

How Legislators Respond to Localized Economic Shocks: Evidence from Chinese Import Competition

James J. Feigenbaum, Harvard University
Andrew B. Hall, Stanford University

We explore the effects of localized economic shocks from trade on roll-call behavior and electoral outcomes in the US House, 1990–2010. We demonstrate that economic shocks from Chinese import competition—first studied by Autor, Dorn, and Hanson—cause legislators to vote in a more protectionist direction on trade bills but cause no change in their voting on all other bills. At the same time, these shocks have no effect on the reelection rates of incumbents, the probability an incumbent faces a primary challenge, or the partisan control of the district. Though changes in economic conditions are likely to cause electoral turnover in many cases, incumbents exposed to negative economic shocks from trade appear able to fend off these effects in equilibrium by taking strategic positions on foreign-trade bills. In line with this view, we find that the effect on roll-call voting is strongest in districts where incumbents are most threatened electorally. Taken together, these results paint a picture of responsive incumbents who tailor their roll-call positions on trade bills to the economic conditions in their districts.

asting roll-call votes ranks among the most visible activities of incumbents, granting them opportunities to take clear policy positions and communicate them to constituents (e.g., Mayhew 1974). Voters care about roll-call votes, favoring incumbents who compile more moderate roll-call records (Ansolabehere, Snyder, and Stewart 2001; Burden 2004; Canes-Wrone, Brady, and Cogan 2002; Erikson and Wright 2000) and exhibiting at least some awareness of, and preferences over, their representatives' specific positions on important votes (Ansolabehere and Jones 2010; Brady, Fiorina, and Wilkins 2011). Despite these facts, incumbent roll-call records display a pronounced within-district divergence, with Republicans and Democrats offering starkly different positions regardless of local preferences (e.g., Bafumi and Herron 2010; Lee, Moretti, and Butler 2004; McCarty, Poole, and Rosenthal 2009). Whether because of personal preferences, party whipping, or other forces, “the choices voters face locally mainly

reflect national positions of the parties” (Ansolabehere et al. 2001, 152).

A separate literature in American politics documents how well economic conditions predict US electoral outcomes (Fair 1978, 2009; Kramer 1971). Voters often “punish” incumbents for economic shocks, even when they likely played no role in their creation (e.g., Achen and Bartels 2004; Bartels 2009; Gasper and Reeves 2011; Healy, Malhotra, and Mo 2010). Despite the salience of economic conditions to campaigns, we understand little of the dynamics that occur *inside* the legislature in response to these conditions, especially when these conditions change unevenly across localities.¹ A hypothesis linking these two literatures together—one for which we find consistent empirical support in this article—is that, even if incumbents generally do not cater their roll-call votes to local constituents, economic roll-call votes are an exception because of their unusual importance to voters.

James J. Feigenbaum (jfeigenb@fas.harvard.edu) is a PhD candidate in the Department of Economics at Harvard University, as well as a doctoral fellow with the Harvard Multidisciplinary Program on Inequality and Social Policy. Andrew B. Hall (andrewbenjaminhall@gmail.com, <http://www.andrewbenjaminhall.com>) is an assistant professor in the Department of Political Science at Stanford University.

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1. There is evidence, though, that voters are aware of local economic conditions and use them to inform their beliefs about national economic conditions (Reeves and Gimpel 2012). In fact, Bisgaard et al. (2015) argue that, in Denmark, perceptions of the national economy are driven by hyper-local, neighborhood-level economic conditions more so than municipality-level conditions. Further, Margalit (2013) shows how individual economic conditions—particularly job loss—can change a voter's support for welfare spending.

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To test this hypothesis, and to explore the links between economic conditions, incumbent behavior, and electoral outcomes more generally, we study quasi-random, localized economic shocks to congressional districts. We take advantage of the disproportionate shocks that occur when China begins exporting a good that a local area of the United States specializes in manufacturing. Autor, Dorn, and Hanson (2013a) find that Chinese import competition—or more broadly, any such exogenous import shock—increases unemployment, decreases labor force participation, lowers wages, and increases use of transfer payment programs and disability programs.² To circumvent the problem that places suffering economic downturns are likely to experience higher import exposure endogenously, we follow Autor et al. (2013a) in instrumenting for the import exposure that these areas face by using Chinese exports in these product spaces to other (non-US) countries. Using geographical information, we disaggregate the commuting zone level data on these shocks and attribute them to congressional districts.

We demonstrate that localized economic shocks from trade cause a pronounced and consistent shift toward protectionism on trade bills but no ideological change on other bills.³ We also investigate the mechanisms underlying this roll-call shift. By testing for heterogeneity in the effect across electoral contexts, we demonstrate that it is the result of incumbents tailoring their trade policy roll-call votes specifically and not the result of electoral turnover in the primary or general election or the result of incumbents becoming more liberal generally. Though the cited literature provides good reasons to believe that voters often blame incumbents for economic shocks, we find that incumbents avoid electoral effects in equilibrium, in our case, perhaps because they are able to take popular positions on foreign trade bills in response to these trade-based economic shocks.⁴ In line with

2. Notably, while the employment effects are concentrated in the manufacturing sector, Autor et al. (2013a) show that the wage effects extend to all sectors of the economy and contribute to a general decline in average earnings region-wide. Note that it may be that the trade shocks themselves or, perhaps more likely, effects of the trade shocks like those identified by Autor et al. (2013a) could drive legislator response. To the extent that labor is mobile between regions, the effects of these trade shocks on both economic outcomes and on the political outcomes that we consider will be diluted. However, the regional economics literature finds consensus that migration in response to labor demand shocks is both slow and incomplete (see, e.g., Blanchard and Katz 1992; Glaeser and Gyourko 2005).

3. There are likely other, non-roll-call effects of import exposure on legislator behavior. However, we are unable to measure outcomes like ITC lobbying or trade-related speech making.

4. In identifying a way in which anticipatory incumbents are able to avoid electoral effects in equilibrium, our findings are similar to those in Clinton and Enamorado (2014), where incumbents are seen to be able to

this view, we establish that the protectionist roll-call response to negative trade shocks is largest in competitive districts, suggesting that incumbents are most responsive to local economic conditions when there is a real electoral threat.

To illustrate our analysis, consider Representative Howard Coble (R, NC), who represented the 6th district in North Carolina throughout our sample period, serving from 1985 to 2015. Coble was a member of the conservative Republican Study Committee, and later a member of the Tea Party Caucus as well. During the 1990s, Coble was in the top decile for conservatism on nontrade bills and was a general supporter of free-trade agreements (including a 1993 vote in favor of NAFTA). Based on our measures of free-trade support, which we describe in subsequent sections, during the 1990s Coble ranked in the top 15% of all House members and in the top 25% among Republicans. But the NC 6th district was hit by a large, negative trade shock during the 2000s; only 8% of districts endured more severe import competition from China. These shocks were driven in large part by the district's specialization in kitchen-cabinet manufacturing and in yarn and thread mills, two manufacturing subindustries in which Chinese imports rose dramatically during the 2000s. In response, Coble shifted his voting record on trade toward protectionism. While still maintaining a strongly conservative voting record on other issues—in the 2000s, Coble remained among the top 10% most conservative representatives in the House—he broke with party orthodoxy, and with his previous track record, on trade bills. In 2003, Coble voted against both the Chile and Singapore free trade agreements. In 2005, he voted against implementation of the free trade agreement with the Dominican Republic and Central America, known as DR-CAFTA. As a result of these and other votes, Coble moved, in the 2000s, from the top 25% of the most free-trade Republicans to the bottom 5%—more than two standard deviations more protectionist than his party's median. As our formal analysis will show, Howard Coble is far from alone in this behavior. As we will establish, Members of Congress (MCs) carefully tailor their roll-call positions on trade bills in response to localized shocks from trade.

The remainder of the article is organized as follows. In the next section, we discuss the theoretical motivations for our study and explain why the empirical analyses that we carry out are relevant for our theoretical understanding of political processes. Next, we briefly describe the major data sets used in the analyses. Following that, we explain the techniques we use to measure roll-call positioning on trade bills and economic shocks from trade. Next we present a series of

fend off the effects of the introduction of Fox News by altering their roll-call behavior.

empirical analyses investigating the effects of localized shocks from trade on roll-call voting and electoral outcomes. Subsequent to these results, we explore effect heterogeneity that informs theories of legislative behavior and points to particular causal mechanisms. Finally, we conclude by discussing the implications of these findings.

THEORETICAL PERSPECTIVES

How legislators cast roll-call votes—and more generally, how they structure the policy portfolio they offer to voters—is a key question in American politics. A central goal of the Democratic system is to translate the preferences of constituents into government action through the electoral mechanism. By forcing incumbents to anticipate reelection needs, regular Democratic elections are thought to create responsive public policy. An extensive literature, stemming from Downs (1957), formalizes these ideas and predicts that legislators should cast roll-call votes—in addition to other such activities—in a manner consistent with the desires of the district's median legislator.

