What Determines Individual Preferences Over Trade and Immigration Policy?

Matthew J. Slaughter*
Dartmouth College and NBER

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This paper uses three years of individual-level data to analyze the determinants of individual preferences over trade and immigration policies in the United States. Different economic models make contrasting predictions about what forces should shape these preferences. We have four main empirical results. First, we find that factor type dominates industry of employment in explaining support for trade barriers This result is consistent with a Heckscher-Ohlin model of the United States in which the country is well endowed with skilled labor relative to the rest of the world. Second, we find that home ownership also matters for individuals' trade-policy preferences. Independent of factor type, home ownership in counties with a manufacturing mix concentrated in comparative-disadvantage industries is strongly correlated with support for trade barriers. This finding suggests that in addition to current factor incomes driving preferences as in standard trade models, in reality preferences also depend on asset values. Third, less-skilled workers are significantly more likely to prefer limiting immigrant inflows into the United States. This result suggests that over time horizons relevant to individuals when evaluating immigration policy, individuals think that the U.S. economy absorbs immigrant inflows at least partly by changing wages. These preferences are consistent with a "multi-cone" Heckscher Ohlin trade model and with a factor-proportions-analysis labor model. Fourth, we find no evidence that less-skilled workers in high-immigration communities are especially anti-immigrationist. These preferences are inconsistent with an area-analysis labor model. The overall message of the paper is that less-skilled Americans are much more likely to oppose globalization.

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1 Introduction

In the United States today there appears to be rising opposition to policies aimed at further liberalization of trade and immigration. On the trade side, several recent political events suggest a marked turn away from liberalization: the current opposition to Chinese accession to the WTO; the Seattle protests in 1999; the failure by Congress to renew "fast-track" negotiating authority for the President in 1997; Pat Buchanan's strong electoral performance in 1996; the NAFTA (North American Free Trade Agreement) debate and close Congressional vote in 1993; and Ross Perot's strong electoral performance in 1992. Clearly, all these events involved issues beyond trade—but trade opposition played a role in all of them. As for immigration, border enforcement policies have toughened considerably in recent years; in 1994 California voters approved Proposition 187, which denied public services such as public education and health care to illegal immigrants; and since 1997 legislation has barred the use of federal money to cover Medicaid costs for poor legal immigrants during their first five years of residence. Taken together, all these actions suggest that many U.S. citizens oppose freer trade and immigration.

What accounts for these policy preferences? The goal of this manuscript is to offer some empirical answers to this question. The main data source is the 1992, 1994, and 1996 National Election Studies (NES) surveys, each of which is an extensive survey of current political opinions based on an individual-level stratified random sample of the U.S. population. The NES surveys contain direct measures of individual preferences over trade and immigration policy: responses to questions about new U.S. trade barriers and new U.S. immigration levels.

The surveys also report a wealth of respondent information including occupation, industry of employment, home-ownership status, and county of residence. Given this information, I built a data set with several plausible measures of "exposure" to freer trade and immigration across factor types, industries, asset ownership, and counties. Merging this information with the NES survey yields an individual-level data set identifying both stated policy preferences and potential trade/immigration exposure through several channels. One can then evaluate how these preferences vary with individual characteristics that economic theory predicts might matter.

It is well known from standard trade theory that trade's effect on the current income of factors of production depends crucially on the degree of intersectoral factor mobility, i.e., on the degree of factor specificity. In a Ricardo-Viner (RV) framework where some or all factors cannot move to other sectors, factor incomes tend to vary by industry of employment. In contrast, in a Heckscher-Ohlin (HO) framework where factors move costlessly across sectors, factor incomes tend to vary by factor type. But the empirical evidence distinguishing RV from HO predictions is limited and inconclusive. In their literature survey, Alt, Frieden, Gilligan, Rodrik, and Rogowski (1996, pp. 713) claim that "In many cases, predictions [of various trade-policy models] have yet to be fully tested ... In a sense, then, this survey is a call to the field to begin the work of testing the implications of these models." Further, and directly to the point of this paper, they observe that with respect to the basic and crucial empirical question (p. 693) "How specific are particular assets? ... the jury is still out."

As for immigration policy, the anticipated effect of immigration on wages is likely to play a key role, as current factor income is a major determinant of individual economic welfare. Because current factor income depends primarily on individual skill levels, there may be a significant link from skills to wages to immigration-policy preferences. Different economic models, however, make contrasting predictions about the nature of this link. In the HO model, immigrants sometimes have no impact on native wages. "Factor-proportions analysis," a framework often used by labor economists researching immigration, predicts that immigrants pressure the wages of similarly-skilled natives nationwide. "Area analysis," an alternative framework in the labor literature, predicts that immigrants pressure the wages of similarly-skilled natives who reside in gateway communities where immigrants settle. In short, there is theoretical uncertainty about the wages-mediated link between skills and immigration preferences.[1]

For both trade and immigration there may be economic considerations above and beyond current factor income. The standard frameworks do not focus on intertemporal consumption choices in which current factor income can be saved and invested for future periods. In reality people do save and invest in a wide range of assets. Accordingly, to understand the links between policy preferences and asset

[1]The terms "area analysis" and "factor-proportions analysis" we borrow from Borjas, et al (1996).

ownership the standard frameworks might need extending--particularly for assets that are neither currently employed factors nor currently produced goods.

The data analysis is organized around these basic trade and labor models, supplemented by a consideration of asset ownership. In no sense are these models "tested": they are used to suggest sensible econometric specifications for what economic forces might explain stated preferences. I analyze trade and immigration-policy preferences in turn, and for each generate two main results.

For trade, first I find that factor type dominates industry of employment in explaining support for trade barriers. Lower skill, measured by education or average occupational earnings, is strongly correlated with support for new trade barriers. In contrast, employment in industries more exposed to trade, measured by tariff rates or net exports, is not strongly correlated with support for new trade barriers. This result suggests there is relatively high intersectoral labor mobility in the United States over the time horizons relevant to individuals when evaluating trade policy, consistent with an HO model in which the United States is well endowed with skilled labor relative to the rest of the world.

The second main finding for trade is that in addition to factor incomes, home ownership also matters for individuals' trade-policy preferences. Assets like housing do not fit into standard trade theory because they are neither currently employed factors nor currently produced goods. Independent of factor type or industry of employment, home ownership in counties with a manufacturing mix concentrated in comparative-disadvantage industries is strongly correlated with support for trade barriers. This finding suggests that in addition to current factor incomes driving preferences as in standard models, in reality preferences also depend on asset values.

For immigration-policy preferences, the first main result is that less-skilled workers are significantly more likely to prefer limiting immigrant inflows into the United States. This result is robust to several different econometric specifications which account for determinants of policy preferences other than skills. This suggests that over time horizons relevant to individuals when evaluating immigration policy, individuals think the U.S. economy absorbs immigrant inflows at least partly by changing wages. These preferences are consistent with an HO trade model and with a factor-proportions-analysis labor model.

The second main result is there is no evidence that less-skilled workers in high-immigration communities are especially anti-immigrationist. These preferences are inconsistent with an area-analysis labor model.

These results are hopefully of interest in their own right. But they also matter for political-economy work. For trade, a complete political-economy model of trade policymaking must somehow characterize individuals' preferences over trade policy. Rodrik (1995, p. 1458) claims this is the first essential element of modeling: "In principle a political-economy model of trade policy must have four elements. First, it must contain a description of individual preferences over the domain of policy choices available to policymakers." The same logic applies to models of immigration policy: individual preferences are a key building block.

There are seven additional sections to this paper. Section 2 surveys the literature on trade-policy preferences and relates our work to this literature. Section 3 summarizes the trade theory underpinning our empirical work, Section 4 the immigration theory. Section 5 discusses the data and our model specifications. Section 6 presents the empirical results for trade-policy preferences, Section 7 for immigration-policy preferences. Section 8 briefly concludes.

2 Literature Survey: What Do We Know About Trade and Immigration-Policy Preferences?

2a The Political Economy of Trade Policy

In the political-economy literature, this paper is related to work focusing on actual political cleavages over trade policy and what those cleavages imply about the sector-specificity of assets.

Irwin (1994, 1996) and Magee (1978) find evidence consistent with trade-policy preferences being determined primarily by industry.[2] Using county-level data to regress county votes on measures of county factor and industry mix, Irwin (1996) finds that industry interests rather than factor interests best explain voting in the 1923 British general election, an election that hinged primarily on the issue of whether Britain should implement new trade barriers. In a similar paper, Irwin (1994) analyzes the 1906 British general election and finds the same basic result. Magee (1978) reports that of 21 industries testifying before the House Ways and Means Committee on the Trade Reform Act of 1973,

[2]In related work, Frieden (1991) examines how national political cleavages are shaped over international finance policy using a Ricardo-Viner framework similar to Irwin and Magee.

in 19 cases trade unions representing the interests of labor took the same position as management and industry trade associations representing the interests of capital. Magee also documents that neither capital nor labor lobbies according to a unanimously shared position across sectors.

In contrast, Beaulieu (1996), Balistreri (1997), Rogowski (1987, 1989), and Midford (1993) find support for factor types explaining trade-policy preferences. Using an individual-level survey of Canadian voters, Beaulieu (1996) finds that factor type rather than industry of employment best explains the 1988 Canadian federal election, an election widely regarded as a national referendum on the Canadian-U.S. Free-Trade Agreement (CAFTA). Balistreri (1997) concludes that the same data are consistent with the generalized Stolper-Samuelson theorem. Using data on Canada's relative endowments of occupations (these data do not contain direct measures of factor endowments, so he works with occupations), he finds that people employed in occupations abundant in Canada relative to the United States--thus the people likely to gain from freer trade--were more likely to favor the CAFTA. Rogowski (1987, 1989) explains several national political coalitions with respect to groups' exposure to international trade as predicted by the Stolper-Samuelson theorem. Midford (1993) expands Rogowski's framework from just three factors--capital, labor, and land--by disaggregating labor and land more finely and by introducing raw materials as well; this allows him to explain some additional real-world political coalitions.

2b The Political Economy of Immigration Policy

Previous research on the determinants of immigration policy in receiving countries has emphasized the variation in immigration politics across countries and over time (Joppke 1998, Kessler 1998, Perotti 1998, Money 1997, Freeman 1992 and 1995). There is general agreement that systematic differences in policies across countries depend on varying political institutions, divergent national histories of settlement and colonialism, and the different effects of a changing international context. Moreover, it seems clear that even within countries the character of immigration politics changes over time. For example, a country's interest groups can dominate the policymaking process during some periods while in other periods partisan electoral competition is central. In contrast to this observed variation across time and space, very little research has focused on the distribution of individual preferences over

immigration policy. Who supports free movement? Who advocates further restrictions? We contend that only once these questions about preferences have been answered adequately can a convincing account of cross-country and over-time variation in policymaking be constructed.

Accounts of individual preferences can usefully be divided into economic and non-economic determinants. Non-economic factors include individual beliefs about civil rights and expectations regarding the cultural impact of immigrants. The civil-rights dimension of immigration-policy preferences has both a non-discrimination aspect as well as a more straightforward free movement of persons element. Individual policy preferences are also likely to depend both on the degree to which individuals think immigrants change native culture and on the desirability of those changes.

Economic determinants are generally hypothesized to be a function of the aggregate costs and benefits of immigration, the fiscal impact on the public sector, and the impact of immigrants on native labor-market returns. This last consideration is arguably the most critical economic factor influencing individual policy preferences, and it is often the most controversial factor as well. Consequently, it is the main issue addressed in this paper.[3]

In previous work, Goldin (1994) and Timmer and Williamson (1998) present historical evidence on the potential impact of labor-market outcomes on immigration policy. Goldin (1994) finds that House Representatives in 1915 were more likely to vote in favor of a literacy test to restrict immigrant inflows the lower were wage increases from 1907 to 1915 in the Representatives' district cities. Goldin interprets this as indirect evidence that immigrants' pressure on native wages contributed to tighter immigration restrictions. Pooling five countries from 1860 to 1930, Timmer and Williamson (1998) find that more-restrictionist immigration policies were significantly correlated with lower unskilled wages relative to average per capita income. They interpret this correlation as evidence that countries with more unequal income distributions tended to restrict immigration to maintain the relative income of the less-skilled.[4]

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[3]Borjas (1995) concludes that U.S. immigration's main economic impact is on the distribution of income, not on its aggregate level. Borjas (1999) presents a comprehensive analysis of current U.S. immigration policy.

[4]Hanson and Spilimbergo (1998) analyze the impact of economic conditions in the United States and Mexico on a different aspect of immigration policy: border enforcement and apprehensions. They find that the Mexican (i.e., not U.S.) purchasing power of U.S. nominal wages is strongly correlated with border apprehensions of illegal Mexican immigrants.

In contrast to the policy focus of Goldin (1994) and Timmer and Williamson (1998), Citrin, et al (1997) use individual-level survey data to study the immigration-policy preferences of a cross-section of U.S. citizens. Controlling for a wide range of factors that potentially shape preferences, they conclude "that personal economic circumstances play little role in opinion formation" (p. 858). Specifically, they find that labor-market competition does not influence preferences. Using information from a national poll, Espenshade and Hempstead (1996) find some mixed evidence that less-educated and lower-family-income individuals are more likely to support immigration restrictions. They interpret this evidence as suggesting that people care about immigration's labor-market impacts on wages, employment, and work conditions.

