


RESEARCH ARTICLE

Why do legislators rebel on trade agreements? The effect of constituencies' economic interests

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Abstract

Most trade agreements are ratified with overwhelming support by legislators throughout the world. This lack of opposition is surprising given the strong distributional consequences of trade and the expectation of conventional political economy theory that parliamentary votes on trade policy should be closely contested between winners and losers of globalization. To analyze the driving forces behind legislators' voting behavior while avoiding the obscuring effect of party discipline, I analyze under which circumstances legislators decide to rebel against their party's position when voting on the ratification of trade agreements. I put forward two hypotheses: First, rebellions are more likely when the trade agreement is with a larger trading partner and when the liberalization through the agreement is more comprehensive. Second, legislators will rebel when their party's position does not align with their constituency's economic interests. These hypotheses are supported by a series of multinomial regression analyses based on an original dataset comprising votes of several thousand legislators from multiple countries on the ratification of trade agreements.

Keywords: preferential trade agreements; legislative behavior; roll-call votes; constituencies' economic interests; trade preferences

Introduction

Trade policies – and specifically free trade agreements – have become a contentious issue in the public discussion. The (now shelved) Transatlantic Trade and Investment Partnership (TTIP) agreement between the EU and the USA has sparked massive public demonstrations throughout Europe and the more recent agreement between the EU and Mercosur, the South American trading bloc, also provoked a heavy public backlash. Despite this rising public opposition against trade liberalization, governments around the world continue to initiate new trade negotiations. Between 2018 and 2020, 41 new trade agreements were signed (Dür *et al.*, 2014) and most of these free trade agreements are ratified without much opposition by legislators in parliaments. This lack of opposition is surprising given the strong distributional consequences of trade, which creates both economic winners and losers through job creation and job losses (Autor *et al.*, 2016). Because of these distributional consequences, conventional political economy theory would actually expect that votes on trade policy are much more contested because politicians in democratic societies should protect the interests of their voters or special interests from their constituency (Grossman and Helpman, 1994). The usually overwhelming support of parliamentarians for trade policy appears to contradict this theoretical expectation. Thus, the question arises: do legislators really take the

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economic interests of their constituencies into account when they vote on the ratification of trade agreements?

Previous studies of this question have nearly exclusively analyzed voting patterns in the US Congress to demonstrate the effect of the economic interests of legislators' constituencies on their voting behavior. Support by US legislators for trade liberalization appears to depend on their constituency's unemployment rates and its export reliance (Gartzke and Wrighton, 1998), financial contributions from labor or business groups (Baldwin and Magee, 2000), changes in inter-industry factor mobility (Hiscox, 2002), the cleavage between import-competing and export-oriented industries (Conconi *et al.*, 2012; Choi, 2015), Chinese import competition and trade shocks (Feigenbaum and Hall, 2015), and vulnerability to offshoring (Owen, 2017). Moreover, scholars identified several important covariates such as ideology (measured by party membership) (Kahane, 1996), the chamber of parliament (Karol, 2007; Ehrlich, 2009), whether legislators belong to the same party as the president (Magee, 2010), and the foreign policy concerns of the US President (Milner and Tingley, 2011). In addition to these US-centered investigations, there exists a small number of single-case studies in Great Britain (Schonhardt-Bailey, 2003), Japan (Kagitani and Harimaya, 2020), Argentina (Murillo and Pinto, 2021), and Brazil (Campello and Urdinez, 2021) that demonstrated the effect of constituencies' economic interest on legislators' voting behavior.

This extensive research has certainly furthered our understanding of the effect of constituency interests on legislative behavior – yet there are important restrictions to the generalizability of these findings. First, the low level of party discipline in the USA, the country's distinctive political and electoral system, and the strong influence of geopolitical concerns on voting behavior makes it hard to know to what degree these findings hold in other countries. Second, these studies focus on an overlapping and limited set of trade agreements that in most cases include the USA as dominating partner and thus are often very similar in their content (Allee and Elsig, 2019). Consequently, the effect of important factors such as agreement characteristics or the distribution of bargaining power between agreement partners on legislators' behavior could not be evaluated. Third, the nexus between constituency interests and legislators' voting behavior on trade policy has not yet been analyzed on a comparative basis.

This paper aims to close these gaps by providing large-scale evidence on ratification of free trade agreements. Ratification is the final step before a trade agreement enters into force, and it is arguably the biggest opportunity for legislators to have a say in their country's trade policy. Moreover, ratification processes are nearly identical in all countries (legislators can only vote in favor, against, or abstain on ratification) and are thus well suited for cross-country comparative research. I have gathered data that includes 13,694 recorded voting decisions from 112 ratification processes in 20 different countries. These votes cover both superficial and deep trade agreements and they also vary substantially in terms of the Gross Domestic Product (GDP) ratio of the countries involved. This wealth of empirical evidence makes it possible to draw conclusions that are more generalizable and allows testing for the effect of agreement characteristics.

When researchers analyze voting behavior of legislators, they face the problem of party discipline which often causes legislators to vote against their constituency's interests (Sieberer, 2006; Carey, 2007). In most democracies, party discipline is strong and legislators are both incentivized to vote with their party and sanctioned if they do not. Thus, we cannot differentiate whether a legislator votes in favor of a trade agreement because she wants to represent her district's interests or because she wants to follow the party line. To overcome this obstacle, I focus my argument on rebellious legislators who vote against the majority of their party. Hanretty *et al.* (2017) have used a similar approach in their work, which showed that rebellions of Conservative legislators in Great Britain on Brexit can be explained by the public opinion in their constituencies. One drawback of this approach is that it most likely underestimates the effect of constituency interests because many legislators who might vote against their party's position will decide against rebellion to avoid being sanctioned. Instead, they might try to protect the interests of their constituency in

alternative ways through influencing the negotiations or by compensating their constituents with other policies (Proksch and Slapin, 2015).

The research question of this paper is thus slightly different than in previous studies: Instead of analyzing determinants of support for, or opposition to, trade agreements, I investigate which factors increase the likelihood of rebellion on ratification of trade agreements. In my first hypothesis, I expect the anticipated economic impact of the agreement to determine voting behavior: The larger the partner country is and the more detailed the agreement is, the higher is the likelihood of a rebellion because the distributional consequences (positive or negative) of this trade agreement will be larger. In my second hypothesis, I argue that the direction of a rebellion depends on the economic situation of the constituency of a legislator. The more a legislator's constituents stand to lose economically from an agreement, the more likely is this legislator to defect from her party if her party is in favor of the agreement and vice versa for legislators from districts that stand to benefit from trade liberalization.

