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Did trade liberalization with China influence US elections?☆

Yi Che [a], Yi Lu [b], Justin R. Pierce [c,*], Peter K. Schott [d], Zhigang Tao [e]

[a] School of Business, East China University of Science and Technology, Shanghai, China
[b] School of Economics and Management, Tsinghua University, Beijing 100084, China
[c] Board of Governors of the Federal Reserve System, 20th St. and Constitution Ave., NW Washington, DC, 20551, United States
[d] Yale School of Management & NBER, 165 Whitney Avenue, New Haven, CT 06511, United States
[e] Cheung Kong Graduate School of Business Beijing, China

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ABSTRACT

We examine election voting and legislators' roll-call votes in the United States over a twenty-five year period. Voters in areas more exposed to trade liberalization with China in 2000 subsequently shift their support toward Democrats, relative to the 1990s, though this boost for Democrats wanes after the rise of the Tea Party in 2010. House members' votes in Congress rationalize these trends, with Democratic representatives disproportionately supporting protection during the early 2000s. Together, these results imply that voters in areas subject to higher import competition shifted votes toward the party more likely to restrict trade.

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1. Introduction

While international trade has long been a contentious issue in US elections, it has become even more controversial in the last two decades, as a surge in imports from China coincided with a steep decline in manufacturing employment. As a result, understanding the relationship between trade and elections is increasingly important, both for its reflection of the underlying distributional effects of trade, as well as its implications for future policy. In this paper, we examine the link between US trade liberalization with China and election voting, and then investigate whether legislators' policy choices rationalize this relationship.

We begin with an analysis of how votes cast for federal office-seekers respond to a substantial change in US trade policy, the granting of Permanent Normal Trade Relations (PNTR) to China in 2000, that effectively eliminated the possibility of a trade war between the two countries. We measure an area's exposure to this liberalization via the industry structure of the county, 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 provide novel evidence on the relationship between trade liberalization and party vote shares. In particular, we find that in the first decade after PNTR, counties more exposed to the change in policy

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* Corresponding author.
E-mail addresses: tccheyi2005@hotmail.com (Y. Che), luyi@sem.tsinghua.edu.cn (Y. Lu), justin.r.pierce@frb.gov (J.R. Pierce), peter.schott@yale.edu (P.K. Schott), zgtao@ckgsb.edu.cn (Z. Tao).

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exhibit relative increases in the share of votes cast for Democrats *vis à vis* the 1990s, a relationship that has not been previously uncovered. Coefficient estimates suggest that moving a county from the 25th to the 75th percentile of exposure is associated with a 2.2 percentage point relative increase in the share of votes cast for Democratic candidates for the House of Representatives, a sizable impact compared to the 49 percent Democratic vote share in the 2000 Congressional election. This reaction is most evident in elections for the House, in line with that body being more sensitive to local concerns than the Senate or the Presidency. We find a a less-pronounced effect in elections for the Senate, and no statistically significant relationship for Presidential elections or for voter turnout.

In the second portion of the paper, we provide a potential explanation for why voters shifted support toward Democrats in the early 2000s. Using a regression discontinuity analysis comparing the legislative votes of Democratic and Republican representatives who win election by small margins, we find that Democrats in the early 2000s were significantly more likely to vote to restrict international trade than their Republican counterparts. We attribute Democrats' anti-trade positions in the early 2000s, in part, to opposition to a pro-trade Republican President, which represented a shift from being more supportive of trade during the Clinton Presidency in the 1990s. Combined with the earlier results for election voting, these findings suggest that voters in areas more exposed to import competition via PNTR were more likely to vote for Democrats in House elections in the early 2000s because representatives from that party were more likely to vote against expanding international trade.

While our main results focus on changes in the first decade of the 2000s, relative to the 1990s, we also extend our analysis to 2016 and the election of Donald Trump. During this second decade of the 2000s, high-profile Republicans began adopting more anti-trade positions, and a perception emerged that voters in areas with large increases in import competition were shifting their votes toward these anti-trade Republicans. Indeed, we find that areas more exposed to PNTR experience relative increases in the favorability of the "Tea Party"—a wing of the Republican party whose views included hostility toward trade agreements—and the number of Tea Party activists. We also find evidence consistent with these moves in our analyses of elections and legislative voting, though we caution that relationships in this latter period are imprecisely estimated. Specifically, we find that the positive relationship between exposure to PNTR and the Democratic vote share disappears by 2016 and that Republicans vote similarly or even more anti-trade than Democrats from 2012 to 2016.

We perform our baseline analysis of election voting at the county-level because county borders are largely stable over time, allowing us to track voting information consistently over long periods that span the redrawing of Congressional districts after each decennial Census. This consistency is important because it allows us to observe outcomes before and after the policy change and also before and after the 2000 to 2002 redistricting period, when a large share of the employment decline during our sample period occurs. Nonetheless, because Congressional elections are determined at the district-, rather than the county-level, we construct a crosswalk using county-district population shares that allows us to examine election data at the district level over our sample period. These constructed district-level data yield results that are qualitatively identical to the county-level baseline.

Our paper relates 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. Margalit (2011), for example, uses plant-level information on Trade Adjustment Assistance to determine that voters are more sensitive to job loss due to foreign competition than other factors. Conconi et al. (2012) 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.[1]

In regards to trade with China, Feigenbaum and Hall (2015) provide the first evidence on the relationship between Chinese imports and political outcomes, examining their impact 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.[2] Our finding that exposure to PNTR is associated with relative increases in Tea Party activity, but not with an increase in the probability of a Tea Party candidate being elected, is consistent with these results.

More recently, Autor et al. (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. These authors also find that majority-white Congressional districts that experience larger increases in Chinese imports 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. Their analysis is conducted with county-district pairs that aggregate to the district-level via weighting, and covers the years 2002 to 2016.[3]

Our analysis provides new information on the relationship between import competition and voting, relative to Autor et al. (2020), while also being broadly consistent with their results. Importantly, because our sample begins in 1992, and compares outcomes in the 2000s to those in the 1990s, we find a shift toward support for Democrats that is not apparent without a comparison

[1] 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 et al. (2020) examine the role of skilled labor abundance in Representatives' votes on trade and immigration bills.

[2] Relatedly, Jensen et al. (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 et al. (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 et al. (2015) and Colantone and Stanig (2018) examine data for Western European countries and find that higher imports from either Eastern Europe or China are associated with increases in the share of votes for nationalist and far right parties.

[3] Bombardini et al. (2020) examine U.S. politicians' expectations regarding the effects of increased import competition from China and find that U.S. legislators had extensive information regarding the "China Shock," but did not place much weight on its negative effects.

to the earlier period. However, when we extend our analysis through 2016, we find that the shift toward Democrats that peaks in 2008 unwinds in the 2010s, indicating a movement back toward Republican candidates in trade-exposed areas, consistent with Autor et al. (2020).

