Economic Insecurity and the Globalization of Production

Kenneth Scheve University of Michigan
Matthew J. Slaughter Tuck School of Business at Dartmouth and NBER

A central question in the international and comparative political economy literatures on globalization is whether economic integration increases worker insecurity in advanced economies. Previous research has focused on the role of international trade and has failed to produce convincing evidence that such a link exists. In this article, we argue that globalization increases worker insecurity, but that foreign direct investment (FDI) by multinational enterprises (MNEs) is the key aspect of integration generating risk. FDI by MNEs increases firms' elasticity of demand for labor. More-elastic labor demands, in turn, raise the volatility of wages and employment, all of which tends to make workers feel less secure. We present new empirical evidence, based on the analysis of panel data from Great Britain collected from 1991 to 1999, that FDI activity in the industries in which individuals work is positively correlated with individual perceptions of economic insecurity. This correlation holds in analyses accounting for individual-specific effects and a wide variety of control variables.

Determining whether international economic integration in advanced economies increases worker insecurity is critical to competing explanations of welfare-state policymaking and the politics of globalization. An influential argument in the welfare-state literature is that increases in economic insecurity from globalization generate demands for more generous social insurance that compensates workers for a riskier environment (e.g., Boix 2004; Burgoon 2001; Cameron 1978; Garrett 1998; Hays, Ehrlich, and Peinhardt 2002; Rodrik 1997, 1998). The connection between globalization and welfare spending in this argument depends on the causal mechanism that international economic integration increases worker insecurity. Claims that no such link exists undermine this explanation for variation in welfare-state spending.

The link between economic integration and worker insecurity is also an essential element of explanations for patterns of public opposition to policies aimed at further liberalization of international trade, immigration, and foreign direct investment (FDI) in advanced economies. Economic insecurity may contribute to the backlash against globalization in at least two ways. First is a direct effect in which individuals that perceive globalization to be contributing to their own economic insecurity are much more likely to develop policy attitudes against economic integration. Second, if globalization limits the capacities of governments to provide social insurance, or is perceived to do so, then individuals may worry further about globalization and this effect is likely to be magnified if labor-market risks are heightened by global integration.

Previous empirical research has focused on whether one particular component of globalization, international trade, generates economic volatility. This research has been inconclusive. Among others, Rodrik (1997, 1998) argues in the affirmative and presents evidence that exposure to external risk from trade, measured by the interaction between trade openness and the standard deviation

Kenneth Scheve is Associate Professor of Public Policy, University of Michigan, 412 Lorch Hall, 611 Tappan Street, Ann Arbor, MI 48109-1220 (scheve@umich.edu). Matthew J. Slaughter is Associate Professor of Business Administration, Tuck School of Business at Dartmouth and NBER, 100 Tuck Hall, Hanover, NH 03755-3514 (Matthew.J.Slaughter@Dartmouth.Edu).

For financial support we thank the National Science Foundation for award #SES-0213671, the Yale Center for the Study of Globalization, the Carnegie Corporation's Globalization and Self-Determination Project at the Yale Center for International and Area Studies, the Leitner Program in International Political Economy, and the Institution for Social and Policy Studies. For very helpful data assistance we thank Simon Harrington at the U.K. Office of National Statistics and Ralf Martin. For helpful comments and discussion we thank Chris Anderson, Samuel Bowles, José Cheibub, Rafaela Dancygier, Keith Darden, Esther Duflo, Jonathan Haskel, Michael Hiscox, Hyeok Kwon, Lisa Martin, Fredrik Sjoholm, Mike Tomz, Michael Wallerstein, three anonymous reviewers, the editors, and seminar participants at Binghamton University, Cornell University, Duke University, the Santa Fe Institute, the University of Michigan and the University of Nottingham.

American Journal of Political Science, Vol. 48, No. 4, October 2004, Pp. 662–674

 ISSN 0092-5853

of a country's terms of trade, is positively correlated with growth volatility. In contrast, Iversen and Cusack (2000) contend that there is no convincing evidence that international trade increases economic insecurity. They argue that Rodrik's correlation is not sufficient and that it is necessary either that price volatility in international markets be greater than in domestic markets or that trade concentrate rather than diversify economic risks. Iversen and Cusack then present evidence that, at least for advanced economies, there is no correlation between trade openness and volatility in output, earnings, or employment.

In this article, we investigate whether international economic integration increases economic insecurity. Our analysis makes a substantial departure from existing research by focusing on a relatively overlooked dimension of globalization: the cross-border flow of FDI within multinational enterprises (MNEs). This focus on FDI rather than trade is rare in the literature, and we argue that this omission matters for both empirical and theoretical reasons.

Empirically, in recent decades, cross-border flows of FDI have grown at much faster rates than have flows of goods and services. UNCTAD (2001) reports that from 1986 through 2000, worldwide cross-border outflows of FDI rose at an annualized rate of 26.2%, versus a rate of 15.4% for worldwide exports of goods and services. In the second half of the 1990s this difference widened to 37.0% versus just 1.9%. Moreover, it is the multinationalization of production that a number of scholars have pointed to as the distinguishing feature of the current phase of globalization compared to previous eras (e.g., Bordo, Eichengreen, and Irwin 1999).

This lack of attention to FDI also matters because, as we will discuss, there are strong theoretical reasons to believe that FDI can substantially influence economic insecurity. The globalization of production by MNEs gives firms greater access to foreign factors of production and thus greater ease of substitution away from workers in any single location. As a result, workers feel more insecure. Stated in terms of the underlying labor economics, the central idea is that FDI by MNEs increases firms' elasticity of demand for labor. More-elastic labor demands, in turn, raise the volatility of wages and employment—and thereby raise worker insecurity.

This theoretical framework motivates our empirical analysis of the relationship between the multinationalization of production and the economic insecurity of workers. We present new evidence, based on analysis of individual-level panel data from Great Britain over 1991–1999, that FDI activity in the industries in which individuals work is positively correlated with individual perceptions of worker insecurity. This correlation holds in analyses accounting for individual-specific effects and a wide variety of control variables. Moreover, FDI exposure has one of the largest substantive effects in accounting for the within-individual variation in insecurity. We regard these individual-level panel results as the first valid evidence consistent with a causal relationship from FDI to worker insecurity.

There are four remaining sections to the article. The next section provides a theoretical framework for the economics of FDI and worker insecurity. The third section describes the data to be used in the study and the econometric models to be estimated. The fourth section reports the empirical results, and the final section concludes.

Theoretical Framework for FDI and Worker Insecurity

Defining Worker Insecurity

Although there are a number of alternative definitions of economic insecurity, most often it is understood to be an individual's perception of the risk of economic misfortune (Dominitz and Manski 1997). Consequently, researchers have focused on the risk of events such as the loss of health insurance, being a victim of a burglary, losing a job, and significant decreases in wages (e.g., Anderson and Pontusson 2001; Mughan and Lacy 2002).

