You Want to Spend My Money How? Framing Effects on Tax Increases via Ballot Propositions

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Abstract

Recent scholarly work has discovered that modest changes in the framing of the titles and summaries of ballot measures can have dramatic effects on voter approval. This work expands upon these findings by exploring the effect of language specificity on support for ballot propositions that require the voter to pay for the measure with tax dollars. Although extensive research has explored ballot measure language complexity (e.g., position on the ballot, electoral effects, and prepossessed knowledge have all been shown to play a role in the outcome for propositions), left unanswered is the role of detailed language in altering support. Utilizing original experimental data, this work explores the framing effects of increasing specificity of proposed use of tax expenditures on support for ballot questions. Ultimately, this research finds that propositions providing more information to voters substantially increases the likelihood of support for those measures. Moreover, this increased specificity also bolsters certainty as to how the money will be spent, and intensifies how strongly voters feel about the issues being considered.

Introduction

In April of 2011, citizens of Pike County, Missouri, were asked to add a fee of $1.50 per ton of waste disposed in the Pike County landfill. This new tax would have resulted

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in an average tax increase of $3 annually. The measure was overwhelming defeated (80% opposed). Almost a year to the day later, voters in Kearney City (Clay County, Missouri) were asked to increase the current waste collection fee by a rate of $1.75 *a month*. The Kearney City measure passed with a solid 58% affirming. The only notable difference between the two measures, besides the amount of the tax, was language detailing how the revenue would be utilized.

Pike County told voters the funds would go toward waste "development projects," while Kearney City detailed that it too would provide solid waste disposal projects, and that it would further utilize the funds to add "curbside recycling." This raises a question "can minor changes to spending specificity account for such disparities?" This article attempts to shed light on this question by exploring the role of ballot language framing effects on support for propositions.

In recent years, ballot measures have become commonplace.[1] In 2016, 35 states featured a total of 162 statewide ballot questions; 72% of which were approved by voters.[2] Every state allows their legislature to place a measure on the ballot, and all but Delaware permit legislatively referred amendments to a state's constitution. In addition, 18 states allow for citizens to directly amend the state constitution though a signature-gathering process, and 21 states allow citizen-led statutory propositions.[3] Although many states feature ballot measures, the requirements to get an item added to an official state ballot vary from state to state. States allowing citizen-derived measures require a number of signatures in support of the measure; the number required is usually a function of the percent of the number of votes cast in the previous gubernatorial election, or are determined at a fixed number by the state legislature (Matsusaka 1995).

Given their pervasiveness, it comes as no surprise that there has been considerable research surrounding ballot propositions (see Smith and Tolbert 2007 for a comprehensive summary). Language complexity (Bishop, Tuchfarber, and Oldendick 1978; Gafke and Leuthold 1979; Reilly and Richey 2011), position on the ballot (Marcinkiewicz and Stegmaier 2014; Matsusaka 2015), party effects (Branton 2003; Hamilton and Ladd 1996), election timing (Anzia 2011; Gong and Rogers 2014; Meredith 2009), and prepossessed knowledge of the measures themselves (Bowler and Donovan 1994; Nicholson 2003) have all been shown to play a role in proposition outcomes. Similarly, ballot questions have also been shown to alter the fiscal behavior of a state, albeit with conflicting results. Finally, states offering ballot measures alter the behavior of state governments, such as being more likely to constrain their state legislators (Tolbert 1998), and imposing supermajority requirements and tax expenditure limits (Bowler and Donovan 1995).

There has also been a notable amount of scholarly attention devoted to the exploration of the taxpayer paradox: wanting something for nothing. Following Sears and Citrin's (1982) seminal work showing Californians demanded increased spending on social services, while simultaneously complaining their taxes were too high, there have since been a number of authors that have worked to shed light on this seemingly contradictory desire (see Page and Shapiro 1992; Sanders 1988; Welch 1985; Winter and Mouritzen 2001).

Left to be explored is the intersection of ballot and taxation literatures; namely, the role of language in altering support for ballot measures calling for an exaction, and how specificity affects voter interest and strength of support. That said, recently Hastings and Cann (2014) have advanced this line of research by examining ballot title language. Burnett and Kogan (2015) extended this further by exploring the effect of changes to ballot title, summary language, and interest group endorsement. Despite these achievements, such efforts have only begun to fully explore framing effects and ballot language. By relying on original experimental data, this article extends the aforementioned research by analyzing changes in ballot language for measures requiring taxpayer exactions.

This article begins by situating the research at the intersection of the two aforementioned literatures: citizen responses to ballot measures via linguistic framing effects, and the sparse research on taxes via ballot measures. This article proposes a simple theory that helps explain why certain measures requiring levies prevail where similar measures fail. Namely, that minor changes in spending specificity has dramatic effects on acceptance, even when tax amount and language complexity are held constant. Next, I introduce three hypotheses regarding ballot language specificity, and its effect on voter support and voter confidence. Then I discuss my data gathering efforts, which consist of two original experiments manipulating the wording specificity of four ballot measures. Finally, I present findings from the experiments and discuss the implications of this work.

Theory and Hypotheses

Ballot Effects, Information Dissemination, and Citizen Comprehension

During elections featuring ballot propositions, citizens primarily turn to media to become informed; correspondingly, these measures increases voter attentiveness (see Barabas and Jerit 2009; Druckman, Peterson, and Slothuus 2013; Jacob 2017, 31–33; Wettstein 2012). Ballot measures also have a reciprocal effect on media coverage. Studies have verified that voter turnout increases with greater media coverage of ballot questions (Lacey 2005; Smith 2001), and that this results in increased turnout in elections with this measures (Tolbert, Bowen, and Donovan 2009; Tolbert and Smith 2005). Others, such as Biggers (2011), take a more nuanced position, arguing that increases in turnout are due to the prevalence of social issues. Nicholson (2003), similarly, found that a combination of factors, including campaign spending, media coverage, voter fatigue, and issue areas on the ballot all successfully predict awareness of propositions. This coincides with increased campaign spending for social issues, as well as greater public interest, since many social issues appearing on ballots are salient issues (e.g., same-sex marriage, marijuana legalization, etc.). Kuklinski and Jerit (2001, 349) gave an example of this behavior when referring to the decision of small-town residents to build a new school: "[t]hey see that the existing school is overcrowded, that continuing to use it will require costly repairs; [they] participate in a deliberative process. After the school is built, they observe problems directly." In

other words, when faced with a decision regarding a ballot measure with little prior information, citizens pull on accessible, direct information in an attempt to make an informed decision.

