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DOES FOREIGN AID AFFECT THE ENVIRONMENT IN DEVELOPING ECONOMIES?

B. MAK ARVIN, PARVIZ DABIR-ALAI, AND BYRON LEW*

Trent University, Richmond University, and Trent University

Preserving the environment is important from both national and international perspectives. Similarly, the provision of foreign assistance from richer to poorer nations is often seen as an imperative. However, there is a noticeable gap in research on how aid flows are linked to the environment in developing economies. Using the method of Granger causality, this paper explores the possible linkages. Results indicate that the external debt of a developing country bears upon the relationship in important ways. The second part of the paper entertains the possibility of spurious causality, tests for cointegration, and present additional results using an error-correction model.

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1. INTRODUCTION

The notion that foreign flows influence the environment in developing countries is not a new line of investigation. The literature, however, concentrates almost exclusively on the relationship between trade and the environment. For example, Lofdahl (2002) investigates whether international trade helps or hurts the environment. Copeland and Taylor (2000) establish a framework under which the impact of trade liberalization on an economy's adopted environmental standard can be assessed.[1] Their most important prediction is that, at the national level, income gain (motivated through additional trade) affects pollution levels differently than income gain achieved through economic growth. The counter-part finding they also report is that economic growth affects pollution levels differently under free trade than under autarky. However, of particular interest to this work is their finding that economic growth in richer countries is likely to have very

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[1] See Copeland and Taylor (2004) for a useful survey of all the issues surrounding trade and the environment.

different environmental effects than economic growth in poorer countries. This has the potential for encouraging controversial, but innovative all the same, international policy trade-offs, whereby a poorer economy may be persuaded to accept some aid in return for sacrificing some of its growth (and hence ability to pollute).

Surprisingly, few studies explore the possible direct relationship between foreign aid and the environment. On the theoretical side, the literature consists of a few papers examining the welfare effects of tying aid to environmental clean up (see, for instance, Chao and Yu (1999)). In the same vein, Hatzipanayotou, Lahiri, and Michael (2002) develop a two country model of aid and cross-border pollution resulting from production activities in the recipient country. They characterize a Nash equilibrium for the donor and recipient country with respect to aid and pollution abatement. Their paper reveals that the medium and longer term impact of cross-border pollution can lead to reductions in the total amount of emissions by encouraging greater levels of international transfers such as aid. Using a scenario-based framework, their paper suggests that these performance-driven transfers can be used to promote a more sensible approach to the design of pollution policy in the polluting country.

Contributions by Branden and Bromley (1981), Hoel (1991), and Dockner and Long (1993) enrich the theoretical strand of the literature considerably. These studies maintain that the utility of an individual country is affected by the worldwide aggregate emission of harmful materials as well as by their reduction. The main problem with this approach is that changes in welfare are measured through a simple additive process. However, a useful implication is that if a group of countries has a comparative advantage in reducing the levels of regional or global pollution, then it would make sense (that is, it would be economically more efficient) for this group to receive resources from those economies less able to combat regional or global pollution. Niho (1996) suggests that whether international transfers improve global levels of environmental quality depend largely on the marginal rate of substitution between the environmental qualities of the trading partners as well as on the relative efficiency in the technology of reducing pollution in the recipient country to that in the donor country.

There is also dearth of studies on the empirical front. Most of the studies deal with the relationship between foreign aid and economic growth and, given the often transitive nature of the link between growth and the environment, consider only implicitly the nexus between aid and the environment. For example, Addison, Mavrotas and McGillivray (2004) suggest that the record on growth (and by extension both pollution and poverty) would have been lower in recent years amongst developing countries, if the amount of official aid had been lower as well. In the context of aid the argument is simple: If environmental quality is a normal good, then poorer countries tend to adopt lower environmental standards. By increasing income in poorer economies, aid can then raise these standards. Related to this is the following argument. Since environmental degradation in many poorer countries can be related, *inter alia*, to lack of funds for environmental clean-up and preservation, aid has a role to at least decelerate such degradation. At the same time, aid may have a deleterious impact on the environment in poorer countries if polluters in relatively well-regulated richer countries seek to relocate their operations to low-income countries whose governments may turn a blind eye to environmental transgressions - in return for aid from richer countries - so as to meet their employment and income priorities. While these types of arguments may be perceived to be generally correct, there appears to be no empirical literature providing a test of their validity through sensitivity, or any other form of quantitative analysis.

As previously mentioned, pollution does not respect national boundaries affecting both the polluters as well as other countries around the globe. Despite this, establishing globally acceptable standards of pollution control, and finding influential signatories, has proven to be a problem. Adoption of the various elements of the Kyoto Protocol has been patchy particularly amongst some of the world's richest and most prolific polluters.[2] Therefore, much of the debate has focused on how emissions within poorer countries may be brought under greater control. Besides the arguments presented in the preceding paragraph, the levels of pollution produced by developing countries may be affected by income transfers (such as aid) from richer countries in two ways. On the one hand, these transfers may lead to unsustainable development at an excessive pace, leading to environmental and ecological degradations. A manifestation of this may be seen, for example, in the acceleration in the rate of exploitation of an economy's natural resource base. This will bring into question the whole issue of time-preference in the use of environmental assets.

A contrary view suggests that these transfers may not only reduce poverty, but encourage greater care of natural resources by the poorer nations. In this connection the work of Asafu-Adjaye (1999) on the environmental Kuznets curve (EKC) is relevant. The EKC, based on the general principles established by the original Kuznets relationship - linking concepts of (income) inequality to measures of development and growth - argues that pollution levels rise in the early stages of development but recede subsequently. Asafu-Adjaye (1999) provides useful estimates of the so-called 'turning points' so as to see the level of development necessary before pollution levels start their decline. As might be expected, he finds that the general thrust of the EKC - and the associated turning points - vary by both country and pollutant type.[3] However, a number of other general insights affecting the conduct of development policy are unearthed. For example, Asafu-Adjaye notes that the EKC relationship is fairly robust when environmental quality is pitched against income. Related to this is the observation that for a large number of developing countries the per capita GDP is significantly below the

[2] This non-compliance has been motivated on the basis of basically two issues. First, it has been claimed by senior members of the US political administration that the harmful atmospheric and environmental consequences of many pollutants have been exaggerated. Second, it has also been argued that the cost to the US economy of meeting Kyoto's provisions is simply too high, thus rendering them unattractive.

