Voting on a Trade Agreement: Firm Networks and Attitudes Toward Openness*

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April 24th, 2022

First draft: June 2nd, 2021

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 with employer-employee data, customs and balance-sheet data, 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 is greater for voters who are aligned with pro-FTA political candidates. We find that import competition plays a role in explaining votes against openness, and that within-industry heterogeneity is key in explaining votes, as compared with sector-level exposure. We also show that citizens considered the expected decrease in consumer prices when exercising their vote.

Keywords: firm networks, trade policy, gains from trade

*Corresponding author: diana.vanpatten@yale.edu. We thank David Argente, Costas Arkolakis, David Atkin, Andy Bernard, Matilde Bombardini (discussant), Ariel Burstein, Lorenzo Caliendo, Thomas Chaney, Davin Chor, Arnaud Costinot, Ben Faber, Cecilia Fieler, Gordon Hanson, Doug Irwin, Pablo Fajgelbaum, Gene Grossman, Amit Khandelwal, Sam Kortum, Giovanni Maggi, Eduardo Morales, Nathan Nunn, Steve Redding, Andrés Rodríguez-Clare, Dani Rodrik, Esteban Rossi-Hansberg, Ken Scheve, Todd Schoellman, Pete Schott, José Vásquez, Eric Verhoogen, David Weinstein, and seminar participants at Yale University, Princeton University, Harvard University, GEP/CEPR Summer Trade Workshop, Federal Reserve Bank of Minneapolis, University of Southern California, Central Bank of Costa Rica, Universidad de Los Andes, U.S. Census Bureau, UC Berkeley Haas, and Columbia University for helpful comments and discussion. The views expressed herein are those of the authors and do not necessarily represent the views of the Central Bank of Costa Rica.

1 Introduction

Given the importance elected officials grant to public attitudes about trade policy, an understanding of the possible correspondence between public sentiments and economic determinants is relevant to policymakers. Survey evidence suggests that economists and the broader public view trade issues in starkly different ways (Blendon et al., 1997; Sapienza and Zingales, 2013). An analysis of the similarities and differences between expert and layperson perspectives on trade could increase the effectiveness of economic policy. Moreover, analyzing the determinants of public attitudes toward trade openness can, in turn, inform economic theory and aid in discriminating between different models that predict 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 nebulous and 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.

At the time, 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.

We measure the extent to which firms, sectors, skill groups, and locations were exposed to the tariff changes implied by the trade agreement. We then examine

[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.

the relationship between these exposures and voter behavior, controlling for other factors that might influence voter choice. Several characteristics of the setting and the data allow us to relate economic drivers to voting behavior precisely and with a causal interpretation. In Costa Rica, each voter is allocated by place of 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 they were 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 in great detail. 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 62% of households to a firm once we exploit the information on partners.

In terms of the economic fundamentals that could be relevant to voters' decision in the referendum, we consider two different channels: earnings and expenditures. To explore the earnings channel, we compare the importance of three factors: firms, sectors, and skill groups. That is: (i) we measure how a firm's dependence on international trade shapes its employees' attitudes toward openness, (ii) we study how a sector's exposure to trade affects the attitudes of people employed in that sector, and (iii) we examine the relevance of factor endowments and occupations. We also analyze the role of local labor market import competition as a relevant factor that might affect voters' earnings.

We measure firms' trade exposure from how a firm's input-output structure would be affected by the change in tariffs that would result from the ratification

of CAFTA, while controlling for other firm characteristics like size and trade with the U.S.[2] This metric is helpful for our identification of causal factors driving voter behavior for several reasons. First, if the agreement were to be ratified, roughly all tariffs (over 99%) would fall to zero, which is a source of plausibly exogenous variation. Second, while a worker of a certain type and with a certain ideology might select herself into a large multinational, and while this selection could be a confounding factor, workers are unlikely to choose an employer based on how the employer's input-output structure interacts with the change in tariffs, *conditional on other characteristics of that firm.* We test this last statement in several ways, including a placebo using municipal-election results. We also verify that exposure to the FTA is unable to explain voter characteristics conditional on our controls.

Our data on firm-to-firm transactions allows us to construct three distinct measures of employer exposure: (i) *direct* trade exposure, which depends on the products the firm is trading (exporting and importing) with the U.S. and the change in the tariffs on those products if the FTA were to be approved; (ii) *indirect* firm-to-firm exposure, whereby an employer is exposed via trading partners who are themselves directly exposed, for instance, does it matter that a domestic firm is trading with firms that have a large *direct* exposure?; and (iii) *indirect* exposure through family networks, whereby an employee is exposed to the FTA via relatives who work for a directly affected firm. The latter behavior would be evidence of altruism or other psycho-social considerations. To the best of our knowledge, this is the first study to consider the role of each of these three measures of trade exposure in shaping public attitudes toward trade policy. Namely, ours is the first study to consider the role of within-industry heterogeneity with information about all firms in shaping popular support, to document the salience of an indirect exposure measure via input-output linkages, and to estimate exposure via relatives.

We find that within-industry heterogeneity (i.e., firm-level exposure) is salient to voters. In particular, a $1,000 increase in the average direct trade exposure of individuals at a voting board is associated with a 3.4 percentage point (pp)

[2]The final tariff schedule was already established when the referendum took place.

increase in the share of votes in favor of the FTA at that board.[3] Indirect exposure for firms that are one link away from a directly exposed firm also matters to voters. The magnitude of the coefficients for indirect exposure is approximately two-thirds of the coefficients for directly exposed firms. This highlights the role of indirect exposure to trade via the firm network in shaping workers' attitudes toward trade; a channel that so far has remained largely unexplored by the literature.

These results are robust to a battery of controls, including voters' political inclination. 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.

Beyond the firm, our examinations of the roles of exposure by sector and occupation in affecting voter choice are designed to test the predictions of the Stolper and Samuelson (1941) and specific-factors models. We find that, conditional on firm exposure, sector exposure is insignificant. This result highlights the importance of within-industry heterogeneity in determining the distributional effects of trade. Moreover, we document that low-skilled voters, who correspond with the relatively abundant factor of production in Costa Rica when compared with the U.S., are more likely to vote *against* the FTA.

Attitudes toward the FTA could be affected by local labor markets and import competition (Autor et al., 2013). Our examination of this channel suggests that import competition in local labor markets might influence voters to position themselves *against* the trade agreement, even conditional on firm-level exposure, regardless of the measure of local import competition we consider.

