Did Trade Liberalization with China Influence US Elections?*

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Abstract

We examine voting and roll-call votes in the United States over a twenty-five year period. Voters in counties more exposed to trade liberalization with China in 2000 subsequently shift their support towards Democrats, relative to the 1990s, but this boost for Democrats disappears after the rise of the Tea Party in 2010. House members' votes in Congress rationalize these trends: House Democrats vote similarly to Republicans on trade-related bills in the 1990s, disproportionately support protection during the 2000s, and then converge once again with Republicans in the 2010s.

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1 Introduction

Globalization has long been a contentious issue in US elections, especially in industrial areas (Frieden (2019)). In recent years, a shift in how these areas vote has had a profound impact on US elections, and in turn on national policies. This change is often framed as a rejection of long-standing support for Democrats in favor of Republicans, who have become increasingly hostile to international trade. In this paper, we find that this shift in voting represents the unwinding of a temporary jump in support for Democrats in the early 2000s, when Democratic legislators were more protectionist than their Republican counterparts.

We begin with an analysis of how votes cast for federal office respond to the US extension of Permanent Normal Trade Relations (PNTR) to China in 2000, one of the most substantial US trade liberalizations in the last 50 years. We measure an area's exposure to PNTR's elimination of trade policy uncertainty with China in terms of its industry structure, and relate this exposure to the share of votes cast for each party in elections for the House of Representatives, the Senate, and the Presidency.

Using a difference-in-differences empirical strategy, we find that in the years immediately after PNTR, counties more exposed to the change in policy exhibit relative increases in the share of votes cast for Democrats *vis a vis* the 1990s. Coefficient estimates suggest that moving a county from the 25th to the 75th percentile of exposure is associated with a 2.4 percentage point relative increase in the share of votes cast for Democrats, a sizable impact compared to the 49 percent weighted-average Democrat vote share across counties in the 2000 Congressional election.[1] This bump in support, however, is short-lived. It begins to dissipate in 2010, coinciding with the rise of the Tea Party wing of the Republican party. By 2014, it has disappeared, returning voters' relative preference for Democrats in exposed regions to the 1990s baseline.

In the second portion of the paper, we explore whether the temporary increase in support for Democrats can be rationalized by the policy choices of legislators once they are elected to Congress by conducting a regression discontinuity analysis comparing the legislative votes of Democratic and Republican representatives who win election by small margins. We find that while both parties voted similarly on bills related to trade in the 1990s, Democrats in the early 2000s were significantly more likely to vote for measures that would restrict international trade than their Republican counterparts. After the rise of the Tea Party wing of the Republican party in 2010 (Rosentiel (2010)), and continuing through the party's more recent hostility toward trade, we find that representatives from the two parties vote similarly on trade-related bills. Together with the patterns of voting described above, these findings suggest voters in areas more exposed to import competition via PNTR were more likely to vote for Democrats during the period that Democratic representatives were more likely to restrict trade. Moreover, they provide an explanation for the more recent decline in support for Democratic candidates in areas more exposed to trade liberalization, as the relative protectionism of Democrats faded.

[1] While this reaction is most evident in elections for the House, a less-pronounced effect is present in elections for the Senate. We find no statistically significant relationship for Presidential elections or for voter turnout.

Because House elections are decided by district, rather than by county, our baseline results with respect to election voting should be interpreted as providing information on how trade liberalization is associated with changes in voters' preferences for candidates of each party over time, without providing *direct* implications for election *outcomes*. Even so, results at the county level are important because they are *suggestive* of changes in election outcomes, and because shifts in voting patterns may alter legislators' policy choices even if the results of elections don't change, as districts become more or less closely contested. Moroever, when we conduct district- and county-level analyses over an identical time period, we find that they yield similar results, indicating that the level of aggregation itself does not have a major influence on our results.

The key advantage of county-level analysis is that it offers insight into the relationship between trade liberalization and voting that is missed at the district-level. This contribution stems from the stability of county boundaries over time, which allows comparison of changes in voting over long intervals that span Congressional redistricting after each decennial Census. Absent strong assumptions, district-level analysis, by contrast, is restricted to periods consisting of five consecutive House elections, e.g., 1992 to 2000 or 2002 to 2010. This limitation is especially problematic for evaluating voting preferences in industrial regions because it precludes comparing outcomes before and after the steepest portion of the post-2000 decline in manufacturing employment. Indeed, two-thirds of the 19 percent decline in manufacturing employment between 2000 and the onset of the Great Recession in 2007 occurs in the redistricting period between November 2000 and November 2002. Any immediate impact of this decline, therefore, cannot be captured by district-level analysis, which is restricted to the periods before or after the steepest employment decline but excluding the period of decline itself.

Our paper relates most directly to the growing literature on the relationship between trade and political outcomes in both political science and economics, with recent research focusing on the trade policy preferences of voters and legislators and the polarization of the electorate. Conconi, Facchini, and Zanardi (2012), for example, find that the import or export exposure of US Congressional districts determines how members of Congress vote on bills to grant Fast Track Authority to the President for trade negotiations.[2] In regards to trade with China, Feigenbaum and Hall (2015) examine the impact of Chinese imports on the roll-call behavior of legislators and electoral outcomes. They find that legislators from districts experiencing larger increases in Chinese imports become more protectionist in their voting on trade-related bills, and that incumbents are able to insulate themselves from electoral competition via their voting behavior.[3]

[2] Blonigen and Figlio (1998) find that legislators' votes for bills related to trade protection are positively associated with direct foreign investment in their districts, and Conconi, Facchini, Steinhardt, and Zanardi (2020) examine the role of skilled labor abundance in Representatives' votes on trade and immigration bills.

[3] Relatedly, Jensen, Quinn, and Weymouth (2017) find that votes for presidential incumbents rise with expanding US exports and fall with rising US imports. In related research on immigration rather than trade, Mayda, Peri, and Steingress (2016) find that the share of votes cast for Republicans in US elections responds to the level of immigration, with the effect varying based on the share of naturalized migrants and non-citizen migrants in the population. Outside the United States, Dippel, Gold, and Heblich (2015) examine data from

More recently, Autor, Dorn, Hanson, and Majlesi (2020) show that increased Chinese import competition has led to increased political polarization, in terms of the partisan rankings of members of Congress, recipients of political contributions, and cable news viewership. Autor, Dorn, Hanson, and Majlesi (2020) also find that majority-white Congressional districts that experience larger increases in Chinese imports between 2002 and 2016 become more likely to elect conservative Republicans to the House, while majority minority districts become more likely to elect liberal Democrats during that period, with a relative increase in the probability of electing a Republican candidate, on net. Our use of a sample period beginning in 1992, however, reveals that counties more exposed to PNTR vote similarly to less-exposed counties in the 1990s, and then shift votes toward Democrats in the early 2000s, before returning to their 1990s baseline in the 2010s.

We make several contributions to this literature. First, we exploit a major change in trade policy that yielded a concentrated shock to imports and labor markets as part of our identification strategy. Second, as mentioned, our analysis covers a longer time period than previous studies, thereby uncovering a shift towards and then away from Democrats, relative to the 1990s, that is not apparent in shorter time horizons. Third, we provide evidence of an economic rationale for the observed voting behavior by showing that voters in areas more exposed to increased import competition via PNTR shifted their votes toward Democrats when they were, in fact, more likely to restrict trade, and then withdrew their support when the relative protectionism of Democrats faded. Finally, we consider the relationship between this policy shock and voting in a range of national political offices, as well as voter turnout.

Our research also relates to a group of papers that establish a causal link between increased import competition and a range of socio-economic outcomes, highlighting the distributional implications of trade. Autor, Dorn, and Hanson (2013) show that local labor markets subject to larger increases in imports from China experience relative increases in the uptake of disability insurance, along with declines in manufacturing employment. Greenland and Lopresti (2016) document an increase in high school graduation rates in import-competing areas, and Greenland, Lopresti, and McHenry (2019) show that these areas experience relative reductions in population growth. Feler and Senses (2017) find that the provision of public goods falls in these areas as property tax revenue falls, and Feler and Senses (2017) and Che, Xu, and Zhang (2018) show that they also experience relative increases in property crime. Pierce and Schott (2020) find that counties with greater exposure to PNTR exhibit increases in mortality due to drug overdoses and Autor, Dorn, and Hanson (2019) find that US regions with rising imports from China exhibit changes in marriage and fertility patterns.

Finally, the results in this paper offer context for the election of President Donald Trump, who has adopted tariff increases with far-ranging effects, underscoring the important policy implications of elections. Amiti, Redding, and Weinstein (2019) and Fajgelbaum, Goldberg, Kennedy, and Khandelwal (2019) find welfare losses resulting from recent US tariffs on China, with Flaaen, Hortaçsu, and Tintelnot (2019), Waugh (2019), Flaaen and Pierce

German labor markets and find that higher imports from Eastern Europe and China are associated with an increase in the share of votes for far right parties.

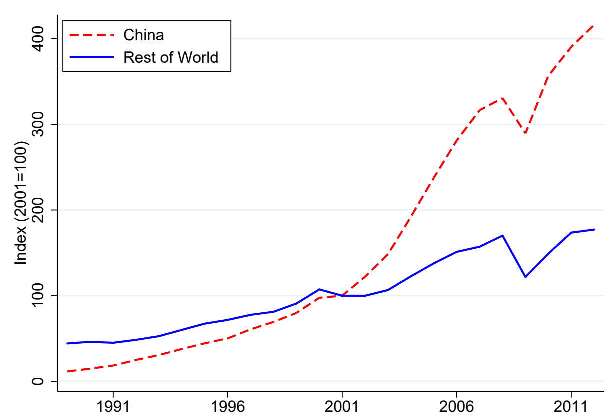
(2019), and Bown, Conconi, Erbahar, and Trimarchi (2020) providing further detail on the trade-offs that can arise between protecting firms and harming consumers and downstream industries. Blanchard, Bown, and Chor (2019) find that Republicans in trade-exposed areas lost electoral support in the 2018 Congressional elections, while Fetzer and Schwarz (2019) and Fajgelbaum, Goldberg, Kennedy, and Khandelwal (2019) examine whether other countries' retaliatory tariffs are geographically targeted.

We proceed as follows. Section 2 describes the growth of China as a US trade partner and focus of political discourse, and section 3 describes construction of variables and data sources. Section 4 presents our empirical strategy and results examining the relationship between exposure to trade liberalization and voting. Section 5 focuses on the regression discontinuity analysis examining how representatives from each political party voted on trade-related bills. Section 6 concludes.

2 China and US Politics

Political discourse over international trade, in both the United States and globally, increasingly focuses on China, mirroring its rapid rise as a global economic power. Over the past forty years, China has jumped from being an insignificant contributor to world GDP to being the United States' largest source of imports, with its share rising from 3 percent in 1990 to 17 percent in 2007, and 21 percent in 2016. A key feature of this increase was a surge in imports following the US granting of PNTR to China in 2000, which is illustrated in Figure 1. US exports to China also grew over this period, but less rapidly, with the result that by 2007 the United States' trade deficit with China exceeded $250 billion US dollars, or 1.7 percent of US GDP, up from 0.3 percent of GDP in 1990.

