WHY VOTERS TURN OUT FOR TAX LIMITATION VOTES**

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ABSTRACT

The paper is a sequel to a previous study (NTJ, March 1980) in which we surveyed 2001 Michigan households to see why they voted for or against some tax limitation proposals on the 1978 ballot. Here we use the same survey to explain why voters went to the polls in the first place. We use an iterative least squares technique to estimate a parameter reflecting the propensity of non-voters to vote "yes" on a tax limit amendment, finding that they were approximately as likely to vote for it as "no" voters. Translated, this means that the "true believers" in tax limitation turned out and voted yes, while those not in favor apparently had milder preferences such that the turnout among the latter group was close to random. This supports a variant of what political scientists call the "alienation" theory.

In a previous paper we analyzed the results of a voter survey given right after the 1978 Michigan vote on a series of tax limitation amendments.1 The results of the analysis were, on the whole, disappointing to those who like to model fiscal choices as the outcome of a rational, informed voting process. Even though tax limitation amendment, the Headlee Amendment, passed, there did not appear to be pervasive feeling either that government spending was excessive in Michigan, that the number of public employees was too great or that public wages were too high. Most of the support for tax limitation came from those who felt taxes would be cut even though they did not favor reductions in spending, or from those who felt the amendment would in some undefined way improve governmental efficiency or voter control.

It is not clear what public finance economists can or should do about these findings. It is obviously costly to give complex voter surveys every time an economist wants to model a public choice outcome. But on the other hand economists should not ignore the results either. The usual model of voting behavior simply relates spending demand to voter attributes (income, tax price, age, race, etc.), glossing over what we find to be a host of complications regarding how different people perceive different fiscal packages, and how they then vote.

In this paper we extend the results of this analysis to another aspect of the voting decision, the question of whether or not a potential voter will go to the polls in the first place. This is an issue that has long fascinated political scientists,2 but here we employ methodology more familiar to economists. Again the rational model would say that voters would go to the polls if their benefits of voting outweighed their costs. Hence if voters were relatively indifferent between two fiscal packages, they will be predicted not to vote, while if they have strong preferences on one side or the other, they will be predicted to vote. As with our analysis for voters, the survey of nonvoters as well as voters makes it possible to test more precisely various turnout hypotheses.

This paper begins with a short review of our results for voters. Then we apply the same model to the turnout decisions finding results that are not very favorable to the indifference hypothesis. We conclude with a few checks on our results, and some interpretations of the new findings.

Explaining Voter Preferences

One can imagine three types of models to explain the votes of voters on, say, a tax limitation issue. The first, and most commonly used by economists, will be termed the attributes model, where votes are related directly to attributes of voters. This might be analogous to a reduced form model in other contexts. Then there

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are two ways of building a more structural model—one by asking and using voter preferences on the underlying issue (do voters think that public spending is too high?), and relating the vote to these answers. The next is more complete in that a whole series of preference-perception variables are formed (do voters think public spending is too high? do they think the amendment will reduce public spending?). Such a model, which we term the interactions model, would not differ at all from the preferences model under the usual assumption where voters have identical perceptions about the impact of a fiscal measure, but it obviously could differ if voter perceptions differ and if these perceptions are differently related to preferences.

These models can be dealt with more formally as follows. Assume that voters possess differentiable utility functions defined over public spending on government provided services and private consumption

\[ U_i = U_i(G, C_i) \]  \hspace{1cm} (1)

where \( U_i \) represents the ith voter's utility level, \( C_i \) is private consumption, and \( G \) is the level of government spending, common to all voters. Individuals maximize utility under their budget constraint

\[ Y_i = C_i + P_iG \]  \hspace{1cm} (2)

where \( Y_i \) is the personal income of the voter and \( P_i \), the voter's tax price for government services (spending), the product of the factor price and the voter's tax share (portion of the property tax base, if that is the financing mechanism).

Maximizing (1) subject to (2) yields an individual's demand for this commonly-provided level of government services, \( G_i^* \)

\[ G_i^* = f(Y_i, P_i, S_i) \]  \hspace{1cm} (3)

where we include in addition the vector \( S_i \) to represent a vector of demographic shift variables (does the voter have children in the public schools, live in a high crime area, etc.). If \( G_i^* > G \), the well-informed voter should vote against a tax limitation amendment, if \( G_i^* < G \) this voter should vote for an amendment that reduces taxes and spending, and if \( G_i^* = G \) this voter should vote for an amendment that limits growth in public spending but against one that reduces spending.