Our paper makes several additional contributions to this literature. First, we exploit a major change in U.S. trade policy as part of our identification strategy to examine the relationship between trade and political outcomes. 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 Democratic representatives were, in fact, more likely to restrict trade. 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 et al. (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 et al. (2019) show that these areas experience relative reductions in population growth. Feler and Senses (2017) find that the provision of public goods decreases in these areas as property tax revenue falls, and Feler and Senses (2017) and Che et al. (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 et al. (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 2016 election of Donald Trump, who adopted tariff increases with far-ranging effects, underscoring the important policy implications of elections. Amiti et al. (2019) and Fajgelbaum et al. (2019) find welfare losses resulting from recent US tariffs on China, with Flaaen et al. (2020), Waugh (2019), Flaaen and Pierce (2019), and Bown et al. (2020) providing further detail on the trade-offs that can arise between protecting firms and harming consumers and downstream industries. Blanchard et al. (2019) find that Republicans in trade-exposed areas lost electoral support in the 2018 Congressional elections, while Fetzer and Schwarz (2019) and Fajgelbaum et al. (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 explores the robustness of the baseline results for House of Representatives elections, and Section 6 extends the analysis through 2016. Lastly, Section 7 focuses on the regression discontinuity analysis examining how representatives from each political party voted on trade-related bills, and Section 8 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 Fig. 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.

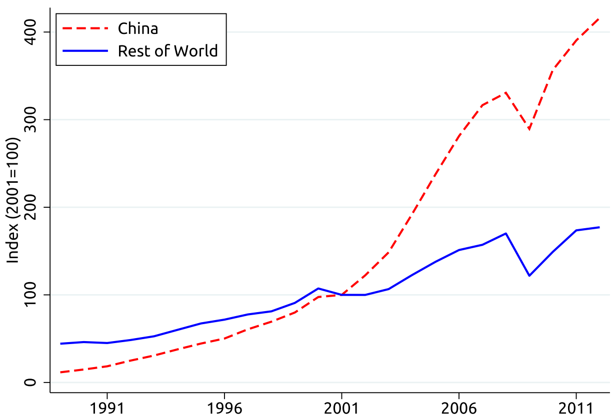

Fig. 1. US imports from China vs rest of world. Source: US Census Bureau. Figure displays indexes of US imports from China and from the rest of the world from 1989 to 2012. The base year for the indexes is 2001.

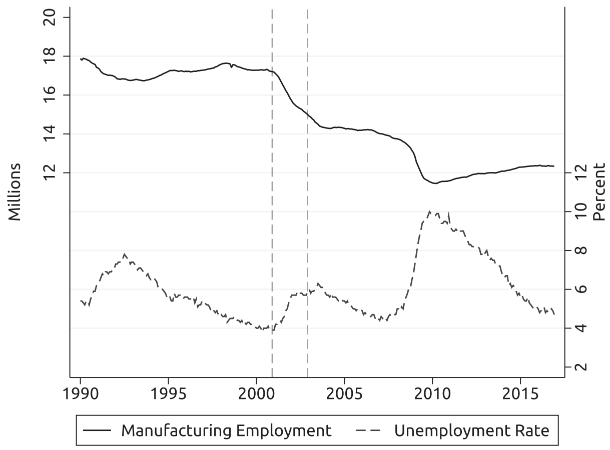

Fig. 2. US manufacturing employment and unemployment rate. Source: US Bureau of Labor Statistics. Figure displays US manufacturing employment (left axis) and the overall unemployment rate (right axis) from 1990 to 2016. Vertical lines highlight the dates of the 2000 and 2002 elections.

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 Fig. 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 et al. (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 et al., 2018; Bloom et al., 2019), there is evidence that the effects of import competition carried through to broader aspects of the labor market. Autor et al. (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]

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, “[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 voters over this time period, as discussed in Section 7.[7]

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.

[4] These trends are consistent with estimates of substantial adjustment costs for workers who switch industries or occupations, as shown in Artuc et al. (2010), Ebenstein et al. (2014), Acemoglu et al. (2016), and Caliendo et al. (2019).

[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 followed up those calls with substantial tariff increases directed primarily at China.

[7] Frieden (2019) argues that political discontent related to trade likely arose due to failure to compensate those harmed by international competition, as well as inattention by political parties to problems faced by large groups of voters.

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 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 an upcoming tariff increase served as a disincentive for firms considering investments associated with increasing US imports from China throughout the 1990s.[8] 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.[9] 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.[10]

We compute counties' exposure to PNTR in two steps. The first calculates exposure for US industries. We follow Pierce and Schott (2016) in defining industry-level exposure 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,

$$\text{NTR Gap}_j = \text{Non NTR Rate}_j - \text{NTR Rate}_j. \tag{1}$$

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 et al. (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 Fig. 3, NTR gaps vary widely across industries, with a mean and standard deviation of 30 and 18 percentage points, respectively. Moreover, as noted in Pierce and Schott (2016), the vast majority of the variation in the NTR gap across industries is attributable to variation in non-NTR rates, which were set 70 years prior to passage of PNTR.[11] 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. Lastly, as we discuss in Section 6, we find that there is no relationship between the NTR gap and the Democratic vote share in years prior to PNTR. This lack of a relationship is consistent with the parallel trends assumption inherent in difference-in-differences estimation.

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

$$\text{NTR Gap}_c = \sum\nolimits_j \left(\frac{L_{jc}}{L_c} \text{NTR Gap}_j \right), \tag{2}$$

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).[12] Congressional district-level NTR gaps are calculated analogously, though calculating district-level NTR gaps and other district-level characteristics can only be accomplished by taking weighted averages of the counties (partial or total) that comprise a district.

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

[8] Intuition for this disincentive can be derived, in part, from the literature on investment under uncertainty, e.g., Pindyck (1993) and Bloom et al. (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.

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

[10] Heise et al. (2015) describe the effect of PNTR on the structure of supply chains, and Feng et al. (2016) discuss the effect of PNTR on entry and exit patterns of Chinese exporters, as well as changes in export product characteristics.

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

[12] We follow the procedure outlined in Autor et al. (2013) to impute suppressed employment values at the industry-county-level.

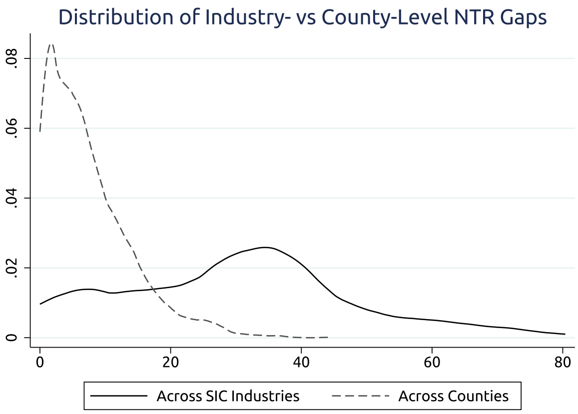

Fig. 3. The NTR gap across industries and counties. Source: Feenstra et al. (2002) and authors' calculations. Figure displays distributions of industry- and county-level NTR gaps.

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, as displayed visually in Fig. 3. This figure also highlights that the county-level NTR gap used in our main difference-in-differences term of interest is continuous, and our estimates therefore are a comparison of more-exposed versus less-exposed counties. The mean and standard deviation of the county-level NTR gap are 6.1 and 4.2 percentage points, and 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.

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 on Presidential elections.[13] 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. The population-weighted average county-level Democratic vote share for the House of Representatives elections 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 et al., 2002) and their exposure to the phasing out of textile and clothing quotas under the global Multi-Fiber Arrangement (Khandelwal et al., 2013),

[13] These data are available for purchase from www.uselectionatlas.org
[14] See, for example, Baldwin and Magee (2000), Conconi et al. (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 home-ownership status, and Conconi et al. (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.