Despite this increased media coverage, on the whole, for most voters "information surrounding ballot measures is often quote poor, leading many voters to rely on the short title and summary . . . that appears on the ballot" (Burnett and McCubbins 2013, 1594). As this low-cost information is also the most likely to comprise considerations, it is the most easily accessed by voters and the source of tremendous influence (Zaller 1992). That said, attitudes need not be coalesced to the point of solidity; people need only indicate their dislikes and preferential boundaries to cast a vote (Diamond 2001).

In addition to awareness, questions surround citizen comprehension; specifically, how likely is the average citizen to understand the questions presented on the ballot? Although many states require ballot language to be simple and concise, even clear language cannot increase knowledge of the political process or heighten external efficacy. Lack of readability has been shown to negatively affect the likelihood of voting for a ballot measure (Michalski and Guile 1990), while higher levels of education have proved to bolster support for direct democracy overall (Collingwood 2012). All of this, of course, presupposes voters are aware of ballot measures prior to arriving to the voting booth; as previous work has shown, this is often not the case (Burnett 2013; Burnett and Kogan 2015; Matsusaka 2005).

One consequence of inadequate comprehension is item omission. Not all citizens choose to vote when presented with a ballot measure. In fact, nonvoting on state legislative ballot measures is often in excess of a quarter of all voters (Magelby 1984). Although the use of simple language, as well as reminding voters of their omissions, can decrease the occurrence of nonvotes, studies attempting to understand voting behavior should afford participants the opportunity to abstain to mimic real-world conditions.

The Framing of Ballot Measures

Early exploration of framing effects (Kahneman, Slovic, and Tversky 1982; Levin, Schneider, and Gaeth 1998; Tversky and Kahneman 1981; 1982) have consistently shown that even modest modifications in the presentation of information can have dramatic effects on individual judgments. As Druckman (2004, 671–72) pointed out, numerous studies of equivalency frames (reception of the "same information in either a positive or negative light") have shown that "[s]eemingly innocuous changes in [descriptions can] alter preferences." Similar effects can also be seen when senders of information orient a message in such as way to draw attention to a subset of related considerations. These "issue frames" (Druckman 2001), while different from equivalency frames, may nonetheless draw similar considerations into the minds of those receiving the message. Slight variations in the presentation of information can have radical effects on individual responses by altering underlying attitudes (see Jacoby 2000; Nelson and Kinder 1996; Zaller 1992).

Early studies of the effect of ballot language on government expenditures largely focused on whether state spending changed under a ballot measure regime, rather than

proposition language. For example, Zax (1989) found states that permitted citizen propositions saw an increase in expenditures; Matsusaka (1995) found that expenditures shrank in the face of ballot measures; Farnham (1990) found no relationship at all. A dispute that was later resolved by recent work on policy congruence (Lax and Phillips 2009; Matsusaka 2010).

These aforementioned studies did not attempt to investigate individual voter considerations on expenditures. More often, the proposition literature has focused on awareness and supporting for ballot measures. This is frequently achieved by asking respondents "[h]ave you heard or read anything about Proposition__, which is also called__" (Nicholson 2003, 404). However, the use of such overarching language omits the subtleties embedded in the language of ballot measures. In addition, awareness assessments may downplay the importance of proposition language by only featuring the title, which may not be enough to serve as a heuristic.

Recent work by Hastings and Cann (2014) and Burnett and Kogan (2015) has done much to advance our understanding of ballot language framing and voter support. As mentioned above, Hastings and Cann (2014) manipulated the titles of ballot measures to determine if subtle differences in the framing resulted in meaningfully different electoral results; the authors found they did. Burnett and Kogan (2015) manipulated the language used in ballot titles and language, along with elite endorsements, for two prominent ballot measures. The authors found both language and endorsements had a significant effect on support. Despite these notable advancements, several issues remain unanswered: do the same gains in support exist for equivalency frames as they do for emphasis frames; do similar results hold for minor issues, or solely salient issues like "California's Proposition 8 and Colorado's Amendment 7 [which] were significant enough to attract substantial public debate and attention from major media organizations" (Burnett and Kogan 2015, 116); do changes in wording solely effect support, or do linguistic changes also alter voter interest and certainty about the overall purpose? The proceeding questions are explored next.

Taxes and Direct Democracy

Research on taxation has long known of the voter paradox of wanting something for nothing (Sears and Citrin 1982). Therefore, it is not surprising that many measures asking for citizens to pay a price meet with reluctance. For example, of the 10 state ballot measures in 2012 that asked voters to pay an additional tax, only 50% passed.[4] Why do some measures succeed while others fail? There have been several proposals attempting to explain the variation in adoption. Pearson (2014, 1279) identified three mechanisms for rejection: "mobilization of interest groups, the information available to voters about a policy, and how the costs and benefits of a policy appear to voters." Alternatively, Ventry (2011) claimed objections to taxes are on the supply-side, that is, Americans are displeased with what they receive in return for the investment. Sociological research has attempted to create a verifiable theory to explain these discrepancies. It suggests the answer lies in visibility; more prominent exactions (e.g., income, property, and capital gains) are more likely to be rejected, while incremental

taxes (taxes paid over time, rather than all at once) are more acceptable (Campbell 2009; Campbell and Morgan 2005; Prasad 2012; Wilensky 2002). This is keeping with political science research that found that many Americans prefer government to be inconspicuous (Balogh 2009; Hibbing and Theiss-Morse 2002); this also explains why bond referenda enjoy greater support (Muir and Schneider 1999). Psychology has suggested individual and group differences may provide an answer. Those who pay larger shares of a particular tax tend to view these levies more negatively (Campbell 2009). Finally, at the group-interest level, research has confirmed in-group considerations surrounding tax opinions; that is, increased favorability for exactions that benefit similarly situated peers (Kidder and William Martin 2012; Lieberman 2001).

For the experiments at issue here, this means we should expect to see subjects averse to increased spending, difficult to persuade subjects to vote yea, and to find acceptance exactions solely if respondents believe they are receiving a return on their investment. From a quantitative perspective, this also means that should language specificity effects exist, results will be *more* difficult to detect.

