Cost Misperception and Voting for Public Goods

Corey Lang

Casey J. Wichman

Michael J. Weir

Shanna Pearson-Merkowitz

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Corey Lang[*], Casey J. Wichman, Michael J. Weir, and Shanna Pearson-Merkowitz

May 13, 2022

Abstract
Public good provision is often determined through referendums by voters, who weigh benefits against costs. We evaluate voter perceptions of the private costs of providing public goods by conducting three exit polls of New England voters and an online survey of California voters. By comparing cost perceptions to actual tax incidence, we find pervasive evidence that voters misperceive costs. Fewer than 20% of voters in our samples reported perceived costs within 25% of true costs. Further, our analysis suggests that actual costs have no statistical bearing on voter choice, but at least in the New England sample voter approval is affected by perceived costs. Thus, misperceptions of referendum costs can lead to voter choice errors, misallocation of public funds, and casts doubt on nonmarket valuation methods predicated on perfect information using voter behavior.

Keywords: Price elasticity, Perceived cost, Economics of information, Public goods, Referendum, Nonmarket valuation
JEL codes: D72, D83, H31, H41, Q24, Q51

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[*] Contact author: 214 Coastal Institute, 1 Greenhouse Rd., Kingston, RI, 02881. Email: clang@uri.edu.
We thank Danny Brent, Roger von Haefen, Christian Vossler, and conference participants at W4133 and Heartland Workshop for useful feedback. This work was supported by the USDA National Institute of Food and Agriculture, Agricultural and Food Research Initiative Competitive Program, Agriculture Economics and Rural Communities, grant number 2018-67023-27689 and the URI Coastal Institute. This research was approved by University of Rhode Island IRB (Reference #714064 and #1628790).The authors declare no financial or personal conflicts of interest.

1 Introduction

Public goods are underprovided in a pure market economy. To rectify this market failure, governments intervene in several ways. Federal and state governments can legislate standards (i.e., Clean Air Act) and budgetary provision (i.e., National Parks). Municipal, county, and state governments also rely on referendums, through which voters directly authorize expenditures for public goods. Every year, millions of Americans directly vote on public good referendums and authorize billions of dollars in public spending for schools, infrastructure, and the environment. An open question however is to what extent these mechanisms supply public goods that match preferences of constituents.

This paper examines referendum voting behavior and specifically probes voters' understanding of referendum costs. A neoclassical model of voting behavior would stipulate that a referendum states a quantity of public good and a price, and the voter performs a cost-benefit calculation to decide their choice. However, if voters misperceive costs, then those individuals' calculations could be wrong, and people may not be voting consistently with their own preferences.

We developed two surveys, an exit poll and an online survey, that asked voters how they voted or planned to vote on a local or state referendum and how much in additional taxes they would pay if the referendum passed. We incentivized accuracy in stated cost estimates by offering $5 cash to respondents for estimates within 10% of their actual cost, which was calculated in real time based on respondents' reported housing value, rent, or income and the specifics of the referendum.[1] The survey also asked about votes for governor and demographic information, as well as questions measuring financial literacy and political knowledge. We conducted our exit poll survey in three New England (NE) municipalities with municipal referendums appearing on their November 2018 ballot. We sought variation in our sample, and to this end chose municipalities in three different states, each targeting a different public good (open space, smart growth, road repairs). Since each municipal referendum was funded by property taxes, for purposes of generalizability we additionally examine a 2020 California (CA) bond referendum on stem cell research because it was funded through income and sales taxes.

[1] Bullock et al. (2015) and Zimmermann (2020) find that monetary incentives mitigate motivated reasoning, which could be an issue here if, for example, voters who rejected the referendum stated very high cost estimates to demonstrate why they voted no and perhaps how they feel about government spending in general.

We conducted this survey online using Amazon Mechanical Turk (MTurk). All surveys were anonymous and self-administered, which mitigates social desirability bias (e.g., Bishop and Fisher 1995). We collected 347 completed surveys in the NE exit polls and 442 surveys in the CA online survey. All three New England referendums passed by sizable margins, but the CA referendum just barely passed with 51.1% approval.

The first thrust of our analysis is to assess voter understanding of cost, and our findings clearly indicate that voters do not understand the personal financial consequences of their vote. In the NE sample, only 45.2% of respondents even stated a cost estimate, which is telling given the cash incentive and costless guess. The proportion of guessers was larger in the CA sample, 77.6%, but still remarkable given people are on MTurk with the expressed motivation to make money.[2] Regression analysis shows that respondents with greater financial literacy were more likely to state a cost estimate and some evidence that referendum approval, higher income, and political party non-affiliation are more likely to guess, but no other variables are statistically significant correlates of guessing. Among respondents that did register a guess, very few are close to their actual cost; only 17.7% of NE and 19.4% of CA voters guessed within 25 percentage points of the actual cost and there is no statistical correlation between perceived and actual costs. Further, there is evidence of a common pattern in guesses: perceived cost distributions are similar across the three NE municipalities and CA despite large actual cost differences, which suggests that voters may have similar beliefs that are decoupled from reality. Additional results find no consistent evidence that any type of respondent is more or less likely to have correct cost perception.

The second thrust of analysis is to estimate voter choice models and assess the degree to which approval is determined by either stated or actual cost. The NE exit poll results suggest that stated cost has a negative and statistically significant impact on approval, as would be expected for the cost parameter (downward sloping demand), but actual cost has a statistically insignificant effect and the coefficient is positive. From these models, we can estimate average willingness to pay for referendum approval, similar to the analysis of a binary contingent valuation choice experiment. Using stated cost, we estimate a WTP of $1,011, whereas using actual cost we estimate a WTP of -$163, which is unreasonable given the large margin of

[2] In both surveys, 100% of respondents who did not answer the open-ended question did answer a follow-up multiple choice question on stated costs.

approval. In the CA analysis, coefficients on both stated and actual cost have the same signs observed in the NE exit poll sample, though both are statistically insignificant from zero, indicating that neither real nor perceived cost has much influence on approval. Calculated WTP estimates are similar to the NE results with WTP equal to $1,143 using stated cost and -$114 using actual costs. The difference in statistical importance of perceived cost could be due to state versus municipal funding, but this is speculative and more research is needed.

In sum, the results provide suggestive evidence that voters are responsive to price, but have incomplete information about the correct price, and are responding to the misperceived price. The implications are that 1) the opacity of true costs may lead some voters to vote against their preferences and referendum outcomes to not reflect aggregate preferences, 2) improved understanding of prices may have a substantial impact on which referendums voters approve and align the provision of public goods with voters' true preferences, and 3) demand modeling using voting data may be biased due to voters misunderstanding of prices.

Our findings contribute to several literatures. First, this article contributes to a growing literature on cost (or price) misperceptions. In other information-scarce contexts, there is evidence that water and electricity customers misperceive prices when making consumption decisions under nonlinear rate structures (Brent and Ward 2019, Ito 2014, Wichman 2014) and that customers may be rationally inattentive to energy costs when investing in energy-using durable goods (Sallee 2014). When consumers make consumption decisions in these settings with imperfect information about price, this drives a wedge between consumers' perceived utility and experienced utility. In the parlance of Allcott and Taubinsky (2015), this "internality" captures the private costs of individuals making wrong decisions due to imperfect information. In the case of decisions that impose external costs on others (e.g., like electricity or water consumption), misperceived costs can exacerbate these externalities.[3] There is a direct correspondence with misperceptions of private costs when voting on public goods. With imperfect information about private costs, voters may vote to approve public goods, thus imposing the costs of provision on themselves and others, without fully realizing the financial

[3] In the case of residential electricity use, many studies find that customers misperceive the cost of electricity to be lower than it actually is, causing consumers to use more electricity than their fully informed self, which increases the social costs from electricity usage (e.g., Jessoe and Rapson 2014). For residential water use, however, there is mounting evidence that water customers *overperceive* the price of water (e.g., Attari 2014, Brent and Ward 2019), and thus providing customers with more information about consumption costs may increase water consumption (Wichman 2017).

burden of their decision. Or the opposite, they vote to reject and the measure fails, thus depriving the community of additional public goods even though the benefits outweigh the true costs. We extend this literature on misperceived prices by providing evidence of voters' misperception of private costs of voting for ballot measures. We find strong evidence that voters have substantial misperceptions of private costs, and because perceived costs can be an important determinant of voter approval, voting errors are likely. We present a back-of-the-envelope analysis that indicates 5.5% of voters in our NE sample and 2.9% in our CA sample would have switched their vote if misperceived prices were corrected. Scouring myriad sources (Trust for Public Land, Cellini et al. 2010, Isen 2014), between 17% and 38% of referendums are within 5% of the passage threshold, which in turn indicates a decent likelihood of externalities resulting from cost misperceptions.