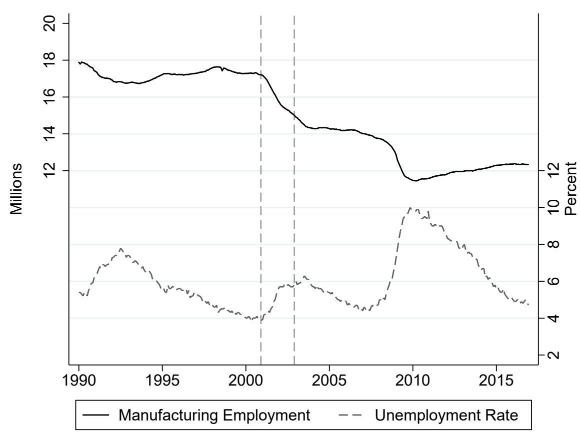
Figure 1: US Imports from China vs Rest of World



Source: US Census Bureau. Figure displays indexes of US imports from China and from the rest of the world from 1989 to 2012. The base year for the indexes is 2001.

The jump in imports from China after 2000 likely resonated with politicians and the public because it coincided with noticeable shifts in the labor market. In particular, the solid line in Figure 2 shows that as the pace of import growth from China stepped up, US manufacturing employment plunged, dropping 19 percent between passage of PNTR in October 2000 and March 2007. Pierce and Schott (2016) show that this decline was steeper in industries more exposed to PNTR, while Autor, Dorn, and Hanson (2013) show that commuting zones with industries facing higher import competition from China experienced greater declines in manufacturing employment. Though non-manufacturing employment increased robustly in some parts of the country (Fort, Pierce, and Schott (2018), Bloom, Handley, Kurmann, and Luck (2019)), there is evidence that the effects of import competition carried through to broader aspects of the labor market. Autor, Dorn, and Hanson (2013), for example, show that workers in regions experiencing higher levels of import competition exhibit greater uptake of social welfare programs such as disability, and Pierce and Schott (2020) show that counties more exposed to PNTR experience both relatively higher levels of unemployment and relatively lower levels of labor force participation during the 2000s.[4]

Figure 2: US Manufacturing Employment and Unemployment Rate



Source: US Bureau of Labor Statistics. Figure displays US manufacturing employment (left axis) and the overall unemployment rate (right axis) from 1990 to 2016. Vertical lines highlight the dates of the 2000 and 2002 elections.

As the US trade deficit with China expanded and concerns over the loss of manufacturing jobs grew, US legislators at various levels of government staked out positions on international trade, influenced by a range of factors. Often, views on trade were shaped by district characteristics, with some representatives from industrial districts more skeptical of trade than those in service-oriented districts. Representative Eva Clayton, for example, a Democrat representing eastern North Carolina, asked in the lead-up to a vote on PNTR for China,

[4] These trends are consistent with estimates of substantial adjustment costs for workers who switch industries or occupations, as shown in Artuc, Chaudhuri, and McLaren (2010), Ebenstein, Harrison, McMillan, and Phillips (2014), Acemoglu, Autor, Dorn, Hanson, and Price (2016), and Caliendo, Dvorkin, and Parro (2019).

"[m]ust eastern North Carolina lose in order for the Research Triangle to gain?"[5]

Party affiliation also was a key factor in how legislators voted on trade-related bills, with the views of the parties on trade changing over time (Irwin (2020)). In the 1990s, Democrats were split between the labor wing of the party that opposed the expansion of trade agreements, and the more pro-trade "New Democrats," exemplified by President Bill Clinton who presided over approval of NAFTA and the granting of PNTR to China (Kamarck and Podkul (2018); Rorty (1998)). In the 2000s, even as the House Democratic leadership joined Republicans in supporting new free trade agreements (FTAs), many rank-and-file Democratic representatives voted against expansion of FTAs (Palmer (2007)). After the Great Recession, with the rise of the "Tea Party," more Republicans in Congress joined Democrats in their opposition to trade agreements. And by 2016, Republican and Democratic candidates for President were not only opposing new trade agreements but calling for the reversal of existing agreements.[6] These changing views for both political parties play a key role in explaining the preferences of individual voters over this time period, as discussed in Section 5.

3 Data

This section describes the data used to measure exposure to import competition from China, voting in elections, and other variables that may affect voting behavior.

3.1 Measuring Exposure to PNTR

We make use of the structure of the US tariff schedule to define a measure of each industry's – and in turn, each county or district's – exposure to PNTR. The US tariff schedule has two basic sets of tariff rates: *NTR tariffs*, which average 4 percent across industries and are applied to goods imported from other members of the World Trade Organization (WTO); and *non-NTR tariffs*, which were set by the Smoot-Hawley Tariff Act of 1930 and are typically substantially higher than the corresponding NTR rates, averaging 34 percent across industries. While imports from non-market economies such as China are by default subject to the higher non-NTR rates, US tariff law allows the President to grant these countries access to NTR rates on an annually renewable basis, subject to approval by Congress.

US Presidents granted China such a waiver every year starting in 1980, but their annual approval by Congress became politically contentious and less certain following the Chinese government's crackdown on the Tiananmen Square protests in 1989. Re-approval remained controversial throughout the 1990s, especially during other flash points in US-China relations including China's transfer of missile technology to Pakistan in 1993 and the Taiwan Straits

[5] See http://history.house.gov/People/Detail/11065.

[6] Among Democrats, Hillary Clinton announced her opposition to the Trans Pacific Partnership (Steinhauer (2016)), while Bernie Sanders proposed "reversing trade policies like NAFTA, CAFTA and PNTR with China that have driven down wages and caused the loss of millions of jobs." The ultimate winner of the 2016 election, Republican Donald Trump, called for a 45 percent tariff on US imports from China (Haberman (2016)), and has followed up those calls with substantial tariff increases directed primarily at China.

Missile Crisis in 1996. Importantly, if annual renewal of the waiver had failed, US tariffs on imports from China would have risen substantially from the temporary NTR levels to the generally much higher non-NTR rates.

The possibility of tariff increases each year served as a disincentive throughout the 1990s for firms considering sinking investments associated with increasing US imports from China.[7] PNTR, which was passed by Congress in October 2000 and took effect upon China's entry to the WTO in December 2001, permanently locked in US tariffs on imports from China at the low NTR rates, eliminating these disincentives, a change that Handley and Limão (2017) estimate is equivalent to a 13 percent reduction in import tariffs.[8] As documented in Pierce and Schott (2016), the industries and products most affected by the policy change experienced larger declines in US manufacturing employment, as well as larger increases in imports from China – including related-party imports – and larger increases in exports to the United States by foreign-owned firms in China.[9]

We compute counties' exposure to PNTR in two steps. The first step is to calculate exposure for US industries. We follow Pierce and Schott (2016) in defining the industry-level impact of PNTR as the increase in US tariffs on Chinese goods that would have occurred in the event of a failed annual renewal of China's NTR status prior to PNTR,

$$NTR\,Gap_j = Non\;NTR\,Rate_j - NTR\,Rate_j. \tag{1}$$

We refer to this difference as the NTR gap, and compute it for each four-digit SIC industry j using *ad valorem equivalent* tariff rates provided by Feenstra, Romalis, and Schott (2002) for 1999, the year before passage of PNTR, and the concordance between Harmonized System and SIC codes from Pierce and Schott (2012). As illustrated in Figure 3, NTR gaps vary widely across industries, with a mean and standard deviation of 30 and 18 percentage points, respectively. As noted in Pierce and Schott (2016), 79 percent of the variation in the NTR gap across industries is attributable to non-NTR rates, set 70 years prior to passage of PNTR.[10] This feature of non-NTR rates effectively rules out reverse causality that would arise if *non-NTR rates* were set to protect industries with declining employment or surging imports. Furthermore, to the extent that *NTR rates* were raised to protect industries with certain characteristics prior to PNTR, these *higher* NTR rates would result in *lower* NTR gaps, biasing our results away from finding an effect of PNTR.

We compute US counties' exposure to PNTR as the employment-share-weighted average NTR gap of the industries active within their borders,

[7] Intuition for these incentives can be derived, in part, from the literature on investment under uncertainty (e.g., Pindyck (1993); Bloom, Bond, and Van Reenen (2007)), which demonstrates that firms are more likely to undertake irreversible investments as uncertainty surrounding their expected profit decreases. Handley (2014) introduces these insights to firms' decisions to export, and Handley and Limão (2017) examine the impact of the reduction of trade policy uncertainty associated with PNTR on trade and welfare.

[8] The passage of PNTR followed the bilateral agreement in 1999 between the US and China regarding China's eventual entry into the WTO.

[9] Heise, Pierce, Schaur, and Schott (2015) describe the effect of PNTR on the structure of supply chains, and Feng, Li, and Swenson (2016) discuss the effect of PNTR on entry and exit patterns of Chinese exporters, as well as changes in export product characteristics.

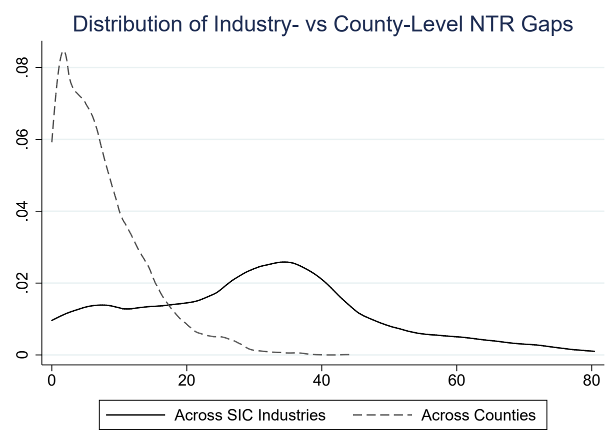
[10] Cross-industry variation in the NTR rate explains less than 1 percent of variation in the NTR gap.

$$NTR\,Gap_c = \sum\nolimits_j \left(\frac{L_{jc}}{L_c} NTR\,Gap_j \right), \tag{2}$$

where L_{jc} is the employment of SIC industry j in county c and L_c is the overall employment in county c, defined as of 1990 to mitigate any potential relationship between counties' industrial structure and the year 2000 change in US trade policy. County-industry-year employment data are from the US Census Bureau's County Business Patterns (CBP).[11] Congressional district-level NTR gaps are calculated analogously.

NTR gaps can only be calculated for products subject to import tariffs, such as manufacturing, agriculture and mining products. NTR gaps for services, which are not subject to import tariffs are, by definition, zero. Given that services comprise a large share of employment, the distribution of the *county*-level $NTR\,Gap_c$ is shifted leftwards relative to the distribution of manufacturing and other *industries* for which the $NTR\,Gap_j$ is defined: the mean and standard deviation of the county-level NTR gap are 6.1 and 4.2 percentage points, as displayed visually in Figure 3. The difference between the 25th and 75th percentiles is 4.0 (=7.5-3.5) percentage points. Importantly, because our analysis below controls for counties' initial share of employment in manufacturing, the county-level NTR gap represents an area's exposure to PNTR's trade liberalization holding constant the extent to which it is intensively engaged in manufacturing activities.

Figure 3: The NTR Gap Across Industries and Counties



Source: Feenstra, Romalis, and Schott (2002) and authors' calculations.

3.2 Election Data

Data on county-level voting are from *Dave Leip's Atlas of US Presidential Elections*, which tracks votes for elections for the House of Representatives and Senate, in addition to data

[11] We follow the procedure outlined in Autor, Dorn, and Hanson (2013) to impute suppressed employment values at the industry-county-level.

on Presidential elections.[12] These data track the number of votes received by candidates for each of these offices in each county, in each election year, as well as the number of registered voters and voter turnout.[13] The population-weighted average Democrat vote share in 2000 – the election closest to the granting of PNTR to China – is 49 percent, with a standard deviation of 22 percentage points.

