Growth, Inequality, Trade, and Stock Market Contagion: Three Empirical Tests of International Economic Relationships

by

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B.A. in Economics
Williams College, 1992

Submitted to the Department of Economics in partial fulfillment of the requirements for the degree of

Doctor of Philosophy in Economics
at the
Massachusetts Institute of Technology
June 1998

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Abstract

This thesis is a collection of three empirical tests of relationships in international economics. More specifically, Chapter 1 examines the relationship between inequality and growth; chapter 2 evaluates the relationship between trade and wage inequality; and chapter 3 investigates the extent of stock market contagion during recent global crisis.

Chapter 1 challenges the current belief that income inequality has a negative effect on economic growth. Previous work on this subject is plagued with econometric problems and has been widely misinterpreted to imply that countries which reduce inequality could improve growth performance. This chapter reassesses this work and addresses these problems. It uses more consistent measures of inequality to reduce measurement error and a panel estimator to correct for omitted variable bias. By focusing on a generalized method of moments technique developed by Arellano and Bond, it directly estimates how changes in inequality are correlated with changes in growth within a given country. Results suggest that in the short and medium term, an increase in a country's level of income inequality has a significant positive relationship with subsequent economic growth. This relationship is highly robust across samples, variable definitions and model specifications, with the one caveat that it may not hold in very poor countries. While these estimates of a short-run positive relationship between inequality and growth within a given country do not directly contradict the previously reported long-run negative relationship across countries, they do suggest that further careful reassessment of the numerous linkages between these two variables and their determinants is necessary.

Chapter 2 evaluates the impact of increased trade on wages and inequality throughout the world. Basic trade theory predicts that increased trade between developed and developing countries would increase wage inequality in wealthy countries and decrease inequality in poorer countries. A wide variety of methods and data sets have been used to test this basic concept, and most conclude that trade has had a minimal impact on relative wages. This empirical work, however, has several critical shortcomings. It tends to focus on a limited sample of countries, inaccurately classifies countries and trade flows as

Abstract (cont)

high- or low-skill intensive, and ignores several relevant variables such as trade with similar countries, capital flows, changes in the supply of skills, labor market structure, technological change, and other country-specific characteristics. This chapter attempts to correct for each of these shortcomings. It compiles a new data set on trade flows between countries, using income levels, average education, and manufacturing wages to classify countries and trade flows as relatively high-skill intensive or low-skill-intensive. Then, using this new data and controlling for many of the factors ignored in previous work, it estimates how changes in trade flows are related to changes in inequality and wages within a given country. It finds that increased trade with low-skill countries has a significant negative relationship with wage inequality and low-skilled wages in high-skill countries. In order to correct for endogeneity and isolate how trade affects wages, the final section of the paper formulates a general-equilibrium model linking wages and trade flows. It is difficult to estimate the model, however, due to the lack of good instruments for trade and wages in a panel framework. Therefore, although the reduced-form estimates suggest a strong relationship between trade and inequality, I am unable to identify the direct impact of increased trade on wages and inequality throughout the world.

The final chapter investigates contagion across stock markets. It begins with a discussion of several conceptual issues involved in measuring contagion. The standard methodology involves testing if cross-country correlations in stock market returns increase during a period of crisis. According to this approach, if two markets are highly correlated during periods of stability and they continue to co-move during a period of turmoil, this may not constitute contagion. The measure of cross-market correlations central to this standard analysis, however, can be biased. The unadjusted correlation coefficient is conditional on market movements over the time period under consideration, so that during a period of turmoil when stock market volatility increases, standard estimates of cross-market correlations will be biased upward. It is straightforward to adjust the correlation coefficient to correct for this bias. The remainder of the chapter applies these concepts in several empirical tests for stock market contagion. Tests based on the unadjusted correlation coefficients suggest that there was contagion across about half of the world's stock markets during the recent East Asian turmoil. If the same tests are based on the adjusted correlation coefficients, however, the degree of contagion falls by as much as seventy-five percent. This suggests that the strong co-movement in global stock markets during the later half of 1997 was more a continuation of strong cross-market linkages than pure contagion from East Asia. Similar tests for contagion after the Mexican peso collapse of 1994 and the U.S. stock market crash of 1987 support these central findings. This chapter closes by applying these results to simulate the impact of select stock market crashes on markets around the world.

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Acknowledgements

First, and by far most important, I would like to thank my parents for their endless support and advice. They have provided everything from the physical sustenance of delicious home-cooked meals and chocolate desserts, to the mental sustenance of unwavering encouragement and a firm grounding in what is important in life. Equally invaluable, they have always maintained complete faith in me, no matter how little they knew about my latest endeavors or how much I doubted myself. I would also like to thank my sister for struggling through drafts of these chapters, and for always reminding me that just because I was an economist, I still had to use proper English.

Next, and nearly as important, I am indebted to my advisors. I would still be enrolled at M.I.T. if Rudi Dornbusch had not constantly pushed me to give an extra breakfast presentation or pull together a draft of a paper. I can not begin to express my appreciation for his exceptional mentoring, concern, and candid advice. I am also indebted to Jaume Ventura for providing an excellent sounding board for new ideas, and for meticulously reading and commenting on my drafts. I am especially grateful for his cheerful support, encouragement, and treatment of me as his colleague. I would also like to thank Roberto Rigobon for his useful comments and discussions, as well as for helping inspire the chapter on stock market contagion.

Further thanks to Andrew Bernard for encouragement in the early stages of this project and for inspiring many of the ideas in this thesis. I am especially grateful to Richard Sabot for originally peaking my interest in economics at Williams College and for years of mentoring and support. Thanks to Daron Acemoglu, Abhijit Banerjee, Richard Eckaus, Jerry Hausman, Michael Kremer, Paul Krugman, and Robert Solow for providing helpful comments on various papers, and to Norman Loayza for an insightful discussion on the econometrics of panel estimation. I greatly appreciate Theresa Benevento's sympathetic ear, and especially for her lessons on how to actually get things done in the Economics Department. Last, but certainly not least, thanks to all of the participants in the international breakfast seminars. The informal discussions were invaluable in shaping my research at its early stages.

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Chapter 1:

A Reassessment of the Relationship Between Inequality and Growth

Abstract: This paper challenges the current belief that income inequality has a negative effect on economic growth. Previous work on this subject is plagued with econometric problems and has been widely misinterpreted to imply that countries which reduce inequality could improve growth performance. This paper reassesses this work and addresses these problems. It uses more consistent measures of inequality to reduce measurement error and a panel estimator to correct for omitted variable bias. By focusing on a generalized method of moments technique developed by Arellano and Bond, it directly estimates how changes in inequality are correlated with changes in growth within a given country. Results suggest that in the short and medium term, an increase in a country's level of income inequality has a significant positive relationship with subsequent economic growth. This relationship is highly robust across samples, variable definitions and model specifications, with the one caveat that it may not hold in very poor countries. While these estimates of a short-run positive relationship between inequality and growth within a given country do not directly contradict the previously reported long-run negative relationship across countries, they do suggest that further careful reassessment of the numerous linkages between these two variables and their determinants is necessary.

I. Introduction

Early economists, such as Nicholas Kaldor and Simon Kuznets, argued that there was a tradeoff between reducing inequality and promoting growth. In the post-World War period, however, many East Asian economies had relatively low levels of inequality (for countries of comparable income levels) and grew at unprecedented rates. In sharp contrast to this experience, many Latin American countries had significantly higher levels of inequality and grew at a fraction of the average East Asian rate. These trends prompted a surge of interest in the relationship between inequality and growth, and in particular, a reassessment of how a country's level of income inequality affects its subsequent rate of economic growth.

Over the past five years, many economists have attempted to measure this relationship by adding inequality as an independent variable to some variant of Barro's standard cross-country growth regression.[1] These studies generally find a negative and just significant coefficient on inequality, leading economists to conclude that inequality has a negative impact on growth.[2] This line of research has received such widespread support that a recent survey of this work concludes: "These regressions, run over a variety of data sets and periods with many different income distribution measures, deliver a consistent message: initial inequality is detrimental to long-run growth."[3]

There are, however, a number of problems with this "message" and the underlying results. First, many of the estimates of a significant negative coefficient on inequality are not robust. When any sort of sensitivity analysis is performed, such as when additional explanatory variables or regional dummy variables are included, the coefficient on inequality often becomes insignificant (although it usually remains negative).[4] Second, all of these studies are plagued with serious econometric problems, such as measurement error in inequality and omitted variable bias. These problems could significantly bias estimates. Third and perhaps most important, these cross-country results have been widely misinterpreted. Abstracting from the robustness and econometric problems, the cross-country growth

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[1] For examples of this "standard" regression, see Barro and Sala-i-Martin (1995).
[2] For example, see: Alesina and Rodrik (1994), Birdsall, Ross, and Sabot (1995), Clarke (1995), and Persson and Tabellini (1991).
[3] Benabou (1996), page 2.
[4] For example, see Alesina and Perotti (1993), Birdsall, Ross and Sabot (1995), Deininger and Squire (1996b), or Perotti (1996).

regressions suggest that countries with lower levels of inequality tend to grow more quickly. This has been construed to imply that governments which undertake policies to reduce inequality could actually improve, instead of reduce, long-term growth performance. While this would be a very encouraging result, the cross-country studies do not directly address this issue of how a change in inequality within one country would affect growth within that country.

This paper will address these shortcomings in this literature and reassess the relationship between inequality and growth. Section II surveys the extensive theoretical and empirical work investigating the channels through which inequality might affect growth. It discusses two major econometric problems with this work, measurement error and omitted variable bias, and suggests using more consistent data and panel estimation in order to minimize these problems. Section III describes the data set and model to be utilized and Section IV estimates this model using a generalized method of moments technique developed by Arellano and Bond. Since this estimator is based on changes in growth and inequality within each country, instead of across countries, it avoids the misinterpretation of previous work. Results suggest that in the short and medium term, an increase in a country's level of income inequality has a strong positive correlation with subsequent economic growth.

Since this significant positive relationship is in sharp contrast to the negative relationship reported in the cross-country literature, Section V investigates why these results differ. It finds that data quality, period length, and estimation technique all influence the sign and significance of the coefficient on inequality. Section VI conducts a detailed sensitivity analysis of this paper's central results, confirming that this positive relationship is highly robust to many permutations of the original sample and model. The one caveat is that these results may not apply to very poor countries, since inequality data for these nations is still extremely limited. Section VII concludes that these estimates of a short-run positive relationship between inequality and growth within a given country do not directly contradict the previously reported long-run negative relationship across countries. Instead, these results should be taken as a complement to existing studies, not only raising doubts about their "consistent message", but also suggesting that further careful reassessment of the numerous linkages between inequality, growth, and their determinants is necessary.

II. Previous Work, Past Problems, and Current Solutions

Motivated by cross-country growth regressions which consistently find that a country's initial level of income inequality has a negative effect on subsequent growth, a number of papers have explored the specific channels by which inequality might affect growth.[5] These explanations cover a wide range of topics, but can be broadly categorized as four sets of theories. First, inequality could slow growth by limiting aggregate levels of investment in human capital.[6] Capital market imperfections make it difficult for the poor to borrow, so that in more unequal societies, a larger share of the population is unable to invest in human capital. This will lower an economy's productivity and growth. Second, inequality can affect growth by increasing sociopolitical instability.[7] When the distribution of resources is more unequal, individuals are more likely to use non-market methods to pursue their goals. Whether this is expressed in violent forms (such as assassinations or coups) or nonviolent forms (such as rent-seeking or corruption), this instability can increase uncertainty and therefore decrease investment, or even disrupt production directly. Even the threat of this instability can undermine the government and force it to undertake inefficient policies to maintain support. Any of these effects could clearly hinder growth performance.

Third, inequality can affect growth by determining voting outcomes and the resultant fiscal policy.[8] In more unequal societies, a greater share of the population will vote for more redistributive policies. The higher rate of taxation necessary to support these policies will discourage effort and investment and therefore slow aggregate economic growth. Fourth and finally, lower inequality (especially within the agricultural sector or between the modern and traditional sectors) can strengthen domestic demand. This will in turn facilitate industrialization, modernization and/or development, all of which can translate into increased aggregate economic growth.[9]

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[5] See Benabou (1996) and Perotti (1996) for excellent surveys of this work.

[6] Theories based on this general framework are developed in: Aghion and Bolton (1993), Banerjee and Newman (1993), Banerjee and Newman (1991), Benabou (1996), Birdsall, Pinckney and Sabot (1995), Birdsall, Ross and Sabot (1995), Galor and Zeira (1993), Perotti (1991), and Piketty (1995). More recently, several authors have expanded this framework to incorporate how inequality affects the joint education/fertility decision and how this in turn affects growth. For an exposition of this theory, see Galor and Zang (1993) or Perotti (1996).

[7] Theories based on this general framework are developed in: Alesina and Perotti (1993), Alesina and Perotti (1994), Benabou (1996), Birdsall, Ross and Sabot (1995) and Sachs (1987).

[8] Theories based on this general framework are developed in: Alesina and Rodrik (1994), Benabou (1996), Bertola (1993), Birdsall, Ross and Sabot (1995) and Persson and Tabellini (1991).

[9] Theories based on this general framework are developed in: Banerjee and Newman (1996), Hazell and Haggblade (1993), and Murphy, Shleifer and Vishny (1988).

While most of these papers attempting to explain why inequality could have negative effect on growth are theoretical, a few include an empirical section testing one or more of these four channels. These empirical tests yield mixed results. Evidence supporting the first channel, that inequality limits investments in human capital, is indecisive, with the strongest evidence supporting some sort of link between inequality and the education/fertility decision.[10] Support for the second channel is stronger, with evidence suggesting that inequality does have a positive effect on political instability and that more political instability does deter investment.[11] Support for the third channel, however, contradicts the above theory, with higher inequality often decreasing (instead of increasing) redistribution and/or more redistribution often increasing (instead of decreasing) growth.[12] Empirical support for the fourth channel, that inequality slows the rate of modernization and industrialization, is nonexistent due to the difficulty in testing these sorts of theories.

These mixed results from testing the specific channels of how inequality affects growth are not surprising, given that the sign and strength of the underlying relationship is far from established. As mentioned above, while most of the cross-country growth studies find that the coefficient on inequality is negative and just significant, these estimates are generally not robust to slight changes in model specification or the inclusion of regional dummy variables. Moreover, all of these studies are plagued with serious econometric problems, of which the two most troublesome are measurement error in inequality and omitted variables bias.

Measurement error is always a concern in cross-country studies. Countries have different definitions of key variables and varying degrees of accuracy and completeness in data collection. One of the variables subject to the most substantial measurement error is inequality.[13] Few countries have collected data on income distribution on a regular basis and most of the data which has been compiled is generated from household surveys, so income statistics are unreliable. Coverage is generally unrepresentative, with larger households, urban areas, and certain regions tending to be over-represented. An even greater concern is the lack of consistency in the definition of income or the unit of account. Some surveys are based on pre-tax income, while others are based on post-tax income, consumption, or

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[10] See Perotti (1996) for a detailed analysis of this inequality/fertility/human capital/growth relationship. Also see Perotti (1991) for a test of the link between inequality, education and growth.

[11] See Alesina and Perotti (1993), Keefer and Knack (1995) or Perotti (1996).

[12] See Perotti (1996), Perotti (1994) or Lindert (1996). For example, Perotti (1996) reports that equality does have a significant negative effect on the marginal tax rate in democracies, but that a higher marginal tax rate has a significant positive (instead of negative) effect on growth.

expenditure. Many income-based surveys do not include earnings from interest, dividends, rents, or even self-employment. Moreover, some measures of inequality are based on the individual as the unit of account, some on the household, some on adult equivalents, and others on economically-active individuals. As a whole, while most studies acknowledge that inequality statistics are plagued with measurement error, they also admit that since no good instrument for inequality exists, it is difficult to correct for this problem.

Fields conducted a detailed search for measures of income distribution meeting three basic requirements: the data must be synthetic (drawn from the same or comparable surveys), the sample must be national (not over-representing certain regions), and the statistics must be based on standard definitions and units.[14] Of the 70 measures of income distribution examined and typically used in cross-country growth regressions, only half met these three basic criteria. This suggests that studies examining the effect of inequality on growth may have a high degree of measurement error in their inequality statistics. As is well-documented in the econometric literature, this measurement error can not only increase standard errors and bias estimates toward zero, but if the errors are systematic, can bias estimates in any direction. Moreover, while most studies acknowledge this problem, they also admit that since no good instrument for inequality exists, it is difficult to correct. As a result, authors are forced to utilize the inaccurate data and accept any bias in their estimates.

In the past few years, however, Deininger and Squire have painstakingly compiled a far more consistent and comprehensive data set on inequality.[15] They began by assembling as many income distribution variables as possible. Then they filtered out those observations which satisfied three minimum standards of quality. First, the data must be based on household surveys rather than national account statistics. This avoids the need to specify functional forms determining income distribution or to make assumptions about the pattern of inequality across countries or over time. Second, the population covered must be representative of the entire country. Third, the measure of income (or expenditure) must be comprehensive, including all sources of income or expenditure, such as income from self-employment, non-wage earnings, and non-monetary income.

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[13] For further discussion on problems with measures of income inequality, see Anand and Kanbur (1993a), Deininger and Squire (1996a), Fields (1994), McGranahan (1979), and Park and Van Ginneken (1984).
[14] Fields (1994).
[15] See Deininger and Squire (1996a) for a complete description of this data set.

While these three criteria do not appear extremely stringent, much of the data used in previous studies does not satisfy them. Deininger and Squire began with about 2600 observations, but only 682 met the requirements to be included in their "high quality" data set. A majority of the measurements used in some of the most well-known analyses of the relationship between inequality and growth did not qualify.[16] Moreover, this new data set also has a significantly greater number of observations and covers a much broader range of countries than in any previous data compilation. As a result, Deininger and Squire's new data set can not only minimize measurement error in inequality and any resulting coefficient bias, but also increase the efficiency of estimates.

Perhaps even more important, this larger data set makes it possible to correct for a second major problem with previous work on inequality and growth: omitted variable bias. Omitted variable bias can be a significant problem in any cross-country growth regression. If a variable which helps explain growth and is correlated with any of the regressors is not included, then coefficient estimates and standard errors will be biased. The direction of the bias is determined by the relationship between the omitted variable and the regressors. Many studies measuring the impact of inequality on growth acknowledge that omitted variable bias could be a problem, and in an attempt to test for this bias, add regional dummy variables. These dummy variables are usually significant, suggesting that region-specific factors affecting growth are not captured by the explanatory variables. When included, they generally render the coefficient on inequality insignificant, suggesting that the coefficient on inequality may actually be capturing the effect of these omitted variables on growth, instead of the direct influence of inequality.

One method of avoiding omitted variable bias is to use a panel instead of the standard cross-country data. Panel estimation controls for differences in unobservable country characteristics, thereby removing any bias resulting from the correlation of these characteristics with the explanatory variables. Papers estimating the neoclassical growth model show that using panel estimation to correct for omitted variable bias can significantly change coefficient estimates.[17] Many studies examining the relationship between inequality and growth admit that this sort of adjustment would be useful, but since panel estimation requires observations across time for each country, as well as across countries, the paucity of inequality data available has made meaningful panel estimation impossible. The new data set compiled

[16] For example, in Persson and Tabellini (1991), one of the first studies to find a significant negative effect of inequality on growth, only 18 of the 55 observations utilized qualify. Moreover, when Persson and Tabellini's model is reestimated using only the "high quality" observations in their original data set, the relationship between inequality and growth evaporates. (See Deininger and Squire, 1996a).

[17] See Caselli, Esquivel and Lefort (1996), Islam (1995), or Knight, Loayza, and Villaneuva (1993).

by Deininger and Squire, however, has observations across time for a number of countries, so panel estimation is finally viable.

To summarize, this paper will use a new data set compiled by Deininger and Squire to avoid two critical econometric problems with previous studies analyzing the relationship between inequality and growth: measurement error and omitted variable bias. Before performing this estimation, however, it is necessary to develop the specific model and data set to be utilized.

III. The Model and The Data

This paper will estimate growth as a function of initial inequality, income, male and female human capital, market distortions, and country and period dummy variables--a model similar to that used in most empirical work on inequality and growth. More specifically, I chose this model since it is almost identical to that used by Perotti in his definitive study finding a negative effect of inequality on growth. The only change from Perotti's model is the addition of the dummy variables. The country dummies are included to control for omitted variable bias, as described above, and the period dummies are included to control for any global shocks, which might affect aggregate growth in any period but are not otherwise captured by the explanatory variables.[18]

In addition to inequality, income, human capital, market distortions, and the dummy variables, I could also include a number of other independent variables. This paper, however, will focus on this simplified specification for three reasons (reasons similar to why it was originally chosen by Perotti.) First, this model is typical of that used to estimate the effect of inequality on growth, so any discrepancy between this paper and previous work can not be explained by model specification. Second, since the sample size is already limited by the availability of inequality statistics, and especially since panel estimation requires a large number of observations, this relatively simple specification helps maximize the degrees of freedom available. Third and finally, by focusing on stock variables measured at the beginning of the periods, rather than flow variables measured throughout the periods, endogeneity is less of a concern.

[18] Also note that excluding the period dummies has little effect on the results.

To summarize, the growth model central to this paper is:

$$GR_{it} = \beta_1 INEQ_{i,t-1} + \beta_2 INC_{i,t-1} + \beta_3 MALED_{i,t-1} + \beta_4 FEMED_{i,t-1} + \beta_5 PPPI_{i,t-1} + \alpha_i + \eta_t + u_{it} \quad (i)$$

where i represents each country and t represents each time period; GR_{it} is average annual growth for country i over time period t; $INEQ_{i,t-1}$, $INC_{i,t-1}$, $MALED_{i,t-1}$, $FEMED_{i,t-1}$, and $PPPI_{i,t-1}$ are respectively inequality, income, male education, female education and market distortions for country i during period $t-1$; α_i are the country dummies; η_t are the period dummies; and u_{it} is the error term for country i during period t.

The data used to estimate this model comes from four sources. Inequality is drawn from Deininger and Squire (1996a) and *INEQ* is measured by the gini coefficient. Income and the resultant growth rates are taken from the World Bank STARS data set, with income measured by the log of real GNP per capita. Human capital statistics come from Barro and Lee (1996) and are represented by average years of secondary schooling for the relevant sex aged over 25. Market distortions are drawn from the Penn World Tables (version 5.6) compiled by Summers and Heston, and are proxied by the price level of investment. Detailed definitions for each of these variables are listed in Table I.

Due to data availability, this paper will focus on growth from 1965-1995. Moreover, since yearly growth rates incorporate short-run disturbances, growth is averaged over five-year periods. This reduces yearly serial correlation from business cycles. It is therefore possible to estimate six periods of growth for each country and I only include countries which have observations across at least two consecutive time periods.[19] Applying these criteria to the above data sets generates a sample of 45 countries and 180 observations. This final data set, with means, standard deviations, and ranges for each of the variables is reported in Table I. Table II lists countries and their corresponding gini coefficients and Table III estimates the correlation matrix.

This final data set, while clearly a vast improvement over that used in past studies of the effect of inequality on growth, still has several problems. First, Table II shows the limited number of observations available for many countries and for earlier time periods. Second, Table IV shows that regional coverage is far from representative; there are no countries from Sub-Saharan Africa, while nearly one-half of the observations are from the OECD/High Income nations. Third, all of the gini coefficients are not based on identical units of account. For example, some are based on the household, while others are based on the individual.[20] All of these shortcomings, however, will be addressed in the sensitivity analysis.

IV. Estimation

There are a variety of different techniques which can be used to estimate equation *(i)*. In order to evaluate which technique is optimal, it is necessary to consider three factors: the relationship between the country-specific effect and the regressors, the presence of a lagged endogenous variable (income) and the potential endogeneity of the other regressors (such as inequality).

The standard methods of panel estimation are fixed effects or random effects. For the purpose of estimating equation *(i)*, the major difference between these two techniques is the information utilized to calculate coefficients. Fixed effects is calculated from differences within each country across time and ignores the differences between countries at any given time. Random effects, on the other hand, is more efficient since it incorporates information across individual countries as well as between periods. The major drawback with random effects is that it is only consistent if the country-specific effects are random and therefore uncorrelated with the other explanatory variables. Moreover, in equation *(i)* it is unlikely that this assumption is satisfied, since a country's individual effect is probably related to its level of inequality, human capital or market distortions. A Hausman specification test allows us to evaluate if this independence assumption is satisfied.

A problem with both fixed effects and random effects, however, is that equation *(i)* contains a lagged endogenous variable (the income term). This is immediately apparent when the equation is rewritten with growth expressed as the difference in income levels:

$$\begin{aligned} GR_{it} &= LINC_{it} - LINC_{i,t-1} \\ &= \beta_1 INEQ_{i,t-1} + \beta_2 LINC_{i,t-1} + \beta_3 MALED_{i,t-1} + \beta_4 FEMED_{i,t-1} + \beta_5 PPPI_{i,t-1} + \alpha_i + \eta_t + u_{it} \end{aligned}$$

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[19] The reason for this selection criteria will be apparent in the discussion on estimation in Section IV.

[20] To minimize the inconsistency due to the fact that some gini coefficients are based on income while others are based on expenditure, I have followed Deininger and Squire's suggestion and added 6.6 to gini coefficients based on expenditure. See Deininger and Squire (1996a) for further discussion of this adjustment and other problems in this data set.

Adding $LINC_{i,t-1}$ to both equations yields:

$$LINC_{it} = \beta_1 INEQ_{i,t-1} + \gamma_2 LINC_{i,t-1} + \beta_3 MALED_{i,t-1} + \beta_4 FEMED_{i,t-1} + \beta_5 PPPI_{i,t-1} + \alpha_i + \eta_t + u_{it}$$

with $\gamma_2 = \beta_2 + 1$.

To simplify the following discussion, this can also be written:

$$y_{it} = \gamma y_{i,t-1} + \mathbf{X}_{i,t-1}{}' \mathbf{B} + \alpha_i + u_{it} \quad (ii).$$

Part I of the appendix shows that even if $y_{i,t-1}$ and u_{it} are not correlated, if t does not approach infinity (which it clearly does not in this model where t=6), then estimation by fixed effects or random effects is not consistent (even as n goes to infinity). Monte Carlo simulation shows that for panels with a comparable time dimension, the bias of the coefficient on the lagged dependent variable can be significant, although the bias of the coefficients on the other right-hand side variables tends to be minor.[21]

One popular technique of correcting for this inconsistency is Chamberlain's π-matrix approach.[22] The fundamental identifying condition for this method of moments estimator is the exogeneity of the explanatory variables (i.e. that $E(x_{i,t}\, u_{i,t})=0$ for all t and x_i, or at least for a large enough subset of the explanatory variables). In the model of equation *(i)*, however, it is unlikely that this condition is satisfied. A whole branch of economics has investigated the Kuznets' relationship of how growth and income might affect inequality, and recent work suggests that growth may free resources for investment in human capital and therefore raise levels of education. This would leave only one variable ($PPPI_{it}$) for identification, which is clearly not sufficient.

Arellano and Bond, however, suggest an alternative estimation technique which corrects not only for the inconsistency introduced by the lagged endogenous variable, but also permits a certain degree of endogeneity in the other regressors.[23] This generalized method of moments estimator differences each variable so as to eliminate the country-specific effect and then uses all possible lagged values of each of the variables as instruments. More specifically, Arellano and Bond rewrite equation *(ii)* as:

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[21] For example, Judson and Owen estimate that when t=5, the bias in their lagged dependent variable from using fixed effects is over 50%, while the bias in their other coefficients is only about 3%. Judson and Owen (1996).

[22] For details on this approach, see Chamberlain (1984) or Crépon and Mairesse (1996).

$$y_{it} - y_{i,t-1} = \gamma(y_{i,t-1} - y_{i,t-2}) + (\mathbf{X}'_{i,t-1} - \mathbf{X}'_{i,t-2})\mathbf{B} + (u_{it} - u_{i,t-1})$$

and for period 3, use $y_{i,1}$ as an instrument for $(y_{i,2} - y_{i,1})$, for period 4 use $y_{i,1}$ and $y_{i,2}$ as instruments for $(y_{i,3} - y_{i,2})$, etc., and follow the same procedure to create instruments for each of the other right-hand side variables. This procedure is described in more detail in part II of the appendix.

Two critical assumptions must be satisfied for this estimator to be consistent and efficient. First, the $\mathbf{X}_{i,t-s}$'s must be predetermined by at least one period: $E(\mathbf{X}'_{it}u_{is}) = 0$ for all $s > t$. Second, the error terms can not serially correlated: $E(u_{i,t}u_{i,t-s}) = 0$ for all $s \geq 1$.[24] A test for serial correlation is described in part III of the appendix and will be performed below.

Table V reports estimates of equation *(i)* using fixed effects, random effects and Arellano and Bond's method of moments technique. Estimates vary significantly based on which technique is utilized, and therefore it is necessary to test the validity of the assumptions underlying each method. First, a Hausman specification test comparing the fixed effects estimates of column 1 with the random effects estimates of column 2 rejects the null hypothesis of no correlation between the regressors and the country-specific effects.[25] As a result, random effects is not consistent and fixed effects should be used instead of random effects. As mentioned above, however, both of these may be inconsistent due to the presence of the lagged income term. Column 3 uses Arellano and Bond's estimation technique to adjust for this inconsistency. Their test for serial correlation is unable to reject the null hypothesis of no second-order serial correlation in the differenced equation, supporting the moment conditions required for the consistency of this estimation technique.[26] Therefore, in the discussion which follows, I focus on the estimates reported in column 3.

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[23] Arellano and Bond (1991). Caselli, Esquivel and Lefort (1996) also utilize this technique in the estimation of a growth model so as to avoid the above problem with Chamberlain's approach.

[24] Note that this is equivalent to the requirement that there is no second-order (or higher) serial correlation in the differenced equation: $E(\Delta u_{i,t}\Delta u_{i,t-s}) = 0$ for all $s \geq 2$

[25] The test statistic is $\chi^2(10)=67.6$. This rejects the null hypothesis at any standard level of significance.

[26] The test statistic is $N(0,1)=0.44$ which is unable to reject the null at any standard level of significance.

Not only do most of the coefficient estimates in Column 3 agree with those traditionally reported in this literature, but most are highly significant.[27] As predicted by models implying conditional convergence, the coefficient on initial income is negative and significant. The coefficient on male education is negative (although not significant), and that on female education is positive and significant. Although this pattern of signs may not support traditional human capital theory, these coefficients (including that on initial income) are similar to those found in other growth models estimated using the same technique.[28] The coefficient on market distortions is negative and highly significant. The one unexpected result, however, is the coefficient on inequality. No matter which estimation technique is utilized, this coefficient is never negative, as is assumed in recent work examining the relationship between inequality and growth. Instead, the coefficient on inequality is positive and significant at the 5% level under each estimation technique. Not only is this sign surprising, but also the magnitude of the coefficient. A ten point increase in the gini coefficient within a given country (which is the difference in inequality in 1985 between the United States and the United Kingdom) is correlated with an increase of 1.3% in average annual growth over the next five years.[29]

It is important to note that the interpretation of this positive coefficient on inequality is different than that in previous work examining the relationship between inequality and growth. As mentioned above, earlier work utilized OLS or IV to estimate some variant of the standard Barro cross-country growth regression. Using these techniques, the resulting estimates of a negative coefficient on inequality suggested that countries with lower levels of inequality tend to have higher rates of economic growth. These estimates do not directly assess a potentially more relevant question; how are changes in a country's level of inequality related to changes in that country's growth performance? The Arellano and Bond estimator, however, specifically addresses this question. It controls for a country's unobservable characteristics or "fixed effect", and instead of analyzing differences in inequality and growth across countries, focuses on changes in these variables within each country across time. The resulting coefficient on inequality can therefore be interpreted as measuring the highly relevant relationship of how changes in inequality are related to changes in growth *within* a given country.

[27] For example, a test that all of the explanatory variables (excluding the country and period dummies) are zero yields the statistic: $F(5,125)=16.8$; a test of the null that all country effects are equal yields the statistic: $F(43,125)=4.6$; a test of the null that all period dummies are zero yields the statistic: $F(5, 125)=16.8$. In each case, the null is rejected at any standard level of significance.

[28] For example, see Caselli, Esquivel and Lefort (1996).

[29] This ten points is also close to one standard deviation in this paper's sample. Note, however, that it is unlikely that one country's gini coefficient could increase by this magnitude in a short period of time.

A second major difference in the interpretation of this paper's results and those reported in earlier work is the time period under consideration. The standard cross-country growth regression estimates how initial inequality is related to growth over the next twenty-five or thirty years, thereby assessing a long-run relationship. Since this paper utilizes five-year panels, however, the coefficients in columns 1-3 reflect a short- or medium-run relationship. As an informal test if this shorter-term, positive relationship between inequality and growth diminishes over time, column 4 estimates equation *(i)* based on ten-year panels.[30] The coefficient on inequality remains positive, although it decreases substantially and becomes insignificant. These results must be interpreted cautiously due to the limited degrees of freedom available, and therefore, until inequality data is available for a greater number of years, it is difficult to draw any conclusions about the long-term relationship between inequality and growth within a given country.

V. What Affects the Coefficient on Inequality?

Why is the coefficient on inequality in Table V consistently positive, while most work in this field finds that it is negative? Column 1 in Table VI reports Perotti's estimates, which are typical in this literature and could differ from those in Table V for five reasons. First, Perotti defines two variables differently. Second, Perotti's sample is larger then mine and there could be a structural difference in the relationship between inequality and growth in the two samples. Third, Perotti's data on inequality is "low quality" and not subject to the stringent consistency requirements of the Deininger and Squire data set. Fourth, as discussed above, this paper focuses on the relationship between inequality and growth over shorter periods of time. Fifth and finally, Perotti focuses on differences across countries (instead of within countries across time) and does not correct for omitted variable bias by estimating the country-specific effects. Therefore, modifying one or more of these five factors should explain why this paper finds the opposite effect of inequality on growth than previously reported.

To test which of these modifications alters the sign of the coefficient on inequality, I make each change independently. First I examine the impact of different variable definitions. Instead of using the

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[30] I only report fixed effects estimates since Arellano and Bond's technique requires observations across an additional period, so that only two ten-year periods are available for estimation. As a result, a number of countries must be excluded from the sample and meaningful estimation is impossible. I focus on fixed effects since it also incorporates only within-country differences, and as explained above, random effects is rejected in favor of fixed effects.

gini coefficient as a measure of inequality, Perotti uses the income share held by the middle class as a measure of equality (and I add a negative sign to his coefficient so as to facilitate comparison with the other columns.) The other variable defined differently is INC. This paper and virtually all other work on growth utilize the logarithm of initial income, while Perotti simply uses initial income. To isolate the effect of these different definitions, I use Perotti's sample (as close as possible using my data sources), low quality measures of inequality, and cross-country estimation. The low quality data is the unabridged data collected by Deininger and Squire, which includes not only the "consistent" measures of inequality used throughout this paper, but also all of the "inconsistent" measures used in past work.[31] Also, in order to use cross-country estimation, equation *(i)* is rewritten:

$$GR_i = \alpha_0 + \beta_1 INEQ_i + \beta_2 INC_i + \beta_3 MALED_i + \beta_4 FEMED_i + \beta_5 PPPI_i + u_i \qquad (iii)$$

where GR_i is average annual growth from 1970-1995 for country i; α_0 is a constant term which does not vary across countries; and $INEQ_i$, INC_i, $MALED_i$, $FEMED_i$, and $PPPI_i$ are measured in 1970 (or the closest year available) and defined as above.[32]

Estimates of equation *(iii)* obtained utilizing this paper's definitions, Perotti's sample and the low quality data set are reported in column 2 of Table VI. A comparison with column 1 shows that while the coefficients on the variables defined differently do change (as would be expected), altering variable definitions does not change the key result found in Perotti's study: that inequality has a significant negative relationship with growth.

