Policymakers' Horizon and Economic Reforms*

Paola Conconi[†]
Université Libre de Bruxelles (ECARES) and CEPR

Giovanni Facchini
Erasmus University, Universitá di Milano, LdA, CEPR and CES-Ifo

Maurizio Zanardi
Université Libre de Bruxelles (ECARES) and Tilburg University

October 2009

PRELIMINARY VERSION

Abstract

This paper investigates the relation between policymakers' term length and their willingness to support economic reforms. We describe a model in which office-motivated legislators have mandates of different length and consider the introduction of a trade liberalization reform, which gives rise to distributional effects that only become known over time. We show that legislators' voting behavior depends on their political horizon and on the trade policy interests of their constituencies. In particular, legislators with shorter mandates are less likely to support the reform, if they represent import-competing constituencies. To assess the validity of these results, we examine the determinants of all votes on major U.S. trade liberalization bills cast between 1973 and 2005. We exploit the particular features of the U.S. Congress, in which House and Senate representatives have respectively two-year and six-year mandates, with one-third of the Senate being up for election every two years. We find that members of the House are generally more protectionist than members of the Senate. However, this difference in voting behavior disappears for senators in the last two years of their mandate and for representatives of export constituencies.

JEL classifications: D72, F10

Keywords: Term Length, Reforms, Uncertainty, Trade Liberalization.

*We are grateful to Arnaud Costinot, Ray Riezman, and André Sapir for stimulating discussions. We wish to thank for their comments Matilde Bombardini, Rick Bond, Mathias Dewatripont, Gene Grossman, Keith Head, Toshihiro Ichida, Giovanni Maggi, Thierry Mayer, Steve Redding, Thierry Verdier, Yoto Yotov, and participants at the Faculty Discussion Group on Political Economy at Harvard University, the 2008 ETSG meeting, the Belgian International Trade Workshop, the Spring 2009 MWIEG meeting, the 2009 CEPR ERWIT meeting, the Worshop on "The New Political Economy of Trade" at EUI, and seminar audiences at the Athens University of Economics and Business, the Université Catholique de Louvain (CORE), the Université Libre de Bruxelles (ECARES), and the Paris School of Economics. We are also indebted to Elena Mattevi for excellent research assistance. Research funding from the FNRS and the European Commission is gratefully acknowledged by Paola Conconi.

[†]Address for correspondence: Paola Conconi, ECARES (Université Libre de Bruxelles), Avenue F. D. Roosevelt 50, CP 114, 1050 Brussels, Belgium; E-mail: pconconi@ulb.ac.be.

1 Introduction

Why do policymakers so often fail to adopt efficiency-enhancing reforms? This paper provides an answer to this fundamental question based on the length of policymakers' terms in office. The basic idea of our analysis was already recognized by the Founding Fathers of the United States. As suggested by Madison (1788) in the Federalist (63),

> The objects of government may be divided into two general classes: the one depending on measures which have singly an immediate and sensible operation; the other depending on a succession of well-chosen and well-connected measures, which have a gradual and perhaps unobserved operation.

In other words, while some policies have immediate and certain consequences, others have implications that only become clear over time. To deal with the latter, Madison suggested the creation of an additional chamber, the Senate:

> The proper remedy for this defect must be an additional body in the legislative department, which, having sufficient permanency to provide for such objects as require a continued attention, and a train of measures, may be justly and effectually answerable for the attainment of those objects.

Thus, in the view of the Founding Fathers of the U.S. Constitution, members of the Senate—who are elected for six-year terms—should be better placed for dealing with long-term decisions than House representatives—whose terms last two years only.

The purpose of this paper is to examine how the length of policymakers' mandates affects their willingness to support economic reforms that give rise to uncertain distributional effects. In particular, we develop a simple theoretical model linking the horizon of the policymakers with their voting behavior on a trade liberalization reform, and assess its empirical implications by examining legislators' voting behavior on all trade liberalization bills voted in the United States Congress since the early 1970's. Although the basic idea is general, trade liberalization reforms constitute an ideal ground for our analysis: lowering trade barriers leads to aggregate gains from trade, but generates important distributional effects; moreover, there is often considerable uncertainty about the extent of the adjustment costs borne by trade-displaced workers, and thus about the identity of the potential winners and losers.[1]

To frame our analysis, we describe a simple two-sector small open economy, in which the only production factor is labor. We consider the introduction of a trade liberalization reform,

[1] The extensive literature on job displacement in the U.S. labor market (see Kletzer 1998 for a review) suggests that individual adjustment costs are indeed hard to predict. Also, recent work by Blonigen (2008) shows that many people do not feel informed enough to state a preference on trade protection.

which would lead to an increase in the relative price of the exported good, making employment in the export sector more attractive. However, switching sector is costly, and individual-specific relocation costs are not known *ex ante*. This implies that, while workers employed in the export sector would unambiguously gain from the reform, import-competing workers may be hurt or benefit from it, depending on the extent of their own relocation costs. In this setting, the seminal paper by Fernandez and Rodrik (1991) shows that uncertainty about who will enjoy the gains from trade after liberalization can lead a rational electorate to oppose the reform *ex ante*, even when welfare is known to increase *ex post* for a majority.

To examine how policymakers' horizon affects their support for the reform, we assume that the decision on whether or not to reduce trade barriers is in the hands of two types of legislators, serving terms of different length. Crucially, legislators with shorter (longer) mandates face re-election before (after) the identity of the winners and losers from the reform is discovered. All legislators are driven by the desire to remain in office. Voters are risk neutral and take into account their representatives' voting behavior when deciding whether or not to re-elect them. In particular, the chances that a legislator retains office are higher if the vote he has cast on the reform is in line with the perceived interests of the majority of his constituency at the time of his re-election.

Uncertainty on the effects of the reform, combined with terms of different lengths in office, lead to different voting behaviors by the two types of politicians. In particular, our theoretical model delivers two important results. First, politicians with longer mandates are more likely to support trade liberalization reforms than politicians with shorter mandates. The intuition behind this result is that a legislator may only support trade liberalization if his own "political horizon"—the time before he faces re-election—is longer than the "reform horizon"—the time it takes for the identity of the winners and losers to be known. Second, term length matters only for politicians representing import-competing constituencies. This is because, in constituencies where most workers are employed in the export sector, there is always a majority in favor of the reform, so the voting behavior of the representatives does not depend on the electoral calendar.

To assess the validity of the predictions of our model, we examine the determinants of trade policy votes in the U.S. Congress. The focus on the United States is not only due to the availability of roll call votes on trade policy, but also to the specific institutional features of the U.S. Congress, in which House and Senate representatives serve respectively two-year and six-year terms, with one-third of the Senate being up for election every two years. The staggered structure of the Senate means that it is always possible to find senators with different remaining time in office voting on the same bill.[2] Our analysis of all trade liberalization votes cast between 1973

[2]In other countries (e.g., France and Australia) legislators belonging to the lower and upper house are elected for periods of different lengths, but all legislators within the same house face elections at the same time. Staggered

and 2005 provides strong evidence supporting the role played by term length in shaping voting behavior. First, we show that senators are more likely to vote in favor of trade liberalization measures than House representatives. We then exploit the staggered structure of the U.S. Senate and compare the voting behavior of senators facing different political horizons at the same point in time. We find that inter-cameral differences disappear for senators who are in the last two years of their mandate (i.e., facing elections at the same time as House members), suggesting that indeed the length of the remaining time in office is key in explaining whether a policymaker is willing to support a trade reform. Finally, in line with the predictions of our model, we show that the voting behavior of congressmen representing export constituencies is not affected by the length of their mandate. Our results are robust to the inclusion of a large variety of controls for the legislators (e.g., party affiliation, age, gender, incumbency gains, presidential aspirations) and their constituency (e.g, size, trade exposure) and to different estimation strategies.

To the best of our knowledge, this is the first paper to systematically investigate how the horizon of elected representatives affects their voting behavior on large-scale economic reforms. Our analysis builds on the pioneering work of Fernandez and Rodrik (1991), which demonstrates the existence of a status-quo bias when reforms have uncertain distributional effect. In their model, the decision of whether or not to reduce trade barriers is taken directly by the electorate. We consider instead a setting in which office-motivated representatives decide on the trade reform, and examine theoretically and empirically the role of term length on their voting behavior. Various other papers have examined the political viability of economic reforms in the presence of distributional effects and uncertainty. For example, Dewatripont and Roland (1995) introduce aggregate uncertainty in the framework of Fernandez and Rodrik (1991) to analyze the optimal sequencing of economic reforms. Alesina and Drazen (1991) show how a stabilization reform can be delayed due to a "war of attrition" between two groups, each of which is uncertain about the costs being incurred by the other group. These papers, however, do not consider the incentives faced by elected politicians and the role of term length.

The importance of electoral calendars when politicians are office motivated has been stressed by the literature on political business cycles. In particular, Rogoff and Sibert (1988) and Rogoff (1990) show that incumbent politicians who want to retain office may increase spending prior to elections to signal greater "competence," when voters are rational, but imperfectly informed. This literature is mainly aimed at explaining cyclical fluctuations in government policies (e.g., fiscal and monetary policy) during mandates of *fixed* length. We instead examine how differences in the length of policymakers' mandates affect their willingness to support structural reforms.

mandates similar to those characterizing the U.S. Senate can be found in both houses of the Congreso Nacional in Argentina.

