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CHAPTER 1

Parallel Histories: The Incumbency Advantage and Electoral Competition

Why is it that we know so little about congressional challengers, about what prompts them to run against seemingly insurmountable odds, or about what precisely they do during their campaigns? In part, we have so little understanding of their campaigns because we have never developed any particular expectations for them. In order to understand the predicament of today’s congressional candidates, and the state of our knowledge of these candidates, two parallel histories are required. First, it is necessary to identify the root causes of the decline of closely contested elections in the United States and the rise of the incumbency advantage in House of Representatives elections. Second, it is necessary to look at the state of political scientists’ theories of ideological competition in elections—theories, depending on one’s point of view, of how candidates do compete in elections or of how rational candidates should compete if they aspire to win office.

These two somewhat parallel developments in the descriptive and theoretical political science literature have pulled the study of campaign behavior in opposite directions. Neither literature truly accounts for the political give and take of campaigns. In the first account, the political actions of the winner are virtually predetermined and those of the loser are irrelevant. In the second, the actions of both are predetermined. In neither case is there allowance for the conflict of ideas, for ideological argument, or for the sheer unpredictability that for many democratic theorists defines politics. Both accounts see candidates as responding in predictable ways to their political environment, but neither sees them shaping that environment. In this book I seek to define circumstances in which candidates for
office can be expected to engage in political expression, to participate in politics out of ideological conviction rather than solely out of a desire to attain elected office. If they do not, in fact, shape their environment, at the least they attempt to do so. In short, I discuss circumstances where candidates are citizens engaging in a form of political participation. Before I do so, however, let us look more closely at political scientists’ expectations about electoral competition.

The Growth of the Incumbency Advantage in Congressional Elections

First, let us consider a few facts about congressional elections. As figure 1.1 shows, incumbent candidates for the House tend to win over 90 percent of the time; notwithstanding such recent watershed years such as 1992 and 1994, only twice since 1948 have more than 10 percent of incumbents been defeated in the general election (Ornstein, Mann, and Malbin 2002, 69). Furthermore, incumbents do not just win reelection, they win comfortably; in 2000, fewer than forty House incumbents received less than 55 percent of the vote. Incumbents tend to represent districts that are drawn to their advantage—in every redistricting cycle, the “incumbent protection gerrymander” is common. They tend to outraise their opponents by tremendous margins—in 2000, the median incumbent outraised his opponent by a twelve-to-one margin ($618,718 to $51,418). Incumbents have numerous perks of office—free mailings, free news coverage, substantial office and travel budgets, to name a few—which enable them to help their constituents, to find out what beliefs voters have and what issues are important to them, and to shape the voters’ perceptions of what incumbents have done to address these concerns. The incumbency advantage is the major obstacle to almost every aspiring member of Congress, and the incumbent’s status, more than anything else, dictates how a congressional challenger will campaign.

All of these facts, however, say little about the content of campaigns—they say little about how the policy proposals and positions of either incumbents or challengers will be framed in a campaign. In order for incumbents to have an advantage in ideological or issue competition with their opponents, we might posit that incumbents must be as good as or better than their opponents at assessing voter preferences, communicating their issue positions to these voters, and assessing different outcome scenarios. The advantage incumbents hold in these areas may be simply described as an advantage in resources. They have the resources to find out what voters want—or, perhaps, to seek to persuade voters that the myriad
votes they have taken in Congress do reflect the voters’ somewhat amorphous views on what they want from government. These resources can be construed in a number of different ways—in terms of money, ability to secure benefits for the district, ability to communicate with voters, and so forth—but essentially, incumbents’ resources are means of gathering information from voters and providing information to voters.

Political scientists have long debated the nature and extent of the incumbency advantage. Many of these studies have sought to isolate particular characteristics of this advantage. A very partial list of the different aspects of the incumbency advantage that have been studied includes consideration of changes in the size of the advantage over time (Erikson 1971; Fiorina 1989), its connection with positions (Erikson and Wright 1989), its connection with campaign communication (Coleman 2000), its relationship to fundraising and PACs (Ansolabehere and Snyder 2001; Alford and Brady 1989), its connection with voters’ partisanship (Garand and Gross 1983; Coates 1995; King and Gelman 1991), and its relationship to the drawing of congressional districts (Cox and Katz 1996).