The measures of exposure discussed so far rely on conditions related to voters' earnings at the time of the vote that may have influenced their choice. However, voters might have been forward-looking and accounted for benefits that would emerge from the approval of the FTA, *but that were not captured by ex-ante con-*

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

ditions.[4] To explore this possibility, our final consideration of the earnings channel studies ex-post outcomes, leveraging that the FTA was actually approved to calculate the discounted change in earnings experienced by a voter *after* the FTA came into force. We find evidence against voters correctly using information that was not captured by ex-ante conditions. Individuals who happened to fare better after the FTA was implemented were not more likely to vote in favor of the FTA, conditional on their wage and exposure at the time of voting. This result supports the notion that ex-ante exposures are a good measure of a worker's perception of what the effects of the FTA might be.

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

2 Related Literature

Our work contributes to literature in economics and political science that asks whether individuals' policy preferences reflect economic principles. This question

[4]For example, a voter might have correctly perceived that she could get a better job if the FTA was approved, and this might have influenced her vote.

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 at all 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). 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 of clear theoretical predictions that we can measure and test precisely. Thus, the fact that we document a non-zero result is, in itself, an important contribution to this debate.

Moreover, the present study 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 de-

scribed 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.

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, in order 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. 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 provides an overview of the setting, including details about the FTA and voting in Costa Rica. Section 4 presents details about the data used in our analysis. We describe our measures of firm exposure to the trade agreement and how they impact attitudes toward trade in Section 5. Section 6 discusses the role of occupational skill groups and sectors in shaping voting outcomes. Section 7 explores the role of import competition in local labor markets. Section 8 studies the role of ex-post outcomes. Political views' effects on voting are explored in Section 9. Section 10 explores the effect of the expenditures channel on voting decisions, and Section 11 concludes.

3 Background

3.1 The Free Trade Agreement: CAFTA

In August 2004, after nearly two years of negotiations, 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 guidelines regarding intellectual property rights and telecommunications provisions. The matter at hand was quite relevant to workers in Costa Rican firms as tariffs for trade with the U.S. were considerable; the U.S. was Costa Rica's main trading partner, accounting for 45% of the country's imports and exports, and Costa Rica's trade-to-GDP ratio was 86%. The agreement would immediately drive most of the tariffs on goods and services

traded with the U.S. to zero.[5] Table B.1 in Appendix B shows the average changes in export and import tariffs by industry, along with each industry's share of in the country's total exports and imports in 2007.[6] 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.

Costa Rica was the only country that had not ratified CAFTA by late 2005, due to delays in the vote of its Legislative Assembly.[7] Thus, as a way to reach a decision before the ratification deadline, and after receiving approval from the Supreme 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 in Appendix A shows a sample of the referendum ballot. Despite the national referendum being about this issue only, participation was high; on October 7$^{\text{th}}$ 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

[5]Ninety-five point nine percent of the tariffs on exports to the U.S., and 83.8% of tariffs on U.S. imports, would go to zero as soon as the agreement was in effect.

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

[7]The opposition delayed the vote on the agreement repeatedly.

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 in Appendix A 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.

4 Data

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]

[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.5 in Appendix B shows the results are robust to using all voting boards.

Family Networks 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 particularly useful later on, to capture individuals who are not in the labor force, but are married to someone who is employed, when thinking about a household's exposure to the FTA. We will also use this data to construct kinship networks 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 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,

[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.

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 26th, 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]

5 Firm Exposure to the FTA and Employee Voting Behavior

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. They also mention that importing can generate a wage premium at importers insofar as imports increase productivity and revenue per worker. Thus, changes in trade costs, like tariffs, can affect worker welfare via earnings. 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

[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.

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.

5.1 Constructing Measures of Firm Exposure

In this section, using the tariff changes by 6-digit HS code that would derive from CAFTA, we calculate measures of each Costa Rican firm's direct and indirect exposure depending on its input-output structure. We then compute the changes to the firm's revenue and cost functions that would come along with the tariff changes in CAFTA.

Direct Firm-Level Exposure to the FTA We construct several measures of a firm's exposure to the trade agreement. 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.[12] In particular, 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 change in export tariffs to the U.S. for product j, 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.

[12] 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.

Importantly, this construction exploits the fact that our data distinguishes between imports of *inputs* and imports of *final goods*, which allows us to restrict the measure to only imports of inputs.[13] We normalize this measure 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 part of equation (1), $Exp_i^{Trade} \propto \Delta w_i$.[14] This measure of a firm's exposure via input-output linkages to the U.S. leverages our data about each firm's balance sheets, customs transactions, and the expected changes in tariffs due to CAFTA. Figure A.3 in Appendix A summarizes the variation in this measure across space.

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

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

[13] This distinction is useful, as imports of final goods might capture an import competition effect that, for the purposes of this analysis, we do not want to consider just yet. We address the impact of import competition on voting outcomes in Section 7.

[14] 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.

for a chain of domestic traders of length K.

5.2 Individual and Household Exposure to the FTA via Earnings

After calculating each firm's direct and indirect exposure, 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.[15] 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 62%. This success rate in matching voters to firms is close to the best possible, as approximately 10% of voters are over 65 years of age and 27% of voters work in the informal sector (Alfaro-Ureña et al., 2022); thus, we are roughly capturing the entire remaining share.[16]

[15]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).

[16]Note that, given the nature of our shock, which hits firms trading internationally, it is not

5.3 Empirical Strategy

Now that we have measured each voter's exposure to the FTA through her earnings, we can study the relationship between these measures and voting outcomes at each voting board. The ideal experiment to establish causal identification would be to randomly assign exposure to voters. As our measure of household exposure to the FTA is highly specific, we can closely approximate this ideal scenario by isolating a component of trade exposure that impacts firms through the FTA shock, but is not a determinant of how workers select into firms. For instance, a highly educated worker might be more likely to work at a large exporting firm, and she might also be more likely to support free trade, regardless of her employer. However, this worker is unlikely to select into a firm depending on its input-output structure and how that structure interacts with the tariff schedule, *conditional on other characteristics of that firm* (like its size, its exports and imports, whether it is a multinational, etc.). Moreover, importantly, the agreement completely eliminated tariffs for more than 99% of goods, which provides a source of plausibly exogenous variation for our "shifts."[17] We then consider the following specification:

$$YesVoteSh_b = \alpha + \beta Exp_b^{Trade} + \sum_n \delta_n IndirectExp(n)_b^{Trade} + \Gamma X_b + D_r + \varepsilon_b, \quad (4)$$

where $YesVoteSh_b$ refers to the share of pro-FTA votes at each voting board b, and Exp_b^{Trade} denotes the average direct trade exposure of employers of citizens assigned to voting board b (in thousands of dollars), and $IndirectExp(n)_b^{Trade}$ stands for the indirect exposure at n links faced by the employers of citizens in voting board b. X_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 level of voting

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.