Reconciling the Ballot Measure and Framing Literatures

The ballot proposition literature has primarily focused on aggregate effects of measures across various issues and over time, but this has often come at the expense of individual-level considerations. For its part, the framing literature has provided a blueprint for the effects of the wording of plebiscites and ballot design. What remains, as Rasinski's (1989, 394) study of wording effects on government spending noted, is our lack of "understanding about when and why such [wording] effects occur." As mentioned, Hastings and Cann (2014) and Burnett and Kogan (2015) laid the groundwork for this line of research by looking at changes in ballot titles, language, and interest group endorsement. This work attempts to expand on their findings.

Citizens are hesitant to fund government projects when there is uncertainty regarding waste, and are generally reluctant to pay new taxes. In addition to being averse to spending, voter uncertainty about these levies drives opposition. Voters often fail to see the connection between the requested tax and the policy outcome; this lack of personal attachment weakens overall support. The argument advanced here emphasizes the effect framing has on support for ballot measures; specifically, that with greater specificity of how exactions will be spent, we should see a corresponding increase in support for those measures. Moreover, this article proposes that providing greater details regarding how tax dollars will be spent will increase voter confidence and strengthen voter opinions about the ballot issue. This is not due to a change in preferences, but by drawing different considerations into the minds of voters at decision time (Zaller 1992). Although the changes in wording are often subtle, aforementioned research on manipulations of ballot titles have uncovered profound effects.

This leads to three hypotheses:

Hypothesis 1: Ballot measures requiring voters to pay a tax will find greater support if the measure includes specific language regarding how the funds will be spent, all things being equal.

Hypothesis 2: Greater language specificity on ballot measures that raise taxes will increase voter certainty of how the government will spend the new funds, *ceteris paribus*.

Hypothesis 3: Greater language specificity of how new tax dollars will be spent will increase how strongly voters feel about that issue, *ceteris paribus*.

To test these hypotheses, I conduct a series of between-group experiments where participants are randomly assigned ballot measures that require voters to pay a tax in exchange for a government service with varying levels of language specificity. The next section explains the first of these manipulations.

Research Design, Data, and Results

To assess the effect of language specificity on ballot support, two experiments were conducted. The first was in the fall of 2012 on the Florida State University (FSU) campus, and relied entirely on a student sample. The second was conducted in the spring of 2013, and relied on a combination of FSU students, and online participants drawn from a pool of volunteers from Amazon's Mechanical Turk (MTurk).[5] Student subjects were recruited from various political science courses by offering extra credit in exchange for participation. MTurk participants were enlisted by offering monetary compensation. Recent work investigating reliability of MTurk samples has found that MTurk results are generally consistent with alternative means of data collection (Bates and Lanza 2013). Buhrmester, Kwang, and Gosling (2011, 5) confirmed that the results from MTurk subjects "met or exceeded the psychometric standards associated with published research." Moreover, following extensive work analyzing the pool of subjects in MTurk, Berinsky, Huber, and Lenz (2012) concluded that "MTurk subjects appear to respond to experimental stimuli in a manner consistent with prior research," and that to date MTurk subjects are "not currently an excessively overused pool, and habitual responding appears to be a minor concern" (Berinsky, Huber, and Lenz 2012, 366).[6] The two recruitment efforts resulted in samples of 341 and 689 (254 of which were MTurk subjects), respectively.

Experiment 1

When presented with actual ballots, voters are often asked to vote on several candidates and proposals on a single ballot. However, given the initial uncertainty surrounding multiple propositions, the first experiment ensures that each subject was presented with only one ballot measure. Subjects in the fall 2012 experiment were divided into two groups: one group was presented with a ballot measure dealing with waste disposal, and the other with a measure regarding school funding. Next, subjects were subdivided into treatment (specific language) and control (unspecific language) groups. All subjects were given the following preface: "[m]any states offer ballot initiatives to allow voters to directly determine policy. We would like to know how you would feel about the following initiative if it were applied to where you live," which was followed by one of four vignettes.

Waste Disposal, Unspecific

Shall Leon County increase its residential solid waste collection fees by $1.75 a month? If the measure is approved, this would increase current fees to $10.75 for residential rates, $8.75 for senior citizen rates, and shall apply to all residentially zoned dwellings within greater Leon County beginning in the upcoming fiscal year: 2013/2014.

Waste Disposal, Specific.

Shall Leon County increase its residential solid waste collection fees by $1.75 a month *to fund current and future maintenance and operations, including operational and capital reserves and capital needs of the County system of solid waste disposal?* This would increase current fees to $10.75 for residential and $8.75 for senior citizen rates.

The italicized text in the specific treatment identifies the additional language that differentiates it from the unspecific condition (the actual treatment did not feature italicized text). Readers will also notice length of the ballot measures appear identical, they are. Previous research has shown that longer measures are likely to have greater roll-off. This could act as a potential confounding factor. To control for this, the first vignette featured ballot measures that have the same number of words (53) in each proposal.[7]

Subjects in the school funding group were given the same preface as above and were exposed to one of the following proposals[8]:

School Funding, Unspecific

The Board of Directors of your school district adopted Resolution 2012/2013-14 concerning a proposition for a capital projects levy. The school district will use the excess levies from this proposition, which will apply to all taxable property within the school district: Collection Years: 2013-2016; Levy Amount:

$1,900,000; Approximate Levy Rate/$1,000 Assessed Value: $0.61.

School Funding, Specific

The Board of Directors of your school district adopted Resolution 2012/2013-14 concerning a proposition for a capital projects levy. *This proposition authorizes the district to undertake major roof repairs to schools and facilities, upgrade computer technologies, replace the central kitchen facility, add classrooms, and upgrade fire alarm systems*; and authorizes the following excess levies for such purposes on all taxable property within the school district: Collection Years: 2013-2016; Levy Amount: $1,900,000; Approximate Levy Rate/$1,000 Assessed Value: $.61.

Both the waste and school questions were taken from real measures that appeared on ballots in different states; the unspecific vignettes above attempt to mimic the language as closely as possible. Both parings of vignettes have the same tax values to

ensure any treatment effects were not a result of the size of the tax. Finally, any county, city, or district names were changed to mimic real-world measures.[9] After being presented with the measure, subjects were asked how they would vote: yes, abstain, or no.

