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"Responsiveness" as a Measure of Representation

John G. Matsusaka*

*USC Marshall School of Business, matsusak@usc.edu

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Abstract

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"Responsiveness" as a Measure of Representation

*John G. Matsusaka**
University of Southern California

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"Responsiveness" as a Measure of Representation

1. Introduction

The empirical study of representation requires being able to measure the quality of representation: how well do legislators reflect the views of their constituents when making public decisions. This study provides an empirical assessment of one popular approach – regressing roll call votes on a measure of constituent opinion – and finds that it does not yield reliable estimates of representation.

Several studies, dating back at least to Achen (1978), discuss the pros and cons of alternative measures.[1] There is general agreement that the quantity we would like to recover is the "distance" or "proximity" between what legislators do and what their constituents would like them to do, typically called "congruence."[2] A general definition of congruence is

(1) $C(Y - Y^*)$,

where Y is a legislator's action (e.g. a roll call vote), Y^* is the action preferred by his or her constituents, and $C(x)$ is a function with a maximum at $C(0)$ that is decreasing in distance from $x = 0$. Typical examples are $C(x) = -|x|$ and $C(x) = -x^2$.

Often data on Y^* are not available, but the researcher has data P (e.g. an opinion index or demographic characteristics) that is correlated with Y^*. If P and $Y^*$are on a different scale, (1) cannot be implemented, and instead some studies estimate regressions of the form

(2) $Y_n = a + bP_n + e_n$,

[1] See also Achen (1977), Romer and Rosenthal (1979), and more recently Golder and Stramski (2010).

[2] More broadly, representation is a multidimensional concept that might also involve process and descriptive issues (Pitkin, 1967). My paper is addressed to the literature that seeks to understand representation as the degree to which government actions or "policies" reflect the values and preferences of the citizens.

where n indexes a legislator, a and b are coefficients to be estimated, and e_n is an error term.[3] The coefficient on the proxy for constituent preferences, b, is referred to as "responsiveness." The working assumption is that b is a proxy for congruence, that is, a larger value of b means a greater congruence between legislators and their constituents. However, several studies have noted that there is no *theoretical or formal* connection between congruence and the coefficient b (Romer and Rosenthal, 1979; Erikson et al., 1993, Ch. 4; Matsusaka, 2001). Therefore, the continuing use of "responsiveness" as a measure of representation must be justified on an empirical basis. The purpose of this study is to investigate whether this empirical assumption is justified. To that end, the study presents what I believe is the first direct evidence comparing the responsiveness coefficient to actual congruence for a sample of votes.

The challenge in assessing the relation between responsiveness and congruence is measuring constituent preferences so that (1) can be implemented. We need data for legislator actions and constituent preferences that are on the same scale. I use roll call votes on laws that were subsequently challenged in a referendum: in these cases, we observe the votes of legislators and constituents on the same law (Gerber, 1996). I construct a data set that includes 3,242 roll call votes, covering 25 laws in nine states, for which district-level referendum election returns are available. For each law, the congruence between a legislator's roll call vote and majority opinion in his or her district is calculated directly; each roll call vote is either congruent or not congruent with majority opinion. I then calculate responsiveness by regressing roll call votes on constituent opinion, and examine how well the responsiveness coefficients predict congruence.

My purpose is not to provide a formal test that can accept or reject the validity of responsiveness regressions. Rather, I explore a number of different cases that correspond to the sort of tests that are common in the literature (for example,

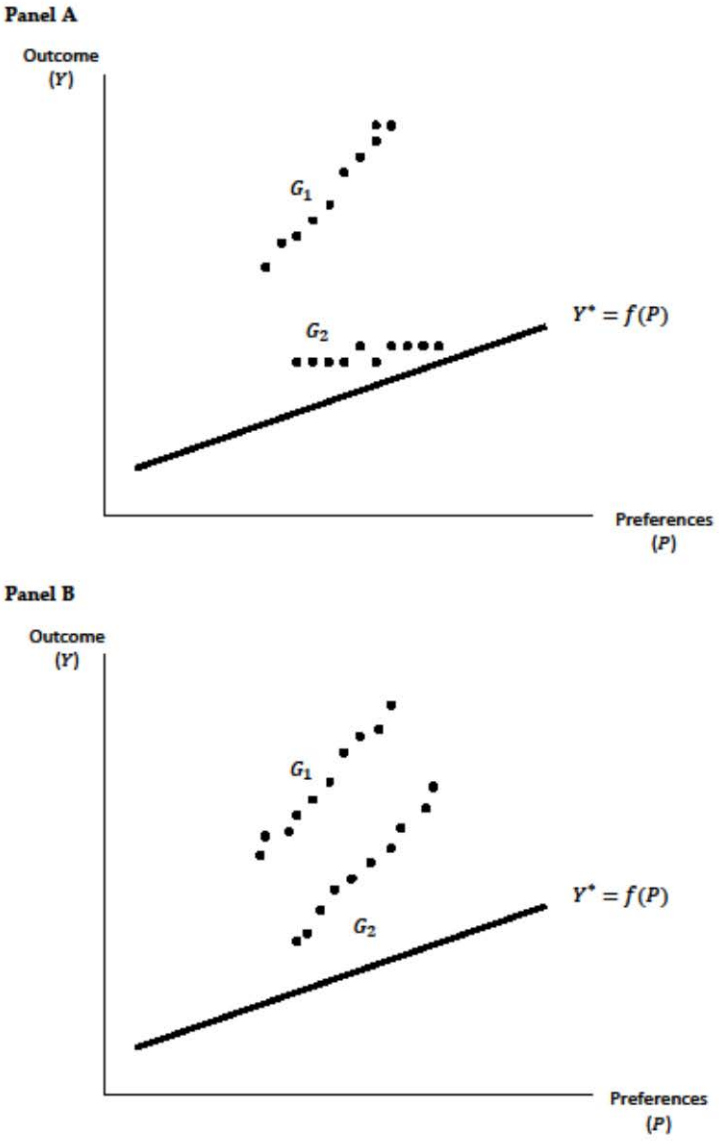
[3] The responsiveness approach has been employed for decades. A sample of recent studies using this approach include Bartels (2008) and Gilens (2005) [both testing if policy is more responsive to rich than poor], Mian et al. (2010) [testing if politicians are more responsive in competitive than noncompetitive districts], and Monogan et al. (2009) [testing if policy is more responsive to public opinion in initiative than noninitiative states].

