Expressive Politics

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CHAPTER 8

Conclusions: Expressive Politics and Invisible Politics

In the end, cynics might say, there is no getting around the fact that almost all of the candidates profiled here lost. However we might wish for a political system that provides a place for people like Floyd Brenholt, Betty Hull, or Shapley Hunter, it is hard to move beyond their opponents’ gracious words in their victory speeches. One might assume that on election night, Reps. James Sensenbrenner, Phil Crane, and Jerry Costello thanked Brenholt, Hull, and Hunter for running spirited campaigns and for talking about the issues. This is standard campaign rhetoric. But does it mean anything? Could one say that congressional politics or voters are somehow better off because of campaigns such as these?

It is difficult to measure the effects of such campaigns, or to measure the more general effects of the prevalence of lopsided campaigns. Although this book is a study of congressional candidates, I first became interested in the effects of long-shot, losing campaigns through a study of presidential campaign politics. One might argue that presidential candidates in a similar long-shot position—for instance, a Barry Goldwater, a George McGovern, or a Walter Mondale—also adapted their strategies to their undeniable underdog status. There is ample evidence that at some point in their campaigns, these three candidates reconciled themselves to the inevitable and sought solace in exploring ways in which they might make a difference even in losing. It is also clear that these campaigns made a difference at some level. In the case of Goldwater, several books have been written about the effects of Goldwater’s campaign on the Republican Party (Goldberg 1995; Perlstein 2001). Goldwater campaign insider Stephen Shadegg argues that Goldwater spent October of 1964 arguing that even if he lost, his campaign would serve as a wake-up call to Americans, as a “defen[se] of American society against its own indulgence, slothfulness, and apathy” (Shadegg 1965, 241). Karl Hess, another campaign staffer, noted the importance for the party of Goldwater’s control of the Republican National Committee:
“this, for the future of the party, would be second in meaningfulness only to an election victory” (Klinkner 1994, 72). In the waning days of the McGovern campaign, McGovern himself told Newsweek that “if we don’t win, maybe some other, more effective political leader will come along and market these ideas better than I can” (Newsweek 1972). And Mondale defended his campaign in September of 1984 by saying, “I may lose this election, but at least I will say something in defeat” (Gillon 1992, 380).

These may all be rationalizations, but given the lessons the parties’ subsequent candidates might be said to have learned from these campaigns, it would seem that each of these three, recognizing the long odds against victory, sought to reap other benefits from his campaign. Can the same be said for congressional candidates? Are there district-level stories to be told that parallel the Goldwater, McGovern, or Mondale stories?

I would not have undertaken this study if I believed these campaigns make no difference. At a minimum, these campaigns can teach both political theorists and empirical political scientists lessons about how to understand congressional campaigns, and perhaps campaign politics more generally. In this chapter I close the book by reiterating the main tenets of my argument, by exploring the lessons researchers can learn, and by exploring the normative consequences of these campaigns for American politics.

Invisible Politics and Invisible Politicians

In this book I have sought to demonstrate that a new theory is needed to explain the issue strategies of challengers to incumbents. In particular, it is necessary to explain the strategies of candidates who emerge to challenge extremely popular incumbents—incumbents who are highly unlikely to be unseated. In chapter 3 I demonstrate empirically that incumbents and challengers differ in the degree of “rationality” in their issue positions—if rationality is defined as seeking to adopt vote-maximizing positions, positions which cater to the preferences of the median voter. But it is too easy to dub the positions of losing candidates “irrational.” Instead, I have argued here that we should see divergent issue positions as responses to unbalanced competition, as positions taken not in order to win but to express political views that would otherwise go unheard.

Giovanni Sartori (1976, 95–96) describes the intra-party decisions which precede establishment of issue positions as “invisible politics.” We are unable to definitively get inside the heads of politicians in order to discern the reasons for the issue positions they take or the personal compromises they make. Stylized reductions of their incentives, such as Mayhew’s “single-
minded election-seeking” or Fenno’s three-pronged categorization of congressional incumbents’ motives, have been of great use in simplifying the strategic calculations of members of Congress and deriving testable hypotheses. But they are mere approximations of members’ personal decision-making processes. And they definitely do not get us very far in understanding the decisions of nonincumbent candidates. The invisible politics of which Sartori speaks, however, is, he writes, “more simple, and more genuine” than is visible, or “rational” political behavior (Sartori 1976, 96). In the case of the candidates I describe here, it is a politics of actual preferences, actual beliefs, which are often put forward without reference to instrumental concerns.

The candidates studied here are invisible in another sense, however. They ran campaigns which garnered scant media interest, scant voter interest, and even scant interest on the part of the incumbent. Not all of the candidates studied here ran “invisible” campaigns, but the majority did. The invisibility of these campaigns has brought about much research by political scientists, but it is ultimately research which moves us toward explaining why incumbents are so safe, and away from the campaigns of those who challenge incumbents. This is our loss, as these campaigns are an important part of political discourse, and they are often happening right under our noses.

The explanation I have provided here for the strategies of these candidates does not diverge dramatically from established game-theoretic models of candidate competition. My explanation does not contain precise candidate equilibria, and hence it does not rise to, or aspire to rise to, the sophistication of many well-established models. This is because the intuition behind it is quite simple. Spatial models of candidate competition generally can be distilled down to three essential variables: information, both about voter preferences and about one’s opponent’s issue position; candidate positions themselves; and the outcome (or expected outcome) of the election given any set of candidate positions. If an incumbency advantage is established, I argue, it must inhere in part to the positions of the incumbent, and it must be a piece of information that is known to both candidates. Where an incumbency advantage exists, then, information is neither particularly costly nor scarce for either candidate—in the incumbent’s case, because of the resources that come with incumbency; in the challenger’s case, because the incumbent’s performance and positions are observable.

Where voter preferences and the opposing candidate’s positions can be known to a candidate, this must only be because that candidate does not adopt positions at the same time as her opponent. That is, candidates position themselves sequentially, not simultaneously. Where candidates are taking positions sequentially and the first mover has behaved rationally, the
vote maximization assumption seems somewhat suspect for the candidate adopting positions second. After all, what is the purpose of a candidate in a plurality or majority rule, first-past-the-post election expending resources in order to get more votes than are necessary to win, or seeking as many votes as possible in what is doomed to be a losing effort? Here, I propose two different alternatives, which I dub the expressive campaign and the party-based campaign. The intuition behind these alternatives is that where an incumbent has an advantage and has adopted a median position, a challenger’s probability of winning is virtually nonexistent. For a challenger in such circumstances, the goal is the campaign itself, not its outcome.

In chapters 3 through 6 I have sought to document the various tenets of this argument—that challengers do not tend to take positions in accordance with voter preferences; that they have information that is “good enough” for them to be aware that they are doing this; that they are eager to speak at length about their desire to express their policy views, even at the expense of votes, in their campaigns; and that parties can influence challenger positions but that well-organized parties choose not to do this. Thus, I conclude that both sequential positioning alternatives hold, that these constructs do serve to explain the strategic choices made by these candidates. In chapter 7 I provide evidence that the patterns noted in the previous three chapters are not atypical, that they are not patterns limited to one particular set of states or one particular election year. Furthermore, chapter 7 presents the possibility that the frequency of expressive campaigns may increase with documented trends in the safety of incumbents. Expressive campaigning, then, may wax and wane over time but remains a major component of American congressional elections.

There are three sets of implications for this argument—implications for theorists of candidate competition, implications for those who study American politics, and implications for those with a normative belief in the importance of competitive elections and substantive discourse about issues in those elections. In the remainder of this chapter, I thus entertain the implications of my argument for each of these fields of inquiry, and I note areas in which additional research would clarify and extend my argument.

### Implications and Directions for Further Research: Theories of Candidate Competition

At the most basic level, the research I present here should encourage those who would construct theories of candidate competition to look more closely at the sequential nature of electoral competition. The work of
Groseclose (2001) and of Feld and Grofman (1991) that I discuss in chapter 2 presages a move in that direction, but the theory I elaborate here seems to square well enough with the self-reported strategic decisions of challengers to incumbents that I find it remarkable that sequential theories of candidate positioning have not gained wider currency. Similar theories of behavior in legislatures have been proposed (see Baron 1995), as have expectation-based models concerning candidate entry (see Cox 1997; Greenberg and Shepsle 1987). It may only be a matter of time before such theories are applied more extensively to campaigns and elections.

The theory I propose here contains several assumptions which represent breaks with established literature. I have sought in the qualitative chapters of this book to demonstrate their validity for the elections I cover. Each of these assumptions can, I argue, be extended and elaborated upon. I shall consider each of these in turn, in accordance with the sequence of empirical chapters I have provided here.

**Information**

The full information assumption has often been questioned when it has been employed to explain politicians’ choices. I have weakened this assumption somewhat, by demonstrating that the candidates considered here had information thresholds—that they were able to acquire “full enough” information for the purposes of their campaigns. In addition, in a sequential model the second mover has the luxury of being able to base choices upon his or her beliefs about the level of information the first mover has. For instance, a challenger can observe the positions taken by an incumbent, and if that incumbent has had substantial electoral success at those positions, he or she may well assume that the incumbent has identified the median. If that challenger wishes to capture the median, then, all he or she has to do is to ape the strategy of the incumbent.

This begs the question, however, of how incumbents gain their information. Hall (1995) addresses the import of the full information assumption for studying legislators’ behavior and concludes that its frequent use in establishing models of legislative behavior provides a test of the assumption, a test which it has frequently passed. The full information assumption seems less controversial, however, in the framework I use here. The unidimensional framework and the relatively simple constraints provided by voters’ ability to process information about candidates—is a candidate, for instance, for or against gun control?—makes the full information criterion much easier to meet in electoral competition than it
is in legislative activity. Nonetheless, an elaboration of where and when information thresholds exist in campaigns, or when a particular level of information is enough, would be welcome.

**Issue Positions**

Measuring candidates’ issue positions is another substantial problem for testing spatial models of candidate behavior. The scale I employ in chapter 3 has no provisions for weighting according to intensity, and it is composed of a series of yes/no question responses—it does not measure whether candidates have more subtle preferences over each of these issues. This is a problem that plagues many spatial analyses. Political scientists have labored to measure the positions of incumbent legislators, yet nonincumbent candidates are far more difficult to measure. I have sought to overcome this problem through my reliance upon the open-ended interview questions I posed to a subset of these candidates, but even before more data are collected on these candidates, a clearer means by which to identify the ideal points of candidates—particularly those without a legislative record—must be constructed.

**Candidates’ Preferences**

One area in which further work is possible using the framework I provide here, and without either conducting a tremendous amount of extra research or laboring to resolve theoretical quandaries such as those I outline above, is in gauging the trade-offs between candidates’ *ex ante* ideal points and positions taken to maximize utility in terms of votes or probability of winning. In the theory I present here, I judged it to be somewhat of a detour to provide indifference curves or other mathematical mechanisms for assessing how a candidate moves from maximization of utility along one dimension toward utility maximization on the other. This all should depend upon the weight each candidate assigns to each incentive. For the sake of illustration, I proposed a simple stylized depiction of this movement with a tipping point—either the challenger believes himself or herself to be competitive (and hence maximizes proximity to the district median) or does not (and hence maximizes proximity to his or her noninstrumental ideal point). One could, of course, posit other, less clear trade-offs; for instance, a candidate’s utility along one dimension could decline in a linear or curvilinear fashion across different positions. A candidate could weigh
these two values differently, and thus find some winning positions inferior to certain losing positions (that is, some issue stances may be nonnegotiable) or the candidate may find probability of winning not to be the stark either/or proposition I have established here. Elaboration of these tradeoffs would not be difficult, although testing them would be. Continued movement among political scientists away from solely positing election-seeking goals or outcome-based goals toward study of more purely ideological or expressive motivations would certainly aid in the development of electoral models which feature candidates with varying prospects for success.

**Political Parties**

Finally, rational choice theories which posit a role for political parties must take fuller account of the differences between in-parties and out-parties, or in the case of my argument here, of parties which do have an opportunity to be competitive and those which do not. In the ongoing debate between Krehbiel and much of the rest of the positive theory community, Krehbiel (1998, 165–72) has argued that inserting parties into legislative models does not help us gain leverage upon legislative decision making. The most important criticism leveled by Krehbiel is that responsible party government or conditional party government theories cannot be tested because they have no provision for estimating legislators’ “true” ideal points—they measure party strength using voting outcomes, which, in turn, are the phenomena they are seeking to explain.

Analysis of nonincumbent candidates provides a way around this problem in two ways. First, it provides a baseline for judging candidates’ preferences prior to reaching office, although this baseline is still complicated by our inability to conclusively distinguish between expressive and strategic positions. Second, however, it also provides, as I posit in chapter 2 and demonstrate in chapter 6, a means of gauging party unity in elections. It may seem paradoxical to look for the voice of the party in the campaigns of candidates who lose their election bids, but this may be the greatest opportunity in American electoral politics to seek to identify party positions without such positions being contaminated by election-oriented behavior. At the very least, identifying party positions in this sense can provide a means of estimating how far election-seeking politicians or officeholders deviate from such party positions.
Implications and Directions for Further Research: The Study of Nonincumbent Candidates

At a minimum, the research I present here should also call to the reader’s attention the paucity of research on congressional challengers. Although Congress may well be among the most studied of American institutions, we know precious little about individuals who run for Congress save those who win or come close to winning. The Maisel and Stone (1997; also Stone, Maisel, and Maestas 1998; Stone and Maisel 2003) candidate emergence study should be of great help in identifying the barriers to competition in many congressional districts, but the congressional candidates who do emerge are rarely studied on the scale I attempt here, let alone on a national scale. There are substantial costs to undertaking such a study; I have sought here to go beyond the scale of studies such as those of Kingdon, Huckshorn and Spencer, and Leuthold, but a truly random, qualitative study of congressional challengers remains prohibitively expensive for the average researcher. It is much easier to conduct qualitative, interview-based research in Washington, where all incumbents are gathered, than it is to venture out across the country to track down unsuccessful candidates.

Surveys such as the *Time/Congressional Quarterly* Candidate Survey discussed in chapters 3 and 6 are one means of remedying this problem. Because the *Time/Congressional Quarterly* study appears not to have been constructed with statistical research in mind, however, its utility for this project was somewhat limited. Many congressional candidates run such low-profile campaigns that they are eager for attention, whether the source is political scientists, journalists, or any other interested party. All but one of the 1996 candidates I was able to locate were eager to speak with me; their enthusiasm indicates that they would be a valuable source of information for any researcher.

More specific implications and research subjects coincide, as is the case with the theoretical implications, with the organization of this book:

*Information*

My argument about information acquisition is that candidates do, regardless of their financial resources, have the ability to gather relatively accurate information about voter preferences. I investigate such information acquisition through relatively open-ended questions to candidates about their information sources and about their rationale for position taking. My conclusions in this regard are admittedly a judgment of the veracity of can-
candidates’ claims and the plausibility of their claims about public opinion. Using district-level survey data on political issues would provide a valuable means of verifying candidates’ claims. In addition, investigating the specific questions asked when candidates do poll, how voters respond, and how well-constructed these surveys are for objectively measuring public opinion would provide a better sense of the value of such polls in measuring voter preferences. Unfortunately, most of these polls are proprietary, and the candidates I interviewed who conducted polls often seemed quite removed from the administration of such polls and not well enough versed in survey research to understand the potential uses of such polls. More detailed studies of such polls thus await the cooperation of the firms who conduct them.

**Issue Positions**

In the theory I have presented here, I posit two different incentives for position-taking—strategic maximization of votes or probability of winning, and expression of one’s actual political beliefs. There are, of course, many other motivations for position taking. As Maisel (1986, 24) argues, many congressional candidates adopt positions with an eye toward the future, toward another race for Congress or another office, or merely with an eye toward gaining customers for their businesses.

Any model which posits ex ante preferences begs the question of how these preferences arise. Such a question is, of course, so large that it goes far beyond the scope of this book and far beyond the issue of studying congressional candidates. Two more modest means by which the formation of issue positions could be studied are, however, to study congressional candidates before and after their campaigns or to track these candidates for a period of time after their campaigns end. I chose not to interview candidates during their campaigns because of concerns that I would not be able to interview them—they would, after all, be busy campaigning—and because I was concerned about the veracity of any information they would provide me. A pre-/post-interview format would, however, enable the researcher to measure how much candidates’ views or statements change after the campaign. In addition, a study such as the Maisel and Stone study would be of use insofar as it gathers information about potential candidates before they actually enter the race.

Likewise, tracking candidates after their campaigns would allow the researcher to investigate whether candidates who take positions geared toward helping them in the future—in another race for Congress, for
instance—do benefit from their campaigns in this manner. Surely some candidates profiled here had such goals in mind—of my case-study candidates, John Shimkus was a second-time candidate for Congress when he won, Lydia Spottswood and Mary Rieder would go on to run again in 1998 and 2000, respectively, and Cynthia Ruccia ran twice for Congress before deciding to use her knowledge and name-recognition to run for a lesser office. Evidence on whether candidates benefit in repeat bids for Congress from their first race is mixed (see Squire and Smith 1984; Levitt 1994; Mack 1998; Milyo 1998; Boatright and Taylor 2001). More qualitative research could, however, shed greater light on the realism of candidates’ future-oriented goals.

Political Parties

The conclusions I reach about the role of political parties in these candidates’ campaigns have been explored by numerous researchers. Such studies are hampered, however, by the difficulty of gaining access to all that goes on within political parties at their various levels. Especially with the increasing role of the national campaign committees in congressional campaigns and the corresponding decline of the local party machines which were the subject of so much party research in the past, it is difficult to identify the role of smaller party units—of the county-level or district-level party organizations. In many of the campaigns considered here, these organizations served as little more than clubs for a small group of the party faithful. Nonetheless, these organizations are important units of study if we wish to shed light upon the candidate recruitment process. Again, efforts such as the candidate emergence study and the Kazee edited volume may help in this regard. The party decline and party centralization arguments leave little room for these types of party organizations. Yet, as I show in a number of the campaigns considered here, party organizations at this level do play a vital role in campaigns. In states with more ideological parties, they may prevent viable candidates from emerging; in states with less ideological parties, they may serve a vital function because they do encourage viable candidates and they may keep a semblance of party organization—of organizational memory, perhaps—in place to be drawn upon in the future. Here, a comparison of competition over time, holding region (and therefore party organization) constant, may provide some answers.
Normative Implications

Throughout this book I have eschewed normative claims about the candidates I study here or about the implications of my theory for representative democracy. In chapters 1 and 2 I sought to distinguish normative theories of how candidates for office should behave from positive theories of how we might actually expect them to behave. As I argue in chapter 1, however, it is possible to understand the genesis of spatial models of candidate competition as a response to normative theories of responsible political parties. Thus, because my theory does posit a role for political parties, it does speak to the prescriptive elements of responsible party theory.

In addition, the candidates whom I consider here are of theoretical interest precisely because they could reasonably expect to lose. Because the vast majority of them did lose, it is difficult to draw normative lessons from their campaigns. These candidates present a paradox because we may well look at the positions they took in their campaigns and conclude that they deserved to lose—they took positions which did not represent the preferences of a majority of their would-be constituents, and most voters, accordingly, voted against them. To claim, based upon these candidates’ commitment to running for office against overwhelming odds, and based upon their often principled adherence to unpopular positions, that they deserved to win would be somewhat unfounded; it would be a normative argument against the positive theory I have outlined here. In fact, it would be nothing more than an expressive statement on my part.

These two claims do lead, however, to several normative results of my theory. First, and most importantly, this theory does provide some grounding for investigating party responsibility. Following Riker’s (1982a, 1982b, 1997) withering normative argument against the responsible party theory, it is evident from the actions of candidates in states which feature ideological, divergent parties that responsible party government is not possible, but that responsible parties are. In other words, a responsible party is a losing party. Conversely, a party which expects to lose has the luxury of taking “responsible” positions. We are able to identify party ideologies in the campaigns of losing candidates, while they are absent in the campaigns of more competitive candidates. This may well have a normative, expressive value—it gives extremists or holders of unpopular views a vehicle for expressing their views, albeit in a losing effort. Those who search for “a choice, not an echo” often need look no further than their own congressional district to find divergent policy views—but they only find them because the outcome of that election is virtually preordained.
Second, one might respond to this finding by seeking to reward such candidates—by, for instance, calling for campaign finance reforms which would remedy the inequalities which exist in congressional campaigns. While there are certainly sound arguments to be made for campaign finance reform, my argument is not necessarily one of them. Campbell (2003) has shown conclusively that the most recent campaign finance reform legislation, the Bipartisan Campaign Reform Act, will have little effect on competition. Even campaign finance reforms that do affect competition would not, according to the logic of the sequential positioning alternatives I propose, yield significantly different types of incumbents. Divergent pairs of positions occur because one candidate has little or no chance of victory. If that candidate’s probability of winning is increased, there is no reason not to expect that candidate to abandon such positions and adopt a more centrist strategy. There is also no reason to expect that the types of ideologically motivated candidates who emerge as long shots would secure their party’s nomination if the expectation was that the race would be competitive. I have argued here that the “quality candidate” measurement developed by Jacobson and others has little bearing upon spatial competition, but the type of candidate that emerges certainly is a major determinant of the type of competition which results. In remedying campaign funding inequalities, we might engender more visible congressional races, and we might well expect greater turnover in Congress. We would not, however, necessarily introduce dramatically new ideas into the policy process. In the end, I would argue, the more we seek to reward divergent, “responsible” party agendas, the more we seek to reward them, the more they will recede from our grasp.

Third, however, one does not need to be elected to office to “represent” voters. As I noted in chapter 1, a median voter scenario predicts that those who hold extreme views, or even views slightly away from the political center, can be left out of government entirely. Few would argue that in the current U.S. system there is not significant variation in the views of elected representatives. Yet the issue positions of congressional challengers, and the similarity or dissimilarity of those positions to those of other elected representatives, can tell us how much of legislative activity corresponds to the merely descriptive traits of ideological or partisan minorities within congressional districts. They can tell us much about the degree to which our legislature does represent ideological minorities within the public. That is, if there are ideas present in the campaigns of congressional challengers which are absent in the discourse of congressional incumbents, we might conclude that significant minorities of the public are not represented in governmental policy deliberation.
To bring this rather theoretical argument into the real world, one might point to the fact that congressional representatives are, on occasion, said to represent constituencies other than their geographical constituency. Mansbridge (2003) refers to this as “surrogate representation.” An African American representative, for instance, might be said to represent the views of African Americans in general, even where those African Americans said to be represented do not reside within his or her congressional district. The same is often said to hold for other racial or ethnic groups, for women, for gays and lesbians, or for religious or ethnic minorities. The same is also said, albeit less frequently, to hold true for members of Congress who espouse particularly extreme ideological sentiments. A particularly liberal member of Congress—a Paul Wellstone or a Ted Kennedy—is often said to represent the views of other liberals nationwide. We cannot conclusively separate out the election-seeking motive of candidates such as these from these candidates’ actual *ex ante* ideal points, but we can do so in the case of many congressional challengers. Matching the positions and issue emphases of particularly liberal congressional challengers to those of incumbents, in this case, can tell us to what degree the policy preferences of the left actually do find representation within Congress. Not only can we identify pure partisan views, but we can also identify particular issues that may or may not find representation within Congress.

