You Want To Spend My Money How?: Framing Effects on Tax Increases via Ballot Propositions.

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Abstract

Recently scholarly work has discovered that modest changes in the framing of the titles and summaries of ballot measures can have dramatic effects on endorsements for said measures. This work expands upon these early 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. While here has been much work surrounding ballot measures in regards to language complexity, (for example, 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 language in altering support for specific ballot measures. By relying on 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 work finds that propositions providing more information to voters 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.

1 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 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 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

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go "towards development projects," while Kearney City detailed that it too would provide solid waste disposal projects, and that it would further utilize the funds to pay for "curbside recycling." This raises a question: can minor changes to spending specificity account for such disparities? This paper attempts to shed light on this question by exploring the role of framing effects on support for ballot measures.

In recent years, ballot measures have become a common occurrence during elections. In 2012, voters in 38 states faced a total of 188 ballot questions; 63% of which were approved by voters (Ballotpedia 2013). Every state allows their legislature to place a measure of the ballot, and all but Delaware permit legislatively-referred amendments to a state's constitution. Additionally, eighteen states allow for citizens to directly amend the state constitution though a signature-gathering process, and twenty-one states allow citizen-led statutory propositions.[1] While many states feature ballot measures, the requirements to get an item added to an official state ballot vary from state to state. All states allowing citizen-derived measures on the ballot 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 (Matsusaka 1995), or are determined at a fixed number by the state legislature. This work explores the effect of language specificity on support for ballot measures that exact a tax, regardless of the origin of the measure, citizen or legislature.[2]

There has been considerable work surrounding ballot propositions. Language complexity (Bishop *et al.* 1978a,b; Gafke & Leuthold 1979; Reilly & Richey 2011), position on the ballot (Marcinkiewicz & Stegmaier 2014; Matsusaka 2014), party effects (Branton 2003; Hamilton & Ladd 1996), and prepossessed knowledge of the measures themselves (Bowler & Donovan 1994; Nicholson 2003) have all been shown to play a role in proposition outcomes. Similarly, ballot measures have also been shown to alter the fiscal behavior of a state. Matsusaka's (1995) investigation of propositions found that states with citizen-led measures have lower levels of state per capita spending, but increased local expenditures. Specifically, he found that "typical initiative state [expenditures are] about 12 percent lower, and local spending is about 10 percent higher than a comparable pure representative state" (Matsusaka 1995, 590).

There has also been been a notable amount of scholarly attention devoted to the exploration of the paradox of taxpayers wanting something for nothing. Going back to 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 Winter & Mouritzen 2001; Sanders 1988; Page & Shapiro 2010; Welch 1985 for just a few).

Left to be explored is the role of language in altering support for ballot measures. Recently, Burnett & Kogan (2015) have advanced this line of research tremendously by analyzing the effect of alterations of the titles of propositions on support. By relying on original experimental data, this manuscript extends the aforementioned research.

This article proceeds as follows: first, I will begin by situating this paper at the intersection of two literatures: the work assessing citizen responses to ballot measures, and the study framing effects. While much work has been done in both literatures, this paper explores a heretofore unanswered question regarding ballot language. Next, I will introduce my three hypotheses regarding

[1] < http://www.iandrinstitute.org/statewide_i%26r.htm >

[2] For this reason, the terms proposition, plebiscite, ballot question, and ballot measure will be used interchangeably.

ballot language specificity, and its effect on voter support and voter confidence. In section 3, I will discuss my data gathering efforts, which consists of two experiments, as well as a brief discussion of measurement. Forth, I will present the findings from my two experiments. Finally, section 4 will discuss the conclusions of this work, and provide future extensions.

2 Theory and Hypotheses

2.1 The Effect of Ballot Measures

2.1.1 Ballot Effects and Information Dissemination

Where do citizens receive information when asked to make an informed decision about a ballot measure? Those most disposed to engage in politics will seek information prior to the election, or will be alerted by the media. Indeed, there have numerous studies that have shown that turnout increases in midterm elections with the appearance of more ballot measures (Tolbert & Smith 2005; Tolbert *et al.* 2009) and greater media coverage (Lacey 2005; Smith 2001). Others, such as Biggers (2011), take a more nuanced position, arguing that increases in turn-out 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 most social issues are salient issues. Kuklinski & Jerit (2001, 349) give an example of this behavior when referring to the decision of small-town residents to build a new school:

> They see that the existing school is overcrowded, that continuing to use it will require costly repairs, and that the property on which the new school would be built is easily accessible. 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 must pull on accessible information to attempt to make an informed decision. 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 & McCubbins 2013, 1594). That said, attitudes need not be coalesced to the point of solidity; people need only indicate their dislikes and preferential boundaries (Diamond 2001).

2.1.2 Citizen Comprehension

In addition to awareness, questions are raised regarding comprehension; specifically, how likely is the average citizen to understand the questions presented on the ballot? While many states require ballot language to be simple and concise, even clear language cannot increase knowledge of the political process or heighten external efficacy. Readability has been shown to negatively affect the likelihood of voting for a ballot measure (Michalski & Guile 1990), while higher levels of education have proved to bolster support for direct democracy overall (Collingwood 2012). As Greenberg *et al.* (2008) point out, there is a notable disconnect between the average level of voter

education and the reading level required to understand ballot measures. 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; Matsusaka 2005; Burnett & Kogan 2015).

A consequence of a lack of understanding 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). The reason for these abstentions include a lack of comprehension and uncertainty surrounding the expected benefit. As Matsusaka (1992, 548) pointed out, traditional costs associated with voting (as dictated by rational voting theory) are essentially reduced to zero once the voter has the ballot in hand, consequently "the only reason he [the voter] abstains on a given proposition is if he does not know what his favorable outcome is." Indeed, several scholars have shown that lower socioeconomic status (Hadwiger 1992) and the voting method (Nichols 1998) both lead to increased voter roll-off. While the use of simple language, as well as reminding voters of their omissions, can decrease the occurrence of non-votes, any experiment attempting to understand voting behavior should afford participants the opportunity to abstain in order to mimic real-world conditions.

While fatigue and lack of comprehension may cause voters to omit items, ballot issues requiring citizens to pay a tax face strong opposition for other reasons. Voters are more sensitive to issues of money (Lanoue 1988; Alt & Chrystal 1983), and are consequently less likely to refuse to answer (i.e. abstain). Similar to these early findings, in their analysis of California ballot measures from 1974-1988, Bowler *et al.* (1992) found that voters were not more likely to abstain when voting on bond issues, but were significantly more likely to vote no. Not surprisingly, voters are averse to additional spending. For the experiments that will be considered here, this simply means we should expect to see subjects averse to increased spending, as well as a heightened difficulty in persuading subjects to vote yea. From a quantitative perspective, this means that should language specificity effects exist, results will be *more* difficult to detect.

