April, 2002

Discussion of "Is Trade Good or Bad for the Environment, Sorting out the Causality" by Jeffrey Frankel and Andrew Rose

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Discussion of Frankel and Rose

This is a interesting paper examining the environmental effects of free trade. The overall goal of the paper is to answer a specific question: That is, what is the effect of trade on a country's environment, for a given level of GDP? In answering this question the authors try to disentangle the direct impact of trade on the environment holding GDP per capita constant, from the indirect effects created when trade stimulates growth and therefore raises GDP capita. The novelty of the paper arises from an explicit link drawn between openness to trade and growth, and their estimation procedures using instrumental variables.

The main empirical results follow from an augmented Kuznet's curve style regression regressing seven different measures of environmental quality on measures of income per capita, population density, a measure of democracy, and openness. To this the authors add an additional equation determining per capita income that is in turn a function of openness, and other country characteristics thought to be important to determining steady state levels of income per capita. Because of concerns about endogeneity, the authors instrument for both openness and income per capita in the Kuznet's curve style regression.

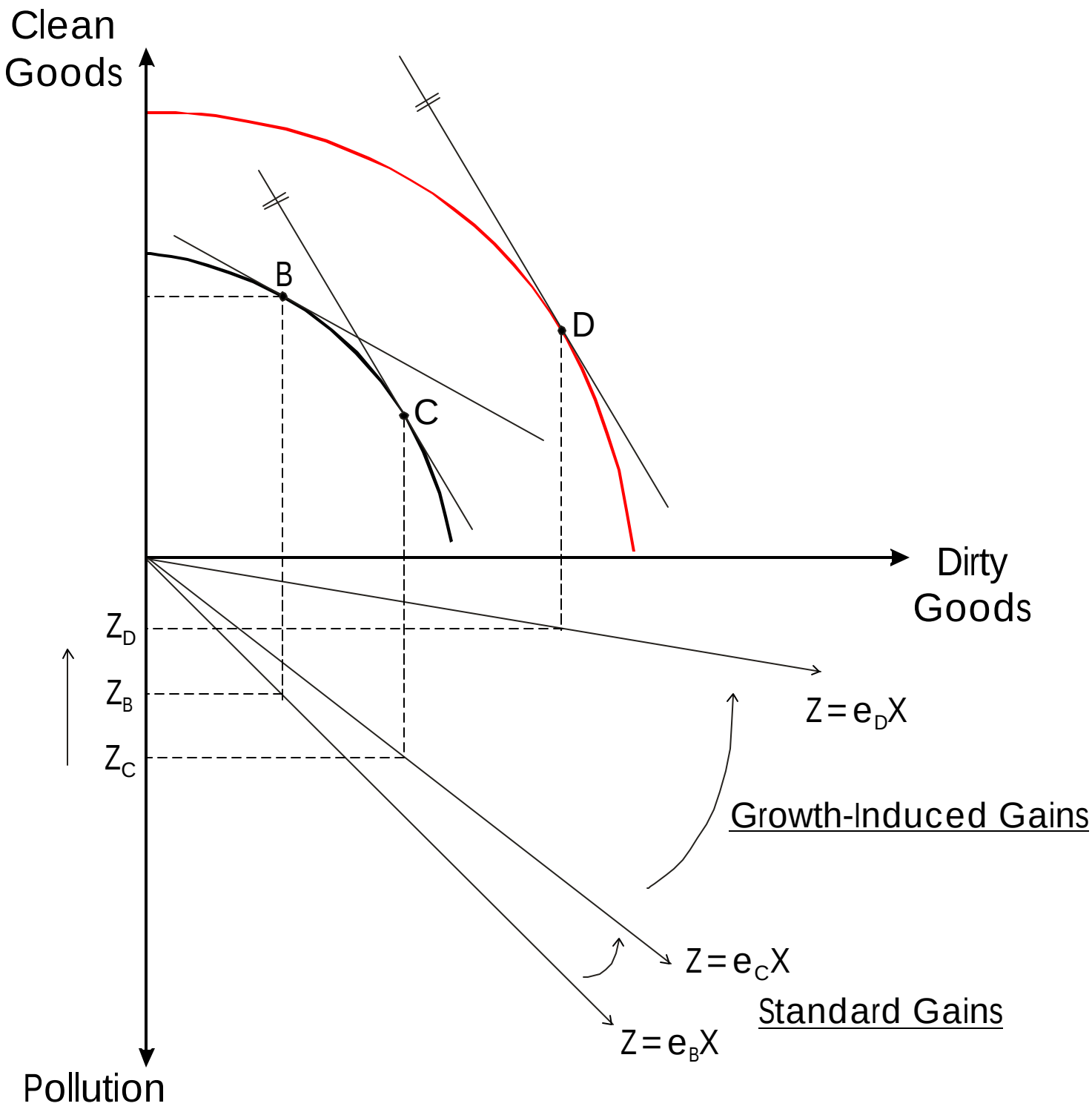
Overall, the results are favorable in that they find results similar to others in the literature; the relationship between income per capita and the measures of environmental quality trace out a Kuznets curve and in most cases the measure of openness to trade is either statistically insignificant or negative. Hence the authors conclude there is no evidence that trade harms the environment, even accounting for possible endogeneity.