[17] Furthermore, for over 96% of goods, the tariffs would fall to zero immediately after the FTA was in place.

board 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 D_r denotes region fixed effects.[18]

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 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.[19] In our data, a linear probability model delivers fitted values that lie within the $[0, 1]$ interval for 100% of voting boards. The linear probability model also admits of a straightforward interpretation. As we are not taking the logs of any of our variables, a linear probability model also has the desirable feature that, after some corrections that will be discussed in Section 5.6, 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.[20] Figure A.4 in Appendix A 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.[21]

5.4 Results on Firm Exposure

Direct Firm Exposure Table 1 shows that direct firm exposure is salient to voters. In general, referendum votes were cast in alignment with the interests of voters' employers. For instance, from Column (1), we can see that an increase of

[18]The 2011 Census was the closest to the 2007 referendum, which is why we use it in our main specification. Table C.14 in Appendix C shows that the results remain statistically equal if we instead use the second-closest census, which took place in 2000. Regions correspond with municipalities.

[19]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.

[20]In Appendix B, 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).

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

$1,000 in an employer's exposure is associated with a 3.4 pp higher share of votes in favor of the FTA. This shift corresponds with a 6.8% increase with respect to the mean. How would the result 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.[22]

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 magnitudes of the coefficients of indirect exposure are approximately two-thirds of the coefficients of directly exposed firms. This result highlights the role of indirect exposure to trade via the firm network in shaping worker attitudes toward trade, which is a channel that has remained largely unexplored in the literature.

Beyond this one-link-away relationship, we do not find any significant effect of firms connected via their network. That is, the exposure of firms two or more links away from a directly exposed firm is insignificant to voters employed by that firm, as reported in Table B.2.

Table 1 also reports details on several control variables. These results provide insights on the drivers of trade preferences more generally. Our findings indicate that voters who are older, employed at firms that trade with the U.S., make higher wages, and who have more years of schooling are more prone to vote in favor of the FTA. We also document that employees working in the public sector are much more likely to vote against the FTA; if every voter at a voting board was employed in the public sector, the voting board would be 83 pp more likely to vote against the FTA. The latter is consistent with one of the implications of the FTA being that public firms that enjoyed monopolies in the provision of services (in particular, the country's insurance provider and telecommunications provider)

[22]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.

Table 1: Direct and Indirect Firm Exposure and Voting Behavior

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.034	0.033
	(0.013)***	(0.013)**
Firm $IndirectExp(1)_b^{Trade}$		0.023
		(0.005)***
Controls/FE	Y	Y
Details on some control variables		
Age	0.006	0.006
	(0.001)***	(0.001)***
Female	-0.050	-0.043
	(0.042)	(0.042)
Wage	0.317	0.316
	(0.032)***	(0.032)***
Years of Schooling	0.009	0.010
	(0.002)***	(0.002)***
Firm's Trade with U.S.	0.188	0.164
	(0.079)**	(0.081)**
Public Sector	-0.829	-0.834
	(0.165)***	(0.165)***
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 (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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

would begin to face competition from private firms. It is worth noting that all of the predicted values derived from these regressions lie between 0 and 1, as shown in Figure A.5 in Appendix A.

5.5 Identification

Our identification relies crucially on the measure of exposure to the trade agreement being orthogonal to other voter characteristics. Our measure of household exposure to the FTA depends on how the employer's input-output linkages with the U.S. interact with the structure of tariff changes. As effectively all tariffs would fall to zero under the FTA, these changes provide plausibly exogenous sources of variation. Further, our exposure measure also depends on household composition. Given these factors, we reason that a worker is unlikely to base her choice of employer on this specific measure, *conditional on other characteristics of that firm.*[23] A threat to this strategy would be, however, if our controls for firm characteristics are not capturing all the selection-relevant factors. To explore this further, we conduct several tests.

First, we construct a placebo test using the results of the 2006 municipal elections as our dependent variable. In this test, our measure of exposure should not be explaining voters' behavior conditional on other voter- and firm-level characteristics. The reason is that national trade policy is not decided by majors, thus, CAFTA was not part of the candidates' agendas. Moreover, our measure of exposure relies on the final tariff schedule, which was not defined until January 2007, while the elections took place in December 2006.

As shown in Table B.6 in Appendix B, we find that the measure of firm-level exposure to the FTA is slight in magnitude and is insignificant as an explanation for voter behavior in the 2006 municipal election. This result supports our claim that, conditional on our controls, our measure of firms' exposure to changes in tariffs is not picking up other confounding factors that might influence voters' behavior.

[23]Partly as the final tariff schedule was unknown prior to January 2007.

Second, using voter characteristics as our dependent variable, we test whether our measure of firm exposure explains these characteristics, conditional on our other controls. If our controls are indeed sufficient, our measure of firm exposure should prove insignificant in explaining voter traits. To explore this, we use individual voter traits as our dependent variable, and regress voter characteristics on our measure of firm exposure (Exp_b^{Trade}), and our vector of controls (X_b) while excluding the dependent variable from the set of controls. Table B.7 shows that when we consider the average worker's wage, skill, and average years of schooling as dependent variables, Exp_b^{Trade} is insignificant in explaining these voter characteristics after including other controls (in Columns (1) (2), and (3), respectively).

5.6 Robustness and Discussion

Separating the Effects of Exports, Imports of Inputs, and Imports of Final Goods While our main, theoretically consistent, 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 gives us the opportunity to separately identify the effects of lowered export tariffs, lowered tariffs for input imports, and lowered tariffs final-good imports. These effects might differ, for instance, if exports are more salient for workers than imports.

