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A Methodology for Testing Comparative Economic Theories: Theory and Application to East–West Environmental Policies[1]

PETER MURRELL

University of Maryland, College Park, Maryland 20742

AND

RANDI RYTERMAN

University of Pennsylvania, Philadelphia, Pennsylvania 19104

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Many variables of interest to comparative economists are difficult to measure or are unavailable for empirical research. To test for the effects of such variables, we offer a methodology based on examining the bias that is introduced when such variables are omitted from a model's estimation. Further, our methodology is able to use data that are available only for some countries, using a procedure to estimate missing observations. We apply the methodology to test whether the high level of environmental damage in East Europe can be explained by a tendency to export commodities intensive in pollution. We find no support for this hypothesis. *J. Comp. Econom.* December 1991, **15**(4), pp. 582–601. University of Maryland, College Park, Maryland 20742; University of Pennsylvania, Philadelphia, Pennsylvania 19104. © 1991 Academic Press, Inc.

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I. INTRODUCTION

Problems of data nonavailability or noncomparability are almost intrinsic to empirical comparisons of countries with different economic systems or at different stages of economic development. Yet there is little research by

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comparative economists that develops specialized econometric and statistical methodologies able to confront such data problems. Hence, the range of results comparing diverse sets of countries is perhaps more limited than it need be. In this paper, we develop a methodology that confronts more common difficulties in empirical comparative economics.

The problems that our methodology addresses are twofold. First, for many variables of interest to comparative economists, there is only scanty information available. This is not surprising. A variable whose chief purpose is to explain intersystem differences will be ignored by economists uninterested in comparative analysis. Consequently, standard sources of data might not provide adequate information on these variables. Moreover, many variables of interest to comparative economists are difficult to measure. In our paper, we call variables that have not been adequately measured for any type of economy nonstandard. Our methodology is developed so that we can test for the effect of such variables on economic performance using much less information than is usually required for empirical work.

The second data problem that our methodology addresses is the fact that many variables are measured for some countries, but not for others. In our paper, we call variables that have been adequately measured, at least for some countries, standard. If one ignores the data that do exist, which is the natural tendency when there are missing observations, useful information is lost. Yet, techniques to incorporate partial data are not used by comparative economists. In the following, we show how information on variables measured only for some countries can help improve the results of cross-country comparisons.

The methodology that we offer as a remedy for these two problems builds upon a procedure developed by Murrell (1990) to estimate missing observations for standard variables. In the present paper, we are concerned with nonstandard variables and their effect on economic performance. We provide a procedure to test for the importance of these variables by examining the impact of their omission on model estimates. Our methodology uses this information as the basis of a nonparametric test.

In the next section, we motivate the methodology and show its relation to existing empirical work. In particular, we show that our methodology generalizes existing techniques. Our generalization makes previously implicit assumptions transparent through formal specification of the model, while allowing for flexibility in the application of the technique. In Section III, we present the basic model and discuss the types of hypotheses that economic theory could generate for such a model. In particular, two variants of hypotheses are examined. Since the properties of our methodology are slightly different under each of these two variants, we discuss those properties in two separate sections. The details of the econometric and statistical methods are presented within the discussion of the first, and more straightforward, variant in Section IV. Section V examines how the assumptions underlying the methodology must change as a consequence of the differences in the a priori information offered by economic theory in the two variants.

In Section VI, we present an example of the application of our methodology by examining the effects of environmental policies on foreign trade in East Europe and in market economies.[2] Testing for such effects has been difficult, given the lack of reliable data on the stringency of environmental policy across countries and on the determinants of foreign trade of East European nations. Using our methodology, we are able to derive empirical results despite these missing data. We present our conclusions in the final section of the paper.

Given our knowledge of the problems associated with East European data, we case the discussion of our methodology in terms of comparisons of socialist economies (SEs) and market economies (MEs). However, the techniques that we develop are certainly relevant for a much wider set of comparisons, since the types of data problems that we address are almost inherent to cross-country empirical exercises. Moreover, we discuss our procedure with the use of foreign-trade data in mind, although the methodology is certainly applicable to problems other than foreign trade.

II. MOTIVATING THE METHODOLOGY

The general model that we use is

$$y_{ij} = \sum_{k=1}^{S} b_{ik} z_{kj} \qquad i = 1, \ldots, N\; j = 1, \ldots, T, \tag{1}$$

where y_{ij} is a measure of trade performance in good i by country j. z_{kj} measures characteristic k of the domestic economy of country j. In the following, we call the z_{kj} factor endowments, but this term is used broadly, to include the nonstandard endowments discussed above. The b_{ik} are coefficients.[3] In the analysis, there are T countries, N internationally traded goods, and S factor endowments.

The one restriction embodied in this general form is that of linearity. In fact, standard trade models do satisfy this restriction, for example Heckscher–Ohlin (Leamer 1984, pp. 158–159). Moreover, in practical applications of nonlinear models, it is common to resort to linearization.

[2] For another application of this methodology, see Ryterman (1991).

[3] The general model is assumed to apply to all countries, regardless of economic system. Although this assumption may appear strong, analogous assumptions are embodied in the majority of tests for systemic differences. For example, consider tests in which a dummy variable is used to account for the effects of economic system on performance. In this and all other cases of nested hypotheses, the models for each system have been embedded in a single higher-level structure.

Given also that our methodology could be adapted for nonlinearities, little is lost in this presentation by focusing on a linear model.

