	-1.84	-1.80
	8	-2.60	-1.66
	12	-1.93	-1.71
	AIC[a]	-1.71 (1)	-1.96 (1)
	BIC[a]	-1.71 (1)	-1.96 (1)
PP Tests	Truncation lags	With trend	With drift
	4	-2.65	-2.03
	8	-2.93	-2.01
	12	-3.00	-2.02
	Optimal lag[b]	-2.43 (3)	-2.06 (4)

* significant at the 10%, ** significant at the 5%, *** significant at the 1%

Note: a) Selected lags are given in parentheses.
b) Selected optimal bandwidth lags of Andrews (1991) are given in parentheses. The Fejer kernel was used for estimating the long-run variance.

Critical Values of the ADF and PP Tests at the 10%, 5%, and 1% are
-3.155, -3.458, - 4.058 for models with trend, and
-2.583, -3.892, -3.501 for models with drift.

Table 2

Unit Root Tests With Intervention

		Log(NOX1)
Park's PP Type Tests	Truncation lags	Breaking Slope & Crash
	4	-4.50**
	8	-4.74**
	12	-4.71**
	Optimal lag[b]	-4.16* (1)

* significant at the 10%, ** significant at the 5%, *** significant at the 1%

Note: a) Selected lags are given in parentheses.
b) Selected optimal bandwidth lags of Andrews (1991) are given in parentheses. The Fejer kernel was used in estimating the long-run variance.
c) Critical Values of Perron's and Park's Tests at the 10%, 5%, and 1% are -3.86, -4.18, -4.75 for models with breaking slope and crash.

Table 3

Intervention Analysis with ARIMA (p,1,q) Specification

Δlog(NOX1)	ARIMA(1,1,0) with Interventions			
	ϕ_1	ω_0	δ_1	δ_2
(A) Abrupt & Permanent $Z_{1t} = D_t$, $\delta_i = 0$	$-.389^*$ (-4.03)	-.014 (-1.58)		
(B) Abrupt & Temporary $Z_{1t} = B_t$, $\delta_i = 0$	$-.373^*$ (-3.84)	.012 (.209)		
(C) Gradual & Permanent $Z_{1t} = D_t$, $\delta_i \neq 0$	$-.395^*$ (-4.04)	-.002 (-.495)	1.92^* (4.27)	$-.962^*$ (-2.30)
(D) Gradual & Temporary $Z_{1t} = B_t$, $\delta_i \neq 0$	$-.380^*$ (-3.84)	.020 (.744)	$.992^*$ (5.64)	$-.937^*$ (-3.95)

* significant at the 5% level. Numbers in parentheses are t-statistics.

Table 4

Intervention Analysis with ARIMA (p,0,q) Specification

Log(NOX1)	ARIMA(2,0,0) with Interventions					
	ϕ_1	ϕ_2	ω_0	δ_1	φ_0	c_1
(A) Abrupt & Permanent $Z_{1t}=D_t$, $Z_{2t}=DT_t^*$, $\delta_i=0$, $c_i=0$	.517* (5.0)	.295* (2.9)	.871* (2.6)		-.018* (-2.7)	
(C) Gradual & Permanent $Z_{1t}=D_t$, $Z_{2t}=DT_t^*$, $\delta_i\neq0$, $c_i\neq0$	.483* (4.7)	.285* (2.8)	.130 (.05)	.861* (4.2)	-.001 (-.03)	.911 (.56)

* significant at the 5% level. Numbers in parentheses are t-statistics.

