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Author
Weller, Nicholas William

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UNIVERSTY OF CALIFORNIA, SAN DIEGO

The Causes and Consequences of Tax Policy

A dissertation submitted in partial satisfaction of the
Requirements for the degree Doctor of Philosophy

in

Political Science

by

Nicholas William Weller

Committee in charge:

      Professor Peter Cowhey, Co-Chair,
      Professor Mathew McCubbins, Co-Chair
      Professor Thaddeus Kousser
      Professor David Lake
      Professor Ramhamohan Paturi

2008

The Dissertation of Nicholas William Weller is approved, and it is acceptable in quality and form for publication on microfilm.

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Co-Chair
_____
Co-Chair

University of California, San Diego

2008

DEDICATION

For my Dad, who convinced me not to attend law school. Though he can't be here to share this with me in person, he's been with me every step of the way.

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

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Although my classmates, professors and advisors have been central to the completion of this dissertation and my PhD more broadly, no one has been as important

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VITA

1999   Bachelor of Arts, Rice University

2003-2008   Research Assistant, Department of Political Science, University of
                California, San Diego

2005   Master of Arts, University of California, San Diego

2006-2007   Teaching Assistant, Department of Political Science, University of
                California, San Diego

2008   Doctor of Philosophy, University of California, San Diego

FIELDS OF STUDY

Major Field: Political Science

ABSTRACT OF THE DISSERTATION

The Causes and Consequences of Tax Policy

by

Nicholas William Weller

Doctor of Philosophy in Political Science

University of California, San Diego, 2008

Professor Peter Cowhey, Co-Chair

Professor Mathew McCubbins, Co-Chair

An extensive literature in political science focuses on government expenditures. In my dissertation, however, I focus on taxation, because governments must tax their citizens in order to engage in any of the choices about distributing that revenue. Each empirical chapter in this dissertation examines a different aspect of tax policy.

In "Trading Policy" I argue that political parties have been central to trade policy in the United States, which runs contrary to the dominant demand-side explanations for trade policy. The research design I use allows me to account for a variety of different constituent factors that could influence voting, and then determine if party has any effect

beyond constituent interests. The results suggest that party plays a significant role in legislative voting on trade policy once we account for constituency effects.

In "Tax Man Cometh" (co-authored with Lissa Ziegler) we examine how a state's choice of tax policy reveals information about state capabilities. In particular, we focus on establishing the validity of a new construct for state capacity – income taxation. We argue that to measure a state's relative level of capacity we should look at the ratio of income taxes to total taxes collected by the state. We demonstrate that this is a valid measure for our construct of state capacity.

In "Diffusion of State Tax and Expenditure Limits" Ellen Moule and I focus how state tax and expenditure limits (TELs) diffuse across the U.S. states. First, we argue that to understand policy diffusion requires paying attention to policy proposals as well as policy adoptions. Second, we separately model the determinants of exposure to a policy and then, conditional on exposure, the determinants of policy adoption. We find that proposal of a tax and expenditure limit is significantly related to whether states with similar income levels have previously adopted a TEL.

Taken together these chapters help us to understand the determinants of tax policy and also what we can infer about a particular country based on its choices of tax policy.

Chapter 1: Trading Policy: Constituents and Party in U.S. Trade Policy

Political scientists have long been concerned with the determinants of congressional trade policy voting in the United States. Trade policy is one of a government's primary foreign economic policies, and it can have significant effects on both the total amount and the distribution of income. At various times, trade policy has also played a significant role in national political debates. The dominant explanations for U.S. trade policy focus on the role constituents play in determining congressional policy outcomes. This literature essentially draws a straight line from constituents' economic interests to politicians' votes, and little attention is paid to the key role that political parties play in Congressional policy making. Although constituency or demand-side explanations dominate the literature, some scholars have incorporated partisan forces into their analysis of trade policy voting. The partisan approach argues that parties exert an effect independent of constituent interests. In this paper I focus on identifying if partisan forces have an independent influence on congressional trade policy voting, which is an issue that has mostly been ignored by the existing literature despite its centrality to the political economy of trade policy in the United States.

In this paper I first situate the trade policy literature into the larger debate about the effect of partisan and constituent forces on legislative voting. I then explain why the existing research designs used in the trade policy literature do not allow us to distinguish between constituency and party effects, even among the few scholars who acknowledge the importance of both. The flaws in the existing empirical analyses necessitate an alternative research design, and I demonstrate how matching politicians based on shared

constituencies allows us to determine if party has an effect on congressional voting in both the U.S. Senate and House. The results suggest that partisan forces are highly important to trade policy voting. Therefore, ignoring the role of partisan forces leads to an incomplete picture of the political economy of trade policy.

The paper proceeds as follows. In the next section I briefly outline the different theoretical arguments about the determinants of congressional voting on trade policy. In Section 2 I delve into the methodological flaws that afflict existing studies of voting on trade policy. Section 3 describes how I remedy these flaws to study trade policy voting in the U.S. Senate and in Section 4 I present empirical results from our Senate analysis. In Section 5 I describe the research design for the U.S. House and in Section 6 I present the results from the corresponding analysis. In Section 7 I discuss and conclude.

Situating the debate about voting on trade policy

A substantial literature has attempted to understand the determinants of Congressional policy. Two primary approaches have been used to explain policy outcomes. The first approach identifies the primary source of public policy as constituent interests that translate into the ideology or preferences of the key policymakers (Schattschneider 1935; Converse 1964; Mayhew 1974; Fiorina 1977; Poole and Rosenthal 1997; Kalt and Zupan 1984; Peltzman 1983, 1985; Mitchell and Munger 1991; Becker 1993; Meltzer and Richards 1981). In this vein of literature scholars vary in the emphasis they place on special interest groups (Schattschneider 1935; Mitchell and Munger 1991) versus overall constituent pressures (Mayhew 1974; Poole and Rosenthal

1997; Kalt and Zupan 1984), but the primary cause of congressional policy in this view is

demand from constituents.

Those who focus on the demand-side of trade voting typically build on economic

theories that relate changes in economic policy (such as tariff rates) to distributional

effects on different sectors of the economy and therefore to voters employed in those

sectors (Grossman and Helpman 19994; Hiscox 2002a, 2002b; Gilligan 1997; Frieden

1997, 1994; Rogowski 1987, 1989; Schonhardt-Bailey 2003).[1] Scholars working in this

vein of research differ in their specific theoretical approach, but they all share a

materialist view of the political process in which the economic characteristics of voters

translate into political preferences (Scheve and Slaughter 2001) that then affect the voting

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[1] For instance, the canonical Stolper-Samuelson Theorem has been used to show how changes in exposure to international trade affects domestic political coalitions between scarce and abundant factors (labor and capital) in an economy (Rogowski 1987). Following Rogowski's seminal work, scholars have utilized the Stolper-Samuelson theory in explaining the relationship between international factors and foreign economic policy.

The Stolper-Samuelson theory assumes that factors are completely mobile, that is labor in one industry can be used as labor in another industry. Therefore, this theory predicts that conflict over foreign economic policy will occur on broad-based class grounds, because the relatively abundant factor in an economy will favor free trade because trade increases the return to the factor whereas the relatively scarce factor will favor protection to protect its domestic rate of return. In this model it does not matter the industry in which one derives income from, the only concern is the factor that creates the income.

Alternatively, the Ricardo-Viner theory assumes that factors are at least partially immobile between industries. This leads to a contradictory prediction, namely that political conflict will occur between industries that seek to protect themselves from economic competition that drives down the return within their industry. In this model, all members of an industry, whether they are owners of capital, land or labor will take consistent positions.

behavior of political representatives.[2] This is essentially the same process modeled by the older scholarship on constituent pressures in American politics. Trade policy is one application of the relationship between constituents' demands and policy outcomes.

The second primary approach to understanding political outcomes has been termed New Institutionalism for its focus on the institutions that structure the policy making process. The institutionalist paradigm includes scholars who focus on the role of political parties in affecting congressional voting and outcomes (Cox and McCubbins 1993, 2005; Cox and Poole 2002; Kiewiet and McCubbins 1991; Rohde 1991; Aldrich 1995; Sinclair 1983, 1995, 2002; Snyder and Groseclose 2000; Jenkins 1999).[3] Party-based theories of congressional politics usually focus on either influence over members' votes or control of the legislative agenda.[4]

Scholars have argued that political parties took distinct positions on trade policy and used the tools at their disposal to influence voting (Fetter 1933; Taussig 1888; Stanwood 1967; Burnham 1981; Sundquist 1983; Epstein and O'Halloran 1996; Brady, Goldstein and Kessler 2002; Bailey, Goldstein and Weingast 1997; McGillvary 1996). For example, Fetter (1933) describes how the Smoot-Hawley Tariff in 1930 led to

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[2] The recent literature on constituency and U.S. foreign economic policy is a sophisticated version of pressure group theory (perhaps exemplified by Schattschneider's 1935 book about the Smoot-Hawley tariff in U.S.) in which coalitions/interest groups directly influence politicians' voting behavior.

[3] Scholars in this vein have also studied how institutions such as congressional committees (Fenno 1973; Shepsle and Weingast 1984, 1987; Krehbiel 1991) and the interaction between the different branches of government (Krehbiel 1998; Cox and McCubbins 2001; Tsebelis 2001) affect policy.

[4] In a comparative analysis Hankla (2006) also focuses on the role of political parties in trade policy. His results also indicate the importance of parties as an institution in affecting trade policy, but he is not explicitly examining the impact of party after accounting for constituency forces.

internal divisions within the Democratic Party. As usual the Southern Democrats opposed tariffs, but Northern Democrats were slowly beginning to favor higher tariffs. When push came to shove, however, nearly all members voted with their party thanks to the efforts of party leadership. Bailey, Goldstein and Weingast (1997) argue that the Democratic Party structured the policy-making process under the Reciprocal Trade Agreements Act to generate policy outcomes preferred by the party membership. McGillvary (1997) argues that party discipline can influence members' votes on trade policy and that political parties craft legislation to benefit their membership. Epstein and O'Halloran (1996) develop a model in which parties and constituents have preferences over the level of tariffs, but party control of government magnifies the actual change in tariff rates that are implemented. Although the mechanism by which partisan forces affect trade policy differs, the general argument in this line of scholars is that party has an effect on policy outcomes independent of constituency forces.[5]

Scholars have largely neglected to tie the trade policy debate to the literature on how constituents and partisan forces determine public policy.[6] The trade policy literature

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[5] The institutional analysis of trade policy voting sometimes begins with the same economic explanations of constituent preferences as the constituency-only approach. However, the institutional approach does not require voters to derive preferences from material interest in trade policy. As McCubbins and Weller (2007) point out tariffs can serve other purposes such as generating government revenue. Therefore preferences over tariffs may also reflects constituents' and party's preferences for the size of government in general. Bensel (2000) argues that during the late 19th century when Republicans were advocating high tariffs they also typically supported larger, more active government spending.

[6] An alternative view would argue that partisan voting we may simply reflect that members with similar ideologies are likely to be members of the same political party, and therefore we are observing the effect or ideology rather than party on voting. Although highly correlated, party does have effects independent of ideology. Between 1860 and

rightly belongs in the larger debate about the forces that affect U.S. policy because it is one policy area among the many that congress determines. To advance our understanding of how partisan and constituent forces affect trade policy I utilize a research design that allows me to determine if partisan forces exert an independent effect on trade policy voting In the process I shed light on the determinants of U.S. foreign economic policy and therefore help us to understand U.S. interaction with the rest of the world. Furthermore, our results may help us to refine our theories of interests and institutions as suggested by Brady and McCubbins (2007) to account for the conditions under which different explanations are more powerful.

Research Design: The Achilles Heel of Existing Trade Policy Studies

The empirical literature on trade policy voting largely relies on cross-sectional regression analyses of individual votes or analysis of the partisan divide on final passage votes. Regardless of the specific co-variates utilized in these regressions one cannot interpret the coefficients as causing the voting outcomes, because there are other, unmeasured variables that correlate with voting. For example, if different levels of factor mobility are the hypothesized cause of a vote, then the existing empirical approaches do not make sure that the observed politicians are identical in all other aspects that could affect voting (i.e. political party).

In Tables 1.1 and 1.2 I present the inference problem related to the determinants of voting (adapted from Greiner 2006). Each senator only votes on a given trade bill one time, but the causal question is how that same senator would vote if she were a member

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1930 in every state a split delegation, the senators voted with their party regardless of ideological placement in NOMINATE.

of a different political party or had a different type of industry in her state. Because it is

impossible to observe Senators voting under these different circumstances scholars have

typically attempted to compare different senators using some form of multivariate

regression. For instance, the standard empirical approach, as represented by Table 1.1,

has been to compare the voting decisions of Senators 1-4 to Senators 5-9 using a variety

of economic control variables to make the Senators comparable. Empirical analyses seek

to make individuals comparable by using regression techniques to "hold constant" lots of

different factors that could affect voting, but this approach may not create comparable

cases (Ho, Imai, King and Stewart 2005; Greiner 2007; Rosenbaum 1995), and therefore

it is not appropriate to draw causal inferences about the determinants of vote choice from

standard regressions.

Table 1.1: Hypothetical Relationship Between Liberal Trade Votes and Political Party

Senator	Treatment	Vote (D)	Vote (R)
1	D	Y	?
2	D	Y	?
3	D	Y	?
4	D	Y	?
5	R	?	N
6	R	?	N
7	R	?	N
8	R	?	N
9	R	?	N

If I simply compare Senators 1-4 to Senators 5-9 in Table 1.1 I would conclude

that party affiliation is a significant determinant of voting. I am not certain, however, that

the first four senators are identical to the second five senators in all ways except for assignment to a political party. Therefore, I cannot make causal inferences about the effect of political party.

Table 1.2: Hypothetical Relationship Between Liberal Trade Vote and Industry

Senator	Treatment	Vote (Exporting)	Vote (Importing)
1	Exporting industries	Y	??
2	Exporting industries	Y	??
3	Exporting industries	Y	??
4	Exporting industries	Y	??
5	Import-competing industry	??	N
6	Import-competing industry	??	N
7	Import-competing industry	??	N
8	Import-competing industry	??	N
9	Import-competing industry	??	N

Likewise a comparison of the two groups of senators in Table 1.2 would lead to the conclusion that the type of industry determines trade voting, but our inability to ensure that the groups are comparable precludes us from drawing causal inferences. In Table 1.3 I present one possible combination of party affiliation and industry. In Table 1.3 the senators who are Democrats also represent states with exporting industries and Republicans represent import-competing industries. As a result of the overlap between partisan and constituent factors I cannot determine if either factor has an effect on trade policy voting. If party affiliation and industry type are the two determinants of voting, then to determine causal impact I need cases where Senators are as similar as possible in all relevant characteristics other than the treatment being applied. That is, I need to

observe Senators who share values for industry type but differ in their partisan affiliation (or vice versa). I could then match Senators who share industry type and investigate if party affiliation affects voting. If there are no Senators who industry type but differ in party affiliation, then I cannot make causal claims about how either variable affects trade voting.

Table 1.3: Hypothetical Overlap Between Constituency Pressure and Political Party

Senator	Treatment #1	Treatment # 2	Vote
1	Exporting industries	D	Y
2	Exporting industries	D	Y
3	Exporting industries	D	Y
4	Exporting industries	D	Y
5	Import-competing industry	R	N
6	Import-competing industry	R	N
7	Import-competing industry	R	N
8	Import-competing industry	R	N
9	Import-competing industry	R	N

To determine if political party has an independent effect on voting requires more than using multivariate regression to hold the different influences constant. It requires a research design in which congress people have identical (or very similar) economic constituencies, but differ in party affiliation. I now turn to discuss how to implement such a research design.

Disentangling Party and Constituency in the U.S. Senate

For cross-sectional analyses a matching research design allows us to create the conditions for causal inference by comparing politicians who are theorized to be comparable on all relevant dimensions except the treatment variable under investigation

(Rosenbaum 1995; Greiner 2006; Ho, Imai, King and Stewart 2007).[7] In this section I seek to determine the effect that partisan affiliation has on trade policy voting once I account for constituency characteristics. A similar approach has been used by other scholars to analyze how often politicians who share a constituency vote together (Glazer and Robbins 1985; Dougan and Munger 1989; Higgs 1989; Schiller 2000, 2002; Poole and Rosenthal 1984; Cox 1987; Bullock and Brady 1983). As I explain in greater detail below, to disentangle party and constituency I focus on states where senators face similar constituency forces but different partisan forces – that is in states with split Senate delegations. The structure of the U.S. Senate makes it possible to design a cross-sectional study that matches politicians based on district factors while varying partisan affiliation.

Each state elects two senators who share the same statewide district, and therefore also face similar economic and social pressures from their geographic constituents as emphasized by preference-based explanations of policy outcomes.[8] By matching senators who are elected from the same state but differ in party affiliation, this research design

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[7] The use of MatchIt (Ho, Imai, King and Stewart 2007) has facilitated the expansion of matching research designs in political science. In this paper I do not utilize a software program to conduct the matching of politicians, because there is an easy, theoretically appropriate way to match politicians based on districts.