Let us suppose that endowments 1 through S-1 are standard endowments, so that their values can be measured, at least for MEs. Such endowments might be the capital stocks, labor forces, and resource endowments of countries. Now, suppose we are interested in testing for the effect of a nonstandard endowment on the pattern of international trade. Let this nonstandard endowment be the Sth endowment. Because of the nature of this variable, we might not have any data on endowment S: the z_{Sj} are unknown. We assume, however, that the direction of the effect of endowment S on the trade of each good, captured in the signs of the b_{iS}, is known.[4] Our objective here is to test hypotheses about the relative values of the z_{Sj} on SEs and MEs, given the information on the b_{iS}.

As is easily seen from (1), there is much extraneous information that must be taken into account in conducting tests. One must consider the effects of endowments that are not of central interest (z_{kj}, $k = 1, \ldots, S - 1$) as predicted by the parameters describing the relationship between endowments and trade (b_{ik}, $k = 1, \ldots, S - 1$). Either one must find a means of controlling for these effects using existing data sources, or one must make strong *ceteris paribus* assumptions about the effect of these extraneous variables on the relationship between foreign trade and the endowment of interest. This trade-off between obtaining more data and making extra assumptions is best exhibited with reference to two existing approaches to examining the information embodied in trade flows.

First, consider Drábek (1983), who focuses on differences in technological prowess betweeen SEs and MEs.[5] He examines their trade flows for sectors in which technology is most important. By comparing these statistics for countries with different types of economies, he makes deductions concerning variations in technological prowess across systems. The assumption implicit in this analysis can be seen using (1). Suppose that endowment S is the level of technological prowess of a country and that Sector 1 is the one in which technology is thought to be most important. Drábek uses commodity export–import ratios as the measure of trade performance, y_{ij}. Therefore, b_{1S} measures the effect of technological prowess on that ratio for commodity 1. Given the importance of technology in Section 1, this effect should be positive and large.

A comparison between an ME, say country j, and an SE, country m, is of central interest. If, as is implicit in Drábek's analysis, one is willing to assume that

[4] In the context of our specific example, we consider the possible sources of such information on the signs of the b_{iS}.

[5] Murrell (1990, Chap. 4) undertakes an analysis that follows that of Drábek.

$$\sum_{k=1}^{S-1} b_{1k} z_{kj} = \sum_{k=1}^{S-1} b_{1k} z_{km}, \tag{2}$$

then $z_{Sj} \gtrless z_{Sm}$ follows immediately from knowledge that $y_{1j} \gtrless y_{1m}$. That is, suppose that the net effect of all standard endowments on trade in commodity 1 is the same in country m as in country j. Then, one can conclude that technological prowess is greater (less) in country j if its measure of trade performance in commodity 1 is larger (smaller) than that of country m. A priori, the assumption in (2) is not very appealing, of course, particularly when data limitations preclude its verification. But, one might be able to justify this methodology in special cases. If one assumes that technological prowess is vital to the production of commodity 1, then b_{1S} might be so large that differences between $b_{1S}z_{Sj}$ and $b_{1S}z_{Sm}$ will dominate differences in the two expressions in (2). This might be the case particularly if countries j and m are matched so that their extraneous characteristics are similar.

The assumption embodied in (2) is strong. An alternative approach, almost the opposite tack, is taken by Rosefielde (1973), who uses the classical Leontief methodology to evaluate the rationality of Soviet trade. Leamer (1980) places the Leontief methodology in the perspective of the Heckscher–Ohlin model. Consider the square version of Eq. (1). Denoting as a_{ik} the elements of the matrix that is the inverse of the matrix comprising the b_{ik}, then the inverse of (1) can be written as

$$z_{kj} = \sum_{i=1}^{N} a_{ij} y_{ij}. \tag{3}$$

One can then obtain estimates of the standard factor endowments, z_{kj}, using data on the a_{ij} and net exports as the measure of foreign trade performance, y_{ij}. Hence, one obtains an estimate of the amounts of standard factor endow- ments that are consistent with a country's foreign trade patterns.[6] Then, if one is concerned about whether nonstandard endowments have affected trade patterns, one might compare the estimates of the standard factor en- dowments obtained from trade patterns to information about the levels of factor endowments obtained directly. If there is an obvious inconsistency between these two sets of information, then one might want to attribute that inconsistency to the effects of some unknown nonstandard endowment, such as economic system. This is the essence of Rosefield's (1973) analysis, recast in terms of our model.

The approach described in the previous paragraph does not rely on the type of strong assumption that was made in Eq. (2). But, there is a cost, the

[6] Although, for the sake of brevity, we have glossed over some important details, this is the essence of any procedure that follows the original Leontief methodology or its modern variants.

data requirements are large. Not only is knowledge of the a_{ik} required, but also, in order to judge whether the factor content of foreign trade is appropriate for a particular country, one must have information on that country's actual factor endowments. The lack of strong assumptions is, of course, the concomitant of the data requirements. Given such data requirements, it is obvious that this procedure will be generally inapplicable in a multicountry context.

Using foreign-trade statistics to test hypotheses about the effects of nonstandard variables, one must steer a course between the Scylla of overly strong *ceteris paribus* assumptions and the Charybdis of requiring so many data that results are available for only a few countries. Given limited stocks of information, one cannot escape this dilemma. However, one can choose an intermediate point between the extremes exemplified by the approaches of Rosefielde and Drábek by efficiently using data that are available on only a subset of countries. This paper presents a methodology that uses such partial data, allowing one to choose among the continuum of alternatives that summarizes the trade-offs between strong assumptions and large data requirements.

