

# Free Trade and American Politics: How Policy Preferences Respond to Local Trade Shocks

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## Abstract

What effect does trade have on American politics? Until recently, the answer to this question was “not much”. Trade has been viewed as a low-salience issue since the 1970s – its once-important status among the policy dimensions superseded by issues such as abortion, gay rights, and gun control. However, recent research and the election of Donald Trump has thrust trade back into the spotlight. In this paper, I document a causal relationship between localized trade shocks and individual policy preferences, highlighting the importance of sociotropic concerns at the local level. In addition, I show that policy preferences on free trade are bundled with opinions on immigrants and US global leadership, assembling to create a nativist response to trade-related economic shocks. These findings further our understanding of how free trade’s unequal economic consequences explain variation in the political economy of trade.

FREE TRADE | POLITICAL ECONOMY | AMERICAN POLITICS | DONALD TRUMP | SOCIOTROPISM

6,946 words

# Introduction

Research on the political economy of free trade has traditionally used cross-country analysis and assumed that individual citizens behave as though they are economic factors of production. Trade's losers (typically unskilled manufacturing labor in the context of the United States) want greater protection while trade's winners (typically skilled labor) prefer open borders. These assumptions, while broadly consistent with cross-national empirics, are challenged by public opinion surveys that reveal weak or inconsistent relationships between trade preferences and economic producer identities.

I contribute to this puzzle in two ways. First, I argue that trade's negative economic consequences are geographically concentrated and thus spill over to affect nearby individuals who are not directly affected by trade shocks. This "sociotropic" response explains the inconsistent relationship between economic theory and public opinion. Second, I argue that these geographically concentrated individuals update not only their trade policy preferences but also along two related policy dimensions that I characterize as "nativist" or anti-foreign: policies dealing with immigration and policies dealing with globalization.

This paper speaks most directly to a recent strand of economic and political economy research which suggests that trade influences the electoral fortunes of political candidates (Jensen, Quinn and Weymouth, 2017; Margalit, 2011) and that these electoral effects translate to political outcomes in the form of increased elite polarization (Autor et al., 2016*a*) and more protectionist representatives (Feigenbaum and Hall, 2015). Looking in Europe, Dippel, Gold and Heblisch (2015) finds that similar trade shocks increase vote shares for far-right parties whose platforms combine protectionist policies with racism and nativism.

The existing literature describes a causal pathway connecting trade's economic outcomes with political outcomes. However, existing research has yet to document the antecedent link in the causal chain: the impact of trade on individuals. In theory, electoral fortunes are

won or lost – and policy platforms adopted or abandoned – based on the preferences of the voters. But do American voters really care about trade? And if so, do they only care in a narrowly defined policy preference sense or does trade elicit a broader bundle of nativist reactions?

Existing research on individual trade opinion is mainly concerned with testing competing economic models of who wins and who loses from trade (Mayda and Rodrik, 2005; Scheve and Slaughter, 2001), competing economic identities (Baker, 2005; Naoi and Kume, 2015), and the influence of non-economic characteristics such as perceived cultural threat (Margalit, 2012), risk acceptance and education (Ehrlich and Maestas, 2010), and inequity aversion (Lü, Scheve and Slaughter, 2012). With a few survey experimental exceptions (see Naoi and Kume (2015)), this literature is not able to characterize how individuals react to changes in their economic and social welfare that are due to trade in a causal sense.

Yet there is a growing literature on the localized externalities of trade shocks, suggesting that “sociotropic” considerations may be crucial to understanding how individuals respond to trade shocks. The concept of sociotropism was popularized by Mansfield and Mutz (2009) who argue that opinions on trade are only weakly related to an individual’s producer identity but strongly related to their perception of how the overall US economy is affected by trade. I extend this result to the localized impact of trade shocks and argue that an individual’s view of the broader US economy is a function of what (s)he sees in their back yard (Alkon, 2017).

Given the geographic agglomeration of industry, it should not be surprising that trade’s negative outcomes are highly concentrated in certain parts of the United States. These localized consequences extend beyond simple economic measures of unemployment and income (Autor, Dorn and Hanson, 2013) to include social malaise such as increased divorce rates (Autor et al., 2017) and mortality due to drug overdoses, alcohol abuse, and suicide (Case and Deaton, 2017). Even for those individuals who are not directly affected by trade-related

job or income loss, the view out their window is of trade's losers.

In the pages below, I make two contributions. First, I document a causally identified relationship between trade-related layoffs and anti-free trade policy preferences that is consistent with a localized sociotropic response. Second, I present evidence suggesting that these individual preferences are packaged with a broader bundle of positions on immigration and US global leadership.

## 1 Empirical Context

The salience of trade in the American political system ebbs and flows. For most of the country's first 150 years, where one stood on "the tariff" determined not just one's occupation but also one's party affiliation (Bauer, Pool and Dexter, 1967). Until the Great Depression, the alignment of Republicans with business and Democrats with labor made the former protectionist and the latter internationalist, reflecting the areas of comparative advantage of the time. But by the 1970s, the changing global economy swapped the identities of trade's winners and losers in the United States. The business coalition undergirding Republican political support recognized that open global markets gave them more opportunities to sell. Meanwhile the labor unions supporting the Democrats saw that unskilled American labor was increasingly exposed to import competition. As their core supporters changed their positions on free trade, the political parties followed suite (Karol, 2009).

The positions of Democrats and Republicans had proved durable until the start of the 21st century, even in spite of the pro-free trade positions of presidents from both sides of the aisle. In particular, Clinton's support of the North American Free Trade Agreement (NAFTA) in 1993 paved the way for Obama's promotion of the Trans-Pacific Partnership (TPP) in the early 2010s, both instances of Democratic presidents going against the majority of their co-partisan legislators. While such patterns may merely reflect the greater insulation

of presidents from organized interests (Bailey, Goldstein and Weingast, 1997), the platform of the ostensibly Republican presidential candidate Donald Trump represents a discontinuous shift in these durable positions. Donald Trump's 2016 campaign characterized his opponent Hilary Clinton as another member of an out-of-touch Democratic Party elite who had turned its back on the American worker, capitalizing on both the cumulative pain of trade's localized economic effects and as well the salience of economic problems following the Great Recession (see Figure 1).

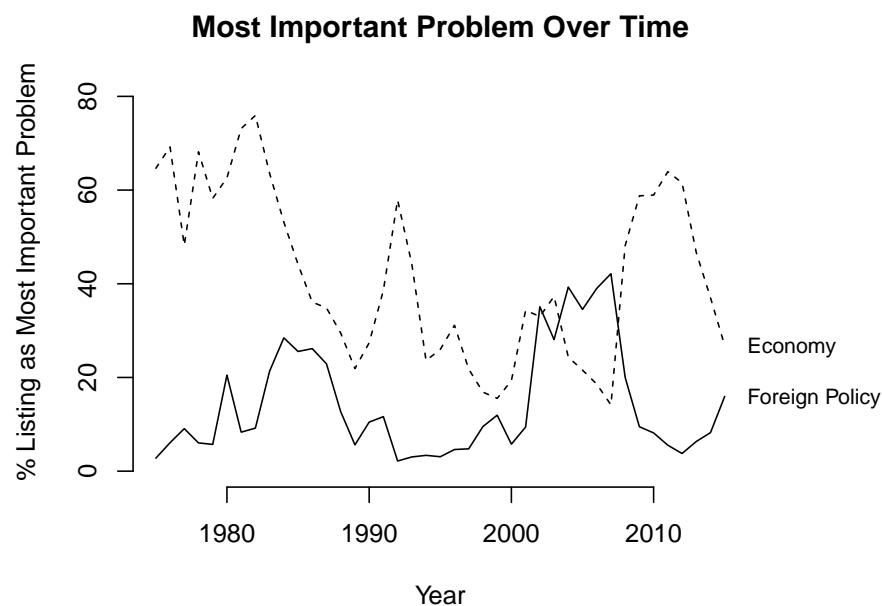


Figure 1: Percent of respondents (y-axis) listing either the economy (solid line) or foreign policy (dashed line) as the most important problem since 1975. *Source: Most Important Problem Dataset provided by Colton Heffington, Brandon Beomseob Park and Laron K. Williams (University of Missouri)*

Occurring at the same time as this partisan shift was the emergence of China as a major player in the global economy. China's rise began in the 1980s under the liberal policy reforms of Deng Xiaoping that unlocked the productive potential of the country's massive workforce. But the full impact of China on global trade wasn't realized until its ascension to the WTO in 2001. Between 1990 and 2010, US imports from China have grown almost 35-fold (see

Figure 2), mainly in manufacturing, putting pressure on import-competing sectors of the US economy.

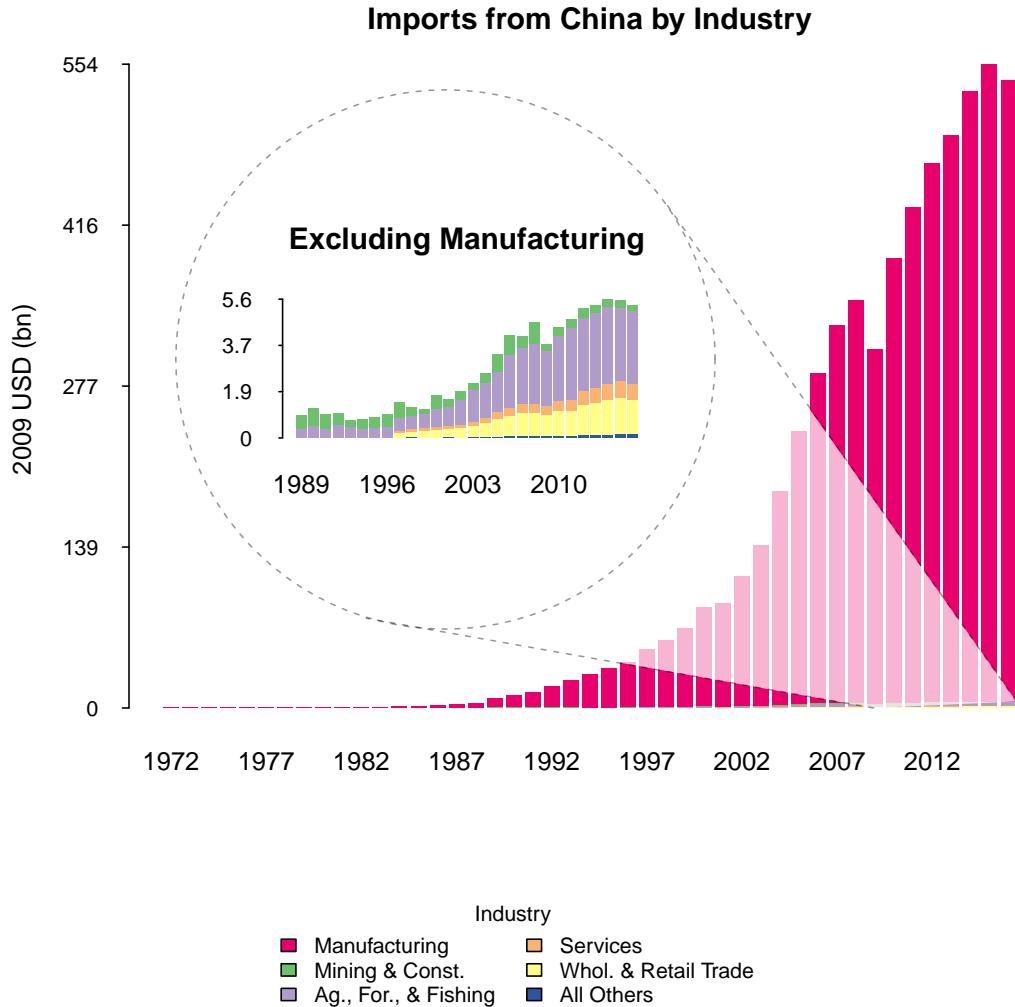


Figure 2: US imports from China by industry over time. Inset focuses only on non-manufacturing industries for which data is available from 1989.

China's rapid economic development has translated into an increasingly competitive labor market for unskilled and semi-skilled workers in the United States. Import competition in trade-exposed industries has caused significant increases in unemployment (Autor, Dorn and Hanson, 2013), along with a host of other labor market maladies (Autor et al., 2014;

Pierce and Schott, 2016*a*). Regional industry agglomeration has meant that these shocks disproportionately affect certain geographic areas. Negative spill-overs into broader social outcomes include declining marriage rates (Autor et al., 2017), declining life expectancy (Case and Deaton, 2017; Pierce and Schott, 2016*b*), and increasing inequality (Pierce and Schott, 2016*a*).

The social costs are also reflected in political outcomes, including increasing elite polarization (Autor et al., 2016*a*) and, in Germany, growing support for far-right parties (Dippel, Gold and Heblisch, 2015). Importantly, research suggests that trade's political consequences are independent from broader macroeconomic phenomena (Jensen, Quinn and Weymouth, 2017; Che et al., 2016).

There are two theoretical mechanisms that could produce the results summarized here (visualized in Figure 3). First, trade shocks cause voters to change their trade policy preferences – changes which translate via the ballot box into reduced support for incumbents and increased support for more extreme representatives who bring policy change (pathway ① in solid lines). Alternatively, trade shocks may go unnoticed by the voters but still produce political changes via special interest lobbying and/or directly affecting the policy preferences of elected leaders (pathway ② in dashed lines). Finally, it is possible that organized interests influence voters directly, illustrated by the vertical dotted line. I attempt to control for this interference by controlling for the FEC campaign contributions as a proxy for special interest activity. In this paper, I estimate the direct impact of trade shocks on voters, capturing the first link the domestic political economy of trade.

My analysis covers a unique period in the history of trade in US politics. Increasing import competition from China coexists with an erosion of the parties' traditional stances on trade, with Democratic presidents becoming more similar to Republicans in their support for free trade (prior to Donald Trump). These trends combine to produce increasingly painful local economic outcomes without a reliable political outlet.

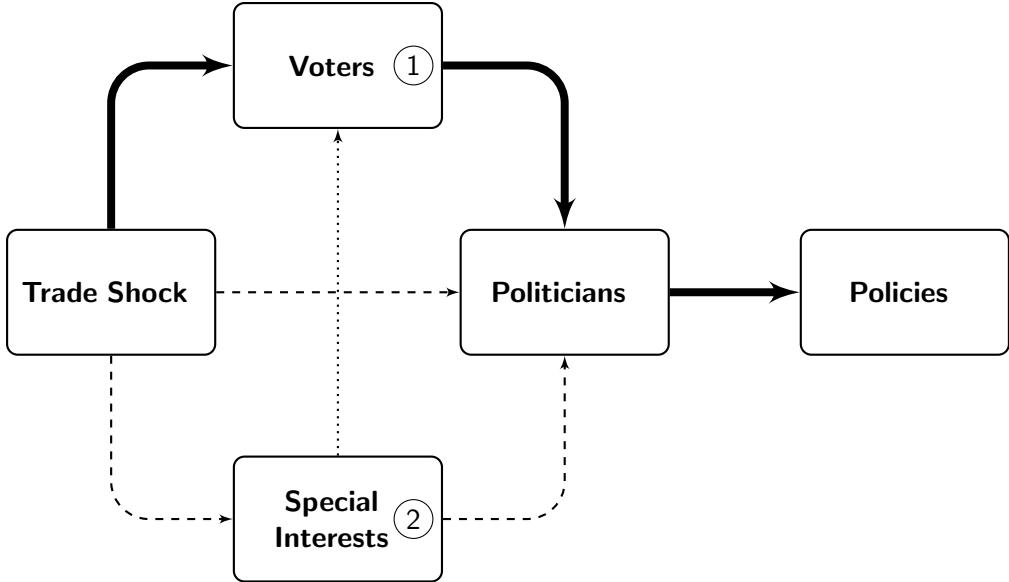


Figure 3: Alternative causal pathways connecting trade shocks and politics. In this paper, I document evidence of the voter mechanism (solid lines).

