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IN THE UNITED STATES DISTRICT COURT
DISTRICT OF ARIZONA

JOHN MCCOMISH, *et al.*,)	Civil Action No. 08-1550-PHX-ROS
Plaintiffs,)	
and)	DECLARATION OF
)	DAVID M. PRIMO, Ph.D.
DEAN MARTIN, a citizen of the State)	IN SUPPORT OF PLAINTIFF-
Of Arizona, *et al.*,)	INTERVENORS' MOTION FOR
)	SUMMARY JUDGMENT
Plaintiff-Intervenors)	
)	
vs.)	
)	
KEN BENNETT, in his official capacity)	
as Secretary of State of the State of)	(Assigned to the Honorable Roslyn
Arizona; *et al.*,)	O. Silver)
)	
Defendants,)	
)	
and)	
)	
CLEAN ELECTIONS INSTITUTE, INC.)	
)	
Defendant-Intervenor.)	

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[1] Admitted *pro hac vice.*

I, David M. Primo, declare under penalty of perjury that the following is true:

1.   I am a citizen of the United States, a resident of the State of New York, and over the age of 18 years. I make this declaration in support of Plaintiff-Intervenors' Motion for Summary Judgment based on my personal knowledge of the facts stated herein.

2.   I have been asked by counsel for the Plaintiff-Intervenors to evaluate the effect of the matching funds provisions of the Arizona Clean Elections law on candidate behavior. I was paid $225 per hour for the work on my initial and rebuttal reports and $250 per hour to prepare for and participate in my deposition; in addition, I will be compensated $225 per hour for all additional work in this case, except that further court testimony or depositions, and related preparation, will be compensated at a rate of $250 per hour.

3.   In this declaration, I describe: 1) my qualifications as an expert in this case; 2) pertinent characteristics of Arizona's matching provisions; 3) the incentives created by the matching provisions; 4) the dataset I used to perform my statistical analysis; 5) the results of my statistical analysis and a related cost-benefit analysis; and 6) my conclusions. A summary of my analysis follows.

Summary

4.   I remain confident in the conclusion, reached in the reports I prepared for this case, that the matching provisions of Arizona's Clean Elections law distort political speech.

5.   Based on a statistical analysis of the matching provisions in Arizona's Clean Elections law, I conclude that these provisions impose significant costs on candidates and, by extension, the public.

6.   Specifically, the matching provisions lead to changes in fundraising and campaign spending in ways that are harmful to free expression.

7.   Based on a regression analysis, I am very confident that privately funded candidates in races where matching funds are awarded change the timing of their fundraising activities, the timing of their expenditures, and, by extension, their campaign strategies.

8.   To the best of my knowledge, I am the only expert in this case to perform original research on the effect of matching funds.

9.   In conducting my data analysis, I rely on data from 2000-2006 and compare patterns of fundraising and campaign spending between privately funded candidates in races where matching funds are triggered and privately funded candidates in races where a match is not triggered. Some of my key findings include the following:

10.   In primaries, privately funded candidates in House races where matching funds are awarded spent 385% more than other privately funded candidates in the final two weeks of the campaign. This is consistent with a strategy to spend money at the last minute to minimize the effectiveness of matching funds given to one's opponent as a result of such spending.

11.   In the period immediately following very competitive general election races, privately funded candidates in races where matching funds are triggered raised

over three times more than privately funded candidates in races where a match is not triggered. This set of results imply that matching funds provisions have a powerful effect precisely where they matter most: in very competitive races.

12. This behavior in general elections is consistent with a strategy of deferring income that could trigger a match until after the election (and, to the extent possible, funding last minute purchases on credit, which does not trigger a match in the general election) in order to reduce the amount of matching funds awarded to a publicly financed candidate.

13. These results, combined with the limited evidence in the literature that Clean Elections have improved elections or the policy process in Arizona, lead me to conclude that the harms to free expression and the free exercise of political activity associated with Clean Elections far outweigh any benefits the law confers on the state of Arizona or its residents. In other words, the costs of this law are significant relative to its benefits.

Qualifications

14. I am an associate professor of political science at the University of Rochester. I joined the faculty in 2002 after receiving my Ph.D. in Political Science from Stanford University. I also have an M.A. in Economics from Stanford, an M.A. in Political Science from Brown University, and a B.A. in Economics and Honors Political Science from Brown. At the University of Rochester, I teach courses on American politics, positive political theory, the interplay of business and government,

entrepreneurship, and campaign finance. My current vita is attached as Exhibit A and incorporated herein by this reference.

15.   My research on campaign finance, public finance, methodology, and political bargaining has been published in peer-reviewed journals including *American Journal of Political Science*; *Journal of Politics*; *Election Law Journal*; *Economics and Politics*; *Journal of Law, Economics, and Organization*; *Business and Politics*; *Independent Review*; *State Politics and Policy Quarterly*; and *Public Choice*. My work has also been published in several edited volumes, and I have published two books. My second book, *Rules and Restraint: Government Spending and the Design of Institutions* (University of Chicago Press, 2007), was awarded the Alan Rosenthal Prize by the Legislative Studies Section of the American Political Science Association. I have also received numerous grants to support my research, including from the National Science Foundation.

16.   My research on campaign finance laws focuses on the impact of these laws on competitiveness, turnout, and perceptions of government. This research establishes that the claims reformers make about the benefits of campaign finance law often to do not hold up in practice. For instance, Primo and Milyo (2006) establish that few campaign finance laws improve citizen perceptions of government, and in fact they may sometimes negatively impact these perceptions. *See* Primo, David M., and Jeffrey Milyo. 2006. "Campaign Finance Laws and Political Efficacy: Evidence From the States." *Election Law Journal* 5(1):23-39.

17.   Most of my research in the area of campaign finance law is quantitative in nature, utilizing data analytic techniques such as regression to probe relationships among campaign finance laws and outputs such as perceptions of government. This work has been cited widely in academic journals, policy reports, and the popular press.

18.   I am familiar with the provisions of the Clean Elections law and with the extant political science literature on public financing of campaigns.

19.   I have previously served as an expert witness in one case, *McConnell v. FEC.* In that case, which dealt with the federal Bipartisan Campaign Reform Act, I focused on the impact of campaign spending on citizen perceptions of government. I showed that at the very time that soft money receipts were increasing, so was citizen trust in government. This simple pattern in the data suggested that it was unlikely that soft money spending was harming citizen perceptions of government.

20.   In this case, I have authored two reports.

21.   In my initial expert report, I demonstrated statistically that the matching provisions of the Arizona's Clean Elections law distort political speech by changing the timing of fundraising and expenditures. In my opinion, the costs associated with these changed political activities far outweigh any benefits conferred by the law. My initial expert report is attached as Exhibit B and incorporated herein by this reference.

22.   In my rebuttal report, I defended my statistical methodologies as sound and appropriate and reaffirmed that I remain confident that the matching provisions distort political speech. I also conducted new statistical analyses demonstrating that the

matching funds provisions have a powerful negative impact in very competitive races. My rebuttal report is attached as Exhibit C and incorporated herein by this reference.

The Clean Elections Matching Provisions

23.   Pursuant to Arizona's Clean Elections law, a candidate who chooses to forgo private funds in return for receiving public monies is provided with a lump sum at the beginning of the primary and general election periods. A.R.S. § 16-951. I will refer to such candidates as "publicly funded" candidates.

24.   Publicly funded candidates are eligible to receive additional public monies through the Clean Elections law's matching provisions. A.R.S. § 16-952.

25.   Candidates who choose not to accept public monies will be referred to as "privately funded" candidates.

26.   The matching provisions operate as follows:

i)   In the primary election, if a privately funded candidate spends in excess of the initial disbursements provided to publicly funded candidates in the same race, then the state of Arizona provides all publicly funded candidates in that race with a dollar-for-dollar match, up to a set amount. A.R.S. § 16-952(A).

ii)   In the general election, determinations are based on money raised, as opposed to spent. A.R.S. § 16-952(B).

iii)   Privately funded candidates are required to submit "trigger reports" periodically throughout both the campaign. A.R.S. § 16-941(B)(2); A.R.S. § 16-958. These reports are used by the state to determine

whether any publicly financed candidates are eligible to receive matching funds.

Incentives Created By The Clean Elections Law

27.   Before turning to the data, it is useful to consider the incentives created by the Clean Elections law.

28.   Privately funded candidates who expect to trigger matching funds may want to time their expenditures or income in order to minimize the effectiveness of any matching funds that are awarded.

29.   For instance, in the primary, a candidate may want to wait until the last two or three days before the primary election to unleash a barrage of spending.

30.   Similarly, in the general election, a privately funded candidate may wait to raise money until the day after a trigger report is due, in order to have use of those funds for a full reporting period. And as the campaign draws to a close, a candidate may wait to raise money from donors until the very last minute, or if expenses can be incurred on credit, until after the general election is completed.

31.   In other words, the matching provisions should encourage a rational privately funded candidate to alter his or her fundraising and campaign expenditures to minimize the impact of matching funds in ways consistent with maximizing the likelihood of victory in the election.

32.   Of course, only privately funded candidates facing at least one other publicly funded candidate and who fear that a match will be triggered should alter their

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behavior. Throughout this declaration, I will refer to these candidates as Type 2 candidates.

33.   Privately funded candidates facing off against other privately financed candidates should not exhibit altered patterns of behavior.

34.   Privately funded candidates facing off against at least one publicly funded candidate in races unlikely to trigger matching funds also should not exhibit altered patterns of behavior.

35.   I will refer to privately funded candidates who should not exhibit altered patterns of behavior as Type 1 candidates.

36.   Type 2 and Type 1 candidates are defined entirely based on the above criteria. For instance, some Type 1 candidates outspend some Type 2 candidates in a given period, but in other cases Type 2 candidates outspend Type 1 candidates. So, candidate spending is not included as a criterion for defining Type 1 and Type 2 candidates.

