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VOTING ON A TRADE AGREEMENT: FIRM NETWORKS AND ATTITUDES TOWARD OPENNESS

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ABSTRACT

We exploit a natural experiment to study the extent to which popular attitudes toward trade are driven by economic fundamentals. In 2007, Costa Rica put a free trade agreement (FTA) to a national referendum. With a single question on the ballot, 59% of Costa Rican adult citizens cast a vote on whether they wanted an FTA with the United States to be ratified, or not. We merge disaggregated referendum results, which break new ground on anonymity-compatible voting data, with employer-employee, customs, and firm-to-firm transactions data, and data on household composition and expenditures. We document that a firm's exposure to the FTA, directly and via input-output linkages, significantly influences the voting behavior of its employees. This effect dominates that of sector-level exposure and is greater for voters aligned with pro-FTA political candidates. We also show that citizens considered the expected decrease in consumer prices when exercising their vote. Overall, economic factors explain 6% of the variation in voting patterns which cannot be accounted for by non-economic factors such as political ideology, and played a pivotal role in this vote.

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1 Introduction

Survey evidence suggests that economists and the broader public view trade issues in starkly different ways (Blendon et al., 1997; Sapienza and Zingales, 2013), and given the importance elected officials grant to public attitudes about trade policy, an understanding of the possible correspondence between public sentiments and economic determinants can be consequential. Moreover, analyzing the determinants of public attitudes toward trade openness can, in turn, inform economic theory and the study of a country's gains from trade and its distributional effects.

This paper studies the extent to which popular attitudes about trade reflect economic fundamentals. This topic is challenging to study, as popular attitudes about economic issues like trade are typically unobservable. To overcome this challenge, we exploit a natural experiment: In 2007, Costa Rica was the first developing country to put a free trade agreement (FTA) to a national referendum. With only one question on the ballot, 59% of all Costa Rican adult citizens voted on the ratification of an FTA with the U.S. (hereafter, CAFTA). This referendum on opening the country's trade policy represents a unique opportunity to observe voting choices that had clear economic consequences for voters. Further, the setting allows for an analysis with unprecedented data quality, which has the promise of setting a new gold standard for empirical work on voting and trade while breaking ground on previously unexplored questions.

Delving further into the specifics, although CAFTA included several countries—the U.S., Central America, and the Dominican Republic—the discussion in Costa Rica was centered around the U.S.[1] This policy decision was consequential to voters, as the U.S. had been Costa Rica's main trading partner for years, accounting for 45% of Costa Rica's imports and exports. The vote was extremely close, with 51.23% of the voters in favor of ratification.

The data available lies at the edge of what is feasible with voting records while respecting confidentiality. In Costa Rica, each voter is allocated by place of

[1] Tariffs with Central America and the Dominican Republic were not part of the FTA. CAFTA was an FTA between the U.S. and each other country individually—Costa Rica, El Salvador, Guatemala, Honduras, Nicaragua, and the Dominican Republic.

residence to a voting center, which is usually housed in a school. Within voting centers, voters are allocated to voting boards, which usually correspond with classrooms, alphabetically. On average, 500 citizens are assigned to each voting board. We obtained official records of voting outcomes by voting board, along with the list of unique national identifiers for each individual voter and the voting board to which she was assigned. We merge these unique national identifiers with employer-employee data, information about employee characteristics (occupation, wage, age, gender, etc.), firms' balance sheets and customs records, and firm-to-firm transactions data. From this rich dataset, we construct a mapping from the disaggregated voting results to individual firms. This mapping allows us to measure the relationship between economic forces and voting outcomes and puts us in a unique position to test whether some observable characteristics of workers are systematically related to their voting choices. We go further and use the identity of each voter's partner (husband or wife) to measure, not only individual exposure, but to construct exposures from the household's perspective. The available data allows us to match 41% of adult citizens to a firm directly, and 52% of households to a firm once we exploit the information on partners.

Armed with the experimental setup and the data, the paper is then divided into three sections which conduct analyses at the voting-board level. The first two sections explore the role of economic fundamentals while distinguishing between the *income* channel and the *expenditures* channel. The third section studies non-economic factors with an emphasis on the influence of *political ideology*. Then, we compare the relative importance of each factor in explaining voting behavior.

A study of the *income* channel depends on the model of real income which one has in mind—a voter's vote can depend, for instance, on whether her employer, industry, skill group, or local labor market were exposed to the tariff changes implied by the trade agreement. Our analysis of this channel uses the role of employers as a benchmark, as we can measure it very precisely and the study of the role of firms is novel. In particular, we explore how a firm's dependence on international trade shapes its employees' attitudes toward openness via (i) firm *direct* trade exposure, which depends on the products the firm is trading

(exporting and importing) with the U.S. and the expected change in the tariffs on those products; and (ii) *indirect* firm-to-firm exposure, whereby an employer is exposed via trading partners who are themselves directly exposed. To the best of our knowledge, this is the first study of the role of within-industry heterogeneity in shaping popular support using information about all firms and documenting the salience of an indirect exposure measure via input-output linkages.

We find that firm-level exposure is salient to voters. In particular, a $1,000 increase in revenue for the employers of individuals at a voting board is associated with a 3.4 percentage points (pp) increase in the share of votes in favor of the FTA at that board.[2] Indirect exposure for firms that are one link away from a directly exposed firm also matters to voters; the coefficient for indirect exposure is approximately two-thirds the size of the one for direct exposure. While we cannot completely rule-out confounding factors that might affect both individuals' selection of jobs and their voting choices on the referendum, a battery of placebo tests and robustness exercises shows that selection of workers into firms played a limited role in driving the effect that we documented.

We document that the salient role of firms persists after accounting for other factors which might affect voters' earnings. In particular, we consider the role of industries, occupations, local labor market import competition, and expectations about future job opportunities. We find that a worker's industry plays a limited role conditional on firm exposure. This result highlights the importance of within-industry heterogeneity in determining the distributional effects of trade. We document that low-skill workers are significantly more likely to vote *against* the FTA. Moreover, commuting zones more exposed to import competition are *less* likely to vote in favor of the FTA. Finally, we find a limited role for expectations playing a role in shaping votes conditional on exposure, which could relate to expectation formation being difficult in the presence of uncertainty or discounting of future outcomes.

Next, we focus on the *expenditures* channel. The FTA would lead to relatively

[2]According to estimates by Alfaro-Ureña et al. (2021), this increase in sales would translate into a $90 wage increase for each worker.

lower consumer prices for at least some goods. This is another channel that voters may have been considering when deciding about the FTA. To measure each voter's exposure through changes in expenditures, we rely on the National Household Income and Expenditure Survey, which asks households how they spend their income across goods and services in a detailed consumption basket. The survey data is rich in respondent characteristics—including income, occupation, location, gender, age, and marital status—and allows us to map a consumption basket to a household based on this set of characteristics, which we observe both in the survey and for each voter. In the spirit of Fajgelbaum and Khandelwal (2016), we then estimate the expected change in the price of this basket from the share of each good or service that is imported from the U.S. and the expected change in its tariffs. We find that both the earnings and expenditure channels are salient to voters: a \$8.3 decrease in the price of a voter's consumption basket increases her probability of voting in favor of the FTA by 1 pp.

We then study the role of non-economic factors, with an emphasis on the role of voters' political inclination. In line with a long literature on political science, we find that political ideology is highly significant; a 1 pp increase in the share of voters at a voting board who align with a pro-FTA political party is associated with a 0.5 pp increase in the share of pro-ratification votes. Moreover, political views interact with trade exposure in an interesting way; we find that high trade exposure is significantly more salient for voting boards composed of voters affiliated with political parties that support free trade. Conversely, we document that voters with political views *against* the FTA are less sensitive to trade exposures that might impact their earnings. The latter results hold after implementing an IV strategy to isolate how the FTA might have influenced voters' choice of party.

Finally, we conduct a broad comparison of the importance of different factors. To do so, we compare partial R^2 across a series of regressions to grasp what percent of the variation in voting behavior can be attributed to each factor. Aligned with the results of the previous paragraph, we find that political alignment plays a relatively important role, accounting for 10% of the variation which cannot be

explained by other factors. However, we can also verify that economic factors play a non-negligible role, explaining 6% of the observed variation in voting behavior which cannot be explained by non-economic factors. Thus, economic fundamentals were particularly key in this setting, in which the referendum was approved with a slim lead in votes, and more generally, might play paramount significance in closely contested elections.

2 Related Literature and Contribution

Our work contributes to the literature in economics and political science that asks whether individuals' policy preferences reflect economic principles. This question is fundamental to the assessment and modeling of trade's welfare implications. Using public opinion polls and surveys, early studies suggested that popular attitudes about trade tend to align with economic self-interest and the predictions of standard trade models (Beaulieu, 2002; Mayda and Rodrik, 2005; O'Rourke et al., 2001; Osgood et al., 2017; Scheve and Slaughter, 2001). However, more recent survey-based studies contradict prior work, question that popular attitudes are connected with economic models, and consistently argue that attitudes toward openness depend mainly on ideology and social and cultural considerations (Hainmueller and Hiscox, 2006; Mansfield and Mutz, 2015; Rho and Tomz, 2017; Sabet, 2016), and are hard to change based on evidence (Alfaro et al., 2023). Our study contributes to this literature by analyzing a setting in which individual responses have concrete implications for trade policy, unlike the hypothetical settings of surveys. Further, as opposed to analyzing attitudes toward trade in general, we focus on a particular trade agreement, which admits clear theoretical predictions that we can measure and test precisely. Thus, documenting a non-zero result is, in itself, an important contribution to this debate.

The present study also builds on work that examines how economic openness impacts domestic politics in the U.S., including Autor et al. (2013), Che et al. (2016), Blanchard et al. (2019), Bombardini et al. (2020), and Autor et al. (2020). These papers mainly examine how the mid-2000s Chinese import surge, known

as the "China Shock," affected political polarization and voting in presidential and congressional elections. Earlier work by Irwin (1994) and Irwin (1995) also analyzed how election outcomes depended on attitudes about trade. In contrast with these studies of presidential or congressional elections, in which voters were deciding on large sets of issues, our design allows us to isolate tariffs' effects on voter decisions, specifically about trade policy. Further, while a standard approach in the literature is to adopt a shift-share approach based on industry composition at the county level, our data allows us to highlight the importance of within-industry heterogeneity and individual firms in explaining voter behavior using precise relationships between disaggregated results and firms.

In a sense, the findings of the survey-based and election-focused papers described above seem to contradict each other, with the former often arguing that popular attitudes are unaffected by economic factors and the latter arguing that trade shocks have a great effect on elections. The present work can help reconcile these perspectives. Our study, which unlike survey-based work observes trade attitudes directly through voting records, suggests that individuals might behave differently—and more selfishly—than what their responses to surveys might suggest. Decisions in the referendum have real and well-defined implications that we also observe, granting a unique perspective on popular attitudes about trade. Further, the paper documents the relevance of expected gains and losses for voters' employers in the FTA referendum. This finding connects the already established literature on the role of economic fundamentals for political outcomes with work in labor economics that shows that employers explain a great deal of an individual's labor market outcomes (Card, 2022) by showing that when voting on an economic policy, workers care about how that policy would affect their employer.

This paper also speaks to the political science literature. Related studies include Urbatsch (2013) and Hicks et al. (2014), who rely on surveys and census data to analyze how districts voted on the CAFTA referendum depending on their composition and political views, and Spilker et al. (2008), who study how exporting firms in Costa Rica changed their exports *after* CAFTA was ratified. Our study complements these works by exploiting disaggregated data at the levels

of voting boards, firms, and individuals, along with employer-employee links, to assess the importance of within-industry heterogeneity and economic and social conditions in explaining the vote.

Our work also contributes to the literature on the distributional effects of trade, by providing direct evidence about the relative salience of various economic factors in shaping individuals' attitudes. This literature usually focuses on either earnings or expenditures exclusively. Literature on the earnings channel, summarized by Goldberg and Pavcnik (2007), finds evidence inconsistent with the effects predicted by Stolper and Samuelson (1941), which would dictate that in countries in which low-skill workers are relatively abundant, wages should increase with trade. These studies usually focus on the analysis of sectors or skill groups. Contemporaneously, Stantcheva (2022) relies on surveys to show that individuals particularly care about adverse distributional consequences from trade. The present work complements these findings by highlighting the key role that individual employers play in shaping employee perceptions of gains and losses.

Studies of the expenditures channel have largely focused on the effects of trade on inequality, both using microdata and by exploiting major reforms in individual countries (Atkin et al., 2018; Faber, 2014; Porto, 2008), and leveraging theoretical frameworks to measure inequalities in gains from trade across consumers as in Fajgelbaum and Khandelwal (2016) and Borusyak and Jaravel (2019). Costinot and Rodríguez-Clare (2014) summarize the literature that quantifies aggregate welfare gains from trade. Our paper leverages the theoretical framework of Fajgelbaum and Khandelwal (2016), links consumption baskets to individual voters, and measures the perceived gains in earnings that voters expect after a pro-trade policy change. We can also compare the salience of the expenditures and earnings channels from the perspectives of both individuals and households.

The rest of the paper is organized as follows. Section 3.1 provides an overview of the setting, including details about the FTA and voting in Costa Rica. Section 3.3 presents details about the data used in our analysis. Section 4 and Section 5 are devoted to analyzing economic factors, and develop, respectively, the study of the income and expenditures channel. Section 6 explores the role of non-economic

factors, and provides a broad comparison between their relevance and that of economic fundamentals, and Section 7 concludes.

3 Background and Data

3.1 The Free Trade Agreement: CAFTA

In August 2004, the United States signed a free trade agreement—known as CAFTA–with Costa Rica, El Salvador, Guatemala, Honduras, Nicaragua, and the Dominican Republic. The agreement included large reductions in tariffs, along with provisions on intellectual property rights, on regulatory agreements (environmental regulation and investors protection), and on liberalizing specific markets which were previously monopolized by the government—the main markets, both in terms of their size and their saliency in the discussion, being the telecommunications (including internet provision) and insurance markets.[3]

In terms of tariff reductions, the matter at hand was quite relevant to workers in Costa Rican firms, as the U.S. was Costa Rica's main trading partner, accounting for 45% of the country's imports and exports, Costa Rica's trade-to-GDP ratio was 86%, and absent the FTA, tariffs for trade with the U.S. could considerably increase. The agreement implied zero tariffs for most of the goods and services traded with the U.S.[4] While most of these goods had zero tariffs by the time of the referendum, the U.S. pledged that if the FTA was not ratified, there would be no renegotiation and, Costa Rica would be expelled from its existing trade preference programs and from the Caribbean Basin Initiative.[5] Thus, a no-vote is more of a vote in favor of tariff increases rather than against tariff decreases. It is worth mentioning that, to the extent that voters are subject to gain-loss asymmetry, this matters in the interpretation of our results.[6]

[3]Note that these provisions can be relevant both for import competition and lower prices (both intimately linked).

[4]In particular, 95.9% of the tariffs on exports to the U.S., and 83.8% of tariffs on U.S. imports, would be zero as soon as the agreement was in effect.

[5]The counterfactual tariffs given a no-vote were printed on CAFTA's agreement for each HS-6 code.

[6]I.e., if people tend to feel the pain of a loss (of openness) more acutely than the benefit of

Table A.1 shows the average changes in export and import tariffs by industry, along with each industry's share in the country's total exports and imports in 2007.[7] The average export tariff, weighted by the importance of each product in total exports, was 3.1%; while the average import tariff, weighted by the imports of each product, was 3.4%. Moreover, the FTA has no expiration date, meaning that its ratification would also decrease uncertainty related to future tariff changes.