To test the second and third hypotheses, all subjects were asked two follow-up questions. To determine the effect of ballot specificity on vote certainty (Hypothesis 2) subjects were asked "[h]ow certain are you about how the money raised by the initiative will be spent?" Subjects were asked to respond on a five-point Likert-type scale ranging from *not at all certain* to *extremely certain*. To assess how the vignettes affected issue strength (Hypothesis 3), respondents were asked "[h]ow strong are your feelings about the issue on this ballot initiative?" This was measured on the same five-point scale. Summary statistics for this and subsequent experiments can be found in the online appendix.

To confirm randomization of assignment, a number of ancillary tests were conducted. The online appendix features a model that regresses demographic control variables on the treatment and control conditions; the regression results provide strong support that the subjects were successfully assigned randomly. Moreover, although random assignment should prevent condition-specific effects, including party effects, all models were estimated with a seven-point party identification variable to ensure consistent results. The results remain substantively unchanged (see the online appendix).

One obvious limitation to this (and all) experimental designs is the amount of time respondents are exposed to relevant political information and the absence of counter-framing effects (Chong and Druckman 2013). Subjects presented with a ballot measure for the first time are forced to make a top-of-the-head response based solely upon considerations related to the information presented; real-world ballot measures may give voters considerably more time to make a decision. That said, there is ample evidence that proposition voters often wait until the eve of the election, or later, to finalize their decision (Lee 1978). Ultimately, presenting subjects with a measure, and then expecting an immediate response is not entirely different from the way many voters approach most direct democratic efforts, especially nonsalient issues.

Critics may inquire if these contrasting proposals constitute the choice between two different laws rather than framing effects. A few points of clarification are in order. First, the specific frames are taken from actual ballot measures (see the online appendix for original language), meaning states and localities are already self-imposing spending guidelines in their ballot language. Second, equivalency frames, like those at issue here, must compare equal gains and losses (Druckman 2004). The amount of the levy here remains constant, only the additional information about the allocation is manipulated. Finally, skeptics are right to identify the use of more restrictive language seemingly ties the hands of public officials, but excises levied to pay for particular policy goals can also be used to free up resources to be spent elsewhere.

Results: Experiment 1, Fall 2012

The first experiment explored the effect of ballot language specificity on support for measures that would increase taxes for two issue areas: waste disposal and school

Table 1. Multinomial Probit Models of Support for Waste Disposal and School Funding Measures.

	Waste disposal	Waste disposal	School funding	School funding
	"Yes"	"No"	"Yes"	"No"
	Coefficients (*SE*)	Coefficients (*SE*)	Coefficients (*SE*)	Coefficients (*SE*)
Waste disposal (specific language)	1.078*** (0.317)	0.125 (0.319)		
Schools funding (specific language)			1.610*** (0.295)	0.910** (0.348)
Constant	−0.063 (0.219)	0.315 (0.207)	−0.897*** (0.211)	−1.337*** (0.243)
Log-likelihood	−173.623	−173.623	−153.023	−153.023
N	170	170	171	171

Note. Multinomial probit, abstain as reference category. Dependent variable is vote on the measure: yes, no, or abstain. Standard errors in parenthesis.
†$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$. (two-tailed)

funding. After reading the ballot measure vignette, student subjects were asked to vote yes, abstain, or no. Table 1 shows the results of a multinomial probit model for the ballot questions where the dependent variable is the vote choice.[10]

Consistent with the first hypothesis, when subjects were given specific language about how their tax dollars will be spent, we see a significant increase in the probability of voting yes. This proved true for both the waste disposal (column 1) and school funding (column 3) measures.

Given the unintuitive nature of multinomial probit coefficients, Figure 1 visualizes the findings of Table 1 by looking at the predicted probably of voting yes, abstaining, or no. As the figure shows, subjects receiving the unspecific vignette (hollow circles) strongly opposed the waste measure (44% voting no, 29% abstaining; top figure), but were largely ambivalent on the school support measure (12% voting no, 66% abstaining; bottom figure). When provided with specific language about how the taxes will be utilized (solid circles), we see sizable changes in support. In fact, for both for waste disposal and support for school funding we see voting pluralities becoming majorities: moving from 27% to 56% voting yes for waste disposal, and from 22% to 61% voting yes for school funding. Finally, as both the specific and unspecific waste disposal vignettes had the same number of words, the robust findings suggest that it is the substance of the frame that is driving voter behavior, rather than simply the length of the measure.