Second, in order to test if sample selection affects the results, column 3 uses the same definitions, low quality data, and cross-country framework as in column 2 , but for the same set of countries as in Column 7 (which are the central results reported in the last section). The coefficient on inequality barely changes (falling from -0.00050 to -0.00047), and although its standard error increases

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[31] When more than one observation on inequality is available per country in a given year, I average all available observations. The resulting "low quality" data contains all but 4 countries in Perotti's sample. I do not use Perotti's low quality measures of inequality since his data set does not contain observations across time, which are necessary for the following comparisons.

[32] I estimate growth from 1970-1995 (with explanatory variables from 1970) so that these estimates are directly comparable with the central results in column 7. Estimates of growth from 1965-1995 (using explanatory variables from 1965) are virtually identical.

slightly (from 0.00022 to 0.000027), a Chow test strongly rejects any structural difference between the countries included in Perotti's sample and those excluded from my sample.[33]

Third, in order to test for the impact of minimizing measurement error, I utilize the same variable definitions, sample, and cross-country framework as in Column 3, but replace the "low quality" inequality statistics with the more consistent measures from the "high quality" data set. The results are reported in Column 4 and show that reducing measurement error slightly strengthens the negative effect of inequality on growth (from -0.00047 to -0.00049), which is expected since measurement error generally biases coefficient estimates toward zero. The standard error changes even less, suggesting that either measurement error is not a significant problem in columns 1-3, or the Deininger and Squire criteria do not significantly minimize any error.

Fourth, to see if changing the period length affects the relationship between inequality and growth, I utilize the same variable definitions and sample as in Columns 4 and 7, but use the panel data set which includes statistics across five-year periods. Then I use OLS to estimate the same cross-country growth model of equation *(iii)*. I do not first-difference or include country dummy variables, so I do not control for any omitted variable bias. Results using the high quality measures of inequality are reported in column 5 (and are virtually identical to those based on the low quality data.) The coefficient on inequality is now positive (although insignificant), suggesting that the length of the period under consideration does have some effect on the relationship between inequality and growth.

Finally, in order to test for the effect of correcting for omitted variable bias, I utilize the same variable definitions, sample and "low quality" data as in column 3, and the shorter periods of column 5, but estimate the panel model of equation *(i)* rather than the cross-country model of equation *(iii)*. The results based on Arellano and Bond's GMM estimator are reported in Column 6. The coefficient on inequality is insignificant and close to zero.

This set of comparisons reported in Table VI has several strong implications. Column 2 shows that the positive effect of inequality on growth found in column 7 is not an artifact of variable definition or model specification. Column 3 shows that sample selection has little influence (at least in a comparison with earlier work), and Column 4 shows that minimizing measurement error has little impact in the cross-country framework. Column 5 shows that in the five-year periods, when I do not control for

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[33] The impact of sample selection will be further investigated in the sensitivity analysis.

the country-specific effects, there is virtually no relationship between inequality and growth. Correcting for omitted variable bias in Column 6 and focusing on within-country changes, but using the low quality measures of inequality, also yields no significant relationship between inequality and growth. When this panel estimation technique is utilized with the more consistent measures of inequality in Column 7, however, the relationship between inequality and growth is positive and significant. It is not surprising that adjusting for measurement error is more important in panel than cross-country estimation; the correlation between the random term in initial inequality and the disturbance from the growth regression would be larger in the five-year periods than in the thirty-year period.[34]

VI. Sensitivity Analysis

Since this positive relationship between inequality and growth challenges previous econometric work, and also since sample selection may influence the coefficient estimates, this section will thoroughly test the robustness of the results. It will estimate a number of variations of the model estimated in Table V, testing if the positive relationship between inequality and growth persists across different samples, variable definitions and model specifications. This section will use Arellano and Bond's methodology whenever possible, but in several cases when the variation being tested limits the sample or periods available, will utilize the computationally less-stringent fixed effects.

One potential problem with the results reported above is sample selection. Since only 45 countries are included, a group of outliers could have a large effect. Even more important, as discussed in Section III and shown in Tables II and IV, period, regional, and country coverage is highly unrepresentative. If the selection mechanism is non-ignorable (i.e. if there is some relationship between the independent variables and the countries and/or periods which are included) then coefficient estimates may be inconsistent and inefficient.[35] Utilizing a fixed effects estimator instead of random effects should minimize this problem, but it is still necessary to test for the influence of sample selection.

First, I test for the affect of removing outliers. I estimate the basic model removing one country at a time, then removing the five observations farthest above and below the country mean for each variable, and next removing the five countries with the lowest or highest average inequality, income or

[34] Also note that measurement error has the predicted effect in the panel framework; it biases the coefficient on inequality toward zero.

growth.[36] Table VII reports the last (and most extreme) of these tests. In each case, although the actual value of the coefficient on inequality does fluctuate, the coefficient always remains positive and significant. A related concern is that different countries are included in different periods. To control for this effect, I reestimate the basic model for a variety of different periods but only include countries which have observations for all of the periods. For example, I estimate growth from 1975-95 for the 24 countries with observations across all 4 periods, or growth from 1970-90 for the 17 countries with data for each of these years. Once again, the coefficient on inequality is always positive and significant at the 5% level.

A similar concern is that if the model's coefficients change over time, then the pooling required to estimate fixed effects would not be permissible and parameter estimates would be biased and inconsistent. Removing any single period from the model or estimating the model for any subset of periods, however, does not significantly change the coefficient on inequality. A test of structural change between 1965-1980 and 1980-1995 further supports this finding: I am unable to reject the null of the equality of coefficients (including country dummies) across the two periods.[37]

Next, I test how the sample's unbalanced regional coverage affects results. I reestimate equation *(i)*, excluding countries from East Asia, Latin America, and the OECD's. This comparison is reported in Table VIII. No matter which of these regions is excluded from the sample, the relationship between inequality and growth remains positive and significant.

Related to this unbalanced regional coverage is another potential problem with the sample: the representation of very poor countries is extremely limited. This is not surprising; wealthier countries tend to keep more accurate statistics and are therefore more likely to have enough consistent measures of inequality to be included in the sample. The relationship between inequality and growth, however, could depend on a country's stage of development. I test for this by experimenting with different functional forms, such as including a squared and/or cubed term for inequality. Results suggest that the relationship between inequality and growth is in fact the linear model specified in equation *(i)*. As an alternate test, I divide the sample into wealthy and poor countries based on initial income and then reestimate equation

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[35] See Verbeek and Nijman (1996) for a discussion of selection bias and its resultant problems.

[36] In order to conserve space, these results and several others referred to in the remainder of this section are not included in the tables. They are available from the author on request.

[37] The F-statistic is $F(54, 72)=0.94$. The critical value at the 5% level is 1.5.

(i) for each group.[38] Table IX shows that no matter which division is utilized, the relationship between inequality and growth remains positive in each group. This relationship may be greater in poor countries, but due to the limited degrees of freedom in each sub-sample and the limited number of very poor countries available, it is difficult to draw any strong conclusions.

In addition to unbalanced sample composition, another concern with this paper is that variable definitions or model specification could drive the results. I reestimate the model for different definitions of education, income, market distortions, and/or inequality. For example, as an alternate measure of education, I use enrollment rates or total years of schooling in primary or secondary education. As alternate measures of income and market distortions, I use GDP per capita and the log of the black market premium, respectively. Finally, as alternate measures of inequality, I utilize the ratio of the income share of the richest 20% of the population to the poorest 40%, the ratio of the income share of the richest 20% of the population to the poorest 20%, or the negative of the income share held by the middle class. Table X reports estimates for these alternate measures of inequality in columns 2-4 and shows that using these different definitions does not affect the main results. Another concern with each of these measures of inequality, including the gini coefficient, is that different sources have been utilized, even for the same country. Column 6 therefore re-estimates the basic model, using only measures of inequality from the same source for each country. Once again, the coefficient on inequality remains positive and significant.[39]

Finally, I estimate a variety of different specifications. Table XI lists additional variable definitions and Table XII reports the estimates.[40] Column 1 replicates this paper's central results for the

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[38] Results are identical if the sample is divided into wealthy and poor countries based on final per capita income or average per capita income.

[39] Note that the change in the magnitude and standard error of the coefficient is what would be expected from random measurement error introduced by using different sources.

[40] Note that due to the large amount of data required to replicate each of these studies, all variable sources and definitions are not identical to those utilized in the original papers. Instead, all variables for this comparison are drawn from Barro and Lee (1997), and in the few cases where the same variable is not available, the closest possible alternative is utilized. Most variables are only available through 1985, so the dependent variable in these regressions is growth from 1965-1990. In order to maintain as much comparability with the previous sample as possible, I utilize fixed effects. Several specifications are also not identical to those originally utilized. For example, Alesina and Perotti use a dummy variable for democracy, but since this dummy variable is constant for most countries, I replace it with political instability. Also, Alesina and Perotti use inequality in land distribution for the interactive term, but this is not available across periods, so I replace it with income inequality. Finally, Caselli et al.'s model reported in columns 15 and 16 includes additional terms such as technological progress and depreciation (which are not available in the Barro and Lee data set).

truncated sample utilized for these regressions; columns 2-10 use models from four well-known papers which estimate the effect of inequality on growth; columns 11-20 add inequality to several models frequently cited in the more general growth literature. These results show that the positive relationship between inequality and growth is not an artifact of model specification. No matter which variables are added or removed, the coefficient on inequality always remains positive. Moreover, the models in columns 2-10 were previously used to show that inequality has a negative effect on growth, but now the relationship is not only positive, but always significant at the 5% level. Equally notable is the stability of the inequality coefficient, which varies far less than most of the other coefficients. As a whole, these comparisons suggest that the positive relationship between inequality and growth is not driven by model specification.

VII. Conclusion

The results reported in this paper clearly challenge the current belief that inequality has a negative effect on growth. Previous work on this subject is plagued with econometric problems and has been widely misinterpreted to imply that countries which reduce inequality could improve growth performance. This paper reassesses this work and addresses these problems. It uses more consistent measures of inequality to reduce measurement error and utilizes panel instead of cross-country estimation to correct for omitted variable bias. By focusing on a generalized method of moments technique developed by Arellano and Bond, it directly estimates how changes in inequality are correlated with changes in growth within a given country. Results suggest that in the short and medium term, an increase in a country's level of income inequality has a significant positive relationship with subsequent economic growth. This relationship is highly robust across samples, variable definitions and model specifications, with the one caveat that it may not apply to very poor countries.

The implications of these results are clearly disappointing; countries may face a tradeoff between reducing inequality and improving growth performance. It is too soon, however, to draw any definitive policy conclusions. Sample selection, endogeneity, serial correlation, and any remaining measurement error could still influence estimates. Not enough data is available to accurately measure this relationship over periods longer than five years or for very poor countries. Moreover, these estimates of a short-run positive relationship between inequality and growth within a given country do not directly contradict the previously reported long-run negative relationship across countries. Instead, they do suggest that the relationship between inequality and growth is far from resolved, and that a careful reassessment of the sign, direction, and strength of the linkages between these two variables and their underlying determinants is necessary.

Appendix

I. The Inconsistency of Fixed Effects with a Lagged Endogenous Variable[41]

The model to be estimated is:

$$y_{it} = \gamma y_{i,t-1} + \mathbf{x}'_{i,t-1}\mathbf{B} + \alpha_i + u_{it} \qquad \text{with } i = 1,\ldots N;\ t = 1,\ldots T$$

Assume that the disturbances satisfy:

$$E(u_{it} | y_{i,t-1}, \mathbf{x}_{i,t-1}) = 0$$

$$V(u_{it} | y_{i,t-1}, \mathbf{x}_{i,t-1}) = \sigma_\varepsilon^2 \quad \text{for all } i \text{ and } t$$

$$Cov(u_{it}, u_{i't'} | y_{i,t-1}, \mathbf{x}_{i,t-1}) = 0 \quad \text{for all } i \neq i' \text{ and } t \neq t'$$

Then write the model in matrix form by stacking all the observations over individuals and time periods:

$$\mathbf{y} = \gamma \mathbf{y}_{-1} + \mathbf{X}_{-1}\mathbf{B} + \mathbf{D}\mathbf{A} + \mathbf{u}$$

where:

$$\mathbf{y} = \begin{pmatrix} y_{11} \\ . \\ . \\ y_{1T} \\ . \\ . \\ y_{NT} \end{pmatrix}, \quad \mathbf{y}_{-1} = \begin{pmatrix} y_{10} \\ . \\ . \\ .y_{1T-1} \\ . \\ . \\ y_{N,T-1} \end{pmatrix}, \quad \mathbf{X} = \begin{pmatrix} \mathbf{X}^1_{10} & . \ . \ . & \mathbf{X}^K_{10} \\ . & . \ . \ . & . \\ . & . \ . \ . & . \\ . & . \ . & . \\ \mathbf{X}^1_{1,T-1} & . \ . \ . & \mathbf{X}^K_{1,T-1} \\ . & . \ . \ . \ . & . \\ . & . \ . \ . \ . & . \\ \mathbf{X}^1_{N,T-1} & . \ . \ . & \mathbf{X}^K_{N,T-1} \end{pmatrix},$$

$$\mathbf{D} = \mathbf{I}_N \otimes \mathbf{1}_T, \quad \mathbf{u} = \begin{pmatrix} u_{11} \\ . \\ . \\ . \\ u_{NT} \end{pmatrix}, \quad \mathbf{B} = \begin{pmatrix} \beta_1 \\ . \\ . \\ . \\ \beta_k \end{pmatrix}, \quad \mathbf{A} = \begin{pmatrix} \alpha_1 \\ . \\ . \\ . \\ \alpha_{Nk} \end{pmatrix},$$

where $\mathbf{1}_T$ is a (Tx1) unit vector.

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[41] This discussion is based on Sevestre and Trognon (1996).

Appendix (cont)

To use fixed effects (also known as Least Squares with Dummy Variables or LSDV), transform this model:

$$\mathbf{W}_N\mathbf{y} = \mathbf{W}_N\mathbf{y}_{-1}\gamma + \mathbf{W}_N\mathbf{X}_{-1}\mathbf{B} + \mathbf{W}_N\mathbf{u} \quad \text{where } \mathbf{W}_N = \mathbf{I}_N \otimes \left(\mathbf{I}_T - \frac{\mathbf{J}_T}{T}\right)$$

(remembering $\mathbf{W}_N\mathbf{D} = 0$)

Then the fixed effects estimator is:

$$\begin{pmatrix}\hat{\gamma}\\ \hat{\mathbf{B}}\end{pmatrix} = \begin{pmatrix}\mathbf{y}'_{-1}\mathbf{W}_N\mathbf{y}_{-1} & \mathbf{y}'_{-1}\mathbf{W}_N\mathbf{X}_{-1}\\ \mathbf{X}'_{-1}\mathbf{W}_N\mathbf{y}_{-1} & \mathbf{X}'_{-1}\mathbf{W}_N\mathbf{X}_{-1}\end{pmatrix}^{-1}\begin{pmatrix}\mathbf{y}'_{-1}\mathbf{W}_N\mathbf{y}\\ \mathbf{X}'_{-1}\mathbf{W}_N\mathbf{y}\end{pmatrix}$$

And as $N \rightarrow \infty$, this can be written:

$$\operatorname*{plim}_{N\to\infty}\begin{pmatrix}\hat{\gamma}-\gamma\\ \hat{\mathbf{B}}-\mathbf{B}\end{pmatrix} = \begin{pmatrix}\operatorname*{plim}_{N\to\infty}\frac{1}{NT}\mathbf{y}'_{-1}\mathbf{W}_N\mathbf{y}_{-1} & \operatorname*{plim}_{N\to\infty}\frac{1}{NT}\mathbf{y}'_{-1}\mathbf{W}_N\mathbf{X}_{-1}\\ \operatorname*{plim}_{N\to\infty}\frac{1}{NT}\mathbf{X}'_{-1}\mathbf{W}_N\mathbf{y}_{-1} & \operatorname*{plim}_{N\to\infty}\frac{1}{NT}\mathbf{X}'_{-1}\mathbf{W}_N\mathbf{X}_{-1}\end{pmatrix}^{-1}\begin{pmatrix}\operatorname*{plim}_{N\to\infty}\frac{1}{NT}\mathbf{y}'_{-1}\mathbf{W}_N\mathbf{u}\\ \operatorname*{plim}_{N\to\infty}\frac{1}{NT}\mathbf{X}'_{-1}\mathbf{W}_N\mathbf{u}\end{pmatrix}$$

and focusing on the top term in the matrix on the right:

$$\begin{aligned}\operatorname*{plim}_{N\to\infty}\frac{1}{NT}\mathbf{y}'_{-1}\mathbf{W}_N\mathbf{u} &= \operatorname*{plim}_{N\to\infty}\frac{1}{NT}\sum_i\sum_t (y_{i,t-1}-y_{i.})(u_{i,t}-u_{i.})\\ &= E\left(\frac{1}{T}\sum_t (y_{i,t-1}-y_{i.})(u_{i,t}-u_{i.})\right)\\ &= -\frac{1}{T^2}\frac{T-1-T\gamma+\gamma^T}{(1-\gamma)^2}\sigma_u^2 \neq 0\end{aligned}$$

Therefore, even if $\operatorname*{plim}_{N\to\infty}\frac{1}{NT}\mathbf{X}'_{-1}\mathbf{W}_N\mathbf{u} = 0$, as long as *T* does not approach infinity (which it clearly does not in our case since *T=6*), the fixed effects estimator with a lagged endogenous variable is not consistent. This is due to the asymptotic correlation between $(y_{i,t-1} - y_{i.})$ and $(u_{i,t} - u_{i.})$.

Appendix (cont)

II. A Generalized Method of Moments Estimator based on Arellano and Bond (1991)[42]

The model to be estimated is:

$$y_{it} = \gamma y_{i,t-1} + \mathbf{x}'_{i,t-1}\mathbf{B} + \alpha_i + u_{it} \qquad \textit{with } i = 1,\dots N;\ t = 1,\dots T$$

where $\mathbf{x}_{i,t-1}$ is a 1x4 vector of the explanatory variables other than income, *N=45* and *T=6.*

This can be written in first differences:

$$y_{it} - y_{i,t-1} = \gamma(y_{i,t-1} - y_{i,t-2}) + (\mathbf{x}'_{i,t-1} - \mathbf{x}'_{i,t-2})\mathbf{B} + (u_{it} - u_{i,t-1})$$

To form a system of *t-2* (i.e. 4) equations:

$$\begin{bmatrix}\Delta y_{i3}\\ \Delta y_{i4}\\ \Delta y_{i5}\\ \Delta y_{i6}\end{bmatrix} = \gamma\begin{bmatrix}\Delta y_{i2}\\ \Delta y_{i3}\\ \Delta y_{i4}\\ \Delta y_{i5}\end{bmatrix} + \begin{bmatrix}\Delta \mathbf{x}_{i2}\\ \Delta \mathbf{x}_{i3}\\ \Delta \mathbf{x}_{i4}\\ \Delta \mathbf{x}_{i5}\end{bmatrix}'\mathbf{B} + \begin{bmatrix}\Delta u_{i3}\\ \Delta u_{i4}\\ \Delta u_{i5}\\ \Delta u_{i6}\end{bmatrix} \qquad (1)$$

As shown above, fixed effects can not consistently estimate this model it *t* does not approach infinity. Therefore, Arellano and Bond propose instrumenting with all possible lagged values of each of the regressors. The instrument matrix $\mathbf{Z}_i$ is therefore the *(t-2) x Q* matrix:

$$\mathbf{Z}_i = \begin{bmatrix} y_{i1} & \mathbf{x}_{i1} & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & y_{i1} & y_{i2} & \mathbf{x}_{i1} & \mathbf{x}_{i2} & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 & y_{i1} & y_{i2} & y_{i3} & \mathbf{x}_{i1} & \mathbf{x}_{i2} & \mathbf{x}_{i3} & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & y_{i1} & y_{i2} & y_{i3} & y_{i4} & \mathbf{x}_{i1} & \mathbf{x}_{i2} & \mathbf{x}_{i3} & \mathbf{x}_{i4} \end{bmatrix}$$

And since the panel used in this paper is unbalanced (i.e., the number of periods available varies across observations), the number of non-zero columns may vary for different $\mathbf{Z}_i$'s.

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[42] This discussion is based on the appendix in Caselli, Esquivel and Lefort (1996). It also draws from Arellano and Bond (1991).

Next, we must assume that the disturbances of the differenced equation are not second-order serial correlated (or higher), and that the $\mathbf{x}_{i,t-s}$'s are predetermined by at least one period:

$$\begin{aligned} &E(\Delta u_{i,t}\Delta u_{i,t-s}) = 0 \quad \text{for all } s \geq 2 \\ &E(\mathbf{x}_{i,t}u_{i,s}) = 0 \quad \text{for all s} > \text{t} \end{aligned} \qquad (2)$$

Note that this does not require that the $\mathbf{x}_{it}$'s are strictly exogenous or even predetermined for a given period. Therefore, it is perfectly acceptable if either:

$$\begin{aligned} &E(\mathbf{x}_{it}\alpha_i) \neq 0 \quad \text{and / or} \\ &E(\mathbf{x}_{i,t}u_{i,s}) \neq 0 \quad \text{for all s} \leq \text{t} \end{aligned}$$

To simplify the following discussion, equation (1) can be expressed:

$$\Delta\mathbf{y}_i = \Delta\mathbf{W}_i'\widetilde{\mathbf{B}} + \mathbf{E}_i$$

$$\text{where } \Delta\mathbf{y}_i = \begin{bmatrix} \Delta y_{i3} \\ \Delta y_{i4} \\ \Delta y_{i5} \\ \Delta y_{i6} \end{bmatrix}; \quad \Delta\mathbf{y}_{i,-1} = \begin{bmatrix} \Delta y_{i2} \\ \Delta y_{i3} \\ \Delta y_{i4} \\ \Delta y_{i5} \end{bmatrix}; \quad \Delta\mathbf{x}_{i,-1} = \begin{bmatrix} \Delta \mathbf{x}_{i2} \\ \Delta \mathbf{x}_{i3} \\ \Delta \mathbf{x}_{i4} \\ \Delta \mathbf{x}_{i5} \end{bmatrix}'; \quad \mathbf{E}_i = \begin{bmatrix} \Delta u_{i3} \\ \Delta u_{i4} \\ \Delta u_{i5} \\ \Delta u_{i6} \end{bmatrix}$$

$$\Delta\mathbf{W}_i = [\Delta\mathbf{y}_{i,-1} \;\; \Delta\mathbf{x}_{i,-1}] \text{ and } \widetilde{\mathbf{B}} = [\gamma \;\; \mathbf{B}]$$

And simplifying one step further:

$$\mathbf{Y} = \mathbf{W}'\widetilde{\mathbf{B}} + \mathbf{E} \text{ where } \mathbf{Y} = \begin{bmatrix} \Delta\mathbf{y}_1 \\ . \\ \Delta\mathbf{y}_i \\ . \\ \Delta\mathbf{y}_N \end{bmatrix}; \quad \mathbf{W} = \begin{bmatrix} \Delta\mathbf{W}_1' \\ . \\ \Delta\mathbf{W}_i' \\ . \\ \Delta\mathbf{W}_N' \end{bmatrix}; \text{ and } \mathbf{E} = \begin{bmatrix} \mathbf{E}_1 \\ . \\ \mathbf{E}_i \\ . \\ \mathbf{E}_N \end{bmatrix}$$

Similarly, define: $\mathbf{Z} = \begin{bmatrix} \mathbf{Z}'_1 \\ . \\ \mathbf{Z}'_i \\ . \\ \mathbf{Z}'_N \end{bmatrix}$

Then, if the two assumptions marked (2) are satisfied, $E(\mathbf{Z}'_i\ \mathbf{E}_i) = \mathbf{0}$ where $\mathbf{0}$ is a Qx1 vector of zeros, and the generalized methods of moments estimator $\tilde{\mathbf{B}}$ is consistent, with:

$$\tilde{\mathbf{B}} = (\mathbf{W}'\ \mathbf{Z}\mathbf{A}_J\mathbf{Z}'\mathbf{W})^{-1}\mathbf{W}'\mathbf{Z}\mathbf{A}_J\mathbf{Z}'\ \mathbf{Y}$$

The matrix $\mathbf{A}_J$ is a symmetric, positive semi-definite matrix of dimension (*Q x Q)* and is obtained in two steps. First, assume that the errors are independent and identically distributed, with a constant variance σ_u^2, and use a known (4 x Q) matrix H:

$$\mathbf{H} = \begin{bmatrix} 2 & -1 & 0 & . & . & 0 \\ -1 & 2 & . & . & . & 0 \\ 0 & . & . & . & . & -1 \\ 0 & . & . & . & -1 & 2 \end{bmatrix}$$

So that: $E[\mathbf{E}_i\mathbf{E}'_i\ ] = \sigma_u^2\mathbf{H}$. Then for the first-stage estimate of $\tilde{\mathbf{B}}$,

$$\mathbf{A}_1 \equiv \frac{1}{N}\sum_{i=1}^{N}(\mathbf{Z}'_i\mathbf{H}\mathbf{Z}_i)^{-1}$$

Since the ε_{it} could be heteroscedastic, in the second stage calculate the estimated errors: $\hat{\mathbf{E}}_i^1 = \Delta\mathbf{y}_i - \Delta\mathbf{W}_i\tilde{\mathbf{B}}_1$ and use these fitted terms to estimate a consistent variance-covariance matrix of the moment conditions. Then use this variance-covariance matrix as the weight for $\mathbf{A}_2$ in the second stage,

so $\mathbf{A}_2 \equiv \frac{1}{N}\sum_{i=1}^{N}(\mathbf{Z}'_i\hat{\mathbf{E}}_i^1\hat{\mathbf{E}}_i^{1\prime}\mathbf{Z}_i)^{-1}$.

Following this procedure, if all of the above conditions are satisfied then the asymptotic covariance matrix of $\tilde{\mathbf{B}}$ is consistent and efficient and collapses to: $\Phi = (\mathbf{W}'\mathbf{Z}\mathbf{A}_2\mathbf{Z}'\mathbf{W})^{-1}$.

III. A Test for Second-Order Serial Correlation[43]

Arellano and Bond (1991) propose a test for second-order serial correlation. Using the model developed above, this test examines the average covariances of the differenced equation: $\phi_i = (\mathbf{E}'_{i,-2}\mathbf{E}_{i*})$ where $\mathbf{E}_{i,-2}$ is the vector of residuals lagged twice (i.e. $\mathbf{E}_{i,-2} \equiv [\, \varepsilon_{i,1} \ldots \varepsilon_{i,T-2}]'$) and $\mathbf{E}_{i*}$ is a vector of the residuals $\mathbf{E}_i$ trimmed to match $\mathbf{E}_{i,-2}$ (i.e. $\mathbf{E}_{i*} \equiv [\, \varepsilon_{i,3} \ldots \varepsilon_{i,T}]'$).

If the assumptions (listed as equation 2) are satisfied, these average covariances are independent random variables across countries with zero means. Since we adjust for heteroscedasticity, however, the variances may be unequal. The procedure then tests the null hypothesis of whether $E(\phi_i) = 0$. The test statistic has one degree of freedom and is defined as:

$$m_2 = \frac{\hat{\mathbf{E}}'_{-2}\hat{\mathbf{E}}_*}{\hat{\mathbf{E}}^{1/2}} \sim N(0,1)$$

Where $\hat{\mathbf{E}}$ are the second-stage estimated residuals, calculated as described in part II of the appendix,

$$\hat{\mathbf{E}}_{-2} \equiv \left[\hat{\mathbf{E}}'_{-2,1} \ldots \hat{\mathbf{E}}'_{-2,N}\right]' \text{ and } \hat{\mathbf{E}}_* \equiv \left[\hat{\mathbf{E}}'_{*,1} \ldots \hat{\mathbf{E}}'_{*,N}\right]' .$$

An alternative test procedure is the Sargan test of overidentifying restrictions. The Sargan test, however, is only valid if the error terms are i.i.d., while the test of second order serial correlation is asymptotically robust to general heteroscedasticity. Since error terms tend to be heteroscedastic in cross-country analyses, I focus on the first test.

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[43] See Arellano and Bond (1991) for further information.

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Table I
Summary Statistics - "High Quality Data"

Code	*Definition*	*Source*	*Year*	*Mean*	*Std Dev*	*Min*	*Max*
FEMED	Average years of secondary schooling in the female population aged over 25	Barro & Lee	1965	0.90	0.95	0.04	3.10
			1970	0.95	0.94	0.04	3.36
			1975	1.11	0.94	0.05	3.62
			1980	1.40	1.10	0.14	5.11
			1985	1.54	0.99	0.20	4.84
			1990	1.76	1.02	0.21	4.69
INC	Ln of Real GNP per capita, in 1987 $US, calculated using the Atlas method	World Bank	1965	7.62	1.46	5.49	9.45
			1970	7.68	1.31	5.63	9.54
			1975	8.19	1.23	5.63	9.81
			1980	8.38	1.34	5.33	9.96
			1985	8.00	1.27	5.07	9.75
			1990	8.28	1.51	5.23	10.04
			1995	8.30	1.55	5.17	10.22
INEQ	Inequality, measured by the gini coefficient. Following Deininger and Squire, I have added 6.6 to gini coefficients based on expenditure (instead of income)	Dein. & Squire	1965	37.8	8.37	24.3	55.5
			1970	40.3	9.45	25.1	57.7
			1975	39.9	9.03	23.3	61.9
			1980	38.1	8.36	21.5	57.8
			1985	37.4	8.59	21.0	61.8
			1990	38.0	9.03	23.3	59.6
MALED	Average years of secondary schooling in the male population aged over 25	Barro & Lee	1965	1.13	0.85	0.18	2.94
			1970	1.27	0.86	0.35	3.27
			1975	1.47	0.92	0.37	3.55
			1980	1.79	1.06	0.57	5.07
			1985	1.90	0.99	0.65	4.81
			1990	2.16	1.02	0.73	4.85
PPPI	Price level of investment, measured as the PPP of investment / Exchange rate relative to U.S.	Summ & Heston	1965	76.7	22.7	40.8	119.2
			1970	68.1	18.9	41.2	107.1
			1975	86.4	24.6	36.5	130.7
			1980	93.5	28.5	44.4	140.7
			1985	61.2	16.3	31.9	94.3
			1990	75.7	31.4	27.9	129.3

NOTE: If the gini coefficient is not available for a given year, the observation is taken from the closest year in the five-year period up to the stated year.
SOURCES: Barro & Lee is data set compiled in Barro and Lee (1996). Dein. & Squire is data set compiled in Deininger and Squire (1996a). Summ & Heston is Summers and Heston "Penn World Tables" version 5.6. World Bank is "World*Data 1995" published by the World Bank and available on CD-Rom.

Table II: Gini Coefficients

Country	*Gini Coefficients*[1]					
	1965	*1970*	*1975*	*1980*	*1985*	*1990*
Australia	-	-	-	39.3	37.6	41.7
Bangladesh	37.3	34.2	36.0	35.2	36.0	35.5
Belgium	-	-	-	28.3	26.2	26.6
Brazil	-	57.6	61.9	57.8	61.8	59.6
Bulgaria	-	-	-	-	23.4	24.5
Canada	31.6	32.3	31.6	31.0	32.8	27.6
Chile	-	45.6	46.0	53.2	-	-
China	-	-	-	32.0	31.4	34.6
Colombia	-	52.0	46.0	54.5	-	-
Costa Rica	-	-	44.4	45.0	47.0	46.1
Denmark	-	-	-	31.0	31.0	33.2
Dominican Rep.	-	-	-	45.0	43.3	50.5
Finland	-	31.8	27.0	30.9	30.8	26.2
France	47.0	44.0	43.0	34.9	34.9	-
Germany	28.1	33.6	30.6	32.1	32.2	-
Greece	-	-	-	-	39.9	41.8
Hong Kong	-	-	39.8	37.3	45.2	42.0
Hungary	-	-	-	21.5	21.0	23.3
India	37.7	37.0	35.8	38.7	38.1	36.3
Indonesia	-	-	-	42.2	39.0	39.7
Ireland	-	-	38.7	35.7	-	-
Italy	-	-	39.0	34.3	33.2	32.7
Japan	34.8	35.5	34.4	33.4	35.9	35.0
Korea (South)	34.3	33.3	36.0	38.6	34.5	33.6
Malaysia	-	50.0	51.8	51.0	48.0	48.4
Mexico	55.5	57.7	57.9	50.0	50.6	55.0
Netherlands	-	-	28.6	28.1	29.1	29.6
New Zealand	-	-	30.0	34.8	35.8	40.2
Norway	37.5	36.0	37.5	31.2	31.4	33.1
Pakistan	-	36.5	38.1	38.9	39.0	38.0
Peru	-	-	-	-	49.3	49.4
Philippines	-	-	-	-	46.1	45.7
Poland	-	-	-	-	25.3	26.2
Portugal	-	-	40.6	36.8	-	-
Singapore	-	-	41.0	40.7	42.0	39.0
Spain	-	-	37.1	33.4	31.8	32.5
Sri Lanka	47.0	37.7	35.3	42.0	45.3	36.7
Sweden	-	33.4	27.3	32.4	31.2	32.5
Thailand	41.3	42.6	41.7	-	-	-
Trinidad & Tob.	-	-	51.0	46.1	41.7	-
Tunisia	-	-	50.6	49.6	49.6	46.8
Turkey	-	56.0	51.0	-	-	-
United Kingdom	24.3	25.1	23.3	24.9	27.1	32.3
United States	34.6	34.1	34.4	35.2	37.3	37.8
Venezuela	-	-	47.7	39.4	42.8	53.8
Average	***37.8***	***40.3***	***39.9***	***38.1***	***37.4***	***38.0***

NOTE: (1) If gini coefficient for the given year is not available, data from the nearest year in the preceding 5 years is utilized.

Table III
Correlation Matrix

	GR	INEQ	INC	MALED	FEMED	PPPI
GR	1.000					
INEQ	0.058	1.000				
INC	0.011	-0.342	1.000			
MALED	0.062	-0.379	0.602	1.000		
FEMED	0.024	-0.358	0.678	0.947	1.000	
PPPI	-0.264	-0.184	0.659	0.325	0.357	1.000

Table IV
Regional Representation

Region	*Number of Countries*	*Number of Observations*
East Asia	7	27
Latin America	9	33
OECD	20	83
Sub-Saharan Africa	0	0
Other	9	37
Total Sample	*45*	*180*

NOTE: Regional divisions follow Barro (1997).
The countries included in each region are: East Asia/Pacific: Hong Kong, Indonesia, Korea, Malaysia, Philippines, Singapore, and Thailand; Latin America: Brazil, Chile, Colombia, Costa Rica, Dominican Republic, Mexico, Peru, Trinidad and Tobago, and Venezuela; OECD/High Income: Australia, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Turkey, United Kingdom, and United States.

Table V
Regression Results: Alternate Estimation Techniques

	5-Year Periods			*10-Year Periods*
Estimation Method	***Fixed Effects (1)***	***Random Effects (2)***	***Arellano & Bond (3)***	***Fixed Effects (4)***
INEQ	0.0036	0.0013	0.0013	0.0013
	(0.0015)	(0.0006)	(0.0006)	(0.0011)
INC	-0.076	0.017	-0.047	-0.071
	(0.020)	(0.006)	(0.008)	(0.016)
MALED	-0.014	0.047	-0.008	-0.002
	(0.031)	(0.015)	(0.022)	(-0.028)
FEMED	0.070	-0.038	0.074	0.031
	(0.032)	(0.016)	(0.018)	(0.030)
PPPI	-0.0008	-0.0009	-0.0013	-0.0003
	(0.0003)	(0.0002)	(0.0001)	(0.0003)
R^2	*0.67*	*0.49*		*0.71*
Countries	*45*	*45*	*45*	*45*
Observs.	*180*	*180*	*135*	*112*
Period	*1965-95*[1]	*1965-95*[1]	*1970-95*	*1965-95*

NOTES: Dependent variable is average annual per capita growth. Standard errors are in parentheses.
R^2 is the within-R^2 for fixed effects and the overall-R^2 for random effects.
(1) Estimates are virtually identical for the period 1970-1995 (with 143 observations).