37.   This theoretical perspective provides the foundation for empirically examining whether Type 2 candidates behave differently than Type 1 candidates.

Data

38.   I utilized a dataset of all candidates for the Arizona House and Senate from 2000 to 2006.[2] The units of observation are candidates. For each candidate, several

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[2] 2008 was omitted from the dataset due to data limitations.

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pieces of data were collected, including income, expenses, and experience. Some data for years prior to 2000 was used in order to construct variables for candidate experience.

39. The dataset was constructed by Dr. Dick Carpenter of the Institute for Justice using data obtained from public sources, specifically the Arizona Secretary of State's Office, the Citizens Clean Elections Commission (CCEC), and state archives. The data obtained from the Secretary of State includes official election results, voter registration totals, and campaign finance data. The data from the CCEC includes campaign finance data. A list of legislative officeholders was obtained from state archives. Dr. Carpenter's approach is typical of the way in which political scientists go about gathering data for similar types of analysis.

40. I received the data in the form of an Excel file and personally placed the file into STATA format so that I could perform my statistical analysis. During the process of placing the data into STATA format, I spot-checked the data for accuracy. I discovered a few errors in the vote totals and worked with Dr. Carpenter to correct those totals prior to working with the data.

41. I am confident that the dataset constructed by Dr. Carpenter is largely accurate. If I had concerns about the overall accuracy of the dataset, I would not have worked with it. In a large dataset such as this one, with well over 1,000 observations, there are bound to be some errors.

42. In addition to correcting errors in the dataset that I personally found, I also corrected the errors pointed out by Dr. Kenneth Mayer in his rebuttal to my initial expert

report. In this declaration, I will refer to statistical analyses performed with the corrected data any time the corrections altered my original conclusions.

43. I followed Dr. Kenneth Mayer's coding scheme when constructing a variable for candidate type (e.g., incumbent).

44. I subdivided the data by chamber and election type and presented results for House primaries, Senate primaries, House general elections, and Senate general elections.

45. To focus on the effect of the matching provisions on candidate behavior, I restricted the sample as follows:

    i) I eliminated independents and third-party candidates from my analysis.

    ii) I eliminated candidates involved in races for which the number of candidates is equal to or less than the number of winners. This restriction is made by election (i.e., primary or general), so it is possible for a candidate to be excluded from the primary election analysis yet appear in the general election analysis, or vice versa.

    iii) I eliminated publicly funded candidates. They do not trigger matching funds or raise money, and they are constrained in their expenditure decisions.

    iv) I eliminated candidates who spent less than $1,000 or raised less than $1,000 in the primary and general election combined (all financial figures are in 2006 dollars).

v)   I eliminated candidates whose records indicated negative income or a credit in the expenses category in any of the subperiods (typically two weeks in duration) in the data.

46.   As restricted, that left me with 117 observations for House primary elections, 46 observations for Senate primary elections, 109 for House general elections, and 59 observations for Senate general elections.

Statistical Analysis

My Methodologies Are Generally Accepted in the Political Science Community

47.   My statistical techniques are appropriate and sound. In order to examine whether Type 2 and Type 1 candidates behave in systematically different ways, I performed three types of statistical analysis: difference of means tests; OLS regression; and tobit. These methods, described in further detail below, are generally accepted research methodologies in political science and appear in practically every overview textbook in econometrics. OLS regressions routinely appear in leading peer-reviewed journals in political science, and tobits and difference of means test also appear in these journals, though with less frequency, because OLS remains the "workhorse" technique for analyzing continuous dependent variables.

48.   In each of my three types of statistical analysis, I used both the amounts of spending and income, in 2006 dollars, as well as the logarithms of these amounts. I converted dollars to logarithms because there were large differences between the smallest and largest values in the dataset. The use of logarithms to eliminate the influence of large values is very common in the campaign finance literature. For

example, Ansolabehere, de Figueiredo, and Snyder (2003, 124) state that they convert campaign spending data into logs to "correct for the skew in . . . total [campaign] spending." They perform this conversion when working with both state-level and federal-level data.

49.   Statistical significance refers to how confident the researcher is that any differences found in the data are not due to chance. In probing the differences between Type 2 and Type 1 candidates, I use a .10 level of statistical significance in one-tailed tests. In many cases, my results are statistically significant at a stricter threshold, such as .05. The .10 level of statistical significance is commonly used in peer-reviewed publications. For instance, I used one-tailed tests with a .10 significance level in my peer-reviewed article "Who Consents? Competing Pivots in Federal Judicial Selection." *See* Primo, David M., Sarah A. Binder, and Forrest Maltzman. 2008. "Who Consents? Competing Pivots in Federal Judicial Selection." *American Journal of Political Science* 52(3):471-489. This article appeared in *American Journal of Political Science*, a leading peer-reviewed political science journal.

50.   In the same issue of that journal in which my article appeared, at least sixty percent of the remaining empirical articles utilized a .10 standard (with two-tailed tests where appropriate). One of these articles that utilized the .10 standard was given the award for best paper in the *American Journal of Political Science* in 2008.

Difference of Means

51.   A difference of means test examines whether two groups differ on some variable in ways that are not due to chance. A difference of means test is a useful "first cut" at the data.

52.   In my reports, I utilized difference of means tests to compare the spending and income of Type 2 and Type 1 candidates.

OLS Regression

53.   The next step in the analysis is to ask whether other factors may be driving the differences between Type 2 and Type 1 candidates. An OLS regression enables the researcher to isolate the effect of a single independent variable on the variable under study after the effects of other variables have been accounted for. Details on OLS can be found in Appendix A of my Expert Report.

54.   In my reports, I utilized regression analysis to account for three important aspects of campaigns: the experience of the candidate, the campaign environment in the election year, and the party of the candidate. In analyses performed for the rebuttal report, several other variables were also used, including the competitiveness of the race.

55.   Adding these variables to the analysis allowed me to isolate the effect of matching funds on Type 2 candidates.

Tobit

56.   Finally, I used another technique, known as tobit, to account for the fact that campaign spending and income data is *censored*. Specifically, candidates sometimes raise no money or spend no money in particular periods. This censoring violates the assumption in OLS regression that the error term (ϵ) and independent

variables (the x's) are uncorrelated. Tobit regression accounts for these problems. The results from the tobit analysis were largely consistent with the OLS findings. Details on tobit can be found in Appendix A of my Expert Report.

Findings and Conclusions

57.   Unless otherwise specified, the results in what follows are based on OLS regressions using the log of spending or income as the dependent variable.

58.   In primaries, privately funded candidates in House races where matching funds are awarded (Type 2 candidates) spent 385% more than other privately funded candidates (Type 1 candidates) in the final two weeks of the campaign. This is consistent with a strategy to spend money at the last minute to minimize the effectiveness of matching funds given to one's opponent as a result of such spending.

59.   In general elections, Type 2 candidates in the House raised more than twice as much money as their Type 1 counterparts in the period following the general election. This is consistent with a strategy of deferring income that could trigger a match until after the election (and, to the extent possible, funding last minute purchases on credit, which does not trigger a match in the general election) in order to reduce the amount of matching funds awarded to a publicly financed candidate. These results were weakened when several other variables were added to the OLS regressions. However, F-tests taking into account analyses performed for all periods implied that a more parsimonious specification (i.e., one with fewer control variables) was statistically superior in most cases.

60.   Moreover, further analyses showed that in very competitive general election races, Type 2 candidates raised over three times more than Type 1 candidates in the period immediately following the general election. These additional analyses imply that matching funds provisions have a powerful effect precisely where they matter most: in very competitive races.

61.   These findings imply that the matching funds provision of the Clean Elections law is leading candidates to alter campaign behavior in ways that harm free expression. Specifically, the law provides an incentive for candidates to time campaign expenditures and income so as to minimize the impact of matching funds in ways consistent with maximizing the likelihood of victory in the election.

62.   My study shows that there are clear harms (i.e., costs) that result from the Clean Elections law. An examination of extant research shows that these harms are not offset by significant benefits.

63.   Public financing, including Clean Elections, is argued by reformers to address at least three perceived problems in the democratic process: limited electoral competitiveness, low levels of trust in government, and excessive "special interest" influence. There is limited support for these claims in the political science literature.

64.   Regarding competition, incumbent reelection rates (one measure of competitiveness) have changed little since public financing was implemented in 2000.

65.   Regarding trust in government and excessive "special interest" influence, I am aware of no scientific evidence that perceptions of government have improved or any "special interest" influence has been reduced due to Clean Elections.

66.   In short, there is minimal scientific evidence that public financing laws have positive effects on participatory democracy, but in my reports I demonstrated that the law imposes real costs on candidates.

Concluding Remarks

67.   I remain confident in the conclusion, reached in the reports I prepared for this case, that the matching provisions of Arizona's Clean Elections law distort political speech.
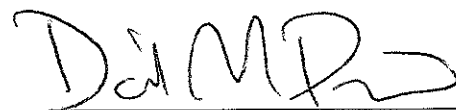
68.   In my opinion, Type 2 candidates are altering their behavior as a result of the Clean Elections law in ways that infringe on their free speech rights. The effects are real and they are substantively meaningful.

69.   Based on my statistical analysis of the data and the extant research on Clean Elections, I conclude that the matching provisions of Arizona's Clean Elections law have deleterious effects on free expression.

70.   Based on my statistical analysis of the data and the extant research on Clean Elections, I conclude that there are few empirically demonstrated benefits to Clean Elections and significant costs to free expression.

I certify and declare under penalty of perjury under the laws of the United States of America that the foregoing is true and correct to the best of my knowledge and belief.

May 26, 2009

David M. Primo, Ph.D.

EXPERT REPORT
McComish v. Bennett

David M. Primo, Ph.D.