Our paper is also related to a vast literature in political science, which has analyzed the effects of term length on legislative behavior. Many papers in this literature focus on the effects of election proximity on senators' responsiveness to the desires of the polity (e.g., Amacher and Boyes 1978, Thomas 1985, Bernhard and Sala 2006). These studies compare senators' "voting scores"—summary indexes of their voting record on particular issues—to measures of their constituencies' positions, and find considerable discrepancies between the two. Other papers ignore constituencies' preferences and simply analyze how the electoral cycle affects senators' ideological position (e.g., Glazer and Robbins 1985, Levitt 1996).[3] Papers in this literature have not examined how term lengths affect policymakers' voting behavior on structural reforms such as trade liberalization.

Finally, our paper contributes to the vast literature looking at the influence of various domestic political factors on the determination of trade policies (see, for example, Grossman and Helpman 1994, Maggi and Rodriguez-Clare 1998, and Grossman and Helpman 2005). This is the first paper to examine the role of politicians' term lenght. Our empirical analysis is also related to a series of papers looking at the determinants of trade policy votes in the U.S. Congress (e.g., Kahane 1996, Baldwin and Magee 2000). In particular, a recent paper by Karol (2007) shows that constituency size cannot explain why senators are less protectionist than House representatives. We find instead that the observed inter-cameral differences on trade policy votes can be explained by the different lengths of the legislators' mandates.

The remainder of the paper is organized as follows. Section 2 describes our theoretical model and derive the main results. Section 3 describes the data used for our analysis. Section 4 presents our empirical results and a series of robustness checks. Section 5 concludes.

2 A Simple Model of Trade Reform

In this section, we extend the simple model described by Fernandez and Rodrik (1991)—in which the electorate of a single constituency directly decides whether or not to undertake a trade liberalization reform—and consider a setting in which this decision is in the hands of office-motivated legislators, who serve mandates of different length and represent different types of constituencies.

[3] Two recent contributions, Titiunik (2008) and Dal Bo and Rossi (2008) use a natural experiment setting to study the effects of different term lengths on legislative participation. The former examines the effects of randomly assigned term lengths introduced within a small group of state senators in Arkansas and Texas. The second is based on a similar setting which arose as a result of the reintroduction of democracy in Argentina in 1983. Both papers reach the conclusion that longer terms in office lead to higher performance (e.g., in terms of floor attendance, or number of bills sponsored by a legislator).

2.1 The economic structure

To frame our analysis, we consider a small open economy producing two goods, Y and X, using labor as the only factor of production under constant returns to scale. We denote with a_y and a_x, respectively, the number of units of labor needed to produce one unit of output in the two sectors. We normalize units in sector y so that $a_y = 1$.

The economy is populated by L individuals, each supplying one unit of labor. Let $P = \frac{p_x}{p_y}$ be the relative price of the two commodities and w_x and w_y be the wages paid to labor in the two sectors. Profit maximization implies that $w_x = \frac{p_x}{a_x}$ and $w_y = \frac{p_y}{a_y}$. We normalize the domestic price of good Y to be equal to unity.

Individuals are risk neutral and have identical and homothetic preferences, which take the standard Cobb Douglas form, i.e. $u(X,Y) = X^{\gamma}Y^{1-\gamma}$, leading to an indirect utility function:

$$V(P,I) = \frac{\phi I}{P^{\gamma}}, \tag{1}$$

where $\phi = \gamma^{\gamma}(1-\gamma)^{1-\gamma}$ is a constant and I denotes individual income.

The status quo is characterized by a tariff being imposed on imports of good Y. The resulting domestic relative price is assumed to be equal to $P = a_x$, implying that $w_x = w_y$. For simplicity, throughout our analysis we assume that tariff revenues are "thrown away" and thus do not affect voters' utility.[4] Initial prices and the corresponding labor allocation are thus exogenously given, with L_y and L_x denoting aggregate employment levels in the two sectors.

Consider now the introduction of a trade liberalization reform, which leads to an increase in the relative price of good X from P to P'. This leads to an increase in w_x, making employment in the export sector more attractive. However, switching sectors is costly, and workers are uncertain about their individual relocation costs. In particular, we distinguish two components in the relocation costs: a fixed cost, θ, common to all individuals; and an individual-specific cost, c_i, capturing different inherent abilities to adjust to shocks in the economy. The distribution $f(c)$ of the individual-specific costs is defined over the interval $[\underline{c}, \overline{c}]$. In the status quo, all agents know the distribution $f(c)$, but do not know their own cost c_i.

From the point of view of the workers who are initially employed in the X sector, the reform is clearly beneficial, since it leads to an increase in their purchasing power ($w_x/p_x = 1/a_x$ is constant, while w_x/p_y increases). As shown in Section 2.3 below, from the point of view of Y workers, trade liberalization has instead an ambiguous effect, since it creates winners and losers: individuals whose relocation costs turn out to be low enough will move to the export sector and may end up gaining from the reform; those with higher relocation costs will remain employed in

[4]Alternatively, we could assume X workers receive all tariff revenues, as in Fernandez and Rodrik (1991).

the import-competing sector and will for sure be hurt.

2.2 The political structure

The decision of whether or not to implement the reform is taken by the country's legislators, who have mandates of different lengths and represent different types of constituencies. In particular, the country is divided into D districts and each district j is populated by $L^j = L/D =$ workers.[5] Each constituency is represented by two types of legislators, denoted by H and S. The only difference between them is in the length of their mandates, which last one period for H representatives, and two periods for S representatives.

Constituencies differ with respect to their economic structure. We denote with $\alpha^j = L^j_y/L^j$ the share of workers employed in the import-competing Y sector in a given district, with the remaining $1 - \alpha^j$ share of the workers being employed in the X sector.

The timing of the game is described by Figure 1. At the beginning of the first period, Nature selects H and S, who can live for T more periods.[6] H representatives face elections at the end of every period, while S representatives only face elections every two periods. In their first period in office, all legislators are called to vote on a trade liberalization reform. For simplicity, we assume that, after this period, no additional trade reform is considered by the legislators. Crucially, the distributional consequences of the reduction in trade barriers are only discovered in the second period, when all import-competing workers pay a fixed cost θ—independently of whether or not they switch sector—and find out their individual-specific relocation costs. This implies that, when H representatives are first up for re-election, it is not yet possible to identify the winners and the losers from the reform among the Y workers. The distributional effects of the reform are instead known when S legislators face elections for the first time.

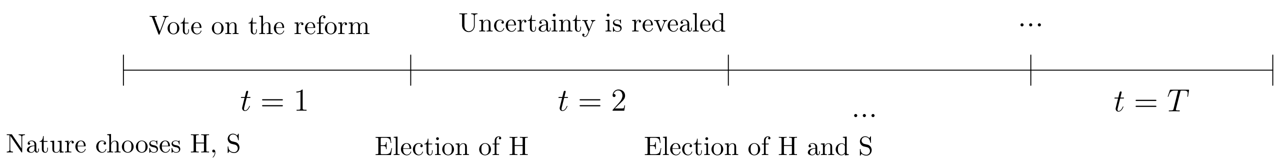

Figure 1: Timing of the game

[5]To focus on the role of policymakers' horizon, our theoretical model assumes away differences in the size of legislators' constituencies. The impact of such differences is accounted for in our empirical analysis of the determinants of trade liberalization votes in the U.S. Congress.

[6]We thus take the composition of the legislative body as exogenous, and focus our analysis on the role played by the horizon of incumbent policymakers. The results of our analysis would be unaffected if we considered legislators with an infinite horizon.

In each constituency, voters decide whether or not to re-elect incumbent representatives. If a reform is approved, legislators are made accountable for their behavior in the following election (i.e., at the end of $t = 1$ and $t = 2$, for H and S legislators, respectively). We assume that, if a legislator has voted on the reform in a way that is in line with (against) the perceived interests of the majority of his constituency, he is re-elected with probability $\overline{p}$ ($\underline{p}$). If instead the reform is not approved, the incumbent legislator faces an exogenously given probability β of being re-elected, with $\underline{p} < \beta < \overline{p}$. With the same probability β the legislator remains in office after all subsequent elections. Voters thus behave retrospectively, and are more likely to re-elect their representatives if they agree with their voting behavior on the reform. Notice that H legislators' chances of retaining office depend on the *expected* utility that individual workers obtain from the reform at the end of the first period (see discussion below).

We assume that policymakers only care about remaining in power.[7] In particular, following Rogoff (1990), among others, we assume that legislator derive fixed "ego rents" Z each period they are in office. Policymakers are risk neutral, do not discount the future, and derive no utility when they are not in office. The payoffs of H and S representatives can thus be written as follows:

$$\Pi_H = Z\Big(1 + p + p\sum_{t=1}^{T-2}\beta^t\Big) \tag{2}$$

$$\Pi_S = 2Z\Big(1 + p + p\sum_{t=1}^{(T-4)/2}\beta^t\Big), \tag{3}$$

where $p \in \{\underline{p}, \beta, \overline{p}\}$. The first term in the parenthesis of equation (2) captures the rents that an H legislator receives with certainty in his first mandate, during which he is called to vote on the reform; the second term captures instead the rents received if the legislator is re-elected after his first mandate, which happens with probability p; finally, the last term represents all the rents that the legislator receives up until period T, if he survives subsequent elections. The same is true for an S representative, with the difference that he only faces elections every two periods. Notice that the longer terms of S representatives imply that, for given re-election probabilities, their expected utility is higher.