Attribute	Mean	SD	Min	Max
Median income ($000)	40.23	10.63	11.21	77.35
Bachelor (%)	13.09	4.97	0	40.3
Graduate (%)	7.18	3.5	0.3	29.7
Non-White (%)	19.39	15.31	0	94.9
65+ (%)	12.53	3.77	1.4	34
Veteran (%)	14.39	2.65	4.2	29
Manufacturing (%)	19.75	11.02	0	91.02
NAFTA exposure	−0.19	0.34	−4.84	0.28
MFA exposure	0.5	1.35	0	21.29
NTR tariff rate (%)	0.59	0.66	0	7.99

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

each of which are calculated based on the employment-share weighted average of their exposure to these policy changes, as in Eq. (2).

We compute counties' exposure to the MFA phase-outs following Brambilla et al. (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 Eq. (2). Unlike other county-level time-invariant variables, this NAFTA exposure measure is then interacted with a *pre*-PNTR indicator, reflecting the fact that NAFTA's liberalization occurred in the pre-PNTR period. Intuitively, counties' exposure to both PNTR and NAFTA rises with their share of employment in manufacturing, with correlation coefficients between the manufacturing employment share and each exposure measure 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 2008, and then expand the analysis to other offices—the US Senate and President—as well to voter turnout. We then discuss the relative advantages of county- versus district-level data before demonstrating the robustness of our baseline House results to the use of synthetic district-level data constructed using county-district population information.

4.1. Baseline empirical strategy

Our baseline difference-in-differences (DID) specification asks whether counties with higher NTR gaps (first difference) experience differential changes in voting after the change in US trade policy (second difference):

$$\text{Dem Share}_{ct} = \theta \text{Post PNTR}_t \times \text{NTR Gap}_c + \text{Post PNTR}_t \times \mathbf{X}_c'\gamma + \mathbf{Z}_{ct}'\beta + \delta_{\mathbf{c}} + \delta_{\mathbf{t}} + \alpha + \varepsilon_{ct}. \tag{3}$$

The dependent variable is the share of votes cast for Democratic candidates for the US House of Representatives in county c in year t. The first term on the right-hand side is the DID term of interest, an interaction of a post-PNTR (i.e., $t > 2000$) indicator with the (time-invariant) county-level NTR gap, as defined in the preceding section. We begin by examining elections in the period from 1992 to 2008, an end point that coincides with the first election during the Great Recession, and the last such election before the emergence of the Tea Party, discussed below. We extend the period of analysis through 2016 and discuss reasons for changes in the relationship between exposure to PNTR and voting in Section 6.

In Eq. (3), $\mathbf{X}_c$ represents the full set of time-invariant demographic and policy control variables described in Section 3. These variables are defined as of 1990—the Congressional election year just preceding our analysis—and are interacted with the $Post: PNTR_t$ indicator to allow the relationship between these county characteristics and voting to differ before and after passage of PNTR. This treatment mirrors the manner in which exposure to PNTR enters the estimation equation. $\mathbf{Z}_{ct}$ represents a matrix of time-varying policy attributes including the average US import tariff rate associated with each county's mix of industries, as well as the county's exposure to the phasing out of the MFA. $\boldsymbol{\delta_c}$ and $\boldsymbol{\delta_t}$ represent county and year fixed effects.

An advantage of this DID identification strategy is its ability to net out characteristics of counties that are time-invariant, while also controlling for aggregate shocks that affect all counties identically in a particular year, such as whether the election occurs during a presidential versus non-presidential election year.[17] Because county population sizes vary substantially, we weight by initial year (1992) population. Standard errors in our baseline estimates 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.

4.2. Exposure to PNTR and House of Representatives elections

The first column of Table 2 reports results for House of Representatives elections using Eq. (3), our preferred baseline specification. As indicated in that column, we find a positive and statistically significant relationship between counties' exposure to PNTR and the share of votes cast for Democrats, relative to the 1990s. In terms of economic significance, the coefficient estimate on the DID term 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 2.2 percentage point increase in the share of votes cast for the Democratic candidate, or 4.6 percent of the 49 percent average Democratic vote share in the 2000 US House elections (as displayed in the last four rows of the table).[18]

We provide a rationale for why voters in areas more exposed to increased import competition via PNTR might shift votes toward Democrats in the 2000s in Section 7. As discussed in detail in that section, Democrats were substantially more likely to take anti-trade positions on legislation in the 2000s, making them an attractive choice for voters seeking representatives who would limit import competition.[19] Moreover, Democratic representatives' move toward anti-trade positions in the 2000s occurred abruptly following the election of a Republican President in 2000, making these policy choices more salient for trade-sensitive voters.

While these results may appear at odds with those from Autor et al. (2020), which finds that higher imports from China are associated with a shift, on net, toward conservative Republican candidates, it is important to note that our paper considers a longer time period. In particular, our analysis begins in 1992 and compares election voting in the first decade of the 2000s to election voting in the 1990s. By contrast, Autor et al. (2020) takes 2002 as a starting point and considers the relationship between imports and subsequent changes in voting. In this sense, our results highlight a shift in voting across time periods that was not considered by Autor et al. (2020). Moreover, our finding that the boost for Democrats in the early 2000s dissipates after the rise of the Tea Party in 2010—discussed below in Section 6—is consistent with the subsequent shift toward Republican candidates found by Autor et al. (2020).[20]

In terms of the control variables, we find that counties with higher household incomes and higher shares of the population with graduate degrees or that are over the age of 65 vote relatively more for Democratic House candidates in the 2000s, relative to the 1990s. In regards to economic significance, the impact of an interquartile shift in exposure to PNTR on the Democratic vote share is larger than an equivalent shift in the shares of the population with graduate degrees or that are over 65. Moving a county from the 25th to the 75th percentile of the distribution for median household income, however, is associated with an increase in the share of votes cast for Democrats in House elections that is roughly three times larger than the impact of PNTR. Lastly, higher exposure to NAFTA is associated with relative increases in the share of votes cast for Democrats in the pre-PNTR period in which the liberalization occurred—consistent with the the relationship for PNTR—though the relationship for NAFTA is only marginally significant.

4.3. The Senate and the Presidency

In this section, we examine the relationship between PNTR and county-level Democratic vote shares for two other offices, the US Senate and President. To do so, we re-estimate Eq. (3) 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.

As indicated in the second column of Table 2, we find a positive and marginally statistically significant relationship between exposure to PNTR and the share of votes cast for Democrats in Senate elections. In terms of magnitude, an interquartile shift in exposure to PNTR is associated with a relative increase in the Democratic vote share of 1.5 percentage points, or 3.1 percent of the average share of votes won by Democratic candidates for Senate across counties in the year 2000 (49 percent). Results in 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.

[17] One disadvantage is that the long sample period renders it susceptible to biased standard errors associated with serial correlation (Bertrand et al., 2004).

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

[19] Democrats' relative opposition to trade in the 2000s followed a period in the 1990s in which Democrats and Republicans voted more similarly on trade-related bills.

[20] We provide a direct comparison of our measure of exposure to PNTR and Autor et al. (2020)'s measure of exposure to import competition in Appendix Section A. As described in that section, we find that when applied to identical time periods and levels of aggregation, the two measures of exposure exhibit similar relationships with the Democratic vote share. Thus, differences in findings between the two papers arise primarily because of the different time periods considered.

Table 2
PNTR and county-level voting for democrats.