It is likely that most people's perceptions of economic insecurity depend heavily on their purchasing power, which in turn depends on both their asset ownership and their labor-market status—both employment and income earned there from. In reality, the large majority of people rely much more on labor income than capital income for purchasing power. Accordingly, we think labor-market status is the main determinant of perceptions of economic insecurity.

In light of this labor-market focus, we conjecture that the economic misfortunes underlying people's economic insecurity stem mainly from more volatile employment and/or wage interactions with their employers. That is, risk-averse workers are not indifferent between employment options that yield the same amount of expected earnings but with differing degrees of certainty. More certain earnings outcomes—due to more certain wage and/or employment realizations—are preferred to less certain ones, and insecurity rises with this uncertainty.[1]

[1] It is important to note that there is now a large body of evidence that labor-market volatility has been rising in many countries, especially in the 1990s, in terms of greater earnings volatility, declining job tenure, and self-reports. Gottschalk and Moffitt (1994) report substantial increases in year-to-year earnings volatility for the United States over the 1970s and 1980s. Looking at the 1990s as well, a

Worker Insecurity in Labor-Market Equilibrium: Why FDI Matters

Equilibrium in a standard competitive labor market is set by the intersection of labor supply and labor demand. The labor-supply curve is aggregated across individuals, and at each point along it the elasticity of labor supply, η^S, is defined as the percentage change in the quantity of labor supplied by workers in response to a 1% increase in the price of labor. Higher wages typically induce a greater quantity of labor supplied.

The labor-demand curve is aggregated across firms, and at each point along it the elasticity of labor demand, η^D, is defined as the percentage decline (in absolute value) in the quantity of labor demanded in response to a 1% increase in the price of labor. This elasticity consists of two parts. The substitution effect tells, for a given level of output, how much firms substitute away from labor towards other factors of production when wages rise. The scale effect tells how much labor demand falls after a wage increase thanks to the rise in the firms' costs and thus the fall in their output and so demand for labor and all other factors. When wages rise, both the substitution and scale effects reduce the quantity of labor demanded.

In accord with a wide range of empirical evidence, we introduce volatility into the labor market by assuming that the labor-demand schedule is stochastic. To see what forces drive this volatility, note that each firm's labor-demand schedule traces out the *marginal revenue product* of its workers as the wage rate varies. A profit-maximizing firm hires workers until the revenue generated by the last worker hired equals the market wage that firm must pay that last worker.

For each firm, its product prices and technology are two key determinants of marginal revenue products. Aggregated across firms, then, the position of the labor-demand schedule depends crucially on all relevant product prices and production technologies. Define $\hat{mrp}$ as the percentage shift in the labor-demand schedule due to shocks to prices and/or technologies. It is straightforward to then show that the resulting percentage change in wages ($\hat{w}$) and employment ($\hat{e}$) are respectively given by $\hat{w} = (\frac{\eta^D}{\eta^S+\eta^D})\hat{mrp}$ and $\hat{e} = (\frac{\eta^D\eta^S}{\eta^S+\eta^D})\hat{mrp}$. If $\hat{mrp}$ is a random variable, then we can write $Var(\hat{w}) = (\frac{\eta^D}{\eta^S+\eta^D})^2 Var(\hat{mrp})$ and $Var(\hat{e}) = (\frac{\eta^D\eta^S}{\eta^S+\eta^D})^2 Var(\hat{mrp})$.

symposium issue of the *Journal of Labor Economics* (1999) documented declines in U.S job stability, especially in the 1990s for large groups of workers such as those with more tenure. Within that symposium issue, Schmidt's (1999) analysis of individual surveys finds that U.S. workers in the 1990s were more pessimistic about losing their jobs than they were during the 1980s—despite the ongoing economic expansion of the 1990s.

The above expressions demonstrate that greater volatility in labor-market outcomes—and thus greater economic insecurity—can arise either from greater aggregate volatility in prices and technology, $Var(\hat{mrp})$, or from a higher elasticity of demand for labor, η^D. The former can be thought of as the volatility of aggregate shocks to labor demand, and the latter can be thought of as the pass-through of those shocks into volatility of wages and employment. In this framework, the link between globalization and labor-market volatility depends on some component of globalization, such as trade or FDI, altering one of these quantities, $Var(\hat{mrp})$ or η^D.

We argue that an important channel through which FDI can affect labor-market volatility is by increasing labor-demand elasticities via the substitution effect. Suppose that a firm is vertically integrated with a number of production stages. A multinational firm can move abroad some of these stages (e.g., Helpman 1984). This globalization of production within multinationals gives access to foreign factors of production, either directly through foreign affiliates or indirectly through intermediate inputs. This expands the set of factors firms can substitute towards in response to higher domestic wages beyond just domestic nonlabor factors to include foreign factors as well. Thus, greater FDI can raise labor-demand elasticities—and so worker insecurity because of more volatile wage and employment outcomes.

This argument does not exclude other mechanisms through which globalization may increase economic insecurity. For example, openness to international trade may increase the volatility of aggregate shocks to labor demand ($Var(\hat{mrp})$). As discussed in the introduction, this is the link examined in much of the previous research on globalization and economic insecurity, and its empirical importance remains an open question. Another example is that theoretically, international trade in final goods—whether mediated by multinationals or not—could also affect insecurity by making labor demands more elastic through the scale effect. This procompetitive effect of trade has been well studied, and FDI can also work on the scale effect (e.g., as foreign firms compete with domestic incumbents).

We have focused on the substitution effect of FDI for several reasons. Most importantly, the substitution effect is direct in that it places domestic workers in competition with foreign labor for employment within the same firm. It is thus likely to have a larger effect on labor demand elasticities.[2] Further, other researchers have emphasized

[2]There are several recent empirical studies documenting that MNEs and FDI do increase labor-demand elasticities through the substitution effect. Slaughter (2001) estimates that demand for U.S.

in theory its possible role in generating insecurity (e.g., Rodrik 1997), but no compelling empirical evidence has been produced.

Before turning to an empirical test of the link between FDI and insecurity, we note one other important aspect of MNEs and labor markets. Many studies across a variety of countries have documented that establishments owned by MNEs pay *higher* wages than do domestically owned establishments. This is true even controlling for a wide range of observable worker and/or plant characteristics such as industry, region, and overall size. The magnitudes involved are usually quite big.[3]

This multinational wage premium may reflect several forces. It could be accounted for by higher worker productivity due to superior technology and/or capital; or by higher worker productivity due to unobservable worker qualities; or by greater profits and therefore more rent sharing with workers. Our theory framework suggests another possibility: that MNEs pay more to compensate workers for the greater labor-market volatility associated with MNEs.

