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The Lagged Adjustment of U.S. Trade to Prices and Income -- A Commentary

by

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The Lagged Adjustment of U.S. Trade to Prices and Income -- A Commentary

John F. Wilson*

Introduction

The appearance of the interesting article by Joseph Miller and Michele Fratianni in the Spring 1974 issue of the Journal of Economics and Business (Temple University) reflects the current high level of interest in lag processes and the adjustment of trade flows.1/ Unfortunately, the results in the Miller-Fratianni (M-F) article raise far more problems than they solve. While the authors' intention is "to stay strictly within the confines of theory" (p. 191) and to put trade studies on a sounder footing, in fact the article contains a number of debatable applications of aggregate demand theory, as well as several errors which vitiate the authors' stated conclusions.

At the outset it should be should stressed that there seems to be no particular problem with the four estimatable functional functions (a) through (d) given by M-F on p. 193. Indeed, what is surprising is how conventional these functions are in view of their "derivation" from utility foundations -- about which more will be said shortly. In fact double-log import and export demand equations using real income and relative prices as the main arguments are, and have been for some time, quite an ordinary tool for laborers in this particular vineyard. Previous researchers in U.S. and foreign trade

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1/ See Joseph C. Miller and Michele Fratianni, "The Lagged Adjustment of U.S. Trade to Prices and Income", Journal of Economics and Business 26: 191-198 (Spring, 1974).

flows have frequently started from the premise of zero-degree homogeneity in price and income responses (which accounts for the use of deflated income and price relatives), and most are also inclined to prefer a constant elasticity over the constant marginal propensity hypothesis (which accounts for the log-linear specification).2/

In this context, however, M-F might well argue that their system of demand functions is not determined by such common assumptions as: a) linear form, b) zero-degree homogeneity, and c) constant elasticities, but follows, perhaps coincidentally, from a tighter specification regarding underlying utility. Nonetheless, after the derivation is complete, the end product is a set of equations which is readily recognizable and has been frequently used before. In fact, the M-F equations are rather spare, due to the authors' deliberate shunning of "mongrel" variables, although as will be noted below, even mongrels have their place in theory. What, then, are the major difficulties the M-F article poses for the reader?

Time Lags and Elasticities: How Many Issues are There?

Early in their article, M-F address the question of trade flows in relation to income and prices as if there were two separate

2/ Interestingly, the homogeneity restriction is an assumption which is called into doubt by one of the references frequently cited in the M-F article. This is Leamer and Stern [16, p. 45], who wrote that "the absence of money illusion. . . is too strong a proposition to be known a priori and imposed on the data." It is, however, by far the hypothesis of choice for empirical testing, both because there are also strong arguments in its favor and as a practical consequence of the collinearity problem which ensues when multiple price level terms are used.

problems: "What are the magnitudes of the long- and short-run price and income elasticities of demand for U.S. exports and imports? What is the time-pattern of adjustments in trade volume to changes in relative prices and income?" (p. 191) These are in fact almost indistinguishable questions, so it is not wholly accurate to say, as M-F do, that the first question has received a great deal of attention but the second almost none. Any study which seeks to distinguish long- from short-run elasticities is by its nature required to deal with the problem of distributed lags, and every known method for estimating lag distributions yields some estimate of the "time-patterns of adjustments". This holds true for systems in which the basic "pattern" is predetermined by the investigator, as in pre-weighted or Fisher lags, as well as where the time path involves exponential coefficient declines (such as Koyck lags), and in more flexible methodologies such as Almon's and the recently developed and very flexible Shiller procedure. Even the most flexible methods are generally used in such a way that they impose some small amount of prior information on the coefficient structure, as for instance through the choice of some polynomial curve and the use of end-point constraints.[3/]

There have in fact been some trade studies in recent years in which lag structures are absent, but by definition such studies

3/ In fact the omission of such constraints in the M-F import equations estimated by the Almon method leads to entirely unacceptable results which will be examined in detail below.

cannot provide information on short-vs-long run elasticities. For instance, Houthakker and Magee's well-known multi-country study [8], Reimer's article on U.S. imports of materials [22], and Rhomberg and Boissoneault's paper [23] omit lag distributions. Whether such studies employ quarterly or annual data, it is generally agreed that estimates of response parameters will be intermediate between some "true" short run value and the "full" long run value.4/ At all events, the two questions posed by M-F represent a false dichotomy, since addressing the one necessarily involves giving answers to the other. In addition, the amount of literature cited by M-F on both subjects is quite understated. Besides the references cited in their article, there is a good deal of additional material of which the interested reader may be aware. Much of this delves into both questions posed by M-F.5/

Aggregate Utility and Demand Functions: Do Derivations Make Sense?