2.3 Impact of the reform on individual voters

In the remainder of Section 2, we show that the difference in the term length between H and S representatives can lead to different voting behavior on the trade reform. To this end, we

[7] Term length would have no impact on voting behavior if legislators were fully benevolent, i.e., only cared about the long-term interests of their constituencies. The qualitative results of our analysis would be unaffected if we assumed legislators to be semi-benevolent.

examine first the perceived effects of the representatives' vote on X and Y workers when H and S representatives first face elections, i.e., at the end of period $t = 1$ and $t = 2$. We then derive conditions under which the majority of voters would be against a reform in the first period, but would support it in the second.

As mentioned earlier, X workers—who experience a real wage increase as a result of the reform and do not face any relocation cost—would always prefer the reform over the status quo and would thus favor policymakers who vote in favor of it. Preferences of Y workers, on the other hand, may instead be different *ex ante* (at $t = 1$, when individual-specific relocation costs are not yet known) and *ex post* (at $t = 2$).

2.3.1 Time $t = 1$

Consider first how workers employed in the import-competing industry would perceive the effect of the reform in period $t = 1$. At this point, they are all identical, since they have not yet discovered their individual-specific relocation costs. However, based on the known distribution $f(c)$, they can evaluate whether the reform is beneficial to them in expected terms. A Y worker will anticipate that he will choose to relocate to industry X only if his own relocation cost, c_i, does not exceed the following critical threshold:

$$\tilde{c} = w'_x - w'_y, \tag{4}$$

with w'_x and w'_y being the equilibrium wages prevailing after the reform is implemented. *Ex ante*, Y workers will only be in favor of the reform if its net expected benefits are non-negative:

$$F(\tilde{c})w'_x - \int_{\underline{c}}^{\tilde{c}} cf(c)dc + (1 - F(\tilde{c}))w'_y - \theta \geq w_y. \tag{5}$$

The left-hand side of equation (5) represents the expected income of Y workers when they are still uncertain about their individual relocation costs: independently of such costs, they have to incur the fixed adjustment cost θ; with probability $F(\tilde{c})$, they will switch to sector X and obtain a wage w'_x, after paying an expected relocation cost equal to $\int_{\underline{c}}^{\tilde{c}} cf(c)dc$; with probability $1 - F(\tilde{c})$, they will instead finding it too costly to relocate and will remain in sector Y, earning a wage $w'_y = w_y = 1$.

We next derive a sufficient condition for all Y workers to be *ex ante* against the reform. Let us define with P^* the price for which equation (5) is satisfied with equality, corresponding to wages $w^*_x = P^*/a_x$ and $w^*_y = w^*_y = 1$ and to a critical cost c^*. By construction, this price is thus such that at time $t = 1$ all Y workers perceive to be unaffected in nominal terms, but hurt in real terms, since their purchasing power is unchanged in terms of good Y, but is strictly lower in terms of good X.

Consider, for example, a setting in which the individual-specific costs are uniformly distributed between 0 and 1. Recall that the status-quo relative price is $P = a_x$. Assume that, as a result of the trade reform, the new domestic relative price increases to $P^* = a_x(1 + (2\theta)^{1/2})$, so that equation (5) is satisfied with equality; the corresponding critical cost is given by $c^* = (2\theta)^{1/2}$. It is straightforward to verify that this reform would lead to a perceived decline in real wages (and thus welfare) for all Y workers.

The above implies that, if the tariff reduction is not sufficiently large (i.e., $P' \leq P^*$), all workers in the import-competing sector will be against the reform *ex ante*, i.e., before the identity of the winners and losers from the reform is known.

2.3.2 Time $t = 2$

Consider now the *ex-post* effect of the trade liberalization reform raising the domestic prices to P^*. Notice that only Y workers with a transition cost below c^*—who relocate as a result of the reform —might favor it over the status-quo. Relocation, however, does not guarantee that a Y worker is better off after the implementation of the reform. A necessary and sufficient condition for this to be the case is given by:

$$V(P^*, I_i^*) \geq V(P, I), \tag{6}$$

where $I_i^* = w_x^* - c_i - \theta$. Under Cobb Douglas preferences (equation (1) above), the latter can be rewritten as

$$\frac{(1 + c^* - \theta - c_i)}{(P^*)^\gamma} \geq \frac{1}{(P)^\gamma}. \tag{7}$$

Continuing to assume that $f(c)$ is uniformly distributed between 0 and 1, we derive the relocation cost of the marginal individual, who is indifferent between the reform and the status quo:

$$\hat{c} = (1 + (2\theta)^{1/2}) + (1 + (2\theta)^{1/2})^\gamma - \theta. \tag{8}$$

Notice that $\int_0^{\hat{c}} cf(c)dc$ is the share of individuals with cost below $\hat{c}$. Hence equation (8) also identifies the fraction of Y voters who switches to be in favor of trade liberalization, after they discover that they actually gain from it.

2.4 Impact of the reform on constituencies

By construction, the reform leading to P^* is *ex ante* opposed by a majority of individuals in all import-competing constituencies. In what follows, we show that in some of these constituencies a majority will actually support the reform *ex post*.

A necessary condition for the majority of constituency j to favor the reform at $t = 2$ is

$$(1 - \alpha^j) + \lambda\alpha^j \geq \frac{1}{2}, \tag{9}$$

where λ is the share of Y workers in favor of the reform P^* at $t = 2$. Notice that in the case of a uniform distribution λ corresponds with the expression $\hat{c}$ in equation (8).

In export constituencies (i.e., $\alpha^j < 1/2$), the majority of the population supports the reform at both $t = 1$ and $t = 2$. On the other hand, in import-competing constituencies (i.e., $\alpha^j \geq 1/2$), the majority of the population may switch from being against the trade reform at $t = 1$ to being in favor at $t = 2$. From (9) we can derive a necessary and sufficient condition for a majority of voters to support the reform *ex post*:

$$\alpha^j \leq \hat{\alpha} = \frac{1}{2(1 - \lambda)}. \tag{10}$$

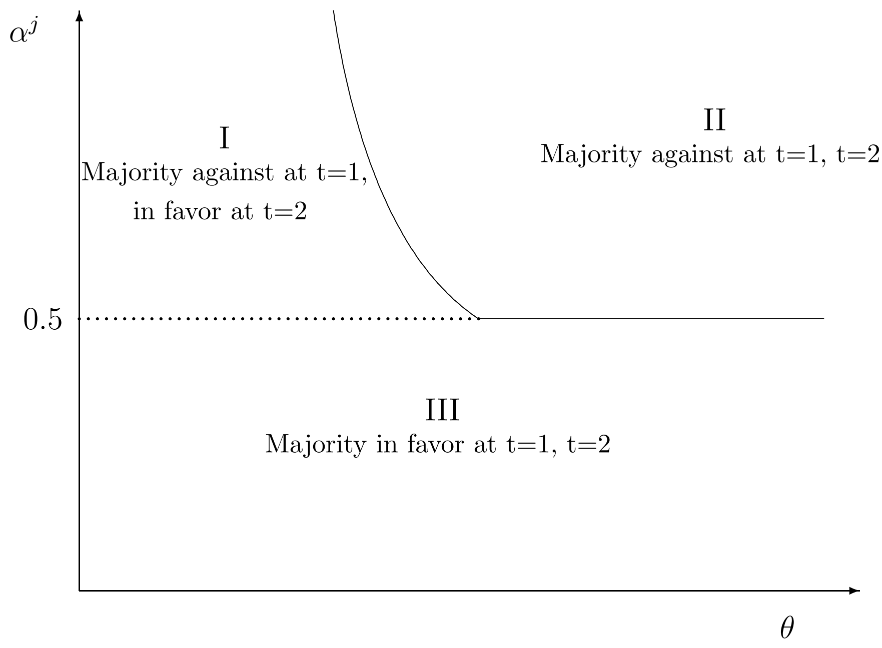

Figure 2: Impact of the reform P^*

Using the fact that $\lambda = \hat{c}$ for a uniform distribution, in Figure 2, we identify three regions in the α^j-θ space (assuming $\gamma = 1/2$): in region I, a majority of the voters is *ex ante* against the trade reform leading to P^*, but supports it *ex post*; region II corresponds to scenarios in which the fixed relocation cost is too high, so that a majority always opposes the reform; finally, region III shows that, in constituencies in which workers initially employed in the import-competing sector make up a minority of the population, the reform would always be supported.[8]

2.5 Legislators' Voting Behavior

We can now move to the analysis of the legislators' voting behavior on the reform. Based on the three scenarios depicted in Figure 2, we can establish the following:

Proposition 1 *S representatives are more likely to support a trade liberalization reform than H representatives.*

PROOF: Let Δ^k be the expected gain for supporting the reform leading to P^* in scenario k, where $k \in \{I, II, III\}$. Using equations (2) and (3), it is straightforward to verify that both H and S representatives gain from supporting the reform in scenario III, i.e.