One main problem with a theory of this sort is that \( G_i^* \) is usually unobservable, and researchers are forced to use a set of proxy variables such as age, education, race, and sex to explain voting behavior. Letting the \( S_i \) vector also include these demographic proxies, we can use this assumption to write the "attributes" model as

\[ Pr(\text{yes}) = g(S_i, P_i, Y_i) \]  \hspace{1cm} (4)

where \( Pr(\text{yes}) \), the probability that a voter will vote yes on a tax limitation amendment, now replaces \( G_i^* \) as the dependent variable. It would be expected that \( \partial Pr/\partial P_i > 0 \) and \( \partial Pr/\partial Y_i < 0 \).

The preferences model tries to eliminate this uncertainty about \( G_i^* \). Voters are asked whether they favor increases in both public spending and taxes, decreases, or no change. One can either do this qualitatively, as in the usual public opinion polls on this topic, or quantitatively, as we tried to do and described in our earlier paper. Either way the results are hypothetical, since voters do not have to vote on the basis of their answers. But when the quantitative approach is used, one can construct and use an estimate of \( G_i^* \). The second model, the preferences model, uses this variable as follows:

\[ Pr(\text{yes}) = \begin{cases} 
\alpha_1, & \text{if } G_i^* > G \\
\alpha_2, & \text{if } G_i^* = G \\
\alpha_3, & \text{if } G_i^* < G 
\end{cases} \]  \hspace{1cm} (5)

Here \( \alpha_i \) through \( \alpha_3 \) will now depend on the properties of this tax limitation amendment. If, for example, this amendment tries to reduce public spending and taxes and all voters perceive this, \( \alpha_1 = \alpha_2 = 0 \). The probability \( \alpha_3 \) will lie somewhere in the range \( 0 < \alpha_3 < 1 \), depending on how draconian the amendment is com-
pared with the tastes of those in the last group. If the amendment is uniformly perceived as holding spending exactly at its present level, $\alpha_3 = 1$, while $0 < \alpha_1 < 1$ and $0 < \alpha_3 < 1$, depending on how dissatisfied the first and third groups are with present spending levels. There are lots of combinations, especially when we allow perceptions about the impact of the amendment to vary, but for most imaginable straightforward tax limitation amendments it will be true that $\alpha_1 < \alpha_2 < \alpha_3$.

The interactions model can be written formally by assuming voters focus on some outcome of the election, say public spending. If they assume the amendment will raise public spending, we attach a subscript $j = 1$. If they assume the amendment will not change public spending, we attach a subscript $j = 2$. If they assume the amendment will lower public spending, we attach a subscript $j = 3$. We can then repeat the process for other outcome variables, such as say desired public sector wage rates $W^*$, and write

$$
\text{Pr(\text{yes})} = \begin{cases}
\alpha_{1j} G^* > G \\
\alpha_{3j} G^* = G \\
\alpha_{3j} G^* < G \\
\alpha_{4j} W^* > W^* \quad j = 1, 2, 3
\end{cases}
$$

(6)

$$
\vdots
$$

$$
\alpha_{3j} X^* < X
$$

This model then allows voters to have different perceptions about the impact of the election, and to combine these perceptions differently with preferences.

The 1978 Michigan tax limitation amendments presented a nice opportunity to test these various hypotheses. On the ballot was one amendment, the Headlee Amendment, which offered voters a fairly clear public spending choice. The Amendment set a limit of 9.4 percent on the ratio of state government own tax revenues to state personal income (in addition to making some less important changes in local bond voting procedures). Relative to some of the more complex tax limitation amendments, which feature limitations plus tax shifts from one type of revenue to another and one level of government to another, perceptions about the impact of the Headlee Amendment should have been quite uniform. Hence if voters wanted state government spending to rise, they should have voted no on the amendment; and if they wanted state spending to fall or stay the same (relative to income), they should have voted yes. As it happened the amendment passed with 52 percent of the overall vote.