Despite this intuitive prediction, a large body of empirical evidence establishes the failure of the median voter theorem in US elections. Studying the US House, Ansolabehere et al. (2001), Bafumi and Herron (2010), Lee et al. (2004), and McCarty et al. (2009) all show that Democratic and Republican candidates offer consistently different positions even when running for election in districts with similar underlying partisanship. A related literature also explores the surprising degree to which incumbent positions appear inflexible. Examining how US House legislators' positions change over time, Poole and Rosenthal (2000) conclude: "we find remarkable and increasing stability... Members of Congress come to Washington with a staked-out position on the continuum, and then, largely die 'with their ideological boots on'" (8). Rather than adapting to the desires of citizens, incumbents appear to offer fixed and unchanging platforms. Partly in response to these findings, so-called citizen-candidate models (Besley and Coate 1997; Osborne and Slivinski 1996) offer a compelling explanation for this rigidity. These models offer a view of elections in which candidates cannot credibly commit to implementing any policies—or voting on any bills in the legislature—in any manner inconsistent with their own, personal beliefs. This inability to commit, a relatively extreme but illuminating assumption, produces equilibrium outcomes in which elected legislators are unresponsive to citizen preferences.

Empirical reality is likely to lie somewhere between the extremes of full flexibility, as in the Downsian model, and full rigidity, as in the citizen-candidate model. On the one hand, we know that candidates are likely to come to campaigns

with preexisting views of their own. There is also good evidence that they cannot easily change their positions—even if voters would prefer different ones than those they offer—without appearing as "flip-floppers" (Tomz and Van Houweling 2015). On the other hand, we also know that politicians are highly strategic. Concerned with their ability to gain reelection (Mayhew 1974), they spend a great deal of effort getting to know constituents, learning their desires, and attempting to implement them (Fenno 1978).

In this article, we investigate one particularly important dimension on which, we argue, incumbents are likely to be flexible: economic policy. Why might we suspect incumbents to be flexible on economic policy even as they are rigid in most of their positions? Our argument is that the unique salience of economic issues to American voters forces incumbents to adapt to their districts' changing desires in this issue area even as they remain immovable on other issues. A large literature documents how responsive American voters are to economic conditions (e.g., Fair 1978, 2009; Kramer 1971). The behavior of candidates conforms to this belief. Bill Clinton's campaign motto was famously "it's the economy, stupid." For this reason, we hypothesize that incumbents, though generally inflexible in their positions, will be surprisingly flexible on economic issues in response to economic conditions, because of their need to ensure reelection at the hands of voters who care disproportionately about economic issues. In particular, we predict that legislators will respond to negative economic shocks by adopting more protectionist policy positions in order to fend off electoral harm.

Implicit in this argument is the idea that economic shocks make citizens demand more protectionist policy. Moore, Powell, and Reeves (2013) study how the economic interests of constituents might drive legislator preferences, focusing on the presence of auto workers in a congressional district. They find that local auto workers influenced roll-call votes of representatives on two recent salient pieces of legislation with direct effects on the auto industry: the 2008 bailout and the 2009 "cash for clunkers" program. However, across other bills supported by the auto industry and its workers but with lower salience, the influence of auto workers wanes. Like Moore et al. (2013), we consider how district-level economic actors can influence legislator roll-call voting, both overall and on issue-specific votes. Echoing their results, we find effects on trade bills but not on other ideological issues. However, while Moore et al. (2013) find the influence of auto workers concentrated on high salience bills, our results generalize to all trade roll-call votes, which includes both high and low salience bills. The difference could be that trade is generally more politically salient than bills having to do with the auto industry; Margalit (2011), for example, shows that presi-

dential vote shares are especially sensitive to job loss from foreign competition.

Because we focus on reelection concerns, we also predict variation in the effect of economic shocks. Though all incumbents may be responsive to economic conditions, those most threatened electorally—that is, those in competitive districts—should be most responsive. In safer districts, with reelection prospects more secure, incumbents may be able to revert to the rigid pattern of positions that the literature has documented for most issue areas. In addition, in testing for flexibility, we must be sure to distinguish it from the mechanism of electoral replacement. We may find that, over time, districts that experience economic shocks see their representatives become more protectionist, but we must take care to investigate whether this ideological shift is the result of a single incumbent changing her position or the result of the voters in the district sending a new representative in her place. Finally, because our hypotheses concern the tailored way in which legislators respond specifically to trade shocks, we should not observe shifts in legislator roll-call voting on *nontrade* bills if our explanation is correct. We test for this too in the coming analyses.

In this section, we have provided theoretical motivations, explained our focus on localized economic shocks, and have laid out the specific tests we will undertake to learn about incumbent positioning. We now turn to describing the data used to perform these tests.

DATA

The analysis draws on five main data sets. We focus on the period 1990–2010, which comprises the full overlap of the data sources and contains China’s emergence as a major source of exports. We divide this period into two decades because the economic data are aggregated to the decade level. We include 431 House districts in our sample. We drop Alaska’s at-large district and Hawaii’s two districts from the analysis due to missing economic data. In addition, we drop Vermont’s at-large congressional district because Bernie Sanders—who represented Vermont in the House from 1991 to 2006—is the only member of a third party in our sample.

The first data set is based on data collected by Autor et al. (2013a), which measures economic activity and import behavior for 1990–99 and 2000–2007. We measure trade shocks to congressional districts at the decade level in terms of import exposure per worker. More details on the construction of these measures will be given below. We combine the County Business Pattern data, which measures the size of the labor force in each county in a given industry, with industry and trade partner level import data from UN Comtrade, which measures the degree of Chinese import compe-

tition faced by a given industry. We spatially merge these data sets from the commuting zone level to the congressional district level. We follow Autor et al. (2013a) in measuring trade shocks for the 2000–2007 period, rather than 2000–2010, because of the large and negative effects of the Great Recession on US manufacturing.

The second data set contains the roll-call votes of all US House members, 1990–2010, and comes from the raw roll-call vote data collected and organized on <http://www.voteview.com>. We use these roll calls to generate district-decade ideological scalings, using a method described below. These scalings are computed separately for two decades, the first spanning 1993–2002 and the second spanning 2003–10.⁵ By dividing the decades in this manner we ensure that the roll-call votes cast on behalf of districts are only scaled together within a single redistricting period.⁶ To be clear, roll-call votes are first cast on behalf of a new district one year after redistricting—hence starting the districts in 1993 and 2003—and roll-call votes cast in the year during redistricting are cast on behalf of the previous decade’s districts.⁷ We merge these scalings with the economic data, and we refer to the merged decades as the “1990s” and “2000s,” respectively.

The third data set provides information on the topical content of the bills voted on in the US House, 1990–2010, as collected and coded in the Rohde/PIPC House Roll-Call Database. We merge these codings with the roll-call votes. We consider “trade bills” to be those with issue codings running from 540 to 549, what the data set calls “foreign trade bills.” We do not include “domestic trade bills” as trade bills, due to the particular foreign shocks we are analyzing.⁸

The fourth data set is on US House elections, 1946–2010. This data set draws from a variety of primary sources, as

5. Data constraints prevent us from making the two roll-call decades symmetric by including 2011 and 2012 in the second decade. The two roll-call decades comprise the maximum number of years for which we have information on roll-call votes cast within specific issue areas within a redistricting period.

6. We have also performed the scalings using 1993–2000 and 2003–10, respectively, to keep with a more standard definition of “decade”; the correlation between the two-year cutoff is 0.98 and produces substantively identical results. We choose to keep in the roll-call data for 2007–10 for purposes of efficiency, but substantively identical results are obtained using only the exact years for which the economic activity data is measured.

7. For simplicity, we do not directly account for the few states that underwent “off-cycle” redistricting during these two decades. Ignoring these changes biases our effect of interest toward zero, although it is unlikely to affect estimates much.

8. For example, NAFTA (H.R. 3450 in the 103rd Congress) is considered a foreign trade bill and is included in the analysis, while the “Prompt Notification of Short Sales Act” (S. 2120 in the 112th Congress) is coded as a domestic trade bill and is omitted.

collected by Dubin (1998) and extended in a series of articles such as that by Ansolabehere, Snyder, and Stewart (2000). And finally, the fifth data set covers US House primary elections, 1946–2010, as compiled by Ansolabehere et al. (2010).⁹

EMPIRICAL STRATEGY

Two methods for constructing trade-specific roll-call scores

We generate roll-call scalings for each congressional district using two completely separate techniques, both of which yield the same substantive results. Because the economic shock data is aggregated at the decade level, we produce these scalings at the decade level, analyzing all roll-call votes cast on behalf of the district within each decade as defined by redistricting and subject to our data constraints—namely, 1993–2002 and 2003–2010. Throughout we refer to the scalings on trade bills as measuring a “protectionist” versus “free trade” dimension of ideology (a claim we are careful to validate). While there may be a correlation between being liberal overall and taking more protectionist positions, our scalings never assume any such link.

Technique 1: Interest group codings of trade bills. In the first technique, we scale the roll-call votes MCs cast for their districts using interest-group codings of free-trade bills. We collected data on “free-trade” roll calls from the Cato Institute’s “Free Trade, Free Markets: Rating the Congress” report.¹⁰ The Cato Institute classifies trade bills into two categories, barriers to trade and trade subsidies, and it identifies whether the ‘yea’ or ‘nay’ vote on each bill is the “free trade position.” We merge these bills with the Voteview roll-call database, and we calculate the proportion of time among these bills that each district votes in the “free trade” direction.¹¹ We focus on trade barrier bills since these are the ones obviously related to foreign trade shocks in the district.¹² Specifically, we first construct the variable

$$CatoVote_{ib} = \begin{cases} 1 & \text{if Cato position is 'yea' and district} \\ & \textit{i}'\text{s legislator votes 'yea' on trade bill } b, \\ 1 & \text{if Cato position is 'nay' and district} \\ & \textit{i}'\text{s legislator votes 'nay' on trade bill } b, \\ 0 & \text{otherwise.} \end{cases}$$

For each district i in each decade, we then calculate

$$CatoScore_i = \frac{1}{B} \sum_{b=1}^B CatoVote_{ib},$$

where B is the total number of trade bills voted on in Congress in a given decade.