[3] See the 1997 special issue of *Environment and Development Economics*, devoted to the environmental Kuznets curve, for additional empirical studies on this topic.

predicted turning points. This suggests that environmental problems in developing economies will more than likely deteriorate over coming years and decades.

The aim of the first part of this paper is to employ the method of Granger causality in order to determine the causal linkage between foreign aid and environmental condition in developing countries. That is, the study seeks to establish whether aid flows impact the environment, whether the environment influences aid flows (i.e., whether donors take into account the environmental condition of a recipient country when disbursing aid), or whether causality proceeds in both directions simultaneously. The use of the Granger methodology is justified by the finding that it is more powerful than alternative tests (see, for example, Geweke, Meese, and Dent (1983)). In addition to conducting a bivariate Granger causality test, the study also introduces a third variable, thus forming the framework for a trivariate causality test. The third variable in this study is the developing countries' level of external debt.[4]

The second objective of this study is to check for spurious causality and non-causality between aid and the environment in developing countries. Therefore, the second part of the paper uses an error-correction model in order to derive results for individual countries.

The balance of the paper is organized as follows. Section 2 describes Hsiao's version of Granger causality, the one that is employed in the first part of this study. Data and sample characteristics are discussed in Section 3. Section 4 presents the empirical results from the standard causality test. The second part of the paper, beginning with Section 5, allows for possible cointegration between the variables and presents individual country results. Conclusions are drawn in Section 6.

2. HSIAO'S VERSION OF GRANGER CAUSALITY

Granger causality as described by Granger (1969, 1980) is based on the principle that if, after conditioning a variable on its own past values, the addition of another variable's current and past values further reduces the prediction error variance, then the additional variable is said to Granger cause the first. Hence, according to this definition, X causes Y if the precision of the estimated current value of Y (denoted by Y_t) is improved by controlling for current and past values of X. That is, the use of temporal information enables one to say something about the direction of causality. A symmetric statement can be made for Y causing X.[5] The regressions for Y and X are

[4] There are many examples of trivariate causality tests. In the context of foreign aid, recent studies include Arvin and Barillas (2002) and Arvin, Cater and Choudhry (2000).

[5] See, for example, Pierce and Haugh (1977) and Hamouda and Rowley (1997) for a discussion of various issues concerning Granger causality.

$$Y_t = a_0 + \sum_{i=1}^{n} a_{1i} Y_{t-i} + \sum_{j=0}^{m} a_{2j} X_{t-j} + u_{1t} , \tag{1}$$

$$X_t = b_0 + \sum_{i=1}^{n} b_{1i} X_{t-i} + \sum_{j=0}^{m} b_{2j} Y_{t-j} + u_{2t} , \tag{2}$$

where u_{1t} and u_{2t} are serially uncorrelated zero mean stochastic error terms. It is worth noting that the Granger test, which uses the regressions above, requires that X and Y be stationary. Thus, the test is based on regressions in which the variables are differenced, in order to achieve stationarity. By considering the statistical significance of the a_{2j} and b_{2j} coefficients, one can determine causality in the Granger sense. Specifically, if the results of a standard F-test indicate that the a_{2j} 's are jointly significant, then X_t can be said to Granger-cause Y_t. Analogously, if b_{2j} 's are jointly significant, then Y can be said to Granger-cause X. If both are significantly different than zero, then there is evidence of feedback or bi-directional causality. In a bivariate case, different patterns of causality might be identified by estimating regressions of X (carbon dioxide emissions per capita as a fraction of GDP - as a measure of the environmental condition)[6] and Y (aid per capita as a fraction of GDP).

However, Lutkepohl (1982) and Serletis (1988), *inter alios*, have demonstrated that Granger causality is severely affected by a bias due to the omission of other relevant variables. Therefore, a bivariate test may not reveal the true nature of causality given that both variables may be simultaneously influenced by, for example, a third, omitted variable. For this reason, this study also entertains a trivariate causal structure. The third variable used for the model is external debt by the LDCs. A brief discussion of the economic justification for inclusion of this variable and the connection between the other two seems in order.

Other things being equal, one might expect that within any cohort of developing countries those carrying a heavier debt burden are more likely to be in need of different forms of international assistance, such as aid flows. The rationale for this relationship can be seen in the fact that debt-servicing alone imposes enormous financial as well as social strains on many heavily indebted countries.

Similarly, and within the same cohort, it seems reasonable to expect - *ceteris paribus* - that greater levels of indebtedness would be associated with less regard for the environment. Again, the rationale follows from how one might anticipate the manner in which priorities are drawn up. The tired career civil servant, in concert with his Minister, is more likely to be focused on dealing with the *real politic* of running the business of the administration's day to day activities and less concerned with bolstering some not-too-familiar environmental principles faxed in from some well-meaning north-European

[6] See the next section for a justification of the use of this variable.

NGO. Of course, this is perhaps too broad a brush to use here, but the key point is that in the minds of many developing-country key decision makers there simply are too many other immediate priorities, which appear to diminish the environmental cause on a more or less continuous basis.

Since a trivariate Granger causality test examines the joint influence of two variables on the third, the structure of the model in a trivariate setting, with Z (external debt per capita as a fraction of GDP) as the third variable, consists of the following regressions

$$Y_t = a_0 + \sum_{i=1}^{n} a_{1i} Y_{t-i} + \sum_{j=0}^{m} a_{2j} X_{t-j} + \sum_{k=0}^{p} a_{3k} Z_{t-k} + u_{1t}\,, \tag{3}$$

$$X_t = b_0 + \sum_{i=1}^{n} b_{1i} X_{t-i} + \sum_{j=0}^{m} b_{2j} Y_{t-j} + \sum_{k=0}^{p} b_{3k} Z_{t-k} + u_{2t}\,, \tag{4}$$

where u_{1t} and u_{2t} are stochastic error terms satisfying the usual conditions. The hypotheses being tested with (3) and (4) are: First, whether X and Z jointly cause Y after controlling for Y's own lags; and second, whether Y and Z jointly cause X after controlling for X's own lags. The null hypothesis that X does not Granger cause Y, conditional on Z, is rejected if the a_{2j}'s are jointly significantly different from zero, based on a standard F-Test. In the same vein, if b_{2j}'s are jointly significantly different than zero, then Y Granger-causes X, given Z.