Regardless of the cause(s) of the multinational wage premium, its existence is important for considering how the globalization of production affects economic insecurity. All else equal, this premium likely makes multinational employees feel *more* secure. Our focus on elasticities and labor-market volatility highlights MNE influences on different dimensions of the overall worker-firm relationship. These contrasting issues of labor-demand elasticities and wage premia suggest that the net impact of MNEs on worker insecurity is ex ante unclear. Whether wage premia fully compensate for increased risks from higher elasticities is an empirical question.

production labor in manufacturing became more elastic from 1960 to the early 1990s and that these increases were correlated with FDI outflows by U.S.-headquartered MNEs. Fabbri, Haskel, and Slaughter (2003) estimate that both U.K.-multinational plants and foreign-owned plants each had larger increases than did U.K. domestic plants in the elasticity of demand for production labor in manufacturing over 1973–1992. An important margin on which MNEs may affect elasticities is on the extensive margin of plant shutdowns. MNEs may be more likely than domestic firms to respond to shocks by closing entire plants. For the manufacturing sectors in at least three countries it has now been shown that plants that are part of an MNE are more likely to close than are their purely domestic counterparts: the United Kingdom (Fabbri, Haskel, and Slaughter 2003); the United States (Bernard and Jensen 2002); and Ireland (Gorg and Strobl 2003).

[3]Doms and Jensen (1998) document that for U.S. manufacturing plants in 1987, multinational wages exceeded domestically owned wages by a range of 5–15%, with larger differentials for production workers rather than nonproduction workers. Griffith (1999) presents similar evidence for the United Kingdom; Globerman, Ries, and Vertinsky (1994) for Canada; Aitken, Harrison, and Lipsey (1996) for Mexico and Venezuela; and Te Velde and Morrissey (2001) for five African countries.

Data Description and Empirical Specification

Data Description

In light of the theory discussion above, the objective of our empirical work is to examine the impact of FDI on economic insecurity. Specifically, we will evaluate how individual self-assessments of economic insecurity correlate with the presence of mobile capital in the form of FDI in the industries in which individuals work. Our data cover Great Britain, which we think is an excellent case to examine both because inward and outward FDI have long figured prominently in the overall economy and because of the high quality of data available.

The individual data are from the *British Household Panel Survey* (BHPS; 2001). This survey is a nationally representative sample of more than 5,000 U.K. households and over 9,000 individuals questioned annually from 1991 to 1999.[4] It records detailed information about each respondent's perceptions of economic insecurity, employment, wages, and many other characteristics. The most important pieces of survey information required for our analysis are a measure of economic insecurity, identification of the respondents' industry of employment, and repeated measurement of the same individual over time.

We measure economic insecurity by responses to the following question asked in each of the nine years of the panel.

> "I'm going to read out a list of various aspects of jobs, and after each one I'd like you to tell me from this card which number best describes how satisfied or dissatisfied you are with that particular aspect of your own present job—job security."

The ordered responses are on a seven-point scale ranging from "not satisfied at all" to "completely satisfied." Consistent with our interest in the labor-income dimension of economic insecurity, this question measures perceptions of employment risks. We constructed the variable *Insecurity* by coding responses in the reverse order from the original question, with a range from 1 for individuals who give the response "completely satisfied" to a 7 for those individuals giving the response "not satisfied at all." Higher values of *Insecurity* thus indicate less satisfaction with job security.

Our theoretical framework hypothesizes that high FDI activity in industries may generate economic insecurity among workers by increasing labor-demand

[4]The BHPS is ongoing, but our data are through 1999 only.

elasticities. Theory does not offer clear guidance on how to measure this crucial concept of FDI exposure, so to test our key hypothesis we constructed three alternative measures.

First, from the U.K. Office of National Statistics (ONS) we obtained data on inward and outward FDI investment positions in all two-digit 1992 Standard Industry Classification (SIC92) U.K. industries from 1991 through 1999.[5] The BHPS records respondent industry of employment by the 1980 Standard Industry Classification (SIC80), so we concorded the FDI data to two-digit SIC80 industries.[6] We then merged the industry-level FDI data with the BHPS survey.

Our first, and main, measure of FDI exposure is a dichotomous industry-level variable *FDI Presence*. We set *FDI Presence* equal to one if two conditions were met: if the industry had any positive FDI investment, inward or outward, and if the industry's activities do not require producers and consumers to be in the same geographic location. If either of these conditions were not met, we coded *FDI* equal to zero. As with all our FDI measures, *FDI Presence* varies by both industry and year.

Our logic in defining *FDI Presence* with these two conditions runs as follows. The first condition of positive FDI investment is straightforward. Any inward or outward FDI activity satisfies this. The second condition recognizes that FDI activity is less likely to alter labor-demand elasticities if business activities cannot be outsourced across countries because the consumer and producer must be in the same geographic location.

Consider the examples of wholesale trade, retail trade, and personal services (e.g., haircuts). The large majority of business activities in these industries require the colocation of producers and consumers: e.g., customers sitting in the barber's chair. The notions of economic insecurity related to FDI that we discussed in the second section focus on the substitutability of business activities across countries. In reality, in many industries, FDI does not have this characteristic; indeed, FDI may arise precisely because foreign customers cannot be served at a distance via international trade. Accordingly, *FDI Presence* identifies not all industries with FDI, but instead only those industries with FDI in which business activities can be outsourced across countries. So for industries such as wholesale trade, retail trade, and personal services we coded *FDI Presence* as zero regardless of the level of actual FDI.

It is theoretically ambiguous if, in addition to the *existence* of FDI activity, the *magnitude* also matters. It may be that more FDI activity indicates greater capital mobility, which in turn raises labor-demand elasticities and perceptions of employment risks. Since the dichotomous *FDI Presence* does not distinguish FDI magnitudes once any FDI is present, we also constructed two continuous measures of FDI exposure that account for magnitudes relative to industry size.

The variable *FDI Total Share* equals the sum of inward and outward FDI stocks divided by U.K. gross value added (again, except for industries that require producers and consumers to colocate, for which the variable was coded zero). The main concern about this measure is that its denominator covers U.K. activity only, but the numerator covers not just inward FDI into the U.K. but also outward FDI out of the U.K. This mismatch of scope cannot be addressed (in part because the data do not disaggregate host countries for outward FDI), but it likely introduces error in our measurement of the underlying concept of FDI exposure.

Our second continuous measure of FDI exposure addresses this concern by including in the numerator only inward FDI. Thus, *FDI Inward Share* equals inward FDI divided by gross value added (again, except for industries that require producers and consumers to colocate, for which the variable was coded zero). This measure generates the opposite trade-off: no mismatch of scope, but in theory outward FDI can matter for FDI exposure just as inward FDI does. Inward and outward FDI flows tend to be highly correlated, however, which suggests that on balance *FDI Inward Share* might be preferred to *FDI Total Share*.

It is important to recognize the level of aggregation for the FDI regressors. Our use of two-digit industries is dictated by ONS rules on public data dissemination. Theoretically, we could imagine measuring FDI exposure more finely at the level of the respondent's company, rather than at the more aggregated industry level.[7] Our specification implicitly assumes that within each industry, all workers perceive FDI threats equally regardless of whether each works for a firm with some FDI. This assumption seems reasonable. We are simply assuming that important features of the labor demand faced by workers are set in the industry of employment rather than the firm.[8]

[5]For his assistance in generating this data, we thank Simon Harrington.