There are several advantages to this first technique. First, it leverages substantive information over the content of bills to ensure that we are tapping into the free-trade vs. protectionist dimension. Importantly, while this protectionist dimension might be correlated with party—we might expect Democrats to be, on average, more protectionist in the recent era—it is not constructed using any information on party. Second, it allows for a simple calculation of the degree to which a district’s representative or representatives are pro- or anti-free trade, because we can average over the votes cast for or against the free-trade position. As a result, this technique avoids the need to apply any modeling or to make any statistical assumptions. However, in using this technique we are relying on a single group’s codings of a select number of bills. To make sure that this does not drive our results, we also perform all analyses with a second, completely separate method of coding bills.¹³

Technique 2: Algorithmic roll-call scaling. The second technique avoids the use of preexisting group codings but requires applying a more in-depth algorithm with its own costs and benefits. In this approach, we generate a simple scalar summary of each roll-call voting on trade bills and on all other bills (separately) by decade using a simple regression of each district’s representative’s (or representatives’) vote on each bill on district and year fixed effects (Fowler and Hall 2013). First, we randomly guess the direction of each bill and code this as 0 or 1 (we can think of these directions as “left” or “right,” but they are completely arbitrary and not based on party). Given these guesses, the method estimates a regression of the form

9. Both the primary and general election data sets were generously provided by Jim Snyder.

10. See <http://www.cato.org/research/trade-immigration/congress>.

11. For barrier bills, we use the subset of Cato’s bills that match the PIPC issue area codings. When conducting a placebo analysis with the trade-subsidy bills, this is not possible because only two of Cato’s trade subsidy bills are in the foreign trade issue area in PIPC. Thus, for the placebo, we include all of Cato’s subsidy bills.

12. The majority of bills included by Cato as a trade subsidy are votes on the Farm Bill and on other farm and crop subsidies. For example, in the 107th Congress, of the six trade subsidy votes identified by Cato, two were for cuts of subsidies (wool and mohair; sugar, respectively), two were votes on the Farm bill (the House version and final passage), and another was to limit farm subsidy payments. The final vote was to defund the Export-Import bank. While these bills were all related to trade, we do not expect

them to be as linked to trade shocks as the votes on free trade and tariffs included by Cato in the barrier bills grouping.

13. In practice, almost all estimated results are stronger when using the *CATOScore*. We have chosen to present estimates in parallel with this second scaling to emphasize the robustness of the findings.

$$Y_{ib} = \gamma_i + \delta_b + \varepsilon_{ib}, \quad (1)$$

where Y_{ib} is a dummy indicating that district i voted to the “right” on bill b .¹⁴ The variables γ_i and δ_b represent district and bill fixed effects, respectively. The coefficients on the district fixed effects summarize how often the district’s representative voted to the right or left. For interpretability, we omit the median district’s fixed effect so that these coefficients reflect voting behavior relative to the median.

The method then iterates to convergence. Given the estimated equation, each bill is checked one-by-one. Those for which the coefficients on the district fixed effects are correlated with the observed ‘yea’ or ‘nay’ remain unchanged, while the others are recoded so that the direction of the bill is flipped. So, for example, if according to the district estimates the left-leaning districts voted ‘yes’ on a bill but the bill is currently coded as a “right”-leaning bill, the bill is recoded to be “left.” Within a few iterations, the method converges so that all bills are coded in agreement with the estimated voting behaviors of the districts. The result is a simple scalar summary of roll-call behavior. For more technical details as well as a full battery of validity tests on regression-based scaling more generally, see Fowler and Hall (2013).

We only choose this technique over more conventional options in the present case (e.g., Clinton, Jackman, and Rivers 2004; Heckman and Snyder 1997; Poole and Rosenthal 1985) because it performs well with small numbers of bills. This allows us to scale legislators using only trade bills, even though there are relatively few of these per congress.¹⁵ To verify the scalings, however, we have also applied W-NOMINATE to the trade roll calls by decade.¹⁶ The resulting scalings correlate with ours at 0.98 but produce noisier estimates when used in our regression analyses—likely due to measurement error from the small number of bills. All of our subsequent findings, however, are substantively unchanged using either the trade scores or the Cato scores.

Using this regression-based method, we estimate district-decade scalings for all trade bills and for all nontrade bills, separately. We call the resulting trade-bill estimates *trade scores*, and we rescale them so that they are in percentage points. Thus, a district with a trade score of -10 is a district

that is 10 percentage points less likely than the legislator from the median district to vote in the rightward direction on a trade bill.¹⁷

Figure 1 compares the estimated trade and nontrade scores for each decade.¹⁸ Though both trade and nontrade bills display a marked amount of unidimensionality—and the correlation between the two scalings is 0.89—there is clearly variation in the way that legislators situate themselves on trade bills versus all other bills.¹⁹ Much of this variance could be the result of fixed constituent interests or personal legislator preferences. However, as the rest of this article shows, changes in local economic conditions help explain these differences too.

Finally, figure 2 shows that the two measures, Cato scores and trade scores, match well. Data points heap somewhat because the Cato scores take on far fewer values than do the trade scores.²⁰ Overall, though, the “Cato score” on barrier bills correlates with our trade score measure at 0.80 and, as we show in the analyses below, produces identical substantive conclusions as the trade score measure—and larger effect sizes. This gives us confidence in the robustness of our findings and also in our interpretation of trade scores as measuring a protectionist-free-trade dimension of preferences.

Leveraging exogenous economic shocks from Chinese import competition

The difficulty in understanding many of the effects of global competition derives in large part from the complexity of measuring import competition with sufficient variation to enable empirical analysis. We avoid this problem using both the variation in regional industrial specialization and the variation in industry level import mix to measure differ-

17. The most liberal district on nontrade bills in the data set is Florida’s 23rd district in the redistricting cycle from 1993 to 2002, represented for the entire period by Democrat Alcee Hastings. The leftmost district on trade bills, however, is Arizona’s 7th district from 2002 on, represented for the entire decade by Democrat Raul Grijalva. Grijalva’s stances on free trade are what might be considered “protectionist.” He voted against the CAFTA implementation bill (HR 3045), against the US-Singapore Free Trade Agreement (HR 2739), and against the United States-Chile Free Trade Agreement Implementation Act (HR 2738)—all bills with significant Democratic support.

18. A color version of this graph is available in the appendix.

19. Note also that the horizontal axis range differs across the two decades. This is the result of (a) a greater clustering of positions representing a more cohesive Democratic party in the 2000s and (b) the differing positions of the median legislator across the two decades.

20. In addition to heaping, there appears to be a change in the overall distribution of points between the two decades, with more distinct clusters of points in the 2000s than in the 1990s. We suspect that this change is the result of increasing polarization over the two time periods.

14. Since Y_{ib} is a binary variable, this regression represents a “linear probability model.” Since all of the explanatory variables are dummies, however, this “model” represents a simple set of conditional means.

15. There are 136 total bills across the two decades: 81 bills in the first decade and 57 in the second.

16. To do so we used the WNOMINATE package in R. Following convention, we fit the model using two dimensions and then extract the scores from the first dimension to use as our measure.

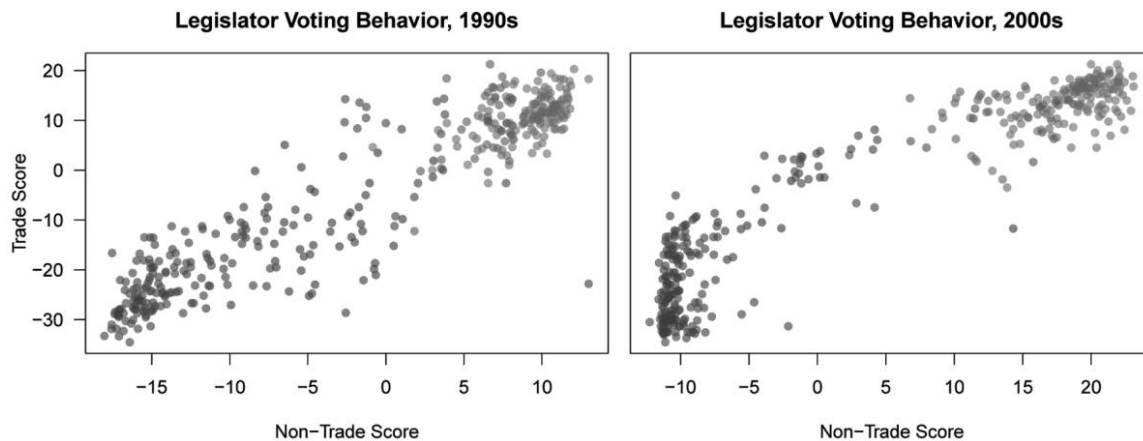


Figure 1. Legislator voting behavior on trade bills vs. all other bills. Legislator trade scores and nontrade scores are highly correlated ($r = 0.89$), but legislators appear to have some leeway to deviate from their overall ideological portfolio when voting on trade bills. Points are colored in a range from dark gray to light gray, indicating the share of that decade the district is represented by each party (fully light gray districts are always Republican; fully dark gray districts are always Democrat).

ential trade shocks at a local economic level.²¹ Measuring regional industrial specialization is relatively straightforward: we count the number of workers in the region in a given industry relative to all workers in the region. However, measuring changes in import competition is more complex.