$$\Delta_H^{III} = (\overline{p} - \underline{p})Z(1 + \sum_{t=1}^{T-2} \beta^t) > 0 \tag{11}$$

$$\Delta_S^{III} = (\overline{p} - \underline{p})2Z(1 + \sum_{t=1}^{(T-4)/2} \beta^t) > 0, \tag{12}$$

while both lose in scenario II:

$$\Delta_H^{II} = (\underline{p} - \overline{p})Z(1 + \sum_{t=1}^{T-2} \beta^t) < 0 \tag{13}$$

$$\Delta_S^{II} = (\underline{p} - \overline{p})2Z(1 + \sum_{t=1}^{(T-4)/2} \beta^t) < 0. \tag{14}$$

This is not surprising, since in these scenarios there is no difference in the perceived effects of the reform ext ante and *ex post*, i.e., at the time of the re-election of the two types of legislators. If we consider instead scenario I, supporting the reform will hurt H representatives—who are up for re-election when the majority of their constituency is still against the reform—but will be beneficial for S representatives, whose re-election occurs after some import-competing workers have shifted from being against to being

[8]Notice that in Figure 2 there is no region in which a majority of voters favors the reform *ex ante*, but opposes it *ex post*, since in our setup the implementation of the reform can only give rise to "positive surprises". This is because the reform forces all Y workers to pay a fixed cost θ. The possibility of "negative surprises"—and thus that some workers shift from being in favor to being against—could occur for negative values of θ, i.e., if Y workers received a fixed subsidy before discovering their individual relocation costs.

in favor of the reform:

$$\Delta_H^I = (\underline{p} - \overline{p})Z(1 + \sum_{t=1}^{T-2} \beta^t) < 0 \quad (15)$$

$$\Delta_S^I = (\overline{p} - \underline{p})2Z(1 + \sum_{t=1}^{(T-4)/2} \beta^t) > 0. \quad (16)$$

Similarly, for reforms resulting in a price $P' \neq P^*$, there always exists a region like III in Figure 2, in which the majority of a constituency only supports the reform *ex post*, implying that S representatives are more likely to support the policy change than H representatives. □

Notice that, whenever the majority of workers in a given constituency is initially employed in the X sector ($\alpha^j < 1/2$), the horizon of the policymakers does not matter: both H and S legislators will always support the reform, no matter how costly this is for the minority of Y workers. It follows immediately that

Proposition 2 *Differences in voting behavior between H and S legislators can only arise among representatives of import-competing districts.*

In order to verify whether the horizon of the policymakers matters in the way predicted by our theoretical model, in our empirical analysis we examine policymakers' voting behavior on trade liberalization reforms in the United States. The U.S. Congress provides a good setting because of two institutional features: i) House and Senate representatives are elected for two-year and six-year mandates, respectively; ii) one-third of the Senate is up for election every two years, together with all House members.

Although our theoretical model only captures the first of these features,[9] by exploiting the staggered nature of the U.S. Senate, we can derive the following two empirical predictions:

1 Senate members should be more likely to vote in favor of trade liberalization bills than House members;

2 Senators who are in the last two years of their mandate should be as protectionist as House representatives.

These two predictions are the empirical counterpart of Proposition 1. Notice that the second one follows from the fact that in any given year, one third of the senators have the same "political horizon" as all House representatives, i.e., they are up for re-election at the same time. Finally, Proposition 2 directly maps into the following empirical prediction:

[9] Our model could be readily extended to capture the staggered nature of the U.S. Senate. To do so, we could simply consider a setting with two generations of S representatives; this would imply that, at the end of each period, half of the S legislators would face elections, together with all H legislators.

3 There should be no difference in the voting behavior of Senate and House members representing export-oriented constituencies.

In what follows, we assess the validity of these three predictions by examining the determinants of roll call votes on trade liberalization bills in the U.S. Congress since the early 1970's.

3 Data

Table 1 lists the votes used in our empirical analysis. These concern all major trade liberalization bills voted in the U.S. Congress between 1973 and 2005,[10] including those on the implementation of the two multilateral trade agreements concluded in this period (the Tokyo and Uruguay Round Agreements), the votes on the conferral and extension of fast track trade negotiating authority to the President,[11] and all but two of the votes on preferential trade agreements.[12]

In our theoretical model, each constituency is represented by two legislators with mandates of different length. Empirically, however, we need to distinguish between the 50 states of the U.S.—electing two representatives each for the Senate—and the 435 congressional districts—each electing one member of the House of Representatives.[13]

Overall, we consider 29 votes, 15 in the House and 14 in the Senate.[14] For each vote, the identity of the congressmen, their party affiliation, their state or district and their vote (in favor or against) have been collected from roll call voting records. For senators, we also record the generation they belong to. As mentioned above, each generation (equal to one-third of senators) is elected every two years, together with the entire House. We define the senators facing election in the following two years, the so-called "in cycle", as belonging to the third generation. Those who face election next are defined as belonging to the second generation, while the first generation includes senators facing elections no sooner than in five years.

[10]The sample period begins in 1973 because of the challenge to construct some of the key regressors (see discussion below about the collection of congressional district data).

[11]See Conconi et al. (2009) for a theoretical and empirical analysis of the role of fast track authority in international trade negotiations.

[12]We exclude the votes on the approval of the free trade areas between the United States and Israel, and the United States and Bahrain, since they were either unanimous or not recorded (i.e, voice votes).

[13]As it can be seen from Table 1, for each decision in the House and Senate less than 435 and 100 votes are reported, respectively. This is because some congressmen may not be present or may decide to abstain. Moreover, a seat in Congress may be vacant at any point in time because of special circumstances (e.g., resignation, death).

[14]The Senate did not vote on the fast track bill of 1998, since the House had already rejected it.

Table 1: Votes on trade liberalization bills

Bill	Description	Vote in House	Vote in Senate
H.R. 10710 Trade Act of 1974	First approval of fast track authority Other provisions: escape clause, antidumping, countervailing duties, trade adjustment assistance, GSP	Dec. 11, 1973 (272-140)	Dec. 20, 1974 (72-4)
H.R. 4537 Trade Agreements Act of 1979	Approved Tokyo Round Agreements Other provisions: extension of fast track authority	July 11, 1979 (395-7)	July 23, 1979 (90-4)
H.R. 4848 Omnibus Trade and Competitiveness Act	Approval of fast track authority Other provisions: strengthening of unilateral trade retaliation instruments, authority of USTR	July 13, 1988 (376-45)	Aug. 3, 1988 (85-11)
H.R. 5090	Approved free trade area between United States and Canada	Aug. 9, 1988 (366-40)	Sept. 19, 1988 (83-9)
H.Res. 101/S.Res. 78	Disapproval of extension of fast track authority	May 23, 1991 (192-231)	May 24, 1991 (36-59)
H.R. 1876	Extension of fast track authority	June 22, 1993 (295-126)	June 30, 1993 (76-16)
H.R. 3450	Approved free trade area between United States, Canada and Mexico	Nov. 17, 1993 (234-200)	Nov. 20, 1993 (61-38)
H.R. 5110	Approved Uruguay Round Agreements	Nov. 29, 1994 (288-146)	Dec. 1, 1994 (76-24)
H.R. 2621	Approval of fast track authority (denied)	Sept. 25, 1998 (180-243)	
H.R. 3009 Trade Act of 2002	Approval of fast track authority Other provisions: Andean Trade Preference Act, trade adjustment assistance, GSP	July 27, 2002 (215-212)	Aug. 1, 2002 (64-34)
H.R. 2738	Approved free trade area between United States and Chile	July 24, 2003 (270-156)	July 31, 2003 (65-32)
H.R. 2739	Approved free trade area between United States and Singapore	July 24, 2003 (272-155)	July 31, 2003 (66-32)
H.R. 4759	Approved free trade area between United States and Australia	July 14, 2004 (314-109)	July 15, 2004 (80-16)
H.R. 4842/S. 2677	Approved free trade area between United States and Morocco	July 22, 2004 (323-99)	July 21, 2004 (85-13)
H.R. 3045	Approved free trade area between United States, Dominican Republic, Costa Rica, El Salvador, Honduras, Guatemala, and Nicaragua	July 28, 2005 (217-215)	July 28, 2005 (55-45)

Notes: Only final votes are reported; with the exception of the votes in 1991, the first (second) number in parenthesis refers to votes in favor of the bill (against it). The Senate did not vote on the bill of 1998, since the House had already rejected it.

The theoretical model predicts that the voting behavior of each legislator should depend on the trade position of his constituency (see our third empirical prediction). In our theoretical model this is captured by the parameter α^j, the share of import-competing workers in district j. In order to calculate its empirical counterpart, we first define an industry as being import (export), if the U.S. as a whole is a net importer (exporter) for that industry in a given year. We then collect information on employment in import-competing and export industries for all constituencies. Such variables are relatively easy to construct for the Senate, since state-level series are readily available. For the House of Representatives, on the other hand, we encountered two main difficulties. The first problem is that district-specific data are not readily available, but must be constructed by aggregating county-level data using the County Business Patterns (CBP), a survey collected by the Bureau of the Census.[15] Importantly, a county may be split into different districts, as exemplified by Figure 3. This depicts the case of Santa Clara County in California, which encompasses four congressional districts, some of which cover parts of neighboring counties. The second issue is that the geographic definition of districts changes over time, following each decennial Census, when districts are re-apportioned following changes in population.

We have addressed these concerns as follows. To obtain district-level date from county level information, we first extract yearly county-level data from the CBP and then aggregate them at the district level. For those counties split across more than one district, we follow Baldwin and Magee (2000), among others, imputing employees proportionally to the share of population of a county assigned to that district. To deal with the problem of redistricting, we have kept track of changes in the boundaries of the electoral districts that occurred after the Censuses of 1970, 1980, 1990 and 2000. For example, Alaska has always had only one congressional district; between the first vote in 1973 and the last one in 2005, California went instead from 43 to 53 districts, while New York went from 39 to 29.