III. THE BASIC MODEL

Brevity of presentation requires that we use matrix representations for the basic equations in (1), which now have error terms added,

$$Y = BZ + E, \tag{4}$$

where Y is $N \times T$, B is $N \times S$, Z is $S \times T$, and E is the $N \times T$ matrix of error terms. Usually, it will be true that $T > N \geqslant S$ and it is important for our methodology that this be the case. This double inequality can be ensured easily by appropriate aggregation of the international-trade data.[7]

We now partition the sets of country and endowment indices to produce a model that corresponds to the empirical problem described above. First, we divide the countries into two groups, SEs and MEs. Let there be T_1 MEs and T_2 SEs ($T_1 + T_2 = T$).

Second, let us take into account the fact that varying amounts of information are available on the different factor endowments. Then, S_1 is the set of indices of factor endowments for which data are available across all countries, and S_2 is the set of factor-endowment indices for which data are known for MEs, but not for SEs. We assume that the last endowment, the Sth, is the nonstandard one that is the focus of the tests. Denote by S_3 the set compris-

[7] The variables for which data are least easily available are the factor endowments. Therefore, the fact that there are fewer factors than goods will follow from considerations of data availability.

ing only the index S. We take 1 to be the index of the constant in the system of equations in (1) and denote by S_0 the set comprising only the index 1. Now, we can economize on notation by letting S_i stand for both subsets of the indices of factor endowments and the numbers of indices in those sets. Hence, $S_0 + S_1 + S_2 + S_3 = S$. Thus, the matrices in (4) can be partitioned in a manner corresponding to the partitions of the indices of countries and factor endowments, obtaining

$$[Y_1 | Y_2] = [B_0 | B_1 | B_2 | B_3] \cdot \begin{bmatrix} Z_{01} & Z_{02} \\ Z_{11} & Z_{12} \\ Z_{21} & Z_{22} \\ Z_{31} & Z_{32} \end{bmatrix} + [E_1 | E_2], \quad (5)$$

where Y_j is an $N \times T_j$ matrix, B_i is an $N \times S_i$ matrix, Z_{ij} is an $S_i \times T_j$ matrix, and E_j is an $N \times T_j$ matrix. Hence, the subscripts on the submatrices show which data they contain. For example, Z_{21} contains factor-endowment data for all variables z_{ij}, where $i \in S_2$ and $j \in T_1$.

This new representation of the basic equations imparts to the methodology both flexibility and practicality. The partitions correspond to patterns of data availability that are frequently encountered in comparative work. For example, for some variables, it is quite usual to be able to obtain data for the MEs, but not for the SEs. Such data would appear in the penultimate row of the partitioned Z matrix. Variables for which comparable measures are available across all countries appear in the second row of the partitioned matrix.

Obviously, we are interested primarily in the significance of the terms B_3Z_{31} and B_3Z_{32} and the differences between them. These terms capture the effect of the non-standard endowment on trade in MEs and SEs. To construct tests, we must formulate hypotheses summarizing the alternative possibilities for B_3Z_{31} and B_3Z_{32}. Here, we consider two variants. The first assumes more a priori knowledge about the nature of these terms, but, consequently, requires less stringent assumptions to justify the econometric procedures.

Variant 1. Let us suppose that we wish to test whether the Sth endowment causes trade patterns to differ across countries. Perhaps, previous researchers have not considered the Sth endowment to be an important determinant of trade patterns. However, a new theory tells us that cross-country variations in the z_{Sj} do produce differences in the trade patterns of countries and that these differences will be prominent when comparing MEs to SEs. An example of this endowment is the laxity of environmental policy.

Given the variant presented above, we can formulate the following versions of the null and alternative hypotheses:

H_0: $B_3z_{Sj} = 0$ and $B_3z_{Sm} = 0$, where j is any ME and m is any SE, and 0 is the zero vector of dimension N.

H_1: $B_3z_{Sj} \neq B_3z_{Sm}$, where j and m are defined as before.[8]

Under H_1, sufficient information is typically available to predict the relative sizes of the paired elements of B_3z_{Sj} and B_3z_{Sm}. The direction of the inequality relating each pair depends on the sign of b_{is} and the size of z_{Sj} relative to z_{Sm}.

Variant 2. The focus in the second variant is on a situation where one must assume that the nonstandard endowment affects trade, but one is not sure whether there is any systematic difference between the SEs and the MEs in the size of this endowment. An example of such an endowment might be level of technology. Given this formulation of the problem, the hypotheses can be written as:

H_0: $E(B_3z_{Sj}) = E(B_3z_{Sm})$, where j is any randomly selected Me, m is any randomly selected SE, and the expectation is understood to be a subjective one, given the random selection of j and m.

H_1: $E(B_3z_{Sj}) \neq E(B_3z_{Sm})$, given j, m, and $E(.)$ defined as before.

Once again, under H_1, information is typically available to predict the relative sizes of the paired elements of $E(B_3z_{Sj})$ and $E(B_3z_{Sm})$.

Before analyzing each variant separately, we close the description of our model with the characterization of the error terms, which is necessary in order to estimate the system in (4). This characterization is obtained by resorting to the general assumption, implied by either null hypothesis, that the same process generates trade outcomes in all countries. Such an assumption is obviously implicit in the formulation of the equations in (1) and is necessary in any analysis that compares MEs and SEs within a single framework.[9] Therefore, the stochastic process generating the error terms is assumed to be constant across countries: $E(e_{ij}e_{nj})$ does not vary with j.