Results of the Granger causality test critically depend on the choice of lag lengths. This is demonstrated through a number of studies, including Guilkey and Salemi (1982) and Thornton and Batten (1985). Maddala (1992, p. 393) claims that the chosen lag length is 'to some extent, arbitrary.' Armah (1997, p. 96) points out that the arbitrariness in the choice of lags is a 'major shortcoming' of the Granger test. Lee (1997) argues that choosing the lag length in an arbitrary *ad hoc* manner makes the model susceptible to misspecifications. Specifically, if the number of lags used exceeds the true order, the power of the test is likely to be reduced. If, on the other hand, the number of lags used is smaller than the optimal number of lags, the regression estimates will be biased and the residuals will be serially correlated. The present study conducts Hsiao's version of the Granger causality test by determining the pattern of the lag structure statistically. Thus, the study follows Hsiao (1979, 1981, 1982) in choosing lag lengths to minimize Akaike's (1969a, 1969b) final prediction error (FPE).[7] The procedure is to examine each of the series for its optimal lag length using the regression

[7] The reader should be cautioned that there is no universal agreement on whether lag lengths should be chosen to minimize FPE. For example, Jones (1989) demonstrates that lag lengths based on an arbitrary *ad hoc* procedure may be preferred under certain circumstances.

$$Y_t = \alpha_o + \sum_{p=1}^{P} \alpha_p Y_{t-p} + e_t \ , \tag{5}$$

where e_t is a serially uncorrelated zero mean stochastic error term. The procedure involves estimating (5) using OLS, allowing different values for P, and computing FPE as

$$FPE = \frac{(SER)^2 (N+k)}{N} \ , \tag{6}$$

where *SER* is the standard error of the regression and k is the lag length used in the regression. The optimal lag length corresponds to that value of P that minimizes the FPE. The advantage of using FPE is that it balances the risk of the bias from choosing a lower lag against the risk of an increased variance when a higher order is chosen (see Islam (1998)). Moreover, choosing the lag optimally does not constrain the number of lags to be the same for every regression.

3. VARIABLES, DATA AND SAMPLES

Global warming in recent decades is clearly attributable to greenhouse gas emissions. Furthermore, by all account, the most important greenhouse gas is carbon dioxide (CO_2). Anthropogenic CO_2 emissions rose from 14 gigatons a year in 1980 to 16 gigatons in 1990 and 24 gigatons in 1997. Loss and deterioration in the quality of life, loss of biodiversity, as well as economic losses are some of the costs of the climate change. According to Conceicao (2003), if the concentration of CO_2 in the atmosphere reaches twice the pre-industrial era, annual global damage would be 1.5 to 2 percent of global GDP - with developing economies losing 2 to 9 percent of their GDP. Poorer countries would suffer the most because "developing economies tend to be more vulnerable, since [they] are more dependent on agriculture and several are located in climatic-stressed regions" (Conceicao (2003), p. 175). Because of all of this, and since the most complete emission data available over time for individual countries is CO_2 emissions, they are used to represent pollution damage.[8]

Data on CO_2 emissions, as well as aid, GDP, and external debt for 130 countries over 1960-1999 is obtained from World Bank (2003).[9] We start our investigation by dividing the

[8] Other measures of environmental degradation include water pollution and deforestation. However, there is often incomplete data on these and other measures, which does not allow creation of a uniform data set for many countries over a long span of time.

[9] Data on CO_2 emissions did not exist for several countries in our sample beyond 1999. So we had the option of either omitting many countries, or having a cut off point in 1999. We chose the latter.

CO_2 emissions per capita in poorer countries by their GDP. This makes sense due to the fact that dividing aggregate carbon dioxide levels by GDP helps us to reflect on, and thus take into account, the level of economic or industrial activity in a poorer country. We cannot simply take the CO_2 emissions per capita without any reference to the size of the economy (measured by GDP) that produces the pollution. In the same vein, aid and debt per capita are both divided by the GDP to account for the size of the economy. For example, a debt per capita of $10,000 may not be problematic for a richer developing country, but it is for a poorer developing country. And a $50 per capita aid is more valuable to a poorer country, than to a richer country with a higher GDP. In other words, by relating to the per capita levels, we get closer to assessing the marginal values more easily.

The next section offers the results from a standard bivariate and trivariate Granger test. The results are first presented for the entire sample of aid-receiving countries. The sample is then divided into two subgroups based on income, using the World Bank's definitions. The data in each of the sub-samples is pooled; that is, recipient countries are not studied individually. Hence, the reported results are for the entire sample of aid-receiving countries and for the two sub-samples. The equations for X and Y in both models are estimated separately using a fixed effects model that allows the intercepts to change and therefore deals with the problem of heterogeneity. Finally, before running each regression, the optimal number of lags for each variable is determined using a fixed effects model. Due to the limited number of observations for each country, the optimal lag length for each variable is constrained to be no greater than five. Taking lags of a magnitude greater than five would mean losing too many degrees of freedom, leading to inefficiency in estimation in the study.

4. RESULTS FROM THE GRANGER TEST

The estimation results are reported in Table 1, with the sign of the causal impact noted in parentheses.[10, 11] As noted above, the bivariate specification has the potential to give a misleading picture of the relationship between pollution emissions and aid. Hence, other than noting that most of the bivariate results are in line with their trivariate counterparts, we choose to discuss only the latter below.

[10] The direction of causality between the dependent and independent variables (+ or −) is given by the sign of the sum of the coefficients of the lagged causal factor.

[11] Due to many missing observations in early years in the series for external debt, the results reported in Table 1 pertain to 1984-1999 during which uniform (continuous) data for all the variables and for all the countries was available.

Table 1. Standard Granger Causality Test Results

		Bivariate Granger Model		Trivariate Granger Model	
Sample	N	$Y = f(X)$	$X = f(Y)$	$Y = f(X,Z)$	$X = f(Y,Z)$
Full sample	130	$X \to Y(+)^{***}$	$Y \to X(+)^{***}$	$X \to Y\|Z(+)^{***}$	$Y \to X\|Z(+)^{***}$
		10.24	9.44	13.41	9.35
		(0.0000)	(0.0000)	(0.0000)	(0.0000)
Lower Income	65	$X \to Y(-)^{*}$	$Y \to X(-)^{***}$	$X \to Y\|Z$	$Y \to X\|Z(-)^{*}$
		2.48	7.35	1.82	3.06
		(0.0428)	(0.0000)	(0.1268)	(0.0173)
Higher Income	65	$X \to Y(+)^{***}$	$Y \to X(+)^{***}$	$X \to Y\|Z(+)^{***}$	$Y \to X\|Z(+)^{***}$
		53.79	50.64	25.11	30.90
		(0.0000)	(0.0000)	(0.0000)	(0.0000)

Notes: (1) N is the number of countries in each sample. (2) Lower income countries are defined as least developed countries plus low-income countries. Higher income countries are defined as lower middle-income countries plus upper middle-income countries plus high-income countries. (3) Definition of variables: X is CO_2 emissions per capita as a fraction of GDP; Y is foreign aid per capita as a fraction of GDP; Z is external debt per capita as a fraction of GDP. (4) Notation: $\to$ indicates Granger causality with the sign of any significant impact noted. (5) ***, **, and * denote, respectively, significance at the 1% , 5%, and 10% levels. (6) Numbers are F-statistics (probability of null hypothesis of no-causality in parentheses).