3.3 Socio-economic Characteristics

Our regression analysis includes controls for socio-economic characteristics that might affect voting behavior and could potentially be correlated with exposure to PNTR. The first of these controls is the share of a county's employment in manufacturing in 1990, to account for the possibility that counties of differing manufacturing intensities may be on different trajectories in terms of voting behavior that are unrelated to their exposure to import competition via PNTR. The manufacturing employment share is calculated using data from the Census Bureau's County Business Patterns for 1990. We also control for additional demographic variables that have been found to be important correlates of voting behavior in the political science and economics literatures on voting.[14] These controls include median household income, and the percentages of a county's population that have a bachelor's degree, have a graduate degree, are non-white, are aged 65 or over, or are veterans, all defined as of 1990 in the Census Bureau's decennial Census.[15] County-level summary statistics for these controls are reported in Table 1.[16]

3.4 Additional Controls for Exposure to Import Competition

We include controls for other changes in US trade policy that occurred during the period of analysis and which also may have affected voting in elections. First, we include time-varying controls for counties' average NTR rate (Feenstra, Romalis, and Schott (2002)) and their exposure to the phasing out of textile and clothing quotas under the global Multi-Fiber Arrangement (Khandelwal, Schott, and Wei (2013)), each of which are calculated based on the employment-share weighted average of their exposure to these policy changes, as in equation 2.

[12] These data are available for purchase from www.uselectionatlas.org.

[13] County boundaries are substantially more stable than those of Congressional districts, whose borders are re-drawn after each decennial census. We incorporate county code changes during our sample period using the set of "Substantial Changes to Counties and County Equivalent Entities" recorded by the Census Bureau and available online at https://www.census.gov/programs-surveys/geography/technical-documentation/county-changes.html.

[14] See, for example, Baldwin and Magee (2000), Conconi, Facchini, and Zanardi (2012), Gilbert and Oladi (2012), Kriner and Reeves (2012), and Wright (2012).

[15] Scheve and Slaughter (2001) show that individuals' trade policy preferences are affected by skill level and homeownership status, and Conconi, Facchini, Steinhardt, and Zanardi (2020) examine the role of skilled labor abundance in representatives' votes on trade and immigration bills.

[16] We exclude Hawaii from analysis in this paper because county-level population data for years prior to 2000 are unavailable. The results discussed below are qualitatively identical when also excluding Alaska, i.e., focusing solely on the continental United States.

Table 1: County Attributes in 1990

Attribute	**Mean**	**SD**	**Min**	**Max**
Median Income ($000)	31.24	8.59	11.21	77.35
Bachelor (%)	9.02	4.21	0.00	40.30
Graduate (%)	4.48	2.73	0.30	29.70
Non-White (%)	12.70	15.65	0.00	94.90
65+ (%)	14.88	4.42	1.40	34.00
Veteran (%)	14.78	2.77	4.20	29.00
Manufacturing (%)	24.12	16.39	0.00	91.02
NAFTA Exposure	-0.32	0.57	-4.84	0.28
MFA Exposure	1.08	2.51	0.00	21.29
NTR Tariff Rate (%)	0.90	1.11	0.00	7.99

Source: US Census Bureau and authors' calculations. Table displays summary statistics of county attributes as of the 1990 decennial Census for the 3121 counties in our sample, weighted by population. Median household income is in thousands of dollars. Bachelor through Veteran refer to the percent of county populaton with noted attribute. Manufacturing refers to the manufacturing share of county employment. NAFTA, MFA, and NTR Tariff Rate refer to county-level exposure to those trade policies as defined in text.

We compute counties' exposure to the MFA phase-outs following Brambilla, Khandelwal, and Schott (2010) and Pierce and Schott (2020). We measure the extent to which industry quotas were binding under the MFA as the average fill rate of the textile and clothing products that were under quota in that industry, where fill rates are defined as the actual imports divided by allowable imports under the quota. Industries with higher average fill rates faced more binding quotas and are therefore more exposed to the end of the MFA. Products not covered by the MFA have a fill rate of zero.

Finally, we control for counties' exposure to US tariff reductions associated with NAFTA, measured as the change in tariff rates on US imports from Mexico from 1994 to 2000. Industry-level measures of NAFTA tariff changes from Hakobyan and McLaren (2016) are aggregated to the county-level following equation 2 and then interacted with a post-PNTR indicator. Intuitively, counties' exposure to both PNTR and NAFTA rises with their share of employment in manufacturing, with correlation coefficients of 0.88 and -0.55, respectively. The correlation between the two exposures themselves is -0.69, indicating that industries with higher NTR gaps were subject to greater tariff reductions under NAFTA.

4 Exposure to PNTR and Voting

This section explores the link between exposure to the US granting of PNTR to China and voting in US elections. We begin by examining voting for the House of Representatives over the period from 1992 to 2016, and highlight distinct changes in voting patterns over time. Next, we expand the analysis to other offices – the US Senate and President – as well to voter turnout. We conclude with a comparison of county- and district-level results and find substantial benefits associated with the use of county-level data, through the ability to consider an extended time period that is not possible with district-level data.

4.1 Baseline Empirical Strategy

We use a highly flexible generalized difference-in-differences approach to estimate the relationship between exposure to trade liberalization and election voting, which allows this relationship to vary from election year to election year. In particular, we estimate the following equation:

$$\begin{aligned} Dem\,Share_{ct} \;=\; & \sum_t \theta_t \, 1\{year = t\} \times NTR\,Gap_c + \\ & \sum_t \gamma_t \, 1\{year = t\} \times \mathbf{X}_c + \beta \mathbf{X}_{ct} + \\ & \delta_c + \delta_t + \varepsilon_{ct}. \end{aligned} \tag{3}$$

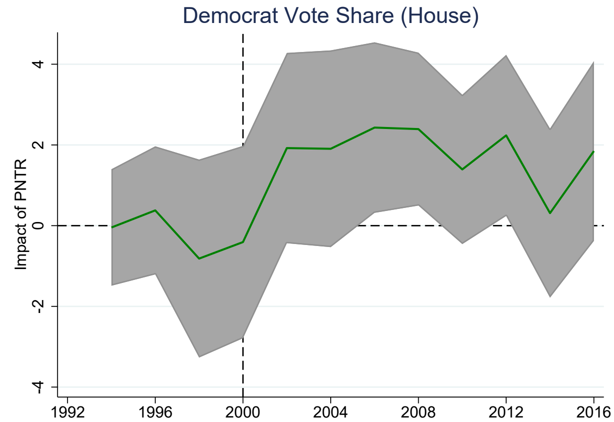
The dependent variable, $Dem\,Share_{ct}$, is the share of votes cast for the Democrat in county c in House of Representatives elections in year t. The first set of terms on the right hand side of equation 3 are interactions of the county-level NTR Gap with indicators for election years 1994 to 2016. This generalization allows us to determine – via coefficient estimates θ_t – the specific years in which any relationship between $Dem\,Share_{ct}$ and the NTR Gap is present, and any changes in that relationship over time, relative to the left-out year 1992. $\mathbf{X}_c$ represents the set of time-invariant demographic and policy control variables described in Section 3. These variables are also interacted with the full set of year dummies, mirroring the manner in which exposure to PNTR enters the estimation equation. The next set of terms, $\mathbf{X}_{ct}$, consists of control variables that vary at the county-year-level, namely the county's exposure to standard NTR tariffs and the phasing out of the MFA. $\boldsymbol{\delta_c}$ and $\boldsymbol{\delta_t}$ represent county and year fixed effects, which capture time-invariant county-level characteristics and aggregate shocks that affect all counties identically in a particular year. Because county population sizes vary substantially, we weight by initial (1992) population. Standard errors are clustered at the state-level, an approach that allows for correlation of errors within states, and which therefore yields conservative estimates of statistical significance.

We summarize the results of estimating Equation 3 in Figure 4. This figure displays the relationship between PNTR and counties' votes for Democratic House candidates in terms of economic significance, i.e., the estimated impact of moving a county from the 25th to the 75th percentile of the NTR Gap distribution. That is, for each year except the omitted year 1992, we multiply the coefficient estimation for the DID term of interest for that year by the weighted interquartile range of the NTR Gap across counties. Shading represents the 90 percent confidence interval for this estimate of economic significance, which is also calculated by multiplying the upper and lower bounds of the confidence interval by the interquartile range of the NTR gap.

Figure 4 highlights three distinct phases of voting. In the first phase, which lasts from 1992 to 2000, we find no relationship between exposure to the trade liberalization and the share of votes cast for Democrats, with the confidence intervals centered around zero. Following the passage of PNTR in 2000, coefficient estimates shift up noticeably, indicating the start of a second phase in the relationship between the trade liberalization and voting. In this phase, the implied impact is positive and statistically significant, implying that counties more exposed to PNTR exhibit relative increases in the share of votes cast for Democrats. After 2008, this disproportionate support for Democrats in trade-exposed counties wanes, beginning the third phase of voting. Following a brief rebound in 2012, coefficient estimates step down again and lose statistical significance, indicating that trade-exposed counties are once again voting similarly to less exposed counties in Congressional elections.

As will be discussed in more detail in Section 5, we find that these changes in the relationship between trade exposure and voting are consistent with the evolution of the two parties' positions on trade. In the early 2000s, when areas more exposed to PNTR exhibit relative increases in the Democrat vote share, Democratic House members were substantially more likely to vote to restrict trade than their Republican counterparts. The decreased support for Democrats between 2008 and 2010, by contrast, coincides with the rise of the Tea Party wing of the Republican Party, whose members were more opposed to trade agreements than the overall population, and more likely both to view China as an adversary and to place a high priority on getting tougher on China with respect to trade than either Democrats

Figure 4: Democrat Vote Share for US House of Representatives



Source: US Census Bureau, Dave Leip's Atlas of US Presidential Elections, and authors' calculations. Figure displays the implied impact of PNTR on the Democrat vote share implied by estimation of the county-year-level OLS difference-in-differences (DID) specification described in equation 3. For each year, the implied impact is the product of the DID term of interest for that year and the weighted inter-quartile range of counties' exposure to PNTR. Shading represents the 90 percent confidence interval for this implied impact. Regressions are weighted by initial (1992) population and standard errors are adjusted for clustering at the state level.

or non-Tea Party Republicans.[17] This trend has continued with the election of President Donald Trump, who has adopted several high-profile rounds of tariff increases, particularly against China, and in Section 5, we show that, in the 2010s, Democratic and Republican House members vote similarly on trade-related bills.