A much more serious problem arises in connection with the M-F derivation of the import and export demand relations. According

4/ As evidence has accumulated since the December 1971 Smithsonian Agreement that full-responses take a good deal longer than a year (a conclusion borne out in M-F as well), the use of annual data no longer can be said to provide an adequate approximation to the long-run parameters.

5/ Among the published studies are works by Branson [1], Branson and Junz [2], Junz and Rhomberg [11], Krause [13], Kreinin [14], Kwack [15], Liu [17], Marston [18], and Price and Thornblade [20]. There are also several unpublished sources, including the 1968 dissertation by Grimm [3], whose topic was quite similar to the one addressed by M-F, Haworth [4], Rao [21], Rodriguez-Mederos [24] and recent work for Project Link [12]. At the time their article was written, M-F could probably not have known of the studies by Wilson [30], Hooper [6] and Hooper-Wilson [7].

to the authors, "The demand function can be derived from additive utility functions of the form proposed by Frisch and Houthakker. Almost without exception, studies of import or export demand have not been based on microfoundations such as the additive utility functions, but the derivation procedure places the model on a sounder theoretical basis and makes explicit the aggregation assumptions." (p. 191) The correctness of the first two assertions may readily be granted: demand functions can be derived from utility functions, additive or otherwise, and it is true that trade, and other macro-economic researchers seldom go through such derivations. But does it therefore follow that carrying through such a derivation really "places the model on a sounder theoretical basis"? For several compelling reasons I would argue no.

In the first place, M-F have not in fact built on "micro-foundations" but on macro foundations. They have used an aggregate utility function, which many theorists claim cannot be shown to exist, even if individual utility functions may be said to exist. There are several fundamental problems in aggregating such individual functions upwards to the community level. Vanek [28, p. 170], for instance, illustrates a case in which an infinity of "well-behaved" community indifference curves can pass through the same point. It is also known that indifference curves for a community of individuals, all of whom behave consistently but choose different points on a consumption possibility frontier, may give what seems to be an inconsistent community consumption pattern following a change in prices.[6]

A second and more decisive objection to the derivation is that M-F have written their additive utility function in the following form:

$$U(m_1, \ldots, m_n) = U_1(m_1) + \ldots + U_n(m_n) \tag{1}$$

with imports of goods and services as arguments, and maximized this function subject to a "budget constraint." Nothing, however, in the entire literature on utility analysis, supports the characterization of consumer utility as a function of <u>imports</u> alone! Utility in the usual sense is some function of <u>consumption</u>, which includes both domestically produced and imported components. It seems entirely impermissible to begin with such a utility function, unless there were a society somewhere which produces nothing and imports everything. If one chooses to make such "derivations", at the minimum it would be necessary to write the additive function in terms of <u>goods consumed</u> (say x_i) as:

$$U(x_1, \ldots, x_n) = U_1(x_1) + \ldots + U_n(x_n) \tag{2}$$

taking account of the additional relation

$$x_i = d_i + m_i - e_i \tag{3}$$

where d_i, m_i, and e_i are goods produced domestically, imported and exported, respectively and which are homogeneous with respect to their characteristics. Of course this vastly complicates the analysis,

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6/ See, for instance, Hicks [5, Chapter 6] and Pearce [1[illegible], Chapter 3 and pp. 108-109]. One should also note in this context that Sato's derivation (25, pp. 103 ff), which is cited by M-F as the basis for their own, is in terms of the <u>individual</u> consumer's preference field, not the community's.

since to complete the system one would need an additional set of equations to explain the shares of d_i and m_i in domestic consumption of x_i.