We leverage the fact that our data distinguishes between imports of *inputs* and imports of *final goods*, which allows us to define the following measures of exposure for firm i via exports, imports of inputs, and imports of final goods, respectively:

$$
\begin{aligned}
Exp_i^X &= \sum_{j=1}^{n} \frac{X_{ji}^{US}}{L_i} \Delta\tau_j^{US,X}, \\
Exp_i^{M\,inputs} &= \sum_{j=1}^{n} \frac{M_{ji}^{US,inputs}}{L_i} \Delta\tau_j^{US,M\,inputs}, \text{ and} \\
Exp_i^{M\,final} &= \sum_{j=1}^{n} \frac{M_{ji}^{US,final\,goods}}{L_i} \Delta\tau_j^{US,M\,final\,goods}.
\end{aligned}
$$

Replacing the measure of trade exposure in equation (4) with these terms lets us estimate the results in Table C.8 in Appendix C. When considered separately, the results show no qualitative change. We find that 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, and the magnitude of this coefficient remains stable and significant across specifications (Columns (1), (4), and (5)). For its part, an increase in exposure through imports of inputs increases the share of pro-FTA votes by over 1 pp (Columns (2), (4), and (5)), while an increase in exposure through imports of final goods increases the share of pro-FTA votes by about 1.5 pp. These last two measures via imports are not significant across specifications, however, suggesting that exports play more of a role in determining voter choices. This result can be interpreted as follows: while an increase in revenue (exports) would unambiguously increase a worker's wage under a rent-sharing scheme, the same is not true of an increase in imports of inputs or final goods, as reduced import prices might function as a substitute for labor in the production process, adversely affecting workers.

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 62% of voters to a firm, instead of 41%, 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.9.

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 included are reported in Table C.10 in Appendix C. Our results hold qualitatively at both the household- and individual-level, and the coefficient remains statistically equal to the coefficient in our main specification.

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.11, 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.

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 with respect to 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.[24] 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 made

[24]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.

a donation within this time period, and we 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 themselves conducted campaigns for and against the FTA that could be financed through donations. Table C.12 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).[25]

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).[26] As reported in Table C.13, we do not find that voters employed in patent-intensive industries behave differently than individuals in other sectors.

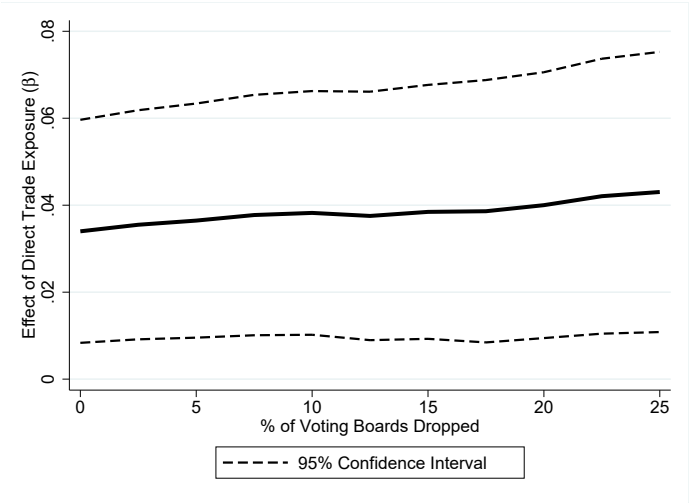
Attenuation Bias If we consider the effect of trade on the voting board as a whole, our estimates are consistent. However, as we compute exposure at the level of voting boards by taking averages of the individuals assigned to vote at each board, our estimates might suffer from attenuation bias if we 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 in 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

[25] 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.

[26] As this measure exists for manufacturing sectors only, we run the regression considering this subset of industries.

different shares of pro-FTA votes. Figure 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 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%. 95% Confidence intervals (dashed lines) are based on robust standard errors clustered by voting center (school).

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 selection 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 auto-

matic and does not consider whether a citizen actually shows up to vote. This fact about the sorting of voters 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 the voting-board level, instead of the list of IDs of the voters that showed up to vote.

Second, we show that in the referendum, while the vote itself depended on how voters were exposed to the FTA, *the decision to vote or not* seems to be orthogonal to the expected gains from the FTA. Instead, as documented in Table E.21, 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 E.21 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 both insignificant and almost zero in magnitude.

Indirect Exposure Through Family Networks Section 5 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, beyond 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.[27] This information allows us to construct the following measure for each individual h:

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

[27]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.

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.18 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).

Voter Awareness and Level of Information The results of Section 5.4 suggest that voters were aware of the FTA's consequences, as we find that exposure through earnings played a significant role in shaping votes. We find suggestive evidence that this result is in line with the level of knowledge about the FTA that was prevalent at the time. From May 2007 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?* The possible answers were: Very informed, Reasonably informed, Little informed, and Not at all informed. According to these 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% of the sample reported to be not at all informed. 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 the FTA (against the FTA)?*

Attitudes Toward Openness in Costa Rica vs. Other Countries It may be helpful 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. This comparison 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 typically not included in surveys that ask respondents about

trade policy, like those by the International Social Survey Programme studied by Mayda and Rodrik (2005), or those regularly conducted by Gallup in the U.S. We overcome these two challenges by leveraging responses of a series of nationally representative surveys conducted just before the referendum, and mentioned in the last paragraph. In particular, we identify questions in the surveys that most resemble questions asked in the other surveys. A comparison of responses across countries is presented in Table F.22. From these findings, we see that Costa Rica was not an outlier during this time period. If anything, Costa Rican attitudes resembled attitudes toward trade in the U.S. in recent years and in Latin America circa 2007.

6 Occupations, Sectors, and Voting Outcomes

This section explores the effects of a voters' occupation and sector on her choice in the referendum, which have been discussed in the literature as potential drivers of popular views about trade. In particular, while the Heckscher–Ohlin model would predict that a worker's skill group is fundamental in determining her views about trade, a specific-factors model holds that the worker's sector is instead the key driver of her view on trade.

First, we classify workers by occupation to measure the importance of skill groups.[28] 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." This leads 57% of Costa Rican workers to be classified as low-skill.

Second, 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) for the case of firm exposure. This measure helps us compare the relative contributions of both occupation and industry exposure against the contribution of firm exposure.

Column (1) in Table 2 shows that the relatively abundant low-skill workers

[28] While our data does not include information on educational attainment, we do observe each worker's occupation, and use it as a proxy for her skill group.

