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Voting on a Trade Agreement: Firm Networks and Attitudes Towards Openness

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We exploit a unique event to study the extent to which popular attitudes towards 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 U.S. to be ratified or not. We merge disaggregated referendum results, which break new ground on anonymity-compatible voting data, with employer–employee, customs, and firm-to-firm transactions data, and data on household composition and expenditures. We document that a firm's exposure to the FTA, directly and via input–output linkages, significantly influences the voting behaviour of its employees. This effect dominates that of sector-level exposure and is greater for voters aligned with pro-FTA political candidates. We also show that citizens considered the expected decrease in consumer prices when exercising their vote. Overall, economic factors explain 7% of the variation in voting patterns, which cannot be accounted for by non-economic factors such as political ideology, and played a pivotal role in this vote.

Key words: Trade policy, Political economy, Firm networks, Gains from trade

JEL codes: F13, F14, F68, O24, D72

1. INTRODUCTION

Survey evidence suggests that economists and the broader public view trade issues in starkly different ways (Blendon *et al*., 1997; Sapienza and Zingales, 2013), and given the importance elected officials grant to public attitudes about trade policy, an understanding of the possible correspondence between public sentiments and economic determinants can be consequential. Moreover, analysing the determinants of public attitudes towards trade openness can, in turn,

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inform economic theory and the study of a country's gains from trade and its distributional effects.

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

Delving further into the specifics, although CAFTA included several countries—the U.S., Central America, and the Dominican Republic—the discussion in Costa Rica was centred 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 agreement stipulated zero tariffs for most traded goods and services. Although many of these goods already had zero tariffs at the time of the referendum, Costa Ricans risked tariffs rising to Most Favoured Nation (MFN) levels if the agreement was not ratified. The vote was extremely close, with 51.23% of the voters in favour of ratification.

The data available lies at the edge of what is feasible with voting records while respecting confidentiality. In Costa Rica, each voter is allocated by place of residence to a voting centre, which is usually housed in a school. Within voting centres, 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 the voting board, along with the list of unique national identifiers for each individual voter and the voting board to which she was assigned. We merge these unique national identifiers with employer–employee data, information about employee characteristics (occupation, wage, age, gender, etc.), firms' balance sheets and customs records, and firm-to-firm transactions data. From this rich dataset, we construct a mapping from the disaggregated voting results to individual firms. This mapping allows us to measure the relationship between economic forces and voting outcomes and puts us in a unique position to test whether some observable characteristics of workers are systematically related to their voting choices. We go further and use the identity of each voter's partner (husband or wife) to measure, not only individual exposure, but to construct exposures from the household's perspective. The available data allow us to match 41% of adult citizens to a firm directly, and 53% of households to a firm once we exploit the information on partners.

Armed with the experimental setup and the data, the paper is divided into three sections, which conduct analyses at the voting-board level. The first two sections explore the role of economic fundamentals while distinguishing between the *income* channel and the *expenditures* channel. We study these outcomes with an unprecedented mapping of votes to economic exposure via trade: on the income side, not just firm direct exposure but also indirect exposure; and on the expenditure side, to cost-of-living measures. The third section studies non-economic

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.

factors with an emphasis on the influence of *political ideology*. Then we compare the relative importance of each factor in explaining voting behaviour.

A study of the *income* channel depends on the model of real income which is in mind; a voter's vote can depend, for instance, on whether her employer, industry, skill group, or local labour market were exposed to the tariff changes implied by the trade agreement. Our analysis of this channel uses the role of employers as a benchmark, as we can measure it very precisely and the study of the role of firms is novel. We explore how a firm's dependence on international trade shapes its employees' attitudes towards openness via (1) firm *direct* trade exposure, which depends on the products the firm is trading (exporting and importing) with the U.S. and the expected change in the tariffs on those products; and (2) *indirect* firm-to-firm exposure, whereby an employer is exposed via trading partners who are themselves directly exposed. To the best of our knowledge, this is the first study of the role of within-industry heterogeneity in shaping popular support using information about all firms and documenting the salience of an indirect exposure measure via input–output linkages.

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

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

Next, we focus on the *expenditures* channel. If the FTA did not pass, consumer prices would increase for at least some goods. This is another channel that voters may have considered 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 are rich in respondent characteristics—including income, occupation, location, gender, age, and marital status—and allow 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. We then estimate the expected change in the price of this basket given the expected changes in tariffs. We find that voting boards where voters consume goods that would become more expensive if CAFTA did not pass (as suggested by the demographic characteristics of voters) support CAFTA: a $8.3

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

decrease in the price of a voter's consumption basket increases the probability of voting in favour of the FTA by 1 pp.

We then study the role of non-economic factors, with an emphasis on the role of voters' political inclination. In line with a long literature on political science, we find that political ideology is highly significant; a 1 pp increase in the share of voters at a voting board who align with a pro-FTA political party is associated with a 0.5 pp increase in the share of pro-ratification votes. Moreover, political views interact with trade exposure in an interesting way; we find that high trade exposure is more salient for voting boards composed of voters affiliated with pro-free trade political parties.[3]

Finally, we conduct a broad comparison of the importance of different factors. To do so, we compare the partial R^2 across a series of regressions to grasp what percentage of the variation in voting behaviour can be attributed to each factor. Aligned with the results of the previous paragraph, we find that political alignment plays a relatively important role, accounting for 9% of the variation which cannot be explained by other factors. However, we can also verify that economic factors play a non-negligible role, explaining 7% of the observed variation in voting behaviour, which cannot be explained by non-economic factors. Thus, economic fundamentals are almost as important as political ideology in explaining the CAFTA vote, and were particularly key in this setting in which the referendum was approved with a slim lead in votes, and more generally, might play paramount significance in closely contested elections.

