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ABSTRACT

This paper provides new evidence on the determinants of individual trade policy preferences using an individual-level data set identifying both stated trade policy preferences and potential trade exposure through several channels for the United States in 1992. There are two 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. The result suggests that there is high intersectoral labor mobility in the United States over the time horizons relevant to individuals when evaluating trade policy. 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. To the extent that trade policy is like other government policies which affect citizens by changing relative product prices, our findings have implications for how individuals form preferences over a wide range of economic policies.

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1 Introduction

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."

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.

These standard frameworks do have some limitations, however. One is that they 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 trade-policy preferences and asset ownership the standard frameworks might need extending--particularly for assets that are neither currently employed factors nor currently produced goods.

Another limitation is that 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."

In this paper we provide new evidence on the determinants of individual trade-policy preferences and on what these preferences imply about the degree of factor specificity. We use a direct measure of these preferences obtained from the 1992 National Election Studies (NES) survey, an extensive survey of current political opinions based on an individual-level stratified random sample of the U.S. population. Our direct measure is responses to a question asking about support for new U.S. trade barriers. The survey also reports each respondent's occupation, industry of employment, home-ownership status, and county of residence. We construct a data set with several plausible measures of "exposure" to freer trade across factor types, industries, asset ownership, and counties. Merging this information with the NES survey, we generate an individual-level data set identifying both stated trade-policy preferences and potential trade exposure through several channels. We then evaluate how these preferences vary with individual characteristics that trade theory predicts might matter.

We present two main empirical results. First, we 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 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. The result suggests that there is high intersectoral labor mobility in the United States over the time horizons relevant to individuals when evaluating trade policy.

Our second main empirical result 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. We find 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 trade models, in reality preferences also depend on asset values.

There are five 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. The following section discusses the data and our model specifications. Section 5 presents the empirical results, while Section 6 concludes.

2 Literature Survey

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.[1] 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, 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--

[1] 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.

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.

All these studies provide valuable information on trade-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, all the studies cited here (except for Balistreri (1997)) infer from some observed political actions--coalition formation, lobbying, or voting--something about trade-policy preferences. These indirect preference measures face the important limitation of being endogenous outcomes of the interaction between trade-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. Epstein and O'Halloran (1996) provide a theoretical and empirical analysis of how political parties can affect the aggregation of interests. More generally, Alt and Gilligan (1994) analyze how given trade-policy preferences can lead to a wide range of political cleavages based on the institutional framework within which preferences are aggregated.

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." Our work is one of the first studies to avoid this problem by using a direct measure of trade-policy preferences.

A second important improvement of our study 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.

Our study's third major improvement is in data quality. We briefly highlight five strengths of our data. First, as just discussed, this study is one of the first to measure trade-policy preferences directly rather than indirectly through policy actions. Second, our units of observation are individuals, not more-aggregated geographic units such as counties. This accords with many political-economy models where individuals are the basic units whose preferences ultimately motivate any trade policies. Third, we have sufficient data to test directly against each other the alternative RV and HO models. Balistreri (1997), Rogowski (1987, 1989), and Midford (1993) all test whether the data are consistent with an HO story, but they never benchmark this against the performance of an RV story. Fourth, 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 systematically accounted for. Finally, our explanatory variables are also theoretically appropriate. 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. Beaulieu (1996) uses two dummy variables and Irwin (1996) uses five class categories.

Overall, this study contributes to the existing literature in at least three important ways: by measuring trade-policy preferences directly rather than indirectly; by analyzing trade's effect on assets as well as income; and by using higher-quality measures of factor type and industry.

3 Theoretical Framework

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 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 the incomes of factors 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 very different factor-income changes from--and thus preferences about--trade liberalization.

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 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.[2]

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-

[2]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.

advantage sectors should support freer trade while those employed in comparative-disadvantage sectors should oppose it. This suggests that empirical evidence on how well skill levels and industry of employment explain individuals' trade-policy preferences might provide support for one model over the other.

There are two important caveats to the theory just summarized. One caveat is nontraded industries, 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.