comparing whether male or female legislators are more likely to cast congruent votes), and assess whether the responsiveness approach "works" for them one-by-one. This collection of cases gives concrete examples that together hopefully paint a broader picture. The intuition is that if there is an underlying empirical connection between congruence and responsiveness, we should observe it fairly consistently. The main finding is that the responsiveness coefficient b is not reliably related to congruence. I conduct a number of comparisons between different groups (male versus female legislators, votes in upper versus lower chambers, etc.) and find that the b coefficients sometimes correctly indicate which group is most congruent, sometimes indicate the opposite relation from the actual one, sometimes indicate a relation where none appears to exists, and sometimes fails to indicate a relationship that exists. Together with previous research showing the absence of a theoretical link, these findings suggest that responsiveness regressions should not be used to compare congruence between legislators and constituents.

2. Theoretical Problems with Responsiveness

Before turning to the empirical assessment of responsiveness, it is useful to review the intuition for the theoretical concerns. The main problem has been flagged in several studies, although the precise applications have differed (Romer Rosenthal, 1979; Erikson et al., 1995, Ch. 4; Matsusaka, 2001). To frame the discussion, recall that conceptually we are interested in knowing (1). Consider then model (2). Denote the relation between constituents' preferred outcome and a proxy for their preferences (such as an ideology index) as $Y^* = f(P)$, where f is an increasing function. Figure 1 gives examples. In a perfectly congruent world, legislators would choose $Y = Y^*$, and all observations would lie on the f function: $Y = f(P)$. In such a case, there would be a positive relation between outcomes and the preference proxy. Similarly, if legislators were fully congruent except for some white noise error in outcomes ($Y = f(P) + e$), we would expect a positive relation between outcomes and the preference proxy. Thus it is reasonable to expect $b > 0$ if representation is effective, and a finding of a positive correlation indicates that some representation is occurring (Erikson et al., 1993).

Figure 1. Hypothetical Observations of Opinion (*P*) and Outcome (*Y*)



The more difficult case – and the case of interest in most studies – is where we wish to *compare* congruence across two groups. For example: (i) Are policy choices in initiative states more or less congruent with popular opinion than policy choices in noninitiative states, or (ii) Do female or male legislators vote more or less congruently with constituent preferences? Denote the two groups we would like to compare as G_1

and G_2. We would like to measure $C(Y_n - f(P_n))$ separately for $n \in G_1$ and $n \in G_2$, but f is not observable. Suppose instead that we estimated (2) separately for each group, producing responsiveness coefficients b_1 and b_2 (or equivalently for our purposes, we could estimate a single regression with an interaction term that allows the coefficient on preferences to vary by group). The question is: what can we learn about relative congruence from the b coefficients?

Figure 1 shows hypothetical cases. The cluster of points G_1 represents opinion-outcome observations for one group and the cluster labeled G_2 represents observations for the other group. Consider Panel A. If regression (2) is estimated separately for groups G_1 and G_2, we would find $b_1 > b_2$ (or, in an interaction framework with G_1 as the null and G_2 as the interaction, we would find a negative coefficient on the interaction term). Some studies assume from this pattern that the outcomes in G_1 are more congruent with constituent preferences than the outcomes in G_2. However, the outcomes in group G_1 in fact are *less* congruent (more distant) from what the public wants than the outcomes in group G_2. The ordering of the responsiveness coefficients is the reverse of the congruence ordering.[4] Panel B illustrates another case, this one in which $b_1 = b_2$ (or equivalently that the coefficient on the interaction term is zero). This example shows why finding equal responsiveness coefficient does not imply that congruence is the same between groups (or: finding a zero coefficient on an interaction terms does not imply that congruence is the same in both groups).[5] Even a finding of $b_1 > 0$ and $b_2 = 0$ would not imply that that G_1 is more congruent than G_2. In short, *any ordering of responsiveness coefficients can be consistent with G_1 being more or less congruent*

[4] On first glance, it might seem from this example that the problem is omission of separate intercepts for the two groups. In general, both intercept and slope coefficients should be allowed to vary by group: as Achen (1978) noted, responsiveness is characterized by both a and b, not by b alone. However, the underlying problem is not about the coefficients: it is that without knowing f, there is no way to infer whether (a_1, b_1) or (a_2, b_2) produce outcomes closer to what constituents prefer.

[5] Hagen et al. (2001) suppose that policy is generated according to $Y = \alpha + \beta Y^* + e$, and show that if $Y^* = \delta P$, then $b = 0$ in (2) if and only if $\beta = 0$. They conclude that equation (2) can be used to test if $\beta = 0$. This is correct, but β is not the object of interest; as shown in panel B we can easily imagine cases in which responsiveness is the same in two groups, but one group chooses policies that are much closer to constituent interest than the other group.

than G_2. Without more structure, there is no logical connection between b and congruence.

The preceding argument is made under fairly general conditions. Still the notion that the responsiveness coefficient is correlated with congruence has some intuitive appeal. If one is willing to make stronger assumptions, perhaps it would be possible to establish a relation between the responsiveness coefficient and congruence (e.g., it might be possible to get traction by making functional form assumptions about the outcome generation process, the function form of f, or the errors). Perhaps a theory can be developed in which high levels of congruence are associated with high levels of responsiveness. Although a theoretical argument justifying a connection between responsiveness and congruence does yet exist, it does not stretch imagination too far to think that such an argument could be created in the future. So even though no logical connection yet exists, perhaps the data generation process in the real world (for reasons we do not yet understand) produces an *empirical* connection between responsiveness and congruence. The rest of this paper examines whether such an empirical connection exists.