2. RELATED LITERATURE AND CONTRIBUTION

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

The present study also builds on work that examines how economic openness impacts domestic politics in the U.S., including Autor *et al*. (2013), Che *et al*. (2016), Blanchard *et al*. (2024), Bombardini *et al*. (2023), 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 analysed 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

3. This result holds after implementing an IV strategy to isolate how the FTA might have influenced voters' choice of party.

of issues, our design allows us to isolate tariffs' effects on voter decisions, specifically about trade policy. Furthermore, while a standard approach in the literature is to adopt a shift-share approach based on industry composition at the county level, our data allow us to highlight the importance of within-industry heterogeneity and individual firms in explaining voter behaviour using precise relationships between disaggregated results and firms.

In a sense, the findings of the survey-based and election-focused papers described above seem to contradict each other, with the former often arguing that popular attitudes are unaffected by economic factors and the latter arguing that trade shocks have a great effect on elections. The present work can help reconcile these perspectives. Our study, 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. In addition, the paper documents the relevance of expected gains and losses for voters' employers in the FTA referendum. This finding connects the already established literature on the role of economic fundamentals for political outcomes with work in labour economics that shows that employers explain a great deal of an individual's labour market outcomes (Card, 2022) by showing that when voting on an economic policy, workers care about how that policy would affect their employer.

This paper also addresses the political science literature. Related studies include Urbatsch (2013) and Hicks *et al*. (2014), who rely on surveys and census data to analyse how districts voted on the CAFTA referendum depending on their composition and political views, and Spilker *et al*. (2018), who study how exporting firms in Costa Rica changed their exports *after* CAFTA was ratified. Our study complements these works by exploiting disaggregated data at the levels of voting boards, firms, and individuals, along with employer–employee links, to assess the importance of within-industry heterogeneity and economic and social conditions in explaining the vote.

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

Studies of the expenditure channel have mainly focused on the effects of trade on inequality, both using microdata and exploiting major reforms in individual countries (Porto, 2008; Faber, 2014; Atkin *et al*., 2018), and leveraging theoretical frameworks to measure inequalities in gains from trade between consumers as in Fajgelbaum and Khandelwal (2016) and Borusyak and Jaravel (2021). 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. Sections 3.1 and 3.2 provide an overview of the setting, including details about the FTA and voting in Costa Rica. Section 3.3 presents details on

the data used in our analysis. Sections 4 and 5 are devoted to analysing economic factors, and develop, respectively, the study of the income and expenditures channel. Section 6 explores the role of non-economic factors, and provides a broad comparison between their relevance and that of economic fundamentals, and Section 7 concludes.

3. BACKGROUND AND DATA

3.1. *The free trade agreement: CAFTA*

In August 2004, the U.S. signed a FTA—known as CAFTA—with Costa Rica, El Salvador, Guatemala, Honduras, Nicaragua, and the Dominican Republic. The agreement included large reductions in tariffs, along with provisions on intellectual property rights, on regulatory agreements (environmental regulation and investors protection), and on liberalizing specific markets which were previously monopolized by the government—the main markets, both in terms of their size and their saliency in the discussion, being the telecommunications (including internet provision) and insurance markets.[4]

The matter at hand was quite relevant to workers in Costa Rican firms, as the U.S. was Costa Rica's main trading partner, accounting for 45% of the country's imports and exports, Costa Rica's trade-to-GDP ratio was 86%, and absent the FTA, tariffs for trade with the U.S. could considerably increase. The agreement implied zero tariffs for most of the goods and services traded with the U.S.[5] While most of these goods had zero tariffs by the time of the referendum, the U.S. pledged that, if the FTA was not ratified, there would be no renegotiation, existing trade preference programmes would not be renewed, and tariffs faced by Costa Ricans would then increase to MFN levels.[6] Thus, a no-vote is more of a vote in favour of tariff increases rather than against tariff decreases.[7]

Figure 1 shows the tariff changes per product for exports and imports, which correspond mainly to the difference between zero and MFN tariff levels, and show significant variation within and between industries. Supplementary Material, Table A.1 shows the average changes in export and import tariffs by industry, along with the share of each industry in total exports and imports in 2007.[8] Moreover, as the FTA had an indefinite duration, its ratification would also reduce future tariff uncertainty.

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

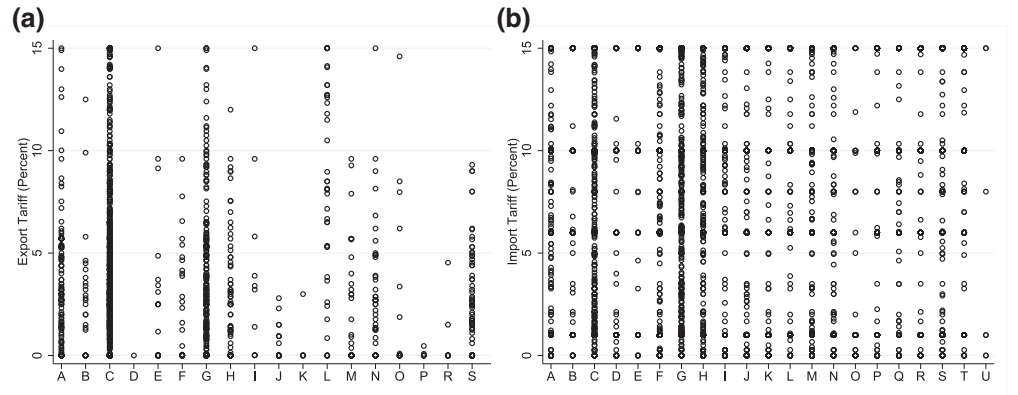
4. These provisions can be relevant both for import competition and lower prices.

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

6. The counterfactual tariffs given a no-vote were printed on CAFTA for each HS-6 code.

7. To the extent that voters are subject to gain-loss asymmetry, this matters in the interpretation of our results. That is, if people tend to feel the pain of a loss (of openness) more acutely than the benefit of a gain of the same magnitude, then one would expect a vote for a reduction of tariffs to have a smaller impact on the measures of exposure which are positive (like firm exposure) and a larger one for measures of exposure which are negative (like import competition).

8. 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%.