Variables	House democratic share$_{ct}$	Senate democratic share$_{ct}$	President democratic share$_{ct}$	Turnout$_{ct}$
Post *x* NTR Gap$_c$	0.561***	0.378*	0.023	0.048
	0.208	0.207	0.091	0.090
Post *x* Median HHI$_c$	0.207***	0.234**	0.075	−0.090
	0.058	0.114	0.048	0.054
Post *x* Percent Bachelors$_c$	0.094	0.083	0.627***	0.474***
	0.171	0.386	0.094	0.111
Post *x* Percent Graduate$_c$	0.440***	−0.187	−0.060	−0.259**
	0.163	0.380	0.111	0.124
Post *x* Percent Non-White$_c$	0.074	0.030	0.093***	0.123***
	0.051	0.038	0.016	0.039
Post *x* Percent Over 65$_c$	0.267**	0.474***	0.053	−0.243***
	0.118	0.175	0.072	0.090
Post *x* Percent Veteran$_c$	−0.127	−0.669**	0.256**	0.517***
	0.293	0.301	0.097	0.137
Post *x* Manufacturing Share$_c$	−0.105	−0.129	0.044	0.036
	0.068	0.092	0.040	0.037
Pre *x* NAFTA Exposure$_c$	−2.438*	−4.708***	−0.941	0.785
	1.245	1.527	0.578	0.779
MFA Exposure$_{ct}$	−0.147	−0.335	−0.961***	0.189
	0.233	0.448	0.194	0.174
NTR$_{ct}$	1.725	0.244	0.108	1.014
	1.299	1.264	0.772	0.763
Observations	27,661	18,836	15,505	14,212
R-squared	0.759	0.695	0.945	0.821
Estimation	OLS	OLS	OLS	OLS
Period	1992(2)2008	1992(2)2008	1992(4)2008	1992(4)2008
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	2.239	1.511	0.090	0.191
Standard error	0.832	0.828	0.364	0.361
Average democratic vote share (2000)	49	49	49	66
Impact/average * 100	4.6	3.1	0.2	0.3

Source: US Census Bureau, Dave Leip's Atlas of US Presidential Elections, and authors' calculations. Table reports difference-in-differences (DID) OLS regression results for the Democratic vote shares of the noted elections and turnout in county c in year t from 1992 to 2008, based on Eq. (3). The first covariate is the DID term of interest, which interacts a dummy for years after 2000 with the county-level NTR gap. The next seven covariates interact the post-2000 dummy with 1990 county attributes. The next covariate captures counties' exposure to NAFTA tariff reduction in the pre-PNTR period. Remaining covariates account for counties' average import tariff and exposure to the MFA in each year. The implied impact of PNTR is the product of the first DID term of interest and the weighted inter-quartile range of 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.

The closer relationship between exposure to PNTR and the Democratic vote share for House elections may be the result of their frequency, which renders Representatives less likely to adopt positions at odds with the preferences of the median voter of their districts. Conconi et al. (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 et al., 2013), any effects might be most apparent in House elections, which cover the smallest geographic area of the offices considered.

4.4. Voter turnout

A large literature examines the impact 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 (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 Eq. (3), 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.[21]

As reported in the final column of Table 2, we find no relationship between exposure to PNTR and voter turnout. This lack of a relationship is consistent with Dippel et al. (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 share of people voting relative to the pre-PNTR period.

4.5. District vs county-level analysis

In this section we discuss the relative merits of using county- versus district-level data to analyze election voting, and then compare our baseline estimates to analogous results derived from district-level data.

We use county- rather than Congressional district-level data in our baseline results because county-level data offer substantial benefits, from a measurement perspective. In particular, the stability of county borders allows voting data to be measured consistently at that level over long periods of time, including before and after periods when Congressional districts are redrawn following each decennial Census.[22] By contrast, because the boundaries of Congressional districts change every ten years, and voting data are only collected based on contemporaneous districts, using district-level data comes with one of two costs. One could consistently measure voting data at the district-level, but be limited to periods of five consecutive elections when districts are largely constant (e.g. election years 1992–2000 or 2002–2010). Or, one could construct district-level voting data that span a redistricting period using population-weighted averages of data for counties or county-district pairs. These weighted averages, however, may not accurately reflect votes in the redrawn districts if vote shares differ across portions of counties or county-district pairs that are split between multiple subsequent districts.[23] Because it is important to compare outcomes before and after the policy change in 2000, and because two-thirds of the steep decline in manufacturing employment between 2000 and the Great Recession occurs during redistricting between the November 2000 and November 2002 elections, these costs of using district-level data are substantial for our research question.[24]

In addition, with a county-level analysis, both the dependent variable—voting—and key independent variables—exposure to PNTR and demographic variables—are collected and reported at that 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.[25] When a county is split across multiple districts, however, the County Business Patterns data do 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 will 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. There are well-documented instances of this type of activity, including the purposeful redrawing of district boundaries to include three steel manufacturing plants in an Ohio Congressional district to benefit its incumbent Representative (Wang, 2011).[26]

Nevertheless, while county-level results are important for their implications 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 (consistent with Feigenbaum and Hall (2015)), Congressional elections are determined at the district- rather than the county-level. As a result, we construct district-level data spanning our sample period using information on the shares of counties' populations that are associated with Congressional Districts. We then compare results from the two different levels of aggregation.

[21] 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.

[22] We incorporate the small number of 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.

[23] Nonetheless, in the next subsection, we follow this procedure to construct synthetic district-level data and find that this district-level analysis yields qualitatively identical results to those using county-level data.

[24] The steep decline in manufacturing employment between November 2000 and November 2002 implies that using 2002 as a starting point—as is necessary with consistently measured district-level data—could miss important information if the 2002 election already reflects the effects of that decline in employment. Indeed, news reports from the time underscore that the 2002 Congressional elections were already 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 in that year (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 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."

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

[26] 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 a negative relationship between imports from China and provision of local government services, as declining property values depress property tax revenues. Dix-Carneiro et al. (2018) and Che et al. (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.

Table 3
PNTR and district-level voting for democrats.

Variables	House democratic share$_{dt}$
Post *x* NTR Gap$_d$	2.112***
	0.766
Post *x* Median HHI in 1990$_d$	0.200**
	0.088
Post *x* Percent Bachelors in 1990$_d$	6.899
	38.812
Post *x* Percent Graduate in 1990$_d$	74.014*
	38.907
Post *x* Percent Non-White in 1990$_d$	2.81
	7.152
Post *x* Percent Over 65 in 1990$_d$	−1.117
	18.572
Post *x* Percent Veteran in 1990$_d$	−6.862
	48.812
Post *x* Manufacturing Share$_d$	−0.524**
	0.258
Pre *x* NAFTA Exposure$_d$	−7.743*
	3.985
MFA Exposure$_{dt}$	−0.469
	0.486
NTR$_{dt}$	6.153
	5.291
Observations	3847
R-Squared	0.7754
Estimation	OLS
Period	1992(2)2008
FE	d,t
Weighting	1992 Pop.
Clustering	State
Implied impact of PNTR	7.03
Standard error	2.56
Average democratic vote share (2000)	48.6
Impact/average * 100	14.5

Source: US Census Bureau, Dave Leip's Atlas of US Presidential Elections, and authors' calculations. Table reports difference-in-differences (DID) OLS regression results for the Democratic vote shares for House district d in year t from 1992 to 2008, based on Eq. (3). The first covariate is the DID term of interest, which interacts a dummy for years after 2000 with the district-level NTR gap. The next seven covariates interact the post-2000 dummy with 1990 county attributes. The next covariate captures counties' exposure to NAFTA tariff reduction in the pre-PNTR period. Remaining covariates account for districts' average import tariff and exposure to the MFA in each year. The implied impact of PNTR is the product of the first DID term of interest and the weighted inter-quartile range of 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.