The paper is similar to others in the EKC literature since it eschews formal theory and estimates a reduced form. In fact, their equation (2) differs very little from that estimated by Grossman and Krueger almost a decade ago. Because of this similarity the paper is subject to the same criticisms that befall all contributions to the Environmental Kuznets curve literature. There is no formal theoretical framework and therefore the causal mechanisms behind the results are unclear, the analysis is extremely aggregative, it assumes a similar mapping from income per capita to a set of very different pollutants, and we all know that estimated EKC's are often quite sensitive to changes in time periods, minor changes in econometric specifications, or data cleaning by the GEMS, etc.

The paper is different from the earlier literature in two ways. The first is that it tries to disentangle what we might call the long and short run effects of trade on the environment. To understand this difference, consider Figure 1.

[Figure 1 here]

In Figure 1 we have a small open economy that exports dirty goods. Dirty goods pollute and emissions per unit of output are given by "e". Pollution is then given by the negative vertical distance from the origin and labeled Z. Emissions per unit output are a choice variable for firms and subject to manipulation by governments by choice of pollution taxes or quotas. Suppose we are a dirty good exporter and we start at production point B, with an emission intensity given by Eb, and hence have pollution Zb. Then in the short run, with a trade liberalization we move to point C as domestic prices move closer to world prices and since world prices are such that our country is an exporter of dirty goods. Since trade raises real income, and environmental quality is a normal good, pollution policy is tightened and the emission intensity falls to Ec. Overall


Clean Goods
B
D
C
Dirty Goods
Z_D
Z_B
Z_C
$Z = e_D X$
Growth-Induced Gains
$Z = e_C X$
Standard Gains
$Z = e_B X$
Pollution

pollution may rise or fall depending on the strength of scale, composition and technique effects.

In my own work with Antweiler and Copeland, this is the end of the story. The short run is the long run and we came to the conclusion that for the average country in our sample, the movement from B to that of C led to less rather than more SO2 concentrations.

Frankel and Rose add to these impacts by altering the long run story to allow for a link between trade liberalization and growth. By assuming that openness to trade also raises growth rates the new long run equilibrium after the trade liberalization – is a point like D – where the entire production possibility frontier has shifted out. If we add in these new growth created income gains – which include in this case just scale and technique effects – it is possible that the indirect effect of openness leads to an actual decline in pollution levels. For example, if these gains came about because trade stimulated research and development and thereby brought with it neutral technological progress, then my own estimates for S02 would indicate that this additional effect of trade would tend to lower pollution levels.