Executive Summary

Public financing, including Clean Elections, is argued by reformers to improve the democratic process, but the existing political science literature finds limited evidence in this regard. In this report, I focus on the costs of such reforms. Specifically, I study the matching provisions in Arizona's Clean Elections law and establish statistically that these provisions impose significant costs on candidates and, by extension, the public. Specifically, these laws lead to changes in fundraising and campaign spending which distort political speech. Based on a regression analysis, I am very confident that this law changes the timing of contributions (i.e., candidate income), the timing of expenditures, and the strategies of candidates. These results, combined with the limited evidence that Clean Elections have improved elections or the policy process in Arizona, lead me to conclude that the harms to free expression and the free exercise of political activity associated with Clean Elections far outweigh any benefits the law confers on the state of Arizona.

In conducting my data analysis, I rely on data from 2000-2006 and compare patterns of fundraising and campaign spending between privately funded candidates in races where matching funds are triggered and such candidates in races where a match is not triggered. Some key findings from my analysis include the following:

- In primaries, privately funded candidates in House races where matching funds are awarded (Type 2 candidates) spent 158% more than other privately funded candidates (Type 1 candidates) in the final two weeks of the campaign. This is consistent with a strategy to spend money at the last minute to minimize the effectiveness of matching funds given to one's opponent as a result of such spending.

- In the general election, Type 2 candidates in the House raised more than double their Type 1 counterparts in the period following the general election. Type 2 House candidates also raised more than Type 1 candidates in the two-week period leading up to the general election. This is consistent with a strategy of deferring income that could trigger a match until the end of the campaign or afterwards to minimize the effectiveness of any matching funds awarded as a result of such fundraising.

EXPERT REPORT
McComish v. Bennett

David M. Primo, Ph.D.

Background

I am an associate professor of political science at the University of Rochester. I joined the faculty in 2002 after receiving my Ph.D. in Political Science from Stanford University. I also have an M.A. in Economics from Stanford, an M.A. in Political Science from Brown University, and a B.A. in Economics and Honors Political Science from Brown.

At the University of Rochester I teach courses on American politics, positive political theory, the interplay of business and government, entrepreneurship, and campaign finance. I have won two College-wide teaching awards at the university: the 2005 Goergen Award for Distinguished Achievement and Artistry in Undergraduate Education, given by the College, as well as a 2005 Undergraduate Professor of the Year Award, given by the University of Rochester Students' Association.

My research on campaign finance, public finance, methodology, and political bargaining has been published in peer-reviewed journals including *American Journal of Political Science*; *Journal of Politics*; *Election Law Journal*; *Economics and Politics*; *Journal of Law, Economics, and Organization*; *Business and Politics*; *Independent Review*; *State Politics and Policy Quarterly*; and *Public Choice*. My work has also been published in several edited volumes. In addition, I have published two peer-reviewed books: *The Plane Truth: Airline Crashes, the Media, and Transportation Policy* (co-authored with Roger W. Cobb, Brookings Institution Press, 2003) and *Rules and Restraint: Government Spending and the Design of Institutions* (University of Chicago Press, 2007). *Rules and Restraint* was awarded the Alan Rosenthal Prize by the Legislative Studies Section of the American Political Science Association. This award is given for the best publication in legislative studies by a young scholar during 2007 of importance to legislators and legislative staff and of merit in strengthening the practice of representative democracy. I have received numerous grants to support my research, including from the National Science Foundation.

My research on campaign finance laws focuses on the impact of these laws on competitiveness, turnout, and perceptions of government, and it has been cited widely in academic journals, policy reports, and the popular press. This research establishes that the claims reformers make about the benefits of campaign finance law often do not hold up in practice. For instance, Primo and Milyo (2006) establish that few campaign finance laws improve citizen perceptions of government, and in fact they may sometimes negatively affect these perceptions.[1] Most of my research in this area is quantitative in nature, utilizing data analytic techniques such as regression

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[1] Primo, David M., and Jeffrey Milyo. 2006. "Campaign Finance Laws and Political Efficacy: Evidence From the States." *Election Law Journal* 5(1):23-39.

to probe relationships among campaign finance laws and outputs such as perceptions of government.

I have previously served as an expert witness in one case, *McConnell v. FEC.* In that case, which dealt with the Bipartisan Campaign Reform Act, I focused on the impact of campaign spending on citizen perceptions of government. I showed that at the very time that soft money receipts were increasing, so was citizen trust in government. This simple pattern in the data suggested that it was unlikely that soft money spending was harming citizen perceptions of government.

My curriculum vitae is attached as Appendix A and is incorporated into this expert report by reference.

I have been asked by the Plaintiff-Intervenors to evaluate the effect of the matching provisions of the Arizona Clean Elections law on candidate behavior. I am being paid $225 per hour for my work on this expert report. I am familiar with the provisions of the Clean Elections law and with the extant political science literature on public financing of campaigns. I approach the study of campaign finance by thinking of it in terms of benefits and costs. In other words, do the benefits of such reforms exceed the costs of implementation, any restrictions on free speech, and the potential distortions to the campaign process? This argument is articulated more fully in Primo and Milyo (2007), which is incorporated into this report by reference as Appendix B.[2] There is minimal scientific evidence that public financing laws have positive effects on participatory democracy, and any benefits of such reforms must be carefully balanced against any costs.

In this report, I focus on the matching provisions in Arizona's Clean Elections law and establish statistically that these provisions impose significant costs on candidates and, by extension, the public. Specifically, these laws lead to changes in fundraising and campaign spending which distort political speech. Based on a regression analysis, I am very confident that this law changes the timing of contributions (i.e., candidate income), the timing of expenditures, and the strategies of candidates. Privately funded candidates in races with publicly financed opponents receiving matching funds are forced to make a choice between getting their message out and potentially providing additional monies to their opponents. They tend to find a middle ground which involves the postponing of spending or income in order to minimize the effectiveness of any matching funds. This burden falls primarily on privately funded candidates in races with a publicly financed opponent who receives matching funds. This result leads me to conclude that the Clean Elections law harms free expression and the free exercise of political activity. I conclude, therefore, that the harms associated with Clean Elections far outweigh any benefits the law confers on Arizona.

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[2] Primo, David M., and Jeffrey Milyo. 2007. "Public Financing of Campaigns: A Statistical Analysis." *Engage* 8:96-100.

Clean Elections Matching Provisions

Under Arizona's Clean Elections law, a candidate who chooses to forgo private funds in return for receiving public monies is provided with a lump sum at the beginning of the primary and general election periods. I will refer to such candidates as "publicly funded" candidates. Candidates who choose not to participate in the Clean Elections system will be termed "privately funded" candidates. Publicly funded candidates are also eligible to receive additional funds through a matching program that operates as follows. In the primary election, if a privately funded candidate spends in excess of the initial disbursements provided to publicly funded candidates in the same race, then the state of Arizona provides all publicly funded candidates in that race with a dollar-for-dollar match, up to a prespecified amount.[3] In the general election, determinations are based on money raised, as opposed to spent. Privately funded candidates are required to submit "trigger reports" periodically throughout the campaign. These reports are used to determine whether any publicly financed candidates are eligible for matching funds.

Theory

Before turning to the data, it is useful to consider the incentives that the Clean Elections law creates. First, privately funded candidates who expect to trigger matching funds may want to time their expenditures or income in order to minimize the effectiveness of any matching funds that are awarded. For instance, in the primary, a candidate may want to wait until the last two or three days before the primary election to unleash a barrage of spending. Similarly, in the general election, a privately funded candidate may wait to raise money until the day after a trigger report is due, in order to have use of those funds for a full reporting period. And, as the campaign draws to a close, a candidate may wait to raise money from donors until the very last minute, or if expenses can be incurred on credit, until after the general election is completed. In other words, the matching provisions should encourage a rational candidate to alter his fundraising and campaign expenditures to minimize the impact of matching funds.

Of course, only certain privately funded candidates should alter their behavior in the above way. Specifically, only privately financed candidates facing at least one other publicly financed candidate *and* fearing that a match will be triggered should alter their behavior. Call these Type 2 candidates. Privately funded candidates facing off against other privately funded candidates, and privately funded candidates facing off against publicly funded candidates in races unlikely to trigger matching funds, should not exhibit altered patterns of behavior. Call these Type 1 candidates. This simple theoretical perspective provides an opportunity to empirically examine whether Type 2 candidates behave differently in campaigns than Type 1 candidates. I turn now to this task.

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[3] A similar matching provision exists for expenditures incurred by outside groups, but I do not focus on these provisions in what follows. Also, the law was modified in 2008. This analysis focuses on 2000-2006, so the 2008 changes are not addressed here.

Data

I utilize a dataset of all candidates for the Arizona House and Senate from 2000 (when Clean Elections began) to 2006.[4] The units of observation are candidates. For each candidate, several pieces of data have been collected, including income, expenses, and experience. This dataset was constructed by Dick Carpenter of the Institute for Justice using data obtained from public sources, specifically the Arizona Secretary of State's Office, the CCEC website, and state archives. The data obtained from the Secretary of State includes official election results, voter registration totals, and campaign finance data. The data obtained from the CCEC includes campaign finance data. Also, a list of legislative officeholders was obtained from state archives. In addition, Kenneth Mayer's coding scheme was followed when constructing a variable for candidate type (e.g., incumbent). Some data for years prior to 2000 was used in order to construct variables for candidate experience.

In what follows, I subdivide all analyses by chamber and election type and present results for House primaries, Senate primaries, House general elections, and Senate general elections.

To focus attention on the question at hand, I restrict the sample in the following ways:

- I eliminate independents and third-party candidates from the analysis.

- I eliminate candidates involved in races for which the number of candidates is equal to or less than the number of winners. This restriction is made by election (i.e., primary or general), so it is possible for a candidate to be excluded from the primary election analysis yet appear in the general election analysis, or vice versa.