2c This Work in Relation To the Literature

All these studies provide valuable information on policy preferences and political behavior. However, our work improves upon them in at least three important ways.

First, and perhaps most importantly, our study uses a direct measure of individual trade-policy preferences. In contrast, most earlier studies infer from some observed political actions--coalition formation, lobbying, or voting--something about policy preferences. These indirect preference measures face the important limitation of being endogenous outcomes of the interaction between policy (and possibly other) preferences and domestic political institutions. Policy preferences and institutions together determine policy actions, so the mapping from preferences to actions is not unambiguous. Mayer (1984) is a classic reference on this point. He endogenizes tariffs as the outcome of economic preferences channeled into some domestic voting structure, and in this framework different voter eligibility rules and participation costs lead to different tariff equilibria from the same preferences.

Rodrik (1995 p. 1459) makes this distinction between preferences and actions when commenting that a complete political-economy model of trade policy "must contain a description of how these individual [trade-policy] preferences are aggregated and channeled, through pressure groups, political parties, or grass-roots movements, into 'political demands' for a particular policy or another." Similarly Alt, *et al*, (1996, p. 707) highlight the problem with reference to Magee's analysis of Congressional testimony: "the difficulty in taking this approach beyond a qualitative comparison of patterns into

measuring the extent of participation across industries is that one is measuring the dependent variable rather than the independent variable to some extent. That is, this testimony is the behavior that a measure of specificity should help you predict; therefore, if one is trying to explain policy outcomes, then there is a big risk of rendering one's conclusions circular."

Second, our study draws heavily on the trade and labor-economics literature on immigration to test properly for the economic determinants of policy preferences. One issue here is we consider trade-policy preferences not only in terms of individual income but more broadly in terms of asset ownership as well. Standard trade theory usually assumes that individual welfare depends only on individual factor income. As will be discussed below, we extend this framework to analyze the empirical relevance of assets as well--particularly assets like housing which are neither currently employed factors nor currently produced goods.[5] For immigration, we test three alternative models of how immigration affects the economic welfare of natives. In contrast, none of the related studies explicitly lays out any models of immigration. Instead, they all simply assume that immigration hurts natives via lower wages, unemployment, and other adverse outcomes. Many important issues have not been explored, such as whether immigration preferences are systematically different in gateway communities.

Third, our study uses measures of individual economic exposure to trade and immigration that follow closely from economic theory. For trade, we have sufficient data to examine the alternative predictions of both the RV and the HO models. Balistreri (1997), Rogowski (1987, 1989), and Midford (1993) all test whether the data are consistent with an HO story, but they do not benchmark this against the performance of an RV story. Our explanatory variables for factor types, industry of employment, and country of residence are directly observed measures in our data. In contrast, many of the papers cited above do not have direct measures on factor type and/or industry. Beaulieu (1996) and Irwin (1994, 1996) infer both factor type and industry from reported data on occupation alone. And unlike Beaulieu (1996), Balistreri (1997) infers only factor type from the same data. These authors make their data inferences carefully, but the inferences almost surely create substantial measurement error which is not

[5] In our analysis we assume that individuals know with certainty the effects of trade policies on individual incomes and asset holdings. This is a common but not universal assumption in the literature. Fernandez and Rodrik (1991) model trade-policy formation in an environment where individuals do not know *ex ante* how trade-policy changes affect their welfare.

systematically accounted for. We measure industry exposure to freer trade with two plausible measures of comparative advantage by industry: tariff rates and net exports. In contrast, Beaulieu (1996) and Irwin (1994, 1996) measure industries just as dummy variables. Similarly, we measure skills with two continuous variables, years of education and national average occupational earnings, both of which are standard measures in labor economics.[6] Beaulieu (1996) uses two dummy variables and Irwin (1996) uses five class categories.

For our immigration analysis, this measurement issue applies most strongly to Citrin, et al (1997) and Espenshade and Hempstead (1996). Citrin, et al interpret educational attainment as a "demographic variable" rather than an "economic factor." Although this choice has some justification in previous studies on the relationship between education and tolerance, we will demonstrate that education measures labor-market skills once other considerations such as gender and political ideology are controlled for. Citrin, et al measure skills with income and with eight dichotomous occupation variables. Only four of the eight cover working individuals, and these -- "white collar," "pink collar," "low threat blue collar," and "high threat blue collar" -- are never defined or justified with reference to economic theory or evidence. Espenshade and Hempstead use dichotomous variables for educational attainment and family -- not individual -- income, with all specifications using both types of variables.

Overall, this current work aims to contribute to the existing literature by working with direct measures of policy preferences, and by analyzing these preferences with a close link to economic theory and data issues.

3 Economic Models of Trade-Policy Preferences

3.1 Trade's Effect on Factor Incomes

In the literature on the political economy of trade policy, it is commonly assumed that individuals evaluate trade policy based on how their current factor incomes are affected without regard for

[6]For example, in the recent research on the rising U.S. skill premium the two most commonly used measures of the skill premium have been the relative wage between college graduates and high-school graduates and the relative wage between non-production workers and production workers (in manufacturing only). See Katz and Murphy (1992) or Lawrence and Slaughter (1993), for example. Berman, et al (1994) document for the United States that employment trends for this job-classification measure track quite closely employment trends measured by the white-collar/blue-collar job classification--which in turn closely reflects the college/high-school classification.

aggregate national welfare. In this paper we follow the general spirit of this convention by assuming that individuals' policy preferences are determined by how policy affects their personal welfare.

The RV and HO models are the two most commonly used models for characterizing trade-policy preferences. In both models, changes in trade policy affect factor incomes by changing the country's relative product prices. The key difference between the two models is their different assumptions about intersectoral factor mobility. Different degrees of factor mobility imply different factor-income changes from--and thus preferences about--trade liberalization.

We use the basic predictions of these two models to organize our data analysis of preferences. In doing this, however, we emphasize two points. First, our analysis does not offer a direct test between the two models. A direct test would require data such as intersectoral factor movements and factor prices. The second point is that preferences may be consistent with both models, not just one. The RV model can be characterized as a short-run version of the more long-run HO model. For example, Mayer (1974) and Mussa (1974) compare wage changes in the two models, and Mussa (1978) formalizes how with intersectoral mobility costs an RV short-run gradually becomes an HO long-run. So each model might be relevant over different time horizons. If individuals evaluate both short-run and long-run effects of trade liberalization, then trade-policy preferences might be explained by both factor type and industry of employment.[7]

The HO model assumes that factors can move costlessly across sectors. This implies that economy-wide, each factor earns the same return in all sectors. Trade liberalization which changes relative product prices changes relative (and possibly real) factor prices according to the Stolper-Samuelson theorem: returns tend to rise (fall) for the factors employed relatively intensively in the sectors whose relative product price rises (falls). In this model it is usually assumed that protection is received by the sectors which employ relatively intensively the factors with which the country is poorly endowed relative to the rest of the world, because in opening from autarky to free trade these factors

[7]Another way both models might accurately describe the economy is that within some time frame specificity might vary across units in the economy (such as industries or factor types). Thus within the same time frame both the HO and RV models might apply, each to different parts of the economy. Alt, Carlsen, Heum, and Johansen (1998) find support for this perspective in their study of firm lobbying behavior in Norway.

suffer income declines. In contrast, the factors with which the country is relatively well endowed relative to the rest of the world enjoy income gains in opening from autarky to free trade. Thus a country's abundant factors support freer trade while its scarce factors oppose it--regardless of the sector of employment for any of these factors.

At the opposite extreme from the HO model, the RV model assumes that some or even all factors cannot move across sectors. This immobility is usually assumed to be caused by some transaction costs to moving. For example, industry-specific human capital gained through on-the-job experience can make workers reluctant to switch sectors. In this model immobile--i.e., specific--factors need not earn the same return in all sectors. Instead, the income of specific factors is linked much more to their sector of employment. In particular, trade-liberalization-induced changes in relative product prices redistribute income across sectors rather than factors. Sectors whose product prices fall--presumably comparative-disadvantage sectors--realize income losses for their specific factors while sectors whose product prices rise--presumably comparative-advantage sectors--realize income gains for their specific factors. As a result, trade-policy preferences are determined by sector of employment. Factors employed in sectors with product prices elevated by trade protection oppose trade liberalization while factors employed in sectors with rising product prices support it.[8]

To summarize, in HO models factors evaluate trade policy based on their factor type while in RV models factors evaluate trade policy based on their industry of employment. What do these two models predict about trade-policy preferences in the United States? Many studies (e.g., Leamer (1984)) have documented that the United States is well (poorly) endowed with more-skilled (less-skilled) labor relative to the rest of the world. According to the HO model, then, in the United States more-skilled workers should support freer trade while less-skilled workers should oppose it. In contrast, the RV model predicts that U.S. workers employed in comparative-advantage sectors should support freer trade while those employed in comparative-disadvantage sectors should oppose it.

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[8]If some factors remain mobile across sectors in a Ricardo-Viner model, their factor prices are not so clearly linked to product-price changes. Changes in real factor prices for these mobile factors are ambiguous: the direction of change depends on the consumption basket of these mobile factors. In the above discussion we focus only on the specific factors.

An important caveat to the theory just summarized is nontraded industries, which are relevant for our empirical work because in 1992 the large majority of U.S. jobs were in the nontraded sector. By definition, trade barriers cannot be granted for these industries. Does this matter for the predictions of the HO and RV models? The HO reasoning still applies if some sectors are nontraded provided there remains costless interindustry factor mobility among all sectors. For the RV model, however, we need to clarify how trade policies affect the product prices of nontraded sectors. Because freer trade tends to raise the level of national income, if we assume positive income elasticities of demand for nontraded goods then freer trade should raise nontraded prices by raising demand for nontraded goods. Thus we predict that in an RV model workers in nontraded sectors should support freer trade. However, because trade policy's effect on nontraded prices works indirectly through nontraded demand, it might be the case that nontraded workers support freer trade less strongly than do comparative-advantage-sector workers. We return to this issue when discussing our industry trade-exposure measures.

3.2 Trade's Effect on Asset Values

In standard trade models individuals spend all current factor income on consumption. In reality people can have current factor income differ from current consumption by accumulating or decumulating assets. To understand the links between trade-policy preferences and asset ownership, this section discusses how trade affects asset values.

Many kinds of assets fit easily into standard trade models. Some assets are currently employed by firms as factors of production; for example, machine tools and office buildings. These assets earn rates of return determined by the economy's set of zero-profit conditions and, if factor-price equalization (FPE) does not hold, the economy's endowments as well. In turn, these rates of return are an important determinant of asset prices. There is a well-developed literature analyzing how these productive assets accumulate over time within open trading economies (see, for example, the surveys of Findlay (1984) and Smith (1984)).

Another kind of asset which fits into standard trade models is currently produced goods such as automobiles. The price of these assets is usually well determined in trade models. The price of traded

products depends on some combination of foreign tastes, technology, and endowments; political and natural barriers to trade; and, if the country is "large," domestic tastes, technology, and endowments. The price of nontraded products depends on factor prices and technology for nontraded production. Under FPE nontraded prices are linked to traded prices through factor prices (see Helpman and Krugman (1985)).

Some assets, however, are neither currently employed factors nor currently produced goods. Residential housing is a major example. Firms do not employ houses as factors of production. And, to a first approximation, firms do not currently produce houses either. At each point in time nearly all of a region's housing stock is not produced: it is the (appropriately depreciated) accumulation of all previous housing construction in earlier periods. Housing construction adds a negligible amount to the current housing stock both because of the time lags involved in construction and because the construction flow is very small relative to the housing stock.[9]

Some assets, then, are not clearly linked to the production side of standard trade models. In the set of national zero-profit conditions these assets appear neither on the factor-cost side nor on the product-price side. Within this class of assets we focus on housing because it is the only asset of this kind reported in our data. Despite this practical limitation, it is important to note that housing constitutes a very large share of people's total wealth holdings. Caplin, Chan, Freeman, and Tracy (1997) report that for the average U.S. household in 1990, the gross value of the primary residence accounted for nearly 90% of total household assets. Similarly, Skinner (1994) reports that among all U.S. homeowners in 1986, for the median homeowner in all age groups housing equity accounted for more than half of his/her total wealth.[10] Even though our data cover only housing, this is the single biggest asset for a significant share of the population.

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[9]The Census Bureau (1997) estimates that on July 1, 1996 the total U.S. housing stock was 110 million housing units. For all of 1996 approximately 1.3 million new homes were constructed nationwide. Based on averages from the 1980s, approximately 0.3 million existing homes became uninhabitable that year due to demolition, disasters, and other causes. Thus the net construction rate in 1996 was about 1 million new homes--0.9% of the existing stock. Also, the Census Bureau estimates that nationwide in 1996, an average of 8.33 months passed from the time a residential-construction permit was issued to the time construction was completed.

[10]Skinner reports median housing equity-to-net worth ratios for six different age groups within the population. These ratios range from 54.5% to 61.3%.