These studies have concurred on three issues. First, they agree that while the incumbency advantage has grown over the past three decades,
the variability in incumbents’ vote percentage has also increased. Thus, while the average incumbent may get a boost simply by virtue of incumbency, volatility in incumbents’ vote margins ensures that incumbents ought not to think themselves safe. Second, following Mayhew’s (1974, 5) “single-minded seeker of reelection” paradigm, we should expect incumbents to be risk-averse. There is little evidence that incumbents can systematically disguise unpopular positions, and therefore we should expect even the most secure incumbents to take positions preferred by the majority of voters. It is not competition that matters for these incumbents, it is the implied threat of competition. We cannot separate incumbents’ advantage in resources from their selection of popular positions. And third, looking solely at incumbents’ margins of reelection may lead us to underestimate the incumbency advantage because we cannot be certain that the same challengers would emerge in the absence of significant advantages in resources. The direct effects of incumbency create an indirect “scare-off” effect, where qualified potential opponents decide not to run, and weaker challengers result (see Jacobson 1997a, 1998).

The advantage incumbents hold is not necessarily a normative issue—none of the studies of the incumbency advantage have posited an optimal reelection rate. From a “good policy” perspective, it would not necessarily seem wrong for incumbents to be reelected if they do, in fact, accurately represent the views of their constituents. Whether they do this or not is more difficult to demonstrate, but studies of congressional behavior have found few reasons why incumbents should not strive to represent the views of the voters. One component of the incumbency advantage, then, is that incumbents have the ability and the motivation to represent the views of their constituents. In the majority of races, we should see incumbents who know what the voters want and have sought to take positions that a majority of voters support.

What Is a Challenger to Do?

All of this research leaves us, however, with little understanding of challengers. As figure 1.2 shows, despite allegations of a growing incumbency advantage, challengers to most incumbents still emerge—in fact, the number of unopposed incumbents has declined in the past decade and reached a modern-day low in 1996. Faced with such an uphill climb to overcome the myriad advantages incumbents hold, what characteristics might we expect challengers, particularly long-shot challengers, to have? Should they be systematically different from incumbents? How should the incumbency
advantage affect the issue positions and the ideological tendencies of non-incumbents?

Challengers are far more difficult to categorize than are incumbents. Incumbents are easily identifiable—a roster of members of Congress is all that is necessary—and in the American system it is a convenient assumption that in any given year most incumbents will seek reelection. They will seek to perform so as to keep their jobs. The universe of challengers, however, is somewhat more murky because one must consider not only those individuals who do run but allow for the fact that there are numerous potential challengers waiting in the wings. If a potential challenger chooses not to run for Congress, this decision is hard to study—we are not necessarily able to analyze the ways in which individual citizens assess their chances of winning an election were they to run, for the simple reason that we cannot identify all of these citizens. Stone, Maisel, and Maestas (1998) have begun a major systematic study of the variables which affect potential challengers’ decisions to run or not to run. Such studies have also been conducted on a smaller scale by Kazee (1992) and Fowler and McClure (1989). A major fact that emerges in each of these studies, however, is that more politically experienced potential challengers tend to wait for elections where they do have a realistic chance of victory. The field is thus often left to individuals who are not professional politicians and who we should not expect to share the motivations of election-seeking politicians.\(^2\)

The task, then, is to discern what their motivations might be and how such motivations affect their position taking.
Many existing studies of challengers provide descriptive data on those who actually do run—their level of political experience; their age, income, gender, and so forth. They are about who runs, not how challengers run. Among the few studies that investigate the strategies of challengers, one of the earliest and best is John Kingdon’s (1966) study of congressional and state legislative candidates in Wisconsin. Kingdon concludes that almost half (48 percent) of these candidates were certain of the outcome of their campaigns before Election Day (Kingdon 1966, 86–88). Among decisive winners and losers, 56 percent claimed to be certain of the outcome. Despite this finding, however, Kingdon spends much of his book analyzing misinformation that candidates received—he explores their tendency to rationalize election outcomes by arguing that voters did not absorb the message they attempted to send them. Kingdon also explores a “congratulation” effect, in which candidates rationalize their campaigns by reasoning that they had an effect upon the election insofar as they stimulated voter interest, raised important issues that would otherwise not have been brought up, and sent a message to the incumbent about the salience of these issues (Kingdon 1966, 23–25).