2.2 The Framing of Ballot Measures

Early academic exploration of framing effects (Tversky & Kahneman 1981, 1982; Kahneman *et al.* 1982) have consistently shown that even modest modifications in the presentation of information can have dramatic effects on individual judgments. This proves true even if the presented information is the same, but the presentation of that information is cast in a positive or negative light (Levin *et al.* 1998). As Druckman (2004, 671-72) points out, numerous studies of equivalency frames (the 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 so-called "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 & Kinder 1996; Zaller 1992). This presents a fundamental challenge to the assumption that rational behavior dictates individual decision making, since variations in issue frames can result in preference shifts, despite logically equivalent alternatives.

This study focuses on issue frames, specifically, how slight variations in wording can

affect the relevant considerations used by voters.[3] Early studies of the effect of ballot measures on government expenditures largely focused on whether state spending changed under a ballot measure regime, but these studies failed to investigate individual voter considerations. Zax (1989) found that 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. After building several models with various plebiscites and controls for voter characteristics, Lascher *et al.* (1996, 769) concluded that there is no supportive evidence that "the initiative process is associated with more responsive [state] policies." Evidence that was later refuted by recent work on policy congruence (Matsusaka 2010; Lax & Phillips 2009). This is hardly surprising given the wide variance in the language used in ballot propositions, as well as the survey-based assessment of citizen support. Often when scholars are interested in determining awareness and support for ballot measures, surveys will ask 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. Additionally, awareness assessments may downplay the importance proposition language by only featuring the title, and this may not be enough to serve as a heuristic.

Recent work by Burnett & Kogan (2015) has done much to advance our understanding of ballot language framing and voter support. Specifically Burnett & Kogan (2015) manipulated the language used in ballot titles, along with elite endorsements, for two prominent ballot measures: California's Proposition 8 and Colorado's Amendment 7.[4] The authors found both language and endorsements had a significant effect on support. Yet, despite these notable advancements, several issues remain unanswered: do the same gains in support exist for changes to ballot text, not just title language; do similar results hold for minor issues, or solely those 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 & 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.

2.3 Reconciling the Ballot Measure and Framing Literatures

The ballot proposition literature has primarily focused on aggregate effects of measures across various states and over time, but this has often come at the expense of individual-level considerations. For example, Greenberg *et al.*'s (2008) exploration of language complexity on ballot amendments dealing with marriage is limited by the lack of data available on voter education, voter language preferences, and reading comprehension levels. 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, Burnett & Kogan (2015) laid the groundwork for this line of research; this work attempts to expand on their findings and bridge these two literatures by further exploring the effect ballot language and the presentation of information has on the considerations available to voters.

[3]That said, the vignettes used in this study hold costs constant. However, since differing language is used to describe the pay-off, this does not qualify as study of equivalence frames.

[4]Proposition 8 proposed to ban state recognition of same-sex marriage; Amendment 7 aimed to repeal the prohibited use of public funds for abortion procedures. A majority of Californians voted yes on Proposition 8, while a majority of Coloradans voted no on Amendment 7.

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. Citizens are hesitant to fund government projects when there is uncertainty regarding waste, and are generally reluctant to pay new taxes. For example, of the 10 state ballot measures in 2012 that asked voters to pay an additional tax, only 50% passed.[5] Moreover, this paper proposes that providing greater details regarding how tax dollars will be spent will increase voter confidence, and strengthen of 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. Armed with the theory discussed above, it is possible to derive three hypotheses: first, H_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. Second, H_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*. Finally, H_3 : regardless of the issue area, greater language specificity of how new tax dollars will be spent will increase how strongly voters feel about that issue, *ceteris paribus*.

3 Research Design, Data, and Results

In order 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). Student subjects were recruited from various political science courses by offering extra credit in one course in exchange for their participation. MTurk participants were enlisted by offering monetary compensation. The recruitment efforts resulted in samples of 341 and 689 (254 of which were MTurk subjects), respectively.

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 counterframing effects (Chong & 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.

3.1 Experiment One

When presented with actual ballot propositions, voters are often asked to vote on several proposals on a single ballot. However, given the initial uncertainty surrounding multiple propositions, the first experiment began by ensuring 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

[5]Considering tax measures that required new taxes be paid < http://ballotpedia.org/2012_ballot_measures >.

language) groups. Specifically, all subjects were given the following prefatory clause: "[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 with one of the following 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 (i.e. the specific language) that differentiates it from the unspecific, control condition.[6] Readers will also notice length of the ballot measures appear identical; in fact, they are. Previous research has shown that longer measures are likely have greater roll-off. This could act as a potential confounding factor; namely, that more words function as an alternative casual mechanism predicting support, or lack thereof. To control for this, the first treatment (waste disposal) 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 prefatory clause as above, but were then 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: $.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*

[6] The actual treatment did not feature italicized text.

[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. In other words, while the specific language had a higher level of sophistication, the unspecific measure was actually easier to read.

[8] Again, the actual treatment did not feature italicized text.

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.

Readers will notice that both parings of vignettes have the same tax exactions, this was to ensure any treatment effects were not a result of the size of the tax. Additionally, all vignettes directly mention the county by name (Leon County, in which Tallahassee resides) in order to mimic real-world measures. In fact, both the waste and school questions were taken from measures that appeared on ballots in different states; the unspecific vignettes above attempt to mimic the language as closely as possible.[9] After being presented with the measure, subjects were asked how they would vote: yes, abstain, or no.[10]

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 two) 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 5-point Likert scale ranging from "not at all certain" to "extremely certain." To assess how the vignettes affected issue strength (hypothesis three), respondents were asked "[h]ow strong are your feelings about the issue on this ballot initiative?" This too was measured on a 5-point scale ranging from "not strong at all" to "extremely strong." Summary statistics for this and subsequent experiments can be found in the appendix.

Finally, to ensure randomization of assignment, a number of ancillary tests were conducted. Cursory analysis of party identification across the treatment and control conditions suggests equal probably of assignment.[11] The 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.

3.2 Results: Experiment One, Fall 2012

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

[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 appendix.

[10]The school funding measures were a bit more complex in regards 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. While 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.