To understand how trade policy affects housing prices we use a simple supply-and-demand framework like that presented in Caplin, *et al* (1997). Each country has many distinct regional housing markets, each of which faces a perfectly inelastic supply schedule at each point in time. Given some inelastic regional supply of housing, prices are determined entirely by regional housing demand. Demand depends on considerations such as tax differentials between renting and owning. As discussed by Caplin, *et al* (1997), it also depends on the level of economic activity in a region. Greater economic activity means more employment, more housing demand.

Trade policy affects the level of regional economic activity. Freer trade tends to shrink some industries and expand others as predicted by the theory of comparative advantage. This is true whether the underlying factor markets are HO or RV. In either case, regions with a higher concentration of activity in sectors with a comparative disadvantage are more vulnerable to adverse housing-demand shocks from freer trade. As regional economic output declines people will be leaving the local labor force either for work elsewhere or, at least temporarily, unemployment. This reduces housing demand and thus housing prices.[11]

The recent literature on geography and trade (e.g., Krugman (1991)) provides an additional reason beyond comparative advantage why trade liberalization might shrink some sectors. In many geography models, regional production patterns relying on various scale economies can depend on barriers to trade. These barriers are usually modeled as natural barriers such as transportation costs, but in principle political barriers should work equivalently. As these barriers fall the interregional pattern of production can change very dramatically if the original equilibrium is unstable. Regions can suffer the loss of entire industries--a much more drastic outcome than just having industries contract according to comparative advantage. If these kind of drastic equilibrium changes are actually possible, homeowners have all the more reason to worry about freer trade reducing housing values.[12]

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[11]The two models do differ in how the changing industrial mix affects factor returns. Again, in the HO model factor returns adjust based on factor type. In the RV model they adjust based on industry of employment. In both models, however, the changing industrial mix affects local employment levels the same way according to the theory of comparative advantage.

[12]Hanson (1997) documents evidence of trade liberalization reallocating economic activity across regions within a country. He finds that after trade liberalization in Mexico during the 1980s, industries located along the U.S.-Mexico border expanded relative to industry around Mexico City.

To summarize: in regions with a greater concentration of activity in comparative-disadvantage sectors, homeowners should oppose freer trade because its contractionary effects in the region tend to reduce homeowners' welfare by lowering housing demand and thus housing values.[13] We hypothesize that this link between trade and asset values operates independently of trade's effect on labor incomes. People's economic welfare depends on both current income and current asset holdings, and freer trade might affect these two channels differently. Consider a more-skilled homeowner in Gary, Indiana, a city with production very concentrated in a comparative-disadvantage sector, steel. Through the income channel this person supports freer trade in an HO model. But through the asset channel this person opposes freer trade. We aim to separate these two channels empirically.

4 Economic Models of Immigration-Policy Preferences

To make the connection between individual economic interests and immigration-policy preferences we focus on how immigration affects individual factor incomes. Different economic models make contrasting predictions about the nature of the link from immigration to factor incomes to policy preferences. This section briefly summarizes three models: the Heckscher-Ohlin trade model, the factor-proportions-analysis model, and the area-analysis model.

Across all three models we make two important assumptions. First, we assume that current factor income is a major determinant of people's economic well-being. Second, we assume that U.S. citizens think that current immigrant inflows increase the relative supply of less-skilled workers. As will be seen below, this assumption about the skill-mix-effects of immigrants is not explicitly stated in the NES question about immigration preferences. But this assumption clearly reflects the facts about U.S. immigration in recent decades. Borjas, et al (1997, p. 6) report that "on average, immigrants have fewer years of schooling than natives—a difference that has grown over the past two decades, as the mean years of schooling of the immigration population increased less rapidly than the mean years of schooling of natives. As a result, the immigrant contribution to the supply of skills has become

[13]Research in the regional-economics literature has documented an empirical link between local industry mix and local housing prices. For the Boston area during the 1980s, Case and Mayer (1996) find that average house prices rose less in housing jurisdictions with a larger share of residents employed in manufacturing in 1980. Case and Mayer hypothesize that this empirical link reflects "displaced manufacturing workers ... reducing their demand for housing" (p. 391).

increasingly concentrated in the lower educational categories." We assume that NES respondents are aware of these facts.[14]

Given these two assumptions, we think that the economic determinants of an individual's immigration-policy preferences depend on how an immigration-induced shift in the U.S. relative endowment towards less-skilled workers affects that individual's factor income. To maintain focus on equilibrium wage determination, in all models we assume that wages are sufficiently flexible to ensure full employment. This allows us to abstract from unemployment, both equilibrium and frictional, though unemployment will be considered in our empirical work. To maintain focus on different skill groups, in all models we assume just two factors of production, skilled labor and unskilled labor. This keeps our analysis as simple as possible.[15]

4.1 The Heckscher-Ohlin Model

The HO trade model usually makes two key assumptions. First, there is one national labor market for each factor. Thanks to sufficient mobility of natives (and immigrants upon arrival), there are no geographically segmented "local" labor markets. The second key assumption is there are more tradable products (i.e., sectors) than primary factors of production, with products differentiated by their factor intensities. Multiple products are essential for establishing many fundamental trade-theory results, such as comparative advantage.

With these assumptions, in equilibrium a country chooses (via the decentralized optimization of firms) the "output mix" that maximizes national income subject to the constraints of world product prices, national factor supplies, and national technology. This output mix consists of both which products actually get produced and the quantities of production. In turn, this output mix helps determine the country's national factor prices. The general intuition is that the technology parameters and world price for each produced sector help determine national wages. In the standard case where the country makes

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[14] This skills gap between immigrants and natives does not address other interesting facts about the distribution of skills among immigrants. For example, Borjas, et al (1997. p. 7) show that the skill distribution of U.S. immigration has been somewhat bimodal at both the high-skill and low-skill ends of the distribution.

[15] In the political economy literature, some researchers analyze the theory of economic determinants of immigration-policy preferences. Benhabib (1996) considers a one-good model in which natives have different endowments of capital. Kessler (1998) focuses on how trade and immigration affect native factor returns in standard trade models. Bilal, et al (1998) consider the case of a three-factor, two-household, two-country world.

at least as many products as the number of primary factors, equilibrium wages are a function of just the world prices and technology parameters of the produced sectors. These wages do not depend on the prices and technology of the non-produced sectors. They also do not depend directly on the level of endowments (only indirectly through the endowments' role in selecting the product mix).

Immigration's wage effects depend on the initial product mix, on the size of the immigration shock, and on whether the country is large or small (i.e., on whether its product mix does or does not have any influence on world product prices). Consider the standard case where the initial output mix is sufficiently diversified that wages depend on just world prices and technology.

In this case, with "sufficiently small" shocks the country absorbs immigrants by changing its output mix as predicted by the Rybczynski Theorem: the same products are produced, but output tends to increase (decrease) in the unskill-intensive (skill-intensive) sectors. Whether wages change depends on whether the country is big or small. If the country is small, world prices do not change and thus there are no wage effects. This insensitivity of national wages to changes in national factor supplies Leamer and Levinsohn (1995) call the Factor-Price-Insensitivity (FPI) Theorem. If the country is large, wages do change: the relative price of unskill-intensive products declines, which tends to lower (raise) wages for unskilled (skilled) workers.

With "sufficiently large" immigration shocks, national wages do change. Large enough shocks induce the country to make a different set of products, which entails a different set of world prices and technology parameters and thus different wages. This absorption of large shocks via changes in both output mix and wages holds whether the country is big or small: in either case wage inequality rises. In the literature on U.S. immigration, Hanson and Slaughter (1999) find immigration-related output-mix changes among U.S. states.

Figure 1 displays the national labor market for the case of a small HO country with three products. The distinguishing feature is the shape of relative labor demand. It has two perfectly elastic portions, each of which corresponds to a range of endowments for which FPI holds. The national output mix varies along the demand schedule. A different set of two products is made on each elastic part; accordingly, different relative wages prevail on each elastic part. On the downward-sloping portions the

country makes only one product. Along these portions output-mix changes are not possible, so immigrants must price themselves into employment by changing wages. Point E_0 designates the initial labor-market equilibrium, with relative labor supply RS_0 and relative wages $(w_S/w_U)_0$. Two immigration shocks are shown. The "sufficiently small" immigration shock shifts RS_0 to RS'. Relative wages do not change, as immigrants trigger Rybczynski output-mix effects with no product-price changes. The "sufficiently large" shock shifts RS_0 to RS". The country now produces a new set of products. As a result the unskilled wage falls relative to the skilled wage (to (w_S/w_U)"), and with fixed product prices this relative-wage decline will be a real-wage decline as well.[16]

The HO model has different predictions about link between skills and immigration-policy preferences. If individuals think FPI holds then there should be no link from skills to preferences. In this case people evaluate immigration based on other considerations. If individuals think that immigration triggers both output-mix and wage effects then unskilled (skilled) workers nationwide should prefer policies which lower (raise) immigration inflows.

4.2 The Factor-Proportions-Analysis Model

Like the HO model, this model also assumes a national labor market. The fundamental difference between the two is this model assumes a single aggregate output sector. Under this assumption there can be no output-mix changes to help absorb immigrants. Accordingly, any immigration inflow affects national wages by the same logic described above. Lower relative wages for unskilled workers induces firms to hire relatively more of these workers. The greater the immigrant inflow, the greater the resultant wage changes. In the labor literature, studies using this framework include Borjas, et al (1996, 1997). These studies calculate immigration-induced shifts in national factor proportions and then infer the resulting national wage changes.

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[16] Three comments on Figure 1. First, the relative-supply schedule is vertical under the assumption that all workers are sufficiently willing to work that they price themselves into employment regardless of the going relative wage. Second, along the national demand schedule the country's output mix progresses according to sector factor intensities. The likely output mixes are as follows. Along the leftmost branch of RD the country makes only the most unskill-intensive product. Along the first flat it makes this product and the "middle" intensity product, switching to only the middle product along the middle downward-sloping branch. The country picks up the most skill-intensive product as well along the second flat; finally, along the rightmost branch it makes only the skill-intensive product. Third, underlying the downward-sloping portions of RD is the assumption of flexible production technologies with factor substitutability. With Leontief technology these portions would be vertical.

Figure 2 displays the national labor market for the factor-proportions-analysis world. Here the relative-labor-demand schedule slopes downward everywhere, with no portions where FPI holds. Initial relative labor supply is again given by the schedule RS_O, with initial equilibrium again at E_O and $(w_S/w_U)_O$. Immigration shifts the supply schedule back to RS', and the national skill premium rises to $(w_S/w_U)'$. Again, for fixed product prices real wages change, too.

This model makes a single prediction about the link from skills to immigration-policy preferences: unskilled (skilled) workers nationwide should prefer policies to lower (raise) immigration inflows. This prediction can also come from the HO model without FPI. Accordingly, evidence of a link between skills and preferences is consistent with both models.

4.3 The Area-Analysis Model

Like the previous model, the area-analysis model also assumes a single output sector. The fundamental difference between the two is this model assumes distinct, geographically segmented labor markets within a country. This assumption is likely untrue in the very long run, but it may be true over shorter time horizons thanks to frictions such as information and transportation costs that people (both natives and immigrants upon arrival) must incur to move. U.S. "local" labor markets are usually defined by states or metropolitan areas. Each local market has its own equilibrium wages determined by local supply and local demand.

If there is literally no mobility among local labor markets, immigrants' wage effects are concentrated entirely in the "gateway" communities where they arrive: immigration lowers (raises) wages for the unskilled (skilled). In contrast, in a national labor market immigrants' wage pressures spread beyond gateway communities. Natives can leave gateway communities when immigrants arrive; immigrants can move on to other communities; or natives can choose not to enter gateway communities as planned pre-immigration. In cases between these two extremes, immigrants affect wages everywhere but to a greater extent in gateway labor markets. The area-studies framework has guided many empirical studies of immigration. Studies such as Card (1990), Altonji and Card (1991), LaLonde and Topel (1991), and Goldin (1994) have tested for correlations between immigrant flows into local labor markets and local native wages.

Graphically, the area-analysis model also looks like Figure 2 -- but with the key difference that now this figure represents local, not national, conditions. Here, immigration shifts only the local relative supply of labor and thus depresses only local unskilled wages. Given this, the area-analysis model predicts the following: unskilled (skilled) workers in gateway communities should prefer policies to lower (raise) immigration inflows. What about workers in non-gateway communities? With no geographic labor mobility over time horizons relevant to individuals when evaluating immigration policy, there should be no correlation between these workers' skills and their preferences. More generally, with some labor mobility workers in non-gateway communities should have qualitatively similar preferences but the skills-preferences link should be stronger among gateway workers. Less-skilled (more-skilled) workers in gateway communities should have stronger preferences for more-restrictionist (less-restrictionist) immigration policies than less-skilled (more-skilled) workers in non-gateway communities.

5 Data Description and Empirical Specifications

5.1 Data for Trade-Policy Preferences

To better understand the theoretical determinants of individual trade-policy preferences we want measures of policy preferences and trade exposure, consistent with the hypotheses outlined above, all at the level of the individual. Accordingly, we combine individual-level data from the 1992 NES survey (1993) with data on average wages, tariffs, trade flows, and county manufacturing activity. These various data were obtained from the Bureau of Labor Statistics (BLS) (1992), the U.S. International Trade Commission (ITC) (1997), Rob Feenstra through the National Bureau of Economic Research (NBER) (1996), the U.S. Bureau of Economic Analysis (BEA) (various years), and the U.S. Census Bureau (1992). Using these data we analyze how our various measures of trade exposure affect individual trade-policy preferences.

We measure preferences by responses to the following question from the 1992 NES survey.

> "Some people have suggested placing new limits on foreign imports in order to protect American jobs. Others say that such limits would raise consumer prices and hurt American exports. Do you favor or oppose placing new limits on imports, or haven't you thought much about this?"

By coding responses 1 for those individuals favoring protection and 0 for those opposing it we constructed the variable *Trade Opinion*. This question requires respondents to reveal their general position on the proper direction for U.S. trade policy. Our theoretical framework hypothesizes that trade policy can affect both individuals' factor income, either based on skill levels or on industry of employment, and the value of their housing assets. To apply our framework to this question, we assume that respondents think that import limits will be placed on comparative-disadvantage sectors. This assumption allows us to construct measures of factor and industry trade exposure which follow closely from the theory. The assumption seems reasonable relative to alternatives such as import limits on comparative-advantage sectors.

One of this question's strengths is that it does not refer to a specific country or a particular trade agreement. Consequently, a respondent's answer should reflect trade-policy preferences rather than preferences on other issues such as human-rights violations in China, competition regulation in Japan, or migration controls in Mexico.[17]

To test whether skill levels are a key determinant of trade-policy preferences, for each individual in the 1992 NES survey we construct two measures of skill. First, respondents were asked to report their occupations coded according to the three-digit 1980 Census Occupation Code classification. We obtained BLS data reporting the 1992 U.S. average weekly wage for each three-digit occupation. Under the assumption that the average market returns for a given occupation are determined primarily by the skills required for that occupation, these average wages, called *Occupational Wage*, measure respondents' skill levels. As a second skill measure, the NES survey also records the years of education completed by each respondent, *Education Years*. Educational attainment is another commonly used measure of skills, so we use the education data as an alternative skills variable. For both measures, according to the HO model U.S. less-skilled workers are more likely to benefit from trade restrictions on comparative-disadvantage sectors and thus are more likely to support new trade barriers.

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[17] In contrast, in Balistreri's (1997) data Canadians were asked specifically about the CAFTA. Respondents may have considered not just freer trade *per se* but other issues related to U.S.-Canadian relations as well.

To test the hypothesis that sector of employment is a key determinant of trade-policy preferences, for each individual in the NES survey we construct two measures of industry trade exposure. Each measure is based on respondents' reported industry of employment coded according to the three-digit 1980 Census Industry Code classification.

Our first industry trade-exposure measure, *Sector Net Export Share*, is the industry's 1992 net exports as a share of output. This variable follows the common assumption that an industry's comparative advantage is "revealed" by its net exports: industries with positive (negative) net exports are assumed to be comparative-advantage (disadvantage) industries and thus likely to realize income gains (losses) for their employed factors from trade liberalization. To construct this variable we obtained through the NBER Feenstra's data on 1992 manufacturing exports, imports, and value of shipments at the four-digit SIC (revision two) level. To cover all truly tradable sectors, we obtained similar data for agriculture and tradable services from various BEA sources. All these data were concorded to the 1980 CIC industries, and then for each industry we calculated *Sector Net Export Share* as exports minus imports divided by value of shipments. For all nontradable sectors we set this variable equal to zero.

For this industry measure, according to the Ricardo-Viner model individuals employed in industries with greater revealed comparative disadvantages (i.e., more negative *Sector Net Export Shares*) are more likely support trade protection for these industries.[18] Notice that this measure matches the continuum across sectors of support for trade barriers discussed in Section 3.1: strong opposition for comparative-advantage sectors, possibly weaker opposition for nontraded sectors, and strong support for comparative-disadvantage sectors.

The second measure is of the industry's 1992 U.S. tariff rate. To construct this variable, *Sector Tariff*, from the ITC we obtained data on 1992 tariff duties collected and customs-value imports at the four-digit Standard Industrial Classification (SIC) (revision three) level.[19] We concorded these tariff

[18] We do not consider any causal link between our two industry trade-exposure measures—for example, the issue explored in Trefler (1993) that greater import penetration triggers calls for protection in an industry. Such links might be interesting for future work but are beyond the direct scope of this paper.

[19] We thank Michael Ferrantino at the ITC for helping us obtain these data.

and import values to the 1980 CIC industries, and then for each industry we calculated an effective tariff rate by dividing tariffs value by imports value. These tariff data cover all tradable industries in agriculture and manufacturing. For all tradable service industries and all nontradable sectors we set the tariff rate equal to zero.

To test this industry measure we assume that industries with higher current protection have more of a comparative-disadvantage. Given this, according to the Ricardo-Viner model individuals employed in industries with higher tariffs are more likely to support trade barriers for the comparative-disadvantage sectors. Notice that by assigning the same value to both zero-tariff tradables and nontradables, this measure restricts workers in the two groups to have the same trade-policy preference on this margin. This is a limitation of our second industry measure given our theory discussion earlier.

Finally, to test the hypothesis that housing values are a key determinant of trade-policy preferences, for each individual in the NES survey we construct two measures of how exposed homeowners are to trade liberalization reducing local economic activity. To construct these measures we exploit two dimensions of the NES survey.

One is that the NES reports whether each respondent or his/her family owns his/her home of residence. From this information we create the dummy variable *House* coded 1 to indicate ownership and 0 otherwise. The other survey dimension we use is that each respondent reports his/her county of residence. We used the 1992 Census of Manufactures (COM) from the Census Bureau to construct two measures of county-level trade exposure. Both measures are based upon the COM's disaggregation of county employment and other economic-activity variables into the twenty two-digit SIC manufacturing industries.[20] First, using the ITC tariff data described above, we identified the ten two-digit SIC manufacturing industries with above-median tariff rates in 1992.[21] We then calculated *County Exposure 1*, the share of county employment accounted for by these high-tariff industries. Second, using the Feenstra data described above, we identified the net-import industries in 1992 (14

[20]Unfortunately, comparably disaggregated county-level data for non-manufacturing industries are not readily available. We thank Clark Bensen at Polidata, Inc., for providing us with the COM data.

[21]These ten high-tariff industries were 21, 22, 23, 28, 30, 31, 32, 34, 38, and 39.

total).[22] We then calculated *County Exposure 2*, the share of county employment accounted for by these net-import industries. These two variables measure each county's comparative-disadvantage employment, where the pattern of comparative advantage is identified either through tariff rates or net trade flows.

We create our measure of homeowners' exposure to international trade by interacting the county-exposure measures with *House* to construct two interaction variables, *County Exposure 1 * House* and *County Exposure 2 * House*. According to the theory presented earlier, homeowners living in counties with a larger share of employment in comparative-disadvantage sectors are more likely to oppose trade liberalization because regional housing values depend, among other things, on the amount of regional economic employment in trade-exposed sectors. Notice that it is not living in a trade-exposed county *per se* that matters for this asset channel. Only residents who own homes care about industry mix and its effect on housing values.

5.2 Data for Immigration-Policy Preferences

We measure immigration-policy preferences by responses to the following question asked in the 1992, 1994, and 1996 NES surveys.

> "Do you think the number of immigrants from foreign countries who are permitted to come to the United States to live should be increased a little, increased a lot, decreased a little, decreased a lot, or left the same as it is now?"

This question requires respondents to reveal their general position on the proper direction for U.S. immigration policy. To apply our theory framework to this question, we assume that respondents think that U.S. immigrant inflows increase the relative supply of less-skilled workers. As we discussed, this assumption clearly reflects the facts about U.S. immigration in recent decades. Later, in our data analysis we revisit this assumption. We constructed the variable *Immigration Opinion* by coding responses 5 for those individuals responding "decreased a lot" down to 1 for those responding

[22] There were 14 net-import industries: 22, 23, 24, 25, 26, 29, 30, 31, 32, 33, 34, 36, 37, and 39.

"increased a lot." Thus, higher levels of *Immigration Opinion* indicate preferences for more-restrictive policy.[23]

Our theoretical framework hypothesizes that immigration policy can affect individuals' factor income according to their skill levels. To test whether skills are a key determinant of immigration-policy preferences, for each individual we again use *Education Years* and *Occupation Wage*, as described above (where *Occupation Wage* now varies by year). As discussed earlier, Citrin, et al (1997) interpret educational attainment as a demographic variable rather than a skills variable. Below we present strong evidence that education measures labor-market skills once other considerations such as gender and political ideology are controlled for. Also, our mapping of occupation categories into average occupation wages captures skills across occupations much more accurately than do poorly defined occupation categorical variables.

In addition to skill measures, we need measures of where respondents live combined with information about gateway communities. For each respondent the NES reports the county, state, and (where appropriate) metropolitan statistical area (MSA) of residence. We combine this information with immigration data to construct several alternative measures of residence in a high-immigration area. First, we defined local labor markets two ways: by a combination of MSAs and counties, and by states. In our MSA/county definition each MSA (with all its constituent cities and counties) is a separate labor market; for individuals living outside an MSA the labor market is the county of residence. Following the extensive use of MSAs in area-analysis studies and Bartel's (1989) finding that immigrants arrive mostly into cities, we prefer the MSA/county definition but try states for robustness. Second, for each definition of local labor markets we try three different definitions of a high-immigration labor market: 5%, 10%, and 20% shares of immigrants in the local population. These immigration and labor-force data are from the 1990 decennial census as reported by the U.S. Bureau of the Census (1994). Altogether, for each of our six primary measures we construct a dichotomous variable, *High*

[23]The 1992 NES survey asked other questions about immigration-related topics which we do not analyze. For example, respondents were asked whether they think Asians or Hispanics "take jobs away from people already here". We do not focus on this question because it does not explicitly address immigration policy. Moreover, its responses cannot clearly distinguish among our three competing economic models. All our models assume full employment, so no natives could have jobs permanently "taken away" from immigrants. Moreover, our models are silent on the dynamics of adjustment. All three models could have immigrants "taking" jobs from natives during adjustment to a new full-employment equilibrium.

Immigration MSA, equal to one for residents in high-immigration labor markets. In the tables we report results for our preferred measure, the MSA/county - 10% definition. Alternative measures are discussed in the robustness checks.[24]

We also constructed several measures of non-economic determinants of preferences. Following previous work in the political-economy literature, we include the following measures in our baseline analysis: gender; age; race; ethnicity; personal immigrant status; party identification; and political ideology. *Gender* is a dichotomous variable equal to one for females. *Age* is a continuous variable. For race we construct the dichotomous variable *Black*, equal to one if the respondent is African-American. For ethnicity we construct the dichotomous variable *Hispanic*, equal to one if the individual self-identifies with a Hispanic ethnic group. *Immigrant* is a dichotomous variable equal to one if the respondent or his/her parents were immigrants into the United States. *Party Identification* is a categorical variable ranging from one for "strong Democrat" to seven for "strong Republican." Finally, *Ideology* is a categorical variable ranging from one for "extremely liberal" to seven for "extremely conservative." In addition to these variables, for certain specifications we included additional regressors which we discuss below.

5.3 Missing Data and Multiple Imputation

Upon constructing the variables of interest and combining them into one individual-level data set, we observed that there was a significant amount of missing data. In the NES survey some individuals did not report either occupation, educational attainment, or industry of employment. This prevented us from constructing some of the factor-income trade-exposure variables for these people. The most serious missing-data problem arose from the homeowners' exposure variables. The county-level COM data suppress some information at the two-digit SIC level to prevent disclosure of individual firms. This hampered our construction of *County Exposure 1* and *County Exposure 2*. Recall that for each county these variables require information on 10 and 14 of the 20 two-digit industries, respectively.

[24] In 1990 immigrants accounted for 7.9% of the overall U.S. population. Accordingly, our 5% cutoff might seem too low, but for completeness we tried it anyway. Also, the 1990 Census MSA data are organized by 1990 MSA definitions, but the 1992 NES survey locates individuals by 1980 MSA definitions. Using unpublished information on 1980-1990 MSA changes obtained from Census officials, we corrected discrepancies as best we could.

Suppressing data for just one industry in the county can be sufficient to prevent construction of one or both of the variables for that county. Overall, when we simply dropped observations with any missing data we lost between 4.4% and 73.4% of the total observations depending on which model was estimated.

This standard approach for dealing with missing values, known as "listwise deletion," can create two major problems. One is inefficiency suffered from throwing away information relevant to the statistical inferences being made. Furthermore, inferences from listwise-deletion estimation can be biased if the observed data differs systematically from the unobserved data. In our case inefficiency was clearly a problem. We also had little reason to believe our data were missing randomly. Individuals of certain types might tend not to report personal information, and the COM suppression probably hits counties with more concentrated industrial mixes.

Alternatives to listwise-deletion for dealing with missing data have been developed in recent years. The most general and extensively researched approach is "multiple imputation" (Schafer (1997), Little and Rubin (1987), Rubin (1987)). This approach has several variations but always involves three main steps. First, some algorithm is used to impute values for the missing data. In this step, m (m>1) "complete" data sets are created consisting of all the observed data and imputations for the missing values. The second step simply involves analyzing each of the m data sets using standard complete-data statistical methods. The final step combines the parameter estimates and variances from the m complete-data analyses to form a single set of parameter estimates and variances. Importantly, this step systematically accounts for variation across the m analyses due to missing data in addition to ordinary sample variation.

Multiple imputation makes a much weaker assumption than listwise deletion about the process generating the missing data. Rather than assuming that the unobserved data is missing completely at random, multiple imputation is unbiased and gives correct uncertainty estimates if the data are missing randomly conditional on the data included in the imputation procedures. Moreover, multiple imputation offers important advantages over ad hoc procedures for dealing with missing data. Imputing sample averages on a variable-by-variable basis biases estimates and standard errors towards zero. Imputing

predicted values from regression models tends to inflate sample correlations and thus bias estimates away from zero. Given all these advantages of multiple imputation, we used this methodology to estimate our models. Details are provided in Scheve and Slaughter (1998, 1999).

Table 1 reports the summary statistics of our trade-opinion measure and our trade-exposure variables calculated by pooling together all 10 of the imputed data sets. Notice that about 67% of respondents favored trade restrictions while 33% were opposed. Just under 68% of respondents were homeowners. Importantly, the means reported in Table 1 are very similar to national means obtained from other data sources.[25]

Table 2 reports the summary statistics of our immigration-opinion measure and explanatory variables calculated by pooling together all 10 of the imputed data sets for each year. The "average" value for *Immigration Opinion* was about 3.6, between the responses "left the same as it is now" and "decreased a little." Also, 23.5% of respondents lived in an MSA/county with an immigrant concentration of at least 10%.[26]

5.3 Econometric Specifications

For trade-policy preferences, our empirical work aims to test how different types of trade exposure affect the probability that an individual supports trade restrictions. Again, we set *Trade Opinion* equal 1 when an individual supports trade restrictions and 0 when opposed. Then $E(\textit{Trade Opinion}_i)$ =

[25]This breakdown of responses for *Trade Opinion* is very similar to responses in other public-opinion polls. Annually from 1983 through 1997, the *Los Angeles Times* has asked 1000-2000 randomly chosen Americans the following question: "Do you think it should be the policy of the U.S. to restrict foreign imports into this country in order to protect American industry and American jobs, or do think there should be no restrictions on the sale of foreign products in the U.S. in order to permit the widest choice and the lowest prices for the American consumer?" Every year somewhere between 63% and 73% of respondents have answered "restrict imports." We thank Karolyn Bowman at the American Enterprise Institute for providing us with these results. Our homeownership rate of 67.9% is close to the national homeownership rate that year. The Census Bureau (1998) reports that during the fourth-quarter of 1992, 64.4% of all households were owner-occupied. Also, in our data a large majority of people work in nonmanufacturing sectors as is the case for the country overall.

[26]The full 1992 breakdown of responses to *Immigration Opinion* is as follows: 58 "increased a lot" (2.3% of the total sample, 2485); 116 "increased a little" (4.7%); 937 "left the same" (37.7%); 552 "decreased a little" (22.2%); and 505 "decreased a lot" (20.3%). In addition we imputed responses for the 87 people (3.5%) responding "don't know / no answer" and the 230 people (9.3%) not asked the question because of survey design (all results reported in the paper are robust to excluding these 230 observations from the analysis). Among our other *High Immigration Area* measures, 43.7% of respondents lived in MSA/county with immigrants accounting for at least 5% of the population, while 8.5% of respondents lived in an MSA/county with immigrants accounting for at least 20% of the population. Finally, we note that the summary statistics in our data are similar to those obtained from the 1992 Merged Outgoing Rotation Groups of the Current Population Survey (CPS). For example, in the 1992 CPS 52.2% of the sample was female, 11.5% was black, and the average age was 43.3.

Pr(*Trade Opinion*$_i$ =1|π_i) = π_i where i indexes each observation and π_i equals the probability that an individual supports trade restrictions. We model the variation in π_i according to a logistic form given below.

$$p_i = \frac{1}{1+\exp(-x_i b)}$$

In this equation x_i is a vector of individual-specific explanatory variables hypothesized to affect the probability of supporting trade restrictions and b is a vector of effect parameters. On each of the 10 final imputed data sets, we estimate these effect parameters using logistic regressions with White robust standard errors to account for any heteroskedasticity.

The theory discussed earlier suggests alternative sets of explanatory variables to include in the x_i vector. Altogether we test 16 different models on each of the data sets. The first four models test just the factor-income regressors individually. The next four specifications, Models 5 through 8, test pairs of explanatory variables, one measuring skills and the other industry. Finally, the last eight specifications, Models 9 through 16, replicate the specifications in Models 5 through 9 but also include our asset regressors. Specifically, we include a homeowner interaction variable and its related county-exposure variable. We include the county-exposure variable itself to verify that is not living in a trade-exposed county *per se* that matters--only homeowners should care about industry mix and its effect on housing values. Overall, Models 1 through 8 test for the role of factor income only in explaining trade-policy preferences. These specifications follow the previous literature most closely. Models 9 through 16 test for both factor-income and asset-value effects as well. All 16 models also include a constant.

For immigration-policy preferences, our empirical work aims to test how skills and other factors affect the probability that an individual supports a certain level of legal immigration. The level of immigration preferred by a respondent could theoretically take on any value, but we do not observe this level. We observe only whether or not the respondent chose one of five ordered categories. Because we have no strong reason to think *ex ante* that these five ordered categories are separated by equal intervals, a linear regression model might produce biased estimates. The more appropriate model for

this situation is an ordered probit which estimates not only a set of effect parameters but also additional parameters representing unobserved category thresholds.

In all our specifications we estimate an ordered probit model where the expected mean of the unobserved preferred immigration level is hypothesized to be a linear function of the respondent's skills, a vector of demographic identifiers, political orientation, and (perhaps) the immigration concentration in the respondent's community. The key hypothesis we want to evaluate is whether more-skilled individuals are less likely to support restrictionist immigration policies as predicted in the HO trade model and in the factor-proportions-analysis model. Accordingly, in our baseline specifications we regress stated immigration-policy preferences on skills, demographic identifiers, and political orientation. In a second set of specifications we also include a dummy variable indicating whether or not the respondent lives in a high-immigration area and an interaction term between this indicator and the respondent's skills. These second specifications can test whether the skills-immigration correlation is strongest in high-immigration labor markets, as in the area-analysis model. To allow for any differences across our three years, we estimate each cross-section separately.

6 Empirical Results on Trade-Policy Preferences

6.1 Testing How Factor Incomes Affect Trade-Policy Preferences

The results of our logistic regressions for Models 1 through 8 strongly support the hypothesis that individuals' skill levels determine trade-policy preferences. Little evidence is found consistent with the hypothesis that industry of employment influences policy preferences.

The actual coefficient estimates and standard errors from Models 1 through 8 using the multiple-imputation methodology are reported in Table A1 in the appendix (listwise-deletion estimates yielding qualitatively similar results are reported in Table A2). But these coefficient estimates alone do not answer our key substantive question of how *changes* in skill levels and industry trade exposure affect the probability that an individual supports trade restrictions. To answer this question we used the estimates of Models 1 through 8 to conduct simulations calculating the effect of changing one variable of interest from average to above-average values while holding the other variables constant at their means.

Our simulation procedure is best described with reference to a specific model and variable of interest. Consider Model 5 and *Occupational Wage* (this model's other regressor is *Sector Tariff*). Recognizing that the parameters reported for this model are estimated with uncertainty, we drew 1000 simulated sets of parameters from their sampling distribution defined as a multivariate normal distribution with mean equal to the maximum likelihood parameter estimates and variance equal to the variance-covariance matrix of these estimates. For each of the 1000 simulated sets of coefficients we then calculated two probabilities. First, we calculated the estimated probability of supporting trade restrictions when *Occupational Wage* and *Sector Tariff* are equal to their means. Second, we calculated the estimated probability of supporting trade restrictions when *Occupational Wage* is one standard deviation above its mean while *Sector Tariff* is held at its mean. The difference between these two estimated probabilities is the estimated difference in the probability of supporting trade restrictions between an individual with average skills and someone with (one standard deviation) above-average skills. We calculated this difference 1000 times, and then to show the distribution of this difference we calculated its mean, its standard error, and a 90% confidence interval around the mean.

Table 3 reports the results of this simulation for Models 1 through 8. Each column reports a different model. Within each column each row reports the estimated effect on the probability of supporting trade restrictions of increasing that row's variable from its sample mean to one standard deviation above its mean, holding fixed all other variables at their means. For example, the results from Model 1 indicate that increasing occupational wage from its mean to one standard deviation above its mean reduces the probability of supporting trade restrictions by 0.074 on average. This estimated change has a standard error of 0.012 and a 90% confidence interval of [-0.095, -0.053].

Models 1 through 4 present these results one regressor at a time. Models 5 through 8 test directly the factor-type versus industry-of-employment hypotheses. This second group of models aim to determine whether one hypothesis better explains the data. In evaluating these results, however, it is important to recall that if individuals evaluate both short-run and long-run effects of trade liberalization then their trade-policy preferences might be explained by both factor type and industry of employment.

Across all models in Table 3, higher skills measured either in terms of higher occupational wage or more education is strongly correlated with lower probabilities of supporting trade restrictions. The mean estimates of probability changes are much larger (in absolute value) than those for the industry measures. These mean estimates are virtually identical whether the specification includes just a skill measure or a skill measure and an industry measure. Moreover, they all are precisely estimated: all have 90% confidence intervals strictly less than zero.

In contrast, higher industry trade exposure has much more ambiguous effects. In Models 3 and 4, greater industry trade exposure is correlated with the hypothesized increase in probability of supporting trade restrictions. But neither of these changes is precisely estimated: both 90% confidence intervals bracket zero. In Models 5 and 6 these results are basically unaffected by including *Occupational Wage*. However, in Models 7 and 8 the inclusion of *Education Years* both reverses the estimated change in probabilities and makes these estimated changes much less precise. Comparing the industry results for Models 5 and 6 with Models 7 and 8, we cannot even conclude with a high degree of confidence that individuals employed in relatively trade-exposed sectors are more likely to support trade restrictions once we control for skill levels.

The key message of Table 3 is that an individual's skill level rather than industry of employment is strongly correlated with the probability of supporting trade restrictions. The effects of skill trade exposure are large and precise; the effects of industry trade exposure are small and uncertain. These results suggest that individuals care about trade policy in a manner consistent with the HO model, and that there is relatively high intersectoral labor mobility in the United States over the time horizons relevant to individuals when evaluating trade policy.

6.2 Testing How Asset Ownership Affects Trade-Policy Preferences

Tables A3 and 4 report results for tests of the hypothesis that individuals care about how trade liberalization affects housing values independent of how it affects factor incomes. Similar to the earlier set of results in Table A1, Table A3 reports the multiple-imputation coefficient estimates from Models 9 through 16. Table 4 reports the results from simulations calculating the effect of changing one variable of interest from average to above-average values while holding the other variables constant at their

means. Before discussing the housing results, we note that the results in Table 4 for skills and industry of employment are substantially the same as the results in Table 3. Even with the inclusion of housing regressors our key conclusion about the relative impact of factor and sector trade exposure on individual trade-policy preferences remains unchanged: factor type dominates industry of employment.

In all eight models reported in Table 4, our estimates of the effect of county trade exposure on individual homeowners' trade preferences are consistent with our theoretical expectations. We estimate that for homeowners, an increase in county trade exposure from its sample mean to one standard deviation above its mean increases the probability of supporting trade restrictions by between 0.029 and 0.039. The results are very similar whether we use *County Exposure 1* or *County Exposure 2*. And in all eight models the probability changes are precisely estimated, with 90% confidence intervals all above zero. These results support our hypothesis that homeowners living in counties with a larger share of employment in comparative-disadvantage sectors are more likely to oppose trade liberalization because regional housing values depend, among other things, on the amount of regional economic employment in trade-exposed sectors.

6.3 Robustness Checks

We checked the robustness of the empirical results in several ways. First, the theory motivating our analysis assumes all individuals work and earn factor income. Accordingly, we limited our data set to individuals currently in the labor force (either working or unemployed but actively seeking work). Like the United States overall in 1992, in the NES survey only about two-thirds of respondents were in the labor force. The results for the working-only sample were qualitatively similar to the full-sample results.[27]

Similar to this, we considered the possibility that the proper unit of observation is the household rather than the individual: perhaps individuals evaluate how trade affects their household's income rather than just their individual incomes. The NES survey reports education, occupation, and industry of employment for spouses of respondents, so in some specifications we used regressors reporting a

[27] 65% of the sample reported being in the labor force, versus the 1992 actual national share of 66.6%. Respondents not currently in the labor force are asked in the NES survey to report as "industry of employment" the industry last worked in.

combination of respondent and spousal information. These household results were generally consistent with the individual ones.

To verify the strength of our data we tried other measures of factor type, industry exposure, and county exposure. For factor type we tried the respondents' reported 1991 annual income. The results were qualitatively similar to those obtained for average occupation wages and education.[28] For industry trade exposure we tried imports as a share of output. Although imports do not measure revealed comparative advantage, so often imports are considered to be harmful that we thought many individuals might focus on imports only when evaluating trade policy. This import measure did not work as hypothesized, however. To test county exposure we tried wage bill instead of employment as the measure of county economic activity. The wage-bill results were qualitatively similar to those obtained for employment.

Similar to this, we also estimated specifications including both *Occupation Wage* and *Education Years* and then specifications including both *Sector Tariff* and *Sector Net Export Share*. This allowed us to test whether each regressor has explanatory power independent of its substitute. When regressed together, both *Occupation Wage* and *Education Years* are significantly less than zero. In contrast, when regressed together both *Sector Tariff* and *Sector Net Export Share* are not significantly different from zero.

Finally, we controlled for many other possible determinants of trade-policy preferences that are not derived from trade theory but that might bias our estimates. First, we reran Models 9 through 16 including *House*, the other separate component of our interaction term. Like *County Exposure 1 (2)*, *House* never entered significantly. Most importantly, with either or both separate regressors the results for our interaction term were qualitatively unchanged (although with *House* the significance of our interaction term fell slightly). This suggests that neither home ownership alone nor residence in a trade-exposed county alone is sufficient to affect trade-policy preferences: it is the combination of the two that matters.

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[28]Despite this similarity, we regard average occupation wages and education to be superior skill measures. These two variables better reflect an individual's long-run earnings capacity; in contrast, annual income can fluctuate more for reasons unrelated to skill (such as illnesses, inheritances, or overtime).

In addition to robustness checks involving homeownership, we also tried a number of other demographic, political, and economic variables. For example, we reestimated Models 1 through 8 adding in the following regressors: age, gender, race, party identification, and union membership (Table A4 reports this particular set of robustness checks). In these specifications, age and race have no consistently significant effect while women, those who identify more strongly with the Democratic party, and union members are significantly more likely to support trade barriers. Controlling for all these variables, however, does not significantly change our key result that factor exposure explains trade preferences much more than sector exposure. In other similar robustness checks (not reported) we also found that our homeownership result holds up to the inclusion of additional regressors.[29]

7 Empirical Results on Immigration-Policy Preferences

7.1 Testing How Skills Affect Immigration-Policy Preferences

Our initial specifications allow us to test the HO and factor-proportions-analysis models. Table 5 presents the results for each year's full sample, where in Model 1 we measure skills with *Occupation Wage* and in Model 2 we use *Education Years*. The key message of Table 5 is that by either measure, skill levels are significantly correlated with *Immigration Opinion* at at least the 99% level. Less-skilled (more-skilled) people prefer more-restrictionist (less-restrictionist) immigration policy. This skills-preferences link holds conditional on a large set of plausible non-economic determinants of *Immigration Opinion*. Among these other regressors *Gender*, *Age*, *Hispanic*, and *Party Identification* are insignificantly different from zero. *Black* and *Immigrant* are mostly significantly

[29]One final objection to our analysis might be whether the estimated effects of *Occupation Wage* and *Education Years* on trade-policy preferences actually reflect returns to skill as we are interpreting them. This question is partly answered by appealing to theory: the empirical results are consistent with and do not falsify the predictions of the HO trade model. One empirical result supporting our interpretation is our robustness check that both *Occupation Wage* and *Education Years* are significant when regressed together. If one variable is thought to measure something other than returns to skill, the significance of the other variable conditional on the first supports our interpretation. A second empirical finding supporting our interpretation is that our main results are attenuated when we analyze the sub-sample of individuals not currently in the labor force. If our variables were not measuring skills then presumably their explanatory power would not be attenuated among non-working individuals. Finally, a third empirical test of our interpretation is to see if our skills variables are systematically related to individual opinions on policy for which skill level is *not* a theoretically relevant variable. We ran such a test on individual support for the death penalty. If *Occupation Wage* and *Education Years* measure returns to skill then they should not be significantly correlated with death-penalty opinions. If they are significantly correlated then our interpretation of them is weakened. The results from these death-penalty regressions support our interpretation. *Occupation Wage* is significantly positively correlated with support for the death penalty only in the specification with no other regressors. *Education Years* is never significantly related to death-penalty opinions.

negative: blacks, and the group of immigrants plus children of immigrants, prefer less-restrictionist immigration policy. *Ideology* is significantly positive: more-conservative people prefer more-restrictionist immigration policy. Our non-skill estimates are similar to those in Citrin, et al (1997) and Espenshade and Hempstead (1995).

The actual coefficient estimates in Table 5 identify the qualitative effect on *Immigration Opinion* of skills and our other regressors. To answer our key substantive question of how *changes* in skill levels or other variables affect the probability that an individual supports immigration restrictions, we used the estimates of Models 1 and 2 to conduct simulations calculating the effect on immigration preferences of changing skills while holding the other variables constant at their sample means. These simulations calculated the estimated probability of supporting immigration restrictions (i.e., the probability of supporting a reduction in immigration by either "a lot" or "a little") when a regressor of interest is increased from its sample mean to its sample maximum (or is changed from zero to one for a dichotomous variable) while holding fixed all other regressors at their means.

Table 6 reports the results of this simulation for our two skills regressors. For 1992, increasing *Occupation Wage* from its mean to its maximum ($513 per week to $1138 per week), holding fixed all other regressors at their means, reduces the probability of supporting immigration restrictions by 0.086 on average. This estimated change has a standard error of 0.031 and a 90% confidence interval of (-0.138, -0.036). The 1992 results for *Education Years* are similar. Increasing *Education Years* from its mean to its maximum (about 12.9 years to 17 years), holding fixed all other regressors at their means, reduces the probability of supporting immigration restrictions by 0.126 on average. This estimated change has a standard error of 0.029 and a 90% confidence interval of (-0.174, -0.081). All three years give the same result: higher skills are strongly and significantly correlated with lower probabilities of supporting immigration restrictions. Table A5 gives simulation results for all variables in Models 1 and 2.[30]

[30]For our simulation procedures we used the Stata program *CLARIFY* (Tomz, et al 1998). These procedures are discussed in King, et al (1998b).

Citrin, et al (1997) assume that *Occupation Wage* and *Education Years* do not measure labor-market skills. For example, *Education Years* might indicate tolerance or civic awareness. To test this possibility, we split our sample between those in the labor force and those not in the labor force and then reestimated Models 1 and 2 on each subsample. We defined the subset of labor-force participants as those individuals reporting they were either employed or "temporarily" unemployed but seeking work. In every year the not-in-labor-force subsample was disproportionately female: about two females for every male, versus a majority of males in the labor-force group. In every year the not-in-labor-force subsample was also much older: an average age of about 60, versus 40 for those in the labor force. It is well known that females and older people have much lower labor-force participation rates than the overall population.

If *Occupation Wage* and *Education Years* measure labor-market skills, then the correlation between these regressors and *Immigration Opinion* should hold only among labor-force participants. If these regressors measure non-labor-market considerations, then their explanatory power should not vary across the two subsamples. Table 7 reports the results. For the labor-force subsample both *Occupation Wage* and *Education Years* are strongly significant, with larger coefficient estimates than the full-sample estimates from Table 5. For the not-in-labor-force subsample the coefficient estimates are much smaller than the full-sample estimates and are not significantly different from zero in two of the three years.[31]

As a second check on the interpretation of our skills regressors, we added to Models 1 and 2 direct measures of ethnic and racial tolerance, as proxied by respondents' answers to three different tolerance statements or questions (e.g., "We should be more tolerant of people who choose to live according to their own moral standards, even if they are very different from our own"). In all specifications greater tolerance was significantly correlated with preferences for less-restrictionist immigration policy, but our

[31] The labor-force subsample is 64.9% of the total 1992 sample, close to the 1992 aggregate labor-force participation rate of 66.6%. The reported occupation for those not in the labor force is their most-recent job. Also, we obtained the same results qualitatively from an alternative specification of our skills test in which we pooled the full sample and interacted skills with a dichotomous variable for labor-force status participation. The split-sample test is more general in that it does not constrain the non-skill regressors to have the same coefficient for both labor-force groups.

significant skills-preferences correlation persisted. Overall, we interpret these two checks as evidence that *Occupation Wage* and *Education Years* measure labor-market skills.

The result that skills correlate with immigration-policy preferences is inconsistent with an HO model in which immigration is completely absorbed by Rybczynski output-mix effects. It is consistent both with the factor-proportions-analysis model and with an HO model in which immigration affects both wages and output mix. By pooling all regions of the country in Tables 2 through 4, however, we have not yet tested the area-analysis model. To do this we modify our initial specifications by adding the regressor *High Immigration MSA* and its interaction with skills. If preferences are consistent with the area-analysis model, then less-skilled (more-skilled) workers in gateway communities should have stronger preferences for more-restrictionist (less-restrictionist) immigration policies than less-skilled (more-skilled) workers in non-gateway communities. These preferences imply a positive coefficient on *High Immigration MSA* and a negative coefficient on its interaction with skills.[32]

Table 8 presents the results for this specification, where Model 3 uses *Occupation Wage* and Model 4 *Education Years*. The results for all the non-skill regressors are qualitatively the same as before. Our skill measures are still negatively correlated with preferences at at least the 95% level. But in neither case is *High Immigration MSA* significantly positive or its interaction with skills significantly negative. In fact, for *Education Years* we obtain the exact opposite coefficients on both regressors at about the 95% significance level. In unreported specifications we tested this specification using our other five definitions of *High Immigration MSA* and/or splitting the sample as in Table 7. In almost every case the interaction term's coefficient was positive but not significant; in no case did the interaction term ever have a significantly negative coefficient or *High Immigration MSA* a significantly positive one. Overall, people living in high-immigration areas do not have a stronger correlation between skills and

[32]The positive coefficient on *High Immigration MSA* would indicate that low-skilled people in high-immigration areas prefer more-restrictive policies relative to low-skilled people living elsewhere. If that high-skilled people in high-immigration areas prefer less-restrictive policies relative to high-skilled people living elsewhere, then we would expect the sum of the coefficients on *High Immigration MSA* and its interaction with skills to be negative. With a positive coefficient on *High Immigration MSA*, this would require the interaction coefficient to be negative (and larger in absolute value than that on *High Immigration MSA*).

immigration-policy preferences than people living elsewhere. This is inconsistent with the area-analysis model.[33]

7.2 Robustness Checks

We checked the robustness of the empirical results by trying other measures of our important skill and immigration regressors. For skills we tried three dichotomous variables of educational attainment (high-school dropouts, high-school graduates, and some college--the omitted group being college and beyond) to look for any non-linearities in how skills affect preferences.[34] We discovered no clear non-linearities: the relative coefficients on the dichotomous measures seemed consistent with an overall linear effect. We also tried respondents' previous-year income, and obtained qualitatively similar results to those for *Occupation Wage* and *Education Years*.[35]

In addition to the six measures of *High Immigration MSA* discussed earlier, we also tried a dichotomous measure of residence in one of the "big six" immigrant states of California, Florida, Illinois, New Jersey, New York, and Texas. Borjas, et al (1997) report that in 1960 60% of all U.S. immigrants lived in these six states and that by 1990 that share had risen to 75%. Borjas, et al (1996) report that in 1992 60% of all U.S. legal immigrants came into California or New York alone; another 20% entered the other four gateway states. We also tried measuring immigration concentration with a continuous variable (the foreign-born share of each area's population) or with *High Immigration MSA*

[33]In Table 8, 1992 estimates using *Education Years* suggest the skills-preferences link may actually be attenuated in high-immigration areas. Although the attenuation was only marginally significant in a few regressions (one of six in Table 8, and very few other unreported estimates), we explored further what might cause it. One possibility is that more-skilled people in gateway communities worry about higher tax liabilities caused by an immigration-induced rise in demand for public services. If this were true, the skills regressor would be conflating two separate effects: the wage effect and the tax effect. To test this hypothesis we added "fiscal" regressors (home ownership; annual family income; and individual responses to the question of whether immigrants "cause higher taxes due to more demands for more public services") to our specification to control for individual tax liability. If the tax hypothesis were true then the skills-preferences attenuation would disappear in specifications which include the fiscal regressors. This did not happen, however. An alternative explanation is that people in high-immigration communities worry less about wage effects than people elsewhere because they have more direct experience of the output-mix effects of the HO model. Of course, there are many potential sociological explanations as well. For example, contact with immigrants may reduce xenophobic attitudes towards them, especially among less-skilled individuals.

[34]In 1992, among those answering the *Education Years* question there were 466 high-school drop-outs, 812 high-school graduates, 572 people with some college, and 570 people with a college degree or higher.

[35]Despite this similarity, we regard average occupation wages and education to be superior skill measures. These two variables probably better reflect an individual's long-run earnings capacity; in contrast, annual income can fluctuate more for reasons unrelated to skill (such as illnesses, inheritances, or overtime).

plus an analogous low-immigration dummy. With all these measures we again found no evidence of preferences consistent with the area-analysis model.

We also checked the robustness of our results by including a number of other regressors. Our main findings on skills and geography were consistently robust to our alternative specifications. Perhaps most importantly, we added a measure of the skill-mix of immigrants in the local community. Recall that the NES immigration-preferences question does not specify any skill level of prospective immigrants, and that we have assumed, consistent with the data, that respondents think U.S. immigrant inflows increase the relative supply of less-skilled workers. Different communities, however, may have very different skill mixes of immigrants, and this may affect how local citizens think about immigration policy.

To try to control for this possibility, we obtained data on the educational attainment of the stock of immigrants in local communities as reported in the 1990 Decennial Census. We then defined the skill-mix of immigrants using three different cutoffs: the share with a college degree or higher; the share with more than a high-school degree; and the share with a high-school degree or higher. Adding this immigrant-skill-mix regressor to Models 3 and 4 does not alter our results for local-labor-market effects. As for the new regressor itself, individuals living in communities with a higher skill mix of immigrants are somewhat more likely to support more immigration: the estimated coefficient in Models 3 and 4 is negative, significantly so in a minority of cases.[36]

Other added regressors included union membership: union members preferred more-restrictionist immigration policy, an effect that was statistically significant in some specifications. Two other regressors were retrospective evaluations of the national economy and retrospective evaluations of personal finances. Both retrospective measures tended to have the expected sign -- those with gloomier retrospections preferred more-restrictionist immigration policy -- but were always insignificant. Finally, we included state unemployment rates, another geography-varying regressor, to control in the cross-

[36] We thank George Borjas for providing us with these data. One might worry that each NES respondent interprets the immigration question to mean immigrants of the same skills as the respondent him/herself. Were this the case, one might reasonably expect every respondent to support fewer immigrants. That reported preferences look very different from this, and that we find a robust skills-preferences correlation, suggests respondents are not interpreting the immigration question this way.

section for any business-cycle effect on immigration-policy preferences. This regressor was always insignificant, however.

8 Conclusion

This paper has used three years of individual-level data to analyze the determinants of individual preferences over trade and immigration policies in the United States. Different economic models make contrasting predictions about what forces should shape these preferences, and theory shows quite clearly that different preferences lead to different trade-policy outcomes. But a major limitation of the political-economy literature has been a lack of clear evidence about what these preferences actually are. The analysis in this paper has led to four main empirical results, two for trade policy and two for immigration policy.

First, we found that factor type dominates industry of employment in explaining support for trade barriers. Lower skill is strongly correlated with support for new trade barriers. In contrast, employment in industries more exposed to trade is not strongly correlated with support for new trade barriers. This result is consistent with a Heckscher-Ohlin model in which the United States is well endowed with skilled labor relative to the rest of the world. It suggests there is relatively high intersectoral labor mobility in the United States over the time horizons relevant to individuals when evaluating trade policy.

The second main empirical result is that in addition to factor incomes, home ownership also matters for individuals' trade-policy preferences. We found that independent of factor type or industry of employment, home ownership in counties with a manufacturing mix concentrated in comparative-disadvantage industries is strongly correlated with support for trade barriers. This finding suggests that in addition to current factor incomes driving preferences as in standard models, in reality preferences also depend on asset values.

Moving to immigration-policy preferences, the third main finding is a robust link between labor-market skills and preferences: less-skilled (more-skilled) people prefer more-restrictionist (less-restrictionist) immigration policy. This link strongly supports the contention that people's position in the labor force influences their policy opinions. It is consistent both with the factor-proportions-analysis

model and with a Heckscher-Ohlin model. Finally, we found no evidence that this skills-preferences link is stronger in high-immigration labor markets. This finding is inconsistent with the area-analysis model.

Overall, U.S. policy opinions over "globalization," broadly defined, cleave mainly along skill lines. The less-skilled the individual, the more likely he or she opposes trade and immigration liberalization. This link between skills and policy preferences suggests the potential for globalization politics to be connected to mainstream redistributive politics over which political parties often contest elections. It also highlights that the median voter in the United States today is likely *opposed* to, not in support of, further trade and immigration policy.

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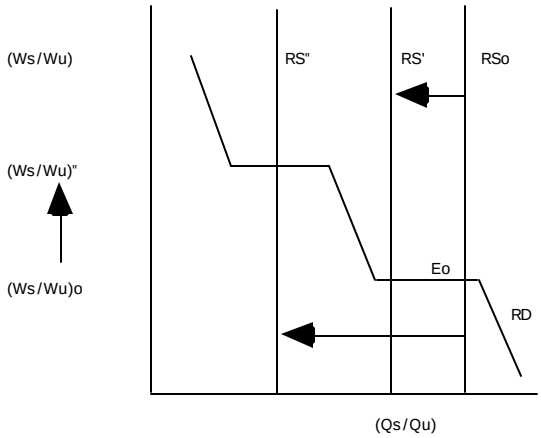
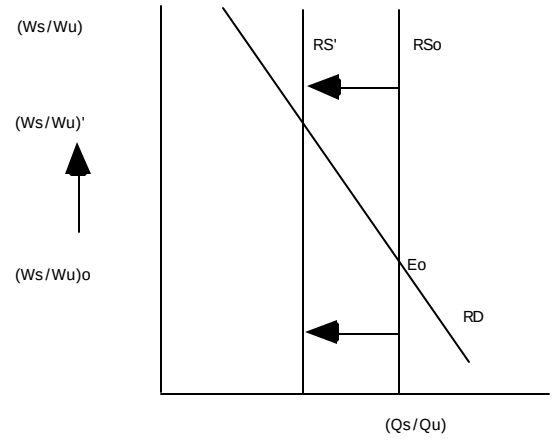
Figure 1
Labor-Market Equilibrium:
The Heckscher-Ohlin Model



Notes: Skilled labor is subscripted "s" and unskilled labor "u". The RS schedule is national relative supply and the RD schedule is national relative demand.

Figure 2
Labor-Market Equilibrium:
The Factor-Proportions-Analysis Model or
The Area-Analysis Model



Notes: Skilled labor is subscripted "s" and unskilled labor "u". The RS schedule is relative supply and the RD schedule is relative demand. For the factor-proportions-analysis model this picture represents the single national labor market; for the area-analysis model this picture represents each separate local labor market.

Table 1
Summary Statistics for Analysis of Trade-Policy Preferences

Variable	Mean	Standard Error
Trade Opinion	0.671	0.470
Occupational Wage	0.532	0.187
Education Years	13.288	2.610
Sector Tariff	0.006	0.019
Sector Net Export Share	-0.004	0.091
County Exposure 1	0.096	0.093
County Exposure 2	0.191	0.334
House	0.679	0.467

Notes: These summary statistics are multiple imputation estimates based on the 10 imputed data sets with 1736 observations for each variable in each data set based off of the 1992 NES data. The variable *Occupational Wage* reports the actual wage divided by 1000.

Table 2
Summary Statistics for Analysis of Immigration-Policy Preferences

Variable	1992	1994	1996
Immigration Opinion	3.595	3.982	3.785
	(1.027)	(1.064)	(0.982)
Occupation Wage	0.512	0.574	0.601
	(0.187)	(0.227)	(0.225)
Education Years	12.923	13.153	13.323
	(2.815)	(2.637)	(2.660)
Gender	0.534	0.534	0.552
	(0.499)	(0.499)	(0.497)
Age	45.755	46.264	47.544
	(17.711)	(17.646)	(17.416)
Black	0.129	0.115	0.122
	(0.336)	(0.319)	(0.327)
Hispanic	0.072	0.046	0.087
	(0.259)	(0.209)	(0.282)
Immigrant	0.181	0.166	0.147
	(0.385)	(0.371)	(0.355)
Party ID	3.701	3.916	3.673
	(2.027)	(2.102)	(2.102)
Ideology	4.237	4.446	4.275
	(1.399)	(1.348)	(1.398)
High Immigration MSA	0.235	0.227	0.215
	(0.424)	(0.419)	(0.411)
Observations	2485	1795	1714

Notes: These summary statistics are multiple-imputation estimates based on the 10 imputed data sets for each year. Each cell reports the variable mean and (in parenthesis) its standard deviation. *Occupation Wage* reports the actual weekly wage divided by 1000.

Table 3
Determinants of Respondent Opinion on International-Trade Restrictions: Factor-Income Models

For each of the eight factor only models, we estimated using multiple imputation with a logit specification the effect of factor and industry exposure to international trade on individuals' trade policy opinions. The parameter estimates from this analysis are reported in the appendix Here we interpret those results by presenting the impact of a one standard deviation increase in each independent variable, holding other variables constant, on the probability that the respondent supports trade restrictions. For each of these changes, we report the mean effect, the standard error of that estimate, and a 90% confidence interval.

Change in Probability of Supporting Trade Restrictions as a Result of a One Standard Deviation Increase in the Independent Variable for Each Model [1]

Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Occupational	-0.074				-0.074	-0.074		
Wage	(0.012)				(0.012)	(0.013)		
	[-0.095, -0.053]				[-0.094, -0.055]	[-0.097, -0.054]		
Education		-0.132					-0.132	-0.133
Years		(0.015)					(0.016)	(0.016)
		[-0.158, -0.107]					[-0.158, -0.106]	[-0.161, -0.106]
Sector			0.012		0.011		-0.001	
Tariff			(0.012)		(0.013)		(0.012)	
			[-0.010, 0.032]		[-0.011, 0.032]		[-0.021, 0.019]	
Sector Net				-0.014		-0.014		0.000
Export Share				(0.013)		(0.013)		(0.013)
				[-0.035, 0.008]		[-0.035, 0.008]		[-0.021, 0.021]

Notes:

[1] Each triple of entries in the table begins with the mean effect from 1000 simulations of the change in probability of supporting trade restrictions due to an increase of one standard deviation from the independent variable's mean, holding all other variables constant at their means. The standard error of this estimate is reported in parentheses. Finally, a 90% confidence interval for the probability change is presented in brackets.

Table 4
Determinants of Respondent Opinion on International-Trade Restrictions: Factor-Income and Asset Ownership Models

For each of the eight factor and asset models, we estimated using multiple imputation with a logit specification the effect of factor, industry, and asset exposure to international trade on individuals' trade policy opinions. The parameter estimates from this analysis are reported in the appendix Here we interpret those results by presenting the impact of a one standard deviation increase in each independent variable, holding other variables constant, on the probability that the respondent supports trade restrictions. For each of these changes, we report the mean effect, the standard error of that estimate, and a 90% confidence interval.

Change in Probability of Supporting Trade Restrictions as a Result of a One Standard Deviation Increase in the Independent Variable for Each Model [1]

Variables	Model 9	Model 10	Model 11	Model 12	Model 13	Model 14	Model 15	Model 16
Occupation	-0.074	-0.075	-0.074	-0.074				
Wage	(0.013)	(0.012)	(0.012)	(0.013)				
	[-0.095, -0.052]	[-0.095, -0.055]	[-0.094, -0.053]	[-0.094, -0.052]				
Education					-0.130	-0.131	-0.130	-0.131
Years					(0.016)	(0.017)	(0.016)	(0.016)
					[-0.156, -0.104]	[-0.158, -0.104]	[-0.157, -0.105]	[-0.157, -0.103]
Sector		0.008		0.010		-0.003		-0.001
Tariff		(0.013)		(0.012)		(0.013)		(0.013)
		[-0.015, 0.029]		[-0.011, 0.030]		[-0.024, 0.017]		[-0.022, 0.020]
Sector Net	-0.013		-0.013		0.002		0.001	
Export Share	(0.012)		(0.012)		(0.012)		(0.012)	
	[-0.033, 0.008]		[-0.034, 0.008]		[-0.019, 0.021]		[-0.020, 0.021]	
Cty Exp 1	0.034	0.034			0.029	0.030		
Cty Exp 1 * House[2]	(0.013)	(0.014)			(0.013)	(0.013)		
	[0.011, 0.055]	[0.012, 0.057]			[0.007, 0.051]	[0.006, 0.051]		
Cty Exp 2			0.039	0.038			0.029	0.029
Cty Exp 2 * House[2]			(0.015)	(0.016)			(0.017)	(0.017)
			[0.014, 0.063]	[0.012, 0.064]			[0.000, 0.056]	[0.001, 0.056]

Notes:

[1] Each triple of entries in the table begins with the mean effect from 1000 simulations of the change in probability of supporting trade restrictions due to an increase of one standard deviation from the independent variable's mean, holding all other variables constant at their means. The standard error of this estimate is reported in parentheses. Finally, a 90% confidence interval for the probability change is presented in brackets.

[2] The one-standard-deviation increase for the county-exposure variable is calculated assuming the individual is a homeowner. This means that the change affects two independent variables: the county variable itself and its interaction with homeownership.