4.2 Quantifying the Relationship Between PNTR and Voting

In this section, we estimate an alternative, difference-in-differences specification that yields parsimonious estimates of different phases in the relationship between trade liberalization and voting. This specification allows us to characterize the economic significance of the relationships between voting and both exposure to PNTR and the control variables in a straightforward manner. In particular, this specification estimates how counties with different levels of exposure to PNTR's trade liberalization vote differently in Congressional elections from 2002 to 2008 and 2010 to 2016 relative to the pre-period, 1992 to 2000:

[17] Newmyer and Liberto (2010) report that 61 percent of the Tea Party's grassroots members were hostile to trade agreements, versus 53 percent for all respondents. A Pew Research Center poll described in Rosentiel (2011) notes 60 percent of Tea Party Republicans said it was very important to "get tougher on econ/trade issues" versus 49 percent for non-Tea Party Republicans and 52 percent of Democrats.

$$
\begin{aligned}
Dem\,Share_{ct} \;=\;& \theta 1\{2000s\}_t \times NTR\,Gap_c \\
&+\lambda 1\{2010s\}_t \times NTR\,Gap_c \\
&+1\{2000s\}_t \times \mathbf{X}_c'\boldsymbol{\gamma} \\
&+1\{2010s\}_t \times \mathbf{X}_c'\mu \\
&+\mathbf{X}_{ct}'\boldsymbol{\beta} + \boldsymbol{\delta_c} + \boldsymbol{\delta_t} + \alpha + \varepsilon_{ct}.
\end{aligned} \tag{4}
$$

The dependent variable is identical to that used in equation 3, the share of votes cast for Democratic candidates for the US House of Representatives in county c in year t. The first two terms on the right-hand side of equation 4 are the DID terms of interest, interactions of the county-level NTR gap with indicators that take the value 1 for election years in voting in the early 2000s (2002 to 2008, or $1\{2000s\}_t$) and the 2010s (2010 to 2016, $1\{2010s\}_t$). As indicated in the next two lines on the right-hand side of equation 4, time-invariant county characteristics are also interacted with indicators for these two time periods. Following equation 3, we again include time-varying county characteristics and county and year fixed effects, weight by 1992 population, and cluster standard errors at the state level.

Results for House elections are reported in Table 2. As indicated in the first column, and in line with the estimates reported above, we find a positive coefficient estimate on the DID term for the years 2002 to 2008 that is statistically significant at the one percent level, indicating that counties more exposed to PNTR's trade liberalization exhibit relative increases in the share of votes cast for Democrats in Congressional elections during those years. Its magnitude implies that moving a county from the 25th to 75th percentile of the NTR gap (from 3.5 to 7.5 percent) is associated with a 2.4 percentage point increase in the share of votes cast for the Democratic candidate, or 4.8 percent of the 49 percent average Democratic vote share in the 2000 US House elections.[18] As indicated in the second row of the table, this boost to Democrats disappears in the next decade, from 2010 to 2016, following the rise of the Tea Party and the increasing hostility toward trade by Republican candidates.

With respect to the control variables, we find that only median household income is statistically significant in both post-PNTR periods. Coefficient estimates suggest that an interquartile increase in this attribute is associated with 2.6 and 5.4 percentage point relative increases in the share of votes cast for Democrats in the 2000s and 2010s versus the 1990s, respectively.

The statistical significance of other covariates is more varied. We find that votes for Democrats are relatively higher in the 2000s, but not the 2010s, among counties with higher initial shares of graduate degrees and persons over age 65. By contrast, we find that votes for Democrats are relatively higher in the 2010s, but not the 2000s for counties with greater initial shares of bachelor degrees and people who are not white. Interquartile increases in the latter two attributes are associated with relatively higher Democrat vote shares of 3.8 and 4.0 percentage points, respectively. For other policy controls, exposure to the expiration of the MFA is statistically significant and of the opposite sign of PNTR, even though

[18] In these calculations, the interquartile ranges and means are weighted by 1992 county population.

Table 2: PNTR and County-Level Voting for Democrats

Variables	$House_{ct}$	$Senate_{ct}$	$President_{ct}$	$Turnout_{ct}$
1{2000s} x NTR Gap_c	0.592***	0.416*	0.01	0.046
	0.21	0.223	0.093	0.093
1{2010s} x NTR Gap_c	0.407	0.362	0.1	-0.093
	0.264	0.283	0.123	0.136
1{2000s} x Median HHI_c^{1990}	0.205***	0.233*	0.075	-0.091
	0.058	0.117	0.048	0.055
1{2010s} x Median HHI_c^{1990}	0.426***	0.251**	0.158***	-0.037
	0.088	0.109	0.056	0.084
1{2000s} x $\%Bachelors_c^{1990}$	0.089	0.014	0.627***	0.475***
	0.171	0.398	0.094	0.111
1{2010s} x $\%Bachelors_c^{1990}$	0.522**	-0.014	1.045***	0.475**
	0.229	0.447	0.12	0.219
1{2000s} x $\%Graduate_c^{1990}$	0.443***	-0.128	-0.06	-0.256**
	0.163	0.387	0.111	0.124
1{2010s} x $\%Graduate_c^{1990}$	0.251	0.431	-0.331**	-0.441*
	0.234	0.404	0.138	0.227
1{2000s} x $\%Non\text{-}White_c^{1990}$	0.073	0.027	0.092***	0.124***
	0.051	0.04	0.016	0.039
1{2010s} x $\%Non\text{-}White_c^{1990}$	0.189***	0.171***	0.264***	0.090**
	0.052	0.049	0.026	0.038
1{2000s} x $\%Over\text{-}65_c^{1990}$	0.255**	0.450**	0.052	-0.245***
	0.118	0.179	0.072	0.09
1{2010s} x $\%Over\text{-}65_c^{1990}$	0.318	-0.168	-0.1	0.046
	0.219	0.244	0.11	0.128
1{2000s} x $\%Veteran_c^{1990}$	-0.136	-0.679**	0.257**	0.521***
	0.292	0.311	0.097	0.137
1{2010s} x $\%Veteran_c^{1990}$	0.028	-0.113	0.301**	0.249
	0.313	0.392	0.139	0.205
1{2000s} x $\%Manufacturing_c^{1990}$	-0.116*	-0.142	0.047	0.036
	0.067	0.097	0.04	0.038
1{2010s} x $\%Manufacturing_c^{1990}$	-0.123	-0.069	-0.007	0.084*
	0.096	0.128	0.057	0.046
1{2000s} x $\%NAFTA_c^{1990}$	1.863	4.592***	1.139*	-0.922
	1.233	1.497	0.572	0.73
1{2010s} x $\%NAFTA_c^{1990}$	1.514	4.434**	-0.016	-1.722**
	1.541	1.839	0.661	0.816
MFA $Exposure_{ct}$	-0.521**	-0.59	-0.827***	0.117
	0.259	0.36	0.196	0.145
NTR_{ct}	1.579	0.133	0.038	1.011
	1.283	1.254	0.814	0.802
Observations	40027	27008	21691	20216
R-squared	0.735	0.72	0.94	0.797
Estimation	OLS	OLS	OLS	OLS
Period	1992(2)2016	1992(2)2016	1992(2)2016	1992(2)2016
FE	c,t	c,t	c,t	c,t
Weighting	1992 Pop	1992 Pop	1992 Pop	1992 Pop
Clustering	State	State	State	State
Implied Impact of PNTR (2000s)	1.624	1.445	0.399	-0.373
Standard Error	1.055	1.131	0.49	0.543
Avg. Value of Dependent Variable (2000)	49	49	49	66
Implied Impact	2.4	1.7	0.04	0.18

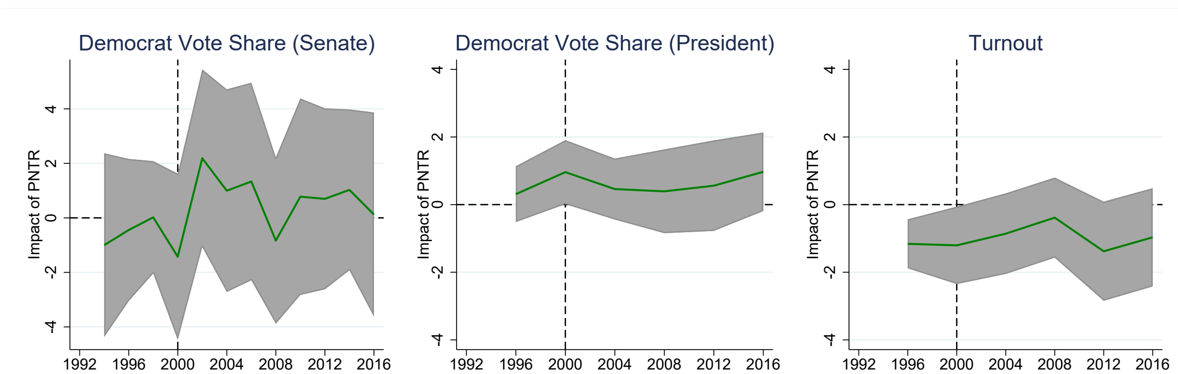
Source: US Census Bureau, Dave Leip's Atlas of US Presidential Elections, and authors' calculations. Table reports difference-in-differences (DID) OLS regression result for the Democrat vote shares of the noted elections and turnout in county c in year t from 1992 to 2016, based on equation 4. The first two covariates are the DID terms of interest, which interact dummies for years 2002 to 2008 and 2010 to 2016 with the county-level NTR gap. The next sixteen covariates interact these dummies with 1990 county attributes. Remaining covariates account for counties' average import tariff and exposure to the MFA in each year. The implied impact of PNTR in the 2000s is the product of the first DID term of interest and the weighted inter-quartile range the NTR Gap. Standard errors adjusted for clustering at the state level are reported below coefficients. *, ** and *** signify statistical significance at the 10, 5 and 1 percent levels.

Pierce and Schott (2016) find that both PNTR and the MFA are associated with declines in manufacturing employment. We note, however, that the negative relationship between voting for Democrats and exposure to the MFA is sensitive to the time period considered–it is positive for a sample ending in 2008–and also small in terms of economic significance, with an interquartile shift associated with a relative change in voting for Democrats that is approximately one twentieth of the size associated with PNTR.[19]

4.3 The Senate and the Presidency

In this section, we examine the relationship between PNTR and county-level Democrat vote shares for two other offices, the US Senate and President. To do so, we re-estimate equations 3 and 4 with the dependent variable being the share of votes cast for Democrats in one of these two types of elections. In contrast to the House elections, which take place every two years, for Presidential elections, observations are defined only for years in which a Presidential election took place, i.e. 1992, 1996, etc. Senate elections occur every six years, with approximately one third of Senators up for election in any given election year. As a result, for the Senate regressions, observations for each county only appear in years in which their states held Senate elections.

Figure 5: Democrat Vote Share for Other Offices



Source: US Census Bureau, Dave Leip's Atlas of US Presidential Elections, and authors' calculations. Figure displays the implied impact of PNTR on the Democrat vote shares for the Senate and Presidency, and turnout, implied by estimation of the county-year-level OLS difference-in-differences (DID) specification shown in equation 3. For each year, the implied impact is the product of the DID term of interest for that year and the weighted inter-quartile range of counties' exposure to PNTR. Shading represents the 90 percent confidence interval for this implied impact. Regressions are weighted by initial year (1992) population and standard errors are adjusted for clustering at the state level.

Results for Equation 3 are summarized visually in Figure 5, while coefficient estimates for Equation 4 are reported in Table 2. In the figure, each panel displays the implied impact of PNTR in terms of moving a county from the 25th to the 75th percentile of the NTR Gap distribution across counties, analogous to Figure 4.

[19]The relationship likely arises because the counties most exposed to the MFA are disproportionately located in southeastern states–especially Alabama and Georgia–that tended to vote Republican during the period we examine.

As indicated in the left panel of Figure 5, results for Senate elections are much less precisely estimated than for House elections, though there is a step up in the implied impact of PNTR in terms of votes for Democrats from 2000 to 2002. As indicated in the second column of Table 2, this relationship across the entire 2002 to 2008 period, while marginally statistically significant, is smaller in magnitude than the relationship for House elections, with an interquartile shift in exposure to PNTR associated with a relative increase in the Democrat vote share of 1.7 percentage points, or 3.4 percent of the average share of votes won by Democratic candidates for Senate across counties in the year 2000 (49 percent). Results in the middle panel of Figure 5, and the third column of Table 2, reveal no statistically significant relationship between exposure to PNTR and the share of votes cast for the Democratic candidate for President.