[11]The distribution of party ID across the two treatment and control conditions (specific versus unspecific) suggests random assignment. The party mean for the waste treatment condition ($\bar{x} = 4.02$) was almost identical to those in unspecific condition ($\bar{x} = 3.98$) as measured on a 7-point Likert scale, with 1 being strong Republican, and 7 being strongly Democrat. The same held true for the specific schools subject pool ($\bar{x} = 4.19$) versus the unspecific ($\bar{x} = 3.8$).

ballot measure vignette, student subjects were asked to vote yes, abstain, or no. Table 1 shows the results of an ordered probit model for these measures.[12]

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 2) measures.

<table 1 about here>

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 (47% voting no, 23% abstaining; top figure), but were largely ambivalent on the school support measure (20% voting no, 51% 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 31% to 54% voting yes for waste disposal, and from 30% to 54% voting yes for school funding. Finally, as readers may recall, both the specific and unspecific waste disposal vignettes had the same number of words; the fact that the findings remain robust regardless of the number of words suggests that it is the substance of the frame that is driving voter behavior, rather than simply the length of the measure.

<figure 1 about here>

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. Below table 2 reveals the results of four ordered probit regressions. The first two models (columns one and two) look at certainty of how the tax dollars would be used (measured on a 5-point Likert scale, with 5 being extremely certain), and how strongly the respondent feels about the measure for the waste disposal treatment (also a 5-point scale). The third and fourth columns feature the same two dependent variables for the school funding ballot measures.

<table 2 about here>

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.*

Conclusions about the third hypothesis are less definitive. While 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 issue areas

[12]Readers may question whether "no, abstain, and yes" are actually ordered categories, or whether a multinomial approach might be more appropriate. Firstly, the use of an ordered model has been used in similar work on voting where "yes, don't know, and no" were viable options (see Vossler & Kerkvliet 2003). Nonetheless, to assuage these concerns I reconsider all ordered models in the appendix as a unordered multinomial regressions. The findings remain consistent, and are actually slightly stronger in the multinomial specification.

if subjects are presented with more than one ballot measure, and if the results are consistent with a non-student population. The second experiment helps elucidate the answers to these questions.

3.3 Experiment Two

As mentioned, a few lingering questions remained regarding the robustness of the findings.

First, 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.[13] Additionally, 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).

Second, unlike the first experiment, this second iteration exposes 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 often extensive ballot, while presenting subjects with multiple issues on which to vote.

Finally, utilized both student and non-student subjects. Like the first experiment, student subjects were recruited from various political science courses; their participation was rewarded with extra credit in one course. Non-student subjects were recruited from Amazon's MTurk. MTurk 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 *et al.* 2013 and Krupnikov & Bauer 2014 for recent examples), and it has even been used in the realm of international relations (see Tomz & Weeks 2013 and Ausderan 2014).

Recent work investigating reliability of MTurk samples has found that MTurk results are generally consistent with alternative means of data collection (Bates & Lanza 2013); Buhrmester *et al.* (2011, 5) confirm 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 *et al.* (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" (2012, 366).[14]

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 vignette and one emergency services vignette. The language of the measures is below (italicized text in the specific treatment identifies the additional language; this did not

[13]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). The original language of these measures can be found in the appendix.

[14]

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.

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?

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 in order 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 Section 3.1 for the wording of these questions).[15] Summary statistics and numerous checks testing for randomized condition assignment can be found in the appendix. Like the previous experiment, randomization checks largely confirm equal probability of condition assignment. That said, party identification proved significant in the emergency services experiment for the pooled and student samples. Thankfully, the disaggregation of the sample groups helps to allay concerns regarding assignment.

[15]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. While this may increase the likelihood of a Type II error, it should not contribute to an incorrect rejection of the null hypothesis.

3.4 Results: Experiment Two, Spring 2013

Below tables 3 and 4 reveal the results of the second experiment. Both the specific and unspecific vignettes hold constant the amount of taxes imposed; additionally, the emergency vignettes feature a nearly identical number of words.[16]

Since this experiment relied on both students and non-students (via MTurk), both tables feature the results of the entire subject pool (column one), and disaggregated subject pools (columns two and three).

Beginning with column one of table 3, we see the first hypothesis is once again confirmed in the traffic relief context, subjects exposed to the specific spending frame were significantly more likely to vote in favor of the ballot measure. Given the unintuitive interpretation of ordered 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 13% and support jump ten points, *ceteris paribus*.

Table 3 also disaggregated the subject pools into student (column two) and MTurk samples (column three). As the table reveals, the results are consistent for both sample populations.[17]

<table 3 about here>

Table 4 displays the results for the emergency services vignettes. Similar to the waste disposal measures in the first experiment, the number of words are almost identical (54 versus 56 words), with slightly more words in the unspecific frame. As the first column of the table reveals, subjects exposed to the specific frame were significant more likely to support the ballot measure. These results were consistent for both the student (column two) and MTurk (column three) samples.

<table 4 about here>

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, 38% opposed the tax, and 29% abstained; this left a mere 33% in favor. For those receiving greater specificity, we again see substantial changes in support: opposition drops to 26%, support jumps to 46%, and abstentions remain relatively unchanged, *ceteris paribus*. While the change in language fails to give the emergency services measure majority support, we nonetheless see massive behavioral shifts.

<figure 2 about here>

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

[16]Like the first experiment, the ordered models were also analyzed as unordered multinomial models; the results remain consistent. See the appendix.

[17]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 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.

these hypotheses are bore out in table 5, which features the results four separate ordered probit regressions.[18]

Beginning with the second hypothesis regarding certainty, the first and third columns reveal that increased specificity raised traffic relief certainty and emergency services certainty. Specifically, in regards 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*.

<table 5 about here>

4 Conclusion

State use of ballot propositions has become increasing common throughout the nation; currently, 38 states turn to voters to make legislative decisions. While there has been considerable work surrounding ballot measures on matters such as language complexity, ballot design, and voter awareness, interest group endorsement (amongst others), left unanswered is the role of language in altering support for ballot measures. While there have been recent efforts to explore alterations in ballot titles, this work was a first step in attempting to fill this linguistic gap. By relying on original experimental data, this work investigated the effect of increasing specificity of proposed use of tax expenditures on support for specific ballot measures. Ultimately, over two experiments, and across several issue areas, the findings presented strongly suggest a relationship between language specificity and increased support for ballot measures that exact a tax.

The results of this investigation have broad practical implications for citizens and special interest groups seeking to use direct democracy as a means to advance a political agenda. Specifically, 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. This appears to be enough to persuade many voters to support the effort. Interestingly, this effect proved robust even as the language used to describe the exaction became more complex.