Table 2: Voter's Occupation, Sector, and Referendum Results

Dependent variable: $YesVoteSh_b$

	(1)	(2)	(3)	(4)
$LowSkillSh$	-0.314	-0.322		-0.321
	(0.080)***	(0.079)***		(0.080)***
Firm Exp_b^{Trade}		0.033	0.034	0.033
		(0.013)**	(0.013)**	(0.013)**
Industry Exp_b^{Trade}			-0.331	-0.210
			(0.744)	(0.727)
Controls/FE	Y	Y	Y	Y
Observations	4,914	4,914	4,914	4,914
Clusters	1,934	1,934	1,934	1,934
Adjusted R^2	0.624	0.624	0.622	0.624

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 (except for regressions including $LowSkillSh$, as it is highly colinear with wages), 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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 0.6 pp fewer citizens voting in favor of the trade agreement. This coefficient remains stable after controlling for firm-level exposure, which in turn have very similar magnitude to that reported in Table 1, and shown in Column (2). This finding is against predictions of the original Heckscher–Ohlin model, but it is in line, for instance, with evidence on how increased foreign trade can lead to increases in within-industry wage inequality (Verhoogen, 2008). Column (3) shows that, conditional on firm exposure, industry exposure plays an insignificant role, which highlights that within-industry heterogeneity is a key driver of voter behavior.[29] Findings remain robust after including all measures simultaneously in Column (4).

[29]In this sense, it is evidence supporting models like Melitz (2003), but not a specific-factors model.

7 Local Labor Markets and Import Competition

Attitudes toward the FTA might be affected by local labor markets and import competition (Autor et al., 2013); we use alternative measures of import competition to understand this possibility below.

7.1 Defining Local Labor Markets and Measures of Import Competition

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.6. Second, we compute the following measure of import competition for each CZ i across j industries:

$$\Delta Local\, M\, Comp_i = \sum_j \frac{M_{ij}^{US} \Delta\tau_j}{L_i} \tag{5}$$

where $M_{ij}^{US}\Delta\tau_j$ represents the expected change in imports of final goods in industry j and located in commuting zone i. We can construct this measure as our data specifies, the imports of final goods and location for each firm. Third, to complement this measure, we construct an additional object where total imports are apportioned to each CZ according to labor share following Autor et al. (2013), namely:

$$\Delta ADH\, M\, Comp_i = \sum_j \frac{L_{ij}}{L_j} \frac{M_j^{US} \Delta\tau_j}{L_i} \tag{6}$$

where $M_j^{US}\Delta\tau_j$ represents the expected change in imports from the U.S. given the change in tariffs for industry j.

7.2 Specification and Results

We consider the following specification:

$$YesVoteSh_b = \alpha_0 + \alpha_1 Exp_b^{Trade} + \alpha_2 \Delta MComp_b + \tilde{\Gamma} X_b + D_r + \tilde{\varepsilon}_b, \tag{7}$$

where $\Delta MComp_b$ is the average measure of import competition in hundreds of USD—which can be defined using equation (5) or equation (6)—of voters at voting board b, and other variables are defined as in equation (4).

Tables D.19 and D.20 show our results using the measures in equations (5) and (6), respectively. 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.

We next compare outcomes for the effect of local import competition between Tables D.19 and D.20. While Table D.19 uses data on imports by CZ, Table D.20 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, magnitudes are over three times larger with the ADH method, as compared with the method using CZ-level data. This difference might result from the apportioning method considering competition based on imports at the national level, and not as narrowly defined as the one considering competition at the CZ-level.

8 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 follows:

$$\sum_{t=2}^{T} \beta^t \frac{wage_h^{2007+t}}{CPI^{2007+t}}, \tag{8}$$

where $T = 2017$ and β depends on the interest rate in 2007.[30] We then follow two alternative approaches. The first approach considers the residual of a regression of the term in (8) 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 Table C.15.

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.[31] Finally, we subtract the present discounted value of the counterfactual real wage from the present discounted value of the actual

[30] 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.

[31] 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.

wage profile, as follows:

$$\Delta Ex\text{-}post_h = \sum_{t=2}^{T} \beta^t \frac{wage_b^{2007+t}}{CPI^{2007+t}} - \sum_{t=2}^{T} \beta^t \frac{(1+gr_{wage}^{Pre-2007})^t wage_b^{2007}}{CPI^{2007+t}}. \quad (9)$$

Using the differences in wage profiles, as opposed to the profiles themselves, carries the advantage that the differences are not colinear with 2007 wage levels. We then run equation (4) including $\Delta Ex\text{-}post_h$. As Table C.16 shows, 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 $\Delta Ex\text{-}post_h$ by the present discounted value of counterfactual wages and run our estimation again, as shown in Table C.17. This evidence suggests that ex-ante exposures are good measures of voters' perceptions of the FTA's effects.

9 Political Views and Trade

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.[32]

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:

$$YesVoteSh_b = \gamma_0 + \gamma_1 Exp_b^{Trade} + \gamma_2 Pres_b^{2006} + \hat{\Gamma} X_b + D_r + \hat{\varepsilon}_b. \quad (10)$$

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

[32] As explained in Section 3, the referendum took place because the Congress was split.

assigned to each board, for the most part, would change only 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) of Table 3, 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) 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 specification (Table 1). This is an unsurprising result, as one of the topics on the agenda for the 2006 presidential candidates was precisely CAFTA.

9.1 When Economic Interest and Ideology Collide

The setup gives us a rare opportunity to analyze the interaction between views on politics and on trade policy. Using the results from Table 3, 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 a voting board having an average trade exposure (Exp_b^{Trade}) of \$19,834.

We can also approach this relationship from a different angle by extending equation (10) with an interaction term between the composition of presidential votes in 2006 and trade exposure. Figure E.7 shows the marginal effect of this regression. One can see that the effect of 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.

Table 3: Politics, Firm Exposure, and Referendum Outcomes

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
$Pres_b^{2006}$	0.513	0.512
	$(0.023)^{***}$	$(0.023)^{***}$
Exp_b^{Trade}		0.026
		$(0.011)^{**}$
Controls/FE	Y	Y
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.700	0.701

Notes: 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

10 Earnings Channel vs. 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 referendum, in September 2007, 64% of respondents answered "yes" to the question: *"Will the FTA benefit consumers?"*[33] This section will approximate the predicted reductions in voters' expenditures and estimate the extent to which these predictions affected voter choice in the referendum.

[33]Details on this survey coincide with those described in Section F.22. This question was asked only in September.

10.1 Measuring Individual 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.[34] 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. Then, we can estimate an expected change in the price of this basket, based on share of the good or service that is imported from the U.S. and its expected change in tariffs.[35] In particular, following Fajgelbaum and Khandelwal (2016), we define the individual expenditure effect of consumer h as

$$Expend_h = \sum_{j=1}^{J} (-\Delta p_j)(s_{j,h} - S_j)(p_j q_j), \tag{11}$$

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, and q_j denotes the quantity of good j. 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 change is negative, it represents a reduction in the cost of living caused by a decrease in prices applied to the the pre-shock expenditure basket. We include the term $p_j q_j$ to have a change in expenditures in dollars that is comparable to other measures in our study.