Note should also be taken of the "budget-constraint" referred to by M-F, for this is essential to their derivation. The constraint employed by Sato [25, p. 105], expresses the usual condition

$$I = \sum_i p_i \, x_i \tag{4}$$

where I = income

p_i = price of the i^{th} good.

This constraint applies to goods consumed, not just to those imported. In what sense is this constraint applicable to the M-F derivation in the form their utility function is given? Certainly one cannot say -- although it is mathematically implied -- that the proper constraint is $I = \sum_i p_i m_i$, for this would suggest that all income is spent on imports! In fact, the M-F utility function has no appropriate budget constraint, and the subsequent derivation is therefore impossible.

A third major problem is posed by the following question: Can a utility function be used at all to derive aggregate trade demand relations? Utility analysis, as developed so far, only applies to consumers, not to producers. But the import and export demands estimated by M-F cover the whole spectrum of commodities and services, and this means that producers' goods are also included, as well as a broad mix of invisibles.

Taking two recent years as examples, in 1965 and 1972 producer goods comprised 58% and 46% of the respective U.S. merchandise import totals.7/ How can M-F apply the concept of aggregate utility to such data? Producer "utility", insofar as it can be said to exist, might conceivably be depicted as a function of profits, but it can hardly be thought of as a function of imports, or even as one of total goods' purchases.8/ So even if the technical aspects of the M-F derivation of trade demand functions from a utility hypothesis had been correct, the conceptual link between the two is so weak that the procedure makes little sense.

Finally, let us consider an aspect of such derivations which is not only ignored by M-F but in much of the demand literature in general. This is that the relation between utility functions and demand functions is very much a "Which came first, the chicken or the egg?" sort of thing. Even ignoring the problems described above, although M-F may argue that a "derivation procedure places the model on a sounder theoretical footing" (p. 191), how can this be the case unless we have some reason to believe the underlying utility hypothesis

7/ Producers' goods are here defined as the sum of industrial supplies and capital goods in the End - Use classification. These percentages are minimums, since producer-bought components in other groups have been ignored.
8/ Further, imports in <u>all</u> goods' groups, including consumer items, are overwhelmingly made by producers or intermediaries searching for resale profits, not by consumers themselves.

is the right one? By what first principle of reasoning is a system of demand equations derived from an arbitrarily specified utility system more believable than a demand system written out directly to conform to some pattern of plausible constraints, such as homogeneity of a certain degree? Why is it better to differentiate one of an infinite possible variety of utility functions to obtain an "associated" demand system, than it is to be able to integrate backwards from an acceptable demand equation to find the "associated" utility system which generates it?

Is this context one should also note what Sato [25, pp. 103-104] writes about the same additive utility system subsequently employed by M-F: "This particular utility function is only a special case. Other types of additive utility functions lead to different demand functions". How, then, is the theoretical basis of the M-F or any other study improved by appealing to utility principles unless the investigators are willing to argue for one particular form of the utility hypothesis to the exclusion of others? M-F are correct in saying that "the demand function can be derived from additive utility functions. . ." (p. 191), but this is not to say it should be so derived. One can just as easily start with a "congenial" demand function and integrate back to the associated utility function if one prefers.

In fact, there is evidence that econometricians do tend to work backwards in this fashion. The basic criterion by which the utility function-demand function relation is judged is most often some prior belief about the attractiveness of the associated demand system, and emphatically not prior a belief about what the true form of the utility relation itself might be. Sato, for instance, clearly recognizes this problem, and writes that "the attraction of an additive utility function lies in the simplicity of demand functions associated therewith" [25, p. 106]. He adds later: "We may note that econometricians' revealed-preference for double-log demand functions may suggest an implicit support to the generalized CES family" [25, p. 121].

So at the root of the matter the acceptability of the demand function appears to be the touchstone. It is convenient that M-F were able to find a familiar demand system which can be derived from the infinite universe of possible utility specifications, but it is most misleading to say that this purely mechanical derivation somehow shores up the theoretical basis of trade demand models.9/

In summary, serious flaws are evident both in M-F's mathematical procedure and their assertion that a trade model is somehow improved by tacking on a utility hypothesis. Of the two, the first problem is fatal. In a way, however, the second is more serious, because far too many investigators have fallen into the same trap of reasoning that "deriving" a demand function somehow vindicates the utility function which lies behind it. It does not.