- I eliminate publicly financed candidates. They do not trigger matching funds or raise money, and they are constrained in their expenditure decisions.

- I eliminate candidates who spend less than $1,000 or raise less than $1,000 in the primary and general election combined. (All financial figures are in 2006 dollars.)

- I eliminate candidates whose records indicate negative income or a credit in the expenses category in any of the subperiods (typically two weeks in duration) in the data.

These restrictions leave me with 117 observations for House primary elections, 46 observations for Senate primary elections, 110 observations for House general elections, and 61 observations for Senate general elections.

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[4] 2008 is omitted due to data limitations.

Empirical Analysis

To determine whether matching provisions cause Type 2 candidates to behave differently than Type 1 candidates, I first need to place each candidate into one of these categories. It is clear that privately funded candidates in races without any publicly funded candidates will not be concerned with matching provisions, and therefore will be Type 1 candidates. It is less clear how privately funded candidates in races with at least one publicly financed candidate will behave, so I adopt a conservative approach for identifying Type 2 candidates. Specifically, I define Type 2 candidates as those privately funded candidates who participate in races in which matching was actually triggered. If we assume that candidates are rational and anticipate that this is the kind of race they are in, then their behavior is the most likely to be affected by the matching provisions. Privately funded candidates in races in which a match was not triggered are categorized as Type 1 candidates. There certainly may be Type 1 candidates who also alter their behavior because they are concerned about triggering a match and, as a result, perhaps avoid triggering a match. However, I am aware of no way to discern which Type 1 candidates adopt such a strategy. My approach is conservative statistically because it makes finding an effect of matching funds *less* likely.[5]

To summarize, candidates in the sample are categorized as follows:

- Type 1 candidates are privately funded candidates who either face only privately financed opponents or face publicly financed opponents, none of whom receive a match in the election.

- Type 2 candidates are privately funded candidates who face at least one publicly financed opponent who receives matching funds as a result of a privately funded candidate's spending or fundraising behavior.

In House general elections, 47% of the candidates in the sample are Type 2 candidates. In House primaries, 42% of the candidates in the sample are Type 2 candidates. In Senate general elections, 41% of candidates fall into the Type 2 category; the corresponding figure for Senate primaries is 24%.

How should the behavior of Type 1 and Type 2 candidates differ if matching funds cause a change in candidate strategies? In the primary, Type 2 candidates should try to postpone expenditures until as late in the campaign as possible in order to minimize the effectiveness of matching funds. In the general election, Type 2 candidates should try to postpone some income until as late in the campaign as possible in order to minimize the effectiveness of matching funds, and in fact should raise as much money as possible *after* the general election and pay for last-minute expenses on credit.

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[5] The reason is that to the extent that candidates who behave like Type 2 candidates are categorized as Type 1 candidates, the smaller the differences between these types of candidates we should observe.

Therefore, I expect to find two patterns in the data caused by matching:

- In primary elections, Type 2 candidates should spend more money than Type 1 candidates as the election draws closer. Earlier in the campaign any differences should be smaller.

- In the general election, Type 2 candidates should raise more money than Type 1 candidates in the periods immediately preceding and immediately following Election Day.

For the primary election analysis, the final six weeks of the campaign are broken down into three two-week periods. I will refer to the last two weeks of the campaign as PR3, the previous two-week period as PR2, and the earlier two-week period as PR1. The eight-week general election is broken down into four two-week periods, denoted GE1, GE2, GE3, and GE4, in chronological order, and a fifth period, GE5, reflecting income received after Election Day in November.

To begin the analysis, I will compare average levels of expenditures and income for Type 1 and Type 2 candidates and assess whether the differences are statistically different, or whether they are most likely due to chance.[6] Table 1H presents the averages by period for Type 1 and Type 2 candidates in the House, and Table 1S presents the same information for the Senate.

One possible problem with comparing spending and income averages is that large expenditures or donations can drive the results. To mitigate this problem, it is common in the literature to compress the variation in spending and income by converting the data to logarithms. The averages of the logged values of income and spending are presented in Tables 2H and 2S.

The third columns of Tables 1H, 1S, 2H, and 2S represent the percentage differences in dollars ($) between Type 2 and Type 1 candidates. Stars next to the period label indicate that the results are statistically significant. Several patterns are present in the data:

- In House primaries, the biggest difference between Type 1 and Type 2 candidates occurs in PR3, the two-week period immediately preceding the primary.
- In House general elections, Type 2 candidates outraised Type 1 candidates in the period immediately following the election. This result is especially pronounced when using the logged values of income. There is also a spike in income for Type 1 candidates immediately before Election Day.
- The results in the Senate are mixed, but when using logged values of spending, there is a significant spike in expenditures in the period immediately preceding the primary.

To summarize, then, a simple comparison of averages shows that the matching provisions of Clean Elections are likely affecting candidate behavior in campaigns. To probe this possibility further, the next step is to ask whether other factors may be driving these differences.

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[6] This is equivalent to running an OLS regression with an indicator variable for a Type 2 candidate as the independent variable. I present a difference of means test initially to make the data presentation simpler.

Table 1H: Arizona House, Differences Between Type 2 and Type 1 Candidates, 2000-2006

Campaign Period (Primary)	Type 1 Candidates (N=68)	Type 2 Candidates (N=49)	% Difference
PR1	7835.81	4581.60	- 42%
PR2*	1783.56	5620.50	215%
PR3*	3835.26	9854.51	157%

Campaign Period (General)	Type 1 Candidates (N=58)	Type 2 Candidates (N=52)	% Difference
GE1*	719.16	990.56	38%
GE2*	1012.25	1769.82	75%
GE3*	1251.01	3002.46	140%
GE4*	1349.57	2807.78	108%
GE5*	923.98	1933.53	109%

Results are in 2006 dollars. * indicates that the difference between candidates is statistically significant at the .10 level in one-tailed t-tests.

Table 1S: Arizona Senate, Differences Between Type 2 and Type 1 Candidates, 2000-2006

Campaign Period (Primary)	Type 1 Candidates (N=35)	Type 2 Candidates (N=11)	% Difference
PR1	6718.40	9344.40	128%
PR2	5654.15	7057.29	39%
PR3	7298.60	11,173.77	25%

Campaign Period (General)	Type 1 Candidates (N=36)	Type 2 Candidates (N=25)	% Difference
GE1	2449.54	1842.86	-25%
GE2*	1489.79	3197.54	115%
GE3	2538.41	2498.70	-2%
GE4	3417.74	2858.25	-16%
GE5*	2680.92	6095.81	127%

Results are in 2006 dollars. * indicates that the difference between candidates is statistically significant at the .10 level in one-tailed t-tests.

Table 2H: Arizona House, Differences Between Type 2 and Type 1 Candidates, 2000-2006

Campaign Period (Primary)	Type 1 Candidates (N=68)	Type 2 Candidates (N=49)	% Difference ($)
PR1*	5.71	7.20	150%
PR2*	5.59	7.08	148%
PR3*	6.44	8.04	160%

Campaign Period (General)	Type 1 Candidates (N=58)	Type 2 Candidates (N=52)	% Difference ($)
GE1*	4.19	5.37	118%
GE2*	4.40	5.42	102%
GE3	5.22	5.52	30%
GE4*	5.16	6.22	106%
GE5*	4.03	5.58	155%

Results are in the natural log of 2006 dollars. * indicates that the difference between candidates is statistically significant at the .10 level in one-tailed t-tests.

Table 2S: Arizona Senate, Differences Between Type 2 and Type 1 Candidates, 2000-2006

Campaign Period (Primary)	Type 1 Candidates (N=35)	Type 2 Candidates (N=11)	% Difference ($)
PR1*	7.38	8.84	146%
PR2	7.41	7.84	43%
PR3*	7.36	8.95	159%

Campaign Period (General)	Type 1 Candidates (N=36)	Type 2 Candidates (N=25)	% Difference ($)
GE1	5.83	5.23	60%
GE2	5.25	6.01	76%
GE3	6.15	5.50	-65%
GE4	5.94	5.53	-42%
GE5	5.26	5.76	51%

Results are in the natural log of 2006 dollars. * indicates that the difference between candidates is statistically significant at the .10 level in one-tailed t-tests.

To do this, I conduct regression analyses that account for three important aspects of campaigns: the experience of the candidate, the campaign environment in the election year, and the party of the candidate.[7] By adding these variables to a regression analysis, I am better able to isolate the effect of being a Type 2 candidate, holding constant the year of election, the experience of the candidate, and his or her party. I focus my attention on regressions using the log of spending and income variables, because this transformation of the data prevents outliers from driving the analysis.[8] The methodology and more detailed statistical results are presented in Appendix C, incorporated into this expert report by reference.

Table 3 presents the results for House and Senate elections. For each of the eight periods (3 for the primary, 5 for the general), I show how much *more* or *less* Type 2 candidates spent compared with Type 1 candidates. For instance, a value of 125% indicates that Type 2 candidates spend 125% *more* than Type 1 candidates in a given campaign period. Differences marked with a * in the tables are statistically significant, meaning that we can be confident that the results are not due to chance. For illustrative purposes, the results for the House are also presented as Figures 1 and 2.

Table 3: Differences in Income and Spending Between Type 2 and Type 1 Candidates, 2000-2006

	PR1	PR2	PR3	GE1	GE2	GE3	GE4	GE5
House	140%*	141%*	158%*	108%*	134%*	25%	77%*	104%*
Senate	59%	-48%	127%	-3.2%	134%*	-57%	13%	79%

Figures reflect the difference in expenditures (periods PR1-PR3) and income (periods GE1-GE5) for Type 2 candidates and Type 1 candidates. Positive numbers indicate Type 2 candidates exceeded the spending or income of Type 1 candidates. Stars indicate that the differences are statistically different at the .10 level in one-tailed tests.