We took a telephone sample of 2001 Michigan households right after the election asking how voters voted and for information on $G^*_t$, $S_t$, $P_t$, $Y_t$ and various perceptions about the likely impact of the amendment. This is all described in the earlier paper and will not be repeated here. We then tried to fit the three different models. The coefficients of both the attributes and preferences model were all in accord with the theory developed above, but here we want to focus on the fit of the equations. Various fit statistics for just the 1028 sample members who voted are shown in Table 1. The first measure is the conventional $R^2$. Since the dependent variable is 1 (if yes), 0 (if no), these $R^2$ measures have an upper limit well below unity and are not terribly meaningful. The second measure is the percent of voters correctly predicted. The third is the percent correctly predicted less that predicted by random chance. Since the Headlee Amendment was voted for by 56 percent in our sample, (indicating the possibility of some selective recall) random chance would indicate that $(.56)^2$

<p>| TABLE 1 |
| FIT STATISTICS OF THE THREE MODELS OF EXPLAINING VOTER BEHAVIOR |
| 1028 HEADLEE AMENDMENT VOTERS |
| Model of Voting Behavior |</p>
<table>
<thead>
<tr>
<th>Fit Statistics</th>
<th>Attributes</th>
<th>Preferences</th>
<th>Interactions</th>
</tr>
</thead>
<tbody>
<tr>
<td>$R^2$</td>
<td>.046</td>
<td>.039</td>
<td>204</td>
</tr>
<tr>
<td>Percent of Votes</td>
<td>Predicted correctly</td>
<td>.628</td>
<td>.591</td>
</tr>
<tr>
<td>Improvement over random assignment</td>
<td>.120</td>
<td>.083</td>
<td>226</td>
</tr>
</tbody>
</table>
would be predicted to vote for the amendment and actually would vote for it, \((.44)^2\) would be predicted to vote against the amendment and actually would, and \((.56)^2 + (.44)^2 = .508\) of the votes would be correctly predicted by random assignment. The difference between .628 and .508 (-.120) is then the improvement over random assignment.

These results are disappointing to adherents of both the attributes and perceptions models. Both models improve on random chance, but not by very much. They also both perform far worse than the more complicated interactions model, even in this Headlee case where the fiscal issue is relatively unclouded. In more complicated fiscal amendments, the differences should be even more pronounced. Using the parlance of the evaluation literature, the marginal costs of asking all the questions needed to implement the interactions model are very high, but so are the marginal benefits of using such a model.

**Explaining Turnout**

Out of our sample of 2001 respondents, 973 stated that they did not vote on the Headlee Amendment. We now try to adapt these models to the turnout decision—why did this many respondents go to the polls?

The literature of both economics and political science turns up three hypotheses to explain non-voting:

a) The random hypothesis. Voting is a random process, and the preferences of a sample of voters can be considered an unbiased estimate of the preferences of the population.4

b) The indifference hypothesis. Non-voters do not vote because their preferences for spending, they are indifferent to the passage of an amendment.5

c) The allocation hypothesis. Non-voters do not vote because their preferences are very different from those of the majority.6

Of the three, the indifference hypothesis fits neatly together with the rational-informed model of voter choice given above. All voters on the margin about an issue, or totally uninformed about the impact of an amendment, would find that their benefits of voting did not outweigh the costs, and they would not vote. It is hard to construct an underlying model that is consistent with the random hypothesis, unless it is that information about the likely impact of the amendment is spread randomly across the population. And it is even harder to construct a theory to rationalize the alienation hypothesis with the usual assumptions about utility functions and taste distributions made by economists. If households have concave utility functions, the cost of any deviation between actual and desired public spending should be a rising function of the deviation, so that more alienated voters should have higher, not lower, turnout rates. Alternatively, the underlying taste distribution must be quite weirdly skewed for nearly one-half of the prospective voters to be so far in the tails that they would be considered "alienated."

The three models are shown graphically in Figure 1. For each model the mean yes probability of the observed sample was .56, as noted above. With the random model, .56 becomes the population mean. For the indifference model, the population mean is about .53, because the half of the population not voting would have an expected yes probability equal to .5 (they were indifferent). Also, the estimated population variance would decline sharply, because all the missing voters are close to the mean. For the alienation model the estimated population variance rises sharply, because the uncounted voters are far from the mean. The population mean in the alienation model could rise or fall, depending on which voters are alienated.