Notice that employment data in the CBP are withheld when their disclosure would allow researchers to identify firms. In such cases, a flag gives the interval where the actual data belongs to (e.g., between 0 and 19 employees, between 20 and 99 employees and so on). These flags have been used to input values (i.e., the mid point of each interval) for the missing observations. In order to minimize the problem of undisclosed data, we use CBP employment data at the 2-digit SIC and 3-digit NAICS levels rather than at more disaggregated levels.

[15]The CBP report annual data on employment by SIC manufacturing industries up to 1997 and by NAICS manufacturing industries from 1998 (see Table A.1 in the Appendix for a list of the manufacturing industries included in our analysis), with very little detailed information for agriculture. However, manufacturing industries represent the lions share of total imports and exports of the United States (i.e., at least 70 percent in each year from 1970 until today). Moreover, many agriculture-related activities are classified as manufacturing and are thus included in our dataset (e.g., dairy products, grain mill products, sugar).

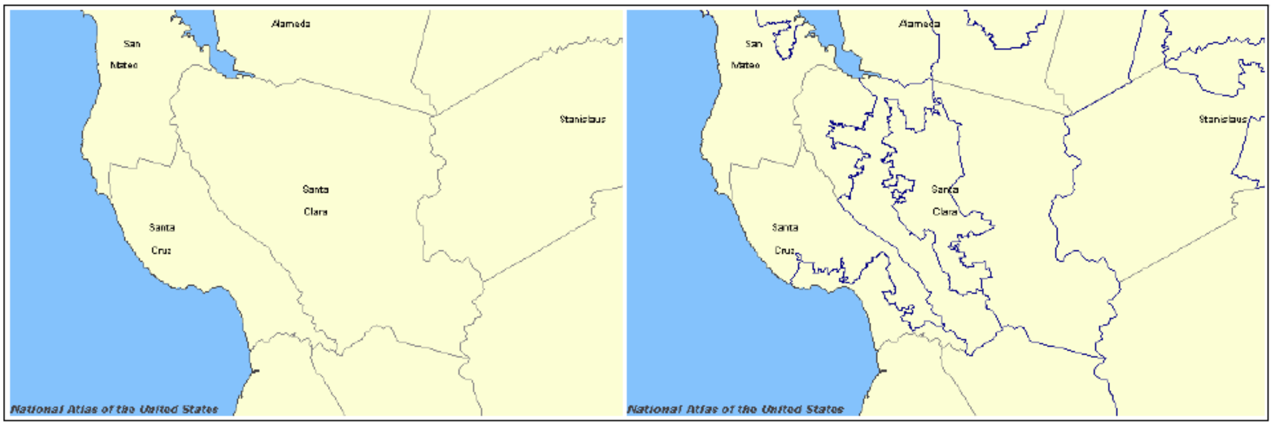

Figure 3: Santa Clara County: Congressional Districts

Using employment data by congressional district—constructed from county-level data as discussed above—and by state, we compute the number of employees in import and export industries for all constituencies. Our theoretical model suggests that voting decisions should be driven by the ratio of import workers to total workers. For each constituency j in year t, we then define our *Trade exposure* variable:

$$\alpha_t^j = \frac{Y_t^j}{Y_t^j + X_t^j}. \tag{17}$$

In our theoretical model, the political horizon of the legislators and the trade interests of their constituencies are the only determinants of the way politicians vote on trade liberalization bills. However, in our empirical analysis we need to control for other legislator and constituency characteristics that may affect voting behavior on trade policy. Table 2 provides definitions and sources for all the variables included in our analysis (top panel), or used in their construction (bottom panel).

It is known that party affiliation is a strong predictor of whether or not politicians support trade reforms. In particular, Democrats have been much more protectionist than Republicans during our sample period.[16] We thus use the dummy variable *Democrat* to verify the role of political parties. We also control for the age and gender of congressmen, since these demographic characteristics have been shown to be important in shaping individuals' trade policy preferences (see Mayda and Rodrik 2005).

[16] Up to the 1950's, Democratic congressmen were actually more pro trade (see Hiscox 1999).

Table 2: Definition of variables and sources

Variable	Definition	Source
Vote_t^i	Vote cast by congressman from constituency j in year t	Up to 1996: ICPSR Study number 4;
	Dummy equal to 1 if 'yea' and 0 if 'nay'	From 1997: http://www.voteview.com
Senate^j	Dummy equal to 1 if congressman j is a senator	As for Vote_t^j
Senator_t^j, 1^{st} generation	Dummy equal to 1 if senator j is in the first two years of his mandate	As for Vote_t^j
Senator_t^j, 2^{nd} generation	Dummy equal to 1 if senator j is in the third or fourth year of his mandate	As for Vote_t^j
Senator_t^j, 3^{rd} generation	Dummy equal to 1 if senator j is in the last two years of his mandate	As for Vote_t^j
Export Senate_t^j	Dummy equal to 1 if $\alpha_t^j < 1/2$ and $\text{Senate}^j = 1$.	As for Trade exposure_t^j and for Senate^j
Export House_t^j	Dummy equal to 1 if $\alpha_t^j < 1/2$ and $\text{Senate}^j = 0$.	As for Trade exposure_t^j and for Senate^j
Democrat_t^j	Dummy equal to 1 if congressman j is a Democrat in year t	As for Vote_t^j
Female_t^j	Dummy equal to 1 if in year t congressman j is female	Up to 1996: ICPSR Study number 7803;
		From 1997 up to 2000: Swift et al. (2000);
		From 2001: Biographical Directory of the U.S. Congress
Age_t^j	Age of congressman j in year t	As for Female_t^j
Population_t^j	Population of constituency j in year t (in millions)	U.S. Census Bureau (constructed)
Trade exposure_t^j	Ratio $\alpha_t^j = \frac{Y_t^j}{Y_t^j + X_t^j}$	As for Y_t^j and X_t^j
Margin of victory_t^j	Difference in votes shares between winner and runner-up in the last election in constituency j	U.S. House of Representatives
Incumbent_t^j	Dummy equal to 1 if congressman j is not in his first mandate	Biographical Directory of the United States Congress
Years in Congress_t^j	Years of service by congressman j up to year t (same house)	As for Female_t^j
Presidential aspirations_t^j	Dummy equal to 1 if congressman j ever participated in a presidential primary after year t	David Leip's Atlas of U.S. Presidential Elections
Y_t^j	Employees in year t of constituency j in import industries	County Business Patterns
X_t^j	Employees in year t of constituency j in export industries	County Business Patterns
Import/export industries	Industries in which the U.S. is a net importer/exporter (annual basis)	Feenstra (1996), Feenstra (1997), Feenstra et al. (2002)
		and U.S. ITC, IMF BoP Statistics
Congressional Districts	Aggregate of counties included in each district	1973-1982: ICSPR dataset 8258;
		1983-2002: provided by Christopher Magee

It is also important to take into account how “safe” the seat of a congressman is, as this may affect how responsive his voting behavior is to the electoral calendar. We use different proxies to capture how secure a seat is: the *Margin of victory* recorded by a congressman in his last election; an *Incumbent* dummy variable equal to 1 for congressmen who are not in their first mandate; and the number of *Years in Congress* since a legislator was first elected.

It could be argued that possible differences in voting behavior between Senate and House members may be driven by differences in the size of their constituencies. In particular, since senators have larger electoral bases, they may be less responsive to narrowly defined industry interests. We thus control for the size of each constituency, as proxied by *Population.* A further possible explanation for why senators may be less protectionist is that often they have *Presidential aspirations* and thus try to appeal to broader audiences. To verify whether this hypothesis is actually at work in the data, we construct a dummy variable which tracks the congressmen that ever participated in presidential primaries during our sample period.

Table 3: Descriptive statistics

Variable	Observations	Mean	Std. dev.
Vote_t^j	7,664	0.687	0.464
Senate^j	7,664	0.174	0.379
Senator_t^j, 1^{st} generation	7,664	0.058	0.234
Senator_t^j, 2^{nd} generation	7,664	0.059	0.235
Senator_t^j, 3^{rd} generation	7,664	0.057	0.231
Export Senate_t^j	7,664	0.023	0.151
Export House_t^j	7,664	0.069	0.254
Democrat_t^j	7,664	0.535	0.499
Female_t^j	7,664	0.098	0.297
Age_t^j	7,664	54.48	10.16
Population_t^j	7,664	1.429	3.030
$\text{Trade exposure}_t^j$	7,664	0.751	0.177
$\text{Margin of victory}_t^j$	7,648	35.15	25.66
Incumbent_t^j	7,664	0.801	0.399
$\text{Years in Congress}_t^j$	7,664	10.15	8.412
$\text{Presidential aspirations}_t^j$	7,664	0.026	0.158

Summary statistics for the main variables of interest are reported in Table 3. We see that, on average, constituencies are import-competing, since *Trade exposure* is above 0.5.

Similarly, in absolute terms there are more congressional districts than states that are export-oriented, although in percentage terms there are more export-oriented states than districts (i.e., 13.4% versus 8.4%). Some of the other summary statistics confirm well-known stylized facts about the U.S. Congress. In particular, female legislators are a clear minority, turnover is very low (i.e., 80% of legislators included in our dataset were holding their seat from a previous election) and the margin of victory is, on average, quite large.