Most importantly for this study, Kingdon finds high percentages among both the most conservative (60 percent) and most liberal (42 percent) candidates who claim to have adopted their issue strategies out of concern only about the importance of those issues, independent of electoral concerns. Such a finding, while it still leaves out almost half of the candidates running, does indicate that significant percentages of nonincumbent candidates consciously stay away from the political center. Kingdon (1966, 133) does differentiate secure and “hopeless” candidates somewhat, noting that the long-shot candidates “tend to construct an undifferentiated appeal rather than an appeal to particular groups within their district. . . . The hopeless candidates, confident that they will lose, decide to ‘speak their minds’ instead of attempting to expand their base of support.”

Subsequent studies, including those of Leuthold (1968), Huckshorn and Spencer (1971), Fishel (1973), and Hershey (1974, 1984) pick up more on Kingdon’s analysis of candidates’ sources of error and misinformation than they do on this latter assertion. In his study of candidates in ten San Francisco area districts in 1960 and 1962, Leuthold (1968, 60) notes that more competitive candidates spent more time acquiring information about public attitudes on particular issues than did less competitive ones, but he does not indicate whether this information led to different types of ideological appeals or whether it was merely a function of campaign resources. Huckshorn and Spencer’s (1971) analysis of a written survey of 1962 challengers, aptly titled The Politics of Defeat, concludes that most candidates
report that their issue positions made little difference in the election outcome. Perhaps as a result of this, Huckshorn and Spencer reiterate Kingdon’s finding that those who believed they were likely to lose ran more issue-oriented campaigns than those who did not, and that candidates in more marginal districts underplayed ideology and instead emphasized experience and ability to help constituents in particular, narrow areas (Huckshorn and Spencer 1971, 208–12). And Maisel’s (1986) study, which employs a written survey of congressional primary candidates, again focuses on the question of why people choose to run for Congress; he notes that the actual issue positions of candidates are deemed by the candidates not to matter in terms of the primary or general election outcome. He thus limits his consideration of issues to noting that challenges to incumbents are more issue-oriented than are open-seat campaigns (Maisel 1986, 84–85).

Maisel enumerates, however, what have come to be the standard reasons for candidate emergence—for why challengers run. In addition to the prospect of victory, where there is one, he notes that candidates frequently referred to providing an alternative to the incumbent, advancing their own careers through the visibility a congressional run would afford them, building a base for future campaigns, and having a platform to air their own political views (Maisel 1986, 24). Subsequent research, such as the case studies of Kazee (1994), Fowler (1993), and Fowler and McClure (1989), has built upon this typology. The study of 1964 candidates by Fishel (1973) and Kazee’s (1980) study of twenty-five candidates in the 1978 elections have also contributed to identification of these factors as motivations for candidate emergence.

Within the scope of broader research on congressional candidates, Jacobson’s (1990, 57–65) now familiar candidate quality measurements, which differentiate candidates by degree of past electoral experience, have become a standard means of gauging the degree of threat to incumbents, and they have also been used to explain incumbents’ vote margins. Jacobson himself does not use his simple binary measure of whether a candidate has previously held elective office or not to distinguish types of challenger traits or strategies, yet others have begun to use his distinctions and to draw even further distinctions (see, for instance, Gerber 1998 on Senate challengers). Herrnson (2000) has conducted a thorough study of campaign strategy, finding no systematic difference in issue strategies according to candidates’ prior experience, but he does reiterate the finding that challengers run more issue-oriented campaigns than do incumbents. This is, Herrnson argues, somewhat counterintuitive within an ideological competition framework because one might expect incumbents to have more resources at their disposal which can be used to speak authoritatively on policy issues (Herrnson 2000, 194–95). In other words,
incumbents must be rationally steering the campaign away from substantive discussions of policy, while challengers are making an effort to learn enough about policy to argue with a reluctant incumbent.