The difficulty with any statistical attempt to explain the behavior of non-voters is that we can never know with certainty how they would have voted had they done so. We must make an assumption that links the underlying behavior of voters to non-voters. The simplest assumption, one we first use and then try
to test, is that non-voters and voters have the same coefficients relating their preferences and perceptions to their propensity to favor the limitation amendment. In extending our voter models to non-voters, we could use any of the models described there, but to conserve space we will utilize only the best-fitting interactions model.

If we assume that the coefficients of the voter model apply as well to non-voters, we can use the data for all 973 non-voters to estimate a model which takes the form:

\[
Pr(\text{yes}) = \begin{cases} 
1 & \text{if voted yes} \\
\gamma \text{ if did not vote} & = Z\beta \\
0 & \text{if voted no}
\end{cases}
\]  

Here \(\gamma\) is a parameter to be estimated which allows the data to determine the scaled value for non-voters in a dependent variable that is best explained by the vector of explanatory variables, \(Z\) is a vector of independent variables, and \(\beta\) a vector of coefficients. With \(\gamma\) chosen to maximize an appropriate criterion, the regression not only gives the impact of explanatory variables on the joint vote, vote yes, vote no, decision, but also tells whether non-voters are more like yes or like no voters. We would expect \(\gamma\) to be close to .5 under the indifference hypothesis, close to .56 (the mean for voters) under the random hypothesis, but substantially different from .5 were non-voters alienated from even voting on the amendment. The estimation technique used does not require \(\gamma\) to lie within the 0–1 range, so that alienation or other explanations could lead to values less than zero or greater than one.

A brief summary of the estimation technique is informative here. Let \(D = (D_Y, D_N, DN)\) represent a vector of three dummy variables indicating the respondent's class (vote yes, not vote, vote no) and let \(\alpha = 1, \gamma, 0\) be a vector of scale parameters. Then the criterion used is to maximize the correlation between \(D\alpha\) and \(Z\beta\). The scale parameter can be estimated using either the technique of canonical correlation or a simpler and cheaper iterative least-squares procedure. We should note that under such a procedure the statistics presented have only large sample
properties, so that the t-statistics are not strictly correct in the sample. The results of the procedure are shown in Table 2. Surprisingly, the all-important \( \gamma \) parameter is estimated to be \(-.108\), implying that the non-voters were slightly less likely to vote for the Headlee Amendment than were no voters. The explanation appears to be that those "true believers" firmly in favor of the amendment voted and voted yes. A larger number had perhaps mild reservations, often voting no but often not caring enough to turn out and vote. Since \( \gamma \) is less than zero, this finding might be interpreted as supporting a weak variant of the alienation hypothesis. But it should be noted that \( \gamma \) is not far away from zero. In terms of preferences, these non-voters are difficult to distinguish from no voters, so it is certainly stretching things to call them an alienated class. A better way to describe our finding is that at least for this tax limitation amendment, the world is made up of true believing yes voters and less intense non-voters and no voters. Getting a tax limitation amendment passed then involves getting the true believing minority to the polls without risking the vote of a large share of the relatively indifferent majority.

Regarding the regression coefficients, the table shows several things. In the left column the variables are numbered for convenience. Next we show the group, whether public employees, private employees, or the aggregate. Next we show perceptions of the amendment, starting with the respondents' view of the most important impact and proceeding to respondents' views on whether the amendment will have a particular effect. The next column lists preferences. The next columns list the number of cases, the coefficient, and the t ratio in this regression. Finally we show, as an addendum, the coefficient in the corresponding regression for voters, the one presented in our previous article.