[6]The BHPS records industry of employment according to the SIC80 classification scheme in all years but does report this information according to the SIC92 system in two of the years in our sample.

[7]Of course, this is only a theoretical possibility. Even if we had firm-level FDI data, it would not be usable because the BHPS does not report the respondent's firm.

[8]Our focus on industries as a relevant aggregate for labor-market effects is also consistent with many empirical findings in the labor-economics literature. For example, a common finding in studies of

Beyond FDI exposure, perceptions of economic insecurity may also be shaped by a number of characteristics of individuals and the industries in which they are employed. Accordingly, for our main analyses we constructed four individual-level and two industry-level variables. The variable *Income* measures annual household income in thousands of U.K. pounds.[9] *Union* equals one if the individual belongs to a workplace union and zero if not. *Education* is a categorical variable ranging from one to four, with higher values for more educational attainment.[10] The variable *Age* equals the respondent's age in years at the time of the survey. *Manufacturing* is an indicator variable equal to one if the respondent's industry of employment is in the manufacturing sector and equal to zero otherwise. Finally, *Sector Unemployment* measures the share of workers unemployed in each respondent's industry of work.[11]

Each of these six control variables is likely to account for some of the differences among individuals in perceptions of economic insecurity. However, it must be acknowledged that other unmeasured or unobservable differences among individuals may also matter. For example, individuals almost surely vary in their degree of risk aversion. In addition, individuals probably vary in their interpretation of the BHPS question. One individual may think about job security in compensated terms conditional on wages and any perceived compensating wage differential. But another observationally similar individual may think without conditioning in this way.

Unmeasured or unobservable individual heterogeneity is, of course, a problem that faces all survey research. But it seems particularly acute here because our key variable to be explained measures perceptions of risk. So to address this heterogeneity, we use the fact that the BHPS records repeated observations for the same individual over many years. We exploit this panel structure by including an individual-specific effect for each respondent. These individual-specific effects capture any time-constant factors across people that drive variation in perceptions of employment risks.

For each year of our panel, Table 1 reports summary statistics of our key variables. The summary statistics and our subsequent analyses are based on the BHPS subsample of private sector, full-time workers who are not self-employed. It is for this group of workers that our theoretical framework most directly applies. *Insecurity* averages just below three in most years, suggesting that the average respondent was fairly satisfied with his or her job security.

FDI Presence, our main measure of exposure to the globalization of production, averages slightly over half in most years—i.e., in most years just over half of respondents worked in FDI-exposed industries. Industries with positive values for *FDI Presence* include metal manufacturing, mechanical engineering, and banking and finance. Among these industries in 1991, the sector with the most respondents was mechanical engineering. The industries meeting our two conditions for being FDI exposed vary over time, with sectors such as instrument engineering and business services being added to the list.

Econometric Models

By matching each BHPS observation with the relevant industry FDI information, we examine how self-assessments of economic insecurity relate to FDI exposure. We formalize the determinants of economic insecurity as follows,

$$Insecurity_{it} = \alpha_i + \beta \mathrm{FDI}_{it} + \gamma Z_{it} + \varepsilon_{it} \tag{1}$$

where the subscript i indexes individuals; the subscript t indexes years; $Insecurity_{it}$ is our measure of economic insecurity; FDI_{it} is one of our measures of FDI exposure; the vector Z_{it} includes dichotomous indicators for each year and, in many specifications, the control regressors discussed above; α_i, β, and γ are parameters to be estimated; and ε_{it} is an additive error term.

The coefficient estimates of β in Equation (1) indicate whether and to what extent individual perceptions of economic insecurity are correlated with FDI exposure. Exposure to FDI activity is increasing in each of our three FDI variables, and we expect this to be positively correlated with the dependent variable *Insecurity.* This is the central hypothesis of our empirical analysis. Thus, our null hypothesis is that $\beta = 0$, with the alternative $\beta > 0$.

The panel nature of the BHPS data is indicated in (1) by the i and t indexes. Pooling individuals across years has obvious advantages but generates a number of

profit-sharing is that wage-bargaining keys off of industry profits above and beyond firm considerations. Of course, over longer time horizons than we consider in this article, workers could be assumed to be facing an economy-wide labor demand curve.

[9]Annual household income is a variable calculated by the BHPS to include income from all sources in the 12 months prior to the September of the survey year, as virtually all of the fieldwork for each survey year is done from September to December.

[10]For example, category one indicates no qualifications or still in school and no qualifications, while category four includes teaching qualifications, first degree, or higher degree.

[11]These data were obtained directly from the ONS and are based on its Labour Force Survey.

Table 1 Summary Statistics

	Year								
Variable	**1991**	**1992**	**1993**	**1994**	**1995**	**1996**	**1997**	**1998**	**1999**
Insecurity	2.978	3.021	2.916	2.941	2.881	2.789	2.681	2.663	2.726
	(1.982)	(1.748)	(1.663)	(1.708)	(1.641)	(1.563)	(1.532)	(1.465)	(1.579)
FDI Presence	0.424	0.424	0.612	0.548	0.567	0.604	0.592	0.577	0.565
	(0.494)	(0.494)	(0.487)	(0.498)	(0.496)	(0.489)	(0.492)	(0.494)	(0.496)
FDI Total Share	0.388	0.394	0.424	0.389	0.405	0.459	0.465	0.570	0.732
	(0.600)	(0.597)	(0.634)	(0.575)	(0.552)	(0.598)	(0.683)	(0.798)	(1.129)
FDI Inward Share	0.189	0.190	0.167	0.151	0.172	0.178	0.185	0.223	0.236
	(0.299)	(0.294)	(0.290)	(0.246)	(0.247)	(0.258)	(0.300)	(0.313)	(0.368)
Education	2.262	2.325	2.391	2.437	2.469	2.511	2.539	2.558	2.538
	(0.897)	(0.894)	(0.900)	(0.911)	(0.905)	(0.901)	(0.884)	(0.870)	(0.876)
Age	35.471	35.696	35.597	35.646	35.650	35.550	35.499	35.809	36.122
	(12.029)	(11.747)	(11.616)	(11.619)	(11.564)	(11.527)	(11.725)	(11.885)	(11.698)
Income	23.766	25.219	25.817	26.377	27.807	29.319	29.650	30.572	30.721
	(13.560)	(14.209)	(13.632)	(14.698)	(15.788)	(16.417)	(17.259)	(20.565)	(22.784)
Union	0.279	0.260	0.231	0.209	0.227	0.210	0.193	0.188	0.205
	(0.449)	(0.439)	(0.421	(0.407)	(0.419)	(0.407)	(0.395)	(0.391)	(0.404)
Manufacturing	0.341	0.329	0.318	0.298	0.314	0.310	0.298	0.286	0.276
	(0.474)	(0.470)	(0.466)	(0.458)	(0.464)	(0.462)	(0.458)	(0.452)	(0.447)
Sector Unemployment	0.091	0.089	0.088	0.077	0.073	0.065	0.051	0.051	0.048
	(0.035)	(0.033)	(0.031)	(0.025)	(0.024)	(0.021)	(0.017)	(0.017)	(0.017)
Observations	2,654	2,292	2,153	2,247	2,379	2,525	2,698	3,060	4,058

Notes: The BHPS sample in each year is private-sector, full-time workers who are not self-employed. Each cell reports the variable mean and, in parentheses, its standard deviation.

estimation issues regarding individual heterogeneity. It is likely that observations over time for the same individual will be more similar than observations across different individuals. This might be due to persistence in or unmodeled characteristics of individual perceptions of economic insecurity. This is particularly pertinent to our analysis because, as discussed above, there are good reasons to think that unobserved factors may affect perceptions of economic insecurity. So (1) allows α to vary across individuals to capture unmeasured or unobserved heterogeneity.