I posit that such a combination fuels broader nativist views which compensate for the sensation of being politically abandoned. As described in Stenner (2005), nativist tendencies increase when the country's order is perceived to be falling apart, particularly when leadership is perceived to be failing. Much of the non-academic writing on trade and Trump in 2016 and 2017 echoes this stance, emphasizing the interaction of psychological characteristics with scarcity to explain Trump's support (see MacFarquhar, 2016; Edsall, 2017; Vance, 2016 for examples). These patterns are broadly consistent with cognitive theories of human behavior under conditions of resource scarcity, in particular the literature on in-group / out-group cleavages which predict that individuals revert to such dichotomies in an attempt to maximize their welfare and justify withholding scarce resources from out-group members (see, for example, Sherif, 1966; LeVine and Campbell, 1972; Ross and Ellard, 1986; Skitka and Tetlock, 1992; Krosch and Amodio, 2014).

My results provide the first individual-level evidence of such dynamics as they relate specifically to trade in the United States. My findings echo current research on similar

authoritarian trends in the United Kingdom by Ballard-Rosa and Mashail Malik (N.d.) and provide empirical support for the theorized mechanisms discussed in Autor et al. (2016a) and Dippel, Gold and Heblitch (2015). I believe that these results point toward a future avenue of research that takes seriously the broader societal externalities stemming from free trade’s localized deleterious consequences and more fully appreciates the implications these consequences have for western liberal democratic ideals.

## Data and Methods

### Opinion Data

To test how individual policy preferences respond to trade shocks, I combine data from several sources. The main outcome measure of interest is an individual’s opinion on trade policy, which I obtain from 19 geo-coded Pew Research Surveys fielded between March of 2000 and March of 2016. The specific question on trade asks whether “free trade has been a good thing or a bad thing for the United States”, allowing respondents to indicate “good”, “bad”, or “I don’t know”. In the main analyses, I code anti-free trade sentiment as 1 if the respondent answered “a bad thing” and 0 otherwise. Inverting the measure to capture support for free trade finds almost identical results, confirming that my findings are not an artifact of how I treat “Don’t know” responses.

In addition, a subset of these surveys ask questions relating to immigration and globalization. Specifically, respondents are asked to choose which of two statements is more closely aligned with their views – “Immigrants today strengthen our country because of their hard work and talents” or “Immigrants today are a burden on our country because they take our jobs, housing, and health care.” Respondents were also asked whether the US “should be the SINGLE world leader, or should play a SHARED role, or should it not play any leadership role?”

I argue that these two questions proxy for an anti-foreign sentiment, or nativism. Negative views of immigrants reflect an aversion to out-group members, particularly when framed in terms of access to scarce resources as in the Pew survey question. And the belief that the US is too active in its leadership role in world affairs reflects an isolationist perspective that prioritizes domestic concerns over international ones.

The Pew surveys also record a variety of other opinions on topics ranging from abortion to gay marriage to gun rights to global warming. If trade shocks activate broader ideological affinities, we would expect to see some or all of these topics respond in a coherent fashion. Conversely, if trade prompts changes only in trade-policy preferences and nativist sentiment, these other issues should not move.

## 1.1 Trade-Related Layoffs

To measure trade shocks, I rely on applications for Trade Adjustment Assistance (TAA) which is a special program developed in the early 1960s by the Department of Labor to provide firms and workers who have been adversely affected by trade with additional federal support (obtained from Public Citizen, 2017). These applications date back to 1975 and contain the estimated number of workers laid off, the address of the firm where they worked, the industry associated with the firm, and the occupational nature of the work. Each application must be certified by the Department of Labor who investigates the claim via interviews, reviews of financial records, and assessment of upstream supplier prices and downstream consumer competition. From these applications, I can determine the precise geographic location of the layoffs. In the main analysis, I assign layoffs to each survey respondent by determining their county of residence and aggregating the layoffs to the county

level (top panel of Figure 4).<sup>1</sup> I then use the detailed geographic information to connect survey respondents to layoffs by measuring how far they are from the firms that lay off workers.

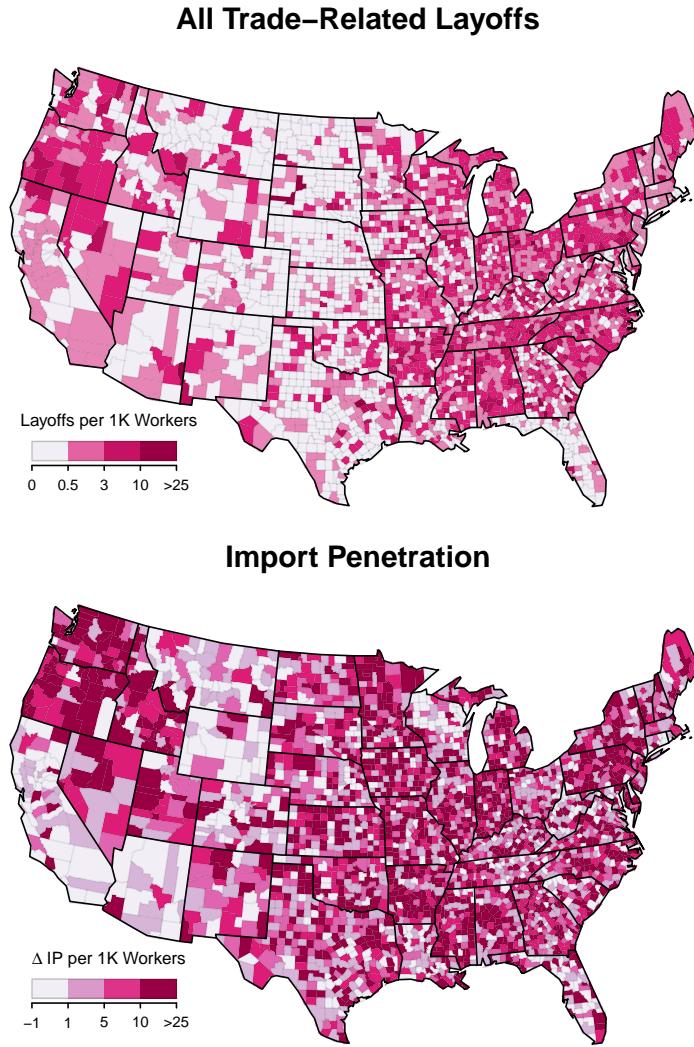


Figure 4: Total trade-related layoffs (certified and non, all industries and occupations) by county from 2000 - 2016 (top panel) versus import penetration (bottom panel).

There has been a flurry of recent research on the construct validity of TAA applications

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<sup>1</sup>I confirm my findings are robust to aggregating and assigning layoffs at the commuting zone (Tolbert and Sizer, 1996) in the Supporting Information.

as proxies for trade shocks. In particular, a cursory inspection of where applications are filed compared to where economic fundamentals predict trade shocks to be most painful highlights a disconnect – TAA applications are made by only a small fraction of trade-displaced workers in certain areas of the United States (see Figure 4). Kim and Pelc (2017) argue that this variation in TAA applications is a function of politics as some legislators promote TAA adoption while others disparage the program, yielding differential uptake among firms and workers.

Insofar as the politics of TAA applications operate in the manner described in Kim and Pelc (2017), my estimates should be conservative. Specifically, individuals who are adversely affected by trade but don't apply for TAA support are in my control group. If these individuals hold more negative views of trade, my estimates of the difference will be biased downward. Furthermore, since TAA compensates trade's losers, the negative outcomes from free trade should be reduced and the negative backlash to free trade should be mitigated (Hays, Ehrlich and Peinhardt, 2005). Both of these patterns bias against my findings, as corroborated in my Supporting Information where I replace TAA data with county-level unemployment and find stronger effects.

Despite these shortcomings, I use TAA data instead of local unemployment for two reasons. First, by using data on layoffs that firms and workers themselves believe to be the result of trade, I sidestep the theoretical debate over who wins and who loses under free trade. Traditional economic theories such as the Stolper-Samuelson and Ricardo-Viner models predict that producer identities matter but differ in which dimension matters most – skill level or industry of occupation (Stolper and Samuelson, 1941). More recent models such as the New New Trade Theory (Helpman, Melitz and Rubinstein, 2008) argue that it is not the factor of production but rather the scale and productivity of the firm that determines trade's winners and losers. TAA applications, while potentially endogenous to political horse-trading among legislators, captures a ground truth in where trade is believed

to have hurt workers, in the minds of the workers themselves.

Second, the TAA data allow me to precisely geolocate the firm at which workers were laid off, facilitating distance-based estimates connecting trade-related job losses to changes in individual opinion. This allows me to characterize the limits of “local” sociotropism using TAA applications as geographic indicators for where the most concrete realizations of trade’s negative consequences appear. These negative consequences influence individual opinions not through the egotropic experience of losing their own job but rather through the sociotropic experience of living in a county, town, or neighborhood where jobs are being lost.

## Correlational Patterns

When confronted with trade’s negative economic effects in one’s own backyard, do policy preferences on free trade change? A simple scatter plot (left panel of Figure 5) of the average number of respondents indicating that free trade has been bad for the United States (y-axis) against the number of trade-related layoffs per 1,000 individuals by county (x-axis) suggests that the answer is “yes”. There is a striking positive relationship between trade-related layoffs and opposition to free trade. This relationship is both statistically and substantively significant, suggesting that a standard deviation increase in trade-related layoffs (corresponding to roughly 2 layoffs per 1,000) is associated with a 3 percentage point increase in the share of respondents believing that free trade has been bad for the United States.

In addition, these opinions appear to be packaged with related nativist opinions. Specifically, those living in areas most adversely affected by trade’s negative consequences hold more negative views of immigrants and US global leadership. Conversely, opinions on other topics typically associated with liberals and conservatives are only weakly associated with trade-related layoffs and inconsistent, as illustrated in Figure 6. There is a positive correlation with opposition to gay marriage that is significant at conventional levels but all other measures are null results.

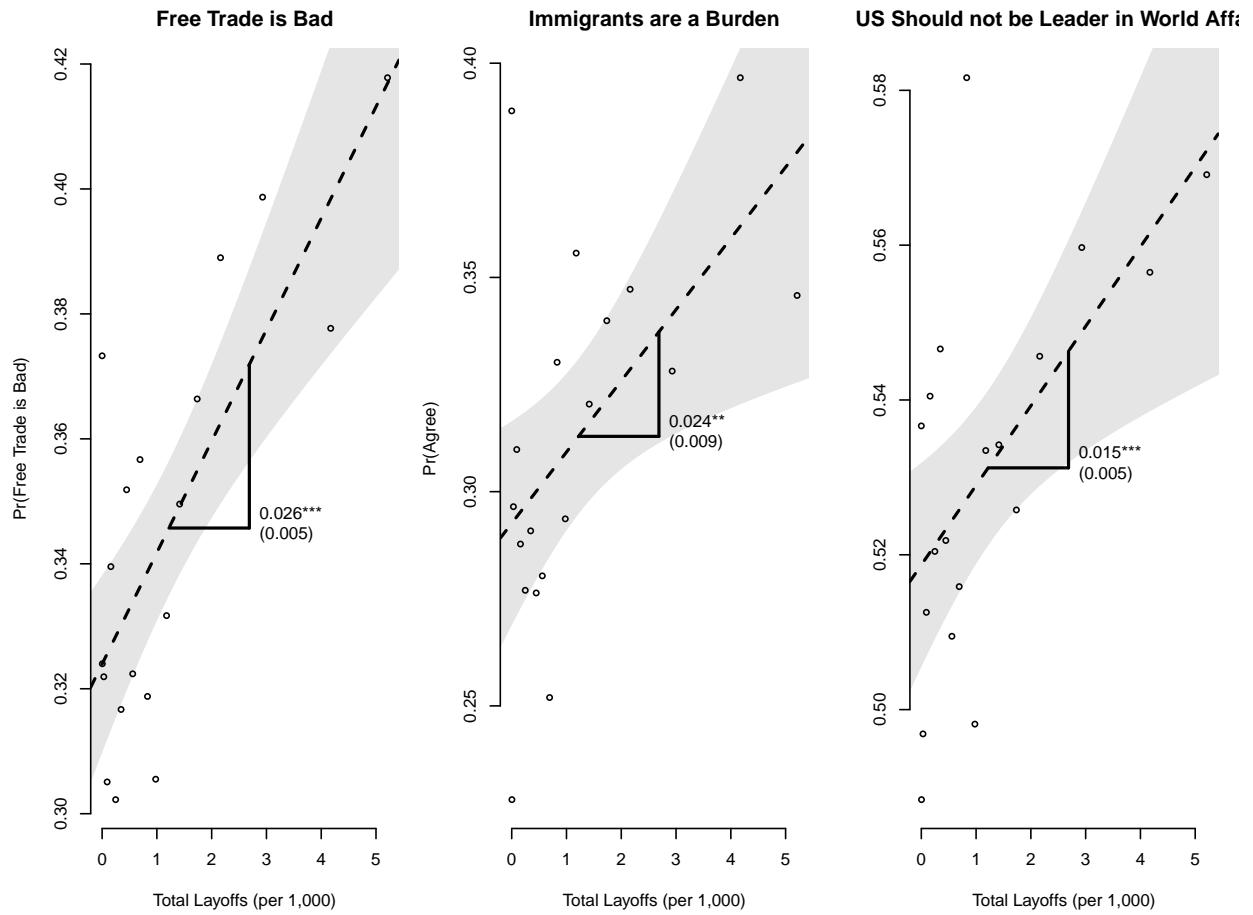


Figure 5: The opinion-layoff relationship in the data. I separate the counties into percentile bins of trade-related layoffs as the share of the population (x-axis) and take the average of responses in these bins saying that free trade has been “bad” for the United States (left panel), saying that immigrants are a burden (middle panel), and saying that the US should not be a leader in world affairs (right panel).