[34] 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. More details on the survey can be found in INEC (2013).

[35] Recall that almost all tariffs would fall to zero under the FTA.

To calculate the price changes for each good or service 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 use the obtained coefficients to predict each voter's exposure to the trade agreement via household-level expenditures.

10.2 Results

Table 4 presents our results. We interpret the coefficient in Column (1) as follows: A 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 (2).

11 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 affects people's lives. Survey evidence suggests that economists and the broader

Table 4: *Expenditures Channel vs. Earnings Channel*

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
$Expend_b$	-0.010	-0.010
	$(0.005)^{**}$	$(0.005)^{**}$
Exp_b^{Trade}		0.034
		$(0.013)^{***}$
Controls/FE	Y	Y
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 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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 for 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 given the opportunity to vote on the ratification of CAFTA. This unambiguous and specific policy choice allows us to measure exposures precisely. We have access to relatively disaggregated data—groups of approximately 500 people on average—about the voting results, which include the ID of each voter. Costa Rica's relatively small informal sector

allows us to match up to 62% of citizens to a formal firm and occupation. In turn, we match firms with customs records, balance sheets, and 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.

We document that a firm's exposure to Costa Rica's free trade agreement significantly influences the attitudes of its employees toward trade policy, and especially so for pro-trade voters. We find that indirect exposure through I-O linkages, a channel that is usually ignored when accounting for exposures to trade shocks, plays a salient role in explaining votes, with a magnitude of about two-thirds the one of the direct effect. Further, we find that high-skilled workers are, on average, more likely to support the free trade agreement, and that within-industry heterogeneity explains votes better than exposure at the sector level. Moreover, we document that ex-ante exposures, which are frequently used in the literature, are a good proxy for the perceived gains from trade.

We also find that local labor market import competition plays some role in explaining the vote shares. These findings remain relevant even after considering political factors. We also compare the importance of the earnings channel relative to the expenditures channel, and document that both are salient in explaining voting behavior.

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Online Appendix for

Voting on a Trade Agreement:

Firm Networks and Attitudes Toward Openness

April 24th, 2022

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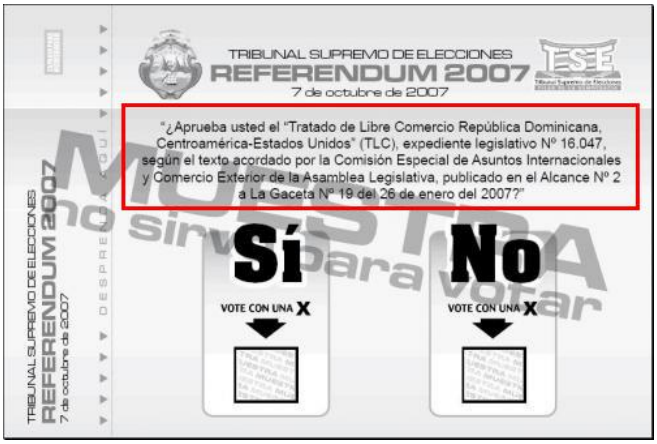
Yale University and NBER

Esteban Méndez-Chacón
Central Bank of Costa Rica

Table of Contents

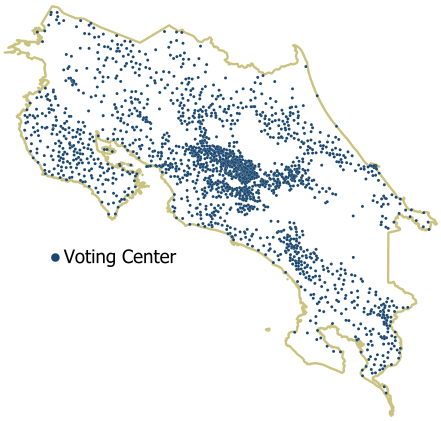
A Additional Figures

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



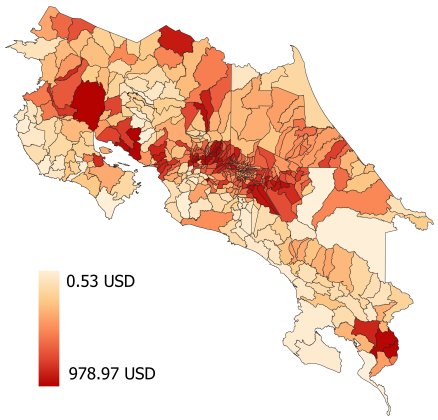
Notes: The figure shows a sample of the single-question ballot used to decide on CAFTA on October 7th, 2007. The text in the red box reads: "Do you approve the "Free Trade Agreement Dominican Republic, Central America-United States" (FTA), legislative file No. 16,147, according to the text approved by 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



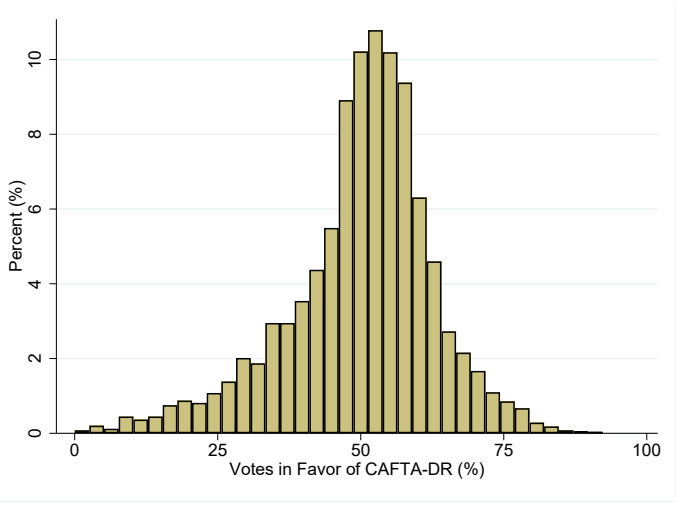
Notes: The figures show the distribution of the voting center across the country for the CAFTA referendum. In Costa Rican elections, each eligible citizen is allocated by her place of residence to a voting center, which is usually located within a school. Within voting centers, voters are allocated alphabetically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

Figure A.3: Average Direct Exposure of Firms by District via their Trade with the U.S.