We focus on changes in import competition from China for two main reasons. First, the rise of China as an American trade partner has been rapid and large, thereby giving us as researchers the chance to evaluate meaningfully large economic effects. Between 1992 and 2005, China's imports to the United States increased more than 500%, measured using either US or Chinese data (Amiti and Freund 2010). The second reason we focus on China and not other major American trade partners like Mexico or Canada is identification. The rise of China as a source of import competition for the United States has been driven in large part by productivity growth in China and changes in global trade policy—notably, China's entry to the World Trade Organization (WTO) in 2001. While the United States is China's main trade partner by total export value, the share of Chinese exports sent to the European Union is similarly large (17.2% vs. 16.3% according to the WTO). Chinese exports to Japan and South Korea are also quite large. In contrast, the United States is the destination of nearly 78% of Mexi-

can exports by value; for Canada, 74.5% of exports by value are sent to the United States.²² Thus, any increases in Mexican or Canadian exports in any given industry are much more likely to be driven by conditions within those industries in the United States.²³ If those domestic conditions also have political effects—weakening special interests or changing local economies—we would be unable to estimate the causal effect of trade shocks on any outcomes. While it would be valuable to measure precisely the political effects of Mexican or Canadian import competition, we are unable to do so in our current identification framework.²⁴

Specifically, following Autor et al. (2013a), we define import exposure per worker as

$$\Delta IPW_{uit} = \sum_j \left(\frac{L_{ijt}}{L_{ijt}} \right) \left(\frac{\Delta M_{ucjt}}{L_{it}} \right), \quad (2)$$

where i is the region (commuting zone), j is the industry (roughly, four-digit SIC codes), and t is the time period (the 1990s or the 2000s). The subscripts u and c identify US and Chinese national-level variables, respectively. The number of workers in region i , industry j , and period t is L_{ijt} . The total number of workers in the United States working in industry j in year t is L_{ujt} . Their ratio thus forms the share of a given industry's workers in region i . This can be used to measure the expected exposure to industry-level shocks

21. Naturally, our measure of trade shocks will draw some variation from differences across regions in terms of overall labor share in manufacturing. However, this variation in manufacturing employment explains only one quarter of the variation in trade shocks. The bulk of the variation in trade shocks between regions is driven by within-manufacturing specialization in different industries. While some industries—including footwear, apparel, furniture, and electrical appliances—faced huge increases in Chinese import competition during our sample period, other industries—e.g., automobiles—did not.

22. These statistics are from the WTO Country Profiles available at <http://stat.wto.org>.

23. These import-export flows were likely also driven by the passage of NAFTA in 1993.

24. To the extent that our estimates of the effects of Chinese-driven trade shocks are generalizable to all trade shocks hitting the US economy and political system, we do provide a rough guide to the possible political effects of other trading partners.

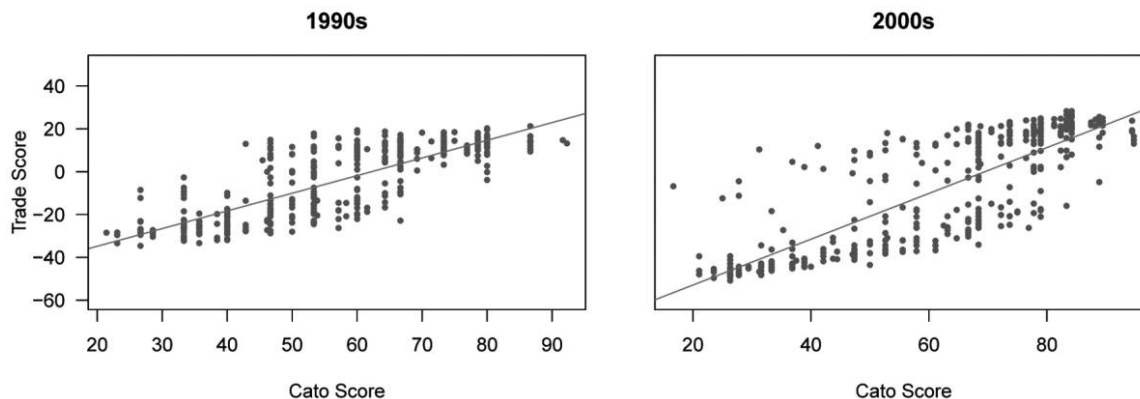


Figure 2. Trade scores and Cato scores. Comparison of district-decade trade scores, calculated from an unsupervised roll-call scaling method, to Cato scores, calculated from the Cato Institute’s coding of bills as pro- or anti-free trade. The measures correlate with each other at $r = 0.8$.

in region i . Given the high levels of regional specialization at the industry level, there is large geographic variation across regions in the potential effects of a given shock to an industry.

The change from $t - 1$ to t of the value of Chinese imports to United States in industry j is ΔM_{ucjt} . The total labor force in region i in year t is L_{it} . Their ratio is then the import shock from Chinese competition in industry j across all workers in region i . The product of these two ratios scales the import shock in a given industry by the exposure to import competition in that industry and region. Summing these terms over all industries gives us the total import shock (or import exposure) per worker in a region.²⁵

However, there is clear cause for concern about endogeneity with these import shock measures. Import shocks may be caused by changes in the United States. In particular, local economic conditions may create an import demand shock, either within an industry or a region, determining the flow of imports from China and other importing countries. To address these concerns, we follow Autor et al. (2013a) and use an instrument that depends both on Chinese import growth to other rich, Western economies,²⁶ as

well as lagged US labor force shares from the previous decade. During this time period, the growth of China’s export sector was driven by increasing competitiveness of manufacturers in China, relative to both the United States and other Western trading partners (Autor et al. 2013a). Specifically, we define the import exposure per worker instrument as

$$\Delta IPW_{oit} = \sum_j \left(\frac{L_{ijt-1}}{L_{ujt-1}} \right) \left(\frac{\Delta M_{ocjt}}{L_{it-1}} \right), \quad (3)$$

where we use the o subscript to denote super-national variables referring to these other rich economies. The first ratio term is simply the lagged version from the previous expression and measures the expected exposure to shocks in industry j in region i in the United States. We assume that industrial labor mix in the previous decade is a good proxy for industrial labor mix in the current decade. However, unlike the current employment share, which could be simultaneously determined by Chinese trade patterns, the lagged version is unaffected by Chinese trade shocks. The change in Chinese imports in industry j and time period t to the other countries o is ΔM_{ocjt} . We instrument for ΔIPW_{uit} with ΔIPW_{oit} .

Aggregating commuter zone shocks to the congressional district level

To construct our measures of both import exposure per worker and the instrument, we follow the methods described in Autor et al. (2013a).²⁷ Data from UN Comtrade allow us to measure both ΔM_{ucjt} and ΔM_{ocjt} . Data from the

25. While increased trade with China and globalization were major geographically varied shocks to local US labor markets during our sample period, there were other large changes to the economy as well. Autor and Dorn (2013) document the large effects of technology and computerization of tasks in manufacturing and other sectors. To the extent that these shocks are correlated, the measured effect of trade shocks in this article could include the effects of technology shocks. However, as documented by Autor, Dorn, and Hanson (2013b), the trade and technology shocks are not highly correlated either over space or time in the United States. The technology shocks were largest in the 1980s and much more geographically dispersed than the Chinese trade shocks considered in this article.

26. Specifically, they are Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland. This set of countries is chosen based on data availability.

27. Complicating the construction, product, and industry codes are reported at different levels of aggregation and specificity in the various data sources. See Autor et al. (2013a) and especially the data appendix for a description of how the merging of trade data and labor force data is accomplished.



Figure 3. US commuting zones and congressional districts, 2000s. Commuting zones are in thin gray lines; congressional districts are overlaid in thicker black lines.

County Business Patterns describe employment by industry and county, which can be aggregated to the various labor force measures required above.²⁸ However, these measures are all constructed at the commuting zone (CZ) level, rather than at the congressional district (CD) level. There are 722 CZs in the continental United States, as compared to the 432 CDs, and every county in the country—urban, suburban, and rural—is assigned to a CZ. Figure 3 overlays the two: CZs are denoted by the thin gray lines, while CDs are denoted by the thicker black lines.