A: Agriculture, forestry and fishing
B: Mining and quarrying
C: Manufacturing
D: Electricity, gas, steam, AC supply
E: Water supply, sewerage, waste mgt. & remediation activities
F: Construction
G: Wholesale & retail trade; repair motor vehicles/motorcycles
H: Transportation and storage
I: Accommodation and food services
J: Information and communication
K: Financial and insurance activities
L: Real estate activities
M: Professional, scientific and technical activities
N: Administrative and support service activities
O: Public admin. and defense; compulsory social security
P: Education
Q: Human health and social work activities
R: Arts, entertainment and recreation
S: Other service activities
T: Activities of households as employers; undifferentiated good/service-producing activities for households own use
U: Activities of extraterritorial organizations and bodies

FIGURE 1
FTA's counterfactual tariffs. (a) Costa Rican exports. (b) Costa Rican imports

Notes: The figures show the tariff differences with and without FTA approval, mainly showing changes between zero and MFN levels. Each ring represents an HS-6 code. For visual purposes, we show changes smaller or equal to 15%, which capture over 98% of products both for exports and for imports, and truncate larger ones at 15%.

Although CAFTA was signed in 2004, signing an FTA only means that the countries agreed on its terms, but it does not make it legally binding. Ratifying an FTA, on the other hand, is the stage in which the countries involved formally approve the agreement (after signing it) and make it legally binding. This stage involves going through the respective domestic legal processes of each country to ensure that the terms of the agreement are in line with their own laws and regulations. By late 2006, Costa Rica was the only country that had not ratified CAFTA due to delays in the vote of its Legislative Assembly, as the opposition delayed the vote on the agreement repeatedly, and the congress—split between opponents and supporterswas not able to get a majority vote on whether to ratify the FTA or not for the next 2 years. 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 of the country could cast their vote *with a single 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 the voting centres to express their opinion on this matter only. Supplementary Material, Figure A.1 shows a sample of the referendum ballot. Although the national referendum was only about this issue, participation was high; in 7 October 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 in the following. First, each eligible citizen is assigned to a voting centre, which usually corresponds to a school, depending on her place of residence. Within the voting centre, 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 on 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 in 4,932 voting boards distributed amongst 1,952 voting centres throughout the country. Supplementary Material, Figure A.2 shows the spatial distribution of the voting centres. This allocation usually does not change dramatically from year to year. In fact, most citizens who voted on a voting board in the 2006 election voted on the same voting board in the 2007 referendum (exceptions mostly being citizens who died, turned 18, or changed residence within that year). We will exploit this persistence in our empirical section to isolate the effect of political alignment as a motive to vote in favour or against the referendum.

3.3. *Data sources*

3.3.1. Voting and referendum results. Data on the results of the referendum were 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.[9] 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. In addition, we also acquired lists with unique national identifiers of voters on each voting board.[10]

3.3.2. National registry. We obtained family network data from the Civil Registry of Costa Rica. These data allow us to identify if a citizen is married and to whom. This will be useful in estimating households' exposure to the FTA, especially for individuals who are not in the labour force but who are married to someone who is employed.

3.3.3. Employer–employee records, firm-to-firm transactions, and customs. We match voters with their employers using data from the Costa Rican Social Security Fund, which tracks formal employment and labour earnings. These data also include 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, 2018).

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

10. 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). Supplementary Material, Table B.1 shows that the results are robust to using all voting boards.

Data on firm-to-firm transactions in Costa Rica are collected by the Ministry of Finance and are available between 2008 and 2017.[11] 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. These data are key in the government's enforcement of tax law and tax collections, including the general sales tax and corporate income tax. These 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.

In addition, we link each firm's identifier with customs records, which are available for the period 2005–17, and which we use to track the individual foreign transactions made by each firm. Each transaction, both for imports and exports, includes a six-digit HS code, along with data on the amount transacted, the quantity traded (and thus the price), and the country of origin or destination. These data also allow us to identify firms operating within a Special Economic Zone.

3.3.4. 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 26 January 2007. That is, the text that was to be ratified by the referendum (see Supplementary Material, Figure A.1 in Appendix A). In addition to tariff changes, the agreement also includes a schedule for the timing with which old tariffs would converge to new ones.[12]

4. INCOME CHANNEL

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

11. 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.

12. 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.

4.1. *Income channel: measures of exposure*

We will construct measures of exposure to CAFTA which are intrinsically imperfect. For instance, our measure of direct firm exposure will be an average of trade-weighted changes in tariffs. This measure is imperfect in the sense that it roughly corresponds to the potential gains/losses from trade in a specific model. The latter can be viewed as a *strength* of the paper, not a weakness: we will propose very simple measures of exposure to CAFTA, and one would have to blindly believe a particular trade model to think these are the "true" measures of exposure; however, even with this unavoidable distance between crude measures and what would be the "ideal" measures, we will find a strong relationship between crude measures and votes, suggesting that the role of economic determinants in explaining votes is very strong and detectable, even with an imperfect measure.

4.1.1. Direct firm-level exposure to the FTA. Recent models of firm heterogeneity imply that trade could affect employment and wages. The literature has proposed several channels by which this might be the case, such as rent sharing, efficiency wages, and assortative matching.[13] As for empirical results, recent work by Alfaro-Ureña *et al.* (2021) has shown how the rent-sharing mechanism is relevant in the Costa Rican case, and particularly so for firms engaged in trade with foreign countries. Alfaro-Ureña *et al.* document that when multinational firms expand, their direct and indirect suppliers are affected, and the salaries of incumbent workers increase due to 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.

Namely, we calculate an average of trade-weighted changes in tariffs, which exploits the variation shown in Figure 1. This measure is motivated by Helpman *et al.* (2016), who propose that the change in the wage bill of a firm i ($\Delta w_i L_i$) is an increasing function of the change in its profits.[14] Thus, we consider:

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

where X_{ji}^{US} represents firm i's sales of product j in the U.S., $\Delta\tau_j^{US,X}$ stands for the expected percentage change in tariffs for product j which is exported to the U.S., M_{ji}^{US} are firm i's purchases of product j from the U.S., and $\Delta\tau_j^{US,M}$ represents the expected change in import tariffs from the U.S. for product j if the agreement were to be ratified.[15] We normalize this exposure by each firm's number of employees (L_i), which would be consistent with the amount that a change in profits would affect a single worker under a rent-sharing scheme. In fact, Alfaro-Ureña *et al.*

13. Helpman *et al.* (2010, 2016) discuss how rent sharing between workers and firms might cause wages to vary with firm revenue. Thus, changes in trade costs, such as tariffs, can affect worker welfare via earnings. Besides rent sharing, alternative mechanisms include efficiency wages (Egger and Kreickemeier, 2009; Amiti and Davis, 2011; Davis and Harrigan, 2011) and assortative matching (Yeaple, 2005; Verhoogen, 2008; Burstein and Vogel, 2010; Bustos, 2011).