Table VI
Regression Results: What Affects the Coefficient on Inequality?

Definitions & Data Set	*Perotti[a] Low Quality*	*D&S Low Quality[b]*	*D&S Low Quality[b]*	*D&S High Quality*	*D&S High Quality*	*D&S Low Quality[b]*	*D&S High Quality*
Estimation &Period	*X-Country 25-Yr*	*X-Country 25-Yr*	*X-Country 25-Yr*	*X-Country 25-Yr*	*X-Country 5-Yr*	*Arel & Bond 5-Yr*	*Arel & Bond 5-Yr*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*
CONS	-0.018	0.046	0.061	0.071	0.018		
	[-1.37]	(0.027)	(0.264)	(0.030)	(0.031)		
INEQ	***-0.118[a]***	***-0.0005***	***-0.0005***	***-0.0005***	***0.0002***	***-0.0001***	***0.0013***
	[-2.84]	***(0.0002)***	***(0.0003)***	***(0.0003)***	***(0.0003)***	***(0.0001)***	***(0.0006)***
INC	-0.002	-0.001	-0.002	-0.004	0.002	-0.053	-0.047
	[-1.77]	(0.003)	(-0.003)	(0.003)	(0.008)	(0.0013	(0.008)
MALED	0.031	0.040	0.039	0.037	0.023	0.047	-0.008
	[4.05]	(0.008)	(0.008)	(0.009)	(0.007)	(0.014)	(0.022)
FEMED	-0.025	-0.035	-0.035	-0.034	-0.023	0.019	0.074
	[-3.06]	(0.008)	(0.008)	(0.009)	(0.007)	(0.009)	(0.018)
PPPI	-0.002	-0.0001	-0.0001	-0.0001	-0.0001	-0.0011	-0.0013
	[-0.31]	(0.0001)	(0.0001)	(-0.73)	(0.0001)	(0.0001)	(0.0001)
R^2	*0.31*	*0.38*	*0.40*	*0.40*	*0.50*		
Countries	*67*	*63*	*45*	*45*	*45*	*45*	*45*
Periods	*1*	*1*	*1*	*1*	*5*	*5*	*5*

NOTES: Dependent variable is average annual per capita growth from 1970-95. T-statistics are in brackets and standard errors are in parentheses. R^2 is the overall-R^2.

a) Variable definitions used by Perotti are different than those used in the rest of this paper. For example, INEQ is measured as the income share held by the middle class (a measure of equality) rather than by the gini coefficient (a measure of inequality) and I add the negative sign to facilitate comparison with the other columns. Also Perotti defines INC as initial income, while I use the log of initial income.

b) Low quality data is average inequality in the unabridged Deininger & Squire data set. This includes statistics accepted as "high quality" as well as those not accepted.

Table VII
Sensitivity Analysis: Dropping Outliers

		Dropping Outliers in Average Inequality		*Dropping Outliers in Average Income*		*Dropping Outliers in Average Growth*	
	Whole Sample (1)	*5 Lowest INEQ Dropped (2)*	*5 Highest INEQ Dropped (3)*	*5 Lowest INC Dropped (4)*	*5 Highest INC Dropped (5)*	*5 Lowest GR Dropped (6)*	*5 Highest GR Dropped (7)*
INEQ	0.0013	0.0036	0.0048	0.0035	0.0040	0.0051	0.0033
	(0.0006)	(0.0004)	(0.0002)	(0.0003)	(0.0001)	(0.0000)	(0.0000)
INC	-0.047	-0.061	-0.063	-0.048	-0.067	-0.147	-0.098
	(0.008)	(0.013)	(0.007)	(0.012)	(0.003)	(0.000)	(0.000)
MALED	-0.008	-0.043	0.026	-0.070	0.013	-0.007	0.047
	(0.022)	(0.006)	(0.000)	(0.020)	(0.009)	(0.018)	(0.009)
FEMED	0.074	0.102	0.036	0.132	0.071	0.069	0.001
	(0.018)	(0.004)	(0.004)	(0.019)	(0.012)	(0.013)	(0.006)
PPPI	-0.0013	-0.0011	-0.0010	-0.0013	-0.0008	-0.0007	-0.0006
	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0001)	(0.0000)	(0.0000)
Countries	***45***	***40***	***40***	***40***	***40***	***40***	***40***
Observs	***135***	***124***	***120***	***114***	***119***	***126***	***118***

Dependent variable is average annual growth from 1970-95. Standard errors in parentheses.
Estimates obtained using Arellano & Bond (1991).

Table VIII
Sensitivity Analysis: Sample Divided By Region

	Arellano & Bond			*Fixed Effects*			
	Whole Sample	*Excluding East Asia*	*Excluding Latin Am.*	*Whole Sample*	*Excluding East Asia*	*Excluding Latin Am.*	*Excluding OECD*
INEQ	0.0013	0.0039	0.0025	0.0036	0.0034	0.0025	0.0045
	(0.0006)	(0.0000)	(0.0003)	(0.0015)	(0.0016)	(0.0016)	(0.0022)
INC	-0.047	-0.088	-0.046	-0.076	-0.097	-0.054	-0.087
	(0.008)	(0.000)	(0.005)	(0.020)	(0.024)	(0.021)	(0.030)
MALED	-0.008	0.003	0.030	-0.014	0.001	-0.021	-0.024
	(0.022)	(0.005)	(0.006)	(0.031)	(0.036)	(0.030)	(0.044)
FEMED	0.074	0.045	0.022	0.070	0.051	0.064	0.097
	(0.018)	(0.000)	(0.006)	(0.032)	(0.037)	(0.031)	(0.046)
PPPI	-0.0013	-0.0007	-0.0011	-0.0008	-0.0005	-0.0010	-0.0004
	(0.0001)	(0.0001)	(0.0000)	(0.0003)	(0.0037)	(0.0004)	(0.0005)
R^2				*0.67*	*0.68*	*0.70*	*0.55*
Countries	*45*	*38*	*36*	*45*	*38*	*36*	*25*
Observs	*135*	*115*	*111*	*180*	*153*	*147*	*97*
Period	*1970-95*	*1970-95*	*1970-95*	*1965-95*	*1965-95*	*1965-95*	*1965-95*

Dependent variable is average annual growth. Standard errors in parentheses.

Table IX
Sensitivity Analysis: Sample Divided by Initial Income

	Whole	*Cutoff: $1000*		*Cutoff: $3000*		*Cutoff: $6000*	
	Sample	*Poor*	*Wealthy*	*Poor*	*Wealthy*	*Poor*	*Wealthy*
INEQ	0.0036	0.0056	0.0024	0.0061	0.0018	0.0042	0.0022
	(0.0015)	(0.0032)	(0.0016)	(0.0021)	(0.0021)	(0.0020)	(0.0017)
INC	-0.076	-0.133	-0.081	-0.111	-0.044	-0.089	-0.131
	(0.020)	(0.038)	(0.029)	(0.026)	(0.042)	(0.026)	(0.059)
MALED	-0.014	0.032	-0.034	-0.011	0.006	-0.022	0.005
	(0.031)	(0.059)	(0.038)	(0.040)	(0.050)	(0.039)	(0.046)
FEMED	0.070	0.103	0.069	0.110	0.018	0.104	0.005
	(0.032)	(0.063)	(0.041)	(0.042)	(0.052)	(0.042)	(0.046)
PPPI	-0.0008	0.0001	-0.0009	-0.0006	-0.0011	-0.0006	-0.0003
	(0.0003)	(0.0001)	(0.0004)	(0.0004)	(0.0006)	(0.0004)	(0.0005)
R^2	***0.67***	***0.67***	***0.69***	***0.68***	***0.74***	***0.62***	***0.89***
Countries	***45***	***11***	***34***	***23***	***22***	***34***	***11***
Observations	***180***	***48***	***132***	***90***	***90***	***126***	***54***

Dependent variable is average annual growth from 1970-95. Standard errors in parentheses. Estimates obtained using fixed effects.
Cutoff dividing rich and poor countries is GNP per capita in 1965. Measured in 1987 $US.

Table X
Sensitivity Analysis
Alternate Measures of Inequality

	INEQ (1)	*RATIO1 20/40* (2)	*RATIO2 20/20* (3)	*-MIDDLE CLASS* (4)	*INEQ* (5)	*Adjusted INEQ* (6)
INEQ	0.0045	0.0164	0.0062	0.171	0.0046	0.0053
	(0.0004)	(0.0005)	(0.0001)	(0.021)	(0.0023)	(0.0020)
INC	-0.080	-0.159	-0.099	-0.084	-0.095	-0.091
	(0.009)	(0.013)	(0.005)	(0.003)	(0.036)	(0.036)
MALED	0.030	0.097	0.022	0.041	-0.051	-0.045
	(0.012)	(0.000)	(0.006)	(0.010)	(0.040)	(0.039)
FEMED	0.032	0.010	0.054	0.021	0.096	0.089
	(0.014)	(0.000)	(0.004)	(0.008)	(0.040)	(0.040)
PPPI	-0.0006	-0.0003	-0.0006	-0.0009	-0.0004	-0.0004
	(0.0001)	(0.0001)	(0.0000)	(0.0000)	(0.0006)	(0.0005)
Countries	***43***	***43***	***43***	***43***	***37***	***37***
Observs	***118***	***118***	***118***	***118***	***122***	***122***
Period	***1970-95***	***1970-95***	***1970-95***	***1970-95***	***1965-95***	***1965-95***
Estimation	***Arel & Bond***	***Arel & Bond***	***Arel & Bond***	***Arel & Bond***	***Fixed Effects***	***Fixed Effects***

Dependent variable is average annual growth over the given period. Standard errors in parentheses.

Ratio1 (20/40) is the income share held by the richest 20% of the population over the income share held by the poorest 40%.
Ratio2 (20/20) is the income share held by the richest 20% of the population over the share held by the poorest 20%.
-Middle Class is the negative of the income share held by the third and fourth wealthiest quintiles.
Adjusted Ineq is inequality only uses measures of inequality from the same source for each country.

Table XI
Variable Definitions for Alternate Specifications

Code	*Definition*
ASSA	Number of assassinations per million population per year
BMP	The Log of (1 + Black Market Premium). Black Market Premium measured as (Black Market Exchange Rate / Official Exchange Rate) - 1
EXP	Ratio of Exports to GDP (in current international prices)
FHIGH	Average years of higher schooling in the female population aged over 25
GCONS	Ratio of real government "consumption" expenditure net of spending on defense and education to real GDP
GDP*HM	Interactive term between a country's per capita income and human capital. Calculated as INC*(MALED+FEMED+MHIGH+FHIGH+LIFEEX) where INC, MALED and FEMED are defined in Table I
GOVED	Ratio of total nominal government expenditure on education to nominal GDP
INV	Ratio of real domestic investment (private plus public) to real GDP
LIFEEX	Life expectancy at birth
MHIGH	Average years of higher schooling in the male population aged over 25
POPGR	Growth rate of the population
POP >65	Proportion of the population aged over 65
PRIM	Total gross enrollment ratio for primary education
PSTAB	Political instability. Calculated as (0.5 * ASSA) + (0.5 * REVO)
PST*IN	Interaction between political instability and inequality. Calculated as PSTAB * INEQ (where INEQ is defined in Table I)
REVCP	Total number of revolutions and coups per year
REVO	Total number of revolutions per year
SEC	Total gross enrollment ratio for secondary education
TOTGR	Growth in the terms of trade (or the terms of trade shock). Measured as the growth rate of export prices minus the growth rate of import prices

SOURCE: All data is taken from Barro and Lee (1997) "Data Set for a Panel of 138 Countries" available on disk from the authors.

Table XII
Sensitivity Analysis: Alternate Specifications

	Basic Model	*Alesina & Perotti (1994)*			*Birdsall, Ross & Sabot (1995)*		*Deininger & Squire (1996a)*		*Perotti(1996)*	
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*
INEQ	0.0048	0.0044	0.0034	0.0034	0.0041	0.0041	0.0038	0.0033	0.0044	0.0050
	(0.0017)	(0.0017)	(0.0016)	(0.0016)	(0.0017)	(0.0017)	(0.0017)	(0.0016)	(0.0016)	(0.0017)
INC	-0.083	-0.110	-0.125	-0.125	-0.094	-0.091	-0.082	-0.106	-0.092	-0.081
	(0.022)	(0.019)	(0.019)	(0.019)	(0.026)	(0.024)	(0.023)	(0.019)	(0.022)	(0.023)
MALED	0.032						0.032		0.052	0.034
	(0.038)						(0.038)		(0.039)	(0.040)
FEMED	0.004						0.005		-0.028	0.000
	(0.042)						(0.041)		(0.043)	(0.044)
PPPI	-0.0009				-0.0008	-0.0008			-0.0010	-0.0009
	(0.0004)				(0.0004)	(0.0004)			(0.0003)	(0.0004)
ASSA					-0.038	-0.061				
					(0.372)	(0.363)				
BMP							-0.043			
							(0.028)			
GCONS					0.225	0.182				
					(0.362)	(0.346)				
INV							0.194	0.364		
							(0.191)	(0.169)		
LIFEEX										-0.001
										(0.005)
POP>65									1.920	
									(0.846)	
PRIM		-0.74	-0.037	-0.038	-0.022					
		(0.117)	(0.108)	(0.108)	(0.112)					
PSTAB			-0.016							
			(0.065)							
PST*IN				-0.000						
				(0.002)						
REVO					-0.006	-0.004				
					(0.034)	(0.032)				
SEC					0.035					
					(0.080)					
R^2	***0.73***	***0.71***	***0.82***	***0.82***	***0.83***	***0.83***	***0.75***	***0.71***	***0.74***	***0.73***
Counts.	***45***	***44***	***40***	***40***	***38***	***38***	***43***	***44***	***42***	***44***
Observs.	***144***	***143***	***104***	***104***	***102***	***102***	***141***	***143***	***140***	***143***

Dependent variable: growth from 1965-90. Standard errors in parentheses. Estimates calculated using fixed effects.

Table XII
Sensitivity Analysis: Alternate Specifications (cont)

	Levine & Renelt (1992)			*B&S**	*Caselli et al. (1996)*					
	(11)	*(12)*	*(13)*	*(14)*	*(15)*	*(16)*	*(17)*	*(18)*	*(19)*	*(20)*
INEQ	0.0035	0.0035	0.0026	0.0037	0.0035	0.0038	0.0026	0.0028	0.0026	0.0028
	(0.0018)	(0.0018)	(0.0017)	(0.0018)	(0.0017)	(0.0017)	(0.0017)	(0.0017)	(0.0017)	(0.0017)
INC	-0.120	-0.124	-0.154	-0.171	-0.121	-0.123	-0.110	-0.107	-0.105	-0.104
	(0.021)	(0.021)	(0.026)	(0.036)	(0.019)	(0.019)	(0.022)	(0.021)	(0.022)	(0.022)
MALED				0.014		0.024	-0.005	-0.000	0.001	0.007
				(0.040)		(0.038)	(0.039)	(0.038)	(0.037)	(0.038)
FEMED				-0.003		0.004	0.037	0.028	0.029	0.026
				(0.053)		(0.043)	(0.044)	(0.042)	(0.042)	(0.042)
BMP			-0.016	-0.077			-0.035	-0.039	-0.042	-0.055
			(0.031)	(0.038)			(0.032)	(0.032)	(0.033)	(0.035)
EXP			0.523							
			(0.169)							
FHIGH				-0.197						
				(0.189)						
GCONS		0.544	0.462	0.894			0.735	0.703	0.728	0.763
		(0.412)	(0.388)	(0.428)			(0.424)	(0.419)	(0.421)	(0.422)
GDP*HM				0.002						
				(0.001)						
GOVED				0.503						
				(1.030)						
INV	0.312	0.392	0.457	0.127	0.306	0.316	0.426	0.463	0.443	0.421
	(0.171)	(0.203)	(0.201)	(0.252)	(0.165)	(0.166)	(0.211)	(0.201)	(0.200)	(0.201)
LIFEEX				-0.013			0.004			
				(0.010)			(0.006)			
MHIGH				0.043						
				(0.135)						
POLST**		-0.019	-0.008	0.069			0.015	0.012	0.232	0.269
		(0.027)	(0.027)	(0.073)			(0.034)	(0.034)	(0.394)	(0.395)
POPGR	-3.25	-1.95	-1.63		-3.17	-3.26				
	(2.15)	(2.28)	(2.10)		(2.08)	(2.16)				
PRIM		-0.008	0.025							
		(0.112)	(0.104)							
SEC	-0.014	0.013	0.018							
	(0.079)	(0.081)	(0.76)							
TOTGR				-0.216						-0.152
				(0.154)						(0.146)
R^2	***0.82***	***0.83***	***0.87***	***0.86***	***0.82***	***0.83***	***0.84***	***0.84***	***0.84***	***0.84***
Counts.	***40***	***38***	***37***	***38***	***40***	***40***	***38***	***38***	***38***	***32***
Observs.	***105***	***102***	***100***	***102***	***105***	***105***	***102***	***102***	***102***	***102***

Dependent variable: growth from 1965-90. Standard errors in parentheses. Estimates calculated using fixed effects.
* B&S refers to Barro and Sala-i-Martin (1995).
**POLST is defined by REVCP in columns 12 & 13, by PSTAB in column 14, REVO in columns 17 & 18 and ASSA in columns 19 & 20.

Chapter 2:

Has Increased Trade Affected the Wages of Pablo, Fritz and Yingye?

Abstract: This paper evaluates the impact of increased trade on wages and inequality throughout the world. Basic trade theory predicts that increased trade between developed and developing countries would increase wage inequality in wealthy countries and decrease inequality in poorer countries. A wide variety of methods and data sets have been used to test this basic concept, and most conclude that trade has had a minimal impact on relative wages. This empirical work, however, has several critical shortcomings. It tends to focus on a limited sample of countries, inaccurately classifies countries and trade flows as high- or low-skill intensive, and ignores several relevant variables such as trade with similar countries, capital flows, changes in the supply of skills, labor market structure, technological change, and other country-specific characteristics.

This paper attempts to correct for each of these shortcomings. It compiles a new data set on trade flows between countries, using income levels, average education, and manufacturing wages to classify countries and trade flows as relatively high-skill intensive or low-skill-intensive. Then, using this new data and controlling for many of the factors ignored in previous work, it estimates how changes in trade flows are related to changes in inequality and wages within a given country. It finds that increased trade with low-skill countries has a significant negative relationship with wage inequality and low-skilled wages in high-skill countries. In order to correct for endogeneity and isolate how trade affects wages, the final section of the paper formulates a general-equilibrium model linking wages and trade flows. It is difficult to estimate the model, however, due to the lack of good instruments for trade and wages in a panel framework. Therefore, although the reduced-form estimates suggest a strong relationship between trade and inequality, I am unable to identify the direct impact of increased trade on wages and inequality throughout the world.

I. Introduction

Two empirical facts are indisputable. Trade flows between developed and developing countries have increased since 1970, and the demand for low-skilled labor in developed countries has decreased since the end of the 1970's. The popular press rarely questions the link between these two trends--that increased trade, is largely responsible for rising inequality in the U.S. and higher unemployment in Europe. OECD newspapers and periodicals no longer even bother explaining that trade liberalization has made it much cheaper to import low-skill-intensive goods than produce them at home, leading to a permanent decline in the demand for low-skilled workers. Politicians further disseminate this story and build on the inherent fears through vivid expressions such as the "giant sucking sound" of U.S. jobs moving to Mexico and the "race to the bottom".

Economists, however, generally believe that the impact of increased trade on low-skilled wages and labor demand has been minor. They have developed numerous arguments to support this claim: from the straightforward assertion that trade with developing nations is simply too small a share of GDP to have a large influence on inequality, to highly complex empirical models which generally estimate only a small impact of trade on wages. In fact, the emerging consensus among economists is that instead of increased globalization, skill-biased technological change has been the greatest impetus behind reduced demand for low-skilled workers in developed countries.

Why is there such a discrepancy between the beliefs of the popular press and the majority of economists? Granted, politicians and the popular press have a long history of ignoring mainstream economics (such as their support for supply-side economics in the 1980's), but in recent years, timely economic concepts and findings tend to be quickly simplified and disseminated to the public. The disagreement over how trade has affected wages and inequality, however, appears to be far from any sort of resolution. This paper will suggest that, at least in this one case, economists should reassess the arguments of the popular press. Economists may have been too quick to disregard the role of trade in the determination of relative labor demands, and there may be some merit to the concerns of Pablo, Fritz, Yingye, and other wage earners around the world.

Before attempting to measure the impact of increased trade on wages around the world, Section II begins by briefly reviewing basic trade theory which predicts that increased trade between developed and developing countries could increase wage inequality in wealthier countries and decrease inequality in poorer countries. It then summarizes the wide variety of approaches and techniques used to test this theory, and the empirical results that generally estimate a minimal impact of trade on wages. Section III suggests, however, that this empirical work has several critical shortcomings. It tends to focus on a limited sample of countries, inaccurately classifies countries and trade flows as high- or low-skill intensive, and ignores several relevant variables such as trade with similar countries, capital flows, changes in the supply of skills, labor market structure and any other unmeasured country-specific characteristics. There are a variety of different methods of correcting for these problems. In particular, Section IV focuses on more accurately classifying the absolute and relative skill abundance of different countries and the skill intensity embodied in various trade flows. It compiles a new data set on trade patterns, using income levels, average education, and manufacturing wages to classify countries and trade flows as relatively high-skill intensive or low-skill intensive.

Then, using this data and controlling for many of the factors ignored in previous work, Section V uses a fixed-effects estimator to measure how changes in trade flows are related to changes in inequality and wages within a given country. It finds that in high-skill countries, increased trade with low-skill countries has a significant negative relationship with wage inequality and low-skill wages. Next, in order to correct for endogeneity and isolate how trade affects wages, Section VI formulates a general-equilibrium model linking wages and trade flows. It is difficult to estimate the model, however, due to the lack of good instruments for trade and wages in a panel framework. Therefore, although the reduced-form estimates suggest a strong relationship between trade and inequality, I am not able to identify the direct impact of increased trade on wages and inequality throughout the world. The reduced form estimates do suggest, however, that if Pablo, Fritz and Yingye are low-wage earners in a wealthy country, they should be concerned with how increased trade with developing countries has lowered their relative wages.

II. The HOS Theory and Empirical Work Testing the Theory

The basic explanation of why increased trade might affect wage inequality has been a key tenet of international trade theory since the 1940's. In its simplest form, the Heckscher-Ohlin theory assumes two countries, which I will refer to as Mexico (M) and the United States (U). The two factors of production, high-skilled labor (H) and low-skilled labor (L), are both used to produce two traded goods: sombreros (S) and televisions (T). The prices of the two goods are P_S and P_T, respectively, and the returns to the two factors are w_H and w_L. Assume that the U.S. has a relative abundance of high-skilled labor and Mexico has a relative abundance of low-skilled labor, or in algebraic terms, $\frac{H_U}{L_U} > \frac{H_M}{L_M}$. The production of sombreros is low-skill intensive, and the production of televisions is high-skill intensive, i.e. $\frac{a_{TH}}{a_{TU}} > \frac{a_{SH}}{a_{SU}}$. Therefore, in autarky, $\frac{P_{TU}}{P_{SU}} < \frac{P_{TM}}{P_{SM}}$ and $\frac{w_{HU}}{w_{LU}} < \frac{w_{HM}}{w_{UM}}$. Since inequality is the ratio of high-skill wages to low-skill wages, inequality is lower in the U.S. than in Mexico, or $INEQ_U < INEQ_M$.

Since the U.S. has a comparative advantage in televisions and Mexico has a comparative advantage in sombreros, when trade opens up between the two countries, the U.S. will export televisions and import sombreros (and the opposite will occur in Mexico.) As a result, the price of televisions will rise in the U.S. and the price of sombreros will fall (and again the opposite will occur in Mexico.) The Stolper-Samuelson extension of this theory explains how the relative returns to the two factors of production are affected by these trade patterns. In each country, the returns to the factor used intensively in the export good will rise and the returns to the relatively less-abundant factor will fall. As a result, in the U.S. high-skill wages will rise and low-skill wages will fall (and the opposite will occur in Mexico.) Inequality will therefore rise in the U.S. and fall in Mexico. In algebraic terms, if the " ′ " indicates values after trade, then:

$$\frac{w_{HU}'}{w_{LU}'} > \frac{w_{HU}}{w_{LU}} \text{ or } INEQ_U' > INEQ_U$$

$$\frac{w_{HM}'}{w_{LM}'} < \frac{w_{HM}}{w_{LM}} \text{ or } INEQ_M' < INEQ_M$$

This simple model is clearly based on a number of completely unrealistic assumptions, (such as only two countries, two goods, two factors, and identical production technologies in the two countries) and it grossly oversimplifies the many complex factors determining trade flows and factor prices. In fact, when Stolper and Samuelson submitted the original article outlining this theory to the *American Economic Review*, the editors acknowledged that it was "a brilliant theoretical performance," but denied it for publication since: "It does not have anything to say about any of the real situations with which the theory of international trade has to concern itself."[1] Over the years, however, the assumptions underlying this basic Heckscher-Ohlin and Stolper-Samuelson framework (which I will refer to as HOS in the remainder of this paper) have been dropped, and the basic framework has been extended in a number of very different models. Moreover, in most of these extensions, the general implications of the theory hold. For example, when the model is extended to a world of many goods and factors, an increase in the relative prices of a bundle of traded products will increase the relative returns of the factors used intensively in those products, and will decrease the relative returns of the other factors.

In fact, the basic interactions driving the HOS theory are so intuitive that we would expect the implications to be fairly robust. When trade between two different economies increases, this will usually affect the prices of goods in the two countries. Production will shift to the industry where profitability rises, which will in turn increase the demand for any factors used in that industry. If any factors are used more intensively in the industry where profitability increases than in the rest of the economy, then prices for these factors should increase (at least in the short-run when factor supplies and technologies are relatively fixed.) It is straightforward to extend these intuitive arguments to explain why the recent increase in trade between developed and developing countries has affected the demand for unskilled labor in wealthy countries. When a developed country begins to trade with a developing country, the wealthy country will import low-skill intensive goods (since they can be produced at a lower cost by the much cheaper labor in the developing country.) This will reduce the price of low-skill intensive goods in the developed country, and production will shift away from these less-profitable industries. This will in turn decrease demand for the low-skilled labor used intensively in this industry, reducing low-skill wages and increasing wage inequality.

While this chain of events appears to be straightforward, a number of models describe situations where trade could have little impact on relative wages, or even where the above effects could be

[1] From Cline (1997), p. 43. Originally cited in Deardorff and Stern (1993).

reversed. For example, in the above scenario the reduction of low-skilled wages in the developed economy could lead to a "factor-intensity reversal," where goods which were previously high-skill intensive switch production processes to become low-skill intensive, and under certain circumstances, the resultant increase in low-skilled wages could decrease inequality. Moreover, even if the basic implications of the HOS theory do hold, it is unclear if the effects would be large and/or significant (even after incorporating the "magnification effect" which shows that a change in relative wages is greater than the underlying change in relative prices.) Therefore, the debate on how increased trade between developed and developing countries has affected wages and inequality boils down to an empirical question. Economists have utilized an amazing potpourri of methods, samples, and techniques in an attempt to answer this question, but the bulk of this work can be broadly categorized into two approaches: analysis based on changes in goods prices or changes in factor demands and supplies.

The first approach focuses on the Stolper-Samuelson implication that any change in relative wages and inequality is initially generated by changes in relative goods prices. According to this argument, in order to measure the impact of trade on wages, we should focus on changes in the relative prices of goods rather than on the actual volumes of goods traded. As specified in the above theoretical work, changes in relative goods prices will capture any structural changes resulting from increased trade, even if the actual change in trade patterns is small. To take this argument to the extreme, even the threat of increased imports could have a large impact on the competitive structure of an economy and change relative prices and wages, even if the actual trade flows never materialize. Therefore, this approach argues that to accurately measure how trade liberalization affects wages, we need to measure how relative goods prices change, and how, in turn, these price changes affect relative wages. An implicit assumption of most of this work is that changes in factor prices are mainly derived from changes in goods prices instead of by any (potentially endogenous) changes in factor supplies or technology.

Numerous empirical papers have used this price approach to measure the impact of trade on wages. The results vary from outright rejection of the HOS theory to moderate support.[2] Lawrence and Slaughter find evidence that the relative price of low-skill intensive manufacturing goods in the U.S. have actually been rising (relative to the price of high-skill intensive goods), thereby rejecting the HOS predictions. Sachs and Shatz (1994) conclude that increased net imports did lower relative prices in low-

[2] The cites referred to in the following paragraph are: Lawrence and Slaughter (1993), Sachs and Shatz (1994), Krugman (1995), and Leamer (1992).

skilled manufacturing goods, and that this exerted some pressure on low-skilled wages and employment, but that the aggregate impact was not enough to account for a significant portion of widening inequality. Krugman uses a very different approach, but arrives at a similar conclusion. He focuses on what the prices of tradeables (and therefore wages) would have been if trade hadn't opened up (instead of what they actually are.) He concludes that relative prices only needed to adjust by a small amount, since new trade volumes are a relatively small proportion of total trade, so that only a small part of the observed changes in relative wages results from changes in trade. Leamer finds similar results over some time periods, but reports a large HOS effect during the 1970's and early 1980's. Specifically, he estimates that between 1972 and 1985, trade reduced U.S. unskilled wages by $1,000 and raised skilled wages by $6,000—obviously a significant effect.

The second general approach to testing how increased trade has affected relative wages focuses on changes in factor demands and supplies instead of changes in goods prices. This "factor-content of trade approach" argues that it is virtually impossible to obtain accurate measures of goods prices. This task is especially difficult because, according to the magnification effect of the HOS theorem, even small changes in goods prices can have large affects on factor prices. It is extremely difficult to differentiate these small changes from the random noise in prices. Equally important, adherents of this factor-content approach argue that the price approach ignores many factors affecting wages, such as changes in factor supplies or changes in the elasticity of factor demands. Therefore, instead of focusing on goods prices, they suggest estimating how changing patterns of trade influence effective factor supplies and factor demands, and then how changes in factor supplies, demands, and the elasticity of substitution impact relative factor prices.

Results obtained using this factor-content approach vary as much as those based on the price-change approach.[3] Bound and Johnson report that most of the change in the relative wages of low-skilled and high-skilled workers is due to technological change instead of trade. Borjas, Freeman, and Katz find that during the 1980's, the labor embodied in trade increased the effective supply of low-skilled labor by about 4-13%, which explains a small (although significant) fraction of the decline in their relative wage. Wood finds some of the strongest effects of trade on relative wages. He makes several adjustments to the standard analysis, such as using factor-requirements of the exporting country (instead of the importer) when calculating the factor-content embodied in trade flows, and finds a significant negative correlation between the change in import penetration from developing countries and the change in manufacturing employment. He argues that between one-third and two-thirds of the decreased demand for low-skilled workers resulted from increased trade between developed and developing countries.

While this brief summary barely touches on the vast amount of empirical work done on trade and relative wages, it does show the very different methodologies utilized and results obtained.[4] Despite this variety of techniques and results, however, some economists feel that a consensus has been reached. For example, Lawrence concludes a survey on this topic with the statement: "There is little support for those positions that ascribe a major role to this story [of increased wage inequality] to expanding trade."[5] Rodrik summarizes a literature review with the statement: "that international influences contributed about 20 percent of the rising wage inequality in the 1980s."[6] He goes on to admit, however, that although many people consider this contribution unimportant, twenty percent is not a small number. Numerous theoretical and empirical arguments have been proposed to explain why this effect may be so small, and each of these arguments has been countered with equally convincing claims.[7] Therefore, although many economists have come to believe that trade has had a small impact on wage inequality, the question is still largely unresolved. Moreover, given the ambiguous predictions of the theoretical models, this question must ultimately be answered by further empirical work.

III. A Modified Factor-Content-of-Trade Approach

The remainder of this paper addresses this empirical question. Most of the analyses discussed above have a limited focus and suffer from several critical shortcomings. This section will discuss some of these shortcomings and suggest how to address them in the panel estimation of a factor-content-of-trade model. More specifically, this section will develop a framework in which to measure the relationship between wages and various types of trade flows. It will discuss the need to carefully classify countries and trade flows by relative skill abundance, as well as to control for changes in trade flows with

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[3] The cites referred to in the following paragraph are: Bound and Johnson (1992), Borjas, Freeman and Katz (1992), Wood (1994) and Wood (1995).

[4] For much more detailed surveys of the empirical work done on this topic, see Cline (1997), Freeman (1995), Gottschalk and Smeeding (1997), Lawrence (1996), Richardson (1995), Rodrik (1996), and Slaughter and Swagel (1997).

[5] Lawrence (1996), p. 14.

[6] Rodrik (1996), p. 22.

similar countries, capital flows, the supply of skills, labor market rigidities, and other country-specific effects such as technological change. The empirical results in the following sections show that making these extensions and adjustments significantly changes estimates of the relationship between increased trade and wage inequality.

Virtually all of the empirical work on trade and wages has focused on this relationship in the U.S., with only a few papers examining the impact of trade in other OECD countries or developing countries. This focus is not surprising given that increased inequality is more of a concern in the U.S. than in other developed nations, and especially given that even in the data-intensive U.S. it is difficult to obtain information on all of the requisite variables. Moreover, extending this analysis to more than one country becomes immensely more difficult, because even if data on wages, production technologies and skill levels does exist, it is rarely comparable across countries. This paper, however, will take a different approach than traditionally followed in this literature. Instead of using dis-aggregated data to estimate the impact of one type of trade flow on workers in one country, it will use a more general framework to simultaneously estimate the various channels and effects predicted by the HOS theory. In other words, it will attempt to estimate how trade between countries of all skill levels impacts low-skilled wages, high-skilled wages, and wage inequality in both high-skill abundant and low-skill abundant countries. A disadvantage of this approach is a loss of specificity in the data available for the analysis, but a major advantage is the ability to compare results across countries and thereby develop a more complete picture of how increased trade is related to wage inequality around the world.

A major complication with this more generalized, global approach is how to categorize trade flows. While it is a fairly simple procedure to divide U.S. trade into that with similar or less-skilled countries, it is a much more difficult accounting exercise to sort out individual trade flows between each country in the world. Further complicating this formidable task is that over time the relative skill rankings of many countries have changed significantly. For example, in the late 1960's the East Asian tigers exported low-skill-intensive goods, while recently they export more "middle-skill" goods, and countries such as China and India, which exported very little in the 1960's, are quickly replacing them as exporters of low-skill goods. Even relative rankings with the U.S. can change quickly. Just twenty years ago Japan had wages one-third of those in the U.S. and trade with Japan would have been categorized as with a "low-skilled" nation. This is clearly not the case today. On a more positive note, the International Monetary Fund has compiled a data set that reports imports and exports between every country in the

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[7] See Cline (1997), Chp 2, for an excellent survey of these arguments and counter-arguments.

world in every year since about 1950. Therefore, although the accounting is still a formidable task, it is possible to sort and classify trade flows between countries over time.

But how should each trade flow be classified? According to the HOS theory described above, trade flows and countries should be categorized by their relative abundance of skilled and unskilled labor. Yet statistics on skill abundance only exist for a few countries. Many empirical studies solve this problem by using income levels as a proxy for skills. For example, one common method of classifying trade flows is by a country's ranking as "high-income" or "low-income" in the World Development Report (WDR).[8] A quick glance at these rankings, however, suggests that income rankings, especially when as broad as those utilized by the WDR, are not an accurate indicator of skill levels. For example, in 1990 "High-Income Economies" in the WDR included Saudi Arabia, Kuwait, and the United Arab Emirates—countries that had a high GNP per capita from oil revenues but levels of education lower than most "middle-income" economies. To address this problem, this paper uses several alternate measures of skill abundance. It not only classifies countries and trade flows by income rankings, but also by educational attainment and manufacturing wages.[9] The empirical results below show that correctly defining relative skill levels is important and can significantly affect estimates of the relationship between trade and wage inequality.