3. Data and Methods

My approach is to calculate congruence directly using (1), calculate responsiveness using (2), and then assess directly how well responsiveness proxies for congruence. In order to calculate both congruence and responsiveness, I examine a set of state laws for which we can observe roll call votes as well as the votes of citizens in each district on exactly the same issues.

At present, 23 American states allow citizens to use the referendum process to challenge state laws approved by elected officials (passed by the legislature and signed by the governor). Implementation details differ, but in these states, if citizens collect a predetermined number of signatures from fellow citizens, an election is held involving

the electorate at large in which voters have the option to approve or repeal the law.[6] I use district level referendum election returns to measure the majority (and median) preferences of constituents in each legislator's district on a law.

To construct the sample, I began by identifying all state-level referendums during the period 2000-2014 using the database maintained by the Initiative and Referendum Institute. From this list of 54 ballot measures, I examined official election returns provided by each state's election division; the necessary data were available for 25 referendums.[7] The bill associated with each referendum was then identified, and the roll call votes on that bill were drawn from legislative records. The final sample after dropping abstentions contains 3,242 roll call votes associated with 25 laws in nine states. Table 1 lists and describes the laws.

The referendums took place in Alaska, California, Maine, Maryland, Michigan, North Dakota, Ohio, South Dakota, and Washington. These states represent a mix of urban and rural, and include both "blue" and "red" states: during the sample period Republicans usually controlled the legislatures of Alaska, Michigan, North Dakota, and South Dakota; Democrats usually controlled California, Maine, Maryland, and Washington; and Ohio experienced mixed control. The laws covered fiscal, political, and social issues, and included hot-button topics of national interest such as same-sex marriage, as well as issues of local interest such as Alaska's law allowing aerial hunting of wolves and North Dakota's law allowing the state university to discontinue use of the "Fighting Sioux" nickname for its mascot. The ideological orientation of the laws was also mixed, with some proposing to move policy in a liberal direction (e.g. allowing same-sex marriage or granting tuition to illegal immigrants) and others proposing to move policy in a conservative direction (e.g. allowing charter schools or limiting collective bargaining by public employees). Voters repealed 40 percent of the laws.

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[6] For example, in California, petitioners have 90 days after approval of a law to collect signatures from eligible voters equal to 5 percent of the number of votes cast in the previous gubernatorial election (as of 2015, roughly 505,000 signatures). For institutional details, see Gerber (1999).

[7] Referendums were excluded if a state did not report sufficiently disaggregated data, if a state changed its district lines between the time of the roll call vote and the time of the referendum election, or if a referendum was abandoned by its sponsors after qualifying for the ballot.

For each roll call vote n, define $Y_n \in \{yes, no\}$ as the legislator's vote, and $Y_n^* \in \{yes, no\}$ as the majority view in the district based on referendum election results. Then congruence on vote n is measured

$$(3) \qquad C_n = \begin{cases} 1 & if \quad Y_n = Y_n^*; \\ 0 & if \quad Y_n \neq Y_n^*. \end{cases}$$

Congruence here is measured at the level of an individual roll call vote, unlike the responsiveness coefficient which is inherently a description of a set of votes. To provide a comparable group-based measure of congruence, I calculate the congruence for group G with votes $n = 1, \ldots, N$ as $C(G) = \sum_{n \in G} C_n / N$.

The group congruence measure $C(G)$ has several appealing features. It has a natural interpretation as the fraction of roll call votes in the group that were consistent with majority opinion in the district. Because there are only two possible outcomes, $\{yes, no\}$, the majority preference is also the median preference within any district. $C(G)$ can also be given a utilitarian interpretation: if we weight all people equally and assume equal preference intensity across citizens, then $C(G)$ indicates the utility-maximizing vote choice within a district.[8]

The responsiveness coefficient for a group G is estimated with a linear probability regression of the form:

$$(4) \qquad D_n^{YES} = a + b \cdot \%Yes_n + e_n,$$

where D_n^{YES} is a dummy variable equal to one if legislator n voted yes on the law and zero if the legislator voted no, $\%Yes_n$ is the percentage of votes cast in favor of the law in legislator n's district in the referendum election (as a fraction of votes in favor and

[8] One general issue is that legislators may not know opinion in their districts on a particular law. In this case, a noncongruent vote might be more of an "honest mistake" than a deliberate decision to go against district preferences. Matsusaka (2015) reports evidence against the honest mistakes hypothesis. For the purposes of the present study, it is not important why congruence and noncongruence occur; the issue is how we can measure them.

against), and $n \in G$.[9] The coefficient b represents responsiveness. To compare responsiveness between groups, I classify one group as the baseline; members of the baseline group have $GROUP_n = 0$, and members of the other group have $GROUP_n = 1$. I then estimate the usual interaction regression, again in a linear probability form:

$$(5) \qquad D_n^{YES} = a + b \cdot \%Yes_n + c \cdot GROUP_n + d \cdot \%Yes_n \cdot GROUP_n + e_n.$$

In this formulation, d captures the differential responsiveness of the two groups. The question is: does $d > 0$ imply that roll call votes in GROUP 1 are more congruent with constituent preferences than votes in GROUP 0?