14. Helpman *et al.* (2016) show that a firm's wage bill is a constant share of its revenue. While Helpman *et al.* (2016) focus on exports, we also consider imports, which is consistent with measures developed by Dhyne *et al.* (2021) for both exports and imports.

15. We consider imports of both inputs and final goods in this measure. Note that, later on when we use this measure in a regression, a sufficient condition for a Bartik-like strategy is for the product-specific tariff changes experienced at the national level to be uncorrelated with the regression's error terms (Borusyak *et al.*, 2021), which is likely as over 95% of the changes in tariffs depend on the difference between: (1) zero (under the FTA) and (2) MFN tariffs (if the FTA is not ratified).

(2021) find that, in the case of Costa Rica, each extra dollar of value added per worker increases wages by 9 cents. This measure of a firm's exposure leverages our data about each firm's balance sheets, customs transactions, and the expected changes in tariffs due to CAFTA. Supplementary Material, Figure A.3 in Appendix A summarizes the variation in this measure across space. When examining correlations, we find that younger, male, and richer individuals tend to have higher firm trade exposure. While equation (1) proposes a compound measure, we will later on decompose it into exports and imports.

4.1.2. Indirect firm-level exposure to the FTA. Our measures of each firm's *indirect* exposure to the trade agreement rely on firm-to-firm transactions data. 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 is carried out 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 $\frac{R_{ki}}{R_i}$ ($\frac{C_{ki}}{C_i}$) 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 can then be described recursively as

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

for a chain of domestic traders of length K.

Individual and household firm exposure. Unlike the measures we will describe below (which are derived from individual's occupations, location, or wage), direct and indirect firm exposures are firm-specific, so we proceed by linking these exposures to the firms' employees. First, as we observe the list of unique IDs of citizens assigned to each voting board, we can match these IDs to our employer–employee data. The data allow us to link 41% of voters to an employer. Second, we can assign each employed voter to her employer's exposure. This is an *individual* measure of exposure to the FTA via earnings. Third, we can go further and calculate measures of *household* exposure using information on each voter's marital status and the identity of his or her spouse. If the voter is married, we calculate the household exposure measure as the weighted average of the exposure of each partner, where the weight corresponds to the share of household income contributed by each partner. That is, we follow the unitary model of the household.[16] This exercise allows us to increase the share of voters that we can match to an employer, from 41% without exploiting partners' IDs to 53%. This success rate in matching voters with firms is close to the best possible, as 9% of the voters are retired, 29% are estimated to be in the informal

16. For instance, if each partner is earning the same wage, then the household's exposure is the average of the exposures of the partners' employers. In contrast, if only one partner is employed, or if the voter is single, the household's exposure is simply the employed voter's exposure.

sector and 6% are estimated to be adult students; thus, we are roughly capturing the remaining share.[17]

4.1.3. Sectors and occupations. We construct measures of exposure to the FTA at the industry level (four-digit ISIC codes), which are analogous to those presented in equation (1), but at the sector level. We also explore the effects of a voter's occupation on her choice in the referendum. To do so, we classify workers by occupation to measure the importance of skill groups; 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 labelled as "high-skill."[18] This leads to 57% of workers being classified as low-skill.[19]

4.1.4. Local labour markets and import competition. Attitudes towards the FTA might be affected by local labour markets and import competition (Autor *et al.*, 2013). To explore this, first, we use the 2011 Population Census to estimate CZs in Costa Rica from observed flows, following Tolbert and Sizer (1996). To the best of our knowledge, such an exercise has not been conducted before for Costa Rica. We report the country's map with the estimated CZs in Supplementary Material, Figure A.4. Second, we construct the following measures of import competition for each CZ i across j industries:

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

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

4.1.5. Expectations about future job opportunities. Measures of ex-ante exposure reflect how voters' conditions at the time of the referendum influence their choice. We now ask whether voting behaviour reflected correct perceptions of the benefits that emerged from the FTA's approval, *but that were not necessarily captured by ex-ante conditions*.[20] Namely, we calculate the discounted change in real earnings experienced by each voter h in the years after the

17. Given the nature of our shock, which hits firms trading internationally, it is not unreasonable to assume that employees working at informal firms have zero direct exposure, as informal businesses, which tend to be smaller and less productive, are unlikely to be engaged in foreign trade. We estimate these groups as follows: a retiree is an adult who has over 65 years of age and is not employed; a college student is an adult under 23 years of age who is not employed *and* who appears as a high-skilled employee after 2013; finally, an informal worker is an adult who is not employed or a student, who is between 18 and 65 years of age, who is not married to an employed worker, and who does not appear amongst the employed within 1 year of 2007—our 29% estimate is close to the 27% reported in other surveys (ILO, 2018).

18. Descriptions of the educational requirements of each occupation are obtained from Costa Rica's Social Security Administration.

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

20. For instance, a worker might have anticipated that she could get a better job if the FTA was approved; this would not be captured by our firm exposure measure.

referendum, as follows:

$$\sum_{t=2}^{2017} \beta^t \frac{wage_h^{2007+t}}{CPI^{2007+t}}. \tag{5}$$

We then consider the residual of a regression of the term in (5) on our direct firm exposure, Exp_b^{Trade}.[21] This residual term, which we call $Ex\text{-}post$, aims to capture drivers of ex-post income that are not captured by ex-ante direct trade exposure.