Another problem with past work examining trade and inequality is that it focuses only on trade between developed and developing countries and ignores trade between similar countries (i.e. between two developed or two developing countries.) Granted, according to the HOS theory, there would be minimal trade between countries with similar factor endowments. This is obviously not the case in the real world, however, where a majority of trade is between countries of comparable income levels. Moreover, trade between countries with the same relative skill abundance could affect relative wages by changing the elasticity of demand for labor, and especially for unskilled labor.[10] To use a concrete example, consider the impact of NAFTA on low-skilled labor in the U.S. According to the standard HOS analysis, the U.S. would import low-skill intensive goods from Mexico and export high-skill intensive goods, which would decrease the demand for low-skilled workers and lower their relative wage. This is shown in Figure I as an inward shift of the demand curve for low-skilled labor from A to B. At the same time, the increased effective supply of labor in the U.S. would increase the elasticity of demand for low-skilled workers (because firms have less power to raise wages and stay in business). This is represented

[8] For example, see Sachs and Shatz (1994).
[9] The exact ranking procedure is described in more detail in the section on data below.

in the figure by a flattening of the demand curve from A to C. The total impact of trade with Mexico on low-skilled wages in the U.S. would be the combination of these two effects, shown as the new demand curve D and wage W .

This is not, however, the end of the story. Under NAFTA, the U.S. simultaneously increased trade with Canada. Since the relative abundance of skills is similar in Canada and the U.S., this increased trade would not affect the position of the labor demand curve. The increased effective supply of low-skilled labor would, however, further increase the elasticity of demand for low-skilled labor and flatten the labor demand curve. This additional effect implies that after freeing trade with both Mexico and Canada, the demand curve for low-skilled labor in the U.S. was actually E instead of D, and the wage was W* instead of W '. Any analysis of the impact of increased trade with Mexico which ignores simultaneous changes in trade with Canada would overestimate the impact on low-skilled wages (measuring the impact as the change from W to W* instead of from W to W '.)

Few economists have corrected for (or even mentioned) this effect of increased trade on the elasticity of demand for labor. This is surprising. Not only is there strong empirical evidence that trade integration increases the elasticity of demand faced by domestic producers (as seen, for example, in a reduction of price-cost margins), but it is widely accepted that the demand for labor is largely a derived demand that is directly linked to elasticities in the product market. In one of the only attempts to measure these effects, Slaughter estimates that the demand for production labor in U.S. manufacturing has become more elastic over time, but the demand for non-production labor shows no clear trend and "the hypothesis that trade contributed to increased elasticities has mixed support, at best."[11] While the impact of trade on labor elasticities is therefore far from resolved, any analysis that omits the effect of increased trade between similar countries when estimating the impact of trade between dissimilar countries, could be biased. This is a valid concern since the volume of trade between developed countries has actually increased more than the volume of trade between developed and developing countries in the past twenty years.

Another concern with much of the empirical work on trade and wages is the assumption that either capital is immobile or that relative levels of capital remain constant in each country.[12] Authors

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[10] See Rodrik (1996) or Slaughter (1997) for a further explanation of this elasticity argument.
[11] Slaughter (1997).
[12] Note, however, that several papers do not make this problematic assumption and do attempt to control for changes in capital. For example, see Sachs and Shatz (1994) or Stokey (1996).

usually justify this assumption with the observation that capital and labor shares have remained fairly constant in western economies. Empirical evidence, however, suggests that capital is highly mobile, especially in the past decade, and capital shares in many developing economies have increased significantly. Evidence also suggests that capital is a complement to high-skilled labor and a substitute for low-skilled labor, so that changes in capital flows could significantly affect the relative return to skills. As a result, not controlling for capital flows could downward bias estimates of the impact of trade on inequality. For example, if a developing country increased trade with a developed country, wages of low-skill workers in the developing country would rise. Since capital is a substitute for low-skilled labor, the return to capital would also rise in the developing country and generate an inflow of capital. This would, in turn, lower unskilled wages and raise skilled wages, thereby muting the impact of increased trade on relative returns and inequality. The opposite would occur in the developed country, where capital would flow out, mitigating the decline in low-skilled wages and the increase in inequality.

Not only does much of the empirical work on trade and wages ignore changes in capital, but it often omits adjusting for changes in the relative supply of skills. While some work based on the factor-content-of-trade approach does make this adjustment, most work based on the price-change approach argues it is not necessary because factor prices are mainly derived from goods prices. This omission, however, could bias estimates of the effect of trade on inequality. For example, according to the HOS theory, increased trade between developed and developing countries has increased high-skilled wages in developed countries and lowered them in developing countries. But at the same time, educational attainment and the relative abundance of high-skilled workers has increased in most of the world. If all else remained constant, this supply change would have reduced the relative wages of high-skilled workers in both types of countries, counteracting the Stolper-Samuelson effect in the developed country and augmenting it in the developing country. Therefore, ignoring this supply change that occurred simultaneously with increased trade would lead us to underestimate the impact of trade on inequality in the developed country and overestimate it in the developing country.

Another factor that is often omitted in empirical analysis of the impact of trade on relative wages is labor market institutions. Several recent papers argue that labor market structure and changes in this structure are critical determinants of the cross-country relationship between trade and wages.[13] Specifically, countries such as the U.S. and U.K. have reformed and deregulated their labor markets, so that their labor markets are fairly flexible. As a result, any change in relative labor demands is quickly

[13] For example, see Fortin and Lemieux (1997), Gottschalk and Smeeding (1997) and Topel (1997).

absorbed by a change in relative wages. Other European countries, however, did not undergo these reforms, so their labor markets are much more rigid. As a result, a similar change in relative labor demands is absorbed by a change in the unemployment rate (which has a higher proportion of low-skilled workers) instead of relative wages. Therefore, if trade decreases the relative demand for low-skilled workers by the same amount in each of these countries, it would generate increased wage inequality in the U.S. and U.K., but have little impact on inequality in the rest of Europe. An analysis that does not control for differences in labor market structures would underestimate the impact of trade on the relative demands for high-skilled and low-skilled labor.

This omission of controls for differences in labor market structure is actually part of a larger problem with most cross-country empirical work: omitted variable bias. If any variable affects relative wages and is correlated with trade, but is not otherwise controlled for in the analysis, then estimates of the effect of trade on wages will be biased and inconsistent. Some variables that could potentially cause this problem are: institutions, preferences, measurement techniques, or skill-biased technological change. There are two methods of adjusting for this omitted variable bias. One is to control for each of these country-specific variables. This is clearly not feasible—especially for a large number of very different countries. The other method is to utilize panel estimation to calculate a constant term or "fixed effect" for each country. This technique controls for any country-specific differences that remain constant across time (although it still can not correct for any differences that change across periods). This has not been done yet (to my knowledge) in any analysis of the impact of trade on relative wages, undoubtedly because it not only requires information on each of the variables across countries, but also across time.

To summarize, this section has discussed a number of variables which are frequently omitted from analyses of the effect of trade on inequality, but which could significantly influence the estimated relationship between these two variables. In particular, it explained why it is necessary to control for trade between similar countries, capital flows, the supply of skills, labor market rigidities, and any other country-specific effects. It also discussed the general approach of this paper: to use a factor-content-of-trade framework to simultaneously estimate how trade with relatively higher and lower skill countries affects wage inequality in both high-skill abundant and low-skill abundant countries. To incorporate all of these variables and effects in this generalized framework, I begin by estimating a reduced-form model. Although this approach lacks a strong micro-foundation, it is a useful indication of any significant relationships between relative wages, trade flows, and any of the other variables discussed above.

Specifically, this reduced form model which I estimate below is:

$$INEQ_{it} = \frac{WGHISK_{it}}{WGLOSK_{it}}$$
$$= \alpha_i + \beta_1 TRHISK_{it} + \beta_2 TRLOSK_{it} + \beta_3 TRSIMIL_{it} + \beta_4 CAP + \beta_5 SKILL_{it} + \beta_6 RIGID_{it} + \varepsilon_{it} \qquad (i)$$

Where *i* represents each country and *t* represents each time period; $INEQ_{it}$ is inequality for country *i* at time *t*; $WGHISK_{it}$ and $WGLOSK_{it}$ are wages for high-skilled and low-skilled workers, respectively; α_i is the country-specific effect; $TRHISK_{it}$ is total trade with relatively higher-skilled countries in a low-skill abundant country *i*; $TRLOSK_{it}$ is total trade with relatively lower-skilled countries in a high-skill country *i*; $TRSIMIL_{it}$ is trade with countries of similar skill-abundance (i.e. trade other than that counted in $TRHISK_{it}$ and $TRLOSK_{it}$); CAP_{it} is the capital stock; $SKILL_{it}$ is a measure of relative skills; $RIGID_{it}$ is a measure of labor market rigidity; and ε_{it} is a randomly distributed error term for country *i* during period *t*.

IV. The Data and Classification Procedures

Before estimating this reduced-form model, it is necessary to specify the data used to measure each of these variables. Some of the statistics, such as the capital stock, are fairly straightforward and are taken directly from standard data sets. Others, such as trade flows with relatively lower-skill-abundant countries, are more complicated, and must be calculated using a combination of data sources. The following section discusses the statistics used to measure each of these variables in order of increasing complexity.

The statistic used to measure the capital stock (CAP_{it}) is the most straightforward. I utilize the ratio of real domestic investment (private plus public) to real GDP as reported in Barro and Lee (1997). My measure of labor market rigidity ($RIGID_{it}$) is only slightly less straightforward. Since there are no internationally comparable measures of labor market structures across countries over time, I simply utilize the unemployment rate reported in the *Statistical Yearbook* published by the United Nations.

Finding data on relative wages is more difficult. While it is possible to obtain data on manufacturing wages or non-agricultural wages for a number of countries, there are no statistics on relatively high- and low-skill wages that are comparable across countries and periods. Therefore, I utilize a narrower definition of high- and low-skill workers. As a proxy for high-skill wages ($WGHISK_{it}$), I average gross annual income for engineers and for skilled industrial workers, and as a proxy for low-skill wages ($WGLOSK_{it}$), I use gross annual income for unskilled or semi-skilled laborers. Inequality, or the relative return to skills, is calculated as the ratio of these two wages. The annual incomes used to calculate these statistics are reported by the Union Bank of Switzerland in *Prices and Earnings Around the Globe* for about fifty countries.[14]

Information on the relative supply of high- and low-skilled workers is available for only a fraction of these countries, but data on educational attainment is widely available and relatively comparable across countries. Some studies therefore suggest using the data on educational attainment combined with observations for a few countries on skill abundance to posit a relationship between these two variables and interpolate the relative supply of skilled workers for most countries in the world.[15] This procedure is very rough, however, since the interpolation generally uses three points to draw two lines, and even these three points are of dubious accuracy and comparability. Moreover, the interpolation is completely unrealistic for developing countries, on which we have no reliable data on relative skill abundance. Therefore, I simply use average years of total education in the population aged over 15, as reported in Barro and Lee (1996), to measure relative skill abundance.

By far, the most difficult statistics to obtain and categorize were those on trade flows. I began with an International Monetary Fund database (that is currently only available in a very rough form from the ICPSR) which reports imports and exports to and from every country in the world annually since about 1950. Then I labeled each country in each year as high-skill abundant or low-skill abundant. Next I considered the flow of imports and exports from each country to each of its partners and categorized each trade flow as with a relatively more-skilled or less-skilled country. Finally, I aggregated these flows for each country, calculating the total amount of trade each country carried out with relatively higher- and lower-skilled partners in each year.

While this procedure was mostly an elaborate accounting exercise, the greatest difficulty arose in how to define countries' skills, not only in terms of their absolute ranking as high- or low-skilled, but also in terms of the relative rankings for each of the trade flows. As mentioned above, many empirical

[14] The Union Bank of Switzerland also reports incomes for other types of workers. The sensitivity analysis below will show that the central results are robust to these alternate measures of skilled and unskilled wages.

studies address this problem by using income rankings as reported in the *World Development Report*. As also discussed above, however, these rankings are highly problematic. Not only is income a poor proxy for skills in many countries (such as the oil economies in the Middle East or the transition economies in Eastern Europe), but also the simple ranking of "high-income" or "low-income" is so broad that it loses many important cross-country differences. There is clearly no easy solution to this problem. Therefore, I utilize three different statistics to categorize countries and trade flows.[16] Although this further complicates the already laborious accounting of trade flows and countries, it does allow a comparison of how different definitions of skill abundance affect estimates of the relationship between trade and wages.

The first categorization procedure repeats the technique frequently used in previous work on this topic—classifying countries according to their income rankings in the *World Development Report*. The World Bank labels each country as high income, middle income or low income in each year. To define absolute skill levels, I follow the standard procedure and consider high-income countries as high-skill abundant, and middle- and low-income countries as low-skill abundant. To define relative skill levels, I classify trade with a country in a higher-income group as trade with a relatively high-skilled nation and trade with a lower-income group as with a relatively low-skilled nation. Note, however, that exports from a middle-income country to a low-income country would be considered trade from higher-skill to a lower-skill nation, even though both are considered low-skill in absolute terms.

Instead of focusing on income levels, the second categorization method uses average years of total education in the population aged over fifteen as a proxy for skills.[17] In absolute terms, a country is considered high-skill if its average education is greater than the mean plus one-quarter of a standard deviation (for the entire sample.) A country is considered low-skill if its average education is less than the mean minus one-quarter of a standard deviation.[18] In relative terms, trade flows are classified as trade with a higher-skill country if the partner's average education is more than 25% greater than in the

[15] For example, see Cline (1997), Chapter 4.

[16] In each case, if the relevant statistic is not available for a country in a given year, I use the average value of that statistic for all "similar" countries, where "similar" is defined according to how the country is classified in the IMF Trade data. Specifically, the eight groupings utilized are: Africa, Asia, Former Communist/Soviet Economies (including Eastern Europe), Industrial Countries, Latin American Mainland (including Central and South America), Latin American Islands, Middle East, and the Pacific Islands.

[17] From Barro and Lee (1996).

[18] This apparently random formula is used so as to balance the need of having a distinct difference between skill levels in the two groups, with the need to keep the sample size in each group large enough to obtain meaningful results. The sensitivity analysis will show that adjusting this formula will not have a significant effect on the results.

originating country, and trade is classified as with a lower-skill country if the partner's average education is less than 25% lower than in the originating country.

The third categorization method utilizes average wages in the manufacturing sector as a proxy for skills.[19] In absolute terms, a country is considered high-skill if its average wage is greater than the mean wage plus one-quarter of a standard deviation (for the sample). A country is considered low-skill if its average wage is lower than the mean wage less one-quarter of a standard deviation. In relative terms, trade flows are classified as trade with a higher-skill country if the partner's average wage is more than 25% greater than in the originating country, and trade is classified as with a lower-skill country if the partner's average wage is at less than 25% lower than in the originating country.

Once this accounting and aggregation procedure is complete, each of the three classification techniques yields three variables: $TRHISK_{it}$ (total trade with relatively higher-skilled countries in a low-skill abundant country *i*), $TRLOSK_{it}$ (total trade with relatively lower-skilled countries in a high-skill country *i*), and $TRSIMIL_{it}$ (trade with countries of similar skill abundance, i.e. all trade not included in $TRHISK_{it}$ and $TRLOSK_{it}$.) Each of these variables is then divided by GDP in the home country. A final point about these variables is that they are aggregate trade flows and are not converted into measures of the actual skills embodied in those flows. Although this conversion would be useful on theoretical grounds, it is difficult for two practical reasons. First, the issue of what production technology (that of the importer or exporter) to utilize for such a conversion is still highly debated, and as shown by Wood and discussed above, this choice can have a significant impact on results.[20] Second, to the best of my knowledge, the data required to make these calculations is not available. For example, I would need information on the average skill content of exports from each country, and given the difficulty in even finding a measure of skills, this information is simply not available for most of the countries in the sample.

After compiling these statistics on trade flows, capital stocks, labor market rigidities, wages and skills, it is necessary to make one final modification to the data set. In order to use panel estimation and calculate the country-specific effect (the α_i), I divide the data into five-year periods. Due to data

[19] This data was specially obtained from Martin Rama at the World Bank. It will be released in 1998 as part of a new data set on wages, labor institutions, and employment around the world. Rama obtained these statistics from the International Labour Office. This data is not used in other parts of the analysis (such as for the dependent variable) since it is only available starting in 1985.

[20] See Wood (1994) and (1995).

availability, it is only possible to estimate four periods: 1980, 1985, 1990, and 1995. Each of the dependent variables in equation (i) is taken from the five-year period preceding the date of the independent variable. This lagged timing structure is utilized for two reasons. First, not only could trade affect relative wages, but relative wages might affect export production and trade patterns. By lagging the right-hand side variables such as trade, this should minimize any feedback effect and potential simultaneity.[21] Second, any impact of trade, the supply of skills, or the capital stock is probably not immediate and may take several years before being fully reflected in wages. Measuring each of the independent variables at the earlier date will allow for this lagged adjustment period.

This final data set includes 42 countries, 4 periods and 152 observations. Table I reports sources, exact dates and detailed definitions for each of the variables. Table II lists the high-skill abundant and low-skill abundant countries according to each of the three different skill categorizations. Some countries, such as the U.S. and Germany are consistently classified as high-skill abundant, and others, such as Brazil and India are consistently classified as low-skill abundant. Other countries, however, switch between high-skill and low-skill depending on the year and/or the definition utilized. Some of these shifts are not surprising. For example, Hungary is ranked high-skill according to educational attainment but low-skill according to income or wages. Similarly, Korea is ranked low-skill by income level, high-skill by educational attainment, and low-skill by wages--but only until 1985. Other switches are more surprising. For example, France is high-skill when ranked by income or wages, but not by education. This suggests that even variables such as educational attainment are subject to measurement error. Taken as a whole, this chart shows the difficulty in using any single measure as an indication of a country's relative skill-abundance, and why estimates of the impact of trade on wages may be sensitive to the skill categorization utilized.

V. Estimation Results: The Reduced Form

Now that the data set and trade variables have been constructed, it is possible to estimate the reduced-form model predicting wage inequality (equation i). Results obtained using fixed effects and random effects are reported in Table III. A Hausman specification test rejects random-effects in each of

[21] Simultaneity is still a potential concern. This will be addressed in more detail Section VI.

the three cases (at any standard significance level), so in the discussion that follows, I focus on the fixed-effects results.[22]

Although most of the estimated coefficients are not significant (at the five percent level), most signs agree with the predictions discussed in section III. Focusing first on the non-trade variables, higher levels of capital have a positive (and occasionally significant) relationship with wage inequality. This supports the assumption that capital is a complement to high-skilled labor and a substitute for low-skilled labor. The negative (although insignificant) coefficient on labor market rigidities suggests that countries with less flexible labor markets tend to have lower levels of inequality. This also supports our priors that in more rigid labor markets, demand shocks will be absorbed mostly by changes in unemployment and have less of an impact on wage inequality. The coefficients that do not agree with our priors are those on skills. A higher abundance of skilled workers appears to have a positive, instead of negative, relationship with wage inequality. This could suggest that: strong positive externalities exist between skilled workers, the relationship between skills and wage inequality is non-linear, or educational attainment is not a good proxy for relative skill levels. These issues will be examined in the sensitivity analysis below.

Turning next to the coefficients on the trade variables, these estimates are more dependent on the skill categorization utilized. In high-skill abundant countries, increased trade with lower-skill countries has a positive relationship with wage inequality. This relationship is highly significant when country skill levels are ranked according to education and wages, but not significant when ranked by income. In low-skill abundant countries, increased trade with higher-skill countries has a negative relationship with wage inequality when countries are ranked by income and wages, but a positive relationship when countries are ranked by education. None of these is significant except that based on wages (which is highly significant.) Trade between similar countries has a positive relationship with inequality (as expected) in two of the three cases, but none of the three estimates is significant.

Taken as a whole, the coefficients on the trade variables suggest several key points. First, trade with relatively lower-skill countries may have a significant positive relationship with wage inequality, supporting the basic HOS predictions. Using the intermediate estimate (in column 2), if a high-skill country increased trade/GDP with relatively lower-skill countries by 0.10, this is correlated with an

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[22] The null hypothesis is the independence of the country-specific effect and the other regressors. The $\chi^2(6)$ test statistics are: 31.6, 36.2 and 85.4 for the estimates using income, education, or wages, respectively, as a measure of skills.

increase of 1.4 in the ratio of relative wages over the next five years.[23] Second, trade with relatively higher-skill countries has less impact on wage inequality. The significant negative estimate (based on wage levels) supports the HOS predictions, but the positive (although insignificant) estimate based on education contradicts this. Third, trade with similar countries does not appear to have a significant relationship with inequality, suggesting that any elasticity effect of trade with similar countries is small. Fourth and finally, the definition of skill used to categorize countries can have a significant impact on coefficient estimates.

Since these results are central to this paper, I do a fairly detailed sensitivity analysis to see if coefficient estimates are robust to changes in the sample, variable definitions and model specification. I begin by testing for the effect of removing outliers. I reestimate equation (i), removing one country at a time and then removing the five observations farthest above and below the country mean for each variable. Then, since the skill categorization of trade flows based on education and wages uses somewhat random divisions (trade is considered to be relatively more or less skill abundant if skills differ by 25% or more), I reclassify trade flows using a 10% or 50% division. Next, I try several different definitions of the supply of skills[24] or include a time trend or period dummies to test if the relationship between trade and wages changes over time. Finally, to test for the effect of model specification, I drop one variable at a time (except for the trade with higher- and lower-skilled countries). In each of these robustness tests, trade with lower-skilled countries continues to have a highly significant positive relationship with wage inequality. A sample of these estimates is reported in Table IV.

This analysis measures how increased trade is related to wage inequality. Focusing on this ratio of high-skilled to low-skilled wages, however, could overlook important information on exactly how trade flows affect inequality. Does inequality increase mostly because low-skilled wages fall? Or because high-skilled wages rise? Or could both wages rise or fall simultaneously, so that there is little impact on relative wages, but a substantial impact on absolute wages? To isolate these effects, I repeat the above analysis, but replace the dependent variable in equation (i) (previously wage inequality) with either high-skilled or low-skilled wages. Results are reported in Table V. Most of the coefficient

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[23] These are realistic magnitudes. For the ratio of trade with lower-skilled countries/GDP, 0.10 is the difference in this ratio between Germany and Japan in 1985. For the wage inequality ratio, 1.4 is the difference in this ratio between Argentina and the Netherlands in the same year.

[24] For example, I replace years of total education with the ratio of enrollment in higher education to enrollment in primary education or the ratio of the number of years of total education to the number of years of primary education. I also test for non-linearities in the relationship between education and wages by adding squared and/or cubed terms for total education.

estimates are insignificant, confirming not only that a wide variety of factors influence absolute wage levels, but that it is difficult to explain a substantial degree of wage changes without more detailed information and more accurate statistics.

Despite this lack of explanatory power, Table V does show several results which merit comment. In columns 1-3, where the dependent variable is high-skilled wages, only one of the coefficients on trade with higher- or lower-skilled nations is significant. This significant coefficient does support the HOS prediction that trade with higher-skilled nations can reduce high-skilled wages in low-skill countries, but we can not place much confidence in the estimate since when skills are categorized by either of the other two methods, the sign is reversed. In columns 4-6, where the dependent variable is low-skilled wages, the regressions have greater explanatory power and the trade variables show more consistent patterns. Trade with relatively higher-skilled countries does have a positive (although insignificant) relationship with low-skilled wages. Trade with relatively lower-skilled countries has a significant negative relationship with low-skilled wages (except when skill divisions are based on income, in which case the sign is still positive but the coefficient is not significant.) These results support the HOS predictions. Moreover, the effect of increased trade with lower-skill countries on low-skilled wages could be substantial. Taking the middle estimate (column 5), if a relatively high-skill country increased trade/GDP with relatively lower-skill countries by 0.10, this is correlated with 26% decrease in low-skilled wages over the next five years.

To summarize, these estimates of the reduced-form relationship between inequality and trade lend mixed support to the HOS theory. The HOS predictions which have the strongest support, however, are those which motivated this paper. Increased trade with relatively low-skill countries does appear to have a significant positive relationship with inequality. Moreover, this positive relationship is driven more by the negative relationship between increased trade and low-skill wages, than on any relationship between trade and high-skill wages. This suggests that in wealthy countries, the concerns of low-skilled workers may be justified. Increased trade with developing countries may have generated the significant decline in their relative wage.

VI. Estimation Results: A General Equilibrium Model

There is one significant problem with the model and estimates of last section: endogeneity. While the estimated coefficients do show a set of relationships between trade and relative wages, they do not indicate the direction of causality. For example, consider the estimates with wage inequality as the dependent variable. For a high-skill country, increased trade with low-skill countries might increase inequality. On the other hand, if wage inequality increased in the high-skill country, its comparative advantage would deteriorate (since the relative cost of producing the high-skill intensive good would increase). This would reduce trade flows with the low-skill abundant country, partially counteracting the impact of trade on relative wages. Not controlling for this simultaneity could downward bias estimates of the impact of trade on relative wages.

The ideal solution is to instrument for trade flows. Unfortunately, no good instrument exists. For example, the best proxy for trade flows, and especially how these flows have changed since 1970, would be a measure of trade barriers. So many barriers to trade are non-quantifiable, however, that they are extremely difficult to measure, and of the few data sets that do try to measure them, none cover enough years for panel estimation. Moreover, even if data on trade barriers did exist, this would capture only part of the reason for increased trade flows in the past two decades. Lower transport costs, such as the "container revolution," may have had just as substantial an impact on trade flows as the reduction in tariffs and quotas. Potentially even more important than either of these changes is the shift of many developing countries toward outward-orientation instead of import-substitution (especially in large economies such as India and China.) This sort of shift would obviously be difficult to capture in any consistent cross-country measure. Therefore, although instrumenting for trade flows would be the most satisfactory method of avoiding endogeneity in equation *(i)*, there is simply no good instrument for changes in trade flows.

An alternate, although more complicated, solution is to take a general-equilibrium approach and formulate a system of equations linking trade and wages. Although this task is complicated by the lack of good instruments for trade flows and wages, it is possible to combine the predictions of the HOS model with general macroeconomic theory to construct an identified system. In order to construct this system of equations, I begin by dividing trade flows into imports and exports between high-skilled and low-skilled countries (utilizing each of the three skill-categorizations discussed above.) Then I base the model on four sets of relationships.

First, the HOS predictions hold. An increase in imports will lower the returns to the factor used intensively in the imports and an increase in exports will increase the returns to the factor used intensively in the exports. There is no effect of an increase in imports or exports on the returns to the factor that is not used intensively in the relevant imports or exports. Second, wages affect national income and aggregate spending on imports. When either high-skilled or low-skilled wages increase, this raises income and spending, and therefore increases both high-skill intensive and low-skill intensive imports. Third, factor costs affect production quantities. When the price of a factor rises, this decreases exports of the good intensive in that factor and increases imports of the good intensive in that factor. There is no effect on the exports or imports of the good that does not use that factor intensively. Fourth and finally, since the "elasticity-effect" of increased trade with similar countries is insignificant in Section IV and in the estimates of Slaughter (1997), I ignore this effect and focus only on trade between countries of different relative skill abundance.

Combining these relationships and sets of assumptions with the concepts discussed in Section III, it is possible to write a six-equation general equilibrium model linking trade and wages. The six equations are:

$$
\begin{aligned}
&(1)\ WGLOSK_{it} = \alpha_i^1 + \beta_1 IMLOSK_{it} + \beta_2 EXLOSK_{it} + \beta_3 CAP_{it} + \beta_4 SKILL_{it} + \beta_5 RIGID_{it} + \varepsilon_{it}^1 \\
&(2)\ WGHISK_{it} = \alpha_i^2 + \beta_6 IMHISK_{it} + \beta_7 EXHISK_{it} + \beta_8 CAP_{it} + \beta_9 SKILL_{it} + \beta_{10} RIGID_{it} + \varepsilon_{it}^2 \\
&(3)\ IMLOSK_{it} = \alpha_i^3 + \beta_{11} WGLOSK_{it} + \beta_{12} WGHISK_{it} + \beta_{13} DEF_{it} + \beta_{14} ER_{it} + \beta_{15} CLOSE_{it} + \varepsilon_{it}^3 \\
&(4)\ IMHISK_{it} = \alpha_i^4 + \beta_{16} WGLOSK_{it} + \beta_{17} WGHISK_{it} + \beta_{18} DEF_{it} + \beta_{19} ER_{it} + \beta_{20} CLOSE_{it} + \varepsilon_{it}^4 \\
&(5)\ EXLOSK_{it} = \alpha_i^5 + \beta_{21} WGLOSK_{it} + \beta_{22} ER_{it} + \beta_{23} CLOSE_{it} + \varepsilon_{it}^5 \\
&(6)\ EXHISK_{it} = \alpha_i^6 + \beta_{24} WGHISK_{it} + \beta_{25} ER_{it} + \beta_{26} CLOSE_{it} + \varepsilon_{it}^6
\end{aligned}
$$

Where i represents each country and t represents each time period; $WGHISK_{it}$ and $WGLOSK_{it}$ are wages for high-skilled and low-skilled workers, respectively; α_i is the country-specific effect; $IMLOSK_{it}$ is total imports from relatively lower-skilled countries into high-skill abundant country i; $IMHISK_{it}$ is total imports from relatively higher-skilled countries into low-skill abundant country i; $EXLOSK_{it}$ is total exports to relatively lower-skilled countries from high-skill abundant country i; $EXHISK_{it}$ is total exports to relatively higher-skilled countries from low-skill abundant country i; CAP_{it} is the capital stock; $SKILL_{it}$ is a measure of total skills; $RIGID_{it}$ is a measure of labor market rigidities; DEF_{it} is the government budget deficit as a share of GDP; ER_{it} is the exchange rate of the local currency in terms of U.S. dollars;

$CLOSE_{it}$ is a proxy for how closed the economy is to trade (measured as total taxes on imports and exports over total trade); and ε_{it} is a randomly distributed error term for country i during period t. Most variable definitions and sources are identical to those used above and are summarized in Table I. Additional variables are summarized in Table VI.

The derivation of each of these equations is fairly straightforward. Equations (1) and (2) combine the HOS predictions with the reduced-form equation *(i)* used above. Therefore, according to the HOS assumptions, we would expect: $\beta_1<0$, $\beta_2>0$, $\beta_6<0$, $\beta_7>0$. If capital is a substitute for unskilled labor and a complement to skilled labor, we would predict: $\beta_3<0$, $\beta_8>0$. As the relative supply of skilled labor increases, high-skilled wages should fall and low-skilled wages should rise, so: $\beta_4>0$, $\beta_9<0$. More rigid labor markets should generally increase wages, with the greatest effect on low-skilled labor and little effect on high-skilled wages: $\beta_5>0$, $\beta_{10}\geq 0$.

Equations (3) and (4) combine the positive effect of income (i.e. wages) on imports, the production effects of changes in factor prices, and several other terms to identify the system. According to the income effect, we would expect: $\beta_{11}>0$, $\beta_{12}>0$, $\beta_{16}>0$, $\beta_{17}>0$. The production effect (that production will fall when the price of the factor used intensively in that good rises, generating an increase in imports of that good) will augment two of these income effects: $\beta_{11}>>0$, $\beta_{17}>>0$. I include a measure of the government budget deficit (*DEF*) to capture the fact that many governments (such as the U.S.) ran substantial deficits during this period, which could have increased imports, so that: $\beta_{13}>0$, $\beta_{18}>0$. The exchange rate captures any exogenous changes in the exchange rate, and since a depreciation would be expected to decrease imports: $\beta_{14}<0$, $\beta_{19}<0$. The *CLOSE* term proxies for the extent of government restrictions to trade, which would be expected to lower imports: $\beta_{15}<0$, $\beta_{20}<0$.

The final two equations of the system, equations (5) and (6), combine the production affect of changes in factor prices with several of the other relationships used to identify the system. According to the production effect, we would expect: $\beta_{21}<0$, $\beta_{24}<0$. The exchange rate should have the opposite impact of in equations (3) and (4) since a depreciation would increase exports: $\beta_{22}>0$, $\beta_{25}>0$. Last but not least, the CLOSE term would have the same effect as in equations (3) and (4), with: $\beta_{23}<0$, $\beta_{26}<0$.

Before estimating the entire system of equations, Table VII reports results if each equation is estimated independently—not instrumenting to control for potential endogeneity between wages and trade. Focusing on the wage equations, there is mixed support for the above predictions. No matter which skill division is utilized, increased imports from low-skill intensive countries have a significant negative relationship with low-skill wages—a relationship that is significant at the 1% level in two of the three cases. Increased exports from low-skill intensive countries have a positive (and significant in two of the three cases) relationship with low-skill wages. Less supportive of the above predictions are the equations predicting high-skill wages. Imports and exports generally have an insignificant impact on high-skill wages.

Next, Table VIII reports estimates of the entire system of equations, using the appropriate instruments so as to control for any endogeneity between wages and trade. Although many of the coefficient signs support the priors discussed above, almost none of the coefficient estimates are significant. This suggests that even in this general equilibrium framework with fairly stringent assumptions about the relationships between different trade flows and wages, it is extremely difficult to instrument for trade and wages. There is simply no variable or combination of variables that is highly correlated with changes in trade flows and consistently measured across countries and time periods. Reformulating the model to include additional variables, such as per capita income, growth, government consumption, the dependency ratio, the terms of trade, and/or the aggregate money supply, has little affect on the efficiency of estimates. Therefore, until better instruments for trade and/or wages are available, it is difficult to estimate the impact of trade flows on wages across time for a cross-section of countries.

VII. Summary

This paper began with a brief review of basic HOS trade theory which predicts that increased trade between developed and developing countries would increase wage inequality in wealthy countries and decrease inequality in poor countries. It then summarizes the wide variety of techniques used to test this theory, and the often contradictory results of these tests. Despite these mixed results, many economists have come to believe that increased trade between developed and developing countries has had a small impact on relative wages. This "consensus" is in sharp disagreement with the opinions expressed in the popular press and the fears of low-skilled workers in wealthy countries.

This empirical work, however, has several critical shortcomings. It tends to focus on a limited sample of countries, inaccurately classifies countries and trade flows as high- or low-skill intensive, and ignores several relevant variables such as trade with similar countries, capital flows, changes in the supply of skills, labor market structure and any other unmeasured country-specific characteristics. There are a variety of different methods of correcting for these problems. In particular, this paper focuses on several improvements. First, it more accurately classifies the absolute and relative skill abundance of different countries and the skill intensity embodied in various trade flows. To do this, it compiles a new data set on trade patterns, using income levels, average education, and manufacturing wages to classify countries and trade flows as relatively high-skill intensive or low-skill intensive. Second, it controls for a number of the factors ignored in previous work, such as capital flows, trade with similar countries, changes in the supply of skills, and labor market rigidities. Third, and perhaps most important, since it is impossible to control for all of the country-specific factors which can affect the relationship between trade and inequality, it uses a fixed-effects estimator.

Incorporating each of these improvements, this paper then estimates several different equations linking trade and relative wages. Reduced-form estimates explaining wage inequality are the most successful. In high-skill countries, increased trade with low-skill countries has a significant negative relationship with wage inequality. This negative relationship is largely driven by the negative relationship between trade and low-skill wages (instead of the positive relationship between trade and high-skill wages.) Due to the potential endogeneity in these reduced-form equations, however, it is impossible to isolate exactly how trade affects wages. To correct for this problem, I formulate a general-equilibrium model linking wages and trade flows. It is difficult to estimate the model, however, due to the lack of good instruments for trade and wages in a panel framework.

Therefore, although the reduced-form estimates suggest a strong relationship between trade and inequality, I am not able to identify the direct impact of increased trade on wages and inequality throughout the world. The reduced form estimates do suggest, however, that if Pablo, Fritz and Yingye are low-wage earners in a wealthy country, they should be concerned with how increased trade with developing countries is significantly related to the decline in their relative wages.