To summarize the results in words, Type 2 candidates in House primaries spend increasingly more than their Type 1 counterparts as the primary election draws closer. In the two weeks prior to the primary, they outspend Type 1 candidates by 158%. None of the effects in the Senate reaches statistical significance in the primaries, though the effect in period PR3 is still the largest of the 3 primary periods (127%) and is very close to being statistically significant.

Type 2 candidates in House general elections raise 104% more than Type 1 candidates *after* Election Day. This is very strong evidence that candidates shift fundraising to after the campaign if they are in races where a match has occurred. Before Election Day, Type 2 candidates in the House appear to spend more than Type 1 candidates *early* in the campaign and *late* in the campaign. This finding suggests that Type 2 candidates feel more of a need than Type 1 candidates to raise money early in the campaign and then hold off on further fundraising until the final two weeks before (or the period after) the general election. Results for Senate general elections are more mixed.

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[7] Candidate experience is measured with a variable taking on the value 1 if the candidate held prior office, and 0 otherwise. To account for the campaign environment in a given election year, a separate variable for each election year is created and added to the regression. In addition, I construct an indicator variable for party taking on a value of 1 if the candidate is a Republican and 0 of the candidate is a Democrat.

[8] As a robustness check, I use a technique, called tobit, that also adjusts for the fact that several values of spending and income take on values of zero in the data; the results of the tobit analyses are comparable to the OLS regressions, so I report the OLS findings here.

Figure 1: Type 2 Candidates Outspend Type 1 Candidates by the Widest Margin Two Weeks Leading Up to House Primaries, 2000-2006
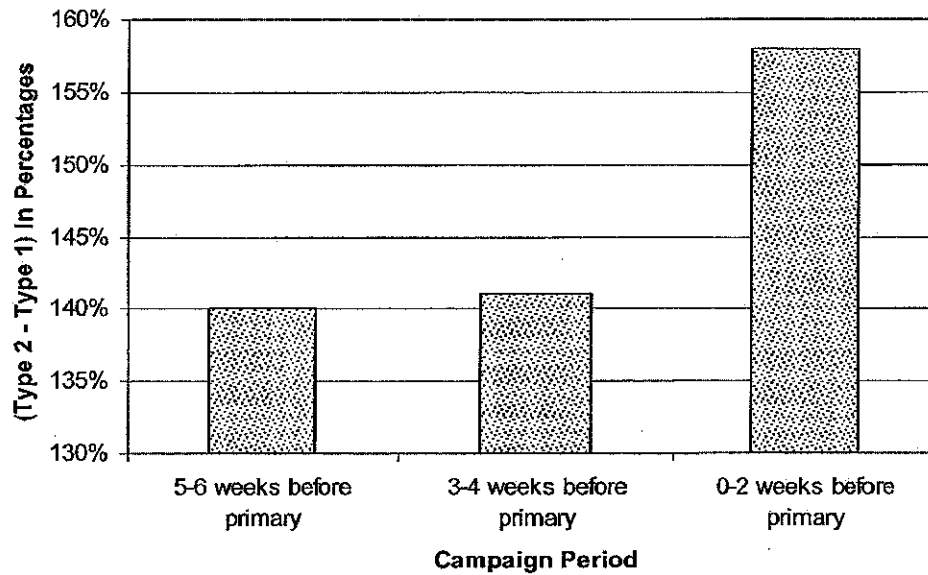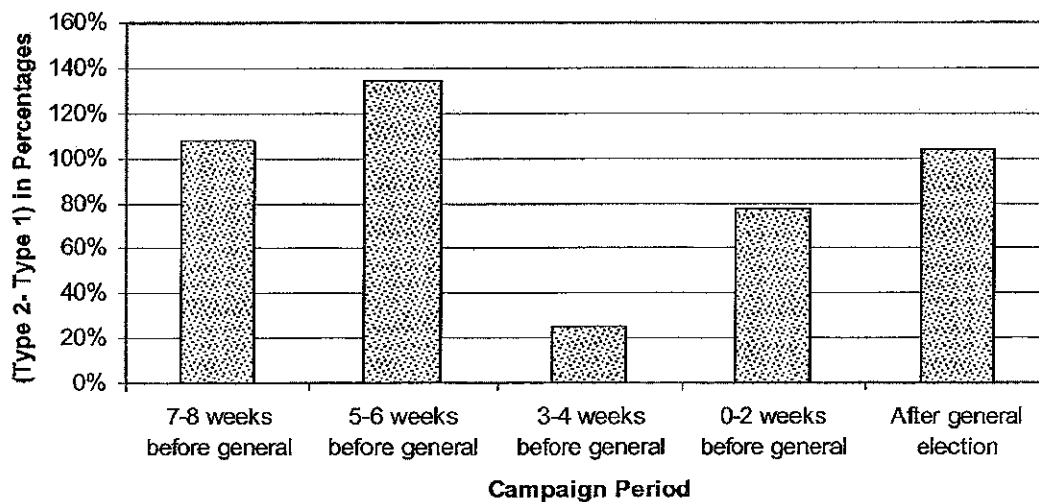

Figure 2: Type 2 Candidates Outraise Type 1 Candidates Right Before and After House General Elections, 2000-2006

The upshot of this analysis is that privately funded candidates in the House who are running in races where matching funds are awarded exhibit behavior that is markedly different from other privately funded candidates. There is limited evidence that Senate candidates also behave differently in primary elections.

Discussion and Conclusion

The findings in this report regarding changes in candidate behavior due to matching funds are noteworthy for several reasons. First, given how few observations exist in the data, it is remarkable that any patterns were detected. Second, my analyses are very conservative. I included as Type 2 candidates only those candidates who *almost surely* had to take into account the matching provisions in their fundraising and spending decisions. If I had included other candidates (say, those who were in races in which a match was almost triggered), the results would very likely be even stronger, since those candidates likely exhibit the same pattern of behavior as candidates already included in the Type 2 category.

In addition, these patterns are consistent with a recent study showing that a large proportion of matching funds allocations—in some elections as much as 65%—were provided in the final week of a campaign.[9] Certainly candidates will often wait until the tail end of a campaign to make a last-minute push for votes, but the figures provided in this article go far beyond a final push. Also, the author supplements his analysis with quotations from candidates, including this one: "You want to have a strategy and a plan of using those funds so that the matching funds are least beneficial to your opponent. In most cases you would think that that's at the last second, so they can't counter it. I think there's benefit to that. I think that's wise."[10] The pattern of results presented in this report is consistent with the sentiments expressed by the candidate's quotation.

In summary, I have shown the following in this report:
- In primaries, privately funded candidates in House races where matching funds are awarded (Type 2 candidates) spent 158% more than other privately funded candidates (Type 1 candidates) in the final two weeks of the campaign. In Senate primaries, Type 2 candidates spent 127% more than Type 1 candidates in the final two weeks of the campaign, though this result narrowly missed achieving statistical significance. In both cases, these differentials were the largest of the time periods studied.
- In the general election, Type 2 candidates in the House raised more than double their Type 1 counterparts in the period following the general election. Type 2 House candidates also raised more than Type 1 candidates in the two-week period leading up to the general election.
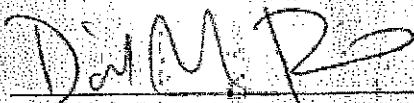
Powerful implications emerge from these findings. First, privately funded candidates who spend or raise funds above a government-set cap, and other privately funded candidates who are involved in an election in which a match has been triggered (collectively, Type 2 candidates), face a burden that other candidates do not face in having to alter their campaign strategies to deal
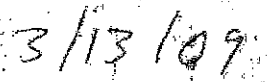
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[9] Miller, Michael. 2008. "Gaming Arizona: Public Money and Shifting Candidate Strategies." *PS: Political Science and Politics* 41:527-532. This study is incorporated into the expert report by reference as Appendix D.
[10] Ibid, p. 529.

with the threat of matching funds. The result of this burden is a distortion in speech, with such candidates skewing their spending and income, relative to other candidates. If candidates have an optimal campaign strategy in the absence of matching funds, then Type 2 candidates are at a disadvantage because they must deviate from their optimal strategy to address these matching funds. This cost is problematic in and of itself, but it is compounded when combined with the weak evidence that the law is having any positive effects.

In closing, my statistical analysis provides strong evidence that candidates are changing their behavior as a result of Clean Elections. Specifically, the matching provisions of Arizona's Clean Elections law distort political speech by causing candidates to alter their campaign strategies. They change the timing of their speech, change their fundraising behavior, and alter their campaign strategies in order to minimize the impact of matching funds. This law, in other words, imposes significant costs on candidates who have to deal with matching funds in their races, and many of these costs are related to when they choose to speak. This law, then, harms free expression and the free exercise of political activity.

_____   _____
David M. Primo   3/13/09
                                                      Date

REBUTTAL REPORT
McComish v. Bennett

David M. Primo, Ph.D.

Executive Summary

In my initial expert report for *McComish v. Bennett*, I focused on the matching provisions in Arizona's Clean Elections law and established statistically that these provisions impose significant costs on candidates and, by extension, the public. Specifically, these rules lead to changes in fundraising and campaign spending which distort political speech. In this rebuttal report, I respond to the rebuttal reports of Dr. Kenneth Mayer and Prof. Donald Green. I have reviewed both reports and remain confident that the Clean Elections law changes the timing of contributions (i.e., candidate income), the timing of expenditures, and the strategies of candidates.