[8] Goff and Grier (1993) argue that there is not likely to be a Condorcet winning platform within a given district (state for senators) and therefore split voting will occur in districts with idiosyncratic voters where their interests cannot be represented by some measure of a district's average policy/economic preferences. The intuition for this result is that when there are many votes taken it is likely that the votes occur across a multi-dimensional space and therefore there is probably not a single platform that is a Condorcet winner. This does not affect the analysis in this paper. First, I am not concerned only with voting difference on a single issue rather than across multiple issues. Therefore, it seems likely that on a given issue (trade policy) there is likely to be a winning policy from the point of view of the district. Second, there is no reason that the idiosyncratic reasons should correlate with party across all of the states in a given analysis unless party has some effect on voting, which is consistent with a partisan influence on voting.

allows us to control for constituency factors that could affect trade policy voting while investigating partisan effects.[9] This amounts to one-to-one exact matching, which Ho, Imai, King and Stewart (2007) argue is one approach to pre-processing data:

> The idea is to match each treated unit with one control unit for which all the values of Xi are identical. Our preprocessed data set thus is the same as the original data set with any unmatched control units discarded and thus with Ti and Xi now independent. If all treated units are matched, this procedure eliminates all dependence on the functional form in the parametric analysis. (If some treated units cannot be matched, then they either need to be adjusted during parametric modeling, which of course risks extrapolation bias, or dropped, which can change the quantity of interest.) It is also highly intuitive since it directly parallels an experiment where we find pairs of units that are identical in all observable ways and assign one from each pair to be treated and the other to be a control. Then no matter what effect Xi has on Yi, we can ignore it entirely since Xi is literally held constant within each pair of units. (Ho, Imai, King and Stewart 2007)

The research design I use matches pairs of observations on X_i, which is simply the state from which a politician is elected, and therefore all state-level factors are identical between two senators from the same state.

One of the key advantages to our research design is that it obviates the need to rely on measures of constituent demands or interests. It is difficult to measure directly the interests of constituents[10] so there is some unspecified amount of error between the construct of constituency demands and the actual measures used in empirical analysis. It is unknown how different possible measures of constituents' demands may affect our empirical results.

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[9] I do not attempt to determine why members of a given political party vote together. That is a separate topic that I do not intend to address in this paper.

[10] Scheve and Slaughter (2001) show that voters' preferences do respond to economic factors but also to a host of other factors which our theories and empirical analyses often do not utilizing in predicting the preferences of a politician's constituents.

One possible objection to this research design is that the geographic constituency (the state) may not be the relevant constituency. Fiorina (1974) and Fenno (1978) point out there may be a difference between a politician's geographic constituency (all district members) and his electoral constituency (those who support him). For a Republican this might mean that only Republican voters are his/her constituents, and therefore rather than appeal to the median voter, the politician may appeal to the median Republican voter and not the overall median voter in his district. The conclusion from this argument is that I must consider carefully the perceived constituency of a politician if I want to understand how constituents affect voting. It is empirically difficult to address this potential problem.[11] This argument also implies that two Democratic senators elected from the same state may not share the same constituency. If perceived constituencies are politician-specific it will be nearly impossible to perform empirical analyses of constituency influence.[12]

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[11] One possibility is to equate a senator's reelection constituency with the constituencies of members of the House of Representatives from the same party in a given state. To measure constituent preferences I would then somehow aggregate across all of the Democrats in a state to determine the Democratic Senators constituency preferences. This might get us closer to estimating a senator's reelection constituency, but it also requires a considerable number of assumptions about correspondence between the House and Senate and about our ability to identify directly constituent interests.

[12] Bailey and Brady (1997) attempt to break states into two groups based on homogeneity of constituent interests. They argue that in heterogenous states we are likely to overstate the importance of political party in determining senate votes. This may be true, but as we report in the results section for vast periods in American history there is essentially no non-partisan trade voting and therefore it is hard to argue that our analytical technique leads to overstating party effects. Also, it's not clear that a state will be heterogenous with respect to a specific issue, in this case trade policy, as opposed to across a variety of issues which is how most scholars tend to model heterogeneity.

In addition to the empirical difficulties posed by Fenno's and Fiorina's arguments there are two theoretical arguments that suggest the multiple constituency problem is not fatal to this research design. First, the median voter model fits well with trade policy, because the policy choices fall along a single dimension. I can conceive of trade policy as being generally about more or less protection for U.S. industries. This may take the place of higher tariff levels for industries currently covered by the tariff or expanding the coverage of tariffs to new industries. In a standard unidimensional spatial model (Downs 1957; Black 1958) the median voter will determine policy choices. The standard spatial model assumes that policy choices can be represented as points on a line and that the actor occupying the position of the median voter will determine the policy that is chosen because no winning coalition can be assembled without that voter. Voters are likely to fall along a continuum of preferences regarding trade policy, because in most states there will be voters that represent a variety of industries or different factors of production. Therefore, if the same set of voters turns out to vote in both of a state's Senate elections, then the same voter will be the median voter and should affect the elected senator's choice of trade policy. Even if a Republican senator draws his support from the right-leaning voters and a Democratic senator from the left-leaning voters, the median voter for both senators should be the same (or nearly the same) in terms of preferences for protection, and therefore the constituency-based models would predict that the Senators should have identical policy preferences. This argument suggests that the concerns raised by Fiorina and Fenno should be minimized in the case of trade policy.

Second, the staggered nature of Senate elections makes it difficult for candidates and voters to successfully carve the electorate into multiple, non-overlapping constituencies. To partition the electorate requires that politicians are unable or unwilling to attempt to appeal to voters in another candidate's constituency. Politicians may attempt to attract an oversized coalition of voters so they can still win if a few voters defect from the group. Creating multiple, non-overlapping pieces also requires that voters can credibly commit to not becoming a part of another senator's constituency. If voters receive some benefits (policy or otherwise) from being a part of a senator's constituents, then voters have an incentive to be in the constituency of both Senators, and there is no credible way for them to commit to only being in one candidate's constituency. For the empirical and theoretical reasons outlined here I continue to adopt empirical approach that focuses on geographic constituency (see Aldrich et al 2007 for another justification for focusing on statewide factors when comparing senators from the same state).

In addition to multiple notions of constituency there are also multiple notions of party that may be worth disentangling. Parties can influence senators' voting behavior through actions internal to Congress and/or external to Congress. Within congress partisan forces can affect a senators vote through a variety of mechanisms such as determining the pieces of legislation that achieve a final passage vote (Cox and McCubbins 2006); rewarding loyalty with committee assignments and other legislative benefits (Goodman and Nokken 2007); or funneling pork to a legislator's district (Poterba and Levitt 1999). Outside of Congress a political party can also take actions to influence members' votes such as supporting an election campaign or preventing possible

challengers from running against an incumbent. In theory, I could begin to tease out the

effects by looking at the voting of lame duck legislators who are only exposed to internal

partisan influences (Nokken 2007). However, these legislators also do not face any

constituency pressures so analyzing these legislators is not helpful to the goal of this

paper, which is to determine if party has an independent effect on legislative voting. The

analytical approach I utilize in this paper does not allow us to separate out the two

different mechanisms for party effects, but it does allow us to determine if party has an

effect on voting.

Table 1.4: Inferences from Observing Party and Vote of a State's Senators

	Same Party	**Different Party**
Same Vote	If they vote against rest of party suggests constituency effect If they vote with rest of party unclear	Constituency for senator that votes against party
Different Vote	Unclear, perhaps some constituency effect (why does it affect only one senator?)	Party for at least one of the senators

In the analysis presented in Section 4 I include senators from states with a split

Senate delegation – that is where Senators in a given state are from different political

parties. Each matched senator is either exposed to the "Republican" treatment or the

"Democrat" treatment, and the district-level influences on a pair of Senators from the

same state are as identical as possible.[13] In Table 1.4 I lay out the inferences I can reach

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[13] Ideally we might match pairs of Senators from the same state (to control for constituency effects) who also have nearly identical ideology scores (from NOMINATE), because ideology is another possible determinant of trade voting. In practice, this is simply impossible because it is not possible to perform a matching analysis if ideology is

about the causes of voting depending on the combination of votes and party affiliation within a given state for senators that share constituency. As the table points out, if party is a significant determinant of voting after accounting for constituency characteristics, then I should find that the voting behavior of Democrats and Republicans is statistically different. However, if constituency characteristics dominate partisan effects, then I should find that party does not have a significant relationship with voting outcomes.

Trade Votes in the U.S. Senate

In Table 1.5 I list the trade policy votes that I analyze in the U.S. Senate and the states in which there is a split delegation. Data on individual senator's votes were drawn from Poole and Rosenthal's VoteView program. For the Senate analysis I utilized the list of important trade votes in Hiscox (2002a, 2002b) that occurred between 1832 and 1994. I used this time period because 1832 is generally when it is argued that political parties begin to have a national identity and platform (McCormick 1966), and therefore this is when I can identify a party with a consistent policy preference across regions. Between 1832 and 1970 the Republicans, Whigs and Anti-Jacksonians are coded as the protectionist party and the Democrats and Jacksonians are coded as the liberal party. After 1970, Republican party is the liberal trade party and the Democrats the protectionist party. I follow the coding of votes as used by Hiscox (2002a, 2002b) so that a vote is coded 1 if it is a protectionist vote and 0 if it is a liberal vote.

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one of the dimensions on which senators are matched. However, we have performed our analysis by looking at a small range of legislators in a given regression. For instance, analyzing moderate legislators together to determine if party is still significant. In all such regressions party is still highly significant. The results are available upon request from the author.

To determine whether political party has an effect on voting I estimate the following regression for the matched pairs of Senators and discard the votes of Senators for whom I lack a match. This regression allows us to estimate the effect of party on trade policy voting independent of state-specific factors which are accounted for using the matching design.

$$Pr(ProtectionVote=1) = \alpha + \beta_1 ProtectionParty + \varepsilon$$

Table 1.5: States with a Split U.S. Senate Delegation

Legislation	States with two voting senators from different parties
1832 Tariff	Indiana, New Jersey, New Hampshire
1842 Tariff	Maine, New York, Connecticut, South Carolina
1846 Tariff	Virginia, Michigan, Maine, Connecticut, Ohio, Georgia, New Hampshire, Tennessee
1861 Morill Tariff	Indiana, Maryland, Minnesota
1888 Mills Bill	Ohio
1890 McKinley	Delaware
1894 Gorman	Delaware, Illinois, Nebraska, North Dakota, California
1897 Dingley	Indiana, Wisconsin, Kansas, Kentucky, West Virginia, Utah, California,
1913 Underwood Tariff	New Hampshire, New York, Illinois, Kansas, Nebraska, Kentucky, Maryland, Washington
1930 Smoot Hawley Tariff	Massachusetts, Iowa, Missouri, Kentucky, Maryland, Oklahoma, Nevada, Wyoming, Washington
1934 RTAA	Connecticut, New Hampshire, Ohio, Wisconsin, Iowa, Kansas, Minnesota, Missouri, Nebraska, South Dakota, Maryland, Idaho, New Mexico, Wyoming
1937 RTA extension	Massachusetts, New Hampshire, Pennsylvania, Michigan, Kansas, Nebraska,
1945 RTAA extension	Massachusetts, Wisconsin, Missouri, Oklahoma, West Virginia, Colorado, Wyoming
1955 RTAA Extension	New York, Illinois, Michigan, Minnesota, Arizona, Nevada, Wyoming, Oregon
1974 Tariff	Delaware, New Jersey, Illinois, Michigan, Ohio, Virginia, Florida, North Carolina, South Carolina, Colorado, Idaho, Utah, Wyoming, Alaska, Hawaii
1994 NAFTA	Maine, Rhode Island, Vermont, Delaware, New York, Pennsylvania, Iowa, Minnesota, South Dakota, Virginia, Florida, Georgia, South Carolina, Kentucky, Oklahoma, Arizona, Colorado, Montana, New Mexico, Washington
1994 GATT	Maine, Rhode Island, Vermont, Delaware, New York, Pennsylvania, Iowa, Minnesota, South Dakota, Virginia, Florida, Georgia, South Carolina, Kentucky, Arizona, Colorado, Montana, New Mexico, Washington

In Table 1.6 I present the overall results for the regression above when I include all of the pieces of legislation, and in later tables I analyze different time periods within the overall sample. The overall regression suggests that party has a significant effect on whether or not a senator casts a protectionist vote. I estimate how a change in party from the liberal to the protectionist party affects the probability of a protectionist trade policy vote using Clarify (King, Tomz and Wittenberg 2001). In the overall cross section a politician switching from the liberal to the protectionist political party increases the probability of a protectionist vote by 0.48.

Table 1.6: Party Effect on Protectionist Voting 1832-1994

	Coefficient (S.E.)	Estimated change in protectionist voting if member switches from liberal to protectionist party [95% C.I.]
Protectionist Party	1.24 (0.17)**	0.44 (0.34, 0.54)
Constant	-0.93 (0.12)**	
Log Likelihood	-155.77	
N	278	

** = significant at 0.01 level

I split the data into two different time periods.[14] The first time period covers from the earliest vote in 1832 up to the votes in the 1970s. I group all of these votes together because the political parties maintain a consistent position on trade policy over this period. During the 1970s, however, partisan positions switch and the Democrats become the party of protectionism and the Republicans the free trade party. The results in Table 7 show that the importance of party is confirmed between 1832 and 1970. The results also

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[14] I split the dataset into a variety of different economic and politically relevant time periods. The substantive results do not change based on when the data are divided.

show that after 1970 there is no significant relationship between political party and

protectionist voting in the U.S. Senate. During the post 1970 period I only analyze three

votes, two of which take place in 1994 (the GATT and NAFTA votes) so there is little

opportunity to find that party is statistically significant. The votes analyzed in this time

period may reflect a lack of partisan forces although party forces have been argued to be

important on trade policy during the last 30 years (see Ladewig 2005 for a discussion of

recent partisan forces and trade policy).

Table 1.7: Estimated Party Effect on Senate Trade Policy Voting 1832-1994

	1832 - 1970		1970-1994	
	Coefficient (S.E.)	Effect of party change [95% C.I.]	Coefficient (S.E.)	Effect of party change [95% C.I.]
Protectionist Party	2.08 (0.24)**	0.69 [0.57, 0.79]	0.19 (0.25)	0.05 [-0.11, 0.21]
Constant	-1.13 (0.17)**		-0.68 (0.18)**	
Log Likelihood	-70.3		-64.23	
N	168		110	

** = significant at 0.01 level

The GATT and NAFTA votes have a patina of non-partisan voting, but in

actuality the politics behind both are highly partisan, but they are not partisan in a way

that I can discover through regression analysis. It is not clear that the Democratic senators

who voted against their party's position in 1994 (by voting for NAFTA and GATT) come

from states where their constituents were particularly likely to benefit from freer trade. It

does appear that the Democrats who voted for NAFTA were often party leaders. This is

not a surprise, as NAFTA was one of President Clinton's major policy proposals

(although it was originally negotiated by a Republican president) and there was tenuous

Democratic support for the legislation. In fact, the passage of NAFTA is one of the significant examples of failed majority party agenda control discussed by Cox and McCubbins (2005). Cox and McCubbins (2005) describe the bargain between President Clinton and Democratic Party leaders that led to the passage of NAFTA despite the majority of the Democrats opposing its passage. In brief, the bargain involved Democratic leaders delivering enough votes to garner NAFTA's passage and support President Clinton despite the party membership's opposition to free trade. Although the final outcome suggests the vote was non-partisan, it is clear that underlying the outcome was a distinctly party-driven political story. The vote on GATT in 1994 reflects a similar bargain between the President and Congressional leadership.

A reader might object that I am explaining away the only votes where partisan effects cannot be found. Further, a reader might claim that if I were to investigate all of the other supposed instances of partisan effects I would find that constituent forces are actually the dominant factors and party only appears to be significant in the regression analysis. These are reasonable questions, but there are a few reasons to believe that the NAFTA and GATT votes are truly anomalous rather than part of a larger pattern. First, as I mentioned earlier the NAFTA votes are an unusual set of votes not just within trade policy but within all Congressional final passage votes over more than 100 years. Cox and McCubbins (2005) find that among the thousands of votes they analyze the NAFTA vote sticks out as one of a few dozen times where the majority party's agenda control breaks down and a bill the majority opposes passes against the majority's wishes. Second, scholars of American politics have found that the political parties have become

more polarized in the last 30 years so the NAFTA vote appears unusual in that context

(McCarty, Poole and Rosenthal 2006). Third, the strength of partisan forces between

1832 and 1970 fits with the historical evidence about parties taking disparate positions on

trade policy (Taussig 1966; Fetter 1933; Stanwood 1903; Irwin 2003). These three

reasons suggest that the overall partisan findings do not reflect a buried constituency

explanation.

Are there states with particularly protectionist/liberal constituents?

One advantage to typical empirical analyses of trade voting is that a measure of

constituency influence (assuming it actually captures constituent demands) can help us

determine whether or not there are some states where the effect of constituency trumps

the effect of party, even if party is significant across the entire group of states. To shed

some light on this issue, I build on the earlier analysis that examined senators from the

same state who differed in party, but I now add to the previous regression different

measures of constituents' economic interests. The addition of state-level economic

variables can help me to determine whether or not there are a considerable number of

states where constituency factors are still important even when pairs of senators are

matched based on their constituencies.

To perform this analysis I utilize the economic data collected by Hiscox (2002a,

2002b),[15] in which he computes the importance of import-competing industries and

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[15] An alternative way to examine the effect of constituency factors that does not limit us
to economic variables is to first examine the states used in the regressions above, and
identify states where the senators represent different parties but vote the same. If a state
meets this criterion it suggests that constituent interests may outweigh partisan influence.
The states where the two senators were from different parties and a protectionist-party

export industries in each state for the relevant years. He also computes measures of the

importance of farm employment, manufacturing employment and manufacturing profits.

I report the results from the regressions run with the import and export measures, but the

other results are substantively similar and available upon request. Based on standard

economic theories the predictions for the import and export variables are:

Imports: increase the probability of a protectionist vote by a senator

Exports: decrease the probability of a protectionist vote by a senator

To estimate whether or not the economic variables have an effect on voting in the

matched group of senators I estimate the following regression:

$$Pr(ProtectionVote=1) = \alpha + \beta_1 ProtectionParty + \beta_2 Imports + \beta_3 Exports + \varepsilon$$

I expect that even with the addition of the economic variables the coefficient on

Protection Party will remain positive and significant. I expect that the constituency

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senator voted liberally or a liberal-party senator voted protectionist are listed in Table A1 for each of the votes we analyze. If we refer back to Table 5 we can identify the states in which there were two senators from different parties, and the states listed in Table A1 are a subset of the states in Table 5.