The second novelty of the paper is the way it deals with endogeneity issues. Other authors in the trade and environment area have also considered various endogeneity issues – for example, Arik's work on FDI often allows for endogenous changes in regulation; Judy Dean's work on river quality in China has trade liberalization linked to endogenous changes in income, and in my own work with Antweiler and Copeland we examined a 2 equation simultaneous system with pollution and income in the appendix to our paper. So endogeneity issues have been raised and addressed before,

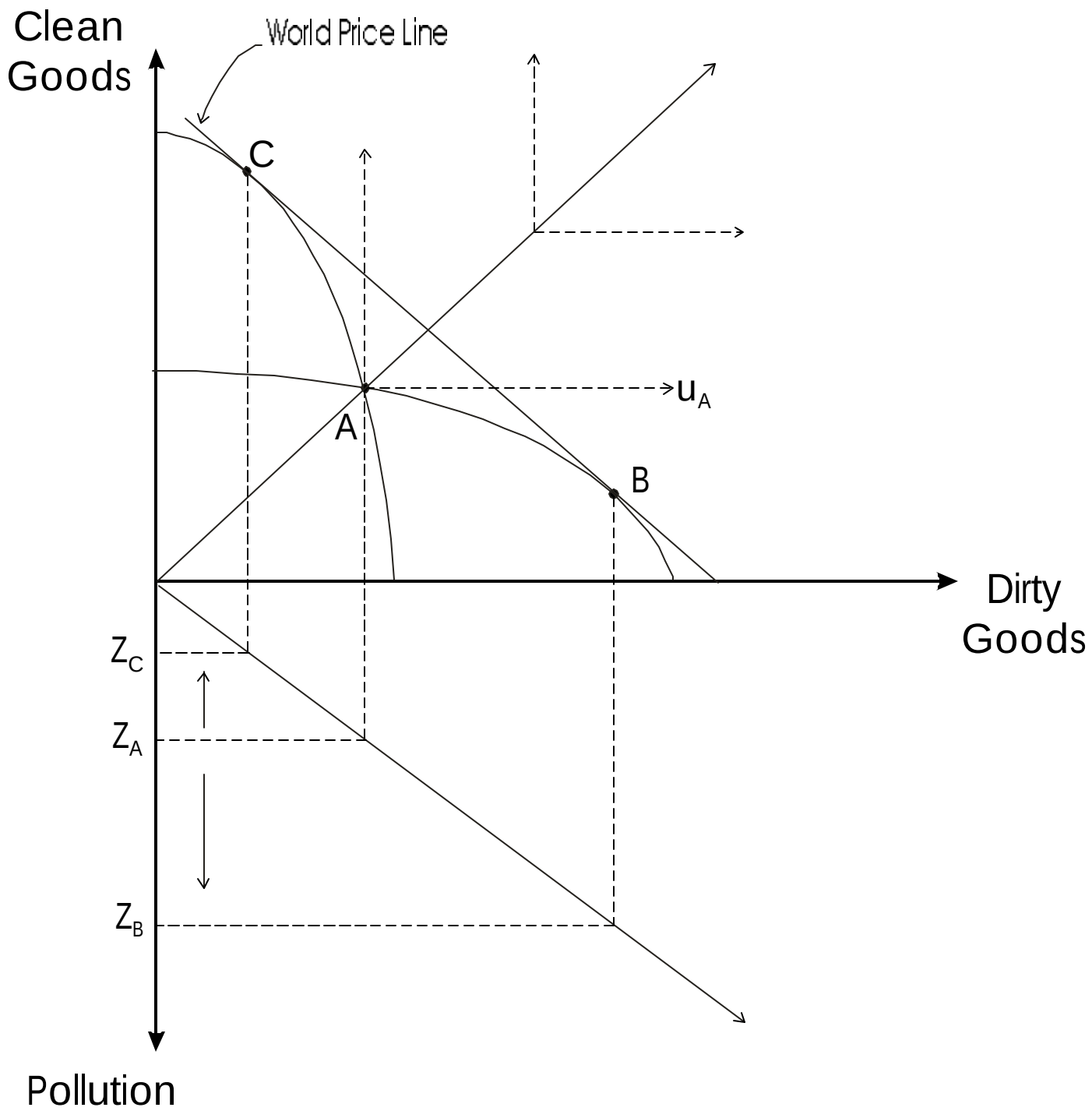
but the new wrinkle here is that Frankel and Rose consider the endogeneity of openness and income per capita. This is different.

I have three comments on the paper.

The first is that the question posed by the authors does not have a well-defined answer.