Interpretation of the regression is much the same as that in the previous article, with one important change. When the model is estimated only for the 1028 voters, the regression constants (to which

<table>
<thead>
<tr>
<th>No.</th>
<th>Group</th>
<th>Perceived Most Important Impact</th>
<th>Perceived that Headlee will</th>
<th>Preferences</th>
<th>Cases</th>
<th>coeff.</th>
<th>t Ratio</th>
<th>Coeff. Voters Only</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>All</td>
<td>Reduce spending</td>
<td>--</td>
<td>More state spending</td>
<td>27</td>
<td>.005</td>
<td>.012</td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>All</td>
<td>Reduce spending</td>
<td>--</td>
<td>Same state spending</td>
<td>105</td>
<td>.211</td>
<td>.462</td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>All</td>
<td>Reduce spending</td>
<td>--</td>
<td>Less state spending</td>
<td>85</td>
<td>.490</td>
<td>.702</td>
<td></td>
</tr>
<tr>
<td>4</td>
<td>Priv.</td>
<td>Reduce spending</td>
<td>--</td>
<td>More state spending</td>
<td>20</td>
<td>.095</td>
<td>.865</td>
<td></td>
</tr>
<tr>
<td>5</td>
<td>Priv.</td>
<td>Reduce taxes</td>
<td>--</td>
<td>Same state spending</td>
<td>121</td>
<td>.210</td>
<td>.413</td>
<td>.129</td>
</tr>
<tr>
<td>6</td>
<td>Priv.</td>
<td>Reduce taxes</td>
<td>--</td>
<td>Less state spending</td>
<td>154</td>
<td>.155</td>
<td>.212</td>
<td></td>
</tr>
<tr>
<td>7</td>
<td>Pub.</td>
<td>Reduce taxes</td>
<td>--</td>
<td>Same state spending</td>
<td>38</td>
<td>.299</td>
<td>.888</td>
<td></td>
</tr>
<tr>
<td>8</td>
<td>Pub.</td>
<td>Reduce taxes</td>
<td>--</td>
<td>Less state spending</td>
<td>16</td>
<td>.494</td>
<td>.333</td>
<td></td>
</tr>
<tr>
<td>9</td>
<td>All</td>
<td>Increase taxes</td>
<td>--</td>
<td>More state spending</td>
<td>16</td>
<td>-.156</td>
<td>-.285</td>
<td></td>
</tr>
<tr>
<td>10</td>
<td>All</td>
<td>Increase taxes</td>
<td>--</td>
<td>Same state spending</td>
<td>48</td>
<td>-.192</td>
<td>-.197</td>
<td></td>
</tr>
<tr>
<td>11</td>
<td>All</td>
<td>Increase taxes</td>
<td>--</td>
<td>Less state spending</td>
<td>35</td>
<td>-.037</td>
<td>-.225</td>
<td></td>
</tr>
<tr>
<td>12</td>
<td>All</td>
<td>Hurt schools</td>
<td>--</td>
<td>More school spending</td>
<td>25</td>
<td>-.081</td>
<td>-.084</td>
<td></td>
</tr>
<tr>
<td>13</td>
<td>All</td>
<td>Increase govt. efficiency</td>
<td>--</td>
<td>Govt. waste much</td>
<td>31</td>
<td>.347</td>
<td>4.1</td>
<td>.358</td>
</tr>
<tr>
<td>14</td>
<td>All</td>
<td>Increase govt. efficiency</td>
<td>--</td>
<td>Govt. waste some</td>
<td>30</td>
<td>.242</td>
<td>2.8</td>
<td>.261</td>
</tr>
<tr>
<td>15</td>
<td>All</td>
<td>Increase voter control</td>
<td>--</td>
<td>Trust govt. usually</td>
<td>53</td>
<td>.443</td>
<td>6.7</td>
<td>.391</td>
</tr>
<tr>
<td>16</td>
<td>All</td>
<td>Increase voter control</td>
<td>--</td>
<td>Trust govt. little</td>
<td>50</td>
<td>.437</td>
<td>6.6</td>
<td>.441</td>
</tr>
</tbody>
</table>
these coefficients are added or subtracted) are .525 for private sector voters and .452 for public sector voters. Since \( \gamma \) is estimated to be close to zero, the new regression constants reflect the influx of 976 "virtual" no voters to the sample. This lowers the overall probability of a yes vote from 578/1028 = .562 for voters to 578/2001 = .289 for the population. All regression statistics reflect this large influx of virtual no voters. The constants have dropped sharply, yielding a much lower probability of voting yes for the omitted class. The sum of the constant and the coefficient, or the probability the class will vote yes, drops for every single variable, reflecting the influx of virtual no voters. And in cases where the influx is very large, the probability drops very sharply. To take one example, for variable 18, there are 200 voters who did not know what the most important impact of the Headlee Amendment would be, with an overall probability of voting yes of .47 for private employees and .40 for public employees. With the non-voters included,
there are now 657 respondents who did not know the most important impact, an influx of 447 of the 973 nonvoters. Now the overall probability of voting yes is only .06 for private employees and .15 for public employees. Apart from this across the board shift in the yes vote probabilities, the relative structure of the coefficients is much the same between the voter and population regression, with a mean absolute difference in coefficients of only .006.