We have information on each person who was employed by the government (in general), and on each person who was employed in one of the government companies subject to the liberalization (in particular). Our main results always control for the share of people at each voting board who were government employees. The coefficient is largely negative, which aligns which severe pushback from government employees against the liberalization. We also have a robustness check where we control for the share of employees at the government companies that would start facing competition if the agreement was approved (on top of the control regarding government employees in general). Not surprisingly, the coefficient is both large and negative.

While CAFTA was signed in 2004, signing an FTA only means that the countries agreed on its terms, but it does not make it legally binding. Ratifying an FTA, on the other hand, is the stage in which the countries involved formally approve the agreement (after signing it) and make it legally binding. This stage involves going through the respective domestic legal processes of each country to ensure that the terms of the agreement are in line with their own laws and regulations. By late 2006, Costa Rica was the only country that had not ratified CAFTA due to delays in the vote of its Legislative Assembly, as the opposition delayed the vote on the agreement repeatedly, and the congress—split between opponents and supporters—was not able to get a majority vote on whether to ratify the FTA or not for the next two years. Thus, as a way to reach a decision before the ratification deadline, and after receiving approval from the Supreme

a gain of the same magnitude, then one would expect a vote for a reduction of tariffs to have a smaller impact on the measures of exposure which are positive (like firm exposure) and a larger one for measures of exposure which are negative (like import competition).

[7] As tariffs would be eliminated under the agreement, these changes correspond with the pre-FTA tariff levels.

Court, the government opted for an unusual route: Costa Rica would be the first developing country to conduct a national referendum to decide on the ratification of a trade agreement.

All adult citizens in the country could cast their vote, *with only one question on the ballot*: whether CAFTA should be ratified or not. Importantly, there was no other issue on the table for this referendum; Costa Ricans attended voting centers to manifest their opinion on this one matter only. Figure A.1 shows a sample of the referendum ballot. Despite the national referendum being about this issue only, participation was high; on October 7^{th} 2007, 59.2% of adult citizens cast a vote. The result of the vote was unexpected, yet undisputed; after newspapers and polls predicted a statistical tie, CAFTA was ratified with the support of 51.23% of the voters.

3.2 Voting in Costa Rica

In Costa Rica, citizens who are 18 years or older are eligible and automatically registered to vote. The logistics of Costa Rican elections are standard, but relevant to the disaggregation we discuss below. First, each eligible citizen is assigned to a voting center, which usually corresponds to a school, depending on her place of residence. Within the voting center, each voter is assigned to a voting board, which usually corresponds to a classroom, alphabetically depending on her last name. On average, approximately 500 people are assigned to vote at each voting board. This is the case for all presidential and municipal elections, and was used for both the presidential election in 2006 and the 2007 referendum. For the referendum, in particular, votes were cast across 4,932 voting boards distributed among 1,952 voting centers across the country. Figure A.2 depicts the spatial distribution of voting centers. This allocation usually does not change dramatically from one year to the next. In fact, most citizens who voted at a voting board in the 2006 election, voted at the same voting board in the 2007 referendum (exceptions mostly being citizens who died, turned 18, or changed their residence within that year). We will exploit this persistence in our empirical section, to isolate the effect of political alignment as a motif to vote in favor or against the referendum.

3.3 Data Sources

Voting and Referendum Results Data on the results of the referendum was obtained from the Supreme Electoral Tribunal of Costa Rica (*Tribunal Supremo de Elecciones de Costa Rica*). While the vote of each citizen is secret, we use data on the results of the referendum by voting board. Each voting board, on average, hosted approximately 500 voters.[8] Thus, although we do not know each person's vote, we observe how citizens voted up to a level of aggregation of only 500 individuals. Further, we also acquired lists with the unique national identifiers of voters at each voting board.[9]

National Registry We obtained family-network data from the Civil Registry of Costa Rica. This data allows us to identify if a citizen is married and to whom. This will be useful to estimate households' exposure to the FTA, especially for individuals who are not in the labor force, but who are married to someone who is employed. We will also use this data to understand whether the exposure of relatives can explain voting behavior.

Employer-Employee Records, Firm-to-Firm Transactions, and Customs We matched voters with their employers using data from the Costa Rican Social Security Fund, which tracks formal employment and labor earnings. This data also includes details on each employee, including her occupation, earnings, and employment history between 2005 and 2017. Importantly, informal workers make up a relatively small share of all workers in Costa Rica (27.4%), which is significantly below the Latin American average of 53.1% (ILO, 2002).

Data on firm-to-firm transactions in Costa Rica is collected by the Ministry of Finance, and is available between 2008-2017.[10] All private businesses and other

[8] If everyone eligible to vote had actually attended, each voting board would have hosted approximately 500 citizens.

[9] Although there were 4,932 voting boards in the referendum, the main analysis considers 4,914 because we exclude voting boards located within jails and on Cocos Island (a protected natural area located about 500 km from Costa Rican mainland). Table B.1 shows the results are robust to using all voting boards.

[10] Note that this dataset is available only starting in 2008. As the referendum occurred in October 2007— although it was not effective until January 2009—this forces us to use 2008 as a proxy for the 2007 domestic network.

entities in the economy, like individuals providing professional services independently and public enterprises, are required to report the amount transacted with every supplier and buyer with whom they generate at least 2.5 million Costa Rican colones—which are approximately 4,200 U.S. dollars—in transactions, along with a tax identifier. This data is key in the government's enforcement of tax law and tax collections, including the general sales tax and corporate income tax. This data can be merged with corporations' annual income tax returns, which cover the universe of formal firms in the country and contain typical balance sheet variables, including sales, input costs, and net assets.

Further, we link each firm's identifier with customs records, which are available for the period 2005-2017, and which we use to track the individual foreign transactions made by each firm. Each transaction, both for imports and exports, includes a 6-digit HS code, along with data on the amount transacted, the quantity traded (and thus, the price), and the country of origin or destination. This data also allows us to identify firms operating within a Special Economic Zone.

CAFTA and Tariff Changes We digitized the tariff changes directly from the CAFTA's text approved by the Special Commission of International Affairs and Foreign Trade of the Legislative Assembly, published in the Alcance No. 2 of La Gaceta—the country's official newspaper—on January 26$^{\text{th}}$, 2007. That is, the text that was to be ratified by the referendum (see Figure A.1 in Appendix A). Besides tariff changes, the agreement also includes a schedule for the timing with which old tariffs would converge to new ones.[11]

4 Income Channel

An FTA can affect individuals by changing their income. In turn, this effect depends on what the boundaries for factor markets are, and the model of real

[11] While most tariffs are ad-valorem, a few are ad-quantum. For these, we use the good's average price (which is available from customs data) and calculate the ad-quantum tariff as a percentage of this price, to make it comparable to ad-valorem tariffs. Most tariffs immediately converge to zero (over 96% of them, both in terms of their number and their value); for the rest, the change to zero is staggered.

income considered. For example, the relevant factors defining changes in a worker's income might be her firm, her industry, her occupation, the sectorial composition of the commuting zone where she lives, or even her expectations about future job opportunities. All these economic factors could affect a voter's position through the income channel. In this section, we will analyze each factor using the firm's exposure as our baseline, as this is a factor that we can measure particularly well and that has been largely unexplored by the literature, and we aim to determine if an employer's exposure remains relevant after accounting for other economic forces. In particular, the next subsection constructs measures of: firm (direct and indirect) exposure, exposure by sector, exposure by occupation or skill, local labor market import competition, and expectations about future job opportunities.

4.1 Income Channel: Measures of Exposure

Direct Firm-Level Exposure to the FTA Recent models of firm heterogeneity imply that trade might affect employment and wages. The literature has proposed several channels by which this might be the case. Helpman et al. (2010) and Helpman et al. (2016) discuss how rent-sharing between workers and firms might cause wages to vary along with firm revenue and generate an export wage premium, mention that importing can generate a wage premium at importing firms insofar as imports increase productivity and revenue per worker. Thus, changes in trade costs, like tariffs, can affect worker welfare via earnings. Besides rent-sharing, which can occur via bargaining or wage posting (Hall and Krueger, 2012; Manning, 2013; Mortensen and Pissarides, 1999), alternative mechanisms include efficiency wages (Amiti and Davis, 2011; Davis and Harrigan, 2011; Egger and Kreickemeier, 2009) and assortative matching (Burstein and Vogel, 2010; Bustos, 2011; Verhoogen, 2008; Yeaple, 2005). As for empirical results, recent work by Alfaro-Ureña et al. (2021) has shown how the rent-sharing mechanism is relevant in the Costa Rican case, and particularly so for firms engaged in trade with foreign countries. Alfaro-Ureña et al. document that when multinational firms expand, their direct and indirect suppliers are affected, and incumbent workers' salaries increase because of rent-sharing. This evidence leads us to derive measures of firm

exposure that would be relevant to employees' economic interests, assuming that they are employed under a rent-sharing scheme.

Thus, to analyze the role of firms, we calculate measures of each Costa Rican firm's exposure depending on the changes to the firm's revenue and cost functions that would come along with the tariff changes in CAFTA.[12] Namely, Helpman et al. (2016) show that a firm's wage bill is a constant share of its revenue, which is the sum of sales across all the markets in which the firm sells.[13] This measure is consistent with the one developed by Dhyne et al. (2021) for both exports and imports. Thus, given the change in tariffs resulting from the FTA, the change in the wage bill of firm i would then be an increasing function of the change in its gains in profit, such that, for a firm paying wage w_i:

$$Exp_i^{Trade} = \sum_{j=1}^{n} \frac{X_{ji}^{US}}{L_i} \Delta\tau_j^{US,X} + \frac{M_{ji}^{US}}{L_i} \Delta\tau_j^{US,M} \propto \Delta w_i, \tag{1}$$

where X_{ji}^{US} represents firm i's sales of product j in the U.S., $\Delta\tau_j^{US,X}$ stands for the expected percentage change in tariffs for product j which is exported to the U.S., M_{ji}^{US} are firm i's purchases of product j from the U.S., and $\Delta\tau_j^{US,M}$ represents the expected change in import tariffs from the U.S. for product j if the agreement were to be ratified.[14] We normalize this exposure by each firm's number of employees (L_i), which would be consistent with the amount that a change in profits would affect a single worker under a rent-sharing scheme—hence the last expression of equation (1), $Exp_i^{Trade} \propto \Delta w_i$. In fact, Alfaro-Ureña et al. (2021) find that, in the case of Costa Rica, each extra dollar of value added per worker increases wages by 9 cents. This measure of a firm's exposure via input-output linkages to the U.S.

[12] To do so, we rely on the tariff changes by 6-digit HS code that are printed on CAFTA's agreement.

[13] While Helpman et al. (2016) focus on exports, we consider both exports and imports. This addition to our focus is consistent with their discussion of how importing increases productivity, which in turn increases revenue per worker and could lead to an importer wage premium.

[14] We consider imports of both inputs and final goods in this measure. Note that, later on when we use this measure in a regression, a sufficient condition for a Bartik-like strategy is for the product-specific tariff changes experienced at the national level to be uncorrelated with the regression's error terms (Borusyak et al., 2021), which is likely as over 95% of the changes in tariffs depend on the difference between: (i) zero (under the FTA) and (ii) Most-Favored Nation (MFN) tariffs (if the FTA is not ratified).

leverages our data about each firm's balance sheets, customs transactions, and the expected changes in tariffs due to CAFTA. Figure A.6 in Appendix A summarizes the variation in this measure across space. When examining correlations, we find that younger, male, and richer individuals tend to have higher firm trade exposure. While equation (1) proposes a compound measure, we will later on decompose it into exports and imports.[15]

Indirect Firm-Level Exposure to the FTA Our measures of each firm's *indirect* exposure to the trade agreement rely on data about firm-to-firm transactions. In particular, we differentiate between the number of links that separate a firm from the shock, and how the shock influences employees' response to the firm's exposure. This construction proceeds in steps. We first calculate indirect exposure for firms that are at most *one link away* from a directly exposed firm. A firm can be linked to another in the network as a seller or as a buyer, and we follow a logic similar to that of the previous section in the calculation:

$$IndirectExp(1)_i^{Trade} = \sum_{k=1}^{K} \left(\frac{R_{ki}}{R_i} + \frac{C_{ik}}{C_i} \right) \frac{L_k}{L_i} Exp_k^{Trade}, \tag{2}$$

where we sum across all firms k to which firm i is selling (buying), and $\frac{R_{ki}}{R_i} \left(\frac{C_{ki}}{C_i} \right)$ represents the fraction of i's total sales (purchases) associated with firm k.

Measures of indirect exposure for firms that are at most n-links-away from a directly impacted firm, then, can be described recursively as

$$IndirectExp(n)_i^{Trade} = \sum_{k=1}^{K} \left(\frac{R_{ki}}{R_i} + \frac{C_{ki}}{C_i} \right) \frac{L_k}{L_i} IndirectExp(n-1)_k^{Trade}, \tag{3}$$

for a chain of domestic traders of length K.

Individual and Household Firm Exposure Unlike the measures we will describe below (which are derived from individual's occupations, location, or wage), the firm's

[15] This is relevant as, for instance, cheaper inputs lower marginal costs, increase demand and induce scaling up of all activities, but they may also induce less demand for labor through a substitution effect.

direct and indirect exposure are firm-specific, so we proceed by linking these exposures to the firms' employees. For each voting board (which usually corresponds with a classroom), we observe the list of unique national identifiers of citizens assigned to the voting board. We then match these unique IDs to our employer-employee data. The data allows us to link 41% of voters to an employer. Once each voter is linked to a firm, we can then assign her the employer's measure of exposure (defined in equation (1)). This is our *individual* measure of exposure to the FTA via earnings. We then go further and calculate measures of *household* exposure using information on each voter's marital status and the identity of his or her domestic partner. If the voter is married, we calculate his or her household exposure measure as the weighted average of the exposure of each partner, where the weight corresponds to the share of household income contributed by each partner. That is, we follow the unitary model of the household.[16] For instance, if each partner is earning the same wage, then the household's exposure is the average of the exposures of the partners' employers. In contrast, if only one partner is employed, or if the voter is single, the household's exposure is simply the employed voter's exposure. This match allows us to increase the share of voters that we can match to an employer, from 41% without exploiting partners' IDs to 52%. This success rate in matching voters to firms is close to the best possible, as 9% of voters are retired, 29% are estimated to be in the informal sector, and 6% are estimated to be adult students; thus, we are roughly capturing the entire remaining share.[17]

[16]This model, frequently used in policy design, implies that the income the household receives is what matters, not the identity of the individual within the household who receives this income. Conversely, some alternative "collective" models weight income asymmetrically depending on the member of the household that receives the income (Alderman et al., 1995).

[17]Note that, given the nature of our shock, which hits firms trading internationally, it is not unreasonable to assume that employees working at informal firms have zero direct exposure, as informal businesses, which tend to be smaller and less productive, are unlikely to be engaged in foreign trade. We estimate these groups as follows: a retiree is an adult who has over 65 years of age and is not employed; a college student is an adult below 23 years of age who is not employed *and* who appears as a high-skilled employee before 2013; finally, an informal worker is an adult who is not employed or a student, who is between 18 and 65 years of age, who is not married to an employed worker, and who does not appear among the employed within one year of 2007–our 29% estimate is close to the 27% reported in other surveys (ILO, 2002).

Sectors We construct measures of exposure to the FTA at the industry level (4-digit ISIC codes), which are analogous to those presented in equation (1), but at the sector level.