Using county-level commuting patterns from the 1990 Census, Tolbert and Sizer Killian (1987) and Tolbert and Sizer (1996) created groups of counties where residents were highly likely to commute within the zone and highly unlikely to commute outside of the zone. Thus, we follow Autor et al. (2013a) and others in treating CZs as local labor markets and as economically relevant and coherent regions where, by construction, the majority of the population both works and lives in the zone. An economic shock to part of the CZ should be felt by workers and voters throughout the CZ.²⁹

28. Trade shocks are measured at the commuting zone level, which are composed of multiple counties.

29. Though there may be spillovers to shocks to neighboring CZs, we expect the political effects of these spillovers to be second order. We have

To link with our political outcome data at the congressional district level, we spatially merge maps of CZs and CDs. More details on the data and this merge are available in appendix B (apps. A–E available online). From the 106th to the 110th congresses, 129 CDs were wholly contained within one given CZ; 118 were wholly contained for the 111th congress.³⁰ For these CDs, we assign the import exposure per worker in the whole CZ to the CD. In doing so, we assume that because the CZ is a relevant economic unit, the shock is equal across the zone, regardless of whether the plants or firms directly affected by the growth of Chinese trade are in a given CD. For the CDs that cross CZ borders, we assign the average of each included CZ, weighted by the

two main reasons to think these spillovers are unimportant. First, commuting zones are designed to capture the relevant sphere of economic activity economically, so shocks in one zone are unlikely to affect other zones (Autor et al. 2013a). Second, voters in one district are, in our view, unlikely to focus on conditions in other districts if these conditions do not reflect their own district's situation.

30. These districts are primarily located in urban centers and are geographically small. For example, throughout our sample period, both the MA 7th district and the MA 8th district were located entirely within the boundaries of CZ 20500, centered on Boston, MA. For another 55 CDs in the 106th congress and 56 CDs in the 111th congress, between 90% and 99% of the district's land area was within only one CZ.

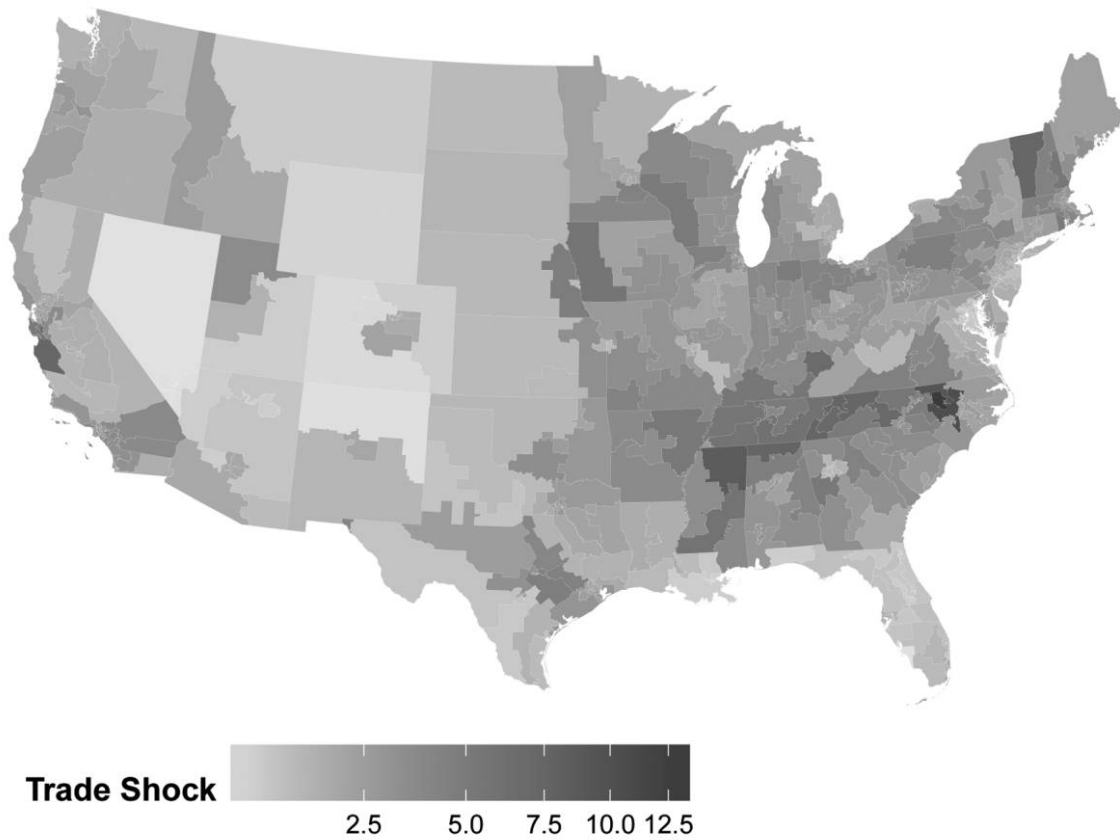


Figure 4. Distribution of trade shocks, US congressional districts, 2000–2007. Darker areas experienced more negative shocks.

CZ’s land area share of the CD.³¹ For example, between 1992 and 2000, the MA 3rd district was split across CZ 20500, centered on Boston, Massachusetts, and CZ 20401, centered on Providence, Rhode Island, and Fall River, Massachusetts. By land area, 70% of the CD was in CZ 20500 and 30% was in CZ 20401.³² Thus, the IPW for the CD is calculated as the $IPW_{20500} \times .7 + IPW_{20401} \times .3$.³³

Figure 4 presents the graphical distribution of these trade shocks in the 2000s. As the map shows, there is quite a bit of variation in the presence and severity of these shocks. Although some parts of the country (most notably a broad swath of the agriculture-focused Midwest) have little manufacturing and thus no trade exposure, major parts of the Eastern portion of the country, as well as some western

parts, do. More importantly, among the locales with more manufacturing, there is significant variation in the intensity of their exposure. This helps explain why we observe no correlation between instrumented trade exposure and partisanship, as shown later in the article.

Estimating causal effects from trade shocks

We are interested in measuring the relationship

$$Y_{it} = \beta_0 + \beta_1 \Delta IPW_{uit} + X_{it} \beta_2 + \varepsilon_{it}, \quad (4)$$

where Y_{it} is the estimated Cato score or trade score for the representative or representatives from district i in decade t , and ΔIPW_{uit} is the import exposure per worker in district i in decade t . The vector X_{it} stands in for a possible set of controlling variables. To isolate the causal effect of these trade shocks, however, we proxy for ΔIPW_{uit} using ΔIPW_{oit} as an instrumental variable as explained above. Thus we estimate

$$Y_{it} = \beta_0 + \beta_1 \hat{\Delta IPW}_{uit} + X_{it} \beta_2 + \varepsilon_{it}, \quad (5)$$

where $\hat{\Delta IPW}_{uit}$ are the predicted values of the trade shock from the first stage regression

$$\Delta IPW_{uit} = \pi_0 + \pi_1 \Delta IPW_{oit} + X_{it} \pi_2 + u_{it}. \quad (6)$$

31. As a robustness check, we assign to each given CD the IPW of the CZ covering the most area in the district. In addition, we also use detailed census block population data to weight by population instead of land share. Results are robust to these alternatives and estimates barely vary.

32. The split for the MA-3 was similar between 2002 and 2010 after redistricting, with 75% in CZ 20500 and 25% in CZ 20401.

33. As described in the Results section, we cluster our standard errors at the state by decade level to account for the fact that some CDs are parts of the same CZ and that some CZs are parts of the same CD.

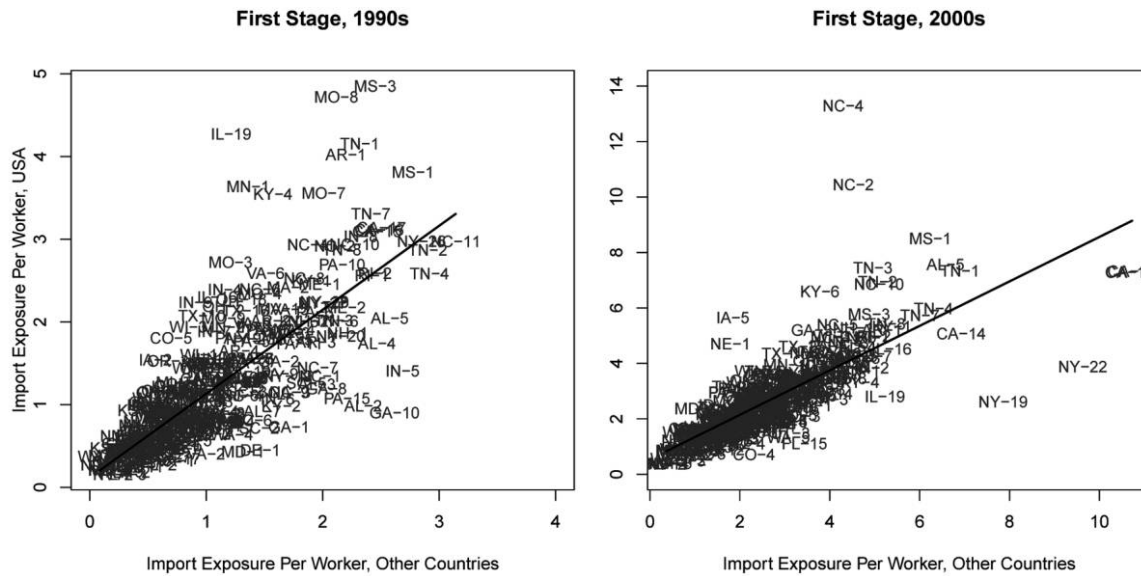


Figure 5. First Stage: Instrumenting for localized trade shocks in congressional districts using Chinese exports to other economies and lagged district labor force. For the 1990s, $F = 81.29$. For the 2000s, $F = 39.74$. Combining the two decades, the overall F -statistic for the first-stage is 271.97.