We also assume that all systematic factors affecting trade patterns are captured in the model. Therefore, if one knew that country j has a particular feature that affects the error term in the equation for good i, one could not use the information to deduce anything about the probability that country m will possess, or lack, the same feature. Thus for all i, $E(e_{ij}e_{im})$ is assumed to be zero for $j \neq m$. To derive maximum-likelihood estimates, we use the usual assumption that the error terms are normally distributed.

To make these assumptions precise, we use $e_{.j}$ to denote the jth column of

[8] Obviously, the possibility omitted in the two hypotheses is that $B_3z_{3j} = B_3z_{3k} \neq 0$. The alternatives assumed in the theory underlying Scenario 1 rule out this case.

[9] Using some of the techniques presented here, Murrell (1990) derives methods for examining whether this assumption is correct.

E; thus $e_{.j}$ contains all errors pertaining to country j. Similarly, $e_{i.}$ denotes a row of E. Then, the assumptions on errors are:

$$E(e_{.j}) = 0 \quad (6)$$

$$E(e_{.j}e'_{.j}) = W, \text{ a general } N \times N \text{ symmetric matrix;} \quad (7)$$

$$E(e_{i.}e'_{i.}) \text{ is a diagonal matrix;} \quad (8)$$

$$e_{.j} \text{ is distributed } N(0, W). \quad (9)$$

Implicit in the above is the assumption that the error terms are distributed independently of all the other terms on the right-hand side of the equations in (1). The assumption is a necessary condition for the consistency of the particular estimating procedures that we present below. This point becomes especially important when we develop the assumptions necessary for the procedures to work on Variant 2.

IV. THE TEST: VARIANT 1

The tests are most easily described in Variant 1 and, therefore, we begin with this case. Under the null hypothesis, the non-standard endowment S has no effect on the pattern of trade of any country. In this case, Eqs. (5) simplify to:

$$[Y_1|Y_2] = [B_0|B_1|B_2] \cdot \begin{bmatrix} Z_{01} & Z_{02} \\ Z_{11} & Z_{12} \\ Z_{21} & Z_{22} \end{bmatrix} + [E_1|E_2]. \quad (10)$$

Then, as shown in Murrell (1990) and explained below, all of the unknowns in this system of equations can be estimated. These unknowns include both the parameters B_i and values of the standard endowments that are unknowns for the SEs, Z_{22}. Using a ˆ symbol to indicate an estimate of the relevant parameter or observation, define:

$$\hat{B} = [\hat{B}_0|\hat{B}_1|\hat{B}_2] \quad \text{and} \quad \hat{Z} = \begin{bmatrix} Z_{01} & Z_{02} \\ Z_{11} & Z_{12} \\ Z_{21} & \hat{Z}_{22} \end{bmatrix}.$$

Given the above assumptions, the following equations define the maximum-likelihood estimates of [10]:[10]

$$\hat{B} = Y\hat{Z}'(\hat{Z}\hat{Z}')^{-1} \quad (11)$$

$$\hat{W} = (Y - \hat{B}\hat{Z})(Y - \hat{B}\hat{Z})'/T \quad (12)$$

and

$$\hat{Z}_{22} = (\hat{B}_2\hat{W}^{-1}\hat{B}_2)^{-1}\hat{B}_2\hat{W}^{-1}(Y_2 - \hat{B}_0Z_{02} - \hat{B}_1Z_{12}). \quad (13)$$

These equations are analogous to those of standard statistical procedures. For a given value of $\hat{Z}_{22}$, Eq. (11) defines $\hat{B}$ as the ordinary least-squares estimate of the coefficients of a set of N independent linear equations. Given values for $\hat{Z}_{22}$ and $\hat{B}$, $\hat{W}$ is calculated by simple algebra. Then, given values for $\hat{B}$ and $\hat{W}$, the estimate of $\hat{Z}_{22}$ in (13) can be interpreted as the generalized least-squares estimate of the coefficients of a set of equations in which the elements of Z_{22} are viewed as parameters and the elements of $\hat{B}$ as variables.

The formulation of the tests now proceeds by noting that, under the null hypothesis, $[\hat{Y}_1|\hat{Y}_2] = \hat{B}\hat{Z}$ is a consistent estimate of trade patterns for both sets of economies.[11] However, under the alternative hypothesis, the omission of the nonstandard variable will result in systematic differences between this estimated matrix of trade patterns and $[Y_1|Y_2]$. The tests search for the presence of these differences, focusing on the patterns of estimated errors, $[\hat{E}_1|\hat{E}_2] = [Y_1|Y_2] - [\hat{Y}_1|\hat{Y}_2]$.

In order to conduct the tests, two further items of information are needed. First, one must formulate expectations about the relative size of z_{Sj} in MEs and SEs, assuming that the alternative hypothesis is correct. Economic theory, which drives the alternative hypothesis, will usually form the basis of these expectations. Without loss of generality, we assume that, under the alternative hypothesis, SEs have a larger endowment of S than MEs.

Second, we require information on the signs of the b_{iS} across equations. Based on the signs of the b_{iS}, we divide the goods indexes into two sets. Let us denote these sets, and the numbers of indexes that they contain, N_1 and N_2, so that the b_{iS} are positive when $i \in N_1$ and negative when $i \in N_2$.

Next, the model is estimated using the procedure described in equations (11) through (13). Now, we can define the following function:

$$F(\hat{e}_{ij}) = 1 \quad \text{if } \hat{e}_{ij} > 0 \text{ and } i \in N_1$$
$$= 1 \quad \text{if } \hat{e}_{ij} < 0 \text{ and } i \in N_2$$
$$= 0 \quad \text{otherwise}$$

Then for each country j, the score $R_j = \sum_{i=1}^{N} F(\hat{e}_{ij})$ can be calculated.