4 Empirical Methodology and Results

The dependent variable in our empirical analysis, $Vote_t^j$, is dichotomous and equals one if the congressman representing constituency j in year t has voted in favor trade liberalization, and zero otherwise. Our baseline specification is thus given by

$$Prob(Vote_t^j = 1) = \Phi\left(\alpha + \beta_1 \mathbf{X}_t^j + \beta_2 \mathbf{Z}\right) \tag{18}$$

where $\Phi(\cdot)$ is the cumulative normal distribution (i.e., probit model); $\mathbf{X}_t^j$ is a matrix of district-specific variables, which are defined for each constituency j; $\mathbf{Z}$ is a matrix of additional controls, which may or may not be time-invariant and district specific (e.g., time or state fixed effects); and α, β_1, and β_2 are the vectors of parameters to be estimated. Depending on the empirical prediction that we examine, the main variable of interest is the *Senate* dummy variable (prediction 1), the dummies variables for the three different generations of senators (prediction 2), or the dummy variables for export-oriented Senate and House constituencies (prediction 3). In order to facilitate the interpretation of the estimated coefficients, in the tables we report marginal effects (calculated at the mean of each regressor).

We start by assessing the first empirical prediction, according to which senators should be more likely to vote in favor of trade liberalization bills. A positive and significant coefficient for the *Senate* dummy would provide empirical support for this prediction. The estimates of various specifications are presented in Table 4. In column (1) we report the results of a regression where the only explanatory variables are the *Senate* dummy and a set of year fixed effects. We find that senators are more likely to support trade liberalization bills.[17] Year fixed effects are jointly significant and suggest that over time the likelihood of voting in favor of trade reforms has declined, indicating an erosion of the support for trade liberalization.

The remainder of the table contains a series of robustness checks to investigate the role played by additional drivers of trade liberalization votes identified by the existing literature. In all the specifications reported in columns (2)-(9), the coefficient estimate for the *Senate* dummy continues to be positively and significantly correlated with voting decisions on trade reforms, thus providing clear support for the first empirical prediction of our theoretical model.

[17] In the simplest possible specification, in which we drop the year fixed effects, the coefficient for the *Senate* dummy is also positive and significant at 1 percent. The estimates of various fixed effects are not reported to save on space. All the results and tests not reported in the text are available upon requests.

Table 4: Trade Liberalization votes: House vs Senate

Regressor	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Senate^j	0.064***	0.083***	0.089***	0.084***	0.081***	0.089***	0.083***	0.092***	0.065**
	(0.014)	(0.014)	(0.020)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.031)
Democrat_t^j			-0.327***	-0.327***	-0.326***	-0.327***	-0.329***	-0.327***	-0.261***
			(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.014)
Female_t^j			-0.034*	-0.034*	-0.034*	-0.032*	-0.027	-0.037**	-0.068**
			(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.030)
Age_t^j			-0.002***	-0.002***	-0.002***	-0.003***	-0.003***	-0.002***	-0.002***
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Population_t^j			0.003	0.003	0.003	0.003	0.004	0.004	0.007*
			(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)
$\text{Trade exposure}_t^j$				-0.128**	-0.127**	-0.129**	-0.127**	0.125**	-0.229***
				(0.062)	(0.062)	(0.062)	(0.062)	(0.062)	(0.074)
$\text{Margin of victory}_t^j$					-0.0003				
					(0.0002)				
Incumbent_t^j						0.025			
						(0.016)			
$\text{Years in Congress}_t^j$							0.002*		
							(0.001)		
$\text{Presidential aspirations}_t^j$								-0.116**	
								(0.052)	
Year effects	included	included	included	included	included	included	included	included	included
State effects		included	included	included	included	included	included	included	included
Observations	7,664	7,664	7,664	7,664	7,648	7,664	7,664	7,664	4,014
Log likelihood	-4,296.29	-3,998.51	-3,519.32	-3,517.11	-3,505.69	-3,515.79	-3,515.22	-3,513.07	-1,845.92
Pseudo R^2	0.10	0.16	0.26	0.26	0.26	0.26	0.26	0.26	0.26
χ^2	716.28***	1,093.34***	1,331.86***	1,327.05***	1,324.59***	1,331.71***	1,328.14***	1,341.52***	647.75***
Predicted Prob.	0.71	0.73	0.75	0.75	0.75	0.75	0.75	0.75	0.76

Notes: Dependent variable, Vote_t^j, equal to 1 if congressman votes in favor of trade liberalization, 0 otherwise. Column (9) excludes votes on preferential trade agreements. Marginal effects reported for all regressors, calculated as discrete changes from 0 to 1 for dummy variables. Robust standard errors in parenthesis; *** denotes significance at 1% level; ** 5% level; * 10% level.

In column (2) we control for unobservable, additive state-specific effects, which are then retained throughout the rest of the table.[18] We then add controls for congressmen's party affiliation and demographic characteristics, as well as for the size of a constituency. Column (3) shows that Democrats are clearly less likely to support trade liberalization bills. Similarly, older and female legislators are also more protectionist, although gender is only significant at 10%. The coefficients for *Trade exposure* are consistently negative and significant, indicating that the larger the share of import-competing workers in a constituency, the less likely that its representative will favor trade liberalization. Interestingly, *Population* is not significant, thus suggesting that the alternative explanation for the pro trade bias of senators does not find support in the data.[19] Overall, these results are robust across the alternative specifications presented in Table 4, as well as for the results of the other tables.[20]

In columns (5)-(8), we add various proxies to capture how safe the seat of a congressman is and the role of presidential aspirations. We see that the *Margin of victory* and incumbency do not significantly affect voting decisions, while the numbers of years a politician has already served in a chamber is positive but only significant at 10%.[21] *Presidential aspirations* do play a role, albeit in the opposite direction that intuition would suggest: congressmen who participated in a presidential primary in the years following the vote were actually less likely to support trade liberalization, which runs against the general belief that presidential candidates should be less protectionist, since they try to appeal to the entire nation.

Finally, column (9) shows the results when excluding the votes for the approval of preferential trade agreements. Thus, this subsample only includes general trade liberalization measures. The specification is the same as in column (4) and by and large the qualitative results are unchanged. However, *Population* is now significant at 10% although the *Senate* dummy remains significant and positive, albeit with a smaller point estimate. *Trade exposure* is now significant at 1% and its influence is much more pronounced than in the previous columns.

We then turn to evaluate the second empirical prediction, where we exploit the staggered nature of the elections for senators. The specific institutional features of the U.S. Congress imply that in every year (i.e., for every vote in the sample) one third of the senators has the same electoral horizon as House members (i.e., faces elections in less than two years). This setup resembles a natural experiment, since the electoral calendars are exogenously assigned to each senate seat, allowing us to compare in a given year senators of different generations.

[18]The state fixed effects are jointly significant.

[19]This is in line with Karol (2007), who finds senators' trade votes to be unrelated to constituency size.

[20]Only the coefficient for *Female* shows some instability, as it becomes insignificant in some regressions and becomes significant at 5% in others.

[21]We experimented with non-linear functional forms and class dummies for the *Margin of Victory*. It seems that only intermediate ranges of such margins (i.e., around 20%) display a positive influence on the likelihood of a vote in favor of trade liberalization bills.

Table 5: Trade Liberalization votes: generations of senators

Regressor	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Senator^j_t, 3^{rd} generation	0.032	0.038	0.031	0.043	0.036	0.047	-0.020
	(0.023)	(0.029)	(0.030)	(0.029)	(0.029)	(0.029)	(0.045)
Senator^j_t, 2^{nd} generation	0.104***	0.103***	0.102***	0.109***	0.102***	0.110***	0.100**
	(0.020)	(0.024)	(0.025)	(0.024)	(0.024)	(0.024)	(0.037)
Senator^j_t, 1^{st} generation	0.107***	0.102***	0.100***	0.106***	0.102***	0.109***	0.109***
	(0.020)	(0.024)	(0.024)	(0.023)	(0.024)	(0.024)	(0.033)
Democrat^j_t		-0.326***	-0.326***	-0.327***	-0.328***	-0.327***	-0.261***
		(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.014)
Female^j_t		-0.035*	-0.035*	-0.032*	-0.027	-0.037**	-0.068**
		(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.030)
Age^j_t		-0.002***	-0.002***	-0.002***	-0.003***	-0.002***	-0.002***
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Population^j_t		0.004	0.003	0.004	0.004	0.004	0.007*
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Trade exposure^j_t		-0.131**	-0.130**	-0.131**	-0.130**	-0.127**	-0.237***
		(0.062)	(0.062)	(0.062)	(0.062)	(0.062)	(0.074)
Margin of victory^j_t			-0.0003				
			(0.0002)				
Incumbent^j_t				0.025			
				(0.016)			
Years in Congress^j_t					0.002**		
					(0.001)		
Presidential aspirations^j_t						-0.115**	
						(0.052)	
Year and state effects	included	included	included	included	included	included	included
Test $3^{rd} = 2^{nd}$ generation	5.95**	4.89**	5.40**	5.00**	4.96**	4.79**	6.51***
Test $3^{rd} = 1^{st}$ generation	6.72**	4.67**	4.99**	4.61**	4.88**	4.52**	8.25***
Observations	7,664	7,664	7,648	7,664	7,664	7,664	4,014
Log likelihood	-3,984.28	-3,513.58	-3,501.83	-3,512.26	-3,511.63	-3,509.59	-1,840.48
Pseudo R^2	0.16	0.26	0.26	0.26	0.26	0.26	0.26
χ^2	1,104.46***	1,331.00***	1,329.29***	1,336.73***	1,332.68***	1,347.13***	648.30***
Predicted Prob.	0.73	0.75	0.75	0.75	0.75	0.75	0.76

Notes: Dependent variable, Vote^j_t, equal to 1 if congressman votes in favor of trade liberalization, 0 otherwise. Column (7) excludes votes on preferential trade agreements. Marginal effects reported for all regressors, calculated as discrete changes from 0 to 1 for dummy variables. Robust standard errors in parenthesis; *** denotes significance at 1% level; ** 5% level; * 10% level.