In sum, the relationship between exposure to PNTR's trade liberalization and the share of votes cast for Democrats seems to be present primarily in elections for the US House of Representatives. This closer relationship for the House may be the result of the frequency of House elections, which makes Representatives less likely to temporarily adopt positions at odds with the preferences of the median voter of their districts. Conconi, Facchini, and Zanardi (2014), for example, find that Senators are more likely than Representatives to support trade liberalization in the first four years of their term, but that they vote similarly to Representatives in the final two years of their terms when they face imminent elections. Relatedly, Karol (2012) has shown that Senators and Presidents are more likely than House representatives to support policies (like free trade) that are in the long-run interests of the country as a whole versus the interests of individual districts. Finally, given that the negative impact of trade liberalization on manufacturing employment can be geographically concentrated (Autor, Dorn, and Hanson (2013)), any effects might be most likely to be apparent in House elections, which cover the smallest geographic area of the offices considered.

4.4 Exposure to PNTR and Voter Turnout

There is a large literature examining the effects of economic conditions on voter turnout, and changes in voting patterns associated with PNTR may be driven, in part, by changes in turnout. Charles and Stephens Jr (2013) find that higher local-area wages and employment decrease turnout in elections for the US House of Representatives and other offices. In addition, a long literature in political science argues that, under certain conditions, economic adversity can increase voter turnout (e.g. Schlozman and Verba (1979)). To examine whether the imposition of PNTR is associated with changes in voter turnout, we re-estimate equations 3 and 4, using county-year-level voter turnout as the dependent variable, with turnout defined as the number of people voting in the election divided by the number of registered voters.[20]

As reported in the right column of of Figure 5, and the final column of Table 2, we find no relationship between exposure to PNTR and voter turnout. This lack of a relationship

[20] We limit the sample for regressions examining voter turnout to years with Presidential elections as turnout data are available only in Presidential election years prior to 2000. For the 57 county-year observations – an average of 11 per election year – in which turnout exceeds 100 percent, we censor turnout to 100 percent, but note that the results are qualitatively identical when these observations are excluded.

is consistent with Dippel, Gold, and Heblich (2015), who find no effect of increased trade competition on turnout in German elections. Furthermore, it suggests that the shift toward Democratic candidates in more-exposed counties is not the result of changes in the number or composition of voters relative to the pre-PNTR period.

4.5 County-Level Versus District-Level Data

Because our baseline analysis is conducted at the county-level while Congressional elections are determined at the Congressional district-level, our results do not *directly* indicate how election outcomes would have *changed* in the absence of PNTR.[21] Rather, our results should be interpreted as providing information on how trade liberalization is associated with changes in voters' preferences for candidates of each party over time. This information is important, because it is indicative of *the possibility* of changes in election outcomes, and, perhaps more importantly, of shifts in voters' preferences that can lead to changes in the policy choices of representatives (Feigenbaum and Hall (2015)). For example, even in the absence of a change in election outcomes, representatives' policy decisions may differ when their districts' voters are more evenly split between Democrats and Republicans, versus when voters predominantly support one party or the other.

We use county-level data in our baseline results because district-level data have a major weakness that is especially problematic in our setting. That is, because Congressional districts are redrawn following each decennial Census, absent strong assumptions, they can only be used to study ten-year intervals (e.g. 1992 to 2000, 2002 to 2010), but are unable to span Congressional redistricting periods. Figure 2 demonstrates why this weakness is a concern for our research question. As shown within the vertical lines marking elections, two-thirds of the steep decline in manufacturing employment that occurs between 2000 and the Great Recession occurs between the November 2000 and November 2002 elections. As a result, using 2002 as a starting point – as is necessary in a district-level analysis – could yield misleading results, as the 2002 election already reflects the effects of passage of PNTR in October 2000 and the subsequent decline in employment. Indeed, as shown in section 4.1, the largest step up in the Democrat vote share associated with exposure to PNTR occurs between 2000 and 2002. Moreover, news reports from the time underscore that the 2002 Congressional elections were influenced by reactions to PNTR with China, and associated employment losses, including for pro-trade incumbent Tom Sawyer (D-OH), who was defeated in a primary (Nichols (2002)):

> "Most, though not all, Republicans back the free-trade agenda pushed by major multinational corporations and Republican and Democratic presidents. Most Democrats oppose that agenda. Since the early 1990s, trade votes in the House of Representatives have tended to be close, however. That has meant that the margin of victory for the corporate trade agenda has often been delivered by a floating pool of Democrats — including Sawyer — who have been willing to vote with

[21] Relatedly, counties may be in different Congressional districts in different years due to the redistricting process.

> free-trade Republicans on key issues such as NAFTA, the General Agreement on Tariffs and Trade and normalization of trade relations with China...Patrick Woodall, research director for Public Citizen's Global Trade Watch, says Sawyer's defeat must be read as very bad news for those free-trade Democrats...'[W]hen you get outside Washington, you start running into Americans who have seen factories closed and communities kicked in the teeth by the North American Free Trade Agreement and all these other trade bills...Tom Sawyer's defeat ought to be a wake-up call for Democrats who think they can get away with voting for a free-trade agenda that does not protect workers, farmers and the environment. Tom Sawyer found out on Tuesday that there are consequences."

Moreover, with a county-level analysis, both the dependent variable – voting –and key independent variables – exposure to PNTR and demographic variables – are defined at the same level of aggregation. For a district-level analysis, exposure to trade liberalization must be calculated as a weighted average of the exposure of counties in the district.[22] When a county is split across multiple districts, however, the County Business Patterns does not provide information on the industrial mix of the portions of the county that fall within each district, so the overall exposure of the county must be used. This mismatch creates measurement error, which may be correlated with voting if the drawing of district boundaries is affected by the desire to include or exclude particular industries or firms within a district's boundaries.[23]

Lastly, we find that using county-level data yields qualitatively identical results to using district-level data when estimates are generated based on an identical sample period. We explore the impact of using different levels of aggregation in Section B of the appendix. As reported in Table A.3, we find that, for the sample period 2002 to 2010 – the longest post-PNTR period possible using district-level data – the relationship between the NTR gap and the Democrat vote share is not statistically significantly different from zero when using either county- or district-level data, potentially due to the 2002 election already being affected by import competition from China and the associated employment decline, as mentioned above. These results provide evidence that any differences arising from the use of county-level data versus district-level data arise primarily from the ability of county-level data to encompass a substantially longer and more informative sample period, rather than from the different levels of aggregation.

[22] The County Business Patterns did not publish district-level data until 2013.

[23] County-level data have two additional benefits over district-level data. First, because counties are typically smaller than districts, they capture greater variation in voting, exposure to PNTR, and demographic characteristics than is possible for most Congressional districts. Second, as smaller geographic units where control over taxation and spending reside, counties may be more likely to capture variation in economic outcomes. Feler and Senses (2017), for example, find that provision of government services in response to import competition varies substantially across counties as declining property values depress property tax revenues, and Dix-Carneiro, Soares, and Ulyssea (2018) and Che, Xu, and Zhang (2018) find that reductions in local government expenditures are associated with relative increases in crime, a further channel through which county-level exposure to trade liberalization may affect voting.

5 Party Affiliation and Legislator Voting Behavior

The previous section establishes that voters in counties facing larger increases in import competition from China experience relative increases in their likelihood of voting for Democratic candidates in the early 2000s, relative to the 1990s and 2010s. One explanation for this change in voting patterns is that residents of these counties shifted their votes over time, seeking to elect candidates from the party that they believed would protect local industries by pursuing legislative positions that restrict international trade. This section investigates this potential explanation by examining differences in the voting of Democrats and Republicans on bills related to international trade using a regression discontinuity approach, in order to determine which party was more likely to favor trade protection, and in what time period.

5.1 Classification of International Trade Bills

Our first steps are to identify the set of trade-related bills appearing in the US House of Representatives over the sample period, classify them as "pro-" versus "anti-trade," and collect legislators' votes for each bill. To identify the set of trade-related bills, we use subject area classifications developed by Comparative Agendas, which collects data on all roll call votes in the US Congress, and classifies them into sub-categories. We include bills under major topic 18, "Foreign Trade," and more specifically those covered by sub-topics 1802, "Trade Agreements," and 1807, "Tariff & Imports."[24] We focus on votes for final passage of a bill, excluding procedural votes. We also exclude bills that do not deal with trade restrictions directly, such as broad appropriations bills.[25]Appendix Table A.1 provides a list of all bills used in the analysis.

We classify bills as pro- versus anti-trade according to whether they remove or install trade barriers, respectively. To determine the classification of each bill, two authors and three research assistants read the text of each bill and gave it one of four preliminary rankings: clearly pro-trade, marginally pro-trade, marginally anti-trade, and clearly anti-trade. The final ranking – reported in Appendix Table A.1 – is the mode of the preliminary rankings. Given rankings' subjectivity, our baseline results focus on bills classified as clearly pro- or anti-trade, though, as reported in Appendix Section A, results are similar when all bills are included.[26] Lastly, House members' votes in Congresses seated following Congressional

[24]Information on Comparative Agenda's classifications is available at https://www.comparativeagendas.net/pages/master-codebook. A key feature of its classification system is that it covers our entire sample period, extending through 2018.

[25]This restriction excludes eight bills, HR2670 (106th Congress), HR3008 (107th Congress), HR2682 (109th Congress), HR4944 (109th Congress), S203 (109th Congress), HR3074 (110th Congress), HR2638 (110th Congress), and HR4380 (111th Congress).

[26]For example, in the 109th Congress, HJRES 27, "Withdrawing approval of the United States from the agreement establishing the World Trade Organization" is ranked as being clearly anti-trade, while HRES57, "Urging the European Union to maintain its arms embargo on the People's Republic of China" is ranked as marginally anti-trade. We note that we obtain qualitatively similar results from 1992 to 2010 if bills are classified according to the economic liberalness of their sponsor, as defined by the National Journal (Che,

elections from 1992 to 2014 are obtained from Govtrack.

5.2 Identification Strategy

We examine the relationship between House members' votes on international trade bills and their party affiliation using the following specification,

$$Pro - Trade_{dh} = \alpha + \beta Democrat_{dh} + \varepsilon_{dh}, \tag{5}$$

where d and h denote Congressional districts and the particular two-year Congress during which representatives serve.[27] The dependent variable $Pro - Trade_{dh}$ represents the share of pro-trade votes cast by a particular representative during a particular Congress. The dummy variable $Democrat_{dh}$ takes the value 1 if the representative is a Democrat and zero otherwise, and ε_{dh} is the error term. As above, we consider three sub-periods corresponding to the 1990s, the 2000s and the 2010s. Here, however, we estimate separate regressions for each sub-period at the district level as our interest is on the average policy choices of legislators in the two main parties, rather than changes in individual legislators' positions over time.

Identification of β requires that representatives' party affiliation be uncorrelated with the error term. As there may be several reasons why this assumption is violated, we follow Lee (2008) in identifying the causal effect of party affiliation on voting behavior using a regression discontinuity (RD) design that compares the legislative voting of Democrats and Republicans elected in close elections.[28] The intuition behind this design relates to the incomplete manipulability of elections. For example, exogenous variation in factors such as weather influences turnout and therefore the ultimate share of votes each candidates receives in a given election. If, in close elections, the outcomes are driven solely by this variation, comparison of the voting records of Democrats versus Republicans where vote shares are near 50 percent is tantamount to a natural experiment. In other words, other than the "treatment" of just winning, all else is assumed to be the same.[29]

Formally, define the assignment variable

Lu, Pierce, Schott, and Tao (2016)).