Under the null hypothesis, there is no reason to expect that the R_j differ between SEs and MEs. Given the assumptions in (6), $E(\hat{e}_{ij}) = 0$. Thus, $E(R_j) = N/2$ for every country, regardless of economic system.

[10] See Murrell (1990, Chap. V) for a proof.

[11] Consistency follows from the use of maximum-likelihood methods.

Now, under the alternative hypothesis, a variable has been omitted in the estimation, $b_{iS}z_{Sj}$. For simplicity, we assume that the mean value of z_{Sj} over the sample of countries is zero, which, given previous assumptions, implies that z_{Sj} will be negative for MEs. Nothing, except unenlightening extra semantics and algebra, is gained by assuming a nonzero mean.[12] In this case, $E(\hat{e}_{ij}) = b_{iS}z_{Sj}$. Given our sign conventions defined above, we can predict that $\hat{e}_{ij}$ has a positive expected value if either j is an SE and $i \in N_1$ or j is an ME and $i \in N_2$. Similarly, $\hat{e}_{ij}$ will have a negative expected value if either j is an ME and $i \in N_1$ or j is an SE and $i \in N_2$. Hence, the expected value of R_j differs between the two sets of countries under the alternative hypothesis. Then, the test can be based on a simple application of the binomial distribution.

Let

$$R_k^* = \sum_{j \in T_k} R_j/(N \cdot T_k), \qquad \text{for } k = 1, 2,$$

remembering that T_1 is the set of indices of the MEs and also the number of such economies, with T_2 playing the same role for the SEs. Under the null hypothesis, R_2^* and R_1^* are distributed as binomial proportions with the same expected value. Hence, under the null hypothesis $(R_2^* - R_1^*)$ has mean zero and variance consistently estimated by

$$\left[\frac{R^* \cdot (1 - R^*)}{N}\right]\left[\frac{1}{T_1} + \frac{1}{T_2}\right] \qquad \text{where } R^* = \frac{T_1 \cdot R_1^* + T_2 \cdot R_2^*}{T}.$$

The normal distribution is a reasonable approximation to the binomial for large enough sample sizes, in the present case when NT is greater than 10 (Snedecor and Cochran, 1980, p. 118). Then, the test to determine whether the nonstandard endowment affects trade patterns boils down to conducting a test using the normal distribution to determine whether $(R_2^* - R_1^*)$ differs significantly from zero.

V. THE TEST: VARIANT 2

The null and alternative hypotheses of Variant 2 admit a wider range of behavior than under Variant 1. Hence, given the same data availability under both variants, stronger assumptions are necessary to conduct the tests. Under Variant 2, theory leads us to the judgment that the non-standard endowment, the Sth, varies between countries and significantly affects trade. The question to be answered by tests is whether the endowment differs significantly between SEs and MEs.

[12] Given that z_{Sj} is an omitted variable, its mean value is absorbed into the constant terms of the regressions. See Ryterman (1988) for treatment of the more general case of nonzero means.

In the present section, we examine the assumptions that are required by our methodology when the competing hypotheses are given by Variant 2. In stating these assumptions, we do not argue that they must hold in general. Rather, the purpose is to gain insight into how one might choose a sample of countries and commodities that satisfy the requirements of the methodology.

The following remarks address the question of the circumstances under which equations (11), (12), and (13) consistently estimate the system (10) when the null hypothesis of Variant 2 holds. To see the problem in its simplest terms, compare (10) and (5). Under the null hypothesis of Variant 1, these two systems are identical. However, under the null hypothesis of Variant 2, one must take z_{Sj} into account. Since this variable is not observed, it will become part of the error terms when applying equations (11), (12), and (13) to estimate (10). That is, the error terms are now $(b_{iS}z_{Sj} + e_{ij})$, rather than e_{ij}. To understand the conditions under which the estimates are still consistent, one must examine when $(b_{iS}z_{Sj} + e_{ij})$ has the same general properties that were assumed for e_{ij} in Section III above.

In the following, once again, we assume that the mean value of z_{Sj} over the sample of countries is zero. Hence, without loss of generality, we can take (6) to hold for $(b_{iS}z_{Sj} + e_{ij})$.[13] For the analog of (7) to hold for $(b_{iS}z_{Sj} + e_{ij})$, we require that $E[(b_{iS}z_{Sj} + e_{ij})(b_{nS}z_{Sj} + e_{nj})]$ not vary with j. Here, we must interpret the expected value in a subjective probability sense, as in the null hypothesis. That is, for any randomly selected country, j, we have no information to predict the size of its Sth endowment. Therefore, the constancy over j of $E[(b_{iS}z_{Sj} + e_{ij})(b_{nS}z_{Sj} + e_{nj})]$ follows from the lack of information on the Sth endowment as expressed in the null hypothesis, not from any actual constancy in the values of the Sth endowment.

For the analog of (8) to hold for $(b_{iS}z_{Sj} + e_{ij})$, we require that $E[(b_{iS}z_{Sj} + e_{ij})(b_{iS}z_{Sm} + e_{im})]$ equals zero when $j \neq m$. Implicit in the null hypothesis is the assumption that the size of the nonstandard endowment in one country is independent of its size in a different randomly selected country. By knowing one country's endowment, one does not gain any information about another's. Together with the existing assumptions on e_{ij}, this implies that $E[(b_{iS}z_{Sj} + e_{ij})(b_{iS}z_{Sm} + e_{im})]$ is zero.