It is also more difficult to interpret the fit statistics. The overall $R^2$ drops slightly, apparently reflecting the addition of a less predictable group of respondents. Since the average yes vote probability has dropped, the predicted mean even for actual yes voters falls below .5, with the consequence that the regression is much less likely to predict that an actual yes voter voted yes. Hence the proportion of votes correctly predicted drops noticeably, though the regression still does much better than random assignment even with this bias in predicting the vote of voters that is introduced by the influx of virtual non voters.

How valid was our assumption of identical preference structures for voters and non-voters? Obviously, we cannot fully answer the question because we are only imputing the vote of non-voters and we do not have an actual event on which to base predictions. However, we did a number of experiments which suggest that the assumption is a reasonably good one and that the results are likely to be robust.

To see if the assumption that the coefficients are the same for voters and non-voters is valid, we first focus on the coefficients of Table 2. We reestimated the interactions model using a full canonical correlation procedure, but with a few insignificant explanatory variables dropped to reduce computational difficulties. The first weighted average of interaction variables, i.e., the first canonical variate, explained 69 percent of the total variance, with the coefficients close to those in Table 2, and the second variate explained the remaining 31 percent. Had the structure of tastes of voters and non-voters been identical (and a linear function of the independent variables) the first variate would have explained 100 percent of the explained variance, and the second variate none. Thus, the fact that our model—essentially equivalent to the coefficients associated with the first canonical variate—explains more than 2/3 of the variance in the dependent variable provides mild (though not decisive) support for our assumption of identical voter-non-voter tastes.

We can test the assumption that the coefficients are the same for voters and non-voters further with a second approach that focuses on predictive power. We used the procedure of multiple discriminant analysis to see how well we could classify respondents as non-voters, yes voters, or no voters. We then compared the number of correct classifications associated with the regression of Table 2 to the number of correct classifications associated with the full multiple discriminant analysis model. Using the assumption of equal prior probabilities of classification, our model predicted 51 percent of the classifications correctly. This compares rather favorably with the 53.7 percent of correct classifications using multiple discriminant analysis. When we set prior probabilities equal to the sample probabilities, the discriminant analysis model predicted 57.9 percent of the individuals correctly, but our model correctly classified 56.1 percent. Clearly the assumption of equal coefficients does not lose much predictive power.

Hence the model described by Table 2 appears to hold reasonably well when evaluated either in terms of its coefficients or its fit. But our main interest in the table is the estimate of $\gamma$, the parameter that enables us to distinguish the various nonvoting hypotheses. We have checked this in another way by reestimating the Table 2 regression controlling for some attributes of turnout. A preliminary regression just explaining turnout turned up the following 5 variables that appear to be positively correlated with turnout:

a) family income
b) Republican party identification
c) age
d) education.
e) white race
f) homeowner status
g) non-recipient of transfer payments

The list appears to be logical, and does correspond with variables identified in the political science literature.

We then added these important turnout attributes as control variables to our tastes-perceptions interactions specification given in Table 2. We conserve space by not showing the coefficients of the 64 independent variables here. Three-fourths of the turnout attributes remained significant, all had the expected signs, and none of our interactions coefficients changed importantly. But most significantly, the estimate of \( \gamma \), the scale parameter identifying the non-voter hypotheses, fell from \(-1\) to \(-1.3\). Thus, to the extent that our prediction about the preferences of nonvoters is biased because of a failure to account for the key determinants of turnout, the bias is a conservative one. Non-voters look more alienated when their turnout attributes are controlled for.

**Implications**

Regarding voter behavior, we found that individual attributes and spending preferences are rather poor proxies for the actual vote of an individual, and do not predict votes much better than a random prediction. A significant improvement in fit is found only when an interactions model is adopted, permitting an examination of the relationship between various perceptions about the impact of the amendment and preferences for more or less government spending.