The results of a multinomial regression analysis supports these hypotheses. Legislators from districts that stand to benefit from trade liberalization but whose party votes against ratification have a high likelihood to rebel and vote in favor of the agreement. The opposite is true for parliamentarians from districts that are expected to lose from increased foreign competition but whose party is in favor of the trade agreement. Moreover, the likelihood of a rebellion against the agreement depends on the potential economic impact of the agreement. Superficial agreements with a small trading partner will not affect many voters in any significant way and thus cause little pressure on a legislator to face sanctions from her party for a rebellion. However, deep and comprehensive agreements with a large trading partner will not only generate much higher scrutiny by the public but also a higher pressure on legislators to demonstrate that they have their constituents' interests at heart. These results indicate that legislators indeed take the interests of their voters into account when they make a decision on trade policy – but only when the stakes are high.

This paper makes three important contributions to the literature on the nexus between voting behavior of legislators and the material interests of their constituents. First, to the best of my knowledge, this is the largest and most comprehensive analysis of voting behavior and the first cross-country sample. Therefore, the conclusions drawn from this data are much more generalizable than those from the existing single country studies, which were predominantly focused on the USA. Second, by analyzing ratification votes on a large number of different trade agreements with different agreement depths and GDP ratios between the partner countries, I can test the influence of agreement characteristics on legislators' voting behavior. The third contribution of this paper is to focus on the factors that determine the likelihood of rebellions on trade policy. As outlined above, this innovation allows to test the presented hypotheses even in countries with strong party discipline.

What drives rebellions on trade policy?

Which factors increase the likelihood of rebellion on ratification of trade agreements? There are certainly many different determinants that are not related to trade policy but might still impact the decision of legislators to rebel against their party policy. The comparative politics literature has highlighted several institutional factors including the structure of the electoral system (Hix, 2004; Carey, 2007), the structure of the party (Shomer, 2009) and the level of electoral competition in the party system (Sieberer, 2006) as well as individual factors such as the reelection chances of the legislator (Sieberer and Ohmura, 2021) or her political experience (Olivella and Tavits, 2014).

Regarding trade policy specifically, two additional reasons might compel a legislator to rebel against the party line although this carries the risk of being sanctioned by her party: on the one hand, trade policy is a very ideological topic as the right generally supports free markets whereas the left stresses equality and social justice, which often results in a skepticism toward globalization

(Milner and Judkins, 2004). On the other hand, trade policy can have significant impacts on the job security and wage levels of citizens (Autor *et al.*, 2016). Both reasons might cause legislators to rebel, be it because of political conviction or to signal to their constituents that the legislator is fighting for their interests.

In this paper, I will focus on the second reason and argue that legislators will take their constituency's economic interest into account when considering the ratification of trade agreements. There are three potential channels through which the material interest of the constituency may translate into a rebellion by the region's representative. First, constituents and especially the firms in the constituency can form interest groups that may attempt to sway their legislator's opinion with the help of campaign contributions (Grossman and Helpman, 1994; Drope and Hansen, 2004), the provision of technical expertise (Potters and van Winden, 1992; Hall and Deardorff, 2006), or political information (Hansen, 1991; Wright, 1996). Second, protecting the region's material interests will increase the likelihood that the legislator gets reelected (Gilens and Page, 2014). Third, legislators might just be intrinsically convinced that it is their duty to represent their constituents' interests as best as possible.

Legislators can generally be expected to remain loyal to the party regardless of their constituency's material interests. When they do rebel, it is because the legislator weighed the costs and benefits of rebelling and decided that the rebellion is worth it. The costs of this decision mainly involve all kinds of repercussions from the party but also possibly electoral disadvantages if the legislator is perceived as being disloyal. The benefits depend on which of the three causal mechanisms outlined in the previous paragraph is at play: rebellion might benefit interest groups that support the legislator or they might signal to constituents that the legislator is protecting their interests even against her own party.

I argue that rebellions in both directions (in favor or against the agreement) should be more likely (a) when it includes a wide range of trade liberalizations and (b) when the trade agreement is with a larger trading partner. The depth of an agreement does not only determine the potential economic impact but also its salience in the public. Far-reaching modern trade agreements that go beyond lowering tariffs and also liberalize investments, the services sector, and public procurement policy are much more likely to come under public scrutiny. Given that signaling diligent representation of constituency interests is probably a key motivator for rebellions, there are little incentives to rebel on an issue with low public salience. Potentially, the higher public salience of a deep trade agreement might also increase the likelihood that the party imposes sanctions against rebels. However, the increase in benefits of a rebellion should still outweigh this increase in costs of a rebellion with rising public salience because party sanctions are always a likely consequence of rebellion but public backlash will only occur in high-salience situations. Thus, the first part of this argument reads as follows:

H1a: The likelihood that legislators rebel against their party's position increases with the depth of the agreement.

The economic impact of a trade agreement and the potential risks and opportunities arising from it also depends on the negotiation of the agreement and the bargaining power of the countries involved. A large economy, such as the USA, can often dictate terms in negotiations with smaller partners and thus reap more benefits whereas the partner has to accept more concessions. An agreement with a stronger negotiating partner will cause a significantly higher incentive to rebel. A common measure of bargaining power in trade agreements is the GDP ratio between the countries (Krasner, 1976; Wagner, 1988; Steinberg, 2002). Additionally, reducing trade barriers to a larger economy will have a heavier impact on the local economy (positively or negatively) than liberalizing trade with a smaller economy, which will change trade flows only marginally.

H1b: The likelihood that legislators rebel against their party's position decreases with the GDP ratio between the country and the agreement partner.

Expressed differently: The depth of an agreement and the GDP ratio between the country and its trading partner determine the benefits of a rebellion – when the agreement is superficial and with a small country, there are no real advantages to a rebellion but the repercussions from the party leadership will remain high. Conversely, when the agreement is deep and with a large country that had a bargaining advantage in the negotiations, the benefits of a rebellion might outweigh the costs.

The second hypothesis focuses on the direction of a rebellion. Here, I argue that the direction of rebellion is dependent on whether the voters in a legislator's constituency stand to gain or lose materially from the trade agreement. When the majority of voters will benefit from a trade agreement, the legislator will be pressured to vote in favor of the agreement even when her party is against it. When her voters are threatened by job losses due to trade liberalization, the legislator should be likely to rebel against the agreement in case her party is in favor of ratification.

But what constitutes the economic interest of a voter in regards to trade policy? The political economy literature on this question has long contrasted the Stolper–Samuelson theorem to the Ricardo–Viner model. The Stolper–Samuelson theorem anticipates that trade liberalization helps the owners of the relatively abundant factor of production and hurts the relatively scarce factor. Initially, scholars mainly focused on conflicts between land-owners, laborers, and capital-owners (Rogowski, 1987). Later, researchers drew the distinction between skilled and unskilled labor as two different factors of production that helps explain differences in trade support among citizens. The Ricardo–Viner model differentiates between importing and exporting economic sectors and assumes that the interests of all workers (and employers) within one sector align. This assumes that inter-industry mobility is very low and thus workers are mostly tied to their industry (Hiscox, 2001).