[27]For example, $h = 110$ represents the 110th Congress, which met from January 3, 2007 to January 3, 2009.

[28]Lee, Moretti, and Butler (2004) and Lee (2008) use RD to investigate the effect of party affiliation on legislators' right-vs-left voting scores.

[29]Using RD to investigate the incumbent advantage, Lee (2008) argues:

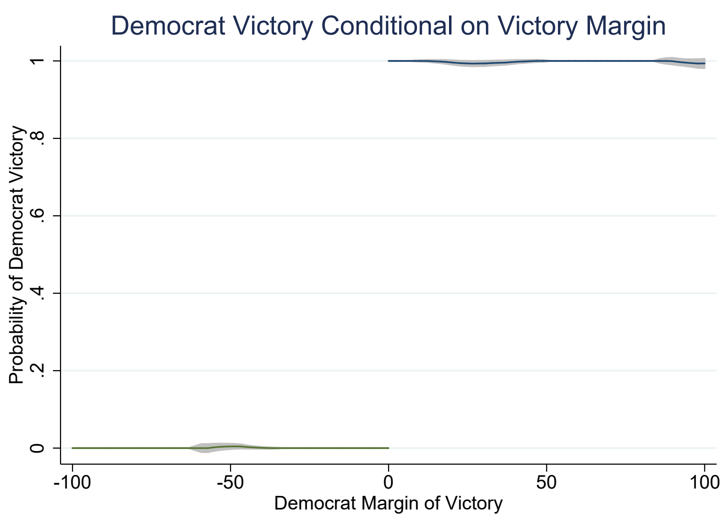
> "It is plausible that the exact vote count in large elections, while influenced by political actors in a non-random way, is also partially determined by chance beyond any actor's control. Even on the day of an election, there is inherent uncertainty about the precise and final vote count. In light of this uncertainty, the local independence result predicts that the districts where a party's candidate just barely won an election—and hence barely became the incumbent—are likely to be comparable in all other ways to districts where the party's candidate just barely lost the election."

$$Margin_{dh} \equiv VoteShare_{dh}^{Democratic} - VoteShare_{dh}^{Republican} \tag{6}$$

as the difference in the share of votes received by the Democratic and Republican candidates in Congressional district d for election to Congress h. Intuitively, given the two-party nature of US politics, the probability of a Democratic candidate winning an election conditional on a positive margin of victory (i.e., $Margin_{dh} > 0$) is near unity and has a discontinuity at the cutoff 0, as illustrated in Figure 6.[30] Hahn, Todd, and Van der Klaauw (2001) show that when $E\left[\varepsilon_{dh}|Margin_{dh} = m\right]$ is continuous in m at the cutoff 0, β in equation (5) can be identified as

$$\hat{\beta}_{RD} = \frac{\lim_{m\downarrow 0} E\left[y_{dh}|Margin_{dh} = m\right] - \lim_{m\uparrow 0} E\left[y_{dh}|Margin_{dh} = m\right]}{\lim_{m\downarrow 0} E\left[Democrat_{dh}|Margin_{dh} = m\right] - \lim_{m\uparrow 0} E\left[Democrat_{dh}|Margin_{dh} = m\right]}. \tag{7}$$

Figure 6: Regression Discontinuity Intuition



Source: Dave Leip's Atlas of US Presidential Elections and authors' calculations. Unit of analysis is a district-year pair across House elections from 1992 to 2014. The horizontal axis is the difference between the Democrat and Republican vote margin. The vertical axis is a dummy variable indicating whether the district is represented by a Democrat. Note that because a district could be controlled by a third party, positive margin does not perfectly predict Democrat representation. Shading represents the 95 percent confidence interval.

Lee and Lemieux (2010) show that $\hat{\beta}_{RD}$ is essentially an instrumental variable estimator, where the first stage is

$$Democrat_{dh} = \gamma I\left\{Margin_{dh} \geq 0\right\} + g\left(Margin_{dh}\right) + \mu_{dh}, \tag{8}$$

[30] Note that there are cases in which a third party wins the election even though the Democratic candidate receives more votes than the Republican candidate. As a result, $\Pr\left[Democratic_{d,t} = 1|Margin_{d,t} = m\right] \neq 1$ when $m > 0$.

and the second stage is

$$y_{dh} = \alpha + \beta Democrat_{dh} + f\left(Margin_{dh}\right) + \varepsilon_{dh}. \tag{9}$$

$I\{.\}$ is an indicator function that takes a value of 1 if the argument in brackets is true and 0 if it is false, while $g(.)$ and $f(.)$ are flexible functions of the assignment variable – i.e. $Margin_{dh}$ – that control for the direct effect of the strength of the Democratic versus Republican parties on the outcome variable y_{dh}. Lee and Lemieux (2010) suggest both parametric and nonparametric approaches to estimate $\hat{\beta}_{RD}$, and we pursue both. Specifically, for the parametric approach, we use all observations and define $g(.)$ and $f(.)$ as third-order polynomial expansions of the assignment variable. For the nonparametric approach, we follow the procedure developed by Imbens and Kalyanaraman (2012) that uses local linear estimation within an optimal bandwidth w^*. Standard errors are clustered on the assignment variable. Further details and robustness checks for the two approaches are provided in Section D of the appendix.

As noted above, the identifying assumption of our RD estimation – that $E\left[\varepsilon_{dh}|Margin_{dh} = m\right]$ is continuous in m at the cutoff 0 – implies that the election outcome at the cutoff point is determined by random factors, i.e., no party or candidate can fully manipulate the election. We provide quantitative support for this assumption using two checks suggested by Lee and Lemieux (2010). First, if election outcomes were fully manipulable, the distribution of the assignment variable ($Margin_{dh}$) would be discontinuous at the cutoff ($Margin_{dh} = 0$). For example, if weather alone determined close elections, it is unlikely that in the districts Democrats win, the margin of victory would be substantially larger than in the districts they lose. We test for this discontinuity using the method developed by McCrary (2008). As shown in the upper left panel of Appendix Figure A.1, the test statistic for a null hypothesis of continuity at the cutoff point is 0.077 with a standard error of 0.119. Thus, we fail to reject the hypothesis of incomplete manipulability, consistent with our identifying assumption.

The second check examines characteristics of Congressional districts – such as median household income and the shares of the population that are not white, are veterans or have a bachelors degree – in the neighborhood of the cutoff point directly. If there were full manipulation at the cutoff, districts on the margin would show discontinuities in distributions of these characteristics at the cutoff point. The remaining panels of Appendix Figure A.1 reveal that none of the distributions of key district attributes exhibit discontinuities at the cutoff 0, indicating that our hypothesis of a valid RD setting cannot be rejected.

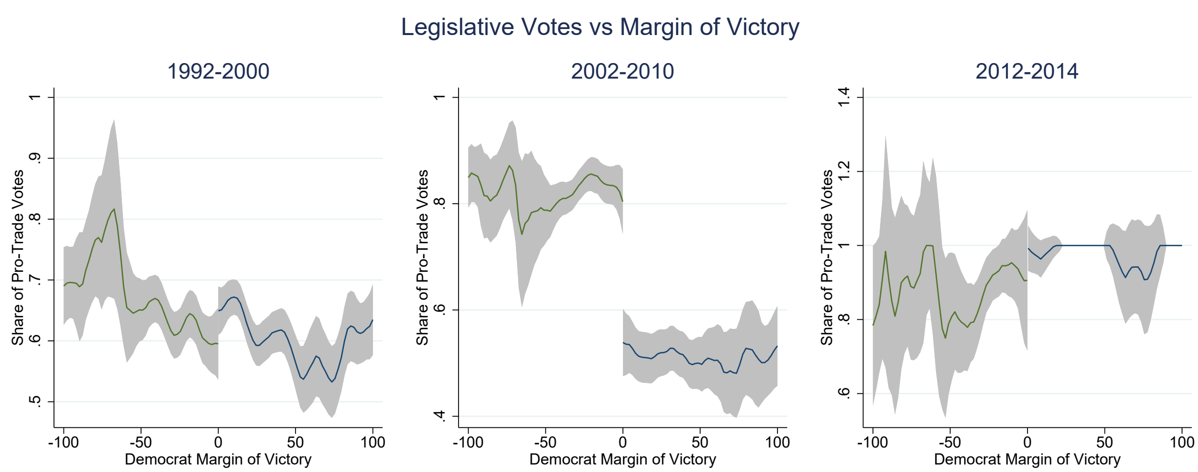
5.3 Results

We consider the relationship between party affiliation and support for trade votes separately by periods of time that we refer to as "constant-district periods." These constant-district periods correspond to the decades in which Congressional districts are generally constant, between the redistricting process that occurs after each decennial Census. We start with a visual presentation of the relationship between Democrats' margin of victory, $Margin_{dh}$, and subsequent votes on trade by the district's representative, y_{dh}, across the 103^{rd} to 107^{th}

(elected in the 1992 to 2000 elections) Congresses, 108^{th} to 112^{th} (2002 to 2010 elections) Congresses, and 113^{th} and 114^{th} (2012 and 2014 elections) Congresses.

As shown in the left panel of Figure 7, in the first of these periods, the share of a district's pro-trade votes is not statistically different on either side of the cutoff point $Margin_{dh} = 0$. This outcome suggests that in the 1990s, Democrat and Republican legislators with narrow margins of victory, on average, voted similarly on legislation related to trade. The center panel, however, indicates that the parties diverge in their voting on trade in the 2000s, after implementation of PNTR. Specifically, in this period, the share of districts' pro-trade votes drops discontinuously at the cutoff point where the Democrat earns a larger share of the vote. Given that the chance of winning the election jumps discontinuously at the same point (see Figure 6), this outcome reveals that Democratic representatives during this period take more anti-trade positions than their Republican colleagues. The final panel of Figure 7, like the first panel, reveals little divergence in voting patterns, indicating that Republicans and Democrats were again voting similarly on trade-related bills, though we caution that this period contains relatively few bills, as illustrated in Appendix Table A.1.

Figure 7: Democrat Votes on Trade Bills



Source: Dave Leip's Atlas of US Presidential Elections and authors' calculations. Unit of analysis is a district-year pair across the election years 1992 to 2014. Figure displays the share of pro-trade votes (vertical axis) versus the Democrat vote share margin of victory (horizontal axis) for three periods corresponding to the terms of the 103rd to the 107th Congresses (elected in the years 1992 to 2000), the 108th through the 112th Congresses (2002 to 2010), and the 113th and 114th Congresses (2012 to 2014). A triangular kernel is used for local linear regressions. Shading represents the 95 percent confidence interval.

Formal regression discontinuity estimation results for the effect of party affiliation on representatives' voting for pro-trade bills, $\hat{\beta}^{RD}$, for each of the three constant-district periods are reported in Table 3. As mentioned above, we use both parametric and non-parametric results, and under both techniques, the formal estimation results are consistent with those in the descriptive figures. Panel A of Table 3 presents results based on parametric estimation, and Panel B of Table 3 presents results based on nonparametric local linear estimation. Standard errors are clustered on the assignment variable.