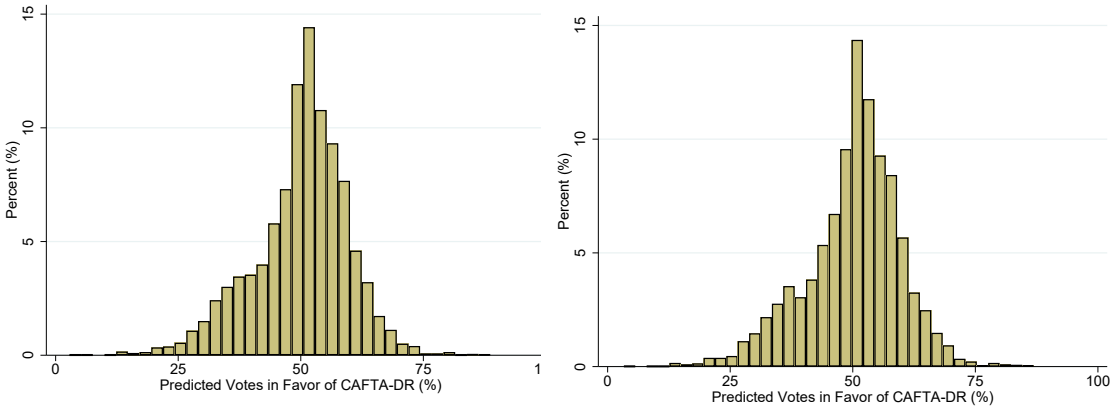
Notes: The figures show the average direct exposure through input-output linkages with the U.S. (Exp_i^{Trade}) for firms in each district, in U.S. dollars per employee.

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

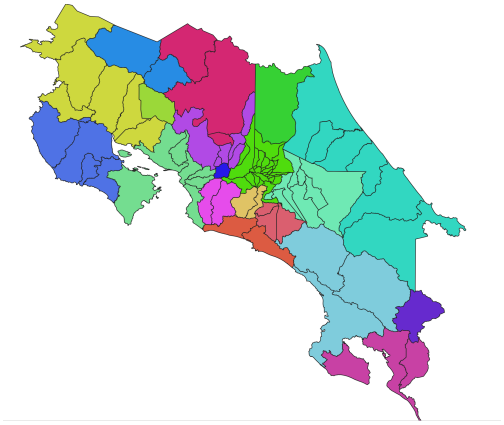


(a) Estimates Column (1) of Table 1

(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 in Table 1.

Figure A.6: 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).

B Additional Tables

Table B.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	65.028	2.868	58.752	2.298
Electricity, gas, steam, air conditioning	0.0001	0	0.331	1.781
Water supply	0.429	0.522	0.013	2.509
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	0.161	2.141
Real estate activities	0.439	13.682	0.359	8.978
Professional, scientific and technical activities	0.1261	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	0.977
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	0.0003	1.841

Notes: The table shows average tariffs by industry, along with each industry's trade as a share of total Cost 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.

Table B.2: Firms' Direct and Indirect Exposure Beyond One Link, and Employee's Voting Behavior

Dependent variable: $YesVoteSh_b$			
	(1)	(2)	(3)
Firm Exp_b^{Trade}	0.034	0.033	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	Y	Y	Y
Observations	4,914	4,914	4,914
Clusters	1,934	1,934	1,934
Adjusted R^2	0.635	0.638	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.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}	0.034	0.033	0.034	0.033	0.034	0.033
	$(0.013)^{***}$	$(0.013)^{***}$	$(0.014)^{**}$	$(0.014)^{**}$	$(0.015)^{**}$	$(0.016)^{**}$
Firm $IndirectExp(1)_b^{Trade}$		0.023		0.023		0.023
		$(0.005)^{***}$		$(0.005)^{***}$		$(0.006)^{***}$
Controls/FE	Y	Y	Y	Y	Y	Y
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	0.635	0.638	0.635	0.638	0.635	0.638

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}	0.039	0.038
	$(0.017)^{**}$	$(0.017)^{**}$
Firm $IndirectExp(1)_b^{Trade}$		0.025
		$(0.006)^{***}$
Controls/FE	Y	Y
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, 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	Y	Y
Observations	4,932	4,932
Clusters	1,952	1,952
Adjusted R^2	0.627	0.629

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.6: Placebo: Firms' Direct Exposure and 2006 Municipal Elections

	Dependent variable: Vote Shares in 2006 Municipal Elections
Firm Exp_b^{Trade}	-0.013
	(0.020)
Controls/FE	Y
Observations	4,834
Clusters	1,934
Adjusted R^2	0.696

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.7: Exposure to FTA is Orthogonal to Voter Characteristics After Including Controls

	(1)	(2)	(3)
	Wage	Low Skill	Years of Schooling
Firm Exp_b^{Trade}	-0.011	-0.008	-0.042
	(0.008)	(0.006)	(0.100)
Controls/FE	Y	Y	Y
Observations	4,914	4,914	4,914
Clusters	1,934	1,934	1,934
Adjusted R^2	0.921	0.789	0.904

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. Except when having the variable on the left hand side of the equation, 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 Additional Robustness Checks

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

	Dependent variable: $YesVoteSh_b$				
	(1)	(2)	(3)	(4)	(5)
Firm Exp_b^X	0.082			0.081	0.082
	(0.026)***			(0.026)***	(0.026)***
Firm $Exp_b^{M,inputs}$		0.013		0.009	0.009
		(0.012)		(0.012)	(0.012)
Firm $Exp_b^{M,final}$			0.089		0.100
			(0.099)		(0.098)
Controls/FE	Y	Y	Y	Y	Y
Observations	4,914	4,914	4,914	4,914	4,914
Clusters	1,934	1,934	1,934	1,934	1,934
Adjusted R^2	0.636	0.635	0.635	0.636	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 C.9: Individual-Exposure: Firms' Direct and Indirect Exposure, and Employee's Voting Behavior

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.034	0.037
	(0.013)***	(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	Y	Y
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.632	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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.10: 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}	0.025	0.033
	$(0.013)^{*}$	$(0.014)^{**}$
Controls/FE	Y	Y
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.636	0.632

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 C.11: Firms' Direct Exposure and the Role of Special Economic Zones

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.034	0.035
	(0.013)***	(0.013)***
Sales in SEZ		-0.040
		(0.142)
Controls/FE	Y	Y
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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.12: The Role of Lobbying Firms