In Table 5 we report the results of various specifications with a focus on the dummy variables referring to the different generations of senators. The third generation includes the senators that face elections first, together with all House members. The second empirical prediction would translate into an insignificant coefficient for this dummy variable. The coefficient for the other two generations should instead be positive and significant, since senators farther away from elections should be less protectionist than House members (i.e., the omitted category).

The specification in column (1) only includes the main dummies of interest, together with year and state fixed effects.[22] The second empirical prediction clearly holds, as the dummy for the third generation is insignificant while the other two dummies are positive and significant at 1%. The tests at the bottom of the table confirm that the estimate for the dummy of the third generation is statistically different from those of the other two generations. This conclusion is robust across the remaining columns where we introduce the same regressors used in the previous table: in all cases, the dummy of the third gneration of senators is insignificant and statistically different from the other two, which are positive and significant. Thus, political horizon plays an important role in a senator's decision to vote in favor or against a trade liberalization bill. Indeed, senators in the last two years of their mandate do not behave differently from House members, while senators in the other generations are even more pro trade than the marginal effect of the *Senate* dummy would suggest in Table 4, where all senators are treated identically.

As for the effect of the additional controls, their impact is very similar to the results reported in Table 4: Democrats, female and older congressmen are more protectionist. The size of a constituency is not a significant determinant of voting decisions, while its trade position does affect congressmen's behavior. Also when it comes to the security of a seat, previous results hold, in that only the number of *Years in Congress* is significant. And again, *Presidential aspirations* make politicians less likely to vote in favor of free trade measures. Similarly, restricting the sample (in column (7)) to more broadly defined trade measures does not change the conclusion.

According to our model, the political horizon of legislators should only matter if it is *ex-ante* impossible to identify with certainty all of the potential beneficiaries of a reform. This is the case, for example, if it is hard to predict which of the workers initially employed in import-competing industries will be able to make a smooth transition to employment in export industries and earn higher wages. Our model also suggests that a divergence in voting behavior between House and Senate representatives should only arise if the length of the adjustment period—the time it takes for the identity of the winners and losers to be known—is intermediate between the term length of the two types of legislators. The results of Table 5 show a significantly different voting behavior between legislators who face elections within two years (i.e., House members and third-generation senators) and legislators with a longer political horizon; there is instead no

[22]Notice that the same qualitative results are obtained when excluding year and state fixed effects.

difference between senators belonging to the first and second generation. These results imply that adjustment periods last between two and four years, an estimate which is in line with the existing literature on trade adjustment costs for the United States. In particular, Yotov (2009), using the Trade Act Participant Report Database for the years 1999-2006, estimates the transition period for trade-displaced workers to be between two and three years.[23] Similarly, Davidson and Matusz (2004) find that, when time and resource costs of retraining are taken into account, the adjustment process in terms of output returning to its pre-liberalization level takes place within 2.5 years.[24]

Finally, we can turn to the results regarding the third empirical prediction of our model, according to which there should be no difference in the voting behavior of Senate and House members who represent export-oriented constituencies. These results are reported in Table 6. In this case, the focus is on the dummy variables *Export House* and *Export Senate*, identifying congressmen whose constituencies are mostly export-oriented (i.e., characterized by $\alpha_t^j < 1/2$). As in the previous table, the first column reports the results of the simplest regression where only these dummies are included along with year and state fixed effects.[25] We see that House members from export-oriented districts are more pro trade than other House members (i.e., the excluded category), although this effect is weaker in the remaining columns.

Importantly, the test statistic at the bottom of the table shows that there is no statistical difference between the voting behavior of congressmen from export-oriented constituencies in the Senate and the House. This result is valid across all the specifications of Table 6, where we follow the approach of the previous tables in adding extra regressors and restricting the sample. Again, the results for the dummy variables on export-oriented constituency are qualitatitevely unchanged and the estimates for the other variables are very similar to the previous ones. Thus, also the third empirical prediction is verified, and is robust to the inclusion of various controls.

[23] The United States provides trade adjustment assistance (TAA) to trade-displaced workers in the form of monetary compensation and retraining. The average time spent in the TAA program is about 20 months. However, a worker enters the program only after exhausting unemployment benefits, implying that at least 26 weeks should be added to the transition period. Note that Yotov (2009)'s estimates are biased downward, since they do not account for the fact that some workers are still unemployed upon exiting the TAA program.

[24] Longer transition periods have been found in some studies that have focused on displaced workers who had a long job tenure with their previous employers, and whose dislocation may not be due to trade liberalization (e.g., Jacobson et al. 1993).

[25] The results are qualitatively identical if we exclude year and state fixed effects.

Table 6: Trade Liberalization votes: House vs Senate (export and import constituencies)

Regressor	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Senate^j	0.087***	0.093***	0.085***	0.098***	0.092***	0.100***	0.071**
	(0.014)	(0.020)	(0.021)	(0.020)	(0.020)	(0.020)	(0.032)
Export Senate_t^j	0.020	-0.004	-0.020	-0.003	-0.004	0.001	0.032
	(0.061)	(0.064)	(0.069)	(0.064)	(0.064)	(0.062)	(0.068)
Export House_t^j	0.077***	0.049*	0.026	0.049*	0.049*	0.048*	0.052*
	(0.025)	(0.024)	(0.032)	(0.026)	(0.026)	(0.026)	(0.027)
Democrat_t^j		-0.326***	-0.326***	-0.327***	-0.329***	-0.327***	-0.262***
		(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.014)
Female_t^j		-0.034*	-0.034*	-0.031*	-0.026	-0.036**	-0.068**
		(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.030)
Age_t^j		-0.002***	-0.002***	-0.002***	-0.003***	-0.002***	-0.002***
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Population_t^j		0.003	0.003	0.003	0.004	0.004	0.006
		((0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)
$\text{Margin of victory}_t^j$			-0.0003				
			(0.0002)				
Incumbent_t^j				0.025			
				(0.016)			
$\text{Years in Congress}_t^j$					0.002**		
					(0.001)		
$\text{Presidential aspirations}_t^j$						-0.116**	
						(0.051)	
Year and state effects	included	included	included	included	included	included	included
Test Export Senate = Export House	0.22	0.44	0.44	0.60	0.42	0.81	0.48
Observations	7,664	7,664	7,648	7,664	7,664	7,664	4,014
Log likelihood	-3,984.45	-3,517.71	-3,505.23	-3,516.46	-3,515.82	-3,513.66	-1,848.99
Pseudo R^2	0.16	0.26	0.26	0.26	0.26	0.26	0.26
χ^2	1,099.46***	1,330.99***	1,326.69***	1,335.68***	1,332.18***	1,344.49***	659.09***
Predicted Prob.	0.73	0.75	0.75	0.75	0.75	0.75	0.76

Notes: Dependent variable, Vote_t^j, equal to 1 if congressman votes in favor of trade liberalization, 0 otherwise. Column (7) excludes votes on preferential trade agreements. Marginal effects reported for all regressors, calculated as discrete changes from 0 to 1 for dummy variables. Robust standard errors in parenthesis; *** denotes significance at 1% level; ** 5% level; * 10% level.

In a series of unreported results,[26] we find that our results are also robust to alternative specifications and methodological approaches. First, we have controlled for additional economic characteristics of the legislators' constituencies. This poses a serious challenge because of data availability: series at the district level are mostly available from the decennial censuses (i.e., the CPB used to construct our measure of trade exposure being a notable exception); series with annual variation are only available at the state level. Keeping in mind these limitations, we have included additional controls at the state level (i.e., real GDP per capita and unemployment rate) to our main specifications in Tables 4 to 6.[27] Real GDP per capita was never significant, while the unemployment rate presented a negative and significant coefficient. In any case, the qualitative results of our analysis concerning the effects of policymakers' term length were unchanged.

Another set of robustness checks is related to our measure of trade exposure. As discussed above, this was constructed based on whether the United States at large is a net importer/exporter in a given industry (see Table 2 for details). It may be argued that this is an imprecise definition when it comes to specific preferential trade agreements (PTAs), since U.S. trade patterns with other agreement members may be quite different from aggregate U.S. trade patterns. For the votes implementing these trade deals, we have thus constructed a different version of our *Trade exposure* variable, based on the net trade position of the United States vis-à-vis PTA partners. The qualitative results for the first two empirical predictions are unchanged, although the *Trade exposure* variable is not significant. Instead, the third empirical prediction is not supported anymore by the data. A possible interpretation of these results is that voters are aware of the general effects of trade liberalization on the U.S. economy, but ignore the effects of specific trade deals.[28]

From a methodological perspective, instead of using robust standard errors, we have clustered the errors by constituency, thus allowing for intra-group correlation over time. Notice that standard errors can only be clustered by state and not by congressional districts. This is because, as discussed in Section 3, congressional districts are redefined by the Census every ten years, implying that the clusters change over time. When we follow this approach, the sign and significance levels of our main variables of interest in all the specifications presented above are unchanged.