The second caveat is that 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. In reality, then, the two models might coexist with each being 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.[3]

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[3]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.

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.[4]

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[4]The Census Bureau (1997) estimates that on July 1, 1996 the total U.S. housing stock was 110 million housing units. For

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.[5] Even though our data cover only housing, this is the single most important asset for a significant share of the population.

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 and thus 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

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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.

[5] 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%.

leaving the local labor force either for work elsewhere or, at least temporarily, unemployment. This reduces housing demand and thus housing prices.[6]

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.[7]

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.[8]

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.

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[6]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.

[7]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.

[8]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).

4 Data Description and Empirical Specification

A convincing empirical test of the theoretical determinants of individual trade-policy preferences requires measures of policy preferences and trade exposure, consistent with the hypotheses outlined above, all at the level of the individual. We develop such a test by combining 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 test how our various measures of trade exposure affect individual trade-policy preferences.

4.1 Data Description

We measure policy preferences by responses to the following question asked in 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.[9]

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.

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

[9] 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.

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.[10] 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.[11] We concorded these tariff 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 in Section 3.1.

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10 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.

11 We thank Michael Ferrantino at the ITC for helping us obtain these data.

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.[12] First, using the ITC tariff data described above, we identified the ten two-digit SIC manufacturing industries with above-median tariff rates in 1992.[13] 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 total).[14] 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

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[12]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.

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

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

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.

4.2 Missing Data and Multiple Imputation

Upon constructing the variables described in Section 4.1 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. 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 list-wise 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.

The first step in our multiple-imputation procedures was to impute missing observations for *County Exposure 1* and *County Exposure 2*. We based our imputations on 46 county-level variables selected from the COM based on their sample correlation with *County Exposure 1* and *County Exposure 2* (which were two of the 46). For example, we selected county employment in textiles because it had one of the highest sample correlations with our county-exposure variables. In general, our 46 variables were various measures of factor endowments and economic output such as educational attainment and employment by industry. Altogether we imputed 10 complete county data sets.

The exact algorithm used for the imputations is a data augmentation method known by the acronym "IP" because it involves two key steps: the imputation step and the posterior step. The goal of the imputation procedure is to estimate a set of parameters (means and variance/covariances of the 46 variables) that can be used to create the 10 imputed data sets. IP employs an iterative sampling scheme where in the first step imputations are drawn from the conditional predictive distribution of the missing data. This distribution depends on the observed data and the assumed or current value of the complete data parameters. In the second step, a new value of the complete data parameters is drawn from its posterior distribution, which is conditioned on the observed data and the current values of the imputations for the missing data. Repeating this iterative sampling scheme produces stochastic subsequences that converge on the stationary predictive distribution for the missing values and the stationary posterior distribution of the complete data parameters.[15]

For the county data set we ran 5000 preliminary iterations of IP and then ran 1000 more to create an imputed data set every 100 iterations of these last 1000. The preliminary iterations ensure that sequences have converged to their stationary distributions. After creating the 10 complete county data sets we merged the NES survey data (including our constructed skill measures and industry measures) with *County Exposure 1* and *County Exposure 2*. The resulting 10 data sets still had substantial amounts of missing individual-level data, however. Consequently, for each of these 10 data sets we ran separate iterations of IP in order to impute values for the missing survey data. We found that 2100 preliminary iterations were more than sufficient for these data sets. An imputation was saved on the last iterations of each of the 10 cases to create our 10 final data sets with no missing data at all. Each of these final data sets contains 1736 observations, equal to the actual number of individuals in the NES survey either supporting or opposing more trade restrictions.[16] Also, each data set contains the exact same non-imputed information (i.e., all observations for the variable *Trade Opinion* plus the non-imputed observations for all the trade-exposure variables). They differ only in their imputed values for missing data.

The second step in our multiple-imputation analysis was to run various logit models separately on each of the 10 final data sets. The last multiple-imputation step was to combine the 10 sets of estimation results to obtain a single set of estimated parameter means and variances. The single set

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[15] See Section 3.4 in Schafer (1997) for a complete description of IP. Also, our methodology assumes that all variables are normally distributed. To make the data fit this assumption more closely we redefined each county-level variable to equal the natural log of the variable plus one.

[16] All the main results reported in this paper are qualitatively the same for the case where imputations are also made by treating as missing data the fact that some respondents did not express a trade-policy opinion (i.e., they chose the option "haven't thought much about this"). For this analysis the multiple-imputation procedures created 10 data sets of 2485 observations equal to the total number of respondents in the NES survey.

of estimated means is simply the arithmetic average of the 10 different estimation results. The single set of estimated variances consists of two parts. The "within" component is simply the arithmetic average of the 10 estimated variances. This accounts for the ordinary within-sample variation. The "between" component is the variance of the estimated parameter means among the imputed data sets. See Section 4.3 in Schafer (1997) for a complete description of the last multiple-imputation step.

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.[17]

4.3 Econometric Model

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 $E(\text{Trade Opinion}_i) = \Pr(\text{Trade Opinion}_i = 1|\pi_i) = \pi_i$ 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.

$$(1)\quad \pi_i = \frac{1}{1+\exp(-x_i\beta)}$$

In this equation x_i is a vector of individual-specific explanatory variables hypothesized to affect the probability of supporting trade restrictions and β 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.

[17]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.

The theory discussed in Section 3 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. These specifications directly test the HO model against the RV model. 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. Here is the hypothesized sign for the regressors specified in x_i.

Regressor	Hypothesized Sign
Occupational Wage	negative
Education Years	negative
Sector Tariff	positive
Sector Net Export Share	negative
*County Exposure 1 * House*	positive
County Exposure 1	zero
*County Exposure 2 * House*	positive
County Exposure 2	zero

5 Empirical Results

5.1 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 are reported in Table A1 in the appendix. However, these coefficients 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 2 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, recall from Section 3 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 2, 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.

To visualize these key results we constructed Figures 1 and 2. Figure 1 plots a smoothed histogram of all 1000 differences calculated for changes in *Occupational Wage* based on Model 5. The histogram lying entirely to the left of zero clearly shows that increasing skills reduces support for trade protection. Figure 2 plots a smoothed histogram of all 1000 differences calculated for changes in *Sector Net Export Share* based on Model 6. Comparing this histogram with Figure 1 shows the relative uncertainty of the effects of increasing industry trade exposure. The mean effect is much smaller in absolute value and it is much less precisely estimated. Increasing *Sector Net Export Share* has the hypothesized effect only 88% of the time, and it is important to note that this case is the *most* precise one for all the industry estimates in Table 2. In many other cases the direction of change is basically a coin flip.

The key message of Table 2 and Figures 1 and 2 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 indicate that individuals care about how trade policy affects their factor income in a manner consistent with an HO model but not with an RV model. This suggests that there is high intersectoral labor mobility in the United States over the time horizons relevant to individuals when evaluating trade policy.

5.2 Testing How Asset Ownership Affects Trade-Policy Preferences

Tables A2 and 3 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, Table A2 reports the actual coefficient estimates from Models 9 through 16. Table 3 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 3 for skills and industry of employment are substantially the same as the results in Table 2. 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 3, 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.

Figure 3 visualizes this key result by plotting a smoothed histogram of all 1000 differences calculated for changes in *County Exposure 1* based on Model 10. The histogram lying almost entirely to the right of zero clearly shows that increasing county trade exposure increases support for trade protection for homeowners.

5.3 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.[18]

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 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.[19] For industry trade exposure we tried imports as a share of output. Although imports do not

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[18] 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.

[19] 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).

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.

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.