9/ Sato, in fact, includes a section in his article entitled: "Derivation of the Utility Function from the Demand Function". Here the tail clearly wags the dog.

Disequilibrium and "Mongrel" Variables -- What Place in Theory?

The comments in above paragraphs should not be construed as an objection to the static functions which M-F finally adopt. As mentioned at the outset, these are much the same as used in many previous studies. But since the purported "link" between these functions and the background utility hypothesis cannot be taken as viable, one must also question M-F's assertion that "although the possible efficacy of other variables [i.e., those besides real income and relative prices] or functional forms exists, such additional variables have no place in a tightly formulated framework." (p. 191) In fact, hardly a trade study exists that does not employ at least a few "ad-hoc", or in the M-F terminology, "mongrel" variables, often with good statistical effect. This is because in any study of aggregate demand -- of which trade demand is just a special case -- the basic form of the demand function represents an equilibrium relation. There are numerous instances in which extraneous factors can disrupt the playing out of these demand forces. Transportation disruptions such as dock stikes are simply the most obvious examples. Such factors often have large quantitative consequences on trade flows, with many millions of dollars of imports and exports advanced or deferred to different dates from which they would have taken place otherwise.[10] Most investigators have included dummies

10/ See, for instance, the estimates made in various issues of the Survey of Current Business following the several dock strikes which took place during the sample period chosen by M-F. A careful and detailed study was also undertaken by Isard and Duus [9], which also shows that such effects are by no means neglibile.

or proxies for such effects, not because they are infatuated with "ad-hoc selection of variables", but because disequilibrium phenomena have an important place in explaining how trade flows evolve as they do.

The same can be said for regression variables often used to quantify other kinds of disequilibria in a national economy, for instance, unemployment rates, levels of capacity utilization, inventory changes, shifting tariff levels (a factor following the Kennedy Round), import quotas and, more recently, export controls.

Even under a strong <u>ceteris paribus</u> assumption that real income and relative prices remain unchanged, many of these other changing factors clearly impact on trade levels. They <u>must</u> in some way be accounted for in any properly specified system of import and export demand equations. While M-F never set forth exactly what in their view constitutes a "mongrel" variable, it seems fair to surmise that measures of disequilibrium such as those noted above are what they had in mind. Certainly the M-F equations contain no trace of such regressors. However, parameter estimates made without taking such forces into account must be deficient in that highly relevant information is omitted from the specification.[11]

Lagged Adjustment -- What Do the Models Tell?

One of the stated objectives put forth by M-F is to study the time-pattern of adjustment of U.S. imports and exports to changes

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11/ Most of the trade studies cited above and in the references have found one or more such influences to be quite statistically significant in addition to the effect of the variables used in the M-F equations.

in income and prices. To do so they employ the familiar Koyck and Almon distributed lag techniques.[12] The former of these gives no direct control over the "length" of the estimated lag, which technically stretches out infinitely. The second method, which has been widely applied in recent years, gives control over the length of the lag to be estimated and makes it possible to estimate separate distribution coefficients for several variables in any equation, but requires that the investigator specify the degree of the polynomial curve on which the coefficients are thought to lie.[13]

Several observations are relevant to the way these models are presented. One is a problem in the terminology M-F use in their description of the "stock-adjustment" model developed on p. 192. This model is written as:

$$m_t = m_{t-1} + \phi(m_t^* - m_{t-1}) \tag{5}$$

where m_t^* is described by M-F as the "long-run, fully adjusted demand function," or, as is more often the case, "desired imports" in period t,

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12/ The term "Koyck lags" will be used here, although the M-F model is technically derived from a partial adjustment hypothesis. Both models are commonly worked into estimatable forms which are nearly indistinguishable and imply an "exponential falloff" in the pattern of lag coefficients, according to the relation $\beta_{t-i} = \gamma\beta_{t-i+1}, 0<\gamma<1$ so that $|\beta_{t-i}| \Rightarrow 0$ as $i \Rightarrow -\infty$