Equation (1) can be estimated via random- or fixed-effects estimators. The random-effects estimator generates consistent parameter estimates if the individual effects are uncorrelated with the other explanatory variables. The fixed-effects estimator is also consistent under this assumption, but is less efficient. Under the alternative hypothesis that the individual effects are correlated with other explanatory variables, only the fixed-effects estimator is consistent. We will use both methods to estimate (1) and report diagnostics to evaluate the estimators. We will also use a number of alternative econometric specifications, including a dynamic panel model with a lagged dependent variable.

Empirical Results

Baseline Specifications and Results

Table 2 reports random-effects and fixed-effects results for Equation (1) for our main measure of FDI exposure, *FDI Presence*. In the first two sets of estimates reported, the only control variables are the year indicator variables. Across these two sets of results, the main substantive finding is a positive correlation between *FDI Presence* and *Insecurity*. The magnitude of the estimated effect is over twice as large in the random-effects specification. The coefficients for the year variables indicate deviations in mean levels of insecurity in each year from the base year 1991. In both specifications the parameter estimates are negative for every year except 1992 and turn significantly negative after 1995. This indication of lower average levels of insecurity in later years is broadly consistent with the U.K. macroeconomic performance over the 1990s:

TABLE 2 Panel Analysis of Economic Insecurity, 1991–1999

	Random Effects			Fixed Effects			Fixed Effects		
Regressor	**Coef.**	**S.E.**	**p-value**	**Coef.**	**S.E.**	**p-value**	**Coef.**	**S.E.**	**p-value**
FDI Presence	0.238	0.024	0.000	0.103	0.032	0.001	0.101	0.037	0.006
Education							0.098	0.049	0.046
Age							0.021	0.045	0.637
Income							−0.002	0.001	0.052
Union							0.100	0.048	0.036
Manufacturing							−0.009	0.046	0.848
Sector Unemployment							3.032	0.603	0.000
Year 1992	0.068	0.038	0.071	0.099	0.039	0.012	0.090	0.059	0.124
Year 1993	−0.093	0.039	0.016	−0.027	0.041	0.508	−0.048	0.098	0.622
Year 1994	−0.070	0.038	0.070	−0.014	0.041	0.737	−0.019	0.141	0.895
Year 1995	−0.090	0.039	0.021	−0.020	0.042	0.635	−0.044	0.185	0.813
Year 1996	−0.194	0.038	0.000	−0.126	0.042	0.002	−0.144	0.228	0.527
Year 1997	−0.284	0.038	0.000	−0.207	0.041	0.000	−0.203	0.273	0.457
Year 1998	−0.291	0.037	0.000	−0.200	0.041	0.000	−0.217	0.318	0.496
Year 1999	−0.241	0.036	0.000	−0.171	0.042	0.000	−0.206	0.362	0.569
Constant	2.831	0.031	0.000	2.856	0.032	0.000	1.693	1.427	0.235
Observations		24,671			24,671			24,075	
Individuals		7,328			7,328			7,163	
T		$1 \leq T \leq 9$			$1 \leq T \leq 9$			$1 \leq T \leq 9$	

initial recession followed by increasingly strong economic growth.

Although the main substantive story is the same across these first two specifications in Table 2, it is still necessary to determine our relative confidence in the two estimators. We employed the Hausman specification test: if the random-effects assumption that the individual-specific effects are uncorrelated with the explanatory variables is true, then coefficient estimates from the two models should not be statistically different. The test statistic, distributed χ^2 with degrees of freedom equal to the number of coefficients (here, nine), equals 55.15. This strongly rejects the null hypothesis that the coefficients do not differ statistically and suggests violation of the key random-effects assumption. Consequently, we prefer the fixed-effects estimator.

The third set of estimates of (1) reported in Table 2 continues with the fixed-effects estimator but adds to the year indicators six additional control variables: *Education, Age, Income, Union, Manufacturing*, and *Sector Unemployment*. The main result is that *FDI Presence* continues to be positively correlated with *Insecurity*. The size and statistical significance of this correlation is virtually unchanged from that in our second specification. Results for the control variables are also of interest. Respondents tend to be more insecure who are more educated, in households with lower income, in unions, or working in sectors with higher unemployment.

The impact of unemployment rates seems straightforward. The result for education may reflect the "aspiration effect" documented in previous studies of job satisfaction: more educated workers tend to "expect more" from all aspects of their jobs, perhaps including job security. The negative correlation between *Insecurity* and *Income* may reflect two influences: individuals may be less concerned about prospective job losses if higher household income offers some insurance, or if they believe their income compensates for those risks. And the union-membership effect may be due to unionized workers having more information about prospective job losses than their nonunionized counterparts. Finally, note that this specification shows no correlation between worker insecurity and either age or working in manufacturing and also that the precision of estimates on all year indicators has fallen below standard significance levels.

To assess the substantive impact of forces shaping worker insecurity, we calculate an "average" effect for each regressor by multiplying each coefficient estimate by each variable's sample variation. We do not, however, want to use the sample variation reported in the summary statistics of Table 1. This is because the year-by-year standard deviations in Table 1 reflect variation across individuals at each point in time, whereas our fixed-effects estimates in Table 2 reflect variation within individuals across time. Consequently, for the estimation subsample in our final specification of Table 2, we decomposed the total variation in each regressor of interest into one part reflecting variation within years across individuals and the other part reflecting variation within individuals over time. For each regressor we then multiplied the standard deviation of the latter by the related coefficient estimate to calculate the impact of a one-standard-deviation change in each regressor on the reported values for *Insecurity*.

These calculations indicate that a one-standard-deviation increase in *FDI Presence* translates into a 0.027 increase in *Insecurity*. This amount is larger than the increases in *Insecurity* stemming from one-standard-deviation changes in *Union* (0.018), *Education* (0.017), or *Income* (0.016), and is exceeded only by the *Insecurity* increase correlated with *Sector Unemployment* (0.058). We think that *Sector Unemployment* is driven mainly by business-cycle fluctuations and thus that among our remaining "structural" controls FDI exposure appears to be the substantively most important determinant of worker perceptions of job insecurity.

What do we conclude from Table 2? Holding other factors constant, individuals employed in FDI sectors systematically report less satisfaction with their job security. This is strongly consistent with the hypothesis that FDI exposure generates economic insecurity in workers. To verify this central conclusion, we now turn to a number of robustness checks.