From the full sample, results suggest that, overall, given a developing country's level of external debt, aid has a detrimental impact on pollution. Furthermore, higher emissions prompt donors to provide more aid. There is thus strong support for bidirectional causality - a self-perpetuating circular flow between aid and pollution. The same pattern of causality is found in the sample of upper income countries, a result that is perhaps not surprising, given that this group includes many newly industrialized developing countries.

A different picture, however, emerges for lower income countries: As donors' aid increases, pollution decreases. Conversely, as aid decreases, pollution emission increases. What is surprising is the finding that, at the same time, donors do not appear to reward countries that reduce their levels of pollution with additional infusions of aid. That is, there is only a one directional causal link.[12]

Finally, it is worth emphasizing that these results, concerning the nexus between aid and pollution, are conditional on Z - the external debt variable.[13]

[12] The statistically significant one directional causal link appears in the trivariate setting. In the bivariate setting, there *is* evidence of statistically significant bidirectional causality. For reasons outlined earlier, one may be more inclined to accept the former than the latter.

[13] This study uses Z as the conditioning variable. Hence, the study does not include a third possible regression in order to determine the causal impact of X and/or Y on Z.

The policy implications of these results are of course clear. Western industrialized countries concerned with global environmental decline should tilt their economic assistance in favour of poorer developing countries.

As is evident, results from the Granger causality test are rather mixed. This leads us to suspect that a range of country-specific characteristics (beside a developing country's level of external debt) enter, and subsequently influence, the aid-environment relationship. The next section confirms this by re-examining the causal relationship between aid and pollution using an error-correction model, from which results for individual countries are derived.

5. SPURIOUS CAUSALITY AND NON-CAUSALITY

Use of an error-correction model rises out of concern that parameter estimates from a standard causality study may be potentially biased and inconsistent if the time series are non-stationary. That is, use of an error-correction model ensures that X and Y do not spuriously cause each other, when in fact they may be causally unrelated. This section uses an error-correction causal structure to derive results for individual countries in our sample over the period 1960-1999.

5.1. Stationarity Tests

Recall from Section 2 that the Granger causality test requires that X and Y be stationary. However, many economic time-series are non-stationary. Thus, they have to be differenced in order to become stationary, though this has the implication that information on the long-run properties of the series is lost. Series that are stationary fluctuate around a mean with a tendency to converge to the mean. On the other hand, non-stationary series wander widely without any tendency to converge to the mean. If non-stationary time series are included in an OLS regression this would result in a spurious or non-sense regression wherein an apparently highly significant relationship between the variables could appear even when in reality there would be no causality between the two (see, for example, Granger and Newbold (1974)).

In order to avoid the possibility of spurious Granger Causality results, the data used in this study are differenced until they become stationary. If a series becomes stationary after it is differenced d times, then it is said to be integrated of order d, denoted as I(d). To test for stationarity of the variables a test developed by Dickey and Fuller (1979, 1981) - commonly referred to as the Augmented Dickey-Fuller (ADF) test - is used. The ADF test is based on the regression

$$\Delta y_t = \alpha + \beta t + \lambda Y_{t-1} + \sum_{i=1}^{p} \gamma_i \Delta y_{t-i} + e_t \,, \tag{7}$$

where βt is a time trend, e_t is a serially uncorrelated, zero mean stochastic error term, and p is selected to minimize Akaike's FPE. By including lagged values of the variable being tested one ensures that there is no serial correlation in the error term e_t. The null hypothesis in the test of the existence of a unit root is $H_0 : \lambda = 0$, versus the alternative $H_1 : \lambda < 0$.[14] In testing H_0, the statistic τ is used, where

$$\tau = \frac{\lambda}{sd(\lambda)} . \tag{8}$$

The statistic τ is not distributed as the student's t and therefore one has to use asymptotic critical values. Here, the asymptotic critical values provided by Davidson and MacKinnon (1993, Table 20.1, p. 708) are used. If the null hypothesis is rejected, then the data are assumed to be stationary. However, if the null hypothesis is not rejected, then it is assumed that the data are non-stationary and therefore differencing the data is appropriate. Since the data can be integrated of any order, if a series is still non-stationary with I(1), an additional ADF test is conducted. This is done to ascertain whether the data have to be differenced twice or more times. Thus, the procedure is to estimate the following regression, where all terms are the same as (7) except that now higher differences are used

$$\Delta\Delta y_t = \alpha + \beta t + \lambda \Delta Y_{t-1} + \sum_{i=1}^{p} \gamma_i \Delta\Delta y_{t-i} + e_t . \tag{9}$$

Once again, the statistic τ is used in testing the null hypothesis. In general, the data have many unit roots, but the countries are seldom integrated of an order greater than one. Thereafter, the Granger causality test is conducted using differenced data in order to ensure the absence of spurious regressions. For example, if both x_t and y_t are found to be integrated of order one, then the following regression is run to test for Granger causality from X to Y

$$\Delta Y_t = a + \sum_{i=1}^{I} b_i \Delta Y_{t-i} + \sum_{j=1}^{J} c_j \Delta X_{t-j} + \xi_t , \tag{10}$$

where a is a constant, ξ_t is a serially uncorrelated, zero mean stochastic error term, and the number of lagged values (*I* and *J*) is determined optimally. Notice that both x_t and y_t are differenced once since I(1) is assumed.

[14] If $\lambda > 0$, then the series is not stationary.