For non-voters, our analysis strongly implies that the turnout decision is not random and independent of preferences: non-voters have different values for the independent variables and a different distribution of predicted votes. However the test is made, we confirm some variant of the alienation hypothesis. But although this hypothesis is confirmed, we do find it to be at least slightly misnamed. Our results suggest that those not voting on tax limitation amendments would have had a very low probability of voting yes if they had voted, even lower than that for no voters. There are not great differences between the preference-perception interactions of non-voters and no voters, but there are between non-voters and yes voters. The model suggested by this is that the mysterious interactions of perceptions and preferences yields a set of true believers in tax limitation—those who vote yes—and a set of uninformed, uncertain, and indifferentagnostics. The former group shows up at the polls and votes for the amendment, while the latter group either does or does not show up, depending on a set of qualifiable attributes that are essentially independent of preferences and perceptions. Other things equal, high turnout rates should then lower the probability that a tax limitation amendment will pass.

In operational terms, it is probably not very helpful to say that the models which may have to be used to explain voting or non-voting behavior are oversimplified and will not work as well as the more complex models used here. Data constraints are data constraints, and researchers who only have data on attributes should do the best possible under the circumstances. At the same time, the more detailed survey analysis described here shows that there is a very high payoff in constructing more detailed electoral models and doing more careful survey work in order to understand why fiscal amendments (or other topics of voting) passed or failed, were popular or unpopular, and how they were or were not perceived.

**Footnotes**

*The work reported here was funded by grants both from the Department of Housing and Urban Development and the National Science Foundation. We have benefited from discussions with Paul Courant, our co-author on the earlier paper, and the comments of Robert Axelrod, John Chamberlin, Albert Covor, Richard Curton, John Kington, Greg Marcus, and Perry Shapiro.


*A long list of such papers are summarized by Robert T. Denton, "Private Choice and Collective Outcomes: Evidence from Public Sector Demand Stud-

A starting value or guess for $\gamma$ can be chosen arbitrarily or can be determined by regressing $DY$ and $DN$ on $Z$ and $DNV$. The coefficient of $DNV$ gives an estimate of $-\gamma$. Given a guess for $\gamma$, one can then regress $D\tilde{a}$ on $Z$ where $\tilde{a} = (1, \gamma, 0)$. The estimated coefficient vector $\beta$ is then used to determine a new estimated value for $\gamma$, calculating $Z\beta$ where $Z$ is a $3 \times k$ matrix whose rows are means of $Z$ within each of the three voting categories and $k$ is the number of explanatory variables. When appropriately normalized (so that vote yes = 1 and vote no = 0), the procedure yields a new value for $\gamma$. One can then iterate until the assumed and estimated values of $\gamma$ converge. It can be shown that convergence occurs at the canonical correlation solution. We could not in fact run a canonical correlation program with such a large number of independent variables, but we did affirm that it gave identical results in a stripped-down variant of the interaction model of Table 2.

The prediction matrix using the Table 2 regression was obtained by assuming equal probabilities of classification. The cutoff points were then determined to lie midway between the mean predicted values for each of the three voting groups.

<table>
<thead>
<tr>
<th>Predicted Vote</th>
<th>Actual Vote</th>
</tr>
</thead>
<tbody>
<tr>
<td>non no yes sum</td>
<td>534 227 212 873</td>
</tr>
<tr>
<td>no 203 122 115 450</td>
<td></td>
</tr>
<tr>
<td>yes 99 123 356 575</td>
<td></td>
</tr>
<tr>
<td>sum 836 452 683 2001</td>
<td></td>
</tr>
</tbody>
</table>

The prediction matrix using multiple discriminant analysis and equal prior probabilities was:

<table>
<thead>
<tr>
<th>Predicted Vote</th>
<th>Actual Vote</th>
</tr>
</thead>
<tbody>
<tr>
<td>non no yes sum</td>
<td>605 268 200 973</td>
</tr>
<tr>
<td>no 133 224 93 450</td>
<td></td>
</tr>
<tr>
<td>yes 90 140 345 578</td>
<td></td>
</tr>
<tr>
<td>sum 731 652 688 2001</td>
<td></td>
</tr>
</tbody>
</table>

The statement in the text focuses on the square of the probabilities along the principal diagonal of these matrices.