13/ In their description of the Almon technique, M-F write that "Estimation becomes possible only if the time-pattern of each coefficient is specified by prior assumption." (p. 193) This is not correct. The lag length must be specified, but only the polynomial degree must be set by prior assumption. Since there are an infinite number of exact polynomial lines of any given degree which can be constructed between two time points, the time-pattern of the resulting estimates is not specified at all, but determined by the data alone.

given the current values of income and prices. Unless one is prepared to specify what is meant by a "stock" of imports (which are a <u>flow</u> phenomenon), it would surely be more appropriate to refer to this specification as a <u>partial-adjustment</u> mechanism, in which some fraction ϕ of the gap between currently desired imports and last period's <u>actual</u> imports is closed in each succeeding quarter.[14/]

A more fundamental problem is that estimation involving geometrically declining lag coefficients involves a basic indeterminacy about what hypothesis is being tested. For instance the <u>adaptive-expectations</u> model, which can be written as:

$$m_t^* - m_{t-1}^* = \delta(m_t - m_{t-1}^*) \qquad (6)$$

where an asterisk again denotes desired values, can be worked into almost identical estimatable form as that produced by the partial adjustment model and the pure Koyck model. As Johnston [10, pp. 300-303] points out, there are still other variants of such "gap-closing" models which can be worked into almost equivalent estimatable forms involving lagged dependent variables. None of these structural models make the same assumptions regarding the behavior of imports and exports, but since the final functional forms are for all practical purposes the same, the danger which inheres -- even if estimates are successfully

14/ Leamer and Stern [16, p. 23] also make this terminological slip.

made -- is that the researcher cannot know which of several hypotheses he may have "proved." A test which proves everything may in fact prove nothing.15/

M-F's use of this technique is also curious, because they are quite right in saying that "some evidence indicates that income adjustments may be much more rapid than price adjustments" (p. 193). In an extensive disaggregated study by the author [30], for instance, price and income lags were estimated separately by the Shiller method, and abundant evidence for this conclusion was also found.16/ Simultaneous and independent work by Hooper [6] yielded much the same results. Why, then, did M-F feel obliged to override both the evidence and their intuition and choose an adjustment hypothesis which constrains the rate of geometric decline to be the same for both income

15/ Note also that to substitute M-F's equation (5) into their equation (6), the variables in (5) must be interpreted as logs. This implies adjustment to disequilibrium proportions, not to gaps in import/export levels, as is usually the assumption.

16/ The Shiller method is a recently developed system of applying Bayesian prior expectations to some degree (1st, 2nd, etc.) of coefficient differences. This method is demonstrably more flexible than the Koyck or Almon procedure, and in fact can be shown to be equivalent to a stochastic Almon -- i.e., while the priors imply a polynomial curve of some degree, estimation results can and often do produce coefficient patterns which deviate from the pattern if the data are sufficiently strong. For elucidation and methodology see Shiller [26] and [27] and Wilson [30].

and price variables, when using the more flexible Almon method alone would have been sufficient?[17] Presumably the reason M-F used both procedures was to compare the long-run elasticity estimates for imports and exports which can be derived from each. In the section which follows more will be said on this and the comparative time pattern of adjustment shown by M-F's empirical results.

The Parameter Estimates -- Are They Plausible?

Quite the most curious aspect of the M-F estimates of lag distributions by the Almon technique is the omission of endpoint constraints (at the far end) in all of their equations. Though the Almon method leaves to the investigator the choice of whether to use an endpoint constraint, it is almost universally agreed that the influence of far-distant values of changes in the regressors must in some sense taper off toward zero. Use of such far-endpoint constraints may therefore be viewed as a legitimate application of prior behavioral

17/ One should also note that the Almon method, in principle, subsumes a geometric decline in coefficient values as a special case for any choice of polynomial curve of 2nd degree or higher. There is thus no need to estimate both Almon and Koyck forms together. Even so, an equation could be specified in the Koyck form, but with two separate geometric adjustment factors (say ϕ and γ) on prices and income. A method exists by which separate estimates of ϕ and γ can be made but at the cost of additional collinearity. See Johnston [10, pp. 299-300]. In either case parameter estimates will be inconsistent due to the violation of the assumption of independence between the dependent variable and the past period error terms.

principles to which the estimated system should conform, and can be made to conform by available statistical techniques.[18/]

The fundamental dilemma of econometricians investigating lag structures is how to satisfy the requirements of theory (i.e., prior beliefs) about how statistical estimates should behave, while at the same time allowing the raw data maximal flexibility to produce a set of parameter estimates in which the investigator's requirements play only a small role. Have the Almon estimates presented in the M-F paper successfully straddled the horns of this dilemma?

