THE DOUBLE-EDGED SWORD OF TRADE

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> You implement that NAFTA, the Mexican trade agreement, where they pay people a dollar an hour, have no health care, no retirement, no pollution controls, et cetera, et cetera, et cetera, and you're going to hear a giant sucking sound of jobs being pulled out of this country right at a time when we need the tax base to pay the debt and pay down the interest on the debt and get our house back in order.
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> H. Ross Perot, U.S. Presidential Debate, October 19, 1992

INTRODUCTION

Within developed countries, does greater trade liberalization increase or decrease poverty? This paper uses data on U.S. states and Canadian provinces from 1986 to 1997 to examine the empirical relationship between international trade and poverty intensity. Its motivation is the fact that political debates on trade policy often appeal to the insecurities of voters, and an understandable source of voter anxiety about the potential impacts of greater trade openness is the prospect of a greater probability and/or a greater depth of income poverty. Although advocates of trade liberalization commonly appeal to its positive impacts on growth and average income, the rate and depth of poverty depend on the lower tail of the distribution of income. It is entirely plausible that greater openness to trade could increase *both* average incomes *and* the inequality of incomes, with an ambiguous net effect on the population with incomes below the poverty line.

As well, greater openness to trade implies both more exports and more imports. The loss of jobs, and downward pressure on wages, in import competing industries creates economic losses for some—but greater employment in the export sector produces economic gains for others. The net impact of trade, at each point in the income distribution, depends on the relative balance of these competing influences. Because it is of interest to know which dominates, in increasing or decreasing the intensity

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of poverty, this paper explicitly distinguishes between the impact of greater exports and imports on poverty intensity.

We emphasize poverty intensity because both the rate and depth of poverty are important. Because both depend on the shape of the lower tail of the distribution of income, identifying the impact of trade requires reliable, comparable income distribution data, preferably from jurisdictions with a range of trade exposure, over a period of time when trade exposure changed appreciably, and with a sufficiently large sample size to control for fixed effects and for the possible impact on poverty of other influences, such as macroeconomic conditions or social assistance policies. With 50 states (and the District of Columbia) and ten provinces, North American data are an obvious candidate, but in using poverty data from states and provinces, it is reasonable to be cautious about the information content of estimates of poverty intensity which may be drawn, at the state or province level, from small samples of households.[1] Hence, a methodological innovation of this paper is to use bootstrap-estimated standard errors of these estimates of poverty intensity at the state and province level to condition weighted least squares estimates of their determinants. As well, a second level of uncertainty arises because any regression that uses a sample of states or provinces as observations is open to the potential critique that a different sample of jurisdictions might have produced different results. Hence, this paper also uses bootstrap methods to assess the possible degree of bias in estimation from sampling variability in jurisdictions.

The next section begins by surveying the available literature on the relationship between poverty and trade, while the section after that presents the conceptual framework for our empirical analysis. Next there is a section which discusses the data and econometric methodology. This is followed by a section which presents results and discusses their implications.

LITERATURE REVIEW

Growth economists have often used cross-country regressions, but they have often approached the relationship between trade and poverty in two steps. In the first step, one examines whether trade raises growth; in the second, the question is whether growth lowers poverty – the implicit assumption is that all sources of growth are similar in their impact on poverty. It is common to find that trade leads to growth; for example, Frankel and Romer [1999] use the geographical components of bilateral trade to instrument for the trade share in growth regressions and find that trade is positively and significantly related to income per capita. Alcalá and Ciccone [2004] show that what they term "real openness" (trade as a share of PPP GDP) is positively related to growth. However, Rodriguez and Rodrik [2001] and Rodrik, Subramanian, and Trebbi [2002] criticize this and other papers purporting to show a trade-growth relationship. Still, Baldwin's [2003] survey concludes that more open trade policy does lead to growth, when combined with macroeconomic stability and good institutions.

Researchers have also found that growth reduces poverty. Roemer and Gugerty [1997] find that faster GDP growth causes faster growth in the incomes of the poorest 40%. Ravallion [2001] and Dollar and Kraay [2001] also find that faster growth reduces poverty by raising the incomes of the poor.

Unlike growth economists, development economists are more likely to use household-level data and are more open to the possibility that trade may make some individuals worse off, even if the average individual in a country is made better off. Reimer [2002] considers many possible links between increased trade and changing poverty, such as factor prices, income, and employment; external shocks that change the terms of trade; and short-run risk and adjustment costs. Winters [2000] claims that, despite the potential increased terms-of-trade volatility, the lower variability of international as opposed to domestic markets should make aggregate risk fall. However, Rodrik [1997] claims that the specialization and decreased diversification caused by openness leads to increased risk, since the stability of domestic production matters, not the stability of the world; he finds that greater exposure to external risk leads to greater volatility in income. Stiglitz [2003] discusses such potential adverse effects of globalization as low job creation, higher risk, and an erosion of social capital.