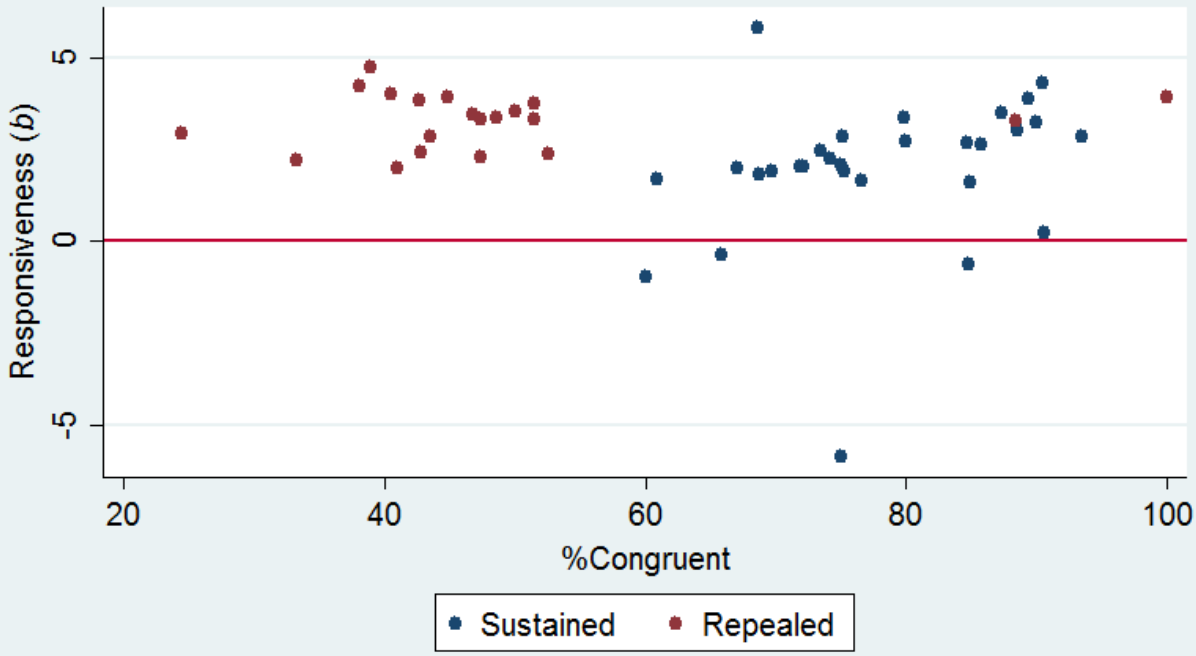
4. Comparison of Congruence and Responsiveness

I begin by estimating congruence (3) and responsiveness (4) for each chamber and each law. The estimated congruence and responsiveness coefficients are reported in Table 2. Congruence ranges from 24.5 percent to 100.0 percent, with overall congruence of 67.0 percent. The responsiveness coefficients are usually but not always positive and generally different from zero at conventional levels of statistical significance, indicating that legislator roll call votes are connected with district preferences at the margin. For the sample as a whole, the responsiveness coefficient of 1.17 means that a 1 percent increase in district opinion in favor of a law is associated with a 1.17 percentage point increase in the probability that a legislator votes yes on the law.

To what extent do the responsiveness coefficients capture the underlying congruence? The measures are not on the same scale, but we should see a positive relation if responsiveness proxies for congruence. Figure 2 plots responsiveness against congruence for the 50 law-chamber observations. As can be seen, there is no reliable connection between congruence and the responsiveness coefficient b.[10] This means that if a researcher was interested in which legislatures were most attentive to constituent

[9] The coefficients from a linear probability regression are easy to interpret as marginal effects. The main conclusions of this study are the same if responsiveness is estimated using a logistic regression instead.

[10] A regression of congruence on responsiveness produces a coefficient of -1.69 with standard error 1.60, and R^2 of .023.



Note. The figure plots the percent of congruent roll call votes against the responsiveness coefficient from a linear probability regression: Y = a + b %Yes, where Y = 1 if a legislator voted yes and %Yes is the percent of votes in favor the district. The unit of observation is a law-chamber.

preferences, knowing the responsiveness coefficient would provide no insight. To illustrate, the chamber with the highest congruence (100 percent), California's senate in 2004, has responsiveness $b = 3.93$, while the chamber with one of the lowest congruences (38.9 percent), California's senate in the second insurance law of 1999, has a *higher* responsiveness coefficient $b = 4.71$. The chamber with the highest responsiveness coefficient of $b = 5.82$, Maryland's lower chamber in 2006, has a middling congruence of 68.6 percent, while the chamber with the lowest responsiveness coefficient of $b = -5.88$, California's senate in 1998, has a higher congruence of 75.0 percent.[11]

The responsiveness coefficient is most often used to compare different groups, for example, to assess whether legislators are more representative in initiative than noninitiative states. To further assess the meaning of the responsiveness coefficient, I

[11] There is no connection between congruence and responsiveness if a logistic regression is used for responsiveness instead of a linear probability regression.

next report a series of comparisons between subgroups of potential interest. For each comparison, I calculate congruence for each group, and then I estimate (5) which introduces an interaction term to capture differences in responsiveness between the groups. The question is: to what extent do differences in responsiveness coefficients indicate congruence differences between the groups?

Table 3 reports comparisons using groups that are state or chamber specific.[12] Panel A compares congruence between the two groups using a regression with an interaction term:

(6) $$C = \alpha + \beta \cdot GROUP + \varepsilon.$$

The intercept α represents mean congruence for GROUP = 0, while congruence for GROUP = 1 is represented by $\alpha + \beta$. In the first column, observations are grouped according to whether the law was repealed (GROUP = 1) or approved (GROUP = 0) in the referendum election. We might expect repealed laws to have been less consonant with constituent preferences.[13] The congruence regression in Panel A shows a clear difference between the two groups: roll call votes on laws that voters subsequently repealed were 29.6 percent less likely to be congruent with district opinion and the difference is statistically significant.

Panel B of Table 3 reports the corresponding responsiveness regression, allowing responsiveness to vary between approved and repealed laws. The coefficient on %Yes is positive and statistically different from zero, indicating that roll call votes are connected to constituent opinion. The coefficient of 1.97 means that a 1 percent increase in district opinion in favor is associated with a 1.97 percent greater chance of a roll call vote in favor. The coefficient of primary interest – on the interaction term d – is tiny and statistically insignificant (the coefficient of 0.03 means that a 1 percent increase in voter support is associated with a 0.03 percent increase in the probability of a roll call vote in

[12] All regressions here and below use standard errors clustered by law-chamber.