It is immediately clear that for many of the votes we examine prior to 1930 (71st Congress) there are few instances of states in which senators come from different parties and one of the senators votes against his or her party. This implies that adding a measure of constituent interests to regressions covering this period will not affect our results about the importance of party, because constituent interests will be insignificant. [15]

After the 79th Congress we begin to observe a greater number of senators voting against their party. This of course coincides with the weaker effect of party that we found in the previous regressions. The weaker party effect may mean that constituent pressures are more important during this period, but it does not necessarily imply that the economic characteristics utilized in most of the literature will correlate with non-party voting among this group of senators. To determine if there is a consistent explanation for the cases where party does not explain votes will require additional research. Overall, the results from this section suggest that adding measures of constituent economic characteristics would add little to our results.

characteristics, as captured by the coefficients on Imports and Exports, will not be

statistically significant in the regression. In Table 1.8 I present the results of this

regression above for each of the time periods utilized in the prior regressions.

Table 1.8: Effect of Economic Variables on Voting in States with a Split Delegation

	1832-1994	1832-1970	1974 - 1994
	Coefficient (s.e.)	Coefficient (s.e.)	Coefficient (s.e.)
Protectionist Party	1.19 (0.17)**	2.12 (0.26)**	0.19 (0.26)
Imports	1.36 (1.29)	3.35 (2.25)	1.51 (1.72)
Exports	-0.14 (0.45)	-1.19 (0.57)*	-5.54 (4.59)
Constant	-0.99 (0.16)	-1.07 (0.25) **	-0.57 (0.32)
Log Likelihood	-148.75	-64.5	-60.09
N	264	156	108

As expected, membership in the protectionist party is highly significant and the

economic variables are largely insignificant in the regressions in Table 1.9.

Table 1.9: Effect of Party Change on Probability of a Protectionist Vote

	1832-1994	1832-1970	1970-1994
Predicted change in probability of a protectionist vote, when party changes from liberal to protectionist [95% C.I.]	0.42 [0.31, 0.53]	0.70 [0.57, 0.81]	0.06 [-.11, 0.22]

Exports and Imports variables set at their mean values within each time period

It is interesting that between 1970 and 1994 both the party and economic variables are

insignificant. If the NAFTA and GATT votes of 1993 and 1994 were really highly

partisan as the stories surrounding them suggest, then the lack of significant of the

economic variables may simply reflect the dominance of political parties. The results in

Table 10 report the predicted effect that a change in political party from liberal to

protectionist has on the probability of a protectionist vote. As expected based on the previous results the effect of protectionist party is substantively very significant in all but the last time period. Overall the results in this section confirm the previous analysis that found political parties to be highly significant when senators are matched on their constituency characteristics.

House of Representatives Research Design

Unlike the U.S. Senate, members elected to the House of Representatives in a given year typically do not come from the same electoral district.[16] However, the close temporal proximity of elections in the House of Representatives makes it possible for us to match the representative from a given district at time t to the representative from the same district at time $t+1$, and then determine whether a change in party affiliation, holding the district constant, affects trade policy votes. This is equivalent to asking whether the representative from Michigan's 1st Congressional District votes the same whether he is a Republican or Democrat.

To conduct this analysis I examine votes that occur close in time, because temporally proximal votes provide a way to minimize changes in the economic characteristics of a Congressional district.[17] I do not attempt to measure directly the constituency characteristics of a given district. Instead I hold the congressional district constant and examine votes that happen temporally close to each other, because it is

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[16] With the exception of states where multiple Representatives are elected at-large in a given year. For now we exclude these Representatives from our time-series analysis.
[17] One possible advantage to analyzing votes in the House is that the smaller size of congressional districts makes it less likely that there is a considerable difference between a politician's geographic and electoral constituency.

unlikely that the economic and social characteristics of a district will change substantially between two votes that occur close in time. This provides us with a reliable way to match Representatives based on district-level constituency characteristics that could impact voting. There are three possible ways that party affiliation in a district can change.

- Districts where the partisan affiliation does not change.

- Districts where a Republican represents the district at time t and a Democrat at time t+1.

- Districts where a Democrat represents the district at time t and a Republican at time t+1

If constituency characteristics dominate voting decisions, then I should observe that representatives from a district vote the same on both pieces of legislation regardless of their party affiliation. However, if partisan factors independently affect voting, then I expect that changes in partisan affiliation, holding the district constant, will lead to changes in voting. If both partisan affiliation and economic factors stay the same, then neither theory predicts a change in voting behavior.

Based on the proposed relationship between political party and congressional votes I offer the following hypotheses:

H1: If district switches from Democratic to Republican it will:
a. increase the probability of a vote switching from liberal to protectionist

H2: If a district switches from Republican to Democratic it will:
a. increase the probability of a vote switching from protectionist to liberal

In each district where the partisan affiliation of the district changed either a politician lost an election to a candidate from a different party, chose not to run and a politician from a different party won, or the politician switched parties. The astute reader will notice that there are two possible ways that the results could indicate partisan influence. First, the politician at time t (say a Democrat) may have voted with his district's preferences on trade policy (by voting liberally), and then lost to a Republican in the next election. Then, on the next trade policy vote the Republican may vote against his district's trade policy preferences (by voting protectionist) but with his party. This would indicate the partisan forces trumping district constituency forces. Second, a politician in a given district at time t (again, presume a Democrat) may have voted against his district's preferences on trade policy (by voting for liberal trade policy) and then lost to a Republican. The newly elected Republican then votes with his district's preferences on trade policy (by voting protectionist). In this second example the partisan effect reveals itself when the Democrat votes with his party but against his constituency in the initial vote. I cannot determine from the proposed methodology which way the partisan influence operates or how parties affect voting, but either explanation is consistent with partisan influence on trade policy voting.

In the analysis of voting in the U.S. House of Representatives I focus on votes regarding the Reciprocal Trade Agreements Act (RTAA) and its subsequent extensions. The RTAA constituted a delegation from Congress to the President of the authority to negotiate reciprocal trade agreements (see O'Halloran 1994 for a discussion). I examine six votes on the RTAA that occur between 1934 and 1945. I compare the following pairs

of votes: the RTAA authorization in 1933-35 to the reauthorization in 1937-39; the 1937-39 reauthorization and the 1939-41 reauthorization; and the 1943-45 reauthorization and the 1945-47 reauthorization. I chose these pairs because they are close in time, which minimizes shifts in the economic composition of congressional districts. Any pair of votes is separated by no more than four years, which is close enough that I do not expect major demographic, economic or social changes within a congressional district. Standard empirical measures of constituent economic preferences are often collected only every few years; therefore, our technique is at least as sensitive to temporal changes as the approach used in other empirical scholarship. Additionally, the votes I compare do not involve any changes in the party of the U.S. president, which could affect voting on the RTAA because it is an issue of congressional delegation to the president.

A critical assumption in this research design is that the economic/constituent forces that influence congressional actors do not change between the two compared votes. This seems like a reasonable assumption because it is unlikely that the constituents in a given Congressional district undergo dramatic changes over the course of a few years.[18] However, these votes cover a nearly fifteen year period during which time the political and economic environment may have changed. In particular, the time period goes from World War II to the beginning of the Cold War. The votes could be classified into two time periods: 1930s and 1940s. It would be a potential problem for this research

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[18] Economic data usually does not see profound shifts over a short period of time. Furthermore, the standard approach to measuring economic variables usually only measures their levels every few years at the most often. There are also many problems with matching the measured data to congressional district and classifying economic data into the theoretically relevant categories.

design if significant effects only occurred at the same time as one of these other potential

regime changes. However, as I show in the next section the results are generally robust

across this twenty year time period. Therefore, I am reasonably confident that it is

partisan changes and not something else driving the results I report.

Trade Policy Voting in the House of Representatives

To determine the effect of partisan and constituent factors I estimate two probit

regressions for each vote comparison to estimate how changes in the partisan affiliation

of a given district affect voting. [19] Voting for the RTAA or one of its extensions is coded

as a liberal vote and voting against it is coded as a protectionist vote.

To test these hypotheses I estimate the following two regressions for each of the

paired vote comparisons.

$$\text{Pr(Liberal vote}_t \mid \text{Protectionist vote}_{t+1}) = \alpha + \beta_1 \text{Dem2Rep}_{t+1} + \beta_2 \text{Rep2Dem}_{t+1} + \varepsilon$$

$$\text{Pr(Protectionist vote}_t \mid \text{Liberal vote}_{t+1}) = \alpha + \beta_1 \text{Dem2Rep}_{t+1} + \beta_2 \text{Rep2Dem}_{t+1} + \varepsilon$$

In each of regression the excluded category is districts where there is no change in the

partisan affiliation of the district's representative. This allows us to interpret the

coefficients relative to some baseline probability of a vote switching. For some of the

paired vote comparisons it was impossible to estimate a coefficient because when

districts switched from Democrat to Republican it perfectly predicted failure in the

regression.

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[19] The empirical literature on constituents and economic factors such as that by Hiscox (2002) and Gilligan (1997) only has economic data at five year intervals. Therefore, these scholars also implicitly assume that the relevant economic factors do not change significantly during the five year intervals between observations. This is essentially the same as the assumption we make about the changes in constituency factors between votes.

Rather than presenting the regression coefficients I utilized Clarify (King, Tomz and Wittenberg 2001) to generate predicted probabilities of a vote changing based on the regressions described above.[20] The results are presented in Figures 1.1 and 1.2. Figure 1.1 presents the predicted effect of a district switching from Democratic to Republican on voting outcomes. Figure 1.2 displays the predicted effects of a district switching from Republican to Democratic on voting outcomes. As expected when districts change from Democratic to Republican the probability of a vote switching from liberal to protectionist increases significantly.
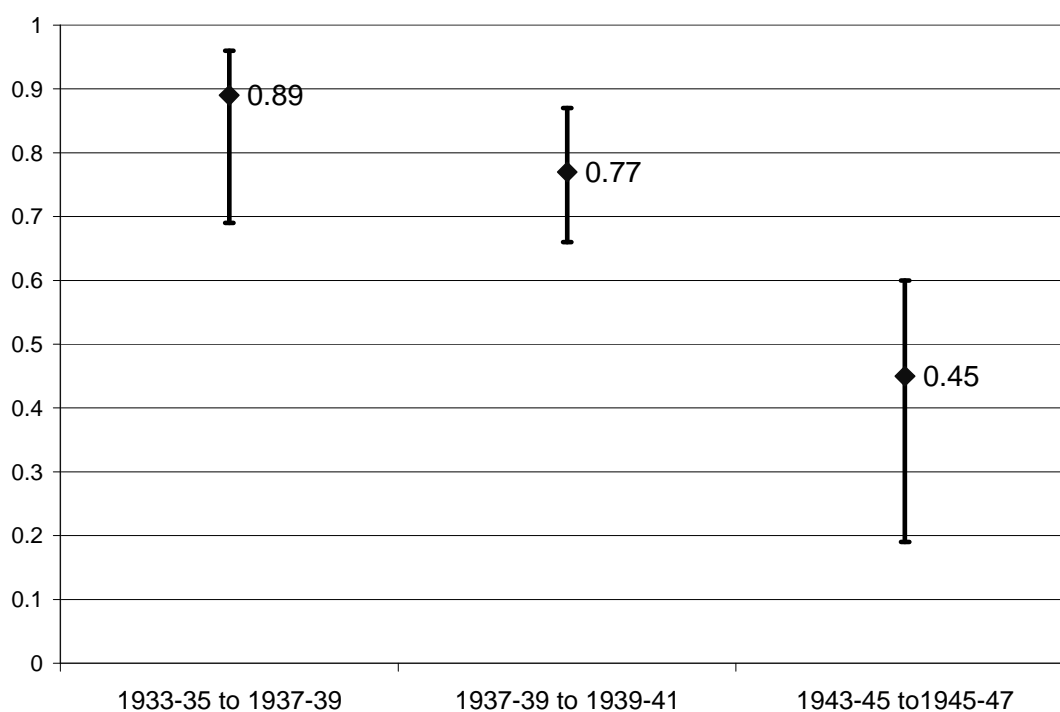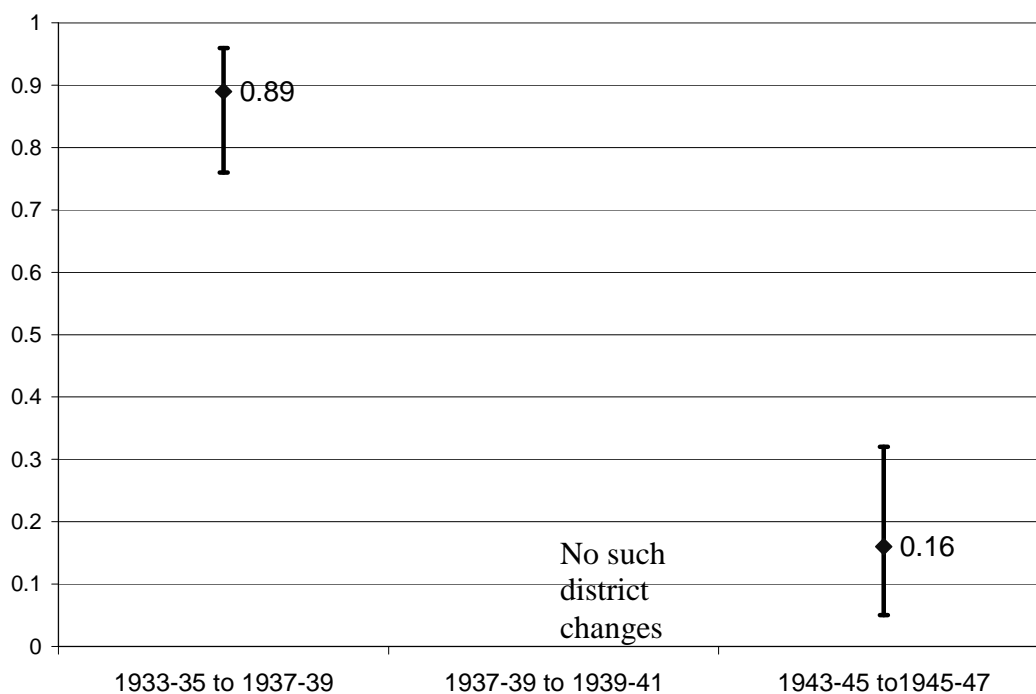

Bars indicate 95% confidence interval

Figure 1.1: Effect of a District Switching from Democrat to Republican on Probability of Vote Switching from Liberal to Protectionist

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[20] Full results and data replication is available at dss.ucsd.edu/~nweller

Overall, changes in the party affiliation of a Congressional district are highly significant predictors of changes in voting behavior. Beyond the perfectly predicted cases, the effect of a partisan switch varies considerably from vote to vote, but the magnitude of the effect is often quite large. [21] For instance, a switch in party affiliation from Democrat to Republican increases the probability of a vote switching from liberal to protectionist by 0.89 when comparing the vote congressional session of1937-39 and the vote from the 1939-41 session. Substantively this is very important as it means that in almost all of districts in which the representative switched from a Democrat to Republican the vote switched from liberal to protectionist, exactly as predicted by party-based explanation. Figure 1.2 shows that a change from Republican to Democrat likewise increases the probability of a vote switching from protectionist to liberal by 0.89. These are the largest effects, but among the votes in this analysis the results were always significant and in the predicted direction.

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[21] An alternative way to think about these results is that in the first period a politician voted with his party (say a Democrat voted liberally) but this upset his constituency, and they elected a Republican. The Republican then voted with his party and against protectionism, which is a sign of voting with party but also the role of constituency. From the party point of view this still suggests that the initial Democratic vote was against the constituency and with the party in a way that caused the representative to lose his job. This still would seem to suggest that partisan factors are important in a Representatives voting calculus, because a member voted with his party to his own detriment.

1
0.9 — ◆ 0.89
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1 — No such district changes — ◆ 0.16
0

1933-35 to 1937-39   1937-39 to 1939-41   1943-45 to1945-47

Bars indicate 95% confidence interval

Figure 1.2: Effect of a District Switching from Republican to Democrat on Probability of a Vote Switching from Protectionist to Liberal

The analysis presented in these figures shows that by and large when district party affiliation changes votes change as well, which is consistent with the partisan explanation for voting and inconsistent with constituency-determined voting.

In this section I tested whether partisan factors affect U.S. House trade policy voting even when accounting for constituency factors. Rather than the standard approach of attempting to measure constituency factors and include such measures in a multivariate regression, I relied on a research design that controls for partisan factors by examining the same congressional district over a narrow time period, thereby holding constant constituency factors. In doing so, my results shed light on the literature that discusses the

determinants of trade policy voting. First, my results demonstrate that parties exert a significant effect on the voting behavior of members of the House of Representatives. The evidence I present in this section demonstrates that political parties are a significant determinant of trade policy voting and the results call into calls into question the overwhelming focus in international relations on constituency based explanations for voting. Second, partisan effects are not constant across time or across changes in party. That is, when a district moves from Democratic to Republican it may not have the same effect as when the district moves from Republican to Democratic. This suggests that the conditions under which party has an effect on voting are not yet fully understood. To understand this aspect will require further research into the districts that changed and how the constituencies did or did not change at the same time.

Conclusion

In this paper I situated the political economy of trade policy into the larger debate in American politics about the effect of constituency and party on congressional voting. This debate is the core of the literature on the determinants of trade policy, but much of the existing trade policy studies have not connected themselves to this long-standing debate in American politics. I then presented research designs for the Senate and House that are superior to the standard method of measuring constituency demands and then attempting to parse our partisan forces. My proposed methods make fewer assumptions about the nature of constituency demands and do not require accurate measurement of the various constructs that constituency-based theories predict will influence congressional voting.

My results suggest that in the U.S. Senate party has been a significant factor in trade policy voting for most of U.S. history. Only in the last 30 years does the influence of party appear to have waned, but closer analysis of the votes taken during that period suggest that partisan forces still influence trade policy votes. The analysis of the votes in the House of Representatives on the RTAA also suggests that party played a large role in determining the voting decisions of Representatives. These findings suggest that delegating policy-making authority to the President did not eliminate the effect of party on trade policy. Instead it appears that even when the issue is delegation to the President parties continue to affect a member's vote.