5.2. Cointegration and Error-Correction

The concept of cointegration, introduced by Granger (1981, 1986, 1994), is relevant to the problem of the determination of long-run relationship between variables.[15] The basic idea behind cointegration is simple. If the difference between two non-stationary series is itself stationary, then the two series are cointegrated. If two or more series are cointegrated, it is possible to interpret the variables in these series as in a long-run equilibrium relationship. Lack of cointegration, on the other hand, suggests that the variables have no long-run relationship; i.e., in principle they can move arbitrarily far away from each other.

Consider two time series x_t and y_t and suppose that they are both I(1).[16] If there exists a linear combination of x_t and y_t that is I(0), then the two series are said to be cointegrated and therefore there is a tendency for them to move together in the long-run. If one finds that a series is cointegrated, an error correction term is added to the modelling procedure in order to capture the short-run dynamics of the model.

Here, the Augmented Engle-Granger (AEG) test for cointegration is used. The test comprises two steps. The first step is to test for the existence of cointegration between the two I(d) time series, x_t and y_t. This involves running a regression of x_t on y_t and checking if the regression residual is stationary. To be precise, the following regression is estimated using OLS

$$y_t = \alpha + \beta x_t + e_t . \tag{11}$$

The residuals from these regressions are then retained and the ADF test is applied to the residuals, as follows

$$\Delta e_t = \alpha t + \beta e_{t-1} + \sum_{i=1}^{p} \phi_i \Delta e_{t-i} + v_t , \tag{12}$$

where αt is a time trend, v_t is a serially uncorrelated, zero mean stochastic error term and where p is chosen to be sufficiently large in order for v_t to be serially uncorrelated.

The null hypothesis for the test of the existence of a unit root is $H_0 : \beta = 0$ versus

[15] The notion of cointegration arose out of concern about spurious time-series regressions. Specifying a time-series regression in terms of two economic variables x_t and y_t, often produces empirical results in which R^2 is quite high, but the Durbin-Watson statistic is quite low. This happens because economic time series are often dominated by smooth, long-term trends. However, such empirical results tell us little about the true nature of the short-run relationship between the variables.

[16] For variables to be cointegrated a necessary condition is that they be integrated of the same order, and the order has to be at least one.

the alternative $H_1 : \beta < 0$. In testing H_0, the statistic τ is used. Once again, asymptotic critical values provided by Davidson and MacKinnon (1993, Table 20.2, p. 722) are used to find the critical value of τ. If the null hypothesis is not rejected, then it is assumed that the error is non-stationary and therefore x_t and y_t are not cointegrated. If, on the other hand, the null hypothesis is rejected then the error is assumed to be stationary and the variables are cointegrated; i.e., there is a long-run relationship between x_t and y_t.

Existence of cointegration has implications for the Granger causality test. If the series are cointegrated of order one, then the first-order difference of each series plus a lagged regression residual from Equation (12) has to be introduced into the model. The lagged error residual is the error correction term and the model is called an error-correction model (Engle and Granger (1987, 1991)). Then, to test for Granger-causality the following model is used

$$\Delta Y_t = a + \sum_{i=1}^{I} b_i \Delta Y_{t-i} + \sum_{j=1}^{J} c_j \Delta X_{t-j} + de_{1t-1} + \xi_t , \tag{13}$$

$$\Delta X_t = h + \sum_{k=1}^{K} e_k \Delta X_{t-k} + \sum_{l=1}^{L} f_l \Delta Y_{t-l} + ge_{2t-1} + \zeta_t , \tag{14}$$

where a and h are constants, ξ_t and ζ_t represent mutually uncorrelated white noise series, and the lag values (I, J, K and L) are determined optimally. e_{1t-1} and e_{2t-1} are the error correction terms obtained from the regression of x_t on y_t and from the regression of y_t on x_t, respectively, assuming y_t and x_t are cointegrated.[17] Again, to conduct the Granger-causality test, the above regressions are estimated with and without ΔX_{t-j} followed by a standard F-test to test whether c_j's are jointly significantly different from zero. If we can reject the null hypothesis, then X Granger-causes Y. Analogously, if f_l's are jointly significantly different than zero, then Y Granger-causes X. If the null hypothesis for each of the above tests is rejected, then there exists bidirectional causality. Needless to say, the use of the error correction model for cointegrated variables ensures that the model captures both the long-term convergence between these two variables and the short-term dynamics.

All of this highlights a fundamental difference between conducting a standard Granger test and one with an error-correction causal structure. In the course of running a

[17] While the residuals from a regression of the two non-stationary variables x_t on y_t may be stationary, it is not necessarily true that the residuals from the regression of y_t on x_t will also be stationary. Therefore, in the context of Granger causality, tests for cointegration between the two variables will be performed twice, with each variable treated alternately as the independent variable.

standard Granger test, one faces, due to differencing, the hazard of losing information on the long-run properties of the series. An error-correction model representation, on the other hand, allows one to take into account the long-run properties of the series, provided they are cointegrated. In that sense, an error-correction model allows one to examine whether there is long-run Granger causality between the variables (see, for example, Agenor and Taylor (1993) for an analogous discussion of this notion in the context of official and parallel exchange rates in developing countries).

5.3. Estimation Results with Error-Correction

Table 2 presents various pattern of causality between foreign aid and pollution for individual developing economies using our complete data set spanning forty years. As expected, the nature of the relationship between aid and pollution varies across countries in our sample. Interestingly, cointegration between aid and pollution is fairly common. From Table 2 it appears that higher infusions of aid increase the level of pollution in Argentina, Belize, Brazil, Burkina Faso, Cape Verde, Chile, Colombia, Comoros, Congo, Dominican Republic, Fiji, Gabon, Guatemala, Indonesia, Iraq, Israel, Jamaica, Kiribati, Malawi, Mali, Mozambique, Nigeria, Papua New Guinea, Peru, Qatar, Rwanda, Samoa, Seychelles, Singapore, Sri Lanka, Trinidad and Tobago, Uganda, Uruguay, and Venezuela. By contrast, in the case of Gambia, Hong Kong, Korean Republic, Kuwait, Libya, Maldives, Morocco, and Somalia higher disbursements of aid contribute to a lower level of pollution. Furthermore, only nine countries (Algeria, Belize, Cote d'Ivoire, Dominican Republic, Indonesia, Israel, Korean Republic, Oman, and Vanuatu) appear to be rewarded with more aid after a reduction in their level of pollution and are penalized after a surge in the same. On the other hand, aid to more countries (Afghanistan, Antigua and Barbuda, Argentina, Burkina Faso, Cape Verde, Chile, China, Colombia, Comoros, Ecuador, Guatemala, Jamaica, Kiribati, Kuwait, Lao, Libya, Malawi, Mali, Mozambique, Nigeria, Papua New Guinea, Peru, Qatar, Rwanda, Seychelles, Singapore, Solomon Islands, Sri Lanka, Tunisia, Uganda, and United Arab Emirates) decreases as these countries' level of pollution decreases.[18] Remarkably, in the case of many countries noted in this paragraph, there is bi-directional causality between foreign aid and pollution.