## 1.2 Methods

Scatter plots offer suggestive evidence of a relationship between opinions on trade and trade shocks. However, they are only correlational. These relationships may confound the effects of trade-related layoffs with other characteristics of both the individual as well as the community where they live. The political considerations summarized in Kim and Pelc (2017) may mean that those who apply for TAA support may already be more protectionist than a randomly chosen individual. If this is the case, the positive relationship documented in Figure 5 may

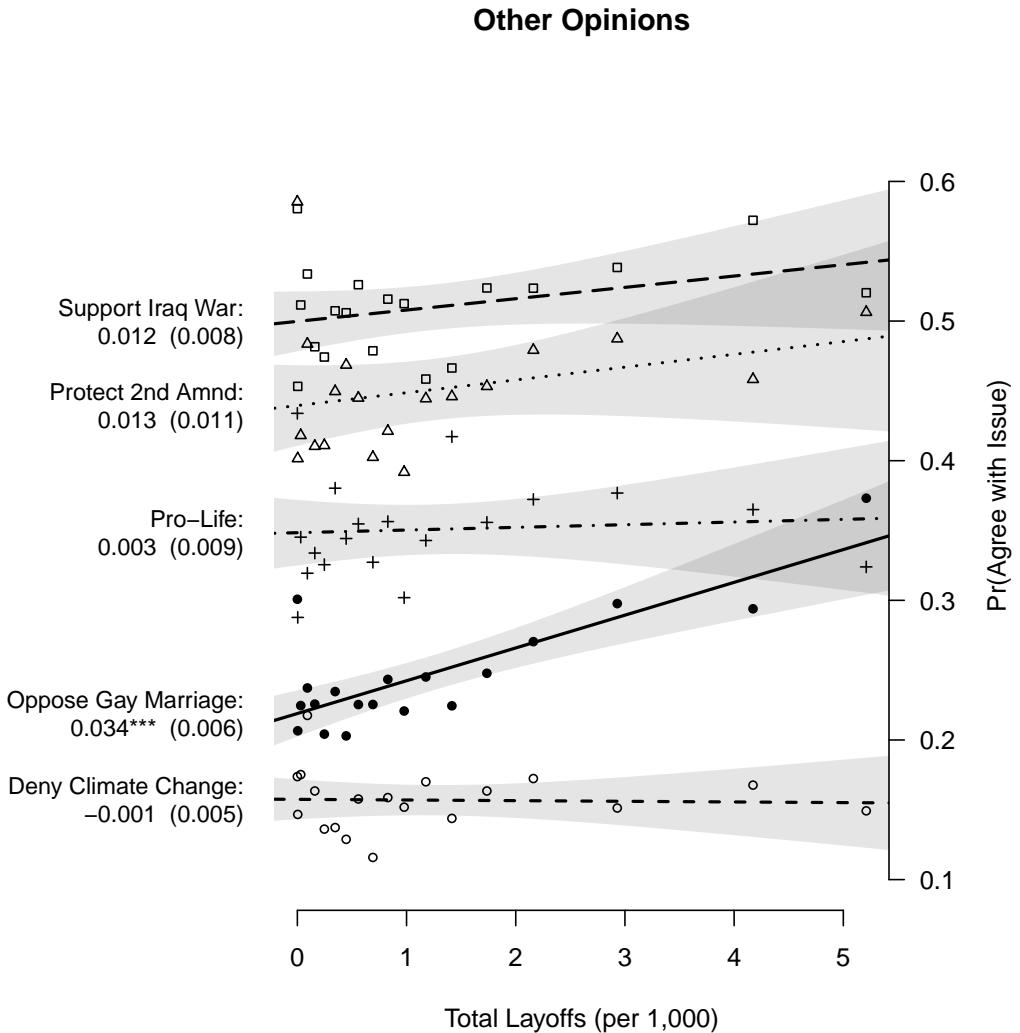


Figure 6: Correlation between trade-related layoffs and other issue opinions.

simply be comparing two different proxies for the same latent protectionism.

To overcome this concern, I use an instrumental variables strategy pioneered in Autor, Dorn and Hanson (2013) in which I exploit plausibly exogenous variation in trade-related layoffs using Chinese import competition. China is an effective instrument for two reasons. First, it is a *valid* instrument in the sense that Chinese productivity growth is plausibly exogenous to individual opinions in the United States except through the impact of its competitive goods in local US labor markets.

Second, trade with China is a *strong* instrument. Under the reforms of Deng Xiaoping and ensuing premiers, China unlocked economic productivity that had previously lain dormant under restrictive Communist economic policies. Reforms spurred a massive rural-urban migration as low-skilled labor flooded industrial centers. And newly internationalist foreign policy further augmented total factor productivity via embedded technology in imports and managerial expertise via foreign direct investment. The scope and scale of China's economic awakening makes its exports unprecedently competitive, increasing trade-related layoffs in the areas of the United States most exposed to Chinese goods.

Following Autor, Dorn and Hanson (2013), I construct this instrument using detailed data on county-level employment by industry classification which I connect with Chinese imports to the US. The variable measured at time  $t$  is the ratio of county-level ( $i$ ) employment ( $E$ ) in an industry ( $j$ ) to total employment in that industry multiplied by the ratio of the change in imports ( $M$ ) from China ( $c$ ) in this industry to the total employment in the county. The first ratio captures how much of a contribution a local labor market makes to an industry's production in the United States. The second ratio captures the share of imports in this industry experienced by the local labor market. The county-level distribution of this variable is summarized in the bottom panel of Figure 4.<sup>2</sup>

$$\Delta IPW_{it} = \sum_j \frac{E_{ijt}}{E_{jt}} \frac{\Delta M_{cjt}}{E_{it}} \quad (1)$$

By using this instrument to predict variation in trade-related layoffs recorded in TAA applications, I argue that I avoid the endogeneity issues discussed above. To the extent that there remains unobserved confounders in my specification, I use a rich set of controls measured at both the individual survey respondent level (left panel of Table 1) and the county level (right panel of Table 1) to ensure that I compare similar individuals living in similar

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<sup>2</sup>See the Supporting Information for a more detailed description of the instrument.

counties who differ only in the county’s exogenous exposure to trade-related layoffs due to China. In addition, I use Coarsened Exact Matching on the same county-level covariates to pair individuals living in counties that did and did not experience layoffs (Blackwell et al., 2009).

The main results are estimated using a linear probability model with region and year fixed effects summarized in Equation 2.

$$y_{ict} = \alpha_{c,t-1} + \beta_1 L_{c,t-1} + \beta_2 \mathbf{X}_i + \beta_3 \mathbf{C}_{c,t-2} + \epsilon_{ict} \quad (2)$$

where  $L$  represents layoffs,  $y$  is the individual’s response to the survey question regarding whether free trade has been good or bad for the United States,  $\mathbf{X}$  is a vector of individual-level controls, and  $\mathbf{C}$  is a vector of county-level controls.

I confirm my findings are robust to estimation via a multi-level model (Gelman and Hill, 2006) to account for correlated random effects at different levels as well as using a logistic specification to account for the binary nature of the dependent variable. The main results cluster the standard errors at the level of the county-year but I confirm the findings are robust to clustering at other units at which treatment is plausibly assigned. All county-level variables are lagged to avoid post-treatment bias. In my Supporting Information, I test the sensitivity of my results to different periods over which I calculate the “shock”, ranging from one year to the full decade used in Autor, Dorn and Hanson (2013).

## 2 Analysis

How do individuals update their policy preferences in response to trade-related layoffs in their county? To begin, I estimate the relationship between trade shocks and opinions on trade, capturing the overall relationship.

Table 1: Summary Statistics

Individual-Level Variables				County-Level Variables			
Outcome	N	Mean	SD	Treatment	N	Mean	SD
Anti Free Trade	29,096	0.36	(0.48)	Total Layoffs (pc)	58,981	1.30	(3.94)
Anti Immigrants	31,370	0.38	(0.48)	Manuf. Layoffs (pc)	58,981	1.12	(3.77)
Anti Globalization	29,785	0.38	(0.49)	Non-MF Layoffs (pc)	58,981	0.19	(0.98)
Covariate	N	Mean	SD	Covariate	N	Mean	SD
Age	59,034	50.44	(18.80)	Unempl. Ins. (% Tot)	58,979	2.77	(1.76)
Less Than HS	58,701	0.09	(0.28)	Business Trans. (% Tot)	58,979	1.47	(0.97)
High School	58,701	0.27	(0.44)	Net Earnings (pc)	58,979	24.44	(9.13)
Some College	58,701	0.27	(0.45)	Prioprieter's Inc. (pc)	58,979	3.26	(2.55)
College Up	58,701	0.32	(0.47)	Empl. in Manuf. (% Tot)	58,979	9.16	(6.12)
White	57,302	0.73	(0.44)	Empl. in Svcs. (% Tot)	58,979	37.08	(10.00)
Black	57,302	0.08	(0.27)	Unemp. Rate	58,979	5.94	(2.31)
Hispanic	57,302	0.10	(0.32)	GOP Voteshare	58,878	0.48	(0.13)
Asian	57,302	0.02	(0.15)	DEM Voteshare	58,878	0.52	(0.13)
Democrat	59,034	0.47	(0.50)	GOP FEC Contribs.	58,981	0.50	(0.26)
Republican	59,034	0.41	(0.49)	FEC Same Dist.	58,981	0.28	(0.24)
Liberal	53,044	0.20	(0.40)	% Black	58,981	0.12	(0.13)
Conservative	53,044	0.38	(0.49)	% White	58,981	0.69	(0.21)
Low Income	49,641	0.29	(0.46)	% Hispanic	58,981	0.13	(0.15)
Middle Income	49,641	0.41	(0.49)	% Adult	58,981	0.55	(0.03)
High Income	49,641	0.29	(0.46)	% Voting Age	58,981	0.74	(0.03)

*Notes:* Left column summarizes data recorded at the individual survey respondent level. Right column summarizes data recorded at the county level.

Table 2 reports a statistically significant increase in the probability that respondents believe free trade is “bad” for the United States across all specifications. In the most conservative estimate, this reflects a 1 percentage point increase in opposition to free trade with each standard deviation increase in the share of the county population laid off due to trade (roughly 3.9 additional individuals laid off per 1,000). In the worst-hit areas of the United States where upwards of ten percent of the workforce is laid off, these effects can exceed a 20 percentage point shift.

This pattern is robust to the inclusion of both individual-level and county-level controls, state and interview date fixed effects, matching affected and unaffected counties with Coarsened Exact Matching, and using only variation in local-level layoffs explained by Chinese

Table 2: Opposition to free-trade policy predicted by trade-related layoffs using linear regression

	OLS					2SLS	
	Biv.	+Ind Ctrl	+Cty Ctrl	+FE	+CEM	to US	<i>Chinese exports...</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Layoffs (per 1,000)	0.021*** (0.003)	0.018*** (0.004)	0.013*** (0.004)	0.010* (0.004)	0.010* (0.004)	0.048** (0.017)	0.027* (0.011)
Pres. Vote (GOP)		0.007 <sup>†</sup> (0.003)	0.004 (0.004)	0.003 (0.004)	0.001 (0.005)	-0.0003 (0.005)	0.001 (0.005)
Income (Transfers)			0.029* (0.014)	0.031* (0.014)	0.079 (0.114)	0.027 (0.099)	0.126 (0.123)
Manuf. Emp. (%)				0.037** (0.014)	0.038** (0.014)	0.175* (0.072)	
Unemp. Rate			0.017*** (0.005)	0.029*** (0.006)	0.032*** (0.008)	0.001 (0.014)	-0.002 (0.008)
Date & Region FE	No	No	No	Yes	Yes	Yes	Yes
Indiv. Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	No	No	Yes	Yes	Yes	Yes	Yes
CEM	No	No	No	No	Yes	Yes	Yes
1st Stage F-Stat						36.03	16.88
Observations	29,081	22,226	22,161	22,161	22,161	22,161	22,161

*Notes:* Linear regressions of opposition to free trade policy on trade-related layoffs per 1,000, standardized. A simple bivariate regression is reported in column (1). Column (2) adds individual-level demographic controls. Column (3) includes county-level controls. Column (4) adds controls for date and region fixed effects while column (5) uses coarsened exact matching to pair counties by government transfers and labor market characteristics. Column (6) uses the import penetration instrument described in Autor, Dorn and Hanson (2013) focusing on Chinese exports to the United States. Column (7) uses the same instrumental variables strategy but uses Chinese exports to other developed countries. Standard errors clustered on the county-year are presented in parentheses. First-stage F-statistics for 2SLS summarized at bottom. Additional controls not shown. <sup>†</sup> < 0.10; \* p < 0.05; \*\* p < 0.01; \*\*\* p < 0.001.

export growth. Exploiting exogenous variation in layoffs via Chinese import penetration more than doubles the estimate to 0.027. These differences point to an attenuating selection effect in which those more likely to apply for TAA assistance are less likely to hold negative views of free trade – a relationship that is consonant with the mission statement of the Trade Adjustment Assistance program. In my supporting information, I find that the strongest effects are for rejected claims in areas with high import penetration values.

## Sociotropism

The results summarized above connect individual survey respondents to localized trade shocks by assigning them to TAA data based on their county of residence. Even without exploiting the geographic granularity of my data, these patterns likely reflect predominantly sociotropic responses since it is highly unlikely that these survey respondents were among those laid off in the TAA data. The average proportion of the county who lose their jobs is only two-tenths of a percentage point. Given random sampling used in Pew surveys, in expectation only 443 of the 22,161 survey respondents were included in the estimated number of lost jobs in the TAA applications.

However, the TAA data may reflect more widespread trade-related economic hardship. Subsetting my data to those surveys for which I have information on respondent employment status (self-reported), I find suggestive evidence of stronger effects for the unemployed (see Figure 7). However, these differences are not statistically significantly different from one another. Furthermore, it is not necessarily the case that the unemployed survey respondents are unemployed because of trade or due to some other circumstance. As such, I turn to a different approach for estimating the localness of the sociotropic concerns.

The geographic granularity of my data allow me to empirically estimate the “localness” of these sociotropic concerns. To do this, I assign each survey respondent to a weighted sum of all layoffs within a specific radius. As illustrated in Figure 8, I divide each layoff by the distance in miles between a given respondent and the location of the firm at which the layoffs occurred. I then sum these weighted layoffs that are within a 25 mile radius of the respondent and assign this value to the individual. I vary the radii to explore the sensitivity of my estimates to different distances and identify the threshold at which local sociotropic concerns disappear.