The second and third hypotheses predicted that frames with greater specificity would also increase certainty about how the funds will be spent, and make voters feel more strongly about the issue. Table 2 reveals the results of four ordered probit regressions. The first two models (columns 1 and 2) look at certainty of how the tax dollars would be used (measured on a five-point Likert-type scale with 5 being *extremely*
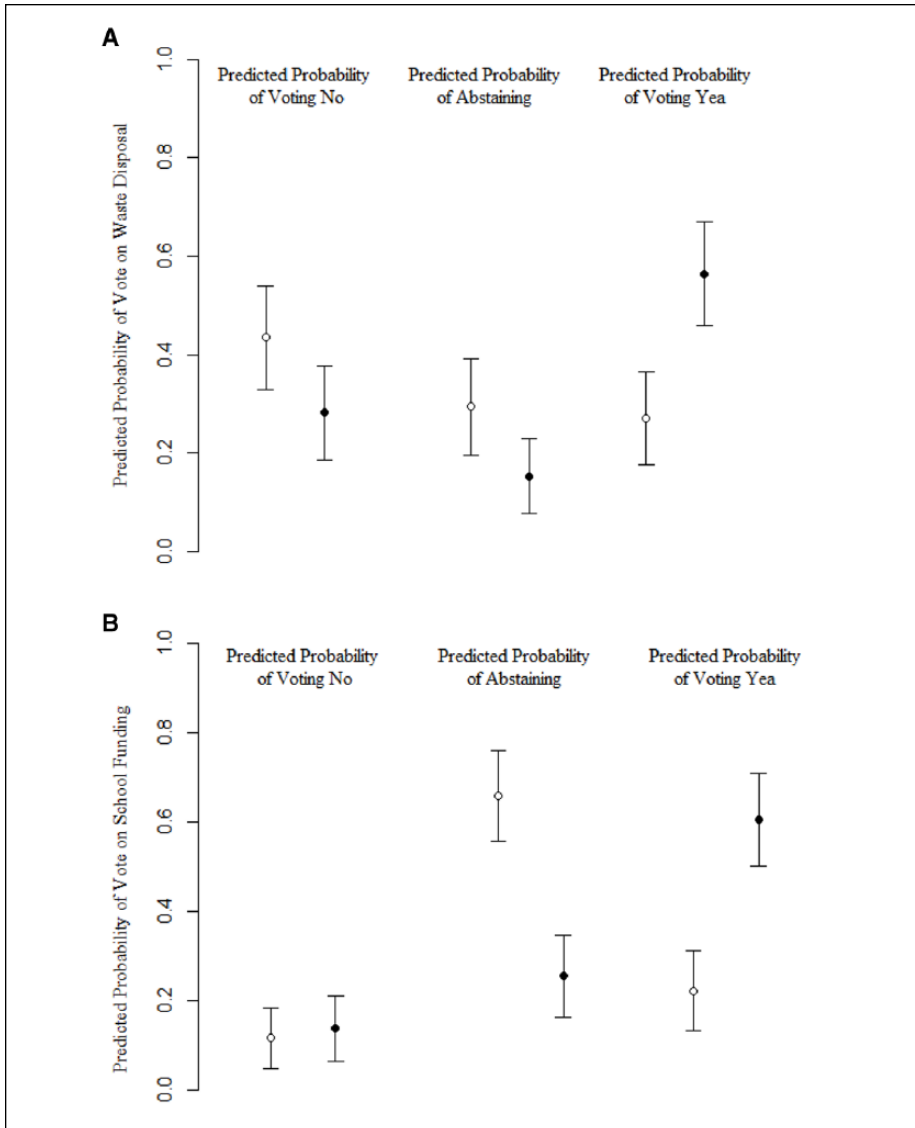
Figure 1. Predicted probability of vote position for waste disposal and school funding measures.
Note. Estimates represent the predicted probability with 95% confidence intervals, two-tailed. Solid circles represented the predicted values for subjects receiving the treatment. Waste Disposal $N = 170$, School Funding $N = 171$.

certain), and how strongly the respondent feels about the measure for the waste disposal treatment (also a five-point scale). The third and fourth columns feature the same two dependent variables for the school funding ballot measures.

Table 2. Ordered Probit Regression Estimates Predicting Certainty of Use and Strength of Position.

	Certainty regarding use of funds coefficients (SE)	Strength of position coefficients (SE)	Certainty regarding use of funds coefficients (SE)	Strength of position coefficients (SE)
Waste disposal (specific language)	0.576*** (0.169)	0.106 (0.163)		
School funding (specific language)			0.743*** (0.169)	0.583*** (0.167)
Cut point 1	−0.369** (0.130)	−1.106*** (0.147)	−0.463*** (0.132)	−0.755*** (0.137)
Cut point 2	0.910*** (0.141)	0.246† (0.127)	0.635*** (0.133)	0.535*** (0.132)
Cut point 3	1.918*** (0.189)	1.212*** (0.150)	1.859*** (0.181)	1.703*** (0.172)
Cut point 4	2.859*** (0.357)	2.042*** (0.227)	2.799*** (0.309)	2.255*** (0.224)
Log-likelihood	−202.756	−220.234	−212.877	−214.116
Pseudo-R^2	.028	.001	.044	.028
N	170	170	171	171

Note. Ordered probit estimates. The certainty and strength dependent variables are measured on five-point Likert-type scales. Standard errors in parenthesis.
†$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$. (two-tailed)

As the table reveals, hypothesis two found strong confirmation. In the waste disposal context, specificity increased those feeling "very" and "extremely" certain by 6.3%, and those feeling "somewhat" certain by 12.7%, *ceteris paribus*. School spending saw even greater gains in respondent certainty. Those with specific ballot vignettes were 10.2% more likely to be "very" and "extremely" certain of spending, and those "somewhat" certain increased by 18.1%, *ceteris paribus*. This is likely due to the subject pool being comprised of college students.

Conclusions about the third hypothesis are less definitive. Although more specific frames increased respondent strength of position in the school funding context (very and extreme certainty grew by 8.8%, and those "somewhat" certain increased by 13.7%, *ceteris paribus*), the waste disposal frame was not statistically significant in affecting strength. This could be due to the issue area; waste disposal may stir less passion in subjects, regardless of how the information is presented. In addition to this question, we are left to wonder whether results hold across different issue areas if subjects are presented with more than one ballot measure, and if the results are consistent with a nonstudent population. The second experiment helps elucidate the answers.

Experiment 2

The second experiment introduces two new issue areas: traffic relief and access to emergency services. Like the previous experiment, the amount of the tax was held constant for both the specific and unspecific frames, and the language was borrowed from real-world ballot measures.[11] The original language of these measures can be found in the online appendix. In addition, like the previous experiment, one pair of the vignettes (the emergency services vignettes) controlled for ballot measure length effects by keeping the number of words at nearly identical levels (54 words to the unspecific, 56 words in the specific).[12]

Unlike the first experiment, the second iteration randomly exposed subjects to both measures after first being asked series of demographic and political knowledge questions. This was done in an attempt to mimic the fatigue caused by asking voters to work their way down an extensive ballot, while presenting subjects with multiple issues on which to vote.

Finally, the second experiment utilized both student and nonstudent subjects. Like the first experiment, student subjects were recruited from various political science courses; their participation was rewarded with extra credit in one course. Nonstudent subjects were recruited from Amazon's MTurk.

Subjects were presented with the following question: "[h]ow you would feel about the following initiative if it were applied to where you live?" After which respondents were randomly assigned one traffic and one emergency services vignette. The language of the measures is given below (italicized text identifies the additional language; this did not appear to the subjects):

Traffic Relief, Unspecific

The proposed amendment reallocates 30% of certain state revenues collected on motor vehicle sales or leases from the General Fund to the Traffic congestion Relief and Safe School Bus Trust Fund. The amendment allocates money for transportation programs.