[13] This is plausible but not necessary. If districts are gerrymandered, even a fully congruent legislature (that is, a legislature in which every member votes according to majority opinion in his or her district) can choose a policy that is not supported by a majority of voters statewide (Gilligan and Matsusaka, 2006).

favor): the responsiveness coefficients reveal no difference in representation between the two groups. A researcher who used a responsiveness regression to measure representation would be led to an incorrect conclusion in this case.

Figure 2 helps understand the pattern by indicating congruence and responsiveness separately for sustained and repealed laws. The difference in congruence between sustained and repealed laws is plain, while no reliable difference in responsiveness can be seen. Apparently, for repealed laws, there was downward shift in congruence among most legislators that operated more-or-less as a fixed term, but their responsiveness to constituent opinion at the margin was unchanged.

Several studies have argued that policies are no more connected to opinion in initiative than noninitiative states, basing that conclusion on a finding of no difference in responsiveness coefficients between the two groups (Lascher et al, 1996; Camobreco, 1998; Monogan et al., 2010). Table 3 and Figure 2 give a clear example that such a conclusion cannot be justified from responsiveness coefficients.

In column (2) of Table 3, observations are separated according to whether a legislator was a member of the upper chamber (GROUP = 1) or the lower chamber (GROUP = 0). This is an interesting comparison because lower chambers are designed to be more closely connected to popular opinion than the upper chamber by requiring members to stand for election more frequently and by having more members (meaning fewer constituents per district).[14] The difference in congruence between upper and lower chambers is only 1.6 percent, not statistically different from zero. In Panel B, the coefficient on the interaction term d is tiny and statistically insignificant. In this case, the responsiveness coefficient would lead to the correct conclusion.

In column (3) Table 3, observations are distinguished based on whether the state had term limits on legislators (GROUP = 1) or not (GROUP = 0).[15] This is an interesting question to ask because there are offsetting theoretical predictions about how term limits affect congruence. On the one hand, term limits are intended to bring about the

[14] All states in the sample have more members in the lower than the upper chamber. Alaska, California, Michigan, Ohio, and Washington have longer terms for members of the upper than lower chamber.
[15] Among the states in the sample, California, Maine, Michigan, Ohio, and South Dakota had legislative term limits during the period of study.

election of "citizen legislators" (rather than career politicians) whose views are presumed to be more consonant with their constituents, which would increase congruence. On the other hand, term-limited legislators have less incentive to follow constituent preferences when they can no longer stand for re-election.[16] Panel A shows that congruence is 9.6 percent lower in term limit states than other states, statistically different from zero at the 10 percent level. While the magnitude of the difference is nontrivial, the precision is at the border of conventional levels of significance. The responsiveness coefficient in Panel B is negative and statistically insignificant.

In most applications, district opinion is measured using a proxy (which is the reason why congruence is not calculated directly). To get a sense of how things might be different with a proxy, Panel C of Table 3 reports responsiveness regressions that proxy district opinion with a dummy variable equal to one if the Democratic candidate won the preceding legislative election. In column (1), the interaction term now shows a negative coefficient, but it is far from statistically significant. Following previous practice in the literature, this would lead to an incorrect inference that congruence is no different for laws that were approved and repealed. In column (2), the proxy variable is statistically insignificant, as it is in column (2). In column (3), the interaction coefficient is negative and statistically different from zero; this is closer to the finding in Panel A in terms of statistical significance.

The next set of estimates, in Table 4, form the comparison groups based on individual-specific sources of variance. These regressions include fixed effects for law-chambers; otherwise the setup is the same as Table 3. Column (1) compares congruence of female and male legislators. There is a growing literature investigating whether female or male legislators behave differently. Anzia and Berry (2011) argue that because of discrimination against women, only the most talented and hard-working female candidates succeed in being elected. They find that female congressional representatives deliver more federal spending to their districts than male representatives. Stadelmann et al. (2014) report that female legislators vote more congruently than male legislators in Switzerland. In contrast, Ferreira and Gyourko (2014) find no difference in the policy

[16] See Besley and Case (2003), Grofman (1996), Kousser (2008), and Mooney (2009).

choices of female and male mayors. The point estimate indicates that female legislators are 1.2 percent less likely to cast a congruent vote than male legislators; this number is small and not different from zero at conventional levels of statistical significance. The responsiveness regression in Panel B produces a negative coefficient on the interaction term that is also not distinguishable from zero statistically.

Column (2) in Table 4 distinguishes legislators by party, either Republican or Democratic (including Green). While there is no a priori reason to expect one party to be more representative than the other, it is interesting to explore if a difference does happen to exist (e.g. Achen (1978) reports some evidence that Democrats are more congruent than Republicans). The point estimate in the congruence regression indicates that Republicans are 2.3 percent less congruent than Democrats, but the coefficient cannot be distinguished from zero. The responsiveness regression in Panel B produces a positive coefficient on the interaction term, but it is statistically insignificant.

Column (3) in Table 4 distinguishes legislators according to whether they belong to the majority or minority party. This aspect of party membership is potentially relevant because a body of theory suggests that the majority party forms a coalition or cartel to advance the party's interest, and the coalition functions by ensuring that its members vote in accordance with the party agenda, not necessarily in accordance with their individual constituent interests (see Cox and McCubbins (1993, 2005)). If members of the majority are under more pressure to conform to the party's goals, they would be less likely to cast congruent votes. The congruence estimates provide some support for this idea by showing that members of the majority party are 20.1 percent less likely to cast a congruent vote, different from zero statistically at the 5 percent level. The responsiveness regression in Panel B reports a negative coefficient on the interaction term that is also statistically different from zero. In this case, the responsiveness regression points in the right direction.

Column (4) of Table 4 distinguishes legislators according to whether they are in their last term under the state's term limits law. The sample is restricted to the five states that have term limits during the sample period. Political agency theory implies that legislators are motivated to mind constituent interest by the need to stand for re-election, so they should be less attentive to constituent preferences as they near the end

of a term. Panel A shows that congruence is 5.2 percent lower for legislators in their last term than other legislators, a modest number that is not statistically different from zero. The responsiveness regression in Panel B also produces a small negative coefficient on the interaction term that is not statistically distinguishable from zero.