Specifically, for each county, we calculate the portion of its population that is located in each Congressional district as of the 1992 election. For each subsequent Congressional election, we use these shares to attribute the number of votes cast for Democrats and the total number of votes cast to each 1992 Congressional district. Summing these votes by 1992 districts allows us to calculate the Democratic vote share for elections from 1992 to 2008 based on that single set of districts.[27] As discussed above, however, the accuracy of these district-level vote shares will depend on the extent to which county-level averages represent the portions of counties that map to different districts over time.

With these constructed synthetic district-level data, we then re-estimate Eq. (3) for House of Representatives elections. Results are reported in Table 3. As indicated in the table, we continue to find a positive and statistically significant relationship between exposure to PNTR and the share of votes cast for Democrats in House of Representatives elections, as in the county-level data. Coefficient estimates for the control variables are also highly similar in sign and significance to those based on county-level data (first column of Table 2), with the exceptions being that the coefficient for the share of the population over 65 loses statistical significance, and that counties with higher initial shares of employment in manufacturing experience relative reductions in

[27] This approach is similar to that used to construct county-district-level data spanning a redistricting period in Autor et al. (2020). Because we aggregate to the district-level, rather than the county-district-level, our approach does not require Census Block-level population information, which Autor et al. (2020) use to calculate the shares of county-district-pairs matched to new Congressional districts.

Table 4
Robustness checks for house of representatives results.

Variables	(1) House democratic share$_{ct}$	(2) House democratic share$_{ct}$	(3) House democratic share$_{ct}$	(4) House democratic share$_{ct}$	(5) House democratic share$_{ct}$	(6) House democratic share$_{ct}$
Post *x* 1{High NTR Gap$_c$}		3.861**				
		1.712				
Post *x* NTR Gap$_c$	0.561***		0.537**	0.301**	0.601***	−0.141
	0.208		0.212	0.134	0.212	0.123
Post *x* Median HHI in 1990$_c$	0.207***	0.129	0.206***	0.212***	0.200***	
	0.058	0.090	0.057	0.058	0.06	
Post *x* Percent Bachelors in 1990$_c$	0.094	0.307	0.086	0.113	0.09	
	0.171	0.276	0.171	0.174	0.168	
Post *x* Percent Graduate in 1990$_c$	0.440***	0.305	0.443***	0.446***	0.453***	
	0.163	0.254	0.164	0.164	0.168	
Post *x* Percent Non-White in 1990$_c$	0.074	0.009	0.073	0.078	0.075	
	0.051	0.072	0.051	0.05	0.052	
Post *x* Percent Over 65 in 1990$_c$	0.267**	0.118	0.266**	0.279**	0.262**	
	0.118	0.154	0.118	0.12	0.114	
Post *x* Percent Veteran in 1990$_c$	−0.127	−0.062	−0.134	−0.131	−0.108	
	0.293	0.365	0.293	0.296	0.298	
Post *x* Manufacturing share$_c$	−0.105	−0.039	−0.112		−0.127*	
	0.068	0.077	0.067		0.068	
Pre *x* NAFTA Exposure$_c$	−2.438*	−0.184	−2.403*	−2.161*	−2.440*	
	1.245	1.033	1.24	1.243	1.269	
MFA Exposure$_{ct}$	−0.147	−0.075	−0.143	−0.097	−0.167	
	0.233	0.224	0.234	0.234	0.233	
NTR$_{ct}$	1.725	2.362*		1.868	1.821	
	1.299	1.203		1.300	1.302	
Observations	27,661	20,275	27,661	27,661	27,661	27,661
R-squared	0.759	0.718	0.759	0.759	0.758	0.750
Estimation	OLS	OLS	OLS	OLS	OLS	OLS
Period	1992(2)2008	1992(2)2008	1992(2)2008	1992(2)2008	1992(2)2008	1992(2)2008
FE	c,t	c,t	c,t	c,t	c,t	c,t
Weighting	1992 Pop.	1992 Pop.	1992 Pop.	1992 Pop.	2000 pop.	1992 Pop.
Clustering	State	State	State	State	State	State

Notes: Table reports difference-in-differences (DID) OLS regression results for the Democrat vote shares for House elections in county c in year t from 1992 to 2008. Column 1 repeats the baseline results from Table 2, column 1. Relative to the baseline, column 2 includes only counties in top and bottom quartiles of exposure to PNTR and replaces NTR gap in the DID term with an indicator for 1 if a county's NTR gap is in the top quartile. Column 3 excludes the NTR rate. Column 4 excludes the interaction of the manufacturing employment share with the post-PNTR indicator. Column 5 weights by 2000 population instead of 1992 population. Column 6 includes only the difference-in-differences term and fixed effects. 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.

the share of votes cast for Democrats in the 2000s. The strong similarities of the results based on county- and district-level data indicate that our baseline findings are not driven by the use of county-level data as a level of aggregation.

5. Robustness of house results to alternate specifications

This section examines the robustness of our baseline results for House of Representative elections to alternate specifications. To assist comparison, column 1 of Table 4 repeats the baseline findings from column 1 of Table 2.

5.1. Discrete exposure

The first robustness check considers an alternative measure of exposure to PNTR. As discussed in Section 3.1, the county-level NTR gap is continuous, with counties experiencing varying levels of exposure to PNTR, as opposed to the binary “treatment” and “control” groups in the canonical difference-in-differences approach. As an alternative, we consider only counties in the top and bottom quartiles of the population-weighted NTR gap distribution, define a binary variable that takes the value one for counties in the top quartile of exposure (and zero for those in the lowest quartile), and interact that binary variable with the post-PNTR indicator. We then include this alternate difference-in-differences term in place of the continuous version in Eq. (3). Note that this specification still compares counties with differing levels of exposure, but the comparison of the most- to the least- exposed counties via a binary variable is closer in spirit to the traditional difference-in-differences approach. As indicated in column 2 of Table 4, we continue to find that higher exposure to PNTR is associated with relative increases in the share of votes cast for Democrats.

5.2. Excluding NTR_{ct} and counties' manufacturing shares

In the next two robustness checks, we consider the relevance of two specific covariates, the NTR tariff rate and the manufacturing employment share. As noted in Eq. (3), the NTR tariff rate appears both in the calculation of the NTR gap and as a separate covariate. Including the NTR rate as a covariate allows for the possibility that standard NTR tariffs, and their changes over time, might have effects separate from the effect of the NTR gap, which measures how much tariffs could have increased before PNTR. We explore the importance of the NTR tariff rate covariate by re-estimating Eq. (3) but excluding the NTR tariff rate. As shown in column 3 of Table 4, excluding the NTR tariff rate yields a coefficient estimate for the main difference-in-differences term that is qualitatively identical to that in the baseline results (column 1).

As discussed in Section 3.1, the NTR gap is only greater than zero for industries in the Harmonized Tariff Schedule. Because these industries consist primarily of manufacturing industries—along with some agriculture and mining industries—there is a correlation of 0.88 between the county-level manufacturing employment share and the NTR gap. The initial manufacturing employment share is an important covariate that lets us isolate exposure to PNTR even conditional on an area's general level of industrialization and determine how the relationship between industrialization and voting may change after 2000 (given the interaction with the *Post* dummy). However, to examine whether inclusion of this covariate renders our results unduly sensitive to the high correlation, we estimate a version of Eq. (3) that excludes the manufacturing employment share. As shown in column 4 of Table 4, we continue to find a positive and statistically significant relationship between exposure to PNTR and the Democratic vote share, though somewhat smaller in magnitude.