Robustness Checks

Our first check was for robustness to alternative measures of FDI exposure. Table 3 replicates our fixed-effects specifications of Table 2 but replaces *FDI Presence* first with *FDI Total Share* in columns 1 and 2 and then with *FDI Inward Share* in columns 3 and 4. In columns 1 and 3 the Z_{it} vector includes just year indicators; columns 2 and 4 add the six additional controls from Table 2. Both estimated coefficients on *FDI Total Share* are approximately 0.04 with a standard error of 0.02. Similarly, both estimated coefficients on *FDI Inward Share* (which, recall, may be the preferred to *FDI Total Share*) are approximately 0.15 with a standard error of 0.05. Thus, we again find a positive and statistically significant correlation between FDI exposure and perceptions of economic insecurity.

Next, we evaluated the sensitivity of our results to alternative approaches to accounting for persistence in panel data. Modeling individual-specific effects is one way of accounting for this correlation. But this approach does not allow us to differentiate between the idea that persistence in observations of insecurity is accounted for by the influence of past experiences of insecurity on present perceptions and the alternative idea that certain individuals just have unobserved characteristics that lead them to have certain types of perceptions (Green and Yoon 2002; Wawro 2002). To make this assessment, it is necessary to add a lag of the dependent variable to the right-hand side of Equation (1).

A lagged dependent variable is obviously correlated with the individual-specific effects; consequently, this specification cannot be estimated via random effects.

TABLE 3 Panel Analysis of Economic Insecurity with Alternative Measures of FDI Exposure, 1991–1999

Regressor	Fixed Effects	Fixed Effects	Fixed Effects	Fixed Effects
FDI Total Share	0.042	0.041		
	(0.020)	(0.020)		
	0.034	0.043		
FDI Inward Share			0.159	0.153
			(0.050)	(0.052)
			0.002	0.004
Year Dummies	Yes	Yes	Yes	Yes
Control Variables	No	Yes	No	Yes
Observations	24,662	24,066	24,662	24,066
Individuals	7,325	7,160	7,325	7,160
T	$1 \leq T \leq 9$	$1 \leq T \leq 9$	$1 \leq T \leq 9$	$1 \leq T \leq 9$

Notes: Each cell reports the coefficient estimate; in parentheses, its standard error; and its p-value.

TABLE 4 Dynamic Panel Analysis of Economic Insecurity, 1993–1999

	Arellano-Bond			Arellano-Bond with Instruments for *FDI*			Arellano-Bond with Instruments for *FDI*		
Regressor	**Coef.**	**S.E.**	**p-value**	**Coef.**	**S.E.**	**p-value**	**Coef.**	**S.E.**	**p-value**
Δ *Insecurity*$_{(t-1)}$	0.198	0.021	0.000	0.231	0.016	0.000	0.231	0.016	0.000
Δ *FDI Presence*	0.107	0.051	0.037	0.266	0.181	0.142	0.281	0.195	0.150
Δ *Education*							−0.045	0.085	0.593
Δ *Age*							−0.039	0.059	0.509
Δ *Income*							0.001	0.001	0.353
Δ *Union*							0.150	0.082	0.068
Δ *Manufacturing*							−0.042	0.132	0.750
Δ *Sector Unemployment*							0.210	1.083	0.846
Δ *Year 1993*	−0.091	0.041	0.027	−0.118	0.052	0.023	−0.120	0.055	0.029
Δ *Year 1994*	−0.043	0.042	0.310	−0.059	0.046	0.200	−0.054	0.047	0.250
Δ *Year 1995*	−0.003	0.041	0.949	−0.019	0.044	0.666	−0.014	0.044	0.747
Δ *Year 1996*	−0.087	0.039	0.026	−0.110	0.042	0.009	−0.103	0.043	0.016
Δ *Year 1997*	−0.173	0.036	0.000	−0.188	0.038	0.000	−0.175	0.038	0.000
Δ *Year 1998*	−0.092	0.033	0.005	−0.099	0.034	0.004	−0.084	0.034	0.015
Constant	−0.026	0.009	0.003	−0.033	0.011	0.002	0.005	0.060	0.930
Observations		13,397			13,397			13,178	
Individuals		3,785			3,785			3,735	
T		$1 \leq T \leq 7$			$1 \leq T \leq 7$			$1 \leq T \leq 7$	

Moreover, the fixed-effects estimator is also biased and inconsistent in the presence of a lagged dependent variable when, as in our data, the number of periods is small. There are a number of alternative estimators for this situation, some of which first-difference the data to deal with individual-specific effects and then use instrumental variables to address the correlation between the error term and lagged dependent variable generated by differencing (see Wawro 2002). We use the Arellano-Bond generalized method-of-moments estimator.

The first three columns of Table 4 reports results for this estimator, which adds a lag of the dependent variable to our econometric model of economic insecurity. In comparing these results with those earlier, note that the number of individuals and total observations has significantly declined. First differencing and the use of lagged instruments results in the loss of the 1991 and 1992 data altogether. It also means that individuals must be retained in the panel for three years to be included in the analysis.

The estimated coefficient on the lagged dependent variable is 0.198 with a standard error of 0.021. This suggests that past shocks to individual perceptions of economic insecurity do affect current perceptions, above and beyond the influence of individual-specific characteristics, though the magnitude of this effect is not large. The estimated β is 0.107 with a standard error of 0.051. This estimate divided by one minus the coefficient estimate on the lagged dependent variable yields the *long-run* effect of FDI exposure on economic insecurity. This long-run impact is 0.133, which is approximately the same magnitude as the analogous fixed-effects estimates in Table 2, and it is statistically significant at the 0.05 level. We regard this to be a quite rigorous test of our central hypothesis. A significant correlation between exposure to FDI and perceptions of economic insecurity remains conditional on controls for individual heterogeneity, for the persistence of perceptions of economic insecurity, and for year-to-year shocks in insecurity.

To assess the validity of these results, we conducted three diagnostic tests recommended by Arellano and Bond (1991).[12] Consistency of their estimator requires the errors to be serially uncorrelated, in which case first-differenced residuals should display negative first-order serial correlation but not second-order serial correlation. The z-value for the hypothesis test under the null hypothesis of no first-order autocorrelation is −20.45, suggesting rejection of this null. The z-value for the hypothesis test

[12] Following Arellano and Bond (1991), we report coefficient estimates based on their one-step estimator with robust standard errors and diagnostics based on their two-step estimator.

under the null hypothesis of no second-order autocorrelation is 1.15, suggesting retention of this null. These two test results are consistent with the assumptions of the Arellano-Bond estimator.

Arellano and Bond (1991) also develop a Sargan test that helps further assess whether the assumptions about serial correlation hold. The null hypothesis of this test is that the model's overidentifying restrictions are valid; rejection of the null suggests the need to respecify the model. The test statistic, distributed χ^2 with 27 degrees of freedom, equals 29.75, indicating that we do not have evidence to reject the null hypothesis that the overidentifying restrictions are valid. Overall, none of the three diagnostic tests raises significant concerns about the basic assumptions required for valid implementation of the Arellano-Bond estimator as in Table 4.