Occupations We also explore the effects of a voter's occupation on her choice in the referendum. To do so, we classify workers by occupation to measure the importance of skill groups. In particular, a worker is classified as "low-skill" if her occupation requires *at most* a high-school diploma, while a worker with an occupation that requires education or training beyond high school is labeled as "high-skill." Descriptions of the educational requirements of each occupation are obtained from Costa Rica's Social Security Administration.[18] This leads to 57% of Costa Rican workers being classified as low-skill.[19]

Local Labor Markets and Import Competition Attitudes toward the FTA might be affected by local labor markets and import competition (Autor et al., 2013); and we construct measures to explore this possibility. First, we use the 2011 Population Census to estimate commuting zones (CZ) in Costa Rica from observed flows, following Tolbert and Sizer (1996). To the best of our knowledge, such an exercise has never before been conducted for Costa Rica. We report the country's map with the estimated CZs in Figure A.3. Second, we construct the following measures of import competition for each CZ i across j industries:

$$\Delta ADH\,Comp_i = \sum_j \frac{L_{ij}}{L_j}\frac{M_j^{US}\Delta\tau_j}{L_i} \text{ and } \Delta M\,Comp_i = \sum_j \frac{M_{ij}^{US}\Delta\tau_j}{L_i}, \qquad (4)$$

[18] In Costa Rica, each occupation is subject to a particular minimum wage, and in turn, minimum wages depend heavily on educational attainment. Therefore, the Social Security Administration creates the categories for each occupation following guidelines by the Ministry of Labor, and has a manual describing the educational attainment that a worker must have to belong to an occupational category.

[19] While we have information at the *census-block* level regarding years of schooling, our data does not include information on educational attainment at the individual level. We, however, do observe each worker's occupation, thus, we use it as a proxy of her skill group. This analysis would therefore vary at the voting-board level, as opposed to one using census-block data on years of schooling, which would only vary at the voting-center level.

where $M_j^{US}\Delta\tau_j$ is the expected change in imports from the U.S. given the change in tariffs for industry j and $M_{ij}^{US}\Delta\tau_j$ is the expected change in imports in industry j and located in commuting zone i. We can construct the second measure as our data specifies, for each firm, their imports and location.

Expectations About Future Job Opportunities Measures of ex-ante exposure reflect how voters' conditions at the time of the referendum influence their choice. Our last measure of the income channel's effect asks whether voting behavior reflected correct perceptions of the benefits that emerged from the FTA's approval, *but that were not necessarily captured by ex-ante conditions*. For instance, a worker might have anticipated that she could get a better job if the FTA was approved. This might have influenced her vote, but would not be captured by our measure of a predicted change in earnings that relies on employer exposure at the time of the referendum, because the anticipated improvement in earnings would result from a change in employer. To test this possibility, we exploit that the FTA was approved—albeit by a small margin and somewhat unexpectedly—and we calculate the discounted change in real earnings experienced by each voter h in the years after the referendum, as follows:

$$\sum_{t=2}^{T} \beta^t \frac{wage_h^{2007+t}}{CPI^{2007+t}}, \tag{5}$$

where $T = 2017$ and β depends on the interest rate in 2007.[20] We consider the residual of a regression of the term in expression (5) on our measure of direct firm exposure, Exp_b^{Trade}.[21] This residual term, which we will call *Ex-post*, is intended

[20] We assume that voters could project at most 10 years into the future, and that they discounted using the prevailing interest rate in 2007. We then compute $\beta = \frac{1}{1+r}$. Note that, given our discount factor, changes in wages experienced in 2017 will have a smaller effect than changes that occurred shortly after 2009. The ex-post real wage schedule's sum starts at $t = 2$, as CAFTA came into effect on January 1st, 2009. Details on this timing are provided in Appendix C.2.

[21] Interestingly, if we include both the firm trade exposure and the dummy of low-skilled, we find that (i) both explain a significant portion of the variation in these ex-post wages; and (ii) the *interaction* between the firm trade exposure and the low-skilled dummy is large, negative, and significant. The second finding aligns with Verhoogen (2008), as it suggests less pass-through from exposure to wages for the low-skilled.

to capture drivers of ex-post income that are not captured by ex-ante direct trade exposure.

4.2 Income Channel: Empirical Strategy

As described in Section 3.3, our data on voting outcomes is available at the voting-board level, and we are able to observe the individuals assigned to each voting board. Moreover, we have a battery of observables, all at the individual level. This breaks new ground on anonymity-compatible voting data: while the secrecy of the vote is preserved by the voting outcomes being aggregated by voting board, voting boards are small (approximately 500 people, on average) and we can construct exposure measures which vary at the voting-board level.

Since the data by voting boards is aggregated, we will run an analysis at the voting-board level. Namely, we consider the following specification:

$$YesVoteSh_b = \alpha + \beta X_b + \Gamma K_b + \lambda_b^r + \varepsilon_b, \tag{6}$$

where $YesVoteSh_b$ refers to the share of pro-FTA votes at each voting board b and X_b is a vector of average exposure measures of voters assigned to voting board b; a vector which will shortly be defined in alternative ways, *but which always results from averaging the exposure measures of individuals assigned to each voting board.* K_b is a vector of voter characteristics (age, wage, gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firms' trade with the U.S.) averaged at the voting-board level, along with voter characteristics averaged at the voting center level (average years of schooling from census data geo-referenced by census-block and average distance to the school); and λ_b^r denotes region fixed effects.[22]

We use a linear probability model for our main regressions. At first blush, a logit model might seem well-suited for our experiment. However, this is not the case as we do not observe our dependent variable at the individual level, but

[22]The 2011 Census was the closest to the 2007 referendum, which is why we use it in our main specification. Table B.2 shows that the results remain statistically equal if we instead use the second-closest census, which took place in 2000. Regions correspond with municipalities.

aggregated at the voting-board level. If individual outcomes are set up in a logit model, the aggregation of this logit would not deliver a well-behaved and intuitive logit at the voting-board level.[23] In our data, a linear probability model delivers fitted values that lie within the $[0, 1]$ interval for 100% of voting boards.[24] The linear probability model also admits a straightforward interpretation. Moreover, a linear probability model has the desirable feature that, after some corrections discussed in Section 4.5, we can interpret our coefficients as *individual-level* effects, and not only as group-level effects.

We cluster standard errors at the voting center level, and weight each voting board depending on its number of voters.[25] Figure A.4 presents the distribution of vote shares across all the voting boards in our sample, which is centered around 50% (mean 49.95%, median 51.54%) and has thin tails.[26]

4.3 Income Channel: Results

We now describe the impact of factors related to the income channel on voting following the specification described in Section 4.2. Thus, all the results correspond with an analysis at the voting-board level.

Direct Firm Exposure Table 1 shows that direct firm exposure is salient to voters. A first takeaway from this table is that the direct effect of firm exposure, described by the first row of columns (1)-(5) and column (9), is *extremely* stable and persists after accounting for many other factors. Across all these specifications, we find that referendum votes were cast in alignment with the interests of voters' employers. To interpret the coefficients, recall that our analysis is conducted at the voting-board level and that our exposure measure is a weighted average of percentage changes in tariffs, with weights in dollars per worker at

[23] As each individual would have different states as independent variables, the aggregation of the standard individual logit model to the voting-board level would deliver a sum of logits on the right-hand side of the estimation equation. This issue is a similar problem to BLP (see Montero (2016) and Rekkas (2007)).

[24] See Figure A.5 for an example.

[25] In Appendix B.1, we show that our results are robust to alternative levels of clustering, and that unweighted estimates yield very similar estimates (see Tables B.3 and B.4, respectively).

[26] Given these characteristics, we do not rely on a censored regression model.

each firm. As an example, consider column (1): we find that an increase of $1,000 in the exposure of the average employer—which is a proxy of the average expected change in profits, in thousands of dollars—is associated with a 3.4 pp higher share of votes in favor of the FTA at a voting board; a 6.8% increase with respect to the mean. Note, however, that a $1,000 change *in profits* is not the same as $1,000 in the pockets of a voter; in fact, Alfaro-Ureña et al. (2021) estimate that such a change would correspond with an average increase in *wages* of $90.[27] How would the referendum's outcome change if Costa Rica was not trading with the U.S. in 2007 and firms had a value of zero in this exposure measure? The FTA would not have been approved; only 46% of people would have voted in favor of the FTA in the referendum under this counterfactual scenario.[28]

Decomposing Direct Firm Exposure While our main measure in equation (1) captures the total expected change in *wage* for a worker under a rent-sharing scheme—which would be proportional to its employer's change in profits divided by the total number of employees—our setting allows us to identify the effects of changes in export and tariffs separately by considering: $Exp_i^X = \sum_{j=1}^{n} \frac{X_{ji}^{US}}{L_i} \Delta\tau_j^{US,X}$ and $Exp_i^M = \sum_{j=1}^{n} \frac{M_{ji}^{US}}{L_i} \Delta\tau_j^{US,M}$. These effects might differ, for instance, if exports are more salient for workers than imports. As shown in columns (7) and (9) of Table 1, we find that average exposure through exports has the largest effect, as a $1,000 increase in exposure via exports leads to an over 8 pp increase in the share of people in favor of the FTA at a voting board—more than twice the effect of our original hybrid measure. On its part, an increase in exposure through imports increases the share of pro-FTA votes by over 1 pp (columns (8) and (9)) and is statistically insignificant, suggesting that exports play more of a role in determining voter choices. One possible explanation for this asymmetric result is that, while an increase in revenue via exports would unambiguously increase a worker's wage under a rent-sharing scheme, the same is not true of an increase in

[27] Alfaro-Ureña et al. (2021) calculate this pass-through from changes in profits due to foreign shocks to changes in domestic wages also for the case of Costa Rica.

[28] This calculation assumes that other variables remain constant at their 2007 values. We then estimate the fitted counterfactual vote shares by voting board, and we aggregate them while taking the number of voters at each voting board into account.

profits via lower costs of imports of inputs or final goods, as reduced import prices might function as a substitute for labor in the production process, adversely affecting workers (Verhoogen, 2008). Other potential explanations include different salience to the worker and different effects on skill intensity.

Indirect Firm Exposure Results related to a firm's direct *and* indirect exposure (for buyers and sellers who trade with a directly exposed firm) are presented in column (2) of Table 1. As shown, indirect exposure for firms that are "one-link-away" from a directly exposed firm matters. The coefficient of indirect exposure is approximately two-thirds the size of the coefficient of directly exposed firms. This result highlights the role of indirect exposure via the firm network in shaping worker attitudes toward trade; a channel which has remained largely unexplored by the literature. Beyond this one-link-away relationship, we do not find effects of firms connected via their network.[29] That is, the exposure of firms two or more links away from a directly exposed firm cannot explain votes of people employed by that firm, as reported in Table B.5.[30]

Decomposing Indirect Exposures Equation (3) groups relationships between firms, regardless of whether an indirectly shocked firm is buying from or selling to a directly shocked firm. We can first ask if the effect is symmetric when considering buyers vs. sellers. As shown in Table B.6, coefficients are exactly the same in both cases. Moreover, the effect disappears for relationships that are more than "one link away" from each other. We can further decompose this indirect effect into four categories: an indirectly shocked firm which is (i) selling to an exporter to the U.S. ("seller2seller"), (ii) selling to an importer from the U.S. ("seller2buyer"), (iii) buying from an exporter to the U.S. ("buyer2seller"), and (iv) buying to an importer from the U.S. ("buyer2buyer"). Column (10) of Table 1 displays the results. We find the effect is positive and significant only for sellers to exporters and buyers from importers, i.e., cases (i) and (iii), but the effect is negative and insignificant for cases (ii) and (iv). This result is intuitive: for sellers to exporters,

[29] This finding is consistent with Dhyne et al. (2022), who document that direct demand effects decay quickly with the distance to direct exporters in the supply chain.

[30] Table B.7 also reports results for direct and indirect firm exposure without controls.

Table 1: Income Channel and Voting Behavior

Dependent variable: $YesVoteSh_b$

	Panel (a): Income Channel Factors					
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exp_b^{Trade}	**0.034**	**0.034**	**0.031**	**0.034**	**0.036**	**0.034**
	(0.013)***	(0.013)***	(0.013)**	(0.013)***	(0.013)**	(0.013)***
$IndirectExp(1)_b^{Trade}$		0.023				
		(0.005)***				
Industry Exp_b^{Trade}			0.037			
			(0.121)			
$LowSkillSh_b$				-0.333		
				(0.079)***		
$\Delta\, MComp_b$					-0.033	
					(0.025)*	
$Ex\text{-}post_b$						0.0001
						(0.0004)
Adjusted R^2	0.636	0.639	0.622	0.624	0.503	0.635

	Panel (b): Decomposition of Firm's Direct and Indirect Exposure			
	Direct			Indirect
	(7)	(8)	(9)	(10)
Firm Exp_b^X	0.082		0.081	
	(0.026)***		(0.026)***	
Firm Exp_b^M		0.014	0.011	
		(0.012)	(0.012)	
Firm Exp_b^{Trade}				**0.031**
				(0.013)**
$IndirectExp(1)_b^{Seller2Seller}$				0.052
				(0.018)***
$IndirectExp(1)_b^{Seller2Buyer}$				-0.042
				(0.018) **
$IndirectExp(1)_b^{Buyer2Seller}$				-0.053
				(0.048)
$IndirectExp(1)_b^{Buyer2Buyer}$				0.025
				(0.005)***
Adjusted R^2	0.636	0.635	0.636	0.638

Notes: The unit of observation is the voting board. All regressions have 4,914 observations and 1,934 clusters. Robust standard errors, adjusted for clustering by voting center, are in parentheses. Voting boards are weighted by their number of voters. Regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics by voting center (years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. All columns but (3) also include employment share by industry; column (3) instead includes employment and trade by industry. For all columns but (5), regions correspond with municipalities; for column (5), we use provinces and each of them spans approximately three commuting zones. We denote: $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

the FTA potentially means more business; for buyers from importers, the FTA might translate into cheaper prices; however, for sellers to importers and for buyers from exporters, the FTA might translate into more competition.

Sectors A worker's industry seems to play a limited role conditional on firm exposure, as shown in column (3) of Table 1, which highlights the relevance of within-industry heterogeneity. Without including the firm exposure measure, the coefficient of sectorial exposure becomes twice as large, as shown in Table B.9.[31]

Occupations Column (4) of Table 1 shows that the relatively abundant low-skill workers are more likely to vote against the FTA. A 1 pp increase in the share of low-skill voters at a voting board ($LowSkillSh$) is associated with approximately 0.3 pp fewer citizens voting in favor of the FTA. This finding is against predictions of the Heckscher–Ohlin model, but it is in line, for instance, with Urbatsch (2013), Hicks et al. (2014), and (Verhoogen, 2008). In fact, if we consider wage schedules *after* the FTA was ratified as a dependent variable, we find that the *interaction* between firm exposure and $LowSkillSh$ is negative and significant, which suggests a lower pass-through from exposure to wages for the low-skilled.

Import Competition Our findings suggest that competition in local labor markets might influence voters to position themselves *against* the trade agreement, as shown in column (5) of Table 1. This finding is robust to using alternative measures of import competition, as described in Appendix C.1.

Expectations About Future Opportunities As column (6) of Table 1 shows, we find no evidence that ex-post differential outcomes factored into voting decisions. The latter could relate to expectation formation being difficult in the presence of uncertainty, or to individuals' stochastic discounting of future outcomes. This evidence suggests that ex-ante exposures are good measures of voters' perceptions of the FTA's effects.[32]

[31] Regressions regarding sectorial exposure do not include industry shares by voting board. Instead, they control for total employment and total trade with the U.S., by industry.