5.3. Weighting by 2000 population

In our baseline empirical approach, we weight observations based on start-of-sample county-level population to avoid any endogenous response in that variable around the time of the policy change. As an additional robustness check, we instead weight based on population in 2000, the year of the policy change. As indicated in column 5 of Table 4, this alternative weighting procedure yields results that are qualitatively identical to the baseline results shown in Column 1.

5.4. Excluding county attributes

Including county fixed effects and controls of *ex ante* county attributes interacted with the *Post* dummy means that our main estimates of interest are conditional on these variables. County fixed effects control for any time-invariant attributes of counties that may influence voting, thereby increasing comparability of “more” versus “less” exposed counties. Interactions of specific *ex ante* county attributes with the *Post* dummy help ensure that any break in trend picked up by the DID coefficient of interest is independent of breaks in trend associated with these attributes, allowing the data to speak in a horse-race among the various covariates. The statistical significance of the latter in the baseline results in column 1 highlights their relevance. There, we find that counties with initially higher incomes, that are more educated, or have larger shares of the population over 65 shift toward voting for Democrats in the 2000s, relative to the 1990s.

As indicated in Column 6 of Table 4, the main DID term of interest is statistically insignificant when county-level control variables are excluded. This outcome is not surprising given that exclusion of relevant statistically significant covariates can lead to biased estimates and, in this case, can obscure the effect of PNTR versus other forces governing voting.

5.5. Excluding county fixed effects

In our baseline specification (Eq. (3)), we include county fixed effects, which capture any time-invariant characteristics of counties, absorb the time-invariant NTR gap term in levels, and yield within-county estimates of the relationship between PNTR and the Democratic vote share. An alternative approach is to estimate a specification in which we exclude county fixed effects, include the NTR gap term in levels, and also include the other covariates both in levels and interacted with the post dummy. We report the results of this specification in Table A.3 in Appendix Section C. As noted there, the result for the DID term of interest is very similar to that in the baseline, continuing to indicate that counties more exposed to PNTR experience relative increases in the share of votes cast for Democrats.

6. Extension to 2016

Researchers and commentators have noted that, over the last decade, Republican candidates simultaneously gained support in industrial areas while becoming more opposed to international trade (Mutz, 2017; Davis and Chinni, 2018). To examine this perception, we extend our analysis through 2016 and adopt a flexible generalized difference-in-differences approach that allows the relationship between exposure to PNTR and voting to vary from election year to election year. In particular, we estimate the following equation:

$$\text{Dem Share}_{ct} \;=\; \sum_t \theta_t \; 1\{\text{year} = t\} \;\times\; \text{NTR Gap}_c + \sum_t \gamma_t \; 1\{\text{year} = t\} \;\times\; \mathbf{X}_c + \mathbf{Z}'_{ct}\beta + \delta_c + \delta_t + \varepsilon_{ct}. \tag{4}$$

Here, 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 Eq. (4) 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 estimating equation. The next set of terms, $\mathbf{Z}_{ct}$, again 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. δ_c and δ_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. We again weight by 1992 county population and cluster standard errors by state.

We summarize the results of estimating Eq. (4) in Fig. 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 estimate 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.

Fig. 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. Importantly, this lack of a relationship between exposure to PNTR and the Democratic vote share in the 1990s also provides support for the parallel trends assumption inherent in the difference-in-differences approach. 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 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. We caution, however, that as shown in Fig. 4, the shift in coefficients from the 2000s to the 2010s is subtle and imprecisely estimated.

As will be discussed in more detail in Section 7, 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 Democratic vote share, Democratic House members were substantially more likely to vote to restrict trade than their Republican counterparts. As discussed further in Section 7, Democrats began taking these strongly anti-trade positions following the election of Republican George W. Bush to the Presidency in 2000. Democrats, as a result, established themselves as the anti-trade party in 2001 and 2002, just as the relationship between PNTR and manufacturing employment began to be realized. As indicated in Fig. 4, support for Democrats then begins to shift up noticeably in the 2002 election.

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 than either Democrats or non-Tea Party Republicans to view China as an adversary and place a high priority on getting tougher on China with respect to trade.

Indeed, we examine the relationship between exposure to PNTR and the rise of the Tea Party and find evidence for a positive relationship with some aspects of Tea Party activity. In particular, we conduct a district-level analysis for House of Representatives elections in which we regress one of three measures of Tea Party activity—a survey favorability measure, the number of Tea Party activists, and an indicator for a Tea Party candidate winning an election—on exposure to PNTR and demographic control variables. We use data from 2010, which is the year associated with the rise of the Tea Party and the only year for which the relevant data are available.[28] Results are reported in Table 5.

As indicated in the Table, we find a positive, albeit marginally statistically significant relationship between exposure to PNTR and two measures of Tea Party activity—the favorability rating of the Tea Party and the number of Tea Party activists. We find no relationship between exposure to PNTR and the probability that a Tea Party candidate ultimately wins an election. These results

[28] Data on favorability and the number of Tea Party activists are from Madestam et al. (2013) and data on whether a Tea Party candidate won an election are from The New York Times (http://archive.nytimes.com/www.nytimes.com/interactive/2010/11/04/us/politics/tea-party-results.html).

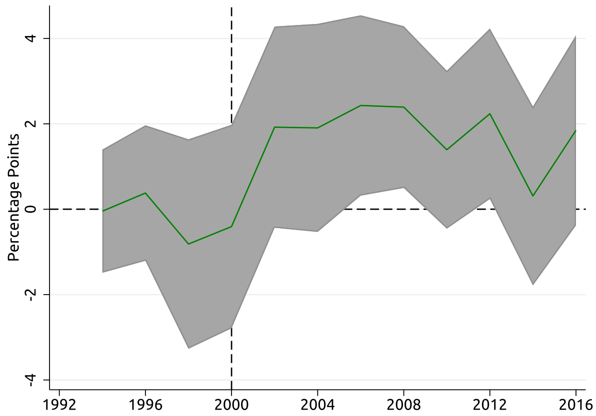

Fig. 4. Implied impact of PNTR: U.S. House of Representatives. Source: US Census Bureau, Dave Leip's Atlas of US Presidential Elections, and authors' calculations. Figure displays the impact of PNTR on the Democratic vote share implied by estimation of the county-year-level OLS difference-in-differences (DID) specification described in Eq. (4). 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.

Table 5
PNTR and tea party activity.