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.034	0.038
	(0.013)***	(0.014)***
Lobbying Firm	-0.562	-0.195
	(0.721)	(0.996)
Firm Exp_b^{Trade} × Lobbying Firm		-4.359
		(7.742)
Controls/FE	Y	Y
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. The variable "Lobbying firm" is defined as the share of individuals at a voting-board that work for a firm that made donations to political parties between January 2007 and October 2007. 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 C.13: Voting and Intellectual Property

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.033	0.070
	(0.013)***	(0.029)***
Patent Intensity	-0.282	0.011
	(0.456)	(0.515)
Firm Exp_b^{Trade} × Patent Intensity		-3.891
		(2.815)
Controls/FE	Y	Y
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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.14: Firms' Direct and Indirect Exposure Using Average Neighborhood Characteristics from the 2000 Census

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}	0.031	0.030
	$(0.013)^{**}$	$(0.013)^{**}$
Firm $IndirectExp(1)_b^{Trade}$		0.023
		$(0.005)^{***}$
Controls/FE	Y	Y
Observations	4,914	4,914
Clusters	1,934	1,934
Adjusted R^2	0.622	0.625

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 C.15: Referendum Results and Ex-Post Outcomes–Approach 1

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Ex-post w_b	0.00002	0.00001
	(0.00010)	(0.00010)
Firm Exp_b^{Trade}		0.034
		$(0.013)^{***}$
Controls/FE	Y	Y
Observations	4,907	4,907
Clusters	1,927	1,927
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, average characteristics of people voting at the voting center, and region fixed effects. 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. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.16: Referendum Results and Ex-Post Outcomes–Approach 2

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Δ*Ex-post*$_b$	0.00003	0.00002
	(0.00009)	(0.00009)
Firm Exp_b^{Trade}		0.034
		(0.013)***
Controls/FE	Y	Y
Observations	4,907	4,907
Clusters	1,927	1,927
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, average characteristics of people voting at the voting center, and region fixed effects. Δ*Ex-post*$_b$ is given in thousands of dollars. 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. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.17: Referendum Results and Relative Change in Ex-Post Outcomes–Approach 2

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Ex-post$_b$	0.0002	0.0001
	(0.004)	(0.004)
Firm Exp_b^{Trade}		0.034
		(0.013)***
Controls/FE	Y	Y
Observations	4,907	4,907
Clusters	1,927	1,927
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, average characteristics of people voting at the school, and region fixed effects. 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 in 2006-2007, which also captures unobserved heterogeneity. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.18: Indirect Exposure Through Family Networks and Voting Behavior

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
Firm Exp_b^{Trade}		0.033
		(0.011)***
$IndirectExp(1)_b^{Relatives}$	0.018	0.005
	(0.013)	(0.012)
$IndirectExp(2)_b^{Relatives}$	0.009	-0.002
	(0.020)	(0.020)
$IndirectExp(3)_b^{Relatives}$	0.000	-0.001
	(0.016)	(0.016)
Controls/FE	Y	Y
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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Import Competition

Table D.19: Import Competition in Local Labor Markets and Referendum Outcomes–Calculation Using Firms' Imports and Location

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
$\Delta Local\ M\ Comp_b$	-0.174	-0.181
	(0.108)*	(0.107)*
Firm Exp_b^{Trade}		0.035
		(0.013)***
Controls/FE	Y	Y
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. 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. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.20: Local Labor Market Effect Calculating Local Imports–Calculation Apportioning Local Imports Using Total Imports and Labor Shares

Dependent variable: $YesVoteSh_b$		
	(1)	(2)
$\Delta ADH\,MComp_b$	-0.460	-0.476
	(0.257)*	(0.254)*
Firm Exp_b^{Trade}		0.035
		(0.013)***
Controls/FE	Y	Y
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. 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. We denote: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E Political Ideology vs. Economic Interest

What do voters do when their political ideology and their economic interest pull them in different directions? Figure E.7 shows, graphically, the heterogeneous marginal effect that results from introducing an interaction term between Exp_b^{Trade} and $Pres_b^{2006}$ in equation (10).

Figure E.7: Marginal Effect of Political Ideology ($Pres_b^{2006}$) for Different Levels of Trade Exposure (Exp_b^{Trade})

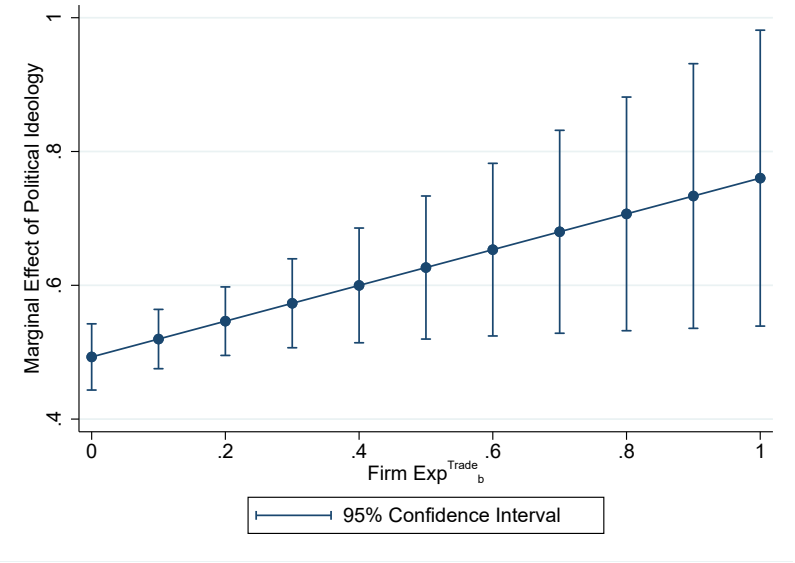


Table E.21: Participation in Referendum Explained by Culture and General Civic Engagement

Dependent variable: Participation in 2007 Referendum

Participation in 2006 Presidential Election	0.749
	(0.019)***
Firm Exp_b^{Trade}	0.008
	(0.006)
Controls/FE	Y
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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

F Comparison Between Attitudes Toward Openness in Costa Rica and 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 for 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 for this question are: An opportunity for economic growth, threat to the economy, both, and neither. Consistently with how we constructed dummies in previous surveys, we generate a variable that is 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 F.22. 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 F.22: 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 F. Percentages in Column (4) result from constructing dummies that are equal to one if the response of 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.