4.2. *Income channel: empirical strategy*

As described in Section 3.3, our data on voting outcomes are available at the voting-board level, and observe the individuals assigned to each voting board and their characteristics. This breaks new ground on anonymity-compatible voting data; while the vote's secrecy is preserved by the voting outcomes being aggregated by voting board, voting boards are quite small (approximately 500 people, on average). We then perform an analysis at the voting-board level. Namely, we consider:

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

where $YesVoteSh_b$ is the share of pro-FTA votes at each voting board b and X_b is a vector of average exposure measures of voters assigned to voting board b, which is defined in alternative ways in the next section, *but that always results from averaging the exposure measures of voters assigned to each voting board.* K_b is a vector of voter characteristics (age, wage, gender, participation rate, employment share by industry, employment share in the public sector, firm size, and firms' trade with the U.S.) averaged at the voting-board level, along with voter characteristics averaged at the voting-centre level (average years of schooling from census data geo-referenced by census-block and average distance to the school); and λ_b^r denotes region-fixed effects.[22] We cluster standard errors at the voting-centre level and weight each voting board by the number of voters.[23]

We rely on a linear probability model, which delivers fitted values in the [0, 1] interval for 100% of voting boards.[24] This model also admits a straightforward interpretation and, under some assumptions, allows for interpreting the coefficients as *individual-level* effects, and not only as group-level effects.[25]

21. We assume that voters could project at most 10 years into the future, and that they discounted using the prevailing interest rate. Details on timing are provided in Supplementary Material, Appendix C.2.

22. The 2011 Census was the closest to the 2007 referendum, which is why we use it in our main specification. Supplementary Material, Table B.2 shows that the results remain statistically equal if instead, we use the second-closest census, which took place in 2000. Regions correspond with municipalities. Details on these censuses can be found in Méndez and Van Patten (2022).

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

24. Supplementary Material, Figure A.5 shows this distribution. At first blush, a logit model might seem well-suited for our experiment, but recall that we do not observe our dependent variable at the individual level. As each individual would have different states as independent variables, aggregating the individual logit model to the voting-board level would deliver a sum of logits on the right-hand side of the estimation equation, instead of a standard logit; a similar problem to BLP (see Montero (2016) and Rekkas (2007)).

25. Further, Supplementary Material, Figure A.6 shows the distribution of vote shares across all the voting boards in our sample, which is centred around 50% and has thin tails, thus, we do not rely on a censored regression model.

4.3. *Income channel: results*

4.3.1. Direct firm exposure. Table 1 shows that direct firm exposure, Firm Exp_b^{Trade}, is salient to voters; across specifications, we find that referendum votes were cast in alignment with the interests of voters' employers and that this effect is extremely stable. To interpret the coefficients, recall that our analysis is conducted at the voting-board level and, as an example, consider Column (1): an increase of $1,000 in the exposure of the average employer—which is a proxy of the average expected change in profits, in thousands of dollars—is associated with a 3.4 pp higher share of votes in favour of the FTA at a voting board; a 6.9% increase with respect to the mean. Note, however, that a $1,000 change *in profits* is not the same as $1,000 in the pockets of a voter; in fact, Alfaro-Ureña *et al*. (2021) estimate that such a change would correspond with an average increase in *wages* of $90.[26]

Decomposing direct firm exposure. While our main measure in equation (1) considers changes in exports and imports, we can explore the effects of these changes separately, so that: $Exp_i^X = \sum_{j=1}^{n} \frac{X_{ji}^{US}}{L_i} \Delta\tau_j^{US,X}$ and $Exp_i^M = \sum_{j=1}^{n} \frac{M_{ji}^{US}}{L_i} \Delta\tau_j^{US,M}$. As shown in columns (7) and (9) of Table 1, we find that a $1,000 increase in exposure via exports leads to an over 8 pp increase in the share of people in favour of the FTA at a voting board—more than twice the effect of the original measure. On its part, an increase in exposure through imports increases the share of pro-FTA votes by 1 pp (columns (8) and (9)) and is statistically insignificant, suggesting that exports play more of a role in determining voter choices. A possible explanation for this asymmetric effect is that, while an increase in revenue via exports would unambiguously increase a worker's wage under a rent-sharing scheme, the same is not true of an increase in profits via lower costs of imports, as reduced import prices might function as a substitute for labour in the production process, adversely affecting workers (Verhoogen, 2008). Other potential explanations include different salience to the worker and different effects on skill intensity.

4.3.2. Indirect firm exposure. Results related to a firm's direct *and* indirect exposure (for buyers and sellers who trade with a directly exposed firm) are presented in Column (2) of Table 1. As shown, indirect exposure for firms that are "one link away" from a directly exposed firm matters. The coefficient of indirect exposure is approximately two-thirds the size of the coefficient of directly exposed firms. This result highlights the role of indirect exposure via the firm network in shaping worker attitudes towards trade; a channel which has remained largely unexplored by the literature. Beyond this one link away relationship, we do not find effects of firms connected via their network, as reported in Supplementary Material, Table B.5.[27]

Decomposing indirect exposures. Equation (3) groups relationships between firms, regardless of whether an indirectly shocked firm is buying from or selling to a directly shocked firm. We can first ask if the effect is symmetric when considering buyers vs. sellers. As shown in Supplementary Material, Table B.6, coefficients are exactly the same in both cases. Moreover, the effect disappears for relationships that are more than "one link away" from each other. We can further decompose this indirect effect into four categories: an indirectly shocked firm which is (1) selling to an exporter to the U.S. (seller2seller), (2) selling to an importer from the U.S. (seller2buyer), (3) buying from an exporter to the U.S. (buyer2seller), and (4) buying to an importer from the U.S. (buyer2buyer). Column (10) of Table 1 displays the results. We find that

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

27. This finding is consistent with Dhyne *et al*. (2022), who document that direct demand effects decay quickly with the distance to direct exporters in the supply chain. Supplementary Material, Table B.7 also reports results for direct and indirect firm exposure without controls.