As a final exercise, I consider the connection between congruence and electoral competition. Scholars have long been interested in whether competition causes elected officials to be more responsive to public opinion. Responsiveness regressions test for competition effects by interacting a measure of competition with a proxy for constituent interests (e.g. Mian et al. (2010)). Table 5 assesses this approach by reporting regressions with competition variables. In column (1), competition is measured as the vote margin, defined as the difference between votes received by the winner and runner-up in the previous legislative election, divided by their combined votes. As can be seen, competition so measured is inversely related to congruence, that is, legislators representing districts with a large vote margin are more likely to cast a congruent vote. The coefficient is different from zero at the 5 percent level. A possible explanation for this pattern is that noncompetitive districts are more likely to elect legislators who are ideologically aligned with their constituents, and ideology determines roll call votes (Matsusaka, 2015). The responsiveness regression produces a *negative* coefficient on the interaction term between district opinion and vote margin. Herethe responsiveness coefficient incorrectly suggests that competition increases congruence.

In column (2) of Table 5, competition is measured as a dummy variable equal to one if the vote margin is less than 2.5 percent. Congruence is 5.6 higher in these extremely competitive districts, but cannot be distinguished from zero statistically. The corresponding responsiveness regression produces a positive sign on the interaction term that is different from zero at the 5 percent level. In this case, the responsiveness regression suggests a relation that may or may not exist.

At least one of the specification in columns (1) and (2) must be incorrect, and perhaps both. These regressions are not reported in order to provide a meaningful test of the effect of competition on congruence, which would require at a minimum controlling for other factors that are known to influence roll call voting, such as ideology. Similarly, I am not arguing that the regressions above identify causal

relations. The point in Tables 3-5 is to estimate whether there is an underlying structure to voting behavior that creates a reliable connection between congruence and the responsiveness coefficient. The various regressions, which include a number of the most obvious conditioning variables, give little reason to believe that the responsiveness coefficient is reliably correlated with congruence.

5. Discussion and Conclusion

Progress in the study of representation depends on being able measure the extent to which representation is taking place. Accordingly, scholars have proposed and explored a number of different approaches to measuring representation. Theoretically, there is general agreement that the ideal measure would be the "distance" between the actions taken by a representative and the actions preferred by his or her constituents, usually called "congruence." Measuring congruence directly is demanding because the researcher must have a measure of the legislative action and constituent preferences on the same scale, and such data are often unavailable. This paper examines an alternative measure, typically called "responsiveness," that has been employed in empirical research when data on legislators and constituents are only available on different scales. Responsiveness is the change in legislator behavior with respect to a change in constituent opinion, estimated from a regression using a sample of legislators.

Several studies have observed that there is no theoretical basis for assuming that responsiveness so measured is correlated with the degree of representation (Romer and Rosenthal, 1979; Erikson et al., 1995; Matsusaka, 2001). Although there is no necessary or strictly logical connection between congruence and responsiveness, one might suspect that such a connection exists in practice. Intuitively, when legislators choose actions that are close to constituent preferences, it is not unreasonable to conjecture that they are also more responsive to preferences at the margin. This purpose of this study is to present what I believe is the first direct evidence on the empirical connection between responsiveness and congruence.

The challenge in executing the study is finding data on legislator actions and constituent preferences on the same scale so that congruence and responsiveness can be compared directly. My strategy is to study roll call votes on laws that were

subsequently challenged in referendums. For these laws, we observe both the choice of legislators through their roll call votes, and the preferences of their constituents though the referendum election returns. The main finding is that that responsiveness is not a reliable proxy for congruence. Perhaps the most striking example is that congruence is 29.6 percent lower for laws that were subsequently repealed by the voters but the responsiveness coefficient is essentially the same for approved and rejected laws.

The evidence in this study is complementary to theoretical work that casts doubt on the responsiveness coefficient as a measure of representation. All roads appear to lead to the conclusion that the responsiveness coefficient is not a valid proxy for the amount of representation. To extent there is a connection between the representation coefficient and congruence in a particular sample, it appears to be largely happenstance.

The implication is that researchers should approach questions related to representation by using direct congruence measures. Traditionally it has been difficult to calculate congruence because of the absence of data on constituent preferences. The severity of this problem is rapidly declining, however, with the growth of opinion surveys and the development of methods to impute district opinion data from broad surveys (Lax and Phillips, 2010; Tausanovitch and Warshaw, 2013). Consequently, there is now a flourishing literature on representation that employs direct measures of congruence. One relatively new approach is to use theory-driven estimates to produce and compare legislator and constituent ideal points based on opinion surveys or ballot measure returns (Gerber and Lewis, 2004; Bafumi and Herron, 2010; Masket and Noel, 2011; Kousser et al., 2014). However, Broockman (forthcoming) shows that this method has a significant limitation in that ideal points estimated from aggregated roll call votes might capture preference consistency rather than ideology. Broockman concludes that the most reliable approach is to calculate congruence at the level of individual votes. Congruence at the level of individual votes can be calculated using opinion survey data or referendum votes, as in this paper. Recent studies in this vein include Portmann et al., (2012), Brunner et al. (2013), Stadelmann et al. (2013, 2014), and Matsusaka (2015).