I will argue that they have not. As evidence, the reader is invited to examine Fig. 1 on the following page, in which the estimated price and income lag-coefficients obtained by M-F in the Almon versions of the import and export demand models have been graphed out.[19/] With the sole exception of the lag-coefficients on the real income variable in the export model, which approach a value of 0 in

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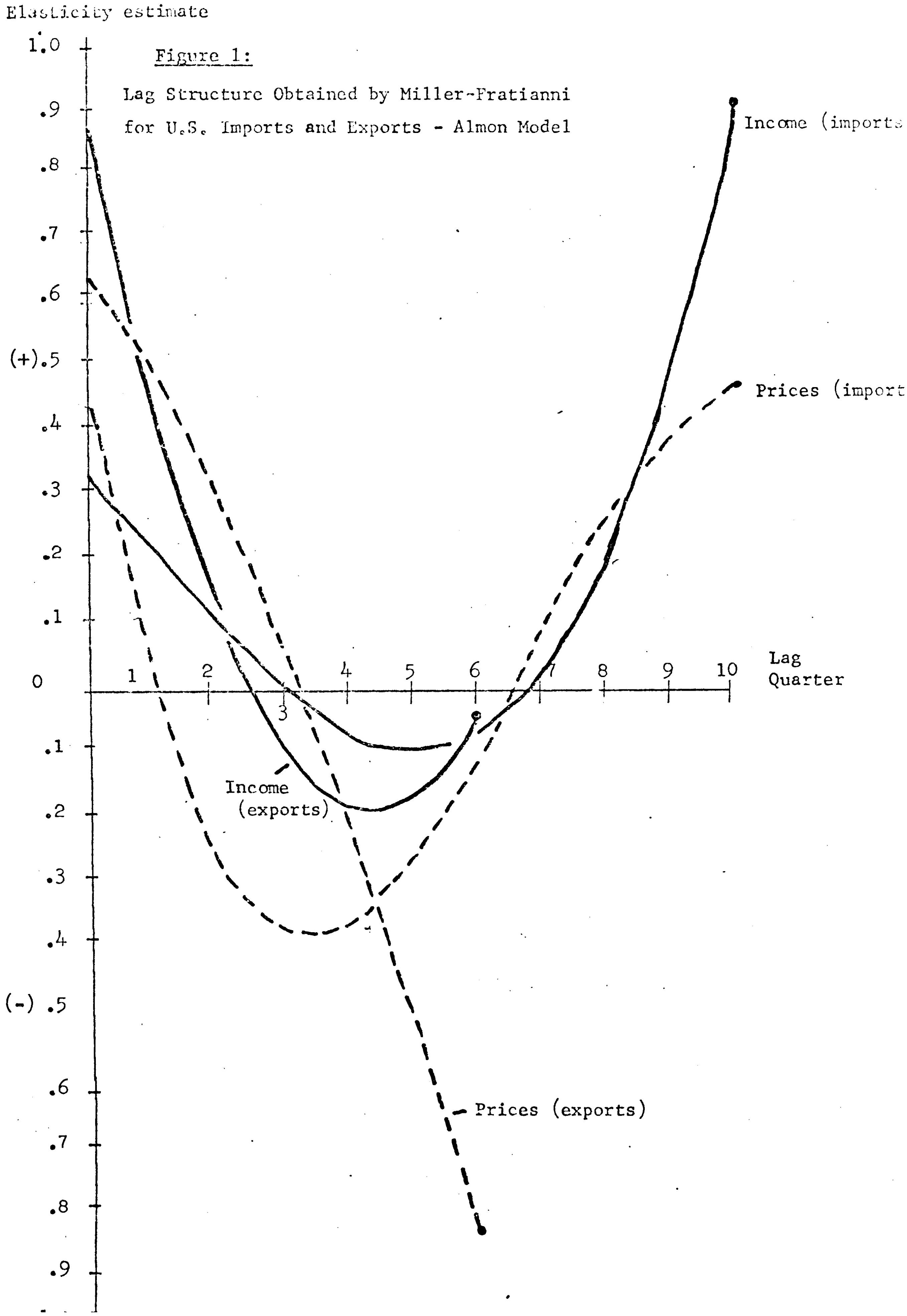
18/ Empirical researchers using the Almon technique often encounter "unacceptable" lag patterns, such as coefficients taking off toward infinity, when these constraints are omitted, but purge the results of such anomalies before they are published. To the extent that this reflects plausible prior beliefs about the necessary attributes of the lag pattern, such "data forcing" is not only defensible but necessary, and may perhaps be argued to compensate for the generally acknowledged and unwanted noise in available time series.