In this rebuttal report, I focus on three areas addressed by Dr. Mayer and Prof. Green: methodology, interpretation of the findings, and the cost and benefits of Clean Elections. In the methodology section, I explain why my statistical techniques are appropriate and sound. I also conduct some new analyses establishing that matching funds provisions have a powerful effect precisely where they matter most: in very competitive races. Specifically, privately-funded candidates in close races in which matching funds are triggered raise significantly more money *after* the general election compared with privately funded candidates in races in which matching funds are not triggered. This is consistent with a strategy of spending money on credit prior to the election to avoid a match (triggered by contributions in the general election), and then raising money after the election in order to pay off debts. In the interpretations section, I establish that restrictions on speech termed by Dr. Mayer to be "trivial" and also dismissed by Prof. Green as insubstantial are nonetheless important. I also rebut claims by Dr. Mayer that my analyses are based on anything but the evidence. In the final section, I place the findings into the larger literature on Clean Elections to establish that the costs of this law are significant relative to its benefits.

REBUTTAL REPORT
McComish v. Bennett

David M. Primo, Ph.D.

INTRODUCTION

I am an associate professor of political science at the University of Rochester. I joined the faculty in 2002 after receiving my Ph.D. in Political Science from Stanford University. I also have an M.A. in Economics from Stanford, an M.A. in Political Science from Brown University, and a B.A. in Economics and Honors Political Science from Brown.

At the University of Rochester I teach courses on American politics, positive political theory, the interplay of business and government, entrepreneurship, and campaign finance. I have won two College-wide teaching awards at the university: the 2005 Goergen Award for Distinguished Achievement and Artistry in Undergraduate Education, given by the College, as well as a 2005 Undergraduate Professor of the Year Award, given by the University of Rochester Students' Association.

My research on campaign finance, public finance, methodology, and political bargaining has been published in peer-reviewed journals including *American Journal of Political Science*; *Journal of Politics*; *Election Law Journal*; *Economics and Politics*; *Journal of Law, Economics, and Organization*; *Business and Politics*; *Independent Review*; *State Politics and Policy Quarterly*; and *Public Choice*. My work has also been published in several edited volumes. In addition, I have published two peer-reviewed books: *The Plane Truth: Airline Crashes, the Media, and Transportation Policy* (co-authored with Roger W. Cobb, Brookings Institution Press, 2003) and *Rules and Restraint: Government Spending and the Design of Institutions* (University of Chicago Press, 2007). *Rules and Restraint* was awarded the Alan Rosenthal Prize by the Legislative Studies Section of the American Political Science Association. This award is given for the best publication in legislative studies by a young scholar during 2007 of importance to legislators and legislative staff and of merit in strengthening the practice of representative democracy. I have received numerous grants to support my research, including from the National Science Foundation.

My research on campaign finance laws focuses on the impact of these laws on competitiveness, turnout, and perceptions of government, and it has been cited widely in academic journals, policy reports, and the popular press. This research establishes that the claims reformers make about the benefits of campaign finance law often do not hold up in practice. For instance, Primo and Milyo (2006) establish that few campaign finance laws improve citizen perceptions of government, and in fact they may sometimes negatively affect these perceptions.[1] Most of my

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[1] Primo, David M., and Jeffrey Milyo. 2006. "Campaign Finance Laws and Political Efficacy: Evidence From the States." *Election Law Journal* 5(1):23-39.

research in this area is quantitative in nature, utilizing data analytic techniques such as regression to probe relationships among campaign finance laws and outputs such as perceptions of government.

I have previously served as an expert witness in one case, *McConnell v. FEC*. In that case, which dealt with the federal Bipartisan Campaign Reform Act, I focused on the impact of campaign spending on citizen perceptions of government. I showed that at the very time that soft money receipts were increasing, so was citizen trust in government. This simple pattern in the data suggested that it was unlikely that soft money spending was harming citizen perceptions of government.

In my initial expert report for *McComish v. Bennett*, I focused on the matching provisions in Arizona's Clean Elections law and established statistically that these provisions impose significant costs on candidates and, by extension, the public. Specifically, these rules lead to changes in fundraising and campaign spending which distort political speech. I have been asked by the Plaintiff-Intervenors to respond to the rebuttal reports of Dr. Kenneth Mayer and Prof. Donald Green.[2] I am being paid $225 per hour for my work on this rebuttal report. I am familiar with the provisions of the Clean Elections law and with the extant political science literature on public financing of campaigns.

I have reviewed both reports and remain confident that the Clean Elections law changes the timing of contributions (i.e., candidate income), the timing of expenditures, and the strategies of candidates. In what follows, I explain why my methodology is sound and why the effects found in the analysis are substantively important. I then place the findings into the larger literature on Clean Elections to establish that the costs of this law are significant relative to its benefits.

METHODOLOGY

The Data

Dr. Mayer begins his report by outlining several instances in which he claims to have found errors in the dataset provided to me by Dick Carpenter. It is my understanding that the specific instances of campaign expenditures and income that Dr. Mayer points out are figures that were provided by the Arizona Secretary of State's Office. As Dr. Mayer also points out, in a dataset this large there are bound to be errors. Finding a handful of errors in a dataset with well over a thousand observations is normal. I have corrected some errors in the data, one pointed out by Dr. Mayer (regarding Steve Poe) and three that I have located (Laughter in House District 2 in 2002, Aguirre in Senate District 10 in 2002, and Valadez in Senate District 29 in 2002 should have been coded as being in a general election race where the number of seats up for election equaled the number of candidates, therefore resulting in these three individuals being dropped from the analysis). Another error pointed out by Dr. Mayer, regarding vote totals, was fixed in the Stata version of the dataset used for the initial analysis. I have updated the tables from the main body

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[2] In what follows I refer to Dr. Mayer's initial expert report as "Mayer ER" and his rebuttal report as "Mayer RR." I refer to Prof. Green's rebuttal report as "Green RR." I refer to my initial report as "Primo ER."

of the initial report that were affected by these changes and have placed the updated tables, as well as an updated version of Figure 2, into Rebuttal Report Appendix A. Appendix A is incorporated into this rebuttal report by reference. The statistical significance and substantive significance of the following findings regarding matching funds are unaffected by these corrections:

- All results for primary elections.
- The results for periods GE1-GE3 and GE5 of the general election in all specifications, and GE4 in the results presented in Tables 1H and 2H.

However, these changes affect the statistical significance of the matching funds variable in the period immediately before House elections (GE4) in the full regression specification.

My Coding Procedures Are Accurate

Dr. Mayer also claims that the codes provided by Dr. Carpenter to me do not match up with the categories that I describe in my report. Dr. Mayer is mistaken. The e-mail from Dr. Carpenter lists five periods for expenditures in the primary, and five periods for income in the general election, among other periods. I utilized primary expenditure periods P2-P4 and general income periods P2-P6 in my report. These correspond to periods PR1-PR3 and GE1-GE5 in my report, respectively.

The Use of Logarithms

Dr. Mayer and Prof. Green raise concerns about my conversion of dollars to logarithms. Dr. Mayer writes that my use of logarithms is "unnecessary," and that log transformations are appropriate when "the data range is such that the largest values are 4- to 5- orders of magnitude greater than the smallest values" (Mayer RR, 5). In fact, such wide ranges in the data occur often in campaign finance data, including the data I am working with. Moreover, the use of log-transformed data is very common in the campaign finance literature. For instance, Ansolabehere, de Figueiredo, and Snyder (2003, 124) state that they convert campaign spending data into logs to "correct for the skew in ... total [campaign] spending."[3]

Prof. Green takes issue with my calculations of percentage changes in spending implied by the differences between logs. This point is well-taken. I was using an approximation calculation in my tables; this approximation is not ideal, though, when differences between candidates are as large as they are in the data. Prof. Green's alternative calculations compare percentage differences in logs, but the relevant comparison is percentage differences in $, which is calculated with the following formula: $100*[e^{(\text{Type 2 Average in Logs} - \text{Type 1 Average in Logs})} - 1]$. New calculations appear in Appendix A, referenced above. However, the results in my initial analysis are, if anything, strengthened by the adjustments that I made in response to Prof. Green's comment, since my initial calculations understated the gap between types of candidates.

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[3] Ansolabehere, Stephen, John M. de Figueiredo, and James M. Snyder, Jr. 2003. "Why is There So Little Money in U.S. Politics?" *Journal of Economic Perspectives* 17:105-130.

My Statistical Techniques Are Appropriate

In my expert report, I compared Type 2 and Type 1 candidates. Both types of candidates are privately financed, but they face different strategic environments. Type 2 candidates run in races in which matching funds are triggered. Type 1 candidates run in races in which matching funds are not triggered. I found the following:

- In primaries, Type 2 candidates far outspent Type 1 candidates in the final two weeks of the campaign. This is consistent with a strategy to spend money at the last minute to minimize the effectiveness of matching funds given to one's opponent as a result of such spending.

- In the general election, Type 2 candidates in the House raised more than double their Type 1 counterparts in the period following the general election. Type 2 House candidates also raised more than Type 1 candidates in the two-week period leading up to the general election. This is consistent with a strategy of deferring income that could trigger a match until the end of the campaign or afterwards to minimize the effectiveness of any matching funds awarded as a result of such fundraising.[4]

Dr. Mayer and Prof. Green make several claims that they believe call into question my results. I group them into three summary claims. First, Dr. Mayer states that I attribute *"all* of [the differences between Type 2 and Type 1 candidates] to the presence of matching funds," when in fact the differences are "wholly explained by other variables" (Mayer RR, 5). Prof. Green makes a related claim about "the potential for unobserved factors to throw off this comparison" (Green RR, 5). Second, Dr. Mayer argues that my categorization of Type 2 and Type 1 candidates biases my results toward finding an effect of matching funds; in fact, he states that my coding "guarantees" that Type 2 candidates will outspend/outraise Type 1 candidates in each two-week period of the race (Mayer RR, 5). Third, Prof. Green constructs a hypothesis based on my analysis and argues that it does not hold up. I will now respond to these three claims.

Claim 1 (Mayer and Green): Other Factors Explain the Differences Between Type 2 and Type 1 Candidates

Dr. Mayer's first point is that I claim that all of the differences between Type 2 and Type 1 candidates are due to the presence of matching funds, when in fact the differences are completely explained by other variables. I have reread my own report carefully, and I can find no instance in which I state that matching funds are the *sole* determinant of disparities across candidates. Dr. Mayer is attacking a claim that I do not make. Moreover, Dr. Mayer offers absolutely no support for his claim that the differentials are *wholly* explained by other variables.

This is not the only instance in which Dr. Mayer mischaracterizes my research. For instance, he focuses on the result that Type 2 candidates outraise Type 1 candidates immediately after the election. Dr. Mayer writes that "this claimed pattern of last-minute spending and fundraising is at the core of Primo's analysis" (Mayer RR, 5). Mayer writes that some of my claims are "illogical, and simply cannot be the result of the process [Primo] identifies" (Mayer RR, 6).

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[4] As I noted above, the result for the general election in the two-week period leading up to the general election is weakened after correcting some errors in the data.

Specifically, Dr. Mayer argues that candidates cannot possibly be raising money after Election Day to pay for items or services purchased on credit prior to Election Day, in order to avoid triggering a match. He writes, "[T]he difference in post-election fundraising cannot have anything to do with matching funds because … [t]hat money cannot be spent, cannot be used retroactively to pay for items purchased on credit (since credit triggers matching funds in the same manner as fundraising), and has no possible impact on the election" (Mayer RR, 6). He further notes that I attribute "great importance to the fact that Type 2 candidates raise twice as much as Type 1 candidates *after the election*, when the fundraising has no consequences" (Mayer RR, 5, emphasis in original).

Dr. Mayer appears to be confusing a *loan* with *credit*. The Arizona Administration Code, Title 2, Chapter 20, Article 1, defines the extension of credit as "the delivery of goods or services or the promise to deliver goods or services to a candidate in exchange for a promise from the candidate to pay for such goods or services at a later date." Further, in its definition of a contribution, the law *excludes* the following: "An extension of credit for goods and services made in the ordinary course of the creditor's business if the terms are substantially similar to extensions of credit to nonpolitical debtors that are of similar risk and size of obligation and if the creditor makes a commercially reasonable attempt to collect the debt, except that any extension of credit under this item made for the purpose of influencing an election which remains unsatisfied by the candidate after six months, notwithstanding good faith collection efforts by the creditor, shall be deemed receipt of a contribution by the candidate but not a contribution by the creditor" (§16-901 of the Citizens Clean Elections Act, A.R.S., Title 16, Chapter 6, Article1). Consistent with this exclusion, credit is not mentioned in the list of items that are considered contributions for the purposes of determining a match, though loans are. Moreover, the Clean Elections law clearly denotes purchases on credit for purchases used in the primary as expenditures for the purposes of determining a match in the *primary*, but here it also terms loans as being considered expenditures. In other words, one of Dr. Mayer's central claims is based on his misunderstanding (or perhaps misinterpretation) of the law, rather than any illogic on my part. And, this misunderstanding is crucial, because it is entirely conceivable that candidates could make purchases on credit at the end of the campaign and they raise money after the election in order to pay off that debt. In doing so, they would be able to expend money before the election but not trigger matching funds via a contribution.

Moving beyond Dr. Mayer's mischaracterizations, what does Dr. Mayer show? Dr. Mayer purports to show that factors such as whether a candidate wins or loses and the size of a candidate's war chest are relevant variables that influence differentials among candidates, and that matching funds therefore do not explain differentials among candidates. Let us examine these two points in turn.

Dr. Mayer is correct that there is a correlation between fundraising after the election and whether a candidate was victorious. However, Dr. Mayer is incorrect in asserting that this relationship implies that there is no relationship between Clean Elections and post-election fundraising. To establish his point, Dr. Mayer argues that the percentage difference between winning and losing candidates is greater than the percentage difference between Type 2 and Type 1 candidates. It is more appropriate, however, to examine whether there is a difference between Type 2 and Type 1 candidates, *conditional* on winning a race. If winning and losing overrides

any effect of matching funds, then there should be no difference between Type 2 and Type 1 candidates who lose, and Type 2 and Type 1 candidates who win. Table 1 depicts the post-election fundraising averages for four categories of candidates: winning Type 2 candidates, losing Type 2 candidates, winning Type 1 candidates, and losing Type 1 candidates. As Table 1 clearly shows, winning Type 2 candidates far outraise winning Type 1 candidates after the election, and losing Type 2 candidates far outraise losing Type 1 candidates.

Table 1. Type 2 Candidates Consistently Outspend Type 1 Candidates After the Election

	Type 1 Candidates	Type 2 Candidates
Winners	$1,132	$1,993
Losers	$ 386	$1,371

Figures are in 2006 dollars.

Dr. Mayer also notes that candidates sometimes have "war chests," or monies available to them at the beginning of a race. He argues that candidates should not amass war chests if they are concerned about triggering a match. This argument assumes that the *only* factor influencing a candidate's strategy is to avoid a match; this is an assumption I did not make in my initial report and do not make in this rebuttal report. In fact, war chests are perfectly compatible with a matching funds system. First, matching funds are not the only strategic consideration in an election, and deterring candidate entry is a possible strategic reason for building a war chest.[5] Second, candidates typically have very limited information about whether they are likely to be Type 2 or Type 1 candidates prior to the beginning of the election. Third, by building up a war chest at the beginning of a race, a privately funded candidate can be ready to counter spending by a publicly financed opponent who receives the initial Clean Elections disbursement in a lump sum at the beginning of the race.

Dr. Mayer goes on to criticize my regression analysis, calling it "fatally flawed" for failing to include several variables that have a "decisive effect on campaign spending," thereby causing an econometric problem known as "omitted variable bias" (Mayer RR, 11). Prof. Green also raises questions about whether I have controlled for all relevant variables. Dr. Mayer specifically asks that I control for competitiveness, whether a House election is fully contested (i.e., has 4 candidates), opponents' spending, and the partisan composition of the district.[6] In response, I performed additional analyses of the House general elections data.

I created several new measures based on Dr. Mayer's comments. First, since the top two vote getters in House races are elected, standard measures of competitiveness are inappropriate. Instead of using off-the-shelf measures, I constructed two measures of competitiveness for each

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[5] The empirical literature is mixed regarding the deterrent effects of war chests.

[6] It is statistically problematic to include opponents' spending in the regression because it may be dependent in part on the spending of the candidate under study. Therefore, I do not consider this variable. In addition, because so few of the Senate candidates in my study were in competitive races, I do not reanalyze the Senate data. I also do not reanalyze the primary data, since Dr. Mayer's focus was on the general election.

candidate. The first assessed whether a winning candidate was within 10 points of the best-performing losing candidate or whether a losing candidate was within 10 points of the worst-performing winning candidate. The second, more stringent, measure performed the same calculations using a 5 point threshold. For instance, if the 1st place candidate received 35% of the vote, the 2nd place candidate had 31% of the vote, the 3rd place candidate had 28% of the vote, and the 4th place candidate had 6% of the vote, then the 1st place candidate would be in a competitive race under the first definition but not the second, because 7% separates 1st and 3rd. The 2nd place candidate would be considered in a competitive race under both definitions, because 3% separates this candidate from the 3rd place candidate. Second, I created two measures of district partisanship. One is a measure of the percentage of registered voters who are registered as Democrats in the district. The second is a measure of the absolute value of the difference (percent Democrat -.5). This second measure captures how big the gap is between Democrats and Republicans and is a measure of the underlying competitiveness of the district. In addition, I created an indicator variable for whether the race had 4 candidates, as well as whether the candidate was an incumbent, a challenger, or was in an open seat, following Dr. Mayer's coding rules. An incumbent was coded as such only if he or she was not redistricted.

Regardless of which definition of competitiveness I use (the 10% threshold or the 5% threshold), the results are weakened when all races are considered together. This is true whether both or just one of the measures of partisanship are included. This is not surprising, given the complex interrelationships present in the data. However, is it necessary to include these additional variables? To examine this question, a researcher can perform a statistical test known as an F-test. It allows one to determine whether a group of variables collectively is likely *not* to have an effect on the dependent variable (in this case, candidate spending). In almost three-quarters of the nearly 100 regressions that I performed, the more parsimonious regression models that I estimated in my initial report are superior to the specifications suggested by Dr. Mayer. In other words, Dr. Mayer's concerns that I was omitting *clearly* important relevant are unwarranted.[7]

Dr. Mayer's concerns do point to an interesting theoretical extension to my argument. Specifically, candidates in competitive races may have different strategies for dealing with the matching funds provisions in the Clean Elections law. To analyze this possibility, I considered competitive and non-competitive races in separate analyses, since strategic considerations may differ in these types of races. The results are striking. Recall that there are two measures of competitiveness: the measure with a 10% threshold, and the measure with a 5% threshold. When I rerun the analyses separating out the candidates using the 5% or 10% thresholds, I find results consistent with my initial analysis: Type 2 candidates in competitive races far outraise Type 1 candidates in competitive races in the period immediately following the general election. Specifically, I find that Type 2 candidates in close races (5% threshold) raise over three times more than Type 1 candidates in close races.[8] *These new analyses imply that the matching funds provisions have a powerful effect precisely where they matter most: in very competitive races.*

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[7] Prof. Mayer noted that a low R^2 value can "confirm that a[n omitted variables] problem exists" (Mayer RR, 11). This is a questionable claim, as my results show.

[8] This figure is based on a regression analysis using the natural log of spending in period GE5 as the dependent variable. The coefficient on Type 2 candidate is 1.45 and is statistically significant at the $p < .05$ level in a one-tailed test. I use the formula mentioned earlier in this report to convert this coefficient to a percentage difference in dollars.