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Table 1. List of Laws

State	Description	Bill	Referendum	Outcome
Alaska	Permits hunters to use airplanes to hunt wolves	SB 267 (2000)	Measure 6 (2000)	Repealed
California	Permits Pala tribe to operate video lottery terminals	SB 287 (1998)	Prop 29 (2000)	Approved
California	Allows third parties to sue insurance companies for unfair claim practices	SB 1237 (1999)	Prop 30 (2000)	Repealed
California	Allows third parties to sue insurance companies (modifies Prop 30 on same ballot)	AB 1309 (1999)	Prop 31 (2000)	Repealed
California	Requires large companies to provide health care coverage	SB 2 (2003)	Prop 72 (2004)	Repealed
California	Authorizes gambling compact with Pechanga tribe	SB 903 (2007)	Prop 94 (2008)	Approved
California	Authorizes gambling compact with Morongo tribe	SB 174 (2007)	Prop 95 (2008)	Approved
California	Authorizes gambling compact with Sycuan tribe	SB 175 (2007)	Prop 96 (2008)	Approved
California	Authorizes gambling compact with Agua Caliente tribe	SB 957 (2007)	Prop 97 (2008)	Approved
California	Allows North Folk tribe casino in Central Valley	AB 277 (2013)	Prop 48 (2014)	Repealed
Maine	Replaces health insurance claims tax with beverage tax	LD 2247 (2008)	Question 1 (2008)	Repealed
Maryland	Changes voting procedures	HB 1368 (2006)	Question 4 (2006)	Approved
Maryland	Allows illegal immigrants to pay in-state tuition rates	SB 167 (2011)	Question 4 (2012)	Approved
Maryland	Congressional redistricting plan	SB 1 (2011)	Question 5 (2012)	Approved
Maryland	Allows same-sex marriage	HB 438 (2012)	Question 6 (2012)	Approved
Michigan	Allows hunting of mourning doves.	HB 5029 (2004)	Proposal 06-03 (2006)	Repealed

North Dakota	Ends use of "Fighting Sioux" college nickname	SB 2370 (2011)	Referred Measure 4 (2012)	Approved
Ohio	Limits interest rate charged by payday lenders	HB 545 (2008)	Issue 5 (2008)	Approved
Ohio	Limits collective bargaining by public employees	SB 5 (2011)	Issue 2 (2011)	Repealed
South Dakota	Bans smoking in restaurants and bars	HB 1240 (2009)	Referred Law 12 (2010)	Approved
Washington	Increases taxes for unemployment insurance	HB 2901 (2002)	R-53 (2002)	Repealed
Washington	Allows charter schools	HB 2295 (2004)	R-55 (2004)	Repealed
Washington	Prohibits insurers from denying certain claims	SB 5726 (2007)	R-67 (2007)	Approved
Washington	Grants domestic partners same rights as married persons	SB 5688 (2009)	R-71 (2009)	Approved
Washington	Allows same-sex marriage	SB 6239 (20120	R-74 (2012)	Approved

Table 2. Congruence and Responsiveness by Law and Chamber

	Chamber	%Congruent	Responsiveness	
			b	s.e.
AK. Wolf hunting (2000)	Lower	52.6	2.38	1.01
	Upper	47.4	2.26	1.54
	Total	50.9	2.34	0.83
CA. Gambling Pala (1998)	Lower	65.8	-0.38	1.39
	Upper	75.0	-5.88	2.72
	Total	68.3	-1.28	1.24
CA. Insurance claims 1 (1999)	Lower	43.4	2.83	0.59
	Upper	50.0	3.53	0.87
	Total	45.8	3.06	0.49
CA. Insurance claims 2 (1999)	Lower	42.7	3.84	0.79
	Upper	38.9	4.71	1.36
	Total	41.4	4.04	0.68
CA. Health insurance (2003)	Lower	88.5	3.26	0.30
	Upper	100.0	3.93	0.39
	Total	92.4	3.45	0.24
CA. Gambling Pechanga (2007)	Lower	90.0	3.24	0.51
	Upper	74.2	2.23	1.21
	Total	85.1	2.96	0.51
CA. Gambling Morongo (2007)	Lower	90.5	4.29	0.58
	Upper	69.7	1.91	1.28
	Total	83.3	3.53	0.58
CA. Gambling Sycuan (2007)	Lower	90.0	3.24	0.51
	Upper	68.8	1.81	1.33
	Total	83.3	2.86	0.55
CA. Gambling Agua Caliente (2007)	Lower	87.3	3.48	0.61
	Upper	71.9	2.03	1.25
	Total	82.1	3.03	0.58
CA. Gambling North Folk (2013)	Lower	24.5	2.91	1.32
	Upper	33.3	2.21	2.27
	Total	27.9	2.74	1.16
ME. Beverage tax (2008)	Lower	46.8	3.45	0.67
	Upper	48.6	3.36	1.72

	Total	47.1	3.44	0.62
MD. Voting (2006)	Lower	68.6	5.82	0.55
	Upper	90.6	0.23	1.21
	Total	72.8	5.19	0.52
MD. Tuition noncitizens (2011)	Lower	88.5	3.01	0.15
	Upper	93.5	2.82	0.33
	Total	89.7	2.97	0.14
MD. Redistricting (2011)	Lower	75.2	2.85	0.33
	Upper	80.0	2.69	0.61
	Total	76.4	2.81	0.29
MD. Same-sex marriage (2012)	Lower	79.9	3.34	0.35
	Upper	89.4	3.88	0.56
	Total	82.3	3.47	0.30
MI. Dove hunting (2004)	Upper	38.1	4.22	0.51
	Lower	40.5	3.98	1.02
	Total	38.7	4.16	0.46
ND. University mascot (2011)	Lower	67.0	1.99	0.90
	Upper	84.8	-0.63	1.01
	Total	72.9	1.14	0.70
OH. Payday lending (2008)	Lower	73.4	2.44	0.74
	Upper	84.8	1.57	1.11
	Total	76.4	2.26	0.62
OH. Collective bargaining (2011)	Lower	51.5	3.72	0.32
	Upper	51.5	3.30	0.87
	Total	51.5	3.64	0.31
SD. Smoking ban (2009)	Lower	60.9	1.66	0.89
	Upper	60.0	-1.00	1.44
	Total	60.6	0.83	0.77
WA. Unemployment tax (2002)	Lower	41.1	1.98	0.54
	Upper	42.9	2.39	0.72
	Total	41.7	2.11	0.43
WA . Charter schools (2004)	Lower	47.4	3.29	1.93
	Upper	44.9	3.90	2.72
	Total	46.6	3.49	1.56
WA. Insurance claims (2007)	Lower	75.3	1.91	0.43
	Upper	76.6	1.65	0.62