However, more recent studies have noted that the globalization of production has upended both the divisions between classes and those between industries. The model of heterogeneous firms developed by Melitz (2003), which is also known as New New Trade Theory, suggests that only very few, highly competitive firms within each industry are able to export to world markets (Bernard *et al.*, 2009), import intermediate goods, and invest abroad (Helpman *et al.*, 2004). These empirical insights imply that only the most productive firms should support trade liberalization regardless of their industry. Researchers have found support for these firm preferences and demonstrated that larger firms are more likely to engage in political activities in support of trade liberalization (Plouffe, 2017; Osgood *et al.*, 2017; Kim and Osgood, 2019). Therefore, the ideal research design would assess the economic interest of a constituency at the firm level. Unfortunately, firm-level data is very difficult to obtain, especially for a cross-country study. Consequently, researchers often draw on proxies as indicators for the productivity of firms in a certain region. A key requirement for high productivity in modern production is having a large share of highly skilled workers (Ciuriak *et al.*, 2015). Therefore, we can derive from New New Trade Theory that highly skilled workers worldwide should benefit from trade liberalization whereas low-skilled workers should be harmed by increasing foreign competition. Indeed, several studies have found support for this expectation (Burstein and Vogel, 2017; Helpman *et al.*, 2017; Lee, 2020).

The important insight from this discussion for determining the economic interests of a constituency is that regions with a large share of highly educated workers and high productivity should be beneficiaries from further trade liberalization whereas regions inhabited mostly by low-skilled workers and characterized by low productivity should fear trade liberalization. Therefore, the constituencies' economic interest, which I expect to determine whether a rebel is in favor or against the trade agreement, is constituted by its skill level, its productivity, and its economic competitiveness. This economic interest will cause rebellious legislators to break with their party if the party policy goes against the material interest of the legislator's constituency.

Therefore, the observed direction of the rebellion – whether the rebel votes in favor or against the trade agreement – should also be driven by the economic interest of the legislator’s constituency. In regions that stand to gain from trade liberalization, we should primarily observe rebels that vote in favor of ratification although their party is against and vice versa in regions that are expected to lose materially from the agreement.

Of course not all voters in a constituency have the same material interest and using aggregate measures to model their policy preferences is not ideal. However, there are several reasons why this approach should be valid. First, people’s material interests are not only affected by their own income but often also depend on the income of their partner, their parents, their children or other family members. Goldstein *et al.* (2008) showed that even beneficiaries of trade can have protectionist attitudes when they are married to somebody who stands to lose his or her job due to globalization. This is in line with research that shows that individuals consider the economic impact of trade on their community as a whole at least as much as its impact on their own economic situation (Mansfield and Mutz, 2009). Second, trade liberalization has widespread effects that are geographically concentrated (Broz *et al.*, 2021). Job losses in import-competing sectors will have ripple effects causing wage declines in sectors such as hospitality, leisure, and personal services, falling property values, and a shrinking local tax base. Highly productive and export-oriented firms also tend to cluster spatially and thus improving economic conditions not just of their employees but of many more inhabitants of the region. Third, given that workers in most countries are rather immobile, it is reasonable to expect them to look for jobs within their region should they lose their previous employment. If there are enough competitive firms in a region to balance the loss of jobs in non-competitive firms, the net effect of trade liberalization for workers might still be positive. Fourth, many voters might be ignorant of their own material self-interest in trade policy and instead base their opinion on sociotropic preferences (Rho and Tomz, 2017; Jamal and Milner, 2019).¹ Therefore, I argue that the material interest of the region as a whole should be the driving force behind legislators’ voting behavior. Expressed as a hypothesis:

H2: The likelihood that legislators rebel against (in favor of) ratification increases with the material losses (gains) of trade liberalization for their constituency.

Research design

Case selection

As a basis for assessing the total universe of ratification votes on trade agreements, I used version 2.1 (2022) of the Design of Trade Agreements (DESTA) database (originally introduced by Dür *et al.* (2014)) to construct a list of all trade agreements that entered into force or were signed between January 2010 and March 2021. I limited my analysis to this period because only a few countries make voting records from earlier years digitally available. Because the voting behavior of legislators in autocracies cannot be expected to reflect the material interests of their constituencies, I excluded all countries with a score below 4 in the Polity V data series (Marshall and Gurr, 2020). Furthermore, I have excluded any ratification votes by the European Parliament because nearly all its members are elected from national constituencies and thus do not represent the material interests of a specific electoral district. Moreover, the political groups comprise multiple national parties and have only very few tools to maintain party discipline (Bowler and McElroy, 2015). However, I do include the ratification votes by national parliaments of EU Member States on CETA, which were held because CETA as a mixed agreement also required ratification by each individual Member State. I have attempted to gather the voting records on

¹For a critique of using sociotropic preferences as substitutes for individual preferences, see Schaffer and Spilker (2019).

all trade agreements in the thus defined universe of cases by searching for them on parliamentary websites or contacting parliamentary officials. Because many countries lack parliamentary databases or do not keep voting records, I only found information on 232 ratification processes. In my estimation, this covers around one quarter of all ratification votes that took place between January 2010 and March 2021. Of these 232 ratification votes, 37 were approved by a voting mechanism such as hand-raising where no vote totals were recorded.

It is interesting to note that the ratification of trade agreements appears to be much less controversial than one might expect based on well-known cases such as CETA between Canada and the European Union, which nearly failed due to opposition in Member State parliaments. Of the 195 ratification votes that recorded at least overall vote margins, 59 passed unanimously and another 87 were approved by at least 80% of legislators. Only two ratification votes had less than 50% votes in favor.² Of course, the rarity of failed ratification votes can be explained to a large degree by the fact that governments will only table the vote when they are reasonably sure that it will pass. Nevertheless, the overwhelming majorities in support of trade agreements that very often include large parts of the opposition are astonishing.

Trade rebels

Of the 195 ratification processes for which detailed voting information was available, only 112 took place in the 20 countries that witnessed at least one rebellion and thus were relevant for this analysis.³ To identify rebels, I assess whether the vote of a legislator differs from the mode voting behavior of the party to which the legislator in question belongs. If a legislator votes differently than the majority of her party, I further assess the direction of the rebellion: if the party majority votes in favor of the trade agreement but a legislator either votes against or abstains, I code her as a ‘Contra Rebel’. If the party majority votes against the trade agreement and a legislator either votes in favor or abstains, I code her as a ‘Pro Rebel’. If the party decides to abstain from the vote, a rebel could be either ‘Contra’ or ‘Pro’, depending on the direction of her rebellion. In 15 instances, there is no clear party position because none of the three vote options (Yes, No, and Abstain) is chosen by more than 50% of the party’s legislators (e.g. the legislators of the Social Democratic Party of Switzerland voted as follows on the ratification of the Central America-EFTA Agreement: 20 Yes, 12 No, and 19 Abstain). I dropped the 416 legislators of these parties from the analysis to make sure that only ‘true’ rebels who vote against a clearly defined party line are included.