While 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. While it is possible to attempt to compare measures across states that share *similar* language, these comparisons will be flimsy at best. The next best option might simply be to confirm the findings of this work across additional issue areas, and over new sample populations.

Future studies should also consider exploring the effect of issue publics and demographic factors on voter malleability. In other words, looking to individual variations to explain which

[18]These are also reanalyzed as multinomial regressions in the appendix. The results are consistent.

voters are more easily moved, and whether these changes in opinion are issue-specific. Finally, studies should also explore manipulating the value of the exactions. While this work was a first volley at addressing the acceptability of ballot measures requiring a new tax, we can easily imagine that voters have threshold of acceptance in spite of 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.

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Table 1: Ordered Probit Models of Support for Waste Disposal and School Funding Measures

	Waste Disposal Coeff/(std err)	School Funding Coeff/(std err)
Waste Disposal, Specific Language	0.600***	
	(0.179)	
Schools Funding, Specific Language		0.633***
		(0.177)
Cut Point 1	-0.085	-0.866***
	(0.129)	(0.142)
Cut Point 2	0.510***	0.532***
	(0.133)	(0.133)
$pseudo - R^2$	0.031	0.038
Log-Likelihood	-175.79	-162.28
N	170	171

$***p < 0.001$, $**p < 0.01$, $*p < 0.05$, $+p < 0.10$ (two-tailed). Ordered probit estimates. Dependent variable is vote on the measure: yes, no, or abstain. Standard errors in parenthesis.

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

	Certainty Regarding Use of Funds Coeff/(std err)	Strength of Position Coeff/(std err)	Certainty Regarding Use of Funds Coeff/(std err)	Strength of Position Coeff/(std err)
Waste Disposal	0.576***	0.106		
(Specific Language)	(0.169)	(0.163)		
School Funding			0.743***	0.583***
(Specific Language)			(0.169)	(0.167)
Cut Point 1	-0.369**	-1.106***	-0.463***	-0.755***
	(0.130)	(0.147)	(0.132)	(0.137)
Cut Point 2	0.910***	0.246+	0.635***	0.535***
	(0.141)	(0.127)	(0.133)	(0.132)
Cut Point 3	1.918***	1.212***	1.859***	1.703***
	(0.189)	(0.150)	(0.181)	(0.172)
Cut Point 4	2.859***	2.042***	2.799***	2.255***
	(0.357)	(0.227)	(0.309)	(0.224)
Log-likelihood	-202.756	-220.234	-212.877	-214.116
$pseudo - R^2$	0.028	0.001	0.044	0.028
N	170	170	171	171

$***p < 0.001$, $**p < 0.01$, $*p < 0.05$, $+p < 0.10$ (two-tailed). Ordered probit estimates. The certainty and strength dependent variables are measured on 5-point Likert scales. Standard errors in parenthesis.

Table 3: Ordered Probit Model of Support for a Traffic Relief Measure (by Sample Source)

	Pooled Sample Coeff/(std err)	Student Sample Coeff/(std err)	MTurk Sample Coeff/(std err)
Traffic Relief	0.271**	0.207+	0.393*
(Specific Language)	(0.097)	(0.123)	(0.161)
Cut 1	-0.840***	-0.802***	-0.917***
	(0.072)	(0.091)	(0.121)
Cut 2	-0.397***	-0.427***	-0.341**
	(0.068)	(0.086)	(0.111)
Log-Likelihood	-557.795	-350.492	-204.424
N	689	435	254
$pseudo - R^2$	0.007	0.004	0.014

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{+}p < 0.10$ (two-tailed). Ordered probit estimates. Dependent variable is vote on the measure: yes, no, or abstain. Standard errors in parenthesis.

Table 4: Ordered Probit Model of Support for a Emergency Services Measure (by Sample Source)

	Pooled Sample Coeff/(std err)	Student Sample Coeff/(std err)	MTurk Sample Coeff/(std err)
Emergency Services	0.341***	0.346**	0.333*
(Specific Language)	(0.087)	(0.109)	(0.145)
Cut 1	-0.315***	-0.364***	-0.232*
	(0.064)	(0.081)	(0.105)
Cut 2	0.436***	0.476***	0.369***
	(0.065)	(0.082)	(0.106)
Log-Likelihood	-742.998	-470.426	-269.453
$pseudo - R^2$	689	435	254
N	0.010	0.011	0.010

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $+p < 0.10$ (two-tailed). Ordered probit estimates. Dependent variable is vote on the measure: yes, no, or abstain. Standard errors in parenthesis.

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

	Certainty Regarding Use of Funds (pooled) Coeff/(std err)	Strength of Position (pooled) Coeff/(std err)	Certainty Regarding Use of Funds (pooled) Coeff/(std err)	Strength of Position (pooled) Coeff/(std err)
Traffic Relief	0.146+	0.267**		
(Specific Language)	(0.081)	(0.081)		
Emergency Services			0.589***	0.452***
(Specific Language)			(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
$pseudo - R^2$	0.002	0.006	0.027	0.016

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $+p < 0.10$ (two-tailed). Ordered probit estimates.The certainty and strength dependent variables are measured on 5-point Likert scales. Standard errors in parenthesis.

Figure 1: Predicted Probability of Vote Position for Waste Disposal and School Funding Measures