I repeat this process for each respondent in my dataset, assigning distance-weighted trade shock exposures across radii ranging from 25 miles to the entire United States. At

## Change in Opposition to Free Trade: By Employment Status

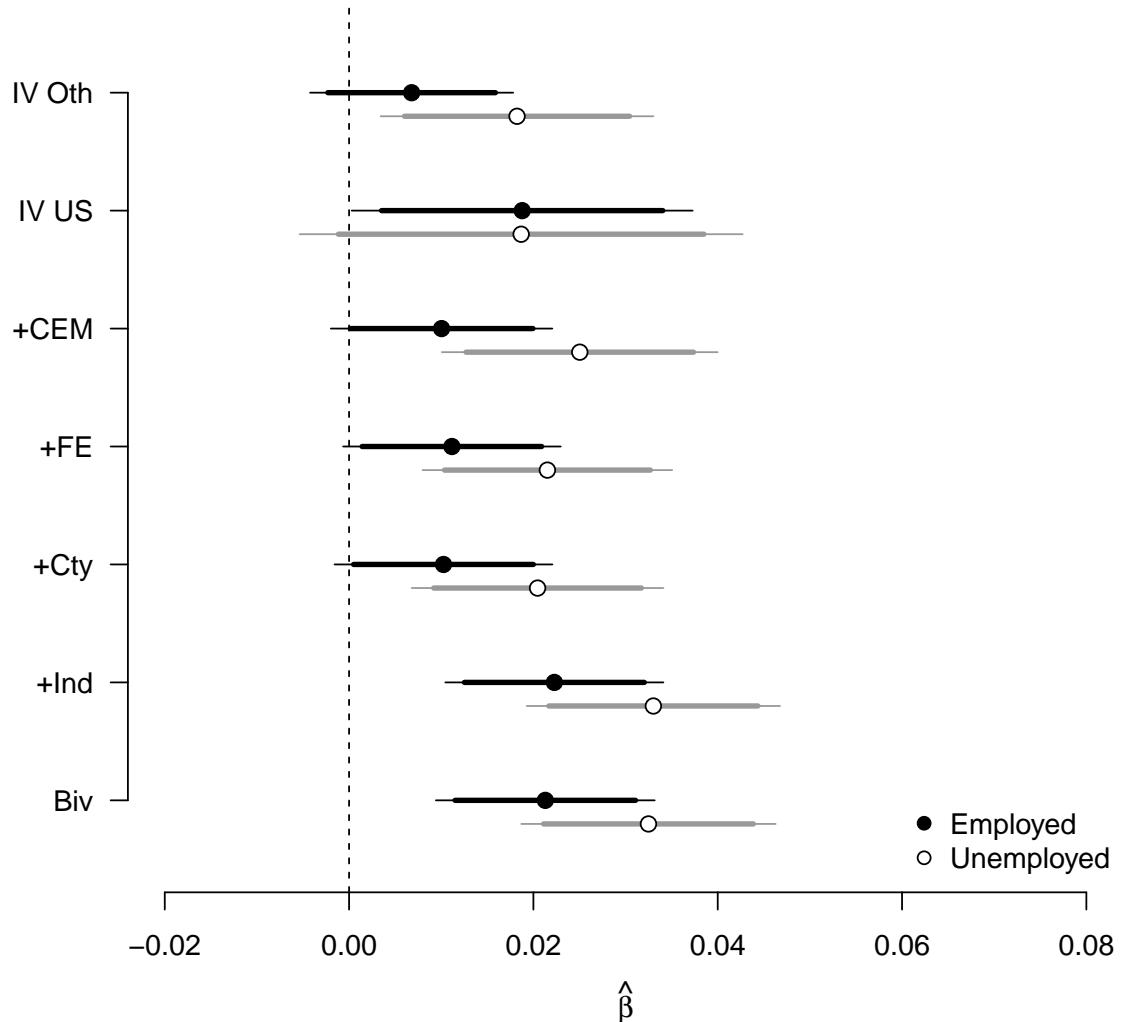


Figure 7: Regression coefficients for the impact of trade-related job loss on opposition to free trade for employed (black circles) and unemployed (white circles) respondents. Thin bars contain 90% confidence intervals, thick bars contain 95% confidence intervals. Specifications listed on y-axis.

the most narrow radius thresholds, it is likely that I am undercounting the intensity of the trade-shock as individuals are likely sensitive to layoffs in areas further away than twenty five miles. Conversely, at the most broad radius thresholds (simply summing all distance-weighted layoffs for each respondent), it is likely that I overestimate the intensity of the shock

## Layoff Assignment Map

Respondent #1,251 near Dallas, TX in September 2000

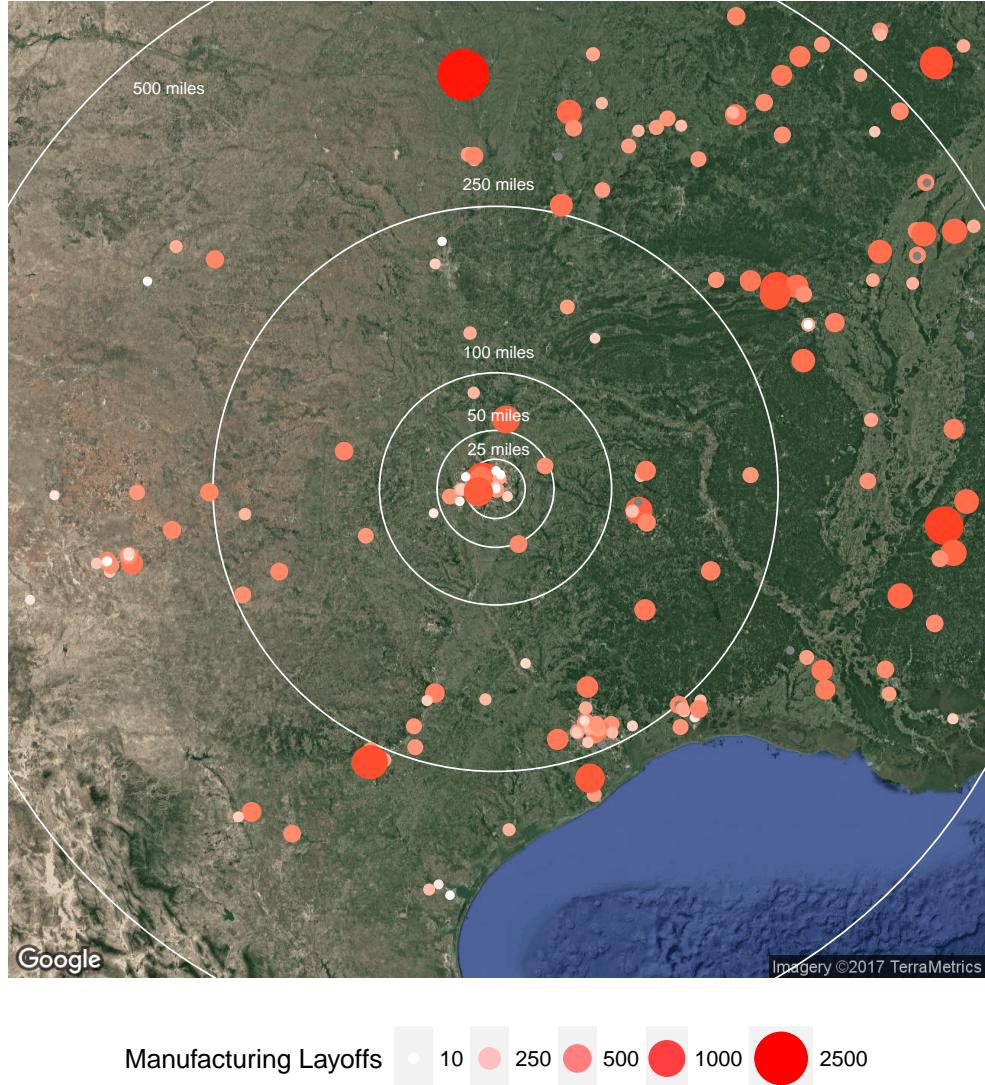


Figure 8: Example of assigning layoffs to survey respondents using respondent #1,251 who was living in Dallas, TX when contacted by Pew Research in September of 2000. Bubbles represent all trade-related layoffs recorded by firm in the preceding year in manufacturing. The size and red shade of the bubbles indicate the number of individuals laid off at each firm. The hollow white circles demarcate radii of 25 miles, 50 miles, 100 miles, 250 miles, and 500 miles around the respondent. The assignment method attributes each layoff within a certain radius to respondent #1,251 weighted by the distance between the respondent and the location of the layoff.

and conflate treatment and control. As illustrated in Figure 9, my results are strongest at thresholds between 25 and 250 miles.

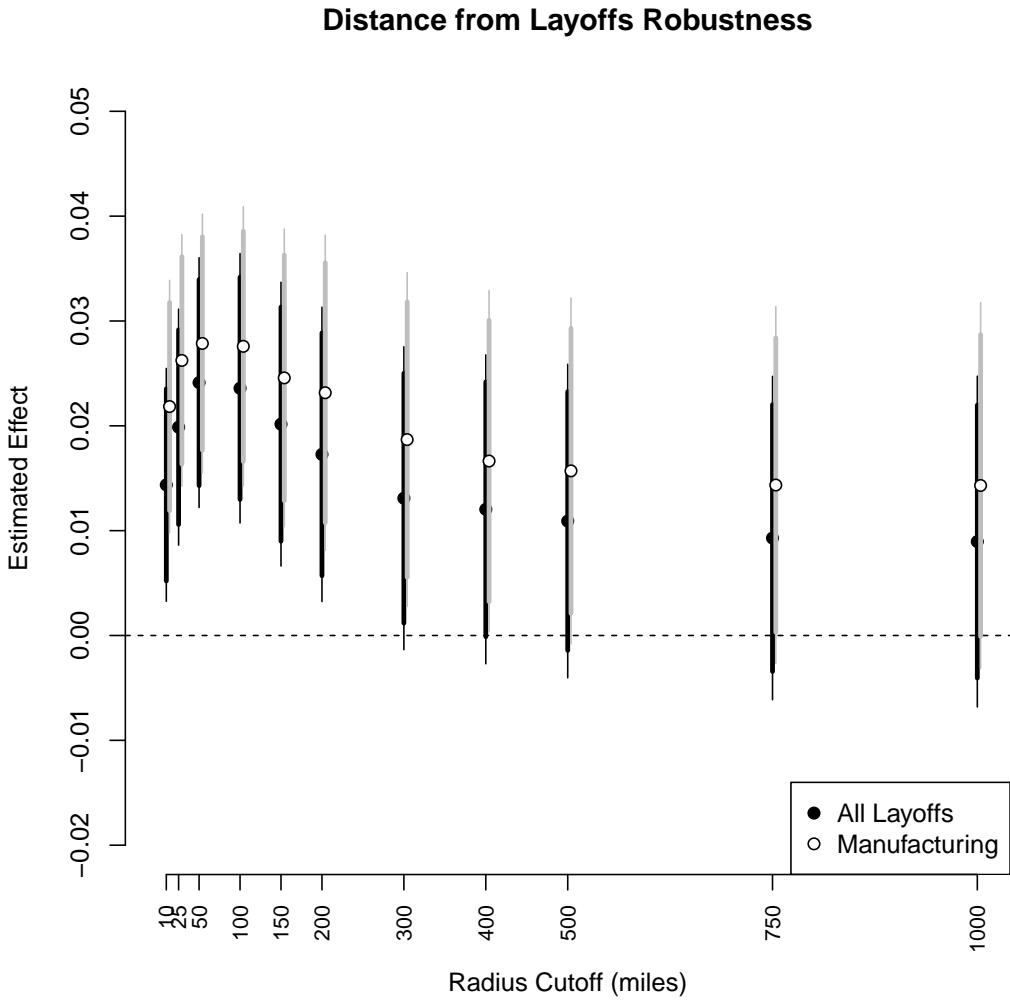


Figure 9: Robustness of main results when replacing county-level aggregated layoffs with distance-weighted measures. X-axis details different radius thresholds for including layoffs based on distance between firm and individual survey respondent. Gray bars and circle uses all layoffs weighted by distance between firm and respondent.

These results suggest that the sociotropic concerns detailed in Mansfield and Mutz (2009) have a local character that decays as the location of the trade-related job losses becomes more distant.<sup>3</sup> This local sociotropism can help explain why regressions of trade opinion

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<sup>3</sup>These findings also highlight the aggregation issues facing researchers using natural experiments with counties as the unit of analysis. Less than 10% of US counties have areas of 255 square miles

on proxies for an individual's skill or industry of occupation provide inconsistent evidence in favor of traditional economic theories of who wins and who loses under free trade. It is not merely who you are but also where you live that determines your experience with, and reaction to, free trade.

## Bundled Opinions

Individuals change their trade policy preferences in response to local trade shocks. But do they change their opinions on any other issues? Figure 10 suggests that they do. Specifically, those living in areas adversely affected by trade are more likely to hold negative views of immigrants and are more likely to believe that the United States should be less involved in global affairs. All coefficients are estimated using the same 2SLS specification as that summarized in Column (6) of Table 2.

These causally identified results echo those presented in the scatter plots of figures 5 and 6. These findings are consistent with a story in which individuals who experience trade's negative consequences adopt more protectionist views of trade policy and more nativist views in general. Importantly, these significant trends contrast powerfully with the insignificant estimates for other topics unrelated to nativist opinions of the United States. The relationship between trade-related layoffs and standard barometers for ideology such as positions on gay marriage, abortion, global warming, and gun rights are tightly estimated zeros.

Combined, these results paint a picture of trade's negative consequences causing increasing resistance to free trade specifically and a rejection of globalization more generally. They contribute to the literature by tracing the first link in the causal chain connecting trade shocks to anti-incumbent voting (Margalit, 2011; Jensen, Quinn and Weymouth, 2017), elite

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or less, making the majority of counties too large to capture areas that are sensible units for aggregating responses to economic outcomes. This relates to the growing methodological literature on causal interference (Aronow and Samii, 2017).

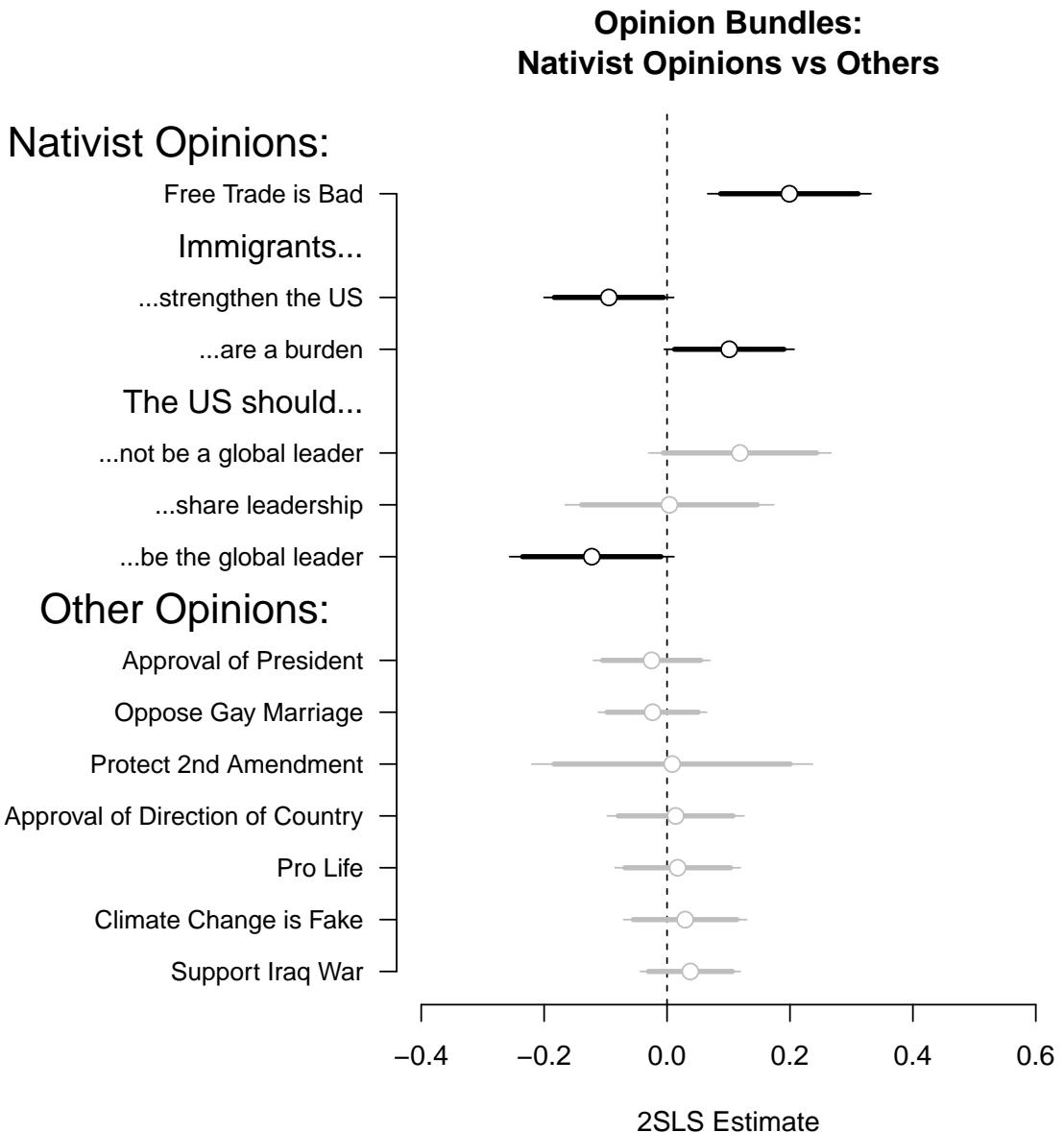


Figure 10: Regressions of other opinions on instrumented layoffs. 95% confidence intervals indicated with thin bars, 90% intervals indicated with thick bars. Significant estimates shaded black. All regressions use region and interview date fixed effects and cluster standard errors at the year-county level.

polarization (Autor et al., 2016*a*), and increasingly protectionist legislation (Feigenbaum and Hall, 2015). Without exit poll data that connects vote choice to trade preferences, a direct test of this mechanism is beyond the scope of my paper. However, in the Supporting In-

formation, I regress trade-related layoffs on Republican presidential vote share, finding that trade shocks were enough to shift state-level votes in four swing states in the 2016 election, more than enough to change the outcome. While numerous factors affected the election of Donald Trump in 2016, my analysis suggests that trade is a non-trivial component.