The empirical results are mixed. Santarelli and Figini [2002] find that trade reduces absolute but not relative poverty. Hertel, Ivanic, Preckel, and Cranfield [2003] use household surveys from 14 developing countries to show that the average household is made modestly better off by trade liberalization, but poor households are often made worse off. In an exhaustive survey, Winters et al. [2004] conclude that the evidence supports the view that trade liberalization reduces poverty in the long run, but not necessarily in the short run.

Researchers studying the effects of trade in industrial countries have tended to focus on income inequality rather than poverty. However, poverty and inequality are different issues, whether the poverty line is drawn using a relative or absolute methodology, since aggregate inequality statistics are inevitably dominated by the non-poor, who remain a large majority of the population however the poverty line is drawn.[2] In cross-country comparisons, there is a far from perfect correlation (0.79) between poverty intensity and the Gini Index of inequality (see Osberg and Xu [2000, 68-69]).

Mahler [2001] finds little evidence of a relationship between globalization and the income distribution for 14 industrial countries. Most other researchers have used industry-level trade and wage data, finding in most cases that, while trade is undoubtedly to blame for a significant part of the rise in inequality over the last generation, it is not the sole or even strongest factor. Wood [1994] attributes almost the entire rise in the skill premium to North-South trade. However, Lawrence and Slaughter [1993] stress that, according to the Stolper-Samuelson theorem of the Heckscher-Ohlin model, rising wage inequality could not be due to trade, given that neither the relative price of unskilled labor-intensive imports nor the skilled-to-unskilled employment ratio has fallen.[3] Krugman's [1995a; 1995b] general-equilibrium analysis suggests that North-South trade is sufficiently small (non-oil imports from low-wage countries to the U.S. amounted to less than 3% of U.S. GDP in 1990) that it could not possibly explain the rise in inequality. Harrigan [1998] also uses a general-equilibrium model to show that imports have played a negligible role in causing the rise in wage inequality. Borjas, Freeman, and Katz [1997] find that trade and immigration contributed less than 20% of the rise in the skilled to unskilled wage ratio. Finally, Cline's [1997] summary and analysis of the evidence states that only one-fourth to one-fifth of the rise in the skilled-to-unskilled wage ratio is due to trade or immigration.

The literature on trade and risk focuses on the rise in instability caused by increased openness. This rise in instability may have many possible sources, such as increased exchange-rate risk (see, for example, Gagnon [1993]). Gottschalk and Moffitt [1994] find that a rise in the instability of earnings can account for one-third of the widening in the U.S. earnings distribution between the 1970s and the 1980s. Rodrik [1997] provides many examples of increased risk due to globalization, such as a rise in the elasticity of demand for unskilled workers. This is caused by an asymmetry between groups that can more easily cross borders (such as capital and highly-skilled workers) and those that are locked in (such as low-skilled workers), and results in greater instability in incomes when there are shocks to labor demand. Rodrik [1998] points out that openness might be thought to lower risk, given that any one economy must be more volatile than the overall world economy. However, openness causes risk by encouraging specialization, which leads to less diversification, and the stability of the world is not what matters, but the stability of domestic production. Empirically, Rodrik finds that greater exposure to external risk (measured by the terms of trade or the product concentration of exports) does lead to greater volatility in income. However, the weakness of all this, from the perspective of this paper's focus on poverty, is that poverty outcomes depend on a particular segment (the lower tail) of the income distribution—which can be poorly predicted by greater volatility or inequality *in general*.

CONCEPTUAL FRAMEWORK

Suppose that we think of the national economy as having two sectors: internationally tradable (T) and non-tradable (N) goods and services. Each individual worker draws his or her income from one sector or the other as per equation (1) or equation (2).

$$Y_{it}^{T} = \overline{Y_i^{T}}\alpha_i^{T} + \varepsilon_{it}^{T} \tag{1}$$

$$Y_{it}^{N} = \overline{Y_i^{N}}\alpha_i^{N} + \varepsilon_{it}^{N}, \tag{2}$$

where

$\overline{Y_i^{T}}$, $\overline{Y_i^{N}}$ are average income in the traded (T) and non-traded (N) sectors in period t,
α^{T}_{i}, α^{N}_{i} are the relative (i.e., ratio to mean) permanent personal advantages of individual i in the traded and non-traded sectors, and
ε^{T}_{it}, ε^{N}_{it} are the shocks in the traded and non-traded sectors.
$E(\alpha^{T}_{i}) = E(\alpha^{N}_{i}) = 0$, $E(\varepsilon^{T}_{it}) = E(\varepsilon^{N}_{it}) = 0$, $\varepsilon^{T} \sim F_{T}(0,\sigma^{T})$, $\varepsilon^{N} \sim F_{N}(0,\sigma^{N})$.

Average income in a country is then given as equation (3) where β_t is the proportion of employment in the tradable sector at time t.

$$\overline{Y_t} = \beta_t\overline{Y_t^{T}} + (1-\beta)\overline{Y_t^{N}} \tag{3}$$

A policy shift to greater openness in international trade may be motivated by the expectation that equation (4) holds. However, it may also be recognized that the tradable sector can be more exposed to shocks (impacting on both wages and employment), which imply that income flows become more uncertain. Equation (5) summarizes the difference in insecurity.