As indicated in the first column of the panel, we find that in the 1990s, the period when

Democratic President Bill Clinton advocated the expansion of US trade agreements (Rorty (1998),Kamarck and Podkul (2018)), Democrats vote similarly to Republicans, based on parametric estimation in Panel A, or are modestly more supportive of free trade, based on non-parametric estimation in Panel B. The results in column two, however, indicate that, under both approaches, Democrats in the period from 2002 to 2010 voted much less frequently in a pro-trade way than Republicans, as rank-and-file Democrats coalesced in opposition to new trade agreements (Palmer (2007)). This result provides a rationale for our earlier finding that voters in counties subject to larger increases in competition from China increase the share of votes cast for Democrats during this period. In terms of economic significance, the coefficient estimate for the 2002 to 2010 period indicates that a Democratic affiliation is associated with a roughly 30 percent reduction in the share of pro-trade votes, relative to Republican affiliation.

In column three of Table 3, however, we find that the differential opposition of Democrats to pro-trade bills dissipates in the the 2010s, under both approaches, with Democrats and Republicans voting more similarly on trade-related bills than in the previous decade. This shift in legislators' voting on trade-related bills again provides a mechanism for our finding in Section 4 that the electoral advantage for Democrats in PNTR-exposed counties began to decline in 2010, and had disappeared by 2014 and 2016.

Lastly, we examine whether this evolution in the voting of Democrat and Republican representatives on trade-related bills is driven by districts with high versus low exposure to PNTR. To do this, we split districts into those with NTR gaps above or below the median and generate regression discontinuity estimates for each group. As indicated in Table 4, from 1992 to 2000, before PNTR, Democratic representatives in high NTR gap districts were actually modestly more pro-trade than Republicans, a relationship that is not present in low-NTR gap districts. After passage of PNTR, however, from 2002 to 2010, Democrats in both high and low exposure districts are significantly more likely than Republicans to vote against pro-trade bills. This change occurs partly because the share of pro-trade votes cast by Democrats goes down from around 60 percent in the 1992 to 2000 period to around 50 percent in the 2002 to 2010 period, and partly because Republicans move from casting pro-trade votes around 65 percent of the time in the 1990s to nearly 85 percent of the time in the 2000s.[31] In the last period, from 2012 to 2016, Democrat and Republican legislators vote similarly on trade-related bills in both low- and high-exposure districts.

In sum, the regression discontinuity results in this section provide an economic rationale for the election voting patterns reported in the first part of the paper, both for specific time periods, as well as for changes in those patterns over time. In the 1990s, prior to passage of PNTR, Democrat and Republican representatives vote similarly on trade-related bills, and election voting is mostly unrelated to exposure to trade exposure to liberalization. After passage of PNTR, from 2002 to 2010, Democrats become much more likely than Republicans

[31] Republicans in high NTR gap districts exhibit less of this move toward pro-trade votes between the 1990s and 2000s, voting for pro-trade bills only 80 percent of the time from 2002 to 2010 versus 88 percent for Republicans in low NTR gap districts. This difference in positions is consistent with Republicans adjusting their policies on trade toward the preferences of the median voter in more exposed districts, as found in Feigenbaum and Hall (2015).

Table 3: Democrat Affiliation and Legislators' Voting for Pro-Trade Bills

Panel A: Parametric Approach

	(1) 1992-2000	(2) 2002-2010	(3) 2012-2016
Democrat	0.063	-0.298***	0.082
	(0.040)	(0.048)	(0.091)
Stock-Yogo	16	16	NA
Kleibergen-Papp	355	240	NA
Observations	2,172	1,738	433

Panel B: Nonparametric, Local Linear Approach

	(1) 1992-2000	(2) 2002-2010	(3) 2012-2016
Democrat	0.078**	-0.326***	-0.25
	(0.031)	(0.032)	(0.058)
Observations	1,406	1,406	313

Notes: Table summarizes the results of representative-Congress level regression discontinuity regressions of the share of pro-trade votes on an indicator for whether the representative is a Democrat. Panel A reports results using parametric estimation with third-order polynomials. Panel B reports results using nonparametric local linear estimation. In this technique, observations are limited to those within the optimal bandwidth. Standard errors clustered at the assignment variable level are reported below coefficients. *, ** and *** signify statistical significance at the 10, 5 and 1 percent level.

Table 4: The Impact of Democrat Affiliation on Trade Bill Voting by High and Low Exposure

Panel A: 1992-2000 (Nonparametric, Local Linear Approach)

	(1) Full Sample	(2) High Exposure	(3) Low Exposure
Democrat	0.078**	0.084**	0.014
	(0.031)	(0.036	(0.046)
Stock-Yogo	16	16	NA
Kleibergen-Papp	627	438	NA
Observations	1,406	941	675

Panel B: 2002-2010 (Nonparametric, Local Linear Approach)

	(1) Full Sample	(2) High Exposure	(3) Low Exposure
Democrat	-0.326***	-0.205***	-0.402***
	(0.032)	(0.061)	(0.061)
Stock-Yogo	16	NA	16
Kleibergen-Papp	222	NA	180
Observations	1,406	358	525

Panel C: 2012-2016 (Nonparametric, Local Linear Approach)

	(1) Full Sample	(2) High Exposure	(3) Low Exposure
Democrat	-0.025	-0.052	-0.109
	(0.058)	(0.107)	(0.092)
Stock-Yogo	NA	NA	NA
Kleibergen-Papp	NA	NA	NA
Observations	313	155	134

Notes: Table summarizes the results of representative-Congress level regression discontinuity regressions of the share of pro-trade votes on an indicator for whether the representative is a Democrat. Each panel reports results for a different decade. Columns 1, 2 and 3 report results for the full sample and for high- and low-exposure districts, where exposure is determined according to the district's NTR gap lying above or below the median. All regressions are nonparametric local linear estimation. The samples for columns 1, 2, and 3, are each restricted be within the regression-specific optimal bandwidth, which means that the number of observations for the full sample can be smaller or larger than the sum of the high- and low-exposure subsamples. Standard errors clustered at the assignment variable level are reported below coefficients. *, ** and *** signify statistical significance at the 10, 5 and 1 percent level.

to vote against pro-trade bills, and voters in counties exposed to PNTR's trade liberalization shift their votes toward Democrats. Finally, from 2012 to 2016, Democrats and Republicans again vote similarly on trade-related bills, and the boost enjoyed by Democrats in the first decade of the 2000s disappears.

6 Conclusion

This paper examines the relationship between exposure to trade liberalization and voting in US elections over a twenty-five year period. In the first portion of the paper, we use a difference-in-differences approach to estimate the impact of county-level exposure to the US granting of Permanent Normal Trade Relations to China on the share of votes cast for Democrats in elections for the House of Representatives, Senate, and President.

We find that US counties more exposed to increased competition from China via PNTR experience relative increases in the share of votes cast for Democrats in Congressional elections in the early 2000s, relative to the 1990s, but that this boost disappears in the 2010s, concomitant with the rise of the Tea Party faction of the Republican Party. In terms of economic significance, we find that, in the 2000s, moving a county from the 25th to the 75th percentile of exposure to PNTR is associated with a relative increase in the Democrat vote share in House elections of 2.4 percentage points, or a 4.8 percent increase relative to the average share of votes cast for Democrats in the 2000 Congressional elections.

In the second portion of the paper, we find evidence that these changes in the relationship between exposure to trade liberalization and voting can be explained by changes in the policy choices of Democratic and Republican representatives over time. Using a regression discontinuity approach, we find that House Democrats in the early 2000s were substantially more likely than their Republican colleagues to vote against legislation supportive of free trade, consistent with the stronger support for Democrats in trade-exposed areas during this period. By the second decade of the 2000s, however, following the rise of the Tea Party wing of the Republican Party, the two parties vote similarly on trade-related bills, providing a rationale for the loss of the boost for Democrats. All told, our results are consistent with voters in trade-exposed areas supporting the party that advocates for trade policies consistent with their economic interests.

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Appendix (Not for Publication)

This appendix contains additional empirical results referenced in the main text.

A List of Trade-Related Bills

Table A.1 provides the list of trade-related bills sourced from Comparative Agendas, along with our rankings of each bill as either pro- or anti-trade. We use four classifications of bills: clearly anti-trade, marginally anti-trade, marginally pro-trade, and clearly pro-trade. The baseline results presented in Section 5 are based on the set of clearly pro- or anti-trade bills. Here, in Table A.2, we also present results based on the full set of bills, including those that are marginally anti- or pro-trade. As indicated in the Table, we continue to find that Democratic representatives' votes were relatively anti-trade in the early 2000s, and we also find that based on this broader set of bills, the shift toward relatively anti-trade positions by Republicans in 2013-2016 is even more pronounced.

B Comparison of County-Level and District-Level Regressions

In this sub-section, we compare regressions estimating the relationship between trade liberalization and voting based on county- and district-level data over an identical time period and find that both levels of aggregation yield qualitatively similar results. Specifically, we estimate the relationship between exposure to PNTR and voting for the House of Representatives across both districts (d) and counties (c) via an OLS regression of the change in the Democrat vote share in geography $g \in \{d, c\}$ between 2002 and 2010, $\mathit{\Delta Dem\,Share}_g^{2002:2010}$, on exposure to PNTR,

$$\mathit{\Delta Dem\,Share}_g^{2002:2010} = \theta\, NTR\, Gap_g + \mathbf{X}_g' + \alpha + \varepsilon_g. \tag{A.1}$$

The 2002 and 2010 elections represent the longest post-PNTR time period over which district-level estimates can be generated given the decennial redistricting that occurs between 2000 and 2002 and also between 2010 and 2012. $\mathbf{X}_g$ represents a vector of time-invariant demographic control variables, such as median household income and the share of the population over 65, which have been found to be important in the economics and political science literatures. These attributes are defined using the 2000 Census for the county-level regression and the 2005 American Communities Survey for the district-level regression.[32]

[32] This is the earliest year available as the American Communities Survey did not begin providing data for all Congressional districts until 2005. We include only demographic characteristics in this regression, relative to the broader set of covariates used in the county-level regressions below, because the employment-by-industry data required to construct them are not available at the district level.

Table A.1: Trade Bills

Bill	Year	Congress	Ranking	Bill	Year	Congress	Ranking
HJRES208	1993	103	4	HR2738	2003	108	1
HR3450	1993	103	1	HR2739	2003	108	1
HJRES373	1994	103	4	HRES252	2003	108	2
HR5110	1994	103	1	HRES329	2003	108	1
HJRES96	1995	104	4	HR4759	2004	108	1
HR1555	1995	104	3	HR4842	2004	108	1
HJRES182	1996	104	4	HRES705	2004	108	2
HR1643	1996	104	1	HJRES 27	2005	109	4
HR3161	1996	104	1	HR 2864	2005	109	3
HJRES79	1997	105	4	HR 3045	2005	109	1
HR2644	1997	105	2	HR 4340	2005	109	1
HCONRES213	1998	105	2	HRES 57	2005	109	3
HJRES120	1998	105	4	HR1053	2006	109	1
HR2621	1998	105	2	HR4954	2006	109	3
HR4276	1998	105	4	HR5602	2006	109	1
HCONRES190	1999	106	1	HR5684	2006	109	1
HJRES58	1999	106	4	HR6406	2006	109	1
HR975	1999	106	4	HR1830	2007	110	1
HJRES103	2000	106	4	HR2264	2007	110	2
HJRES90	2000	106	4	HR3688	2007	110	1
HJRES99	2000	106	4	HR515	2009	111	3
HR4444	2000	106	1	HR 5307	2010	111	3
HCONRES262	2001	107	3	HR2832	2011	112	1
HJRES50	2001	107	4	HR3078	2011	112	1
HJRES55	2001	107	4	HR3079	2011	112	1
HR2500	2001	107	3	HR3080	2011	112	1
HR2722	2001	107	3	HR4105	2012	112	4
HR3005	2001	107	1	HR6156	2012	112	1
HR3009	2001	107	1	HRES841	2012	112	3
HJRES101	2002	107	4	HR1295	2015	114	1
HRES414	2002	107	3	HR2578	2015	114	3
HRES450	2002	107	1	HR4923	2016	114	2
HRES509	2002	107	1	HRES819	2016	114	3

Source: Comparative Agendas and authors' calculations. Table lists the set of trade-related bills considered by the US House of Representatives from 1992 to 2016. These bills are identified via the Comparative Agenda subject-area classifications 1802 ("Trade Agreements") and 1807 ("Tariff Imports"). From this set, we keep only votes on final passage of a bill (i.e., we exclude procedural votes) and also exclude bills that deal with trade only tangentially, such as broad appropriations bills. Bills are ranked as "pro-trade" or "anti-trade" separately by two of the authors and three research assistants. A ranking of 1 denotes "clearly pro-trade" bills, a ranking of 2 denotes "marginally pro-trade" bills, a ranking of 3 denotes "marginally anti-trade bills," and a ranking of 4 denotes "clearly anti-trade bills." The final ranking displayed in the table is the modal rank across these reviewers.