The quantity of interest β_1 measures the causal effect of trade shocks (as measured by import exposure) on trade roll-call bill voting behavior in the district under two primary assumptions. First, the instrument must have a first-stage effect. Figure 5 graphs the first stage (eq. [6]) for each decade, respectively. In both decades the first stage is extremely strong. For the 1990s, $F = 81.29$. For the 2000s, $F = 39.74$. Combining the two decades, the overall F -statistic for the first-stage is 271.97. This suggests that the division of CZs into congressional districts has successfully preserved the information from the original CZ-level analysis in Autor et al. (2013a), and it establishes that the “first stage” assumption of two-stage least squares is met.

Second, Chinese import exposure in other countries must not have a direct effect on roll-call voting behavior in the district except through its effect on district import exposure—the so-called exclusion restriction. Autor et al. (2013a) present a bevy of theoretical evidence and arguments for why Chinese exports to other major economies should not affect local US economies except through its effects on local economic conditions via the import shocks with which they are correlated.³⁴

Correlated product demand between the United States and other rich countries could be one potential threat to the exclusion restriction. Consider a simple example: If the demand for sneakers grows in both the United States and

other high-income countries, Chinese manufacturers may begin producing more sneakers. However, this increase in demand would also lead to more production of sneakers in the local labor markets of the United States specializing in this industry. In fact, as Autor et al. (2013a) point out, this would lead to an underestimate of the effects of Chinese import competition in the United States, biasing effects toward zero.³⁵

Negative productivity shocks in the United States could also drive increased Chinese exports to both the United States and other high-income countries as Chinese exports replace the faltering US manufacturers both domestically and abroad. We find this scenario unlikely given the huge increase in Chinese exports in a variety of industries over this time period. China’s share of global manufacturing exports rose from 2% in 1990 to 12% in 2007. In addition, China’s annual growth in total factor productivity (TFP) averaged more than 8%, faster than TFP growth in the United States or other major economies (Autor et al. 2013a; Brandt, Van Biesebroeck, and Zhang 2012). Finally, China also grew as an exporter to the United States relative to Mexico and other Central American countries, from 40% of imports to 64% between 1991 and 2007 (Autor et al. 2013a).

If these export decisions do not affect local economic conditions through other channels, it is unlikely that they

34. We review the most important of these arguments for our purposes here, but we encourage readers to consult their robustness checks for more information. For example, they report alternate results using a gravity model of trade.

35. Autor et al. (2013a) show that the effects of the Chinese trade shocks are similar in magnitude to the estimated effects of trade shocks derived from a gravity model of bilateral trade and conclude that import demand shocks are not a large concern in this setting.

would affect congressional roll-call voting behavior in the district. It is rather difficult to imagine why an incumbent would alter her behavior, or why voters would change their voting decisions, based on observing Chinese exports to other non-US economies in a manner separate from observing the resulting local economic effects.³⁶ Although the exposure measure does include information on the share of labor devoted to a particular industry in each district—which could plausibly be affected directly by Chinese export decisions in other countries—Autor et al. (2013a) use lagged labor force information to avoid precisely this issue.

We also report a variety of placebo tests in the analyses below which suggest—along with their substantive implications about legislator behavior and electoral outcomes—that the instrument is not exerting effects on the political climate except through its profound effects on local economic conditions. In addition, several of the analyses focus on contrasts *across* districts, which would difference out any fixed violation of the exclusion restriction.³⁷ For these reasons, we are comfortable with the IV exclusion restriction assumption for our case.

RESULTS

Localized shocks and protectionist voting

We first estimate the effect of localized economic shocks on district-level roll-call trade scores, using OLS and two-stage least squares to estimate equation (5). We include a decade fixed effect to ensure that we only compare districts within each decade, and we also control for the share of the decade that each district was represented by a Democrat, which significantly increases our precision. Because economic shocks can matter for electoral outcomes, it is possible that this variable is *post-treatment*. However, excluding it from the regression changes the estimate for the quantity of interest by less than two hundredths of a percentage point because while it is strongly correlated with roll-call voting, it is entirely uncorrelated with instrumented trade shocks

36. There is the possibility that some members of Congress could have connections to groups that might care about Chinese exports to other countries. For example, a member of Congress could be supported by interest groups who hold business interests in Europe. As a result, these groups could lobby the member of Congress based on Chinese export behavior in Europe. We cannot rule out this possibility, although we would point out that, since the member of Congress is unlikely to be able to influence economic activity in Europe (or elsewhere), such a pattern of behavior might be unlikely or at least relatively unimportant.

37. For example, we find that the roll-call response is strongest in more competitive districts—difficult to explain through an exclusion restriction violation but consistent with a story of legislative response to local economic conditions.

($r = -0.01$). We therefore include it for purposes of precision.³⁸

Table 1 presents the main results.³⁹ We focus on the third and fourth columns—labeled “IV”—which presents the two-stage least squares results. In these columns, a \$1,000 increase in import competition per worker is estimated to cause a 0.7 percentage-point decrease in the probability that the district’s representative casts a “right-leaning” or “free-trade” vote in Congress, according to the trade score measure, and a larger 2.10 percentage-point decrease in the probability that the district’s representative casts a “free-trade” vote according to the Cato score. A likely explanation for the fact that the effect size is substantially larger with the Cato score is that the Cato Institute focuses on the most salient trade bills, while the PIPC data code a larger number of trade bills that may go unnoticed by voters. The Cato-based estimates thus focus on the set of bills on which members of Congress are most likely to heed the changing preferences of their constituents, while the trade-score-based estimates include a larger set of bills, some of which members of Congress can vote on without pressure from voters.

For clarity, figure 6 shows this same estimation graphically. The outcome variables—our two roll-call-based scalings—and instrumented import exposure are first residualized with respect to the control variables and then divided into bins of equal sample size and plotted against each other, along with lines of best fit.⁴⁰ The negative relationship between exogenous import exposure and protectionist voting is clear in both panels.

These are meaningful effects. Consider, for example, a district that goes from the median level of exposure (1.5; i.e., \$1,500) to the maximum observed exposure (13.3). Our estimates would predict a decrease of almost 25 percentage points in the probability that the district’s representative

38. When using the Cato score, this control variable is unnecessary and does not provide the same precision gains. We include it in column 4 for consistency of presentation.

39. The first column shows one reason why the instrumental variables strategy is necessary. When correlating district economic conditions with trade roll-call voting directly, little relationship is found. This is likely because import exposure per worker is itself a function of local economic conditions. Districts doing worse economically for reasons unrelated to foreign trade are unlikely to see changes in their roll-call votes on trade bills, thus attenuating the effect. Our strategy short-circuits this problem by finding random variation in import exposure, and thus in local economic conditions.

40. The residualizing and binning were performed using the binscatter package in Stata. The program divides variables into bins of equal sample size and locates the bins in the (x,y) plane at the mean value for each variable in the bin.

Table 1. Economic Shocks and Congressional Voting, US House 1990–2010

	District Trade Score			Cato Score
	OLS (1)	First Stage (2)	IV (3)	IV (4)
Import exposure per worker (IPW)	-.11 (.30)		-.70 (.38)	-2.10 (.75)
IPW non-US		.84 (.10)		
Dem share	-44.19 (1.78)	.05 (.07)	-44.16 (1.75)	-25.78 (1.47)
N	862	862	862	862
Decade fixed effect	Yes	Yes	Yes	Yes

Note. Localized economic shocks from trade cause more protectionist roll-call voting. State-decade clustered standard errors are in parentheses. IPW is measured in thousands of US Dollars. IV = two-stage least squares results.

casts a free-trade vote on a trade bill in response to this shock, using the Cato score.⁴¹ Imagine two otherwise identical congressional districts, both invested in manufacturing but differing in the particular mix of industries in which they produce goods. Imagine that Chinese export decisions produce a negative shock from trade in one of these district but not the other. The IV results in this section demonstrate that the affected district's legislator's roll-call record on trade bills will shift noticeably to the left, that is, in the protectionist direction, relative to the otherwise identical district that suffered no such shock. Economic conditions thus exert a clear pull on the roll-call votes legislators cast in Congress. This result does not, however, speak to the mechanisms underlying this effect. How and why do these shocks affect trade roll-call voting? We now turn to these questions.

Incumbent response, not electoral effects

As we discussed in the “Theoretical Perspectives” section, one obvious potential explanation for the link between negative trade shocks and protectionism is electoral. Perhaps voters respond to negative economic shocks by “punishing” incumbents and replacing them with more protectionist representatives. This is broadly consistent with both the literature on economic conditions and voting (Fair 1978, 2009; Kramer 1971) and the literature on shocks and retrospective voting, which generally finds that voters—rationally or irrationally—

41. The calculation is $(13.3 - 1.5) \times (-2.10) = -24.78$.

reward or punish incumbents for exogenous shocks (e.g., Achen and Bartels 2004; Bartels 2009; Gasper and Reeves 2011; Healy et al. 2010).

Table 2 tests for electoral effects in both the primary and general election. In the first column, we use the same main IV specification from equation (5) with the average number of contested primaries for each decade-district as the dependent variable.⁴² A \$1,000 increase in import exposure per worker is estimated to cause a 0.4 percentage-point increase in the probability the incumbent faces a primary challenge, and we cannot reject the null hypothesis that there is no effect. In the second column, we use the average number of “serious” primary contests within the district-decade as the dependent variable, where we define “serious” to mean any primary in which the eventual winner of the general election receives less than 95% percent of all within-party contributions.⁴³ Again, we find no effect, and this time the sign is in the opposite direction. A \$1,000 increase in import exposure per worker is estimated to cause a 0.8 percentage-point *decrease* in the probability of a “serious” primary challenger, and we cannot reject the null of no effect.