Variables	Number of tea party activists	Tea party favorability	Tea party candidate wins
NTR Gap$_d$	0.014*	0.013*	−0.008
	0.007	0.007	0.007
Median HHI in 1990$_d$	0.001	0.011***	−0.001
	0.003	0.003	0.002
Percent Bachelors in 1990$_d$	0.011	−0.011	−0.018
	0.012	0.012	0.012
Percent Graduate in 1990$_d$	−0.003	−0.064***	0.030**
	0.015	0.012	0.015
Percent Non-White in 1990$_d$	−0.007***	−0.010***	0.003**
	0.002	0.001	0.001
Percent Over 65 in 1990$_d$	−0.064***	−0.014*	−0.004
	0.006	0.007	0.008
Percent Veteran in 1990$_d$	0.157***	0.055***	−0.0002
	0.016	0.011	0.010
Observations	432	433	433

Notes: Table displays results of a district-level, OLS cross-sectional regression for the 2010 House of Representatives elections. Dependent variables include the number of tea party activists, the favorability of the Tea Party, and an indicator for whether a Tea Party candidate wins an election. 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.

draw a connection between exposure to import competition via PNTR and some aspects of the anti-trade Tea Party wing of the Republican Party.[29]

This new class of Republicans may have provided voters seeking to elect anti-trade politicians with an alternative to Democrats, explaining the attenuation of the boost for Democrats observed toward the end of the sample in Fig. 4.[30] This trend continued with the 2016 election of Donald Trump, who adopted several high-profile rounds of tariff increases, particularly against China. In the next section, we show that in the 2010s, Republicans become at least as anti-trade as Democrats.[31]

[29] Furthermore, they are consistent with Feigenbaum and Hall (2015)'s finding that incumbent Representatives in trade-exposed districts were able to insulate themselves from challengers by adopting anti-trade positions.

[30] 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.

[31] It is difficult to determine whether Tea Party districts are responsible for the protectionist turn of the Republican party because general hostility to trade in Congress led to only a small number of bills being considered from 2010 forward. Furthermore, these bills were largely limited to uncontroversial matters with bipartisan support such as continuing AGOA trade preferences for Sub-Saharan African countries.

7. Party affiliation and legislator voting behavior

The previous section establishes that voters in counties facing larger increases in import competition from China are more likely to vote for Democratic House candidates in the early 2000s, relative to the 1990s. One explanation for this change in voting patterns is that residents of these counties shifted their votes 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 during which time period.

7.1. 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".[32] A key feature of this classification system is that it covers our entire sample period, extending through 2016. 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.[33] Appendix Table A.4 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.4—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 D, results are similar when all bills are included.[34] Lastly, House members' votes in Congresses seated following Congressional elections from 1992 to 2014 are obtained from Govtrack.

7.2. Identification strategy

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

$$\mathrm{Pro}-\mathrm{Trade}_{\mathrm{dh}}=\alpha+\beta\mathrm{Democrat}_{\mathrm{dh}}+\varepsilon_{\mathrm{dh}}, \tag{5}$$

where d and h denote Congressional districts and the particular two-year Congress during which representatives serve.[35] The dependent variable $Pro\text{-}Trade_{dh}$ 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.

We consider the relationship between party affiliation and support for trade votes separately for three 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, i.e., 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. Splitting the sample at different time periods would involve either making strong assumptions to bridge districts across redistricting events or mixing districts that may not be comparable.[36]

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.[37] The intuition behind this design relates to the incomplete manipulability of elections. For example, exogenous

[32] Information on Comparative Agenda's classifications is available at https://www.comparativeagendas.net/pages/master-codebook. We add two bills that are clearly trade-related but do not appear in Comparative Agendas' list. These bills are HJRES121 in 1998 (105th Congress) and HJRES57 in 1999 (106th Congress). There is extensive overlap between the bills covered in Comparative Agendas and those from the Cato Institute employed by Feigenbaum and Hall (2015). For the 112th Congress, for example, both lists include bills covering implantation of the Colombia, Panama, and Korea FTAs, the application of CVD laws to non-market economies, and repeal of the Jackson-Vanik annual reviews of NTR status for Moldova and Russia.

[33] 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).

[34] 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 et al., 2016).

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

[36] In Appendix section J, we also discuss results that split the sample periods to correspond with Presidential elections. The general shifts in legislative voting on trade that we find when separating periods by Presidencies are broadly similar to our baseline results.

[37] Lee et al. (2004) and Lee (2008) use RD to investigate the effect of party affiliation on legislators' right-vs-left voting scores.

variation in factors such as weather influences turnout and therefore the ultimate share of votes each candidate 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.[38]

Formally, define the assignment variable

$$\text{Margin}_{\text{dh}} \equiv \text{VoteShare}_{\text{dh}}^{\text{Democratic}} - \text{VoteShare}_{\text{dh}}^{\text{Republican}} \tag{6}$$

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.[39] Hahn et al. (2001) show that when $E[\varepsilon_{\text{dh}}|\text{Margin}_{\text{dh}} = m]$ is continuous in m at the cutoff 0, β in Eq. (5) can be identified as

$$\hat{\beta}_{\text{RD}} = \frac{\lim_{m\downarrow 0} E[y_{\text{dh}}|\text{Margin}_{\text{dh}} = m] - \lim_{m\uparrow 0} E[y_{\text{dh}}|\text{Margin}_{\text{dh}} = m]}{\lim_{m\downarrow 0} E[\text{Democrat}_{\text{dh}}|\text{Margin}_{\text{dh}} = m] - \lim_{m\uparrow 0} E[\text{Democrat}_{\text{dh}}|\text{Margin}_{\text{dh}} = m]}. \tag{7}$$

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

$$\text{Democrat}_{\text{dh}} = \gamma I\{\text{Margin}_{\text{dh}} \geq 0\} + g(\text{Margin}_{\text{dh}}) + \mu_{\text{dh}}, \tag{8}$$

and the second stage is

$$y_{\text{dh}} = \alpha + \beta \text{Democrat}_{\text{dh}} + f(\text{Margin}_{\text{dh}}) + \varepsilon_{\text{dh}}. \tag{9}$$

$I\{.\}$ 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. Lee and Lemieux (2010) suggest both parametric and nonparametric approaches to estimate $\hat{\beta}_{\text{RD}}$, 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 Appendix Sections F and G.

As discussed above, the identifying assumption of our regression discontinuity estimation, which is that $E[\varepsilon_{\text{dh}}|\text{Margin}_{\text{dh}} = m]$ 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 A.3, 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.3 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.

Lastly, we caution that our RD estimates represent weighted average treatment effects, with the weights being proportional to the *ex-ante* likelihood that a representative's realization of the assignment variable is close to the threshold, i.e. comes from a district with an expected close election. Therefore, if the behavior of representatives facing close elections is different from the general population of representatives, our RD estimates may not capture the overall voting behavior of the party on trade bills.[40]

[38] 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.”

[39] See Appendix Figure A.2 for a visual representation of this discontinuity. 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, $\Pr[\text{Democratic}_{d,t} = 1|\text{Margin}_{d,t} = m] \neq 1$ when $m > 0$.

[40] In Appendix Section K, we also report results of an OLS regression of the pro-trade vote share on a Democrat vote dummy for the three periods we consider in the RD analysis. Results are broadly consistent with those found with the RD approach. We find that Democrats were modestly more protectionist than Republicans from 1992–2000, become much more protectionist from 2002–2010, and then shift to being relatively more pro-trade than Republicans from 2012–2014.

Table 6
Democrat affiliation and legislators' voting for pro-trade bills.

	(1)	(2)	(3)
	1992–2000	2002–2010	2012–2014
Panel A: Parametric approach			
Democrat	0.026	−0.298***	−0.082
	(0.035)	(0.048)	(0.091)
Stock-Yogo	87	85	NA
Kleibergen-Papp	410	265	NA
Observations	2174	1738	433
Panel B: Nonparametric, local linear approach			
Democrat	0.045*	−0.326***	−0.025
	(0.026)	(0.032)	(0.058)
Band	0.45	0.57	0.47
Observations	1576	1406	313

Notes: Table summarizes the results of district-year level regression discontinuity specifications of the share of pro-trade votes on an indicator for whether the representative is a Democrat. Column headers refer to the years in which the representatives are elected (their two-year service begins in January of the following years). Panel A reports results using parametric estimation with third-order polynomials. Panel B reports results using nonparametric local linear estimation, in which 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.