(4) $$\overline{Y_t^T} > \overline{Y_t^N}$$

(5) $$\sigma^N < \sigma^T$$

Presumably, the labor supply of individuals to sectors is driven by some combination of their attitudes to risk, the correlation of relative abilities across sectors, the average income differential (equation (4)) and the difference in stochastic variability (equation (5)). In a full general equilibrium model, relative labor supplies by sector would interact with sectoral elasticities of demand to determine the equilibrium relative income ratio. For present purposes, all we need to assume is that equations (4) and (5) hold and that the industrial structure of states (provinces) differs. Each state (province) of workforce n_s within a country of size N has a specific industrial structure β_s. Equation (6) summarizes the overall relation.

(6) $$\beta_t = \sum n_{st} / N \cdot \beta_{st}$$

Overall, then, the greater dispersion of income in the tradable sector opens up the possibility that poverty will rise as labor flows into traded industries, but the combination of a higher mean income with greater variance of incomes in the traded sector means that there is no clear prediction about poverty probability.

If F_T and F_N denote the cumulative distribution function of income in tradable and non-tradable sectors (for a person of given characteristics), then poverty probability in a state (province) would be given by equation (7).

(7) $$R_s = \beta_s(F_T(P)) + (1 - \beta_s)(F_N(P))$$

This paper distinguishes between poverty, and the more general case of lower income, because, in addition to worrying in general about the loss in utility produced by a possible income loss, people worry about poverty because there is something different about being poor. Adam Smith wrote of the importance of meeting customary consumption norms: “the want of which would be supposed to denote that disgraceful degree of poverty, which, it is presumed, nobody can fall into without extreme bad conduct.”[4] Sen [1985] introduces the vocabulary of functionings or capabilities, such as the capability of appearing in public without shame as to shabby clothing. Bourguignon and Fields [1997] note that if there is something qualitatively different about being poor, the utility function is discontinuous at the poverty line. Hence, there is good reason for individuals to care about both their probability of poverty and the depth of that poverty, if it occurs.

If, as equations (1) and (2) summarize, the current pre-tax income of individuals can be neatly divided into permanent differentials and stochastic shocks, one could categorize the corresponding social transfers as arising from social assistance or social insurance programs. The simplest characterization of a social assistance program that aims at redistribution of permanent income is in terms of its guarantee rate (g_0) and implicit tax rate (t_0) which together determine net transfers B_i^0, as per equation (8).

$$B_i^0 = g_0 - t_0 \overline{Y} \alpha_i \tag{8}$$

Social insurance programs (like unemployment insurance) can be characterized as a form of co-insurance, or risk-sharing, among individuals, in which people with positive income shocks pay taxes and those who get negative shocks receive benefits (B_i'). A simple characterization[5] of such a system is equation (9):

$$B_i' = -t_1 \varepsilon_1 \tag{9}$$

In this highly simplified world, the after-tax income of workers in non-tradables is given by equation (10), while equation (11) gives after-tax income in the tradable sector.

$$Y_{it}^{N*} = g_0 + (1 - t_0)\overline{Y_t^N}\alpha_i^N + (1 - t_1)\varepsilon_{it}^N \tag{10}$$

$$Y_{it}^{T*} = g_0 + (1 - t_0)\overline{Y_t^T}\alpha_i^T + (1 - t_1)\varepsilon_{it}^T \tag{11}$$

If social assistance and social insurance programs are delivered at the state (provincial)[6] level then one must add a subscript to denote the state-specific level of welfare payments (g_{os}) and the state-specific replacement rate in social insurance (t_{is}). Denote the cumulative distribution function corresponding to equation (10) as F_s^{N*} and that corresponding to equation (11) as F_s^{T*}. After taxes and transfers, when the poverty line is P, the poverty rate in a given state (province) is then given by equation (12).

$$R_s = \beta_s F_s^{T*}(P) + (1 - \beta_s) F_s^{N*}(P) \tag{12}$$

We cannot unambiguously predict the impact of trade openness ($\partial R_s/\partial \beta_s >< 0$), but the implication of greater risk pooling in social insurance programs is clear ($\partial R_s/\partial t_{1s} < 0$) and so is the impact of greater generosity in social assistance ($\partial R_s/\partial g_{os} < 0$).

In reality, of course, UI and social assistance programs are highly complex programs. Both simultaneously redistribute income between lifetime income classes and between contingencies (such as employment or unemployment) that are experienced within lifetime income classes. In reality, program designers and administrators cannot easily distinguish permanent and transitory differences in earnings capacity, or the voluntary and involuntary utilization of that capacity. A major part of the design and administration of these programs is driven by the incentive problem, and program managers' desire to minimize their impact on labor supply. Boadway and Cuff [1999] is an example of recent theoretical literature that outlines why, in an environment of imperfect information, program designers will utilize both types of programs, and will also institute controls for job search and work effort.

However, the bottom line for present purposes is that variations in UI and social assistance generosity are likely to have distinct effects on the intensity of poverty. The exposure of a region to trade, on the other hand, has ambiguous effects.