Traffic Relief, Specific

The proposed amendment reallocates 30% of certain state revenues collected on motor vehicle sales or leases from the General Fund to the Traffic congestion Relief and Safe School Bus Trust Fund. The amendment allocates money for transportation programs *including: highway expansion, specific freeway interchange improvements, mass transit improvements, purchasing buses, and expanding light and commuter rail. It provides funds for environmental enhancement, transportation impact mitigation programs, and transportation safety programs.*

Emergency Services, Unspecific

Should the state constitution be amended to enact a tax on parcels of property valued at $197 per year on each parcel of real property within the State of Florida, with an annual cost of living adjustment not to exceed 3%, and terminating on June 30, 2023, in order to preserve existing emergency services?

Table 3. Multinomial Probit Model of Support for a Traffic Relief Measure by Sample Source.

	Pooled	Pooled	Students	Students	MTurk	MTurk
	"Yes"	"No"	"Yes"	"No"	"Yes"	"No"
	Coefficients (SE)	Coefficients (SE)	Coefficients (SE)	Coefficients (SE)	Coefficients (SE)	Coefficients (SE)
Traffic relief (specific language)	0.335* (0.167)	−0.063 (0.191)	0.456* (0.219)	0.211 (0.244)	0.186 (0.262)	−0.571† (0.322)
Constant	1.163*** (0.114)	0.186 (0.127)	1.204*** (0.145)	0.226 (0.162)	1.097*** (0.183)	0.121 (0.206)
Log-likelihood	−557.69	−557.69	−349.52	−349.52	−203.73	−203.73
N	689	689	435	435	254	254

Note. Multinomial probit, abstain as reference category. Dependent variable is vote on the measure: yes, no, or abstain. Standard errors in parenthesis. MTurk = Mechanical Turk.
†$p < .10$. *$p < .05$. **$p < .01$.***$p < .001$. (two-tailed)

Emergency Services, Specific

Should the state constitution be amended to enact a parcel tax of $197 per year on parcels of property within the State, with an annual cost of living adjustment not exceeding 3%, and terminating on June 30, 2023, to preserve existing emergency services, *prevent the closure additional fire stations, and prevent the layoff of existing firefighters?*

Finally, after being asked how they would vote on the two prescribed measures (yes, no, or abstain), subjects were asked two follow-up questions to test the second and third hypotheses. Identical to the first experiment, subjects were asked about how certain they were the money raised by the ballot measure would be spent, and how strong their feelings were about the issue (see the previous experiment for the wording of these questions). Summary statistics and checks testing for randomized condition assignment can be found in the online appendix. Like the previous experiment, randomization checks largely confirm equal probability of condition assignment.

Results: Experiment 2, Spring 2013

Tables 3 and 4 reveal the results of the second experiment. As this experiment relied on both students and nonstudents (via MTurk), both tables feature the results of the entire subject pool (column 1, Tables 3 and 4), and disaggregated subject pools (columns 3 through 6, Tables 3 and 4). Like the first experiment, the multinomial models were also reanalyzed as ordered probit models; the results remain consistent (see the online appendix).

Beginning with column 1 of Table 3, we see the first hypothesis is once again confirmed in the traffic relief context, subjects exposed to the specific spending frame

Table 4. Multinomial Probit Model of Support for a Emergency Services Measure by Sample Source.

	Pooled	Pooled	Students	Students	MTurk	MTurk
	"Yes"	"No"	"Yes"	"No"	"Yes"	"No"
	Coefficients (SE)	Coefficients (SE)	Coefficients (SE)	Coefficients (SE)	Coefficients (SE)	Coefficients (SE)
Emergency services (specific language)	0.842*** (0.150)	0.303* (0.153)	0.904*** (0.187)	0.352† (0.191)	0.755** (0.254)	0.237 (0.259)
Constant	−0.166 (0.104)	−0.052 (0.102)	−0.316* (0.132)	−0.207 (0.129)	0.096 (0.172)	0.216 (0.169)
Log-likelihood	−734.017	−734.017	−463.380	−463.380	−267.157	−267.157
N	689	689	435	435	254	254

Note. Multinomial probit, abstain as reference category. Dependent variable is vote on the measure: yes, no, or abstain. Standard errors in parenthesis. MTurk = Mechanical Turk.
†$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$. (two-tailed)

were significantly more likely to vote in favor of the ballot measure. Given the unintuitive interpretation of multinomial probit coefficients, Figure 2 (top) explores the results visually. Unlikely the previous experiment, the traffic relief measure found strong support in the specific and unspecific conditions; that said, the effect of the specific treatment frame was substantial. For those in the unspecific condition (hollow circles), we see that approximately 20% were opposed and 65% in favor; however, for those exposed to the treatment frame, we see opposition drop to 14% and support jump 10 points, *ceteris paribus*.

Table 3 also disaggregates the subject pools into student (columns three and four) and MTurk samples (columns 5 and 6). As the table reveals, the results are consistent, and even stronger for the student sample, but not the MTurk sample. Instead of a significant increase in "yes" responses, we see a significant decrease in the "no" responses. It is not clear why this difference occurs, however, it could simply be a function of sample size or because of the overwhelming support this measure received. This helps mitigate concerns that the use of student samples may skew results because of a potential relationship between education level and support for ballot measures (Collingwood 2012). The online appendix features models searching for a conditional relationship between ballot framing language and education level, however the coefficients on the interactions in both the traffic and emergency services vignettes were not statistically significant.