### 3 Conclusion

Trade's negative economic consequences have been causally connected with shifting electoral fortunes (Margalit, 2011; Jensen, Quinn and Weymouth, 2017), increasing polarization (Autor et al., 2016*a*; Che et al., 2016; Dippel, Gold and Heblich, 2015), and more protectionist legislation (Feigenbaum and Hall, 2015). In this paper, I demonstrate that trade also has an impact on voter opinion. Specifically, I show that opposition to free trade policies increases in areas adversely affected by free trade. In addition, I document evidence of bundled opinions on immigrants and globalization.

My results fill in the missing link in the causal chain connecting local economic shocks to political outcomes by demonstrating a significant nativist backlash against free trade at the level of individual policy preferences. Importantly, the bundled opinions on immigrants and globalization are consistent with a broader nativist response to trade's deleterious effects.

My findings are consistent with interpretations of Donald Trump's election that emphasize the role played by free trade. Trump campaigned on a platform that spoke to these issues and was able to mobilize voters sharing the opinions that I document in this paper. In the Supporting Information, I corroborate the findings of Autor et al. (2016*b*) by regressing county-level vote shares for Trump on instrumented trade-related layoffs. I find that counties more negatively affected by free trade voted disproportionately for Donald Trump in the 2016 election, and that these trade-motivated votes were enough to shift the electoral college votes in four swing states. While not an explicit test of the mechanism of policy preferences,

these results highlight the importance of trade's negative consequences on contemporary politics.

This paper does not address several interesting questions about Trump's election and the broader relationship between economic shocks and preferences. First, there has been an ongoing debate over the relative influence of trade and racism in the election of Trump. Future research should test whether racial resentment is endogenous to economic scarcity. Second, while the evidence I present is causal, it is not conclusive. Without geocoded exit poll data, I am unable to directly test the mechanism connecting trade policy preferences with individual votes. Finally, my analysis does not explore whether and how policy interventions might mute or augment the political response to trade's negative consequences. I see all three questions as areas of fruitful research.

Free Trade and American Politics:  
How Policy Preferences Respond to Local Trade Shocks  
**Supporting Information**

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## A Data Sources

I combine a variety of data sources to construct my final dataset which nests individual survey respondents within counties / commuting zones where I aggregate the total number

of layoffs. I itemize my sources below, along with a brief description of the data and link (if applicable) to download it.

- 1) **Survey Data:** My main outcome of interest is a randomly sampled survey respondent's opinion on free trade, immigration, and globalization. I obtained these data from the Pew Research Center at [<http://www.people-press.org/datasets/>].

In order to connect survey respondents with firm-level layoffs, I required as accurate a measure of their residence as possible. I used zip code centroids to assign latitude and longitude coordinates to each survey respondent and then measured the distance in miles between each respondent and all firms that laid off workers in the preceding year. The publicly available Pew Research Center data prior to 2009 includes zip codes. For more recent surveys, I obtained geocoded data via special request. I am not able to share these more recent surveys in their raw form without authorization from the Pew Research Center.

- 2) **Layoff Data:** My main treatment variable is the estimated number of workers laid off due to trade which I obtain from applications to the Trade Adjustment Assistance branch of the Department of Labor. A publicly available subset of these data are available from Public Citizen (Public Citizen, 2017) at [<https://www.citizen.org/our-work/globalization-and-trade/trade-adjustment-assistance-taa-database-documentation-0>]. Alternatively, researchers can download TAA applications directly from the Department of Labor's website at [[https://www.doleta.gov/tradeact/taa/taa\\_search\\_form.cfm](https://www.doleta.gov/tradeact/taa/taa_search_form.cfm)].

- 3) **Instrument Data:** I follow Autor, Dorn and Hanson (2013) in constructing the import penetration instrument and combine two different data sources. To calculate the share of each county employed in a particular industry, I use the County Business Patterns (CBP) data which records employment by 4-digit SIC code or 6-digit NAICS code each year for all counties and establishment sizes. These data can be found on the Census website at [<https://www.census.gov/programs-surveys/cbp/data.html>]. I run David Dorn's imputation code to account for missing data at smaller levels of geography, which can be found on his website at [<http://www.ddorn.net/data.htm>].

To calculate the change in imports from China in the associated product category, I download trade data from USITC which can be access at [<https://dataweb.usitc.gov/>].

- 4) **County Controls:** In addition to the individual-level controls which are contained in the Pew Research Center surveys, I also control for a variety of county-level (or commuting zone level) factors, including:

- the local unemployment rate which can be found at [<https://download.bls.gov/pub/time.series/la/>]
- local government transfers which can be found at [<https://www.census.gov/govs/local/>]

- county-level income data and employment data which can be found at  
[<https://www.bls.gov/bls/proghome.htm#employment>]
- county-level demographic data which can be downloaded from  
[<https://data2.nhgis.org/main>]
- county-level voting data which can be downloaded from  
[<http://library.cqpress.com/elections/download-data.php>]
- FEC contribution data which can be downloaded in raw form from  
[[https://classic.fec.gov/finance/disclosure/ftp\\_download.shtml](https://classic.fec.gov/finance/disclosure/ftp_download.shtml)]  
or in a more organized format from  
[<https://data.stanford.edu/dime>]

## B Robustness

My findings may be spurious or falsely significant if my identifying assumptions are incorrect. In the following sections, I subject the main results to a series of robustness tests and placebo checks.

### B.1 Specification Robustness

I subject my findings to a variety of robustness checks, starting with alternative measures of the treatment measure of layoffs. TAA applications contain information on industry of in which those affected worked, the certification status of the Department of Labor's review of the application, the group responsible for submitting the application, and the stated reason for the layoffs. While the main findings use all reported layoffs, I confirm the robustness of different measures of the layoffs in Figure B.1.

As illustrated, the main results are robust to most divisions of the TAA data. And the layoffs that do not reproduce the main result are largely intuitive. Specifically, TAA applications filed by workers (those whose livelihoods are directly affected) support the main findings while those filed by non-workers (unions and state officials) do not. Also intuitive is the difference in layoffs in production versus service tasks where employees in the former category are likely less mobile and therefore more negatively affected by trade-related layoffs.

The Department of Labor's certification process both determines whether an application is credibly connected with trade and categorizes the type of trade shock that is used to justify the certification. Overall, the fact that only certified layoffs produce the shift in trade policy preferences is reassuring for my results. Similarly, the strongest relationships obtain when using only layoffs categorized as owing to import competition or changes in upstream suppliers. Changes in downstream demand for intermediate goods has no significant relationship with opinions on trade, nor do shifts in production.

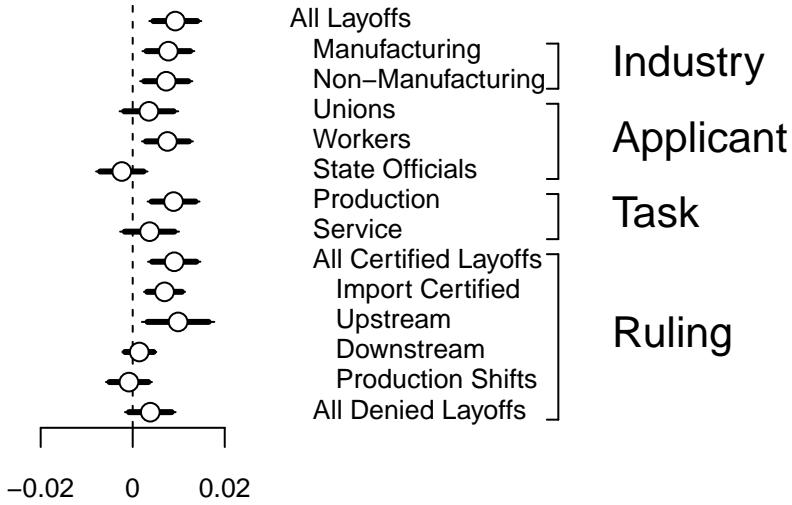


Figure B.1: Treatment robustness by type of TAA application. 95% confidence intervals indicated with thin bars, 90% confidence intervals indicated with thick bars.

## B.2 Logistic Regressions

The main findings use a linear approximation to facilitate interpretation. However, since the outcome variables of interest are binary, I confirm the robustness of the main results to using logistic regressions. As illustrated in Table B.1, the main findings hold when replacing a linear approximation with a logistic regression.

## B.3 Mixed Effects Models

In my main analysis, I use state and date fixed effects to control for any unobserved confounds that don't vary along these dimensions. However, the nature of my data suggest a different model described in Gelman and Hill (2006): multilevel or “mixed effects” models. Specifically, my unit of observation is the individual survey respondent who is nested within a county (or commuting zone) at which I posit that the treatment of trade shocks is realized. A multilevel model allows me to control for correlated random effects that are experienced at different levels. Specifically, I can control for random effects at the level of the county without losing unsustainable degrees of freedom. The specification can be written as:

$$y_{ict} \sim \text{logit}(\alpha_{c,t-1} + \beta_1 L_{c,t-1} + \beta_2 \mathbf{X}_i, \sigma_y^2); \quad \text{where} \quad \alpha_{c,t-1} \sim N(\gamma_0 + \gamma_1 \mathbf{C}_c, \sigma_\alpha^2) \quad (3)$$

where  $L$  represents layoffs,  $y$  is the individual's response to the survey question regarding whether free trade has been good or bad for the United States,  $\mathbf{X}$  is a vector of individual-level controls, and  $\mathbf{C}$  is a vector of county-level (or commuting zone-level) controls. Individuals  $i$

Table B.1: Opposition to free-trade policy predicted by trade-related layoffs using logistic regression

	Biv.	+Ind. Ctrls	+Cty Ctrls	+RE	+CEM	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Layoffs (% of pop.)	0.090*** (0.013)	0.082*** (0.015)	0.057*** (0.015)	0.041** (0.015)	0.035* (0.016)	
Layoffs IV						0.058*** (0.016)
Prev. Pres Vote GOP		0.029 <sup>†</sup> (0.015)	0.018 (0.015)	0.013 (0.016)	0.007 (0.019)	0.012 (0.016)
Income (Transfers)			0.184* (0.075)	0.181* (0.087)	0.102 (0.418)	0.161 <sup>†</sup> (0.082)
Manuf. Emp. (%)			0.194** (0.073)	0.225* (0.090)	1.135*** (0.303)	0.192* (0.084)
Unemp. Rate			0.093*** (0.021)	0.180*** (0.025)	0.199*** (0.034)	0.159*** (0.023)
RE	No	No	No	Yes	Yes	Yes
Individual Controls	No	Yes	Yes	Yes	Yes	Yes
County Controls	No	No	Yes	Yes	Yes	Yes
CEM	No	No	No	Yes	Yes	Yes
Observations	29,081	22,226	22,179	22,179	22,179	22,179

*Notes:* Logistic regressions of opposition to free trade policy on trade-related layoffs as a share of county labor force, standardized. A simple bivariate regression is reported in column (1). Column (2) adds individual-level demographic controls. Column (3) includes county-level controls. Column (4) adds controls for date and county random effects while column (5) uses coarsened exact matching to pair counties by government transfers and labor market characteristics. Column (6) uses the import penetration instrument described in Autor, Dorn and Hanson (2013). \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$ .

are nested within both counties  $c$  and time periods  $t$ .

Figure B.2 plots the coefficients estimated using multilevel models that include random effects for geography and date, aggregating layoffs at both the level of the county (black bars and solid points) and the commuting zone (gray bars and hollow points). As illustrated, the coefficients are substantively similar to those summarized in the main results and are even more significant across all specifications.

## B.4 Other Measures of Economic Shocks

The main results use trade-related layoffs as recorded by the TAA as the main treatment of interest. The motivation to do so is two-fold. First, the TAA data is geocoded down to

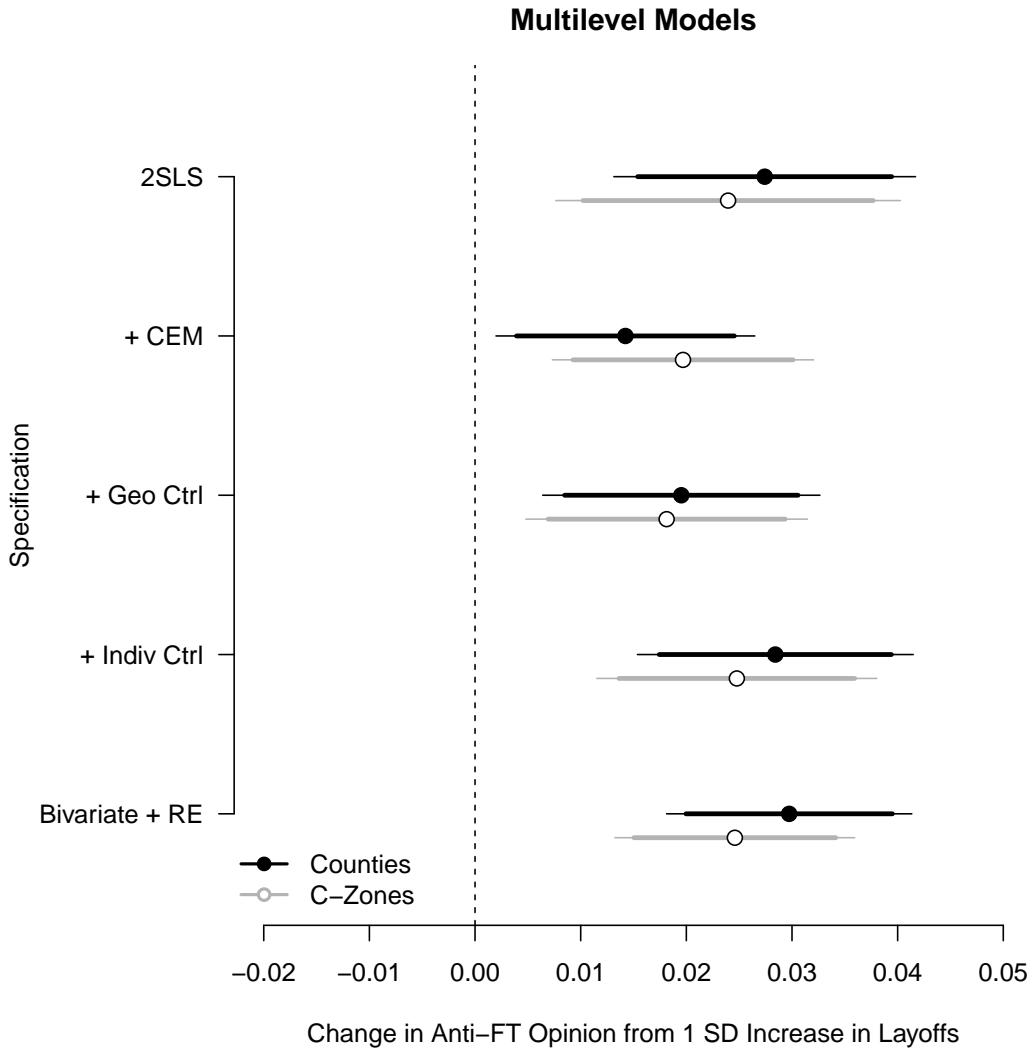


Figure B.2: Coefficients (x-axis) estimated using different specifications (described on y-axis) that capture the relationship between a 1 standard deviation increase in trade-related layoffs and opposition to free trade. Thin bars capture 95% confidence intervals and thick bars capture 90% confidence intervals. Solid points and black bars represent trade-related layoffs aggregated to the county level while hollow points and gray bars represent the commuting zone level.

the firm address, allow for more sophisticated treatment assignment while accounting for potential SUTVA violations. Second, the TAA data represent layoffs that those who were adversely affected *recognized* as occurring due to trade. Other measures of economic shocks (i.e., unemployment rates, GDP per capita, etc.) must rely on one of several competing economic models of trade's winners and losers in order to connect these coarser measures to trade. The benefit of the TAA data is that these data are generated by trade's losers to

*perceive* themselves to have lost due to trade.