The second two columns of table 2 explore general-election outcomes. A \$1,000 increase in import exposure per worker is estimated to cause a 0.8 percentage-point decrease in average incumbent vote share and a 0.1 percentage-point increase in the probability of incumbent reelection, both substantively negligible effects that we cannot distinguish from zero, and again in conflicting directions.

Electoral effects do not appear to be driving the change in roll-call voting we observe. How do we square this with the large literature on retrospective voting and economic shocks? We suspect that two factors distinguish the current setting from those usually studied. First, these shocks are highly salient due to their pronounced effects on district unemployment and income (Autor et al. 2013a). Voters may therefore be unusually aware of the source of these shocks and the fact that they are not related to the incumbent's actions in Congress. Second, unlike broad shocks to the whole economy or narrow shocks that have no clear policy link (e.g., shark attacks), trade shocks offer the opportunity

42. The data on primary elections come from Ansolabehere et al. (2010), as updated by those authors. Specifically, we calculate “contested primary” for district i in decade T as $\frac{1}{N_{iT}} \sum_{t \in T} \text{contested}_{it}$ where N_{iT} is the number of elections in district i in decade T and t indexes election-years, and contested is an indicator variable for a contested primary.

43. The data for this come from FEC primary sources. We do not define “serious” based only on the incumbent's share of primary donations because separating primary and general donations is inaccurate (and, for our use, unnecessary).

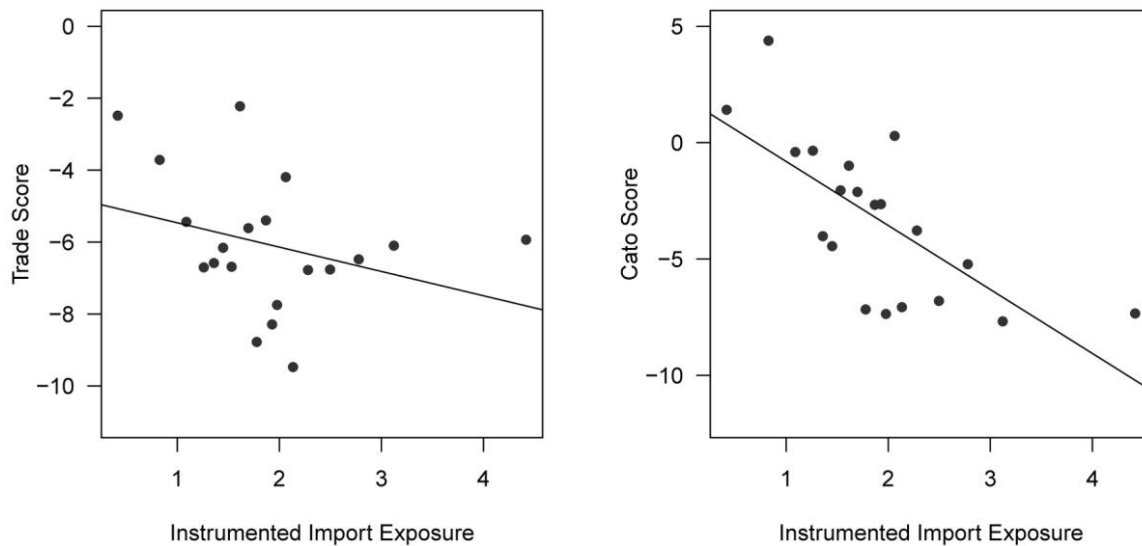


Figure 6. Impact of trade shocks on trade roll-call voting, US House 1990–2010. Each point represents an equal-sized bin of instrumented import exposure and the given roll-call score for district-decade observations. With both measures of trade roll-call voting, exogenous import exposure is seen to cause more protectionist (more negative) voting. The plot represents a nonparametric version of the IV regressions in table 1, corresponding to equation (5). For more information on how the plots were constructed, see note 40.

for incumbents to *respond* and thus to mitigate electoral effects *in equilibrium*.

We now turn to evidence for this hypothesized incumbent response. In the first column of table 3, we reestimate equation (5) using legislator roll-call scores on all *nontrade* bills. We find no effect of import exposure on voting behavior on other roll calls. As we reviewed in the “Theoretical Perspectives” section, this suggests that incumbents are catering their trade roll calls specifically in response to economic shocks. Since partisan affiliation is a central predictor of roll-call behavior, the lack of an effect on nontrade bills strongly suggests that districts are *not* changing the party of their representative in response to shocks, too, in line with the electoral analysis above.⁴⁴

In the second column we employ a similar placebo test using the Cato Institute’s rating on trade-subsidy bills. These trade subsidies concern issues largely unrelated to US manufacturing—like agricultural subsidies—and so should not be linked to trade shocks that affect the district manufacturing sector. As the table shows, we find a precise null effect on this placebo test.

For completeness, the third column presents the estimated results when we use the *difference* between the district’s trade score and nontrade score as the outcome variable. The

estimated effect (−0.75) is similar to the original estimate using just the trade score as the outcome variable (−0.7) and in fact somewhat larger in magnitude, again suggesting that overall changes in the district—either a switch in the incumbent party or an overall ideological shift on the part of the incumbent legislator—do not drive the main result we observe.

A final possible mechanism is that districts react to trade shocks by replacing the incumbent with a new co-partisan who, while mirroring the previous incumbent on nontrade bills, is more protectionist in terms of trade policy. We know this is unlikely because the electoral analysis showed no changes in the likelihood of primary contests or serious primary challenges. The fourth and fifth columns test this another way by estimating the effect only among districts that always have the *same* incumbent for the entire decade. Using both trade scores and the Cato scores, we see that the effect remains large and negative even in these districts. Hence, primary election turnover is not the explanation.

With these other possibilities rejected, we conclude that incumbents react to these shocks by differentially changing their roll-call behavior only on trade bills. This is the only mechanism consistent with the findings in tables 2 and 3. In districts that never change their incumbents, we still see the presence of a negative trade shock leading to significantly more liberal voting on trade bills, using both the tradescore and the Cato score. It must therefore be the case that incumbents change their voting behavior on trade bills in response to localized economic shocks from trade.

44. Further bolstering this view, a regression like that in equation (5), with *Dem Frac* as the outcome variable, shows that there is a precisely estimated zero effect of trade shocks on the proportion of the decade that the district has a Democratic representative.

Table 2. Economic Shocks and Incumbent Electoral Outcomes, US House 1990–2010

	IV	IV	IV	IV
	Contested Primary (1)	Serious Primary (2)	Ave. Incumbent Vote (3)	Ave. Incumbent Win (4)
Import exposure per worker (IPW)	.004 (.02)	–.008 (.01)	–.008 (.01)	.001 (.01)
<i>N</i>	862	862	862	862
Decade fixed effect	Yes	Yes	Yes	Yes

Note. Shocks have no effect on primary or general-election outcomes, in equilibrium. State-decade clustered standard errors are in parentheses. Estimated from 2SLS as described in equation (5). IPW is measured in thousands of US Dollars. Outcome variables are share variables running from 0 to 1. IV = two-stage least squares results.

Taken together, these results paint a picture of responsive incumbents who tailor their roll-call positions on trade bills to the economic conditions in their districts. This helps explain why, *in equilibrium*, we find no electoral effects. In addition to direct gains from tailoring their roll-call votes, the observed change in voting behavior may also be a proxy for a variety of ways in which the incumbent can stave off electoral penalty. Legislators may introduce new legislation, behave differently in committee, secure federal grants, perform specific constituency service, and act in many other ways that could help demonstrate to constituents that they are responding to negative shocks from trade in the district. We see the roll-call response that we observe as a primary indicator of incumbent responsiveness more generally—keeping in mind that roll-call voting is itself one of the incumbent’s most visible and thus most potent available actions.

Beyond interpreting the revealed preferences of incumbents from their response to these trade shocks, are there theoretical reasons to believe voters would reward incumbents for responding to negative trade shocks in this manner? There is good evidence that voters’ preferences for redistribution, for example, change in response to economic conditions (Bisgaard, Sønderskov, and Dinesen 2015; Brunner, Ross, and Washington 2011; Doherty, Gerber, and Green 2006) and job loss (Margalit 2013).⁴⁵ If the tailored response of incumbents is rooted in this kind of “electoral connection,” we might expect the effect to be largest when incumbents are most threatened electorally. This testable prediction, studied in the next subsection, addresses one part of *why* we find responsiveness on trade roll calls.

45. Though unrelated to the arguments in their article, Milner and Tingley (2011) also find that unemployment appears to be negatively correlated with voting for foreign trade in Congress, although the coefficient on unemployment is not statistically significant.

Roll-call response stronger in more competitive districts

Incumbents respond to localized economic shocks from trade by moving to the left on trade bills. Do they do so in response to electoral pressure? Or do incumbents change their roll-call behavior because of personal preferences, national party platforms, or other such factors? To investigate these questions, we study the variation in the effect across levels of electoral competition.