DATA AND REGRESSION METHODS

Our goal is to determine the effect of trade exposure on poverty intensity, using data from Canadian provinces and U.S. states in 1986, 1991, 1994, and 1997.[7] In addressing this issue, sampling variability can be important at both the household and the regional level, since sampling variability influences both the reliability of each jurisdiction's estimated level of poverty and the reliability of cross-jurisdiction regressions.

When estimates of a population characteristic (like the rate or average depth of poverty) are derived from an absolutely small sample from the population, sampling variability can be an important source of heteroskedasticity, if that estimate is subsequently used as the dependent variable in a regression. The problem of sampling variability in this sense is that a different sample of households could equally well have been drawn within states or provinces, so that although any given random sample provides an unbiased estimate of the dependent variable, in a cross state/province regression, the dependent variable has a standard error whose size depends on the size of the sample of households within each state or province. Since U.S. micro data from the Current Population Survey varies widely in sample size at the state level, we construct the standard errors of our poverty intensity estimate to correct for heteroskedasticity, so that jurisdictions with smaller standard errors of estimate of the dependent variable are weighted more heavily.

A different level of sampling variability surrounds the selection of the jurisdictions that are the units of observation in cross state/province regressions. Historical happenstance has determined that California and North Dakota each count as one state, even though California's population is over fifty times that of North Dakota. If California had been split into 50 separate states, each with a weight equal to that of North Dakota, would the results of a cross state/province regression change? In cross-country regressions, India, Pakistan and Bangladesh now enter as unique observations, and cross-state regressions within each country take their present political borders as given—but if British rule of India had ended a bit differently, they might now all be part of a single successor state to the British Raj. Even within North America, had events unfolded a bit differently, Canada would now be a part of the United States[8] and, more recently, Quebec could very easily not be a part of Canada.[9] Indeed, the sample of states that comprise the U.S. would now be different, if the secession of the Confederacy had succeeded. The implication of all this is that if history had taken a different political path, this might well have produced a different set of boundaries between jurisdictions and a different sample of observations for a cross state/province regression. Would our estimates of the impact of trade on poverty then be different?

Our measure of poverty is the Sen-Shorrocks-Thon (SST) index of poverty intensity. We use this rather than the poverty rate, since simply counting the number of the poor ignores the depth of their poverty. An alternative measure, the average poverty gap, considers only the average shortfall of income below the poverty line. By contrast, the SST index of poverty index includes the poverty rate, the average poverty gap ratio, and inequality among the poor.[10]

We assume that income is shared among the members of the household, and use the Luxembourg Income Study (LIS) equivalence scale, which calculates the equivalent income of each family member as total after-tax household income divided by the square root of household size. Our measure of the poverty line is set at half the median equivalent income. Our data sources are the Luxembourg Income Study (LIS) for the United States, which is based on the Current Population Survey; for Canada, we use the Survey of Consumer Finance.

Using weighted least squares analysis to correct for heteroskedasticity results in more efficient estimators. Since there is not an easily calculated measure of dispersion for the SST index, bootstrapping is used to estimate a variance for each state and province. By sampling with replacement, and recalculating the SST for each region, the variance can be estimated by calculating the standard deviation of 300 repetitions, and then used as the weighting factor for the weighted least squares estimation. Those states and provinces that have less variation around the point estimate measure of poverty receive more weight in the regression.

To measure the openness of a state or province to trade, we take the product of the industrial composition of the labor force (by state/province) and the trade exposure of the industries (by country). Export and import data are obtained for two-digit categories, including agriculture, mining, and manufacturing industries, but not services. The industry-level labor force data uses the same industrial classifications. For each state or province, the proportion of the labor force in each industry as a percentage of all employment is calculated. Finally, the proportions of the labor force in each industry are multiplied by the share of exports or imports in output, and then summed for each province or state. (We use a quadratic form to allow for the possibility that trade may have a non-linear impact as the amount of export or import exposure rises.) Our sources for trade data are Strategis for Canada and the Statistical Abstract for the U.S. Our labor force data are from the Canadian Labor Force Survey and, for the U.S., the ICPSR Archive. GDP by industry comes from CANSIM for Canada and the Statistical Abstract for the United States.

Control variables are the unemployment rate, unemployment insurance benefits, social assistance payments, and average earnings. In addition, we include dummy variables for the years 1991, 1994, and 1997. On the theory that the disadvantaged are at the back of the queue for jobs, we use the natural log of the male unemployment rate because this specification gives the greatest anti-poverty weight to changes in unemployment occurring at low levels of unemployment. We expect unemployment to have a positive coefficient. (The data are from CANSIM (Canada) and the Statistical Abstract (U.S.).) Since unemployment is an important risk factor in poverty, the generosity of unemployment insurance benefits is a potentially important variable. We summarize the very different systems seen in the various states and provinces as the ratio of total unemployment insurance benefits in the province or state to the total number of unemployed individuals, and expect a negative coefficient. (The Canadian data are from CANSIM; the U.S. data are from the Department of Labor Handbook.) We also include a measure of the generosity of social assistance in each state and province. Although we realize that social assistance systems are even more complex than unemployment insurance programs, with widely varying rules and regulations, we summarize social assistance generosity by the maximum amount that could be received by a single parent with one child. (For the United States, this is the dollar amount of maximum Aid to Families with Dependent Children/Temporary Aid to Needy Families for a family of two in each state. For Canada, the benefits available in each province are reported by the National Council of Welfare.) Since this is a measure of the legislated provisions open to an individual with given characteristics, but not the value of benefits actually paid, it is not endogenous with respect to the choices of individuals to apply for assistance. We expect this variable to have a negative coefficient. Our final control variable is average weekly earnings in each state or province. (The data sources are CANSIM (Canada) and the Department of Labor Handbook (U.S.).) We expect the earnings variable to have a negative coefficient.