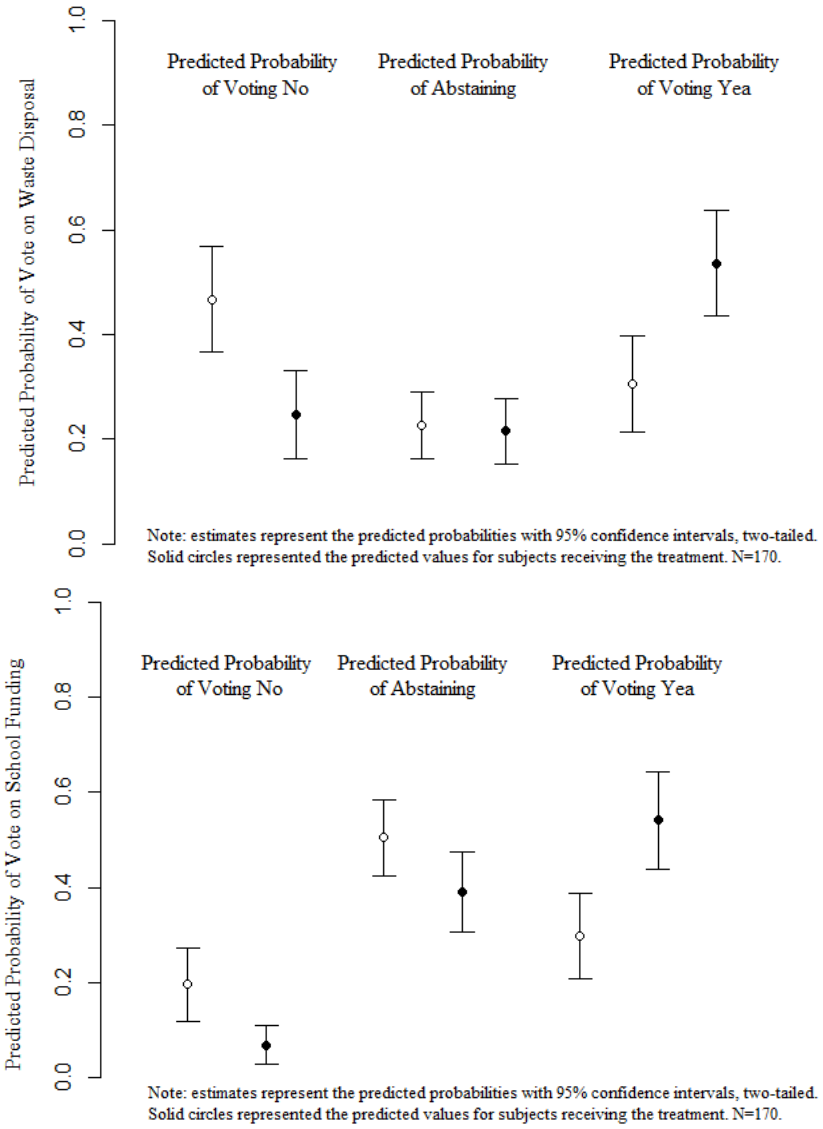
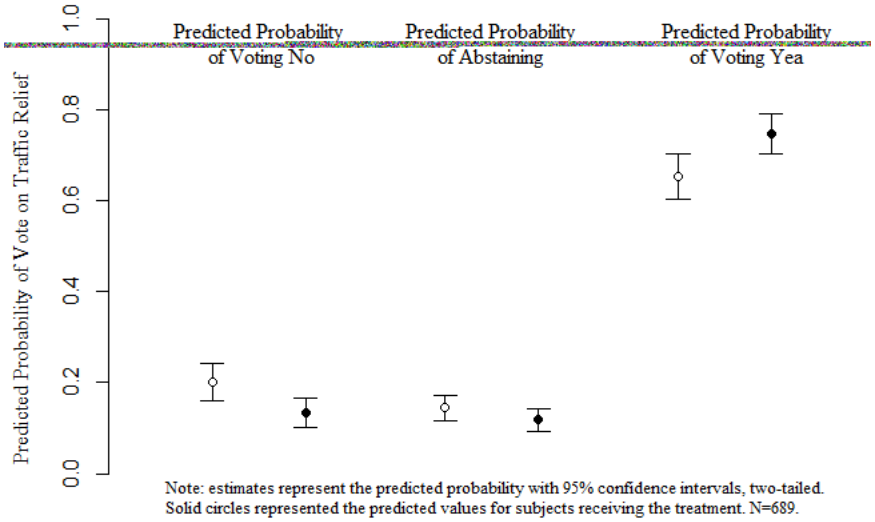
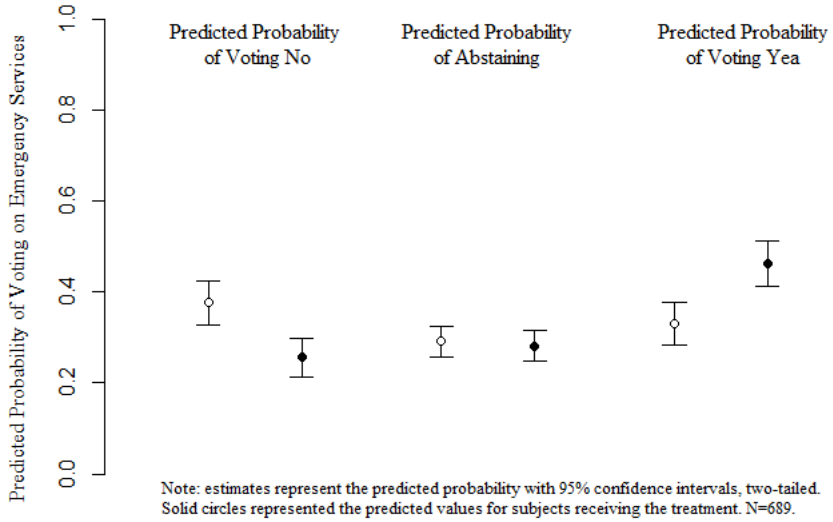


Figure 2: Predicted Probability of Vote for Emergency Services and Traffic Relief Measures



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.



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.

Appendix

Original Ballot Language and Experimental Ballot Language

As mentioned in the full text, the language for all the treatments was derived from ballots presented to voters in various locations. The comparisons of the experiment and real-world language is found below.

Waste Disposal Language

Experiment: Waste Disposal, Unspecific (Flesch-Kincaid Grade Level: 7.7)

> 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.

Experiment: Waste Disposal, Specific (Flesch-Kincaid Grade Level: 9.6)

> 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.

Actual Language: Kearney City Waste Collection Fee Increase (April 2012). Clay County, Missouri.[19] (Flesch-Kincaid Grade Level: 6)

> Shall Kearney increase its residential solid waste collection fees by $1.75 a month to add curbside recycling? That would increase current fees to $10.75 for residential and $8.75 for senior citizen rates.

School Funding Language

School Funding, Unspecific (Flesch-Kincaid Grade Level: 11.9)

> 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: $.61.

School Funding, Specific (Flesch-Kincaid Grade Level: 16.7)

[19] The measure passed 57.5% to 42.5%.

> 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.

Actual Language: A Bremerton School District Levy Addition (August 2012). Bremerton School District, Kitsap County, Washington.[20] (Flesch-Kincaid Grade Level: 12)

> The Board of Directors of Bremerton School District No. 100-C adopted Resolution 2011/2012-14 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 to West Hills STEM, and upgrade fire alarm systems; and authorizes the following excess levies for such purposes on all taxable property within the District: Collection Years: 2013-2016; Levy Amount: $1,900,000; Approximate Levy Rate/$1,000 Assessed Value: $.61. As provided in Resolution 2011/2012-14. Should this proposition be approved?

Traffic Relief Language

Traffic Relief, Unspecific (Flesch-Kincaid Grade Level: 12.3)

> 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 (Flesch-Kincaid Grade Level: 17.6)

> 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.