In addition to robustness checks involving homeownership we also tried a number of other variables. Specifically, we reran several of our models modified by including age, gender, party identification, past voting, race, retrospective evaluations of U.S. economic performance, and union membership. Age, past voting behavior, and race had no consistently significant effect. In contrast, those significantly more likely to support trade barriers included the following groups: women; those who identify more strongly with the Democratic party; those who thought the economy had done poorly in the past year; and union members. Controlling for all these variables, however, did not significantly change either of our key results that factor exposure explains trade preferences much more than sector exposure or that homeownership in trade-exposed counties matters as well.[20]

6 Conclusion

In the literature on the political economy of trade policy it has been long known that individuals' preferences over trade policy play a central role. Theory shows quite clearly that different preferences lead to different trade-policy outcomes. But a major limitation of this literature has been a lack of clear evidence about what these preferences actually are.

In this paper we have provided new evidence on the determinants of individual trade-policy preferences. To do this we constructed a new data set covering the United States in 1992. This is an individual-level data set identifying both stated trade-policy preferences and potential trade exposure through several channels. The data's comprehensiveness allowed us to test directly against each other the factor-income predictions of the Heckscher-Ohlin and Ricardo-Viner models, the two most commonly used frameworks in the literature. We were also able to go beyond the factor-income focus of these models to test whether asset ownership matters as well. We generated two main empirical results.

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. The result suggests that there is high intersectoral labor mobility in the United States over the time horizons relevant to individuals when evaluating trade policy.

[20] 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 test of our interpretation is to see if these 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.

Our second main empirical result 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. 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 trade models, in reality preferences also depend on asset values.

Our findings are based on people's opinions about trade policy. But many other government policies such as exchange-rate and antitrust policies also affect citizens by changing relative product prices. As with trade policy, precisely how individuals form preferences over these other policies has long been a subject of controversy. Our research strongly suggests that individuals' preferences over these types of policies are determined primarily by their factor type and their asset ownership, not their industry of employment.

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Table 1
Summary Statistics

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. The variable *Occupational Wage* reports the actual wage divided by 1000.

Table 2
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 Wage	-0.074				-0.074	-0.074		
	(0.012)				(0.012)	(0.013)		
	[-0.095, -0.053]				[-0.094, -0.055]	[-0.097, -0.054]		
Education Years		-0.132					-0.132	-0.133
		(0.015)					(0.016)	(0.016)
		[-0.158, -0.107]					[-0.158, -0.106]	[-0.161, -0.106]
Sector Tariff			0.012		0.011		-0.001	
			(0.012)		(0.013)		(0.012)	
			[-0.010, 0.032]		[-0.011, 0.032]		[-0.021, 0.019]	
Sector Net Export Share				-0.014		-0.014		0.000
				(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 3
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.

Figure 1
Estimated Effect of Increasing Occupational Wage
From its Mean to One Standard Deviation above its Mean
(Model 5 Estimates)

We simulated the consequence of this change and plotted the results as smoothed histograms, called kernel density plots. In our simulation, an increase from the mean to one standard deviation above the mean reduced the probability of supporting trade restrictions on average by 0.073.