Regression methods

A popular starting point to analyze the determinants of poverty is ordinary least squares (OLS) regression. To begin, we model poverty intensity as a function of export and import exposure as well as export and import exposure squared. As mentioned, controls include the natural log of the male unemployment rate, average weekly earnings, monthly unemployment benefits per unemployed person, maximum weekly social assistance paid to a single parent with one child, and a set of dummies for years 1991, 1994, and 1997.[11] OLS allows for straightforward interpretation, with the coefficients on continuous variables being interpreted as the linear effect on the dependent variable of a unit change in the explanatory variable. A logged explanatory variable, such as the male unemployment rate, has a coefficient that can be interpreted as the absolute change in poverty given a relative change in the unemployment rate. The standard error of the coefficient is the usual standard deviation of the sampling distribution of the estimator.

Since the SST index is calculated for each province and state using survey micro data from each country, the sample size within states and provinces is a serious concern. Prince Edward Island has the fewest number of observations for a province—876—whereas the state with the fewest observations, Wyoming in 1986, has only 104. Although the poverty estimates are unbiased, unequal standard errors of estimate may lead to the problem of heteroskedasticity.

Our second estimation method is thus to use weighted least squares (WLS) to correct for heteroskedasticity. Unlike a sample mean, there is not a readily available analytical formula for estimating the standard error that surrounds a point estimate of the SST, but Efron and Tibshirani [1986] suggest that 50 to 200 resamples is a sufficient number to estimate a bootstrap standard error around a point estimate. For our model, an estimate of the standard error around each regional SST is obtained by bootstrapping using 300 resamples. We then weight the observations in the original OLS model by the inverse of the standard error. Regions with smaller SST variability and, therefore, more reliability in this measure are weighted more heavily with this WLS procedure.

Bootstrapping can also be used in a regression model framework when crucial conditions are not met. For example, if the distribution of the residuals in an OLS regression is non-normal, hypothesis tests in the model may be unreliable. Our model has a sufficient number of observations to satisfy the normality assumption.[12] The estimated bias can be measured as the difference between the actual point estimate and the expected value of the bootstrapped estimate. As Mooney and Duval [1993] note, Efron [1982] suggests that if the ratio of the estimated bias to the standard error is less than 0.25, then the bias of the estimate is not a problem.

A third estimation technique is to use bootstrapping in our regression model to measure the robustness of results, given a different mix of regions. In our sample, we have 61 jurisdictions representing states and provinces. Bootstrapping allows us to draw a different sample of regions and check the robustness of results, as an answer to the question: "Suppose our sample had a different make-up of regions; would this matter?" From the current 61 observations, we randomly draw a new sample of 61 that could include any individual state or province multiple times or not at all. Estimates of the regression coefficients are obtained from this new sample. The standard deviations of the coefficients are obtained from 1000 replications and used as an estimate of the standard error. Significance of the determinants is analyzed using a standard t-test. The double bootstrapping—in estimating poverty intensity as well as in the regressions—is a novel addition of this paper.

Simple regression models can sometimes suffer from omitted variable bias. We therefore also use panel data techniques to handle unobserved factors, where the change in variables is analyzed rather than the levels. By doing so, we exclude unobserved effects, which are fixed over time. One method of handling this problem is fixed-effects estimation. If we take the mean of all variables over time, take the difference from each individual value and model this transformed data, the unobserved fixed effect drops out. Suppose our original model is as follows:

$$y_{it} = \beta x_{it} + a_i + u_{it}, t = 1, 2, \ldots T \tag{13}$$

where a_i = constant, unobserved effects.

If we take the mean over time for each cross-sectional unit we have:

$$\overline{y_i} = \beta \overline{x_i} + a_i + \overline{u_i} \tag{14}$$

Subtracting equation (14) from equation (13) leaves

$$(y_{it} - \overline{y_i}) = \beta(x_{it} - \overline{x_i}) + (u_{it} - \overline{u_i}) \quad (15)$$

Since the a_i term is constant over time, it disappears in the transformation.

For this study, we pool four years of data for each of the 61 states and provinces. There may be factors inherent in particular states or provinces that have an influence on poverty, but are not captured in our model. To avoid possible omitted variable bias, we analyze the determinants of poverty as differences from the mean. Of course, one problem with fixed-effects estimation is that only within-region, not between-region, variation is considered; that is, only changes over time, not differences across provinces and states, are used to calculate the regression coefficients. If there is little variation over time in the variables, then we can expect few coefficients to be statistically significant.