My results suggest that political representatives do not simply respond to their constituents' demands when they cast their votes. Instead, political parties exert an independent effect on senators' and representatives' voting behavior. One implication from this is that the exclusively demand-side explanations for trade policy voting misconstrue congressional voting on trade policy. To better understand the political economy of trade policy requires us to incorporate both the material interests of constituents and the institutions that affect policy outcomes into our theories of Congressional policy making.

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Table 1.A1: Constituent-driven Voting in States with Split Delegations

House (Year)	States where protectionist-party senator voted liberally	States where liberal-party senator voted protectionist
22 (1832)	None	New Hampshire, New Jersey
27 (1842)	South Carolina	
29 (1846)	Tennessee	Connecticut
37 (1861)	None	None
50 (1888)	None	None
51 (1890)	None	None
53 (1894)	None	None
55 (1897)	None	None
63 (1913)	None	None
71 (1930)	None	Florida, Nevada, New Mexico
73 (1934)	Wisconsin, Kansas, Nebraska, South Dakota	
75 (1937)	None	New York
79 (1945)	Massachusetts, Michigan, Iowa, Missouri, Oregon	Massachusetts, Texas, Colorado
84 (1955)	Massachusetts, New York, Illinois, Wisconsin, Minnesota, Arizona	Nevada, Wyoming
93 (1974)	Delaware, New Jersey, Michigan, Ohio, North Dakota, Colorado, Idaho, Utah, Wyoming	Massachusetts, New Jersey, Virginia, Colorado, Alaska
103 (1994) NAFTA	Maine, Rhode Island, Vermont, Delaware, Iowa, South Dakota, Virginia, Florida, Georgia, Oklahoma, Arizona, New Mexico, Washington	Maine, New York, South Carolina, Montana
103 (1994) GATT	Maine, Rhode Island, Delaware, New York, Pennsylvania, Iowa, South Dakota, Virginia, Georgia, Kentucky, Arizona, New Mexico, Washington	Vermont, New York, South Carolina, Colorado, Montana

Chapter 2: Tax Man Cometh: Income taxation as a measure of state capacity (with Melissa Ziegler)

Introduction

State capacity is widely discussed by scholars in political science and economics, but there is no consistent definition nor agreed upon measure of the concept. In this paper we review the prominent definitions and measures of state capacity and demonstrate that the underlying theme in the various definitions is the ability of a state to implement public policy. We adopt a definition of state capacity that focuses on policy implementation and propose to measure the construct by examining the proportion of total tax revenue from income taxes. We make three major contributions in this paper. First, unlike other scholars, we focus explicitly on income tax collection rather than looking at overall tax collection. Second, we demonstrate the construct validity of our measure. Third, we compare our measure to the most commonly used indicator for state capacity, tax/GDP, to demonstrate the construct validity of that measure and how ours performs better for our large sample of cases.

The paper proceeds as follows. In Section 1 we review the existing definitions of state capacity. We then present our definition and measure of state capacity in Section 2. In Section 3 we describe other measures of state capacity and explain why they are inappropriate measures of our construct. In Section 4 we demonstrate construct validity of our measure by focusing on face, convergent and discriminant validity. In section 5 we compare our measure of state capacity with the most commonly used tax-based measure (total tax/GDP). In Section 6 we conclude.

What is State Capacity?

State capacity is a commonly used term in political science, economics and public policy. Despite its importance, an agreed-upon definition and measure have been elusive. In this section we review the existing definitions of state capacity and argue that at their core these measures are concerned with a state's ability to implement public policy. The various definitions of state capacity can be usefully broken into two rough categories, corresponding to Mann's (1993) work. Mann divides capacity into "despotic" and "infrastructural" power. Both types of definitions are focused upon policy implementation, but they differ in how states develop the capacity to implement policy. First, despotic power is the capability of leaders and bureaucrats to act *autonomously* from societal actors. This may include imposing force, ensuring compliance with unpopular legislation, or implementing policy against espoused wishes of the masses. This "top-down" view of capacity is commonly seen in literature on bureaucratic organization, in particular in the state promotion of economic development (c.f., Johnson 1982; Haggard 1990). Related to this, Grindle's "technical capacity," state capacity is similar in that it is the "ability to formulate and manage macro-economic policies." Accordingly, technical capacity requires skilled decision-makers at the highest levels of government that can make tough choices to ensure economic stability and promote economic growth. The literature on despotic power is centrally concerned with policy implementation, evident most clearly when states overcome societal resistance. Mann's second type of capacity is infrastructural power. Infrastructural power refers to the means of the state to *penetrate society* to perform state tasks. A state with this type of

capacity is able to implement policy and enforce laws because of its effective *interaction* with society, of policy implementation and enforcement of laws. This view of state capacity is akin to Migdal's (1988) and Midgal, Kohli and Shue's (1994) idea of "social control" whereby the state is able to act in concert with citizens, especially to regulate social behavior and extract resources.

In studies of capacity built upon this definition, policy implementation requires "synergy" with societal actors (Evans 1995). Social capital within society and between state and societal actors strengthens ties between state and society and promotes the effectiveness of state activity (Putnam 1993). These state and society relationships can produce a tighter connection between supply and demand for government services, improving capacity by reducing resistance to government programs and providing channels for conflict resolution (World Bank 1996; Grindle 1995). Policy implementation is the driving interest of works on infrastructural power. Despotic and infrastructural power both demand "institutional capacity," or states with "authoritative and effective 'rules of the game' to regulate economic and political behavior" (Grindle 1995). The political structures must both provide incentives for leaders and bureaucrats to implement policies. The design of institutions of government is therefore crucial to capacity; states must be able to credibly commit to policies and ensure compliance at a reasonable cost (Pierre and Painter 2005).

The reviewed literature, we argue, has a unified interest in explaining success at policy implementation. Where they disagree is upon the best way for a state to do this and who are the crucial actors. In this paper we focus on how to find evidence for

capacity, not with the means of acquiring it. Our measure should be a valid indicator for all studies that define state capacity as policy implementation.[22]

Our definition and measure of state capacity

Our definition

We define state capacity as the ability of a state to implement policy. If a state wants to change the status quo by enacting and enforcing a policy choice and it cannot do so, then the state lacks capacity. A state capable of implementing policy, even or especially against societal resistance, is a high capacity state. This is the basis for our construct and, we argue, is the construct that many scholars of state capacity have in mind.

In our definition, a high capacity state is able to effectively implement a wide range of policies. Importantly, even the most powerful state will not be able to implement every type of policy or implement all policies perfectly. Our definition does not suggest that a high capacity state can implement every conceivable type of policy or that it can guarantee perfect compliance with a policy. Our definition focuses on a state's ability to implement policy relative to the ability of other states. A highly capable state will be better able to implement policy than a less capable state. Accordingly, the measure we propose in the next section can provide us information about a state's relative capacity.

Our measure

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[22] See, for example, Chaudry, et al. (1994), Geddes (1993), Grindle (1997), Nelson (1994), Haggard (1997), Tendler (1997), Turner and Hulme (1997).

We operationalize state capacity as personal and corporate income taxes divided by total government tax revenue (Personal and Corporate Income Tax Revenue/Total Tax Revenue). Our measure of state capacity ranges from 0 to 1 and allows us to identify how much of total tax revenue comes from income taxes. Our measure of income taxes includes capital gains, personal and corporate income taxes. We draw our data on tax collection from the World Bank's World Development Indicators dataset. [23]

In the denominator of our measure we use total tax revenue rather than total government revenue. The rationale for this choice is that we believe we understand the theoretical relationship between different forms of tax revenue and state capacity; however, we lack theoretical expectations about the relationship between non-tax revenue and state capacity so we exclude such sources from our analysis.[24]

We utilize the proportion of total tax revenue rather than the level of income taxation for multiple reasons. First, for our construct we need to know the use of income taxes relative to other forms of taxation, because the key to our construct is how effective

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[23] There are clearly some concerns about the quality of the data. First, there is a considerable amount of missing data which is likely to be non-random (Rosendorff and Vreeland 2006). We expect but do not investigate in this paper that data on income taxation is more likely to be missing in countries with low state capacity, the same countries where income taxes are a small proportion of total taxes. We argue that our construct of state capacity is valid across countries and time periods, therefore we do not expect these missing data to affect significantly our results or findings about the validity of our construct and measure.

[24] For example, it is not clear to us the relationship between borrowing and state capacity. If borrowing occurs at low interest rates (reflected minimal risk) and governments have a stable revenue stream then borrowing may reflect high levels of state capacity. However, if countries have to pay a high risk premium to borrow, but the premium does not discourage borrowing, then we might believe borrowing indicates low state capacity. This is merely a single example to demonstrate the ambiguities that occur when we include non-tax revenues in our analysis. See also Fauvelle Aymar 1999: 408-409 and Lieberman 2004.

the state has been at collecting income taxes relative to its effectiveness at other, less difficult to collect, methods of tax collection. We develop this idea more in the section on construct validity and in our discussion of the dominant taxation measure for state capacity: total tax revenue/GDP. Second, the level of both income and total taxes are likely to be a function of factors that affect the potential tax base (such as the overall size of the economy). Using the proportion of taxation from income taxes allows us to implicitly control for factors that could affect the level of taxation but not the ratio.

In Table 2.1 we present basic descriptive statistics about income taxation in 2005 that reveal a wide range of income tax collection among the countries for which we have data. In Table 2.2 we present a list of the countries in our dataset and the percentage of total taxes that come from the income tax in 2005. State capacity varies across countries, so variance in income tax collection across countries is a crucial to the validity of our indicator.

Table 2.1: Descriptive Statistics about Income Tax Collection in 2005

	Mean	Std. Deviation	25th percentile	50th percentile	75th percentile	90th percentile
Income Tax/Total Tax	32.3%	18.2%	17.6%	29.8%	43.8%	55.8%

We focus on taxation in our measure because it represents a policy area for which there are relatively good data about state action. Additionally, taxation, unlike health, education or some other policy can be directly and objectively measured. Also unlike other policies such as universal education or national healthcare, the decision of whether to tax citizens is not a matter of debate for modern states. Weyland succinctly describes these important qualities of taxation:

> State capacity is visible through taxation and taxation is fuel that allows states to exercise and develop capacity. Taxation is thus a core task of the state; all states should devote a high level of effort to fill their coffers even during economic crisis or social disturbance. If a state has capacity to implement policy, this should be visible in the effective collection of taxes (Weyland 1996).

While this sentiment is common in state capacity literature, the use of taxation data is less common, the focus on *income* taxation is mostly missing in the literature, and no one has demonstrated, quantitatively, that income taxation is a good construct for capacity. The existing literature that uses tax collection as a measure of state capacity typically relies on arguments for its face validity. However, face validity can only help to show that the measure is a reasonable representation of the construct. The standard arguments for face validity do not provide evidence that the measure itself actually varies as we would expect with other measures, which is necessary for us to have confidence in the validity of the construct and related measure. After we briefly review other measures of state capacity and argue why they are not sufficient for our use, we will then turn to demonstrating construct validity of our measure.

Table 2.2: Income Tax Revenue/Total Tax Revenue

Country	Income Tax/Total Tax Revenue	Country	Income Tax/Total Tax Revenue
Bosnia and Herzegovina	3.7	Morocco	37.9
Maldives	4.1	Lithuania	38.3
Moldova	4.3	Venezuela, RB	39.1
Macao, China	5.2	Colombia	39.1
Afghanistan	7.0	Greece	40.3
Russian Federation	10.7	Thailand	40.4
Cambodia	10.9	Germany	41.7
Jordan	13.0	Czech Republic	42.0
Croatia	13.2	Korea, Rep.	42.7
Belarus	13.4	Netherlands	43.8
Bolivia	14.0	Luxembourg	44.6
Sri Lanka	15.6	Philippines	45.8
Uruguay	15.7	Israel	46.0
Benin	16.0	France	46.2
Sweden	16.1	Austria	46.2
Nepal	16.2	Ireland	47.6
Paraguay	17.4	United Kingdom	50.6
Mauritius	17.6	Kazakhstan	53.4
Seychelles	19.0	Italy	54.4
Jamaica	19.3	South Africa	55.1
Slovak Republic	19.6	Norway	56.6
Togo	20.0	Iran, Islamic Rep.	58.8
Armenia	22.8	Belgium	59.0
Kuwait	22.8	New Zealand	65.6
Bulgaria	23.1	Australia	71.1
Latvia	23.3	Canada	74.6
Burkina Faso	24.0	United States	90.9
Bahrain	24.1		
Costa Rica	24.8		
Poland	25.0		
Cote d'Ivoire	26.0		
Guatemala	27.7		
Pakistan	28.3		
Slovenia	28.4		
Nicaragua	28.6		
El Salvador	31.1		
Peru	31.6		
Ukraine	31.9		
Fiji	32.5		
Hungary	33.0		
Finland	36.4		
Tunisia	36.5		
Chile	37.5		

What are the current measures of capacity?

The existing measures of state capacity can be classified in two categories according to their method of procuring data. The first group of indicators relies upon survey research, either from country experts or, more commonly, firm owners or potential investors in the country. The second group uses indirect measures (such as taxation) to measure capacity. The measure we proposed in Section 2 for state capacity builds on the measures in the second group, with important improvements to more closely approximate our definition of state capacity.

Examining survey-based measure of state capacity

Survey-based indicators are commonly used measures of state capacity in academic and policy research. In these survey designs, country experts, bureaucrats or business people are asked their opinions about the policy environment in a country. Researchers use data from selected survey responses or they develop a composite indicator of questions across several surveys. The most popular surveys are the Business Enterprise Environment Survey published by the World Bank, the International Country Risk Guide published by Political Risk Services, and Transparency International's Corruption Perceptions Index.

These surveys are inappropriate as measures for capacity. The surveys are intended to gauge the business climate of a country, which is conceptually distinct from the ability of a state to implement public policy. Surveys provide information about the business climate by collecting opinions on the likelihood of bureaucratic red tape or

bribery, or failures in basic public services such as utilities, for example. A state can be very effective in implementing policy that harms the business environment and be said to have strong state capacity. The standards of global capital, in other words, may differ from theoretical conceptions of governance and state capacity. Moreover, these surveys are not designed to measure policy implementation, but other concepts such as state capture or corruption that may be related to capacity but are not equivalent to it.

Can survey-based *indices* create good measures of state capacity?

State capacity scholars have recognized a variety of problems with existing surveys (World Bank 2000, Knack 2006). In an effort to improve upon the results from a single survey, several widely-used indexes have been developed that combine the results of capacity-related surveys. The most commonly used indexes of this type are Kaufmann, Kraay, and Mastruzzi's Governance Indicators and the regional "Barometer" series. The justifications for indexing are clear under some conditions, but the means of transformation are not grounded in theory and the combination of several biased surveys is unlikely to produce indexes with reduced "error."

The rationale for creating an index is that existing surveys of business environment, corruption, bureaucratic delay and other questions are, by themselves, incomplete portrayals of state capacity. By combining the survey questions to include all of the theoretical components of capacity, the index collapses multiple data points into a single data point. Using only one indicator for state capacity has clear advantages, but also adds problems (Knack 2006).

For an index to be useful the many data points combined must all be related to each other – that is there must be a single dimension or construct that underlies the data points. If not, it will still be possible to calculate an index, but the index itself will not be meaningfully related to the construct defined by the researcher. The result may be a number or series of numbers with an unclear relationship to the construct of interest.

Another reason for an index is a concern that any one survey has statistical errors or bias. Since the respondents or questions for any one survey may be biased by country-specific peculiarities or poor wording, the hope is that by averaging or otherwise combining the results of surveys that errors will "wash out" and a "true" measure of capacity will emerge. There is no guarantee that aggregating biased data will result in an accurate indicator of a given construct. Combining biased data to develop an unbiased measure can only occur if each component of the index is biased in such a way that aggregation eliminates the bias. This implies that the various biases cancel out each other. This is an empirical claim that is often neither claimed nor supported by researchers who develop these indexes.

The reasons we outlined in this section suggest that survey-based measures do not capture our construct for state capacity. It is also not clear that creating an index escapes the flaws in the surveys and it may introduce other types of errors into the measure.

How does our measure compare to other taxation-based capacity measures?

Another class of indicators for state capacity uses taxation data as a proxy for policy implementation. Unlike surveys, taxation-based capacity measures are indirect, and they exploit objective data to capture evidence of successful policy implementation.

Our measure follows in the tradition of these taxation-based indicators. The most common of these is total tax revenue as a percentage of GDP (Peters 1991; Cheibub 1998; Steinmo 1998). In these formulations, the ability to collect a large proportion of revenue would provide solid evidence that a state is able to extend its authority over economic activity in the state.

All forms of taxation reveal some amount of state capacity but, in modern states, advanced taxation in the form of income is a better indicator of capacity. We agree with the logic of this indicator but wish to improve upon it. Customs taxation, excise taxation and some forms of sales taxes are achievable by nearly all states. Total taxation as a percentage of GDP, then, is a coarse measure that we can improve upon, theoretically and with available data.

We also take inspiration from the measure of Relative Political Extraction (RPE) developed by Kugler and his co-authors (Organski and Kugler 1980; Arbetman and Kugler 1997). RPE is a measure of actual taxation compared to the amount of tax a country is expected to collect based on the structure of its economy. This measure of capacity is very useful for cross-country and inter-temporal analysis of tax extraction, but they is a different construct than state capacity as policy implementation. As their indicator suggests, the structure of taxation can be driven by economic development and by the monitorability of some types of economic activity, but the political choices to tax at high or low levels, or to tax in economically efficient ways will not be evident in this measure. For instance, if a state had high levels of foreign trade and derived a lot of revenue from trade taxes then the RPE measure is likely to suggest that the state is

relatively capable. Thus, similar to total tax revenue /GDP, the RPE measure does not make use of the theoretical relationship between difficult of taxation and tax collection that we believe provides a better measure of our construct of state capacity.