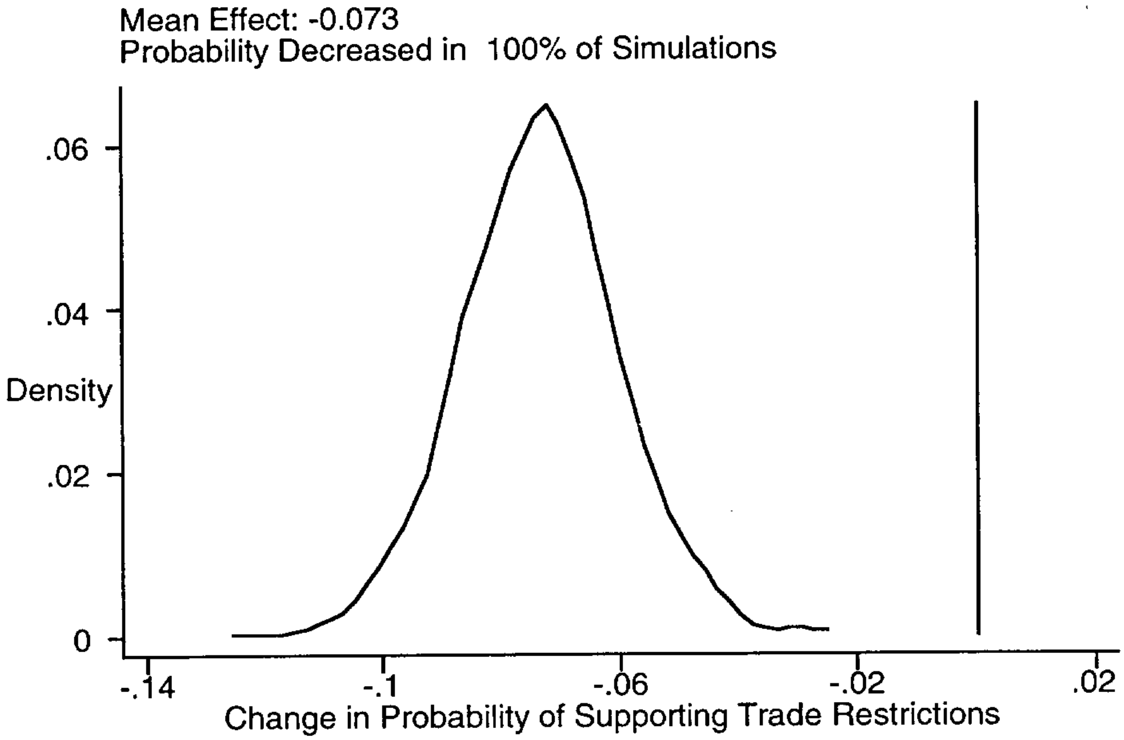


Figure 2
Estimated Effect of Increasing Sector Net Export Share Measure From its Mean to One Standard Deviation above its Mean
(Model 6 Estimates)

We simulated the consequence of this change and plotted the results as smoothed histograms, called kernel density plots. In our simulation, an increase from the mean to one standard deviation above the mean reduced the probability of supporting trade restrictions on average by 0.015.