Rebellions on trade agreements – as I define them – are not too frequent but also not extremely rare. Of the more than 13,000 votes in the sample, 1002 (or 7.7%) are classified as rebellions. The frequency of rebellions also varies strongly between countries: In Czechia nearly 20% of legislators rebelled and in France and the USA, at least 15% of legislators cast rebellious votes. Figure 1 shows the share (and direction) of rebels for each country in the sample. There is also significant variation in the direction of rebellions: Whereas Switzerland and the USA have predominantly pro rebels, all other countries have mostly contra rebels and several countries did not witness any pro rebellions at all. This distribution is of course largely determined by the position of the parties in these countries: If all parties vote in favor of ratification – which happens often – pro rebellions are logically impossible.

²These concern the ratification of CETA by Cyprus and France; the ratification of CETA in France succeeded due to a large number of abstentions. See Figure A1 in the Online Appendix for a histogram of approval shares.

³See Table A3 in the Online Appendix for a detailed overview.

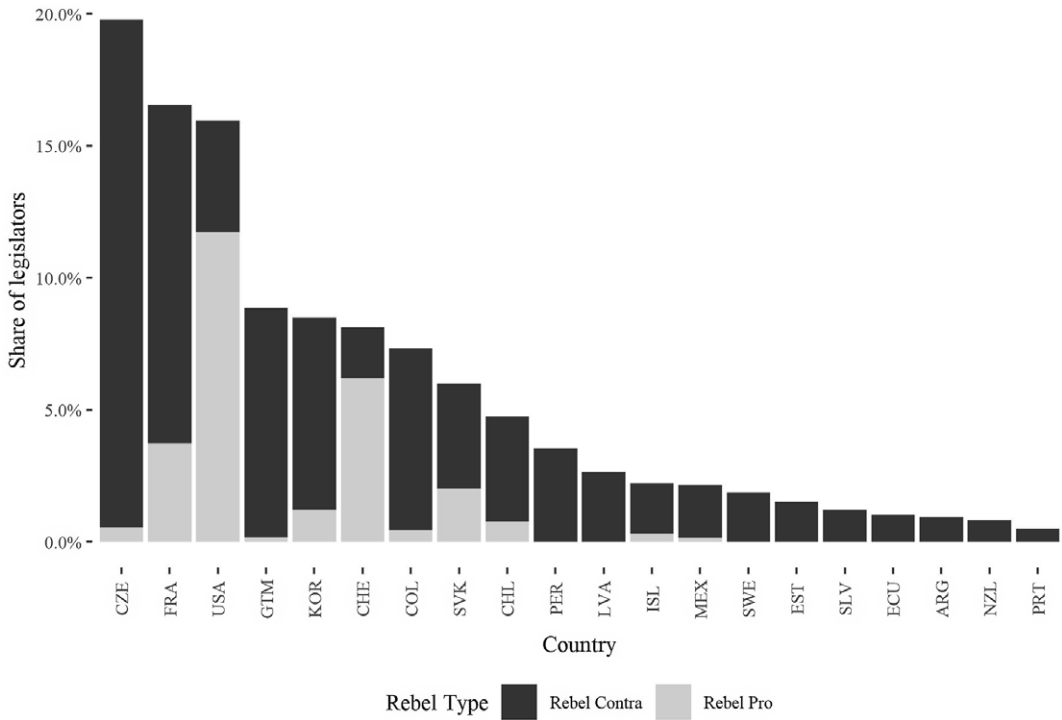


Figure 1. The frequency and direction of rebellions varies much between countries.

Explanatory variables

To test H1a, I use the depth index of trade agreements made available by the DESTA database (Dür *et al.*, 2014). Their index is based on the number of key provisions that can be included in trade agreements and ranges from 0 (indicating a very shallow agreement) to 7 (indicating a very deep agreement). Most of the coded agreements have high scores on the depth index, which reflects the fact that trade agreements overall have become ever deeper since the beginning of the millennium. To account for the causal mechanism expressed in H1b, I calculate the ratio between the GDP of the country and its partner. I use the data on GDP in current US dollars from the World Economic Outlook 2019 published by the International Monetary Fund (International Monetary Fund, 2019). I apply the logarithm to reduce the effect of extreme outliers in the sample. Because the European Commission negotiates trade agreements on behalf of all Member States of the European Union, I assign all Member States the GDP of the whole European Union to reflect that smaller Member States of the European Union punch well above their weight in trade negotiations.

For H2, I use three different measures to capture the material interests of electoral districts. The first two originate from the Subnational Human Development Database (Smits and Permanyer, 2019). First, I used the mean years of schooling of the population aged 25+ as an indicator of the skill level in this district. Second, I use the Gross National Income (GNI) per capita in thousands of US dollars (2011 PPP) to capture the wealth of a district. GNI per capita is not the best measure of a district’s productivity but it is the only one that is widely available. Both these measures are divided by the country average. The third indicator is a measure of a district’s trade competitiveness described in detail by Huber *et al.* (2021). Broadly summarized, this approach uses trade data at the national level and employment shares by industry at the regional level (from labor force

surveys) to calculate the export-to-import ratio at the regional level in a similar way to how national trade competitiveness is often calculated (e.g. by Conconi *et al.* (2012)). The resulting trade competitiveness variable measures how closely aligned the district's economic structure is with the comparative advantage of its country. Competitive regions have positive values and non-competitive regions negative values. Unfortunately, this measure is not available for all countries in the sample⁴ and is already centered to the country average. Thus all three indicators measure within-country differences in constituencies' economic interests (which is accounted for in the following regression analyses by the incorporation of country fixed effects). These three indicators are available for administrative divisions that coincide with the electoral districts in most countries that use proportional representation. Except for Senators in Mexico and the USA who are elected in states as a whole, legislators from countries with majoritarian systems are elected in single-member districts that are nested within the administrative divisions for which these measures are provided.⁵ Legislators who are elected nationally are dropped from the main analysis because they do not represent the material interests of a specific electoral district within the country. Section 7.2 in the Online Appendix provides descriptive statistics, and Section 7.3 details the data availability for each country.