19/ Reference is here made to the data M-F give in their Tables 2 and 3 on p. 197. There is no need to graph out the time shape of the "stock-adjustment model" coefficients shown in Table 1, since the pattern is quite familiar. The long-run price elasticity (-.413) given in M-F's Table 1, equation 1.b. is in error. The correct value is -1.118.



Elasticity estimate
1.0
.9
.8
.7
.6
(+).5
.4
.3
.2
.1
0
.1
.2
.3
.4
(-) .5
.6
.7
.8
.9
1
2
3
4
5
6
7
8
9
10
Lag Quarter
Figure 1:
Lag Structure Obtained by Miller-Fratianni for U.S. Imports and Exports - Almon Model
Income (imports
Prices (import
Income (exports)
Prices (exports)

period t-6,[20] the remaining three sets of coefficients (the price parameters in both export equations and real income parameters in the import function, far from declining as theory requires, all seem headed for the wild blue yonder in the most distant quarter included in the lag structure.[21]

Is the reader therefore asked to believe that as one goes back in time, ever more distant changes in real income and relative prices have greater and greater absolute effects on the quantity of U.S. imports and exports, as seems to be implied by M-F's results? For example, referring to the import equation, is one to believe that a 1% change in real income eight quarters ago will cause a .203% change in real imports, while the same percentage change in income

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20/ M-F aver that "best results . . . were obtained when the polynomial constraint was omitted" (p. 195), and imply that no constraints were used in any of the equations. But the coefficients on the income term of the export equation approach zero in such perfect fashion -- which is highly unusual without the aid of the constraint -- that I suspect an endpoint constraint was in effect in this regression.

In support of the no-constraint approach, M-F argue that "zero constraints on the interpolation distributions imply the existence of an irregular pattern of lagged adjustment, possibly rising then falling over several quarters." Not at all. No polynomial is "irregular." Also, there is no theoretical objection to rising, then falling adjustment over several periods, so long as no unacceptable sign changes occur. Various investigators, e.g., Grimm [3], have found such patterns. Further, since any polynomial is a special case of all higher order curves, every interpolation polynomial used in the Almon method is capable of giving coefficient estimates which conform to a 1st degree curve or straight line. So zero constraints imply nothing about curvature. In fact, even without zero constraints, the M-F coefficients "rise then fall" over time, and vice versa.

21/ This observation may not be strictly true for the price term in the import function, since the polynomial is of the 3rd degree, and the inflection point appears to be at t-8, but the lag coefficients on all three terms are still rising (absolutely) at their respective terminal points.

nine quarters back increases imports by .487% and one ten quarters back raises imports by .887% all of this referring to trade flows in the current period? I certainly hope not, or we have lost our compass indeed. Imagine what the eleventh, twelth and twentieth period elasticities would be in such a system!

Fundamentally, what seems to be wrong here is that M-F have been too generous with their data, allowing the numbers to dictate too much, with the investigators imposing too little in the way of their own prior expectations. In consequence, the M-F estimates of the Almon model are very hard to accept. It is not just due to wanton capriciousness that empirical investigators demand that elasticity estimates diminish toward zero in the distant past, but for good reason, since it seems inconceivable that ever more distant events can exert greater and greater influence on our present lives as time goes by. While conventional wisdoms have been disproven before, surely when investigators such as M-F elect to tell a story which is greatly at variance with prevailing views, they thereby assume a heavy burden of persuasion, and must explain such results to a skeptical audience. Unfortunately there is no mention of or explanation for the anomalous pattern of these results in the M-F article.