By pooling together Canadian provinces and American states, we are implicitly assuming that there is no structural difference in the model between the two countries—but the effect of one or more of the explanatory variables on poverty intensity may be different in the United States than it is in Canada. A Chow statistic tests whether the betas are the same in Canada and in the United States.

RESULTS AND DISCUSSION

Table 1 presents our results. The first three columns display the OLS, GLS, and bootstrap results, which are remarkably consistent. Import exposure significantly raises poverty, and export exposure significantly lowers poverty, in each of these specifications. The square of export exposure is insignificant, but the square of import exposure is significant and negative. In the bootstrap specification (3), the import exposure and squared import exposure coefficients imply a point of inflection of 22% (0.6165/2×1.4027); below 22%, import exposure raises poverty, but as import exposure rises above 22%, it begins to reduce poverty.

The control variables also have similar results in the first three columns, and their signs match our predictions. Unemployment is significant in raising poverty, while unemployment insurance is significant in lowering it. More generous social assistance benefits lower poverty, but the coefficient becomes insignificant once the bootstrap method is employed. Higher average earnings are associated with lower poverty, but the fact that the coefficient is never significant can be read as an indication of the uncoupling of low-wage labor markets from general market trends.

The dummy variables measure poverty intensity relative to the base year (1986) and indicate a secular trend to higher poverty in 1991 and 1997, but insignificantly higher in 1994 than in 1986.

These regressions indicate that trade has a significant effect on poverty, and that the import effect raising poverty dominates the export effect lowering poverty; the bootstrap regression in column (3) shows a coefficient on import exposure of 0.62 but a coefficient on export exposure of -0.49. However, knowing that exports reduce poverty and imports raise it presents a policy conundrum, as the two go together. As an illustration of the joint impact of trade liberalization, we simulate the *net* effect of

TABLE 1
Determinants of Poverty

	(1) OLS	(2) GLS	(3) Bootstrap	(4) Fixed Effects	(5) U.S.	(6) Canada
Import exposure	.509047*	.6165*	.6165*	-0.317	1.338***	-0.021
	(.21271)	(.1745)	(.23215)	(0.447)	(0.785)	(0.802)
			[.00353]	[-0.114]	[0.053]	[-0.1909]
Export exposure	-.45900*	-.4933*	-.4933*	0.117	-1.000	-0.216
	(.22324)	(.1739)	(.22823)	(0.209)	(1.071)	(0.423)
			[.02346]	[0.042]	[-0.011]	[0.1868]
Import exposure squared	-1.0795	-1.4027*	-1.4027*	0.319	-6.637**	-0.565
	(.68924)	(.4601)	(.6877)	(1.590)	(2.920)	(-2.382)
			[-.0535]	[0.564]	[-0.219]	[0.4087]
Export exposure squared	.47066	.5115	.5115	-0.234	-0.808	0.359
	(.63608)	(.4469)	(.5839)	(0.510)	(6.149)	(1.231)
			[-.0311]	[-0.152]	[0.062]	[-0.3710]
Male unemployment rate (ln)	.06569*	.0526*	.0526*	0.027*	0.031*	-0.015
	(.00677)	(.0066)	(.0067)	(0.009)	(0.007)	(0.037)
			[.0019]	[0.001]	[-0.0004]	[-0.0055]
Monthly UI benefits per unemployed person	-0.085*	-.067*	-.067*	-0.010	0.018	0.010
	(0.013)	(.011)	(.012)	(0.010)	(0.025)	(0.029)
			[-0.0028]	[-0.0005]	[-0.0002]	[-0.0069]
Social assistance: weekly maximum benefit for 1 parent and 1 child	-0.091*	-0.038*	-0.038	0.009	-0.027**	-0.007
	(0.02)	(0.018)	(0.038)	(0.013)	(0.012)	(0.220)
			[-0.013]	[-0.002]	[0.0011]	[0.0296]
Average weekly earnings	-0.00016	-0.013	-0.013	-0.001	-0.006	0.106
	(0.025)	(0.024)	(0.026)	(0.049)	(0.064)	(0.365)
			[0.0028]	[-0.002]	[0.0059]	[-0.0560]
1991 dummy	.03982*	.02357*	.02357*	0.003	0.038*	-0.009
	(.00752)	(.00629)	(.00842)	(0.004)	(0.011)	(0.012)
			[.00276]	[-0.0006]	[0.0001]	[-0.0009]
1994 dummy	.01068***	.00704	.00704	0.003	0.025*	0.007
	(.00629)	(.0057)	(.0063)	(0.003)	(0.008)	(0.015)
			[.00054]	[-0.0003]	[0.0001]	[-0.0041]
1997 dummy	.01973*	.01526*	.01526*	0.008	0.038*	0.010
	(.00718)	(.00665)	(.00697)	(0.005)	(0.010)	(0.022)
			[.00016]	[0.0007]	[-0.0004]	[-0.0044]
Intercept	.01527	.03028		-0.004	-0.025*	-0.002
	(.02085)	(.01932)		(0.003)	(0.007)	(0.010)
				[0.0026]	[0.00003]	[0.0023]
Number of observations	244	244	244	244	204	40
Adjusted R^2	0.4496	0.4540		0.145	0.264	0.480