Control variables

Furthermore, I include some additional variables in the analyses to control for alternative explanations of rebellions. First, there might be a gender gap in the likelihood to rebel: existing research suggests that women are more consensus-oriented and risk-averse than men, which might reduce their likelihood to rebel (Papavero and Zucchini, 2018; Finke, 2019; Dingler and Ramstetter, 2021); gender is also known to be an important driver of individual-level trade preferences (Mayda and Rodrik, 2005). I also control for the fact whether or not the legislator is part of the government coalition. On the one hand, legislators belonging to the government might have less incentives to deviate from the party policy because exclusion from their party would also result in losing access to the government and influence on policy making (Carey, 2007). On the other hand, in certain situations legislators belonging to the majority might be more prone to rebel as they want to differentiate themselves from within-party competition (Kirkland and Slapin, 2019). To account for differences in the electoral system of a country, I include a categorical variable that measures whether a legislator is elected through proportional representation or a majoritarian electoral system (Election Guide, 2022). The expectation here is that party unity is lower in majoritarian systems (Carey, 2007). Majoritarian systems also tend to generate more protectionist trade policies than proportional systems (Grossman and Helpman, 2005; Evans, 2009; Hatfield and Hauk, 2014).⁶ To control for the timing of the electoral cycle during which the vote takes place, I include the time until the next scheduled election (in fractions of years). This accounts for the tendency of lawmakers to become more protectionist with increasing election proximity (Conconi *et al.*, 2014; Kagitani and Harimaya, 2020). Because right-wing parties are ideologically inclined to support trade liberalization whereas left-wing parties tend to oppose capitalist globalization (Milner and Judkins, 2004), I also control for the economic left-right position of a legislator's party. The party's ideology might also determine the likelihood of rebellions in general, although there is debate whether left-wing parties are more likely to experience rebellions than right-wing parties or the other way round (Close, 2018). I use variable V4 from the Global Party Survey (Norris, 2020), which classifies the current stance of a party on economic issues such as privatization, taxes, regulation, government spending, and the welfare state (measured on a continuous

⁴In the main model, this affects only Guatemala.

⁵It would be preferable to use district-level values in these countries but such information is not widely available.

⁶Other studies find that open-list proportional systems are as likely as majoritarian systems to produce rebels (Coman 2015; Sieberer and Ohmura 2021). I control for this in one alternative model.

scale from 0 for far-left to 10 for far-right). Because this expert survey was taken just once (so far) in 2019, there is no information available for some parties in my sample that ceased to exist by 2019. In instances where a clear successor party exists, I fill these gaps in the data manually. To control for any additional country-specific factors (such as institutional designs of the legislature, specific aspects of the electoral system, or general levels of trade support), I include country fixed effects. Figure 5 in the Online Appendix provides information on the average value of all independent variables based on whether the legislator is loyal, a contra rebel, or a pro rebel.

Model specifications

In the main model, I calculate the likelihood of each of the three outcomes (loyal, rebellion contra, rebellion pro) in a multinomial logistic regression model with loyal legislators as the reference category. Because multinomial logistic regression models that lack one category would produce country fixed effects with standard errors valued 0, I limit the sample in the main model to those countries that have at least one contra rebel and one pro rebel. This leaves a sample size of 8,470 including legislators from Chile, Czechia, France, Guatemala, the Republic of Korea, Mexico, Switzerland, and the USA for the main model (One less conservative robustness check includes all countries with rebels and thus has a larger sample size of 12,885 from all 20 countries in the total sample). This estimation strategy is ideally suited to test the hypotheses because there are three categories that are not ordered in any meaningful way in the dependent variable. However, this method suffers from the problem of rare events because 90.7% of legislators in this restricted sample are loyal whereas only 5.3% are contra rebels and only 4.6% are pro rebels. Consequently, there might be a small-sample bias that causes the model to underestimate the probability of the two kinds of rebellions (King and Zeng, 2001).

Results

Main model

Table 1 presents the results of four multinomial logistic regression models that estimate the likelihood that a legislator is loyal, a contra rebel, or a pro rebel. For each model, two sets of coefficients are displayed – one that shows the likelihood of a contra rebellion and one that shows the likelihood of a pro rebellion, both times calculated against the baseline of loyalists. The first model does not include any indicators of the material interest of the constituency; Models 2 through 4 rotate the three different indicators. In all four models, the coefficients for the logged GDP ratio and the depth of the agreement are in the expected direction and statistically significant – but only for the likelihood of a contra rebellion. For a pro rebellion, these coefficients are not statistically significant. These results indicate that legislators are more likely to vote against an agreement even if that means defying their own party when an agreement is deep or with a larger trading partner, which is in line with H1a and H1b, respectively.

The indicators for the material interests of the constituency are negative and statistically significant for the likelihood of a contra rebellion. This means that the higher the skill level (measured with the mean years of schooling), the productivity (measured with the logged GNI per capita), or the trade competitiveness of a constituency is, the lower is the likelihood of a contra rebellion. However, when these indicators are low and the constituency might be impacted negatively by the trade liberalization, the likelihood of a rebellion against the agreement increases. However, the picture is less clearer for the likelihood of pro rebellion: In Models 2 and 3, which uses the mean years of schooling and the GNI per capita as indicators, respectively, do I find the expected positive and statistically significant effect. However, the coefficient for subnational trade competitiveness in Model 4 is positive but not statistically significant. Nevertheless, these results yield partial support for H2.

Table 1. Rebel behavior on trade ratification, only countries with both types of rebel

	Model 1		Model 2		Model 3		Model 4	
	Contra	Pro	Contra	Pro	Contra	Pro	Contra	Pro
Explanatory variables								
Logged GDP ratio	-0.26*** (0.08)	-0.04 (0.08)	-0.27*** (0.08)	-0.05 (0.08)	-0.27*** (0.08)	-0.05 (0.08)	-0.33*** (0.09)	-0.06 (0.08)
Agreement depth	0.19*** (0.06)	-0.10 (0.08)	0.20*** (0.06)	-0.09 (0.08)	0.19*** (0.06)	-0.09 (0.08)	0.25*** (0.07)	-0.07 (0.08)
Mean school years			-2.12*** (0.52)	4.45*** (1.70)				
GNI per capita					-0.62*** (0.23)	0.88** (0.36)		
Subn. trade comp.							-1.71*** (0.40)	0.17 (0.57)
Control variables								
Female	0.38*** (0.12)	-0.13 (0.14)	0.41*** (0.12)	-0.14 (0.14)	0.41*** (0.12)	-0.17 (0.14)	0.37*** (0.13)	-0.16 (0.14)
Party left-right	-0.14*** (0.02)	-0.86*** (0.05)	-0.15*** (0.02)	-0.88*** (0.05)	-0.14*** (0.02)	-0.89*** (0.06)	-0.15*** (0.03)	-0.91*** (0.06)
Years to election	-0.25*** (0.05)	0.13** (0.06)	-0.25*** (0.05)	0.12** (0.06)	-0.25*** (0.05)	0.13** (0.06)	-0.23*** (0.06)	0.13** (0.06)
Majoritarian	2.09*** (0.29)	2.55*** (0.28)	2.10*** (0.29)	2.60*** (0.28)	2.08*** (0.29)	2.61*** (0.28)	2.01*** (0.29)	2.60*** (0.28)
Government	-0.77*** (0.12)	1.49*** (0.19)	-0.79*** (0.12)	1.58*** (0.19)	-0.77*** (0.12)	1.56*** (0.19)	-0.80*** (0.12)	1.59*** (0.19)
(Intercept)	-3.90*** (0.45)	-0.89* (0.53)	-1.78*** (0.68)	-5.34*** (1.80)	-3.28*** (0.50)	-1.72*** (0.65)	-4.16*** (0.50)	-0.97* (0.55)
AIC		5104.91		5037.65		5050.53		4665.95
BIC		5316.24		5263.01		5275.88		4875.27
Log Likelihood		-2522.46		-2486.83		-2493.26		-2302.97
Deviance		5044.91		4973.65		4986.53		4605.95
Num. obs.		8470		8453		8453		7922
K		3		3		3		3