To take this argument a step further, one might also hypothesize a particular drawing of legislative districts such that particular ideological views find no representation within government. One might suppose, for instance, that holders of a particular view are divided such that they are a majority in no congressional district, but that they represent a large minority of the constituents of a larger geographic entity. Rural areas of a state, for instance, might be allocated to congressional districts such that they are a minority within each of several congressional districts even though the sum total of rural residents within the state might be equivalent to one or more congressional districts. Such views may not find representation within government, but given the often low costs of mounting a congressional challenge (especially a long-shot challenge that few election-oriented politicians would undertake), such views should show up in the platforms of congressional challengers. The ability to assess such views, the degree to which they are distinct, and the degree to which they find representation within Congress, can tell us much about the representativeness, in both the descriptive and the “action-oriented” senses, of our government. I make no claims to have answered this question here, but I would argue that I have established a groundwork and a theoretical rationale for doing so.
Fourth, and finally, I direct the reader’s attention back to the justifications for the study of congressional challengers—and of political “losers” more generally—that I provide in the introductory chapter. Several of the reasons I provide there for concern with congressional challengers—the fact that they do occasionally win, that they keep incumbents busy, that they can send messages to incumbents, that they can assist other candidates—are difficult to prove empirically. All of these functions are outcome-oriented; that is, they go beyond the campaign itself. Each of these potential reasons proposes a link, however tenuous it may be, between the issues raised in the campaign and the legislative activity of an officeholder, whether that office holder be the challenger himself or herself (following an election victory), the incumbent being challenged, or another, more successful, candidate of the challenger’s party.

It seems easier, however, to simply say that these candidates are here, and that for this reason alone they are worthy of understanding. As I noted in chapter 1, political theorists have sought to provide justifications for political participation for its own sake, irrespective of its instrumental consequences. These candidates provide an example of such participation. I certainly saw much of this in my research. During one interview, our conversation was frequently interrupted as the candidate’s neighbors stopped by to drop off checks intended to help defray campaign costs; another interview took place following a sparsely attended yet boisterous meeting of the Butler County, Ohio, Democratic Party; and still another took place at the annual convention of the Democratic Socialists. Each of these interviews featured candidates who had been defeated handily, yet the enthusiastic reception these candidates received (after their election defeat) from neighbors, friends, and colleagues was proof that these candidates and their allies were enthusiastic political participants, not single-minded seekers of election. Such participation may be said to provide a channeling of dissent for disenchanted ideological minorities, a voice for the unrepresented, a legitimization of minority views, or merely an opportunity to exercise free speech. We cannot prove empirically that these candidacies served any purpose, but the observations I have made here seem to provide evidence that they fulfill this function.

In a similar manner, academics and nonacademics of all political viewpoints frequently hold competition to be a good in and of itself in American life. It is possible to argue that competition is valuable without overly concerning ourselves with the ideas placed into the competition. In Federalist #53, James Madison argues forcefully that the heterogeneity of economic and cultural circumstances within the United States ought to be fully represented within the American Congress (Madison 1961 [1787],
333). Investigating the campaigns of nonincumbent candidates is, as I argue above, a crucial means of verifying whether or not this is so. According to the argument I have presented here, expressions of such heterogeneity only occur, again, where the competition is particularly unbalanced.

Another early theorist of American democracy, Thomas Jefferson, might be seen as arguing that even in such cases the ideas presented are still worthy of study, that they make us all stronger even where we do not share them. Writing on the necessity of tolerance of and freedom for minority religious viewpoints in the early American Republic, Jefferson argues that “It does me no injury for my neighbour to say that there are twenty gods, or no god. It neither picks my pocket or breaks my leg. . . . Truth can stand by itself. Why subject it to coercion? To produce uniformity. But is uniformity of opinion desirable? No more than of face and stature” (Jefferson 1999 [1785], 165–66).

Jefferson’s argument can easily be read as an argument for the introduction of diverse viewpoints into political debate. These ideas can inform us, can shape our own thinking, even where they are not successful at the ballot box. At the very least, they can make us question our own opinions, even if we are only to reaffirm these same opinions after thinking about them. No more powerful argument for the importance of candidate divergence need be made. In this book I have argued that competitive elections and governmental behavior are not the correct place to look for divergent, contrasting views. This does not mean that such views do not, or should not, exist in American elections. It is easy to argue that our public policy is ill-served by the advantage incumbents hold in American elections. It is harder to see virtue in the same phenomena. I would not argue that my theory provides a rationale for identifying virtue in such circumstances, but I would argue that it provides a rationale for applying more careful scrutiny to both candidates in such elections and, in the end, for seeking to understand the rational motivations behind the campaigns of candidates even where these campaigns are unlikely to end in victory.