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Table I
Summary Information

Variable	*Definition*	*Source*	*Years*
CAP	Ratio of real domestic investment (private plus public) to real GDP (averaged over the given years) (SH 5.5)	B&L (1)	1970-75, 1975-80, 1980-85, 1985-90
DUMHISK	Dummy variable; equals 1 for a high-skill country and 0 otherwise	Calc.	1975, 1980, 1985, 1990
DUMLOSK	Dummy variable; equals 1 for a low-skill country and 0 otherwise	Calc.	1975, 1980, 1985, 1990
INCDV	Income classification. 1=Low income; 2=Middle income; 3=High income.	WDR	1978, 1980, 1985, 1990
INEQ	Ratio of WGHSK to WGLSK	Calc.	1982, 1985, 1991, 1994
RIGID	Percentage of unemployed in the total population	UN	1975, 1980, 1985, 1990
SKILL	Average years of total schooling in the total population aged over 15	B&L (2)	1975, 1980, 1985, 1990
TRHISK (when divided by INCOME)	DUMLOSK*Avg (trade with higher skilled partners / GDP); higher skilled if $INCDV_p > INCDV_c$	Calc.	1973-75, 1978-80, 1983-85, 1988-90
TRLOSK (when divided by INCOME)	DUMHISK*Avg (trade with lower skilled partners / GDP); lower skilled if $INCDV_p < INCDV_c$	Calc.	1973-75, 1978-80, 1983-85, 1988-90
TRHISK (when Divided by EDUC)	DUMLOSK*Avg (trade with higher skilled partners / GDP); higher skilled if $(SKILL_p/SKILL_c) > 125\%$	Calc.	1973-75, 1978-80, 1983-85, 1988-90
TRLOSK (when Divided by EDUC)	DUMHISK*Avg (trade with lower skilled partners / GDP); lower skilled if $(SKILL_p/SKILL_c) < 75\%$	Calc.	1973-75, 1978-80, 1983-85, 1988-90
TRHISK (when Divided by WAGE)	DUMLOSK*Avg (trade with higher skilled partners / GDP); higher skilled if $(WGMA_p/WGMA_c) > 125\%$	Calc.	1973-75, 1978-80, 1983-85, 1988-90
TRLOSK (when Divided by WAGE)	DUMHISK*Avg (trade with lower skilled partners / GDP); lower skilled if $(WGMA_p/WGMA_c) < 75\%$	Calc.	1973-75, 1978-80, 1983-85, 1988-90
TRSIMIL	Avg trade/GDP with "similar" countries, i.e. trade other than that included in TRHISK or TRLOSK	Calc.	1973-75, 1978-80, 1983-85, 1988-90
WAGE	Annual earnings in manufacturing for all workers, in 1987 $US	Rama	1980, 1985, 1990
WGHISK	Avg gross income per year of engineers and skilled industrial workers, in 1987 $US	UBS	1982, 1985, 1991, 1994
WGLOSK	Gross income per year of unskilled or semi-skilled building laborers, in 1987 $U.S.	UBS	1982, 1985, 1991, 1994

NOTE: Total sample is 42 countries and 4 periods.
SOURCES: B&L (1) is "Data Set for a Panel of 138 Countries" collected in Barro and Lee (1997).
B&L (2) is data set compiled in Barro and Lee (1996). Calc. means calculated for this paper. Rama is data compiled by Martin Rama at the World Bank. UBS is Union Bank of Switzerland, various years. UN is *Statistical Yearbook* published by the United Nations. WDR is the World Bank, *World Development Report* for the given year.

Table II
Country Classifications According to Different Skill Definitions

High-Skill Abundant Countries			*Low-Skill Abundant Countries*		
Divided by Income	***Divided by Education***	***Divided by Manuf. Wage***	***Divided by Income***	***Divided by Education***	***Divided by Manuf. Wage***
Australia	Argentina	Australia	Argentina	Bahrain (75-80, 90)	Argentina
Austria	Australia	Austria	Bahrain (75-80, 90)	Brazil	Brazil
Canada	Austria (85)	Bahrain (75-80, 90)	Brazil	Columbia	Egypt (75-80)
Cyprus (90)	Canada	Canada	Columbia	Egypt (75-80)	Hungary (90)
Denmark	Cyprus (85-90)	Columbia	Cyprus (85)	India	India
Finland	Denmark	Denmark	Egypt (75-80)	Indon. (75, 80-90)	Kenya (85-90)
France	Finland	Finland	Greece	Kenya (85-90)	Korea (75-85)
Germany	Germany	France	Hong Kong (75-85)	Malaysia (80-85)	Mexico (75-85)
Hong Kong (90)	Greece (80-90)	Germany	Hungary (90)	Mexico (75-85)	Netherlands (75)
Ireland	Hong Kong	Greece (90)	India	Portugal	Panama
Israel (90)	Hungary (90)	Hong Kong	Indonesia	Singapore (90)	Philippines
Italy	Ireland	Indon. (75, 80-90)	Israel (75-85)	South Africa	Portugal
Japan	Israel	Ireland	Kenya (85-90)	Thail. (75-80, 90)	Singapore (75-80)
Netherlands	Japan	Israel	Korea	Turkey (75-80)	South Africa (85)
Norway	Korea	Italy	Malaysia (80-85)	Venez. (75, 85-90)	Thail. (75-80, 90)
Singapore (90)	Netherlands	Japan	Mexico		
South Africa (75)	Nigeria (80-90)	Malaysia (80-85)	Nigeria (80-90)		
Spain (85-90)	Norway	Netherlands (80-90)	Panama		
Sweden	Panama (90)	Nigeria (80-90)	Philippines		
Switzerland	Sweden	Norway	Portugal		
United Kingdom	Switzerland	Spain (75-80, 90)	Singapore (75-85)		
United States	United Kingdom	Sweden	So. Africa (80-90)		
	United States	Switzerland	Spain (75-80)		
		Turkey (75-80)	Thail. (75-80, 90)		
		United Kingdom	Turkey (75-80)		
		United States	Venezuela		
		Venezuela (85)			

Table III
Regression Results: Reduced-Form Relationships with Wage Inequality

	Fixed Effects			*Random Effects*		
	Skill defined by INCOME (1)	*Skill defined by EDUC (2)*	*Skill defined By WAGE (3)*	*Skill defined by INCOME (4)*	*Skill defined by EDUC (5)*	*Skill defined by WAGE (6)*
CONST	-6.25*	-2.45	-10.11*	4.72*	6.21*	6.54*
	(2.42)	(2.64)	(3.92)	(1.06)	(1.05)	(1.49)
TRLOSK	4.29	13.96*	22.39*	10.12*	12.47*	10.81*
	(3.63)	(2.71)	(3.48)	(2.02)	(2.21)	(2.87)
TRHISK	-0.30	1.58	-10.82*	2.85*	-6.68*	1.14
	(2.35)	(4.59)	(3.05)	(1.04)	(3.20)	(1.03)
TRSIMIL	2.02	0.48	-0.83	-0.82	0.28	-0.32
	(1.07)	(1.02)	(0.90)	(0.52)	(0.34)	(0.52)
CAP	10.10*	4.68	6.12	-4.06	-3.95	-5.05
	(4.50)	(4.60)	(5.87)	(2.83)	(2.92)	(4.26)
SKILL	0.63*	0.38	1.53*	-0.17	-0.45*	-0.38*
	(0.22)	(0.24)	(0.38)	(0.10)	(0.11)	(0.15)
RIGID	-0.03	-0.05	-0.04	-0.03	-0.00	-0.06
	(0.06)	(0.06)	(0.07)	(0.05)	(0.05)	(0.06)
R^2	***0.41***	***0.40***	***0.58***	***0.40***	***0.32***	***0.28***
Countries	***42***	***42***	***42***	***42***	***42***	***42***
Observs	***152***	***152***	***124***	***152***	***152***	***124***

NOTES: Dependent Variable is INEQ (High-Skilled Wage/Low-Skilled Wage).
Standard errors in parentheses. * denotes significant at the 5% level.

Table IV
Sensitivity Analysis: Reduced-Form Relationships with Wage Inequality

	Standard Results (1)	*10% EDUC Divisions (2)*	*50% EDUC Divisions (3)*	*Add $SKILL^2$ (4)*	*Add Time Trend (5)*	*Add Period Dummies (6)*	*Drop TRSIMIL (7)*
CONST	-2.45	-4.03	-4.95	-7.07	0.08	-2.04	-1.83
	(2.64)	(2.33)	(2.69)	(3.52)	(2.83)	(3.03)	(2.49)
TRLOSK	13.96*	8.93*	11.36*	14.42*	15.51*	14.88*	14.29*
	(2.71)	(1.83)	(4.22)	(2.68)	(2.77)	(2.78)	(2.50)
TRHISK	1.58	-0.28	-5.92*	0.49	1.63	3.22	1.84
	(4.59)	(1.11)	(1.87)	(4.56)	(4.50)	(4.45)	(4.58)
TRSIMIL	0.48	0.52	4.51*	0.36	-0.21	0.44	
	(1.02)	(1.06)	(1.20)	(1.02)	(1.06)	(1.07)	
CAP	4.68	9.13*	5.36	2.83	7.34	8.50	3.89
	(4.60)	(4.32)	(4.84)	(4.63)	(4.79)	(4.80)	(4.53)
SKILL	0.38	0.43	0.50*	2.07*	-0.10	0.18	0.39
	(0.24)	(0.22)	(0.25)	(0.85)	(0.33)	(0.35)	(0.24)
RIGID	-0.05	-0.01	-0.10	-0.11	-0.10	-0.06	-0.08
	(0.06)	(0.07)	(0.08)	(0.07)	(0.07)	(0.08)	(0.07)
				-0.12*	0.40*		
				(0.06)	(0.19)		
R^2	***0.40***	***0.47***	***0.32***	***0.44***	***0.44***	***0.48***	***0.40***
Countries	***42***	***42***	***42***	***42***	***42***	***42***	***42***
Observs	***152***	***152***	***152***	***152***	***152***	***152***	***152***

NOTES: Dependent Variable is INEQ (High-Skilled Wage/Low-Skilled Wage). Skills classified by EDUC.
Estimates obtained using fixed effects. Standard errors in parentheses. * denotes significant at the 5% level.

Table V
Regression Results: Reduced-Form Relationships with High- and Low-Skill Wages

	Dependent Variable: High-Skill Wages			*Dependent Variable: Low-Skill Wages*		
	Skill defined by INCOME (1)	*Skill defined By EDUC (2)*	*Skill defined by WAGE (3)*	*Skill defined by INCOME (4)*	*Skill defined By EDUC (5)*	*Skill defined by WAGE (6)*
CONST	9.711*	9.383*	6.861*	10.411*	9.581*	8.475*
	(0.627)	(0.669)	(1.043)	(0.645)	(0.660)	(1.09)
TRLOSK	0.334	-0.917	0.073	-1.132	-2.612*	-2.695*
	(0.938)	(0.685)	(0.928)	(0.958)	(0.679)	(0.937)
TRHISK	0.070	0.021	-1.563*	0.348	0.009	0.189
	(0.609)	(1.154)	(0.811)	(0.625)	(1.10)	(0.827)
TRSIMIL	0.171	0.587*	0.206	-0.114	0.510*	0.419
	(0.277)	(0.260)	(0.240)	(0.285)	(0.257)	(0.249)
CAP	0.026	0.048	0.726	-2.065	-1.092	-0.431
	(1.165)	(1.166)	(1.564)	(1.194)	(1.145)	(1.632)
SKILL	-0.035	-0.019	0.329*	-0.140*	-0.096	0.024
	(0.057)	(0.062)	(0.102)	(0.058)	(0.061)	(0.106)
RIGID	-0.004	-0.009	0.009	-0.003	-0.003	0.016
	(0.016)	(0.016)	(0.020)	(0.017)	(0.016)	(0.020)
R^2	***0.04***	***0.07***	***0.22***	***0.14***	***0.22***	***0.18***
Countries	***42***	***42***	***42***	***42***	***42***	***42***
Observs	***152***	***152***	***124***	***152***	***152***	***124***

NOTES: Dependent variables in logs. Estimation technique is fixed effects.
Standard errors in parentheses. * denotes significant at the 5% level.

Table VI
Summary Information: Additional Variables

Variable	*Definition*	*Source*	*Years*
CLOSE	Proxy for how closed the economy is to trade; ratio of total taxes on imports and exports to total trade	W.B.	1971-75, 19765-80, 1981-85, 1986-90
DEF	Deficit; total government budget deficit as a share of GDP	W.B.	1971-75, 19765-80, 1981-85, 1986-90
ER	Exchange Rate; expressed as local currency per U.S.$	W.B.	1971-75, 19765-80, 1981-85, 1986-90
EXHISK (when divided by INCOME)	DUMLOSK*Avg (exports to higher skilled partners / GDP); higher skilled if $INCDV_p$>$INCDV_c$	Calc.	1973-75, 1978-80, 1983-85, 1988-90
EXLOSK (when divided by INCOME)	DUMHISK*Avg (exports to lower skilled partners / GDP); lower skilled if $INCDV_p$<$INCDV_c$	Calc.	1973-75, 1978-80, 1983-85, 1988-90
EXHISK (when Divided by EDUC)	DUMLOSK*Avg (exports to higher skilled partners / GDP); higher skilled if ($SKILL_p$/$SKILL_c$)>125%	Calc.	1973-75, 1978-80, 1983-85, 1988-90
EXLOSK (when Divided by EDUC)	DUMHISK*Avg (exports lower skilled partners / GDP); lower skilled if ($SKILL_p$/$SKILL_c$)<75%	Calc.	1973-75, 1978-80, 1983-85, 1988-90
EXHISK (when Divided by WAGE)	DUMLOSK*Avg (exports to higher skilled partners / GDP); higher skilled if ($WGMA_p$/$WGMA_c$)>125%	Calc.	1973-75, 1978-80, 1983-85, 1988-90
EXLOSK (when Divided by WAGE)	DUMHISK*Avg (exports to lower skilled partners / GDP); lower skilled if ($WGMA_p$/$WGMA_c$)<75%	Calc.	1973-75, 1978-80, 1983-85, 1988-90
IMHISK (when divided by INCOME)	DUMLOSK*Avg (imports from higher skilled partners / GDP); higher skilled if $INCDV_p$>$INCDV_c$	Calc.	1973-75, 1978-80, 1983-85, 1988-90
IMLOSK (when divided by INCOME)	DUMHISK*Avg (imports from lower skilled partners / GDP); lower skilled if $INCDV_p$<$INCDV_c$	Calc.	1973-75, 1978-80, 1983-85, 1988-90
IMHISK (when Divided by EDUC)	DUMLOSK*Avg (imports from higher skilled partners / GDP); higher skilled if ($SKILL_p$/$SKILL_c$)>125%	Calc.	1973-75, 1978-80, 1983-85, 1988-90
IMLOSK (when Divided by EDUC)	DUMHISK*Avg (imports from lower skilled partners / GDP); lower skilled if ($SKILL_p$/$SKILL_c$)<75%	Calc.	1973-75, 1978-80, 1983-85, 1988-90
IMHISK (when Divided by WAGE)	DUMLOSK*Avg (imports from higher skilled partners / GDP); higher skilled if ($WGMA_p$/$WGMA_c$)>125%	Calc.	1973-75, 1978-80, 1983-85, 1988-90
IMLOSK (when Divided by WAGE)	DUMHISK*Avg (imports from lower skilled partners / GDP); lower skilled if ($WGMA_p$/$WGMA_c$)<75%	Calc.	1973-75, 1978-80, 1983-85, 1988-90

NOTE: Total sample is 42 countries and 4 periods.

SOURCES: Calc. means calculated for this paper.
W.B. is "World*Data 1995" published by the World Bank and available on CD-Rom.

Table VII: General Equilibrium Model Estimated as Single Equations

Skills	$WGLOSK_{it}$ =	-8.82*	$IMLOSK_{it}$	+7.89*	$EXLOSK_{it}$	-1.84	CAP_{it}	-0.12*	$SKILL_{it}$	-0.00	$RIGID_{it}$
		(2.56)		(2.67)		(0.95)		(0.05)		(0.01)	
Defined	$WGHISK_{it}$ =	-0.05	$IMHISK_{it}$	-0.42	$EXHISK_{it}$	-0.37	CAP_{it}	-0.03	$SKILL_{it}$	-0.01	$RIGID_{it}$
		(0.51)		(0.91)		(0.97)		(0.05)		(0.01)	
By	$IMLOSK_{it}$ =	-0.02	$WGLOSK_{it}$	+0.04*	$WGHISK_{it}$	+0.17	DEF_{it}	+0.00	ER_{it}	-0.05	$CLOSE_{it}$
		(0.02)		(0.02)		(0.17)		(0.00)		(0.36)	
Income	$IMHISK_{it}$ =	+0.06	$WGLOSK_{it}$	-0.06	$WGHISK_{it}$	-1.04*	DEF_{it}	-0.00	ER_{it}	0.32	$CLOSE_{it}$
		(0.04)		(0.05)		(0.38)		(0.00)		(0.80)	
	$EXLOSK_{it}$ =	+0.01	$WGLOSK_{it}$	+0.00	ER_{it}	-0.19	$CLOSE_{it}$				
		(0.02)		(0.00)		(0.37)					
	$EXHISK_{it}$ =	-0.02	$WGHISK_{it}$	+0.00	ER_{it}	-0.09	$CLOSE_{it}$				
		(0.02)		(0.00)		(0.36)					
Skills	$WGLOSK_{it}$ =	-6.57*	$IMLOSK_{it}$	+3.31	$EXLOSK_{it}$	-1.72	CAP_{it}	-0.10*	$SKILL_{it}$	-0.01	$RIGID_{it}$
		(3.15)		(3.69)		(0.94)		(0.05)		(0.01)	
Defined	$WGHISK_{it}$ =	-0.46	$IMHISK_{it}$	-0.01	$EXHISK_{it}$	-0.37	CAP_{it}	-0.03	$SKILL_{it}$	-0.01	$RIGID_{it}$
		(0.79)		(1.14)		(1.00)		(0.05)		(0.01)	
By	$IMLOSK_{it}$ =	-0.00	$WGLOSK_{it}$	-0.00	$WGHISK_{it}$	+0.06	DEF_{it}	-0.00	ER_{it}	-0.17	$CLOSE_{it}$
		(0.01)		(0.01)		(0.05)		(0.00)		(0.10)	
Education	$IMHISK_{it}$ =	+0.04	$WGLOSK_{it}$	-0.06	$WGHISK_{it}$	-0.25	DEF_{it}	-0.00	ER_{it}	-0.20	$CLOSE_{it}$
		(0.03)		(0.03)		(0.29)		(0.00)		(0.61)	
	$EXLOSK_{it}$ =	-0.00	$WGLOSK_{it}$	-0.00	ER_{it}	-0.16	$CLOSE_{it}$				
		(0.00)		(0.00)		(0.09)					
	$EXHISK_{it}$ =	-0.02	$WGHISK_{it}$	-0.00	ER_{it}	-0.17	$CLOSE_{it}$				
		(0.02)		(0.00)		(0.39)					
Skills	$WGLOSK_{it}$ =	-7.42*	$IMLOSK_{it}$	+5.65*	$EXLOSK_{it}$	-1.27	CAP_{it}	-0.04	$SKILL_{it}$	+0.01	$RIGID_{it}$
		(1.92)		(1.76)		(1.19)		(0.08)		(0.02)	
Defined	$WGHISK_{it}$ =	-0.06	$IMHISK_{it}$	-0.37	$EXHISK_{it}$	+0.39	CAP_{it}	+0.20*	$SKILL_{it}$	+0.01	$RIGID_{it}$
		(0.57)		(0.90)		(1.21)		(0.08)		(0.02)	
By	$IMLOSK_{it}$ =	-0.02	$WGLOSK_{it}$	+0.04	$WGHISK_{it}$	+0.14	DEF_{it}	+0.00	ER_{it}	+0.23	$CLOSE_{it}$
		(0.02)		(0.02)		(0.19)		(0.00)		(0.49)	
Wages	$IMHISK_{it}$ =	+0.04	$WGLOSK_{it}$	-0.10	$WGHISK_{it}$	-1.52*	DEF_{it}	-0.00	ER_{it}	-0.42	$CLOSE_{it}$
		(0.07)		(0.07)		(0.59)		(0.00)		(1.49)	
	$EXLOSK_{it}$ =	+0.01	$WGLOSK_{it}$	+0.00	ER_{it}	-0.12	$CLOSE_{it}$				
		(0.02)		(0.00)		(0.53)					
	$EXHISK_{it}$ =	-0.06*	$WGHISK_{it}$	+0.00	ER_{it}	-0.61	$CLOSE_{it}$				
		(0.03)		(0.00)		(0.82)					

NOTES: Country-specific effects not reported. Robust standard errors. * Denotes significant at the 5% level. Sample size: 42 countries.

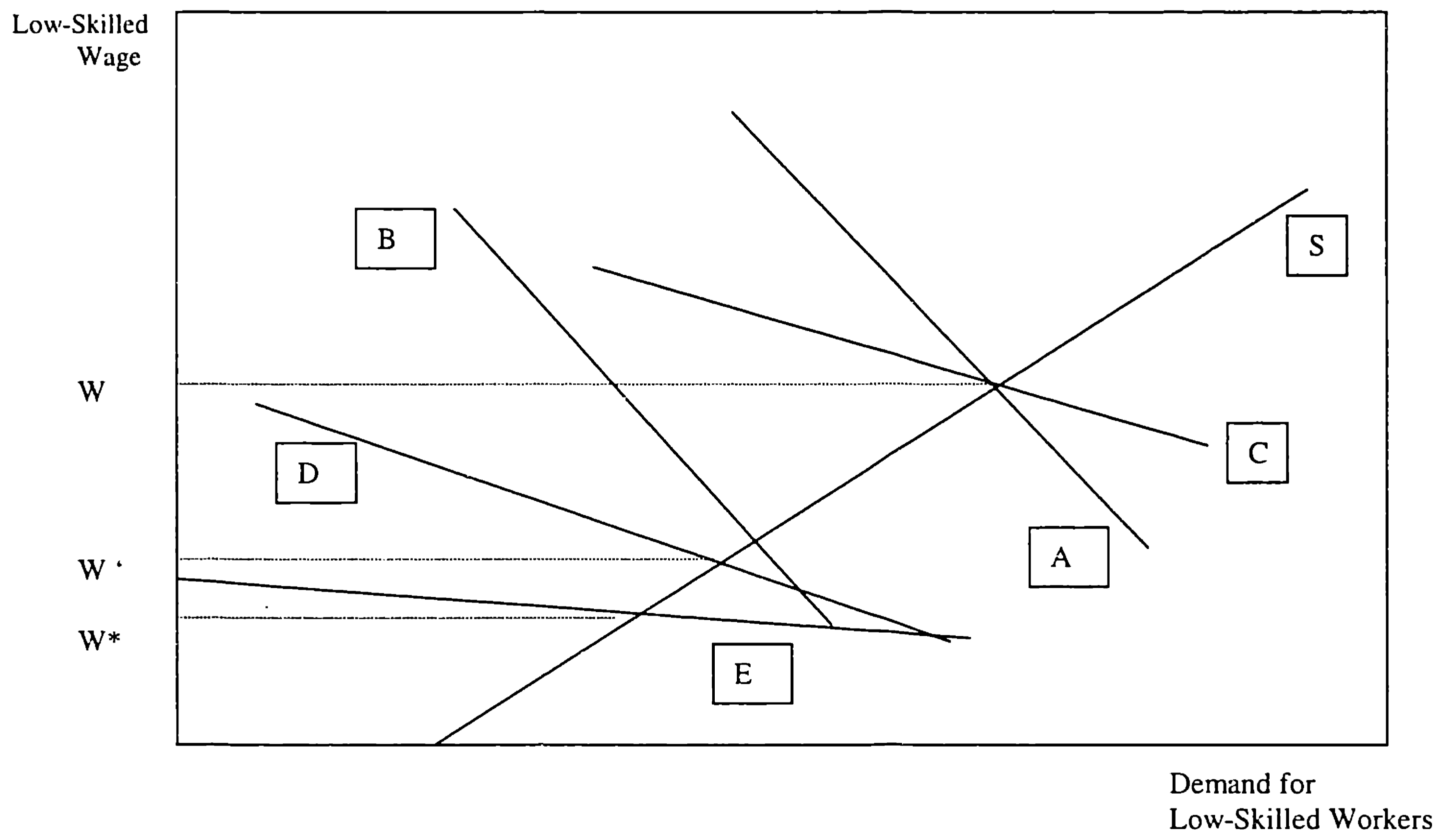
Table VIII: General Equilibrium Model Estimated As Full System

Skills	$WGLOSK_{it}$ =	+70.68	$IMLOSK_{it}$	-63.41	$EXLOSK_{it}$	-1.86	CAP_{it}	-0.16	$SKILL_{it}$	-0.03	$RIGID_{it}$
		(87.30)		(85.88)		(4.25)		(0.11)		(0.06)	
Defined	$WGHISK_{it}$ =	-5.64	$IMHISK_{it}$	+11.17	$EXHISK_{it}$	+1.91	CAP_{it}	-0.06	$SKILL_{it}$	+0.02	$RIGID_{it}$
		(5.42)		(12.60)		(2.53)		(0.09)		(0.04)	
By	$IMLOSK_{it}$ =	+0.08	$WGLOSK_{it}$	-0.10	$WGHISK_{it}$	+0.29	DEF_{it}	+0.00	ER_{it}	-0.78	$CLOSE_{it}$
		(0.07)		(0.10)		(0.25)		(0.00)		(0.74)	
Income	$IMHISK_{it}$ =	-0.12	$WGLOSK_{it}$	+0.31	$WGHISK_{it}$	-1.47*	DEF_{it}	-0.00	ER_{it}	+2.45	$CLOSE_{it}$
		(0.17)		(0.24)		(0.59)		(0.00)		(1.75)	
	$EXLOSK_{it}$ =	+0.07	$WGLOSK_{it}$	-0.00	ER_{it}	-0.10	$CLOSE_{it}$				
		(0.05)		(0.00)		(0.40)					
	$EXHISK_{it}$ =	-0.03	$WGHISK_{it}$	+0.00	ER_{it}	-0.14	$CLOSE_{it}$				
		(0.06)		(0.00)		(0.50)					
Skills	$WGLOSK_{it}$ =	-91.98	$IMLOSK_{it}$	+115.9	$EXLOSK_{it}$	+5.38	CAP_{it}	-0.12	$SKILL_{it}$	+0.02	$RIGID_{it}$
		(170.3)		(218.9)		(14.66)		(0.18)		(0.07)	
Defined	$WGHISK_{it}$ =	-16.40	$IMHISK_{it}$	+30.07	$EXHISK_{it}$	+8.91	CAP_{it}	+0.28	$SKILL_{it}$	+0.06	$RIGID_{it}$
		(22.06)		(35.62)		(11.68)		(0.36)		(0.10)	
By	$IMLOSK_{it}$ =	-0.01	$WGLOSK_{it}$	-0.02	$WGHISK_{it}$	+0.09	DEF_{it}	-0.00	ER_{it}	-0.29	$CLOSE_{it}$
		(0.02)		(0.03)		(0.07)		(0.00)		(0.20)	
Education	$IMHISK_{it}$ =	-0.03	$WGLOSK_{it}$	+0.19	$WGHISK_{it}$	-0.[illegible]	DEF_{it}	-0.00	ER_{it}	+1.31	$CLOSE_{it}$
		(0.13)		(0.18)		(0.45)		(0.00)		(1.31)	
	$EXLOSK_{it}$ =	+0.00	$WGLOSK_{it}$	-0.00	ER_{it}	-0.16	$CLOSE_{it}$				
		(0.01)		(0.00)		(0.10)					
	$EXHISK_{it}$ =	+0.06	$WGHISK_{it}$	-0.00	ER_{it}	+0.31	$CLOSE_{it}$				
		(0.07)		(0.00)		(0.59)					
Skills	$WGLOSK_{it}$ =	+29.66	$IMLOSK_{it}$	-11.86	$EXLOSK_{it}$	+2.32	CAP_{it}	+0.10	$SKILL_{it}$	+0.03	$RIGID_{it}$
		(54.68)		(30.91)		(6.82)		(0.26)		(0.07)	
Defined	$WGHISK_{it}$ =	+23.68	$IMHISK_{it}$	47.49	$EXHISK_{it}$	+3.53	CAP_{it}	+0.67	$SKILL_{it}$	-0.06	$RIGID_{it}$
		(35.37)		(68.69)		(8.24)		(0.74)		(0.13)	
By	$IMLOSK_{it}$ =	+0.12	$WGLOSK_{it}$	-0.05	$WGHISK_{it}$	+0.16	DEF_{it}	-0.00	ER_{it}	-0.50	$CLOSE_{it}$
		(0.15)		(0.17)		(0.51)		(0.00)		(2.18)	
Wages	$IMHISK_{it}$ =	-0.60	$WGLOSK_{it}$	-0.06	$WGHISK_{it}$	-0.85	DEF_{it}	+0.00	ER_{it}	-1.41	$CLOSE_{it}$
		(0.59)		(0.69)		(2.06)		(0.00)		(8.84)	
	$EXLOSK_{it}$ =	+0.24	$WGLOSK_{it}$	-0.00	ER_{it}	+0.27	$CLOSE_{it}$				
		(0.16)		(0.00)		(0.85)					
	$EXHISK_{it}$ =	-0156	$WGHISK_{it}$	+0.00	ER_{it}	-1.47	$CLOSE_{it}$				
		(0.14)		(0.00)		(1.64)					

NOTES: Country-specific effects not reported. Robust standard errors. Sample size: 42 countries.

Figure I
Effects of Increased Trade with
Countries of Similar and Different Relative Skill Abundance

Demand and Supply for Low-Skilled Workers



Chapter 3:

Measuring Stock Market Contagion: Conceptual Issues and Empirical Tests

Abstract: This paper investigates contagion across stock markets. It begins with a discussion of several conceptual issues involved in measuring contagion. The standard methodology involves testing if cross-country correlations in stock market returns increase during a period of crisis. According to this approach, if two markets are highly correlated during periods of stability and they continue to co-move during a period of turmoil, this may not constitute contagion. The measure of cross-market correlations central to this standard analysis, however, can be biased. The unadjusted correlation coefficient is conditional on market movements over the time period under consideration, so that during a period of turmoil when stock market volatility increases, standard estimates of cross-market correlations will be biased upward. It is straightforward to adjust the correlation coefficient to correct for this bias. The remainder of the paper applies these concepts in several empirical tests for stock market contagion. Tests based on the unadjusted correlation coefficients suggest that there was contagion across about half of the world's stock markets during the recent East Asian turmoil. If the same tests are based on the adjusted correlation coefficients, however, the degree of contagion falls by as much as seventy-five percent. This suggests that the strong co-movement in global stock markets during the later half of 1997 was more a continuation of strong cross-market linkages than pure contagion from East Asia. Similar tests for contagion after the Mexican peso collapse of 1994 and the U.S. stock market crash of 1987 support these central findings. This paper closes by applying these results to simulate the impact of select stock market crashes on markets around the world.

I. Introduction

In the second half of 1997, stock market values in East Asia fluctuated wildly. Figure I graphs the movements of stock indices for several major markets during this period. It is immediately apparent that many of the movements in East Asia were mirrored, to varying degrees, by stock markets in other countries. For example, in late October and early November (and to a lesser extent in August), not only did the markets in Hong Kong, Indonesia, Korea, and Thailand plunge dramatically and then partially rebound, but the markets in Brazil, Germany, Mexico and the U.S. followed this general pattern. Why did so many markets, markets of very different structures and sizes located in very different regions and countries, tend to show such similar movements? Was there contagion from East Asia to stock markets around the globe? Or are these markets so closely integrated that a high degree of co-movement should be expected?

These sorts of questions are of fundamental importance on many different levels. Economists and academics study these topics in an attempt to better understand the relative roles of expectations and fundamentals in the valuation of stocks and the transmission of shocks across markets. Investors focus on these issues since a basic tenet of investment strategy is that stock markets in different countries should display relatively low correlations (since many economic disturbances are country-specific), so international diversification should substantially reduce portfolio risk and increase expected returns. A high degree of contagion across markets would undermine much of this rational for international diversification. International institutions and policy makers are perhaps most concerned with the possibility and extent of contagion. The decision to intervene in sovereign economies and dedicate massive amounts of money to stabilization is largely driven by the fear that a crisis or shock in one country will be transmitted to markets throughout the world. If there is no chance of contagion, would the U.S. even consider dedicating $18 billion of funds (in addition to the money already donated) to an I.M.F. stabilization fund?

While contagion can take many forms, such as pressure for a country to devalue its currency or a sharp rise in its cost of capital, this paper only investigates contagion across stock markets. The first half of the paper focuses on conceptual issues involved in measuring this contag[illegible]tion II briefly reviews the relevant empirical literature and explains the standard approach toward measuring contagion— testing for a significant increase in the cross-country correlation in stock market returns during a period of crisis. Using this methodology, if two markets are highly correlated during periods of stability and they continue to co-move during a period of turmoil, this may not constitute contagion. Section III then discusses these conventional techniques in more detail and proves that the correlation coefficients central to these analyses are biased. These unadjusted correlation coefficients are actually conditional on market movements over the time period under consideration, so that during a period of turmoil when stock market volatility increases, standard estimates of cross-market correlations will be biased upward. We show how to adjust the correlation coefficient to correct for this bias.

The remainder of the paper applies these concepts in several empirical tests for stock market contagion. Section IV performs likelihood ratio tests to ascertain if average cross-market correlations increased significantly during the recent East Asian turmoil. Tests based on the unadjusted correlation coefficients suggest that there was contagion across about half of the world's stock markets during the recent East Asian turmoil. If the same tests are based on the adjusted correlation coefficients, however, the degree of contagion falls by as much as seventy-five percent. This suggests that the co-movement in global stock markets during the later half of 1997 was more a continuation of strong cross-market linkages than pure contagion from East Asia. Section V shows that these key results apply to situations other than the recent East Asian turmoil, by repeating the likelihood ratio tests for contagion after the Mexican peso crisis at the end of 1994 and the U.S. stock market crash in 1987. Section VI then uses the results of these analyses to simulate the impact of a stock market crash in Brazil, Korea, Russia, or the U.S. on markets around the world. Section VII concludes.

II. Empirical Literature on Stock Market Contagion

While a number of papers have examined speculative attacks on currencies, contagion in the banking sector, or theoretical models of contagion in stock markets, relatively few have empirically investigated contagion across stock markets. The bulk of the work that has been done on this subject focuses on the impact of the U.S. stock market crash in 1987. The most frequently cited of these articles is by King and Wadhwani, who find a significant increase in correlations between the New York, Tokyo and London stock markets after the crash of 1987.[1] Their analysis focuses on hourly trading data so as to identify price jumps that occur in all markets whenever one market reopens. Other papers testing for contagion between these three countries in 1987 utilize an autoregressive conditional heteroscedasticity

[1] King and Wadhwani (1990).

(ARCH) framework.[2] These papers find significant spillovers across markets and that the international transmission of volatility does not occur evenly (with the greatest sensitivity in Japan.)

Several other papers expand the analysis of stock market contagion in 1987 to include a larger sample of countries. Lee and Kim consider twelve major markets (mostly OECD) and find that the average correlation of weekly U.S. dollar returns increased from 0.23 before the U.S. stock market crash to 0.39 afterwards. They emphasize that cross-market correlations tend to increase when markets are more volatile.[3] Bertero and Mayer report that stock markets in some countries reacted sharply to the U.S. stock market crash, while others displayed a high degree of stability and were relatively unaffected by this global shock.[4] They use regression analysis to show that a market's stability and resistance to contagion is not significantly related to its structure (as measured by market size and average volume), but the speed of contagion is affected by market characteristics (such as the presence of circuit breakers and/or capital controls.) They also report that correlations across major regional indices increase after the crash and remain above-average for months.

A few papers examine cross-market correlations during periods other than 1987. For example, Longin and Slonik consider seven OECD nations from 1960 to 1990 and report that average correlations in stock market returns between the U.S. and other countries have risen by about 0.36 over this thirty-year period.[5] Most papers that consider these longer time periods utilize cointegration in order to focus on the long-run relationships between markets.[6] They generally find that stock markets across countries and/or regions are co-integrated, and that this integration has increased over time. Since these papers focus on such long-run relationships, however, they ignore the short-term fluctuations that constitute contagion and are therefore not directly relevant to the questions investigated in this paper.

This brief overview of previous empirical work attests to the wide variety of techniques, samples and periods that can be used to identify and measure contagion across stock markets. Central to each of these diverse papers, however, is the same fundamental concept of contagion. Contagion is defined as a significant increase in stock market co-movements after a major shock, and to measure these co-

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[2] Hamao et al. (1990) and Theodossiou et al. (1997).
[3] Lee and Kim (1993).
[4] Bertero and Mayer (1990).
[5] Longin and Slonik (1995). Jorion (1989) performs a similar analysis for sixteen industrial countries over a number of different periods between 1959 and 1986. He finds fairly low cross-market correlations during most of this period and argues that these correlations actually decreased after 1979.
[6] For example, see Chou et al. (1994) or Cashin, Kumar and McDermott (1995).

movements, each paper uses the simple, unadjusted correlation coefficient. Therefore, if the returns in two markets (such as the U.S. and Canada) are highly correlated during periods of stability, and the correlation between these markets remains high but does not increase significantly after one of the markets crashes, this would not be interpreted as contagion. While this definition may differ from the "contagion" feared in the popular press, this paper utilizes the same fundamental approach and defines contagion as a significant increase in cross-market correlations. The last section of this paper will discuss shortcomings of this concept of contagion.

III. Bias in the Correlation Coefficient

As discussed above, the methodological basis of most work analyzing stock market contagion is the simple, unadjusted correlation coefficient. Specifically, the cross-market correlation in stock returns is calculated as $\rho_{i,j}$, where:

$$\rho_{i,j} = Corr\left(R^i R^j\right) = \frac{Cov\left(R^i R^j\right)}{\sigma_i \sigma_j} = \frac{\frac{1}{n}\sum_{t=1}^{n}\left(R_t^i - \mu_i\right)\left(R_t^j - \mu_j\right)}{\sigma_i \sigma_j} \qquad (1)$$

for two markets *i* and *j* (with $i \neq j$), where R_t^i is the return to stock market *i* at time *t*, μ_i is the mean return in stock market *i* over the period from 1 to *n*, σ_i is the standard deviation of returns over the same period, and each variable is defined likewise for market *j*.

This unadjusted correlation coefficient, however, may be biased. This bias results from the fact that the simple correlation of equation (1) does not control for the abnormal variance of stock market returns during periods of turmoil relative to the average variance during periods of stability. Ronn raises this concern in the context of intra-market correlations in stocks and bonds, but does not address this issue with respect to cross-market correlations.[7] Appendix A shows that the standard formula (equation 1) for estimating the correlation between two markets actually estimates the correlation *conditional* on the movement in one of the markets over the time period under consideration. Therefore, when using equation (1) to calculate the correlation coefficient over a period of time *t*, instead of estimating the true cross-market correlation in returns, you actually estimate:

[7] Ronn (1998).

$$\rho_{i,j}^{t} = Corr\left(R^{i} R^{j} \middle| \left(R^{j}\right)^{2} \right) = \left(\frac{1+\delta}{1+\delta \tilde{\rho}_{i,j}^{2}} \right)^{1/2} \tilde{\rho}_{i,j} \tag{2}$$

where $\tilde{\rho}_{i,j} = Corr\left(R^{i} R^{j}\right)$ is the true correlation in returns between the two markets, and

$$\delta \equiv \frac{\left(R^{j}\right)^{2} - \sigma_{j}^{2}}{\sigma_{j}^{2}}.$$

The critical implication of equation (2) is that the magnitude of the conditional correlation $\rho_{i,j}^{t}$ is increasing in $\left(R^{j}\right)^{2}$. Therefore, during periods of increased volatility in market *j*, the estimated, unadjusted correlation between markets *i* and *j* will be greater than the true, unconditional correlation during this period. In other words, estimates of the correlation coefficient based on equation (1) will be biased upward. This could lead to the incorrect conclusion that cross-market correlations significantly increase after a shock when markets are more volatile. This bias alone could generate the finding of contagion in the studies discussed above.

It is straightforward to adjust for this bias. Simple manipulation of equation (2) to solve for the unconditional correlation $\tilde{\rho}_{i,j}$ yields:

$$\tilde{\rho}_{i,j} = \frac{\rho_{i,j}^{t}}{\left[1+\delta-\delta\left(\rho_{i,j}^{t}\right)^{2}\right]^{1/2}} \tag{3}$$

where δ and $\rho_{i,j}^{t}$ are defined above. To simplify the following discussion, we will refer to $\rho_{i,j}^{t}$ as the unadjusted (or conditional) correlation and refer to $\tilde{\rho}_{i,j}$ as the adjusted (or unconditional) correlation.

To clarify the concept and relevance of this adjustment, Figure II graphs the correlation in stock market returns between Hong Kong and the Philippines during 1997.[8] The dashed line is the unadjusted correlation in daily returns and the solid line is the adjusted correlation. While the two lines generally move up and down together, it is clearly important to correct for the bias generated by changes in market volatility. During the relatively stable period in the first half of 1997, the unadjusted correlation is always lower than the adjusted correlation. On the other hand, during the relatively tumultuous period of the last quarter, the unadjusted correlation is significantly greater than the adjusted correlation. An analysis based on the unadjusted correlations would find a significant increase in cross-market correlations in the later period and would therefore indicate contagion. The adjusted correlations do not increase by nearly as much in the later period, however, so an analysis based on these unconditional correlations might not find evidence of contagion.

But is this effect significant? Does adjusting cross-market correlations to account for changing market volatility influence estimates of contagion? To answer these questions, the next two sections perform a number of empirical tests.

IV. Contagion During the East Asian Crisis?

Our first set of tests compares average correlations in stock market returns during periods of relative stability to those during the East Asian crisis. To calculate stock market returns, we utilize daily values of the aggregate stock market index reported by Datastream and demean these indexes so as to satisfy the assumptions required for the adjustment in equation (3).[9] Specifically, if S_t^i is the value of the stock market index in country i at time t, and the period during which we examine market co-movements is from t=1 to T, then the demeaned rate of return is[10]:

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[8] Correlations are calculated as quarterly moving-averages. The exact procedure, definitions, and data source used to estimate this graph are described in more detail below.

[9] Datastream does not calculate a stock market index for Brazil, India, or Russia. Instead we utilize: the Bovespa stock index, the Bombay national stock index, and the Russia MT index, for each of these countries respectively.

[10] The standard adjustment is made for weekends and other dates when the market is closed. For example, if t is a Monday (so the market was closed on t-1 and t-2), then we calculate:

$$R_t^i = \left(S_t^i \big/ S_{t-1}^i - 1 \right) - \sum_{t=1}^{T} R_t^i \qquad (4)$$

But should we focus on market returns measured in local currencies or in a common base currency such as the U.S. dollar? These two measures will differ by changes in the exchange rate of the local currency for the base currency. It is straightforward to show that the return on a foreign stock market index for a U.S.dollar-based investor is the sum of the change in the value of the index in foreign currency and the change in the exchange rate:

$$\begin{aligned}
R_{t,\$}^i - \sum_{t=1}^{T} R_{t,\$}^i &= \left(S_t^i E_t^i \big/ S_{t-1}^i E_{t-1}^i - 1 \right) - \sum_{t=1}^{T} S_t^i E_t^i \\
R_{t,\$}^i &= \left(S_t^i E_t^i \big/ S_{t-1}^i E_{t-1}^i - 1 \right) \\
&\cong \ln\left(S_t^i E_t^i \big/ S_{t-1}^i E_{t-1}^i \right) \\
&= \ln\left(S_t^i \big/ S_{t-1}^i \right) + \ln\left(E_t^i \big/ E_{t-1}^i \right) \\
R_{t,\$}^i &= R_{t,FC}^i + E_t^i \qquad (5)
\end{aligned}$$

Where $R_{t,\$}^i$ is the rate of return on the stock market index in country *i* at time *t* in U.S.\$; S_t^i is the price of the index in foreign currency (FC) at time t; E_t^i is the spot exchange rate for the currency in country *i* in \$/FC; and $R_{t,FC}^i$ is the rate of return on the stock market index in foreign currency.

Therefore, depending on the relative movement of the exchange rate and the stock market index, returns expressed in U.S. dollars can be higher or lower than those in local currency.[11] Moreover, these

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$$R_t^i = \left[\left(S_t^i \big/ S_{t-3}^i \right)^{1/3} - 1 \right] - \sum_{t=1}^{T} R_t^i$$

Also note that demeaning each observation has no effect on correlation estimates.

[11] As an empirical issue, however, the correlation between changes in the spot exchange rate and the value of aggregate stock market indexes is fairly low. Levich calculates $Corr(R_{FC}; E_{\$,FC})$ for 19 industrial countries between

relative movements can affect estimates of contagion across stock markets. For example, during a period when the dollar is depreciating against most currencies, dollar returns in non-U.S. markets increase more than local currency returns, and as a result, the correlation of these dollar returns in the non-U.S. markets would be higher than those based on local currencies. So which measure should we utilize? In order to focus solely on stock market movements and abstract from any concurrent pressures on exchange rates, we should utilize returns based on local currencies. On the other hand, from the viewpoint of a dollar-based investor concerned with the benefit of diversification across markets, we should focus on returns based on the dollar. Both of these approaches have merit, so in the discussion which follows, we will examine co-movements in stock markets valued in both local currencies and U.S. dollars.

Before actually examining these co-movements, it is necessary to select the markets on which to focus. All of the work discussed in Section II examines a small sample of countries—often just the three largest markets or stock markets in industrial countries. This was a logical choice for the time period these papers covered, because in 1987 the stock markets in many emerging economies were small and restricted—if they even existed at all. With such limited samples, however, it is obviously difficult to draw any conclusions about contagion within emerging markets, or between developed and emerging markets. Since we would like to investigate these forms of contagion, and especially since the liquidity of many emerging markets has increased significantly over the past few years, this paper substantially augments earlier samples. It considers the relative movements of twenty-nine markets: the twenty-five largest markets (as ranked by market capitalization at the end of 1996), plus the Philippines (to expand coverage of East Asia), Argentina and Chile (to expand coverage of Latin America), and Russia (to expand coverage of emerging markets outside of these two regions.) Table I lists these countries with total stock market capitalizations and average market volumes. It also defines the regions utilized throughout this paper.[12] Since several of these markets are still fairly small and illiquid, we test for the effect of sample selection in the analysis that follows.

Next, we utilize equations (1) and (4) to calculate average, unadjusted correlations in daily returns between each of the 29 markets during two periods: a period of relative stability in the East Asian markets (from 3/1/93 through 2/28/98, excluding 8/7/97 through 11/6/97) and during a period of relative turmoil in the East Asian markets (from 8/7/97 through 11/6/97). We choose 8/7/97 as the start

1980 and 1995 and estimates that correlations are near zero across all countries, with absolute values generally less than 0.25. (From Levich, 1998, p. 540.) I am unaware of any evidence on the relationship between these two variables in developing countries.

[12] Note that Japan and Korea are considered East Asian and Mexico is considered Latin American.

of the "turmoil" period since this is the date when the Thai market started its continuous twenty-five day plunge--a plunge that quickly drew attention to the East Asian economies and was soon mirrored in other markets. We allow this period of turmoil to last three months so as to include the October plummet of the Hong Kong market. We start the period of calm in 1993 since the resulting five-year period (less the period of turmoil) is long enough to minimize short-term fluctuations and should therefore provide a fairly accurate estimate of average cross-market correlations. While this choice of dates is a bit capricious, at the end of this section we show that altering the dates by months (or even years in the case of the longer first period), does not significantly affect the central results.

Tables II and III report unadjusted correlation coefficients for each pair of countries based on stock market returns in local currencies and U.S. dollars, respectively. The top triangle of each matrix lists average correlations during the period of stability, and the bottom triangle lists average correlations during the period of East Asian turmoil. Several patterns are immediately apparent. First, markets within the same region tend to be more highly correlated than markets in different regions. Second, correlations based on local currencies are often different than those based on U.S. dollars. The differences, however, do not appear systematic, with about an equal chance for a correlation based on local currencies to be greater or less than that based on U.S. dollars.[13] Third, and central to this paper, about three-quarters of the correlation coefficients are greater during the turmoil period than the stable period.[14] This suggests that there may have been contagion across stock markets during the East Asian crisis.

But is this increase in cross-market correlations during the East Asian crisis significant? To ascertain if this effect is meaningful, we use a likelihood ratio test to examine if the correlation coefficient increases significantly during the turmoil period.[15] The null hypothesis is that the correlation coefficient in the stable period is greater than or equal to that in the turmoil period. Formally, the test is:

$$H_0:\ \rho^0_{i,j} \geq \rho^1_{i,j}$$
$$H_1:\ \rho^0_{i,j} < \rho^1_{i,j}$$

[13] Specifically, correlations based on local currencies are greater than those based on U.S. dollars in 516 of the 812 cases. Patterns are similar over the stability and turmoil periods.
[14] Specifically, 293 of the correlations based on local currencies and 295 of the correlations based on U.S. dollars are higher in the turmoil period. (Given a total of 406 correlations in each case.)
[15] This test is also known as a two-sample t-test or a heteroscedastic t-test. We utilize this test since it allows the variance in returns to differ across the two periods and does not force us to make any assumptions about the relationship between these two variances. There are several slightly different variants of this test, but for the

where $\rho_{i,j}^t$ is the correlation coefficient between countries *i* and *j* during period *t*, calculated according to equation (1); *t=0* is the stable period, and *t=1* is the turmoil period. Each of the correlation coefficients is subject to a Fisher transformation[16] so as to be normally distributed, and then it is possible to calculate the test statistic U:

$$U = \frac{\overline{X}_0 - \overline{X}_1}{\left(\frac{s_0^2}{n_0} + \frac{s_1^2}{n_1}\right)^{1/2}} \tag{6}$$

where $\overline{X}_t$ and s_t^2 are the estimated sample mean and variance, respectively, after the Fisher transformation, and n_t is the sample size for period *t*. The test statistic is distributed as a t-statistic with 72 degrees of freedom.[17]

Applying this test to the correlation coefficients calculated in Tables II and III, we reject the null hypothesis (at the 5% level) for 211 of the 406 coefficients (52%) based on local currencies, and for 203 of the 406 coefficients (50%) based on U.S. dollars. The results of individual tests for each pair of countries are reported in Tables IV and V. A "N" in the matrix reflects an inability to reject the null hypothesis and suggests no contagion, and a "C" in the matrix represents a rejection of the null hypothesis and indicates contagion. Although the number of rejections is similar if the correlations are

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purposes of this analysis, all yield the same results. For a further discussion of these issues, see Dixon and Massey (1983), Hoel (1984), or Ostle and Malone (1988).

[16] The Fisher transformation is: $Z_{i,j}^t = \frac{1}{2}\ln\left(\frac{1+\rho_{i,j}^t}{1-\rho_{i,j}^t}\right)$. When the sample size is large, the distribution of $Z_{i,j}^t$ is approximately normal with mean $\mu_t = \frac{1}{2}\ln\left(\frac{1+\rho_{i,j}^t}{1-\rho_{i,j}^t}\right)$ and variance $\sigma_t^2 = \frac{1}{n_t - 3}$.

[17] The degrees of freedom are calculated according to Satterthwaite's formula:

$$\frac{\left(s_0^2/n_0 + s_1^2/n_1\right)^2}{\frac{\left(s_0^2/n_0\right)^2}{n_0 - 1} + \frac{\left(s_1^2/n_1\right)^2}{n_1 - 1}}$$

based on U.S. dollars or local currencies, individual results for any pair of countries often differ across the two tables.[18]

Despite these differences, the two tables do show several analogous patterns. In both cases, the region showing the most contagion is the OECD's, with virtually all intra-regional correlations (except with the U.S.) increasing significantly during the turmoil period. Most of the OECD's (with Canada and the U.S. the two notable exceptions) also experienced contagion with many of the East Asian nations. Within East Asia, there is evidence of contagion between about a half of the countries. One of the greatest concerns during the East Asian turmoil, however, was of contagion spreading to other emerging markets. Here the evidence is also mixed; there is contagion between most of the East Asian nations and Russia and South Africa, mixed results for contagion between East Asia and Chile, China, India, and Mexico, and little contagion between East Asia and Argentina and Brazil.

This analysis based on the unadjusted correlation coefficients suggests that during the East Asian crisis, there was a substantial amount of contagion in stock markets around the world. Correlations between about half of the stock markets in our sample increased significantly during the period of East Asian turmoil. As discussed in section III, however, the correlation coefficients forming the basis of this analysis could be biased. Does correcting the correlation coefficients to remove this bias significantly affect the finding of a substantial amount of contagion?

To test for this affect, we use equation (3) to adjust the correlation coefficients and remove this bias. Then we follow the same procedure as above to estimate the correlation matrices for stock market returns during the same periods of East Asian stability and turmoil. Tables VI and VII report these matrices based on local currencies and U.S. dollars, respectively. A comparison with the unadjusted correlation matrices of Tables II and III shows that although many of the patterns in cross-market correlations do not change, the adjustment does affect the magnitude of the coefficients. As suggested in the discussion of Section III, the adjusted correlations are usually higher than the unadjusted correlations during the more stable period and lower during the more volatile period.[19]

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[18] In 74 of the 406 cases.

[19] For example, during the stability period 374 of the 406 adjusted correlations (based on local currencies) are greater than the unadjusted correlations. During the turmoil period, only 89 of the adjusted correlations are greater than the unadjusted correlations. In both cases, many of the "unusual" patterns are correlations with China.

Even after this correction, about half of the adjusted correlation coefficients are still greater during the turmoil period than the stable period. This is significantly less than found using the unadjusted correlations.[20] But for the correlations that do increase during the turmoil period, are the changes large enough to constitute contagion? To test for contagion, we repeat the likelihood ratio tests for the adjusted correlation coefficients. Now we reject the null hypothesis in 24% of the cases when the correlations are based on local currencies and in 30% of the cases based on U.S. dollars. This is in sharp contrast to the rejection for 52% of the unadjusted correlations based on local currencies and 50% of the unadjusted correlations based on U.S. dollars. Correcting for the bias in the correlation coefficients significantly reduces the proportion of countries subject to contagion during the East Asian crisis—by about 75% when the correlations are based on local currencies.

The pattern of markets subject to contagion after adjusting the correlations reflects this significant reduction. For example, in Table VIII (based on local currencies), only 14% of the markets within East Asia now show evidence of contagion with other markets in this region. Some of the OECD's (such as Italy) are still subject to contagion with East Asia, and co-movements between many of the OECD's still increase significantly during the turmoil period, but contagion between East Asia and the OECD's and within the OECD's is substantially reduced. Contagion between East Asia and some emerging markets actually increases (such as with Chile) while in other cases it decreases (such as with Russia and South Africa.) As a whole, when we adjust the correlation coefficients to remove the bias from increased market volatility, there is still evidence of contagion, but the extent of this contagion is significantly diminished from that estimated with the unadjusted coefficients.

This finding of such a low degree of contagion in our sample, and especially within regions such as East Asia, highlights exactly how economists define contagion. If two markets which are highly correlated during relatively stable periods continue to be highly correlated during more volatile periods, this is not necessarily contagion. It is only considered contagion if the cross-market correlation significantly increases from its average level. Therefore, even though the Singapore stock market plummeted as the Hong Kong market crashed, since these two markets generally move together (with a cross-market correlation of 0.56), this is not considered contagion. While this interpretation may not coincide with the fears of "contagion" expressed in the popular press, it is logical. If the U.S. market

[20] Using the unadjusted correlations, 293 of the correlations based on local currencies and 295 of the correlations based on U.S. dollars are higher in the turmoil period than the stable period. Using the adjusted correlations, 202 of the correlations based on local currencies and 248 of the correlations based on U.S. dollars are higher in the turmoil period. (Given a total of 406 correlations in each case.)

falls and the Canadian market follows, is this contagion? When two countries are closely linked through trade and other economic fundamentals, should we be surprised that they continue to co-move after one is subject to a large shock?

While these tests for contagion during the East Asian crisis appear fairly straightforward, there are three potential problems with this analysis. First, Table I shows that several of the stock markets included in the sample are fairly small or illiquid. As a result, prices in these markets may be driven largely by the valuation of one company, or by the actions of one investor or the government. To remove this potential impact, we repeat the above analysis for only the ten largest markets (as ranked by total market capitalization at the end of 1996.[21]) The second row of Table X reports these results and shows that focusing only on large and highly liquid markets actually strengthens our central findings. Using the unadjusted correlations, there is contagion between 76% of the markets based on local currencies and 67% based on U.S. dollars. This is significantly more contagion then in the larger sample. Using the adjusted correlations, these statistics fall to 16% and 29%, respectively, both of which are lower than in the larger sample. Therefore, correcting for the bias in the correlation coefficients actually has a greater effect on larger, more liquid markets.

A second concern with this analysis is that daily returns are not an accurate measure of cross-market correlations due to the lagged timing of market openings and closings. To reduce this problem, some studies prefer to focus on correlations in weekly returns. Although this lower-frequency analysis loses many of the important daily co-movements in markets, we repeat our analysis using weekly instead of daily returns. Results are reported in the third row of Table X. The evidence of contagion based on both the unadjusted and adjusted correlations is similar to that based on daily returns. Adjusting the correlation coefficients to remove the bias from increased market volatility still significantly reduces the estimated extent of cross-market contagion.

A final concern is that results are driven by the somewhat capricious choice of dates used to define the periods of "stability" and "turmoil." If contagion from East Asia occurred during only part of the three-months defined as the "turmoil" period, if contagion originally occurred at an earlier date (such as during the Mexican peso crisis), or if it continued to occur after the "turmoil" period, then the tests performed above might underestimate the extent of contagion. To test for this, we repeat the likelihood

[21] These ten markets are: Hong Kong, Japan, Australia, Canada, France, Germany, the Netherlands, Switzerland, the U.K., and the U.S.

ratio tests using four different period definitions. First, we shorten the turmoil period to only the one-month after the crash of the Hong Kong market. Second, we lengthen the turmoil period to six-months, starting with the fall of the Thai market. Third, we shorten the stability period to exclude the time before and during the Mexican peso crisis. Fourth and finally, we shorten the period of stability to exclude the period directly following the turmoil in the East Asian markets. The last four rows of Table X report these test results. While the number of cases of contagion does fluctuate based on how the two periods are defined, the central result endures; using the adjusted instead of the unadjusted correlation coefficient reduces the evidence of contagion by between one-third and one-half, so that while there was some contagion during the East Asian turmoil, this contagion was limited.

To summarize, these tests illustrate the importance of correctly measuring cross-market correlations when testing for stock market contagion. The simple, unadjusted correlation coefficient is biased, and adjusting this coefficient to correct for changing market volatility not only affects the magnitude of cross-market correlations, but also significantly influences estimates of contagion. Correcting the correlation coefficients reduces the evidence of contagion during the East Asian crisis by about half, suggesting that only about one-quarter to one-third of our sample was subject to contagion during this period. Moreover, these results highlight exactly what we mean by contagion. When two markets that tend to be highly correlated during relatively stable periods continue to be highly correlated during more volatile periods, this is not contagion. It is only contagion when cross-market correlations significantly increase above their average levels during a period of crisis.

V. Contagion After the 1994 Mexican Peso Collapse? Or the 1987 U.S. Stock Market Crash?

The results from these tests for contagion during the East Asian turmoil are clear. Market co-movements increased between a number of countries, providing evidence that there was contagion between many countries and regions. The scope of this contagion, however, is significantly diminished when the correlation coefficients are adjusted to remove the bias from changing market volatility. But are these results unique to the East Asian crisis? Do they also apply to other periods of market turbulence? To test if these central findings apply to other situations, this section repeats the likelihood ratio tests, based on both unadjusted and adjusted correlation coefficients, to measure the extent of contagion after the Mexican peso collapse of 1994 and the U.S. stock market crash of 1987.

At the end of 1994 the Mexican government suffered a balance of payments crisis, leading to a collapse of the peso and a crash of the Mexican stock market. Investors feared that this could generate a crisis in other emerging stock markets—especially in Latin America. To test for stock market contagion after the Mexican peso crisis, we repeat the likelihood ratio tests developed in section IV. We utilize the same sample of 29 countries listed in Table I, and define the period of turmoil in the Mexican market as 12/1/94 through 2/28/95 and the period of stability as the four-year period from 1/1/93 to 1/1/97 (less the period of turmoil).[22] Results based on both the unadjusted and adjusted correlation coefficients are reported in the first row of Table XI, and results if the turmoil period is extended to six months (from 12/1/94 through 5/31/95) are reported in the second row. Individual test results for each pair of countries are reported in Table XII.[23]

The first two rows of Table XI show several different patterns than found during the East Asian crisis. First, estimates based on the unadjusted correlations suggest significantly less contagion during the Mexican peso crisis than the East Asian turmoil. This is not surprising since the Mexican stock market is much smaller than those in East Asia. Second, and more surprising, estimates based on the adjusted correlation coefficients suggest a similar amount of contagion during the Mexican and East Asian crisis. Third, and related to this fact, the amount of contagion during the Mexican crisis actually increases when the correlation coefficients are adjusted (instead of decreasing as they did during the East Asian turmoil.) This indicates that during the Mexican crisis, many markets actually moved less than during the "stability" period, so adjusting the correlation coefficients to account for changing volatility increased (instead of decreased) the coefficients during this turmoil period. Fourth and finally, even the adjusted correlation coefficients show a fairly low degree of contagion during the Mexican peso crisis, with only 17-34% of the sample subject to a significant increase in cross-market correlations during this period.

The sample of 29 countries utilized for these tests includes both developed and developing countries, but during the Mexican peso crisis the greatest concern (especially after intervention by the I.M.F) was of contagion from Mexico to other emerging markets. To isolate how the Mexican peso crisis affected different regions, the third row of Table XI reports only test results for contagion between

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[22] The turmoil period starts on December 1, despite the fact that the actual peso collapse did not occur until the middle of the month, since this was the beginning of the continuous decline of the Mexican stock market. The stability period ends at the start of 1997 so as to exclude any contagion during the East Asian crisis.
[23] To save space, we only report results based on the adjusted correlation coefficients in U.S. dollars. Results based on the unadjusted coefficients or local currencies are available on request.

Mexico and the other 28 countries, and the last four rows of the table further break down these results into contagion between Mexico and countries in each of the four regions. These rows show little evidence of contagion between Mexico and any set of countries. After adjusting the correlation coefficients, there is not even a significant increase in cross-market correlations between Mexico and any other country in Latin America. The stock markets of Argentina, Brazil and Chile, however, did plummet as the Mexican market crashed (although not by as much as the Mexican market.) This highlights an important aspect of our measure of contagion. The Latin American markets were highly correlated before the Mexican peso crisis, so that when the Mexican stock market crashed and the other Latin American markets followed, this was not a significant increase in cross-market correlations. Markets that were closely linked before the peso crisis remained closely linked during the crisis, and according to the standard definition, this is not contagion.

Before the Mexican peso crisis, another period of stock market turmoil when investors feared contagion was the U.S. stock market collapse in October of 1987. To test for contagion during this period, we repeat the likelihood test procedure described above. We utilize similar time periods to define the periods of stability and turmoil, but since many of the smaller stock markets in our sample of 29 countries were not in existence or were highly regulated at this time, we focus only on the ten largest markets.[24] Results when the turmoil period is defined as lasting three months or six months are reported in the first two rows of Table XIII. Results of tests between just the U.S. and the other nine markets are summarized in the last two rows. The outcomes of individual tests based on U.S. dollars are reported in Table XIV.

Several patterns are immediately apparent. First, no matter which time period, currency, or correlation coefficient you consider, there is substantially more contagion after the U.S. market crash than during the East Asian or Mexican crisis. Second, adjusting the correlation coefficient significantly reduces the evidence of contagion—decreasing the number of countries subject to contagion by over 100% when the turmoil period is defined as lasting three months. Third, there is more contagion over the six-month than the three-month turmoil period. This is the same trend as during the East Asian crisis, but the opposite of during the Mexican crisis, and suggests that the size of the market(s) subject to the initial shock may affect the duration of the contagion across markets.

To summarize, although these tests for contagion during the Mexican peso crisis of 1994 and U.S. stock market crash of 1987 show several different patterns than found during the East Asian crisis, they continue to support the central results of this paper. The simple, unadjusted correlation coefficient is a biased measure of cross-market correlations. Adjusting this coefficient to correct for changing market volatility will not only affect estimates of cross-market correlations, but also significantly affect estimates of contagion across stock markets. Moreover, these results further highlight exactly what we mean by contagion. It is not contagion when two markets that are highly correlated during periods of stability continue to be highly correlated during periods of crisis (such as Mexico and Chile during the Mexican peso collapse.) It is only contagion when cross-market correlations significantly increase during a period of crisis from their average levels.

VI. Simulations: Global Effects of Select Stock Market Crashes

The above analysis has focused on correlations and contagion across stock markets during three major financial crisis: the East Asian turmoil of 1997, the Mexican peso collapse of 1994, and the U.S. stock market crash of 1987. This section uses these results to simulate the effect of a future financial crisis on global stock markets. Specifically, we estimate the impact on stock markets around the world if the Brazilian, Korean, Russian or U.S. market crashes. We chose these four dissimilar countries in order to be able to compare the global effects of a crisis originating in different regions and different-sized markets. We also chose these four markets since there are strong arguments why each of them might crash in the near future.

As the starting point of this analysis, we utilize the value of each stock market on the last date in the sample: 2/28/98. As an initial shock, we assume that the value of either the Brazilian, Korean, Russian, or U.S. stock market index falls by 1.5 standard deviations.[25] Then, we assume two scenarios for each crash: "no contagion" or "contagion" following the pattern during the recent East Asian crisis. In the no contagion scenario, we use the average, adjusted, cross-market correlations during the period of

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[24] Specifically, we utilize about four years of data and define the period of stability as 1/1/84 through 10/13/87 and the period of turmoil as starting on 10/14/87. The ten countries in the sample are: Hong Kong, Japan, Australia, Canada, France, Germany, the Netherlands, Switzerland, the U.K. and U.S.

[25] Standard deviations are calculated from 3/1/93 through 2/28/98 (the time period utilized for the analysis of the East Asian crisis.) For comparison, the Hong Kong market fell by 2.7 standard deviations in the first three-months of the East Asian crisis, the Mexican market fell by 1.1 standard deviations in the first three months of the Mexican peso crisis, and the U.S. market fell by 1.3 standard deviations in the first three months after the U.S. crash of 1987. (Where each of the standard deviations is also calculated over the period used in the corresponding analysis above.)

relative stability, as estimated in section III, to predict the resultant movement in stock markets around the globe. In the contagion scenario, we use the average, adjusted, cross-market correlations during the period of East Asian turmoil, as also estimated in section III.

Tables XV and XVI estimate the percent change in the stock market index, based on local currencies and U.S. dollars respectively, in each of the 29 markets after the four hypothetical stock market crashes. After the Brazilian crash, most of the 29 markets in our sample lose value. In the no contagion scenario, the impact in East Asia is small (averaging only -3% in local currencies), while the impact in the OECD's is about three times as large, and the impact in Latin America is four times greater still. While these overall patterns are similar in the contagion scenario, the impact is magnified in many countries (such as in Argentina and Mexico), while it is diminished in other markets (such as in Hong Kong and Spain.) In both scenarios, the simulated impact of the Brazilian crash on other markets appears to be closely related to the degree of economic integration between the two countries.

The simulated impact of the Korean crash shows many different patterns. In the no contagion scenario, most of the 29 stock markets still lose value, but the effect is much more balanced across regions. The impact of a Korean crash on most of East Asia is similar to that on Latin America and the OECD's (averaging about -4 % across regions for calculations based on local currencies.) In the contagion scenario, the negative effect on global markets is again more evenly distributed than in the Brazilian example, and in most cases the effect is significantly larger (except in Latin America where Brazil had a larger impact.) These differences undoubtedly reflect the fact that the Korean economy is considerably larger than that of Brazil and more closely integrated with the world as a whole.

The simulations of a Russian or U.S. crash are not nearly as realistic, indicating a fundamental shortcoming of this type of analysis. According to the last four rows of Tables XV and XVI, the impact of a Russian crash on most markets is significantly greater than that of a U.S. crash. This counterintuitive outcome results from the information used to construct these estimates. The basis of these simulations is cross-country correlations---and these correlations do not show causation. As a result, if two markets (such as the U.S. and Canada) are highly correlated, it is difficult to disentangle whether a movement in the U.S. market generates a similar movement in Canada or vice versa. In the context of the tables, correlations between many of the East Asian nations and Russia increased significantly during the East Asian crisis, so that when we use these correlations to estimate the impact of a Russian crisis, we simulate a large impact in East Asia. On the other hand, there was virtually no contagion between East Asia and the U.S., so that when we use these correlations, we predict little impact of a U.S. market crash on East Asia.

Basically, the correlations used for the contagion scenarios in this analysis only capture market relationships after the East Asian crisis and can not accurately predict any out-of-sample effects. As suggested in the analysis of the Mexican peso crisis and U.S. stock market crash of 1987, there could be substantially less contagion after a crash in a smaller stock market, or significantly more contagion after a crash in the world's largest market. This is also an issue in the Brazilian simulation, but given that the size of the Brazilian market is close to that of many of the East Asian nations, it is less "out-of-sample" than Russia or the U.S.

VII. Summary and Conclusions

This paper investigates contagion across stock markets. The first half of the paper focuses on conceptual issues involved in analyzing this contagion. It clarifies the standard approach toward measuring contagion— testing for a significant increase in cross-country correlations in stock market returns during a period of crisis. Using this methodology, if two markets are highly correlated during periods of stability and they continue to co-move during a period of turmoil, this may not constitute contagion. The paper then discusses these conventional techniques in more depth and proves that the correlation coefficients central to previous analyses are biased. These unadjusted correlation coefficients are actually conditional on market movements over the time period under consideration, so that during a period of turmoil when stock market volatility increases, standard estimates of cross-market correlations will be biased upward. We show how to adjust the correlation coefficient to correct for this bias.

The remainder of the paper applies these concepts in several empirical tests for stock market contagion. We perform likelihood ratio tests to ascertain if average cross-market correlations increased significantly during the recent East Asian turmoil. Tests based on the unadjusted correlation coefficients suggest that there was contagion across about half of the world's stock markets during the recent East Asian turmoil. If the same tests are based on the adjusted correlation coefficients, however, the degree of contagion falls by as much as seventy-five percent. This suggests that the co-movement in global stock markets during the later half of 1997 was more a continuation of strong cross-market linkages than pure contagion from East Asia. Repeating these likelihood ratio tests for contagion after the Mexican peso crisis of 1994 and the U.S. stock market crash of 1997 shows that these key results apply to situations other than the recent East Asian turmoil. Compared to after the East Asian crisis, there was significantly less contagion after the Mexican peso crisis, and substantially more after the U.S. stock market crash. In each case, however, adjusting the correlation coefficient to adjust for bias from changing market volatility significantly affects the estimates of contagion.

The final section of the paper uses the results of these analyses to simulate the impact of stock market crashes in Brazil, Korea, Russia, or the U.S. on markets around the world. A crash in Brazil is predicted to have a large affect in Latin America and muted affects in the rest of the world, while a crash in Korea would have a medium-sized impact on markets across the globe. Given the limits of this analysis and the information available from the East Asian crisis, it is difficult to make out-of-sample predictions for the impact of a crash in Russia or the U.S.