We also improve upon past capacity measures that utilized income taxation but did not use a ratio measure or did not demonstrate construct validity. For example, Chaudhry (1997) sees direct taxation, especially income tax collection, as vital to state-building. Levi's (1988) exploration of "quasi-voluntary compliance" focuses primarily on income taxes because the high enforcement costs of incomes taxes make them particularly strong tests of citizen cooperation with states and state force. Neither author analyzes whether this construct is an accurate measure of what they intend to study. Futhermore, Levi does not control for the size of the tax base through a ratio measure.

Our measure of state capacity is clearly in the same family as other measures based on tax collection. However, we are proposing a new measure of state capacity and we demonstrate construct validity for our proposed measure.

Income taxes are a good construct for state capacity

In this section we demonstrate construct validity for our proposed measure to show it is a valid indicator for state capacity. Showing construct validity is necessary for any good measure of construct; this has been performed for many of the most utilized measures in political science. For instance, Poole and Rosenthal (1985, 1997) explicated the validity of their NOMINATE measure of legislator ideology. Campbell, Converse, Miller and Stokes (1960) showed that the survey questions they utilized captured their

construct of political knowledge. A variety of scholars have argued that their measure of democracy captures the construct they have in mind (Przeworski et al 2000; Dahl 1971).

Shadish, Cook and Campbell (2003) argue that there are two fundamental requirements for construct validity. First, researchers must start with a well explicated construct of interest by specifying exactly what is being measured and how it represents the definition. Second, researchers must be able to determine whether there is a reasonable match between the construct and their data; they must go beyond arguing that the construct is reasonable and demonstrate that it is so. We demonstrate the construct validity of state capacity according to these guidelines.

As we elaborated in Section 1, our definition focuses on the state's ability to implement policy. We believe that our measure – income tax collection – captures a state's general ability to implement policy and in this section we present evidence that income tax revenue is a valid measure. We focus on three ways to develop construct validity in this section of the paper: face, convergent and discriminant validity.

Face Validity

To demonstrate face validity necessitates one's measure of a given construct to be "on its face" a reasonable measure. In this section we provide a lengthy discussion of income taxation to demonstrate face validity. There are three key points to our argument for face validity. First, we believe that tax collection, in general, is a good starting place for a construct of state capacity because collected taxes represent the outcome of an implemented state policy. Second, we focus on income taxes collected as a more specific measure of state capacity, because income taxation is a particularly difficult type of tax

collection and therefore provides a more refined measure of the concept. Third, we argue

that a state's ability to collect income taxes will be related to the state's ability to

implement other types of policy.

Collecting taxes requires the state to take action

Our indicator of state capacity assumes that states need and want to collect

revenue. We find this a very safe assumption, because without revenue a state cannot

accomplish any tasks it deems necessary. Furthermore, this assumption is consistent with

many theories of the state (North 1981; Tilly 1992) and with other measures of state

capacity (Levi 1988; Therkildsen 2000). The methods used to acquire fiscal resources,

however, are a political choice. These choices, we argue and demonstrate, vary

systematically with states' capabilities, making it a useful indicator of state capacity.

Taxation requires substantial effort from states. An extensive literature in

political science and economics has demonstrated the importance of taxation as a crucial

linkage between state and societal actors.[25] States must provide services to citizens in

exchange for tax resources and states must therefore interact with citizens to monitor

economic behavior and to collect and spend tax resources. Taxation of any kind,

however, requires a functional state bureaucracy. The types of taxes collected require

widely differing levels of effort by the state and its bureaucratic apparatus. As we argue

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[25] See Ziegler (2008) for an extended discussion of how the state-society network relates
to state capacity.

below, income taxation is a particularly compelling measure of state capacity because it

is lucrative for states but requires substantial capacity to implement.[26]

Taxes are difficult to collect; they depend on these necessary conditions

      We assume not only that governments seek revenue in general but that they seek

tax revenue. In particular we presume, all else equal, that states want income tax revenue

because it has the following properties that states find attractive, relative to other taxes:

low variance in quantity collected, lucrative relative to other taxes, and less distortion of

economic behavior.

      In order for a government to tax effectively, however, three necessary conditions

must be met by a state. To collect any type of taxes the following necessary, but not

sufficient, conditions must exist:

    1) Economic actors must be known to the state

    2) The state must be able to determine an amount to tax the actors

    3) The state must be able to extract income, given from conditions 1 and 2. This

      implies that economic actors voluntarily comply or the state uses force to extract

      revenue

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[26] It is not necessary for a state to implement an effective tax bureaucracy to acquire some types of revenue. For example, petro-states or states that rely heavily on foreign aid are able to amass resources without developing networks with citizens. As a consequence, states that rely on revenue from non-tax sources are likely to have weak state capacity in bureaucracies other than those dealing with oil or aid resources (Karl 1982, 1997; Gibson and Hoffman 2002). The capacity required to collect taxes, which requires broad and deep "reach" into society, has not proven necessary for these states and thus has not been cultivated.

These conditions highlight the importance of information, monitoring, and use of force for successful taxation. These conditions are agnostic as to the type of taxes collected. Even where only small sectors of a state are taxed, as in so-called petro-states, governments must have knowledge of the actors involved in oil extraction, a formula for taxing oil, and the ability to enforce compliance with imposed taxes.

Taxation requires meeting the three conditions above, and it will be relatively more or less difficult to meet these conditions depending on the type of tax implemented. One of the easiest forms of taxes to collect is trade taxes. All three conditions can be met for trade taxes with relative ease. For a government to collect a tax on international trade, little more is needed than a customs house at the borders and ports.[27] Since trade must flow through these critical "gates," government interaction with the actors that must use them provides the information required for conditions 1 and 2 to be met. Condition 3, enforcement and collection, is also less demanding than other taxes since force can be concentrated at these "gates." The ease of collecting these taxes helps to explain why countries have relied on them for considerable amounts of revenue, both historically and currently in many less-developed countries.[28]

Trade taxes may be "easy," but they impose considerable economic costs on a society and have limited revenue potential (Tanzi and Zee 2001). Sales taxes have moderate revenue potential, are moderately distortionary and are moderately difficult to collect. Importantly, income tax systems, especially once automated, are the steadiest

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[27] Of course, the existence and maintenance of borders and ports comes before this. Part of the motivation for governments to assist, develop or control these infrastructures comes from their desire to tax them.

and most lucrative form of tax revenue. All else equal, most governments would prefer to tax from the widest base possible and to do so in the least distortionary manner.

Of course, all else is not equal, and political concerns weigh heavily into decisions about taxation. Nevertheless, if they were possible, most governments would want to garner more from "efficient" taxes. The taxes with the widest, most stable bases are those on income and property. These are effectively applied in developed nations; once these governments were able to tax activities that are more difficult to monitor, such as services and income, trade taxes became less important for government revenue. This suggests a preference for more difficult, information intensive taxes should they be feasible to enforce. These taxes, however, require extraordinary amounts of information to meet the three necessary conditions for taxation.

Taxation involves capacity and willingness

Some states that are capable of taxing income may reject income levies as their preferred method of taxation. A capable state may not choose to collect income taxes if such taxes are politically unattractive or other revenue sources provide sufficient revenue. Capable states unwilling to collect income taxes may use other high capacity taxes such as those on property or a complex VAT tax as a substitute. It is possible, therefore, that our measure of state capacity – income tax revenue relative to total taxation – could underestimate the capacity of some states. This is not a devastating critique however, because it will be uncommon that capable states do not collect income taxes. We find this consistent with the fact that nearly every country has passed an income tax, but tax collection varies widely as is evident in Tables 1 and 2.

Our income tax collection measure is unlikely to overestimate state capacity. Weak states cannot collect substantial amounts of individual or corporate income tax, because these states cannot meet the necessary conditions outlined in the previous section. In other words, income taxes cannot be collected in the absence of reasonably high state capacity. For instance, in a Petrostate, the state may meet the necessary conditions to tax the oil industry, which is relatively concentrated and easy to monitor. At the same time, the rest of society may be excluded from the state's reach. Our proposed measure of state capacity will show that these are weak states, because the scope of taxation does not include significant income tax collection. It is a strength of our measure that it is consistent with other studies of state capacity that suggest that petrostates and other states that rely on a single resource (such as diamonds) are often weak states (Karl 1982, 1997; Crisp 2000).

How can we delineate capability from will?

A common problem in definitions of capacity is that what a state *does not* do is substantively different than what a state *cannot* do. That is, states may be fully capable of income taxation but they may nonetheless opt not to collect this type of revenue. In this case, the lack of will to collect income taxes and the lack of capacity to collect them will observationally equivalent. We argue in this section that states with the capacity to tax income are likely to actually do so, and therefore reveal their level of capacity.

Table 2.3 Interaction Between Political Will and State Capacity

	State Capacity	
Political Will	*Yes*	*No*
Yes	High income tax Collection	Low or zero income tax collection
No	Low income tax collection	Low or zero income tax collection

The interaction between political will and capacity and the outcome for income taxation is shown in table 2.3 above. In this section, we are primarily interested in discussing the states in the lower left-hand cell – lacking will but possessing state capacity. In theory, high capacity for taxation and lack of political will to tax is possible. In practice, we believe that the capacity to tax will not go unused since maintaining capacity consumes scarce resources for a state; this is particularly true for administratively demanding income taxes.

Related to this, capacity in the income tax administration will not be a priority when those with power are highly resistant to that type of tax. States unable to overcome societal resistance to taxation are likely to be states that lack capacity for policy implementation in general. With scarce resources for state activity, government leaders will rationally devote their energy to the types of taxation or means of funding that will be successful. For many developing countries, trade taxes or possibly VAT or sales taxes fill this role because they are more feasible, politically, than income taxes. They also require less administrative capacity and compliance than income taxes. We argue, first, that political will to implement policies is a driver of capacity that is clearly missing in these cases. While theoretically possible, the likelihood that states develop the

administrative capacity, especially collection of information and the technical know-how required for effective taxation but fail to use it is low for this vital state task.

We recognize that some capable states will have the ability to implement policy, but they may be blocked from doing so by political actors. As a counter-example, it is reasonable to assume that the United States national bureaucracy had the means to institute a nationalized healthcare system as proposed by the Clinton administration in 1993-94. Congress and other political leaders, however, were able to defeat these propositions. The United States, in this case, had the capacity but not the will to institute this reform. We believe that the policy arena of taxation, especially a policy as economically attractive as income taxation, will be a least-likely case for the presence of capacity and the absence of will. The decision to tax is not optional in the way of many policies; a state must have resources to govern and these must be almost entirely collected through tax resources. Put simply, if states can tax, they will.

To summarize, we argued earlier that income taxes are a difficult type of tax to collect because they require the state to have significant, detailed information about citizens' economic activity. In the absence of a threat of coercion, many citizens will not voluntarily give their income to the state; therefore, the state must possess the ability to threaten the use of force against citizens who do not voluntarily comply with the tax requirements. To possess both the relevant information and the ability to utilize force requires a state with considerable capacity to implement policy. We believe that income tax collection is a good measure of overall state capacity because it requires the ability to learn about citizens and impose force, which are two characteristics that will improve the

ability to implement policy. Furthermore, states with the means to tax income will be likely to do so and therefore we will be able to observe capacity.

Convergent validity

In this section we focus on establishing convergent validity, which means that "measures that should be related are in reality related" (Trochim 2000). Convergent validity requires demonstrating that a proposed measure varies as predicted with other measures that are theoretically related to the construct of interest. In this section we show that our proposed measure of state capacity correlates positively with a variety of other measures related to a state's ability to implement policy. If these empirical measures vary with income taxation as we predict, then we can be more confident that our measure captures the construct of state capacity.

In this section we examine the cross-sectional correlation between our measure of state capacity and other variables that we believe are theoretically correlated with state capacity. The goal of the cross-sectional analysis is to determine whether or not our construct is valid across a variety of states using the World Development Indicators

Infrastructure indicators: We examine the correlation between our measure of state capacity and a variety of different infrastructure measures. We expect infrastructure to correlate with state capacity because state actors are usually central to the financing and construction of large infrastructure projects. In a cross sectional analysis we examine the following infrastructure measures: broadband subscriptions, telephone subscribers, internet users, and kilometers of rail lines. We expect each measure to vary positively

with state capacity because building rail lines, telephone lines or broadband capacity

requires a government that can implement public policy.

   Size of government: One alternative measure of state capacity is total tax revenue

divided by the size of the economy (proxied by Gross Domestic Product). This is a rough

measure, but we expect it will correlate positively with income tax collection. If a state

has the ability to tax income it is likely that the state is also large (relative to non-income-

taxing states) and therefore we expect a positive correlation. We also discuss why our

measure better captures policy implementation than the most common measure, total

taxes / GDP, in later sections.

Table 2.4: Correlation between Income Tax/Total Tax and Other State Capacity Indicators in 2005 for a Sample of Countries

Indicator	Broadband subscribers per 1000	Telephone subscriber 1000	Internet Users per 1000	KMs of Rail Lines	Telephone Mainlines	Tax revenue percent of GDP
Average Correlation (p-value)	0.54 (0.001)	0.45 (0.001)	0.39 (0.001)	0.46 (0.001)	0.34 (0.01)	0.23 (0.05)

   We present the results of our cross-sectional correlations in Tables 2.4. The

measures we examined correlate as predicted with our indicator of state capacity. The

consistent correlations between income taxation and other elements of state capacity

provide evidence that income tax collection is a valid measure of state capacity. The

strength of our construct and measure is further demonstrated in the next section where

we show that income taxes are not correlated with a variety of measures that we predict

should not be related to state capacity.

Discriminant Validity

Discriminant validity is an important, if often overlooked, component of construct validity. The purpose of discriminant validity is to show that our chosen measure of state capacity does not correlate with measures of other concepts that are not theoretically related to state capacity. Although in social science we expend most of our research effort to determine the presence of a relationship (either negative or positive) between two variables, our task in this section is to show the lack of a relationship between two variables. Discriminant validity serves a critical purpose in construct validity; if our measure of state capacity varies even with measures that we do not believe are related to state capacity, then it suggests that we may not be capturing state capacity but rather some underlying factor that determines state capacity as well other attributes of a state. Given the wide variety of hypothesized correlates of state capacity, discriminant validity is a particularly important aspect of construct validity because it places a high theoretical burden on us to identify concepts and measures unrelated to state capacity.

We now present a variety of different measures that we predict should not be correlated with state capacity. We continue to draw data from the World Bank's World Development Indicators dataset.

Geographic state size: The geographic area of a country should not be related to state capacity. Our definition and measure examines the ability of states to implement policy, and we do not expect this to vary consistently with the size of a country.

Land Usage: We utilize multiple indicators of land usage including agricultural land, arable land, forested land and land used for cereals. These factors are largely

exogenous to state action and thus we do not expect them to correlate with income tax collection.

Military personnel and spending: We do not expect more capable states to employ a larger percentage of the labor force in the military or to spend a greater percentage of their GDP on the military. Military indicators may be related to capacity or power in international relations, but we do not expect states that have stronger *internal* policy capacity to expend greater resources on the military. The lack of a correlation between our construct of state capacity and military resources is particularly important because it demonstrates that our measure differentiates between other definitions of state capacity.

GDP per energy unit: Economic activity per unit of energy is a measure of how efficient an economy is relative to its energy consumption. A country's energy efficiency will largely be a function of the private sector economy and therefore should not correlate with state capacity.

Table 2.5: Discriminant Validity for Income Tax Measure

Indicator	Log Total Area	Agriculture Land	Arable Land	Forest Area
Average Correlation (p-value)	0.44 (0.01)	0.21 (0.17)	-0.14 (0.38)	-0.09 (0.43)
	Land used for cereal production	GDP per energy unit	Military spending as percent of GDP	
Average Correlation (p-value)	-0.14 (0.38)	0.21 (0.17)	0.05 (0.70)	

In Table 2.5 we present the bivariate correlation coefficient and the level of significance of the correlation for our measures of discriminant validity. It is clear that only the coefficient for total land area is significant. None of the other correlations come close to approaching statistical significance. We investigate the correlation between land area and income taxes in Figure 2.1. It is clear from visual inspection of the relationship between land area and income tax collection that the slightly positive relationship is being driven by a few outlier countries. On one hand the U.S., Canada, and Australia are large countries that rely on income taxes. On the other hand, Maldives, Seychelles, and Chinese Macao are small countries that do not rely on income taxes for government revenue. A simple visual inspection reveals that if we exclude the few countries in the upper right and the lower left of the figure (in the red boxes) there is no relationship between land area and income taxation. In fact the correlation between the two the above six countries removed from the data is 0.15 with a p-value of 0.22.
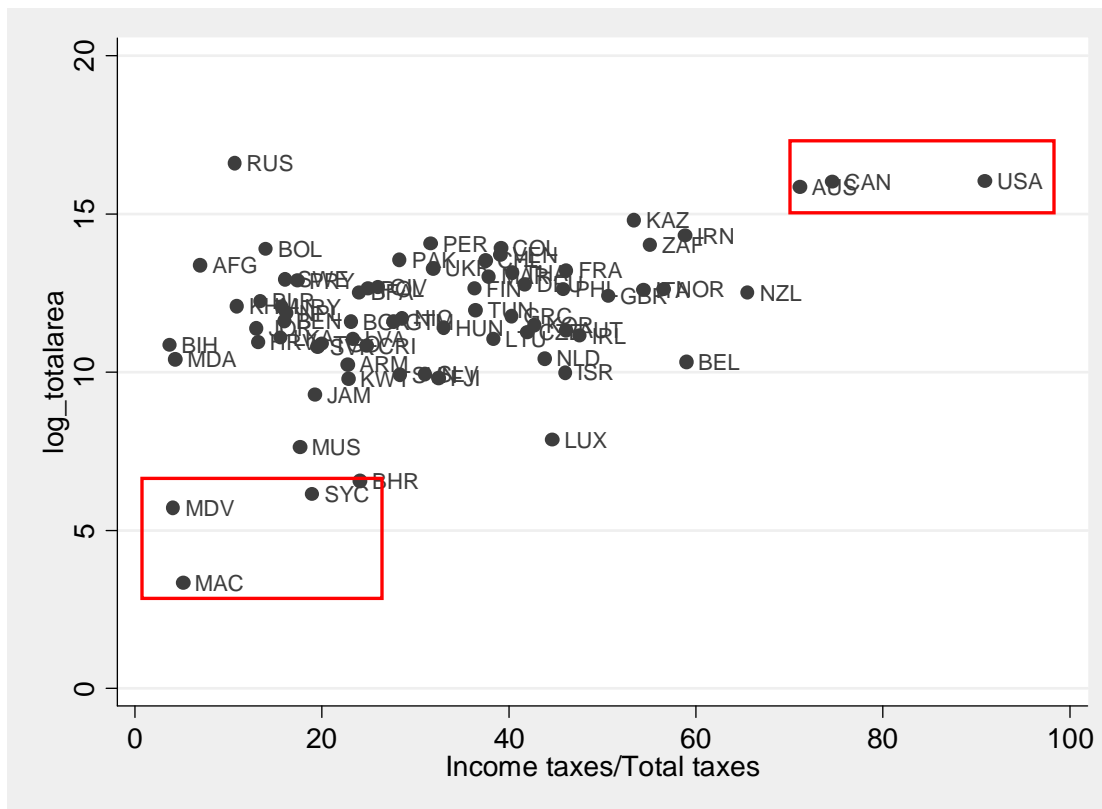
Figure 2.1: Scatter plot of income tax/total tax and log of total land area

The results we presented in this section show that income taxation as a proportion of total government taxation does not correlate with measures that are unrelated to state capacity. This is an important, if somewhat unusual, way to demonstrate the value of a proposed measure. If we had found that income taxation had a significant correlation with indicators for which we did not expect a correlation then it would appear that some other, unmeasured variable determined both income taxation and land area and use, military capacity and economic energy efficiency. The demonstration of discriminant validity gives us considerable confidence that our measure captures state capacity rather than the presence of a latent variable that is related to state capacity and a variety of theoretically unrelated variables.