[32] An alternative approach is to construct a counterfactual wage, which results from using the real wage growth of voters *before 2007* to project the wage path from 2007 onward. This

4.4 Placebo Exercises and Selection

Pinning down the impact of employers' exposure relies on the measure of household exposure to the trade agreement being orthogonal to other voter characteristics. This measure depends on how the employer's input-output linkages with the U.S. interact with the structure of tariff changes. As virtually all tariffs are zero under the FTA, and would be MFN tariffs otherwise, these changes provide plausibly exogenous sources of variation. Note that our measure of exposure is *firm-specific*, and varies *within* sector, and further varies depending on household composition. These factors aid in making the case that a worker is unlikely to base her choice of employer on this specific measure, *conditional on other characteristics of that firm*. We proceed to more formally explore the role of confounding factors which might affect both voter's job choice and their voting choices.

Selection into global firms We construct placebo exposures for firms trading with countries *other than the U.S.* These measures are computed following equation (1) for each firm, but with exports and imports *to other countries not including the U.S.* in the numerator. As the FTA is not changing tariffs with other countries, this placebo allows us to test if workers who choose to work at firms that engage in foreign trade are special in a way that is being captured by equation (1), but that is not directly related to CAFTA. Results are presented in Table B.12. Reassuringly, not only the resulting coefficient is statistically insignificant, but it is negative. This placebo remains insignificant if we consider only firms trading with the European Union, Costa Rica's second-largest trading partner at the time. We again obtain null results when conducting an analogous exercise for firms' indirect exposure.[33]

Selection into firms that would gain from the FTA We conduct robustness exercises to explore the role of movers; it may be possible that workers' observed the potential gains from the FTA, and then selected their employer based on these gains. CAFTA was signed in 2004 (i.e., the involved parties agreed upon the tariff

approach and its results are described in Appendix C.2.

[33]These results are presented in Table B.13.

schedule), and it was ratified in the U.S. House (by one vote) in July 2005. While Costa Rica also had to ratify the FTA for it to be effective (and this happened with the referendum), people might have anticipated CAFTA's ratification and moved to firms with larger gains. The close referendum outcome makes betting on a particular result risky, but does not rule-out selection. We speak to this type of selection leveraging the historical employer-employee data in three ways, which are detailed in Appendix B.5 and summarized here. First, we identify workers who changed firm between January 2006 (when our employer-employee data begins) and October 2007, and classify these moves into categories depending on how the worker's move changed her exposure to the FTA. The dynamics of these categories are plotted in Figure B.1. Overall, patterns do not suggest that firms with larger gains attracted a larger share of workers. Second, we calculate the share of people at each voting board who changed employers between January 2006 and October 2007, use this share as an explanatory variable, and interact it with our measure of firm exposure. Table B.14 shows the interaction term is a zero, both in magnitude and significance, which—consistent with Figure B.1's narrative—does not support a story where selection significantly drives the effect. Our third exercise is an IV strategy. We fix the employer-employee network in January 2006 and use exposures based on this fixed network to instrument for "true" exposure measures. The idea is to "shut-down" the effect coming from movers. Table B.15 shows this IV's results. The coefficient for firm exposure is slightly larger—albeit statistically equal—in the IV vs. the OLS; the latter again speaks against a story of selection into firms as a way of anticipating the FTA's gains.

The previous results suggest that selection of workers into firms engaged in foreign trade or into firms that would gain from the FTA was not the main driver of the effect we documented. However, we cannot completely rule-out confounding factors that might affect both individuals' selection of jobs and their voting choices in the referendum. In this sense, our estimate is akin to a LATE, as it measures the effect of, for instance, workers of certain type making certain voting choices.

4.5 Income Channel: Robustness and Discussion

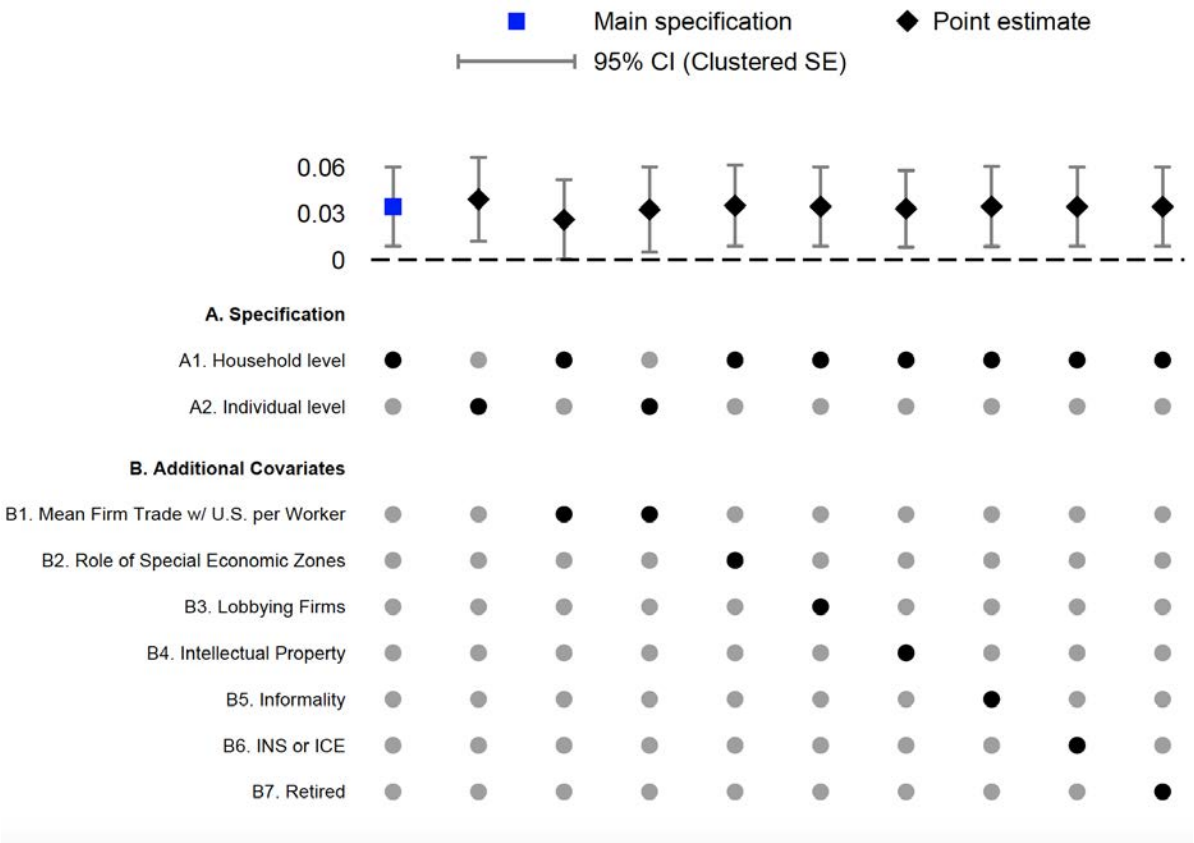
Figure 1 summarizes a series of robustness exercises, all of which are explained and reported in detail in Appendix C.1. As shown in the figure, results when considering only individual-level exposure are statistically equal—albeit larger—to the ones leveraging household-level exposure (panel (A.2)). Moreover, as shown in panel (B.1), our design is robust to the inclusion of a demanding additional control, both at the household- and individual-level, namely: $\sum_{j=1}^{n} \frac{X_{ji}^{US}+M_{ji}^{US}}{L_i}$. This term is similar to our main regressor described in equation (1), but it omits the exogenous tariff changes implied by the FTA. Adding this control is quite demanding in terms of variation, but it carries the benefit that identification would come solely from changes in tariffs, which can be regarded as exogenous shifts, as we have argued above.

Further, firms that trade with the U.S. might be operating within a Special Economic Zone (SEZ). Such firms might face lower tariffs than others at least for a number of years, which would alter the impact that the FTA would have on their profits. Thus, we include the share of production by firms within an SEZ as a control and report the point estimate of firm exposure, which remains largely unchanged, in panel (B.2). We also find that the effect persists after accounting for firms which engaged in lobbying before the referendum took place (panel (B.3)); this result is further discussed and interacted with firm exposure in Appendix C.1.

The FTA included guidelines regarding intellectual property (IP) rights. Our regressions control for industry shares, which would indirectly capture the differential IP intensity across sectors. We can, however, also include a variable with the patent intensity by industry, as measured by Hu and Png (2013).[34] As reported in panel (B.4), we do not find that voters employed in patent-intensive industries behave differently than individuals in other sectors. This null result can be interpreted as evidence of the inattention of voters to alternative forces, other than tariffs which can be affected by the FTA; while provisions on IP protection

[34] As this measure exists for manufacturing sectors only, we only consider patent intensity for this subset of industries. For this regression, industry shares are defined at one digit, while the patent intensity depends on 2-digit industry definitions, so that the industry shares do not absorb the patent-intensity variation.

Figure 1: The Impact of Firms' Direct Exposure: Robustness Exercises



Notes: In the bottom panel, black dots indicate the specification of the regression that generates the point estimate which is vertically aligned with these dots. Individual tables with these regressions are reported in supplementary Online Appendix C.

probably have an impact on firms, voters may not be attentive to this effect as much as they are attentive to (potential) tariff changes.

Finally, panels (B5), (B6), and (B7) control, respectively, for the share of informal workers, the share of workers employed at the National Insurance Institute (INS) or the Costa Rican Institute of Electricity (ICE), and the share of retirees assigned to each voting board, none of which significantly alters the effect of direct firm exposure.[35]

[35]In particular, being employed at the INS or the ICE was potentially relevant, as these public institutions had monopolies in insurance and telecommunications, and the FTA would force for both of these industries to face competition (see Section 3.1). Appendix C also discusses the

In what follows, we discuss three other relevant dimensions: selection into voting, levels of information among voters, and compare Costa Rican attitudes with those of other countries.

Selection into Voting 59% of all eligible citizens voted in the 2007 referendum. If citizens chose whether or not to vote in a way that is related to their exposure, the resulting bias could influence our results. We address this potential concern in two different ways. First, recall from Section 3.2 that all adult citizens are assigned to a school (voting center) by their place of residence, and are sorted into classrooms (voting boards) alphabetically by surname. This sorting is automatic and does not consider whether a citizen actually shows up to vote. This design gives us a straightforward way to avoid selection bias: all our main results construct measures of exposure using the *entire* list of IDs assigned to each voting board, while controlling for the degree of participation (abstentionism) at each voting board, instead of the list of IDs of the voters that showed up to vote.

Second, we show that, while the referendum's vote itself depended on voters' exposure, *the decision to vote or not* seems to be orthogonal to the expected gains from the FTA. Instead, as documented in Table C.9, people who are accustomed to voting and participating in civic activities tended to vote in the referendum. Not only is the correlation between participation in the referendum and in the 2006 presidential election 84% and significant at the 1% level, but also Table C.9 shows that (i) participation in the 2006 presidential election strongly explains participation in the referendum, and (ii) the effect of firm exposure *cannot* explain participation in the referendum, as it is statistically insignificant and almost zero in magnitude.

Voter Awareness and Level of Information The results of Section 4.3 suggest that voters were aware of the FTA's consequences. We find suggestive evidence that these results align with the prevalent level of knowledge about the FTA

limited evidence we find on altruism by leveraging data on extended family networks (Section C.4) and on how attenuation bias does not seen to be large in this setting (Section C.3), which would be expected in the absence of heterogeneous effects; a pre-condition to interpret group-level effects as individual-level effects.

at the time. From May to October 2007, a local consulting firm conducted a series of nationally representative surveys to track the evolution of the public opinion toward CAFTA, which Rodríguez et al. (2008) summarize. These surveys include the question: *What is your level of information about the FTA?* According to the surveys, by October 5th 2007—two days before the referendum—72.2% of people reported to be very informed or reasonably informed, 22.2% reported to be little informed, and only 5.6% reported to be not at all informed.[36] By the same date, 100% (94.4%) of respondents answered "yes" to the question: *In the last month, have you seen/heard/read advertising in favor of (against) the FTA?*

Attitudes in Costa Rica vs. Other Countries It may be helpful to benchmark attitudes toward openness and globalization in Costa Rica at the time of the referendum against views in other countries. This comparison poses two main challenges: (i) the referendum took place almost 15 years ago, and (ii) we need a way to measure attitudes that is reasonably comparable across countries, despite Costa Rica typically not being included in surveys which ask respondents about trade policy, like those by the International Social Survey Programme (ISSP) studied by Mayda and Rodrik (2005) or those regularly conducted by Gallup in the U.S. We overcome these challenges by leveraging a series of nationally representative surveys conducted just before the referendum, and mentioned in the last paragraph, and identifying questions in these surveys which most resemble questions asked by ISSP and Gallup. A comparison of responses across countries is presented in Table C.10, and shows that Costa Rica was not an outlier; Costa Rican attitudes resembled those in the U.S. in recent years and in Latin America circa 2007.

5 Expenditures Channel

The FTA could also lead to lower consumer prices, which would be positive for voters. In fact, when Costa Ricans were surveyed one month before the refer-

[36]The possible answers were: Very informed, Reasonably informed, Little informed, and Not at all informed.

endum, in September 2007, 64% of respondents answered "yes" to the question: *"Will the FTA benefit consumers?"*[37] This section will approximate the predicted reductions in voters' expenditures and estimate the extent to which these predictions affected voter choice in the referendum.

5.1 Measuring Exposure Via Expenditures

To measure each voter's exposure to the trade agreement via expenditures, we rely on the National Household Income and Expenditure Survey (*Encuesta Nacional de Ingresos y Gastos de los Hogares*). This survey aims to understand households' expenditure structure, and it asks households how they spend their incomes across goods and services in a detailed consumption basket. The survey is representative at the regional level, and the results include several respondent characteristics, including income, occupation, location, gender, age, and marital status. We use the last survey that was conducted before the 2007 referendum, in 2004. The sample included 5,287 housing units.

The survey allows us to *map a consumption basket to each household* based on this large set of characteristics, which we observe both in the survey and for each voter. Details on this exercise are provided in Appendix D. Then, we can estimate an expected change in the price of this basket, based on the share of the good that is imported from the U.S. and its expected change in tariffs. In particular, following Fajgelbaum and Khandelwal (2016), we define the individual expenditure effect of consumer h as

$$Expend_h = \sum_{j=1}^{J} (-\Delta p_j)(s_{j,h} - S_j)(p_h q_h), \tag{7}$$

where p_j denotes the price of good j, $s_{j,h}$ denotes the share of good j in the total expenditures of individual h, S_j denotes the share of good j in average expenditures. It follows that $-\Delta p_j s_{j,h}$ represents an expenditure-share weighted average of price changes, and defines the consumer's expenditure effect. If this

[37] Details on this survey coincide with those described in Section C.10. This question was asked only in September.

change is negative, it represents a reduction in the cost of living caused by a decrease in prices applied to the pre-shock expenditure basket. We include the term $p_h q_h$, which captures the expenditures of household h, to have a change in expenditures in dollars that is comparable to other measures in our study.

To calculate the price changes for each good j, we first identify the share of total domestic absorption of good j that is imported from the U.S., and we denote this quantity $s_j^{M,US}$. Second, we assume complete pass-through such that

$$-\Delta p_j = s_j^{M,US} \Delta \tau_j,$$

where $\Delta \tau_j$ is the change in the tariff that would take place if the FTA were to be ratified. Note that assuming complete pass-through in this particular setting might not be unreasonable, as the majority of voters are unlikely to take a more-sophisticated approach for predicting a change in the price of her consumption basket.