7.3. Results

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 6.[41] As mentioned above, we use both parametric and non-parametric results, reported in Panels A and B, respectively. 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 on trade-related bills, 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 are much more anti-trade than Republicans in their legislative voting, 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.[42] 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, 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 similarly on trade-related bills, though we caution that this period contains relatively few bills—as illustrated in Appendix Table A.4—due to both parties' hostility toward trade during this time.[43]

We next consider aspects of the nature and timing of changes in the two parties' voting on trade-related legislation. One issue to consider is why Democrats would become anti-trade relative to Republicans in the 2000s, when the two parties had voted similarly on trade-related bills in the 1990s. Two potential explanations include, first, that a party's control of the Presidency influences voting on trade-related bills and, second, that districts initially represented by Democrats were more exposed to PNTR, leading them to recognize the potential impact of trade shocks and change their views on trade bills.[44]

We find evidence that party control of the Presidency contributes to the shifts by the parties from the 1990s to 2000s. Appendix Table A.6 indicates a sharp break in representatives' votes on trade-related bills following the change from Democratic President Bill Clinton to Republican President George W. Bush. As shown in that table, the pro-trade share of votes cast by Democratic Representatives drops from 63 percent in the Congress elected in 1998 (the last Congress of Bill Clinton's presidency) to 40 percent in the Congress elected in 2000 (the first Congress of the G.W. Bush presidency). The Republican pro-trade vote share increases from 72 percent to 80 percent over the same time period. The clear timing of this shift indicates that the Presidential election mattered to Representatives' voting on trade bills.

We also find that areas that were more Democratic in 1992 were somewhat more exposed to PNTR (correlation of 0.12), suggesting that Democrats may have acquired information about the impact of trade shocks in the 1990s. This relationship is gone by 2000, when PNTR is enacted: The correlation between the Democratic vote share and the NTR gap in 2000 is 0.02, and a

[41] A visual representation of the regression discontinuity results is provided in SectionF of the Appendix.
[42] Lu et al. (2018) provide another mechanism, in which areas subject to larger increases in import competition from China see more negative media coverage of China, which may increase the salience of the negative aspects of trade.
[43] We obtain qualitatively identical results using the bias-corrected estimator from Calonico et al. (2014). In Appendix section J, we present and discuss results in which the cutoffs between periods are based on Presidential elections. As discussed in that section, we continue to find broadly similar shifts in the parties' views on trade using these alternate cutoffs.
[44] Bombardini et al. (2020) consider politicians' expectations of the implications of the "China Shock," and the extent of information available to them at the time.

Table 7
The impact of democrat affiliation on trade bill voting by high and low exposure.

	(1)	(2)	(3)
	Full sample	High exposure	Low exposure
Panel A: 1992–2000 (nonparametric, local linear approach)			
Democrat	0.045*	0.067**	−0.022
	(0.026)	(0.032)	(0.039)
Stock-Yogo	16	16	NA
Kleibergen-Papp	627	438	NA
Observations	1576	948	679
Panel B: 2002–2010 (nonparametric, local linear approach)			
Democrat	−0.326***	−0.205***	−0.402***
	(0.032)	(0.061)	(0.061)
Stock-Yogo	16	NA	16
Kleibergen-Papp	222	NA	180
Observations	1406	358	525
Panel C: 2012–2014 (nonparametric, local linear approach)			
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 district-year level regression discontinuity specifications of the share of pro-trade votes on an indicator for whether the representative is a Democrat. Panel titles refer to the years in which the representatives are elected (their two-year service begins in January of the following years). Columns 1, 2 and 3 report results for the full sample and for districts with high and low PNTR exposure, respectively, 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 to be within the regression-specific optimal bandwidth, with the result that the number of observations for the full sample can be smaller or larger than the sum of the high- and low-exposure sub-samples. Standard errors clustered at the assignment variable level are reported below coefficients. *, ** and *** signify statistical significance at the 10, 5 and 1 percent level.

regression of the Democratic vote share on the NTR gap in that year yields a coefficient that is not statistically significant.[45] Providing a fuller examination of the reasons for changes in the parties' views on trade over time would be a fruitful avenue for future research.

Another issue to consider is why Republicans did not *immediately* adopt protectionist positions in the early 2000s if those positions were benefiting Democrats. We discuss two potential reasons for this delay in adopting protectionist positions in Section H of the Appendix. First, Republican representatives may have felt pressured to support the pro-trade positions of the Republican George W. Bush administration, consistent with the shifts in parties' positions across Presidential administrations, as discussed immediately above. Second, we find some limited evidence—albeit imprecisely estimated—that Democratic gains associated with PNTR were modestly larger in "safe" Democratic districts, where the party typically won by large margins. Gains in these districts may have been of less concern to Republicans given that they would not lead to changes in the number of districts represented by Republicans (Feigenbaum and Hall, 2015).[46]

To provide additional perspective on these results, we examine whether the evolution in the voting of Democratic 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 7, 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.[47] In the last period, from 2012 to 2014, Democratic and Republican legislators vote similarly on trade-related bills in both low- and high-exposure districts.[48]

[45] The decline in correlation from 1992 to 2000 could occur endogenously if pro-trade positions by some Democrats in the 1990s led the party to be punished by voters.

[46] Further examination of these and other reasons for the delay in Republican adoption of protectionist policies is another topic deserving of future research.

[47] 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). Furthermore, it helps explain the smaller difference between Democrats' and Republicans' positions in high-exposure districts, relative to low-exposure districts in the 2000s, as reported in Panel B.

[48] In Appendix Section I, we also estimate ordinary least squares regressions that examine the relationship between the share of pro-trade votes cast by legislators and the exposure of their district to PNTR, an indicator for Democratic affiliation, and interaction of the NTR gap and Democrat terms. We find that higher exposure to PNTR is associated with more anti-trade views across parties in the 1990s and early 2000s and that Democrats are especially anti-trade in the early 2000s.

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 liberalization. After passage of PNTR, from 2002 to 2010, Democrats become much more likely than Republicans to vote against pro-trade bills, and voters in counties exposed to PNTR's trade liberalization shift their votes toward Democrats. Finally, for Congresses elected in 2012 and 2014, Democrats and Republicans again vote similarly on trade-related bills, and the boost enjoyed by Democrats in the first decade of the 2000s disappears.

8. 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 first decade of the 2000s, relative to the 1990s, and that this shift is present in both county- and constructed district-level data. 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 Democratic vote share in House elections of 2.2 percentage points, or a 4.6 percent increase relative to the average share of votes cast for Democrats in the 2000 Congressional elections. This relationship is robust to alternate specifications, excluding the NTR tariff rate or manuacturing employment share, and alternative weighting. We also show that this shift in voting toward Democrats unwinds in the 2010s, concomitant with the rise of the Tea Party faction of the Republican Party, though this change in the 2010s is not precisely estimated. Related results indicate that exposure to PNTR is associated with some aspects of Tea Party activity.

In the second portion of the paper, we find evidence that the relationship between exposure to trade liberalization and voting can be explained by 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 election 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, though the set of trade bills considered in this period is small. All told, our results are consistent with voters in trade-exposed areas shifting support toward the party that advocates for trade policies consistent with their economic interests.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jinteco.2022.103652.

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