Evidently, the results are mixed. Furthermore, no obvious grouping of developing countries with common characteristics emerges with respect to a particular causal finding. Needless to say, the mixed results should not be surprising given the high degree of heterogeneity among developing nations. What is certain is that, in the case of some countries, the possible link between foreign aid and pollution cannot be ignored. Obviously, specific institutional and country-specific characteristics are necessary to unveil the true nature of the relationship between aid and the environment in each developing economy.

[18] For explanations of why there may be positive or negative links between aid and pollution see our earlier discussion in Section 1.

Table 2. Results for Individual Countries with Error Correction

Country	N	Optimal Lags (X,Y)	Order of Integration (X,Y)	Error 1	X→Y	Error 2	Y→X
Afghanistan	22	1,3	1,1	nonstationary	2.90 (+) (0.0941)*	nonstationary	2.31 (0.1173)
Algeria	40	1,5	1,0		5.08 (-) (0.0137)***		0.70 (0.6522)
Angola	15	0,1	1,2		0.31 (0.5878)		2.08 (0.1756)
Antigua and Barbuda	23	5,1	0,1		3.15 (+) (0.0598)*		0.68 (0.5227)
Argentina	38	1,2	0,1		3.41 (+) (0.0462)**		3.06 (+) (0.0433)**
Bahamas, The	36	1,0	1,0		0.35 (0.7066)		0.12 (7275)
Bahrain	20	2,1	1,1	nonstationary	1.21 (0.3482)	nonstationary	1.07 (0.3722)
Bangladesh	28	5,2	3,2		1.12 (0.4101)		0.22 (0.8780)
Barbados	34	2,1	2,1		0.89 (0.4594)		1.73 (0.1969)
Belize	40	2,1	2,0		9.53 (-) (0.0001)***		2.62* (+) (0.0891)
Benin	40	5,1	1,1	nonstationary	1.51 (0.2133)	nonstationary	1.03 (0.3721)
Bhutan	20	3,1	1,2		0.16 (0.9530)		1.00 (0.4017)
Bolivia	40	1,1	2,1		0.78 (0.4688)		0.99 (0.3831)
Botswana	28	3,4	0,2		1.57 (0.2357)		1.34 (0.3026)
Brazil	40	5,5	1,0		1.46 (0.2370)		3.31 (+) (0.0179)**
Brunei	25	3,2	2,2	nonstationary	0.90 (0.9361)	nonstationary	0.38 (0.7696)
Burkina Faso	40	1,1	1,1	nonstationary	3.29 (+) (0.0493)**	nonstationary	3.42 (+) (0.0443)**
Burundi	38	4,1	1,1	nonstationary	0.27 (0.7666)	nonstationary	0.27 (0.7624)
Cambodia 1960-1974	15	0,3	1,0		0.02 (0.9022)		0.05 (0.9933)

Cambodia 1987-1999	13	3,1	1,0		1.01 (0.5190)		0.34 (0.7340)
Cameroon	40	1,3	1,1	nonstationary	0.51 (0.6054)	nonstationary	1.07 (0.3907)
Cape Verde	14	1,1	1,0		3.65 (+) (0.0748)*		7.83 (+) (0.0131)**
Central African Republic	40	5,1	1,1	nonstationary	0.72 (0.6394)	nonstationary	1.07 (0.3567)
Chad	40	2,1	1,1	stationary	0.60 (0.5571)	stationary	0.05 (0.9870)
Chile	40	3,3	1,1	stationary	12.79 (+) (0.0000)***	stationary	7.29 (+) (0.0004)***
China	21	1,2	1,1	nonstationary	3.71 (+) 0.0531*	stationary	2.48 (0.1108)
Colombia	40	1,2	1,0		9.35 (+) (0.0006)***		5.22 (+) (0.0048)***
Comoros	20	1,1	1,1	nonstationary	4.93 (+) (0.0239)**	nonstationary	5.12 (+) (0.0214)**
Congo, Dem. Rep.	40	4,1	1,0		1.72 (0.1638)		3.64 (+) (0.0392)**
Congo, Rep.	40	1,2	1,1	stationary	0.5 (0.6136)	nonstationary	1.11 (0.3590)
Costa Rica	40	1,3	1,1	nonstationary	0.15 (0.8631)	nonstationary	0.15 (0.9615)
Cote d'Ivoire	40	1,2	1,1	stationary	5.80 (-) (0.0073)***	nonstationary	0.61 (0.6137)
Cyprus	25	1,3	1,1	stationary	0.67 (0.5268)	stationary	1.37 (0.2937)
Dominica	23	5,2	1,0		3.42 (0.562)		1.55 (0.2757)
Dominican Republic	38	1,4	1,0		5.00 (-) (0.0145)**		4.89 (+) (0.0028)***
Ecuador	40	1,1	1,1	stationary	3.00 (+) (0.0634)*	nonstationary	2.06 (0.1436)
Egypt, Arab Rep.	40	3,2	1,1	nonstationary	0.14 (0.9641)	nonstationary	0.80 (0.5051)
El Salvador	40	3,3	1,2		1.40 (0.2599)		1.28 (0.3037)
Equatorial Guinea	17	0,3	1,1	stationary	0.73 (0.4321)	stationary	0.45 (0.7677)
Ethiopia	19	1,1	2,2	nonstationary	0.79 (0.4782)	nonstationary	0.79 (0.4744)