Finally, through a lasso regression, we select the variables that better explain each household's exposure via expenditures. We then predict each voter's exposure to the trade agreement via household-level expenditures. Appendix D gives more details on how to generate this mapping and an example of how to compute changes in prices. It is worth noting that, unlike the measure for firm exposure, *every single voter is assigned an expenditures exposure via their observables* through this mapping (even if they are informal, unemployed, not in the labor force, etc).

5.2 Expenditures Channel: Results

Similarly to the analysis of the income channel, the study of the expenditures channel is run at the voting-board level. To do so, we follow equation (6) and use the exposure to the FTA via household-level expenditures, averaged across the individuals assigned to a voting board, as our main independent variable.

Table 2 presents our results. Column (1) shows results *without* including any controls. As expected, the coefficient without controls or fixed effects is larger than the ones in columns (2) and (3), but the overall message remains unchanged

Table 2: Expenditures Channel vs. Earnings Channel

Dependent variable: $YesVoteSh_b$			
	(1)	(2)	(3)
$Expend_b$	-0.022	-0.011	-0.011
	(0.002)***	(0.005)**	(0.005)**
Exp_b^{Trade}			0.035
			(0.013)***
Controls	No	Yes	Yes
Observations	4,914	4,914	4,914
Clusters	1,934	1,934	1,934
Adjusted R^2	0.084	0.635	0.636

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are given in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

across specifications. We interpret the coefficient in column (2) as follows: The average household whose expenditures would decrease by \$1 if the agreement were to be approved—on top of the decrease in expenditures experienced by the average consumer (\$7.3)—is 1 pp more likely to vote in favor of the FTA. In other words, a one-standard deviation (1.556) decrease in a voting board's average exposure via expenditures is associated with the share of voters in favor of a trade agreement at that board being 1.63 pp greater. This effect is significant even after controlling for firm-level exposure, as reported in column (3).

6 Non-Economic Factors and Comparison

In this section, we first explore the role of a potentially crucial non-economic factor: political alignment. We then proceed by comparing the role of political alignment and demographics (non-economic factors) in explaining voting behavior with the one of economic factors, with an emphasis on firm-level exposure.

6.1 Political Alignment

Voter behavior might be influenced by political views, and political views might, in turn, be correlated with economic factors. To explore this possibility, we use the results of the 2006 presidential election as an explanatory variable. First, we divide political parties according to whether they were for or against the FTA. To make this classification, we follow Vargas Cullell (2008), who documents how each party voted in the Congress when it was trying to decide whether to approve CAFTA.[38]

Then, we include the share of 2006 presidential votes for a pro-FTA party at each voting board ($Pres_b^{2006}$) in our main regression, as follows:

$$YesVoteSh_b = \gamma_0 + \gamma_1 Exp_b^{Trade} + \gamma_2 Pres_b^{2006} + \hat{\Gamma} X_b + D_r + \hat{\varepsilon}_b. \tag{8}$$

The measure $Pres_b^{2006}$ is particularly informative given that the 2006 presidential election happened only slightly over a year before the 2007 referendum, and the composition of voting boards changed very little within this year; the citizens assigned to each board, for the most part, would only change if someone turned 18 years old, died, or moved her residence. We verify that voting boards remained almost constant by following all 2007 voters back to the voting boards where they were assigned in 2006. Thus, $Pres_b^{2006}$ is a good measure of voters' political affiliations at the time of the referendum, and allows us to determine whether the role of the firm's exposure is relevant even after accounting for voters' political motivations.

As shown in column (1) in panel (a) of Figure 2, a 1 pp increase in $Pres_b^{2006}$ is associated with a 0.51 pp increase in the share of pro-ratification voters. Column (2) in panel (a) of the same figure shows that this association holds even after accounting for the effect of political affiliation. Note that the magnitude of the coefficient for a firm's exposure is smaller when including $Pres_b^{2006}$ as an additional regressor, even though it remains statistically equal to the coefficient in our main

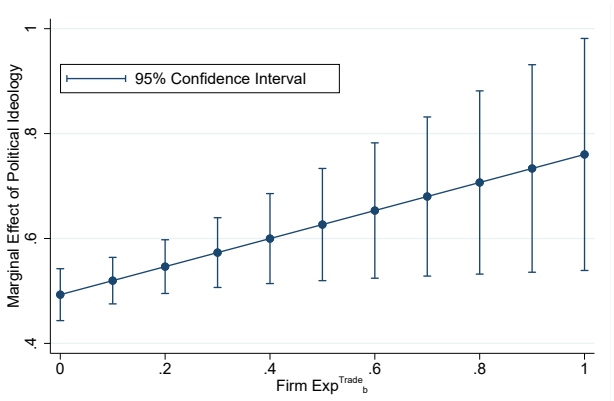
[38] As explained in Section 3.1, the referendum took place because the Congress was split.

Figure 2: Politics, Firm Exposure, and Referendum Outcomes

Dependent variable: $YesVoteSh_b$

	(1)	(2)
$Pres_b^{2006}$	0.515	0.514
	$(0.023)^{***}$	$(0.023)^{***}$
Exp_b^{Trade}		0.026
		$(0.011)^{**}$
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.700	0.701

(a) Political Affiliation and Voting



(b) Marginal Effect of Political Ideology

Notes: Panel (a): The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center, are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people assigned to the voting center (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel (b): This figure plots the marginal effect of political ideology ($Pres_b^{2006}$) for different levels of direct trade exposure (Exp_b^{Trade}).

specification (Table 1). This is an unsurprising result, as one of the topics on the agenda for the 2006 presidential candidates was precisely CAFTA.

IV Strategy As shown above, the coefficient on firm exposure becomes smaller once we account for political alignment. The latter, in turn, could be explained by people's position with regard to the FTA's approval influencing their presidential vote in 2006. To orthogonalize our notion of political preferences from the FTA, we employ an IV strategy. Namely, we use votes for pro-FTA political parties in the 2002 presidential election—before any discussions on CAFTA were on the table—to instrument for the 2006 election votes for these parties. Further details on the construction of this instrument are presented in Appendix E, and results are presented in Table E.1. As expected, we find that the coefficient of firm exposure is larger and closer to the values presented in Table 1 when using the instrument; however, it is remarkable that overall the effects remain quite similar

to those presented in Figure 2.

When Economic Interest and Ideology Collide The setup gives us a rare opportunity to analyze the interaction between views on politics and trade policy. Using the results from panel (a) of Figure 2, we can make a back-of-the-envelope calculation of the effect of political alignment on voters' sensitivity to an extra dollar of trade exposure. We estimate that if *all* voters at a voting board voted for a pro-FTA presidential candidate, the effect on *referendum votes* is equivalent to the voting board having an average trade exposure (Exp_b^{Trade}) of \$19,834.[39]

We can also approach this relationship from a different angle by extending equation (8) with an interaction term between the composition of presidential votes in 2006 and trade exposure. Panel (b) of Figure 2 shows the marginal effect of this regression. One can see that the effect of the presidential-vote composition is heterogeneous depending on the level of trade exposure. We find that high trade exposure, as measured by Exp_b^{Trade}, is significantly more salient for voting boards composed of voters with pro-trade political preferences. Conversely, voters with anti-trade political ideologies are less sensitive to trade exposures that might impact their earnings.

6.2 Comparison Across Factors

This section aims to provide a broad comparison of the importance of different types of factors. Namely, we compare partial R^2 across regressions, after removing certain factors, to grasp what percent of the variation in voting behavior can be attributed to each.

To do so, we consider equation (6) while including direct firm exposure, expo-

[39]Namely, we can calculate the trade exposure (in dollars) which delivers a change in the probability of voting pro-FTA which is equivalent to the impact of having all voters on a voting board having a pro-FTA political alignment. In particular, we find that a \$19,834 mean firm exposure at a voting board would lead to a 53.7% increase in the probability of voting in favor of the FTA (i.e., the effect of having 100% of voters being pro-FTA according to Table 2). Given the 9 cents on the dollar pass-through documented by Alfaro-Ureña et al. (2021), this result implies that if a voting board had on average \$1,785 of "money in their pocket" due to the FTA, this would have an equivalent effect to everyone at the voting board having a pro-FTA ideology.

sure via expenditures, and political alignment as explanatory variables. Note that this regression includes a battery of demographic and economic controls as well. We then define as economic factors: firm exposure, firm size, firm's trade with the U.S., exposure via expenditures, and employment shares by industry; and as non-economic factors: political alignment and demographics, which include: age, wage, gender, participation, and years of schooling.[40]

Table 3 presents the partial R^2 which results from removing each element from the full specification. A comparison of columns (1) and (3) confirms the relatively large coefficient for political alignment in Table 2. However, we can also verify that economic factors play a non-negligible role in explaining the observed variation in voting behavior. The latter was particularly true in this setting, in which the referendum was approved with only a 1 pp lead in votes.[41]

Table 3: Comparison Across Factors—Partial R^2

Economic Factors (1)	Non-Economic Factors (2)	Political Alignment (3)
-6.2%	-11.3%	-10.2%

Notes: The table presents the partial R^2 which results after removing each factor from a full specification given by equation (6) with direct firm exposure, exposure via expenditures, and political alignment as explanatory variables.

7 Concluding Remarks

While the general public tends to hold a wide variety of views about the consequences of trade, economists have strong and specific priors about how trade

[40]Note that wage and years of schooling are not solely non-economic. We include them in this category to be conservative and potentially get a lower bound of the role of economic factors.

[41]We do the partial R^2 exercise while removing factors "in block." Removing only the measure of firm exposure and then evaluating the partial R^2 to see its importance would be an unfair comparison with other factors, because we are including controls precisely to remove variation which is not exogenous from the exposure. When adding these controls, the measure of firm exposure has limited, but cleaner, variation, which is what we exploit. Then, the partial R^2 would irremediably underestimate the relevance of firm exposure alone. Thus, we instead remove economic factors all at once.

affects people's lives. Survey evidence suggests that economists and the broader public hold starkly different views on trade issues (Blendon et al., 1997; Sapienza and Zingales, 2013). If people were given the choice to cast a vote on a specific trade policy, how would they vote? Would they vote based on their own economic interest and in line with predictions from economic theory? A better understanding of the determinants of the public's attitudes toward trade policy may strengthen the ability of economists to aid policy makers in communicating the consequences of policy decisions to the public, and in designing trade policy so that it leads to welfare benefits *and* garners popular support. Moreover, insights about the determinants of popular attitudes may be relevant to how economists understand the distributional effects of trade.

This paper exploits the natural experiment afforded by a national referendum held in Costa Rica in which every adult citizen was allowed to vote on the ratification of CAFTA. This unambiguous and specific policy choice allows us to observe individual's preferences on the topic. Moreover, we leverage voting-board-level data on voting outcomes, along with information on the individuals who compose each voting board to break new ground on anonymity-compatible voting data: while the secrecy of the vote is preserved by the voting outcomes being aggregated by voting board, voting boards are small (approximately 500 people, on average) which leads to a precise analysis. We match voters to their employers, and in turn match firms with customs records, balance sheets, records of firm-to-firm transactions. We also create a mapping between citizens and data about household composition and expenditures. To the best of our knowledge, this mapping represents the frontier of data quality compatible with a secret ballot.

The paper studies the role of both economic and non-economic factors. Regarding economic factors, we first examine those related to the income channel. A key message of the paper is that employers' exposure to the FTA, via its impact on employees' earnings, plays a relevant role in shaping votes, especially for pro-trade voters. We also document that indirect exposure through input-output linkages plays a salient role in explaining votes, with a magnitude of about two-thirds the one of the direct effect. While the role of firm exposure persists after ac-

counting for other factors related to the income channel, these factors also deliver interesting results.[42] We find that high-skilled workers are more likely to support the FTA, that within-industry heterogeneity is more significant in explaining votes than exposure at the sector level, and that local import competition can explain votes against free trade. Moreover, we document that ex-ante exposures, frequently used in the literature, are a good proxy for the perceived gains from trade.

The study of the income channel is complemented by analyzing the role of the expenditures channel. This analysis is possible by leveraging expenditures surveys to construct a correspondence between consumption baskets and levels of exposure, and then creating a mapping where every voter is assigned an expenditures exposure via their observables. We find that households whose expenditures would decrease by more under free trade with the U.S. are more likely to vote in favor of the FTA.

In terms of non-economic factors, our main emphasis is on political alignment, which has been singled as a potentially crucial determinant. Indeed, we find that supporting a pro-FTA political party is an important determinant of individual's votes, and document that the effect of presidential-vote composition is decreasing on the level of trade exposure.

A comparison across factors determines that political alignment plays a relatively important role, as it accounts for 10% of the variation which cannot be explained by other factors. The latter highlights the potential relevance of considering non-economic factors when modelling individual behavior. Crucially, economic factors, also play a non-negligible role, explaining 6% of the observed variation in voting behavior which cannot be explained by non-economic factors. Hence, economic fundamentals played a pivotal role in this particular context, characterized by the narrow approval margin of the referendum. Moreover, in closely contested elections, they are likely to wield significant influence.

[42] While some of these forces have been studied by the previous literature, we revisit them while leveraging a more disaggregated and precise level of analysis.

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Online Appendix for

Voting on a Trade Agreement:

Firm Networks and Attitudes Toward Openness

June 2nd, 2023

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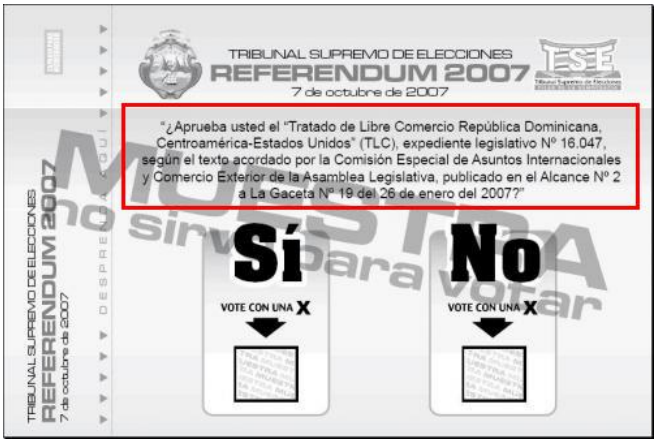
Table of Contents

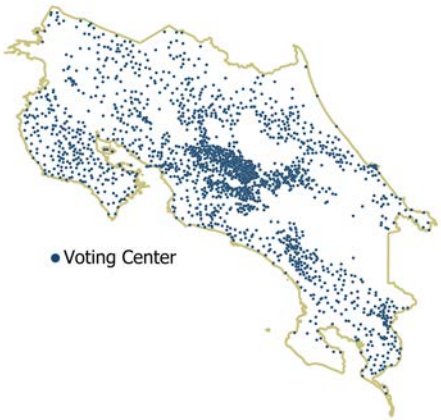
A Background and Summary Statistics

Figure A.1: Sample of the Referendum's Ballot



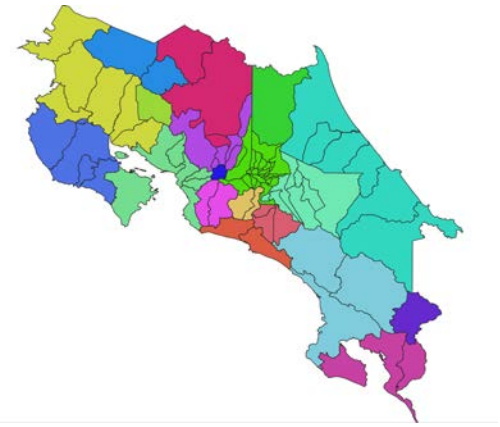
Notes: The figure shows a sample of the single-question ballot used to decide on CAFTA on October 7th, 2007. The text in the red box reads: "Do you approve the "Free Trade Agreement Dominican Republic, Central America-United States" (FTA), legislative file No. 16,147, according to the text approved by the Special Commission of International Affairs and Foreign Trade of the Legislative Assembly, published in the Alcance No. 2 of La Gaceta [the country's official newspaper] on January 26th, 2007?" Voters could only give a yes-or-no answer.