Claim 2 (Mayer): Primo's Set Up Guarantees He Will Find Differences Between Candidates
Dr. Mayer claims that the "very manner in which Primo sets up his analysis guarantees that he will find the differences that he claims to observe" (Mayer RR, 8). Specifically, Dr. Mayer believes that because Type 2 candidates outspend Type 1 candidates overall, then my analysis finding that Type 2 candidates outspend Type 1 candidates in certain sub-periods is "guaranteed" (Mayer RR, 9). He even asks the following: "Can it be a surprise that Type 2 candidates, who raise twice as much as Type 1 candidates in the general election, raise more than Type 1 candidates in each 2 week period, or that they raise twice as much in the two weeks immediately before the election? Indeed, it has to be true" (Mayer RR, 9). Dr. Mayer's claim is incorrect. As an example, assume that a Type 2 candidate spends $2,000 in a race, and a Type 1 candidate spends $1,000. Further suppose that there are 3 periods under study, and the Type 2 candidate spends $100 in period 1, $200 in period 2, and $1,700 in period 3. The Type 1 candidate, on the other hand, spends $300 in period 1, $300 in period 2, and $400 in period 3. In two of three periods, the Type 1 candidate outspends the Type 2 candidate. And, as my statistical results show, there is not a statistically significant difference between candidate spending in each period. In other words, my results are not a foregone conclusion due to my method.

Claim 3 (Green): Differentials Between Candidates Do Not Grow Over Time
Prof. Green writes that "the essence of Professor Primo's argument is that the gap between the two types of candidates *grows larger* as we move from the middle of the election season to the end" (Green RR, 8). This is certainly one implication of my argument, but it is one that, for reasons Prof. Green notes, is very difficult to find evidence for, given all the other strategic incentives in a campaign. It is not surprising, then, that Prof. Green conducts a meta analysis and fails to find temporal patterns in the data.[9]

INTERPRETATIONS OF THE FINDINGS

"Trivial" Restrictions on Speech Are Not Trivial
Dr. Mayer and Prof. Green adopt a similar strategy when considering my finding that Type 2 House candidates outraise Type 1 candidates in the period following Election Day: claiming that the differences are unimportant.[10] Dr. Mayer writes that the difference is "trivial" and has no "substantive significance" (Mayer RR, 10). Green writes that the "political consequences are negligible" (Green RR, 8). But a reduction in free speech is a reduction in free speech, and candidates whose rights have been negatively affected by the law are almost sure to disagree about how trivial or negligible the effect on their speech actually is. First, let us be clear that the differences are far from trivial (see Figure 2—Updated in Appendix A). Second, the *presence* of the effect is itself important. As a thought experiment, suppose that a state enacts a new law

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[9] It is not clear why Prof. Green chose to use meta analysis rather than pooling the observations and creating a series of interaction terms.

[10] Both Dr. Mayer and Prof. Green point out that the effects in the House are stronger than the effects in the Senate. I concur that this is an issue that merits further study.

restricting campaign expenditures, and in the first election under this new law expenditures are reduced in campaigns by a "trivial" 5 percent. Such a law would still be in violation of the landmark decision *Buckley v. Valeo*, 424 U.S. 1 (1976). In the case of Clean Elections, of course, we have both the presence of an effect and a substantively large effect. In sum, I stand by the conclusion I reached based on an original data analysis, and which I have defended against Dr. Mayer's and Prof. Green's claims: speech has been distorted as a consequence of this law.

Prof. Green makes a related theoretical argument which is equally problematic. Prof. Green claims that the "strategic incentives associated with the matching funds system are overshadowed by other strategic incentives that impel nonparticipating candidates to raise and spend beyond the matching threshold" (Green RR, 4). I agree that there are many strategic factors in a campaign. While my findings show that matching funds do alter campaign strategies in meaningful ways, let us suppose that Prof. Green is correct and that other strategic incentives overshadow the impact of the strategic incentives surrounding matching funds. Even if this were true, it would *still* mean that candidate strategies are distorted relative to if no matching funds provisions were in place, just that some factors are outweighed by others.

My Claims Are Based on the Evidence

Dr. Mayer believes that several of my claims are "completely normative and subjective" (Mayer RR, 11). First, Dr. Mayer criticizes my use of the word "distort" to describe the effect of matching funds (Mayer RR, 11-12). Given my theoretical discussion explaining why the matching funds provisions encourages candidates to alter the timing of fundraising or expenditures, the word "distort" is consistent with the use of the term in economics. Second, Dr. Mayer finds problematic my usage of the phrase "harms free expression" to describe the effects of matching funds. However, this assessment follows logically from my finding that matching funds alter the timing of candidate activities. Such a distortion *does* harm free expression. Finally, Dr. Mayer argues that my assessment that the benefits of Clean Elections are outweighed by the costs "is not an expert opinion based on an informed analysis of empirical evidence" (Mayer RR, 12). The full text of my statement in the initial report reads, "[The results in my report], combined with the limited evidence that Clean Elections have improved elections or the policy process in Arizona, lead me to conclude that the harms to free expression and the free exercise of political activity associated with Clean Elections far outweigh any benefits the law confers on the state of Arizona" (Primo ER, Executive Summary). This *is* an informed analysis of the empirical evidence, though I did not elaborate in detail on the basis for my judgment in my initial expert report. I do so in what follows.

CLEAN ELECTIONS: A SIMPLE COST-BENEFIT ANALYSIS

I approach the study of campaign finance by thinking of it in terms of benefits and costs. In other words, do the benefits of such reforms exceed the costs of implementation, any restrictions on free speech, and the potential distortions to the campaign process? This argument is articulated more fully in Primo and Milyo (2007).[11] Public financing, including Clean Elections,

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[11] Primo, David M., and Jeffrey Milyo. 2007. "Public Financing of Campaigns: A Statistical Analysis." *Engage* 8:96-100.

is argued by reformers to address at least three perceived problems in the democratic process: limited electoral competitiveness, low levels of trust in government, and excessive "special interest" influence.[12] The existing political science literature finds limited support for these claims.

In a co-authored article, Primo, Milyo, and Groseclose (2006) find little effect of public financing on competitiveness in gubernatorial elections and note that the modest positive effects of the laws on legislative elections, as identified by other authors in the same conference volume (Mayer, Werner, and Williams 2006), are likely to dissipate over time.[13] Primo, Milyo, and Groseclose (2006) is incorporated into the expert report by reference as Appendix B. Malhotra (2008) finds evidence that Clean Elections had some pro-competitive effects in the 2000 Arizona Senate election, but there are some methodological limitations of his approach.[14] For instance, Malhotra uses only one year of data during which the Clean Elections law was in force—the first year, 2000—so it is hard to know if these findings continue to this date. Dr. Mayer's expert report also contains some evidence that competitiveness has increased since the inception of Clean Elections, but it also shows evidence of a trend that Primo et al. (2006) suspect was likely to occur: a reversion back to reelection rates that are similar, and in some cases, higher, than before Clean Elections was created. In fact, in a published piece closely tied to Dr. Mayer's expert report, Dr. Mayer and his co-authors wrote that the 2004 House election was "something of a disappointment to campaign finance reformers: the percentage of incumbents in competitive races in 2004 was the same as it was in 2000 (about 36 percent), declining from a post-1990 record of 47 percent in 2002" (Mayer, Werner, and Williams 2006, 259-260). In addition, the strategic electoral environment in Arizona was changed significantly by term limits during the same period that Clean Elections were put into effect and that Dr. Mayer studied, so it is very difficult to disentangle these two changes. Overall, then, there is mixed evidence regarding the impact of Clean Elections on competitiveness.

The evidence is even weaker regarding other benefits. For example, Primo and Milyo (2006) find a (small) *negative* relationship between public financing laws and citizen perceptions of government.[15] In addition, I am aware of no statistical research establishing that policymaking

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[12] See Findings and Declarations, §16-940, of the *Citizens Clean Elections Act*, A.R.S., Title 16, Chapter 6, Article 2.

[13] Primo, David M., Jeffrey Milyo, and Tim Groseclose. 2006. "State Campaign Finance Reform, Competitiveness, and Party Advantage in Gubernatorial Elections." In *The Marketplace of Democracy: Electoral Competition and American Politics*, eds. Michael P. McDonald and John Samples. Washington, DC: Brookings Institution Press, 268-285.

Mayer, Kenneth R., Timothy Werner, and Amanda Williams. 2006. "Do Public Funding Programs Enhance Electoral Competition?" In *The Marketplace of Democracy: Electoral Competition and American Politics*, eds. Michael P. McDonald and John Samples. Washington, DC: Brookings Institution Press, 245-267.

[14] Malhotra, Neil. 2008. "The Impact of Public Financing on Electoral Competition: Evidence from Maine and Arizona." *State Politics and Policy Quarterly* 8(3):263-281.

[15] Primo, David M., and Jeffrey Milyo. 2006. "Campaign Finance Laws and Political Efficacy: Evidence From the States." *Election Law Journal* 5(1):23-39.
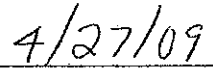
in public financing states is significantly different from states without such laws and that any "special interest" influence has been reduced. I am also not aware of any statistical evidence that corruption or the appearance of corruption has been reduced in measurable ways due to this law. (In his rebuttal report, Prof. Green quotes several individuals who share a belief that such an effect exists, but on its own this is not evidence of an effect.) In short, there is minimal scientific evidence that public financing laws have positive effects on participatory democracy, but my report demonstrates that the law imposes real costs on candidates.

CONCLUSION

I have addressed the concerns raised by Dr. Mayer and Prof. Green in my rebuttal report. These experts claim that my statistical analyses are not sound and that the findings—even if real—are substantively unimportant. In response, based on my revised and additional analyses, I remain confident in my judgment that candidates are altering their behavior as a result of the Clean Elections law in ways that infringe on their free speech rights. The effects are real and they are substantively meaningful. In summary, based on an original statistical analysis of campaign finance data and the extant research on Clean Elections, I conclude that this law has deleterious effects on free expression.

David M. Primo   4/27/09
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David M. Primo   Date