Fiji	40	2,2	1,0		1.43 (0.2541)		27.17 (+) (0.0000)***
French Polynesia 1966-1980	15	4,1	2,1		0.38 (0.8390)		2.22 (0.2561)
French Polynesia 1983-1999	17	2,1	1,1	stationary	2.73 (0.1136)	stationary	2.26 (0.1669)
Gabon	40	1,1	0,0		0.41 (0.6652)		5.23 (+) (0.0103)**
Gambia, The	34	4,1	1,1	nonstationary	1.35 (0.2803)	nonstationary	4.77 (-) (0.0191)**
Ghana	40	2,2	1,1	nonstationary	0.67 (0.5747)	nonstationary	0.74 (0.5335)
Grenada	23	1,2	1,1	nonstationary	0.29 (0.7531)	nonstationary	0.57 (0.6405)
Guatemala	40	1,1	1,1	nonstationary	3.15 (+) (0.0556)*	nonstationary	3.10 (+) (0.0578)*
Guinea	14	1,3	1,1	nonstationary	0.42 (0.6805)	nonstationary	0.99 (0.5034)
Guinea-Bissau	27	1,3	1,1	nonstationary	0.82 (0.4577)	nonstationary	0.95 (0.4590)
Guyana	40	1,1	1,1	stationary	0.16 (0.8496)	nonstationary	0.21 (0.8107)
Haiti	40	4,2	1,1	stationary	1.53 (0.2291)	nonstationary	1.73 (0.1845)
Honduras	40	2,1	1,1	nonstationary	0.31 (0.7368)	nonstationary	0.72 (0.4925)
Hong Kong, China	40	2,5	1,2		1.33 (0.2876)		2.80 (-) (0.0329)**
India	40	1,1	1,1	nonstationary	0.31 (0.7330)	nonstationary	0.49 (0.6152)
Indonesia	40	3,1	1,1	stationary	6.71 (-) (0.0011)***	nonstationary	9.65 (+) (0.0009)***
Iran, Islamic Rep.	21	1,0	1,1	nonstationary	1.76 (0.2090)	nonstationary	1.99 (0.1799)
Iraq	31	1,1	1,1	nonstationary	2.22 (0.1293)	stationary	2.98 (+) (0.0701)*
Israel	40	5,1	1,1	nonstationary	2.79 (-) (0.0312)**	nonstationary	3.83 (+) (0.0347)**
Jamaica	39	5,1	2,1		2.82 (+) (0.0310)**		2.80 (+) (0.0798)*

Jordan	35	3,1	1,1	nonstationary	0.27 (0.8473)	nonstationary	0.67 (0.5200)
Kenya	40	3,5	1,0		1.57 (0.2136)		1.95 (0.1142)
Kiribati	30	1,1	0,1		3.07 (+) (0.0650)*		2.61 (+) (0.0941)*
Korea, Rep.	40	5,5	1,2		4.91 (-) (0.0028)***		3.43 (-) (0.0160)**
Kuwait	38	3,5	1,0		5.29 (+) (0.0039)***		13.97 (-) (0.0000)***
Lao PDR	16	2,2	1,1	nonstationary	6.55 (+) (0.0193)**	nonstationary	1.09 (0.4142)
Liberia	40	2,4	0,0		0.48 (0.6968)		0.26 (0.9310)
Libya	31	2,4	0,0		9.83 (+) (0.0083)***		14.36 (-) (0.0000)***
Macao, China	18	1,0	0,1		0.65 (0.5376)		0.59 (0.4568)
Madagascar	40	1,1	1,1	nonstationary	0.84 (0.4410)	nonstationary	0.82 (0.4486)
Malawi	36	2,1	1,1	nonstationary	5.51 (+) (0.0042)***	nonstationary	6.13 (+) (0.0062)***
Malaysia	30	4,3	1,1	nonstationary	1.32 (0.3042)	nonstationary	0.19 (0.9420)
Maldives	20	0,1	1,2		1.11 (0.3105)		2.85 (-) (0.0914)*
Mali	33	3,3	1,1	nonstationary	2.76 (+) (0.0550)*	nonstationary	3.18 (+) (0.0343)**
Malta	40	5,2	1,1	stationary	0.95 (0.476)	nonstationary	1.02 (0.3996)
Mauritania	40	1,2	1,1	nonstationary	1.36 (0.2701)	nonstationary	1.18 (0.3200)
Mauritius	20	1,1	1,1	nonstationary	2.16 (0.1521)	nonstationary	0.38 (0.6880)
Mexico	40	3,4	1,0		0.98 (0.4370)		0.56 (0.7290)
Morocco	40	1,1	1,0		0.33 (0.7206)		4.07 (-) (0.0261)**
Mozambique	20	2,1	2,2	nonstationary	4.04 (+) (0.0366)**	nonstationary	3.23 (+) (0.0787)*
Nepal	40	5,1	1,1	nonstationary	0.53 (0.7780)	nonstationary	1.59 (0.223)

New Caledonia	28	2,1	1,1	nonstationary	0.59 (0.6299)	nonstationary	0.25 (0.7783)
Nicaragua	39	4,1	1,1	nonstationary	0.90 (0.4935)	nonstationary	0.31 (0.7377)
Niger	40	2,1	1,1	nonstationary	1.58 (0.2132)	nonstationary	1.26 (0.2982)
Nigeria	40	3,1	1,1	nonstationary	2.51 (+) (0.0628)*	nonstationary	4.65 (+) (0.0174)**
Oman	36	1,4	0,1		2.65 (-) (0.0889)*		1.07 (0.3978)
Pakistan	28	1,1	1,1	stationary	0.74 (0.4886)	nonstationary	1.14 (0.3391)
Panama	20	2,5	1,1	nonstationary	0.54 (0.6683)	nonstationary	0.98 (0.4943)
Papua New Guinea	35	2,3	1,1	nonstationary	12.21 (+) (0.0000)***	nonstationary	6.31 (+) (0.0013)***
Paraguay	40	2,1	2,1		0.44 (0.7285)		0.21 (0.8903)
Peru	40	1,5	1,1	nonstationary	5.80 (+) (0.0083)***	nonstationary	3.96 (+) 0.0060)***
Philippines	40	1,1	1,1	nonstationary	0.22 (0.8019)	stationary	0.73 (0.4908)
Qatar	30	2,2	1,1	stationary	13.50 (+) (0.000)***	stationary	32.53 (+) (0.0000)***
Rwanda	38	1,1	1,1	nonstationary	5.68 (+) (0.0078)***	nonstationary	5.83 (+) (0.0069)***
Samoa	22	1,1	2,2	nonstationary	1.17 (0.2038)	stationary	3.08 (+) (0.0756)*
Sao Tome and Principe	24	1,1	0,1		1.95 (0.1697)		1.98 (0.1650)
Saudi Arabia	40	1,2	0,0		0.20 (0.8229)		0.41 (0.7295)
Senegal	40	3,3	0,1		0.31 (0.8688)		0.43 (0.7835)
Seychelles	40	1,4	1,1	stationary	10.43 (+) (0.0004)***	nonstationary	2.86 (+) (0.0331)**
Sierra Leone	40	1,1	1,1	nonstationary	0.57 (0.5684)	nonstationary	0.60 (0.5545)
Singapore	40	1,2	1,0		8.29 (+) (0.0013)***		9.40 (+) (0.0001)***
Solomon Islands	33	1,3	1,1	nonstationary	2.80 (+) (0.0819)*	nonstationary	2.11 (0.1125)