As an additional methodological check, we have estimated our specifications on the second empirical prediction by trade liberalization bill. This allows us to put more emphasis on cross-

[26] All results omitted in the text are available upon request.

[27] These data are from the U.S. Census.

[28] For example, in recent years, the U.S. is a net importer of "Textile Product Mills" in aggregate terms, but is a net exporter of these goods toward Australia, Chile, Singapore, and the Caribbean countries with which it has a PTA. It is not clear that the average voter would know of these differences when evaluating his representative's voting record.

sectional variation instead of the time dimension, at the cost of losing the variability observed over various votes. In particular, we cannot include state fixed effects because of the lack of variation in congressmen's votes in some states. Similarly, for the recent years, most constituencies are import-competing, thus precluding the possibility to investigate the third empirical prediction on export-oriented constituencies on a systematic basis. Keeping these caveats in mind, we used the votes on 14 trade liberalization bills (we excluded the 1998 bill on fast track authority, which never reached the Senate since it was rejected in the House). Out of 14 regressions, our results on the political horizon of legislators are clearly confirmed for the adoption of 5 trade measures, i.e., senators in the third generation are as likely to vote in favor of the bill as House members.[29]

In general, our empirical analysis provides strong support for the empirical predictions derived from our theoretical model concerning the relationship between legislators' political horizon and their voting behavior. Other control variables, even when significant, do not affect this result.

5 Conclusions

In this paper, we have examined the role of term lengths on policymakers' willingness to undertake economic reforms. Although the basic idea of our analysis applies more generally, we have focused on the introduction of trade liberalization reforms. Our theoretical and empirical results show that uncertainty about who will enjoy the gains from trade liberalization implies that office-motivated legislators may only support tariff reductions if their own political horizon—the time before they face elections—is longer than the reform horizon—the time it takes for the identity of the winners and losers to be known

In line with what argued by the Founding Fathers of the United States, our analysis suggests that senators may be better placed than House representatives to deal with trade liberalization reforms and other policies measures that, though efficiency enhancing, may give rise to uncertain distributional effects. Our results also imply that structural reforms may be more easily undertaken in countries in which politicians serve longer terms in office, and in which domestic redistributive policies guarantee that potential losers get adequately compensated after the change of policy.

[29]In 4 other cases, the estimates had the expected signs, but were not always significant; in the remaining 5 cases, the coefficient estimates were not in line with the predictions of our model.

References

Alesina, A. and A. Drazen (1991). Why are stabilizations delayed? *American Economic Review 81*, 1170–1188.

Amacher, R. C. and W. J. Boyes (1978). Cycles in senatorial voting behavior: Implications for the optimal frequency of elections. *Public Choice 33*, 5–13.

Baldwin, R. E. and C. S. Magee (2000). Is trade policy for sale? Congressional voting on recent trade bills. *Public Choice 105*, 79–101.

Bernhard, W. and B. R. Sala (2006). The remaking of an American Senate: The 17th amendment and ideological responsiveness. *Journal of Politics 68*, 345–357.

Blonigen, B. A. (2008). New evidence on the formation of trade policy preferences. Mimeo, University of Oregon.

Conconi, P., G. Facchini, and M. Zanardi (2009). Fast track authority and international trade negotiations. CEPR Discussion Paper No. 6790, (http://164.15.69.62/ecare/personal/conconi$/web/FTA.pdf).

Dal Bo, E. and M. Rossi (2008). Term length and political performance. NBER Working Paper 14511.

Davidson, C. and S. Matusz (2004). Should policy makers be concerned about adjustments costs? In D. Mitra and A. Panagariya (Eds.), *The Political Economy of Trade, Aid and Foreign Investment Policies*, pp. 31–68. Elsevier, Springer-Verlag and Edward Elgar.

Dewatripont, M. and G. Roland (1995). The design of reform packages under uncertainty. *American Economic Review 85*, 1207–1223.

Feenstra, R. C. (1996). U.S. imports, 1972-1994: Data and concordances. NBER Working Paper 5515.

Feenstra, R. C. (1997). U.S. exports, 1972-1994, with state exports and other U.S. data. NBER Working Paper 5990.

Feenstra, R. C., J. Romalis, and P. K. Schott (2002). U.S. imports, exports, and tariff data, 1989-2001. NBER Working Paper 9387.

Fernandez, R. and D. Rodrik (1991). Resistance to reform: Status quo bias in the presence of individual specific uncertainty. *American Economic Review 81*, 1146–1155.

Glazer, A. and M. Robbins (1985). How elections matter: A study of U.S. senators. *Public Choice 46*, 163–172.

Grossman, G. M. and E. Helpman (1994). Protection for sale. *American Economic Review 84*, 833–850.

Grossman, G. M. and E. Helpman (2005). A protectionist bias in majoritarian politics. *Quarterly Journal of Economy 120*, 1239–1282.

Hiscox, M. J. (1999). The magic bullet? The RTAA, institutional reform, and trade liberalization. *International Organization 53*, 669–698.

Jacobson, L., R. J. LaLonde, and D. G. Sullivan (1993). Earnings losses of displaced workers. *American Economic Review 83*, 685–709.

Kahane, L. (1996). Congressional voting patterns on NAFTA: An empirical analysis. *Journal of Economics and Sociology 55*, 395–409.

Karol, D. (2007). Does consituency size affect elected officials' trade policy preferences? *Journal of Politics 69*, 483–494.

Kletzer, L. G. (1998). Job displacement. *Journal of Economic Perspectives 12*, 115–136.

Levitt, S. D. (1996). How do senators vote? Disentangling the role of voters' preferences, party affiliation and senators ideology. *American Economic Review 86*, 425–441.

Madison, J. (1788). Federalist Paper 63. In J. Jay, A. Hamilton, and J. Madison (Eds.), *The Federalist or the new Constitution*. London: Everyman Edition.

Maggi, G. and A. Rodriguez-Clare (1998). The value of trade agreements in the presence of political pressure. *Journal of Political Economy 106*, 574–601.

Mayda, A. M. and D. Rodrik (2005). Why are some people (and countries) more protectionist than others? *European Economic Review 49*, 1393–1430.

Rogoff, K. (1990). Equilibrium political business cycles. *American Economic Review 80*, 21–36.

Rogoff, K. and A. Sibert (1988). Elections and macroeconomic policy cycles. *Review of Economic Studies 55*, 1–16.

Swift, E. K., R. G. Brookshire, D. T. Canon, E. C. Fink, J. R. Hibbing, B. D. Humes, M. J. Malbin, and K. C. Martis (2000). Database of congressional historical statistics [computer file]. Ann Arbor, MI: Inter-university Consortium for Political and Social Research.

Thomas, M. (1985). Election proximity and senatorial roll call voting. *American Journal of Political Science 29*, 96–111.

Titiunik, R. (2008). Drawing your senator from a jar: Term length and legislative behavior. Mimeo, University of California, Berkeley.

Yotov, Y. V. (2009). Labor market imperfections, political pressure, and trade protection patterns. Mimeo, Drexel University.

Appendix

Table A.1: List of SIC and NAICS industries

SIC	Description
20	Food and Kindred Products
21	Tobacco Products
22	Textile Mill Products
23	Apparel and Other Finished Products Made From Fabrics and Similar Materials
24	Lumber and Wood Products, Except Furniture
25	Furniture and Fixtures
26	Paper and Allied Products
27	Printing, Publishing, and Allied Industries
28	Chemicals and Allied Products
29	Petroleum Refining and Related Industries
30	Rubber and Miscellaneous Plastics Products
31	Leather and Leather Products
32	Stone, Clay, Glass, and Concrete Products
33	Primary Metal Industries
34	Fabricated Metal Products, Except Machinery and Transportation Equipment
35	Industrial and Commercial Machinery And Computer Equipment
36	Electronic and Other Electrical Equipment and Components, Except Computer Equipment
37	Transportation Equipment
38	Measuring, Analyzing, And Controlling Instruments; Photographic, Medical and Optical Goods; Watches And Clocks
39	Miscellaneous Manufacturing Industries
NAICS	**Description**
311	Food Manufacturing
312	Beverage and Tobacco Product Manufacturing
313	Textile Mills
314	Textile Product Mills
315	Apparel Manufacturing
316	Leather and Allied Product Manufacturing
321	Wood Product Manufacturing
322	Paper Manufacturing
323	Printing and Related Support Activities
324	Petroleum and Coal Products Manufacturing
325	Chemical Manufacturing
326	Plastics and Rubber Products Manufacturing
327	Nonmetallic Mineral Product Manufacturing
331	Primary Metal Manufacturing
332	Fabricated Metal Product Manufacturing
333	Machinery Manufacturing
334	Computer and Electronic Product Manufacturing
335	Electrical Equipment, Appliance, and Component Manufacturing
336	Transportation Equipment Manufacturing
337	Furniture and Related Product Manufacturing
339	Miscellaneous Manufacturing