Nevertheless, the instrumental variables strategy should, in theory, capture exogenous economic shocks due to trade. As such, alternative measures of local economic outcomes would be expected to yield similar results. In Figure B.3, I regress negative views of free trade on county-level economic measures that are instrumented using the Autor, Dorn and Hanson (2013) strategy. Given that increases in Chinese import penetration produce differently signed first stage coefficients, I normalize all estimates to reflect a standard deviation decline in the economic outcomes listed on the y-axis. For example, an increase in Chinese imports produces an increase in the unemployment rate as well as a decrease in net earnings at the county level. Both of these produce increases in opposition to free trade policy and are signed as such, yielding the consistently positive coefficient plot summarized in Figure B.3. As illustrated, all coefficients with first-stage t-statistics in excess of 10 are positive and significant, suggesting that trade-shocks consistently produce more negative views of free trade policy, regardless of the treatment measure adopted. Indeed, the only measures of local economic outcomes that are not significant are those for whom the first-stage relationship is extremely weak.

## B.5 IV Duration

In Autor, Dorn and Hanson (2013) original paper and in many of the ensuing applications, they examine the impact of changes in Chinese imports over long periods of time (1990 to 2000 and then 2000 to 2007). My work uses a shorter two-year period. While the exclusion restriction argument doesn't change based on the time period, it is possible that the first-stage strength is sensitive to the choice of duration. To test, I estimate the first-stage relationship between trade-related layoffs and the change in Chinese import penetration calculated over different periods. As illustrated in Figure B.4, the choice of duration does impact the first-stage strength of the IV regression although longer periods yield *weaker* relationships.

## B.6 Bootstrapped 2SLS

Traditional 2SLS regressions combine the first and second stages to yield more conservative measures of the estimate's standard error. However, the canned algorithms found in R and Stata do not appropriately account for how uncertainty in the first stage of the regression interacts with uncertainty in the second stage. To address the anti-conservative inference issues with these methods, I replicate the 2SLS procedure by bootstrapping both stages. Doing so confirms the main findings with a significant positive association between opposition to free trade and trade-related layoffs, as illustrated in Figure B.5.

## B.7 Sensitivity to Clustering Choices

In theory, one should cluster standard errors at the unit at which treatment is administered. In practice, determining this unit isn't always straightforward. The SUTVA concerns

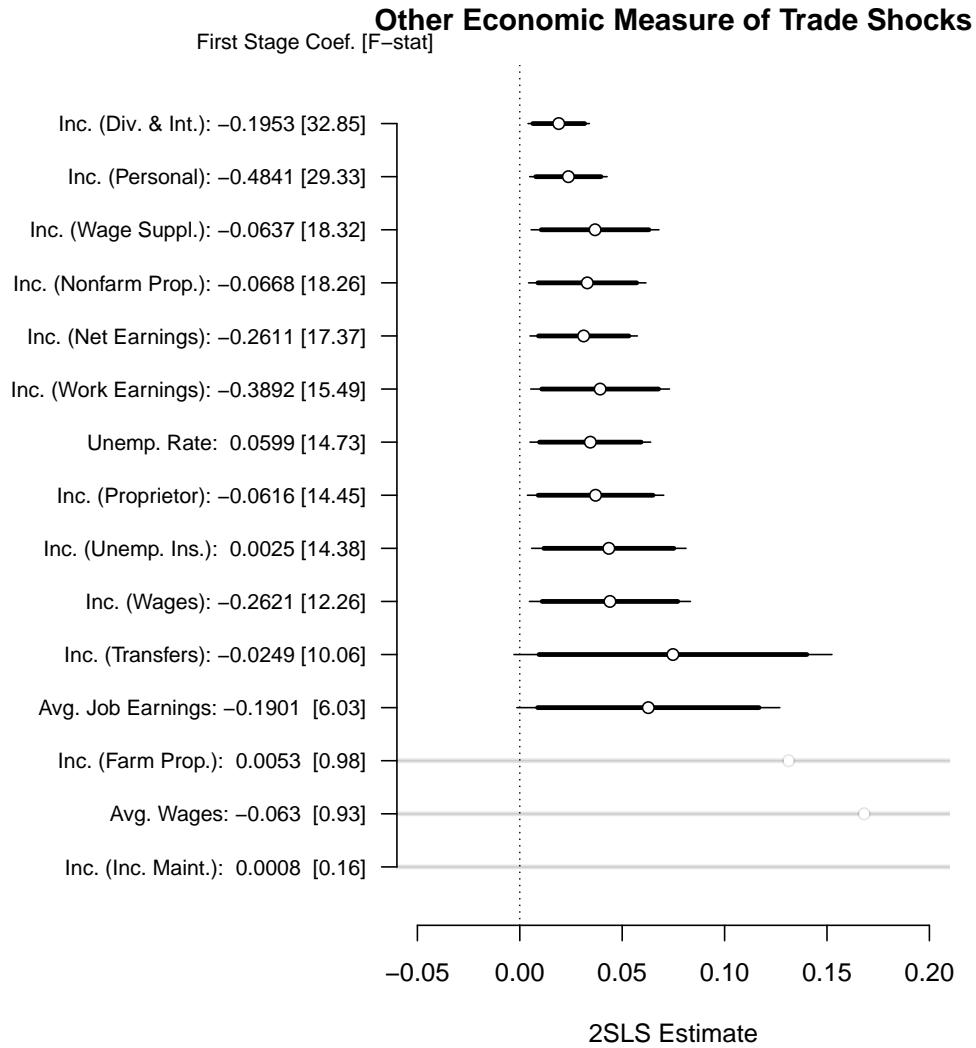


Figure B.3: 2SLS estimates of how trade shocks impact free trade views via a number of different local economic measures (y-axis). Coefficients are re-signed to capture the impact of an economic decline in the indicator—stemming from Chinese imports—on the opinion that free trade has been bad for the United States.

summarized in the body of the paper are related to treatment-unit uncertainty.

My main results are estimated clustering the standard errors at the interview date-county level. In Figure B.6 below, I re-estimate my results clustering the standard errors at the level of the county, the commuting zone, the state, and at the interview date-commuting zone and interview date-state level.

My main findings on free trade preferences are robust to different clustering choices. However, the findings on the packaged nativist opinions are less so. The least significant specification is produced by clustering the standard errors at the level of the state. I argue

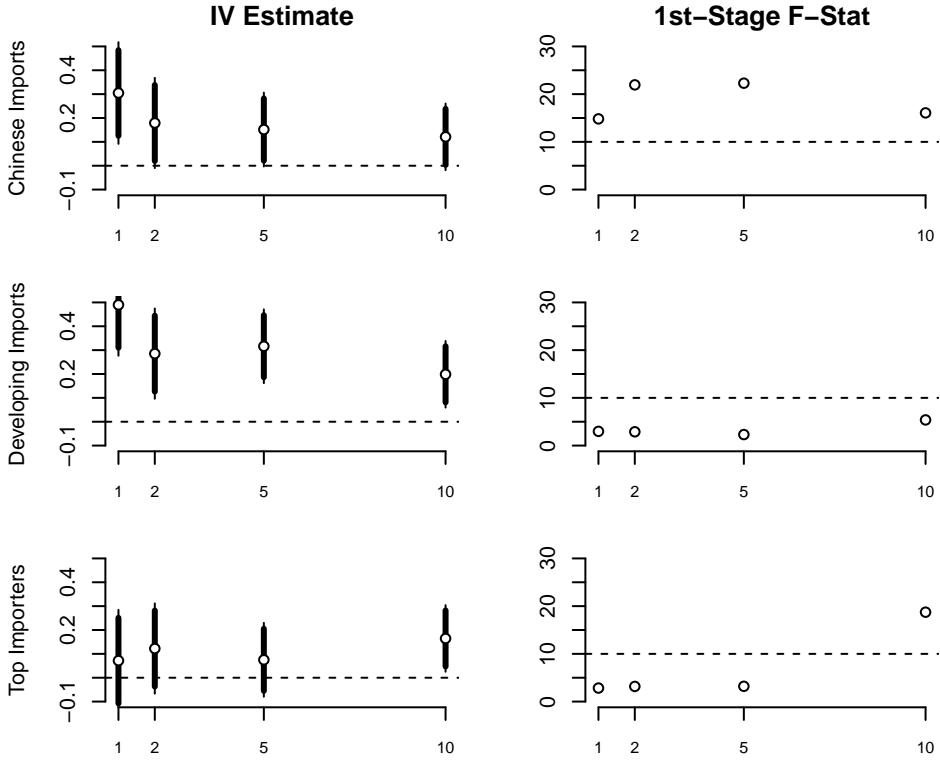


Figure B.4: Sensitivity of results and first-stage strength to duration of period over which import penetration instrument is measured.

that the state is (in most cases) too large a geographic area to realistically be considered the unit at which treatment is assigned. Buying this argument would require believing that layoffs at a firm in one part of Oklahoma are experienced equally by every member living in the state and not experienced at all by individuals living in Nebraska. Nevertheless, the sensitivity of my findings to the choice of clustering unit warrants caution when interpreting my results.

## B.8 Sensitivity to Omitted Variables

An alternative way to check the robustness of my results is to ask how strong an unobserved confound would have to be to undermine my findings. To do so, I generate a random variable that is correlated with both my layoff data and the opinion data and then include it as an additional control in the main specification (Imbens, 2003). I compare the coefficient on layoffs in this adjusted specification with my original estimate in Figure B.7 by plotting the correlation between the simulated unobservable and layoffs (x-axis) and opinion (y-axis) and shade the points by whether the revised coefficient estimate on layoffs is outside the 95% confidence interval around the original estimate. For the sake of comparison, I also plot the observable covariates based on their correlations with layoffs and opinions.

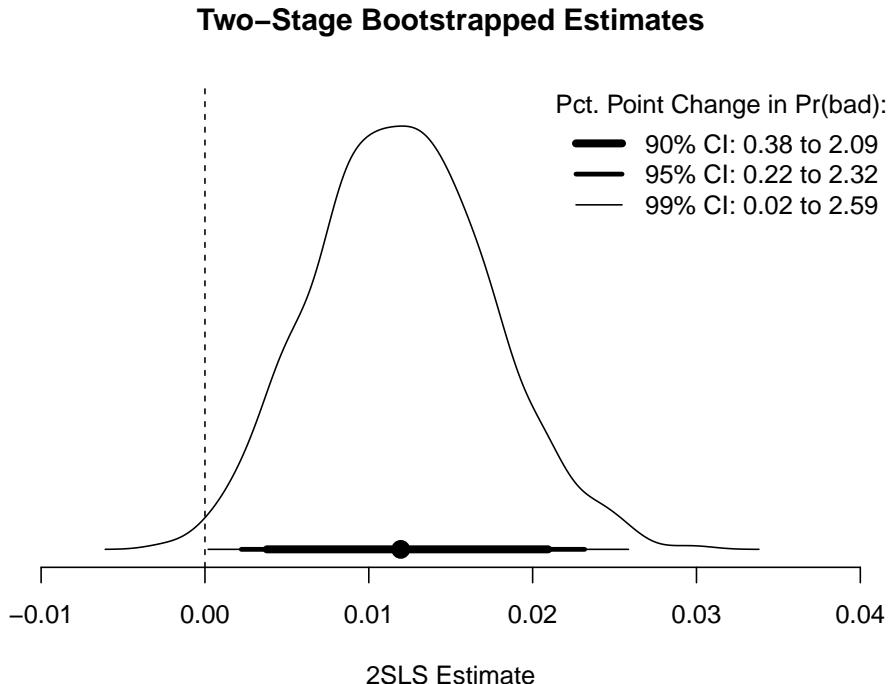


Figure B.5: 1,000 bootstrapped 2SLS estimates along with 90%, 95%, and 99% confidence intervals. Regressing whether individual responds that free trade has been bad for the US on trade-related layoffs predicted by variation in Chinese import penetration, controlling for full battery of individual and county level controls along with CEM weights and year and county random effects.

As illustrated, in cases where the unobserved confound is correlated relatively strongly with either layoffs or opinions, the adjusted coefficient on layoffs is outside the original coefficient's 95% confidence interval. However, the position of the most strongly correlated of the observed covariates gives reason for confidence in the sensitivity of the main findings. Substantively, the unobserved confound must be more strongly associated with layoffs than the either share of the county that is Hispanic or unemployment insurance transfers to undermine my findings. Similarly, the unobserved confound must be more strongly correlated with opinions on trade than either the youngest or oldest age cohorts. It is difficult to imagine what such an unobservable may be.