An ancillary problem related to the lag estimates is that lag patterns in all of the four sets of Almon estimates produce a number of wrong-signed coefficients. The coefficient sum (long-run estimate) on the price term in the import relation also has an "incorrect" sign, and is statistically insignificant as well.[22] In this context it will be recalled that all the M-F estimates, both for imports and exports, were made using a deflated dependent variable, which is a proxy for volume movements. Now in volume terms there is nothing in current theory which would readily explain such sign-changing behavior in lag-coefficients.[23] As the M-F model is formulated, one would expect that all income coefficients should be

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22/ In the author's experience with trade equations estimated by various lag techniques, long-run elasticity estimates from different equations are fairly stable, even if control parameters (e.g. lag lengths and curve degrees) are varied in such a way that short-run estimates change greatly. M-F obtain the following long-run elasticity results:

		MODEL	
		"Stock Adjustment"	Almon
Imports	Prices	-.576	.204
	Income	1.677	2.003
Exports	Prices	-1.170	-.084
	Income	1.015	.900

The income effects are indeed similar in both models, but the long-run price estimates differ markedly, which may possibly be a side-effect of the rigidly constrained lag pattern produced by the partial-adjustment hypothesis. Also, though M-F set out to explore the "time pattern of adjustment" of trade flows, the final conclusion to be drawn on this point is most uncertain, due to the radically different apparent adjustment pattern produced by the two models. This can be verified by any interested reader who might want to trace out the partial-adjustment parameters and compare their time-shape to the Almon estimates on Fig. 1 above.

23/ If the dependent variable were nominal values, however, one might be able to cite the currently fashionable "J-effect", which helps explain responses to exchange rate changes or sharp changes in relative prices.

positive and all relative price coefficients should be negative. None of the four sets corresponds to this expectation, and at least some of the wrong-signed elasticities are statistically significant. Again there is not a word in the text to explain this curiosity, which badly needs explaining, since such results are quite different both from what theory leads one to expect and from what other empiricists have in fact found.

Conclusions

In the course of this commentary I have stated a series of strong objections both to the premises and results of the M-F study of lagged-adjustment in U.S. trade flows. Some of the mathematics are simply incorrect, and some of the results need far more explanation in light of current theory. One must conclude that the authors have not succeeded in strengthening the theoretical foundations of trade estimation, and have in fact relied on variables that almost everyone else uses in such work, "the classical explanatory variables of relative prices and income." The omission of disequilibrium effects is not justified, even if "rigorous" criteria are applied. The results, mainly the lag coefficients, are plagued by implausible patterns and by many wrong signs, for which no explanation is given.

These are all serious defects, but pointing them out is necessary given the importance of the topic M-F have treated. Indeed, they are to be commended for the timeliness of their presentation on a subject which has occupied a good deal of both official and academic attention since December, 1971, and promises to occupy much more as the world moves farther away from the fixed exchange rate regime of the Bretton Woods era.

I have not meant to give the impression that there are no valid conclusions in this study. Indeed, M-F's observation that "adjustment of import demand proceeds at a considerably slower pace. . . than export demand. . ." [p. 198] seems well-taken, although no-one is yet quite sure why this might be.[24] It is surely an interesting topic for future work. Also, it is gratifying to note M-F's comment that "unexpected changes in trade arrangements and structural changes occur and may distort any predictions coming from models like these." [p. 198] In the literature on trade flows there are far too few such acknowledgments that the world changes and that, hence, parameter estimates are not stable, inviolate and eternal. Far more research should also go into determining the nature and extent of such shifts.

Finally, M-F have also done trade research a service by again raising important questions concerning relative rates of adjustment, cost-absorption, pass-through and a host of other issues which econometricians in this field have not yet been able to solve to general satisfaction. It is encouraging to see others at work on these important problems.

24/ See for instance the results obtained by Hooper-Wilson [7].

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