Table 4 displays the results for the emergency services vignettes. As mentioned above, the number of words are almost identical in both conditions. As the first column of the table reveals, subjects exposed to the specific frame were significantly more likely to support the ballot measure. These results were consistent for both the student (column 2) and MTurk (column 3) samples. Interestingly, the "no" response was also positive and statistically significant for the pooled and student samples; this suggests a departure from the abstention category for the treated.[13]
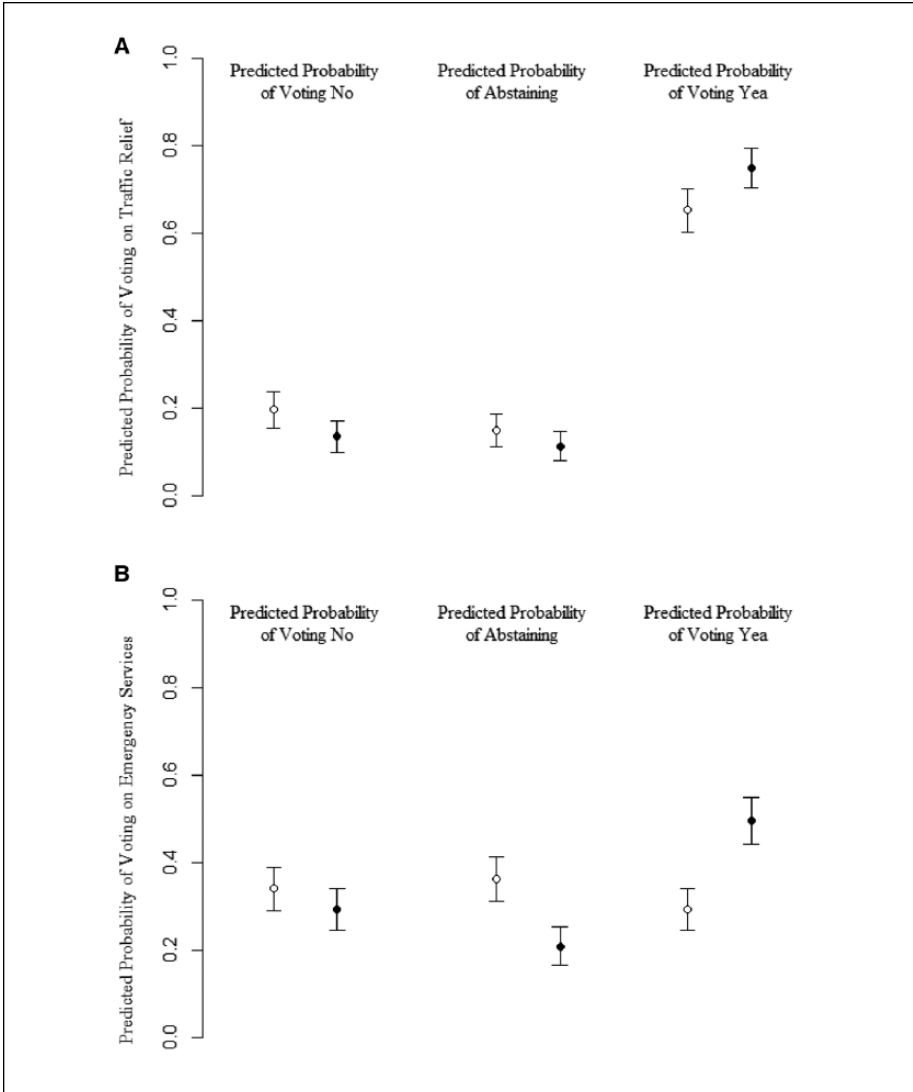
Figure 2. Predicted probability of vote for traffic relief measures and emergency services.
Note. Estimates represent the predicted probability with 95% confidence intervals, two-tailed. Solid circles represented the predicted values for subjects receiving the treatment. $N = 689$ for both measures.

The results of the pooled samples from Table 4 is also explored below in Figure 2 (bottom). Increased funding for emergency services was viewed with a great degree of opposition and ambivalence. For the unspecific frame, 34% opposed the tax, and 36% abstained; this left a mere 29% in favor. For those receiving greater specificity, we

Table 5. Ordered Probit Regression Estimates Predicting Certainty of Use and Strength of Position.

	Certainty regarding use of funds (pooled)	Strength of position (pooled)	Certainty regarding use of funds (pooled)	Strength of position
	Coefficients (*SE*)	Coefficients (*SE*)	Coefficients (*SE*)	Coefficients (*SE*)
Traffic relief (specific language)	0.146†	0.267**		
	(0.081)	(0.081)		
Emergency services (specific language)			0.589***	0.452***
			(0.082)	(0.081)
Cut point 1	−1.062***	−1.186***	−0.569***	−0.861***
	(0.072)	(0.076)	(0.066)	(0.069)
Cut point 2	0.154*	0.167**	0.424***	0.249***
	(0.063)	(0.063)	(0.064)	(0.063)
Cut point 3	1.373***	1.248***	1.419***	1.273***
	(0.078)	(0.075)	(0.077)	(0.074)
Cut point 4	2.298***	2.226***	2.545***	1.972***
	(0.135)	(0.122)	(0.135)	(0.097)
Log-likelihood	−869.885	−878.750	−923.439	−945.108
N	690	690	689	689
Pseudo-R^2	.002	.006	.027	.016

Note. Ordered probit estimates. The certainty and strength dependent variables are measured on five-point Likert-type scales. Standard errors in parenthesis.
†$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$ (two-tailed).

again see substantial changes in support: opposition drops slightly to 29%, support jumps to just shy of 50%, and abstentions plummeted to 21%, *ceteris paribus*. Although the specific language fails to give the emergency services measure majority support, we nonetheless see massive behavioral shifts.

Finally, to test the second and third hypotheses, subjects were once again asked about their certainty regarding the use of proposition funds, and the strength of their position. The results of these hypotheses are bore out in Table 5, which features the results four separate ordered probit regressions.

Beginning with the second hypothesis regarding certainty, the first and third columns reveal that increased specificity raised traffic relief certainty and emergency services certainty. Notably, in regard to specific language about tax dollars spent on traffic relief, those identifying as "very" and "extremely" certain increased by 2.5%, and those "somewhat" certain by 3.3%; those "very" and "extremely" certain of taxes spent to fund emergency services increased by 12.6%, and those "somewhat" certain by 10.5%, *ceteris paribus*.

The third hypothesis was solidly confirmed in this second round of experiments. Increases in specificity correspondingly bolstered strength of respondent position by 5.8% for those feeling "extremely" and "very" strong about the issue, and 4.9% for

those feeling "somewhat" strongly about traffic relief, *ceteris paribus*. Similarly, those feeling "extremely" and "very" strongly about emergency services increased by 10.5%, and those feeling "somewhat" strongly about the issue by 7.5%, *ceteris paribus*.

Conclusion

State and local use of ballot propositions has become increasing common; currently, 38 states permit voters to make legislative decisions. At the time of publication (early 2018), there were already five state and local measures asking voters to pay additional taxes in exchange for various state-sponsored benefits, including police services, parks and recreation, and university support. Although there has been considerable work surrounding ballot measures on matters such as language complexity, ballot design, voter awareness, and interest group endorsement, *inter alia*, left unanswered is the role of specific language in altering support for ballot measures that require voters to pay an exaction. Although there have been recent efforts to explore alterations in ballot titles, this work is a first step in attempting to fill a linguistic gap. By relying on original experimental data, the presented research investigates the effect of increasing specificity of the proposed use of tax expenditures on support for specific ballot measures. Ultimately, over two experiments, and across several issue areas, the findings strongly suggest a relationship between language precision and increased support for ballot measures that levy a tax. This has potentially broad implications for citizen groups and politicians seeking to persuade voters to adopt policies, even when those policies come at a cost. In short, those wishing to increase support for a ballot measure that requires taxpayer support may do so by simply providing additional details on how the assessed taxes will be utilized.