There is no one effect of trade on the environment even holding income constant. The reason for this is simple – its comparative advantage. To see this consider the figure below. Here I have drawn 2 countries with identical real incomes in trade, at common world prices. I have also assumed that pollution policy is rigid and unchanging in the movement from autarky to trade so that emissions per unit of output is constant during the change. The countries in autarky are at point A and have very different autarky prices – i.e. comparative advantage. When they drop their trade barriers they specialize and move to B and C respectively. Holding pollution policy constant, we can see that emissions or environmental damage changes very differently across countries. A reduction in trade costs will move countries to specialize in their comparative advantage sectors. For some countries this will mean a movement towards specialization in relatively environmentally benign activities – for others it may cause them to specialize more in either dirty industries or environmentally damaging activities. Hence, openness to trade, more trade, or trade liberalization should not have the same effect on countries even after we account for income per capita differences.

For example, suppose we take two countries with the same real GDP per capita – for the sake of argument let them be Brazil, which in 1999, had a per capita income of around 4,410 US – and take the Czech Republic with a per capita income of 4, 770.


Clean Goods
World Price Line
C
A
B
u_A
Dirty Goods
Z_C
Z_A
Z_B
Pollution

Suppose both of them had existing trade barriers in place and then reduced them. Would we expect that for these two countries, with the same real GDP per capita, that trade would have the same effect on their rate of deforestation? No, because Brazil is one of the world's most forested countries (over 62% of its land in 1999) and the world's biggest deforesters over the 1990-1995 period. The Czech Republic is neither.

The same is true for the other environmental indicators as well. Consider China which has a lot of dirty coal, and Cote D'Ivoire that does not. They have similar incomes per capita, but should we expect openness to trade to affect their SO2 concentrations in the same way? Again, no. Since trade allows countries to specialize in different activities, and since different activities have different environmental implications, we shouldn't expect trade per se to have any one effect on environmental quality. Therefore, to adequately capture the impact of more trade, we are going to need to condition on those country characteristics determining comparative advantage.

So my first comment is that the authors need to allow for the impact of trade to differ across countries. In my own work with Werner Antweiler and Brian Copeland, I did this rather crudely by interacting openness measures with country characteristics suggested by the pollution haven and factor endowment hypotheses. While this was perhaps an acceptable first step, a much better approach would be to work through the theory exactly to determine how and what country characteristics should enter.

My second comment is with regard to endogeneity.

The word endogeneity appears a lot in the paper, and the authors state this is in fact the key difference between their work and others. While endogeneity may be an issue, the fact is that the OLS estimates are virtually indistinguishable from the IV

estimates and hence if endogeneity is present it has had little effect on the results. Lets look at the estimates themselves and ask, if we were naïve and just ran OLS, without correcting for endogeneity would our point estimates for the effect of openness be far off? Below in Table 1, I have listed the OLS estimate besides the IV estimate for openness across all six of the pollutants. Besides this, I ask whether the OLS coefficient estimate falls within the 95% confidence interval of the IV estimates. This in effect, asks, if you only did the OLS estimation would your point estimate be rejected once you corrected for endogeneity. First, in all cases but two the sign of the coefficient estimate on openness is the same across estimations. Second, the magnitudes are in most cases very similar. And finally, if we ask the question are the OLS estimates within the confidence intervals for the IV estimation the answer is in each case YES. Therefore, the practical impact of instrumenting is small.

This of course does not mean that endogeneity is not present or important, and this brings me to a related point. One implication of my first comment on comparative advantage is that country specific characteristics are lurking somewhere in the error term of their equation (2). But the authors use as instruments for this equation, country characteristics such as schooling rates, investment rates, and perhaps population growth rates. But these country characteristics are likely to be exactly those that should be interacted with openness to start with since they determine comparative advantage. And hence the instruments are likely to be correlated with the error.

Therefore, the theoretical insight that openness is different for different countries carries with it a consequence for empirical work – factor endowments are not going to be

good instruments for income because they should already be present in the estimating equation.

Table A

ENVIRONMENTAL INDICATOR	OLS ESTIMATES TABLE 2	IV ESTIMATES TABLE 3	OLS ESTIMATES IN CI OF IV?
CO2	.016 (1.97)	-.001 (-.06)	YES
DEFORESTATION	.002 (.78)	.001 (.37)	YES
ENERGY DEPLETION	-.013 (-1.53)	-.033 (-1.67)	YES
NO2	-.302 (-1.91)	-.324 (-1.75)	YES
SO2	-.303 (-3.72)	-.210 (-2.36)	YES
SUSPENDED PM	-.256 (-.79)	.037 (.13)	YES
WATER POLLUTION	-5403 (-2.55)	-9960 (-2.66)	YES

Finally, my last comment has to do with interpretation.