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Entries are unstandardized coefficients from a multinomial logistic regression model. Standard errors in brackets. Dependent variable is likelihood of rebellion either in favor or against the agreement compared to the baseline of loyal legislators. Country fixed effects omitted.

Turning to the control variables, we see that right-wing legislators are consistently less likely to rebel in either direction, which indicates stronger overall party cohesion of right-wing parties. Legislators from majoritarian electoral systems are also more likely to rebel generally. Female legislators have a higher likelihood of being contra rebels but there is no significant gender difference with pro rebels. Interestingly, the other two control variables point into different directions for the two types of rebellion. Legislators close to re-election and opposition legislators are all the more likely to be contra rebels but legislators with a long duration until the next election and legislators belonging to the government are more likely to be pro rebels.

Figure 2 illustrates the size of the effect of the key independent variables. These predicted probabilities of each possible outcome (the legislator remains loyal, she rebels and votes against the agreement, or she rebels and votes in favor of the agreement) are calculated with all other variables set to their average values. It is important to note that the predicted probabilities are very small for some independent variables because of the small sample selection bias and thus the probability of rebellions is probably underestimated.

Relevant for H1a is the first set of charts. When legislators vote on rather superficial agreements, they have a likelihood of about 97.5% to remain loyal. However, this likelihood drops below 95.5% for the most far-reaching modern trade agreements. Conversely, the likelihood of a contra rebellion is around 1.5% for superficial agreements but around 4.5% for deep agreements. Meanwhile, the likelihood of a pro rebellion does not change significantly with different agreement depth. The second row of charts is relevant for H1b and shows the effect of the GDP ratio: When the economy of the country is ten times larger than the economy of the trading partner (i.e. the GDP ratio is +1), only 3% of legislators rebel against the agreement. However, when the country's GDP is ten times smaller than the partner's GDP (i.e. the GDP ratio is -1), the likelihood of a contra rebellion increases and reaches nearly 6%. These findings support H1a and H1b – at least regarding contra rebellions. When an agreement is superficial and thus unlikely to affect many people in a constituency, a legislator has little reason to face potential party sanctions and rebel. The same is true when the agreement opens the door to more imports from a much smaller economy, which will cause only very limited economic disruptions. However, when the trade agreement is far-reaching and might affect a large share of constituents or when the trade agreement is with a larger economy, legislators have significant incentives to rebel against their party when it supports the agreement.

The last three rows in Figure 2 demonstrate the effect of the material interest of the constituency for the different indicators. Legislators from a constituency with low mean years of schooling have a likelihood of close to 8% to rebel and vote against ratification whereas legislators from regions with high skill levels have a likelihood of below 2% to do the same. The opposite effect is visible for the likelihood of a pro rebellion: Only legislators from constituencies that are expected to benefit from trade liberalization due to their high skill level have a noticeable likelihood to rebel in favor of ratification (due to the small sample selection bias, the absolute likelihoods are quite small). A similar picture emerges from the model that uses GNI per capita as indicator. In regards to subnational trade competitiveness, the figure shows that legislators from less competitive constituencies are significantly more likely to rebel against the agreement than those from competitive regions. However, there is no significant effect of subnational trade competitiveness on the likelihood of a pro rebellion. Overall, these predicted probabilities support H2 and demonstrate that legislators do take into account the material interests of their constituents.

Robustness checks

In the following sections, I perform a series of robustness tests. First, I employ eight alternative specifications of the main model. Second, I use jackknife resampling to ensure that the results are not driven by a single country or agreement. Third, I investigate potential interaction effects between the explanatory variables and the control variables.