This article also contributes to a strand of research in economics that uses voting data on real-world referendums to estimate voter preferences and infer valuation. Votes are a powerful source of revealed preferences that additionally can yield insights about political economy. How referendum cost is handled varies across studies. Some articles simply do not include a cost measure in their model of voter choice (Wu and Cutter 2011, Holian and Kahn 2015, Altonji et al. 2016), which is problematic because cost is correlated with socioeconomic characteristics that are included. Others make comparisons across jurisdictions and assess how aggregate support varies with the funding vehicle (e.g., property tax, bonds) and the amount of revenue to be raised (Kotchen and Powers 2006, Banzhaf et al. 2010). As these authors note, interpretation of cost is difficult in these specifications because increasing cost also implies increasing public goods. In contrast, Burkhardt and Chan (2017), Vander Naald and Cameron (2017), and Anderson et al. (2019) study statewide referendums and examine how spatial variation in household cost correlates with support and use these estimates to calculate WTP for public goods. One critical assumption of these models is that voters correctly perceive costs; if this assumption fails, as we find, then estimated WTP is unlikely to reflect true WTP.[4] However, Vossler et al. (2003) find indirect evidence that voters are well-informed about cost.[5] That being said, based on our

[4] A second critical assumption is that aggregate voting data can be used to recover individual preferences. Lang and Pearson-Merkowitz (2022) examine this issue and find aggregate voting is likely to produce biased estimates.
[5] These authors conduct a contingent valuation survey using the same language as actual referendums taking place around the same time. However, half of respondents received additional information about referendum cost, and regression results suggest that the additional information has no impact on approval. However, as discussed in the article, this particular referendum was part of the public discourse and was held in a small college town, which is not

findings on a variety of referendums, voter knowledge of costs is likely the exception rather than the rule.

Lastly, this article contributes to a long-standing debate about whether voters have enough knowledge of referendums and initiatives to choose correctly, and hence whether direct democracy as an institution can reflect true preferences. The information requirements to correctly vote with your preferences are much greater with initiatives than with electing representatives because there are fewer cues and no history with a given political party and their general policy priorities (Bowler and Donovan 2000). Bowler (2015) finds that two-thirds of surveyed voters in California felt ballot question wording was confusing and 20% were unsatisfied with the amount of information provided on the ballot. Burnett (2019) finds that voters are far more knowledgeable about endorsements than specific facts about initiatives and campaign spending helps voters learn about relevant endorsements but not facts.[6] Bowler and Donovan (2000) conclude that voters are not well informed but most use reasoning and cues to make correct choices. Our paper documents a new area of incorrect information, the individual price tag of a referendum. This in itself is not surprising given the documented ignorance about proposed policy specifics. However, we additionally find evidence that incorrect prices have a statistically significant effect on voters' choices, meaning that in this case, Bowler and Donovan's (2000) optimism about uninformed voters making correct choices may not hold.[7]

2 Conceptual framework

We begin by developing a voter choice model, and then we show how misperceived referendum costs can lead to both individual voting errors and biased estimates of willingness to pay (WTP). Consider the following voter choice model for an individual voter *i*:

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true for the vast majority of local public finance referendums. Tangential evidence of voters' understanding of financial consequences comes from empirical tests of Fischel's (2001) homevoter hypothesis, which argues that homeowners' approval of local public finance issues is based in part on the expected impact to their home value, which capitalizes both tax increases and amenity values (Cellini et al. 2010, Lang 2018). However, votes may still reflect self-interest even if benefit or cost perceptions are imperfect. Further, recent empirical tests (e.g., Dehring et al. 2008, Ahlfeldt and Maennig 2015) come from large place-based decision (stadium, airport), which is far more salient than an average referendum.

[6] Dyck and Lascher (2019) also find that state-level initiatives increase voter participation but not voters' interest in politics.

[7] Prendergast et al. (2019) conduct a national survey that elicits perceptions of the amount of local open space and then they derive actual amounts at multiple spatial scales for each georeferenced respondent. Similar to this study, there is a large degree of misperception in open space abundance, and when asked about a hypothetical open space referendum, respondents' votes were statistically responsive to perceived open space, but not actual.

$$y_i^* = \beta_0 + \beta cost_i + \boldsymbol{X_i \gamma} + \varepsilon_i \quad (1)$$

where y_i^* is the latent expected utility voter *i* would derive from the referendum if it passes, $cost_i$ is the annual dollar value of actual cost (from additional taxes) that the voter would face if the referendum passes, and $\boldsymbol{X_i}$ is a vector of voter attributes. We expect that $\beta < 0$ to be consistent with microeconomic theory. The vote cast, which is observed, is determined by the sign of y_i^*, specifically $Approve_i = 1(y_i^* > 0)$. Within this context, voter *i* would only vote in favor of a referendum that provides positive expected utility.

The model described above assumes that individual voters have full information on the costs of the policy. There are good reasons to believe that the cognitive effort to calculate changes in tax burdens are significant, especially when ballots do not communicate them clearly. To incorporate cost misperception into this framework, we include $\alpha_i \geq 0$ as a misperception parameter, such that perceived cost equals true cost scaled by misperception, $\widetilde{cost_i} = \alpha_i cost_i$. $\alpha_i = 1$ implies perfectly accurate cost perception, which is the typical modeling assumption, $\alpha_i < 1$ implies that voter *i* underestimates costs, and $\alpha_i > 1$ implies that voter *i* overestimates costs.[8] α_i can also be thought of as the ratio of perceived costs to actual costs.

Incorporating misperception into the choice model above we have the following:

$$\tilde{y}_i^* = \beta_0 + \beta \widetilde{cost_i} + \boldsymbol{X_i \gamma} + \varepsilon_i = \beta_0 + \beta \alpha_i cost_i + \boldsymbol{X_i \gamma} + \varepsilon_i \quad (2)$$

where $\tilde{y}_i^*$ is latent utility if the referendum passes with the perceived cost of $\alpha_i cost_i$. Similarly, the vote cast is defined $\widetilde{Approve_i} = 1(\tilde{y}_i^* > 0)$.

It is straightforward to show how cost misperception can influence individual voter expected utility and potentially vote choice. By differencing Equations 1 and 2 we observe the following relationships between latent utilities conditional on misperception:

$$\tilde{y}_i^* - y_i^* \begin{cases} < 0 \;\; if \;\, \alpha_i > 1 \\ = 0 \;\; if \;\, \alpha_i = 1 \\ > 0 \;\; if \;\, \alpha_i < 1 \end{cases} \quad (3)$$

In words, if costs are overestimated, then expected utility is less than if true costs were known. Similarly, if costs are underestimated, then expected utility is greater than if true costs were known. If α_i deviates substantially from one or if y_i^* is near zero, then cost misperception can lead to an error in vote choice; the real vote deviates from what is optimal under perfect

[8] This perception parameter is defined analogously in water, electricity, and energy efficiency settings where individuals misperceive the costs of their actions (Ito, 2014; Sallee, 2014, Wichman, 2014; Brent and Ward, 2019).

information: $\widetilde{Approve_i} \neq Approve_i$. In this instance, it is useful to think of the wedge between perceived costs and actual costs as an *internality*, à la Allcott and Sunstein (2015), which leads to self-caused errors in choices. Further, if a sufficient proportion of voters misperceive costs, it is feasible that the referendum will pass when it should fail or fail when it should pass and an inefficient amount of the public good is provided.

We now turn our attention to the issue of researchers analyzing voting data to understand valuation of public goods. Researchers observe real votes (or more typically aggregated real votes) that are a function of misperceived costs, but they do not observe misperception and instead calculate true costs (e.g., Burkhardt and Chan, 2017). Hence, they may estimate a logistic regression model similar to:

$$\widetilde{Approve_i} = \beta_0 + \beta cost_i + \boldsymbol{X_i \gamma} + \varepsilon_i \tag{4}$$

While regressing true votes on perceived costs or regressing votes under perfect information on true costs would both reveal true preferences, the mismatch of true votes on true costs in Equation (4) does not. Researchers are often interested in voter choice because it provides a means to estimate the value of public goods. For this, researchers can estimate average WTP using the regression estimates following the method developed by Hanemann (1984, 1989):

$$WTP = \frac{1}{-\hat{\beta}} \ln\left(1 + e^{(\widehat{\beta_0} + \overline{\boldsymbol{X}}\hat{\boldsymbol{\gamma}})}\right) \tag{5}$$

where $\widehat{\beta_0}$, $\hat{\beta}$, and $\hat{\gamma}$ are estimated logit parameters and $\overline{\boldsymbol{X}}$ is a vector of sample average values. Given the inaccuracy of parameter estimates from Equation (4), average WTP will be incorrect as well.

What we hope to communicate through this conceptual framework is that voting behavior and analysis of voting behavior both hinge critically on voters understanding their true individual costs. In the following sections, we examine the extent to which perceived costs deviate from actual costs among voters and how those misperceptions can be consequential for voters and analysts.

3 Data

3.1 New England referendums and actual cost calculation

We surveyed three municipalities that held referendums in the November 2018 election. We chose municipalities that offered variation in geography, the purpose of the referendum, and the financing mechanism. Table 1 provides summary information about our three survey towns: Cumberland, RI, Plainville, MA, and South Windsor, CT. Cumberland's referendum was for street pavement improvements and requested authorization for the town to sell $2.5 million in bonds. Plainville's referendum was to adopt the Community Preservation Act (CPA), which is a statewide Massachusetts program that provides state matching funds for municipalities that increase property taxes for the CPA. Funds raised through the CPA must be spent in three categories, open space conservation, affordable housing, and historic preservation, and each category must get at least 10% of funds raised.[9] Plainville proposed a 1% increase in property taxes, applied only to property value exceeding $100,000. South Windsor's referendum focused on open space acquisition and requested authorization for the town to sell $2 million in bonds. All three referendums passed by large margins, ranging from 66-88% approval.[10]

For each referendum, we calculated how much each household could expect to pay in additional property taxes. This calculation is straightforward for Plainville as it only requires an individual's property value. For municipalities issuing bonds, the calculation requires calculating the total property tax base in a town and each housing unit's share of that total. For renters, we estimate the sales equivalent of their property and assume that there is perfect pass through of additional property taxes to rental rates. Table 1 presents average household costs and Table A.1 in the online appendix presents a more detailed picture of expected payments for different housing values and rents. For Cumberland, RI and South Windsor, CT, we report total costs, because this is how our cost perception question was phrased, but also report annual costs in square brackets below for comparison purposes.[11] For example, the total average household cost in Cumberland is $184.71, and we calculate that the corresponding annual tax payment is $19.50.

3.2 Survey design and implementation

For the NE exit poll, teams of three trained, graduate and undergraduate student pollsters were stationed at three polling locations, one for each sample town, starting at 7am and staying until dusk. Pollsters would approach voters as they left the poll, asking if they would be willing to take a survey that was confidential, anonymous, and to be used for research purposes only.

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[9] See https://www.communitypreservation.org/ for more information.
[10] The online appendix presents exact wording of each ballot question.
[11] We convert these total bond costs to annual using an assumption of a 10-year payback with 1% interest.

Surveys were self-administered and completed on tablets, which mitigates social desirability bias. Teams were also equipped with pop-up canopies in case of rain, and students wore University of Rhode Island clothing and buttons that identified them as a “University of Rhode Island Exit Poller.”

The key question of the survey asked respondents about their understanding of their personal cost of the referendum they just voted on. The exact question wording was: “If the [referendum name] is approved, how much do you think your household will end up paying for it [per year]?”. “Per year” was only included in Plainville because that referendum’s cost was stated as an annual property tax increase. The question intentionally did not mention payment mechanism (property taxes) in order to not confuse renters, who may not connect their rental rates to property taxes. Importantly, the survey offered a fill-in-the-blank box for their response. This format avoids anchoring bias that could be present if we offered multiple choice options or a slider bar with a given range. To be clear, the question was designed to elicit voters’ *perception* of how much their household would pay if the referendum passes, rather than what they are willing to pay for implementing the referendum. Anchoring bias could obfuscate misperception assessment.

In order to incentivize truthful and thoughtful responses, the question said that if their estimate was correct, they would be paid $5 cash. When recruiting voters, pollsters also advertised that participants could earn $5. Later in the survey, we asked whether they rented or owned, and subsequently how much their house was currently worth or how much they currently pay in monthly rent. For both the house value and rent question, the survey initially offered a fill-in-the-blank box for the response, but if respondents selected that they did not know, they were then prompted with the same question, but given multiple choice options. Fully 100% of respondents who did not answer the open-ended cost question did answer the multiple choice question, which suggests that nonresponse is a function of high cognitive costs rather than this question being “sensitive” or voters being too time-constrained to register a guess. Note that although we are surveying voters on their perceived costs, we are not eliciting their willingness-to-pay for the referendum, which would be more in line with stated-preference studies. Rather, we ask voters about their perceived private costs after they just voted for/against the referenda; this is more akin to intercepting customers after grocery shopping and surveying them on the salience of tax-inclusive prices for good they just purchased (e.g., as in Chetty et al. 2009). We programmed the survey in Qualtrics and had logical expressions embedded to determine if they got the amount correct based on their reported housing value (within 10% of what we estimated), and then displayed a final screen that stated if they got it correct and were entitled to $5.[12]

The first two questions of the survey asked how the respondent just voted for governor and for the referendum. The exact wording of the referendum was presented in the survey when asking how they voted. These questions were followed by the cost question, after which the survey additionally asked about the respondent's household income, age, and educational attainment. Lastly, the survey asked two questions measuring financial literacy and one measuring political knowledge. The financial literacy questions concerned bank accounts, interest rates, and inflation rates, and were taken directly from Lusardi and Mitchell (2014). The political knowledge question asked about John Roberts' job.[13]

3.3 California Proposition 14, actual cost calculation, and survey design differences

For reasons of generalizability and potential concerns about relying on self-reported property values to measure misperception of costs (see footnote 12), we chose to also survey voters in a statewide referendum, funded by income and sales taxes, in a different part of the country. Further, one can imagine scenarios in which voters are more or less informed about municipal versus statewide referendums, and similarly scenarios in which property taxes may be more or less salient than income and sales taxes.

[12] One concern is that respondents may inaccurately report the market value of their home due to ignorance, which would lead to measurement error in our actual cost calculation and incorrect conclusions about cost misperception. Our survey was designed to be anonymous to mitigate social desirability bias, and thus we did not ask for their address and cannot validate their house value. However, in a non-anonymous survey in Rhode Island, Bakkensen and Barrage (2021) find a very strong correlation (ρ=0.89) between self-reported market value and the Zillow Zestimate, suggesting reassurance with our cost calculation. Further, in 2018, the New England property market was quite stable, with only modest appreciation over the past five years, unlike the rapid appreciation occurring during 2020-2021, which would give owners plenty of time to learn the value of their home. Lastly, while some error is expected in a respondent's estimate of their home's value, the magnitude of errors in referendum cost estimates cannot be due to this error alone. In our NE Exit Poll sample, 64% of respondents guess a referendum cost that is over 50% off of their true cost. If that was entirely due to inaccuracy in home value estimates, it would be as if a person with a home worth $300,000 reported it having a value less than $150,000 or greater than $600,000, which seems implausible. Further, approximately equal numbers of respondents guessed above true cost as below, indicating our cost calculation is not uniformly biased upwards or downwards. Finally, the way we calculate an individual's tax burden is to distribute the total tax bill of the referendum to all properties proportional on value. So, if all properties are assessed at a lower percentage of their market value, that would not affect the distribution of this new tax bill and the resulting estimates of private costs.

[13] Complete text of the survey is available in the online appendix.

As a second case study, we chose California Proposition 14, which appeared on the November 3, 2020 general election ballot and, if passed, would authorize the state to issue $5.5 billion in general obligation bonds for stem cell research. Bond repayment comes from general revenue, which is over 90% personal income and sales taxes. The referendum barely passed with 51.1% approval. See Table 1 for a summary and the online appendix for exact ballot language.

Accounting for interest payments, the California Attorney General estimated bond repayment would cost the state $260 million per year for 30 years (this estimate actually appears in the language on the ballot). Using tax rates and population statistics, we determined the proportion of annual Proposition 14 payments that would be paid by different income groups and then calculated lower and upper bounds of annual payments by income bin, which are displayed in appendix Table A.2. The average cost to CA households is about $20.53, but the median is only $10.26. Full details on our cost estimates can be found in the appendix.

We recruited online survey respondents through Amazon Mechanical Turk (denoted "MTurk") between October 27 and November 4, 2020. Eligible respondents were 1) aged 18 years or older, 2) lived in California, and 3) were planning to vote or already voted in the 2020 election. Respondents were guaranteed compensation of $1.00 upon completion of a "10-minute Academic Study". It was advertised in the task description that respondents would have the opportunity to earn bonus payments in addition to the guaranteed $1.00 compensation. We implemented best practices from the literature to avoid multiple survey attempts by the same MTurk workers paired with methods to screen out respondents attempting to take the survey from outside the United States (Moss and Litman 2018; Kennedy et al. 2018; Waggoner et al. 2019). Further details on survey distribution can be found in the appendix.

After answering a set of basic demographic questions (race, income, gender, education, home ownership status), respondents were shown the text of Proposition 14 from the California state ballot and asked how they voted/planned to vote on Proposition 14. Mirroring the New England survey, respondents were then presented with the key question of the survey that asked them to estimate how much Proposition 14 would cost their household annually if it were to pass using a fill-in-the-blank box for their response. Respondents were told at the end of the survey whether their guess was correct to incentivize completion of the full survey. If respondents correctly estimated their household cost within 10% of our estimated range (similar to the New England survey), they were awarded $5 upon completion of the survey. Similar to the NE Exit

Poll survey, if respondents did not answer the open-ended question, we provided them with a follow-up multiple choice question on their private cost. Respondents were randomly shown one of three sets of answer choices to avoid anchoring bias. The survey concluded with questions regarding political affiliation, financial literacy, and political knowledge. We chose to ask about general political affiliation instead of vote for president because President Trump was a polarizing and controversial candidate.

3.4 Summary statistics

A total of 347 voters completed our NE exit poll and 428 people completed our CA online survey. Each of these sample sizes were the result of a series of sample cuts to account for incompleteness and data quality checks. The sample cuts for the NE exit poll were straight forward: a respondent was dropped from the exit poll sample if they did not complete the full survey. In the MTurk sample, respondents were dropped if they did not complete the full survey or completed the survey in less than one-half the median sample response time. Further, some MTurk respondents had extremely high cost estimates, the likes of which were not present in the NE sample. Seven respondents guessed $260 million, equal to the total annual cost for the whole state, and 13 more guessed over $100,000. We assume these types of respondents did not understand the question and thus should not be included in the sample. We exclude respondents from the CA sample if their stated cost estimate was greater than $1000. We acknowledge that this cutoff is arbitrary and perform a sensitivity check on this sample truncation in the appendix.

In Cumberland and South Windsor, our sample approved of the referendum at a similar proportion to the official vote (86.7% approve in Cumberland and 75.8% approve in South Windsor). However, in Plainville, our sample consisted disproportionately of approvers – 77.1%, which is over 11 percentage points higher than the official count. A similar situation occurred in the CA online survey in which 78.5% of the sample voted to approve Proposition 14 which is 27.4 percentage points higher than the true outcome. To counteract this, we calculate sample weights that equate our sampled approval rates to official outcomes in all sample locations. This has little consequence for respondents in Cumberland and South Windsor, but for Plainville and the CA online sample the weights give more statistical influence to respondents that voted "reject".

Table 2 presents summary statistics for both samples using sample weights. In New England, about 77% voted in favor of the studied referendums and about 48% voted for the Democratic nominee for governor[14], while 51.1% of the California sample voted in favor of the referendum and 39.7% reported a Democratic affiliation. About 70% of both samples report having a college degree, which is considerably higher than the general population in each geographic area sampled and the national average of 31.5%. It is well-documented that the voting population does not mirror the general population; voters tend to be better educated, wealthier, and older (Leighley and Nagler 2013, Lang and Pearson-Merkowitz 2022). On average, respondents answered 1.3 of the two financial literacy questions correctly in New England while respondents in California answered 1.5 of the two questions correctly, which is consistent with recent survey evidence from the general population (Lusardi and Mitchell 2014). About 60% answered the political knowledge question correctly in New England while about 80% answered the same question correctly in California.

Only 45.3% of New England respondents decided to answer the question about referendum cost while 83.5% of the California sample answered the cost question.[15] We interpret the lower proportion of cost guesses in the New England sample as strong evidence that many voters are uninformed about costs. If respondents checked "do not know" they were provided with a follow-up multiple choice question. In both samples, 100% of respondents answered the multiple choice question (N=190 in NE Exit Poll; N=96 in CA MTurk), which we interpret as evidence that the cognitive costs of even registering a guess are large without having multiple choice answers to provide reasonable bounds on private costs. The second column gives sample statistics for the 157 and 346 respondents that did answer the cost question in the NE and CA samples, respectively. On average, New Englanders overestimated how much their household would pay, guessing $243 when it was closer to $187. The degree of overestimation was substantially higher by Californians who guessed their household cost to be $144 while the actual cost was closer to $15 on average for our respondents. However, these statistics hide substantial heterogeneity by town and by individual, which will be explored in the next section.

[14] 43.3% voted for the Republican nominee, and the remainder voted for an independent. No independents ran in Massachusetts.

[15] Note that we present summary statistics using sample weights. The sample sizes among all CA MTurk respondents and those who answered the referendum are 428 and 332, respectively, meaning ~78% of respondents answered the cost question. This differs from the 83.5% statistic presented in Column 1 of Table 1 because of the application of sample weights.

Several characteristics are similar between columns, with referendum approval, governor choice (in New England), age, political knowledge, and the distribution of responses all being about the same across those who guess and those who do not. We examine determinants of answering the cost question and correlates of accuracy using multiple regression in the next section. For most analyses, we omit respondents that did not answer the open-ended cost elicitation question. While these respondents did answer the multiple choice cost elicitation question, in hindsight, the choices presented were too accurate. Pooling both types of respondents makes the multiple choice respondents appear more accurate, but that is purely an artifact of the small range of options in the question.

4 Results

4.1 Cost misperception

4.1.1 Evidence of misperception from raw data

We first document evidence of voters' misperception of the private costs associated with their vote. In Figure 1, we plot the distribution of stated costs from the NE Exit Poll sample for each town separately. As shown, voters in each city exhibit a range of stated costs with a long right tail. Although there is apparent bunching at zero, only 7 voters (4.4%) responded with a stated cost of $0. On average, voters appear to possess little information about actual costs. Mean stated costs for Cumberland, Plainville, and South Windsor are $198, $300, and $216, while mean actual costs for these voters are $308, $45, and $243.[16] So, Cumberland and South Windsor voters underestimate their private costs, while voters in Plainville drastically overestimate their costs. We present an analogous figure for our CA MTurk sample in Figure 2. Similarly, many respondents reported a low stated cost, but there is a long right tail that suggests substantial overestimation of private costs.[17] Average stated costs for these respondents are $98.78 relative to a weighted sample mean of actual costs of $15.06.

All densities have a fairly similar distribution with significant mass at small dollar amounts, mass declining with dollar increases, and a long right tail. The similarity in distribution

[16] Note that these mean actual costs differ from those presented in Table 1 because these are weighted sample averages.

[17] In the case of California and Plainville, the range of stated costs is large relative to the average true value. We investigate accuracy in finer detail for those guessing small costs in Appendix Figures A.1 and A.2, which limits the range to $500 in the NE Exit Poll Sample and $150 in the CA MTurk sample. The figures indicate that the vast majority of voters perceive costs substantially different than actual costs.

is notable given the significant differences in costs. This pattern may suggest a common distribution of beliefs that is decoupled from the specifics of a given referendum.

4.1.2 Correlation with true cost

Next, we explore the correlation between stated and actual costs in a regression framework to control for characteristics of the voter. In Table 3, we regress stated cost on actual cost and control for a variety of individual characteristics for both of our survey samples. In Column 1, we present unconditional correlation between stated and actual cost for our NE Exit Poll, and find a negative but small and insignificant relationship between stated and actual cost. We also explore the correlation between actual and stated costs for respondents who did not answer the open-ended cost question, but did answer the multiple choice question. These results are presented graphically in Figure A.3 and show virtually no correlation between stated and actual costs, even when answers are constrained by reasonable choices in the "ballpark" of actual costs. In Column 2, we add town fixed effects and the suite of individual characteristics from Table 2. The results in Column 2 suggest that there is a positive and larger conditional correlation, however this relationship is not statistically significant. None of our other voter characteristics are meaningfully correlated with stated cost, suggesting that no single characteristic changes the cost that a voter states in a systematic way. In Columns 3 and 4, we repeat this analysis with our CA MTurk sample.[18] The unconditional correlation in Column 3 is positive and somewhat large, but not statistically different from zero. When we add city fixed effects and additional individual characteristics as controls in Column 4, the correlation between stated and actual costs remains positive and statistically insignificant. Notably, no other characteristics correlate significantly with stated cost in this specification, including an indicator for whether the voter has already voted, which corroborates the findings from NE that there is not any one group that routinely guesses high or low and misperception is spread throughout the population.

[18] As noted previously, we trim our sample to include only respondents who reported a stated cost of $1000 or less to remove extreme outliers. We perform a sensitivity check on this sample truncation in Table A.3 in the appendix by re-estimating these correlations for the full sample (including all respondents) as well as respondents with stated cost below $5000, $1000 (for comparison), and $500. The full, unrestricted sample produces nearly meaningless results due to outliers with unbelievably high reported costs (i.e., orders of magnitude greater than the respondent's reported annual income). For stated costs of $5000, $1000, and $500, however, we find relatively strong agreement, with statistically similar results across all groups.

4.1.3 Determinants of misperception

Our descriptive analysis so far provides strong evidence that voters misperceive the costs associated with voting for/against the referendums. We now construct several variables that capture misperception empirically and explore the correlation between voter characteristics. We define the following misperception variable, which is the empirical analog of α_i in Section 2:

$$\alpha_i = \frac{Stated\ Cost_i}{Actual\ Cost_i} \tag{6}$$

Using α_i, we create several variables to explore misperception. First, we construct a dummy variable for an "accurate" guess which we define as $\alpha_i \in [0.75, 1.25]$ or, in other words, whether voter's stated cost fell within 25 percentage points of the actual value. For our CA MTurk sample, because the actual value is defined by a range, we allow for a 25-percentage point buffer above the maximum or below the minimum of that range. In our NE Exit Poll sample, 17.7% of voters registered an accurate guess according to this definition, whereas it was 19.4% in our CA MTurk sample. Additionally, we construct a binary variable for overperceiving costs equal to one if $\alpha_i > 1$ and zero otherwise. In our NE Exit Poll sample, 40.2% of voters overperceive their private cost of the referendum. The same statistic for our CA MTurk sample is 55.3%.

To unpack the drivers of misperception, we regress several outcome variables, including whether a voter registered a guess for the stated cost, as well as our two variables that capture misperception of costs, on individual characteristics and town fixed effects. We estimate these relationships using logit regression with sample weights to adjust our sample to the observed referendum outcome.

Table 4 presents the results of the misperception analysis for both samples. Parameters shown are marginal effects from our logit regression evaluated at sample mean values. Columns 1 – 3 show results for the NE Exit Poll sample and Columns 4 – 6 show results for the CA MTurk sample. In Columns 1 and 4, the dependent variable is a dummy variable for whether the voter registered a guess at the bond cost. In Column 1, we find that neither whether the voter approved the bond or whether they voted for a Democratic governor display an economically meaningful correlation with the likelihood that the voter registered a cost guess. Income is strongly positively correlated with guessing; middle income voters were about 18 percentage points more likely to guess than low income and high-income voters were 31 percentage points more likely. Additionally, financial literacy increases the likelihood that a voter registered a guess by 17 percentage points for each financial question answered correctly. Our measures of education, whether the voter rents, political knowledge, and age are all statistically insignificant and point estimates are substantively small. The parallel analysis for the CA MTurk sample indicates some similarities and some differences. Similar to the exit poll, financial literacy increased the likelihood that a voter guessed at their cost, and education, whether the voter rents, political knowledge, and age are all statistically insignificant. In contrast, we find that respondents who voted to approve the referendum were less likely (by 11.8 percentage points) to report a cost guess and income did not correlate with guessing. While Democrats and Republicans have similar likelihood of guessing, as in NE, we find that political independents were more likely to report a cost guess in CA.

These results suggest that there are some small differences in the population who answered the open-ended cost question. Particularly, more financially literate and wealthier respondents (in the NE Exit Poll) are more likely to answer that question. This brings up a question of selection into registering a guess: we might believe that respondents who answered the cost question will be more informed voters because they took the time to consider and answer the open-ended question. But, as we show, even this sample of potentially more informed voters who guessed at their costs did not report accurate costs. Further, all of the nonrespondents to the open-ended question answered the multiple-choice choice question, but there is little correlation between stated and actual costs among this set of voters as well (as shown in Figure A.3).

In Columns 2 and 5, the dependent variable is a dummy variable for whether the voter registered a guess that was accurate, defined as being with 25% of the correct value (NE) or range of values (CA). In Column 2, among all the variables considered, only the effect of renters is statistically significant. Renters are 11 percentage points less likely than homeowners to state costs accurately, which could reflect a disconnect between paying rent and property taxes (Oates 2005).[19] Interestingly, financial literacy, political knowledge, and education are not correlated with accuracy. In Column 5 (CA), many of the same variables as with NE do not have a statistically significant effect on accuracy: approval, democrat, age, education, and political

[19] Because we anticipate that renters may not have good information on how bond costs translate into tax burdens, we perform a sensitivity check of the previous analysis excluding renters. This analysis is presented in online appendix Table A.6.

knowledge. The results do suggest that political independents are less likely to state an accurate private cost (by 11.6 percentage points) despite being more likely to register a guess. We also find that respondents with incomes greater than $100,000 were about 10 percentage points less likely to answer the cost question correctly. Respondents who answered the financial literacy question correctly were about 8 percentage points more likely to answer correctly. Notably, having already voted does not influence whether the respondent reported their private cost correctly.[20]

In Columns 3 and 6, the dependent variable is a dummy variable for whether the voter overperceived costs (stated > actual).[21] In Column 3 (NE), the only statistically significant effect is our measure of financial literacy, which reduces the likelihood of overperception by 11 percentage points. On average, voters who approved the bond were less likely to overperceive the bond cost by about 17 percentage points, which could be related to valuation, but this effect is not statistically significant. In Column 6 (CA), we find that respondents who approve of the referendum, who have already voted, are in the highest income bracket, and have high political knowledge are all less likely to overperceive. Political independents are more likely to overperceive, and financial literacy is uncorrelated with overperception.

Overall, in Table 4, we find very little consistency across columns and across samples in what correlates with cost perception accuracy. This finding bolsters the conclusion from Table 3 that no single group is more or less knowledgeable about true costs and that misperception is widely and evenly spread throughout the voting population.

4.2 Voter choice

The second thrust of analysis is to examine vote choices, with a focus on the effect of cost. In order to make costs comparable across referendums, which in turns enables WTP calculation, we use annual costs in our model. Plainville, MA and California residents reported annual cost guesses, but Cumberland, RI and South Windsor, CT residents were asked about

[20] In Table A.4 and A.5, we explore the sensitivity of our definition of "accurate" perceptions. For the NE Exit Poll Sample, our results for +/-10 percentage points are largely consistent with the +/- 25 percentage points definition. When we expand the definition of accuracy to +/- 50 percentage points, we find that renters are even less likely to register accurate guesses, which suggests that renters likely possess very little knowledge of the costs that accrue to property owners.

[21] Note that, by symmetry, the coefficients in Column 3 can be multiplied by -1 and interpreted as changes in the likelihood that the voter *underperceives* her private costs.

total costs. We convert these total bond costs to annual using an assumption of a 10-year payback with 1% interest.[22] Following our framework in Section 2, we estimate the following model of voter choice:

$$Approve_i = \beta_0 + \beta Cost_i + \boldsymbol{X_i \gamma} + \varepsilon_i \tag{7}$$

where $Approve_i$ is an indicator equal to one if voter *i* voted for the referendum and zero otherwise. $Cost_i$ is the annual dollar value of either the stated or actual cost for voter *i*'s household . $\boldsymbol{X_i}$ is a vector of voter attributes, including partisan affiliation, income, education, age, and the two measures of financial literacy and political knowledge. $\boldsymbol{X_i}$ also includes indicator variables for town or city, which capture differences in the referendums and other observable and unobservable differences in the towns. We estimate this equation using logit with sample weights for our NE Exit Poll sample and our CA MTurk sample separately and using stated or actual cost separately.[23]

In Table 5, we present marginal effects from estimation of Equation (7) evaluated at sample averages. Columns 1 and 2 present results for NE Exit Poll sample and Columns 3 and 4 are for the CA MTurk sample. In Column 1, we include stated, or perceived, cost as the cost variable although we divide actual and stated costs by 1000 to make coefficients more legible. The marginal effect is -0.28, which is statistically significant at the $p<0.05$ level. The sign of this coefficient conforms with our intuition: when perceived costs increase, voters are less likely to approve the bond. This is akin to a demand curve sloping down. The magnitude implies that, on average, a $1000 increase in the annual *perceived* cost of the public good reduces the likelihood that an individual votes in favor of the referendum by 28 percentage points, all else equal. Of course, a $1000 increase in cost is unrealistic since the average annual stated cost for our NE Exit Poll sample is $113 (average annual actual cost is $35). In more sensible units, a $10 dollar increase in perceived cost decreases the likelihood that an individual votes in favor of the referendum by 0.28 percentage points. This effect makes intuitive sense and its magnitude should reveal the tradeoff voters are willing to make to supply the public good. In Column 2, we

[22] We do not know the actual horizons and rates used by Cumberland and South Windsor, but 10-year and 1% are common values in the municipal bond market. We perform sensitivity checks on these assumptions in the online appendix and results are nearly identical.

[23] Equation 7 with actual cost mirrors the specifications used by many observational studies of voter choice (Vossler and Kerkvliet 2003, Burkhardt and Chan 2017, Vander Naald and Cameron 2017, Lang and Pearson-Merkowitz 2022), which is our objective. It is important to note that this is a descriptive regression model and may not have a causal interpretation because there may be unobserved determinants of approval that are correlated with cost.

include actual cost as the cost variable. The marginal effect is a positive 134 percentage point increase for a $1000 increase in stated cost and is statistically insignificant. The sign of this effect is counterintuitive as it violates the law of demand. However, the confidence intervals are large suggesting that actual cost has no real bearing on voter behavior. The magnitude implies that a $10 increase in the annual *actual* cost of the public good increases the likelihood that an individual votes in favor of the referendum by 1.34 percentage points, but this estimate is statistically similar to zero.

Transitioning to the CA sample, Columns 3 and 4 in Table 5 have the same pattern, we include stated cost and actual cost sequentially in the voter choice regression. In Column 3, we estimate a negative relationship between referendum approval and stated cost. A $10 increase in stated annual cost reduces the likelihood that a respondent voted for the referendum by 0.17 percentage points. This effect is similar in magnitude when compared to the NE Exit Poll estimate in Column 1, but it is not statistically different from zero. In Column 4, we estimate a positive and insignificant relationship between approval and actual cost. In this specification, the magnitude of the effect is similar to that of the NE sample – a $10 increase in *actual* annual costs is associated with a 1.50 percentage point increase in the likelihood of approval.

To summarize, across all four specifications, only the coefficient on stated costs in the NE Exit Poll sample exhibits a statistically significant effect. For actual costs (in both samples) and stated costs (in the CA MTurk sample), the cost coefficients are not statistically different from zero. Further, actual costs in both samples have a counterintuitive sign. Overall, actual cost has no statistical influence on voter approval, however this may not be because voters are indifferent to costs when deciding how to vote. Rather, our results are suggestive of imperfect information. Voters misperceive costs and they may determine approval based in part on those misperceived costs. We can discern this conclusion, in part, because the coefficient on perceived cost is statistically significant and the expected sign in our NE Exit Poll sample.

Only a few of the other estimated marginal effects are statistically significant. Results suggest that Democrats are more likely (and, to a lesser extent, Independents) to vote approve in both samples, which is consistent with the party's general ideology on public goods and government spending, and consistent with many prior public good voting studies (e.g., Holian and Kahn 2015, Altonji et al. 2016). We also find that financial literacy negatively affects approval by a large margin in both samples. For each financial literacy question answered correctly, approval drops 12-16 percentage points, on average. To the best of our knowledge, no other paper has tested how financial literacy correlates with referendum approval. It is difficult to speculate about why this might be because we are conditioning on understanding of costs, and through Table 4 we have already established that financial literacy only weakly correlates with correctly perceived costs. One possibility is that voters who are well versed in banking, interest rates, etc., have a kneejerk adverse reaction to spending money. For the CA MTurk sample, we also find that political knowledge and having already voted also reduces the likelihood that respondents vote to approve the referendum, and a college degree is increases the likelihood, the latter perhaps due to a familiarity with stem cell research or support for research in general.

4.3 Willingness-to-pay estimates

In addition to estimating the determinants of voter choice, we can use these results to estimate willingness to pay for these public goods. The objective of this analysis is not to calculate a true WTP for the public goods in question, but to illustrate the magnitude of the wedge between voters' WTP for public goods using perceived versus actual private costs. Economic theory would predict a positive willingness-to-pay for desirable public goods. A negative WTP would indicate public bads – provision that actually harms society.

Following the method developed by Hanemann (1984, 1989) and used in the context of voting by Vossler and Kerkvliet (2003), Burkhardt and Chan (2017), Lang and Pearson-Merkowitz (2022), among others, WTP is calculated as follows:

$$WTP = \frac{1}{-\hat{\beta}} \ln(1 + e^{(\widehat{\beta_0} + \overline{\boldsymbol{X}}\,\hat{\boldsymbol{\gamma}})}) \tag{8}$$

where $\hat{\alpha}$, $\hat{\boldsymbol{\gamma}}$, and $\hat{\beta}$ come from the logit results of the estimated voter choice model. $\overline{\boldsymbol{X}}$ is a vector of sample average values, and thus we estimate WTP for the average voter in our sample. We calculate WTP separately for the models using stated and actual costs, and we calculate standard errors using the delta method.

We present our estimates of WTP at the bottom of Table 5. For the NE Exit Poll sample, our estimated WTP using stated costs is \$1,011 with a standard error of \$371. When using actual costs, however, we calculate a WTP of -\$163 with a standard error of \$318. So, estimated WTP using stated costs makes economic sense and is consistent with these referendums passing by large margins. In contrast, our estimate of WTP using actual cost is senseless because it implies that referendum passage reduces welfare. For the CA MTurk sample, our estimates illustrate a similar story as the NE Exit Poll sample, WTP is $1,143 using stated costs and -$114 using actual costs. Notably, across both samples, the sign for WTP based on actual costs is negative, which is unbelievable primarily because all referendums passed, suggesting that voters valued the public goods positively. WTP based on stated cost, however, has a sensible sign across both samples, though is larger in magnitude than would be expected for projects of this scale. All WTP estimates, however, do have large standard errors.

4.4 Robustness checks and sensitivity analysis

We implement several sensitivity checks for our analysis, some of which have been mentioned already, and present these results in the online appendix. In Table A.8, we present coefficients for the voter choice model except estimated using a linear probability model. Coefficients on stated cost and actual cost are very similar to Table 5, though in the NE Exit poll the coefficient on stated cost is now statistically significant at the 1% level and the coefficient on actual cost increases in magnitude but is not statistically different from the logit estimate. We find other variables, such as partisan affiliation, financial literacy, and political knowledge, to have similar effects on vote choice as in the logit specification.[24]

Given some uncertainty about cost calculations with the New England referendums, we additionally assess the sensitivity of results to varying (a) the conversion rate from rental rate to property value and (b) the terms of the annualized bond cost (using combinations of 10- to 15-year payback horizons at 1% or 2% interest rates). Our results are qualitatively identical across different rental conversion rates, which only affects the actual cost variable (as shown in Table A.9), and for three different combinations of bond horizons and interest rates (as shown in Tables A.10–A.12).

In our last robustness check, we revisit our sample truncation for the CA MTurk sample to explore how extreme outliers affect our voter choice estimates. In Table A.13, we present

[24] In the NE Exit Poll sample, only 9.1% of our sample are renters, and all of them voted to approve, so the logit cannot estimate a marginal effect for renters. One advantage of the linear probability model is that we can include renters as a variable for the NE Exit Poll sample. In Table A.8, the coefficient on renter is positive and large in magnitude in both columns, and is statistically significant in Column 2. This is a relatively common finding in public good voting, which Oates (2005) dubbed the “renter effect.” Oates argues that homeowners are more likely than renters to be aware of their tax obligations related to local public spending and thus less likely to approve. However, in this setting, we are controlling for perceived or actual cost, which calls into question the traditional explanation for renters’ voting behavior.

results for the voter choice regressions that include four different samples: the full sample, including all respondents to the MTurk survey, as well as samples that exclude respondents with stated costs greater than $5000, $1000 (our main sample), and $500. Across all samples, none of the cost coefficients are statistically different from zero except for stated cost in the full sample, which includes annual stated costs that exceed respondent annual income. The magnitudes do change across samples, however. For the full sample, the stated cost coefficient is -0.00029 and increases to -0.031 for the <$5000 sample, -0.17 for the <$1000 sample, and then decreases to -0.019 for the <$500 sample. This comparison suggests, perhaps unsurprisingly, that removing extreme outliers in stated cost is important for determining how stated costs affect referendum approval. The coefficient on actual cost is -1.02 for the full sample and -0.39 for the <$5000 sample, which is the opposite sign than our primary results in Table 5. The coefficient becomes positive for the <$1000 sample and increases to 2.75 in the <$500 sample. Despite this sign flip as a result of the sample truncation, the standard errors on the actual cost coefficient are quite large, which suggests no statistical difference between any of these cost coefficients. This sensitivity test continues to imply that, for the CA MTurk sample, costs are not a strong determinant of voter choice.

5 Discussion and Conclusion

This paper presents primary data collected from voters immediately after voting as well as reported voting behavior via an online survey. We elicited incentivized estimates of private costs of public goods referendums, as well as approval of said referendums and political and socioeconomic information. We provide strong evidence that the majority of voters misperceive the private costs of providing public goods through referendums. We find no evidence of systematic under- or over-estimation of costs, nor any specific type of voter who is more or less informed. Given the widespread imperfect information, it would be reasonable to conclude that voters do not consider costs when voting. However, we find this to be false. Analyzing voter approval, we find that perceived cost does affect approval in one of our samples and is negative, as would be expected.

There are two implications. First, these findings limit the degree to which voting behavior might reveal the value of public goods, unless information about perceived cost is collected. Using voting behavior and actual costs to infer willingness-to-pay for public goods hinges critically on the assumption that voters possess full information. Thus, any valuation method that assumes full information about actual costs may provide misleading results.

Second, because voters misperceive costs but still partially base approval on costs, they may be making errors at the ballot box. Information provision could correct both internalities (e.g., voting for the referendum when in fact a voter would prefer not to provide the public good at its true private cost) and externalities (e.g., voting for the referendum and incurring its cost on other taxpayers). To assess the magnitude of this possibility in our setting, we provide a back-of-the-envelope analysis. Using our primary voter choice regression estimates with stated cost as an independent variable (Columns 1 and 3, Table 5), we predict each individual's vote based on 1) stated costs and 2) what their vote would have been if they had perfect information about their true costs.[25] Thus, we have two predicted votes for each individual. Using these predictions, we can calculate how many respondents would have changed their vote upon receiving updated, accurate information. For our NE Exit Poll sample, we estimate that 5.5% (SE = 1.8%) of voters would have changed their vote with perfect information. Now, is 5.5% of voters making an internality error enough to cause an externality of the referendum outcome being counter to median preferences? Recall that our referendums in the NE Exit Poll sample passed by approximately 75%, so a 5.5 percentage point change in votes is not consequential for these outcomes. For closer referendums, however, it is feasible that reconciling cost misperceptions could be pivotal for providing the quantity of the public good that accords with voter preferences. Within our CA MTurk sample, the same statistic suggests that 2.9% (SE = 0.93%) of respondents would change their vote if given perfect information. This statistic is smaller because the difference in stated and actual costs is smaller in the CA MTurk sample. That said, the CA referendum passed with 51.1% approval, which suggests that a 2.9% change in voter choice is nontrivial when considering the implications of referendums that pass (or do not pass) by narrow margins.

There is no comprehensive database of all referendums held in the United States, but we use a variety of sources to assess how frequently referendums fall within 5% of the passing threshold. Of referendums regarding land conservation, 22.8% are within 5% (Trust for Public

[25] We assign a vote of approve if the predicted probability is greater than or equal to 0.5. The predicted probability with true costs is calculated using the model coefficients from Table 5, Column 1, but using the values of true costs instead of stated costs.

Land); of municipal school spending referendums in California, 34.8% are within 5% (Cellini et al. 2010); and across a range of jurisdiction types and spending purposes in Ohio, between 17.4% and 38.1% of referendums are within 5% (Isen 2014).[26] The number of close outcomes suggests that externalities of individual cost misperceptions are likely to be present in many referendums across the country.

One concern about our analysis is that our studied referendums may not be the best examples of voters needing to be well-informed. Average annual costs are modest and only one of the outcomes was close. These conditions may allow voters to be uninformed without adverse consequences. However, this argument relies on the assumption that voters know that costs are modest, and our empirical analysis clearly shows the majority of voters misperceive costs and many voters believe costs are onerous. Further, it assumes that voters are aware of how close outcomes will be and that they are motivated by closeness, which have been shown to be false (Coate et al. 2008, Gerber et al. 2020). While information on the distribution of personal costs of referendums does not exist, we have no reason to believe these costs are anomalously low.[27]

Going forward, the findings presented here provide a strong rationale for states, counties, and municipalities to provide significantly enhanced information about private costs of referendums to voters. However, research is needed to understand how voters internalize cost information and how to best present the information. A useful starting point would be the decades of contingent valuation research, which emphasizes the critical importance of communicating details of cost and making them salient (Johnston et al. 2017).

[26] All statistics are authors' calculations. Trust for Public Land's Land Vote Database is publicly available. Cellini et al. graciously posted their data and code. From these two sources, we calculated the exact proportion within 5%. Isen (2014) presented many histograms by vote share, and we were able to estimate the proportion within 5% from these graphs.

[27] For example, about two-thirds of Massachusetts' municipalities have voted to adopt the CPA, and a 1% property tax surcharge (like Plainville) is a common funding amount. Further, California's $5.5 billion bond is in the middle of the range of California bonds studied by Burkhardt and Chan (2017).

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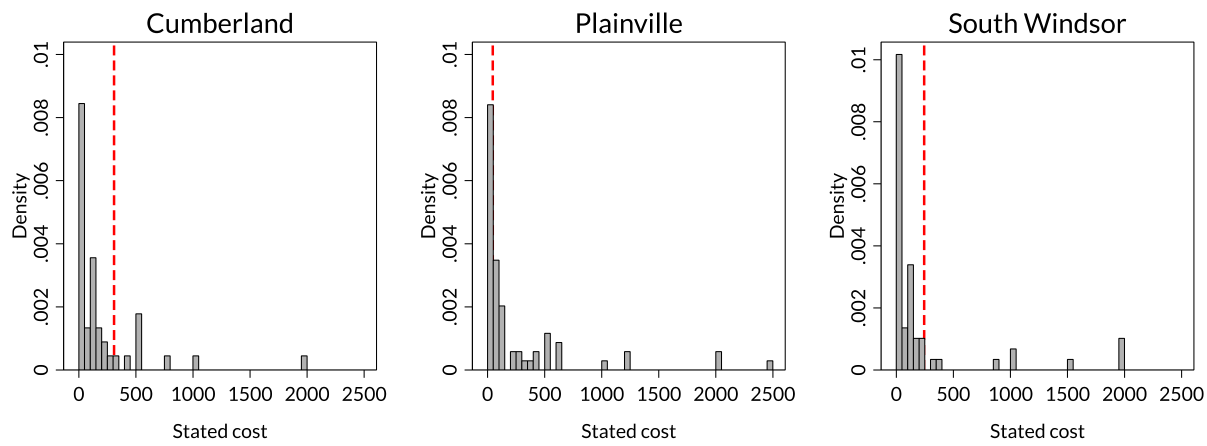
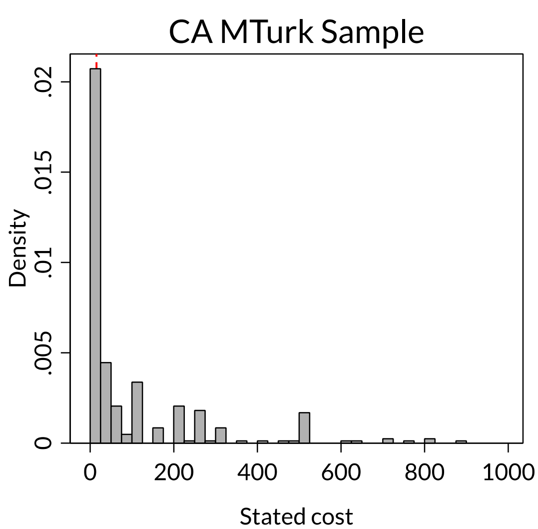
Figures and Tables

Figure 1: Distribution of stated cost by town – NE Exit Poll Sample



Notes: Vertical dashed red line equals town-specific average actual cost for sample respondents (from left to right: \$308.47, \$45.20, and \$242.70). One extreme observation is excluded for purposes of scale (\$7500 in South Windsor).

Figure 2: Distribution of stated cost – CA MTurk Sample



Notes: Vertical dashed red line equals average actual cost for CA sample respondents (\$15.06).

Table 1: Referendum characteristics

Location	Purpose	Financing	Average cost per household	Approval rate
Cumberland, Rhode Island	Street pavement improvements	$2.5 million bond	$184.71 total [$19.50 per year]	88.0%
Plainville, Massachusetts	Community Preservation Act (open space, affordable housing, historic preservation)	1% increase in property taxes, applied above $100K in value	$30.80 per year	66.0%
South Windsor, Connecticut	Open space acquisition	$2 million bond	$205.44 total [$21.69 per year]	73.9%
California	Stem cell research	$5.5 billion bond	$20.53 per year	51.1%

Notes: Average cost per household is calculated based on all residents, not just survey respondents. Plainville and California provide annual costs. Annual costs for Cumberland and South Windsor are calculated assuming a 10-year payback period and a 1% interest rate. All referendums had a pass threshold of 50% approval.

Table 2: Summary statistics

	All respondents		Answered Cost Question	
	Mean	St. Dev.	Mean	St. Dev.
Panel A: NE Exit Poll Sample				
Answered cost question (%)	45.3	49.8	100.0	0.0
Actual cost ($)	167.3	144.9	186.9	162.0
Stated cost ($)			242.7	459.4
Correct stated cost (within 25pp) (%)			17.7	38.3
Approved (%)	77.2	42.0	76.2	42.7
Voted Democrat for governor (%)	48.1	50.0	46.6	50.0
College degree (%)	70.4	45.7	76.3	42.7
Age (years)	52.1	16.9	51.6	15.5
Income (50k-100k)	0.3	0.5	0.3	0.5
Income (>100k)	0.5	0.5	0.6	0.5
Renter (%)	15.3	36.0	9.1	28.8
Financial literacy (# correct of 2)	1.3	0.8	1.5	0.7
Political knowledge (# correct of 1)	0.6	0.5	0.6	0.5
Cumberland (%)	26.5	44.2	28.2	45.1
Plainville (%)	37.9	48.6	37.8	48.7
South Windsor (%)	35.7	48.0	34.0	47.5
Observations	347		157	
Panel B: CA MTurk Sample				
Answered cost question (%)	83.5	37.1	100.0	0.0
Actual cost ($)	14.5	15.1	15.1	16.0
Stated cost ($)			98.8	164.5
Correct stated cost (within 25pp) (%)			19.4	39.6
Approved ($)	51.1	50.0	51.1	50.1
Democrat (%)	39.7	49.0	39.2	48.9
Independent (%)	21.1	40.8	23.9	42.7
College degree (%)	72.3	44.8	71.9	45.0
Age (years)	40.5	13.3	40.7	13.5
Income (50k-100k)	0.4	0.5	0.4	0.5
Income (>100k)	0.2	0.4	0.2	0.4
Renter (%)	43.6	49.7	45.1	49.8
Financial literacy (# correct of 2)	1.5	0.7	1.6	0.7
Political knowledge (# correct of 1)	0.8	0.4	0.8	0.4
Los Angeles (%)	37.8	48.5	36.8	48.3
San Francisco (%)	16.2	36.9	17.5	38.1
Observations	428		332	

Notes: All statistics are calculated using sampling weights to be representative. Answered Cost Question refers to the sample of respondents that answered the open-ended cost elicitation question.

Table 3: Correlation between stated and actual costs

	Dependent Variable: Stated Cost ($)			
	NE Exit Poll Sample		CA MTurk Sample	
Independent Variables	(1)	(2)	(3)	(4)
Actual cost ($)	-0.097	0.466	1.111	1.687
	(0.246)	(0.323)	(1.043)	(1.615)
Democrat		-64.530		-11.384
		(79.456)		(20.765)
Independent				-9.229
				(22.811)
Income (50-100k)		203.245		12.972
		(132.173)		(24.105)
Income (>100k)		-47.634		-12.685
		(114.071)		(42.032)
College		-31.297		-47.809*
		(124.344)		(24.555)
Age		-2.561		0.875
		(3.282)		(0.763)
Renter		-74.801		14.983
		(103.490)		(20.254)
Financial literacy		-74.074		-27.430
		(62.443)		(16.798)
Political knowledge		72.691		-60.907*
		(105.783)		(34.101)
Already Voted				-51.128**
				(20.781)
Observations	157	157	332	332
R-squared	0.001	0.090	0.012	0.109
City FE	-	Y	-	Y

Notes: For the NE Exit Poll Sample, "Democrat" is defined as whether respondent voted for the democratic gubernatorial candidate; for the CA MTurk Sample, "Democrat" or "Independent" are self-reported political affiliations. Constant term included but not displayed. All regressions are estimated using sampling weights to be representative. Constant term omitted from table. Robust standard errors reported in parentheses. *, **, and *** represent significance at p<0.1, p<0.05, and p<0.01 levels.

Table 4: Unpacking the drivers of voter misperception

	NE Exit Poll Sample			CA MTurk Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Independent Variables	Answered cost question	Correct stated cost (+/- 25pp)	Stated > Actual	Answered cost question	Correct stated cost (+/- 25pp)	Stated > Actual
Approved	-0.001	-0.004	-0.171	-0.124***	0.029	-0.108
	(0.071)	(0.069)	(0.117)	(0.036)	(0.049)	(0.067)
Democrat	-0.027	0.028	0.152	-0.022	-0.059	0.071
	(0.061)	(0.058)	(0.095)	(0.036)	(0.060)	(0.080)
Independent				0.088**	-0.122**	0.144*
				(0.036)	(0.048)	(0.081)
Income (50-100k)	0.183**	-0.096	0.094	0.007	-0.044	-0.094
	(0.086)	(0.079)	(0.162)	(0.026)	(0.050)	(0.079)
Income (>100k)	0.311***	-0.051	-0.009	0.015	-0.105**	-0.399***
	(0.076)	(0.112)	(0.160)	(0.036)	(0.049)	(0.086)
College	0.015	-0.008	-0.167	0.005	0.058	-0.038
	(0.067)	(0.063)	(0.110)	(0.033)	(0.055)	(0.080)
Age	0.000	0.002	-0.002	0.001	0.002	-0.001
	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.003)
Renter	-0.070	-0.112*	0.064	0.039	0.039	-0.042
	(0.090)	(0.061)	(0.175)	(0.026)	(0.054)	(0.068)
Financial literacy	0.172***	0.042	-0.111*	0.071***	0.081*	-0.062
	(0.042)	(0.043)	(0.062)	(0.025)	(0.044)	(0.056)
Political knowledge	0.024	-0.001	-0.025	0.038	0.062	-0.150**
	(0.061)	(0.055)	(0.105)	(0.033)	(0.053)	(0.070)
Already Voted				0.029	-0.002	-0.158**
				(0.024)	(0.046)	(0.066)
Observations	347	157	157	428	332	332
Log pseudolikelihood	-211.1	-62.02	-86.61	-150.9	-165.5	-206.0
City FE	Y	Y	Y	Y	Y	Y

Notes: Coefficients presented are marginal effects from logit regression evaluated at sample means. For the NE Exit Poll Sample, "Democrat" is defined as whether respondent voted for the democratic gubernatorial candidate; for the CA MTurk Sample, "Democrat" or "Independent" are self-reported political affiliations. All regressions are estimated using sampling weights to be representative. Constant term omitted from table. Robust standard errors reported in parentheses. *, **, and *** represent significance at p<0.1, p<0.05, and p<0.01 levels.

Table 5: Voter choice regressions and willingness to pay estimates

	Dependent Variable: Approve (1=yes)			
	NE Exit Poll Sample		CA MTurk Sample	
Independent Variables	(1)	(2)	(3)	(4)
Stated cost ($1000s)	-0.28**		-0.17	
	(0.12)		(0.22)	
Actual cost ($1000s)		1.34		1.50
		(2.26)		(2.92)
Democrat	0.17**	0.19***	0.45***	0.46***
	(0.070)	(0.069)	(0.067)	(0.067)
Independent			0.22***	0.22***
			(0.084)	(0.084)
Income (50-100k)	-0.18	-0.19	-0.071	-0.084
	(0.26)	(0.23)	(0.082)	(0.084)
Income (>100k)	-0.21	-0.19	-0.090	-0.14
	(0.16)	(0.16)	(0.093)	(0.13)
College	-0.0081	-0.0081	0.15*	0.16*
	(0.089)	(0.085)	(0.084)	(0.083)
Age	0.00060	0.0015	-0.0048*	-0.0048*
	(0.0026)	(0.0026)	(0.0029)	(0.0029)
Renter			-0.057	-0.056
			(0.074)	(0.074)
Financial literacy	-0.14**	-0.12*	-0.16***	-0.15***
	(0.070)	(0.065)	(0.056)	(0.055)
Political knowledge	-0.0061	-0.042	-0.24***	-0.23***
	(0.081)	(0.082)	(0.087)	(0.089)
Already Voted			-0.19***	-0.18***
			(0.068)	(0.068)
Observations	157	157	332	332
Log pseudolikelihood	Y	Y	Y	Y
City FE	-70.5	-74.1	-190	-189.8
WTP estimate ($)	1010.63	-163.03	1143.0	-113.6
	(370.65)	(317.74)	(1464.94)	(236.0)

Notes: Coefficients presented are marginal effects from logit regression evaluated at sample means. For the NE Exit Poll Sample, "Democrat" is defined as whether respondent voted for the democratic gubernatorial candidate; for the CA MTurk Sample, "Democrat" or "Independent" are self-reported political affiliations. All regressions are estimated using sampling weights to be representative. Constant term omitted from table. Robust standard errors reported in parentheses. *, **, and *** represent significance at p<0.1, p<0.05, and p<0.01 levels.