The assumptions in the three previous paragraphs can be regarded as innocuous. They follow form the essential nature of the null hypothesis that no information about the Sth endowment is implied by knowing the country's index number or the endowments of other countries. However, these assumptions are not sufficient to guarantee the workability of our methodology in Variant 2. More problematical assumptions follow.

[13] Throughout this section, we continue to assume that (5), (6), (7), and (8) hold for the e_{ij}.

In deriving Eqs. (11), (12), and (13) as the maximum-likelihood estimates of (10), it was assumed that the errors are independent of other terms on the right-hand sides of those equations. Hence, if the estimates of (10) using Eqs. (11), (12), and (13) are to be consistent when the error term is $(b_{iS}z_{Sj} + e_{ij})$, z_{Sj} must be independent of $(z_{1j}, \ldots, z_{S-1,j})$ as j varies. The Sth endowment must vary across countries independently of the other endowments. Obviously, this assumption will hold only either in special cases or if the sample of countries has been carefully chosen in order to satisfy the assumption.

To understand the final additional assumption necessitated by Variant 2, reflect upon the interpretation of Eq. (13) offered in the paragraph following the presentation of that equation. Equation (13) can be interpreted as the generalized least-squares estimate of a relationship in which the z_{kj}, $k \in S_2$ and $j \in T_2$, are viewed as coefficients and the b_{ik}, $k \in S_2$, are viewed as data. Then, if $(b_{iS}z_{Sj} + e_{ij})$ is to be independent of all data on the right-hand side of equation (10), it is necessary to assume that b_{iS} varies over commodities, i, independently of b_{ik}, $k \in S_2$. The implication of this assumption is easy to summarize only in a situation where the theory underlying (1) has been made explicit. However, the most prominent candidates for that theory would lead to the interpretation of the b_{ik} as representing parameters of the production process.[14] Thus, this assumption can be viewed as requiring that the intensity of use of the Sth endowment should vary across sectors independently of the intensity of use of endowments in the set S_2, those endowments for which no data are available for the SEs. The degree of faith in this assumption, therefore, will depend critically on the amount of data available on the SEs.

As is made transparent in the previous paragraph, the absence of complete information creates the need for extra assumptions. When this point becomes clear, objections to making the assumptions lose some force. It is obvious that similar assumptions must be implicit in existing analyses that rely on the same data sets. Seen in this way, an important property of our methodology is that it forces explicitness of the assumptions that might be hidden in analyses using a less precise theoretical and statistical framework.

Armed with these assumptions, the methods developed in Section IV can now be applied. Equations (11), (12), and (13) provide consistent estimates of (10).[15] Under the null hypothesis, the estimated errors for each country are randomly distributed around zero. This will not be the case under the alternative hypothesis. As described above, information on the cross-sectoral importance of the Sth endowment can be used to conduct tests based on the normal approximation to the binomial. There is no need to describe these tests here, for they are identical to those developed under Variant 1. In the following section, our methodology is applied to study the implications of systemic differences in the stringency of environmental policy across countries.

[14] For example, see Leamer (1984).

[15] We have not strictly provided a proof of consistency. However, consistency follows from the fact that the error terms including the effect of the nonstandard endowment have the same properties as assumed for the error terms in the previous section.

VI. AN APPLICATION

The level of environmental degradation in East Europe has been a focus of attention in recent years, especially since the political changes of 1989. Sláma (1986) shows that there are substantial differences in the level of sulfur dioxide emissions between socialist and capitalist countries and that these differences cannot be explained simply by variations in levels of aggregate economic activity. He provides a partial explanation for these differences, including the high-energy content of East European production and the use of high-polluting energy sources, especially brown coal. However, Sláma (1986, pp. 287 and 292) concludes that the relative pattern of primary energy use across countries accounts for only part of the differences in sulfur dioxide pollution between East and West.

In the application of our methodology, we consider another explanation for differences in the level of pollution between East and West, differences in the share of pollution-intensive commodities produced in each region.[16] Presumably, environmental policy is more lax in the East than in the West, reducing the relative cost of producing goods intensive in pollution in the East.[17] In this case, trade provides an opportunity for countries in East Europe to specialize in the production of pollution-intensive goods, while countries in the West specialize in goods not intensive in pollution.[18] Indeed, this presumed pattern of specialization is implicit in much of the popular commentary that characterizes the causes of East European environmental degradation. In that commentary, the communist leaders are presumed to have sold the environment cheaply in order to earn foreign exchange to pay back their hard currency debts.

The success with which East European governments have been able to translate environmental degradation into hard-currency exports can be evaluated using the methodology developed above. Typical cross-sectional models of trade, such as Heckscher–Ohlin, show that the relationship between trade performance in a specific good and factor endowments is constant

[16] Here the term pollution-intensive good is used to mean a good whose production produces more than average levels of pollution or requires more than average amounts of pollution-abatement cost.

[17] Some East European countries have had very strict environmental laws and standards, all of which seem to have been flagrantly ignored.

[18] See McGuire (1982) for a theoretical discussion of this point.

across all countries (Leamer, 1984). In the present application, we use this model and view the laxity of a country's environmental policy as one of a country's factor endowments, in addition to the more traditional endowments of labor, capital, and land.[19].