Figure A.2: Geographical Distribution of the Voting Centers in the Referendum



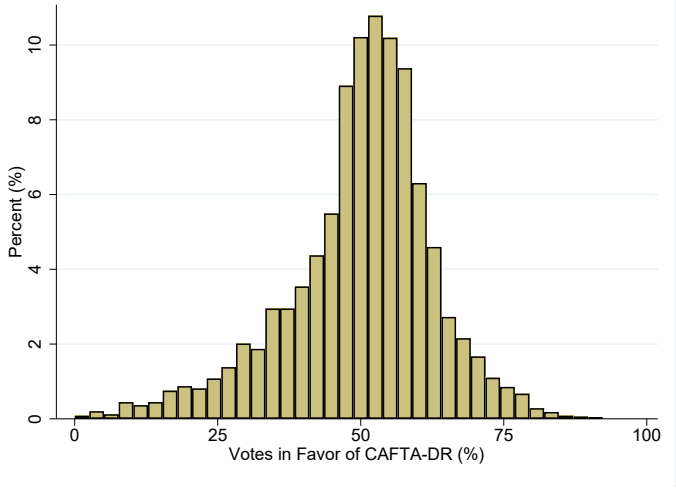
Notes: The figures show the distribution of the voting center across the country for the CAFTA referendum. In Costa Rican elections, each eligible citizen is allocated by her place of residence to a voting center, which is usually located within a school. Within voting centers, voters are allocated alphabetically to voting boards, which usually correspond with classrooms.

Figure A.3: Estimated Commuting Zones of Costa Rica



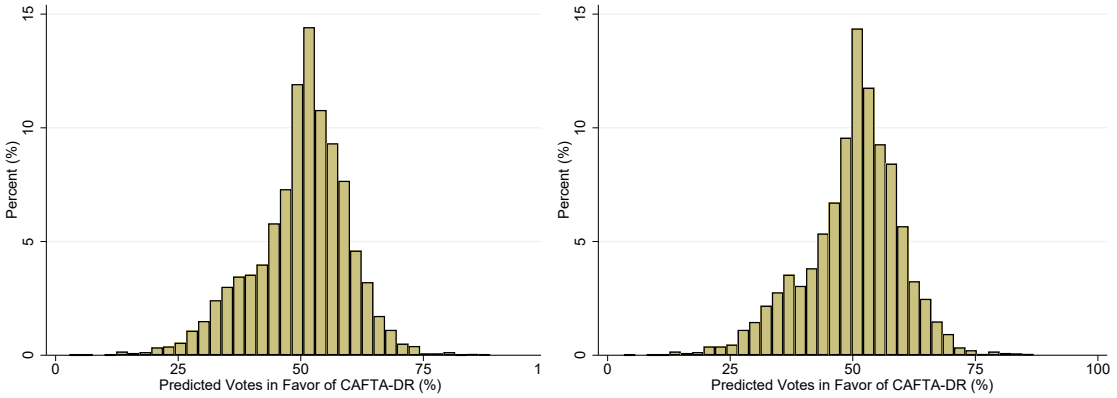
Notes: The figure shows the estimated Costa Rican commuting zones (CZs). These CZs were estimated based on observed flows of workers across locations (municipalities), which were documented in the 2011 Population Census, following Tolbert and Sizer (1996).

Figure A.4: Distribution of Shares in Favor of the FTA by Voting Board



Notes: The histogram shows the percentage of voters at each voting board in favor of the CAFTA free trade agreement. The distribution has a mean of 49.95%, a median of 51.54%, and a standard deviation of 12.93.

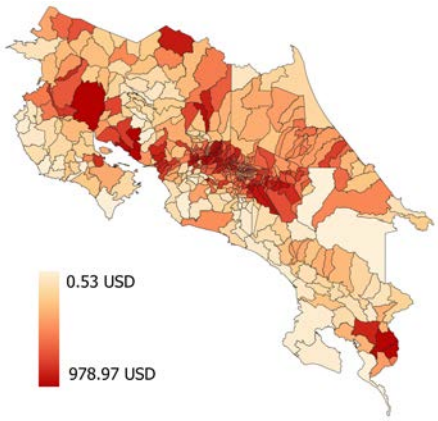
Figure A.5: Distribution of Predicted Shares in Favor of the FTA by Voting Board



(a) Estimates Column (1) of Table 1 (b) Estimates Column (2) of Table 1

Notes: The histogram shows the fitted value of the percentage of voters at each voting board in favor of the CAFTA free trade agreement, based on the estimates of columns (1) and (2) in Table 1.

Figure A.6: Average Direct Exposure of Firms by District via their Trade with the U.S.



Notes: The figures show the average direct exposure through input-output linkages with the U.S. (Exp_i^{Trade}) for firms in each district, in U.S. dollars per employee.

Table A.1: Export and Import Tariff Changes

Industry	Share of Total Exports	Average Export Tariff	Share of Total Imports	Average Import Tariff
Agriculture, forestry and fishing	20.118	2.778	2.592	1.892
Mining and quarrying	0.004	2.067	0.089	2.715
Manufacturing	65.027	2.868	58.753	2.298
Electricity, gas, steam, air conditioning	0.0001	0	0.331	1.781
Water supply	0.432	0.532	0.013	2.492
Construction	0.222	0.731	0.904	4.407
Wholesale and retail trade; repair of motor vehicles and motorcycles	11.508	5.093	30.755	4.205
Transportation and storage	0.243	7.899	0.634	4.670
Accommodation and food service activities	0.010	5.039	0.212	10.704
Information and communication	0.009	0.432	1.264	1.671
Financial and insurance activities	0.137	0.114	0.159	2.160
Real estate activities	0.439	13.682	0.359	8.978
Professional, scientific and technical activities	0.126	1.787	0.346	3.238
Administrative and support service activities	0.093	8.663	0.934	2.967
Public administration and defence; compulsory social security	0.000	6.614	1.370	34.681
Education	0.191	0.563	0.030	3.188
Human health and social work activities	0	0	0.064	2.507
Arts, entertainment and recreation	0.001	0.204	0.077	8.778
Other service activities	1.437	0.218	1.110	0.979
Activities of households as employers; activities of households for own uses	0.004	4.800	0.006	8.602
Activities of extraterritorial organizations and bodies	0	0	0.0004	1.841

Notes: The table shows average tariffs by industry, along with each industry's trade as a share of total Costa Rican trade in 2007. We consider the weighted average tariff paid by firms that belong to each industry to construct weighted average of tariffs by industry. As tariffs would be eliminated under the agreement, changes correspond, for the most part, with the pre-FTA tariff levels.

B Income Channel: Details and Results

B.1 Direct and Indirect Firm Exposure

Table B.1: Firms' Direct and Indirect Exposure, and Employee's Voting Behavior - All Voting-Boards (Includes Jails and Cocos Island)

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.037	0.036
	$(0.013)^{***}$	$(0.013)^{***}$
Firm $IndirectExp(1)_b^{Trade}$		0.021
		$(0.005)^{***}$
Controls/FE	Yes	Yes
Observations	4,932	4,932
Clusters	1,952	1,952
Adjusted R^2	0.627	0.630

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: Firms' Direct and Indirect Exposure Using Average Neighborhood Characteristics from the 2000 Census

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.034	0.035
	$(0.013)^{***}$	$(0.013)^{**}$
Firm $IndirectExp(1)_b^{Trade}$		0.023
		$(0.005)^{***}$
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.635	0.638

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school according to the 2000 Census (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: Firms' Direct and Indirect Exposure, and Employee's Voting Behavior - Alternative Cluster Level for Standard Errors

Dependent variable: $YesVoteSh_b$

	Cluster Level					
	Voting center (School)		District		Municipality	
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exp_b^{Trade}	0.035	0.034	0.035	0.034	0.035	0.034
	(0.013)***	(0.013)***	(0.014)**	(0.014)**	(0.015)**	(0.016)**
Firm $IndirectExp(1)_b^{Trade}$		0.023		0.023		0.023
		(0.005)***		(0.005)***		(0.006)***
Controls/FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,914	4,914	4,914	4,914	4,914	4,914
Clusters	1,934	1,934	469	469	81	81
Adjusted R^2	0.636	0.639	0.636	0.639	0.636	0.639

Notes: The unit of observation is the voting board. The cluster level to compute the standard errors is indicated on top of each column, and the standard errors are presented in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: Firms' Direct and Indirect Exposure, and Employee's Voting Behavior - Unweighted Estimates

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.040	0.039
	$(0.017)^{**}$	$(0.017)^{**}$
Firm $IndirectExp(1)_b^{Trade}$		0.025
		$(0.006)^{***}$
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.556	0.560

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: Firms' Direct and Indirect Exposure Beyond One Link, and Employee's Voting Behavior

Dependent variable: $YesVoteSh_b$			
	(1)	(2)	(3)
Firm Exp_b^{Trade}	0.035	0.034	0.032
	$(0.013)^{***}$	$(0.013)^{***}$	$(0.013)^{**}$
Firm $IndirectExp(1)_b^{Trade}$		0.023	0.024
		$(0.005)^{***}$	$(0.007)^{***}$
Firm $IndirectExp(2)_b^{Trade}$			0.006
			(0.009)
Firm $IndirectExp(3)_b^{Trade}$			-0.003
			(0.007)
Controls/FE	Yes	Yes	Yes
Observations	4,914	4,914	4,914
Clusters	1,934	1,934	1,934
Adjusted R^2	0.636	0.639	0.638

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.6: Indirect Exposure as a Seller vs. as a Buyer–Importance at Different Distances from a Directly Shocked Firm

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.034	0.028
	(0.013)***	(0.013)**
Firm $IndirectExp(1)_b^{Seller}$	0.022	0.021
	(0.007)***	(0.007)***
Firm $IndirectExp(1)_b^{Buyer}$	0.023	0.022
	(0.005)***	(0.005)***
Firm $IndirectExp(2)_b^{Seller}$		0.003
		(0.009)
Firm $IndirectExp(2)_b^{Buyer}$		0.041
		(0.028)
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.639	0.639

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.2 Additional Results Related to Table 1

Table B.7: Firms' Direct and Indirect Exposure: No Controls

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.183	0.170
	$(0.029)^{***}$	$(0.029)^{***}$
Firm $IndirectExp(1)_b^{Trade}$		0.030
		$(0.006)^{***}$
Controls/FE	No	No
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.032	0.041

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center are in parentheses.

Table B.8: Firms' Direct Exposure via Exports, Imports of Inputs, and Imports of Final Goods (Separately)

Dependent variable: $YesVoteSh_b$				
	(1)	(2)	(3)	(4)
Firm Exp_b^X	0.082			0.081
	$(0.026)^{***}$			$(0.026)^{***}$
Firm $Exp_b^{M,inputs}$		0.013		0.009
		(0.012)		(0.012)
Firm $Exp_b^{M,final}$			0.091	0.102
			(0.098)	(0.098)
Controls/FE	Yes	Yes	Yes	Yes
Observations	4,914	4,914	4,914	4,914
Clusters	1,934	1,934	1,934	1,934
Adjusted R^2	0.636	0.635	0.635	0.636

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.9: Exposure at the Sector Level

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Industry Exp_b^{Trade}	0.061	0.037
	(0.122)	(0.121)
Firm Exp_b^{Trade}		0.035
		(0.013)***
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.599	0.599

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share in the public sector, firm size, and firm trade with the U.S., industry size, and industry trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We do not include industry employment shares as controls in these regressions. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.3 Local Labor Markets and Import Competition

As mentioned in the main text, we use the 2011 Population Census to estimate commuting zones (CZ) in Costa Rica from observed flows, following Tolbert and Sizer (1996), and we compute the measures of import competition for each CZ i across j industries as stated in equation (4), following the logic in Autor et al. (2013).

B.3.1 Specification and Results

We consider the following specification:

$$\begin{aligned} YesVoteSh_b = \alpha_0 + \alpha_1 Exp_b^{Trade} + \alpha_2 \Delta MComp_b + \\ \tilde{\Gamma} X_b + D_p + \tilde{\varepsilon}_b, \end{aligned} \tag{9}$$

where $\Delta MComp_b$ is the average measure of import competition in hundreds of USD—which can be defined using either of the measures in equation (4)—of voters at voting board b, D_p are province fixed-effects (each province hosts three CZs, on average), and other variables are defined as in equation (6).

Table B.10: Import Competition in Local Labor Markets and Referendum Outcomes–Calculation Using Firms' Imports and Location

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
$\Delta Local\,M\,Comp_b$	-0.032	-0.034
	$(0.013)^{**}$	$(0.013)^{**}$
Firm Exp_b^{Trade}		0.036
		$(0.015)^{***}$
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.500	0.501

Notes: The unit of observation is the voting board. Local imports calculated using firms' imports and location. Import competition measure is in hundreds of USD. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. Region fixed-effects are defined at the province level, and each province hosts three CZs, on average. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Tables B.10 and B.11 show our results using the definitions in equation (4). With either measure, our findings suggest that competition in local labor markets might influence voters to position themselves *against* the trade agreement, as shown in column (1) of both tables. Column (2) in both tables shows that this effect remains stable after controlling for firm-level exposure. Next, we compare outcomes for the effect of import competition between Tables B.10 and B.11. While Table B.10 uses data on imports by CZ, Table B.11 instead uses data on *total* imports and apportions the imports to CZs according to labor shares. We find that both approaches deliver qualitatively equivalent results. However, coefficients are noisier with the ADH method, as compared with the method using CZ-level data.

Table B.11: Local Labor Market Effect Calculating Local Imports–Calculation Apportioning Local Imports Using Total Imports and Labor Shares

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
$\Delta ADH\,MComp_b$	-0.014	-0.019
	(0.025)	(0.025)
Firm Exp_b^{Trade}		0.030
		(0.015)**
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.497	0.496

Notes: The unit of observation is the voting board. Local imports calculated using each firm's location and total U.S. imports. Import competition measure is in hundreds of USD. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. Region fixed-effects are defined at the province level, and each province hosts three CZs, on average. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.4 Supportive Evidence on Identification

Table B.12: Placebo: Exposure for Firms Trading with Countries Other than the U.S.

Dependent variable: $YesVoteSh_b$

	(1)	(2)	(3)	(4)
	All countries except U.S.		E.U.	
Placebo $Exp\_b^{Trade,\, not\, U.S.}$	-0.010	-0.016		
	(0.020)	(0.019)		
Placebo $Exp\_b^{Trade,\, E.U.}$			0.015	0.008
			(0.039)	(0.038)
Firm Exp_b^{Trade}		0.036		0.035
		(0.013)***		(0.013)***
Controls/FE	Yes	Yes	Yes	Yes
Observations	4,914	4,914	4,914	4,914
Cluster	1,934	1,934	1,934	1,934
Adjusted R^2	0.635	0.636	0.635	0.635

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects.

Table B.13: Placebo: Direct *and Indirect* Exposure for Firms Trading with Countries Other than the U.S.