Table A.2: RD Results: All Trade Bills

Variables	1992-2000	2002-2010	2012-2016
Democrat	0.030	-0.167***	0.255***
	(0.036)	(0.040)	(0.043)
District and Representative Controls	Yes	Yes	Yes
Observations	2,172	1,739	435

Source: Dave Leip's Atlas of US Presidential Elections and authors' calculations. Table summarizes the results of representative-Congress level regression discontinuity regressions of the share of pro-trade votes on an indicator for whether the representative is a Democrat using all trade bills. Standard errors clustered at the assignment variable level are reported below coefficients. *, ** and *** signify statistical significance at the 10, 5 and 1 percent level.

As indicated in Table A.3, we find no statistically significant relationship between exposure to PNTR and the Democrat vote share at either the district-level (left panel) or county-level (right panel). Coefficient estimates for demographic attributes are mostly statistically insignificant in both specifications, with the exception of median household income in the district-level regression, and R-squared statistics are also very similar across the two specifications.

C Tests for the Appropriateness of the Regression Discontinuity Approach

The first panel of Figure A.1 displays the McCrary (2008) test of whether there is a discontinuity in the density of Democrats' winning margin over Republicans. Specifically, the test statistic considers whether there is a discontinuity in the density function at the point at which the Democrat margin of victory is zero, with a null hypothesis that there is no discontinuity at this cutoff. The test yields an estimated statistic of 0.077 with a standard error of 0.119, indicating that we fail to reject the null hypothesis of continuity at the cutoff. The remaining panels examine the distributions of important district-level attributes plotted against the Democrat margin of victory. As discussed in the draft, none of these distributions exhibit discontinuities at the cutoff point at which the Democrat margin of victory is 0.

D Alternative Regression Discontinuity Approaches

In the main text (Panel A of Table 3), we implement a parametric estimation approach using third-order polynomial functions of $g(.)$ and $f(.)$ with potentially different coefficients on the two sides of the cutoff point, making use of all observations over the domain of the assignment variable. Following Lee and Card (2008), we calculate standard errors clustered

Table A.3: PNTR and Voting at the District- and County-Level, 2002-2010

Variables	Dem Share$_{dt}$	Dem Share$_{ct}$
NTR Gap$_g$	0.055	-0.004
	0.278	0.166
Median HHI$_g$	0.303**	0.093
	0.147	0.118
%Bachelors$_g$	-0.823	0.091
	0.501	0.261
%Graduate$_g$	1.001	0.307
	0.769	0.391
%Non-White$_g$	0.014	0.016
	0.058	0.070
%Over 65$_g$	0.455	-0.225
	0.401	0.276
%Veteran$_g$	-0.097	0.452
	0.763	0.413
Observations	433	3,077
R-squared	0.044	0.028
Estimation	OLS	OLS
Units	Districts	Counties
Period	2002-2010	2002-2010
Weighting	Population	Population
Clustering	State	State

Source: US Census Bureau, Dave Leip's Atlas of US Presidential Elections, and authors' calculations. Table reports OLS regression results for the Democrat vote share for House elections in district d in year t (first column) and county c in year t (second column). Sample period is the change from 2002 to 2010. The first covariate is the county- or district-level NTR gap. The next six covariates are demographic attributes based on the 2000 Census for counties and the 2005 American Communities Survey for districts, the first year for which demographic data are available at the Congressional District level for this time period. Standard errors adjusted for clustering at the state level are reported below coefficients. *, ** and *** signify statistical significance at the 10, 5 and 1 percent levels.

Table A.4: RD Results: Alternate Polynomial Functions

Panel A: Second-Order Polynomial Function

	(1) 1992-2000	(2) 2002-2010	(3) 2012-2016
Democrat	0.104***	-0.315***	0.007
	(0.031)	(0.037)	(0.065)
Covariates	Yes	Yes	Yes
R2	0.02	0.24	0.06
Observations	2,172	1,738	433

Panel B: Fourth-Order Polynomial Function

	(1) 1992-2000	(2) 2002-2010	(3) 2012-2016
Democrat	0.082	-0.243***	0.089
	(0.050)	(0.061)	(0.111)
Covariates	Yes	Yes	Yes
R2	0.02	0.24	0.07
Observations	2,172	1,738	433

Notes: Table summarizes the results of representative-Congress level regression discontinuity regressions of the share of pro-trade votes on an indicator for whether the representative is a Democrat using a parametric estimation approach with either second-order (Panel A) of fourth-order polynomials (Panel B). Standard errors clustered at the assignment variable level are reported below coefficients. *, ** and *** signify statistical significance at the 10, 5 and 1 percent level.

at the assignment variable level. Here, we report results using second- and fourth-order polynomials to examine the sensitivity of our estimates using third order polynomials. As indicated in Table A.4, we obtain similar results using these alternative polynomials.

The nonparametric approach to regression discontinuity estimation used in the main text (Panel B of Table 3) is a "local linear" estimation that uses observations within a window of width w on both sides of the cutoff point and assumes that $g(.)$ and $f(.)$ are linear, with potentially different slopes on the two sides of the cutoff point. We implement this approach using the procedure developed by Imbens and Kalyanaraman (2012) to calculate the optimal bandwidth w^*, and estimate standard errors that are clustered on the assignment variable. In Table A.5, we report robustness checks using different bandwidths, specifically, halving and doubling w^*, as in Lee and Lemieux (2010). As indicated in the table, results with these alternative bandwidths are similar to those reported in Section 5.

Table A.5: RD Results: Alternate Bandwidths

Panel A: Half Optimal Bandwidth

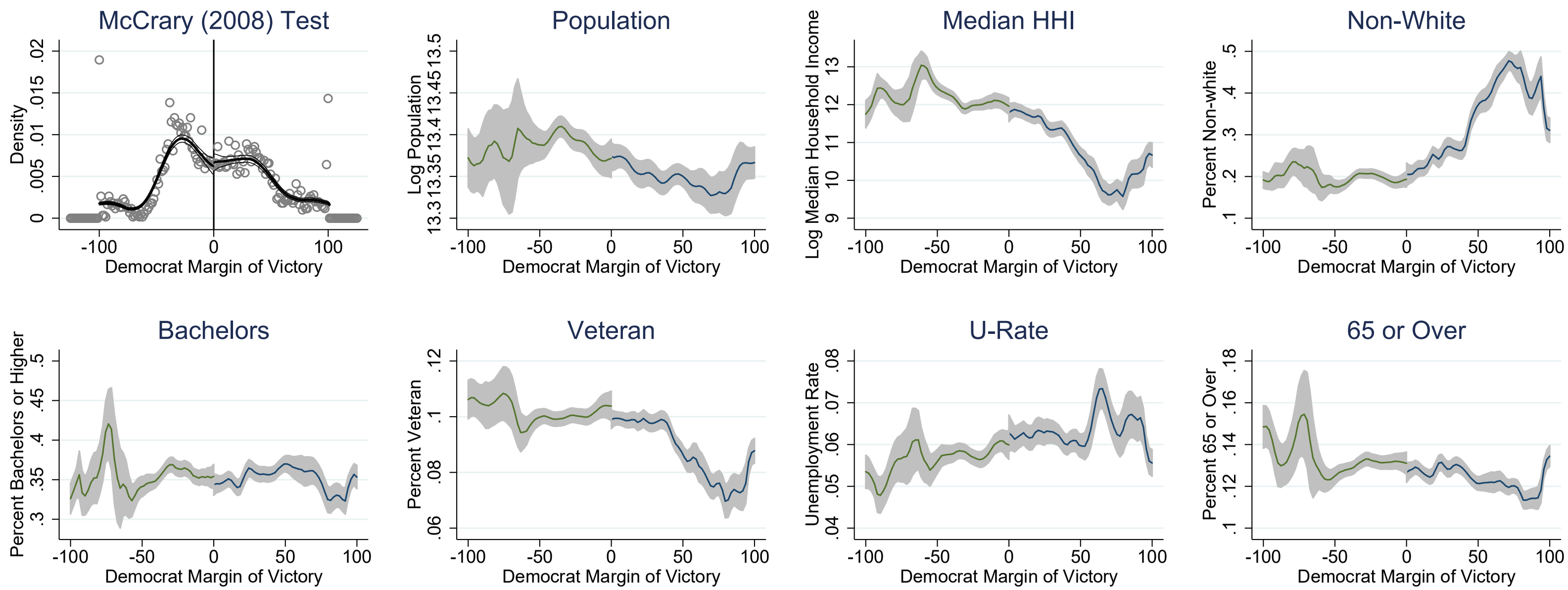
	(1) 1992-2000	(2) 2002-2010	(3) 2012-2016
Democrat	0.094**	-0.292***	0.061
	(0.044)	(0.044)	(0.080)
Covariates	Yes	Yes	Yes
R2	0.01	0.23	0.03
Observations	691	680	145

Panel B: Double Optimal Bandwidth

	(1) 1992-2000	(2) 2002-2010	(3) 2012-2016
Democrat	0.084***	-0.313***	0.089**
	(0.025)	(0.024)	(0.040)
Covariates	Yes	Yes	Yes
R2	0.02	0.24	0.05
Observations	1,926	1,738	401

Notes: Table summarizes the results of representative-Congress level regression discontinuity regressions of the share of pro-trade votes on an indicator for whether the representative is a Democrat using a non-parametric local linear approach with either half (Panel A) or double (Panel B) the optimal bandwidth size. Standard errors clusterd at the assignment variable level are reported below coefficients. *, ** and *** signify statistical significance at the 10, 5 and 1 percent level.

Figure A.1: RD Identifying Assumption Tests



Source: Dave Leip's Atlas of US Presidential Elections and authors' calculations. Observations are defined at the district-year level for the election years 1992 to 2014. The horizontal axis for all panels is the difference between the Democrat and Republican vote shares. The upper left panel displays the McCrary (2008) test of whether there is a discontinuity in the density of the Democrat win margin across districts. The estimated discontinuity, 0.077 with a standard error of 0.119 is statistically insignificant, indicating that the null hypothesis of continuity is not rejected. The remaining seven panels examine the distributions of district-level attributes plotted against the Democrat margin of victory. Shading represents the 95 percent confidence interval. Note that because a district could be controlled by a third party, a positive Democrat margin of victory does not perfectly predict that a Democrat represents the district.