Treating environmental policy as an endowment and using a model that corresponds to (1) above, Tobey (1990, p. 192) conducts an empirical analysis of market-economy trade and finds that "the stringent environmental regulations imposed on industries in the late 1960's and early 1970's by most industrialized countries have not measurably affected international trade patterns in the most polluting industries."[20] Thus, there is no empirical evidence to suggest that differences in environmental regulation affect the structure of trade between market economies. In this case, the present empirical exercise is consistent with the structure of Variant 1 of our methodology. Hence, we do not make strong assumptions concerning the cross-country correlation between the environmental resource endowment and other resource endowments in order to apply our tests.

The trade model that we use is also consistent with the equations in (1), but two data limitations distinguish our precise specification of the model from Tobey's and others who consider only market economies in their research. First, the standard data set on national factor endowments, complied by Leamer (1984) for 1975, does not extend to East European countries. This problem is solved by using our methodology to estimate these endowments, as described above.

Second, there is no source of foreign-trade data that provides comparable observations for both CMEA and non-CMEA countries on levels of trade within commodity categories sufficiently disaggregated for the present study. This problem, as well as the solution we adopt, is described in detail in Murrell (1990). To measure the trade performance of countries, we use mirror statistics on exports and imports for 1975, derived form the reports of Western countries compiled by the United Nations. Because these mirror statistics do not include data on intra-CMEA trade, they can only be considered to capture the structure of trade, and not levels of trade. Hence, the standard Heckscher–Ohlin model must be recast to derive a relationship between the structure of trade and resource endowments.[21] In this case, the

[19] To justify this viewpoint, think of the degree of laxity of environmental policy as being directly related to the amount of pollution emissions that a nation produces. Moreover, as is common in environmental economics, view those pollution emissions as an input into the production function of firms. Hence, laxity of environmental policy plays an exactly analogous role in the production structure of a country as the more usual factor endowments.

[20] Complementary research conducted by Leonard (1988), Pearson (1985, 1987), and Walter (1982), using alternative methodologies and data, confirm this result. Incidentally, Tobey (1990) used our methodology, among others, to test his hypothesis.

[21] For details, see Murrell (1990).

y_{ij} measure country j's net exports of commodity i normalized using country j's total volume of trade and the z_{kj} measure country j's per-capita endowment of resource k. The model is estimated for 46 market economies and 9 socialist economies. Eleven standard endowments are included: capital, three types of labor, four types of land, and three types of minerals. Data on these endowments for market economies are provided by Leamer (1984).[22]

Given that measurements of the laxity of environmental policy are highly imprecise, we view this endowment as nonstandard and test its effect of trade performance using our procedure. In view of the empirical evidence provided by Sláma (1986) and popular commentary, we assume that this variable, z_{Sj}, is larger in socialist economies than in market economies.

The b_{iS} represent the amounts by which the normalized net exports of good i change with an increase in the laxity of environmental legislation. This interpretation suggests a source of information for determining the sign of the b_{iS} for each commodity i. If a product requires much pollution or pollution abatement in its manufacture, then more of that product will be exported as laxity in environmental policy increases. Similarly, if a product is not pollution-intensive, then less of that product will be exported as laxity increases. Thus, the signs of the b_{iS} depend on the pollution intensity of commodities.

Following Tobey (1990), we assume that the ranking of industries according to pollution intensity does not vary across countries.[23] In this case, we can use the share of pollution abatement costs in total costs in the United States to indicate the pollution intensity of commodities. These data were reported in Kalt (1985) for U.S. industries and were matched to the 3-digit level of the Standard International Trade Classification (Rev. 1) (SITC) for the purposes of the present exercise. Then, the 3-digit SITC goods were divided into two sets, those whose production requires relatively large amounts of pollution or pollution abatement costs and those requiring smaller amounts.[24] Within each of these sets, 10 commodity aggregates were produced from the much larger number of 3-digit SITC categories. The methodology of principal components was used to produce each set of aggregates. Hence, there remained 20 aggregates, half pollution-intensive and half

[22] For more precise details on the data, the reader should consult Leamer (1984, Chap. 4) and Murrell (1990, Chap. 2).

[23] This is a weak form of the assumption that technology is constant across countries, which is common in international trade theory and empirical international economics.

[24] The dividing line between the two groups is to some extent arbitrary. In Kalt's (1985) data, pollution abatement costs range from near zero to 2.89% of total costs. We chose 1.10% as the dividing line between the two groups, a number that divides the data approximately in half and does not arbitrarily divide any major product groups. The products immediately above this dividing line are nonmetallic minerals and simple manufactures of metal. The products immediately below are pharmaceuticals, soaps, and automobiles.

non-pollution-intensive. For the former group, it is assumed that the b_{iS} are positive and for the latter, negative.

There are two ways in the implementation of our methodology in which the procedures place further controls to ensure that the results are not biased by extraneous factors. First, we compare the East European countries to a set of market economies that are as closely related in geographic proximity and level of development as possible. Hence, the results control, as best as possible, for factor endowments, both physical and economic, that might have been omitted from the analysis. Thus, we compare the East European nations to the following set of countries: Austria, Greece, Ireland, Italy, Portugal, Spain, and Turkey, which we call low-income OECD countries.