TABLE 1

Income channel and voting behaviour. Dependent variable: $YesVoteSh_b$

	Panel (a): Income channel factors					
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exp_b^{Trade}	**0.034** (0.013)***	**0.034** (0.013)***	**0.035** (0.013)***	**0.033** (0.013)***	**0.037** (0.015)**	**0.034** (0.013)***
$IndirectExp(1)_b^{Trade}$		0.023 (0.005)***				
Industry Exp_b^{Trade}			0.036 (0.121)			
$LowSkillSh_b$				−0.334 (0.079)***		
$\Delta\, MComp_b$					−0.034 (0.013)***	
$Ex\text{-}post_b$						0.0000 (0.0001)
Adjusted R^2	0.636	0.639	0.599	0.624	0.501	0.636

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

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

the effect is positive and significant only for sellers to exporters and buyers from importers, *i.e.* Cases (1) and (3), but the effect is negative and insignificant for Cases (2) and (4). This result is intuitive: for sellers to exporters, the FTA potentially means more business; for buyers from importers, the FTA might translate into cheaper prices; however, for sellers to importers and for buyers from exporters, the FTA might translate into more competition.

4.3.3. Sectors and occupations. A worker's industry seems to play a limited role conditional on firm exposure, as shown in Column (3) of Table 1, which highlights the relevance of within-industry heterogeneity. Without including the firm exposure measure, the coefficient of sectorial exposure becomes twice as large, as shown in Supplementary Material, Table B.8.[28] Moreover, Column (4) of Table 1 shows that the relatively abundant low-skill workers are more likely to vote against the FTA. A 1 pp increase in the share of low-skill voters at a voting board (*LowSkillSh*) is associated with 0.3 pp fewer citizens voting in favour of the FTA. This finding is against predictions of the Heckscher–Ohlin model, but it is in line, for instance, with Urbatsch (2013), Hicks *et al*. (2014), and Verhoogen (2008).[29]

4.3.4. Import competition. Our findings suggest that competition in local labour markets might influence voters to position themselves *against* the trade agreement, as shown in Column (5) of Table 1. This finding is robust to using alternative measures of import competition, as described in Supplementary Material, Appendix B.3.

4.3.5. Expectations about future opportunities. As Column (6) of Table 1 shows, we find no evidence that ex-post differential outcomes factored into voting decisions. The latter could relate to expectation formation being difficult in the presence of uncertainty, or to individuals' stochastic discounting of future outcomes. This evidence suggests that ex-ante exposures are good measures of voters' perceptions of the FTA's effects.[30]

4.4. *Addressing selection*

To measure the impact of possible income gains from trade on referendum votes, the ideal (yet impossible) experiment would be to take two identical individuals, randomly assign one to work at a firm that would gain from trade, another to a firm that would lose, and compare their votes. Instead, the unique event we study features workers that have endogenously chosen to work in different firms (some that benefit more, or less, from trade with the U.S.). A valid concern is reverse causality: a worker that favours free trade with the U.S. may endogenously choose to work at a firm that benefits from trading with the U.S. We now conduct some exercises which alleviate this concern of confounding factors which might affect both voter's job choice and their voting choices.

4.4.1. Counterfactual tariffs. Virtually all tariffs are zero under the FTA, and would be MFN tariffs otherwise. This fact is helpful for our purposes, since those tariffs were not applied to Costa Rican trade in the recent past.[31] Thus, whatever factor may have determined a worker's employment choice—including the volume of trade with the U.S.—is not necessarily correlated with the potential loss from CAFTA not passing. Moreover, as shown in Supplementary Material, Table C.2, our design is robust to the inclusion of a demanding additional control, both at the household- and individual-level, namely: $\sum_{j=1}^{n} \frac{X_{ji}^{US}+M_{ji}^{US}}{L_i}$. This term is similar to our main regressor described in equation (1), but it omits the exogenous tariff changes implied by the

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

29. In fact, if we consider wage schedules *after* the FTA was ratified as a dependent variable, we find that the *interaction* between firm exposure and *LowSkillSh* is negative and significant, which suggests a lower pass-through from exposure to wages for the low-skilled.

30. We present results following an alternative approach in Supplementary Material, Appendix C.2.

31. Recall that, absent the FTA's ratification, the preferential tariffs Costa Rica had been enjoying would not be renewed, so effectively tariffs would increase to MFN levels.

FTA. Adding this control is quite demanding in terms of variation, but it carries the benefit that identification would come solely from changes in tariffs, which can be regarded as exogenous shifts, as we have argued above.

4.4.2. Selection into global firms. We can also construct placebo exposures for firms trading with countries *other than the U.S.* These measures are computed following equation (1) for each firm, but with exports and imports *to other countries not including the U.S.* in the numerator. As the FTA does not change tariffs with other countries, this placebo allows us to test if workers who choose to work at firms that engage in foreign trade are special in a way captured by equation (1), but not directly related to CAFTA. Results are presented in Supplementary Material, Table B.11. Reassuringly, not only the resulting coefficient is statistically insignificant, but it is negative. This placebo remains insignificant if we consider only firms trading with the European Union, Costa Rica's second-largest trading partner at the time. We again obtain null results when conducting an analogous exercise for firms' indirect exposure.[32]

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

4.5. *Income channel: robustness*

Figure 2 summarizes a series of robustness exercises, all of which are explained in detail in Supplementary Material, Appendix C.1. Our results are unchanged by considering individual-level exposure (panel A2) and controlling for a firm's trade with the U.S. (panel B1), the share of production by firms within a Special Economic Zone (panel B2), and the share of firms which engaged in lobbying prior to the referendum (panel B3). We also find that voters employed in patent-intensive industries behave similarly to those in other sectors (panel B4), even though the FTA had guidelines regarding intellectual property (IP) rights. This null result can be interpreted as evidence of the inattention of voters to alternative forces, other than tariffs, which can be affected by the FTA. Finally, panels B5, B6, and B7 control, respectively, for the share of informal workers, the share of voters employed at the National Insurance Institute (INS) or the Institute of Electricity (ICE), and the share of retirees assigned to each voting board, none of which alters the effect of direct firm exposure.[33] Supplementary Material, Appendix C.3 discusses other three dimensions: the role of selection into voting, the high levels of awareness and information amongst voters, and makes a comparison of Costa Rican attitudes with those of other countries.