Dependent variable: $YesVoteSh_b$

	(1)	(2)
	All countries except U.S.	E.U.
Placebo $Exp\_b^{Trade,\ not\ U.S.}$	-0.023	
	(0.019)	
Placebo $IndirectExp(1)\_b^{Trade,\ not\ U.S.}$	0.017	
	(0.022)	
Placebo $Exp\_b^{Trade,\ E.U.}$		-0.031
		(0.040)
Placebo $IndirectExp(1)\_b^{Trade,\ E.U.}$		0.009
		(0.010)
Firm Exp_b^{Trade}	0.035***	0.034***
	(0.013)	(0.013)
$IndirectExp(1)\_b^{Trade}$	0.022***	0.023***
	(0.005)	(0.005)
Controls/FE	Yes	Yes
Observations	4,914	4,914
Cluster	1,934	1,934
Adjusted R^2	0.638	0.639

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects.

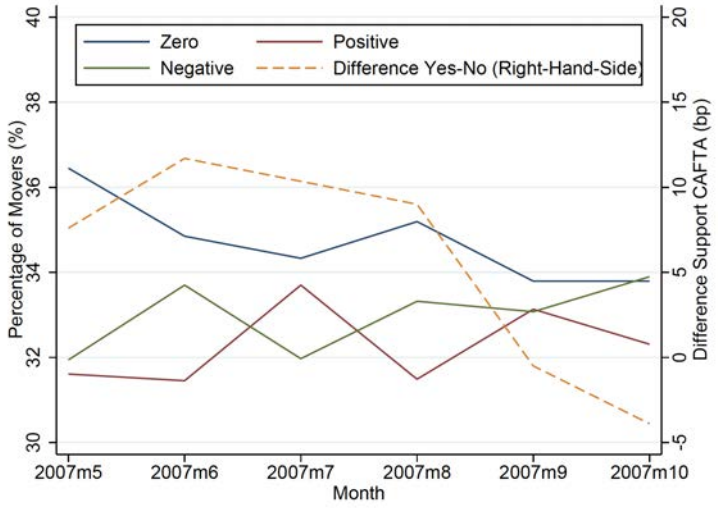
B.5 Details: Selection into Firms that Would Gain from the FTA

In this subsection, we develop several exercises to explore the role of movers. It may be possible that workers' observed the potential gains from the FTA, and then selected their employer based on that. CAFTA was signed in 2004 (i.e., the involved parties agreed upon the tariff schedule), and it was ratified in the U.S. House (by one vote) in July 2005.

While Costa Rica also had to ratify the FTA for it to be effective (and this happened with the referendum), people might have anticipated CAFTA's ratification and moved to firms with larger gains prior to the referendum. The close outcome is a bit helpful in this regard, as betting on a particular outcome was risky, but does not rule-out selection. To speak to this type of selection, we leverage the employer-employee data, and then identify the share of workers who moved to another firm prior to the referendum. We then classify these moves into three categories: (i) the worker moved from a firm with zero exposure to a firm with zero exposure ("zero"); (ii) the worker's move led to an increase in her exposure to the FTA ("positive"); and (iii) the worker's move led to a decrease in her exposure ("negative"). The dynamics of these categories between May 2007, when the referendum was announced, and October 2007, when the referendum occurred, are plotted in Figure B.1. In particular, Figure B.1 shows these shares on its left-hand-side axis, alongside with the difference between the share of people who report an intent to vote in favor vs. against the FTA in opinion polls which were conducted from May-October and published in the country's main newspaper, in the right-hand-side axis.[43] Overall, these patterns do not seem to suggest that the "positive" category became more relevant; moreover, these shares do not seem to meaningfully respond to the results of the opinion polls, which pointed to a statistical tie, with the difference in "yes" vs "no" being slightly positive at the start and barely negative a few months prior to the vote. In fact, the correlation between the polls' results and each category is statistically insignificant.

[43]The polls were conducted by Rodríguez et al. (2008).

Figure B.1: Movers Since Referendum's Announcement and Opinion Polls' Outcomes



Notes: The left-hand-side axis shows the percentage of movers after classifying them into three categories: (i) the worker moved from a firm with zero exposure to a firm with zero exposure ("zero"); (ii) the worker's move lead to an increase in her exposure ("positive"); and (iii) the worker's move lead to a decrease in her exposure ("negative"). The right-hand-side axis shows the difference between the share of people who report an intent to vote in favor vs. against the FTA in opinion polls prior to the referendum.

While these patterns are informative, we conduct two more rigorous exercises. In the first one, we calculate the share of people on each voting board who changed employers between January 2006 (when our employer-employee data starts) and October 2007. We then include this share as an explanatory variable in our regressions and interact it with our measure of firm exposure. The idea is to grasp how relevant movers are in driving the effect. Table B.14 displays the results; note that the interaction term is a zero, both in magnitude and significance, which—consistent with the narrative from Figure B.1—does not support a story where selection into firms significantly drives the effect.

Table B.14: Share of Employees which Changed Employer and Firms' Exposure

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.035	0.034
	$(0.013)^{***}$	$(0.013)^{***}$
Movers	0.045	0.045
	(0.036)	(0.037)
Firm Exp_b^{Trade} × Movers		(0.0005)
		(0.013)
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.644	0.644

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Finally, our last exercise is an IV strategy. Namely, we fix the employer-employee network in January 2006, and we use exposures based on this fixed network as an instrument to predict "true" exposure measures. The idea is to "shut-down" the effect coming from movers, at the expense of having a potentially noisier measure. Table B.15 shows this IV's results. Note that the coefficient for firm exposure is slightly larger—albeit statistically equal—using the IV than under the OLS; the latter again would speak against a story where there is selection into firms as a way of anticipating the FTA's gains, which would have instead led to a lower IV coefficient.

Table B.15: IV Strategy: Firms' Exposure Instrumented Using January 2006 Employer-Employee Network

Dependent variable: $YesVoteSh_b$		
	Reduced Form	IV
	(1)	(2)
Firm $Exp^{Trade}_{b,2006}$	0.040	
	(0.016)**	
Firm $\widehat{Exp}^{Trade}_{b,2007}$		0.041
		(0.017)**
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.635	-
First Stage F-Statistic	-	259.3

Notes: Column (1) shows the reduced-form results of using the firm exposures based on the January 2006 employer-employee network as an explanatory variable. Column (2) shows the results of instrumenting the October 2007 firm exposures using the January 2006 employer-employee network. The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Robustness Checks

C.1 Results Corresponding with Figure 1

Individual vs. Household Exposure We constructed our measures of direct and indirect exposure (equations (1) and (3)) at the household level. This had the benefit of allowing us to match more voters to a firm, as we could link partners of employed people even if they were themselves unemployed. We find that this choice has no considerable effect on our results. The results when considering only individual exposure are statistically equal—albeit larger—to the ones leveraging household exposure. We report these findings in Table C.1.

Table C.1: Individual-Exposure: Firms' Direct and Indirect Exposure, and Employee's Voting Behavior

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.039	0.037
	(0.014)***	(0.014)***
Firm $IndirectExp(1)_b^{Trade}$		0.031
		(0.010)***
Firm $IndirectExp(2)_b^{Trade}$		0.008
		(0.010)
Firm $IndirectExp(3)_b^{Trade}$		-0.005
		(0.009)
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.633	0.635

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Additional Control Our design, both at the household and individual level, is robust to the inclusion of a demanding additional control, namely:

$$\sum_{j=1}^{n} \frac{X_{ji}^{US} + M_{ji}^{US}}{L_i}.$$

This term is similar to our main regressor described in equation (1), but it omits the exogenous tariff changes implied by the FTA. Adding this control is not standard in the shift-share literature as it is quite demanding in terms of variation, but it carries the benefit that identification would come solely from changes in tariffs, which can be regarded as exogenous shifts, as we have argued above. The results with this additional control are reported in Table C.2. Our results hold qualitatively at both the household and individual level, and the coefficient remains statistically equal to the coefficient in our main specification.

Table C.2: Firms' Direct and Indirect Exposure Controlling for Average Firm Trade with U.S. per Worker (the "Share" in our Instrument)

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
	Household	Individual
Firm Exp_b^{Trade}	0.026	0.033
	$(0.013)^{**}$	$(0.014)^{**}$
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.636	0.633

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Special Economic Zones Firms that trade with the U.S. might be operating within a Special Economic Zone (SEZ). Such firms might enjoy lower tariffs than other firms, at least for a number of years, which would alter the impact that the FTA would have on their profits. To control for this possibility, we include the share of production by firms within an SEZ as a control variable. As shown in Table C.3, we find that an employer having a larger share of sales within an SEZ reduces the likelihood that a worker would vote for the FTA, although this effect is not statistically significant. Our results about the role of firm exposure remain unchanged after including this control.

Table C.3: Firms' Direct Exposure and the Role of Special Economic Zones

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.035	0.035
	$(0.013)^{***}$	$(0.013)^{***}$
Sales in SEZ		-0.041
		(0.142)
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.636	0.635

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Lobbying Firms Some firms might exert more pressure on their workers than others, or might be more vocal about their political views or their position on trade policy. Since we have no information about the actions that individual firms took with respect to the FTA, we use comprehensive lobbying data as a proxy. In particular, we analyze data provided by the Supreme Electoral Tribunal on all contributions made by each firm from January 2007 to October 2007.[44] The data includes details about the amount donated, the exact date of the donation, the political party that received the donation, and the unique national identifier of the donating firm, which we can link to our other data about firms. Then, we construct an indicator variable that equals one if the firm donated within this time period to a party which was in favor of the FTA, zero if the firm made no donations, and minus one if the donation was to a party against the FTA. We then include the average of this variable by voting board as a control. This control is intended as a proxy for firms being vocal about the FTA, as political parties them-

[44]We choose this time period as presidential elections took place in February 2006, municipal elections took place in December 2006, and the referendum took place in October 2007. Including the months before January 2007 could contaminate the analysis with donations intended to support presidential or municipal candidates for reasons other than the FTA. No elections besides the referendum took place between January and October 2007.

selves conducted campaigns for and against the FTA that could be financed through donations. Table C.4 shows our results. We find that employment with a lobbying firm makes no difference in voter choice, as shown in column (1), and interacting our lobbying measure with trade exposure also leaves our results unchanged, as shown in column (2).[45] While this analysis could in theory be conducted constructing two separate measures for pro-FTA and anti-FTA contributions, such a split delivers noisier and insignificant results.

Table C.4: The Role of Lobbying Firms

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.034	0.035
	(0.013)***	(0.013)***
Lobbying Firm	0.345	-0.005
	(0.720)	(0.934)
Firm Exp_b^{Trade} × Lobbying Firm		2.976
		(6.352)
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.635	0.635

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Intellectual Property The FTA included guidelines regarding intellectual property (IP) rights. Our regressions control for industry shares, which would indirectly capture the differential IP intensity across sectors, and its effect on votes. We can, however, also include a variable with the patent intensity by industry, as measured by Hu and Png (2013).[46] As reported in Table C.5, we do not find that voters employed in patent-

[45] Our results remain unchanged if we include a control that uses the amount of money per worker donated by the firm instead of the dummy variable for lobbying firms.

[46] As this measure exists for manufacturing sectors only, we run the regression considering this subset of industries. For this regression, industry shares are defined at one digit, while

intensive industries behave differently than individuals in other sectors.

Table C.5: Voting and Intellectual Property

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.033	0.070
	(0.013)***	(0.028)***
Patent Intensity	-0.295	0.024
	(0.456)	(0.530)
Firm Exp_b^{Trade} × Patent Intensity		-3.797
		(2.784)
Controls/FE	Yes	Yes
Observations	4,738	4,738
Clusters	1,765	1,765
Adjusted R^2	0.639	0.639

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. The variable "Patent Intensity" is the mean patent intensity, as measured by Hu and Png (2013), corresponding with employers of voters at each voting board. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Informal Workers and Other Groups Different degrees of informality may alter results. While there is no individual-level data on informality status, we can proxy for it relying on observable characteristics. We define an informal worker as an individual who is between 18 and 64 years of age in 2007, is not employed or a student, is not married to an employed, *and* does not appear as an employed person within one year of 2007. The latter intends to exclude unemployed job searchers. Using this definition, 29% of citizens are classified as informal; a number remarkably close to the 27% reported in national surveys (ILO, 2002). As shown in column (2) of Table C.6, however, we do not find the share of informal workers at each voting board plays a statistically significant role in shaping vote shares. The same is true when we control for the share of INS or ICE workers and the share of retirees, as shown in columns (3) and (4); the effect of

the patent intensity depends on 2-digit industry definitions, so that the industry shares do not absorb the patent-intensity variation.

Table C.6: Informality, Voting, and Other Checks

Dependent variable: $YesVoteSh_b$

	(1)	(2)	(3)	(4)
$FirmExp\_b^{Trade}$	0.035	0.035	0.035	0.035
	(0.013)***	(0.013)***	(0.013)***	(0.013)***
Informality		-0.158		
		(0.058)***		
INS or ICE			-0.420	
			(0.416)	
Retired				-0.043
				(0.080)
Controls/FE	Yes	Yes	Yes	Yes
Observations	4,914	4,914	4,914	4,914
Cluster	1,934	1,934	1,934	1,934
Adjusted R-squared	0.636	0.637	0.636	0.635

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

firm exposure does not change when introducing these additional variables.[47]

C.2 Voting and Ex-Post Outcomes

Measures of ex-ante exposure reflect how voters' conditions at the time of the referendum influence their choice. In this section, we ask whether voting behavior reflected correct perceptions of the benefits that emerged from the approval of the FTA, *but that were not necessarily captured by ex-ante conditions*. For instance, a worker might have anticipated that she could get a better job if the FTA was approved. This might have influenced her vote, but would not be captured by our measure of a predicted change in earnings that relies on employer exposure at the time of the referendum, because the anticipated improvement in earnings would result from a change in employer.

To test this possibility, we exploit the fact that the FTA was indeed approved—albeit by a small margin and somewhat unexpectedly—and we calculate the discounted change in real earnings experienced by each voter h in the years after the referendum, as in equation (5). Note that this expost-analysis begins in January 2009. The reason is

[47]The definition of retirees and students coincides with the one provided in Section 4.1, footnote 17.

that, while CAFTA was ratified in late 2007, it did not become effective until January 1st, 2009, as there were legal obstacles that needed to be overcome before the agreement could be fully implemented in the country.

We then follow two alternative approaches. The first approach—reported in Table 1—considers the residual of a regression of the term in (5) on our measure of direct firm trade exposure, Exp_b^{Trade}. This residual term, which we will call *Ex-post* w_h, is intended to capture drivers of ex-post income that are not captured by ex-ante direct trade exposure. We include *Ex-post* w_h in our main specification, and find that it has no explanatory power and is almost zero in magnitude, as reported in panel (a) of Table C.7.

In our second approach, we construct a counterfactual wage, which results from using the real wage growth of voters *before 2007* (defined as gr_{wage}) to project the wage path from 2007 onward.[48] Finally, we subtract the present discounted value of the counterfactual real wage from the present discounted value of the actual wage profile, as follows:

$$\Delta \textit{Ex-post}_h = \sum_{t=2}^{T} \beta^t \frac{wage_b^{2007+t}}{CPI^{2007+t}} - \sum_{t=2}^{T} \beta^t \frac{(1+gr_{wage}^{Pre-2007})^t wage_b^{2007}}{CPI^{2007+t}}. \tag{10}$$

Using the differences in wage profiles, as opposed to the profiles themselves, carries the advantage that the differences are not collinear with 2007 wage levels. We then run equation (6) including $\Delta \textit{Ex-post}_h$. As columns (1) and (2) of panel (b) in Table C.7 show, and consistent with findings from our first approach, we find no evidence that ex-post differential outcomes factored into voting decisions. The same result holds if we divide $\Delta \textit{Ex-post}_h$ by the present discounted value of counterfactual wages and run our estimation again, as shown in columns (3) and (4) in panel (b) of Table C.7. This evidence suggests that ex-ante exposures are good measures of voters' perceptions of the FTA's effects.