Second in applying the methodology, we treat these countries and the East European countries identically by employing no information on the factor endowments of a particular market economy when we estimate the error terms for that particular country. We accomplish this by applying the estimating procedure separately for each market economy in the above list, omitting that country's data from the factor endowments, and estimating the country's endowment data. That is, for the purposes of estimating the errors of a particular market economy, we treat that country as if it had the same problems of data availability as a centrally planned economy. This procedure ensures that no property of the results could be caused by any asymmetry in the treatment of the two sets of countries.[25]

The null hypothesis that we test is that differences in environmental policy do not affect the structure of trade. If the null hypothesis is accepted, then there is no evidence that the East European nations have been able to transform environmental degradation into gains from trade. Alternatively, rejection of the null hypothesis implies that socialist economies tend to trade environmental quality for foreign exchange to a greater extent than comparable market economies. Given the distribution of z_{Sj} across countries and the signs of the b_{iS} under the alternative hypothesis, the error estimates for countries with lax environmental policy should be positive for commodities that are pollution-intensive and negative for commodities that are not pollution-intensive. The opposite is true for countries with stringent environmental policy.

The results of the tests on the error terms are produced in Table 1. Column (1) lists the country of country grouping for which the results apply. Column (2) specifies the numbers of errors whose sign is tested, which is 20 times the number of countries in each group. Column (3) specifies the number of errors that support the hypothesis that the country or country grouping in

[25] See Murrell (1990, Sections 5.5, 5.7, and 5.8) for precise details of the procedures for estimations of the error terms of the model.

TABLE 1

TESTING FOR THE EFFECTS OF ENVIRONMENTAL POLICY ON THE STRUCTURE OF TRADE OF EAST EUROPEAN COUNTRIES

(1) Country or Group	(2) Number of error terms generated	(3) Number indicating a lax antipollution policy	(4) (3) as a proportion of (2)	(5) Test statistic[a] for difference from low-income OECD economies
Low-Income OECD	140	67	0.48	n.a.
Bulgaria	20	8	0.4	−0.66
Czechoslovakia	20	7	0.35	−1.1
GDR	20	7	0.35	−1.1
Hungary	20	7	0.35	−1.1
Poland	20	9	0.45	−0.24
Romania	20	10	0.5	0.18
Yugoslavia	20	6	0.3	−1.49
Soviet Union	20	11	0.55	0.60
Albania	20	10	0.5	0.18
East European 6[b]	120	48	0.4	−1.27
East European 9[c]	180	75	0.42	−1.11
Latin American 10	200	100	0.5	0.39

[a] Using the normal approximation and a one-sided test, a test statistic greater than +1.65 rejects the null hypothesis at the 5% level.
[b] Bulgaria, Czechoslovakia, GDR, Hungary, Poland, Romania.
[c] All East European countries listed above.

question has a lax environmental policy. Column (4) shows the proportion of such errors. Column (5) provides the approximately normally distributed statistic for the test of the null hypothesis, comparing the country or group to the set of seven low-income OECD countries listed above.

The results in the table indicate that the null hypothesis cannot be rejected. In fact, six of the nine East European countries have a test statistic that has the opposite sign than would be expected on the basis of the alternative hypothesis that the environmental degradation of East Europe affected trade patterns. To see whether perspective can be added to these results by examining a different region of the world, at a lower level of development than either the low-income OECD countries or the East European nations, we derived equivalent results for a grouping of Latin American countries.[26] Again, as can be seen from the last row of the table, differences in environ-

[26] Argentina, Bolivia, Brazil, Chile, Columbia, Ecuador, Mexico, Paraguay, Peru, Uruguay, and Venezuela.

mental policy were apparently not translated into differences in trade patterns in 1975.

There are alternative explanations for the results that we have obtained, and especially for their seeming inconsistency with popular preconceptions. The first, and most likely, is that pollution costs are a rather small element of total costs, even in nations with stringent environmental policy. Therefore, differences in environmental policy do not translate into significant differences in the structure of trade. In that case, the leaders of the communist countries made a very poor bargain with nature when, in the 1980's, the environment was ravaged in an attempt to maintain macroeconomic balance. Second, since the debt crisis did not begin until the late 1970's and our empirical exercise is for 1975, the results might suggest that the stringency of East European environmental policy was closer to that of the market economies in the 1970's than in the 1980's.[27]

VII. CONCLUSION

The methodology presented here provides comparative economists with an alternative to traditional approaches to hypothesis testing. Classical methods are usually very intensive in their need for data comparable across different economic systems. These needs are often incompatible with data availabilities for cross-country analyses, especially those that involve East European countries. Our methodology allows the construction of empirical tests with less information than is normally required for empirical work.

We should emphasize that our methodology is flexible in application. It can be applied when all endowments can be measured across all countries or when none can or, of course, in any intermediate situation. That is, when either the set S_1 or the set S_2 is empty, as well as when they are both nonempty. This flexibility is important, especially when one considers the usual doubts about the meaningfulness of some SE statistics, doubts that are now the subject of debate in the Soviet Union (Ericson 1988). Give such considerations, one might be hesitant to rely upon analyses that, for example, used capital-stock figures from Western and Eastern sources as if they were comparable. The advantage of our methodology is that it allows the researcher to choose just how much domestic data to trust and include in the analysis.

Of course, there is a cost to using a reduced amount of data. If one omits endowment information for the SEs, one must assume that the same relationships between the excluded endowments and the dependent variables are applicable to both MEs and SEs. Therefore, when choosing how much data to include in any particular statistical analysis, the economist must

[27] Results in Murrell (1990, p. 117), analyzing trade patterns from 1975–1983, indicate that this is a possibility. But these results rely upon invoking assumption (2) above.

weigh problems of data quality against the additional theoretical restrictions imposed by conducting an analysis that is more sparing in its data requirements. Given conformity of the underlying analysis to our basic framework, whatever choice one makes along the trade-off between more data and more assumptions, our methodology can be applied.

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