A third important robustness issue is the possibility of estimation bias due to endogeneity and/or measurement error. On endogeneity, it might be argued that individual FDI exposure is not strictly exogenous because individuals may choose their industry of employment based (at least partly) on their perceptions of economic insecurity. Less-secure risk-averse workers might choose not to work in FDI-exposed sectors, while risk-loving workers might choose the opposite. On measurement error, it might be argued that all of our FDI regressors are imperfect measures of the underlying economic concept of interest, labor-market riskiness linked to multinationals.

The endogeneity of worker industry choice is certainly a possibility. That said, in our panel a substantial proportion of changes over time in individual FDI exposure arise from changes in industry FDI status, which are clearly exogenous relative to individual perceptions, rather than from changes in individual industry of employment. Moreover, to the extent that people do switch industries endogenously, this should bias the coefficient on FDI exposure down, away from our hypothesized positive effect. These considerations mean that our results reported thus far may *underestimate* the effect of FDI exposure on perceptions of economic insecurity.

Nonetheless, both to relax the strict exogeneity assumption and to address measurement concerns, we used the panel structure of the data by allowing previous errors (i.e., unforecasted realizations of *Insecurity*) to influence future changes in *FDI Presence*. The model estimated is the same dynamic panel as in the first part of Table 4, but now *FDI Presence* is instrumented for using its lagged levels and changes in the same way that the Arellano-Bond estimator instruments for lagged *Insecurity*. This approach accounts for potential endogeneity, and it also yields consistent estimates on *FDI Presence* in the case of random measurement error.

The results of this analysis are reported in the balance of Table 4 for specifications with and without a full set of control variables. As expected, the coefficient estimate on *FDI Presence* is substantially larger than in the first part of Table 4: the implied long-run effect is now 0.35, well over twice as large as before. The standard error is also now larger, but the coefficient estimate is still significant at the 0.15 level. One method for evaluating whether relaxing the strict exogeneity assumption is warranted is to compare the p-value of the Sargan test across specifications. For the three sets of estimates in Table 4 the p-values are essentially identical, which yields little information on the value of instrumenting. Based on these results and our related discussion, we conclude that endogeneity and measurement error are not serious problems for the key finding that FDI exposure influences perceptions of economic insecurity.[13]

A fourth important robustness issue we considered was specification choice and potential omitted-variable bias. Tables 2 through 4 include as controls time-constant person-specific factors as well as a core set of six additional regressors. We verified that our FDI-insecurity correlation of interest maintained for specifications including other possible controls. We investigated a number of other possibilities including industry unionization rates and possible measures of "deindustrialization" (proxied with changes in absolute or relative levels of industry employment or sales). Including these variables did not substantially change our results nor were these variables significantly correlated with the dependent variable *Insecurity*.

A fifth and final robustness check we mention is sensitivity to estimation sample. Our core results are for a sample of private-sector, full-time, not-self-employed workers: the labor-market participants for which the theoretical framework most directly applies. Our FDI-insecurity correlation of interest maintained in estimates of key specifications using broader samples.

Conclusion

A central question in political and academic debates about international economic integration is whether globalization increases worker insecurity. Previous empirical research has focused on whether one particular component

[13]For measurement error, we also investigated whether small changes in the industry-by-industry coding of the *FDI* variable had any effect on its coefficient estimates. For example, we reestimated all tables when agriculture and mining industries were not included as possible FDI-exposed industries. All changes we investigated had minimal impact on our core results.

of globalization, international trade, generates economic volatility. The findings in this research have been mixed. In this article, we argue that FDI by multinational enterprises is an important aspect of globalization generating worker insecurity. FDI gives firms greater access to foreign factors of production, and thus greater ease of substitution away from workers in any particular location. As a result, workers feel more insecure. Stated in terms of the underlying labor economics, the central idea is that FDI by MNEs increases firms' elasticity of demand for labor. More elastic labor demands, in turn, raise the volatility of wages and employment, all of which tends to make workers feel more insecure.

The article provides the first empirical test at the individual level of the relationship between the multinationalization of production and the economic insecurity of workers. Our analysis of panel data from Great Britain over the 1990s finds that FDI activity in the industries in which individuals work is positively correlated with individual perceptions of economic insecurity. This relationship holds even in fixed-effects specifications for which only variation within rather than across respondents is used to estimate the correlation between exposure to FDI and perceptions of insecurity.

We regard these individual-level panel results as the first valid evidence consistent with a causal relationship between FDI and worker insecurity. While the role of trade in increasing worker insecurity remains an open question, our findings establish a clear link between international economic integration—through the mechanism of FDI—and insecurity.

Our use of U.K. data offers some important advantages. The key idea of MNEs raising worker insecurity via labor-demand elasticities assumes both cross-country FDI mobility and competitive labor markets. With its high degree of FDI inflows and outflows and relatively flexible labor markets, the United Kingdom closely matches this theoretical framework. Moreover, publicly available U.K. data allowed us to create a data set with key ingredients including individual measures of economic insecurity over many years, industry of employment, and FDI exposure for those industries. In countries with less flexible labor markets, increases in labor-demand elasticities from increases in FDI might not alter the employment risks facing workers. Understanding how national labor-market institutions mediate the link from FDI to worker insecurity seems a fruitful agenda for future research.

Our findings have significant implications for the politics of globalization and for the determinants of welfare-state policymaking. On the politics of globalization, our findings extend existing explanations for public backlash against globalization in advanced economies. This literature suggests that the backlash is not purely a constructed phenomenon and that the labor-market consequences of liberalization affect policy opinions. Individuals whose real and/or relative wages are lowered by liberalization are likely to have more skeptical opinions (Gabel 1998; Mayda and Rodrik 2001; O'Rourke and Sinnott 2001; Scheve and Slaughter 2001a, 2001b, 2004). These labor-market arguments, however, largely focus on the relationship between globalization and the level and distribution of earnings.

In contrast, the approach of our article recognizes that risk-averse workers care about the volatility of their earnings, in addition to their level—in particular, volatility from the risk of unemployment. These concerns about volatility are likely to help shape opinions about economic integration (see also Hays, Ehrlich, and Peinhardt 2002).

In broadening the focus of labor-market concerns from just earnings levels to earnings volatility, we note that our findings suggest possible ambivalence in policy attitudes towards FDI. On the one hand, multinationals tend to pay higher wages. But as this article has analyzed, on the other hand multinationals also tend to have more elastic labor demands. Indeed, the higher wages may be a compensating differential for the riskier outcomes. Higher wages should tend to make workers favor policies to attract FDI, but riskier employment should pull in the opposite direction. We regard this tension to be an important issue in any research attempting to explain the politics of foreign-investment policies in advanced economies.