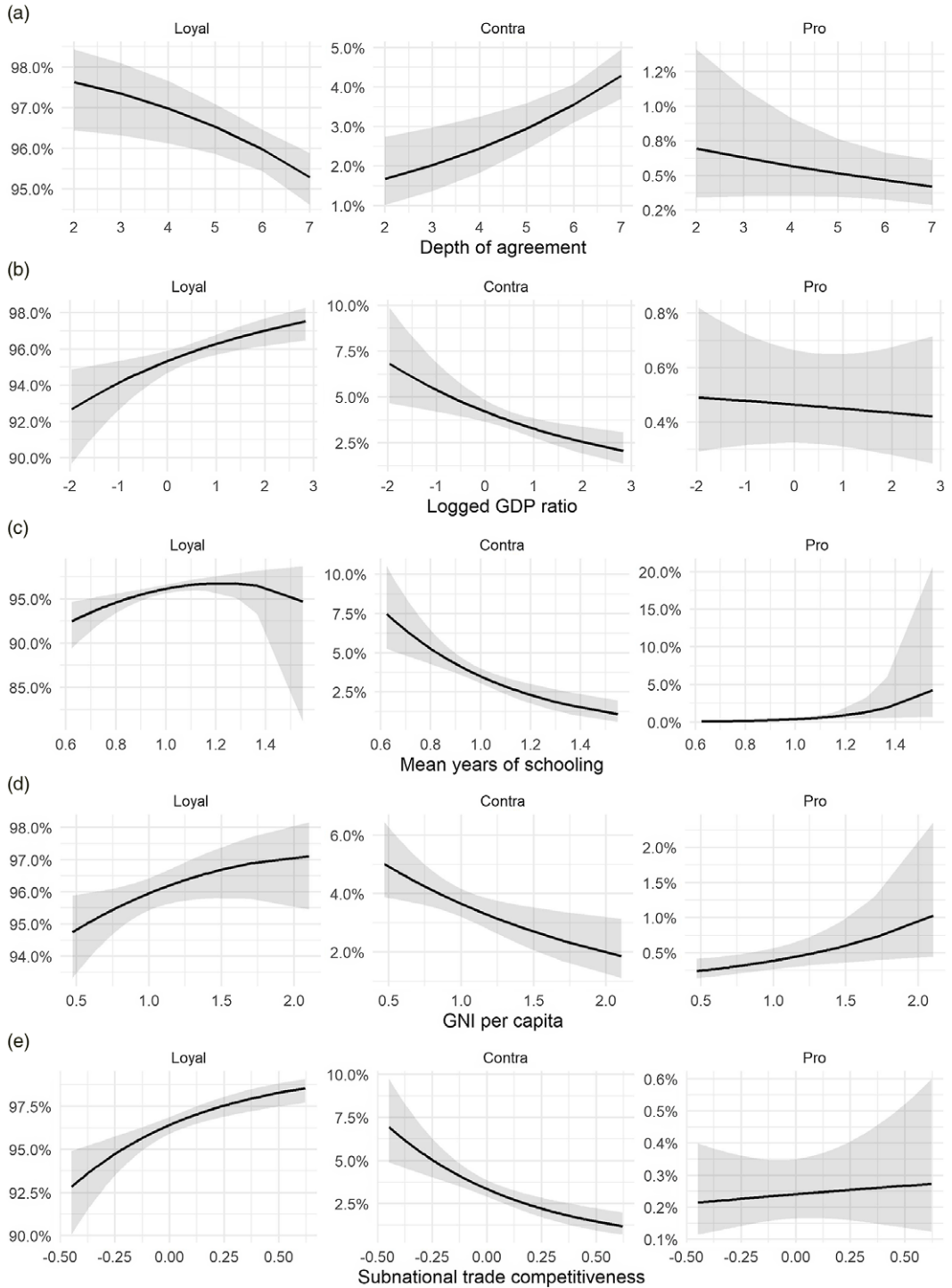


Figure 2. Predicted probability of rebellions depending on variation in key variables. Note: The ribbon represents 95% confidence intervals. Agreement depth and logged GDP ratio are based on Model 1, mean years of schooling on Model 2, logged GNI per capita on Model 3, and subnational trade competitiveness on Model 4 in Table 1.

Alternative model specifications. In the first alternative model specification, I address the concern that some legislators are included more than once in the sample because they voted on multiple agreements by including clustered standard errors. To address concerns that the country fixed effects correlate with the country-level control variables in the model, the second model does not include fixed effects. As outlined in the Section 3.5, the main model is limited to countries that have at least one contra rebel and one pro rebel. Therefore, I include all countries – even those with just one type of rebel – in the third additional test. It is important to note that this generates some country fixed effects with standard errors valued 0 in the columns where this country lacks the respective rebel. Including more countries also allows me to differentiate between closed-list and open-list proportional systems. This is not possible in the main model because too many country-level control variables would lead to complete separation (Cook *et al.*, 2018). To further expand the sample, the fourth check also includes legislators from national electoral districts.

In the fifth robustness check, I add several additional control variables to the main model. As older legislators might be less rebellious (either because they are more risk-averse than younger legislators or because they are more likely to belong to the party establishment), I control for the age of the legislator in years (Stratmann, 2000). This model also includes the chamber of parliament (lower chamber, upper chamber, or unicameral). Lastly, legislators might be more willing to rebel when ratification is certain (and party leaders might be more lenient towards rebels in such cases). Therefore, I control for the eventual vote margin (share of affirmative votes) in parliament. The sixth test controls for the absolute gains or losses the constituency might expect from trade liberalization based on the predictions by the Stolper–Samuelson theorem (whereas the main explanatory variables measure relative gains and losses within the country). For this purpose, I include an additional dummy variable that takes the value 1 when the constituency has a beneficial factor endowment vis-à-vis the partner country (e.g. has a relative abundance of low-skilled workers when matched with a country with many high-skilled workers). In the seventh additional test, I exclude those legislators who abstained from voting to address possible concerns that these legislators are not ‘real’ rebels.

The eighth and ninth tests shall ensure that the results remain reliable even when the analysis is limited to those cases where the median voter theorem applies (Black, 1948). In the eighth test, I remove all legislators from the sample who were elected in constituencies with more than 10 seats. Arguably, the causal link between the economic interest of the constituency as a whole and the voting behavior of the legislators representing it decreases with the number of legislators that are elected in this constituency. This is because legislators might be able to cater to ever smaller segments of the electorate. The ninth test pursues this argument even further and limits the sample to those legislators who were elected through a majoritarian electoral system. Thus, the sample in these regression models is much smaller and it includes only legislators from Czechia, France, Mexico, the USA, and South Korea.

Figure 3 shows the coefficients of the five main variables from these nine alternative model specifications as well as from the main model for comparison. In the case of contra rebellions, the coefficients are all in the expected direction and remain statistically significant in nearly all instances. The only exception to this are the model specifications with only legislators from majoritarian systems or only from constituencies with at most ten seats: here the coefficient for GNI per capita is not significant anymore. This likely is a result of the smaller sample size. Overall, these additional checks demonstrate the robustness of the results of the main model in regards to contra rebellions.

Turning to pro rebellions, the coefficients for agreement depth and GDP ratio are not statistically significant in most models – just like in the main model. However, in the test with only majoritarian systems, the coefficient for agreement depth actually turns negative, which counters H1a but might be attributable to the highly selected set of countries included in this particular sample. In the model without country fixed effects, the coefficient for the GDP ratio is positive, which somewhat weakens the support for H1b as this would indicate that legislators are actually