Throughout the conceptual discussion in the beginning of the paper and the empirical results in the later half of the paper, two key themes continually emerge. First, when defining stock market contagion as a significant increase in market co-movement, it is critically important to measure this co-movement accurately. The unadjusted correlation coefficient is a biased measure of cross-market correlations, and adjusting this coefficient to correct for changing market volatility will not only affect estimates of cross-market correlations, but can significantly reduce estimates of stock market contagion. Second, when we apply these adjusted correlation coefficients to the standard tests for contagion, we find significantly less evidence of contagion during the recent East Asian turmoil than previously believed. This highlights the fact that we do not interpret contagion as a high degree of correlation between markets, but instead as a significant increase in this correlation after a shock. Therefore, in order to understand why a shock to East Asia is transmitted throughout the globe, instead of focusing on what explains the changes in market co-movements, we ought to focus on what explains the high level of integration across markets during periods of relative stability.

Appendix A: Proof of the Bias in the Unadjusted Correlation Coefficient[26]

R^i and R^j are the demeaned returns to stock markets *i* and *j*, calculated according to equation (4), so that $E(R^j)=0$. Assume R^i and R^j are bivariate normal.

Define: $$u \equiv \left[R^i R^j\right]'$$

and utilize the standard definition of an unconditional covariance matrix:

$$Cov(u,u')=\begin{bmatrix}\sigma_i^2 & \sigma_{i,j}\\ \sigma_{i,j} & \sigma_j^2\end{bmatrix}.$$

Then use the variance-decomposition rule that:

$$Cov = E(Conditional\ Covariance)+Var(Conditional\ Expectation)$$

$$Cov\left(u,u'\middle|(R^j)^2\right)=E\left[Cov\left(u,u'\middle|R^j\right)\middle|(R^j)^2\right]+Var\left[E\left(u\middle|R^j\right)\middle|(R^j)^2\right]$$

$$=E\left(\left[\begin{matrix}\sigma_i^2-\dfrac{\sigma_{i,j}^2}{\sigma_j^2} & 0\\ 0 & 0\end{matrix}\right]\middle|(R^j)^2\right)+Var\left(\left[\begin{matrix}\mu_i+\dfrac{\sigma_{i,j}}{\sigma_j^2}R^j\\ R^j\end{matrix}\right]\middle|(R^j)^2\right)$$

$$=\begin{bmatrix}\sigma_i^2-\dfrac{\sigma_{i,j}^2}{\sigma_j^2} & 0\\ 0 & 0\end{bmatrix}+(R^j)^2\begin{bmatrix}\dfrac{\sigma_{i,j}^2}{\sigma_j^4} & \dfrac{\sigma_{i,j}}{\sigma_j^2}\\ \dfrac{\sigma_{i,j}}{\sigma_j^2} & 1\end{bmatrix}$$

[26] Proof is based on that in Ronn (1998). Originally based on Stambaugh.

$$= \begin{bmatrix} \sigma_i^2 + \left(\dfrac{\left(R^j\right)^2 - \sigma_j^2}{\sigma_j^2} \right) \dfrac{\sigma_{i,j}^2}{\sigma_j^2} & \left(R^j\right)^2 \dfrac{\sigma_{i,j}}{\sigma_j^2} \\ \left(R^j\right)^2 \dfrac{\sigma_{i,j}}{\sigma_j^2} & \left(R^j\right)^2 \end{bmatrix}$$

$$= \begin{bmatrix} \left(1 + \delta \tilde{\rho}_{i,j}^2\right)\sigma_i^2 & (1+\delta)\sigma_{i,j} \\ (1+\delta)\sigma_{i,j} & (1+\delta)\sigma_j^2 \end{bmatrix}$$

where $\tilde{\rho}_{i,j} = \dfrac{\sigma_{i,j}}{\sigma_i \sigma_j}$ is the unconditional correlation and $\delta \equiv \dfrac{\left(R^j\right)^2 - \sigma_j^2}{\sigma_j^2}$.

And substituting these terms from the conditional covariance matrix into the conditional correlation matrix yields:

$$\rho_{i,j}^t = Corr\left(R^i R^j \middle| \left(R^j\right)^2 \right)$$

$$= \left(\frac{\sigma_{i,j}}{\sigma_i \sigma_j} \right) \Bigg| \left(R^j\right)^2$$

$$= \frac{(1+\delta)\sigma_{i,j}}{\left[\left(1 + \delta \tilde{\rho}_{i,j}^2\right)\sigma_i^2\right]^{1/2} \left[(1+\delta)\sigma_j^2\right]^{1/2}}$$

$$= \left(\frac{1+\delta}{1+\delta \tilde{\rho}_{i,j}^2} \right)^{1/2} \tilde{\rho}_{i,j}$$

Q.E.D.

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Table I
Stock Market Characteristics
(1996-Year end)

Region	*Country*	*Total Market Capitalization (in millions of US$)*	*Total Value Traded (in millions of US$)*
East Asia	Hong Kong	449,381	166,419
	Indonesia	91,016	32,142
	Japan	3,088,850	1,251,998
	Korea	138,817	177,266
	Malaysia	307,179	173,568
	Philippines	80,649	25,519
	Singapore	150,215	42,739
	Taiwan	273,608	470,193
	Thailand	99,828	44,365
Latin America	Argentina	44,679	n.a.
	Brazil	261,990	112,108
	Chile	65,940	8,460
	Mexico	106,540	43,040
OECD	Australia	311,988	145,482
	Belgium	119,831	26,120
	Canada	486,268	265,360
	France	591,123	277,100
	Germany	670,997	768,745
	Italy	258,160	102,351
	Netherlands	378,721	339,500
	Spain	242,779	249,128
	Sweden	247,217	136,898
	Switzerland	402,104	392,783
	United Kingdom	1,740,246	578,471
	United States	8,484,433	7,121,487
Other Emerging Markets	China	113,755	256,008
	India	122,605	109,448
	Russia	37,230	n.a.
	South Africa	241,571	27,202

Source: International Finance Corporation. *Emerging Stock Markets Factbook 1997.*

Table II
Did Correlations Change During the East Asian Turmoil?
Unadjusted Correlations in Local Currency

	Hon	Indo	Jap	Kor	Mal	Phi	Sin	Tai	Tha	Arg	Bra	Chil	Mex	Aus	Bel	Can	Fra	Ger	Ita	Net	Spa	Swe	Swi	UK	US	Chin	Indi	Rus	S.Af
Hon	**1.00**	0.30	0.22	0.06	0.43	0.29	0.53	0.17	0.32	0.15	0.10	0.17	0.15	0.40	0.25	0.24	0.22	0.36	0.12	0.30	0.20	0.23	0.25	0.26	0.15	0.01	0.04	0.16	0.19
Indo	0.51	**1.00**	0.14	0.10	0.29	0.31	0.41	0.05	0.29	0.08	0.04	0.09	0.10	0.19	0.20	0.13	0.16	0.17	0.05	0.21	0.11	0.13	0.17	0.18	0.10	0.00	0.10	0.12	0.17
Jap	0.52	0.35	**1.00**	-0.01	0.12	0.06	0.17	0.10	0.09	0.04	0.05	0.03	0.06	0.22	0.15	0.13	0.16	0.21	0.08	0.21	0.14	0.16	0.16	0.17	0.06	0.01	-0.01	0.03	0.15
Kor	0.11	0.12	0.30	**1.00**	0.13	0.11	0.04	0.06	0.11	0.10	0.05	0.10	0.11	0.12	0.10	0.12	0.06	0.07	0.05	0.09	0.06	0.09	0.05	0.13	0.10	0.03	0.07	0.09	0.06
Mal	0.38	0.49	0.12	0.19	**1.00**	0.25	0.54	0.14	0.32	0.10	0.04	0.10	0.10	0.21	0.12	0.14	0.10	0.15	0.03	0.14	0.07	0.12	0.14	0.17	0.08	-0.01	0.06	0.04	0.11
Phi	0.51	0.54	0.32	0.17	0.46	**1.00**	0.37	0.10	0.22	0.07	0.00	0.09	0.11	0.19	0.13	0.12	0.10	0.19	0.04	0.17	0.09	0.11	0.11	0.15	0.06	0.00	0.02	0.06	0.12
Sin	0.66	0.53	0.40	0.26	0.65	0.61	**1.00**	0.15	0.34	0.17	0.09	0.12	0.12	0.29	0.20	0.21	0.20	0.24	0.10	0.24	0.14	0.20	0.21	0.20	0.13	0.01	0.08	0.18	0.15
Tai	-0.01	0.26	0.04	0.29	0.17	0.23	0.20	**1.00**	0.10	0.00	0.04	0.05	0.06	0.10	0.05	0.03	0.04	0.07	0.05	0.08	0.04	0.08	0.09	0.05	0.00	0.00	0.02	-0.01	0.09
Tha	0.09	0.36	0.34	0.44	0.31	0.24	0.31	0.06	**1.00**	0.05	0.03	0.11	0.08	0.16	0.15	0.13	0.10	0.17	0.09	0.15	0.08	0.14	0.15	0.13	0.05	0.04	0.05	0.15	0.15
Arg	-0.09	-0.20	0.13	0.07	-0.07	-0.04	-0.07	-0.23	0.12	**1.00**	0.48	0.44	0.42	0.11	0.25	0.29	0.26	0.20	0.17	0.25	0.28	0.25	0.23	0.27	0.35	0.00	0.04	0.10	0.07
Bra	-0.05	-0.03	0.06	0.17	0.03	0.12	-0.05	-0.20	0.24	0.75	**1.00**	0.40	0.27	0.13	0.13	0.19	0.14	0.13	0.10	0.15	0.19	0.14	0.13	0.17	0.21	-0.01	0.09	0.12	0.06
Chil	0.31	0.23	0.31	0.22	-0.04	0.23	0.15	0.31	0.07	0.22	0.17	**1.00**	0.30	0.12	0.23	0.17	0.21	0.15	0.15	0.20	0.24	0.22	0.23	0.27	0.22	-0.01	0.07	0.06	0.10
Mex	0.00	-0.08	0.02	-0.01	0.02	0.03	-0.08	-0.26	-0.07	0.77	0.66	0.33	**1.00**	0.12	0.16	0.27	0.15	0.14	0.09	0.18	0.17	0.15	0.15	0.19	0.29	0.04	0.02	0.10	0.03
Aus	0.61	0.45	0.60	0.45	0.28	0.39	0.60	0.30	0.36	-0.09	-0.15	0.36	-0.13	**1.00**	0.29	0.23	0.21	0.38	0.14	0.35	0.23	0.33	0.28	0.23	0.12	-0.01	0.02	0.09	0.28
Bel	0.55	0.23	0.45	0.20	0.25	0.24	0.34	-0.04	0.18	0.21	0.24	0.55	0.32	0.50	**1.00**	0.25	0.55	0.55	0.35	0.61	0.45	0.48	0.53	0.51	0.24	0.02	0.04	0.14	0.23
Can	0.18	-0.02	0.11	0.11	0.07	0.07	-0.01	-0.20	-0.09	0.55	0.46	0.45	0.76	0.10	0.56	**1.00**	0.28	0.26	0.16	0.33	0.26	0.30	0.26	0.34	0.61	-0.02	0.02	0.12	0.14
Fra	0.65	0.28	0.56	0.34	0.26	0.35	0.45	0.01	0.30	0.12	0.16	0.49	0.20	0.65	0.87	0.48	**1.00**	0.53	0.43	0.66	0.63	0.53	0.57	0.64	0.29	0.03	0.02	0.08	0.21
Ger	0.64	0.41	0.48	0.28	0.26	0.38	0.48	0.17	0.28	-0.05	0.02	0.44	0.00	0.69	0.71	0.32	0.80	**1.00**	0.34	0.64	0.48	0.46	0.58	0.48	0.20	0.03	0.05	0.10	0.25
Ita	0.55	0.27	0.54	0.33	0.35	0.30	0.53	0.11	0.23	-0.07	-0.08	0.26	-0.02	0.63	0.69	0.29	0.78	0.67	**1.00**	0.41	0.42	0.35	0.36	0.37	0.17	0.01	0.04	0.05	0.11
Net	0.53	0.21	0.39	0.24	0.26	0.27	0.30	0.01	0.17	0.19	0.28	0.49	0.32	0.43	0.86	0.57	0.86	0.76	0.65	**1.00**	0.58	0.61	0.66	0.65	0.29	0.00	0.04	0.16	0.21
Spa	0.63	0.40	0.49	0.38	0.32	0.34	0.52	0.20	0.21	-0.15	-0.14	0.46	-0.03	0.74	0.71	0.33	0.86	0.75	0.79	0.69	**1.00**	0.50	0.51	0.55	0.26	0.01	0.03	0.12	0.18
Swe	0.60	0.21	0.48	0.34	0.26	0.33	0.44	0.01	0.21	0.11	0.11	0.45	0.19	0.63	0.81	0.52	0.90	0.78	0.77	0.83	0.84	**1.00**	0.55	0.54	0.26	-0.01	0.05	0.13	0.20
Swi	0.55	0.29	0.39	0.25	0.33	0.38	0.41	0.14	0.29	0.11	0.20	0.48	0.18	0.54	0.81	0.46	0.85	0.80	0.71	0.88	0.71	0.80	**1.00**	0.54	0.23	0.03	0.00	0.16	0.23
UK	0.59	0.28	0.39	0.22	0.25	0.40	0.43	-0.02	0.20	0.25	0.30	0.44	0.25	0.46	0.78	0.44	0.80	0.67	0.66	0.81	0.71	0.82	0.83	**1.00**	0.34	0.01	0.05	0.10	0.22
US	-0.10	-0.20	-0.12	-0.05	0.00	-0.04	-0.24	-0.23	-0.07	0.70	0.64	0.39	0.78	-0.22	0.39	0.82	0.24	0.03	0.01	0.41	0.04	0.25	0.27	0.29	**1.00**	-0.03	0.03	0.05	0.04
Chin	0.22	0.10	0.27	0.08	-0.01	0.15	0.10	0.05	0.07	-0.17	-0.11	-0.08	-0.33	0.20	0.07	-0.13	0.25	0.23	0.24	0.14	0.26	0.20	0.14	0.25	-0.21	**1.00**	0.06	0.00	0.01
Indi	0.19	0.29	0.00	-0.02	0.33	0.32	0.24	-0.17	0.26	0.19	0.10	-0.03	0.15	0.01	0.00	-0.01	0.04	-0.03	-0.06	-0.01	-0.01	0.00	0.02	0.09	0.09	-0.04	**1.00**	0.06	0.06
Rus	0.74	0.27	0.61	0.41	0.24	0.36	0.53	0.08	0.27	-0.08	-0.01	0.33	-0.08	0.75	0.69	0.21	0.82	0.76	0.75	0.67	0.76	0.80	0.71	0.63	-0.09	0.27	-0.05	**1.00**	0.10
S. Af	0.74	0.41	0.55	0.33	0.35	0.44	0.63	0.18	0.32	-0.04	-0.01	0.29	-0.17	0.76	0.62	0.13	0.72	0.77	0.72	0.58	0.71	0.71	0.70	0.67	-0.13	0.34	0.05	0.82	**1.00**

NOTES: Top triangle of matrix is average correlations during the period of stability (3/1/93 through 2/28/98, except the period of turmoil).
Bottom triangle of matrix is average correlations during the period of East Asian turmoil (8/7/97 through 11/6/97).

Table III
Did Correlations Change During the East Asian Turmoil?
Unadjusted Correlations in U.S. Dollars

	Hon	Indo	Jap	Kor	Mal	Phi	Sin	Tai	Tha	Arg	Bra	Chil	Mex	Aus	Bel	Can	Fra	Ger	Ita	Net	Spa	Swe	Swi	UK	US	Chin	Indi	Rus	S.Af
Hon	**1.00**	0.31	0.17	0.08	0.43	0.27	0.53	0.20	0.33	0.17	0.11	0.16	0.12	0.37	0.15	0.25	0.19	0.30	0.10	0.26	0.18	0.20	0.18	0.24	0.15	0.03	0.05	0.16	0.19
Indo	0.44	**1.00**	0.09	0.19	0.43	0.36	0.47	0.10	0.33	0.11	0.05	0.14	0.08	0.18	0.08	0.14	0.11	0.08	0.05	0.12	0.06	0.08	0.06	0.10	0.13	-0.01	0.06	0.12	0.14
Jap	0.40	0.21	**1.00**	-0.02	0.16	0.04	0.20	0.09	0.08	-0.01	0.00	0.02	0.05	0.16	0.28	0.07	0.19	0.29	0.06	0.29	0.15	0.16	0.25	0.20	0.01	0.02	-0.02	0.07	0.14
Kor	0.10	0.15	0.09	**1.00**	0.18	0.15	0.07	0.06	0.19	0.09	0.06	0.07	0.08	0.13	0.07	0.10	0.06	0.03	0.04	0.08	0.04	0.10	0.03	0.11	0.08	0.04	0.08	0.10	0.07
Mal	0.33	0.55	0.01	0.09	**1.00**	0.34	0.60	0.18	0.43	0.12	0.04	0.12	0.08	0.22	0.09	0.17	0.11	0.13	0.02	0.15	0.06	0.11	0.11	0.14	0.10	0.01	0.08	0.07	0.13
Phi	0.38	0.54	0.24	0.16	0.49	**1.00**	0.37	0.13	0.31	0.08	0.02	0.11	0.09	0.17	0.09	0.11	0.09	0.13	-0.01	0.11	0.04	0.06	0.06	0.11	0.06	0.02	0.02	0.06	0.11
Sin	0.61	0.56	0.28	0.19	0.65	0.59	**1.00**	0.19	0.40	0.18	0.07	0.14	0.10	0.28	0.15	0.22	0.19	0.20	0.06	0.23	0.12	0.16	0.15	0.19	0.13	0.02	0.08	0.18	0.15
Tai	-0.01	0.26	0.02	0.39	0.15	0.17	0.18	**1.00**	0.13	0.02	-0.01	0.06	0.08	0.12	0.02	0.08	0.02	0.02	0.02	0.05	0.01	0.06	0.04	0.04	0.00	-0.03	0.05	-0.01	0.09
Tha	0.03	0.21	0.13	0.47	0.29	0.31	0.25	0.10	**1.00**	0.07	0.02	0.12	0.06	0.17	0.10	0.14	0.09	0.14	0.06	0.13	0.07	0.11	0.09	0.11	0.05	0.05	0.06	0.16	0.14
Arg	-0.09	-0.28	0.19	0.08	-0.05	-0.05	-0.08	-0.20	0.09	**1.00**	0.66	0.43	0.44	0.11	0.09	0.29	0.19	0.09	0.13	0.14	0.23	0.18	0.09	0.17	0.35	0.01	0.05	0.08	0.09
Bra	-0.04	-0.08	0.10	0.18	0.06	0.08	-0.02	-0.17	0.18	0.75	**1.00**	0.52	0.44	0.09	0.08	0.22	0.15	0.08	0.15	0.13	0.22	0.19	0.02	0.16	0.28	-0.05	0.09	0.13	0.10
Chil	0.49	0.22	0.33	0.35	0.04	0.22	0.28	0.24	0.01	0.22	0.21	**1.00**	0.33	0.10	0.13	0.17	0.15	0.07	0.10	0.13	0.20	0.16	0.13	0.17	0.21	-0.05	0.09	0.08	0.09
Mex	0.39	0.05	0.22	0.26	0.16	0.16	0.19	-0.17	0.01	0.59	0.56	0.61	**1.00**	0.11	0.08	0.20	0.12	0.06	0.05	0.11	0.18	0.13	0.06	0.11	0.21	0.01	0.04	0.05	0.08
Aus	0.71	0.41	0.41	0.30	0.31	0.43	0.61	0.23	0.24	0.01	-0.04	0.55	0.47	**1.00**	0.12	0.26	0.14	0.25	0.11	0.23	0.14	0.22	0.15	0.22	0.10	-0.03	0.00	0.08	0.27
Bel	0.43	0.05	0.23	0.11	0.16	0.05	0.20	-0.02	0.01	0.31	0.27	0.51	0.52	0.49	**1.00**	0.12	0.52	0.63	0.24	0.66	0.45	0.39	0.60	0.46	0.11	0.04	0.00	0.15	0.21
Can	0.34	0.04	0.08	0.19	0.18	0.02	0.08	-0.17	-0.06	0.48	0.44	0.57	0.81	0.36	0.55	**1.00**	0.24	0.18	0.14	0.26	0.22	0.26	0.16	0.29	0.56	-0.03	0.05	0.10	0.16
Fra	0.62	0.18	0.38	0.30	0.18	0.24	0.35	0.04	0.18	0.18	0.18	0.60	0.57	0.68	0.78	0.55	**1.00**	0.54	0.36	0.63	0.59	0.47	0.54	0.56	0.23	0.06	0.00	0.09	0.21
Ger	0.62	0.39	0.41	0.27	0.19	0.24	0.41	0.23	0.21	-0.04	0.02	0.55	0.33	0.68	0.52	0.37	0.68	**1.00**	0.26	0.69	0.47	0.39	0.63	0.45	0.11	0.06	0.02	0.13	0.23
Ita	0.50	0.24	0.30	0.31	0.35	0.17	0.44	0.15	0.19	-0.09	-0.12	0.31	0.29	0.56	0.50	0.29	0.67	0.52	**1.00**	0.30	0.37	0.33	0.24	0.30	0.12	0.05	0.03	0.05	0.11
Net	0.53	0.17	0.20	0.20	0.20	0.14	0.21	0.02	0.03	0.28	0.35	0.57	0.62	0.44	0.68	0.68	0.74	0.62	0.44	**1.00**	0.53	0.52	0.68	0.61	0.20	0.02	0.02	0.18	0.22
Spa	0.58	0.35	0.31	0.35	0.21	0.26	0.38	0.25	0.16	-0.16	-0.19	0.52	0.30	0.68	0.56	0.33	0.80	0.66	0.71	0.52	**1.00**	0.46	0.45	0.48	0.22	0.03	0.02	0.13	0.15
Swe	0.64	0.22	0.33	0.33	0.27	0.25	0.39	0.08	0.22	0.15	0.11	0.61	0.59	0.70	0.65	0.61	0.86	0.70	0.66	0.78	0.76	**1.00**	0.43	0.46	0.19	0.03	0.01	0.12	0.17
Swi	0.41	0.26	0.20	0.15	0.31	0.19	0.31	0.10	0.13	0.22	0.26	0.46	0.41	0.42	0.68	0.54	0.71	0.65	0.50	0.79	0.54	0.68	**1.00**	0.48	0.12	0.06	-0.01	0.16	0.22
UK	0.51	0.07	0.09	0.09	0.20	0.20	0.38	-0.05	0.09	0.41	0.37	0.37	0.57	0.50	0.55	0.52	0.64	0.43	0.42	0.67	0.48	0.67	0.59	**1.00**	0.27	0.07	0.04	0.10	0.20
US	-0.10	-0.18	-0.15	-0.07	0.06	-0.04	-0.20	-0.23	-0.13	0.70	0.64	0.34	0.60	-0.13	0.33	0.72	0.18	-0.06	-0.11	0.43	-0.07	0.16	0.32	0.35	**1.00**	-0.02	0.04	0.04	0.06
Chin	0.22	0.08	0.17	0.09	-0.03	0.12	0.06	0.03	0.08	-0.17	-0.10	0.05	-0.11	0.21	-0.06	-0.05	0.21	0.19	0.22	0.08	0.22	0.16	0.02	0.12	-0.21	**1.00**	0.06	-0.01	0.02
Indi	0.17	0.20	0.05	-0.02	0.27	0.33	0.24	-0.19	0.23	0.19	0.10	-0.05	0.11	0.05	-0.07	-0.02	0.00	-0.08	-0.12	-0.06	-0.05	-0.02	-0.04	0.12	0.09	-0.05	**1.00**	0.05	0.07
Rus	0.75	0.23	0.37	0.40	0.22	0.28	0.46	0.09	0.22	-0.08	0.01	0.58	0.43	0.75	0.48	0.40	0.74	0.70	0.67	0.63	0.67	0.79	0.50	0.46	-0.09	0.27	-0.06	**1.00**	0.07
S. Af	0.74	0.37	0.31	0.34	0.34	0.33	0.56	0.20	0.22	-0.05	0.03	0.47	0.31	0.75	0.42	0.30	0.64	0.71	0.64	0.51	0.61	0.69	0.51	0.59	-0.14	0.33	0.02	0.82	**1.00**

NOTES: Top triangle of matrix is average correlations during the period of stability (3/1/93 through 2/28/98, except the period of turmoil).
Bottom triangle of matrix is average correlations during the period of East Asian turmoil (8/7/97 through 11/6/97).

Table IV
Contagion (C) During the East Asian Turmoil or Not (N)?
Unadjusted Correlations Based on Local Currencies

	Hon	Indo	Jap	Kor	Mal	Phi	Sin	Tai	Tha	Arg	Bra	Chil	Mex	Aus	Bel	Can	Fra	Ger	Ita	Net	Spa	Swe	Swi	UK	US	Chin	Indi	Rus
Indo	C																											
Jap	C	C																										
Kor	N	N	C																									
Mal	N	C	N	N																								
Phi	C	C	C	N	C																							
Sin	C	C	C	C	C	C																						
Tai	N	C	N	C	N	N	N																					
Tha	N	N	C	C	N	N	N	N																				
Arg	N	N	N	N	N	N	N	N	N																			
Bra	N	N	N	N	N	N	N	N	C	C																		
Chil	C	N	C	N	N	N	N	C	N	N	N																	
Mex	N	N	N	N	N	N	N	N	N	C	C	N																
Aus	C	C	C	C	N	C	C	C	C	N	N	C	N															
Bel	C	N	C	N	N	N	C	N	N	N	N	C	C	C														
Can	N	N	N	N	N	N	N	N	N	C	C	C	C	N	C													
Fra	C	N	C	C	C	C	C	N	C	N	N	C	N	C	C	C												
Ger	C	C	C	C	N	C	C	N	N	N	N	C	N	C	C	N	C											
Ita	C	C	C	C	C	C	C	N	N	N	N	N	N	C	C	N	C	C										
Net	C	N	C	C	N	N	N	N	N	N	N	C	C	C	C	C	C	C	C									
Spa	C	C	C	C	C	C	C	N	N	N	N	C	N	C	C	N	C	C	C	C								
Swe	C	N	C	C	N	C	C	N	N	N	N	C	N	C	C	C	C	C	C	C	C							
Swi	C	N	C	C	C	C	C	N	C	N	N	C	N	C	C	C	C	C	C	C	C	C						
UK	C	N	C	N	N	C	C	N	N	N	C	C	N	C	C	C	C	C	C	C	C	C	C					
US	N	N	N	N	N	N	N	N	N	C	C	C	C	N	C	C	N	N	N	C	N	N	N	N				
Chin	C	N	C	N	N	N	N	N	N	N	N	N	N	C	N	N	C	C	C	N	C	C	N	C	N			
Indi	N	C	N	N	C	C	C	N	C	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N		
Rus	C	C	C	C	C	C	C	N	N	N	N	C	N	C	C	N	C	C	C	C	C	C	C	C	N	C	N	
S. Af	C	C	C	C	C	C	C	N	C	N	N	C	N	C	C	N	C	C	C	C	C	C	C	C	N	C	N	C

NOTES: The period of East Asian turmoil is defined as lasting from 8/7/97 through 11/6/97. Test procedure described in text.
The null hypothesis is that the correlation coefficient during the period of stability is greater than or equal that during the period of turmoil.
"N" suggests no contagion since we are unable to reject the null hypothesis. "C" suggests contagion since we are able to reject the null.

Table V
Contagion (C) During the East Asian Turmoil or Not (N)?
Unadjusted Correlations Based on U.S. Dollars

	Hon	Indo	Jap	Kor	Mal	Phi	Sin	Tai	Tha	Arg	Bra	Chil	Mex	Aus	Bel	Can	Fra	Ger	Ita	Net	Spa	Swe	Swi	UK	US	Chin	Indi	Rus
Indo	C																											
Jap	C	N																										
Kor	N	N	N																									
Mal	N	C	N	N																								
Phi	C	C	C	N	C																							
Sin	C	C	N	N	C	C																						
Tai	N	C	N	C	N	N	N																					
Tha	N	N	N	C	N	N	N	N																				
Arg	N	N	C	N	N	N	N	N	N																			
Bra	N	N	N	N	N	N	N	N	N	C																		
Chil	C	N	C	C	N	N	C	C	N	N	N																	
Mex	C	N	C	C	N	N	N	N	N	C	C	C																
Aus	C	C	C	C	N	C	C	N	N	N	N	C	C															
Bel	C	N	N	N	N	N	N	N	N	C	C	C	C	C														
Can	N	N	N	N	N	N	N	N	N	C	C	C	C	N	C													
Fra	C	N	C	C	N	C	C	N	N	N	N	C	C	C	C	C												
Ger	C	C	C	C	N	N	C	C	N	N	N	C	C	C	N	C	C											
Ita	C	C	C	C	C	N	C	N	N	N	N	C	C	C	C	C	C	C										
Net	C	N	N	N	N	N	N	N	N	C	C	C	C	C	N	C	C	N	C									
Spa	C	C	C	C	N	C	C	C	N	N	N	C	C	C	C	N	C	C	C	N								
Swe	C	N	C	C	C	N	C	N	N	N	N	C	C	C	C	C	C	C	C	C	C							
Swi	C	C	N	N	C	N	C	N	N	N	C	C	C	C	C	C	C	N	C	C	C	C						
UK	C	N	N	N	N	N	C	N	N	C	C	C	C	C	C	C	C	N	C	C	N	C	C					
US	N	N	N	N	N	N	N	N	N	C	C	C	C	N	C	C	N	N	N	C	N	N	C	N				
Chin	C	N	N	N	N	N	N	N	N	N	N	N	N	C	N	N	N	N	C	N	C	N	N	N	N			
Indi	N	N	N	N	C	C	C	N	C	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N		
Rus	C	N	C	C	N	N	C	N	N	N	N	C	C	C	C	C	C	C	C	C	C	C	C	C	N	C	N	
S.Af	C	C	C	C	C	C	C	N	N	N	N	C	C	C	C	C	C	C	C	C	C	C	C	C	N	C	N	C

NOTES: The period of East Asian turmoil is defined as lasting from 8/7/97 through 11/6/97. Test procedure described in text.
The null hypothesis is that the correlation coefficient during the period of stability is greater than or equal that during the period of turmoil.
"N" suggests no contagion since we are unable to reject the null hypothesis. "C" suggests contagion since we are able to reject the null.

Table VI
Did Correlations Change During the East Asian Turmoil?
Adjusted Correlations in Local Currency

	Hon	Indo	Jap	Kor	Mal	Phi	Sin	Tai	Tha	Arg	Bra	Chil	Mex	Aus	Bel	Can	Fra	Ger	Ita	Net	Spa	Swe	Swi	UK	US	Chin	Indi	Rus	S.Af
Hon	**1.00**	0.33	0.22	0.06	0.46	0.30	0.56	0.17	0.33	0.16	0.10	0.17	0.16	0.43	0.27	0.24	0.23	0.40	0.12	0.34	0.22	0.24	0.28	0.28	0.16	0.01	0.04	0.18	0.22
Indo	0.26	**1.00**	0.14	0.10	0.31	0.33	0.43	0.06	0.30	0.08	0.04	0.09	0.10	0.21	0.22	0.13	0.17	0.19	0.06	0.24	0.11	0.14	0.19	0.18	0.11	0.00	0.10	0.13	0.20
Jap	0.46	0.30	**1.00**	-0.01	0.13	0.06	0.18	0.11	0.09	0.05	0.05	0.03	0.06	0.24	0.16	0.14	0.17	0.24	0.08	0.24	0.15	0.17	0.18	0.18	0.06	0.01	-0.01	0.03	0.17
Kor	0.07	0.08	0.19	**1.00**	0.14	0.12	0.04	0.06	0.11	0.10	0.05	0.10	0.11	0.13	0.11	0.12	0.07	0.08	0.05	0.11	0.06	0.09	0.06	0.14	0.10	0.03	0.07	0.10	0.07
Mal	0.23	0.31	0.07	0.11	**1.00**	0.26	0.56	0.14	0.33	0.10	0.04	0.10	0.10	0.23	0.13	0.14	0.11	0.17	0.03	0.17	0.07	0.13	0.16	0.18	0.09	-0.01	0.06	0.05	0.13
Phi	0.31	0.33	0.18	0.10	0.27	**1.00**	0.39	0.10	0.22	0.08	0.00	0.09	0.11	0.21	0.14	0.12	0.11	0.21	0.04	0.19	0.09	0.11	0.12	0.15	0.06	0.00	0.02	0.07	0.14
Sin	0.44	0.33	0.24	0.15	0.43	0.40	**1.00**	0.15	0.35	0.18	0.09	0.12	0.13	0.31	0.22	0.22	0.21	0.26	0.10	0.27	0.15	0.20	0.22	0.21	0.14	0.01	0.08	0.20	0.17
Tai	-0.01	0.19	0.03	0.22	0.12	0.17	0.15	**1.00**	0.10	0.00	0.04	0.05	0.06	0.11	0.06	0.04	0.05	0.08	0.05	0.09	0.04	0.08	0.10	0.05	0.00	0.00	0.02	-0.01	0.10
Tha	0.06	0.25	0.24	0.31	0.21	0.17	0.21	0.04	**1.00**	0.06	0.03	0.11	0.08	0.18	0.17	0.13	0.10	0.19	0.09	0.17	0.09	0.14	0.17	0.14	0.06	0.04	0.05	0.17	0.17
Arg	-0.08	-0.17	0.11	0.06	-0.05	-0.03	-0.06	-0.19	0.10	**1.00**	0.48	0.44	0.43	0.11	0.27	0.29	0.28	0.22	0.17	0.29	0.30	0.27	0.25	0.28	0.37	0.00	0.04	0.11	0.08
Bra	-0.05	-0.02	0.06	0.15	0.03	0.11	-0.04	-0.18	0.22	0.71	**1.00**	0.40	0.27	0.14	0.14	0.19	0.15	0.15	0.11	0.17	0.20	0.15	0.14	0.18	0.22	-0.01	0.09	0.13	0.07
Chil	0.38	0.28	0.38	0.27	-0.05	0.28	0.18	0.38	0.09	0.27	0.21	**1.00**	0.30	0.13	0.25	0.17	0.22	0.17	0.15	0.23	0.26	0.23	0.25	0.28	0.23	-0.01	0.07	0.06	0.11
Mex	0.00	-0.06	0.01	-0.01	0.01	0.02	-0.06	-0.19	-0.05	0.67	0.54	0.25	**1.00**	0.13	0.18	0.28	0.16	0.16	0.09	0.21	0.18	0.16	0.17	0.20	0.31	0.04	0.02	0.11	0.03
Aus	0.36	0.24	0.35	0.25	0.15	0.21	0.36	0.16	0.19	-0.04	-0.08	0.19	-0.07	**1.00**	0.31	0.23	0.22	0.42	0.15	0.39	0.24	0.35	0.30	0.25	0.13	-0.01	0.02	0.10	0.31
Bel	0.32	0.12	0.25	0.10	0.13	0.12	0.18	-0.02	0.10	0.11	0.13	0.32	0.17	0.29	**1.00**	0.25	0.57	0.60	0.36	0.67	0.48	0.50	0.57	0.53	0.25	0.02	0.04	0.15	0.26
Can	0.14	-0.02	0.09	0.09	0.05	0.06	-0.01	-0.16	-0.07	0.45	0.38	0.36	0.67	0.08	0.46	**1.00**	0.29	0.29	0.16	0.37	0.28	0.31	0.28	0.35	0.63	-0.02	0.02	0.13	0.15
Fra	0.44	0.17	0.36	0.21	0.15	0.21	0.28	0.01	0.18	0.07	0.09	0.31	0.12	0.44	0.72	0.30	**1.00**	0.58	0.44	0.71	0.65	0.55	0.61	0.66	0.31	0.03	0.02	0.09	0.23
Ger	0.35	0.20	0.24	0.13	0.12	0.18	0.24	0.08	0.13	-0.02	0.01	0.21	0.00	0.40	0.41	0.15	0.51	**1.00**	0.35	0.69	0.50	0.48	0.62	0.49	0.21	0.02	0.05	0.12	0.28
Ita	0.43	0.20	0.42	0.24	0.26	0.22	0.41	0.08	0.16	-0.05	-0.06	0.19	-0.01	0.50	0.57	0.21	0.67	0.54	**1.00**	0.46	0.44	0.36	0.39	0.39	0.18	0.01	0.04	0.06	0.13
Net	0.26	0.09	0.18	0.11	0.11	0.12	0.13	0.01	0.07	0.08	0.12	0.23	0.14	0.20	0.57	0.28	0.58	0.44	0.34	**1.00**	0.61	0.63	0.70	0.67	0.31	0.00	0.04	0.18	0.24
Spa	0.40	0.23	0.29	0.21	0.18	0.19	0.31	0.11	0.11	-0.08	-0.08	0.27	-0.02	0.51	0.48	0.19	0.68	0.52	0.58	0.46	**1.00**	0.52	0.55	0.57	0.28	0.01	0.03	0.14	0.21
Swe	0.42	0.13	0.32	0.21	0.16	0.21	0.28	0.01	0.13	0.07	0.07	0.30	0.11	0.44	0.64	0.35	0.79	0.60	0.60	0.67	0.69	**1.00**	0.58	0.56	0.28	-0.01	0.05	0.14	0.23
Swi	0.30	0.15	0.20	0.13	0.17	0.20	0.21	0.07	0.15	0.06	0.10	0.26	0.09	0.30	0.56	0.25	0.63	0.55	0.45	0.68	0.45	0.55	**1.00**	0.56	0.24	0.03	0.00	0.18	0.25
UK	0.41	0.17	0.25	0.14	0.16	0.26	0.28	-0.01	0.12	0.15	0.19	0.28	0.15	0.30	0.61	0.29	0	0.48	0.47	0.65	0.52	0.66	0.67	**1.00**	0.36	0.01	0.05	0.11	0.24
US	-0.06	-0.12	-0.07	-0.03	0.00	-0.02	-0.14	-0.13	-0.04	0.49	0.43	0.23	0.58	-0.13	0.24	0.63	0.14	0.02	0.01	0.25	0.02	0.14	0.16	0.17	**1.00**	-0.03	0.03	0.05	0.04
Chin	0.30	0.14	0.36	0.12	-0.02	0.20	0.14	0.07	0.10	-0.24	-0.15	-0.12	-0.44	0.28	0.10	-0.18	0.34	0.31	0.33	0.19	0.35	0.28	0.20	0.34	-0.28	**1.00**	0.06	0.00	0.01
Indi	0.19	0.29	0.00	-0.02	0.33	0.31	0.24	-0.17	0.26	0.19	0.10	-0.03	0.15	0.01	0.00	-0.01	0.04	-0.03	-0.06	-0.01	-0.01	0.00	0.02	0.09	0.09	-0.04	**1.00**	0.06	0.07
Rus	0.50	0.15	0.38	0.23	0.13	0.20	0.32	0.04	0.15	-0.04	0.00	0.18	-0.04	0.52	0.45	0.12	0.61	0.53	0.52	0.44	0.53	0.58	0.48	0.40	-0.05	0.15	-0.03	**1.00**	0.13
S. Af	0.44	0.19	0.28	0.15	0.16	0.21	0.34	0.08	0.15	-0.02	0.00	0.13	-0.08	0.46	0.33	0.06	0.42	0.47	0.41	0.30	0.40	0.41	0.40	0.37	-0.06	0.16	0.02	0.54	**1.00**

NOTES: Top triangle of matrix is average correlations during the period of stability (3/1/93 through 2/28/98, except the period of turmoil).
Bottom triangle of matrix is average correlations during the period of East Asian turmoil (8/7/97 through 11/6/97).