Table 5
Determinants of Immigration-Policy Preferences:
Testing the Heckscher-Ohlin and Factor-Proportions-Analysis Models

	1992		1994		1996	
Regressor	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Occupation Wage	-0.349		-0.811		-0.541	
	(0.130)		(0.135)		(0.133)	
Education Years		-0.044		-0.074		-0.059
		(0.010)		(0.011)		(0.012)
Gender	-0.022	-0.008	0.022	0.083	-0.020	0.024
	(0.048)	(0.046)	(0.056)	(0.054)	(0.060)	(0.057)
Age	-0.000	-0.002	0.000	-0.002	0.004	0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Black	-0.207	-0.225	-0.222	-0.211	-0.238	-0.241
	(0.080)	(0.080)	(0.091)	(0.092)	(0.096)	(0.097)
Hispanic	-0.064	-0.122	-0.306	-0.360	-0.124	-0.172
	(0.111)	(0.110)	(0.136)	(0.137)	(0.120)	(0.121)
Immigrant	-0.158	-0.150	-0.213	-0.193	-0.220	-0.207
	(0.066)	(0.066)	(0.076)	(0.076)	(0.087)	(0.087)
Party ID	0.003	0.008	-0.006	-0.002	-0.023	-0.016
	(0.013)	(0.013)	(0.016)	(0.016)	(0.016)	(0.016)
Ideology	0.057	0.050	0.054	0.041	0.080	0.072
	(0.020)	(0.020)	(0.028)	(0.029)	(0.025)	(0.025)
Observations	2485	2485	1795	1795	1714	1714

Notes: These results are multiple-imputation estimates of ordered-probit coefficients based on the 10 imputed data sets for each year. Each cell reports the coefficient estimate and (in parenthesis) its standard error. In both models the dependent variable is individual opinions about whether U.S. policy should increase, decrease, or keep the same the annual number of legal immigrants. This variable is defined such that higher (lower) values indicate more-restrictive (less-restrictive) policy preferences. For brevity, estimated cut points are not reported.

Table 6
Estimated Effect of Increasing Skill Levels on the Probability of Supporting Immigration Restrictions

Increase Skill Measure From Mean to Maximum	Year	Change in Probability of Supporting Immigration Restrictions
Occupation Wage	1992	-0.086
		(0.031)
		[-0.138,-0.036]
Education Years		-0.126
		(0.029)
		[-0.174,-0.081]
Occupation Wage	1994	-0.337
		(0.050)
		[-0.416,-0.252]
Education Years		-0.112
		(0.019)
		[-0.143,-0.081]
Occupation Wage	1996	-0.201
		(0.047)
		[-0.274,-0.120]
Education Years		-0.085
		(0.017)
		[-0.113,-0.057]

Notes: Using the estimates from Model 1 and 2, we simulated the consequences of changing each skill measure from its mean to its maximum on the probability of supporting immigration restrictions. The mean effect is reported first, with the standard error of this estimate in parentheses followed by a 90% confidence interval.

Table 7
Differential Impact of Skill on Immigration-Policy Preferences:
Labor-Force Participants and Non-Labor-Force Participants

Sample	Year	Occupation Wage	Education Years
In Labor Force	1992	-0.396	-0.077
		(0.158)	(0.013)
Not In Labor Force		-0.248	-0.012
		(0.254)	(0.015)
In Labor Force	1994	-0.886	-0.092
		(0.149)	(0.014)
Not In Labor Force		-0.648	-0.053
		(0.263)	(0.018)
In Labor Force	1996	-0.703	-0.089
		(0.162)	(0.015)
Not In Labor Force		-0.088	-0.013
		(0.302)	(0.020)

Notes: This table displays multiple-imputation estimates of ordered probit coefficients for our skill regressors when the sample is limited to either respondents currently in the labor force or those not currently in the labor force. The standard error of each estimate is listed in parentheses. Each specification includes all the other control variables from Table 5.

Table 8
Determinants of Immigration-Policy Preferences: Testing the Area-Analysis Model

	1992		1994		1996	
Regressor	Model 3	Model 4	Model 3	Model 4	Model 3	Model 4
Occupation Wage	-0.334		-0.801		-0.572	
	(0.161)		(0.151)		(0.150)	
Occ. Wage x	-0.030		0.119		0.231	
High Imm. MSA	(0.309)		(0.291)		(0.319)	
Education Years		-0.054		-0.074		-0.061
		(0.011)		(0.013)		(0.013)
Educ. Years x		0.038		0.012		0.016
High Imm. MSA		(0.019)		(0.024)		(0.030)
High Imm. MSA	-0.005	-0.501	-0.218	-0.299	-0.206	-0.264
	(0.168)	(0.264)	(0.192)	(0.343)	(0.225)	(0.441)
Gender	-0.021	-0.009	0.023	0.081	-0.022	0.023
	(0.048)	(0.046)	(0.056)	(0.054)	(0.060)	(0.057)
Age	-0.000	-0.002	0.000	-0.002	0.004	0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Black	-0.204	-0.224	-0.206	-0.196	-0.231	-0.236
	(0.080)	(0.078)	(0.091)	(0.092)	(0.097)	(0.098)
Hispanic	-0.057	-0.085	-0.250	-0.299	-0.102	-0.150
	(0.117)	(0.115)	(0.138)	(0.138)	(0.121)	(0.125)
Immigrant	-0.154	-0.151	-0.176	-0.158	-0.206	-0.198
	(0.069)	(0.069)	(0.079)	(0.079)	(0.090)	(0.090)
Party ID	0.003	0.009	-0.007	-0.003	-0.023	-0.015
	(0.013)	(0.013)	(0.016)	(0.017)	(0.016)	(0.016)
Ideology	0.057	0.050	0.052	0.040	0.079	0.072
	(0.020)	(0.020)	(0.029)	(0.029)	(0.025)	(0.025)
Observations	2485	2485	1795	1795	1714	1714

Notes: These results are multiple-imputation estimates of ordered-probit coefficients based on the 10 imputed data sets for each year. Each cell reports the coefficient estimate and (in parenthesis) its standard error. In both models the dependent variable is individual opinions about whether U.S. policy should increase, decrease, or keep the same the annual number of legal immigrants. This variable is defined such that higher (lower) values indicate more-restrictive (less-restrictive) policy preferences. For brevity, estimated cut points are not reported.

Appendix Table A1
Determinants of Individual Opinion On International-Trade Restrictions: Factor-Income Models

Multiple-Imputation Inferences for Logistic Regression Coefficients

Explanatory Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Constant	1.642	3.648	0.696	0.711	1.625	1.642	3.651	3.650
	(0.168)	(0.350)	(0.054)	(0.051)	(0.170)	(0.168)	(0.355)	(0.351)
Occupational Wage	-1.716				-1.711	-1.720		
	(0.288)				(0.288)	(0.288)		
Education Years		-0.217					-0.217	-0.217
		(0.025)					(0.025)	(0.025)
Sector Tariff			2.730		2.420		-0.137	
			(2.994)		(3.089)		(2.976)	
Sector Net Ex Shr				-0.697		-0.720		0.027
				(0.612)		(0.614)		(0.615)
# of Observations	1736	1736	1736	1736	1736	1736	1736	1736

Appendix Table A2
Determinants of Individual Opinion On International-Trade Restrictions: Factor-Income Models

Listwise-Deletion Inferences for Logistic Regression Coefficients

Explanatory Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Constant	1.590	3.739	0.712	0.768	1.552	1.526	3.725	3.656
	(0.176)	(0.363)	(0.055)	(0.054)	(0.181)	(0.186)	(0.377)	(0.383)
Occupational Wage	-1.672				-1.617	-1.480		
	(0.298)				(0.303)	(0.321)		
Education Years		-0.226					-0.223	-0.216
		(0.026)					(0.027)	(0.027)
Sector Tariff			2.764		3.162		-0.642	
			(2.996)		(3.177)		(2.976)	
Sector Net Ex Shr				-0.642		-0.711		0.113
				(0.616)		(0.617)		(0.622)
# of Observations	1530	1621	1660	1568	1463	1371	1552	1467

Appendix Table A3
Determinants of Individual Opinion
On International-Trade Restrictions:
Factor-Income and Asset-Ownership Models

Multiple-Imputation Inferences for Logistic Regression Coefficients

Variables	Model 9	Model 10	Model 11	Model 12	Model 13	Model 14	Model 15	Model 16
Constant	1.567	1.552	1.596	1.578	3.586	3.590	3.594	3.593
	(0.174)	(0.176)	(0.172)	(0.174)	(0.355)	(0.358)	(0.355)	(0.359)
Occupational Wage	-1.766	-1.759	-1.746	-1.738				
	(0.292)	(0.292)	(0.291)	(0.290)				
Education Years					-0.218	-0.218	-0.216	-0.216
					(0.025)	(0.025)	(0.025)	(0.025)
Sector Tariff		1.828		2.275		-0.573		-0.128
		(3.189)		(3.132)		(3.056)		(3.004)
Sector Net Ex Share	-0.624		-0.653		0.084		0.044	
	(0.609)		(0.609)		(0.615)		(0.615)	
County Exposure 1	-0.334	-0.301			-0.570	-0.567		
	(0.849)	(0.852)			(0.881)	(0.880)		
County Exp 1 * House	2.195	2.182			2.183	2.185		
	(0.951)	(0.951)			(0.966)	(0.964)		
County Exposure 2			-0.146	-0.136			-0.183	-0.184
			(0.296)	(0.298)			(0.311)	(0.311)
County Exp 2 * House			0.780	0.779			0.675	0.676
			(0.375)	(0.376)			(0.392)	(0.391)
# of Observations	1736	1736	1736	1736	1736	1736	1736	1736

Appendix Table A4
Determinants of Individual Opinion
On International-Trade Restrictions:
Factor-Income Models Plus Additional Control Variables

Listwise-Deletion Inferences for Logistic Regression Coefficients

Explanatory Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Constant	1.586	3.491	0.858	0.900	1.527	1.497	3.449	3.405
	(0.267)	(0.407)	(0.202)	(0.206)	(0.274)	(0.280)	(0.424)	(0.431)
Occupational Wage	-1.343				-1.273	-1.153		
	(0.314)				(0.320)	(0.338)		
Education Years		-0.219					-0.213	-0.209
		(0.026)					(0.027)	(0.028)
Sector Tariff			3.606		3.482		0.187	
			(3.306)		(3.341)		(3.332)	
Sector Net Ex Share				-0.486		-0.551		0.165
				(0.619)		(0.614)		(0.636)
Age 18-29	-0.486	0.178	-0.043	-0.092	-0.078	-0.123	0.135	0.068
	(0.183)	(0.181)	(0.175)	(0.180)	(0.187)	(0.194)	(0.185)	(0.190)
Age 30-44	-0.263	-0.065	-0.362	-0.387	-0.260	-0.296	-0.081	-0.115
	(0.154)	(0.155)	(0.149)	(0.154)	(0.158)	(0.164)	(0.159)	(0.164)
Age 45-59	-0.220	-0.042	-0.338	-0.321	-0.266	-0.257	-0.095	-0.090
	(0.168)	(0.171)	(0.163)	(0.170)	(0.173)	(0.180)	(0.176)	(0.183)
Gender	0.178	0.335	0.393	0.384	0.220	0.228	0.347	0.369
	(0.117)	(0.113)	(0.110)	(0.113)	(0.121)	(0.124)	(0.117)	(0.120)
Race	0.029	0.154	0.094	0.160	0.064	0.129	0.171	0.214
	(0.177)	(0.183)	(0.168)	(0.171)	(0.181)	(0.184)	(0.187)	(0.191)
Party Identification	-0.075	-0.070	-0.102	-0.106	-0.084	-0.088	-0.077	-0.078
	(0.029)	(0.028)	(0.027)	(0.029)	(0.030)	(0.031)	(0.029)	(0.030)
Union Member	0.576	0.530	0.596	0.506	0.597	0.514	0.552	0.467
	(0.157)	(0.158)	(0.158)	(0.160)	(0.164)	(0.166)	(0.165)	(0.166)
# of Observations	1498	1590	1626	1536	1435	1345	1524	1441

Appendix Table A5
Estimated Effect of Varying Regressors on the Probability of Supporting Immigration Restrictions

	1992		1994		1996	
Regressor	Model 1	Model2	Model 1	Model 2	Model 1	Model 2
Occupation Wage	-0.086		-0.337		-0.201	
	(0.031)		(0.050)		(0.047)	
Education Years		-0.126		-0.112		-0.085
		(0.029)		(0.019)		(0.017)
Gender	-0.013	-0.010	0.008	0.029	-0.008	0.008
	(0.019)	(0.018)	(0.020)	(0.020)	(0.023)	(0.021)
Age	-0.012	-0.047	0.006	-0.035	0.067	0.033
	(0.024)	(0.025)	(0.027)	(0.028)	(0.029)	(0.031)
Black	-0.071	-0.078	-0.086	-0.080	-0.096	-0.095
	(0.028)	(0.027)	(0.035)	(0.034)	(0.038)	(0.036)
Hispanic	-0.009	-0.029	-0.123	-0.138	-0.053	-0.067
	(0.039)	(0.040)	(0.052)	(0.053)	(0.045)	(0.047)
Immigrant	-0.065	-0.061	-0.081	-0.072	-0.087	-0.081
	(0.026)	(0.027)	(0.030)	(0.031)	(0.034)	(0.035)
Party ID	0.007	0.016	-0.008	-0.003	-0.032	-0.022
	(0.025)	(0.025)	(0.021)	(0.021)	(0.023)	(0.022)
Ideology	0.095	0.086	0.071	0.056	0.108	0.099
	(0.039)	(0.041)	(0.035)	(0.038)	(0.031)	(0.033)

Notes: Using the estimates from Model 1 and 2, we simulated the consequences of changing each regressor on the probability of supporting immigration restrictions. Each continuous variable we changed from its mean to its maximum; each dichotomous variable we changed from zero to one. The mean effect is reported first, with the standard error of this estimate in parentheses.