[48] Our data on wages starts in 2006, which poses a challenge for the estimation of $gr_{wage}^{Pre-2007}$. To overcome it, we use a random-effects panel-data GLS regression to estimate the average wage growth of a person within the same age-sex-industry-occupation-sector group in 2006-2007, which also captures unobserved heterogeneity. The fixed-effects panel data GLS regression delivers statistically equal results.

Table C.7: Referendum Results and Ex-Post Outcomes

Dependent variable: $YesVoteSh_b$

	(1)	(2)	(3)	(4)
	Panel (a): Approach 1			
Ex-post w_b	0.00002	0.00001		
	(0.00010)	(0.00010)		
Firm Exp_b^{Trade}		0.035		
		(0.013)***		
Adjusted R^2	0.634	0.635		
	Panel (b): Approach 2			
	Levels		Percentage changes	
Ex-post w_b	0.00003	0.00002	0.0002	0.0001
	(0.00009)	(0.00009)	(0.004)	(0.004)
Firm Exp_b^{Trade}		0.034		0.034
		(0.013)***		(0.013)***
Adjusted R^2	0.635	0.635	0.635	0.635
Controls/FE	Yes	Yes	Yes	Yes
Observations	4,908	4,908	4,908	4,908
Clusters	1,928	1,928	1,928	1,928

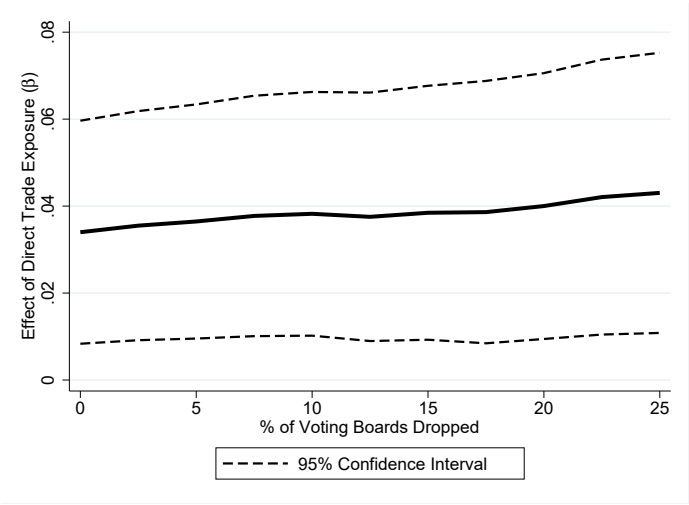
Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics, average characteristics of people voting at the voting center, and region fixed effects. Panel (a) presents results following estimation Approach 1. Panel (b) uses estimation Approach 2. In Approach 2, we use a random-effects panel-data GLS regression to estimate the counterfactual average wage growth of a person within the same age-sex-industry-occupation-sector group as each voter in 2006-2007, which also captures unobserved heterogeneity. Columns (1) and (2) of panel (b) present results when considering income changes in levels, while columns (3) and (4) of panel (b) consider income changes in percentages. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.3 Attenuation Bias

If we consider the effect of trade on the voting board as a whole, our estimates are consistent. However, as we compute exposure at the level of voting boards by taking averages of the individuals assigned to vote at each board, our estimates might suffer from attenuation bias if we interpret coefficients at the individual level. To address the fact that we do not observe individual votes, we leverage differences in voting behavior across voting boards. Intuitively, if the within-voting board correlation is one, then our estimates are consistent. This would be the ideal case in which we observe exactly how

each individual behaved (for instance, if everyone on a board votes in the same way). In general, however, the correlation would be positive but not perfect, though we can use information on the distribution of votes within a board to assess the attenuation bias. In particular, we examine how outcomes change across voting boards with different shares of pro-FTA votes. Figure C.1 shows how our estimates change if we consider only voting boards in which the distribution of votes in favor or against the FTA was relatively extreme. We use a range of cutoffs, from dropping 0% to dropping 25% of the voting boards that are closer to a 50-50 "yes" vs "no" outcome. Overall, we find evidence of a bias—estimates become slightly larger as the subset of voting boards considered return voting results closer to the tails—but the magnitude of the bias is small. The coefficient remains very stable and statistically equal to the value it takes when considering all voting boards.

Figure C.1: Impact of Trade Exposure After Dropping Voting Boards with Results Close to 50-50



Notes: The figure shows how our main estimate changes when we consider only voting boards where the difference in "yes" vs "no" votes is larger than a certain threshold, for different thresholds that range from 0% to 25%. The 95% Confidence intervals (dashed lines) are based on robust standard errors clustered by voting center (school).

C.4 Indirect Exposure Through Family Networks

Section 4.1 shows that workers vote depending on their *own* household's earnings exposure to the FTA. We now explore whether voting behavior is influenced by the exposure of close relatives. In other words, we test whether the exposure of family members, be-

yond someone's partner, can explain the observed voting patterns. We leverage data on family networks in Costa Rica, which allows us to identify a voter's g-degree relatives.[49] This information allows us to construct the following measure for each individual h:

$$IndirectExp(g)_h^{Relatives} = \sum_{n=1}^{N} \frac{w_n}{\sum_{n=1}^{N} w_n} \frac{Exp_n^{Trade}}{N},$$

where we sum across the N g-degree relatives of person h, and then take an income-weighted average of the relatives' direct trade exposures, calculated as in equation (1). Table C.8 shows our results after averaging $IndirectExp(g)_h^{Relatives}$ for individuals at each voting board. As shown in column (1), we do not find evidence that voters' choices respond to the level of exposure of their close relatives. This result holds true after controlling for self-exposure in column (2).

[49] First-degree relatives include parents, siblings, and children. Second-degree relatives include grandparents, grandchildren, uncles, aunts, nephews, and nieces. Third-degree relatives include great-grandparents, great-grandchildren, great-uncles/aunts, and first cousins.

Table C.8: Indirect Exposure Through Family Networks and Voting Behavior

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}		0.033
		(0.011)***
$IndirectExp(1)_b^{Relatives}$	0.019	0.006
	(0.012)	(0.012)
$IndirectExp(2)_b^{Relatives}$	-0.003	-0.007
	(0.019)	(0.019)
$IndirectExp(3)_b^{Relatives}$	-0.000	-0.001
	(0.016)	(0.016)
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.635	0.636

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.5 Participation in the Referendum

Table C.9: Participation in Referendum Explained by Culture and General Civic Engagement

Dependent variable: Participation in 2007 Referendum

Participation in 2006 Presidential Election	0.749
	(0.019)***
Firm Exp_b^{Trade}	0.006
	(0.006)
Controls/FE	Yes
Observations	4,914
Clusters	1,934
Adjusted R^2	0.898

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting center (school), are in parentheses. Voting boards are weighted by their number of voters. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.6 Attitudes in Costa Rica vs. Other Countries

This section aims to benchmark attitudes toward openness in Costa Rica, at the time of the 2007 referendum, against views on trade openness and globalization in other countries. In particular, we ask: were views toward trade in Costa Rica more positive than in most other countries prior to the referendum? This poses two main challenges: (i) the referendum took place almost 15 years ago, and to make an accurate comparison, we need information on views during that time period, and (ii) we need a way to measure attitudes toward openness that is reasonably comparable across countries, even though Costa Rica is not included in surveys that ask respondents about trade policy, like those by the International Social Survey Programme (ISSP) studied by Mayda and Rodrik (2005), or those regularly conducted by Gallup in the U.S.

We overcome these two challenges by (i) obtaining *microdata* on *a series* of nationally representative surveys conducted by PROCESOS (a local consulting firm) and analyzed by Rodríguez et al. (2008) during the months preceding the referendum—one

of them being the same week of the vote, and (ii) identifying questions in these surveys that are comparable to those asked by the ISSP in other Latin American countries, and by Gallup in the U.S.

Concretely, the questions we focus on in the representative surveys conducted across Costa Rica are:

(a) *Is globalization something that harms or benefits the country?*

(b) *Is trade liberalization something that harms or benefits the country?*

For both of these questions, the survey gave the following possible answers: Harms, neither harms nor benefits, benefits, both, and depends. Following Mayda and Rodrik (2005), we construct a dummy variable that takes the value of one if the response was "benefits." Using the same logic across different surveys will allow us to make them comparable.

Then, we leverage the 2003 and 2013 ISSP studies, which survey 43 different countries of the world (not including Costa Rica), and consider the following question for three countries in particular: Mexico and Chile—which are the Latin American countries in the sample that are closer to Costa Rica in GDP per capita—and the U.S.

(c) *Free trade leads to better products becoming available in [COUNTRY].*

The possible answers to this question were: Agree strongly, agree, neither agree nor disagree, disagree, and strongly disagree. We constructed a dummy variable that takes the value of one if the response was "agree strongly" or "agree."

Finally, Gallup Poll Social Series includes a question on views of foreign trade; in particular, they ask:

(d) *What do you think foreign trade means for America? Do you see foreign trade more as an opportunity for economic growth through increased U.S. exports, or a threat to the economy from foreign imports?*

The possible answers to this question are: An opportunity for economic growth, a threat to the economy, both, and neither. Consistently with how we constructed dummies in previous surveys, we generate a variable equal to one if the answer was "An opportunity for economic growth," and zero otherwise.

The results of comparing the responses across these surveys and countries are presented in Table C.10. For each survey, we present the responses for the years closest to the 2007 referendum in which the surveys were conducted and these questions were

asked. From these findings, *it is hard to conclude that Costa Rica is an outlier during this time period, and if anything, resembles attitudes toward trade in the U.S. in recent years.*

Table C.10: Comparison of Attitudes Toward Openness Across Countries

(1)	(2)	(3)	(4)	(5)
Country	Year	Question	Percentage Pro-Openness	Source
Costa Rica	2007	(a)	64%	Rodríguez et al. (2008)
Costa Rica	2007	(b)	80%	Rodríguez et al. (2008)
Chile	2003	(c)	79%	ISSP
Mexico	2013	(c)	63%	ISSP
U.S.	2003	(c)	57%	ISSP
U.S.	2006	(d)	43%	Gallup
U.S.	2017	(d)	72%	Gallup
U.S.	2020	(d)	79%	Gallup

Notes: The questions referenced in column (3) correspond with those in *italics* enumerated in Section C.6. Percentages in Column (4) result from constructing dummies equal to one if the response to the question was pro-trade or pro-openness, and zero otherwise. We include the last two rows to give some perspective on the current attitudes in the U.S.

D Expenditures Channel: Details and Results

This section provides additional details on the construction of our measure of exposure via expenditures. The starting point of this construction is the National Household Income and Expenditure Survey (*Encuesta Nacional de Ingresos y Gastos de los Hogares (ENIGH)*). This survey aims to understand households' expenditure structure, and how they spend their income across goods and services in a detailed consumption basket. In fact, the National Household Income and Expenditure Survey is used to identify the articles that constitute the basket that determines the Consumer Price Index and its corresponding weights. The survey is representative at the regional level, and the results include several respondent characteristics, such as income, occupation, location, gender, age, and marital status. We use the last survey conducted before the 2007 referendum, which took place in 2004, and sampled 5,287 housing units. More details on the survey can be found in Instituto Nacional de Estadística y Censos (INEC) (2006).

With the survey at hand, we can then analyze which consumption baskets tend to be purchased by households. For these surveyed households, we then compute an expected change in the price of their consumption basket, based on the share of the good that is imported from the U.S. and its expected change in tariffs. This computation,

described in equation (7), follows Fajgelbaum and Khandelwal (2016). Note that this term, which will be our measure of exposure via expenditures, will have units which are in U.S. dollars, and depends on the expected price changes of goods in the consumption basket. As explained in Section 5, we calculate the price changes for each good based on the share of total domestic absorption of the good multiplied by the expected tariff change; thus, assuming complete pass-through.[50] Assuming complete pass-through in this particular setting might not be unreasonable, as most voters are unlikely to take a more-sophisticated approach to predict a change in the price of their consumption basket. For instance, consider a good which is sourced 20% from the U.S., while the other 80% of the good's consumption originates either from Costa Rica or from other countries, and assume that the good would face a tariff drop of 3%; the expected change in price for would then be 6%.

Now, of course, not all Costa Rican households are included in the survey, and we have only computed this exposure through expenditures for those in the survey. Thus, we make progress by using a lasso regression to select the observable characteristics which better explain each household's exposure via expenditures. In doing this, we leverage that we have a rich set of observables *both in the survey and for every other household in the country.*

As a first step to implement the lasso regression, we identify the set of explanatory variables in the ENIGH that are also found in, and strictly comparable to, observables in the other datasets. In particular, the ENIGH data contains the census-block (the smallest statistical territorial unit division of the country) where the surveyed household is located. Moreover, we identify the closest voting center to each census-block and define a neighborhood as the set of census blocks that share the nearest voting center. Then, using data from the 2000 Population and Housing Census, which is also geo-referenced at the census-block level, we can obtain observable characteristics for all census-blocks, and consequently, for all neighborhoods.[51]

As a result, the variables considered by the lasso regression as potential predictors include household-level characteristics (like wage, occupation, and demographics), and neighborhood-level characteristics, such as:

- Location (region, urban/rural area).

[50] The domestic absorption for each good category is obtained from national accounts data.

[51] The 2000 Census was the closest to the 2004 ENIGH, which is why we use it in this estimation.

- Head of household characteristics (average age, occupation composition, average years of schooling).
- Household dwelling characteristics (quality of the dwelling's material, incidence of overcrowding, type of sanitary service, main source of water).
- Neighborhood members' demographic composition (age, sex, migrant, social security coverage, academic achievement, average years of schooling).
- Neighborhood members' economic composition (employment status, average income earners per household, occupation).

Then, we split the households in the ENIGH into training and testing samples, consisting of 75% and 25% of the data, respectively. Using the training sample, we estimate the lasso regression, considering a 10-fold cross-validation, an adaptive lasso, and a plugin iterative formula to find the optimal value of the tuning parameter.

The plugin estimator gives the model with the lowest mean squared error of the out-of-sample prediction. The corresponding predictors selected by the lasso in this model at the household level are the wage and occupation, while at the neighborhood level, are the age, migrant, and sex composition; the social security coverage; employment status; academic achievement, and the average years of schooling.

Finally, we use the selected variables to predict each voter's exposure to the trade agreement via household-level expenditures.

E Political Alignment: Details and Results

Table E.1: Results on Political Alignment Using the 2002 Presidential Vote Shares to Instrument for the 2006 Presidential Vote Shares

Dependent variable: $YesVoteSh_b$		
	Reduced Form	IV
	(1)	(2)
$Pres_b^{2002}$	0.297	
	$(0.033)^{***}$	
$\widehat{Pres_b}^{2006}$		0.498
		$(0.050)^{***}$
Firm Exp_b^{Trade}	0.032	0.032
	$(0.013)^{**}$	$(0.011)^{***}$
Controls/FE	Yes	Yes
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.648	-
First Stage F-Statistic	-	479.4

Notes: The unit of observation is the voting board. Column (1) shows the reduced form results of using the share of votes for a pro-FTA political party in the 2002 presidential election at a voting board as an explanatory variable. Column (2) instead shows the results using these 2002 election shares as an instrument to predict values for the corresponding 2006 election share. All regressions control for voter's average characteristics (age, wage (thousands of USD), gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firm trade with the U.S.), and average characteristics of people voting at the school (average years of schooling from census data geo-referenced at the census-block level and distance of the average voter to the school); and region fixed effects. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.