Actual Language: California Proposition 51, Vehicle Taxes Allocated to Transportation Projects (November 2002).[21] (Flesch-Kincaid Grade Level: 16.2)

[20] The measure passed 59.7% to 40.3%.

[21] The measure was defeated 57.8% to 42.2%.

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. Allocates money for transportation programs including: highway expansion, specific freeway interchange improvements, mass transit improvements, purchasing buses, and expanding light and commuter rail. Provides funds for environmental enhancement, transportation impact mitigation programs, and transportation safety programs. Allocates money to 45 specific projects and for remainder specifies distribution percentages, restricts fund uses, and provides accountability measures.

Should the sales and use taxes raised from the sale or lease of motor vehicles be permanently allocated to specific transportation projects?

Emergency Services Language

Emergency Services, Unspecific (Flesch-Kincaid Grade Level: 24)

> 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?

Emergency Services, Specific (Flesch-Kincaid Grade Level: 26.1)

> 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?

Actual Language: East Contra Costa County parcel tax for Fire Services, Measure S (June 2012) Contra Costa County, California.[22] (Flesch-Kincaid Grade Level: 30.4)

> To preserve existing emergency services, add paramedic services and prevent further layoffs of up to one half of existing firefighters and the closure of up to 3 additional fire stations, shall an ordinance be adopted to enact a parcel tax of $197 per year on each parcel of real property within the District, with an annual cost of living adjustment not to exceed 3% and terminating on June 30, 2023?

[22]The measure was defeated with 56.2% voting no, 43.8% voting yes

Summary Statistics

Table 6: Summary Statistics for the Waste Disposal and School Spending Treatments

Variable	N	Mean	Std. Dev.	Min	Max	Notes
Approval (Waste)	170	1.059	0.882	0	2	0=No, 1=Abstain, 2=Yes
Certainty (Waste)	170	2.076	0.863	1	5	1=Not at all certain, 5=Extremely certain
Strength (Waste)	170	2.447	0.917	1	5	1=Not strong at all, 5=Extremely strong
Approval (Schools)	171	1.287	0.682	0	2	0=No, 1=Abstain, 2=Yes
Certainty (Schools)	171	2.281	0.922	1	5	1=Not at all certain, 5=Extreme certain
Strength (Schools)	171	2.368	0.920	1	5	1=Not strong at all, 5=Extremely strong

Table 7: Summary Statistics for the Traffic Relief and Emergency Services Treatments

Variable	N	Mean	Std. Dev.	Min	Max	Notes
Approval (Traffic), Pooled	689	1.534	0.734	0	2	0=No, 1=Abstain, 2=Yes
Approval (Traffic), Students	435	1.517	0.787	0	2	0=No, 1=Abstain, 2=Yes
Approval (Traffic), MTurk	254	1.563	0.724	0	2	0=No, 1=Abstain, 2=Yes
Certainty (Traffic), Pooled	693	2.545	0.885	1	5	1=Not at all certain, 5=Extremely certain
Strength (Traffic), Pooled	693	2.449	0.866	1	5	1=Not strong at all, 5=Extremely strong
Approval (Emerg.), Pooled	689	1.077	0.841	0	2	0=No, 1=Abstain, 2=Yes
Approval (Emerg.), Students	435	1.083	0.822	0	2	0=No, 1=Abstain, 2=Yes
Approval (Emerg.), MTurk	254	1.067	0.875	0	2	0=No, 1=Abstain, 2=Yes
Certainty (Emerg.), Pooled	692	2.403	0.994	1	5	1=Not at all certain, 5=Extremely certain
Strength (Emerg.), Pooled	692	2.546	1.013	1	5	1=Not strong at all, 5=Extremely strong

Randomization Checks

Below are a number of ancillary tests that ensure random assignment of the subjects. Beginning with the first experiment (waste disposal and school spending), random assignment was tested by regressing respondent characteristics on treatment assignment. If the assignment was indeed random, these variable should not predict treatment/control assignment. Unfortunately, there were few demographic questions asked (largely because a student sample often has little in the way of variation over factors like education). That said party affiliation (measured on a 7-point scale, with 7 indicating strongly identifying as a Democrat) and the number of previous surveys taken (ranging from 0 to 3, with 3 indicating 3 or more surveys taken) were collected. The party ID variable indicated a surprising number of moderates; it had a mean of 4.02 (standard deviation of 2.25). The survey count variable indicated most students had taken very few surveys previously (mean 0.45, standard deviation of 0.84).

Table 8 features the results of a probit regression predicting condition assignment in the first experiment. As the table reveals, neither of the two descriptive variables predicted treatment assignment for waste disposal or school spending in a statistically significant way.

Table 8: Randomization of Treatment Assignment, Waste Disposal and School Spending (Experiment 1)

	Waste Disposal	School Spending
Party ID (Dem)	0.009	0.050
	(0.043)	(0.044)
# of Surveys	-0.133	-0.157
	(0.113)	(0.122)
Constant	0.019	-0.124
	(0.199)	(0.206)
Log-Likelihood	-117.135	-117.037
N	170	171
Pseudo-R^2	0.006	0.013

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. (two-tailed).

Probit estimates. Standard errors in parenthesis.

This same process was repeated for the second experiment, however, in this iteration we have a number of descriptive variables to work with, as well as student and MTurk samples. Subjects were asked about their level of education (ranging from 1-7, with 1 being High School or less, and 7 being >6 years of education; $\bar{x} = 3.41,\ \sigma = 1.49$), their party affiliation (same measurement scheme as above; $\bar{x} = 4.43\ \sigma = 2.13$), the number of surveys previous taken (this time we increased the accuracy of the measurement by allow it to vary from 0 to 100 to accommodate the MTurk sample; $\bar{x} = 10.29,\ \sigma = 21.36$), political knowledge (this was assessed by asking subjects three questions about current events; range was from 0-1 in increments of 1/3, $\bar{x} = 0.83,\ \sigma = 0.25$), race (respondents were asked if they were White, Black, Asian, Native American, or other; dummies were created for White, $\bar{x} = 0.75,\ \sigma = 0.43$, and Black, $\bar{x} = 0.07,\ \sigma = 0.26$, the reference category is Asian, Native American or Other), and sex (1=male, 0=female; $\bar{x} = 0.48,\ \sigma = 0.5$).[23] Just as above, these variables were regressed on treatment assignment (specific versus unspecific).