## B.9 SUTVA Violations

The main results placed individual survey respondents within counties and assign layoffs aggregated at the county level. As such, these results hinge on the assumption that an individual only notices layoffs that occur within her county but is oblivious to layoffs in

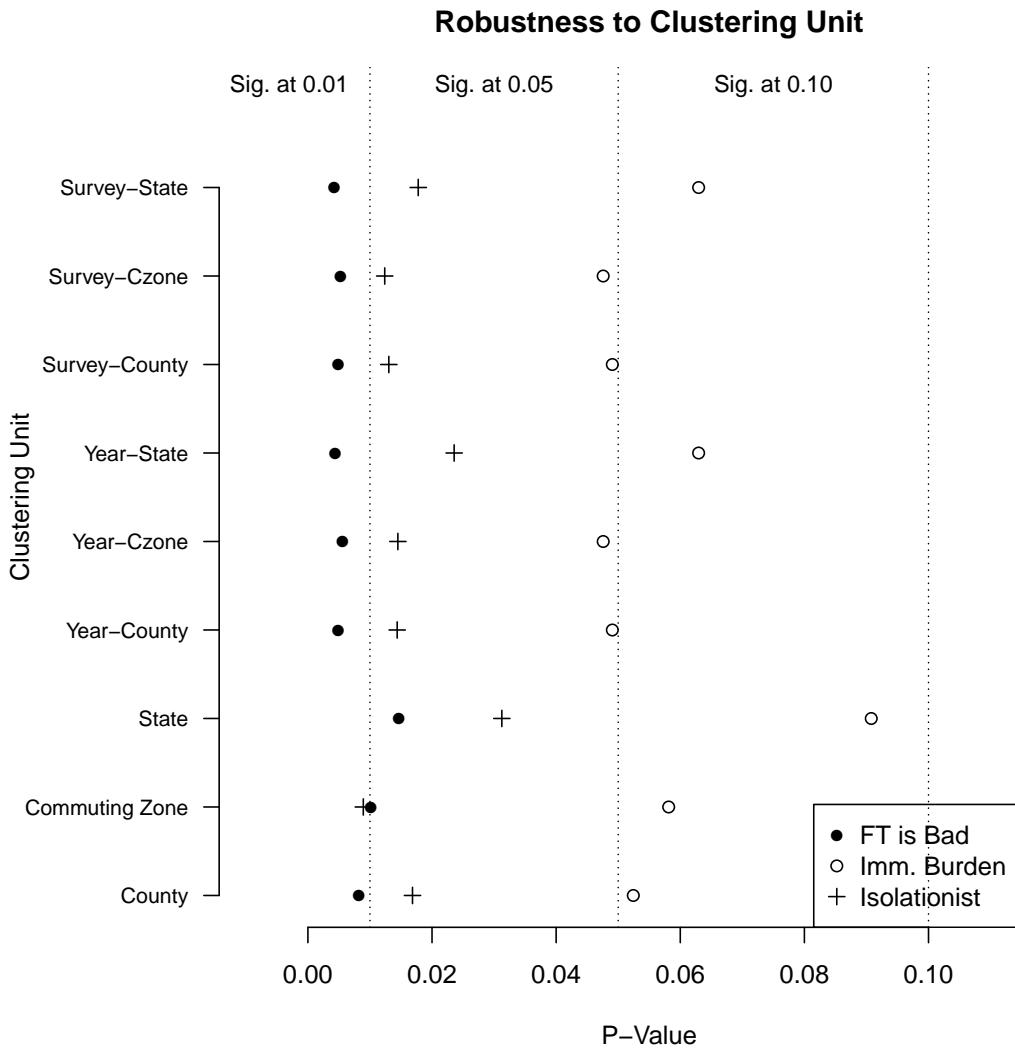


Figure B.6: Sensitivity of findings to choice of clustering unit. P-values of main findings summarized on x-axis for opinions on free trade (dark circles), opinions on immigrants (hollow circles), and opinions on globalization (crosses). The y-axis lists the different units at which I cluster my standard errors.

neighboring counties. This assumption is known as the Stable Unit Treatment Assignment Assumption or SUTVA.

To the extent that SUTVA fails, estimates comparing affected and unaffected survey respondents are likely to be biased. The direction of the bias could go in one of two ways. If treated individuals receive a stronger “dose” of trade-related layoffs due to exposure to both layoffs in their county as well as layoffs in adjacent areas, they may inflate the estimated effect of layoffs. Conversely, the estimates may be attenuated if individuals assigned to control actually experience trade-related layoffs from adjacent areas, muddying the comparison

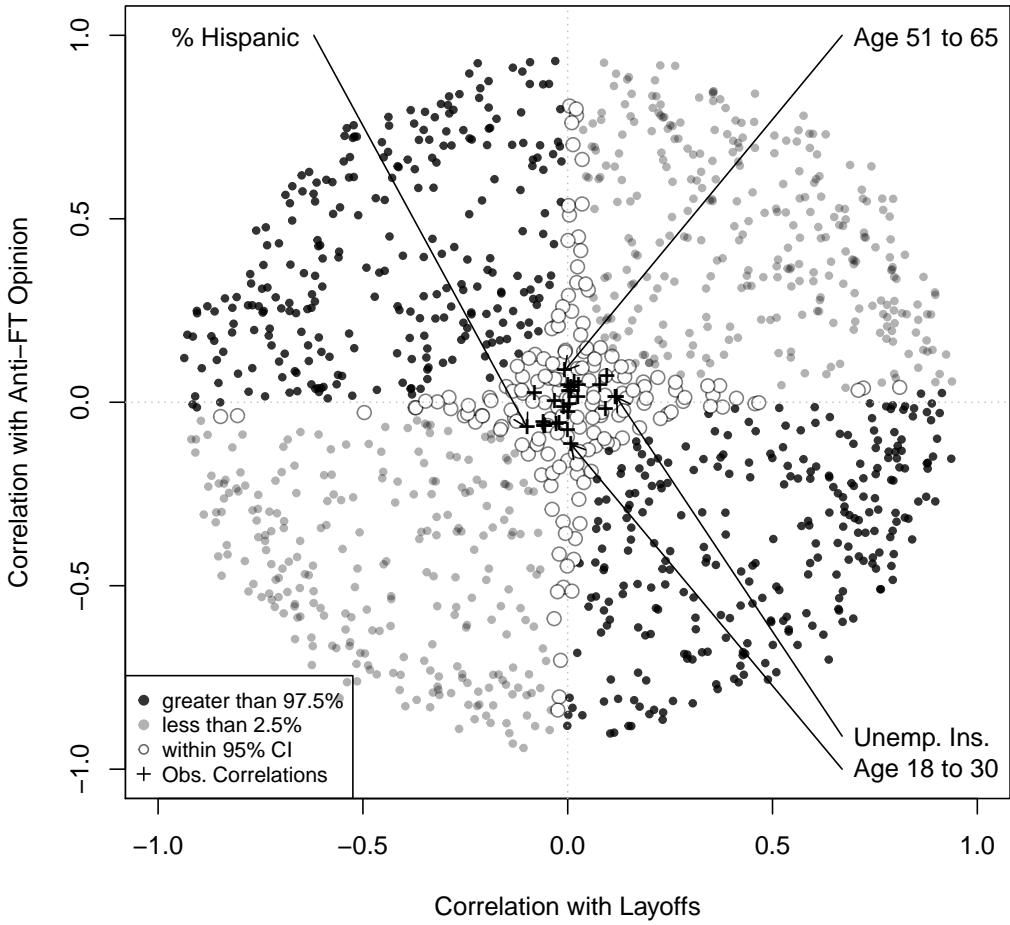


Figure B.7: Sensitivity analysis. Each point is a randomly generated confound which is correlated with layoffs and opinion as indicated on the x and y-axes respectively. Points are shaded by how significantly the updated estimate of the effect of layoffs on opinion differs from the original estimate. Black solid circles represent updated estimates greater than the 97.5% confidence bound on the original estimate while grey solid circles represent updated estimates less than the 2.5% lower bound on the original estimate. Hollow circles are updated estimates that fall within the 95% confidence interval of the original estimate. Black crosses represent the observable covariates used in the main specification. The covariates most strongly correlated with layoffs and opinion are labeled.

between treated and control opinions.

My analysis of local sociotropism in the body of my paper speaks to the SUTVA concern by leveraging the precise geocoded nature of my data to estimate the relationship between trade shocks and opinions as a function of distance. In addition, I also substitute counties

with commuting zones. Commuting zones are geographic areas that are defined by a high degree of within-zone commuting patterns and a low degree of between zone commuting patterns Tolbert and Sizer (1996). They are designed to more accurately capture geographic units associated with economic activity. The main findings persist – and are in fact magnified – when using commuting zones, as illustrated in Table B.2.

Table B.2: Opposition to Free Trade ~ Trade-Related Layoffs: Commuting Zone Aggregation

	Biv. (1)	+Ind. Ctrl. (2)	+Cty Ctrl. (3)	+RE (4)	+CEM (5)	2SLS (6)
Layoffs (% of pop.)	0.045*** (0.009)	0.041*** (0.008)	0.032*** (0.008)	0.023*** (0.007)	0.025*** (0.008)	0.109** (0.053)
RE	No	No	No	Yes	Yes	Yes
Individual Controls	No	Yes	Yes	Yes	Yes	Yes
County Controls	No	No	Yes	Yes	Yes	Yes
CEM	No	No	No	Yes	Yes	Yes
1st Stage F-Stat						26.23
Observations	22,114	22,114	22,114	22,114	22,114	22,114

*Notes:* Linear probability regressions of opposition to free trade policy on trade-related layoffs as a share of county labor force, standardized. A simple bivariate regression is reported in column (1). Column (2) adds individual-level demographic controls. Column (3) includes county-level controls. Column (4) adds controls for date and county random effects while column (5) uses coarsened exact matching to pair counties by government transfers and labor market characteristics. Column (6) uses the import penetration instrument described in Autor, Dorn and Hanson (2013). \* p < 0.05; \*\* p < 0.01; \*\*\* p < 0.001.

## C Heterogeneous Effects

My main results control for a number of individual-level characteristics, including economic covariates (education and income), demographic covariates (age, gender, and race), and political covariates (party affiliation and ideology). In the figures below, I document heterogeneous effects across these covariates to speak to the existing reduced form research connecting individual characteristics with opinions on trade. My results are largely intuitive, with stronger effects among less educated and less wealthy respondents, as well as among respondents in the prime of their careers (age 31 - 65).

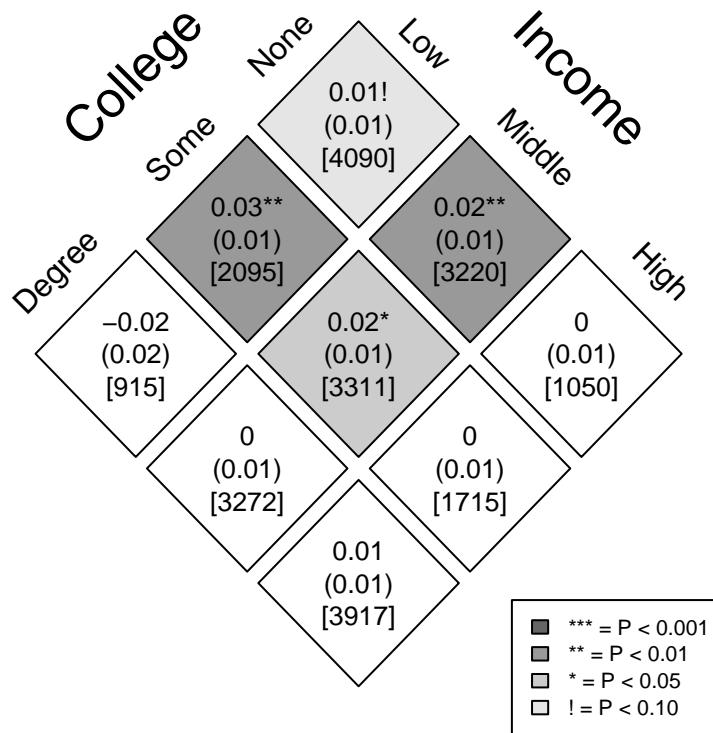


Figure C.1: Heterogeneous effects by income and educational attainment. Each diamond presents the estimated effect of trade-related job loss on the probability that the respondent indicates that free trade has been bad for the US, the standard error in parentheses, and the number of observations falling into each category in brackets. Diamonds are shaded by statistical significance, given in the legend.

One interesting nuance in these heterogeneous effects appears when I look at party affiliation by presidential term. Here we see significant effects for self-identified Republicans during Obama's presidency and significant effects for self-identified Democrats during Bush's presidency. I posit that these patterns reflect the influence of elite communication as both president's pursued free trade legislation during their tenure (Bush with CAFTA, Obama

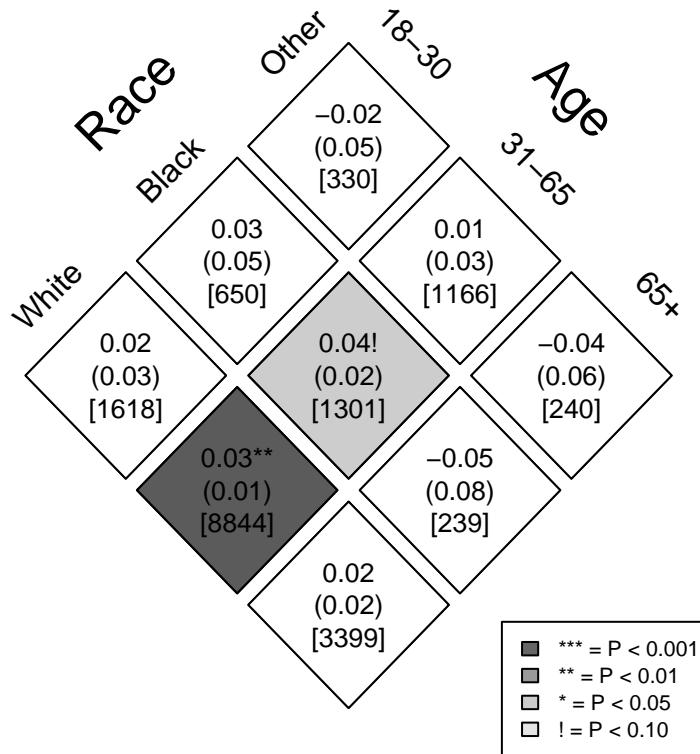


Figure C.2: Heterogeneous effects by age and race. Each diamond presents the estimated effect of trade-related job loss on the probability that the respondent indicates that free trade has been bad for the US, the standard error in parentheses, and the number of observations falling into each category in brackets. Diamonds are shaded by statistical significance, given in the legend.

with TPP). The difference is particularly strong for self-identified Democrats who respond strongly to trade-related job loss during the Bush years but exhibit no response whatsoever during Obama's term.

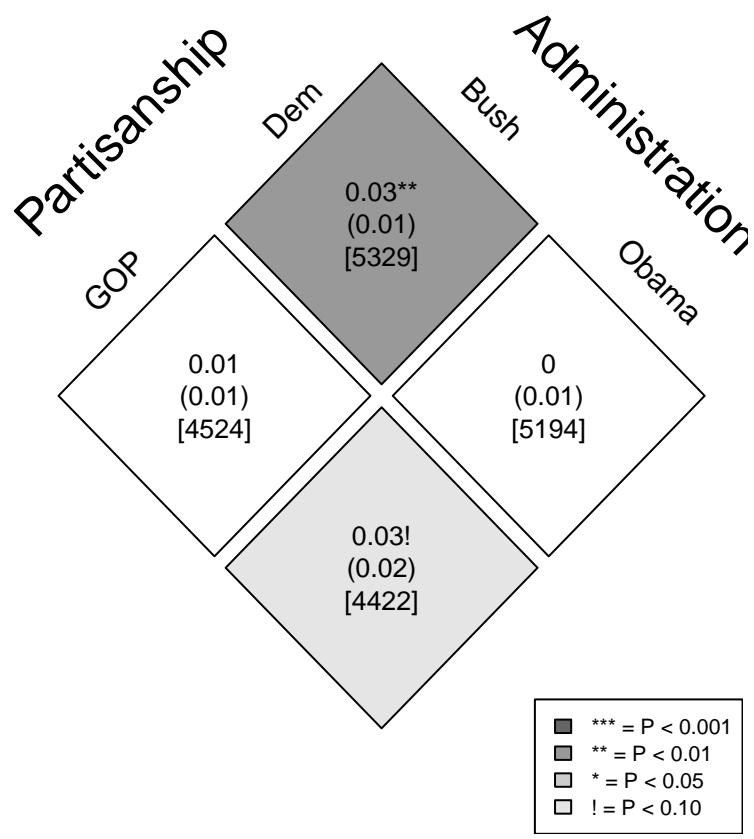


Figure C.3: Heterogeneous effects by party alignment with the president over the Bush and Obama administrations. Each diamond presents the estimated effect of trade-related job loss on the probability that the respondent indicates that free trade has been bad for the US, the standard error in parentheses, and the number of observations falling into each category in brackets. Diamonds are shaded by statistical significance, given in the legend.