Comparing Tax-Based Measures of Capacity

We have shown that income taxes divided by total taxes is a valid measure of

state capacity. The last step we take in this paper is to compare our proposed measure of

capacity to another widely used measure of state capacity – total government tax revenue

divided by gross domestic product. In this section we examine some of the same

correlations we presented for our measure as well as examine the differences and

similarities in how the two measures rank the level of state capacity for the countries in

our sample.

Table 2.6: Convergent validity for Total Tax/GDP

Broadband subscribers per 1000	Telephone Subscriber	Internet Users per 1000	KMs of Rail Lines	Good Sanitation	Clean Water	Telephone Mainlines
0.30 (0.02)	0.48 (0.001)	0.45 (0.001)	-0.12 (0.35)	0.14 (0.22)	0.14 (0.18)	-0.18 (0.09)

In Table 2.6 we present a variety of convergent correlations between total tax

revenue/GDP and the infrastructure measures we utilized earlier. It is clear from the table

that quite often tax revenue/GDP shows a significant correlation with other indicators of

infrastructure development. This suggests that tax revenue/GDP is a viable indicator of

state capacity. However, the table also demonstrates that for a number of variables where

we expect a correlation with state capacity, it does not exist. The lack of expected

correlations leads us to believe that this measure fails to capture some important

components of state capacity. In fact, it is notable that the variables for which total

tax/GDP fails to correlate seem to be those where a particularly large amount of state

capacity is needed—provision of clean water and effective sanitation. We also examine

discriminant validity for the same indicators discussed earlier. In our analysis total

tax/GDP did not correlate with any of our chosen indicators, which suggest that the total

tax measure is also not driven by some underlying factor that determines taxation as well

as a host of other features of a country. The results of the correlations suggest that income

tax is a superior measure because it correlates with more aspects of state capacity than

does total taxes/GDP.

Table 2.7: High Capacity on Income Tax Measure and Low Capacity on Total Tax Measure

Italy
Costa Rica
Austria
Kazakhstan
Burkina Faso
Venezuela
Peru
Colombia
Czech Republic
El Salvador
Republic of Korea
Kuwait
Bahrain
Guatemala
Pakistan
Philippines
Germany
Canada
United States
Iran, Islamic Rep.

The correlation between our measure (total income tax/total tax) and total tax

revenue/GDP is 0.23, which suggests a reasonable amount of overlap between the two

different measures. To compare the measures directly, we rank order the states from most

to least capable for each of the two measures. We then compare the rank orderings for

each measure, and we select for further analysis the states where there is considerable

difference (more than 10 places different on the two measures) in the ranking between the

two measures – either high income tax capacity and low total tax capacity or high total

tax capacity and low income tax capacity. The results of this comparison generate the

countries listed in Tables 2.7 and 2.8. Table 2.7 lists the countries that appear to have

high capacity on the income tax measure, but low capacity on the total tax measure.

Table 2.8 lists countries ranked as having low capacity on the total tax measure, but high

capacity on the income tax measure. It is tempting to just look at the list and see which

ranking better fits with our intuition about a given state's capacity, but doing so does little

to prove the superior validity of any measure. Instead, for each of the two different

groups of countries displayed in Tables 2.7 and 2.8 we look for correlations with our

other variables related to state capacity that we used to analyze the full sample above.

Table 2.8: Low Capacity on Income Tax and High Capacity on Total Tax

Jordan
Seychelles
Bosnia and Herzegovina
Macao, China
Croatia
Jamaica
Moldova
Belarus
Bulgaria
Sweden
Maldives
Russian Federation
Uruguay
Fiji
Mauritius
Bolivia
Morocco
Slovenia
Finland
Israel

We turn first to the countries listed in Table 2.7, those with high capacity in the income tax measure and low capacity in total tax/GDP. The correct measure should be positively correlated with our various indicators of state capacity, because across this group of countries those with higher capacity (on either measure) should have higher levels on our various indicators. If either state capacity measure has no correlation or a negative correlation, then it suggests that the measure is inappropriate among this group of countries. If the total tax measure is more appropriate we expect positive correlations with the other measures. Among this group of countries we find positive, statistically significant correlations between income tax and broadband users per 1000 people, telephone users per 1000 people, telephone mainlines, percent of the population with clean water and percent of the population with good sanitation. None of our infrastructure indicators correlate significantly with the total tax/GDP measure of state capacity. This

set of results clearly suggests that the income tax measure is more appropriate among these cases.

We perform the same analysis for the cases in Table 2.8 in which the income tax measure suggests low capacity and the total tax measure suggests high capacity, and we again expect that the superior measure should demonstrate a positive correlation with our other indicators of state capacity. Both of the competing measures fail to correlate at a significant level with the different infrastructure indicators. This result suggests that neither measure is superior among these cases.

In this section we have investigated whether our proposed measure of state capacity is superior to the standard, tax-based measure of state capacity. We found that for many of the countries the two different measures are similar in their ranking of relative state capacity, but there are some countries where the two measures rank countries quite differently. Among those countries we found that our proposed income tax measure is superior because it correlates as expected with infrastructure indicators of state capacity. These results provide further evidence that our income tax-based measure is a valid indicator of state capacity.

Conclusion

In this paper we presented a definition of state capacity that focuses on the ability of a state to implement public policy. This is at the core of most prior characterizations of state capacity, but this definition has not been plainly explicated and linked to a valid measure. Unlike the majority of works on capacity, we do not focus on where capacity comes from, but rather propose a new measure for state capacity and then demonstrate

the validity of the measure. We demonstrated in this paper that we can measure our concept of state capacity by examining the proportion of total tax revenue from income tax collection. Income tax collection is a good measure for our construct of state capacity because income taxes represent the outcome of policy implementation. We demonstrate construct validity for our proposed measure through face validity, convergent validity and discriminant validity. Importantly, we show that our measure is a better representation, theoretically and statistically, than the dominant measure of capacity focused upon total tax collection. The variety of ways that we demonstrate construct validity gives us confidence that our measure indeed is appropriate for our construct of state capacity.

This chapter was co-authored with Melissa Ziegler, and I thank her for allowing me to use this chapter in my dissertation.

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Chapter 3: The Spread of the Tax Revolt: The Diffusion of State Tax and Expenditure Limits (co-authored with Ellen Moule)

In the late 1970s, Howard Jarvis set out to limit taxation in the state of California. He started a grassroots organization, the United Organization of Taxpayers, and built support for a popular initiative that would permanently reduce property taxes. While limits on local tax collection had been in place for many years, no state had yet to constitutionally limit state tax increases. In the June election of 1978, he gave the voters Proposition 13 and it passed by a margin of 65%. Property taxes plummeted and that very next November no less that 15 other states put anti-tax initiatives on their ballots. This movement was dubbed the tax-revolt, and, as is commonly remarked, it swept the nation.

In this paper we study how tax and expenditure limits (TELs) diffused through the U.S. states. The study of policy diffusion analyzes the spread of policy innovations across government jurisdictions. The policy innovation of TELs is an excellent case for the study of policy diffusion. They have been spreading across the US states for over 30 years and have reached a diverse selection of states. In sum, 38 statewide TELs have been adopted via the initiative and referendum process since 1970. Further, most TELs are passed through the initiative process, relatively unchartered territory for the policy diffusion literature.

We make two central contributions to the policy diffusion literature. First, we argue that to understand policy diffusion it is appropriate to focus on when policies are proposed, rather than simply when policies are passed as is done in the extant literature.

Policy proposal is evidence that diffusion occurred because it constitutes serious consideration and widespread knowledge of the policy. The standard mechanisms of diffusion do not require policy adoption as evidence for diffusion, and focusing only on instances of successful policy adoption as evidence of diffusion is theoretically inappropriate. Further, as we will show, estimation that relies only on cases where policy was adopted leads to inaccurate conclusions about how diffusion occurs.

Second, we argue that failure to account for the diffusion process also leads to mistaken inferences about the determinants of policy adoption. We show that, conditional on a policy being put on the ballot, the determinants of policy adoption differ substantially from a model that ignores policy proposals. We use a two stage event history model to analyze both the policy diffusion and policy adoption processes. Our findings suggest that many of the variables previously associated with TEL adoption are actually better determinants TEL proposal, not passage. Our estimation of policy passage suggests that the determinants of passage may lie more in election-specific differences than statewide trends in tax burden or population characteristics

This paper proceeds as follows. First, we review the literatures on both the determinants of TELs and the processes of policy diffusion. In Section 2 we discuss the mechanisms of diffusion, highlighting the importance of the initiative and referendum processes. We argue that policy proposals are evidence for diffusion in Section 3. In Section 4 we discuss the standard mechanisms of diffusion and propose looking at similarity in per capita income levels as another possible pathway for diffusion. We utilize in Section 5 a two stage event history model to study diffusion, which we argue

better models the distinct processes of diffusion and adoption, as well as their interdependence. Finally, we present the empirical results and conclude.

Previous Research on TEL adoption

Tax and expenditure limits are in place in over half of the American states. Since 1970, 79 TELs have been proposed using the initiative and referendum process alone. As such, it is not surprising that there is a substantial literature on both TEL adoption specifically, and the tax revolt in general. Despite this large literature, there has been little quantitative work that explains how the revolt spread from one state to another. The process of diffusion is an important political phenomenon, which provides insight to both the timing and pathways of the spread of policy.

The earliest literature exploring the passage of TELs looked at the types and characteristics of voters who supported the limits. Studies of Michigan, Wisconsin, and Massachusetts (Mariotti 1978, Courant et. Al 1981; Stein et. Al 1983; Ladd and Wilson 1983) took theories of rational economic voters and applied them to support for TELs. Using aggregate survey data, these studies all found that voters who supported TELs were property owners, high-income individuals, and those who experience a relatively large tax increases over a short period.

Later studies brought the unit of analysis up to the state level to test similar hypotheses. For example, Lowery and Sigelman (1983) hypothesize that passage of TELs is associated with heavier tax burdens, perceptions of public waste or inefficiency, political dissatisfaction, conservative swings in popular political ideology, low levels of political participation, high political competition, and states whose neighbors have

already passed TELs. They test these hypotheses cross-sectionally. In their results, they concede that while the variables representing political dissatisfaction do better than the rest, none of their twenty-five total variables either singularly nor jointly performs well. In addition to these limited findings, by very nature of their cross-sectional analysis, they can and do not comment on the timing of TEL adoption. Most importantly, both this study and earlier work at the individual unit of analysis is silent on how and why TELs were brought to the voters in the first place.

Most recently, Skidmore and Alm (1999) use panel data to explain the rise of the tax revolt. Skidmore and Alm, much like the methods employed in this analysis, explain both instances of TELs getting on the ballot as well as instances of TEL passage through a set of simultaneous equations. We believe this to be a significant contribution to the literature, and one that more accurately estimates the determinants of TEL passage. One weakness in their model, however, is that they use the same variables to predict passage and placement on the ballot and ignore external determinants of TELs. While they have several robust results when it comes to passage – states with increasing levels of income and states that experience increases in both property taxation and local revenues relative to state revenues both increase the probability of passing a TEL – they have mostly null results when it comes to predicting placement on the ballot. Instead, the only consistently robust result is that placement on the ballot is less likely once a state has already had a TEL.

One reason for these null findings is that the authors do not test variables relating to the diffusion of policy innovations. Their finding tells us nothing about exposure to

TELs and do very little to explain the timing of enactment. While the authors do not purport to be explaining the diffusion process, this is an obvious next step to understanding the dynamics of the tax revolt. We build upon prior analysis by adding variables that explain the diffusion of TELs across states.

In sum, while the previous literature on the determinants on TELs successfully identifies internal determinants that make a state susceptible to passing a TEL, it largely ignores external determinants that explain how the policy innovation diffused to the state in the first place. In other words, while we have a good idea of what makes a state likely to pass a TEL, no one has yet investigated what causes their spread. Bridging the literatures on the tax revolt and policy diffusion will provide greater insight on the spread of the tax revolt.

Policy Proposals as Evidence for Diffusion

The literature on diffusion begins with Rogers (1962) who defines diffusion as "the process by which an innovation is communicated through certain channels over time among the members of a social system" (Rogers 1962, p. 5). Rogers identifies five stages of the total diffusion process: awareness, interest, evaluation, trial, and adoption. The puzzle central to the subsequent literature, therefore, is explaining this process, particularly in terms of the effect of previous adopters on a state's decision. Despite Rogers' five stages, the diffusion literature has only analyzed the final stage – policy adoption – to draw conclusions about the diffusion process as a whole. [29] This is

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[29] The key exception being Mintrom 1997 and Mintrom and Veragi 1998. These papers test for the effects of diffusion on policy consideration as well as policy adoption. However, they do these tests independently instead of a two-stage model.

problematic because many these conclusions are based only on a small subset of cases of diffused policy, those that actually passed.

We argue that a better way to understand the diffusion process is to focus on when a policy, in our case a TEL, is proposed. The proposal of a TEL provides additional insight into the policy diffusion process, because it allows us to examine the full set of cases where a TEL is considered rather than only those instances where a TEL is passed. A more complete picture of the policy process can help shed light on why and how information about a policy moves from states to state.

The heavy focus on policy adoption in the policy diffusion literature has censored observations of state-to-state learning of policy. This is a particularly significant, because the theoretical foundation of the diffusion literature is based on a process of learning. Specifically, the diffusion literature has identified three unique determinants of the spread of policy from state to state, all based upon a learning process: imitation, emulation, and competition (Berry and Berry 1996). Imitation and emulation occur when states purposefully mirror their neighbor's policies, perhaps with improvements. Competition follows the same mechanisms, but with a specific intent to preempt residents or businesses from relocating (or locating) to the competitor state. The basis of each of these theories is that diffusion is grounded in learning from the policy innovation of a neighboring state.[30]

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[30] Other connections between learning and policy diffusion exist. Rogers (1962) noted that numerous studies in the fields of sociology and education had demonstrated that the cumulative frequency of innovation adoption is S-shaped, identical to that of the classical psychology learning curve (Lindblom 1965; Simon 1947). Gray's (1973) comparison of

Despite the significant theoretical emphasis on the learning process in the previous literature on diffusion, the variable of interest in the vast majority of these studies is policy adoption, which we argue is not a valid measure of whether or not learning occurred. As noted by Gray (1973), "The observable data are the adoptions by states of particular laws. When states first learned of the idea is unknown". (p. 1174). We believe this conclusion is mistaken because it ignores observable data available at the proposal stage. Policy innovations that are considered, but fail to pass, are evidence that policy makers are exposed to the policy innovation. As such, serious consideration of a proposal, in and of itself, is evidence that diffusion occurred.

The presence of a proposal on an election ballot represents the fact that some decision-makers were aware of the innovation and interested in applying it in their state. Failing to obtain the requisite number of votes for passage does not discount the fact that the policy innovation diffused in terms of learning.[31] Our central argument is that ignoring observations of failed proposals precludes capturing the full process of learning and communication between states. As we will show in our analysis of TELs, this can lead to null findings of a diffusion process.

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adoption rates for individual policies and program areas (education, civil rights, and welfare) confirms this pattern.

[31] This assertion begs the question of where to draw the line of when policy learning can be deemed to have occurred. It could be said that any contingent of people could propose a policy through the initiative process, though it might not make it to the ballot. We argue that learning has inherently taken place when a policy is put up for a final passage vote. Since we are looking specifically at policies passed through the initiative or referendum process, a final passage vote in this context is any ballot measure put to a vote in a general, special, or primary election. We thus define a state as exposed to the policy innovation if such a vote occurs.