	Total	75.7	1.82	0.35
WA. Domestic partners (2009)	Lower	72.2	2.01	0.29
	Upper	75.0	2.07	0.42
	Total	73.1	2.03	0.24
WA. Same-sex marriage (2012)	Lower	84.7	2.65	0.31
	Upper	85.7	2.62	0.44
	Total	85.0	2.64	0.25
All Laws	Lower	66.5	1.17	0.06
	Upper	68.1	1.19	0.10
	Total	67.0	1.17	0.05

Notes. The unit of observation is a roll call vote. "Congruence" is equal to 1 if a legislator's roll call vote corresponded to the majority vote in the referendum election on the law. Responsiveness is the coefficient *b* from regressing a dummy for a yes vote on the percentage in favor in a district. The units are such that the *b* coefficient indicates the marginal "effect" of a 1 percent increase in district opinion on the probability of a legislator's roll call vote.

Table 3. Congruence and Responsiveness by Groups

	GROUP		
	Law repealed = 1	Upper chamber = 1	Term limits = 1
	(1)	(2)	(3)
Panel A. Congruence Regression			
GROUP = 0 (α)	78.1***	66.5***	71.7***
	(1.8)	(3.7)	(3.4)
GROUP = 1 increment (β)	-29.6***	1.6	-9.6*
	(4.0)	(5.5)	(5.4)
Panel B. Responsiveness Regression (Direct measure of district opinion: %Yes)			
Constant (a)	-0.49***	0.04	-0.22
	(0.12)	(0.10)	(0.16)
%Yes (b)	1.97***	1.17***	1.52***
	(0.21)	(0.18)	(0.25)
GROUP = 1 (c)	0.34**	0.01	0.32
	(0.15)	(0.14)	(0.18)
%Yes × GROUP = 1 (d)	0.03	0.01	-0.29
	(0.31)	(0.24)	(0.31)
Panel C. Responsiveness Regression (Ideology proxy for district opinion: Democrat won last election)			
Constant (a)	0.45***	0.44***	0.31***
	(0.08)	(0.08)	(0.08)
Democrat (b)	0.38***	0.36***	0.53***
	(0.09)	(0.12)	(0.10)
GROUP = 1 (c)	0.03	0.07	0.29**
	(0.12)	(0.11)	(0.11)
Democrat × GROUP = 1 (d)	-0.15	-0.11	-0.39**
	(0.20)	(0.17)	(0.17)

Note. Each column of each panel reports a regression. Panel A reports regressions of congruence on the GROUP dummy. Coefficients are multiplied by 100 to represent percentages. Panels B and C report estimates from linear probability regressions of a vote in favor on %Yes in the district referendum (Panel B) or a dummy = 1 if the Democratic candidate won in the previous legislative election (Panel C) and the GROUP dummy and an interaction. The unit of observation is a roll call vote. Standard errors clustered at the law-chamber level are reported in parentheses. All regressions are based on 3,242 roll call observations. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent.

Table 4. Congruence and Responsiveness by Legislator Characteristics

	CHAR			
	Female = 1	Republican = 1	Majority Party = 1	Last Term = 1
	(1)	(2)	(3)	(4)
Panel A. Congruence Regression				
CHAR = 0 (α)	67.3***	68.0***	79.5***	63.5***
	(0.7)	(3.7)	(5.2)	(0.9)
CHAR = 1 increment (β)	-1.2	-2.3	-20.1**	-5.2
	(2.5)	(8.5)	(8.3)	(3.5)
Panel B. Responsiveness Regression				
Constant (a)	-0.83***	-0.47**	-1.06***	-0.79***
	(0.07)	(0.21)	(0.14)	(0.08)
%Yes (b)	2.88***	2.40***	3.07***	3.18***
	(0.13)	(0.35)	(0.37)	(0.17)
CHAR = 1 (c)	0.17*	-0.33	1.01***	-0.01
	(0.09)	(0.29)	(0.19)	(0.11)
%Yes × CHAR = 1 (d)	-0.26	0.18	-1.44***	-0.05
	(0.16)	(0.53)	(0.45)	(0.21)

Note. Each column of each panel reports a regression. Panel A reports regressions of congruence on the characteristic (CHAR) dummy. Coefficients are multiplied by 100 to be interpreted as percentages. Panel B reports estimates from linear probability regressions of a vote in favor on %Yes in the district referendum and the CHAR dummy and interaction. The unit of observation is a roll call vote. Standard errors clustered at the law-chamber level are reported in parentheses. Each regression includes law-chamber fixed effects. Regression (1) is based on 3,242 observations, regressions (2) and (3) are based on 3,241 observations, and regression (4) is based on 1,597 observations. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent.

Table 5. Congruence and Responsiveness by District Competitiveness

	COMPETITION	
	Vote Margin (1)	Dummy = 1 if Vote Margin < 2.5% (2)
Panel A. Congruence Regression		
Constant (α)	63.9*** (1.6)	66.7*** (0.2)
COMPETITION (β)	0.08** (0.04)	5.6 (4.1)
Panel B. Responsiveness Regression		
Constant (a)	-0.95*** (0.10)	-0.78*** (0.07)
%Yes (b)	3.15*** (0.19)	2.80*** (0.14)
COMPETITION (c)	0.30* (0.14)	-0.36*** (0.13)
%Yes × COMPETITION (d)	-0.58** (0.26)	0.64** (0.26)

Note. Each column of each panel reports a regression. Panel A reports regressions of congruence on COMPETITION. Coefficients are multiplied by 100 to represent percentages. Panel B reports estimates from linear probability regressions of a vote in favor on %Yes in the district referendum and COMPETITION and an interaction. The unit of observation is a roll call vote. Standard errors clustered at the law-chamber level are reported in parentheses. All regressions are based on 3,242 roll call observations. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent.