There May Be Hope: Political Efficacy and Campaign Finance Reform

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Abstract

This paper attempts to bring conceptual clarity to the political-psychological measure of political efficacy, and provides a new empirical test of the proposition that campaign finance reform, particularly in the form of publically financed elections, makes for a more efficacious-feeling citizenry. The empirical findings come from doing a longitudinal analysis of a single state, Arizona, using a method, called "synthetic control," in which the unit of analysis is compared to a synthetic copy of itself that does not receive the "treatment," which in this case is campaign finance reform. Where previous studies have found public systems of campaign finance to be associated with lower levels of citizens' sense of external efficacy, this one finds that in the immediate aftermath of the reform there was a substantial boost in how responsive citizens perceived their government to be. The effect, however strong, appears temporary, but shows that efficacy can be rather sensitive to changes in political institutions.

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> The people of Arizona declare our intent to create a clean elections system that will improve the integrity of Arizona state government by diminishing the influence of special-interest money, will encourage citizen participation in the political process, and will promote freedom of speech under the U.S. and Arizona Constitutions. Campaigns will become more issue-oriented and less negative because there will be no need to challenge the sources of campaign money.
>
> -Preamble to Arizona's Citizens Clean Elections Act of 1998 (§ 16-940)

Chapter 1: The Definition and the Point of Researching Political Efficacy

Citizens living in a polity have feelings about the relationship between themselves and their government. These attitudes are the product of a myriad of personal and environmental factors. How citizens conceive of and relate to their government informs researchers as to the psychological dimension of modern politics, and in particular, how citizens of democracies support their political systems. Political scientists since the 1950s (Campbell et al. 1954) have tried to understand what the content of these feelings are, who holds them, what behaviors they are associated with, and what brings about changes in them. Balch (1974) and others have conceptually separated these feelings into two types, internal and external. They have chosen the word *efficacy* to denote the general sense of these feelings because they are supposed to refer to how able a citizen feels to effect change or influence in her government. "Internal" efficacy is understood to refer to a person's sense of competence when it comes to understanding the complexity of politics and political communication. "External" efficacy is meant to refer to how responsive a citizen thinks the government is to the wants and demands of people similar to her. External political efficacy is also sometimes understood as diffuse support for one's political system (Balch 1974; Iyengar 1980). However, in what sense the individual supports her political structures varies according to the question being answered,

and since researchers have used different questionnaire items in different combinations over the course of the literature on political efficacy, exactly how one paper's findings relate to another is not always clear.

Developing a clearer and deeper understanding of exactly what political scientists mean by, and more importantly, what they measure, with the concept of political efficacy is crucial to the progress of the field for two reasons. First, political efficacy is an often-used variable in analyses of political behavior; and second, it is an important concept to be understood in its own right. Political efficacy is what political scientists call it, but others have referred to a similar idea as *political hope* (Karp 1974). Describing it as hope may help us to see that it is an integral motivating factor in political life. If one has hope, then one has, presumably, reason to be hopeful. If one has reason to be hopeful, then the political system that is the foundation for that hope is in some significant way working properly. Judging a democracy purely on its objective outputs, such as gross domestic product per capita, or literacy rate, or the like, does not tell the whole story of a country's democratic performance. Incorporating perceptions of government responsiveness and perceptions of ordinary citizens' efficacy at influencing government provides a fuller picture. This is because a country's citizens, in order to be democratically governed, that is, be the governors themselves, must *feel* that they have a say in their government. Feelings of inefficacy and powerlessness are characteristic of subjects, not citizens.

If what we seek is to understand what makes for an efficacious, hopeful citizenry, then we must try to explain variations in political efficacy. I do this in two ways. First, in order to have a clearer image of what sorts of people report feeling efficacious, I review the literature on efficacy at the level of the individual. Second, in order to dig deeper into the mechanisms affecting feelings of efficacy, I examine a situation of institutional change to see if it brought about a concomitant change in perceived efficacy. There is little research (for some examples,

see Primo and Milyo 2006; Kim 2015) that has asked whether and what sort of impact the implementation of theoretically efficacy-enhancing institutions have on the political efficacy of citizens. Uncovering these mechanisms is an essential step in research on political efficacy and quality-of-government research in general.

This study takes such a step by exploiting variation in campaign finance laws in the several United States. Arizona is used as a test case because of its unique publically financed campaign system (see epigraph) implemented for state elections. A quantitative technique called the synthetic control method is employed, which allows us to compare Arizona's experience with a hypothetical and statistically estimated Arizona in which the law was not passed, in order to ask the question: "Did the passage of campaign finance reform in Arizona produce a change in how respondents in Arizona answered questions about external efficacy?" The study finds that the passage of the law made a substantially positive, though short-lived, impact. Due to its evanescent effect on efficacy, I conclude that it was the law's potential, rather than its actual functioning, that drove the observed effect. The results are further complicated by the fact that the reform was passed through a citizen-initiated ballot measure, raising questions about the relationship between external efficacy and direct democracy.

In his book on the costs of voting and voting turnout, Hanmer (2009) concludes: "Motivation, not costs, represent the most significant barrier to higher turnout in the United States" (145). Therefore, an investigation of motivation, interpreted here as political efficacy, is needed if we are to understand this most significant barrier. More importantly, higher efficacy may mean not only higher turnout, but also a more engaged and critical electorate. A sense of pervasive powerlessness or apathy is difficult to overcome, but it is the duty of public opinion research to bring its workings to light.

Chapter 2: Your Efficacy Isn't My Efficacy

The Psychological Logic of Political Efficacy

Because this paper investigates the relationship between government institutions and citizens' sense of efficacy, I to focus on external political efficacy. Changes in the political system, whether they be new electoral rules or quotas for minority representatives, can be expected to change how people feel about the responsiveness or agreeableness of their government.

Bandura's (1982) work on the percept of self-efficacy in human agency provides the key building blocks to start with. We learn from his work that "efficacy in dealing with one's environment is not a fixed act or simply a matter of knowing what to do" (Bandura 1982, 122). It is instead a "generative capability" that incorporates cognitive, social and behavioral skills in the pursuit of some action. Therefore, when tapping external efficacy, we should not expect a great deal of variance to be accounted for in any linear regression models for two reasons: 1) the action whose plausibility is under consideration is different for each respondent, and 2) the types of skills (e.g., cognitive or social) that respondents prioritize in their perception of self-efficacy differ from person to person.

Thus, when a survey question asks if people "have a say" in their government, the question is general enough that it most logically applies to the political system as a whole, but respondents may have in mind a specific facet of it, say, foreign policy or education policy. Or they may only be considering the power of voting and not other forms of political engagement. As researchers, we must be cognizant of this vagueness in our measures and future efficacy batteries should include policy- and action-specific items. As Bandura puts it, "Perceived self-efficacy is concerned with judgments of how well one can execute courses of action required

to deal with *prospective situations*" (122, emphasis mine). Which types of prospective situation respondents may envision is critical to their answer, and since this is hidden from us, we must try to gauge it indirectly.

The survey items that seek to tap political efficacy typically provide four possible answers (strongly agree, agree, disagree, strongly disagree), omitting one that would allow respondents to give a neutral response (neither agree nor disagree), but since 1988 the American National Survey has included such an option. Giving respondents only four possible categories to belong to is probably more reasonable given that we should not expect there to exist very many types of efficacy-feeling people. What these items give us, then, is a categorization of respondents into four types; what it does not give us is a something like a mood thermometer. Thus, we should expect responses to be sticky and resistant to change, particularly when analyzing data from a single item at a time. Research on political efficacy should therefore ask questions about what socio-demographic, behavioral, personality traits and experiences are associated with how responsive a person perceives her government, public officials, or public policy to be. Certainly we should also ask what it takes for a person to move from the most-negative type to a higher one; and whether this differs from moving from an inefficacious feeling to an efficacious one.

As to the second reason mentioned above, we must recognize that when asked questions about internal and external self-efficacy, respondents will consider important different combinations of efficacy-influencing factors, i.e., skills and resources, such as time and money, when deciding their response. Some will consider cognitive skills to be of great importance when it comes to having a say in government affairs. Many people may simply not know what they would need to do to raise their voice. Others will disregard such shortcomings and instead prioritize social skills. What we as researchers do not know, then, is what skills are most important to citizens when they reflect on their political efficacy. Studies that use changes in

institutional structures can at least intimate the sorts of factors that respondents use when they do so, because we assume that citizens understand what the purpose of a reform is, that is, how it is intended to improve government responsiveness or accountability. We can then ask ourselves what skills, in theory, would be activated in the use of the reform. For example, if a new law is passed whose purpose is to set aside time in a representative's schedule to listen to her constituents in a public forum, and these constituents feel a concomitant boost in internal efficacy, then we can say that social and cognitive skills can be tapped for helping raise the perceived degree of internal efficacy.

Furthermore, what one person considers to be well-skilled may be for another to be weak-skilled, though from a more objective perspective those two respondents may exhibit similar skill levels. Two individuals may exhibit very similar degrees of cognitive skill, but their self-assessments may be widely different because of how they view themselves. Thus, variation in self-criticalness may be responsible for a considerable amount of variation among respondents. However, since I am mostly interested in self-efficacy *in interaction with* the political system, and since most external-efficacy survey questions frame the interaction not as taking place with the individual respondent but rather with "people like" the respondent, problems such as this may be kept to a minimum.

Most of the literature that has used feelings of political efficacy in its analysis has focused on political participation (e.g., Balch 1974; Finkel 1985; Blass, Roberts, and Shaw 2012). Researchers have wanted to understand to what extent feelings of efficacy play a role in citizens' decisions to vote, protest, work for or contribute to a campaign, contact a representative or boycott products. In a monograph on the determinants of political membership, Morales (2011) finds that feelings of political efficacy do not explain different cross-national trends in membership in political parties and other political organizations. What her study finds is that the "openness" of the political system is a great determining factor in

people's political mobilization. That is, cross-national differences are more a matter of the institutional and organizational contexts in which they operate than individual attitudes. However, her analysis acknowledges that the individual's experience is still a key dynamic in the macro-context, since the perceived receptiveness of political authorities affects the *anticipated* costs and benefits of political action (Morales 2011, 19).

However, it should be noted that deciding to join a political party is not always the most relevant action for political participation, and therefore is not an ideal indicator of citizens' political efficacy. It is generally a very rare political act and in the American context, for example, joining a party is in many cases pointless other than as an expressive act because many states do not require membership to vote in party primaries. The important point to be derived from Morales' study, in conjunction with insight from Bandura's work, is that, in Bandura's words, "self-percepts of efficacy operate as cognitive mediators of action" (126). The actions that you take are filtered through how capable you perceive yourself to be.

How Political Efficacy Is Measured in the Literature

Political Efficacy and Political Participation

In a seminal work on political efficacy, Finkel (1985) showed, using time-series data from 1972 to 1976, that external efficacy is both a cause and an effect of voting and participating in a political campaign. This is an important insight but the empirical evidence has some weaknesses since he used latent factor analysis instead of the questions asked directly to the respondents. Moreover, the groups of questions he used as indicators for external and internal efficacy were chosen based on how they contributed to the fitness of his model, not on conceptual consistency. While goodness-of-fit is important for any model, certain questionnaire items that are more indicative of external efficacy were used in the factor analysis

for internal and vice-versa. While Finkel's research brings us closer to understanding the dynamics between efficacy and participation, it still leaves us with hazy concepts with which to investigate the minds of citizens.

In a rare analysis that operates with an action-specific efficacy measure, Lee (2010), in the wake of mass protests for democracy, surveyed eight hundred Cantonese-speaking permanent residents of Hong Kong about their protest behavior and various senses of efficacy. Though he was chiefly interested in collective efficacy, or, "an individual's judgment of the capabilities and power of the group" (394), Lee includes measures of self-efficacy as controls; his external efficacy questions asked specifically how "responsive to public opinion" (399) Hong Kong politics were. Interestingly, it is only external efficacy that reaches statistical significance in his linear regression model estimating the perceived chance of having direct elections of the Chief Executive of Hong Kong in 2012. Furthermore, the percentage of variation accounted for by the efficacy measure is virtually equal to that of variation accounted for by his demographic variables, which include education and income, variables that are historically most predictive of efficacy (Guest 1974; Gilens et al. 2001). Here we find evidence that external efficacy, when posited in relation to a specific political object (in this case the likelihood of elections being democratic), can play a substantive positive role in citizens' formulation and understanding of the political world. And because only *external* efficacy reaches such a status, we have further reason to believe that it is the concept we want to use as a gauge of a citizenry's hopefulness, an active sense of trust.

In a different model estimating the intention to participate in pro-democratic protest, we learn something else about the measure of *external efficacy*. The linear regression model (Lee 2010, 404), which accounts for 31.9% of the variance in the data, reports that feelings of external efficacy have a statistically significant negative impact on intention to protest. In fact, it is the only variable to show such direction, and it has roughly the opposite strength of the

variable for internal efficacy. Lee's results show us that external efficacy can indeed have a *negative* influence on political participation, most likely because it indicates a belief that the system is already responding to one's political preferences.

How do we reconcile this finding with Finkel's (1985)? It all depends on the sort of political participation being explained: if one perceives one's government to be responsive to people like oneself, then one needn't resort to illegal demonstration, or even legal demonstration, depending on the typical avenues of communication between citizens and government. In Finkel's case, 1970s United States, it was traditional forms of participation, such as voting and campaigning; in Lee's case, early 21st-century Hong Kong, it was protest against the political status quo. In the former, activity took place in the context of representative democracy, in the latter, authoritarianism. Here we find evidence of Balch's formulation of external efficacy as "diffuse political support." It is positively associated with system-supporting activity and negatively associated with system-challenging activity.

Political Efficacy as a Measure

Other researchers have focused solely on the measure and conceptualization of political efficacy. Balch's (1974) work was an attempt to clarify what respondents to the efficacy questions understood when they answered them. He correlated each of the four now-standard survey items with each other, and with items about participation, trust, political knowledge, and political interest. Though he used an unrepresentative sample of undergraduates, his findings reveal some plausible relationships between efficacy and other political measures.

Overall, he found rather weak correlations, but some reasonably strong (>.30) ones that help up home in on what exactly is being tapped by questions of political efficacy. The two external-efficacy questions that he asked, which were four-point scales in response to two

statements, 1) "people like me don't have any say about what the government does (*No Say*)," and 2) "I don't think public officials care much what people like me think (*Officials*)," correlated relatively well, .28 and .35, respectively, with the responses to questions about whether the government is run by a few big interests or for the benefit of all the people (Balch 1974, 7-8). Second, the highest correlations of his analysis (.34 and .47) were for the two aforementioned external-efficacy items and an item stating disgust with "the way leaders of this country blatantly ignore what we want them to do" (21). (Craig and Maggiotto (1982) also find a strong (.45) correlation between *No Say* and *Officials*.) Third, Balch reports some interesting correlations between his external-efficacy items and two conventional-participation questions. The key finding here, which the author does not draw much attention to, perhaps because the overall correlations are not very strong (<.18), is that there is a significant leap in correlation from people who said they "had" contacted a politician to people who said they "would." The same is true for individuals who said they had worked for a candidate. Here it seems that actual participation has the effect of discouraging people. Not much research since then has dealt with what these efficacy questions actually mean, but based on Balch's and Finkel's work, I believe we can say that they capture a variant of trust and approval, with sensitivity to real-life experiences.

What we can say is that, perhaps due to conceptual differences or survey imperfections, there is not perfect overlap between questions designed to measure external efficacy. This problem can be worked around using principal component analysis or latent factor analysis, or using summary indicators, on the individual questions themselves, or relying on sum combinations of strongly correlated items. I will focus on the last method because of its conceptual clarity. Most importantly, we should not simply continue to rely on vague and disparate measures of a concept we call "efficacy," but instead work with the instruments we have, that is, standard measures, and try to understand what they are telling us. We need to be

sure we are looking at how they refer to the way respondents feel about the responsiveness of their government.

In order to see how respondents feel about the receptive quality of their government, I rely on items that were asked in surveys since the 1970s (the *No Say* and *Officials* questions). Moreover, I can make use of new quantitative methods for comparative case study in conjunction with longitudinal data from the American National Election Survey in order to explore the relationship between a citizen's political context and her sense of political efficacy. I try to explain efficacy as a dependent variable, and investigate whether and to what extent theoretically efficacy-enhancing institutions affect citizens' sense of external efficacy.

Political Efficacy as That Which Is to Be Explained

The Individual-Level Characteristics Associated with Political Efficacy

In most of the literature, efficacy is used as an explanatory variable or a control, not as the outcome in need of explication. Recent research has, however, turned to efficacy as the response variable, such as Merolla et al. 2012; Milyo and Primo 2006; and Gilens et al. 2001. On the level of the individual, it has been known for some time that being a strong partisan is strongly associated with feelings of political efficacy, which makes sense because if you identify strongly with a political party, you likely did so because you support the system of which that party is a part. As might be expected, higher income and education are also closely tied to efficacy, though it is not clear through exactly what mechanisms these operate. Research also finds that people sense of political efficacy is sensitive to events in their economic as well as political sphere of life, and that when someone is elected who reflects respondents' racial or other socio-economic identities, particularly when the respondents identifies with a

marginalized or minority group within society, it can raise the efficaciousness expressed in their responses.

Merolla et al. (2012) asked whether external efficacy would increase in response to an election. Specifically, they probed into whether African-Americans felt greater efficacy after the election of Barack Obama, a change in favor of descriptive representation. Using panel data collected before and after the election of 2008, the authors estimated an ordinary least squares regression reporting a statistically significant though substantively modest positive effect (.5 on a 20-point scale) on African-Americans' external efficacy, which they measured using a summary variable. (The summary variable combines variants of the *No Say* and *Officials* questions with one other responsiveness-tapping item.) Among Democrats, whose candidate won the election, both whites and blacks reported a similar increase in efficacy, but this was only true for strong partisans. Among weak partisans, efficacy increased much more among blacks, showing that partisanship is a crucial factor among all citizens, while descriptive representation may come into play when partisanship is not at issue in determining the change in efficacy observed in response to the respondent's descriptive representative winning a presidential election.

Another, curious, finding was the strong effect of a non-political variable on external efficacy: home ownership (Merolla et al. 2012, 869). This makes sense given the collapse of the housing market in late 2008, and indeed, the negative effect of home ownership does not occur until the last wave of the panel, which comprises the end of 2008 and the first half of 2009. From this study, we can see that sharp changes in the value of citizen-respondents' assets can have substantive effects on their sense of the political system's responsiveness. We could further interpret this as efficacy's sensitivity to information about the quality of the political system or the politicians in office. Thus, we learn two things: external efficacy is sensitive to

new information, and is affected by conditions that are not obviously political, such as home ownership.

Institutions within the Political System and Political Efficacy

The subset of the efficacy literature that touches on institutional effects is rather undeveloped, but there have been a few papers on this topic published in recent years. In a paper prepared for the APSA, Gilens et al. (2001) ask whether feelings of efficacy are enhanced or injured by opportunities for direct democracy, specifically in the form of citizen-initiated propositions and other ballot measures. The authors use newspaper coverage as an indicator of media exposure, and examine whether information about propositions has any effect on citizens' efficacy. They theorize that learning about the opportunity to directly shape government policy should increase external efficacy. What they find, using survey data from the 1998 ANES, is that the difference between living in a state that does not have propositions and living in a state that received the highest observed amount of statewide media attention is equivalent to an increase of 3.2 years of education. This finding applies to the authors' efficacy scale, but their OLS coefficient for the *No Say* question is twice as large, and it is the only external-efficacy question to reach statistical significance. Whatever the *No Say* item taps, then, is related to government *policy* more so than it is public officials or the government per se, (because the *Officials* item was not statistically significant). We also learn that this sense of "having a say" is sensitive to institutional frameworks such as ballot initiatives, but that learning that they exist is a prerequisite to their having a significant effect.

Gilens et al.'s (2001) statistical results also show us that respondents distinguish between two conceptually similar questions. In the model in which the *No Say* item reached substantive and statistical significance, the question asking "how much attention does the government pay people" (21) does not. Apparently, then, there is something different about

having a say and being paid attention to. While it is not perfectly clear what the difference is, Sances' (2012) study, which uses a unique question for tapping external efficacy, may help us to tease it out.

In a study bringing together institutional change and political communication, Sances (2012) performed two experiments, one of which exposed subjects to an advertisement for a Congressional candidate while manipulating the information they received concerning the message's source. His research tests the hypothesis that large single-source campaign contributions to candidates leads to the "appearance of corruption" (54), and depresses voters' sense of external efficacy. The claim being tested here was that voters would believe that these candidates, if elected, would serve the interests of those who contributed to their campaign in the hopes of receiving funds again in the future. There should be some difference, he hypothesized, between subjects who were told that a television advertisement was provided by a rich corporation or by a trade union. Sances finds no difference in levels of government trust or political efficacy between the various treatment groups. It did not matter whether the advertisement contained a disclaimer or not, and if it did, it did not matter whether the provider was a labor- or business-representing interest. It would seem, then, that voters do not care whether political advertisements are paid for by business or labor, one of which groups, Sances must assume, the subjects sympathize with. But the appearance of corruption would probably be more likely if it were more obvious that the policies that the candidates propounded were noticeably influenced by who was paying for the advertising.

Furtermore, Sances' experiment omits a treatment condition in which the advertisement shown was one paid for by a citizens' group or the like. As Lupia (1994b) showed, voters can use cues about providers of political communication to help them choose according to their interests. In the case of Sances' experiment, voters were not given a choice that differed very much from the status quo. Perhaps there would have been a significant difference between

subjects shown an ad that had been created by a community organization instead of the typical power-holders. This may not have been Sances' primary research question, but it would have been a prime opportunity to test whether *seeing others succeed* (in this case, the citizens' group) can have an impact on one's perception of efficacy. There is also evidence for this in the psychological literature, Bandura again: "Seeing similar others perform successfully can raise efficacy expectations in observers who then judge that they too possess the capability to master comparable activities" (1982, 126-7).

It should also be noted that Sances' measure of efficacy was a question asking whether the government is "pretty much run by a few big interests looking out for themselves or that it is run for the benefit of all the people" (72). As we saw before, *No Say* efficacy is positively affected by living in a context where citizen initiatives receive high statewide press, leading us to believe that the right kind of information empowers citizens. Why is it then that information that big interests are financially supporting one's representatives does not have the opposite effect? This is even more puzzling because the question Sances asked was precisely concerned with the influence of big interests versus ordinary people. My speculation is that had the experiment included a condition wherein subjects saw that the advertising had been paid for by non-traditional or financially disinterested parties, such as consumer or environmental groups, or that the policy positions were visibly influenced by the financier, then there would have been a pronounced difference on Sances' measurement of external efficacy.

A recent paper about direct democracy and political efficacy in the Japanese Journal of Political Science, Kim (2015) finds that in those municipalities in Japan that were given the chance to vote for the first time, due to municipal mergers promoted by the national government, efficacy rose substantially. In this case, the institution of the popular vote caused citizens to report higher levels of efficacy as operationalized by Kim. While the author's findings are intriguing and seem to corroborate the intuition that participatory democracy leads

people to perceive their government as more responsive, her analysis is muddled up by combining measures of internal and external efficacy in her "internal efficacy" measure. Her "internal" efficacy variable is constructed by summing two others: the *No Say* and *Complex* items. It is not unreasonable to believe that having the right to vote would positively influence both of these percepts, but theoretically, voting's impact on what the government does (*No Say*) and on how difficult it is to understand what is going on in politics (*Complex*) ought to be quite different. It may be presumed that the practice of holding elections causes citizens, especially voters, to perceive their government as more responsive. But establishing elections, in itself, does not seem to be directly causally linked to perceiving politics to be more understandable.

Many studies have similarly combined internal and external efficacy items into one, usually calling it internal efficacy. Such inconsistency means that readers must be careful when synthesizing the literature, but it also means that some studies that appear unrelated to each other at first glance can actually provide one another insight. For example, Semetko and Valkenburg (1998) demonstrated, using panel data from post-unification Germany, that "attentiveness to news exerted a positive influence on internal efficacy" (206). They found that Germans who watched the news, whether they had been from East or West Germany, reported higher levels of internal efficacy, and not the other way around. That is, they found no evidence to believe that individuals with higher levels of internal efficacy paid more attention to political news. Their analysis, however, is difficult to interpret because they too combine the *Complex* and *No Say* items into one. They justify this choice by pointing to their models' level of fit (similar to Finkel 1985), and to some previous research's doing the same. But, as mentioned above, institutional change may not improve both dimensions of efficacy .

It is important to note that simply because models showing that attentiveness to news leads to higher *Complex* and *No Say* efficacy does not mean that both items capture what

researchers term "internal" efficacy. It could be that as one rises, the other rises as well, but for entirely different reasons. It seems most plausible that great attention to political news will, in many cases, lead one to believe that one can understand politics and government. News reports may do this by contextualizing and explaining current events to the point where frequent listeners feel they have a grip on what is happening.

Crucially, however, news reports may be describing a state of affairs that is inimical to external, *No Say*, efficacy. It just so happens that in 1990s Germany, learning about how the government was being run also had the effect of improving one's sense of the government's responsiveness. Attending to the news, learning about current politics, had the additional, but not necessary, effect of perceiving the government was, or could be, more responsive. Learning about politics can raise both measures of efficacy without both being the same thing. Indeed, internal efficacy should be sensitive to learning itself, while external efficacy should be sensitive to *what* is being learnt. Benz and Stutzer (2004) speak directly to this distinction in their paper on the relationship between the possibility to participate in politics and the desire to become informed about politics. They find that residing versus not residing in a polity that was holding a referendum on the Maastricht Treaty had a substantial positive influence on the levels of information of citizens. Having a say incentivizes voters to learn more.

My analysis will ask whether deviations from the status quo, such as the re-unification of two independent states or the implementation of universal suffrage, can have significant impacts on individuals' sense of efficaciousness. In the United States, citizens, politicians, activists, lawyers, scholars and other observers have lamented the power that money carries in politics. However, we do not yet possess an analysis of what sort of effect change in the way that money can be used in electioneering, lobbying and political advertising can have on the way political actors behave and the messages that citizens receive. Campaign finance reform is a fertile field for such research.

Campaign Finance Laws and Political Efficacy

The relationship between specific institutional political arrangements and citizens' sense of external efficacy is a topic still in its infancy. One area of the political system in the United States that is often credited with depressing efficacy and trust is campaign finance. Supreme Court judges have claimed that public awareness of the opportunity for the undue influence of large financial contributions will harm trust and confidence in the political system (*Buckley v. Valeo*, 424 U.S. 1, 1976). Recently, scholars have sought to put empirical evidence behind the Court's assertions (e.g., Primo and Milyo 2006; Miller and Panagopoulos 2011; Blass, Roberts, and Shaw 2012). Such research examines whether legal or statutory changes, especially ones made with the express intent to impact on people's perceptions of the government, do in fact change how citizens perceive their political efficacy. Primo and Milyo (2006) do this by estimating a probit model for three different efficacy questions, two external and one internal. The external items are the ones we are familiar with, *No Say* and *Officials*. Primo and Milyo use natural variation in campaign finance laws at the state level to explore whether regimes of public financing are efficacy-enhancing, as some observers speculate. Systems of public financing for political campaigns are thought to aid feelings of trust and efficaciousness.

Their dataset comprises biennial data from the American National Election Survey for the years between 1948 and 2000. While they find that the usual individual-level characteristics, high income, high education and high partisanship, are associated with higher senses of external political efficacy, their coefficients for the institutional-level variables tell a more complex story. This is partly due to the fact that some laws were in effect for only portions of the years covered in the data. Therefore, there is likely a great deal of heterogeneity among the year-by-year samples as time has gone on, as education has become more widespread, as

race and gender relations have changed, and the meaning of partisanship has changed, to name a few.

Despite these outside influences, Primo and Milyo still find that two of their institutional variables are substantively and statistically significant in their *No Say* model. First, contribution limits for organizations only (and not individuals) are associated with reports of higher efficacy, with a magnitude on par with strong partisanship. The second and more surprising finding is that respondents who live in states with regimes of public funding with expenditure limits for political candidates are less likely to say that they "have a say" in government; the same goes for the "do officials care?" outcome variable. Implementation of a public-financing law is associated with a 5% decrease in the probability that a hypothetical respondent will respond that people "have a say" (Primo and Milyo 2006, 34).

There are several caveats to these findings that the authors make, and some that I believe should be added. The first, which the authors try to address in an appendix, is that the *No Say* question item addresses efficacy with respect to the "government," not specifically state government. In a separate model they use a state-specific trust variable, and they find that, although the public-financing variable retains its negative direction, it loses its statistical significance. This casts doubt on the validity of the negative coefficient for public financing. There may be some noise in the public-financing variable that, if accounted for, may yield starker results.

A second and related caveat is that the models Primo and Milyo estimate do not take into account the quality and effectiveness of the public-financing laws. A state may pass a law that provides for the public financing of political campaigns, but if that law does not provide the right incentives, and if it not therefore useful, then there is not much difference between having and not having such a law. Indeed, for several decades there has been a public-financing system in place for presidential candidates, but it is rarely taken advantage of. For example,

when Barack Obama was running for his first term in office, he announced that he was going to rely on the public system, but once he discovered that this would hinder his chances at raising enough money compared to his competitors, he dropped that position (Malbin 2011). Follow-up research should account for the varying quality of these publically funded regimes.

Third, insofar as political efficacy is determined by the salience of campaign messages, and insofar as publically financed systems make for less lucrative, less salient campaigns, we should expect to see the result that Primo and Milyo (2006) report. However, it is not likely that such a simple relationship between salience of an election, perhaps thought of as number of advertisements aired on television, and efficacy. Certainly some elections are salient in an efficacy-promoting way and some are salient in a efficacy-depressing way. My paper tries to address this problem by using a time-series analysis that is long enough to detect a systematic change in efficacy despite fluctuations in the salience and competitiveness of elections.

Finally, Primo and Milyo also estimate a similar probit model for an efficacy-like state-referring variable. The item, which was only available for the year of 1996, asks which level of government the respondent places more "faith and confidence in" (Primo and Milyo 2006, 37-38). When regressing on this response variable, the authors find that public funding shows a negative but statistically insignificant coefficient (z-stat=1.00). Though they interpret this result as comporting with their main finding that public funding exerts a negative influence on the perception of the government, it should lead us to question the strength of that negative force.

Using data from the 2004 National Annenberg Election Survey, Miller and Panagopoulos (2011) estimate logistic regressions with efficacy and trust measures as response variables. By separating respondents by the state in which they reside, the researchers used state-level variation in campaign-finance regimes as a means of testing the hypothesis that full public funding of elections increases efficacy. Their analysis addresses the problem we saw in

Primo and Milyo (2006) where the quality of the public-finance system was indeterminate by focusing on two states, Maine and Arizona, which implemented full funding systems in the 2000 elections, and "therefore seem to be logical places in which to search for a more trusting or efficacious electorate" (Miller and Panagopoulos 2011, 239). The authors found that residents of either state did not have a greater likelihood of giving an efficacious response to the *No Say* question; the magnitude of the standard error was larger than that of the coefficient. One reason for this finding may be that the *No Say* question applies to "government" writ large, and is therefore not very sensitive to state-level phenomena. Data from one election year is likely to be quite noisy because of contextual factors that play into such a broad question. Moreover, since it was only the second election with full public funding, many of the theoretical benefits of such elections may not have made their way into the public consciousness.

Living in a fully funded state did, however, provide a large and statistically significant negative coefficient for the question of whether or not candidates try to keep their campaign promises while in office. What may be more striking than the finding itself is the fact that the authors do not comment on it, especially because it concerns the focal independent variable of the paper. What the coefficient seems to mean is that living in Arizona or Maine in 2004 made you much less likely than residents of the other forty-eight states to say that you think candidates try to keep their campaign promises. Since this question also applies to no specific governmental level, its results could also be attributed to feelings towards the federal government that are characteristic of these states in that time, but why Arizona and Maine should be so different is not obvious. If we are to use government-wide survey questions, then we must try to use a technique such that perceptions of state government will affect government-wide perceptions enough that they produce differences in efficacy detectable with our government-wide measures. However, when a respondent answers an efficacy question

whose referent is simply "the government," we do not know if she thinks about one level of government or if she considers all the levels of government with which she is familiar. Only reasonably large-scale or important state-level phenomena should be salient enough to produce an effect in most respondents, a prediction that is borne out in Gilens et al. (2001).

A state-level phenomenon that I submit fits the bill is full-fledged campaign finance reform, especially in the form of public financing for qualifying candidates. Such reforms have taken place at the federal, state, and city levels with varying success, suggesting a negative impact on efficacy at the state level. However, reformers still believe that putting campaign money in the hands of candidates who find enough support among the electorate and who promise to abide by certain spending limits should engender a field of politicians who pay more attention to their constituencies as opposed to special interests.

How Publically Financed Elections May Improve People's Feelings of Efficacy

Improvement in political efficacy should come from people's feeling that the political system is amenable to and reflective of their interests. This means that during campaigns, voters should see candidates who look like them and talk like them, but also address issues that make them feel that "officials care." This assumes that there will be some efficacy-enhancing effect from descriptive representation, but also that there will be such an effect from bringing policy in line with public opinion. Therefore, I argue that candidates who rise through a publically funded system will, on average, tap into, respond to, and represent public opinion better than privately funded candidates.

I expect the public funding of elections to produce higher levels of aggregate external efficacy for several reasons. Many of these reasons are what drove reformers to pass the so-called "clean elections" laws in Arizona and Maine. Instead of depending on special interests to finance their campaigns, these candidates could spurn such dependency and rely on wide

popular support. In public finance systems that provide funds to candidates who accrue a certain number of signatures or small donations, such candidates receive public funding because a large segment of the electorate wants to see said candidate bring his or her ideas to the political debate. In this way, voters will help determine who does, though not necessarily who does not (because participation in such systems must be voluntary under the Supreme Court's interpretation) make it to the initial stages of a campaign. The requirement for this sort of grassroots support may change the political debate in such a way as to better represent the interests and concerns of the electorate, as opposed to the interests and concerns of the wealthy portion of the electorate (see e.g., Lessig 2011 and Köppl-Turyna 2013). If voters see that the candidate pool has qualitatively changed, and they become convinced that these candidates are people who entered politics in order to become public servants, not the servants of special interests, then we should expect to see a greater level of perceived external efficacy after reforms that bring such a system into place.

The primary mechanism through which, I believe, public funding can improve efficacy is by opening up the field of political contestants to less professional but locally well-connected citizens. In theory, there are a number of well-intentioned publically minded citizens, perceiving themselves already to be very efficacious, who simply do not have the resources to make a run for office. Publically funded elections are intended to provide this sort of civically minded and community-connected individual with the capacity to run for office where before he or she would need to depend on frugal campaigning or wealthy donors to bankroll him or her. Studies have already shown Arizona's system to engender more competitive state senate races. Malhotra (2008) found that open-seat, but especially incumbent-occupied, races were more competitive, in that they had more "effective" candidates, when the public system was used (274-75). Thus, if the campaign finance system is such that it incentivizes and gives hope

to otherwise politically hopeless citizens, then it will engender a political landscape that contains more "average" people and more competitive races.

Furthermore, the fact that political hopefuls do not need to find a wealthy financier means that the sheer number of candidates or pre-candidates, so to speak, ought to rise. This may lead to voters' receiving more personal political communication, e.g., telephone calls, canvassers, especially from local politicians and candidates, which in turn may lead to voters feeling that the political system is more interested in them. Which, also in turn, may lead to more attentive and accountability-demanding voters (Benz and Stutzer 2004), and a growingly responsive representative system.

The Public Financing of Elections in Arizona and New York City

On November 3, 1998, Arizona voters approved by a 51.2-to-48.8 margin a citizen-initiated ballot measure to establish a five-member commission tasked with administering an alternative campaign finance system that would provide public monies and additional reporting, (e.g., public debates), to qualifying political candidates (Bayless 1998; League of Women Voters 1999). The initiative came partially in response to what was perceived to be a highly corrupt state legislature. In 1991, seven state legislators were indicted for corruption after an undercover agent distributed bribes and other incentives in exchange for support for legislation to legalize casino gambling. And in 1997, a year before the Act's passage, the governor of Arizona, Fife Symington, resigned after being convicted of bank and wire fraud (AZ Central 2012). The reasons for reform vary from polity to polity, which makes comparative case study tricky. This analysis exploits a statistical case-selection technique in order to minimize this problem.

In the case of Arizona's Citizens Clean Elections Act, participants in the publically financed campaign system must collect a set number of $5 donations from their district

(Citizens Clean Elections Commission 2010). After the minimum is reached, which in Arizona is about two hundred donations (with districts comprising roughly 170,000 residents), the candidate receives a lump sum, which he or she is then free to use for campaigning (Malhota 2008). There are limits to how much private funding participants can receive, as well as total spending limits. The act also put limits on how much money private contributors could give to non-participants, how much non-participants could receive from political action committees, and how much private contributors could give in aggregate to such committees during a single election cycle.

The requirement to collect money instead of signatures establishes a higher standard for the kind of support participants must carry, and therefore the degree to which they resonate with voters. Arizona's system also requires its candidates to participate in a number of televised debates and other communications platforms. This ensures that debates showcase the full field of candidates, meaning that they cannot shirk the debates if they think that public dialogue would hurt their chances. It also means that candidates who might otherwise be ostracized by the major parties for strategic or personal reasons will still get their opportunity to argue for their positions in public and be on equal footing with the other office-seekers. A wider, more competitive field should tap into the opinions of more citizens, thus raising perceptions of how represented they are, at least in the pre-election phase. And though their candidate may lose, at least he or she had the chance to make an impact on the political discussion, possibly raising *No Say* efficacy.

We should expect the effect of "clean elections" laws on efficacy to take some time, though with a reasonably sharp increase immediately following its (conspicuous) implementation. The reason that its effect should be drawn out over the course of a few elections is that incumbents may not think they need to participate in the public system in order to finance their campaign: they already have a pool of donors and contacts who can provide

them support. They also have more name recognition than most potential publically financed candidates, and more political experience to tout, among other advantages of incumbency.

Another reason we might expect an increase in efficacy is due to the passage of the law itself. If the law is perceived as making the government less tied to special interests, then these citizens will experience a concomitant rise in external efficacy. Crucially, to have long-lasting effect, the law must also appear to deliver results. Reforms may sound effective in writing, but if they do not translate into tangible changes for voters and citizens, then people's sense of efficacy may return to where it was, or even lower.

We could speculate as to which of the efficacy measures should be more sensitive to clean elections reform. It seems as though the passage of the reform itself should be measured by the *No Say* item because it, of the two items, refers more broadly to the political system. Citizens may feel that because elections have changed in such a way as to attenuate the power and influence of wealthy contributors, people like them will have more of a say. They would not necessarily feel that officials care more about them based on this knowledge because they have not seen officials do so yet; they could easily remain skeptical of politicians despite the clean elections reform. However, over time, as the public system "works its magic," citizens may actually see that their elected officials care about them, or at least seem to care more than their privately funded predecessors.

New York City's experience with publically financed campaigns can provide some evidence to buttress our theoretical predictions. In a report by the Brennan Center at the New York University School of Law, Migally and Liss (2010) lay out what has changed in New York City electoral politics since the implementation of a publically financed system. In 2009, they say that "the system helped a crop of challengers actually defeat incumbents. All five incumbents who lost election in 2009 were defeated by candidates who participated in the program" (20), leading them to believe that New York's system contributed to a higher level

of competition and a smaller role for private, special interest funding. In addition to spending parity, the authors note that "under the NYC system, candidates are incentivized to build networks of small donors who become networks of organizers" (18), which, they argue, leads to voters feeling a sense of inclusion in political considerations through more local meetings, rallies, and the like.

The argument, therefore, is that the quality and diversity of candidates will change in an efficacy-enhancing way as a result of publically financed elections. This assumes that the candidates who in the end are the most popular and competitive in the general election are candidates who came up through the public system. If the political landscape is still dominated or strongly influenced by privately financed candidates, then the debate surrounding the campaign will be influenced by their financiers' interests, or conversely, that the policies implemented after their election will reflect their financiers' interests.

Chapter 3: A Comparative Case Study of Arizona's Clean Elections Act

Synthetic Control as a Method to Analyze the Link Between System-Level Changes and Individual-Level Efficacy

In order to gauge the impact of system-level changes on individuals' perceptions of external efficacy, this paper takes advantage of a statistical technique called Synthetic Control (Abadie et al. 2015). Its creators have published articles employing the method where they tried to measure the effect of the passage of a citizens' proposition (Proposition 99) in California in 1988 whose purpose was to curtail the sale and spread of cigarette smoking. Comparing California's cigarette sales to the country's at large would not be a compelling argument because California, even before Proposition 99, was already selling fewer packs per capita than the national average. Thus, Abadie et al. (2012) used their technique to "synthesize" an artificial California to serve as a control against which to compare the actual California. This synthetic California would act as the counterfactual: the California in which Proposition 99 was never passed. The synthetic control is generated by combining and weighting a handful of other, similar cases, in this case, other states in the Union. It is up to the investigators to choose which states are among those that the program can select from in order to create the synthetic control, but the program determines which of these are sufficient to produce a control case that matches the treatment case in its pre-treatment characteristics; for example, in the California case, the retail price of cigarettes and GDP per capita. The technique is intended to mesh the systematic qualities of quantitative methods with the explanatory power of qualitative methods, in particular, comparative case study.

This chapter presents a synthetic experiment using residence in the state of Arizona as its treatment. The analysis was performed using a package for R designed by Abadie et al. (2011) called "Synth." Arizona was selected because of its statewide public financing system and other electoral reforms that were implemented after the passage of the Citizens Clean Elections Act in 1998. It was also selected because it served as one of the two states on which Primo and Milyo (2006) and Miller and Panagopoulos (2011) based their analyses of the effect of campaign finance laws on political efficacy. Furthermore, Arizona's experience suits this sort of method because its institutional change took place far enough in the past for there to be a few elections' worth of data points; and, equally as important, the change took place recently enough to provide a healthy number of pre-treatment data points for the purpose of constructing a quality synthetic control.

Since the synthetic-control method does not produce estimates of the independent variables in the manner of a linear regression, the robustness of its findings does not depend on a linear relationship existing between the dependent and independent variables. One of the benefits of this method is precisely in its difference from commonly used statistical techniques. It allows us to address the question of change across time, instead of simply estimating the time-slice effects of a group of explanatory variables on a response variable. All together, those time-slice studies provide us with the knowledge about which variables produce significant effects on our outcome variable, knowledge which we can then use to try other methods. Even though Primo and Milyo (2006) use the ANES Cumulative Data File, they do not take advantage of its longitudinality, instead exploiting it for its large number of observations, some of which happen to have been made in states at times when there existed public financing systems. Here I exploit the same dataset, but do so in a different way. If my results corroborate Primo's and Milyo's, then the argument that publically financed state elections do not raise the

efficacy of the electorate is strengthened. If my results contradict theirs, then there is greater reason to delve deeper into the positive effects campaign finance reforms may have.

Data and Operationalization

The dataset employed for this analysis was the May 14, 2015 version of the American National Election Studies (ANES) Cumulative Time Series Data File (1948-2012). However, due to the specific needs of the synthetic-control method, the data file was modified in several ways. The most crucial of these is the need for a balanced data panel with which to provide Synth. Therefore, all the cases (states) in the years analyzed must have at least one observation in the variables of interest. And because the ANES time-series surveys take samples that are meant to be representative of the national population, not state populations, some years there are only a few respondents from any given state, and some of the less populous states may not have an respondents at all. Thus, to cut down on this problem, only surveys starting from 1972 were used (N = 40,358), and all of the states for which any years were missing were removed from the donor pool. Also for this reason, the other state that researchers have used as a test case for campaign finance laws and efficacy, Maine, was not studied.

Arizona itself is not the most populous state, ranking 14th in 2009 (U.S. Census Bureau), and in some years only a handful of respondents were interviewed in the state. Therefore, I decided to drop those years for which Arizona observations were lacking from the data; these turned out to be 1988, 1990, 2002, 2004, 2006 and 2010. It is unfortunate that many of these years come post-treatment, but this is also due in part to the ANES Time Series' not being performed in 2006 or 2010. The problem of missing observations could also be addressed by randomly imputing the missing values, but here I stick to the data that were actually collected and see what comes out.

The benefits of using sixteen states as potential controls lie mainly in their geographic and political diversity. Moreover, as Primo and Milyo (2006) note, we are using residents of the several states as either being subject to this natural experiment's "treatment" or not; that is, living in post-1998 Arizona is the relevant treatment. Therefore, so long as there are relatively large numbers of subjects in either experimental condition, our results are reliable. We do not need a representative sample of each state in order to draw inferences of the sort we are doing here.

The synthetic-control model that I estimate is based on six variables that have been shown to be associated with external efficacy (Iyengar 2003; Primo and Milyo 2006; Merolla et al. 2013). They are age, race, education, income, home ownership and strength of partisanship. By far the most powerful effects were provided by education and income, which has been found time and again in the literature (Balch 1974; Guest 1974; Gilens et al. 2001). The dependent variable is external efficacy operationalized as the ANES's external efficacy index, which takes into account the responses to two questions, the *No Say* and *Officials* items. All variables were entered as averages per state per survey year into the panel that Synth operated with. For example, California would have one observation for each variable for the year 1972 and so on, reflecting the average age, average educational attainment, etc.

Age has continued to show a statistically significant effect on efficacy, with the finding usually being that "older respondents tend to believe that politics is too complicated and that they have no say" (Miller and Panagopoulos 2011, 243). The same can be said for poorer and nonwhite respondents. Earlier research, especially Guest (1974), focused on the difference between whites and blacks with respect to feelings of efficacy and powerlessness, which made sense during a time in American history when these identities characterized the main racial divisions in the country. With the growing Hispanic populations in many of parts of the country, we may ask whether "nonwhiteness" still has a negative association with political

efficacy. My analysis, however, assumes that what is important to model is the percentage of the state that identifies as white or does not identify as white. This is to keep to the convention used in previous research, to keep the model simple, and also to construct a statewide percentage variable, as this is the sort of variable that the authors of Synth recommend (Abadie et al. 2015).

Education has also tended to correlate with feelings of greater external efficacy, though the effect is usually more pronounced for internal efficacy, as one might guess. It is unclear whether there is a linear relationship between education and external efficacy, since we can think about it in similar terms as we did for the effect of attentiveness on efficacy in Semetko and Valkenburg (1998). However, insofar as those who are more highly educated are also wealthy, and insofar as wealth serves as a direct instrument with which to influence politics or as an indicator that one is capable of understanding and extracting what one wants from the world, education should correspond to greater feelings of responsiveness coming from the political world.

Strong partisans also report feeling that government is more responsive to people like them, and it is therefore a crucial variable to include in any model dealing with political efficacy. We do not know whether a heightened sense of efficacy comes before identifying as a strong partisan, or vice-versa; but it seems obvious that voters who identify strongly with a party most likely do so because they feel that that party amplifies their voices. Therefore, strong partisanship reflects a certain support for the political system.

Home ownership was included in the model because of its importance in recent research on political efficacy, particularly Merolla et al. (2013). It is especially important given that it serves as a measure capable of accounting for individuals' sensitivity to the performance of the economy, and, crucially, to the housing-market collapse of 2008 and other devaluations in real estate. Furthermore, questions regarding approval of the nation's economic performance go

only as far back as 1980, whereas home ownership was measured in much earlier ANES time-series studies.

Age—Respondents were between the ages of 18 and 96, anyone over 96 was coded as 96. This variable appeared as the average age of the respondents surveyed in a given year in a given state.

Race—This variable is coded as the percentage of a state's sample that reported being white. Respondents who said they were white were coded 1; those who said they were something else were coded 2.

Education—Respondents were assigned to one of four categories according to the highest level of education completed. The lowest level was grade school only (8^{th} grade), the highest was college degree or higher. In between were: high school diploma and some college.

Income—As this study uses longitudinal data, the income variable is one that reflects into which of five percentile groups respondents placed themselves and their immediate family members. The percentile groups were: 0 to 16, 17 to 33, 34 to 67, 68 to 95, and 96 to 100.

Home ownership—This variable captures the share of respondents who reported owning their residence. Respondents were coded as 0 if they did not report owning their domicile, 1 if they did.

Strength of partisanship—This variable reflects the share of respondents who initially reported themselves as either a strong Republican or a strong Democrat. Independents were omitted from the analysis.

External Efficacy Index—This, the outcome variable, is a measure that combines agree or disagree responses to two external-efficacy questions. The wording of these has changed slightly over the years, but the most recent formulations are: 1) "Public officials don't care much what people like me think," and 2) "People like me don't have any say about what the

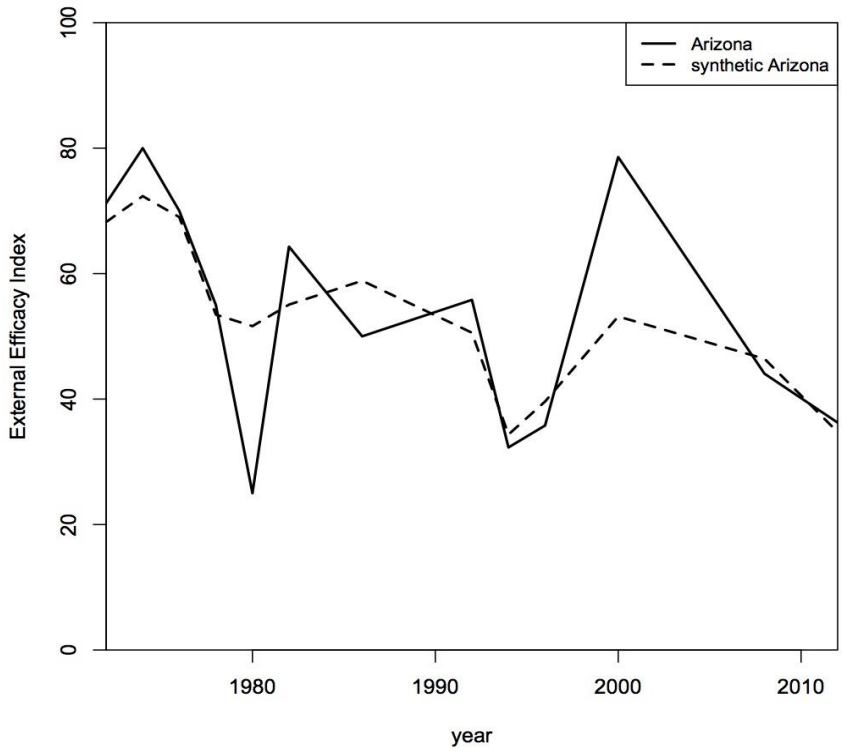
government does." The index variable gives respondents 100 points for each efficacious response they give, 0 for each inefficacious response, and 50 to those who answered "neither agree nor disagree" (only asked after 1988). The scores for these two variables are then summed and divided by the number of valid responses given. Thus, scores are multiples of 25, and 100 is the maximum score. This is also the variable that Chamberlain (2012; 2013) has used in his time-series analyses of external efficacy in the United States.

The index is numerical, but the numbers come from answers to yes-or-no questions. The fact that it is a numerical index allows us to more subtly trace a reasonably coherent percept across many people in many places over great lengths of time. Its being an index makes it sensitive to more efficacy-impacting stimuli than a single question. But then when it comes to interpreting changes in the index, we do not know which question the treatment was more impactful on, or if it was more or less equally impactful on both.

Results: A Spike in the Desert

Figure 1 plots the trends in external efficacy in Arizona and the synthetic Arizona generated by Synth. From the figure we can see that in the year 2000, which is the first election year after the passage of the Citizens Clean Elections Act (1998), there was a spike in the external-efficacy index in the actual Arizona as compared to its synthetic control. This jump represents more than 25 points on the 0-100-scale index, an increase of over 50% from the previous year for which there were data, 1996. After 2000, the efficacy-index scores drop back down to where they hypothetically would have been had Arizona not passed any campaign finance reforms.

Figure 1: Arizona vs. Synthetic Arizona



Note: The year of treatment is 2000. N = 40,358.

It should be noted that in 1980 we also see a drastic difference between the actual and synthetic Arizonas. While we should expect for there to be some noise in the estimates, as some years contain only a few observations, we should also try to understand why we observe a drop in 1980. One theory for why we see a jump in 2000 may be that a Republican, George W. Bush, was elected president of the United States; and we might therefore expect a similar event to cause a similar change. However, the opposite occurs in 1980, since that was a year in which another Republican, Ronald Reagan, was elected. Electoral victory, then, should not be considered a primary explanation for what we observe in 2000.

Similar to both Abadie et al.'s (2012) analysis gauging the effect of cigarette-regulation reform in California, and their analysis (2015) of the effect of the reunification of West and

East Germany, my synthetic-control model returned weights for five control units. The configuration of the model's weights are displayed in Table 1. In descending order, they are Minnesota, New Jersey, Florida, Ohio, and Texas. The other states in the donor pool contributed no weight to the synthesis of the artificial Arizona against which actual Arizona (with the Citizens Clean Elections Act) was compared. Later I will run robustness checks to test the sensitivity of the main result to changes in the state weights.

Table 1 Synthetic Weights for Arizona

State	Synthetic Control Weight
Minnesota	0.545
New Jersey	0.271
Florida	0.145
Ohio	0.026
Texas	0.009

Note: There were sixteen total states in the donor pool: They are: California, Florida, George, Illinois, Indiana, Massachusetts, Michigan, Minnesota, New Jersey, New York, North Carolina, Ohio, Pennsylvania, Tennessee, Texas, Virginia.

Apart from 1980 and 2000, synthetic Arizona and actual Arizona are largely indistinguishable. This would appear to mean that the efficacy trends observed in Arizona are what would be expected barring any external shocks or profound institutional reforms. Such concordance between the treatment and control groups lends some credence to the inferences we make based on the lines in the graph, but the synthetic-control method provides us with other avenues through which to check the reliability of our results.

Abadie et al. (2012; 2015) discuss these checks in their work explicating the method, and I shall take advantage of these. There are two chief ways, what Abadie et al. term in-time and in-space placebos. In-time placebo tests change the time at which the treatment is applied in the model, and in-space placebo tests perform the same analysis, but on one or more of the control units instead. If the treatment period is set to a time at which the real-world change did not occur, then we should not see any marked change between the line plots for the synthetic and treatment units. If one of the control units, which were selected because they did not

experience the same condition as the treatment unit, is set as the treatment unit, then, again, we should not see any marked difference between itself and its synthetic control after the supposed treatment time. Deviation from what is predicted by the synthetic control, therefore, should only occur during the time and in the place where the treatment occurs.

Table 2 External Efficacy Predictor Means before Arizona's Clean Elections Reform

	Arizona	Synthetic Arizona	16-State Sample
External-efficacy Index	53.7	52.9	47.5
Age	47.9	45.1	45.2
White	1.17	1.11	1.18
Income (1-5)	3.01	2.99	2.92
Education (1-4)	2.65	2.66	2.52
Home ownership percentage	1.17	1.31	1.29
Strength of partisanship	1.29	1.25	1.32

Note: Age, white, income, education, home ownership, and strength of partisanship are averaged for the 1972-96 period. The last column reports the average for the 16 U.S. states in the donor pool.

Table 2 compares the pre-reform characteristics of Arizona to those of synthetic Arizona, as well as to those of the sixteen states in the donor pool. The results in Table 2 suggest that the synthetic Arizona provides a much better comparison than the donor pool in terms of the external-efficacy index, income and education variables. The superiority of the synthetic Arizona as a comparison is not as pronounced for the age, white, and home ownership variables. Strength of partisanship is more or less the same. Perhaps the weakness of the synthetic control's predictor means with regard to the independent variables indicates the difficulty that exists in trying to predict external efficacy with only demographic variables.

The fact that only seven (7) observations are recorded in 2000 in Arizona raises serious concern. The problem of missing or small-n state-level data is the most significant problem plaguing this study. Thankfully, however, we can run reliability and robustness checks to strengthen our belief in the findings. One way of testing the potential impact of the small number of observations, especially in the first post-treatment year in the treatment unit, is to

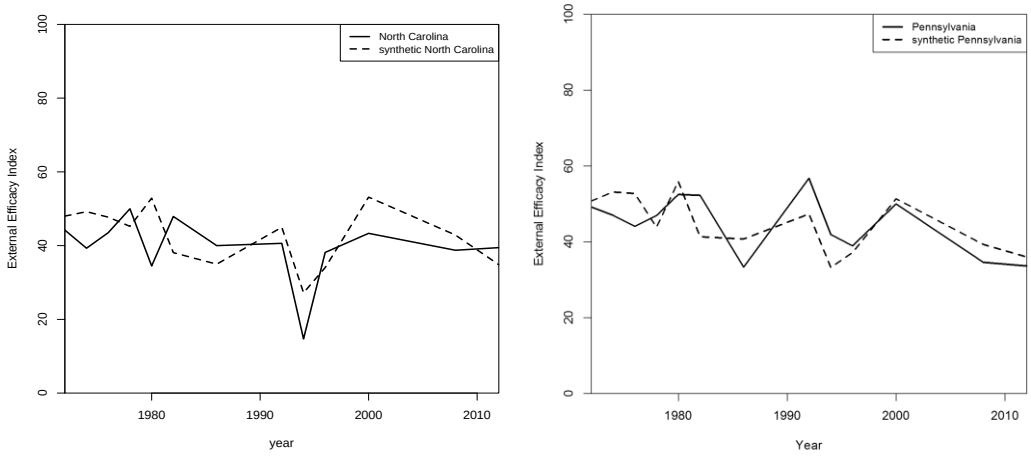
run placebo tests using the other states that have dangerously few observations, but still made it into the analysis because they had observations for all the relevant years. Two states fit this description: North Carolina and Pennsylvania.

In-Space Placebo Tests

North Carolina had the second-fewest observations after Arizona in 2000: fifteen respondents were interviewed. Figure 2 shows the line plot with North Carolina as the treatment unit. As we can see, the actual North Carolina does not deviate very significantly from its synthetic control pre-treatment, nor does it do so in 2000. The same is true for Pennsylvania, which had twenty-seven observations for the year 2000. Figure 3 shows that external efficacy in the state founded by William Penn hugs even tighter to its synthetic counterpart throughout the measurement period. These two comparisons serve to illustrate that states with relatively few observations in the year of importance did not exhibit such drastic change as did Arizona from what would be expected. Therefore, it is possible that, due to sampling error, in 2000 Arizona could have generated practically any fluctuation in its external-efficacy index. What we observe, however, is that it took a direction we would theoretically expect with a magnitude we ought to interpret with caution.

But since Arizona is the weakest state in the dataset, we should look closely at the relationship between its yearly number of observations and its correspondent results. One such unsettling result is 1980, the only other data point in any of the models that compares to 2000's. The number of respondents for this year is six; undoubtedly, a good portion of the fluctuation is due to sampling error.

Figure 2: North Carolina **Figure 3: Pennsylvania**



Note: The year of treatment is 2000.

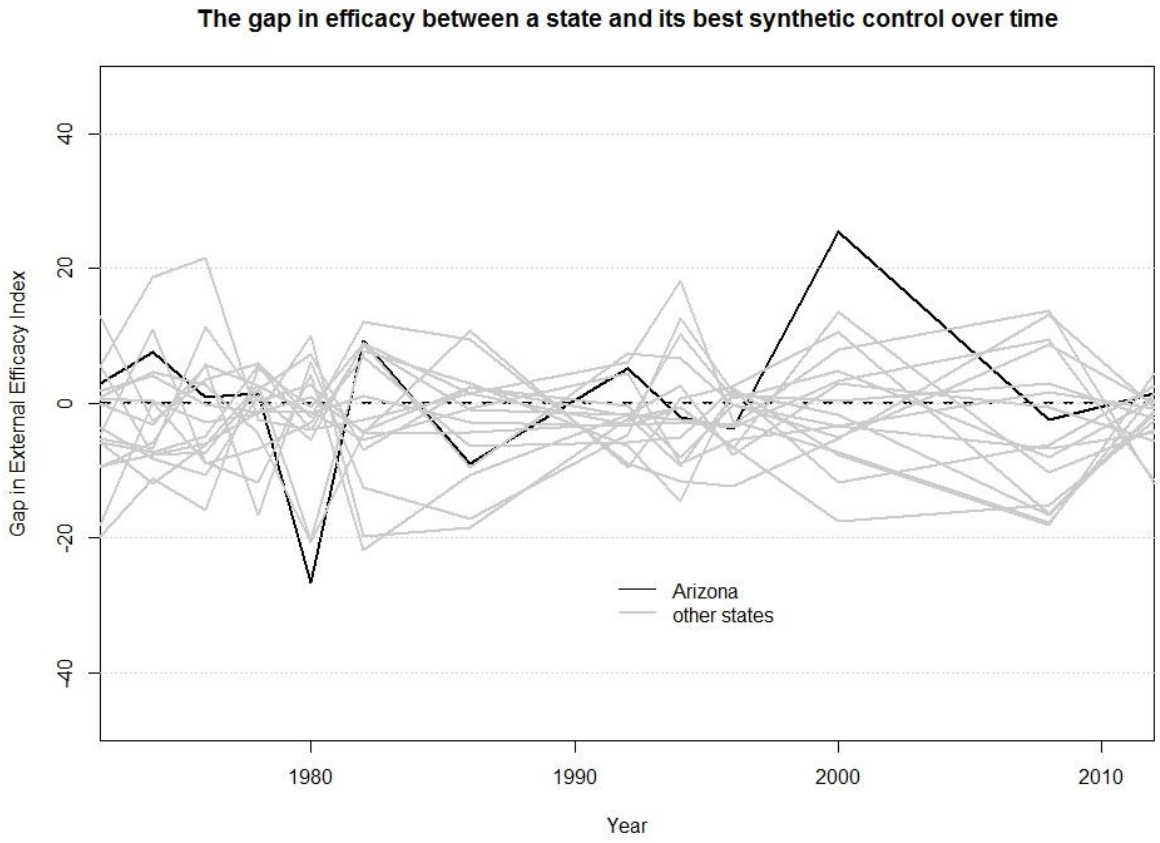
There are three observations I can make to buttress the claim that 2000's data point is not entirely due to chance. The first is that, as can be seen in Figure 4, which shows the difference ("gap") between the control and treatment units for all units in the model, 1980 also saw similar sudden dips in external efficacy in other states. This may be a small indication that there was some cross-country efficacy-damaging event that happened to be picked up in a conspicuous way by the few individuals who were interviewed in 1980. Second, because 1980 comes at a point in time relatively close to the beginning of the time series, there are fewer data with which to calculate an optimal prediction. Thus, the earlier in the time series, the more wild any fluctuations based on sampling error will be. The third and most important reason to believe that 2000 is not mere fluctuation but is instead evidence is that it is predicted by theory.

Figure 4 shows the difference between each treatment unit and its synthetic control over time, the "gap" line plot. This represents in-space placebos tests for all the units in the donor pool as well as our treatment unit, Arizona, which is shown in bold in the graph. Arizona's external efficacy score rose twenty-six points in 2000. To put this into perspective, recall that the index is made up of two questions, the "do officials care?" and "people like me have no

say" items. An efficacious response to one of these questions is coded as fifty points; therefore, a jump of twenty-five points is like answering half as efficaciously as a fully efficacious response. If the ANES Cumulative Data File used a four-point scale instead of a two-point scale, then we might be able to say that the Citizens Clean Elections Act brought the efficacy level of Arizona residents up one point, such as from "slightly disagree" to "slightly agree." Or, since we might interpret the twenty-six point jump as reflecting some degree of sampling error, we could imagine that on a five-point Likert scale, which some other surveys and studies have used, the 1998 election reforms in Arizona caused respondents in 2000 to report that they felt their efficacy to rise one level from where it would have been had the reforms not become law. This comparison might come in form of moving from "slightly disagree" to "neither agree nor disagree." But how do we know if the reforms had the effect of moving already efficacious individuals into more efficacious ones, or if it had the effect of lifting inefficacious ones to semi-efficaciousness?

Herein lies another difficulty in interpreting these findings. The external-efficacy index is continuous, but we must remember that we are, after all, working with ordinal data. Further research should explore among whom exactly change takes place, for this will tell us something of great importance about the psychological impact of political reform. If Hanmer's (2009) work on voting-registration reform is any indication, then change is more likely to come from the less advantaged socio-economic parts of society; but registering to vote and perceiving the government to be amenable to influence from people like oneself are not the same thing. The latter, external efficacy, shapes to some extent whether people do or do not vote.

Figure 4: All the Gaps



Robustness Test

By iteratively eliminating the least-weighted control unit from the donor pool, we can check whether the synthetic-control combination produced by Synth leans too heavily on one control unit. Removing control units means we sacrifice goodness of fit: Synth will generate estimations for the predictor variables, e.g., average share of strong partisans, that are not as close to the actual treatment unit's averages, i.e., Arizona. Yet, by doing this, we can evaluate whether our results are driven by any particular control state.

The overall pattern observed in Figure 2, which depicts the comparison of Arizona to its synthetic counterpart, also holds for the models that contain four, three, and two control-unit states (graphs not shown). The main result of the initial finding is left intact, and the weights of the controls stay quite proportional throughout the process. In the four-state

combination, shown in Table 3, New Jersey picks up a considerable amount of the slack left behind after the removal of Texas, but the model still displays a similar finding. The performance of the model depends on a almost equal balancing of two control units; it does not suffer from overdependence on one alone.

Table 3 Synthetic Weights from Combinations of Control States

Synthetic Combination	State and Synthetic Control Weight				
Five-state combination	Minnesota 0.545	New Jersey 0.271	Florida 0.0145	Ohio 0.026	Texas 0.009
Four-state combination	Minnesota 0.460	New Jersey 0.497	Florida 0.038	Ohio 0.004	
Three-state combination	Minnesota 0.513	New Jersey 0.479	Florida 0.007		
Two-state combination	Minnesota 0.516	New Jersey 0.484			
One-state combination	Minnesota 1				

Note: The combinations sum to one.

Abadie et al. (2015) caution users of the synthetic-control method that "units affected by the event or intervention of interest or by events of a similar nature should be excluded from the donor pool," (500) though the donor pool should be comprised of units with similar characteristics. Thus, since Minnesota possessed some form of partial public financing for state offices starting in the 1970s (Mayer and Wood 1995), I also created a model omitting Minnesota. The results from the main analysis remain unchanged: the year 2000 still shows a more than 20-point leap in the external-efficacy index.

Discussion

The most notable finding of this study is that Arizonans did appear to feel substantially more efficacious after the passage of the Citizens Clean Elections Act of 1998. Equally important to note, however, is that this effect dissipated after 2000, the first election year after

the law's enactment. The initial uptick and subsequent drop in efficacy show that perhaps the reforms did not live up to their potential, or that the sort of effect that such reforms have on efficacy is limited by other concerns, or that the memory of the reform was short-lived. It is safer to say that reforming campaign-finance laws *can* and *probably did* have a positive impact on external political efficacy, and that its effect is probably ephemeral. However, this is only one state and one reform. There are other sorts of campaign finance reform that can be implemented, such as donor-matched systems (See e.g., Migally and Liss 2010; Malbin 2012), and these may provide a longer-lasting boost to efficacy.

It may be going too far to say that reforming campaign finance laws, "cleaning up elections," causes a rise in external efficacy because of the possibly confounding effect brought to bear by direct democracy. The fact that the Citizens Clean Elections Act was passed via citizen initiative on the 1998 ballot (with 51.2% of the vote) should not be forgotten or dismissed (Bayless 1998). The research on direct democracy's effect on external efficacy usually asks the question: does the opportunity or practice of direct democracy increase citizens' sense of efficacy? We may ask in this instance if the passage of this single ballot initiative had the substantial effect we observed above. It is also possible that one or a combination of the other ballot measures that passed in 1998 may have contributed to 2000's spike. One such measure is Proposition 103, which allowed for open primaries, meaning that voters did not have to be registered with a particular party in order to vote in a primary election. I am not aware of any research on the effect of such electoral reforms on efficacy, but it is not outlandish to think that some people might feel more efficacious as a result of having the ability to vote in any primary they wish without having first to register with a party. It would be expected to have an impact on independent voters who would otherwise be excluded from voting in primaries.

If the hop in efficacy of 2000 is not mere sampling error, and if it is due to the clean elections reform, and since its effect was short-lived, it is likelier than not that the reason had something to do with the hope and promise surrounding the law than the effect of the law itself. Although participation in the Clean Elections program has risen since its inception, in 2000 only 26% of candidates in the primary elections worked in the system, and only 29% of candidates in the general election did so (Citizens Clean Elections Commission 2010). These numbers hit their peak in 2008 when over 60% of candidates in both stages of the electoral cycle had committed to campaigning through the Clean Elections program. Thus, there does not appear to be a strong relationship between the percentage of candidates using the system and the efficacy of the electorate, since Arizona's external-efficacy index scores dropped back down to their secular trend after 2000. But give that the system was brand new in 2000, the fact that there were any candidates running with the help of the public system may have been enough to inspire Arizonan voters, especially coming off the heels of the corruption scandals in state politics in recent years. If the data were available, performing a similar analysis of Maine and its clean elections program would provide valuable insight into the strength and duration of such reforms.

Though Primo and Milyo (2006) and others have found public financing systems to have negative effects on efficacy, our analysis shows that they can have a positive effect, and furthermore, that Arizona's external efficacy did not drop below its synthetic control after the treatment's effect wore off, further contradicting Primo and Milyo's (2006) finding of a negative effect. There may be a negative effect of public financing as compared to other systems of finance, but there does not appear to be a negative effect in the same state as compared to itself before it had public financing. Therefore, it may be too soon to declare public campaign finance systems as deleterious to citizens' sense of efficacy.

Figure 1's results may be novel because no other research has looked at the effect that the institution of a public financing system has had on external political efficacy in one polity through time. This paper speaks to what sort of effect it can have on people who go from one sort of system into another. It does not use the treatment (living in a state with publically financed elections) as if it were the same in all places at all times, as Primo and Milyo (2006) do. Their probit models contain a public-finance variable that groups together all the people who lived in any sort of public-finance system in any given year. If each state is a petri dish of varying characteristics, then even adding a perfectly uniform treatment to each will likely produce different outcomes, simply because the initial conditions were not the same. This unavoidably confounds the effects of various types of campaign finance laws, as well as the varying reasons for and conditions underlying each state's decision to reform, and each reform's life after its passage.

The Citizens Clean Elections Act itself has not remained static. One of its key provisions, which allowed participating candidates to receive extra funds if their privately financed competitors surpassed a certain fundraising threshold, was struck down by the U.S. Supreme Court (Liptak 2011). Since participation in the system is voluntary, this ruling weakens the act's incentives and its safeguards against well-financed candidates outside the system. The larger point is that the treatment of "living in a state with publically financed elections" is too vague to conclude that such elections have a negative effect on efficacy.

Another kink in the analysis comes from the wording of external-efficacy survey items. My analysis tries to gauge the effect of state-level change with (probably) federal-level survey questions. The items themselves are rather standard, having been asked since the 1950s, but they do not specify exactly which level of government the respondent should think of when thinking of how to answer. It is likely that respondents mostly interpret it as referring to the federal government, as it is usually more spoken about in political media, but this will vary

from place to place and across time as the media through which people informed themselves change and as political culture varies across the country (Chamberlain 2013). Interpreting my finding as reflecting a change in state government therefore requires us to believe that, when answering the efficacy items in 2000, respondents were influenced by the reforms that were passed in 1998. This we simply cannot know for sure. What we can say is that perhaps due to the salience of the reform, citizens felt that their state political system, which is not explicitly excluded from the efficacy questions, was more responsive to people like them, that they "had a say" and that "public officials cared."

We could speculate which of the efficacy measures should be more sensitive to clean elections reform. It seems as though the passage of the reform itself should be measured by the *No Say* item because it, of the two items, refers more broadly to the political system. Citizens may feel that because elections have changed in such a way as to attenuate the power and influence of wealthy contributors, people like them will have more of a say. They would not necessarily feel that officials care more about them based on this knowledge because they have not seen officials do so yet; they could easily remain skeptical of politicians despite the clean elections reform. However, over time, as the public system "works its magic," citizens may actually see that their elected officials care about them, or at least seem to care more than their privately funded predecessors.

Chapter 4: Improving the Study of Political Efficacy

It may be the case that the election of 2000 produced some other radical, efficacy-enhancing change in Arizona citizens, but the synthetic-control method gives us evidence to argue that some event between 1998 and 2000 caused a substantial increase in people's perceptions of government. I argue that the evidence supports the proposition that the Citizens Clean Elections Act was the primary reason for this, though it appears to be because of the act's potential, or perhaps the very fact of its passage. Since the act itself did not raise citizens' efficacy to a higher plateau, the finding may be stronger evidence for the efficacy-enhancing potential of referenda or initiative victory, and efficacy's tendency to revert to a stable baseline.

Further research should try to disentangle the effects of success on the ballot and institutional reform. For example, in 2012, ANES performed a panel study in a handful of American states asking questions about direct democracy and specific ballot measures. It would be interesting if institution reforms of the magnitude of the CCEA produced similar increases in external efficacy. Perhaps there is a stronger relationship between efficacy and the salience of referenda (e.g. Gilens et al. 2001) than efficacy and the institutional import of referenda, or perhaps there is another consideration, such as cultural import (e.g., legalizing same-sex marriage), but more research needs to be done on the subtleties of direct democracy and external efficacy.

Still more needs to be done with respect to efficacy and campaign finance reform. Though it is not available to for public release, there exists time-series polling data at Cornell's Institute for Survey Research on residents of New York State and the City of New York that would enable researchers to test the effect of the City's as-yet uncopied money-matching campaign-finance system. Donors who give relatively small amounts (less than $200) have their donations multiplied by the city government, thereby amplifying the power of their small

contribution. Research has shown that this has increased the number of small donors as a percentage of the total donor pool (Malbin, Brusoe, and Glavin 2012). The synthetic-control method would again allow us to gauge the change in efficacy that may have arisen from New York City's decision to increase its matching multiplier from one to four in 2001, and four to nine in 2005 (Migally and Liss 2010).

The fit of the measure to the phenomenon is a field where improvement can be made in research on external political efficacy. As Lee (2010) demonstrated, researchers can create models estimating efficacy with reasonably high model fit so long as the questions asked pertain quite directly to the action that respondents should be considering. That is, asking about the influence one can have on the government is a reasonably good measure of overall efficacy, but this sort of efficacy may be too vague to be sensitive to the sorts of political change that political scientists are interested in studying. Though it is unreasonable to expect projects like the American National Election Study to field a stable of questions about highly specific modes of political behavior, such finer-tuned instruments are what is needed to learn the workings of people's perceptions of their ability to interact with government. In the 2000 National Annenberg Election Study, questions were asked about the trust respondents had in their local government, but the item was dropped from subsequent studies. And in 1996, the ANES asked which level of government (federal, state, or local) respondents had more "faith and confidence" in. Such specific government-level referents would make the interpretation of efficacy studies less opaque. Perhaps even questions about respondents' willingness to contact their officials with a pressing concern are a step in the right direction. What increases willingness to participate is already a topic with a firm empirical base (see e.g., Neblo, Esterling, and Lazer 2010).

Iyengar's (1980) analysis of ANES panel data from 1972-1976 found evidence to support the claim that the sense of political efficacy remains "more or less unchanged" (254)

after it is acquired early in life. Though the strength of the result is questionable, it challenges, or at least complicates the assertion that people's sense of external efficacy remains unchanged. It is surprising that a single statute would boost external efficacy to such an extent, though it is true that the euphoria wore off. We must ask why this is the case, and we must ask how important a sense of external efficacy is if it is not sensitive to sweeping reforms such as those in Arizona and elsewhere. That is, if efficacy does not budge in light of a more receptive political environment, why should policy-makers and reforms try to move it? The present analysis shows that reforms whose purpose it is to wrest power away from big-money interests and back into the hands of ordinary citizens do produce a marked change in how responsive citizens perceive their government to be. They do not meet such reforms with cynicism or disillusion, but can in fact greet them with optimism. That such optimism does not stay at its peak is what we would expect from everyday psychology, but its evanescence may also have something to do with the specifics of Arizona or any number of confounding factors, not the least of which is our measure's vagueness concerning which level of government is its referent.

Reforms of any kind are going to change as they run up against pre-existing laws, disaffected enemies, and other opponents, especially when it comes to the high-stakes regulation of campaign finance. They will also suffer from forgetfulness, though with further research we may find that the psychological impact of a reform persists despite its not being consciously reflected upon. That we see a boost in support for the political system despite no evidence to its improved operation other than the passage of a citizen initiative evidences the claim that people can respond to substantial, issue-based reform with political hope.

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