In addition, ignoring the proposal stage also leads to estimation errors on the determinants of policy adoption. We believe that ignoring the policy proposal stage overestimates the relationship between policy adoption and variables related to diffusion (i.e. the effect of policy adoption by a state's geographic, ideological, or economic neighbors). It makes intuitive sense to assume that two states that consider similar proposals and both pass them are more alike than two states that consider identical policy proposals with opposing outcomes; however, if latent variables that are related to policy adoption are correlated with variables that dictate the diffusion of policy innovations, we may make serious errors in estimating the causes of policy adoption. This problem was previous noted in the experimental work on diffusion conducted by Tyran and Sausgruber (2003). The authors note that "If, for example, neighboring states are similar in some unobserved dimension, and if this dimension affects the decision to adopt a policy, regional patterns of policy adoption may be misinterpreted as regional diffusion." (Tyran and Sausgruber 2003). We strongly agree with the assertion and argue for a disentangling of the policy adoption and policy diffusion processes.

Mechanisms of Diffusion

To explain the spread of the tax revolt, it is necessary to discuss the mechanisms for diffusion. By "mechanism" we are referring to the process by which policy entrepreneurs learn from previous innovators. This encompasses the question of "Who did the policy entrepreneurs learn from, and why?" In this paper, we will test three possible mechanisms for diffusion: geography, national trends and economic similarity.

The workhorse variable in the state policy diffusion literature is a measure of the

number of one's geographical neighbors that have previously passed the policy innovation in question. Geographic channels of learning are the most common assertion in previous literature on policy diffusion (Gray 1994; Lutz 1986; Stream 1999; Mooney and Lee 1995; Berry and Berry 1990; Walker 1969). Mooney (2001) characterizes the reliance on geography as "readily accepted" for two reasons. First, geographic proximity serves a useful heuristic for policymakers and citizens to use to decide how a policy might work in their own state. As noted by Mooney, " policymakers and citizens look to other states in a satisficing search for solutions to problems, and the states to which they look first are their neighbors, due to familiarity, ease of communication, cross-mixing of media and population, and common values." Second, geographic proximity is supposed to be a more salient channel of learning due to increased competition between neighboring states. In order repel losing residents and businesses to neighboring states a as a result of uncompetitive public policies (Dye 1990; Tiebout 1956). Even more generally, politicians in nearby states may have direct communication with one another (Crain 1966; Foster 1978) and the spread of information is aided by overlapping media markets (Karch 2007)

A second mechanism for diffusion is the national interaction model (Berry and Berry 1999). This model sits in opposition to geographic diffusion mechanisms because it posits that all states have an equal probability to adopt an innovation, conditional only on the number of prior adopters. As noted by Berry and Berry, this model suggests that "officials from other states interact freely and mix thoroughly with officials from states that have not adopted it ..." (Berry and Berry 1999, p.227). Like the geographic diffusion

model, the national interaction model's foundation is in the learning process. Specifically, Berry and Berry (1999) focus on the importance of learning through national associations such as the National Governor's Association or the National Conference of State Legislatures.

In this paper we propose a third mechanism for diffusion. Before we explain our proposed mechanism we discuss the characteristics that a mechanism for diffusion must possess, because this underlies our choice of diffusion mechanism.

As we pointed out earlier the previous literature on state policy diffusion focuses on the role of information in the spread of public policies. The presence of national newspapers and television in the 1970s and the internet and email in the 1990s means that information about other states has been available to policy makers during this entire period we are studying. Therefore, if communication is the core mechanism for diffusion it cannot be communication about policy adoption, per se. Instead, it seems more likely that communication affects policy proposals and adoption is by providing information about whether voters are likely to support a TEL and the consequences of going through the cost of proposing a TEL via the initiative process. To put it another way, the idea of limiting taxes and expenditures is known by policy entrepreneurs either prior to or very quickly following the passage of the first one through media and networks of policy makers. Although entrepreneurs may be aware of the idea of a TEL, when they observe TELs pass in other states they learn information about how political and economic circumstances are related to TELs. Even with all of the information available to policy entrepreneurs they are not clairvoyant – only about ½ of the proposed TELs in our dataset

actually pass. Therefore, it seems reasonable that they will be attuned to information that

will help them learn about the costs and benefits of proposing a TEL on the ballot. The

need for information about support for a TEL implies that diffusion should occur through

channels that tell policy makers something about how similar their state is to another

state. This implies that a mechanism of diffusion must capture some aspect of why a state

would choose to copy another state's policy choice. To put it another way, diffusion

occurs because policy actors in some states choose to follow the policy choices of another

state – not simply because there is information available about a given policy innovation.

This means that to study the process of diffusion we want a construct and related measure

that captures how information about policy in state A affects policy in state B. To model

the relationship between policies in different states we look at similarity in per capita

income.[32] Income levels are likely to be related to spread of tax and expenditures policy

for multiple reasons. First, per-capita income is commonly associated with political

preferences and activity (Rosenstone and Hansen 1993). Second, there is a long political

economy literature that relates income levels to preferences in size of government (i.e.

Wagner's Law). Third, income levels are significantly related to policy diffusion in other

research (Berry and Berry 1990). These reasons all suggest that income levels are a

likely conduit for the spread of policy. It is important to note that because income is

related to many political and economic phenomena, even if we find that similarity in

income levels are significantly related to the diffusion of TELs we cannot infer that

income is the actual causal mechanism through which diffusion occurs. Similarly,

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[32] We explain our actual measure in the section "Modeling TEL diffusion and adoption."

research that finds diffusion to be related to the policy choices of neighboring states cannot actually determine the path through which policy spreads. Our measure of per capita income similarity suggests where state actors may look for information about policy proposals.

Importance of the Initiative Process

In this paper we focus on policies that are proposed and passed via the initiative and referendum process. In addition to being a major avenue for policymaking in some states, this process also provides information about when a policy is proposed to voters and when the proposal is passed by voters. These are both important reasons to focus on policy diffusion and adoption that occurs via the initiative and referendum process. We conduct our empirical analysis both within only the states with the initiative as well as with all states in the sample. The logic of the split is rather straightforward. The process that generates policy proposals in an initiative state is likely to be fundamentally different than the process in a non-initiative state. In an initiative state there are two ways for policy proposals to appear on voters' ballot – initiative and legislative referendum. The theoretical work on the initiative process suggests that the outside option of the initiative may affect legislative politics (Gerber 1999; Matsusaka and McCarty 2001), which gives us reason to believe the policy process may not be similar in initiative and non-initiative states.

Research Design and Methodology

As we argue in this paper, the policy making process involves at least two stages – policy diffusion and then policy adoption. In this section we explain the research design

and methodological approach we utilize. Our analysis considers states with the initiative

process and states without the initiative process, because the TELs in our dataset may

have passed through either the initiative or referendum.

The standard approach in the diffusion literature is to estimate policy diffusion

and adoption together using a duration analysis of the time until policy is adopted (see

Berry and Berry 2007 for a review). These models code a state-year as 0 if the state does

not adopt a given policy that year and 1 if the state does. This design ignores the fact that

observations are of three distinct types. First, there are states where a TEL is proposed

and passed in a given year. Second, there are states where a TEL is proposed but does not

pass in a given year. Third, there are states were a TEL is neither proposed nor passed in

a given year. The methodology we utilize allows us to study the differences between each

different type of observation and recognizes that policy proposal is evidence for

diffusion. Previous research on diffusion has treated the second and third type of states

the same although they are likely to differ in systematic ways.

In the equations below the selection equation determines whether or not we see a

TEL proposed on a ballot. The critical point is that we only observe a TEL proposed, z_i,

when some function exceeds a critical threshold T. The selection equation estimates the

probability of observing a TEL proposed. The outcome variable y_i is only observed for

the subset of cases where a TEL is proposed, that is where $z_i > T$. Each equation is

assumed to have normally distributed, independent errors with mean zero and constant

variance. However, the rationale for a selection model is that we believe the errors in the

selection and outcome equation, u_i and e_i, are correlated then simply estimating a model

for passage without taking account of proposal will lead to biased estimates.

$$
\left.\begin{aligned}
z_i^* &= Wi'\alpha + e_i \\
z_i &= 0 \text{ if } z_i^* \leq T \\
z_i &= 1 \text{ if } z_i^* > T
\end{aligned}\right\} \quad \textit{Selection Equation}
$$

$$
\left.\begin{aligned}
y_i^* &= x_i'\beta + u_i \\
y_i &= y_i^* \text{ if } z_i = 1 \\
y_i &\text{ not observed if } z_i \leq T
\end{aligned}\right\} \quad \text{Outcome Equation}
$$

Our data consist of two different binary variables: TEL proposal and TEL adoption. The

Heckman selection model consists of two probit equations that are linked via the

correlation in their errors. To estimate the two models simultaneously we utilize Stata's

Heckman Probit command, which utilizes maximum likelihood to estimate the selection

and outcome equation. The alternative is to follow Heckman's (1979) two stage

approach, however Freedman and Sekhon (2008) advice against this approach in favor of

a maximum likelihood approach. Accordingly, we utilize the maximum likelihood

approach in this paper.

To account for the time until a TEL is proposed we take advantage of the fact that

binary time series cross-section data is identical to duration data once we account for

temporal dependence in the data (Anderson and Hill 1982; Beck, Katz and Tucker 1998).

Beck et al. (1998) point out that "Annual BTSCS data are equivalent to grouped duration

data with an observation interval of one year." The identical nature of BTSCS data and

grouped duration data provide the solution for our empirical estimation strategy. We

utilize this approach because it will allow us to condition adoption on the determinants of

proposal. Following Beck, Katz and Tucker (1998), we augment a standard probit model with a series of cubic splines.[33] The cubic splines used for predicting proposals were created from a variable that, for each state, counted upwards from 1970, and reset when a tax limit was proposed.[34] The four sets of cubic splines are equally spaced over our time series.[35] For our passage equation, the comparison model which is a standard probit instead of a Heckman probit, we create cubic splines from a variable that counts from 1972 to the years until a state passes a TEL. As demonstrated by Beck et al. (1998) the addition of cubic splines to a standard binary dependent variables model allows us to estimate the hazard rate for "failure," which involves having a TEL proposed on the ballot in a given year.

Because a TEL can be proposed multiple times we must decide whether to study only the first time a TEL is proposed, every time a TEL is proposed or the times a TEL is proposed until the first one passes in a given state. We study all of the TEL proposals until a TEL is passed. We believe this is the most reasonable set of cases given our primary interest in the diffusion of TELs. Once a state passes a TEL it is likely that determinants of voter support change, because they then have a clear example of the consequences of a TEL whereas beforehand they can only look to states other than their own.

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[33] It is possible that the choice of a probit functional form affects our results (Buckley 2002), but for now we rely on the probit because it allows us to present a sample selection model. Boehmke et al. (2006) develop a duration model that takes account of sample selection, but their model does not allow for the use of time-varying covariates. Therefore, the best approach is to use the Heckman probit model with cubic splines in the selection stage to account for the effect of time on selection.

[34] 1972 was the year in which the first TEL was placed on a state-wide ballot

[35] Splines created at 0, 3, 9 and 23 years at risk for a proposal

As far as we are aware, previous scholars have not taken account of both policy proposal and adoption in studying the process of diffusion, but as we outlined previously there are important theoretical reasons to study both processes. It is also important to understand the factors that affect proposals because they are a necessary precondition for policy passage.

Data on Tax and Expenditure Limits

This paper looks at the proposal and passage of TELs in the US states from 1972 to 2000 that occurred via the initiative and referendum process, excluding Alaska.[36] We define a TEL as a constitutional or statutory act that limits revenue or expenditures at the state level. Specifically, we include measures that limit general revenues or expenditure, property taxes, or sales taxes. We exclude gas tax limits or any revenue limit that only applied to a certain segment of the population (such as to the elderly or a school district). We do not include proposals that simply reduce taxes or propose supermajority limits for tax increases.

Our data was gathered from a keyword search on the Ballot Measures Database of the National Conference of State Legislatures. This database contains all initiatives since 1902 and all referendum since 1998. To supplement this limited data availability, we rely on the work of Skidmore and Alm (1999). These authors code TELs from The State Tax Review (Commerce Clearing House, Chicago) and State Government News (Council of State Governments, Lexington, Kentucky) for the years 1978-1990. In the data analysis we present in this paper there are 69 proposed TELs, 32 of which pass.

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[36] In addition, Hawaii is excluded in all models that estimate the effect of geographic neighbors on policy passage and adoption.

About 1/3 of the TELs proposed in our dataset appear on the ballot via the referendum process and the other 2/3 via the initiative process.

We turn now to the variables used in the selection and the passage equations.

Modeling TEL diffusion and adoption

In this section we describe the variables we use to model the diffusion of TELs. The variables in the selection equation are designed to estimate the probability of TEL proposal. The variables used in the passage equation capture a state's probability of passing a TEL, conditional on proposal. Proposal and passage are not independent. In fact, we expect that many variables correlated with passage are likely to be correlated with proposals; however, we argue that some variables should theoretically only be related to proposals. In Table 3.1 we present a summary of the variables we expect to affect proposal and/or passage. Because TEL passages are a subset of TEL proposals it may be that we observe some correlations with passage, but the actual causal process works through the proposal stage. We will account for this in our selection model.

Table 3.1: Relationship Between Variables and Proposal or Passage

	Proposal Stage	Passage Stage
Popular liberalism	X	X
Government liberalism	X	X
Elderly population rate	X	X
School-age population rate	X	X
Total tax burden	X	X
Property tax burden	X	X
Gubernatorial Election Year	X	X
Presidential Election Year	X	X
Total number of initiatives on the ballot	X	X
Per capita income	X	X
Presence of the state initiative process	X	
Number TELs passed last 5 years	X	
Spatial lag of income similarity	X	
Previously Propoesed TEL	X	
Number of neighbors to adopt a TEL in last 5 years	X	
Cubic Splines	X	

The other decision we must make in studying TEL proposals and passage is how to account for the fact that both proposal and passage are repeated events. Although these are repeated events since our primary purpose is to study diffusion and how a policy spreads across states we focus only on the first instance of a TEL proposal and passage. We find it unreasonably that policy makers and voters in a state will need to look at the experiences of other states for subsequent decisions to propose or pass a TEL, because at that point they have their own individual experience on which to draw about the effect of a TEL that is passed.

The first variables we focus on are those related to the mechanisms of policy diffusion, which we argue should affect policy proposals but not passage. As previously mentioned, we test three mechanisms of diffusion: learning from geographic neighbors,

learning from economic neighbors, and learning from national trends. We explain the coding of each variable in turn.

In this analysis, we look specifically at the effects of recent geographic neighbor adoptions on a state's propensity to propose a TEL. We define geographic neighbors as any two states with a non-zero shared border. We create a variable that counts the number of a state's geographic neighbors who have passed a TEL in the previous 5 years. The motivation for the emphasis on recent passage is twofold. First, there are reasons to suggest that the effect of a state's policy actions on its neighbors is not constant over time. While we do not attempt to model diminishing influence, we believe that more recent actions by states should have a greater influence on their neighbors. Second, unlike previous analyses of policy adoption, the policy considered in this analysis is a repeating event. Whereas most previous studies of policy adoption drop states from the analysis after adoption, states in our dataset remain in the analysis since it is possible to pass multiple TELs over time. Adding a component of recentness in our diffusion variable allows us to explain why a state may adopt TELs in two different time periods.

In addition to the standard geographic variable for diffusion we propose analyzing the role that similarity in per capita income has on TEL diffusion. Our measure of the similarity in income levels between states is a spatial lag (This approach has been most common in international relations. See Beck, Gleditsch and Beardsley 2006 and Franzese and Hays 2006). This variable is based on the idea that states are likely to learn from policies adopted by states with a similar levels of income when they are looking to adopt tax and expenditure limits. To create a spatial lag that relates the influence of state j on

state *i* based on their similarity in per capita income we apply the following formula to
create a state-by-state weighting similar to that from Phillips (2005).

$$Wi_j = \frac{\sqrt{\dfrac{1}{|PerCapIncome_i - PerCapIncome_j|}}}{Si}$$

$$Si = \sum_j \frac{1}{\sqrt{|PerCapIncome_i - PerCapIncome_j|}}$$

The numerator of the weight consists of the inverse of the absolute difference between
state *i*'s level of per capita income and state j's reliance on per capita income. Taking the
inverse of this difference means that the smaller the difference between states, the larger
the numerator will be.

The denominator of the weight consists of the inverse of the sum of all absolute
differences between state *i* and all other states. The denominator of this equation is
important because it puts relative differences between states on a standard scale. Finally,
we take the square root of the numerator and denominator to reduce the influence of
cases that are very different from each other.

With this approach, we create a unique weighting for each state *j's* influence on
state *i*. A large value to the weight – representing close proximity between states in terms
of tax structure - suggests that state j has a large influence on state *i's* decision to propose
a TEL or not. Each weight variable is multiplied by whether or not state *j* has adopted a
TEL in the last five years. The sum of the resulting vector, the spatial lag, represents how
similar state *i* is in terms of per capita income to states that have passed a TEL in the last

five years. Larger numbers for the variable suggest greater similarity, so we expect a positive coefficient if diffusion happens through states with similar per-capita incomes.

To test whether diffusion occurs as a result of a nationwide trend for more TELs, we also test for a variable that is a count of the number of states that have adopted a TEL in the last five years. This variable will help to capture any national trends in TEL proposals that occur independent of the income or geographic similarity channel.

In addition to our diffusion variables, there are several institutional factors that we believe mediate exposure to a TEL. Most importantly, there is significant variation in direct democracy between states. While we could use a measure such as signature requirements, this variable fails to capture anything other than formal, institutional rules. Instead, we use the number of initiatives or referendum on the ballot in a given year as a proxy for ease of policy proposal and the predominance of initiative usage in some states. The number of initiatives on the ballot also captures the fact that initiatives and referendum are not equally likely to occur each calendar year. By controlling for the total number of initiatives we account for a variety of institutional and non-institutional factors that make proposal more likely. In addition to this variable, we also include dummy variables indicating whether or not it is a presidential or gubernatorial election year. As we explained in Section 5 we are examining all of the TELs proposed in a state until one passes. This means that we have multiple events in a given state. To account for time and other factors that may make the first TELs different than subsequent TELs we include a dummy variable that is coded one in the year following a TEL's proposal in a given state and is coded zero before a TEL is proposed. We believe that the general determinants of

TEL proposal will not change between the first and subsequent TEL proposals, but it does seem likely that if a TEL is proposed and fails that it is less likely one will be proposed in future years.