Table 9 reveals that, on the whole, treatment assignments appear to be sufficiently random. There is one point of note, however. The party affiliation variable was statistically significant for the emergency treatment experiment for the student sample, which continues to remain within the bound of significance when the students are pooled with MTurk subjects. Namely, the positive coefficient tells us that those identifying as being affiliated with Democrats were more likely to be assigned into the specific condition than their Republican leaning counterparts. There are two points that should help allay concerns about randomization: first, party proved to the only variable of several that suggested a randomization concern. Second, and more importantly, the models featured in the text were reconsidered with the inclusion of a party ID variable (see the "Reanalysis Including a Party Control Variable" section of this appendix), and this additional variable did not substantively change the findings in any meaningful way.

[23]Note that all of the means and standard deviations provided were for the pooled sample.

Table 9: Randomization of Treatment Assignment, Traffic Relief and Emergency Services (Experiment 2)

Treatment:	Traffic	Traffic	Traffic	Emergency	Emergency	Emergency
Subjects:	Pooled	Student	MTurk	Pooled	Student	MTurk
Education	-0.011	-0.038	-0.002	0.011	0.029	0.016
	(0.034)	(0.054)	(0.044)	(0.034)	(0.055)	(0.044)
Party ID (Dem)	-0.011	-0.023	0.014	0.050*	0.095**	-0.032
	(0.024)	(0.029)	(0.044)	(0.024)	(0.029)	(0.044)
# of Surveys	0.000	0.076	0.001	-0.001	0.065	0.000
	(0.002)	(0.065)	(0.003)	(0.002)	(0.064)	(0.003)
Pol. Knowledge	0.192	-0.036	0.433	-0.060	0.005	-0.070
	(0.202)	(0.262)	(0.325)	(0.202)	(0.264)	(0.326)
White	0.012	-0.005	0.364	0.126	0.218	0.033
	(0.143)	(0.157)	(0.446)	(0.144)	(0.159)	(0.434)
Black	-0.103	-0.194	0.387	0.351	0.119	0.758
	(0.221)	(0.263)	(0.529)	(0.223)	(0.264)	(0.529)
Male	0.065	0.001	0.169	0.068	0.107	0.022
	(0.101)	(0.127)	(0.169)	(0.101)	(0.128)	(0.169)
Constant	-0.099	0.256	-0.894	-0.382	-0.753*	-0.014
	(0.243)	(0.328)	(0.539)	(0.245)	(0.333)	(0.531)
Log-Likelihood	-453.455	-285.137	-165.150	-450.221	-280.009	-164.637
N	656	414	242	656	414	242
Pseudo-R^2	0.003	0.006	0.015	0.010	0.024	0.017

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. (two-tailed). Probit estimates. Standard errors in parenthesis.

Reanalysis Including a Party Control Variable

Readers may have noticed the absence of a party control variable; subject randomization makes it extremely unlikely that other explanatory variables should prove significant, but this does not prevent chance assignment of "too many people of a particular type [to] one of the treatment groups" (Ansolabehere & Iyengar 1995, 172). In other words, there is a chance, albeit unlikely, of an omitted variable problem. Moreover, the inclusion of relevant variables can lead to nontrivial increases in efficiency (Franklin 1991), with negligible increases in bias (Green 2009). While the randomization checks (see above) suggest that party did not predict assignment in the first experiment, it was significant for the emergency services measure (experiment 2) for the student and pooled samples. Consequently, both experiment 1 and 2 were reanalyzed with a party identification variable included.

Beginning with table 10, we see the results of table 1 with party included in columns one and three, and the original models (with party omitted) in columns two and four. The inclusion of a party control variable had a negligible effect on the results. Namely, the party ID variable was only significant for the waste disposal measure, and it worked to *increase* the size of the coefficient. Finally, the AIC and BIC suggest a better model fit for the inclusion of a party variable in the waste

disposal experiment, but not for school funding.[24]

Table 10: Reconsideration of the Waste and School Models with Party Included

	With party Model 1 Coeff/(std err)	Original Model 1 Coeff/(std err)	With Party Model 2 Coeff/(std err)	Original Model 2 Coeff/(std err)
Waste Disposal, Specific Language	0.609***	0.600***		
	(0.180)	(0.179)		
Schools Funding, Specific Language			0.617***	0.633***
			(0.177)	(0.177)
Party ID (Dem)	**0.105****		**0.050**	
	(0.040)		**(0.040)**	
Cut 1	0.338	-0.085	-0.681***	-0.866***
	(0.209)	(0.129)	(0.204)	(0.142)
Cut 2	0.951***	0.510***	0.727***	0.532***
	(0.216)	(0.133)	(0.205)	(0.133)
$pseudo - R^2$	0.050	0.031	0.043	0.038
Log-Likelihood	-172.29	-175.79	-161.49	-162.28
N	170	170	171	171
AIC	352.6	357.6	330.9	330.6
BIC	365.13	366.99	343.54	339.98

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $+p < 0.10$ (two-tailed). Ordered probit estimates. Standard errors in parenthesis.

Table 11 reconsiders the results found in tables 3 and 4 (with party control included) in columns one and three. The second and fourth columns present the original results. While both of the party ID variables are significant and positive, we see negligible changes in the size of the coefficients (slightly larger for the traffic relief measure, and slightly smaller for the emergency services measure), but no change in the significance of the findings. That said, both the AIC and BIC suggest better model fit for models including a party control.

[24]For AIC and BIC, smaller values suggest a better fit.

Table 11: Reconsideration of the Traffic and Emergency Models with Party Included

	With Party Model 1 Coeff/(std err)	Original Model 1 Coeff/(std err)	With Party Model 2 Coeff/(std err)	Original Model 2 Coeff/(std err)
Traffic Relief	0.275**	0.271**		
(Specific Language)	(0.098)	(0.097)		
Emergency Services			0.315***	0.341***
(Specific Language)			(0.087)	(0.087)
Party ID (Dem)	**0.112*** **		**0.079*** **	
	(0.023)		**(0.020)**	
Cut 1	-0.372**	-0.840***	0.016	-0.315***
	(0.120)	(0.072)	(0.107)	(0.064)
Cut 2	0.084	-0.397***	0.778***	0.436***
	(0.119)	(0.068)	(0.109)	(0.065)
N	-545.667	-557.795	-735.427	-742.998
Log-Likelihood	689	689	689	689
$pseudo - R^2$	0.029	0.007	0.020	0.010
AIC	1099.3	1121.6	1478.9	1492
BIC	1117.5	1135.2	1497	1505.6

$***p < 0.001$, $**p < 0.01$, $*p < 0.05$, $+p < 0.10$ (two-tailed). Ordered probit estimates.