In the first four columns of table 4, we estimate the effect only in “safe” districts (Republican and Democratic, respectively). We define “safe” to mean districts in which the 1992 (for the first decade) or 2002 (for the second decade) US House Democratic vote share is above 0.6, for Democratic safe districts, or below 0.4, for Republican safe districts.⁴⁶ In the fifth and sixth columns, we restrict the sample to competitive districts, defined to be those where the 1992 or 2002 US House Democratic vote share was between 0.4 and 0.6. Here, we find that a \$1,000 increase in import exposure per worker causes a 1.29 percentage-point decrease in the district’s trade score and a 3.01 percentage-point decrease in the district’s Cato score—effects larger than in the main results from table 1 and much larger than the effects in safe districts for either party. Though telling, we should be cautious in interpreting these results, as they are noisy. While the negative effects in competitive districts are estimated to be roughly twice as large in magnitude for each score (–1.29 vs. –0.66 for the trade score outcome, –3.01 vs. –1.59 for the Cato score outcome), we cannot

46. To avoid any post-treatment issues, we only use Democratic vote share for US House in the first year post-redistricting for this “normal vote.” We do not use presidential vote share because for the 2000s this would only be available in 2004, already post-treatment.

Table 3. Economic Shocks and Congressional Voting: Testing Theories of Legislative Behavior, US House 1990–2010

	All Districts	All Districts	All Districts	Districts with Same Incumbent	
	IV	IV	IV	IV	
	Nontrade Score (1)	Subsidy Score (2)	Trade minus Nontrade (3)	Trade Score (4)	Cato Score (5)
Import exposure per worker (IPW)	.05 (.17)	.42 (.70)	-.75 (.33)	-.98 (.51)	-2.36 (1.05)
Dem share	-25.81 (.57)	-15.04 (2.82)	-18.34 (1.34)	-44.14 (1.78)	-25.31 (1.56)
<i>N</i>	862	862	862	571	571
Decade fixed effect	Yes	Yes	Yes	Yes	Yes

Note. State-decade clustered standard errors are in parentheses. Estimated from 2SLS as described in equation (5). IPW is measured in thousands of US Dollars. IV = two-stage least squares results.

reject the null hypothesis that the effect is equal across safe and competitive districts.

Figure 7 presents the same general results graphically for each of the two roll-call measures. First, the dependent variable is residualized with respect to the control variables and divided into bins of equal sample size. These bins are compared to equal-sized bins of instrumented import exposure, also residualized with respect to the control variables. This procedure is applied separately to competitive districts and to all safe districts, both Democratic and Republican.⁴⁷ The panels also include lines of best fit for both sets of districts. As the panels both show, the slope for competitive districts appears to be more negative than for safe districts, suggesting greater roll-call responsiveness.

The more threatened an incumbent is electorally, the more she modifies her trade voting based on localized economic shocks from trade. This reinforces the notion that incumbents are responding to trade shocks for electoral reasons, attempting to fend off the electoral threat these shocks present.

Media coverage and incumbent responsiveness

In the same spirit as this last analysis, we also examined whether incumbents were more responsive on trade bills in districts where voters receive more media information about incumbent behavior, using the media congruence measure from Snyder and Stromberg (2010). This measure reflects the

47. The residualization and binning were performed using the binscatter package in Stata. The program divides variables into bins of equal sample size and locates the bins in the (x, y) plane at the mean value for each variable in the bin.

proportion of news stories in the local media that pertain to in-district political behavior (and not to politicians in other districts) and thus measures how much information voters have easy access to about their own representatives. We re-estimated equation (5), subsetting the sample to only districts with high or low levels of media congruence, respectively. Unfortunately, the results were too imprecise to warrant presentation, although we do present them in section C of the appendix. Districts with high levels of congruence, and thus more information about incumbent behavior, do seem to exhibit higher levels of responsiveness on trade bills, though we draw no strong conclusions due to the imprecision of the estimates.

CONCLUSION

Using new data on congressional district trade shocks and new scalings of districts based on foreign-trade bills, we have shown how incumbents in the US House respond to localized economic shocks. We circumvent the usual inferential problem—namely, that economically depressed areas are likely to differ from other areas in their ideology—by using quasi-random variation in local economic conditions, obtained by leveraging differential shocks from Chinese export behavior across industries (Autor et al. 2013a). In response to these shocks, incumbents vote more to the left, toward protectionism, on foreign trade bills. This finding is consistent using a regression-based scaling technique on trade bills and using interest group codings of the free trade position on trade bills.

The observed changes in voting patterns are *not* the result of electoral turnover in the primary or general elec-

Table 4. Economic Shocks and Congressional Voting across District Normal Vote: US House, 1990–2010

	Safe Rep District		Safe Dem District		Competitive District	
	IV		IV		IV	
	Trade Score (1)	Cato Score (2)	Trade Score (3)	Cato Score (4)	Trade Score (5)	Cato Score (6)
Import exposure per worker (IPW)	-.58 (.42)	-2.37 (.99)	-.66 (.77)	-1.59 (1.46)	-1.29 (.63)	-3.01 (1.27)
Dem share	-30.02 (4.23)	-15.39 (8.11)	-54.88 (6.94)	-38.61 (14.73)	-40.23 (1.85)	-23.68 (2.24)
N	312	312	324	324	226	226
Decade fixed effect	Yes	Yes	Yes	Yes	Yes	Yes

Note. State-decade clustered standard errors are in parentheses. Estimated from 2SLS as described in equation (5). IPW is measured in thousands of US Dollars. IV = two-stage least squares results.

tion, nor do they result from incumbents becoming more liberal overall in their voting behavior. As such, the findings point to the targeted roll-call strategy incumbents deploy in response to economic shocks. This response, as we have shown, becomes more pronounced in districts where the incumbent is most worried about reelection.

Roll-call votes are only one arrow in the incumbents' quiver, but they are an important one, and one that voters appear to care about (e.g., Ansolabehere and Jones 2010; Brady et al. 2011; Canes-Wrone et al. 2002). The findings in this article shed light on how incumbents structure the roll-call profile

they present to voters at reelection time. Economic conditions exert a discernible pull on the ideological positions incumbents of both parties take on trade policy. Incumbents are thus surprisingly responsive to district conditions, even if they are relatively unresponsive to constituent preferences on average (e.g., Ansolabehere et al. 2001).

This position-taking behavior presumably helps incumbents remain in office, as we find no electoral effects from economic shocks related to trade. This runs counter to the robust finding across numerous political studies that economic fluctuations correlate with incumbent performance, even in cases where

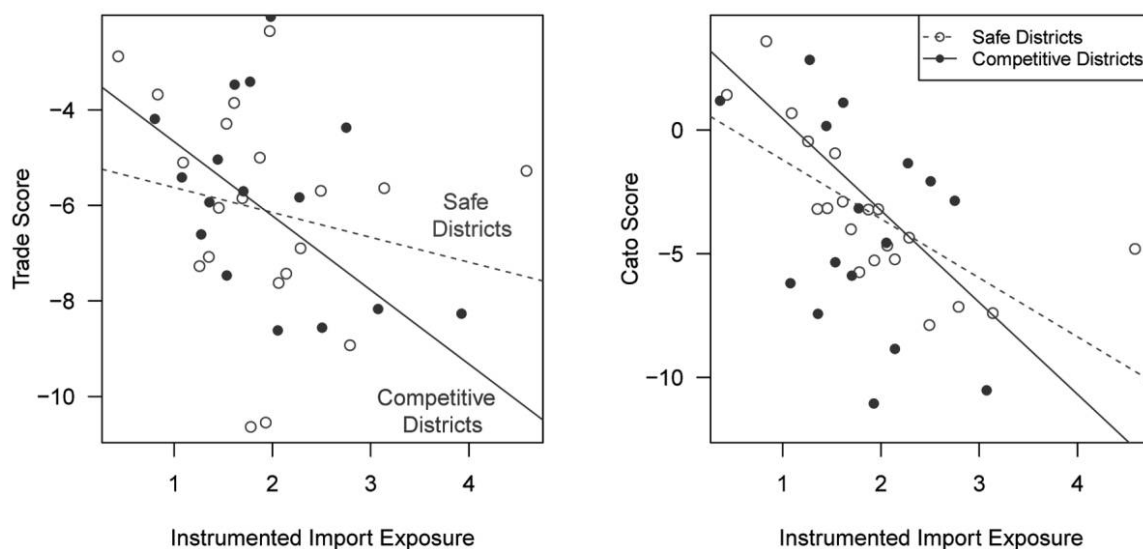


Figure 7. Impact of import exposure on trade scores across levels of electoral competition. Trade shocks appear to cause a larger increase in protectionist voting in competitive districts, those where incumbents face a more pressing electoral threat. The plots represent nonparametric versions of regressions like those in table 4. For more information on how the plots were constructed, see note 47.

the incumbent cannot be plausibly held accountable for the factors producing the fluctuations. Economic shocks from trade are somewhat unique, we suspect, in the opportunity they present to legislators to address their potential electoral effects. Incumbents can respond to these shocks by opportunistically altering voting behavior on pertinent bills while holding the rest of their position-taking portfolio constant. We suspect that other kinds of shocks could result in similar equilibrium effects, when they relate to coherent issue areas within which incumbents can tailor policy positions while leaving their overall ideological portfolio intact.

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