## D Instrumental Variable

Following Autor, Dorn and Hanson (2013), I construct the Chinese import penetration instrument using detailed data on county-level employment by 6-digit NAICS industry classification which I connect with Chinese imports to the US measured at the same level. Following Autor, Dorn and Hanson (2013), consider the production function of a local labor market  $i$  who supplies other regions  $n$  with goods of a certain industry  $j$ . Exports  $X_{nij}$  from region  $i$  to region  $n$  in industry  $j$  can be expressed as:

$$X_{nij} = \frac{T_{ij}(w_{ij}\tau_{nij})^{-\theta}}{\phi_{nj}} X_{nj} \quad (4)$$

where  $T_{ij}$  is region  $i$ 's productivity in industry  $j$  and is a function of region  $i$ 's production cost  $w$  in industry  $j$  and the cost of trading  $\tau$  between region  $i$  and region  $n$ , subject to a productivity dispersion parameter  $\theta$ . The numerator thus captures the components of region  $i$ 's exports to region  $n$  in industry  $j$  that are a function of region  $i$ 's economic characteristics.

The denominator  $\phi_{nj}$  is the sum of the same factors for competing regions who also export to region  $n$ , including China. I index these competing regions by  $h$ , yielding  $\phi_{nj} = \sum_h T_{hj}(w_{hj}\tau_{nhj})^{-\theta}$ . Finally,  $X_{nj}$  reflects the total demand for good  $j$  in market  $n$ .

Region  $i$ 's total production is thus the sum of these exports across all regions  $n$  in the United States  $u$ , across all industries  $j$ . We can derive the log change in demand for region  $i$ 's goods in industry  $j$  via:

$$\hat{Q}_i = - \sum_j \frac{X_{uj}}{X_{uj}} \frac{X_{ucj}(\hat{A}_{cj} - \theta\hat{\tau}_{cj})}{Q_i} \quad (5)$$

where the first ratio is the share of region  $i$ 's production in industry  $j$  relative to total US consumption of industry  $j$ . The second ratio is the growth in US imports from China due to increasing Chinese productivity ( $\hat{A}$ ) and changes in the costs of trade ( $\tau$ ) relative to the total output in region  $i$ .

Following Autor, Dorn and Hanson (2013), I empirically proxy for these factors with employment measures ( $E$ ) and total imports ( $M$ ) from China, to create:

$$\Delta IPW_{uit} = \sum_j \frac{E_{ijt}}{E_{jt}} \frac{\Delta M_{ucjt}}{E_{it}} \quad (6)$$

In theory, this measure captures local level variation in import competition that is due to variation in Chinese productivity and the costs of trade. Insofar as the empirical proxy may also reflect variation due to US demand shocks for Chinese goods, I use variation in Chinese exports to other developed countries.

By using this instrument to predict variation in trade-related layoffs recorded in TAA applications, I argue that I avoid endogeneity issues between trade-related layoffs and trade policy preferences. To the extent that the remains unobserved confounders in my specification, I use a rich set of controls measured at both the individual survey respondent level (age,

sex, educational attainment, race, and presidential vote in the prior election) and the county level (local unemployment rate, median household income, share of population employed in manufacturing, county-level Democratic vote share in the preceding presidential election, share Hispanic, share Black, share adult, and per capita unemployment transfers) to ensure that I compare similar individuals living in similar counties who differ only in the county's exogenous exposure to trade-related layoffs due to China. Alternatively, I use Coarsened Exact Matching on the same county-level covariates to pair individuals in counties that did and did not experience layoffs.

## E Voting Behavior

My main results suggest that individuals adopt more protectionist policy preferences in response to trade-related layoffs and that these views are packaged with broader nativist stances on immigration and globalization. However, these results don't conclusively demonstrate that trade shocks are connected with the political outcomes described in Margalit (2011), Autor et al. (2016a), Jensen, Quinn and Weymouth (2017), and Feigenbaum and Hall (2015) via trade policy preferences. In the following section, I test whether the relationship between voting behavior and trade-related layoffs is consonant with the relationship between opinion and layoffs documented in the body of my paper. In these regressions, I include the same county-level controls as well as the CEM weights to ensure I compare otherwise similar counties who differ only in their number of trade-related layoffs. I analyze each election since 1980, regressing the Republican vote share on the number of trade-related layoffs accumulated over the preceding presidential term. I plot each election's coefficient and confidence interval in Figure E.1. For the years where I have data on Chinese import penetration, I also plot the 2SLS results.

As illustrated in Figure E.1, trade-related layoffs are significantly associated with Republican majority vote shares over time. Note that these regressions control for the overall unemployment rate as well as changes in income and employment, meaning that layoffs due to trade have an independent effect on voting behavior above and beyond broader economic factors. This result comports with the findings of Jensen, Quinn and Weymouth (2017) who show that trade's economic benefits and costs have an independent effect on voting even after controlling for broader macroeconomic factors.

However, the dynamics of Figure E.1 complicate the purely anti-incumbent narrative posited in Jensen, Quinn and Weymouth (2017) and Margalit (2011). Not including the two most recent elections, my findings suggest that Republicans have historically been punished at the ballot box. This narrative is consistent with the traditionally pro-free trade position adopted by the Republican Party in the early 1970s (Karol, 2009). However, the strong positive association between trade shocks and GOP support in the 2016 suggests that Trump's anti-free trade message attracted many votes in these historically Democratic counties.

To what extent did these trade-related layoffs actually influence the outcome of the 2016 election? To test this, I simulate a counter-factual 2016 election at the county-level in which there were no trade-related layoffs. I then sum the total votes for Trump and Clinton across

## Trade–Related Layoffs on Republican Presidential Vote Shares

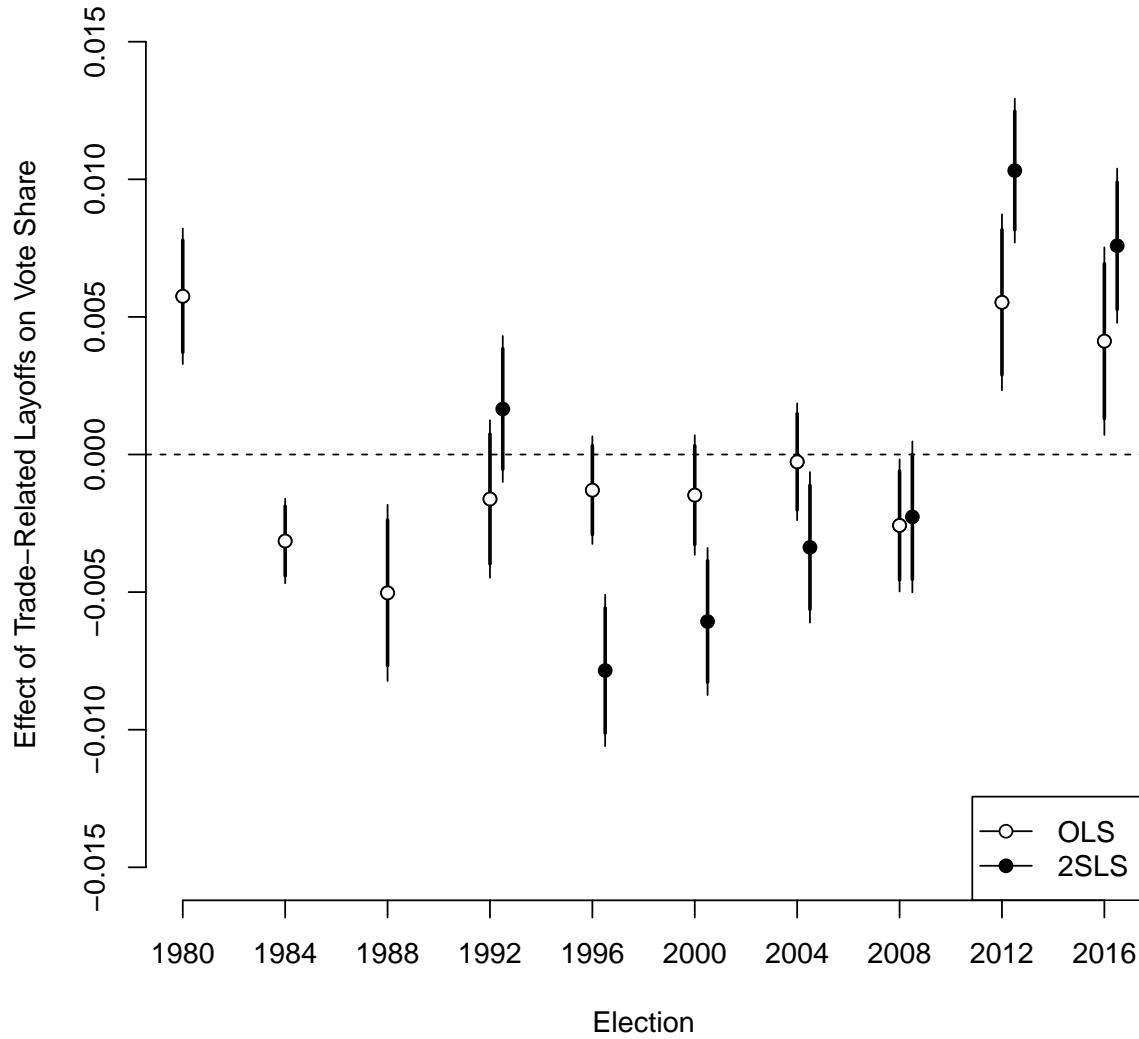


Figure E.1: Republican party vote share in each presidential election from 1980 to 2016. White circles are OLS estimates where instrumental variable data is not available. Black circles are estimates using the import penetration instrument from Autor, Dorn and Hanson (2013).

counties to determine who would have won each state. Under this simulation, there are four states in which the outcome would have flipped for Clinton had the electoral bump from trade been absent: Florida (29 electoral votes), Pennsylvania (20), Michigan (16), and Wisconsin (10). Any three of these would have been enough to alter the election's outcome of 304 electoral votes for Trump versus 227 for Clinton.

These results are consistent with similar analysis conducted by Autor et al. (2016*b*) who find a Republican electoral advantage in areas most negatively affected by Chinese import penetration. Without geocoded exit poll data, it is impossible to directly test the mechanism of trade policy preferences and nativist views on individual votes. However, this analysis, combined with the opinion results presented in the main body of the paper, are consistent with an important role played by trade shocks in the election of Donald Trump in 2016.

## F The Intuition of Trade Opinions in a Local Sociotropic Context

### F.1 Existing Theories

Traditional models of trade preferences start from the assumption that individuals think as though they are rational economic factors of production. Informed by seminal economic models such as the Heckscher-Ohlin or Ricardo-Viner frameworks which offer predictions on who wins and who loses when countries open to trade (see Stolper and Samuelson (1941)), these IPE models assume that (1) individuals connect economic outcomes to trade and (2) base their trade policy preferences on what maximizes their wealth. Connecting policy preferences to policy outcomes requires an additional assumption about the barriers to collective action for these groups. For trade to matter in politics, Gene Grossman and Elhanan Helpman argue that the losers must be sufficiently concentrated (either in terms of geography or shared interests) to effectively advocate for protection from free trade (Grossman and Helpman, 1992).

The majority of empirical work that focuses specifically on trade opinions looks for which dimensions of producer identity are most prognostic of trade opinions. These approaches typically consist of comparing different proxies for producer identity (i.e., industry, skill level, or firm productivity) in terms of how well they predict trade opinions (see Scheve and Slaughter (2001), Mayda and Rodrik (2005), and Naoi and Urata (2013)). More recent empirical research expands the economic identities of interest to include consumers (see Baker (2005) and Naoi and Kume (2015)). Nevertheless, the vast majority of IPE research on trade opinions starts from the assumption that individuals rationally update their policy preferences based on how trade affects their economic outcomes.

More recent IPE research has started to apply cognitive models of preference formation in order to understand how opinions on trade are influenced by social and psychological factors. One notable conclusion from this research is the relationship between out-group anxiety and protectionist preferences (see Mansfield and Mutz (2009, 2013), Margalit (2012), and Daniels and Von Der Ruhr (2005)). Research on the informational mechanisms by which individuals learn about trade suggests that elite communication is an important determinant of individual preferences (see Hicks, Milner and Tingley (2014) and Naoi and Urata (2013)). Similarly, aversion to risk (Ehrlich and Maestas, 2010) and inequity (Lü, Scheve and Slaughter, 2012) are also demonstrated to be prognostic of trade preferences.

The research summarized above treats preferences as fixed and attempts to explain observed variation by individual characteristics. In my paper, I look at how exogenous changes in an individual’s environment produce changes in their trade policy preferences. I sketch out my intuition of preference formation based on cognitive models pioneered by Fishbein and Ajzen (1977) and Iyengar (1990).

## F.2 The Cognitive Intuition for Preference Formation in a Local Context

I define a person’s preference as their utility comparison between alternatives. An individual confronted with two objects has a preference for one over the other based on which maximizes their expected utility. Embedded in this definition are two attributes that warrant further discussion.

First, the utility comparison is a prospective cognitive exercise. The individual must project their utility conditional on obtaining each of the objects and then rank order these projected utilities. For example, when comparing a liberal and a protectionist trade policy, the individual must assess her utility under both policies before arriving at her policy preference.

Second, each object is comprised of myriad attributes (or “features”) that bear on the utility calculation. These features differ in both their impact on an individual’s utility as well as in their salience (or “access”) to the individual. For example, a free trade agreement’s features include the identity of the trading partners, the instruments (i.e., tariffs or quotas) that will be affected, the projected impacts on local and foreign labor markets, and even the implications for international conflict. Depending on the personal experience of a given individual, different features may be more or less salient.

The localness of sociotropic effects enters the framework through the accessibility of different features. Put simply, the salience of a certain attribute of free trade is a function of an individual’s exposure to it, independent of whether she experiences the attribute directly. The further away from home trade’s negative consequences are, the less salient these consequences are in the weighted assessment used to develop an opinion.

Thus, preference formation occurs at the intersection of personal experience and external events. Both factors influence the accessibility of different features which in turn determine the prospective utility comparison. An auto worker in the United States is more likely to access the local labor market implications of a free trade agreement with South Korea and predict a negative utility outcome as a result, based purely on her experience as an auto worker. The barista who serves the auto worker may not naturally access the same feature based on experience alone. But should the auto worker lose her job, she may no longer patronize the barista’s coffee shop, reducing the barista’s tips. Now the barista’s access to the negative labor market consequences of free trade increases due to external events and he also updates his policy preferences in favor of protectionism.

The example summarized above captures how experience and external events interact to generate policy preferences but is overly simplistic. In a complex environment with so-

phisticated policy choices, it is heroic to assume that changes in access to features occur naturally. However, political entrepreneurs can facilitate access to certain features by creating a narrative that helps individuals understand their situation. Donald Trump provided a ready narrative that increased access to both trade's negative effects as well as the attendant features of immigration and globalization.

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