Standard errors in parenthesis. The estimates are for the pooled student and MTurk samples.

Reanalysis as an Unordered Multinomial Choice

As mentioned in footnote 12, readers may question the appropriateness of treating "no, abstain, or yes" votes as an ordered variable. To assuage these concerns, table 12 reconsiders the models presented in tables 1, 3, and 4 as unordered multinomial choices. Using abstain as the reference category, the table below reveals that subjects receiving more specific language consistently support the ballot measures more than their unspecific counterparts. These results are statistically significant for all four of the ballot questions.

Table 12: Reconsideration of the Ordered Models as Multinomial Choices

	Study 1: Abstaining is the Reference Category				Study 2: Abstaining is the Reference Category			
	Waste Disposal "No" Coeff (std err)	Waste Disposal "Yea" Coeff (std err)	School Funding "No" Coeff (std err)	School Funding "Yea" Coeff (std err)	Traffic Relief "No" Coeff (std err)	Traffic Relief "Yea" Coeff (std err)	Emergency Services "No" Coeff (std err)	Emergency Services "Yea" Coeff (std err)
Waste Disposal	0.221	1.390**						
(Specific Language)	(0.431)	(0.426)						
School Funding			1.117*	1.941***				
(Specific Language)			(0.497)	(0.368)				
Traffic Relief					-0.082	0.425+		
(Specific Language)					(0.284)	(0.231)		
Emergency Services							0.404*	1.070***
(Specific Language)							(0.201)	(0.194)
Constant	0.392	-0.083	-1.723***	-1.081***	0.268	1.465***	-0.066	-0.211
	(0.259)	(0.289)	(0.343)	(0.265)	(0.184)	(0.154)	(0.128)	(0.133)
Log-Likelihood	-173.623	-173.623	-153.023	-153.023	-557.690	-557.690	-734.017	-734.017
N	170	170	171	171	689	689	689	689
Psuedo-R^2	0.043	0.043	0.093	0.093	0.007	0.007	0.022	0.022

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $+p < 0.10$ (two-tailed). Multinomial Logit regression estimates. Dependent variable is vote for the ballot measure: yes, no, or abstain. Reference category: abstain. Standard errors in parenthesis. Traffic relief and emergency services featured the pooled student and MTurk samples.

For ease of interpretation, table 13 features the first differences when moving from the control (unspecific ballot language) to the treatment (specific ballot language) conditions for the ordered and multinomial models, *ceteris paribus*. The table shows that change in support (voting yea) is strikingly similar for both specifications. The only deviation of note is the school funding measure, but here, like all of the models, the use of a multinomial model actually suggests a *greater* effect than those reported by the ordered models. In other words, the results presented in tables 1, 3, and 4 may actually downplay the size of the treatment effect.

Table 13: Comparing Ordinal and Multinomial Models: First Differences Moving from Unspecific to Specific Ballot Language

	First Differences	
	Ordered Probit	Multinomial Logit
Waste Disposal, No	-0.220 (0.063)	-0.148 (0.072)
Waste Disposal, Abstain	-0.011 (0.013)	-0.140 (0.062)
Waste Disposal, Yea	0.231 (0.066)	0.289 (0.071)
School Funding, No	-0.127 (0.039)	0.023 (0.053)
School Funding, Abstain	-0.115 (0.037)	-0.397 (0.069)
School Funding, Yea	0.243 (0.065)	0.374 (0.069)
Traffic Relief, No	-0.067 (0.024)	-0.059 (0.028)
Traffic Relief, Abstain	-0.026 (0.010)	-0.038 (0.025)
Traffic Relief, Yea	0.093 (0.033)	0.097 (0.034)
Emergency Services, No	-0.120 (0.030)	-0.045 (0.035)
Emergency Services, Abstain	-0.010 (0.005)	-0.154 (0.033)
Emergency Services, Yea	0.130 (0.033)	0.200 (0.036)

First differences when moving from unspecific language (control) to specific language (treatment). Coefficients reported with standard errors in parentheses. Multinomial logit reference category: abstain

Education Levels and Ballot Measure Support

Footnote 17 mentioned the potential for education to moderate the effect of support for ballot measures. One might wonder if subjects with higher levels of education are more predisposed to support plebiscites, or that greater specificity may only affect those with a greater ability to understand the measure itself. The Fall 2012 experiment relied entirely on a student sample, meaning there was little variance in education levels; however, the Spring 2013 iteration pulled on students and non-student subjects (via MTurk). Consequently, it is possible to test for a moderating effect between education and ballot language specificity.

Table 14 reconsiders tables 3 and 4 using only MTurk subjects with the inclusion of a control for education (columns 1 and 3), and test for a moderating effect between education and the treatment (columns 2 and 4). Education is measured on a seven-point scale, with one being high school or less, and seven being six years of college or more.[25] As the table reveals, the education

[25]For MTurk subjects, education has a mean of 3.78 (a little less than 3 years of college) with a standard deviation

variable is not statistically significant for any of the specifications; more importantly, neither the specific traffic language nor specific emergency services language is statistically significant when interacted with education. In other words, there does not appear to be a moderating effect between the specificity of the ballot measure and a respondent's level of education.

Table 14: Traffic Relief and Emergency Services Moderated by Education

	Traffic Relief with Education	Traffic Relief Interacted with Education	Emergency Services with Eduction	Emergency Services Interacted with Education
Traffic Relief	0.396*	-0.073		
(Specific Language)	(0.162)	(0.362)		
Traffic*Education		**0.125**		
		(0.086)		
Emergency Services			0.304*	0.141
(Specific Language)			(0.146)	(0.328)
Emerg*Education				**0.043**
				(0.078)
Education Level	**-0.019**	**-0.070**	**0.012**	**-0.008**
	(0.042)	**(0.055)**	**(0.039)**	**(0.053)**
Cut Point 1	-0.996***	-1.196***	-0.201	-0.274
	(0.203)	(0.247)	(0.177)	(0.220)
Cut Point 2	-0.422*	-0.618*	0.406*	0.333
	(0.196)	(0.240)	(0.178)	(0.221)
Log-Likelihood	-200.150	-199.102	-267.044	-266.890
N	251.000	251.000	251.000	251.000
$pseudo - R^2$	0.015	0.020	0.008	0.009

$***p < 0.001$, $**p < 0.01$, $*p < 0.05$ (two-tailed). MTurk subjects. Ordered probit estimates. Standard errors in parenthesis. Education ranges from 1-7 with 1 being High School or less, 7 being 6 years or more of college.

of 1.88.