Notes:
The dependent variable is the Sen-Shorrocks-Thon index of poverty intensity. Standard errors are in parentheses, with the bias (point estimate less bootstrap mean) reported below the standard error in columns (3) to (6).
* indicates significance at the 99% level, ** at the 95% level, and *** at the 90% level.

a simultaneous one-percent rise in both export exposure and import exposure in all provinces and states. This calculation is provided in Table 2, which shows, for each province and state, the initial (1997) levels of export exposure and import exposure, the SST poverty index in 1997, and the percentage change in the poverty index if both export exposure and import exposure increased by one percentage point. The table shows that the effect of increased trade is negligible in all cases. For all ten provinces, an increase in both exports and imports would lower poverty slightly, by between 0.15 percent (in Quebec) and 1.31 percent (in Ontario). For U.S. states, a one-percentage point rise in both export and import exposure would raise poverty in 40 of the 51 states; that is, 73% of the U.S. population lives in jurisdictions that can expect trade liberalization to produce an increase in poverty. However, what Table 2 clearly shows is that, even in the states with the largest effects, poverty would change by much less than one percent. So while the effect of trade on poverty is *statistically* significant, it may not be *economically* significant.

The fourth column of Table 1 presents the fixed-effects results. As discussed in section IV, this is our preferred specification, as the use of fixed effects controls for each region's unexplained heterogeneity. The results show that most coefficients become insignificant, and many of them change sign. The fact that the trade variables become insignificant in column (4) could show that the significance found in the earlier regressions was spurious, due only to the unexplained variability that is now held constant. Alternatively, it could be due to the fact that the fixed-effects method regresses changes in poverty intensity on changes in the independent variables, and many of the variables changed little, if at all, over this period. In any event, now the only significant coefficient (with the expected positive sign) is on the unemployment rate.

The final two columns of Table 1 separate the fixed-effects regressions for the United States and Canada, rather than combining the states and provinces as in column (4), on the grounds that a Chow test (with an F statistic of 5.46) indicates that the coefficients estimated over the separate groups are not the same. None of the variables is now significant in the Canadian regression, possibly due to the low sample size of 40, representing 10 provinces in four years. However, the U.S. regression indicates that unemployment and social assistance are significant and of the expected sign, and the three year dummies are significant. The export exposure variables are insignificant, but the import exposure variables are significant at the 90% or 95% level. Together, the linear and quadratic import exposure variables imply an inflection point of 10% ($1.338/2\times6.637$)— i.e., that up to a point slightly higher than the average U.S. import exposure in 1997 of 9.37%, greater import exposure increases poverty. We conjecture that if import exposure raises poverty intensity only at low levels of imports, this may explain why the variable is insignificant for Canada—since Canadian provinces have higher imports than U.S. states.

Even if trade could be shown to have a much larger adverse impact on poverty, the appropriate response would not be to restrict trade. Rather, an awareness that trade, while raising average GDP, may harm those at the bottom of the income distribution should lead policymakers to re-think the recent retrenchment of social assistance in the U.S. and Canada.

This paper uses bootstrapping to construct the standard errors surrounding poverty estimates in order to correct for heteroskedasticity, and to confirm the robustness of our results to sample selection sensitivity across jurisdictions. Using the Sen-Shorrocks-Thon index of poverty intensity and measures of export and import exposure, we show that exports lower poverty, and imports raise it, for Canadian provinces and U.S. states over the period 1986 to 1997. While these effects are statistically significant (at least in the OLS and GLS specifications), they are never economically significant.

TABLE 2
Percentage Change in SST Index when Imports and Exports Increased by 1%, 1997