We also include variables that may influence the decisions of actors who incur the costs of placing a TEL on the ballot. It seems likely that policy proposers strategically choose when and where to propose or not propose, and for this reason we expect that the variables that effect passing a TEL, which we will discuss in more detail below, will also be associated with proposal of a TEL. These variables include tax burdens, ideology, and population characteristics. We expect these variables to effect proposal in the same direction that they affect passage.

We turn now to a discussion of the determinants of passage. The previous literature on TELs suggests that high tax burden is positively associated with adoption of a TEL. Here, we identify two types of tax burden: property tax burden and total tax burden. Each of these variables is calculated from state and local taxes and property taxes divided by total state personal income. The motivation for this ratio is that taxpayers are more concerned with the proportion of their income spent on taxes, than the overall level of taxes collected. The ratio measure also allows us to compare across different states. We expect that tax burden should be positively associated with TEL passage and proposal.

We also believe that government and citizen ideology influences whether or not a TEL gets proposed and passed. To measure popular ideology and institutional ideology,

we rely on Berry et. al's longitudinal index from 1970 to 2000.[37] We expect that popular liberalism decreases likelihood of TEL passage while institutional liberalism increases the likelihood of TEL passage. This latter prediction is grounded in the idea that citizens may favor tax limits if they fear that their government has a preference for high taxing ad spending policies.

The population characteristics we are most interested in are elderly population rate, school age population rate and per capita income. A large proportion of elderly population, we predict, should increase passage of TELs since this demographic group is positively associated with low-tax preferences. The causal effect of a greater proportion of school-age children is less clear. While tax limits are associated with reductions on spending on schools (Downes and Figlio 1997), studies at the individual unit of analysis have showed that parents with school age children are more likely to support TELs (Stein et. al 1983). We include the level of per capita income (adjusted for inflation) in both the proposal and passage equations. Many standard political economy models posit a relationship between level of income and preferences over the size of government, so we make sure to account for income levels in predicting whether a TEL is proposed or passed. Now that we have provided a discussion of the different variables we use in our analysis we turn to the results.

Results

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[37] There is considerable debate about the exact concept captured in Berry et al.'s measure. It may not capture ideology, but it does seem to correlate with many factors related to public mood, which is relevant for our needs (see Meinke et al. 2005 for an argument about the measure's validity).

We focus primarily on a Heckman selection model in which we estimate the determinants of passage, conditional on the model for TEL proposal. In Table 3.2 we present the results of our primary model, where the selection variable is TEL proposal and the dependent variable is whether a given proposed TEL is actually adopted.[38] We look at all states together as well as only states that have the initiative process.

In Table 3.2 we focus on the effect of income similarity on diffusion of TELs. In both models population characteristics, in particular more children, are associated with a lower probability of TEL proposal. Interestingly, the tax burden variables do not appear to be significant determinants of TEL proposal in either regression. However, the number of TELs passed nationally and the income similarity between states are both significantly related to TEL proposal. The negative coefficient for national TEL passage suggests that TEL proposals come in waves in which if many are passed over previous years it makes proposal less likely. At the same time, the results suggest that if states with similar per capita income pass a TEL the probability of one being proposed in a similar state increases. This fits nicely with our expectation in which policy actors learn about voter preferences by observing similar states and follow suit from the other states. The institutional and political variables are also significant in the way that we expected, which suggests that election characteristics (presidential or gubernatorial) and the ease of use of the initiative system are all associated with TEL proposal. Finally, it is worth noting that having proposed a TEL previously is not related to future proposals; however, the cubic

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[38] The results of a Cox proportional hazard model that allows multiple TELs to be proposed for each state are substantively similar to the results presented here. We are mostly interested in the two-stage process of policy adoption so for now we focus on the results of the Heckman Probit Model.

splines are significant in both regressions suggesting that there are time factors we are

correctly modeling in this regression. Buckely (2002) showed that prior diffusion studies

found a positive result for diffusion because they failed to account for time trend effects.

Table 3.2: Income-related Diffusion of TEL Proposals

	All States	Initiative states
Passage Equation	Coefficient (s.e.)	Coefficient(s.e.)
Popular liberalism	-0.02 (0.02)	0.0007(0.2)
Government liberalism	0.05 (0.02)***	0.04 (0.02)**
Elderly population rate	-18.1 (14.1)	-15.1 (16.9)
School-age population rate	-8.06 (15.9)	-6.67 (16.3)
Total tax burden	9.97 (28.5)	29.2 (43.3)
Property tax burden	-47.0 (24.3)**	-71.4 (39.4)*
Per Capita Income	0.003 (0.01)	0.008 (0.02)
Presidential Election Year	-0.40 (0.46)	-0.90 (0.58)
Gubernatorial Election Year	0.58 (0.56)	0.24 (0.90)
Constant	1.14 (5.29)	-1.28 (6.17)
Proposal Equation		
Popular liberalism	-0.008 (0.007)	0.002 (0.009)
Government liberalism	0.002 (0.006)	-0.008 (0.008)
Elderly population rate	-8.48 (4.93)*	-13.7 (8.74)
School-age population rate	-20.0 (10.4)**	-26.2 (15.2)*
Total tax burden	-3.43 (7.87)	-2.1 (15.3)
Property tax burden	1.23 (9.03)	3.64 (15.8)
Per capita income	0.002 (0.009)	0.005 (0.01)
State initiative process	0.64 (0.20)***	---
Number of initiatives on ballot	0.10 (0.01)***	0.11 (0.02)***
Gubernatorial Election Year	0.72 (0.17)***	0.87 (0.22)***
Presidential Election Year	0.46 (0.18)***	0.53 (0.22)**
Previously proposed TEL	-0.45 (0.29)	-0.66 (0.39)*
National number TELs passed 5 years	-0.06(0.03)**	-0.07 (0.04)*
Spatial lag of income similarity	2.05 (0.89)**	2.29 (1.19)**
Proposal Risk 1	0.14 (0.05)***	0.13 (0.07)*
Proposal Risk 2	-1.76 (0.49)***	-1.59 (0.75)**
Proposal Risk 3	3.18 (0.92)***	2.68 (1.44)*
Constant	3.03(3.72)	5.14 (6.04)
Rho	0.59 (0.22)**	0.65 (0.45)
Total Observations	1102	442
Uncensored Observations	50	41

*= significant at 0.10; ** = significant at 0.05 level; *** significant at 0.01 level

We turn now to the passage equation from the Heckman probit. First, note that the estimate of rho, or the correlation in the errors between the two equations, is significantly

different than zero in the regression for all states but not for the sub-sample of initiative states. This suggests that depending on the sample we are looking at selection may be a valid concern. Again, the results of the passage equation are highly similar in both regression. Institutional liberalism is associated with greater passage of TELs, which is mostly consistent with Gerber's (1998) claim that the initiative process can be used by voters to control their state legislature. Contrary to our expectations, a higher property tax ratio is associated with lower probability of a TEL being passed. These results are only weakly significant, but they may suggest that the standard story about TELs being part of a tax revolt requires greater investigation.

In Table 3.3 we present the results of regressions in which the key diffusion variable is the number of neighboring states that have passed a TEL in the previous five years. Again, these regressions include all instances of a TEL proposal until the point that a TEL is passed in a given state. First, in the selection or proposal equation we find that that diffusion variable is only significant among the sub-sample of states with the initiative process. This may suggest that initiative states react differently to the actions of their neighbors than other states. Alternatively, it may be picking up the fact that many states with the initiative process are located in the Western United States and these states also pass TELs. It will take further investigation to untangle whether this result holds up across all states or only occurs in Western states.

Table 3.3: Geographic Diffusion of TELs

	All States	Initiative states
Passage Equation	Coefficient (s.e.)	Coefficient(s.e.)
Popular liberalism	-0.01 (0.22)	0.005 (0.02)
Government liberalism	0.05 (0.02)***	0.04 (0.02)**
Elderly population rate	-19.5 (13.7)	-19.1 (17.7)
School-age population rate	-10.5 (16.1)	-10.1 (16.9)
Total tax burden	7.79 (27.9)	22.4 (40.7)
Property tax burden	-41.8 (23.1)*	-60.8 (34.4)*
Per Capita Income	0.001 (0.01)	0.006 (0.02)
Presidential Election Year	-0.32 (0.45)	-0.73 (0.48)
Gubernatorial Election Year	0.61 (0.54)	0.37 (0.75)
Constant	2.00 (5.3)	0.36 (6.61)
Proposal Equation		
Popular liberalism	-0.007 (0.007)	0.003 (0.009)
Government liberalism	0.002 (0.006)	-0.009 (0.008)
Elderly population rate	-6.82 (4.7)	-10.3 (7.4)
School-age population rate	-17.1 (9.7)*	-21.3 (14.0)
Total tax burden	-2.62 (7.5)	0.22 (14.5)
Property tax burden	-0.18 (9.11)	1.25 (16.2)
Per capita income	0.005 (0.009)	0.01 (0.01)
State initiative process	0.59 (0.19)***	--
Number of initiatives on ballot	0.10 (0.01)***	0.11 (0.02)***
Gubernatorial Election Year	0.69 (0.17)***	0.83 (0.21)***
Presidential Election Year	0.45 (0.17)***	0.51 (0.22)**
Previously proposed TEL	-0.39 (0.28)***	-0.58 (0.36)*
National number TELs passed 5 years	-0.02 (0.03)	-0.06 (0.04)*
Neighbors passed a TEL	0.18 (0.15)	0.42 (0.16)***
Proposal Risk 1	0.14 (0.05)***	0.13 (0.07)**
Proposal Risk 2	-1.73 (0.49)***	-1.65 (0.74)**
Proposal Risk 3	3.13 (0.94)***	2.9 (1.4)**
Constant	1.91 (3.52)	2.56 (5.6)
Rho	0.65 (0.18)***	0.73 (0.33)
Total Observations	1093	433
Uncensored Observations	49	40

*= sig at 0.1 level; ** = sig at 0.05 level; *** = sig at 0.01 level

Dependent Variable equals 1 in a state-year if a TEL passes and 0 otherwise

Difference in the number of observations between Table 3.2 and 3.3 occurs because Hawaii is not included when the diffusion variable is geographic neighbors

Other than this difference the proposal results are consistent between the two equations and essentially identical to the results in Table 3.2. The results in the passage equation are also quite similar in Table 3.2 and 3.3. The estimated value of rho or the correlation in the errors in the two equations is significant among the entire sample of states but not in the sub-sample of initiative states. The fact that the value of rho is significant among some samples and depending on the exact variables utilized leads us to believe that there is a significant correlation between proposal and passage of TELs.

Table 3.4 Income-based Diffusion Modeled as Policy Adoption

	All States	**Initiative states**
Popular liberalism	-0.02 5(0.01)***	-0.03 (0.02)*
Government liberalism	0.02 (0.007)***	0.03 (0.01)***
Elderly population rate	-7.0 (7.0)	-5.2 (13.3)
School-age population rate	-20.2 (14.4)	-16.0 (22.1)
Total tax burden	-3.2 (8.6)	-4.64 (14.9)
Property tax burden	-12.1(9.8)	-13.6 (15.5)
Per capita income	0.005 (0.01)	0.02 (0.02)
State initiative process	0.53 (0.26)**	--
Number of initiatives on ballot	0.06 (0.01)***	0.07 (0.27)***
Gubernatorial Election Year	0.71 (0.21)***	0.88 (0.35)***
Presidential Election Year	0.24 (0.23)	-0.07 (0.36)
National number TELs passed 5 years	-0.05 (0.03)	0.04 (0.05)
Spatial lag of income similarity	0.28 (0.95)	-2.8 (1.5)
Passage Risk 1	0.16 (0.14)	0.17 (0.17)
Passage Risk 2	-1.11 (0.49)**	-1.27 (0.6)**
Passage Risk 3	2.33 (0.97)***	2.73 (1.22)**
Constant	2.14 (5.14)	-1.73 (9.05)
Total Observations	1102	442

For comparison, we now estimate a standard diffusion model where a state-year is coded 1 if a TEL passes and 0 otherwise. The regressions for this analysis are essentially identical to those described earlier but with a different dependent variable. This is the

most common model in previous studies of state policy diffusion (Berry and Berry 1996).

The results in Table 3.4 shows that serious misestemation of the diffusion process can

occur when only a subset of diffused cases, those where the policy passed, are considered

evidence for diffusion. While most of the variables that represent "internal determinants"

(institutions, demographics, ideology) have similar signs and significance to our model of

policy proposal, the diffusion variables differ greatly. We will turn to these differences

shortly, but first there are some additional differences between the models to note.

Presidential elections are not associated with a higher probability of TEL passage

although they were associated with TEL proposals. The results of the passage regressions

suggest that there is overlap in the determinants of passage and proposal, but the overlap

is not perfect. Of greatest interest in these results are the interpretations of the variables

that indicate the presence or absence of a diffusion process. While we find positive

evidence of diffusion in the analysis of TEL proposal – that states follow innovating

states that are in similar income levels - no relationship is discernable when we look only

at passed TELs. First, neither the number of TELs passed in the last 5 year nor the spatial

lag of income similarity is significant determinants of passage. This follows with our

theoretical argument that these variables should affect proposals and not passage.

The comparison between the determinants of TEL proposals and adoptions

empirically demonstrates our theoretical point about the need for better theoretical

reasons to study adoption to the exclusion of proposal. The variables that affect passage

are in fact different than those that affect proposal. Therefore, if we simply study

adoption and claim to be studying diffusion we have to be confident that adoption is the

proper indicator of diffusion. If it is not, then we will make mistaken claims about the determinants of policy diffusion.

If we use the Heckman probit results to generate predicted probabilities and then predict passage of the TEL based on whether the predicted probability is above or below 0.5 we get the results reported in Table 3.5. A null model in which failure is predicted for all TELs would correctly classify the outcome in 26 of 50 cases. The Heckman probit for all states utilizing the income diffusion variable correctly predicts 34 of the 50 cases. This means our model predicts 12 more cases correctly than the null model for a proportional reduction in error of approximately 0.33.

Table 3.5: Predicting TEL Passage from Heckman Probit Model

	Heckman Model	
	Predicted Pass	Predicted Fail
Passed	9	15
Failed	1	25

We have presented a variety of results about the determinants of TEL diffusion. The results confirm our earlier argument that diffusion of tax and expenditure limits is most likely to happen between states that have similar income levels. Furthermore, our major methodological argument – the usage of a selection model to account for policy proposals – is clearly useful, because when we utilize a selection model we find that there is a significant correlation between the errors in estimating proposal and passage.

Conclusion

In this paper we make a theoretical and a methodological addition to the extant diffusion literature. First, we argue that from a theoretical point of view policy proposals

are evidence of the learning that is at the core of the diffusion process. The diffusion process is focused on learning, and we argue that a policy proposal is evidence that policy entrepreneurs have learned about a policy. We also argue that actors in a state will learn about tax and expenditure limits by looking at states with similar income levels. To capture this aspect of learning we create spatial lags based on the similarity between a state and recent adopters of TELs. Our results suggest that similar per capita income levels is a significant factor in TEL proposal. We compare models for TEL proposal and adoption, and find that different factors are related to adoption than proposal. This suggests that studying adoption is not the same as studying passage.

Second, we argue and demonstrate empirically that when studying policy adoption – as often done in the diffusion literature –it is critical to account for the factors that affect policy proposal. It seems unlikely that policy proposal is random, conditional on the models used to estimate policy adoption, and if that condition is not met then our estimates of policy adoption will be biased. To account for the bias we suggest the use of Heckman probit selection model. We use a Heckman probit model to first account for TEL proposals and then use the inverse Mills' ratio to account for the selection hazard in the second stage where we estimate the determinants of TEL passage. When we compare a Heckman and a non-Heckman model for policy adoption we find significantly different results. This provides empirical confirmation that ignoring the policy proposal stage will affect our conclusions about policy adoption. Taken together our results suggest that when studying diffusion we must pay more attention to the non-random nature of policy proposals.

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Conclusion

The three papers in this dissertation each address a different aspect of tax policy, but I believe there are some common conclusions that we can draw from the different papers. For instance, consider the following points:

1. If trade taxes are a significant source of government revenue then political parties may be important players in trade policy.

2. If governments rely considerably on trade taxes then they will not have significant income tax collection, which implies that the state has low capacity.

3. This implies that the move from low to high capacity is likely to require political parties that are interested in changing tax policy and developing administrative and bureaucratic capacity.

In this dissertation I have not endeavored to test the effect of political parties on the development of state capacity; however, the papers suggest that parties are likely to be critical players in the development of the state. This is a conclusion that requires direct analysis, but it is certainly a fruitful direction for future research. In particular this is a conclusion that may help to inform policy advice to developing countries. Advice to developing countries largely ignores the role of political parties in developing state capacity and the conditions for successful governance. Instead, the recommendations often include legal changes, increases in technical capacity or policy proposals, but none of these address the core political process and the key actors in the process. It may well be that advice to developing countries would do well to consider what gives political

parties the incentive to develop state capacity. This is a line of research I intend to pursue in the future.

The work on tax and expenditure limits begins to unpack how and why diffusion across political boundaries happens. In addition to showing that studying policy proposals is useful, Moule and I also demonstrate that policies spread among similar states in a way that has not been well-conceptualized previously in the diffusion literature. This finding opens the way for new theorizing about the process of diffusion and the role of communication in that process. To date much of the research on diffusion has not paid considerable attention to who is learning, what is being learned and how the learning takes place. By demonstrating that diffusion occurs among states with similar income levels, we believe that we are opening the door for better theorizing about diffusion because this mechanism of diffusion seems more closely connected to politics and policymaking than the standard geographic arguments for diffusion. This will also prove to be a fruitful area for further research.