Table VII
Did Correlations Change During the East Asian Turmoil?
Adjusted Correlations in U.S. Dollars

	Hon	Indo	Jap	Kor	Mal	Phi	Sin	Tai	Tha	Arg	Bra	Chil	Mex	Aus	Bel	Can	Fra	Ger	Ita	Net	Spa	Swe	Swi	UK	US	Chin	Indi	Rus	S.Af
Hon	**1.00**	0.33	0.17	0.08	0.46	0.30	0.55	0.20	0.35	0.18	0.11	0.16	0.12	0.38	0.15	0.25	0.19	0.31	0.10	0.27	0.19	0.21	0.18	0.24	0.16	0.03	0.05	0.18	0.20
Indo	0.27	**1.00**	0.09	0.19	0.46	0.39	0.49	0.11	0.35	0.11	0.05	0.14	0.08	0.19	0.08	0.15	0.11	0.08	0.05	0.13	0.06	0.08	0.06	0.10	0.13	-0.01	0.06	0.13	0.16
Jap	0.34	0.18	**1.00**	-0.02	0.17	0.04	0.20	0.09	0.09	-0.01	0.00	0.02	0.05	0.17	0.29	0.07	0.19	0.30	0.06	0.31	0.16	0.16	0.26	0.20	0.01	0.02	-0.02	0.08	0.15
Kor	0.09	0.13	0.08	**1.00**	0.19	0.16	0.07	0.06	0.20	0.09	0.06	0.07	0.08	0.13	0.07	0.10	0.06	0.03	0.04	0.09	0.04	0.10	0.04	0.12	0.08	0.04	0.08	0.11	0.07
Mal	0.18	0.33	0.00	0.05	**1.00**	0.37	0.62	0.18	0.45	0.12	0.04	0.12	0.08	0.23	0.09	0.18	0.11	0.13	0.02	0.16	0.06	0.11	0.11	0.14	0.10	0.01	0.08	0.08	0.14
Phi	0.20	0.30	0.12	0.08	0.27	**1.00**	0.39	0.13	0.32	0.09	0.02	0.11	0.09	0.17	0.09	0.11	0.09	0.14	-0.01	0.12	0.04	0.07	0.07	0.11	0.06	0.02	0.02	0.06	0.13
Sin	0.42	0.38	0.17	0.12	0.45	0.40	**1.00**	0.19	0.42	0.18	0.07	0.14	0.10	0.29	0.16	0.22	0.19	0.21	0.06	0.25	0.12	0.16	0.16	0.20	0.14	0.02	0.08	0.20	0.16
Tai	-0.01	0.18	0.02	0.28	0.11	0.12	0.13	**1.00**	0.14	0.02	-0.01	0.06	0.08	0.13	0.02	0.08	0.02	0.03	0.02	0.05	0.01	0.06	0.04	0.04	0.00	-0.03	0.05	-0.01	0.10
Tha	0.01	0.12	0.07	0.28	0.17	0.18	0.14	0.06	**1.00**	0.07	0.02	0.12	0.06	0.18	0.10	0.14	0.10	0.14	0.06	0.14	0.08	0.11	0.10	0.11	0.06	0.05	0.06	0.17	0.15
Arg	-0.08	-0.24	0.16	0.06	-0.04	-0.04	-0.07	-0.17	0.08	**1.00**	0.67	0.43	0.44	0.12	0.09	0.30	0.19	0.09	0.13	0.15	0.24	0.19	0.10	0.17	0.37	0.01	0.05	0.09	0.10
Bra	-0.03	-0.07	0.08	0.15	0.05	0.07	-0.01	-0.14	0.15	0.69	**1.00**	0.51	0.39	0.10	0.08	0.22	0.15	0.08	0.17	0.13	0.24	0.21	0.02	0.18	0.28	-0.06	0.09	0.15	0.12
Chil	0.57	0.26	0.39	0.42	0.05	0.26	0.34	0.29	0.01	0.27	0.25	**1.00**	0.33	0.10	0.13	0.17	0.16	0.07	0.10	0.14	0.20	0.17	0.13	0.17	0.22	-0.05	0.09	0.08	0.10
Mex	0.39	0.05	0.23	0.26	0.16	0.16	0.20	-0.17	0.01	0.60	0.56	0.62	**1.00**	0.12	0.09	0.21	0.12	0.07	0.05	0.12	0.18	0.13	0.06	0.12	0.22	0.01	0.04	0.06	0.09
Aus	0.52	0.27	0.26	0.19	0.20	0.28	0.42	0.15	0.15	0.00	-0.02	0.38	0.31	**1.00**	0.13	0.26	0.14	0.26	0.11	0.24	0.14	0.22	0.15	0.22	0.11	-0.03	0.00	0.09	0.30
Bel	0.33	0.04	0.17	0.08	0.12	0.04	0.15	-0.02	0.01	0.23	0.21	0.40	0.41	0.38	**1.00**	0.13	0.53	0.66	0.24	0.69	0.46	0.40	0.61	0.46	0.12	0.04	0.00	0.17	0.24
Can	0.28	0.03	0.07	0.15	0.15	0.02	0.06	-0.14	-0.05	0.41	0.38	0.50	0.75	0.30	0.47	**1.00**	0.24	0.19	0.14	0.27	0.23	0.26	0.16	0.29	0.58	-0.03	0.05	0.11	0.18
Fra	0.49	0.13	0.28	0.21	0.13	0.17	0.25	0.03	0.13	0.13	0.13	0.47	0.44	0.55	0.66	0.42	**1.00**	0.56	0.36	0.65	0.61	0.48	0.55	0.56	0.24	0.06	0.00	0.10	0.23
Ger	0.41	0.24	0.24	0.15	0.11	0.14	0.24	0.14	0.12	-0.02	0.01	0.35	0.20	0.46	0.32	0.22	0.47	**1.00**	0.26	0.72	0.49	0.40	0.64	0.46	0.12	0.06	0.02	0.14	0.26
Ita	0.46	0.21	0.27	0.28	0.32	0.15	0.40	0.13	0.17	-0.08	-0.10	0.28	0.26	0.51	0.45	0.26	0.62	0.47	**1.00**	0.32	0.38	0.33	0.25	0.30	0.13	0.05	0.03	0.05	0.12
Net	0.32	0.09	0.11	0.11	0.11	0.07	0.11	0.01	0.02	0.15	0.20	0.35	0.39	0.25	0.44	0.44	0.51	0.39	0.25	**1.00**	0.55	0.53	0.69	0.61	0.21	0.02	0.02	0.20	0.24
Spa	0.41	0.23	0.20	0.24	0.14	0.17	0.25	0.16	0.10	-0.10	-0.13	0.36	0.20	0.51	0.40	0.22	0.65	0.49	0.55	0.37	**1.00**	0.47	0.46	0.49	0.23	0.03	0.02	0.14	0.17
Swe	0.55	0.17	0.27	0.27	0.22	0.20	0.32	0.06	0.17	0.12	0.09	0.52	0.50	0.61	0.56	0.53	0.80	0.62	0.58	0.70	0.68	**1.00**	0.45	0.47	0.20	0.03	0.01	0.13	0.19
Swi	0.29	0.18	0.14	0.10	0.22	0.13	0.21	0.07	0.09	0.15	0.18	0.33	0.29	0.30	0.53	0.40	0.57	0.50	0.36	0.66	0.39	0.53	**1.00**	0.48	0.12	0.06	-0.01	0.17	0.25
UK	0.44	0.06	0.08	0.08	0.16	0.17	0.33	-0.05	0.07	0.36	0.31	0.32	0.50	0.44	0.48	0.46	0.58	0.37	0.36	0.60	0.41	0.61	0.52	**1.00**	0.29	0.07	0.04	0.11	0.22
US	-0.06	-0.10	-0.09	-0.04	0.03	-0.02	-0.12	-0.13	-0.08	0.48	0.43	0.20	0.40	-0.08	0.19	0.51	0.10	-0.04	-0.06	0.26	-0.04	0.09	0.19	0.21	**1.00**	-0.02	0.04	0.04	0.07
Chin	0.32	0.12	0.24	0.13	-0.04	0.17	0.08	0.05	0.12	-0.25	-0.14	0.07	-0.16	0.30	-0.08	-0.07	0.30	0.27	0.31	0.12	0.32	0.24	0.03	0.18	-0.30	**1.00**	0.06	-0.01	0.03
Indi	0.17	0.20	0.05	-0.02	0.28	0.35	0.24	-0.20	0.24	0.20	0.10	-0.05	0.12	0.05	-0.07	-0.02	0.00	-0.09	-0.13	-0.06	-0.05	-0.02	-0.05	0.12	0.09	-0.06	**1.00**	0.05	0.08
Rus	0.53	0.13	0.21	0.24	0.13	0.16	0.27	0.05	0.12	-0.04	0.01	0.36	0.26	0.53	0.29	0.24	0.52	0.48	0.45	0.41	0.44	0.58	0.31	0.27	-0.05	0.15	-0.03	**1.00**	0.09
S. Af	0.47	0.19	0.15	0.17	0.17	0.17	0.31	0.10	0.11	-0.02	0.01	0.25	0.15	0.47	0.22	0.15	0.37	0.44	0.38	0.28	0.35	0.42	0.28	0.33	-0.07	0.17	0.01	0.57	**1.00**

NOTES: Top triangle of matrix is average correlations during the period of stability (3/1/93 through 2/28/98, except the period of turmoil).
Bottom triangle of matrix is average correlations during the period of East Asian turmoil (8/7/97 through 11/6/97).

Table VIII
Contagion (C) During the East Asian Turmoil or Not (N)?
Adjusted Correlations Based on Local Currencies

	Hon	Indo	Jap	Kor	Mal	Phi	Sin	Tai	Tha	Arg	Bra	Chil	Mex	Aus	Bel	Can	Fra	Ger	Ita	Net	Spa	Swe	Swi	UK	US	Chin	Indi	Rus
Indo	N																											
Jap	C	C																										
Kor	N	N	C																									
Mal	N	N	N	N																								
Phi	N	N	N	N	N																							
Sin	N	N	N	N	N	N																						
Tai	N	N	N	C	N	N	N																					
Tha	N	N	N	C	N	N	N	N																				
Arg	N	N	N	N	N	N	N	N	N																			
Bra	N	N	N	N	N	N	N	N	C	C																		
Chil	C	C	C	C	N	C	N	C	N	N	N																	
Mex	N	N	N	N	N	N	N	N	N	C	C	N																
Aus	N	N	C	N	N	N	N	N	N	N	N	N	N															
Bel	N	N	N	N	N	N	N	N	N	N	N	N	N	N														
Can	N	N	N	N	N	N	N	N	N	C	C	C	C	N	C													
Fra	C	N	C	N	N	N	N	N	N	N	N	N	N	C	C	N												
Ger	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N											
Ita	C	N	C	C	C	C	C	N	N	N	N	N	N	C	C	N	C	C										
Net	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N									
Spa	C	N	C	N	N	N	C	N	N	N	N	N	N	C	N	N	N	N	C	N								
Swe	C	N	C	N	N	C	N	N	N	N	N	N	N	C	C	N	C	C	C	C	C							
Swi	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N						
UK	C	N	N	N	N	N	N	N	N	N	N	N	N	N	C	N	N	N	C	N	N	C	C					
US	N	N	N	N	N	N	N	N	N	C	C	N	C	N	N	N	N	N	N	N	N	N	N	N				
Chin	C	N	C	N	N	C	N	N	N	N	N	N	N	C	N	N	C	C	C	C	C	C	N	C	N			
Indi	N	C	N	N	C	C	C	N	C	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N		
Rus	C	N	C	N	N	C	N	N	N	N	N	N	N	C	C	N	C	C	C	C	C	C	C	C	N	N	N	
S. Af	C	N	N	N	N	N	C	N	N	N	N	N	N	C	N	N	C	C	C	N	C	C	C	C	N	N	N	C

NOTES: The period of East Asian turmoil is defined as lasting from 8/7/97 through 11/6/97. Test procedure described in text.
The null hypothesis is that the correlation coefficient during the period of stability is greater than or equal that during the period of turmoil.
"N" suggests no contagion since we are unable to reject the null hypothesis. "C" suggests contagion since we are able to reject the null.

Table IX

Contagion (C) During the East Asian Turmoil or Not (N)?

Adjusted Correlations Based on U.S. Dollars

	Hon	Indo	Jap	Kor	Mal	Phi	Sin	Tai	Tha	Arg	Bra	Chil	Mex	Aus	Bel	Can	Fra	Ger	Ita	Net	Spa	Swe	Swi	UK	US	Chin	Indi	Rus
Indo	N																											
Jap	C	N																										
Kor	N	N	N																									
Mal	N	N	N	N																								
Phi	N	N	N	N	N																							
Sin	N	N	N	N	N	N																						
Tai	N	N	N	C	N	N	N																					
Tha	N	N	N	N	N	N	N	N																				
Arg	N	N	N	N	N	N	N	N	N																			
Bra	N	N	N	N	N	N	N	N	N	N																		
Chil	C	N	C	C	N	C	C	C	N	N	N																	
Mex	C	N	C	C	N	N	N	N	N	C	C	C																
Aus	C	N	N	N	N	N	C	N	N	N	N	C	C															
Bel	C	N	N	N	N	N	N	N	N	N	N	C	C	C														
Can	N	N	N	N	N	N	N	N	N	C	C	C	C	N	C													
Fra	C	N	N	N	N	N	N	N	N	N	N	C	C	C	C	C												
Ger	C	C	N	N	N	N	N	N	N	N	N	C	N	C	N	N	N											
Ita	C	N	C	C	C	N	C	N	N	N	N	C	C	C	C	N	C	C										
Net	N	N	N	N	N	N	N	N	N	N	N	C	C	N	N	C	N	N	N									
Spa	C	C	N	C	N	N	N	N	N	N	N	C	N	C	N	N	C	N	C	N								
Swe	C	N	N	C	N	N	C	N	N	N	N	C	C	C	C	C	C	C	C	C	C							
Swi	N	N	N	N	N	N	N	N	N	N	N	C	C	C	N	C	N	N	C	N	N	C						
UK	C	N	N	N	N	N	C	N	N	C	C	C	C	C	N	C	N	N	N	N	N	C	N					
US	N	N	N	N	N	N	N	N	N	C	C	N	C	N	N	N	N	N	N	N	N	N	N	N				
Chin	C	N	C	N	N	N	N	N	N	N	N	N	N	C	N	N	C	C	C	N	C	C	N	N	N			
Indi	N	N	N	N	C	C	C	N	C	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N		
Rus	C	N	N	N	N	N	N	N	N	N	N	C	C	C	N	N	C	C	C	C	C	C	C	C	N	N	N	
S. Af	C	N	N	N	N	N	C	N	N	N	N	C	N	C	N	N	C	C	C	N	C	C	N	N	N	N	N	C

NOTES: The period of East Asian turmoil is defined as lasting from 8/7/97 through 11/6/97. Test procedure described in text.
The null hypothesis is that the correlation coefficient during the period of stability is greater than or equal that during the period of turmoil.
"N" suggests no contagion since we are unable to reject the null hypothesis. "C" suggests contagion since we are able to reject the null.

Table X
Sensitivity Analysis: Contagion During the East Asian Turmoil

					Market Pairs showing Contagion			
				Total Possible	Unadjusted Correlations		Adjusted Correlations	
Sample	Returns	Entire Period[1]	Turmoil Period	Market Pairs	Local Currency	U.S. Dollars	Local Currency	U.S. Dollars
29 markets	Daily	03/01/93 to 02/28/98	08/07/97 to 11/06/97	406	211 (52%)	203 (50%)	98 (24%)	123 (30%)
10 markets	Daily	03/01/93 to 02/28/98	08/07/97 to 11/06/97	45	34 (76%)	30 (67%)	7 (16%)	13 (29%)
29 markets	Weekly	03/01/93 to 02/28/98	08/07/97 to 11/06/97	406	183 (45%)	142 (35%)	108 (27%)	105 (26%)
29 markets	Daily	03/01/93 to 02/28/98	10/17/97 to 11/16/97	406	223 (55%)	230 (57%)	98 (24%)	110 (27%)
29 markets	Daily	03/01/93 to 02/28/98	08/07/97 to 02/06/98	406	238 (59%)	257 (63%)	148 (36%)	184 (45%)
29 markets	Daily	06/01/95 to 02/28/98	08/07/97 to 11/06/97	406	202 (50%)	188 (46%)	66 (16%)	95 (23%)
29 markets	Daily	03/01/93 to 11/06/97	08/07/97 to 11/06/97	406	225 (55%)	211 (52%)	109 (27%)	135 (33%)

NOTE: 1) Period of stability is entire period less the period of turmoil.

Table XI
Summary Results: Contagion During the Mexican Peso Crisis

					Market Pairs showing Contagion			
				Total Possible	*Unadjusted Correlations*		*Adjusted Correlations*	
Sample	*Test for Contagion*	*Entire Period*	*Turmoil Period*	*Market Pairs*	*Local Currency*	*U.S. Dollars*	*Local Currency*	*U.S. Dollars*
Full Sample	Full Sample	01/01/93 to 12/31/96	12/01/94 to 02/28/95	406	112 (28%)	67 (17%)	140 (34%)	97 (24%)
Full Sample	Full Sample	01/01/93 to 12/31/96	12/01/94 to 05/31/95	406	85 (21%)	59 (15%)	104 (26%)	67 (17%)
Full Sample	Mexico	01/01/93 to 12/31/96	12/01/94 to 02/28/95	29	3 (10%)	4 (14%)	0 (0%)	1 (3%)
East Asia	Mexico	01/01/93 to 12/31/96	12/01/94 to 02/28/95	9	1 (11%)	0 (0%)	0 (0%)	0 (0%)
Latin America	Mexico	01/01/93 to 12/31/96	12/01/94 to 02/28/95	3	2 (67%)	3 (100%)	0 (0%)	0 (0%)
OECD	Mexico	01/01/93 to 12/31/96	12/01/94 to 02/28/95	12	0 (0%)	1 (8%)	0 (0%)	1 (8%)
Other Emerging	Mexico	01/01/93 to 12/31/96	12/01/94 to 02/28/95	4	0 (0%)	0 (0%)	0 (0%)	0 (0%)

NOTES: Period of stability is entire period less the period of turmoil. All calculations based on daily returns. Regional divisions defined in Table I.

Table XII
Contagion (C) During the Mexican Peso Crisis or Not (N)?
Adjusted Correlations Based on U.S. Dollars

	Hon	Indo	Jap	Kor	Mal	Phi	Sin	Tai	Tha	Arg	Bra	Chil	Mex	Aus	Bel	Can	Fra	Ger	Ita	Net	Spa	Swe	Swi	UK	US	Chin	Indi	Rus
Indo	N																											
Jap	C	N																										
Kor	N	N	N																									
Mal	C	C	N	N																								
Phi	N	N	N	C	N																							
Sin	N	C	C	N	N	N																						
Tai	C	C	N	N	C	C	C																					
Tha	C	C	N	N	C	C	C	C																				
Arg	N	N	N	N	N	N	N	N	N																			
Bra	N	N	N	N	N	N	N	N	N	C																		
Chil	N	N	N	N	N	N	N	N	N	C	C																	
Mex	N	N	N	N	N	N	N	N	N	N	N	N																
Aus	N	C	N	C	N	N	N	N	N	N	N	N	N															
Bel	N	N	N	C	N	N	C	N	N	N	N	N	N	C														
Can	C	C	C	C	C	N	C	C	C	N	N	N	N	N	N													
Fra	N	N	N	N	N	N	N	N	N	N	N	N	N	N	C	N												
Ger	N	N	N	C	C	C	N	C	N	N	N	N	N	C	C	N	N											
Ita	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N										
Net	N	N	N	N	N	N	N	N	N	N	N	N	N	N	C	N	C	N	N									
Spa	N	N	N	N	N	N	N	N	N	N	N	N	C	N	N	N	N	N	N	N								
Swe	N	C	N	N	N	C	C	N	C	N	N	N	N	N	N	N	C	C	C	C	N							
Swi	C	C	N	C	N	C	N	N	N	N	N	N	N	C	C	N	C	C	N	C	N	C						
UK	N	N	N	N	N	N	N	N	N	N	N	N	N	N	C	N	N	C	N	N	N	N	N					
US	C	N	N	N	C	N	C	N	C	N	N	N	N	N	N	C	C	N	N	N	N	N	C	N				
Chin	N	C	N	N	N	C	C	N	N	N	N	N	N	N	N	C	C	N	C	N	N	C	N	N	N			
Indi	C	C	N	N	C	N	C	C	C	C	C	C	N	N	N	C	N	N	N	N	C	N	N	N	C	N		
Rus	N	N	N	N	N	N	N	N	N	N	N	N	N	C	N	N	N	N	N	N	N	N	N	N	N	N	N	
S. Af	N	N	N	N	N	N	N	N	C	N	N	C	N	N	N	C	N	C	N	N	N	N	N	C	C	N	C	N

NOTES: The period of the Mexican peso crisis is defined as lasting from 12/1/94 through 2/28/95. Test procedure described in text.
The null hypothesis is that the correlation coefficient during the stability period is greater than or equal to that during the turmoil period.
"N" suggests no contagion since we are unable to reject the null hypothesis. "C" suggests contagion since we are able to reject the null.

Table XIII
Summary Results: Contagion After the U.S. Market Crash of 1987

					Market Pairs showing Contagion			
				Total Possible Market Pairs	*Unadjusted Correlations*		*Adjusted Correlations*	
Sample	*Test for Contagion*	*Entire Period*	*Turmoil Period*		*Local Currency*	*U.S. Dollars*	*Local Currency*	*U.S. Dollars*
Full Sample	Full Sample	01/01/84 to 10/13/87	10/14/87 to 01/13/88	45	42 (93%)	41 (91%)	14 (31%)	14 (31%)
Full Sample	Full Sample	01/01/84 to 10/13/87	10/14/87 to 04/13/88	45	43 (96%)	42 (93%)	33 (73%)	28 (62%)
Full Sample	U.S.	01/01/84 to 10/13/87	10/14/87 to 01/13/88	9	7 (78%)	7 (78%)	1 (11%)	1 (11%)
Full Sample	U.S.	01/01/84 to 10/13/87	10/14/87 to 04/13/88	9	8 (89%)	8 (89%)	5 (56%)	4 (44%)

NOTES: Period of stability is entire period less the period of turmoil. All calculations based on daily returns. Regional divisions defined in Table I.

Table XIV
Contagion (C) After the U.S. Market Crash of 1987 or Not (N)?
Adjusted and Unadjusted Correlations Based on U.S. Dollars

Unadjusted Correlations

	Hon	Jap	Aus	Can	Fra	Ger	Net	Swi	UK
Jap	C								
Aus	C	C							
Can	C	C	C						
Fra	C	N	N	C					
Ger	C	C	C	C	C				
Net	C	C	C	C	C	C			
Swi	C	C	C	C	C	C	C		
UK	C	C	C	C	C	C	C	C	
US	C	N	N	C	C	C	C	C	C

Adjusted Correlations

	Hon	Jap	Aus	Can	Fra	Ger	Net	Swi	UK
Jap	N								
Aus	N	C							
Can	N	C	N						
Fra	N	N	N	C					
Ger	C	N	C	C	C				
Net	N	N	N	C	N	N			
Swi	C	N	N	C	N	N	N		
UK	N	C	N	C	N	C	N	N	
US	N	N	N	N	N	N	N	C	N

NOTES: The period of turmoil after the U.S. market crash is defined as lasting from 10/14/87 through 1/13/88. Test procedure described in text. The null hypothesis is that the correlation coefficient during the stability period is greater than or equal to that during the turmoil period. "N" suggests no contagion since we are unable to reject the null hypothesis. "C" suggests contagion since we are able to reject the null.

Table XV
Simulated Effects of Select Stock Market Crashes Based on Correlations in Local Currencies

	Hon	Indo	Jap	Kor	Mal	Phi	Sin	Tai	Tha	Arg	Bra	Chil	Mex	Aus	Bel	Can	Fra	Ger	Ita	Net	Spa	Swe	Swi	UK	US	Chin	Indi	Rus	S.Af
Brazil																													
No Con.	-5	-2	-3	-3	-2	0	-5	-2	-2	-29	-75	-24	-16	-8	-7	-11	-8	-8	-6	-10	-11	-8	-8	-10	-12	1	-5	-7	-4
Contag.	2	1	-3	-8	-2	-6	2	9	-12	-47	-75	-12	-34	4	-7	-22	-5	-1	3	-7	4	-4	-6	-10	-26	8	-5	0	0
Korea																													
No Con.	-3	-4	0	-53	-6	-5	-2	-3	-5	-4	-2	-4	-5	-5	-5	-5	-3	-3	-2	-5	-3	-4	-2	-6	-4	-1	-3	-4	-3
Contag.	-3	-3	-8	-53	-5	-4	-6	-9	-14	-2	-6	-12	0	-11	-4	-4	-9	-6	-11	-4	-9	-9	-5	-6	1	-5	1	-10	-6
Russia																													
No Con.	-13	-10	-2	-7	-3	-5	-15	1	-13	-8	-9	-4	-8	-7	-11	-10	-7	-8	-4	-13	-10	-11	-13	-8	-4	0	-5	-117	-10
Contag.	-43	-11	-30	-17	-10	-15	-25	-3	-11	3	0	-14	3	-44	-38	-8	-55	-46	-44	-36	-46	-51	-40	-32	3	-11	2	-117	-46
U.S.																													
No Con.	-4	-3	-1	-3	-2	-2	-3	0	-1	-10	-6	-6	-8	-3	-6	-17	-8	-5	-4	-8	-7	-7	-6	-9	-28	1	-1	-1	-1
Contag.	1	3	2	1	0	1	3	3	1	-13	-11	-6	-15	3	-6	-17	-4	0	0	-6	-1	-4	-4	-4	-28	7	-2	1	1

NOTES: Simulated effect of a 1.5 standard deviation fall in market on left-hand column. Reference date for market values is 2/28/98.
Correlations utilized for simulations are the adjusted correlations estimated in Table VI.
No contagion scenario is based on correlation from the stability period and contagion scenario is based on the period of East Asian turmoil.

Table XVI
Simulated Effects of Select Stock Market Crashes Based on Correlations in U.S. Dollars

	Hon	Indo	Jap	Kor	Mal	Phi	Sin	Tai	Tha	Arg	Bra	Chil	Mex	Aus	Bel	Can	Fra	Ger	Ita	Net	Spa	Swe	Swi	UK	US	Chin	Indi	Rus	S.Af
Brazil																													
No Con.	-4	-2	0	-2	-2	-1	-3	0	-1	-30	-49	-22	-17	-4	-3	-9	-6	-3	-7	-5	-10	-9	-1	-7	-11	2	-4	-6	-5
Contag.	1	3	-3	-6	-2	-3	1	5	-6	-31	-49	-10	-25	1	-8	-16	-5	0	4	-8	5	-3	-7	-13	-18	5	-4	0	-1
Korea																													
No Con.	-9	-23	2	.	-24	-19	-8	-7	-25	-11	-7	-8	-9	-16	-8	-12	-7	-3	-5	-10	-5	-12	-4	-14	-10	-5	-9	-13	-9
Contag.	-10	-15	-9	.	-6	-9	-14	-38	-38	-7	-19	-62	-35	-24	-9	-19	-27	-19	-37	-13	-30	-35	-12	-9	5	-16	2	-30	-21
Russia																													
No Con.	-13	-9	-6	-8	-5	-4	-14	1	-12	-6	-10	-6	-4	-6	-12	-7	-7	-10	-3	-15	-10	-9	-12	-7	-3	1	-4	-109	-6
Contag.	-43	-9	-15	-17	-9	-11	-20	-3	-9	3	0	-28	-19	-43	-22	-17	-43	-38	-35	-31	-35	-49	-23	-20	3	-11	2	-109	-48
U.S.																													
No Con.	-4	-3	0	-2	-3	-2	-4	0	-1	-10	-7	-6	-6	-3	-3	-15	-6	-3	-3	-5	-6	-5	-3	-7	-28	1	-1	-1	-2
Contag.	1	3	2	1	-1	1	3	3	2	-13	-11	-5	-10	2	-5	-13	-3	1	2	-7	1	-2	-5	-5	-28	7	-2	1	2

NOTES: Simulated effect of a 1.5 standard deviation fall in market on left-hand column. Reference date for market values is 2/28/98.
Correlations utilized for simulations are the adjusted correlations estimated in Table VI.
No contagion scenario is based on correlation from the stability period and contagion scenario is based on the period of East Asian turmoil.

Figure I
Stock Market Indices in Local Currencies

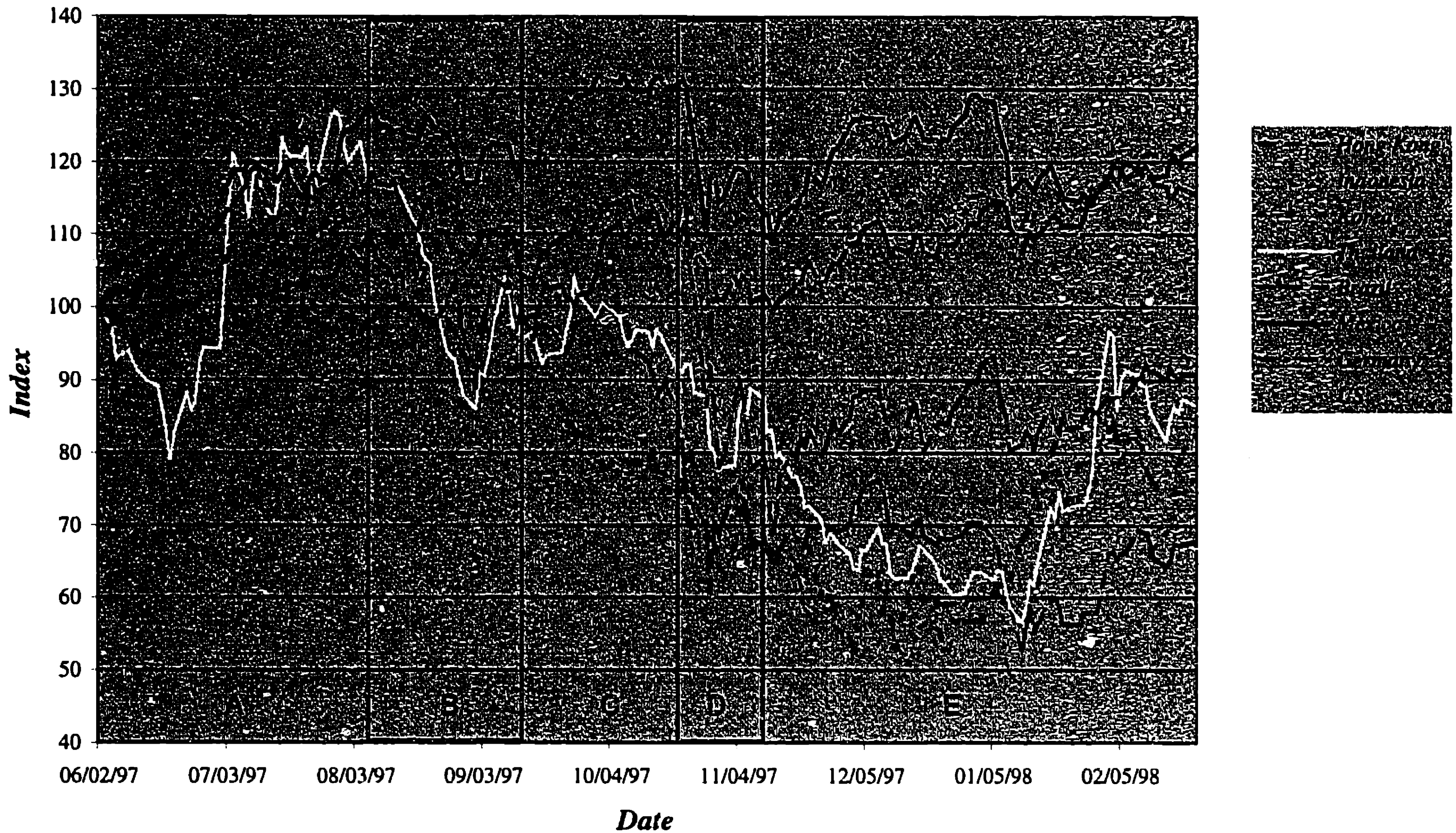

Figure II
Stock Market Correlations Based on Local Currencies:
Hong Kong and the Philippines