	Import Exposure	Export Exposure	Actual SST	% Change in SST
Newfoundland	0.07086	0.12985	0.10417	-0.15725
PEI	0.08856	0.17913	0.05516	-0.25929
Nova Scotia	0.11491	0.17737	0.09967	-0.22198
New Brunswick	0.09418	0.14163	0.08914	-0.15271
Quebec	0.19055	0.30134	0.08338	-0.45717
Ontario	0.27557	0.30023	0.06778	-1.31281
Manitoba	0.21091	0.3404	0.07359	-0.75435
Saskatchewan	0.18991	0.30698	0.07494	-0.62012
Alberta	0.1539	0.24294	0.05802	-0.45551
British Columbia	0.10805	0.16085	0.07877	-0.17365
Alabama	0.14082	0.08533	0.16364	-0.03027
Alaska	0.05137	0.02359	0.06735	0.10078
Arizona	0.07123	0.05561	0.14219	0.04728
Arkansas	0.14315	0.09371	0.17234	-0.05285
California	0.10402	0.06899	0.13365	0.03495
Colorado	0.06322	0.04813	0.05988	0.07046
Connecticut	0.10052	0.08203	0.06401	-0.00101
Delaware	0.06106	0.0513	0.08447	0.04998
Dist. of Columbia	0.00317	0.00257	0.21681	0.00572
Florida	0.04819	0.03469	0.14669	0.06372
Georgia	0.09939	0.06505	0.09985	0.04908
Hawaii	0.01951	0.01098	0.124	0.04956
Idaho	0.09533	0.06958	0.10606	0.03183
Illinois	0.09136	0.07235	0.10104	0.02432
Indiana	0.14885	0.11107	0.06313	-0.12905
Iowa	0.10409	0.08498	0.08859	-0.01032
Kansas	0.10976	0.0844	0.07998	-0.00652
Kentucky	0.1364	0.0892	0.13969	-0.03526
Louisiana	0.08301	0.0444	0.15556	0.08608
Maine	0.1412	0.07342	0.09473	0.00099
Maryland	0.04673	0.03589	0.05769	0.05552
Massachusetts	0.08574	0.06351	0.11085	0.05776
Michigan	0.13522	0.1022	0.10261	-0.09129
Minnesota	0.08669	0.07158	0.09041	0.0258
Mississippi	0.16433	0.09361	0.14575	-0.09213
Missouri	0.11157	0.07294	0.11172	0.02661
Montana	0.05112	0.02981	0.13583	0.08084
Nebraska	0.07112	0.05949	0.07427	0.04758
Nevada	0.03105	0.02231	0.07857	0.07121
New Hampshire	0.12554	0.09199	0.07358	-0.04077
New Jersey	0.06975	0.04855	0.0822	0.07917
New Mexico	0.05882	0.03639	0.18489	0.07291
New York	0.07644	0.04588	0.15859	0.0871
North Carolina	0.13672	0.08915	0.105	-0.04231
North Dakota	0.05666	0.04349	0.11847	0.09292
Ohio	0.11723	0.09173	0.10351	-0.02917
Oklahoma	0.10322	0.06549	0.12826	0.05018
Oregon	0.0991	0.06986	0.10042	0.03315
Pennsylvania	0.09851	0.06788	0.1041	0.04272
Rhode Island	0.12284	0.07188	0.12856	0.029105
South Carolina	0.1407	0.09476	0.12898	-0.06311
South Dakota	0.10247	0.07858	0.14561	0.011891
Tennessee	0.13754	0.08623	0.13757	-0.0258

TABLE 2 (continued)
Percentage Change in SST Index when Imports and Exports Increased by 1%, 1997

	Import Exposure	Export Exposure	Actual SST	% Change in SST
Texas	0.0883	0.05668	0.1603	0.061218
Utah	0.08271	0.05933	0.0754	0.065315
Vermont	0.10464	0.07404	0.07785	0.027804
Virginia	0.08077	0.05639	0.12004	0.065236
Washington	0.10446	0.07817	0.09321	0.013897
West Virginia	0.07714	0.06212	0.17547	0.029668
Wisconsin	0.13258	0.10183	0.06612	-0.08677
Wyoming	0.06203	0.03727	0.12344	0.087958

NOTES

1. Wyoming has the smallest number of observations—only 104 in 1986.
2. Even if the poverty line is drawn in a purely relative way, poverty can be eliminated at the same time as inequality is rising. For example, a society which started with a distribution of income like [4,7,10,15,25] would have a poverty rate of 20% if the poverty line was set at half the median. A series of transfers which produced the distribution [6,7,10,11,27] would both eliminate relative poverty and increase economic inequality (as measured by the coefficient of variation).
3. However, Feenstra and Hanson [2001] show that trade in intermediate inputs, rather than final goods, could play a strong role in widening the wage gap.
4. Smith, Wealth of Nations, Book V, Ch. II, Art IV, Cannan Edition, p. 399.
5. A linear tax/benefit schedule would break even (save for administration costs) if shocks were symmetric. However, in reality, social insurance and welfare programs are anything but simple.
6. Canadian UI (now called EI) is a federal program, but its provisions vary with local unemployment, which differentiates its impact by province.
7. These are the only recent years for which household-level data are available for both Canada and the United States.
8. For example, British victory in the crucial Battle of Queenston Heights in the War of 1812-14 depended heavily on the element of surprise and advance intelligence of the battle plans of the invading U.S. forces.
9. The Quebec sovereignty referendum of 1995 was defeated by less than 0.2% of votes cast.
10. The Sen-Shorrocks-Thon (SST) index of poverty intensity is calculated as $I = (rate) \times (gap) \times (1 + G(x))$, where *rate* is the percentage of the population with incomes below the poverty line, *gap* is the average percentage gap between the incomes of the poor and the poverty line, and $G(x)$ is the Gini index of inequality of the poverty gap among all people.
11. Ideally, a dummy variable indicating the observation is a province would be included to control for the "Canada effect," but a fixed-effects model does not allow for non-varying indicators over time. For comparability across specifications, we exclude it for all runs.
12. A plot of the residuals confirms this.

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