Somalia	31	0,3	1,1	nonstationary	0.01 (0.9327)	nonstationary	4.14 (-) (0.0120)**
Sri Lanka	40	1,1	1,1	nonstationary	3.38 (+) (0.0459)**	nonstationary	3.14 (+) (0.0561)*
St. Kitts and Nevis	19	2,1	2,1		2.04 (0.1722)		1.30 (0.3146)
St. Lucia	21	1,1	1,1	stationary	0.31 (0.7352)	nonstationary	0.45 (0.6453)
St. Vincent and the Grenadines	27	1,1	0,0		4.89 (0.0174)		0.62 (0.5471)
Sudan	40	3,1	0,1		0.56 (0.6911)		0.07 (0.9296)
Suriname	40	2,1	2,1		1.15 (0.3427)		0.24 (0.7861)
Swaziland	36	5,3	1,1	stationary	10.28 (0.0000)	stationary	0.47 (0.7564)
Syrian Arab Republic	40	2,1	2,1		0.32 (0.8112)		0.22 (0.8025)
Thailand	40	1,1	1,1	nonstationary	0.05 (0.9530)	nonstationary	0.03 (0.9678)
Togo	40	2,1	1,1	nonstationary	0.32 (0.7269)	stationary	0.87 (0.4675)
Tonga	25	0,2	1,1	stationary	0.04 (0.8482)	nonstationary	0.58 (0.6326)
Trinidad and Tobago	39	5,5	2,1		0.48 (0.8167)		6.95 (+) (0.0004)***
Tunisia	39	3,4	2,2	stationary	5.28 (+) (0.0036)***	nonstationary	0.46 (0.8018)
Turkey	32	2,2	1,0		0.74 (0.5388)		1.42 (0.2628)
Uganda	20	2,3	3,3	nonstationary	5.19 (+) (0.0336)**	nonstationary	5.46 (+) (0.0257)**
United Arab Emirates	26	3,0	0,1		3.95 (+) (0.0192)**		0.17 (0.6874)
Uruguay	40	2,1	1,0		1.48 (0.2391)		2.53 (+) (0.0957)*
Vanuatu	21	1,1	1,1	stationary	9.56 (-) (0.0024)**	nonstationary	3.01 (0.0798)
Venezuela, RB	40	5,5	2,1		1.49 (0.2299)		2.57 (+) (0.0504)*
Vietnam	15	2,2	0,1		2.53 (0.1537)		1.93 (0.2266)

Zambia	36	4,1	1,1	nonstationary	0.60 (0.6976)	nonstationary	1.15 (0.3332)
Zimbabwe	36	1,1	1,1	nonstationary	0.27 (0.7670)	nonstationary	0.18 (0.8350)

Notes: (1) Error 1 is the residual from the regression of Y on X and Error 2 is the residual from the regression of X on Y; the column indicates whether they are stationary or non-stationary. The nature of the error is reported only for those countries in which both X and Y are integrated of the same order. In other words, the error is reported only for countries in which cointegration may exist. (2) Definition of variables: X is CO_2 emissions per capita as a fraction of GDP; Y is foreign aid per capita as a fraction of GDP. (3) Notation: → indicates Granger causality with the sign of any significant impact noted. (4) ***, **, and * denote, respectively, significance at the 1%, 5%, and 10% levels. (5) Numbers are F-statistics (probability of null hypothesis of no-causality in parentheses). (6) In the case of Cambodia and French Polynesia, the series were split due to missing observations within the time series.

6. CONCLUSION

Some causes of environmental degradation in developing countries obviously have nothing to do with aid. For instance, Margulis (2004) finds that deforestation in Amazonia is linked to large-scale expansion of profitable cattle ranching in this region. Nonetheless, our study suggests that the contributing effect of aid cannot be ignored. At the same time, it should be clear that our study is by no means designed to explain, and certainly not to measure separately, all the varied, intricate, and complex determinants of foreign aid to, and environmental conditions in, poorer economies around the world.

Hence, the approach followed in the first part of this study is to check for the existence of a causal link between aid and the environment using a standard Granger test. The findings demonstrate that an empirical link between aid and pollution exists in some of the samples. The mixed nature of results is confirmed in a stronger form when an error-correction model is used in the second part of the paper to test for causality for individual developing countries - where it is verified that aid and pollution are linked, but only in a subset of countries.

The absence of a consistent causal pattern in this study can be attributed to the heterogeneity among developing countries and to the multifaceted nature of the relationship between aid and the environment. Case studies probing deeper into the exact nature of this relationship may be a fruitful area for future research.

In the end, if there is a link between aid and pollution, what is the solution besides rewarding good countries with aid and punishing polluters with reduced levels, or in extreme cases no aid?[19] At least, in part, we need to promote policies that facilitate the

[19] Since developed countries cannot tax developing economies for producing pollution, they can use aid in order to bring about compliance. In that sense a low level of aid may be seen as a tax on pollution.

generation of income and employment for these countries through environmental/natural resource management. This promotion would be made easier if one or more international institutions were able to provide dedicated attention to the provision of this international public good. The kind of international body, such as the one being discussed here, does not exist at present. Any such organization would need to be charged with the kind of responsibility currently expected of the current crop of international financial institutions (IFI's) - such as the World Bank, Inter-American Development Bank, or the IMF - but from an environmental auditing perspective which will probably overlap with other purely financial or economic considerations in many cases. This supranational body would need to work closely with the IFI's and inter-governmental organizations in terms of coordinating national and international policy in order to limit the accumulations of greenhouse gases in the atmosphere.

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Mailing Address: Mak Arvin; Department of Economics, Trent University, 1600 West Bank Drive, Peterborough, Ontario, Canada K9J 7B8. Tel: (705) 748 1011, Fax: (705) 748 1567. E-mail: marvin@trentu.ca

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