5. EXPENDITURES CHANNEL

If the FTA did not pass, consumer prices would increase for at least some goods, which would adversely affect voters. In fact, when Costa Ricans were surveyed 1 month before the referendum, in September 2007, 73% of respondents answered "yes" to the question: "Will the FTA

32. These results are presented in Supplementary Material, Table B.12.

33. Being employed at the INS or the ICE was potentially relevant, as these public institutions had monopolies in insurance and telecommunications, and the FTA would force both of them to face competition (see Section 3.1).

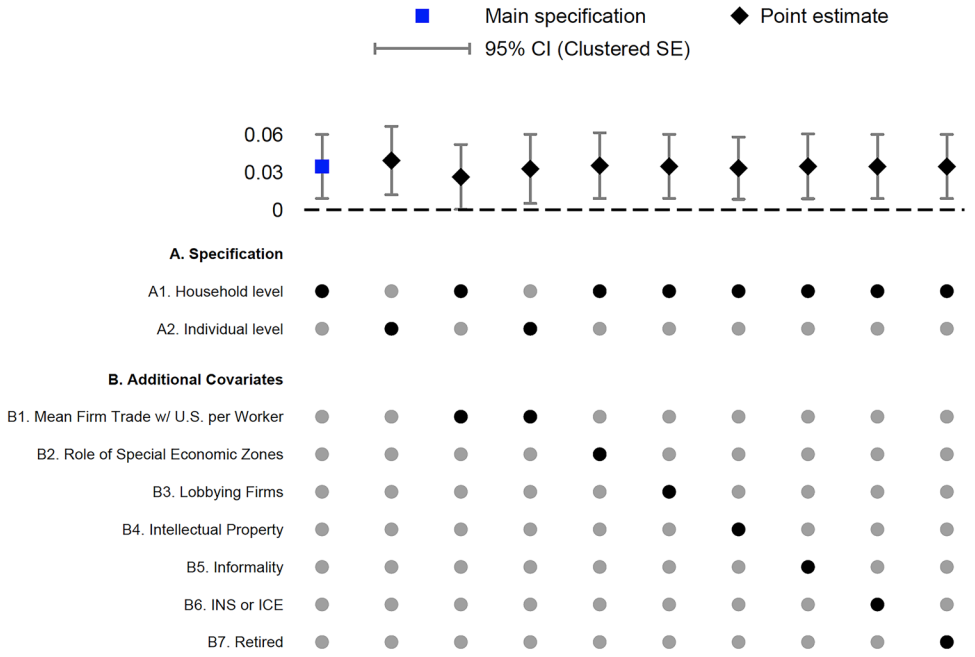


FIGURE 2
The impact of firms' direct exposure: robustness exercises

Notes: In A and B, black dots indicate the specification of the regression that generates the point estimate which is vertically aligned with these dots. Individual tables with these regressions are reported in Supplementary Material, Appendix C.

benefit consumers?"[34] This section will approximate the predicted effects in voters' expenditures and estimate the extent to which these predictions affected voter choice in the referendum.

5.1. *Measuring exposure via expenditures*

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

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

34. Details on this survey coincide with those described in Supplementary Material, Section C.3. This question was asked only in September.

we define the expenditure effect of consumer h as

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

where p_j denotes the price of good j, $s_{j,h}$ denotes the share of good j in the total expenditures of individual h, S_j denotes the share of good j in average expenditures. It follows that $-\Delta p_j s_{j,h}$ represents an expenditure-share weighted average of price changes, and defines the consumer's expenditure effect. If this change is negative, it represents a reduction in the cost of living caused by a decrease in prices applied to the pre-shock expenditure basket. We include the term $p_h q_h$, which captures the expenditures of household h, to have a change in expenditures in dollars that is comparable to other measures in our study.

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

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

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

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

5.2. *Expenditures channel: results*

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

Table 2 presents our results. Column (1) shows results *without* including any controls. As expected, the coefficient without controls or fixed effects is larger than the ones in columns (2) and (3), but the overall message remains unchanged across specifications. We interpret the coefficient in Column (2) as follows: The average household whose expenditures would decrease by $1 if the agreement were to be approved—on top of the decrease in expenditures experienced by the average consumer ($7.3)—is 1 pp more likely to vote in favour 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 favour of a trade agreement at that board being 1.63 pp greater. This effect is significant even after controlling for firm-level exposure, as reported in Column (3).

TABLE 2

Expenditures channel vs. earnings channel. Dependent variable: $YesVoteSh_b$

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

Notes: The unit of observation is the voting board. Robust standard errors, adjusted for clustering by voting centre (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 and distance of the average voter to the school); and region-fixed effects. We denote: $^{**}p < 0.05$, $^{***}p < 0.01$.

6. NON-ECONOMIC FACTORS AND COMPARISON

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

6.1. *Political alignment*

Voter behaviour 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.[35] Then, we include the share of 2006 presidential votes for a pro-FTA party at each voting board ($Pres_b^{2006}$) in our main regression, as follows:

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

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

As shown in Column (1) in Figure 3a, a 1 pp increase in $Pres_b^{2006}$ is associated with a 0.5 pp increase in the share of pro-ratification voters. Column (2) in Figure 3a of the same figure

35. As explained in Section 3.1, the referendum took place because the Congress was split.

(a)

Dependent variable: $YesVoteSh_b$

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

(b)

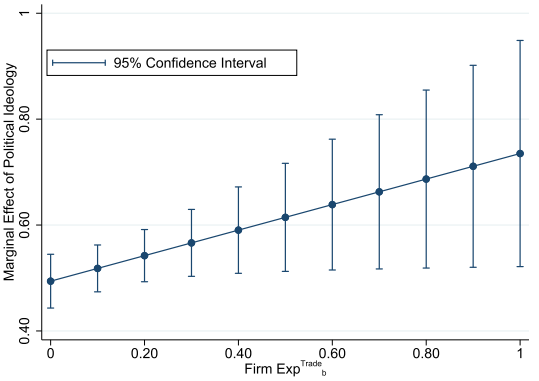


FIGURE 3

Politics, firm exposure, and referendum outcomes. (a) Political affiliation and voting. (b) Marginal effect of political ideology

Notes: (a) The unit of observation is the voting board. Robust standard errors clustered by voting centre (school), are in parentheses. Voting boards are weighted by number of voters. Regressions control for voters' average characteristics, average characteristics of people assigned to the voting centre, and region-fixed effects. We denote: $^{**}p < 0.05$, $^{***}p < 0.01$. (b) This figure plots the marginal effect of political ideology ($Pres_b^{2006}$) for different levels of direct exposure (Exp_b^{Trade}).