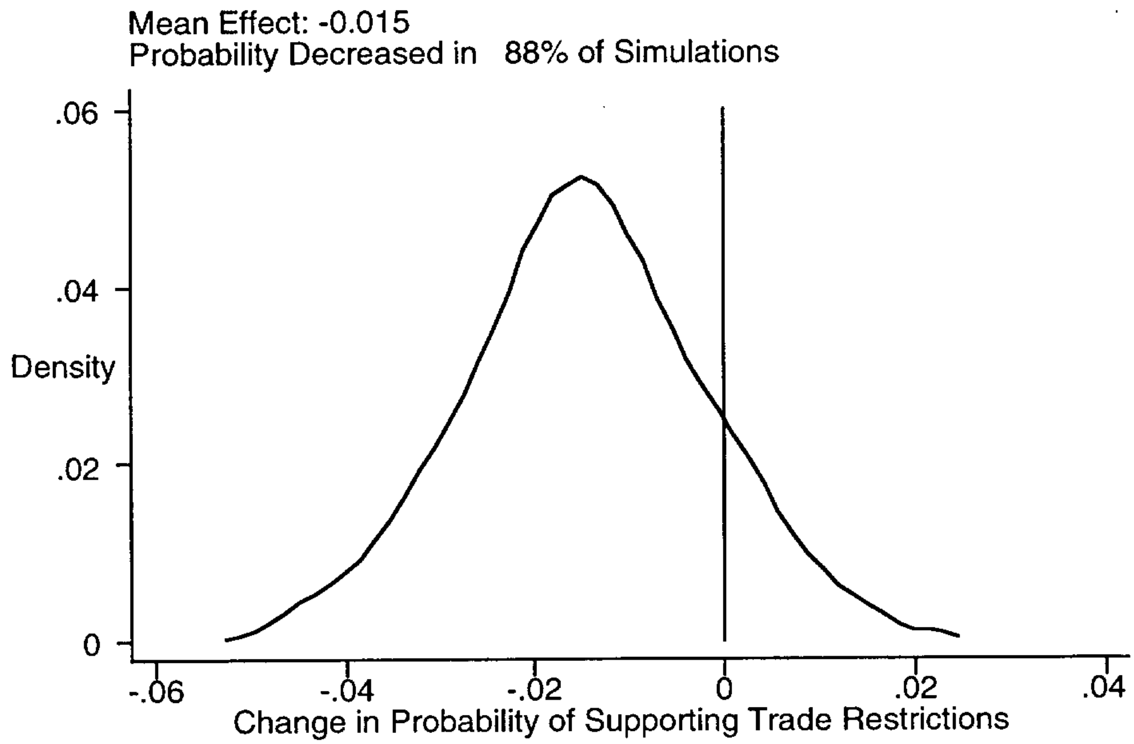
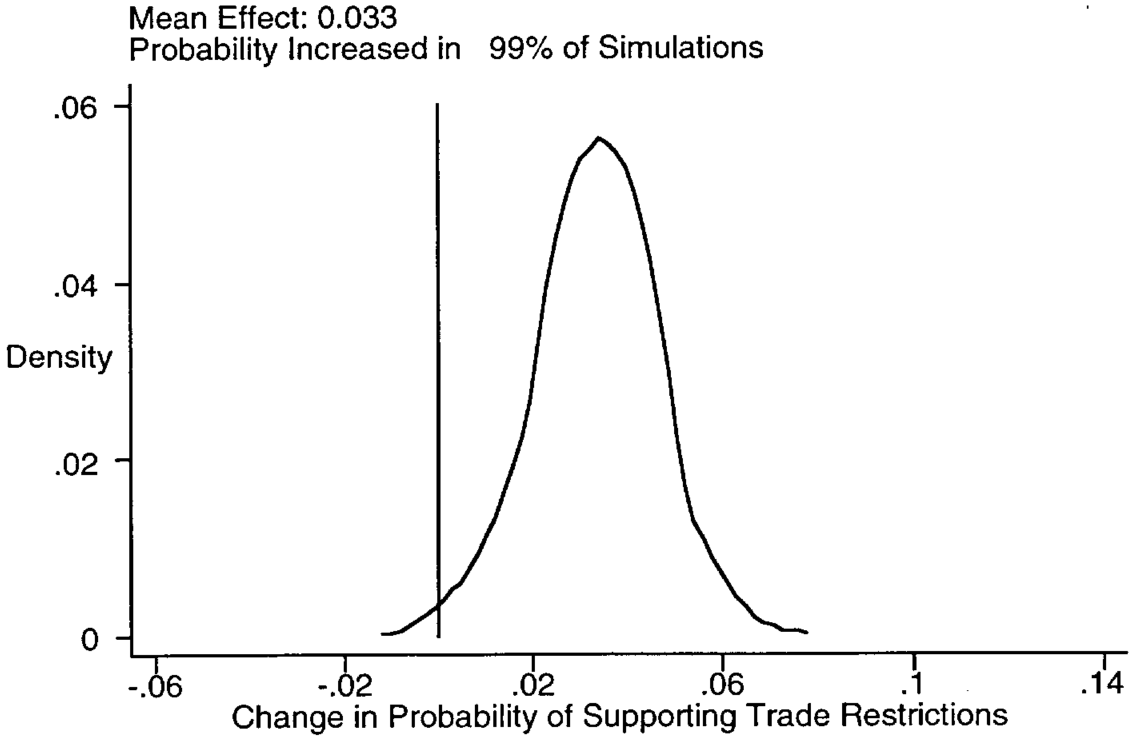


Figure 3
Estimated Effect of Increasing County Exposure 1
From its Mean to One Standard Deviation above its Mean
If Respondent Is a Homeowner
(Model 10 Estimates)

We simulated the consequence of this change and plotted the results as smoothed histograms, called kernel density plots. In our simulation, an increase from the mean to one standard deviation above the mean increased the probability of supporting trade restrictions on average by 0.033.



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 Share				-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 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