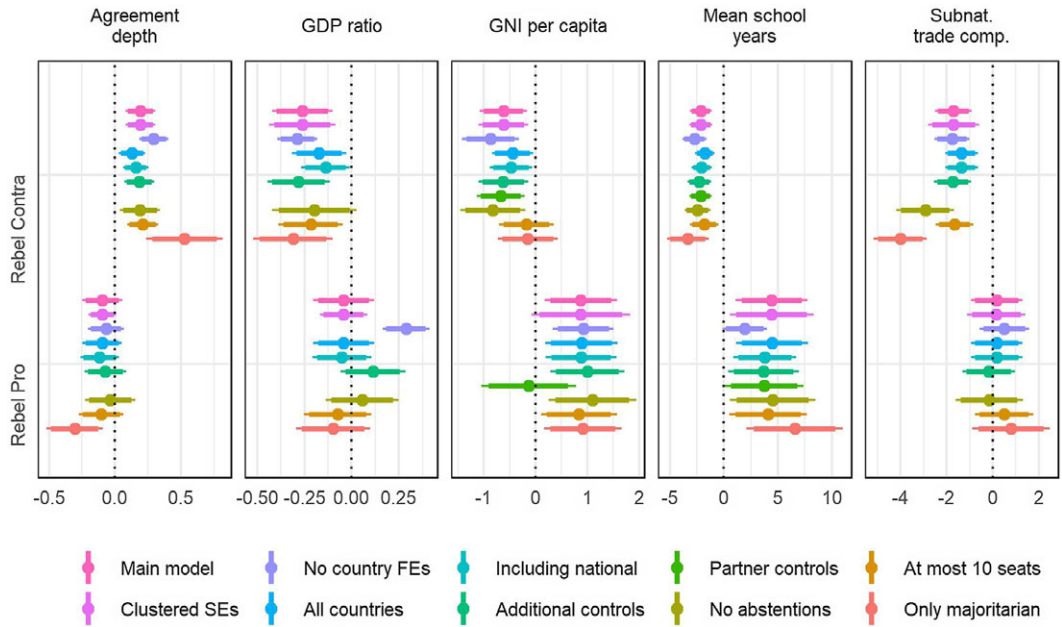


Figure 3. Coefficients of main variables from alternative model specifications.
 Note: Points are unstandardized estimates from a multinomial regression analysis. Ranges represent 90 and 95 percent confidence intervals. Based on models in Tables 5 to 13. Estimates for agreement depth and GDP ratio are based on the baseline models without indicators of constituencies’ economic interest.

more likely to rebel in favor of an agreement with larger trading partners. Overall, these hypotheses concerning the agreement characteristics apparently apply very well to contra rebels but less so for pro rebels. However, the coefficients for the mean years of schooling and GNI per capita remain positive and statistically significant for pro rebels in all but one model, which supports H2. The only model where GNI per capita is not statistically significant is the one that includes the dummy for a beneficial factor endowment, which is itself positive and statistically significant and thus can be assumed to have absorbed the predictive power of the main explanatory variable (See Table A9 in the Online Appendix). The coefficient for subnational trade competitiveness is not significant in any model for pro rebels.

Jackknife resampling. To test whether the results are driven by specific agreements or countries, I recalculated the main model reported in Table 1 where each time another agreement or country is dropped from the sample. Section 7.6 in the Online Appendix shows the results of these tests. For contra rebellions, the coefficients of all explanatory variables vary little when individual agreements are removed. When entire countries are dropped from the sample, the coefficients remain in the expected direction but are no longer significant in some instances (which is not surprising due to the reduction in observations). However, when removing the observations from South Korea, the coefficient of subnational trade competitiveness swings from negative to positive (albeit being not significant), which indicates that the support for H2 using this indicator for the material interests of a constituency might be driven by South Korea.

For pro rebellions, the coefficients for mean years of schooling and GNI per capita are no longer significant when the USA as a whole is removed from the sample. This is not surprising given that it represents 63% of pro rebels. Interestingly, the coefficient for the depth of the agreement is now significantly positive and the coefficient for the GDP ratio significantly negative, which would support H1a and H1b, respectively. These hypotheses seem to hold for pro rebels, too – but only outside of the USA.

Interaction effects. To further investigate the causal mechanism underlying the hypotheses, Section 7.7 in the Online Appendix provides the results from variations of the main model including interaction effects between the explanatory variables and the control variables. These results are mostly in line with the theoretical expectations discussed in Section 3.4. For example, Figure A11 shows that women react more to changes in the depth of agreements than men, which is in line with findings that women only rebel on matters close to their heart (Dingler and Ramstetter, 2021). Differences in agreement depth also matter more for legislators that were elected through majoritarian systems, which reflects the notion that they are less dependent on their party and can act more in line with their constituency's interests.

Conclusion

What determines the likelihood that legislators rebel and defect from their party's trade policy? To what degree are these rebellions a reflection of the economic interests of their constituency? This paper tested two hypotheses to answer these questions: First, it has argued that the likelihood of rebellion increases with the likely economic impact of the agreement measured by the depth of the agreement (H1a) and the GDP ratio (H1b) between the country and its trading partner. Second, the paper argued that the direction of a rebellion (i.e. whether a rebellious legislator votes in favor or against the agreement) is dependent on whether their constituents will gain or lose from the trade liberalization (H2). There are various reasons why legislators wish to take this step despite potential sanctions from their party: these reasons include pressure from interest groups, an improved chance at re-election or an intrinsic conviction. Regardless of the exact causal mechanism, both hypotheses are (at least partially) supported by a series of multinomial analyses based on an original dataset comprising votes of several thousand legislators from several countries on the ratification of trade agreements. The empirical evidence is strongest for rebellions against the agreement: The likelihood of such a 'contra' rebellion increases with the depth of the agreement and decreases when the GDP of the country of the legislator is higher than the GDP of the partner country, while this effect is not found for 'pro' rebels (except when the USA is removed from the sample). Furthermore, for all three indicators of constituencies' economic interests, the empirical evidence presented in this paper shows that if the residents in the legislator's electoral district stand to lose materially from a trade agreement but the legislator's party supports ratification, the legislator has an increased likelihood to rebel and vote against the agreement. The opposite is true for legislators from districts with high productivity and high skill levels, which might gain from trade liberalization: these legislators are tempted to vote in favor of ratification even if the majority of their party is against it.

This paper makes important contributions to the political economy literature by confirming the causal link between the material interests of constituents and the voting behavior of legislators, which seems to be at odds with the empirical observation that most trade agreements are ratified with overwhelming parliamentary support. By analyzing ratification votes in many different countries and on various different trade agreements, this paper demonstrates that not all trade agreements cause the same pressure on legislators to rebel. Only those trade agreements that are far-reaching, and will have big impacts, generate a high likelihood of rebellions. Superficial trade agreements with small partner countries only rarely witness rebellions. The results in this study also allow for more generalizability than previous studies, which mostly focused on the USA. Although this paper has focused on the ratification of trade agreements, these results have implications for all aspects of the domestic politics of international trade: Decision-makers are indeed aware of the distributional consequences of trade liberalization and take the economic interests of their constituencies into account – but only when the stakes are high enough.

This paper also speaks to the larger literature on party discipline and legislative behavior beyond trade policy. First, the mechanisms that can explain rebellions on trade agreements should

also apply to other economic policy areas, such as taxation, that have generated winners and losers in different regions of a country. Second, this research has underlined the fruitfulness of moving beyond analyzing the vote choice of legislators where the preferences of a legislator cannot be disentangled from party pressure. Future research should continue to focus more on rebellions and other types of legislative behavior that truly reveal legislators' policy preferences.

Supplementary material. To view supplementary material for this article, please visit <https://doi.org/10.1017/S1755773922000364>.

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