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, although 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.

6.1.1. IV strategy. As shown above, the coefficient on firm exposure becomes smaller once we account for political alignment. This can happen if people's position with regard to the FTA's approval influenced their presidential vote in 2006. To orthogonalize our notion of political preferences from the FTA, we employ an IV strategy. Namely, we use votes for pro-FTA political parties in the 2002 presidential election—before any discussions on CAFTA were on the table—to instrument for the 2006 votes for these parties. Further details on the construction of this instrument are presented in Supplementary Material, Appendix E, and results are presented in Supplementary Material, Table E.1. As expected, we find that the coefficient of firm exposure is larger and closer to the values presented in Table 1 when using the instrument; however, it is remarkable that overall the effects remain quite similar to those presented in Figure 3.

6.1.2. When economic interest and ideology collide. The setup gives us a rare opportunity to analyse the interaction between views on politics and trade. Based on Figure 3a, we do a back-of-the-envelope calculation of the effect of political alignment on voters' sensitivity to an extra dollar of trade exposure. We estimate that if *all* voters at a voting board voted for a pro-FTA presidential candidate, the effect on *referendum votes* is equivalent to the voting board having an average trade exposure (Exp_b^{Trade}) of \$19,838.[36]

36. Given the 9 cents on the dollar pass-through (Alfaro-Ureña *et al.*, 2021), this result implies that if each person at a voting board had on average \$1,785 of "money in their pocket" due to the FTA, this effect would be akin to everyone at the voting board having a pro-FTA ideology.

TABLE 3

Comparison across factors—partial R^2

Economic factors (%) (1)	Non-economic factors (%) (2)	Political alignment (%) (3)
−6.8	−11.6	−9.1

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

Beyond this comparison, we can also extend equation (8) with an interaction term between the composition of presidential votes in 2006 and trade exposure. Figure 3b reports the marginal effect of this regression and shows that the effect of the presidential vote is heterogeneous depending on the level of trade exposure considered. We find that trade exposure, as measured by Exp_b^{Trade}, is significantly more salient for voting boards composed of voters with pro-trade political preferences. Conversely, voters with anti-trade political ideologies are less sensitive to trade exposures that might impact their earnings.

6.2. *Comparison across factors*

We now provide a broad comparison of the importance of different factors. Namely, we compare partial R^2 across regressions, after removing certain factors, to grasp what percent of the variation in voting behaviour can be attributed to each. To do so, we consider equation (6) while including direct firm exposure, exposure via expenditures, and political alignment as explanatory variables. Note that this regression includes a battery of demographic and economic controls as well. We then define as economic factors: firm exposure, firm size, firm's trade with the U.S., exposure via expenditures, and employment shares by industry; and as non-economic factors: political alignment and demographics, which include: age, wage, gender, participation, and years of schooling.[37]

Table 3 presents a partial R^2 that results from removing each element from the full specification and calculating the percentage change in R^2 with respect to the full model. A comparison of columns (1) and (3) confirms the relatively large coefficient for political alignment in Table 3. However, we can also verify that economic factors play a non-negligible role in explaining the observed variation in voting behaviour. The latter was particularly true in this setting, in which the referendum was approved with only a 1 pp lead in votes.[38]

7. CONCLUDING REMARKS

While the general public tends to hold a wide variety of views about the consequences of trade, economists have strong and specific priors about how trade affects people's lives. Survey evidence suggests that economists and the broader public have starkly different views on trade

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

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

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 towards trade policy may strengthen the ability of economists to aid policy makers in communicating the consequences of policy decisions to the public and in designing trade policy so that it leads to welfare benefits *and* garners popular support. Moreover, insights about the determinants of popular attitudes may be relevant to how economists understand the distributional effects of trade.

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

The paper studies the role of both economic and non-economic factors. Regarding economic factors, we first examine those related to the income channel. A key message of the paper is that employers' exposure to the FTA, via its impact on employees' earnings, plays a relevant role in shaping votes, especially for pro-trade voters.[39] We also document that indirect exposure through input–output linkages plays a salient role in explaining votes, with a magnitude of about two-thirds the one of the direct effect. Moreover, within-industry heterogeneity—firm-level exposure—is more significant in explaining votes than exposure at the sector level.

The study of the income channel is complemented by analysing the role of the expenditures channel. This analysis is possible by leveraging expenditures surveys to construct a correspondence between consumption baskets and levels of exposure, and then creating a mapping where every voter is assigned an expenditures exposure via their observables. We find that voting boards where voters consume goods that would become more expensive if CAFTA did not pass (as suggested by the demographic characteristics of voters) support CAFTA.

In terms of non-economic factors, our main emphasis is on political alignment, which has been singled as potentially crucial. Indeed, we find that supporting a pro-FTA political party is an important determinant of individual's votes, and we document that voting boards where voters are politically aligned with pro-trade parties are more sensitive to the economic determinants of the CAFTA vote.

A comparison amongst factors finds that economic determinants are almost as important as political ideology in explaining the CAFTA vote. Hence, economic fundamentals played a pivotal role in this context, characterized by the narrow approval margin of the referendum. Moreover, in closely contested elections, they are likely to wield significant influence.

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39. This measure, which might inform future work, is reported in the replication package as part of Supplementary Material, Figure A.3.

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Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

Data Availability

The data and code underlying this research are available on Zenodo at https://doi.org/10.5281/zenodo.14270925

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