Table 5

Estimated ARMA Models

	Estimation Results
Case 1	$\log(NOX1_t) = \underset{(.113)}{.4799} \log(NOX1_{t-1}) + \underset{(.114)}{.2981} \log(NOX1_{t-2}) + \hat{u}_{1t}$ $\hat{\sigma} = .0043$, -2 logL = -181.9, Q(20) = 12.45 Turning point = 39, $N(45.3, 3.48^2)$ Difference Sign = 40, $N(34.5, 2.43^2)$ Rank test = 1244, $N(1207.5, 295.9^2)$ AIC = -175.49
Case 2	$\Delta\log(NOX1_t) = \underset{(.110)}{.4172} \log(NOX1_{t-1}) + \hat{u}_{2t}$ $\hat{\sigma} = .0046$, -2 logL = -174.7, Q(20) = 14.3 Turning point = 40, $N(44.7, 3.46^2)$ Difference Sign = 38, $N(34.0, 2.42^2)$ Rank test = 1165, $N(1173, 289.6^2)$ AIC = -170.52

Table 6

Comparison of Emission Savings (Percent of Total) for Selected Years [a,b]

	EPA Quinqennial Estimates					*Our Quinqennial Estimates*			
Pollutant	1975	1980	1985	1990		1975	1980	1985	1990
NO_x	2.73	9.52	20.78	28.53	Case 1	-1.44	2.07	14.90	26.70
					Case 2	7.13	14.74	32.16	48.44

[a] Although our model provides annual savings, the EPA's estimates are quinqennial, from which they linearly interpolate other years. For the sake of comparison, therefore, we also only report quinquennial figures.
[b] The Percentage of emissions saved for both the EPA and our estimates are calculated using the following rule: $(NC_{it} - C_{it})/NC_{it}$; where NC_{it} (C_{it}) represents the no-control (control) estimate of emission i in time period t.
[c] Case 1 is using level data and Case 2 uses difference data.

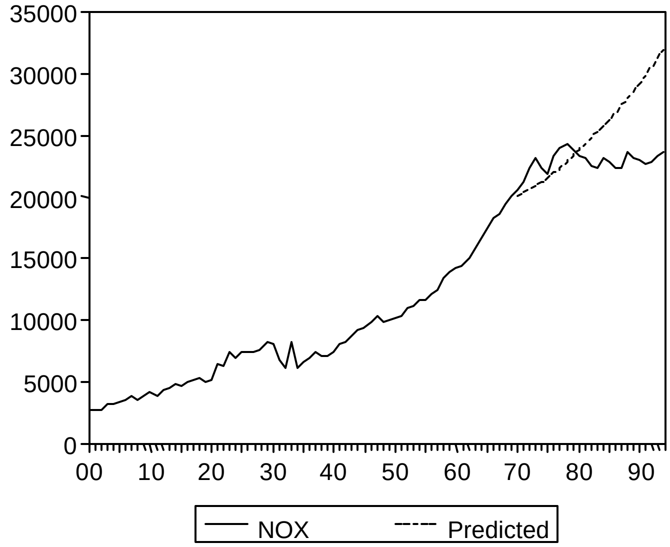

Figure 1a. Actual and Predicted NO_x Emissions (Case 1)

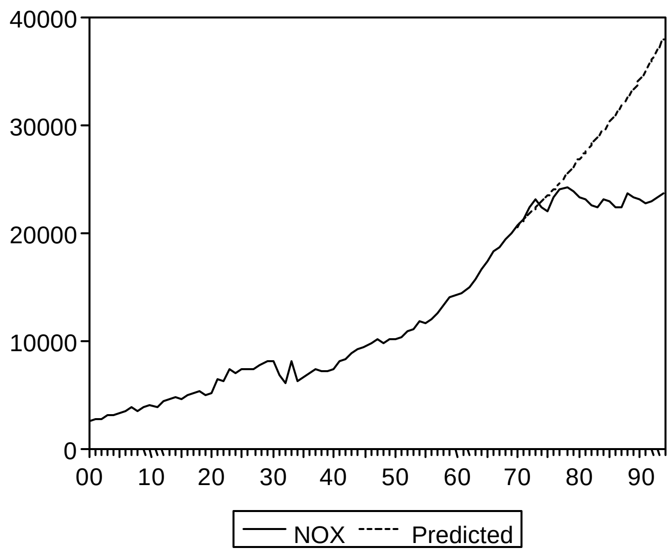

Figure 1b. Actual and Predicted NO_x Emissions (Case 2)