Although this work sheds light on a heretofore explored effect, there is much room for advancement. One obvious critique of this study, and of most studies that rely on experiments to test real-world conditions, is that the results may not reflect voter behavior on an actual ballot. There is no easy solution to this problem. Although it is possible to attempt to compare measures across states that share *similar* language, these comparisons are flimsy at best. Barring an instance where variation in proposal language naturally occurs on the same ballot, the next best option might simply be to confirm the findings of this work across additional issue areas, and over new sample populations. That said, as scholars have found that most voters generally have little knowledge of ballot measures (see Lee 1978; Nicholson 2003), first-time exposure to a measure in an experimental setting may not be totally different from initial exposure at the voting booth.

There is a potential weakness to the real-world application of the findings: the results presented above may have profound practical implications those seeking to use direct democracy as a means to advance a political agenda, however, the use of definitive language may seemingly constrain politicians. This hurdle can be easily overcome. Simple subtly in language could mitigate spending obligations. For example, ballot measure proponents could use general terms to detail the use of funds, rather than solidify specific project goals or outlays. Building on this idea, it was interesting

to note the experimental wording effects proved robust even as the language used to describe the exaction became more complex. Nevertheless, the number of items discussed could be a concern as taxpayers can only be expected to handle a handful of pieces of information (Cowan 2010; Jonides et al. 2008).

Left to be explored is the effect specificity has on support for measures that do not require an exaction. Although past research has shown that lengthy ballots lead to voter fatigue, and long ballot measures can lead to roll-off, none have ascertained if the gains obtained by providing additional information offset these losses. This raises the possibility that scholars have undervalued the persuasive power of details have on support.

Future studies should also consider exploring the effect of issue publics and demographic factors on voter malleability. In other words, looking to individual variations, if any, that explain which voters are more easily moved, and whether these changes in opinion are issue-specific. In a similar vein, studies should also explore manipulating the value and type of the exactions. Although this work was a first volley at addressing the acceptability of ballot measures requiring a new tax, we can easily imagine that voters have thresholds of acceptance despite language specificity. It would be interesting to determine, assuming such a threshold exists, if this elasticity point is universal or specific to other factors, such as voter income, party affiliation, or regional preferences. Alternatively, one could imagine voters may be more accepting of progressive taxes, or taxes that only affect a portion of voters (such as ad valorem taxes), but not flat taxes. Both the tax and language specificity literatures could benefit greatly from additional scholarly attention on this topic. These future gains are made especially interesting by the tremendous practical implications of turning voting minorities into majorities.

Declaration of Conflicting Interests

Funding

Notes

1. The terms proposition, plebiscite, ballot question, and ballot measure will be used interchangeably.
2. See Ballotpedia's 2016 Ballot Measures. See http://www.ballotpedia.org/2016_ballot _measures.
3. See http://www.iandrinstitute.org/docs/A_Comparison_of_Statewide_IandR_Processes.pdf.
4. See http://ballotpedia.org/2012_ballot_measures.
5. MTurk (Mechanical Turk) is an online platform to facilitate tasks that necessarily require human involvement; what Amazon.com calls Human Intelligence Tasks, or HITs. MTurk has evolved from simple crowd-sourcing tasks to include use as a means for social scientists to collect national samples; this is made possible by the diverse participant pool. Political science has been increasingly turning to MTurk on matters of

political psychology in the experimental setting (see Gerber, Huber, and Doherty 2013 and Krupnikov and Bauer 2014 for recent examples), and it has even been used in the realm of international relations (see Ausderan 2014; Tomz and Weeks 2013). For information about the accuracy and reliance on MTurk samples in experiments, see Clifford, Jewell, and Waggoner (2015); Levay, Freese, and Druckman (2016); and Mullinix et al. (2015).

6. The authors found three points of caution, however: that MTurk subjects tend to be younger, more ideologically liberal, and they have higher rates of attentiveness than respondents from other sources.

7. In terms of readability, the two measures were not far off. The unspecific measure had a Flesch–Kincaid readability (which ranges from 0–100, with higher values being easier to read) of 60.2 and a grade level of 7.7 years, while the specific measure had a readability of 45.9 and a grade level of 9.6 years.

8. The school funding measures were a bit more complex in regard to ease of reading. The unspecific measure had a Flesch–Kincaid readability score of 46.1 and a grade level of 11.9 years, while the specific measure had a readability of 25.8 and a grade level of 16.7 years. Although this may raise concerns of roll-off, such a result would run counter to the hypotheses. In other words, if readers that encounter longer, more complex measures are more likely to vote no or abstain, we only increase the likelihood of a Type II error, rather than Type I.

9. The waste disposal vignette was taken from a 2012 Clay County, Missouri measure. The school funding measure was adapted from a 2012 Bremerton School District Levy Addition (Kitsap County, Washington). The original language for these measures can be found in the online appendix.

10. Readers may question whether "no, abstain, and yes" are multinomial categories or whether an ordinal approach might be more appropriate. To assuage these concerns all presented models were reestimated in the online appendix as ordered probit regressions. The findings remain consistent.

11. The traffic relief vignette was taken from California Proposition 51 (November 2002); the emergency services measure was adapted from East Contra Costa County parcel tax (June 2012).

12. The unspecific and specific traffic relief measures had a Flesch–Kincaid readability (with higher scores indicating easier to read) of 40.6 and 10.3, and a grade level of 12.3 years and 17.6 years, respectively. The emergency services measures had a readability of 18.9 (unspecific) and 8 (specific), and a grade level of 24 and 26.1, respectively. We see that both the specific treatments are harder to read and more sophisticated. Although this may increase the likelihood of a Type II error, it should not contribute to an incorrect rejection of the null hypothesis.

13. These are also reanalyzed as ordinal regressions in the online appendix. The results are consistent.

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Supplemental Material

Supplemental material for this article is available online.

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