This is a paper that tries very hard and in my view too hard, to be pro-trade. When the authors find a negative coefficient on openness they interpret this as meaning openness is good for the environment. To be frank, I don't know what the coefficient sign on openness is reflecting because there are a dozen stories I can tell about how openness affects factor accumulation, technology transfer, FDI, political economy and the like. Some work in the direction of lowering environmental damage, others work the other way. Until we have a good explanation for this result, I think it is premature to declare openness is good – especially in light of my previous comments about country characteristics. And I am not being doctrinaire or overly conservative about this either. Almost ten years ago, in the Grossman and Krueger's original 1993 NAFTA study Gene Grossman and Alan Krueger found a very similar result for SO2. The coefficient on openness in their SO2 regressions was both negative and significant. Grossman and Krueger however remarked that

"We have no good economic explanation for this finding", page 17, NBER 3914. I think that statement is still true today.

And I think the authors stretch a bit when they conduct their back of the envelope calculation showing how energy depletion falls with trade liberalization. They show how for energy depletion that the direct beneficial effect of openness could swamp the indirect negative effect for a country in the low income rising segment of the Kuznets curve. Here the authors mix coefficient estimates from OLS in table 1 with IV in Table 3. And then, they don't work out the full long run steady state impacts. Fortunately on page 11, they told me how to do it and when I redo the calculation using IV estimates for both and

calculating the steady state effects, I find the small beneficial effect they report turns out to now be a 10% point increase in energy depletion. Maybe I did this calculation wrong, but all of you can check the paper yourself.

Finally, we should always be careful to remember that failure to find an effect of trade on the environment is not the same thing as saying trade has a beneficial or at most a negligible effect. Failing to reject the null hypothesis is easy in many circumstances and this could very well arise from misspecification, noisy data, or improper econometric methods. In the trade and environment area, the finding of no finding is quite common and often welcomed. For example, many empirical studies have found that pollution abatement costs have no significant effect on trade flows, but I think it is far too early to toss in the towel here. Perhaps we should reconsider the theory and estimation methods first before we embrace these findings of no finding.

Stopped Here.

Not too long ago, a famous economist warned us of a growing tendency among researchers to search for a negative finding - a null hypothesis that cannot be falsified - rather than a positive one. In ending I think I should quote him and all of us, including the authors, should heed his wise words. He noted that

"It used to be the goal in econometric work was to get results that were statistically significant, to reject the null hypothesis. In order for an author to stand up in front of a conference proudly, or to expect to publish his paper in a journal, he or she sought to get significant results. This is difficult to do in macroeconomics. The world is a complicated place, it is unlikely that the few key variables that emerge from the particular theory one has developed will actually go far toward explaining a real-world time series. So what we have done – quite cleverly—is to redefine the rules. Now the goal is to fail to reject the null hypothesis, to get results that are statistically <u>insignificant</u> – in essence, to find <u>nothing</u>. It is far easier to find nothing than to find something."

Jeff Frankel, in the "Zen and the Art of Modern Macroeconomics", comments on Alan Stockman, Monetary policy in an era of change, Nov. 1988.

Estimating Equations

$$\ln(Y/Pop)_{90,i} = b_0 + a(X + M/Y)_{90,i} + b_1 \ln(Pop)_i + g \ln(Y/Pop)_{70,i}$$

$$+ d_1(I/Y)_i + d_2 n_i + d_3(School1)_i + d_4(School2)_i + u_i$$

(.1)

$$Enviro_{i,1995?} = j_0 + j_1(Y/Pop_1)_{90,i} + j_2(Y/Pop_2)_{90,i} + j_3(Y/Pop_3)_{90,i}$$

$$+ m(X + M/Y)_{90,i} + p(Polity)_{90,i} + l(LandArea/Cap)_{90,i} + e_i$$

(.2)