On the welfare state, our research provides compelling individual-level evidence of a link between globalization and perceptions of worker insecurity. It is left for future research to determine whether there is, in turn, a link from increased insecurity to greater demand for social insurance. Nevertheless, our findings are critical to this debate because they provide strong evidence for the premise of the argument connecting globalization to worker demands for social insurance—that is, that international economic integration raises worker insecurity. Moreover, if globalization limits the capacities of governments to provide social insurance, or is perceived to do so, then individuals may worry further about globalization and this effect is likely to be magnified by increased labor-market risks from global integration.

References

Aitken, Brian, Ann Harrison, and Robert E. Lipsey. 1996. "Wages and Foreign Ownership: A Comparative Study of Mexico, Venezuela, and the United States." *Journal of International Economics* 40(3–4):345–71.

Anderson, Christopher, and Jonas Pontusson. 2001. "Welfare States and Employment Insecurity: A Cross-National Analysis of 15 OECD Countries." Presented at the annual meetings of the American Political Science Association.

Arellano, Manuel, and Stephen Bond. 1991. "Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations." *Review of Economic Studies* 58(2):277–97.

Bernard, Andrew B., and J. Bradford Jensen. 2002. "The Death of Manufacturing Plants." National Bureau of Economic Research Working Paper No. 9026.

Boix, Carles. 2004. "Between Protectionism and Compensation: The Political Economy of Trade." In *Globalization and Egalitarian Redistribution*, ed. Pranab Bardhan, Samuel Bowles, and Michael Wallerstein. Washington: Russell Sage Foundation. Forthcoming.

Bordo, Michael, Barry Eichengreen, and Douglas Irwin. 1999. "Is Globalization Today Really Different from Globalization a Hundred Years Ago?" In *Brookings Trade Forum 1999*, ed. Dani Rodrik and Susan Collins. Washington: Brookings Institution Press, pp. 1–50.

Burgoon, Brian. 2001. "Globalization and Welfare Compensation: Disentangling the Ties that Bind." *International Organization* 55(3):509–51.

Cameron, David. 1978. "The Expansion of the Public Economy: A Comparative Analysis." *American Political Science Review* 72(4):1243–61.

Dominitz, Jeff, and Charles F. Manski. 1997. "Perceptions of Economic Insecurity." *Public Opinion Quarterly* Volume 61(2):261–87.

Doms, Mark E., and J. Bradford Jensen. 1998. "Comparing Wages, Skills, and Productivity Between Domestically and Foreign-Owned Manufacturing Establishments in the United States." In *Geography and Ownership as Bases for Economic Accounting*, ed. R. Baldwin, R. Lipsey, and J. Richardson. Chicago: University of Chicago Press, pp. 235–55.

Fabbri, Francesca, Jonathan E. Haskel, and Matthew J. Slaughter. 2003. "Does Nationality of Ownership Matter for Labor Demands?" *Journal of European Economic Association* 1(2):698–707.

Gabel, Matthew. 1998. "Economic Integration and Mass Politics: Market Liberalization and Public Attitudes in the European Union." *American Journal of Political Science* 42(3):936–53.

Garrett, Geoffrey. 1998. *Partisan Politics in the Global Economy*. Cambridge: Cambridge University Press.

Globerman, Steven, John Ries, and Ilan Vertinsky. 1994. "The Economic Performance of Foreign Affiliates in Canada." *Canadian Journal of Economics* XXVII(1):143–56.

Gorg, Holger, and Eric Strobl. 2003. "Footloose Multinationals?" *The Manchester School* 71(1):1–18.

Gottschalk, Peter, and Robert Moffitt. 1994. "The Growth of Earnings Instability in the U.S. Labor Market." *Brookings Papers on Economic Activity 2*. Washington: Brookings Institution, pp. 217–54.

Green, Donald P., and David H. Yoon. 2002. "Reconciling Individual and Aggregate Evidence Concerning Partisan Stability: Applying Time-Series Models to Panel Survey Data." *Political Analysis* 10(1):1–24.

Griffith, Rachel. 1999. "Using the ARD Establishment Level Data to Look at Foreign Ownership and Productivity in the U.K.." *Economic Journal* 109(June):F416–F442.

Hays, Jude, Sean Ehrlich, and Clint Peinhardt. 2002. "Globalization, the Size of Government, and Labor Market Institutions: Maintaining Support for Openness among Workers." Presented at the annual meeting of the Midwest Political Science Association.

Helpman, Elhanan. 1984. "A Simple Theory of International Trade with Multinational Corporations." *Journal of Political Economy* 92(3):451–71.

Iversen, Torben, and Thomas Cusack. 2000. "The Causes of Welfare State Expansion." *World Politics* 52(April):313–49.

Journal of Labor Economics. 1999. Symposium Issue on "Changes in Job Stability and Job Security." October, Volume 17, Number 4, Part 2.

Mayda, Anna Marie, and Dani Rodrik. 2002. "Why Are Some People (and Countries) More Protectionist Than Others?" National Bureau of Economic Research Working Paper No. 8461.

Mughan, Anthony, and Dean Lacy. 2002. "Economic Performance, Job Insecurity, and Electoral Choice." *British Journal of Political Science* 32(3):513–33.

O'Rourke, Kevin, and Richard Sinnott. 2001. "The Determinants of Individual Trade Policy Preferences: International Survey Evidence." *Brookings Trade Forum: 2001*. Washington: Brookings, pp. 157–206.

Rodrik, Dani. 1998. "Why Do More Open Economies Have Bigger Governments?" *Journal of Political Economy* 106(5):997–1032.

Rodrik, Dani. 1997. *Has Globalization Gone Too Far?* Washington: Institute For International Economics.

Scheve, Kenneth F., and Matthew J. Slaughter. 2001a. "What Determines Individual Trade-Policy Preferences." *Journal of International Economics* 54(2):267–92.

Scheve, Kenneth F., and Matthew J. Slaughter. 2001b. "Labor-Market Competition and Individual Preferences Over Immigration Policy." *Review of Economics and Statistics* 83(1):133–45.

Scheve, Kenneth F., and Matthew J. Slaughter. 2004. "Public Opinion, International Integration, and the Welfare State." In *Globalization and Egalitarian Redistribution*, ed. Pranab Bardhan, Samuel Bowles, and Michael Wallerstein. Washington: Russell Sage Foundation. Forthcoming.

Schmidt, Stefanie R. 1999. "Long-Run Trends in Workers' Beliefs about Their Own Job Security: Evidence from the General Social Survey." *Journal of Labor Economics* 17(4):S127–S141.

Slaughter, Matthew J. 2001. "International Trade and Labor-Demand Elasticities." *Journal of International Economics* 54(1):27–56.

Te Velde, D.W., and O. Morrissey. 2001. "Foreign Ownership and Wages: Evidence from Five African Countries." CREDIT Discussion Paper.

United Nations Conference on Trade and Development. 2001. *World Investment Report: Promoting Linkages*. New York: United Nations.

Wawro, Gregory. 2002. "Estimating Dynamic Panel Data Models in Political Science." *Political Analysis* 10(1):25–48.