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ABSTRACT

The conventional view in the direct democracy literature is that spending against a measure is more effective than spending in favor of a measure, but the empirical results underlying this conclusion have been questioned by recent research. We argue that the conventional finding is driven by the endogenous nature of campaign spending: initiative proponents spend more when their ballot measure is likely to fail. We address this endogeneity by using an instrumental variables approach to analyze a comprehensive dataset of ballot propositions in California from 1976 to 2004. We find that both support and opposition spending on citizen initiatives have strong, statistically significant, and countervailing effects. We confirm this finding by looking at time series data from early polling on a subset of these measures. Both analyses show that spending in favor of citizen initiatives substantially increases their chances of passage, just as opposition spending decreases this likelihood.

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To what extent does campaign spending shape the electoral fate of citizen initiatives? This empirical question has important implications for the normative debate over the role of money in direct democracy elections (Gerber 1999, Broder 2000, Matsusaka 2004). If groups with a concentrated interest in public policy can use their organizational advantages to raise large campaign war chests that translate into votes for the initiatives that they propose, direct democracy may not simply amplify the "voice of the people" as its early advocates promised (see Grantham, 1983, Ellis 2002, Goebel 2002, Piott 2003). By contrast, if spending in favor of initiatives does not increase their chances of passage, powerful interests will not gain an advantage through the money that they can raise.

While this important question has generated much scholarship from political scientists, economists, and legal scholars, two distinct streams of the literature have provided different answers to it. A long series of works that treat campaign spending levels as exogenous created a conventional wisdom that expenditures have asymmetric effects: Money spent in opposition to ballot propositions (including both citizen initiatives and measures placed on the ballot by legislatures) lowers their expected vote shares or chances of passage, while spending in favor of them is drastically less effective (Allen 1979, Lydenberg 1979, Mastro et al. 1980, Lowenstein 1982, Magleby 1984, Schockley 1980, 1985, Schmidt 1989, Ji 1998, and Bowler and Donovan 1998). A recent review of the literature reports that these studies found "that heavy spending against a measure tended to lead to the measure's defeat, whereas heavy spending in favor of a measure had a minimal impact (Lupia and Matsusaka 2004, p. 470)." Though only a few studies

have probed why spending might have this asymmetric effect[1] or why ballot proponents spend so much even if their money appears to be wasted,[2] many have pointed to the reassuring normative implications of this empirical finding. "Such studies imply a role for money in direct democracy that cuts against popular stereotypes. … [T]he deep pockets of business groups do not allow them to 'buy' favorable policy (Lupia and Matsusaka 2004, pp. 471-2)." At the same time, the asymmetric effect of campaign spending may enable such groups to defend a status quo policy against an energetic challenge.

Another stream of scholarship (Zisk 1987, Gerber 1999, Garrett and Gerber 2001) has found, often by explicitly considering that spending levels are endogenous to the nature of a proposition and its backers (Banducci 1998, Stratmann 2005, 2006), that the effects of pro and con spending can be symmetric under certain political conditions.[3] These works have found that both proponents and opponents of a ballot measure can win more votes when they spend more money suggesting a different view of money and direct democracy. If a well-organized interest or business group opens its deep pockets for a ballot campaign, it may indeed be able to buy support for a favorable outcome. Balancing this worry is the assurance that groups seeking to defend existing policy have no predetermined advantage in a campaign against those who wish to overturn the status quo.

[1] Lowenstein (1982, pp. 547-570) examines possible rationales for this asymmetric effect, rejecting explanations based on voter confusion or a predisposition to vote against propositions, and instead positing that the kind of ballot measures that attract high levels of support spending are likely to be less popular with voters from the outset. This line of reasoning serves as the basis for our two-stage model, which confirms Lowenstein's conjecture.

[2] Gerber's (1999, p. 82) survey of 156 interest groups active in proposition campaigns provides some potential solutions to this puzzle: Group representatives often contended that they backed ballot measures more to send signals to legislatures, draw attention to their agendas, or respond to their members than to make policy directly. Note that Gerber's investigation focused specifically on the inability of business groups to "buy" support for propositions that promised a narrow economic benefit, while expenditures by citizen groups appeared to be more influential.

[3] Garrett and Gerber (2001) find that support spending can be effective in the absence of opposition spending, Gerber (1999) shows that spending by citizen groups rather than by business can boost a proposition's electoral fortunes, and Zisk (1987) finds symmetric effects for support and opposition spending in approximately half of all cases.

We seek to address this consequential controversy in the literature by analyzing a comprehensive dataset of ballot propositions in California[4] from 1976 through 2004 and applying a novel approach to dealing with the endogeneity of spending levels, isolating citizen initiatives from other types of propositions, and addressing other estimation issues. We argue that the much of the existing literature underestimates the effect of spending in favor of citizen initiatives because narrow interests often spend large sums to pass measures that begin campaigns with slim majorities, while proponents of measures with a wide appeal can spend less and still win. Borrowing insights and techniques from earlier studies as well as from the literature on the effects of spending in candidate elections, we highlight the critical importance of estimation choices. We begin by taking the traditional approach to estimating the effect of spending on ballot measures, which produces the asymmetric finding. When we treat campaign spending as exogenous and assume that it has constant returns, it appears that opposition campaigns hurt ballot measures while spending in favor of them is wasted.

This puzzling asymmetry disappears, however, when we use more flexible econometric techniques that allow for diminishing returns and consider how the concentration of costs and benefits can determine financial support for a policy proposal (Lowi 1964, Stigler 1971, Peltzman 1976, Wilson 1980). Just as expenditures in opposition to citizen initiatives diminish their support, campaign spending in favor of initiatives appears to have a positive and significant effect on their chances at the ballot box. According to our final model, spending an additional $100,000 in support of an initiative increases its chances of winning by a predicted 1.43 percent, which is almost as large as the 1.90 percent predicted decrease in the likelihood of passage that would

[4] We focus only on California ballot measures because they serve as the dataset for nearly all of the existing literature (see Lee 1978, Lowenstein 1982, Magleby 1984, Myers 1988, Price 1988, Bowler and Donovan 1998, Gerber and Garrett 2001, Stratmann 2006) and because a historical archive describing their policy content is available (Hastings Law Library, 2005). See Smith (2001), Lupia and Matsusaka (2004), and Smith and Tolbert (2007) for reviews of the literature on money and propositions across all states.

result from an additional $100,000 of opposition spending. We supplement our instrumental variables approach with a time series analysis of a more limited set of initiatives, showing that both pro and con expenditures appear to shift the level of popular support for ballot measures over the course of a campaign. In the end, our analysis provides more evidence in favor of newer works challenging the conventional wisdom in this area by showing that interest groups, for better or for worse, can boost the chances of passing an initiative that they favor just as effectively as opponents can spend to stop them.

I. Reasons to Doubt the Conventional Wisdom

While earlier studies compared the effect of spending patterns on the success rates of ballot measures through bivariate studies (Lee 1978, Allen 1979, Lydenberg 1979, Lowenstein 1982, Owens and Wade 1986), recent works use multivariate models to predict the fate of propositions by the levels of spending for and against them, along with other control factors (Ji 1998, Bowler and Donovan 1998, Gerber 1999). Table 1 presents the most basic multivariate approach with our data. It reports the results of a probit model explaining the success or failure of 329 propositions in California from 1976 to 2004. The coefficients on these variables can be given a causal interpretation only if pro and con spending levels have been exogenously and randomly assigned to propositions. We also include controls for the type of election, the type of proposition, the presence of a competing initiative, the number of other measures on the ballot, and economic conditions. We coded all dichotomous variables that could be posed as questions with a "1" indicating an answer of "yes" and a "0" indicating "no." We estimate robust standard errors, and include year fixed effects to account for the possibility that fiscal or electoral conditions make one year a good or bad time for all initiatives on the ballot.

[Table 1 About Here]

The results in Table 1 parallel the conventional finding in the literature. Money spent against a proposition has a negative and strongly significant effect on its chances of passage. An additional $1 million in opposition spending leads to an estimated 4.1% decrease in the probability that it will pass. By contrast, the impact of spending in favor of a ballot measure is substantively minuscule and falls far short of statistical significance. This basic model, which follows the approach of many previous studies, reproduces the puzzling conventional wisdom that support and opposition spending have asymmetric effects on the outcomes of direct democracy elections.[5] Instead of accepting this counterintuitive result, we explore the substantive methodological assumptions upon which it rests. Relaxing each assumption has a clear econometric implication, and we explore the consequences of these estimation choices later in our empirical findings section. In this section, we focus on our conceptual objections to the basic model reported in Table 1, providing data and referencing other works that support our critiques.

A first troubling feature of the basic model is that it treats every proposition that appears on a ballot as the same type of phenomenon, ignoring key differences in the path that a proposal took to reach the ballot. We wish to draw a distinction between measures put on the ballot by legislatures (the three[6] types of measures that are commonly referred to as "legislative propositions") and direct citizen "initiatives". Legislative propositions are less controversial, much less likely to inspire spending on either side, and more likely to win than initiatives qualified by voters and interest groups. The reasons for this lie in the rules that govern access to the ballot

[5] In addition to the works that have informed the conventional wisdom, another stream of research finds that neither side can spend effectively in proposition campaigns: Lee (1978), Owens and Wade (1986), Myers (1988), and Price (1988) all conclude that neither opposition nor support spending plays a central role in determining the fate of propositions.

[6] "Legislative propositions" include constitutional amendments proposed by the legislature, legislative bonds, and legislative proposals to alter statutes that have been put in place by the citizens through initiatives (Legislative Analyst's Office 2009).

and in the realities of California politics. For an idea to reach the ballot through the legislative process, it must win two-thirds votes in both the Senate and the Assembly. Neither party ever held such a supermajority during our period of study, meaning that some level of bipartisan agreement was necessary to send every one of these legislative propositions to the voters (National Conference of State Legislatures 2002, State Net 2005). Because of this, the measures were often ideas with broad support that did not generate intense controversy or massive campaign spending.

Initiatives, by contrast, need not reflect the views of a wide cross-section of California officeholders or voters. What they require is the intense support of a group that is sufficiently well organized and well funded to collect a large number of voter signatures in a short amount of time. To qualify for the ballot, statutory initiatives must be signed by the number of registered voters that equals 5% of the votes cast in the last gubernatorial race, while initiative constitutional amendments must be backed by 8% of that total. With only 150 days to collect these names, nearly all initiative authors pay signature gathering firms $1-3 per name to collect the requisite number of valid names (Gerber et al. 2001, p. 10). If a group is willing to pay more than a million dollars to put its idea on the ballot, rather than spending a smaller sum lobbying the legislature to pass it, we can expect that its proposal is opposed by a majority of legislators in Sacramento (Lowenstein 1982, Kousser and McCubbins 2005). We can also expect that these citizen-backed initiatives are less likely to draw wide support in the electorate than their legislatively-proposed counterparts.

A look at the ballot measures in our sample demonstrates the differences between legislative propositions and initiatives. The 192 legislative propositions that reached the ballot from 1976 to 2004 attracted an average of $478,406 spent in support of them and $220,273 spent in opposition, in constant 1982-84 dollars. Much more money flowed into initiative campaigns.

Mean spending on the 137 citizen-qualified measures in our sample was $3.6 million for pro campaigns and $2.4 million against them (again adjusted for inflation). Initiatives also did much worse at the polls than legislative propositions. In our sample, 43% of citizen-authored measures passed, while 70% of the legislature's proposals were accepted by voters. Because they are so clearly different from each other, we argue that these two types of propositions should not be combined into the same analysis. Doing so would bias the estimated effects of spending in favor of ballot measures toward zero, since it includes a class of measures (legislative propositions) that generate little support spending and have high chances of passage because of the policy proposals that they contain. Separating them from the analysis of citizen initiatives is a first step toward breaking the endogenous link between the nature of a proposition and the spending that it attracts, and thus isolating the "treatment" effect of campaign expenditures.

Some previous works have made the same division of ballot measures that we propose. While the more comprehensive multivariate analyses by Ji (1998), Gerber (1999), and Bowler and Donovan (1998) include both initiatives and legislative propositions, many of the earlier bivariate analyses such as Lowenstein (1982), Shockley (1985), Price (1988), Meyers (1988), and Schmidt (1989) looked only at citizen-authored initiatives. Garret and Gerber (2001) study both types of ballot measures but include a dummy variable indicating the type of proposition in their models. Because it is the citizen initiative that most commentators are referring to when discussing the impact of money on direct democracy, analyses of this class of propositions will be most relevant to the normative debate. We focus only on initiatives in the rest of our analysis.[7]

[7] Because they are also initiated by citizens, are often controversial, and frequently generate expensive campaigns, we include the eight referendums on the ballot during our period of study into the "initiative" category. These are attempts to overturn laws passed by the legislature which qualify for the ballot in the same manner as initiatives.

Our second objection to the conventional approach is that it assumes that spending will have a constant, linear effect on election outcomes. Under this reasoning, if the Indian tribes that spent $66 million in support of Proposition 5 in 1998 (California Secretary of State, 1999) had spent a $67^{th} million on it, this would have given their initiative the same boost that the second million dollar would have provided to a less lavishly funded campaign. This strains common sense, and also fails to fit with the broader literature on the role of money in American politics. Studies of the effects of campaign spending in Congressional candidate races (Green and Krasno, 1988), Senate contests (Gerber, 1998) and state legislative races (Kousser and LaRaja, 2002) have found that high spending brings diminishing marginal returns. This makes sense, because having enough money to attain a basic level of recognition and support for a cause would seem to be more important than saturating the airwaves with more and more commercials. Rather than assuming linear effects, these works test alternative specifications and often find that nonlinear transformations of expenditures provide a better fit of the data.

In our empirical section, we follow a similar strategy, examining whether the square root of spending provides a better prediction of electoral outcomes than raw expenditures, to allow for a diminishing marginal value of money. (In Section III, we consider log transformations as well.) At the same time that it provides a better fit for what we know about money and campaigns, this approach also gives less weight to outlying initiatives that generated enormous spending levels. While 88% of the initiative campaigns in our sample spent less than $5 million either for or against an initiative, 2% of them spent $20 million or more (in 1982-84 dollars). A linear specification would give equal weight to the money spent in all initiatives, allowing the outliers to drive much of the results. Since the very highest expenditures in our dataset are often made by

support campaigns, this could disproportionately bias the estimated effect of pro spending toward zero. Our transformation of spending levels will avoid this pitfall.

Our final[8] reason to doubt the findings from our basic model and much of the existing literature is perhaps the most serious one: Campaign spending on an initiative may be endogenous to its chances of passage. Most existing works treat spending for or against a measure as if it were randomly assigned to the initiative and uncorrelated with characteristics such as its prospects early in the campaign. The literature on the role of campaign spending in candidate races highlights the problems created by such an assumption, and suggests one possible solution. Ever since Jacobson's (1980) influential *Money in Congressional Elections*, scholars have recognized that contributions to US Congressional nominees are likely to be endogenous to their chances of winning. Strong candidates, the reasoning goes, do well in elections and attract lots of money, creating a correlation between fundraising prowess and electoral performance when there may be no causal link. Donors may simply be heaping funds on those who will win anyway, perhaps because they wish to influence the members once in office.

We posit that campaign spending, especially money that goes to support initiatives, is also endogenous to their electoral chances but in just the opposite way. Money may flow to initiatives that are in danger of failing. Since contributors cannot hope to buy influence over the unchanging language of a ballot measure, there is no reason to pile money on one that is already headed for passage. Yet if an initiative looks vulnerable, its supporters – especially those who have already

[8] Another potential improvement to the conventional model would separate out money spent gathering signatures to qualify initiatives for the ballot from the money spent on the campaigns supporting them. Unfortunately, the California Secretary of State's office reports qualification and campaign spending separately for only 75 of the 137 initiatives in our sample. In an earlier draft of this paper, which we will make available to interested readers, we used this limited data to estimate qualification spending for all 137 initiatives and analyzed the effects of only campaign spending on initiative passage. This did not change the sign or significance of any of our estimated spending effects, and the models were generally robust to this change. Consequently, we use combined qualification and campaign spending in all of the analysis here since it is reported for all of the initiatives in our sample. Results are available from the authors (see also SSRN version of paper).

invested substantial sums to qualify it for the ballot – may contribute large amounts to campaign for its survival. If this story is correct, consider its implications for the conventional econometric approach. The model in Table 1 found that spending in favor of propositions has no effect on their chances of winning. But this could be an artifact of the fact that a proposition's unmeasured initial chances of passage are omitted from this model, negatively correlated with support spending, and positively correlated with its performance on election day. If this plausible chain of relationships holds true, it will bias the estimated effect of support spending downward toward zero (as long as it is uncorrelated with other regressors). Money spent in favor of propositions may indeed increase their chances of passage, but the bias created by the endogenous nature of campaign expenditures may be hiding its true effect.

In order to uncover our estimate of the impact of spending on citizen initiatives, we follow the approach of Jacobson and others by finding instruments that capture the unmeasured characteristics of an initiative that determine its campaign finances. Jacobson (1980, p. 140) used a "candidate quality" measure based on political biographies, along with other exogenous variables, to predict financial support in the first stage of a two-stage least squares model.

Two previous authors have employed instrumental variables estimation techniques in ballot measure financing papers. In an often overlooked and rarely cited paper, Banducci (1998) is the first author to recognize this problem and attempts an instrumental variables estimation on only proponent spending. Her two key instruments are opponent spending and whether there is business regulation. While the former is clearly endogenous itself and thus requires its own instruments, the latter seems a somewhat promising start to the analyzing the problem—the degree to which business interests are affected. Even with these weak instruments, Banducci is able to

get results which show that proponent spending may have an effect in some cases on ballot measure outcomes.

Stratmann (2005, 2006) also recognizes the endogeneity problem in spending. Stratmann examines spending on television advertising in five major media markets in all statewide ballot measures in California from 2000-2004 and its effect on county level voting outcomes using a fixed effects estimation, and finds that television advertising for and against affect vote outcomes. This novel and effective research design may, by its nature, face limits to its external validity: The data set covers only 18 initiatives and five media markets. Moreover, television advertising accounts for only 38% of total group expenditures, leaving 62% of expenditures outside the analysis (Stratmann 2006: 792). The main method that Stratmann (2005, 2006) uses to address endogeneity – the inclusion of county- and ballot-level fixed effects – has also faced criticism.[9]

Taken together, though, Banducci (1998) and Stratmann's (2005, 2006) works effectively challenge the conventional assumption that spending on propositions is exogenous, suggest the value of the instrumental variables approach as an alternative, and find that support and opposition spending may have symmetric effects. To build on their work, we analyze all California ballot measures from 1976 to 2004 (329 in all), we examine all spending (rather than only television advertising), and we use multiple instruments that vary across each observation in the dataset to obtain better identification of the econometrics. We gather new measures of how concentrated the

[9] de Figueiredo (2005) provides a lengthy and detailed explanation of why fixed effects does not solve the endogeneity problem for the ballot measures dataset. To summarize that critique, if counties have different underlying preferences for different ballot measures, then ballot fixed effects (which control for differences in ballot measures) and separate county fixed effects (which control for differences in counties) do not solve the endogeneity problem. Rather, one needs ballot-county fixed effects. (See Mundlak 1963 for a further discussion of this issue.) Unfortunately, the Stratmann research design does not allow for these controls. As a separate robustness check, Stratmann (2006) does report the use of (but few of the results from) an instrumental variables estimation with three instruments: the number of measures on the ballot, the number of undecided voters, and the price of advertising per voter. However, these instruments are plagued with precisely the same problems as the fixed effects measures—they vary by county or by ballot measure (on the state level)—but do not vary by county-ballot measure as would be required of standard instruments in a research design of this type.

costs and benefits of an initiative are as our major instruments. Our coding system is described more fully in the section on data sources below, but is based on the logic set forth by Lowi (1964), Stigler (1971), Peltzman (1976), and Wilson (1980) and summarized in Baron (1996, pp. 155-8). Previous work by Donovan et al. (1998) applies this same typology to California ballot propositions.

When a policy brings concentrated benefits to one group rather than spreading its advantages around to the entire population, this group will be able to solve its collective action problem and organize a strong effort to push for the policy. The same logic applies for the policy's opponents, and the proposal's success will often be determined by the way that the concentration of its effects structures the nature of the political competition over it. Under this theory, initiatives that would yield large but narrow payoffs, such as expansions of tribal gaming or relaxations of industry regulations, can be expected to raise a lot of money. But we hypothesize that these are precisely the types of measures which are likely to begin their campaigns with a low probability of passage. They do not promise a broad benefit, and in some cases may impose costs on a broad cross-section of voters. Examples of this include Proposition 31 in 2000, which imposed limits on insurance claims, and Proposition 64 in 2004, which imposed limits on unfair labor practice suits. Voters were initially wary of these limits, with only 26% and 21% of voters supporting them, respectively, when the Field Poll gauged public opinion at the outset of their campaigns. Yet these types of initiatives promise a tangible benefit to concentrated industry groups. Their political consultants will have more money at their disposal, but a tougher job to do. Our measures, then, will capture the way that the initial prospects of a proposition help to determine its finances. The scope and magnitude of the costs and benefits of initiatives may help to predict spending levels in a first stage model, and should not be ignored.

In summary, our three reasons to doubt the conventional finding that initiative supporter waste their money are substantive objections that have methodological implications. We believe that analyses of propositions should be based on a sample of comparable phenomena such as citizen initiatives, rather than mixtures of legislatively- and citizen-proposed measures. Instead of assuming linear effects, we should test whether spending on initiative campaigns yields constant or diminishing returns. Finally, the endogenous nature of initiative spending should be taken as seriously as the endogenous nature of candidate spending. In an online appendix, we present a simple model that formalizes our conceptual approach and motivates both our assumption of diminishing marginal returns to spending and our use of instrumental variables.

II. Data Sources

In order to estimate the effects that spending for and against California ballot propositions has had on their chances of passage, we analyze data from official state sources reporting their campaign finances, the subject areas which they addressed, their potential policy impacts, and their final votes. We cover the 1976 to 2004 period, with exception of two initiatives and 37 legislative propositions for which no campaign finance documents were available.[10] This gives us a total of 329 propositions, including 137 initiatives qualified by citizen groups and 192 legislative propositions. We used the 1976 through 2004 editions of the California Secretary of State's *The Statement of the Vote* and *Supplement to the Statement of Vote* to distinguish between the two routes to the ballot. We used these same sources to collect information on the proposition's vote

[10] In the University of California's library system, we could not find spending data on Propositions 1-4 in the 1979 Special Election, Propositions 25-34 from the 1984 General Election, Propositions 42-50 and 52 from the 1986 Primary Election, Propositions 53-60 from the 1986 General Election, Propositions 152-154 from the 1992 Primary Election, and Propositions 168, 169, 171, 173, and 174 from the 1993 Special Election. Since only two of these were citizen initiatives, this randomly missing data should not bias our main analysis, which looks only at initiatives.

share, whether or not it was a counter initiative to another measure on the same ballot, whether it was a bond, and which type of election, primary or general, was being contested. To identify the policy area covered by the proposition, we used *A Study of California Ballot Measures* (California Secretary of State, 2002) and updated it for the 2003 and 2004 elections ourselves. Our spending figures come from standardized reports published by California's Secretary of State and Fair Political Practices Commission.[11] All expenditures were deflated into constant 1982-84 dollars using the California CPI (California Department of Finance, 2005).

Our most complicated task was to gather new variables assessing whether initiatives would bring diffuse or concentrated costs and benefits, and how large they might be. All three of the authors made these judgments independently after reading summaries of the measures produced by the California Legislative Analyst and collected in the Hastings Law Library California Ballot Measures Databases (Hastings Law Library, 2005). We did not converse about any of our coding choices. For each proposition, we recorded two objective measures – whether this was a citizen-proposed bond and whether it changed tax levels – and six subjective, interval variables. As noted below, four of these variables will become the omitted instruments used in the first stage equation and two will be used in both the first stage and second stage equations. These six came from measures of the magnitude and the scope of the economic benefits and costs that the initiative could provide to some sets of winners and losers, compared with the status quo. We define winners for an initiative as those who would benefit from changing policy from the status quo to the initiative's proposal. The losers are those who would pay the costs of changing policy from the

[11] Different reports cover different years, so our sources vary from California Fair Political Practices Commission (1986) for all elections from 1976 to 1986, California Fair Political Practices Commission (1988, 1989) for the 1988 elections, California Fair Political Practices Commission (1991a,b) for the 1990 elections, California Secretary of State (1993) for the 1992 General Election, California Secretary of State (1995) for the 1994 elections, California Secretary of State (1997) for the 1996 elections, California Secretary of State (1998, 1999) for the 1998 elections, and California Secretary of State (2005) for the 2000-2004 elections.

status quo to the initiative's proposal. We estimated expected benefits for the winner and costs for the losing group, over a five year period. The best way to describe our new measures is to give examples of recent California initiatives that fall into each category of different variables. In fact, we began with the following set of examples to guide our subjective coding of the benefits that would go to the backers of an initiative if it was successful, and used a parallel set of measures to code the costs that would be paid by those who would lose if the initiative passed.

• *Magnitude of Economic Benefits (four ascending levels):* "0" if there is no economic effect on anyone or if the effects are negligible. For instance, the ban on exportation on horses for slaughter contained in 1998's Prop. 6 had little economic effect on any human, except for perhaps a negligible boost for those selling glue made from a different technology. "1" for a gain of thousands or hundreds of thousands of dollars. This is the total that a small number of lawyers might expect to gain after 2004's Prop. 59 changed public records law to make it easier to bring legal cases. "2" for a gain of millions or tens of millions of dollars. California businesses probably expected to save about this much by avoiding unjustified lawsuits under the limit on unfair business practice suits imposed by 2004's Prop. 64. "3" for a gain in the hundreds of millions or billions. Those who profit from California's tribal gaming casinos and those who wanted to end the tribes' monopoly on Las Vegas-style gaming may have expected to gain benefits on this massive scale from 2004's unsuccessful Propositions 68 and 70. The county and city governments which will benefit from the protection of their revenues guaranteed by 2004's Prop. 1A probably expect to gain about this much over a five year period, since state budgets have occasionally taken back more than $1 billion normally allotted to local governments. These variables will be two of the instruments omitted from the first stage estimation.

• *Scope of Effect on Industries (three unordered categories).* We code an initiative as bringing a "Concentrated Industry Benefit" only if an initiative primarily benefits a concentrated group of 1-3 industries, similar to Prop. 70's concentrated effect on the gaming industry. An initiative brings a "Diffuse Industry Benefit" if an initiative a broad range of industries, similar to 2004's Prop. 64's limit on lawsuits. An initiative brings "No Industry Benefits" when it is a measure like 2004's Prop. 62, which contained a change in primary election law that promised no clear effect on any industry. Note that this extends the ideas of Banducci's (1998) business regulation instrument. These variables will be the two additional instruments omitted from the first stage.

• *Scope of Benefits for Citizens (four ascending levels):* "0" if it benefits a concentrated group of a thousand or fewer residents. "1" if it benefits a group of Californians in the tens of thousands, such as the 29,000 or so prisoners who would get early release under 2004's Prop. 66, which altered the state's "Three Strikes" sentencing law. "2" if it benefits a class of Californians in the hundreds of thousands or millions, such as the one million uninsured residents whose employers would have been forced to contribute to their health insurance under 2004's Prop. 72. "3" if it benefits all Californians equally, as Prop. 59's expansion of open records did. Because benefits (costs) to citizens are likely to affect both the ability of proposition proponents (opponents) to raise money as well as the ballot outcome directly through citizen voting, these variables are included in both the first stage and second stage equations.

While many published papers rely on a single coder for more subjective variables, we included three independent codings for each variable for each initiative. After each of us coded each initiative, we created a combined measure by taking the median of the three scores. Consequently, if our judgment was unanimous or if two out of three of us agreed on a score, the

consensus score became the final measure. Our subjective coding correlates quite highly for five of these six measures. We conduct a Cronbach's alpha test for the inter-rater reliability of the codings. Each of the six variables, except one, has a scale reliability coefficient of at least .62 and as high as .86. One outlier (Scope of Costs for Citizens) has a reliability of .41. This means that it is a noisy measure of citizen cost scope. We think it prudent to keep this variable, despite the noise, because it is likely that a broad scope of a measure will have an impact on vote outcomes. That said, when we have independent coders assess this same measure (as described in the robustness section), the alpha test has a reliability of 0.74. Thus, there does seem to be some merit to including this variable. A separate measure of Spearman correlation coefficients, which is sometimes preferred in the literature, reflects the reliability found in the Cronbach's alpha test. While there is not a consistent standard for levels on statistical congruence of inter-rater reliability measures, the .60 to .80 range is seems to be the widely accepted range in the literature for acceptable reliability of coding of this type (Nunnally 1978; Murphy and Davidshofer 2001). Thus for at least five of the six variables coded, there seems to be relatively good consistency across coders and our combined scores worked well as instruments. As a final method for the reader to gain confidence in our coding, we present a sample of initiatives in Appendix Table A1 with the results of our coding system for inspection.

III. Empirical Findings

A. Results

Using this new data, we can reevaluate the conventional finding reached by many previous studies and in the analysis reported in Table 1. Table 2 presents the full model which reflects all

our substantive suggestions, yields results stand in clear contrast to the conventional wisdom about the influence of money in initiative campaigns.[12]

[Insert Table 2 About Here]

Model 2 in Table 2 integrates all of our objections to the conventional approach by taking seriously the possible endogeneity of spending levels. Because contribution levels may be affected by an unmeasured initial assessment of an initiative's likelihood of passage, spending could be correlated with the errors in our models through this omitted variable. To purge spending levels of their links with the error term, we follow the approach that models of candidate spending take and estimate a two-stage model. The first stage uses a set of instruments – our measures of the size and concentration of the benefits and costs brought by each initiative, along with other controls – to predict spending by both "yes" and "no" campaigns. We analyze the square root of spending levels in all of the following models, incorporating our finding of diminishing returns. The results of the first stage model, reported in Appendix Table A2, generally fit with our expectations. More money can be raised to support initiatives when they promise larger economic benefits or smaller economic costs, when the scope of their effects on citizens are concentrated, when they qualify for general election ballots, and when they would lock in policy gains through a constitutional amendment. Opposition forces spend more when the economic stakes are highest, when industry costs are significant and concentrated, and when counter initiatives are on the ballot.

One concern that arises is the quality of the instruments. We examine this from both a conceptual and statistical approach. Conceptually, the instruments which we use in the first stage

[12] In de Figuieredo, Ji, and Kousser (2007), we alter the conventional model to incorporate the econometric implications of our three objections to the traditional approach, one by one. We report results at each step, making the empirical consequences of each of our alterations clear.

and are omitted in the second stage are those instruments which relate to the costs, benefits, and structure of industry. The logic underlying this is that these industry-specific factors are likely to affect the amount of money a group can raise in support of or opposition to a given ballot measure, but are unlikely to affect the vote outcome directly. The effect operates through the money the industry raises. Contrast this to the effect of the costs and benefits to citizens. These might affect both the amount of money raised from citizens and the way in which citizens vote, so we include these variables in both stages of the analysis. Industries, unlike citizens, cannot vote.

Statistically, we examine the quality of the instruments through a number of different measures. In this first stage, the coefficients have the expected sign, and the t-statistics on the coefficients of the key instruments are large, We subject the instruments to further testing in conducting a Hausman specification test for instrumental variables. The Hausman test rejects consistency of the non-instrumented model at the 95% level of confidence, and points to the two-stage model as a better model for estimating the effects of spending on outcomes with valid instruments.

That said, it is also likely the case that the collective action logic set forth by Lowi (1964), Stigler (1971), Peltzman (1976), and Wilson (1980) does not govern the fundraising fate of every initiative. Some grassroots groups have managed to raise large sums to back environmental, political reform, and animal rights initiatives, even when these measures would not bring a large financial benefit to a concentrated group. Many initiative campaigns in California are funded by a wealthy donor or a political patron using the measure to help his or her campaign, and the fundraising success of these types of measures would also be poorly predicted in our model. Thus, though our predictions are not perfect in every case, the instruments in the first stage do help

us to more reliably estimate the effects of money on initiative campaigns in our second stage model.

This model, the instrumental variables probit reported in Table 2, confirms our intuition that both predicted support and predicted opposition spending affect an initiative's chances of passage. Note that the coefficients are nearly identical in magnitude, though opposite in sign, and both of these measures are statistically significant in their expected directions, indicating that the positive effects of support spending are just as large as the negative effects of opposition money. We translated these coefficients into predicted changes in an initiative's probability of passage brought by a shift in spending, holding all other variables in the model constant at their mean values. These are nonlinear approximations for spending and outcomes because we use the square root of spending. First, we examined the potential effect of increasing support spending from its mean value of $3.6 million (in 1982-84 dollars). Boosting support spending by $100,000 from this level would bring an estimated 1.45 percent increase in an initiative's chances of passage. Increasing the amount of opposition spending from its mean of $2.4 million (again deflated to 1982-84 dollars) would have almost exactly the opposite effect. An additional $100,000 in spending by the opposition campaign would lead to an estimated 1.93 percent decrease in an initiative's chances. Both amounts are statistically different from zero, but not statistically different from each other. This fits with our clear intuition that expenditures on either side of the issue should have equal and offsetting effects. It also stands in striking opposition to our basic model, which uses many conventional techniques to analyze exactly the same data. When we apply the lessons of the broader literature on money and elections, we come down on the side of the new stream of the literature challenging the conventional wisdom that the supporters of initiatives cannot spend their way to victory.

B. Robustness

To explore the robustness of our regression results, we examined a number of alternative statistical specifications. First if we replicate the instrumental variables specification including qualification spending, the results are largely the same. The marginal value of pro-money decreases, which is to be expected given that the pro-money numbers are inflated by the amount of qualification spending. Although the scholarship in this area looks primarily at a proposition's chances of passing, we also conducted parallel analyses of a measure's vote share. The results of these least squares regressions point in the same direction as our probit models, but do not achieve statistical significance. When we estimate the conventional model, support spending appears to have a negative, statistically insignificant effect on vote share, as in our previous Model #1. In our final two-stage least squares model, its coefficient is positive (0.004) but only slightly larger than its standard error (0.0039). The disjuncture between the probit and least squares results indicates that support spending has a greater impact on shifting vote shares from 45% to 55% than it does on increasing support from 70% to 80%, as one would expect. In this paper we focus on the first boost—getting the initiative from the loss column to the win column, because this is what most politicians care about and scholars write about.

Second, we pursue a number of other robustness checks. The results of these analyses are presented in Table 3.[13] Because many of the tests involve sub-samples of our 137 observations, it is reasonable to expect the statistical significance of the coefficients to decline because of the lower power of the tests. In general, we avoid tests involving sub-samples of less than 50 observations.

[13] We present only the coefficients for support and opposition spending. However, all models are estimated using the instrumental variables procedures and all the variables (unless otherwise noted) found in Table 2.

The first set of robustness tests break the sample by time. Model 3 presents results for initiatives on the ballot through 1989, while Model 4 presents results for the subsample of initiatives on the ballot from 1990 and later. The coefficients on support and opposition spending remain roughly symmetric, and in three of the four cases, the coefficients are statistically significant at the 95% level of confidence.

The second set of robustness tests slice the data by election type—general, off-year (no presidential election), and on-year (presidential election).[14] The general and the midterm elections exhibit positive (negative) coefficients for support (opposition) spending; in both Models 5 and 6, the coefficients are statistically significant at the 90% or 95% level. However, Model 7 suggests that neither support nor opposition spending exert a statistically significant effect in the national election sub-sample.. We do not have a good answer for why this is the case but speculate below.

The third set of robustness analysis examines the vote share (as a continuous variable on [0, 100]) rather than vote outcome (a dichotomous variable [0,1]) as the dependent variable. Model 8, using all 137 citizen ballot propositions, generates the same result found in Table 1: opposition spending is highly effective while the effect of support spending is not statistically different from zero. This result is not surprising because there are many initiatives for which the campaign is a "slam dunk" to begin with, and others that have little chance of passing. To solve this problem, we pursue two approaches. First, in the next section of the paper, we examine polling data to discover how the initial characteristics of the initiative affect money raised and ballot question outcomes. Second, in the current dataset, we eliminate initiatives from the analysis where the outcome was greater or less than 65-35 (35-65). It is these initiatives where money is likely to have lower effect. In Model 9 we present the results for this sub-sample of potentially

[14] There are two few observations to examine primary elections.

competitive initiatives. Table 3 shows that when we limit our analysis to these closer initiatives, there are statistically significant, substantively significant, and almost symmetric effects of money on vote share. An additional $100,000 spent for (against) moves the vote share up (down) by about 0.9%.

We conduct our fourth and fifth set of robustness analyses in Models 10 and 11. In Model 10, we include in our regressions changes in California gross state product (GSP) as an additional economic indicator variable to complement the California unemployment variable. In Model 11, we remove qualification spending from the dependent variable. As Stratmann (2005, 2006) has argued, money spent by initiative backers to gather the signatures necessary to qualify their measures for the ballot may have very little effect on their eventual success at the ballot box. In both Models 10 and 11, the coefficients on proponent spending and opponent spending are almost equal and opposite in sign as well as statistically significant.

A sixth robustness check examines the concern that because the authors coded the initiatives, there may be bias in the coding. To address this, we also employed a graduate student and an undergraduate political science student to code the ballot initiatives using the original coding sheet with three additional questions. Neither coder was aware of the aims of the project, nor had they read any draft of this paper. Because of omissions in coding, four observations were dropped leaving us with 133 observations. The results are presented in Model 12 of the paper. It shows that when we use these coders, the coefficient on "yes" money is statistically significant at the 90% level and the coefficient on "no" money is statistically significant at the 95% level. The effects of money on vote share are again substantively significant and almost symmetric.

A seventh concern that has been articulated is that the Economic Benefits variable has a somewhat odd scaling. To address this concern, we used an alternative scaling of Economic

Benefits that contained five, rather than four, categories and altered the cut-offs between them.[15] Because this concern was articulated only late in the revision process and because we were afraid of infusing author bias into the coding, we had the independent coders code this variable and then re-analyzed the data. The results are presented in Model 13 and are extremely similar to Model 12, the model with the independent coders and the original coding of the variable. A third measure of Economic Benefits, which simply asked coders to write down the best estimated in dollars of the economic benefit again yields similar results (not reported here).

Overall, the results of this section are similar to the results in the Model 2. The exceptions are Models 7 and 8. We have already discussed Model 8. With respect to Model 7, we speculate that national political campaigns overwhelm the proposition spending; however, this is merely speculation. Overall, the results of these robustness checks suggest a result similar to the main result of the paper in the previous subsection—the effects of opposition and support spending on initiative outcomes are roughly symmetric, with the effect of opposition spending somewhat larger than that of proponent spending.

IV. Supporting Evidence from Polls

To increase our confidence in this finding, we gathered data from public opinion polls taken at the beginning of initiative campaigns and tested two additional implications of our claims. First, if we are correct that spending on initiatives is endogenous to their initial chances of passage, then we should observe a strong link between these early polling figures and subsequent spending. Second, we can deal with this endogeneity by taking advantage of the time series

[15] In this alternative coding scheme, coders were asked to categorize an initiative as a "0" if it promised no or negligible economic benefits, a "1" for an estimated gain between $1 and $500,000, a "2" for an estimated gain between $500,001 and $5,000,000, a "3" for an estimated gain between $5,000,001 and $100,000,000, and a "4" for an estimated gain greater than $100 million.

created by early polls in order to measure the treatment effects of spending. If we are correct that support as well as opposition expenditures affect an initiative's fate, we should see well-funded ballot measures performing better during a campaign.

The data presented in Table 4 and Figure 1 confirms both of these predictions. All of the polling results come from surveys conducted by The Field Poll (Field Poll, 2006) approximately three months prior to each election, after initiatives have already qualified for the ballot.[16] Since survey organizations do not poll voters on every initiative, often focusing on the more controversial measures, we could not incorporate polling data into full multivariate models. But we did obtain early support levels for 34 of the 46 citizen-authored measures that appeared on California ballots from 1998 until 2004.[17] Since these are recent elections held over a relatively short time frame, we do not deflate the value of campaign spending, instead using expenditures in current dollars for the analysis presented in Table 4.

The table reports the results of a pair of "seemingly unrelated regressions" predicting expenditures by the support and opposition campaigns for initiatives. In both models, we use three independent variables. The first variable measures how close the initiative race was, according to early Field polls. Our measure of the closeness of the initiative race, which parallels measures of the competitiveness of candidate campaigns, records the leading margin that one side

[16] We take our polling figures in each election from the first survey in which The Field Poll asked respondents about a broad range of initiatives. The polls were in the field during February for the 1998 primary (results reported in release #1867), August for the 1998 general election (#1895), February for the 2000 primary (#1951), August for the 2000 general election (#1976, #1977), January for the 2002 primary (#2027), September for the 2002 general election (#2053), April for the 2003 special election (#2071), February for the 2004 primary (#2107), and August for the 2004 general election (#2127-#2131). Our analysis goes back as far as the Field Poll's online archive of reports permits.

[17] As we expected, the 34 initiatives in our sample here generated more controversy and spending then the 12 initiatives that appeared on the ballot from 1998 to 2004 but were not covered by the Field Poll. The 34 polled initiatives featured an average of $12.4 million in support spending and $9.8 million in opposition spending, and 13 of them (38%) passed, while spending on the remaining 12 (six of which passed) averaged $2.7 million in support and $0.9 million in opposition. The fact that we still find a positive effect for support spending on a subset of initiatives that was particularly contentious and which provoked high levels of opposition spending makes us even more confident in this finding. In chronological order, the 34 initiatives included in our sample are Props 223, 226, 227, 5, 8, 9, 10, 21, 22, 23, 25, 26, 28, 30, 31, 36, 38, 39, 45, 49, 50, 52, 54, 56, 61, 62, 63, 64, 66, 67, 68, 70, 71, and 72.

holds by recording the absolute value of 50 percent minus the initiative's early support level.[18] The second and third independent variables measure the concentration of each initiative's expected economic effects; these use the measures we compiled for our instrumental variable analysis, and are measured as "concentrated benefit (costs)" if between one and three industries were benefited (hurt), and as "ideological" if the initiative had little or no effect on industry.

[Insert Table 4 About Here]

This analysis makes it clear that spending levels in initiative campaigns are endogenous to the characteristics of measures and to early electoral signals. The first column of coefficients in Table 4 confirms our contention that the supporters of initiatives spend more money when early polls indicate that their campaign will be a close one. For every percentage point decrease in the margin by which the initiative is trailing or leading, its supporters spend $252,531 more on their campaign. That means that the sponsors of an initiative which is in a dead heat in the initial polls will spend approximately $10 million more on their campaign than the backers of an initiative that appears to be winning or losing by a 70% to 30% margin. Close initiative races generate high levels of support spending, just like competitive candidate campaigns. In addition to this demand-side dynamic, factors that influence the supply of money available to initiative supporters are also important here. An initiative that promises to bring a concentrated industry benefit will generate

[18] We also obtained substantively similar results when we conducted two alternative analyses using different measures of the "competitiveness" of an initiative campaign. The first considers that possibility that an initiative polling at a few points above 50% in early polls will be viewed differently by potential financial backers than an initiative polling at a few points below 50%. We operationalize this by creating a variable indicating whether an initiative leads or trails in the polls, and interacting this indicator with our measure of the size of the margin. This interaction term yields coefficients that are less than $1/10^{th}$ the size of their standard errors in both the models of support and of opposition spending, indicating that campaign financiers do not view these situations differently. Our second alternative analysis takes into consideration the historical pattern that initiatives in California lose support over the course of the campaign. Based on the fact that the average winning initiative in our dataset began its campaign with the support of 57% of decided voters, we coded another measure of competitiveness that recorded the absolute value of the distance between 57% and the initiative's early support level. As in the analysis reported in Table 4, higher levels of competition measured in this way led to a statistically significant rise in support spending, but not opposition spending.

an estimated $15.6 million more in support spending, compared to an initiative that promises no benefits or a diffuse impact. Both of these effects are statistically significant at the 95% confidence level in a two-tailed test. Ideological initiatives have no statistically significant effect on the ability of supporters to raise money.

The second column examines opposition spending. It shows that spending in opposition to an initiative is driven by the concentration of the costs that it threatens to bring, but not by how close the contest appears in early polling. The estimated effect of the initial margin is substantively small and less than the magnitude of its standard error. A closer parallel to the dynamics of support spending, though, is apparent in the strong and statistically significant effect of an initiative's concentration of costs on the level of opposition spending that it generates. Initiatives that harm a one to three industrial sectors generate an estimated $15.4 million more in opposition spending than initiatives with negligible or diffuse costs. This value is roughly the same size as the estimated effect of concentrated benefits on support spending. Again, ideological initiatives have no statistical effect on the ability of supporters to raise money.

Why would support spending be affected by early margins, but opposition spending be largely unrelated to that same margin? We do not, in fact, find this result surprising. Another conventional wisdom about direct democracy, which Figure 1 below confirms, is that initiatives tend to lose support over time. Thus, supporters are incentivized to spend money in support of their initiative, even though it is winning, in order to secure a victory as support trends downwards. The opposition, on the other hand, has the opposite incentive. Because the initiative will lose support over time, the early polls are indicative of only a ceiling of support a measure will earn. The opposition will wait to see how fast the initiative will naturally lose support before raising and injecting money into the campaign. Thus, the initial polling margin is fraught with

noise for opposition spending, and thus is not statistically significant. Note, however, that the coefficient on concentrated costs is large and statistically significant, as we would expect.

These findings shed some light on why the conventional model in the literature yields asymmetric effects for support and opposition spending on an initiative's chances of winning. The results in Table 4 indicate that spending by pro- and anti-initiative campaigns is not randomly distributed across ballot measures. Both types of spending can be predicted by the concentration of benefits or costs, and support spending also appears to be endogenous to an initiative's expected chances of winning. Supporters are able to raise money when an initiative is caught in a close race. It is close races that supporters are most likely to raise large sums of money, but they are also most likely to lose.[19] This produces a misleading correlation between an initiative's support spending and its ultimate electoral fate in the conventional model, which our two-stage estimation procedure corrects. The correction explains the dramatic increase in the coefficient on support spending from Model 1 to Model 2. Opposition spending, by contrast, is not clearly linked to a ballot measure's initial likelihood of passage. In the conventional model, there was no spurious correlation present to obscure the actual power of opposition spending. We should not expect its coefficient to change dramatically from Model 1 to Model 4, and in fact it does not.

Finally, Figure 1 provides some evidence in favor of our contention that initiatives with a funding advantage will do better over the course of a campaigns. The lines in this graph simply chart the shift in support from early polling to election day for initiatives that generate one-sided spending. The solid line tracks the 16 initiatives with more than twice as much support as opposition spending, while the dashed line represents the 11 initiatives for which opponents spent more than double the amount that supporters spent (initiatives with relatively equivalent spending

[19] Since most initiatives (21 of the 34 in our sample) begin by leading in the polls, spending is particularly high on the initiatives that have the lowest chances of passage.

are omitted). Just as the conventional wisdom predicts, both types of initiatives generally lose favor during campaigns. Yet this drop is much less steep for initiatives with twice as much support as opposition spending. This class of ballot measures loses only 5% on average, staying above the critical 50% mark. Voter approval of initiatives that generate mostly negative spending drops by 11%, down to 40%. Overall, the message from this figure is that massive support spending on initiatives helps them to perform better over the course of a campaign, often dampening the normal drop in voter approval enough to lead them to victory.

[Insert Figure 1 About Here]

V. Conclusions

Much of the previous literature on direct democracy finds that campaign spending in favor of a proposition has a weak or nonexistent effect on its chances of passage, while opposition spending exerts a strong impact on its fate. We argue that this puzzling asymmetry is an artifact of methodological flaws. We follow a new stream of the literature that sees expenditure levels as endogenous to the content of citizen initiatives, and incorporate our substantive contention into an instrumental variables estimation strategy.

Our analysis of ballot propositions in California from 1976 through 2004 begins with a basic model that produces the conventional results. After raising a series of concerns about the traditional approach, we estimate a new model that incorporates the lessons of new works on direct democracy and the broader literature on campaign finance. We show that after isolating citizen initiatives, allowing for diminishing marginal returns, and taking the endogenous nature of spending patterns seriously, the finding that created the conventional wisdom disappears. Our two-stage model reveals that both support and opposition spending on initiatives exert strong and statistically significant effects on their chances of passage. The effects are of approximately the

same scale (with opposition spending a bit more effective than support spending) but move in opposite directions. Fitting with clear intuition, and the findings of more recent work on money and direct democracy (Stratmann 2004, 2005), support spending helps initiatives while opposition spending hurts them.

One example of how support spending can boost an initiative's chances of passage comes from California's November, 2004 General Election. Proposition 71, a $3 billion bond to fund stem cell research, generated controversy because it addressed a heated social issue and because some of its backers could potentially receive research funding from it. It appeared to be struggling in a early Field Poll, with 45% of polled voters backing it and 42% opposing. Yet the campaign saw $35 million spent in favor of it and less than $625,000 spent in opposition. Based on this funding imbalance and other factors, our main model predicted that the initiative had a 91% probability of winning, and indeed the measure won, with 59.1% of the vote.

We suggest four broader lessons from this finding. First, the demonstrated effect of support spending on a California initiative's fate will likely hold in other political environments that feature direct democracy. While California may feature greater usage of the initiative process than nearly all other American states (Initiative and Referendum Institute 2006), and though successful initiatives there may be more insulated against legislative tinkering than in most states (Bowler and Donovan 2004), there is no reason to believe that these or other unique features of the state alter the fundamental relationship between direct democracy campaigns and voter sentiment. The effects that spending might have on the passage of initiatives in other American states or referendums around the world is certainly still worth investigating, and our analysis provides an empirical approach for doing so. Second, our approach, like that of Banducci (1998) and Stramann (2004, 2005), is guided by lessons learned from the literature on money in congressional

elections. The fact that incorporating them brings econometric findings into line with clear intuition highlights the importance of these lessons. When studying direct democracy elections or candidate races in other political contexts, scholars are wise to pay close attention to issues such as diminishing returns to expenditures and the endogeneity of spending levels. Third, our investigation of endogeneity provides another demonstration of the explanatory power of the ideas about concentration of costs and benefits set forth by Lowi (1964), Stigler (1971), Peltzman (1976), and Wilson (1980). The sorts of initiatives that attract lots of support often promise a concentrated benefit to a small group, making them more vulnerable to defeat in the first place. Capturing this dynamic in our two-stage model is what allows us to uncover the strong boost that financial support gives initiatives.

Finally, the somewhat symmetric effects that support and opposition spending appear to have on the fate of initiatives carry implications for the normative debate over the role of money in direct democracy. Because opposition forces have no overwhelming advantage in the effectiveness of their campaigns, campaign finance is not the source of the status quo bias that may exist in American direct democracy. Yet because business groups and other interests with money to spend can greatly increase an initiative's chances of passage, observers are right to be concerned about the ability of moneyed interests to "buy" favorable policy.

Table 1. A Basic Model Predicting a Proposition's Probability of Passage

	Model #1: The Basic Model	
	Coefficient (standard error)	Predicted Change in Probability of Passage
Support Spending (in millions)	0.008 (0.02)	0.3% increase
Opposition Spending (in millions)	-0.105** (0.04)	4.1% decrease
Is This Proposition Voted on During a Primary (rather than general) Election?	0.24 (0.18)	9.2% increase
Is This Proposition Voted on During a Midterm (rather than presidential) Election?	-0.45 (0.40)	17.5% decrease
Is This a Counter Proposition?	0.052 (0.061)	2.0% increase
Number of Propositions on Ballot	-0.027 (0.022)	1.0% decrease
Is This a Citizen Initiative?	-0.84** (0.19)	32.3% decrease
Change In Unemployment Rate	-0.67** (0.31)	26.2% decrease
Constant	0.59 (0.41)	
Topic Fixed Effects	*included*	
Year Fixed Effects	*included*	
Number of Cases	329	
Wald Statistic	70.54	
Adjusted R-Squared	0.18	

*Notes: The dependent variable is coded as 1 if the proposition passed and 0 if it failed. ** indicates significance at the 95% confidence level in a two-tailed test. All standard errors are robust for the probit estimation procedure.*

Table 2: Instrumental Variables Probit Estimation

	Model #2: Two-Stage Estimation Second Stage
Support Spending (in millions)[+]	0.0013**
	(0.0007)
Opposition Spending (in millions) [+]	-0.0013**
	(0.0004)
Is This Proposition Voted on During a Primary Election?	0.684
	(0.67)
Is This Proposition Voted on During a Midterm Election?	8.862**
	(1.93)
Is This a Counter Initiative?	-0.239
	(0.52)
Number of Propositions on Ballot	-0.064
	(0.05)
Is This a Constitutional Amendment? (not statutory)	-0.225
	(0.52)
Change In Unemployment Rate	.0921
	(1.33)
Scope of Costs for Citizens	-0.631**
	(0.24)
Scope of Benefits for Citizens	0.721*
	(0.38)
Constant	-9.512
	(2.21)
Topic Fixed Effects	*included*
Year Fixed Effects	*included*
Number of Cases	137
Wald Statistic	104.78

*Notes: Each model incorporates the innovations of the previous model. The dependent variable is coded as 1 if the proposition passed and 0 if it failed. Table entries are coefficients and standard errors for a two-stage instrumental variables probit estimation procedure. * indicates significance at the 90% confidence level in a two-tailed test. ** indicates significance at the 95% confidence level in a two-tailed test.*

Table 3: Robustness Analysis

	Model 3 1976 to 1989	Model 4 1990 to 2004	Model 5 General Election	Model 6 Midterm Election	Model 7 National Election	Model 8 Vote Share on All Props	Model 9 Vote Share on 35%-65% Props	Model 10 Inclusion of CA GSP	Model 11 No Qual Spending	Model 12 Independent Coders	Model 13 Independent Coders and Alternative Measure of Benefits
Support Spending	0.0167** (0.0047)	0.0007 (0.0005)	0.0013* (0.0008)	0.2480** (0.0933)	0.0010 (0.0021)	0.0047 (0.0043)	0.0094** (0.0038)	0.0012** (0.0006)	0.0012** (0.0006)	0.0009* (0.0005)	0.0008* (0.0005)
Opposition Spending	-0.0187** (0.0042)	-0.0011** (0.0005)	-0.0016** 0.0006	-0.1554** (0.0680)	0.0016 (0.0011)	-0.0101** (0.0026)	-0.0091** (0.0042)	-0.0012** (0.0004)	-0.0011** (0.0004)	-0.0008** (0.0003)	-0.0008** (0.0003)
Number of Cases	50	86	97	59	74	137	102	137	137	133	133

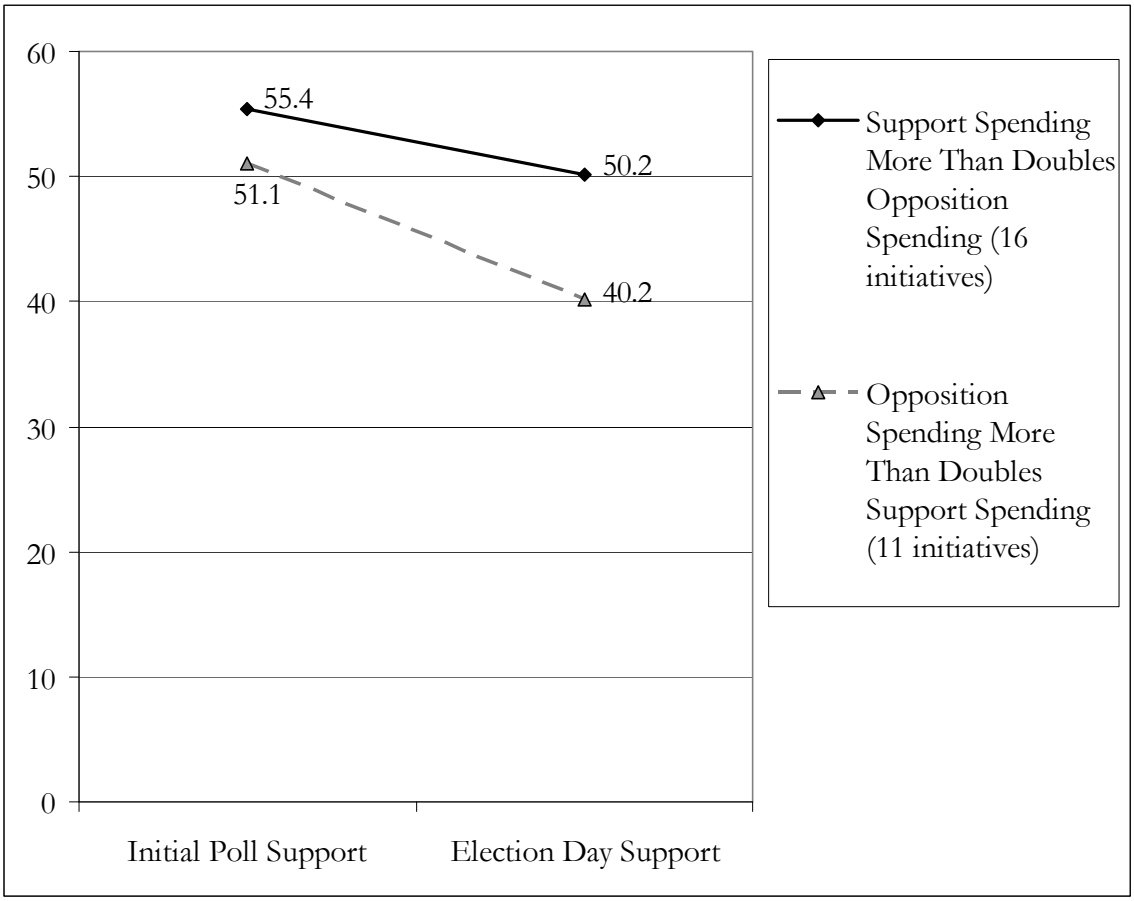
*Notes: Table entries are second stage coefficients from two-stage models that include all of the control variables contained in Model #2. * indicates significance at the 90% confidence level in a two-tailed test. ** indicates significance at the 95% confidence level in a two-tailed test;*

Table 4 . The Effect of Early Polling on Spending Levels

	Estimated Effect on Support Spending (standard error)	*Estimated Effect on Opposition Spending (standard error)*
Early Polling Margin (magnitude of either side's lead, in percentage points)	-$252,531** ($126,735)	$69,874 ($102,408)
Are Industry Benefits Concentrated?	$15.6 million** ($4.8 million)	
Are Industry Costs Concentrated?		$15.4 million** ($5.1 million)
Is this an Ideological Initiative?	-$6.7 million ($5.0 million)	-$4.7 million ($4.8 million)
Constant	$17.8 million** ($6.2 million)	$6.0 million ($5.0 million)
Number of Cases	34	34
R-Squared	0.41	0.51

*Notes: These are the results of a seemingly unrelated regressions model estimating both support and opposition spending levels. ** indicates significance at the 95% confidence level in a two-tailed test.*

Figure 1. Change in Support for Initiatives during Campaigns, 1998-2004 elections



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Appendix Table A1. Examples of Initiative Cost and Benefit Coding

Year	Proposition	Scope of Benefits for Citizens	Scope of Costs for Citizens	Magnitude of Economic Benefits	Scope of Benefits to Industries	Magnitude of Economic Costs	Scope of Costs to Industries
1978	13, Revenue Limit	3	2	3	concen.	3	none
1982	15, Handgun Registration	3	2	1	diffuse	2	concen.
1984	40, Campaign $ Limits	3	2	1	diffuse	1	concen.
1984	41, Limit on Welfare $	3	3	3	diffuse	3	none
1986	65, Toxics Exposures	3	0	1	none	2	diffuse
1988	99, Cigarette Tax	3	2	3	diffuse	3	concen.
1988	103, Auto Insurance	2	1	3	diffuse	3	concen.
1990	140, Term Limits	3	0	0	none	0	none
1992	161, Euthanasia	0	0	0	none	0	none
1994	184, Three Strikes	3	1	0	none	0	none
1994	187, Illegal Immigration	3	2	3	none	3	none
1996	209, Affirm. Action	2	2	1	diffuse	2	concen.
1996	215, Medical Marijuana	0	0	0	concen.	0	none
1998	227, Bilingual Education	2	2	0	none	0	none
2000	36, Drug Treatment	3	2	3	concen.	1	none
2000	38, School Vouchers	2	3	3	none	3	diffuse
2002	51, Transport Funding	3	3	3	concen.	3	diffuse
2002	52, Election Day Regist.	3	1	0	none	0	none
2004	64, Lawsuit Limits	1	1	2	diffuse	2	diffuse

Notes: Table entries report the median value of the coding decisions made by the three authors.

Appendix Table A2. First Stage Results from Two-Stage, IV Probit Estimation

	Support Spending (square root)	*Opposition Spending (square root)*
Magnitude of Economic Benefits	262.84*	299.93*
	(151.53)	(157.28)
Are Industry Benefits Diffuse?	-124.87	-12.76
	(297.11)	(308.37)
Are There No Industry Benefits?	-358.04	123.91
	(301.05)	(312.46)
Scope of Benefits for Citizens	-335.79**	-144.22
	(143.75)	(149.20)
Magnitude of Economic Costs	-299.56*	-15.98
	(157.42)	(163.38)
Are Industry Costs Diffuse?	113.29	-517.19
	(307.21)	(318.85)
Are There No Industry Costs?	-379.41	-991.53**
	(317.20)	(329.22)
Scope of Costs for Citizens	29.55	-42.40
	(122.67)	(127.32)
Is This a Bond?	27.18	-651.76
	(459.60)	(477.02)
Does This Change Tax Levels?	-130.69	102.13
	(263.35)	(273.33)
Is This Proposition Voted on During a Primary Election?	-534.55*	-146.47
	(278.45)	(289.01)
Is This Proposition Voted on During a Midterm Election?	-1,029.88	426.43
	(910.08)	(944.58)
Is This a Counter Initiative?	211.15	447.60*
	(254.43)	(264.07)
Number of Propositions on Ballot	-8.21	-30.52
	(26.97)	(27.99)
Is This a Constitutional Amendment?	584.14**	359.96
	(217.86)	(226.11)
Unemployment Rate	-168.96	139.58
	(283.31)	(294.05)
Change in Unemployment Rate	-717.01	-208.38
	(581.87)	(603.92)
Constant	3,788.43 (2,528.98)	1,066.84 (2,624.85)
Topic Fixed Effects	*Included*	*Included*
Year Fixed Effects	*Included*	*Included*
Number of Cases	137	137
F-Statistic	2.12	1.94
Adjusted R-Squared	0.21	0.19

*Notes: * indicates significance at the 90% confidence level in a two-tailed test. ** indicates significance at the 95% confidence level in a two-tailed test.*

Online Appendix: A Formal Model

Here we present a formal model to add insight as to the intuition in the main part of the paper. This model is not meant to be the definitive empirical set up for campaign finance of ballot initiatives. A structural model would be more appropriate, but that is beyond the scope of this paper. Rather, the model below is designed to provide the necessary intuition for the estimation strategy.

The production function for votes is characterized by the following equation:

$$y_i = X_i^{\beta} Z_i^{\gamma} M_i^{\delta} N_i^{\lambda} \qquad (1)$$

where y_i is the vote share on a given initiative *i*, X_i is a set of observable and unobservable characteristics about the initiative, Z_i is a set of variables that characterize environmental factors, such as the economy, M_i is the amount of money spent on the campaign by donors in support of the ballot proposition and N_i is the amount of money spent by donors against the proposition.[1]

To deal with the problem of omitted variable bias, we use instrumental variables to identify the empirical model. To accomplish this, we must find instruments that are correlated with M_i and N_i, but uncorrelated with the omitted elements of X_i that are found in the estimated error term. To derive such an instrument, we examine the utility function of the donors to ballot proposition *i*. We assume that the donors attempt to maximize their utility, which is characterized by:

$$\max\{p_i^M y_i - M_i\} \text{ for supporters and} \qquad (2a)$$

$$\max\{p_i^N y_i - N_i\} \text{ for opponents of the ballot measure} \qquad (2b)$$

Where p_i^M and p_i^N represent the shadow values of passage of the ballot measure to the respective donor. The optimal level of donation is M_i^* and N_i^* which equates the marginal benefit from donations equal to the marginal cost. We know from the first order condition that the marginal benefits from donating are:

$$\delta p_i^M X_i^{\beta} Z_i^{\gamma} M_i^{\delta-1} N_i^{\lambda} = \frac{\delta p_i^M y_i}{M_i} \text{ for proponents and} \qquad (3a)$$

$$\lambda p_i^N X_i^{\beta} Z_i^{\gamma} M_i^{\delta} N_i^{\lambda-1} = \frac{\lambda p_i^N y_i}{N_i} \text{ for opponents.} \qquad (3b)$$

[1] This is a standard production that is used in microeconomics to generate goods with capital and labor, where the amount of labor is endogenous.

The marginal cost of donating for both proponents and opponents is equal to one. Equating the marginal benefits to the marginal costs give us the optimal amount of lobbying, which simplifies into:

$$M_i^* = (\delta p_i^M X_i^\beta Z_i^\gamma N_i^\lambda)^{1/(1-\delta)} \quad (4a)$$

$$N_i^* = (\lambda p_i^M X_i^\beta Z_i^\gamma M_i^\delta)^{1/(1-\lambda)} \quad (4b)$$

where M_i^* and N_i^* are the optimal levels of lobbying for proponents and opponents, respectively. This suggests that each respective p^* is a valid instrument for M and N, because they are correlated with M and N but uncorrelated with the estimated error term. However, because p_i^M and p_i^N are unobservable, we must use instruments which are correlated with p_i^M and p_i^N. We use the expert coding results as our instruments which we believe to be correlated with the shadow prices as elucidated in the paper and further discussed below. In particular, we use the concentration and magnitudes of industry benefits and costs for each initiative's proponents and opponents as the instrument. The more concentrated the costs and benefits, the more likely each party is to overcome collective action problems. The higher the benefits (costs), the more likely an industry is to contribute as well. Moreover, the more concentrated the benefits (costs), the higher (lower) the shadow value (costs) of the ballot initiative. Industries cannot vote, but they do actively seek to influence initiative elections through contributions (Gerber, 1999). Hence these instruments are likely to be correlated with the shadow values but not correlated with the estimated error.

In our empirical specification, we use the ballot proposition passage as the dependent variable. This is because if there is an effect of money, we are most likely to see the greatest marginal effects of money around the 50% vote level, where money is likely to make a difference. Note, however, that we do also use vote share in percentage terms in Models 8 and 9 later in the paper.