By far the most systematic study of challengers’ motivations for running, however, is that of Canon (1990, 1993). Canon subdivides political amateurs—challengers without prior legislative experience—into “ambitious” and “experience-seeking” amateurs. Canon further divides the experience-seeking amateurs into policy amateurs and hopeless amateurs (Canon 1990, 25–32). The bulk of his attention is devoted to what amateurs do when elected—do they adapt to the election-seeking environment of Congress, or do they maintain their outsider status? The resolution to this question depends upon the costs of their candidacy and the value they place upon maintaining their seat as opposed to advancing policy goals. This work contributes greatly toward illuminating the rational reasons candidates may have for running apart from an election-seeking orientation, and it identifies a class of candidates, and even of incumbents, who problematize the “single-minded seekers of election” paradigm.

The literature on the incumbency advantage and the literature on congressional challengers thus coexist with scarcely any mention of patterns of issue competition between incumbents and challengers. In virtually all literature on congressional challengers, the difficulty, even the futility, of challenging an incumbent is taken as a given. In the literature on the incumbency advantage, there is frequent discussion of the effects such an advantage has on the quality of the competition, yet there is little attention given to the ideological nature of the competition. The literature on congressional challengers provides some clues, most notably in the assertions of Kingdon, Huckshorn and Spencer, and Herrnson that noncentrist issue positions are taken more frequently by long-shot challengers than by more competitive candidates. Canon’s typology of amateur candidates provides the groundwork for arguing that such positions are, in fact, rational responses to severely circumscribed chances of election.

Most studies of challengers, however, downplay the effect of campaign issues. It may well be true that, as the candidates interviewed by Maisel and by Huckshorn and Spencer argue, issue positions play a relatively minor role in determining these election outcomes. Yet issue positions clearly must be taken by these candidates. They will be asked in campaign appearances what they believe should be done about a particular policy problem, and they must have something to talk about in the campaign appearances they
make. In addition, many of these candidates became involved in running uphill battles for Congress precisely because they have strong beliefs about issues which are not being addressed. It is difficult to argue that a substantial proportion of candidates are sufficiently motivated by their issue beliefs to run for office but that these issues beliefs eventually do not matter. These beliefs may not determine the outcome of the race, but they do determine the type of information voters receive about politics and the nature of the choices voters receive. To look for such beliefs, though, we must enter an entirely different realm of political theory, the realm of ideological competition and theories of rational issue positioning.

A Brief History of the Median Voter

In order to bring about a comparison between the actual ideological strategies of candidates and the idealized notion of issue competition in elections, it is necessary to consider the origins of rational choice theories of campaigning and their most prominent exemplar, the spatial theory or median voter model of competition. It is important to ask, that is, whether these models shed light upon congressional elections. And if so (or if not), why have they gained such prominence within political science? The limitations of these theories—and the reasons why they seem so inaccurate when applied to the vast majority of today’s elections—are, I contend, rooted not in the idea of making issue competition intelligible but in a small number of the assumptions about issue competition that undergird these theories. Whereas the concepts behind these theories—their so-called scientific aspects—purport to stand apart from historical trends, I contend that it is the very rootedness of these theories in outdated assumptions about elections that make it necessary to amend them if they are to have any use for studying today’s elections.

The rise of spatial models of party competition closed off an area of heated debate in the study of American political parties. Since the late nineteenth century, political science has been punctuated by periods of spirited debate about the nature of our two-party system. Much of this debate culminated in a 1950 report by the American Political Science Association (APSA) calling for a “more responsible two party system,” one in which the two parties would offer coherent, yet divergent, packages of policy proposals to the public, which could then make an informed choice about the direction in which it wished American public policy to go.

Anthony Downs’s *An Economic Theory of Democracy*, published in 1957, extended the concept of market competition to its logical political
conclusion, and at the same time it exposed the inconsistency of pairing an economic conception of political parties with normative calls for the parties to espouse contrasting viewpoints and to design long-range plans that would “cope with the great problems of modern government” (American Political Science Association 1950). Employing Hotelling’s (1929) theory of locational or spatial economic competition, Downs posited that a rational political party would, in two-party competition, seek out an ideological position at the middle of the electorate’s preference distribution, just as competing entrepreneurs would, in Hotelling’s argument, rationally seek to locate their businesses side-by-side. The two parties would then, under full information conditions, mimic each other, thus encouraging voters to make decisions not about policy, but about nonissue traits of the parties. Subsequent research has elaborated upon many of Downs’s points, arguing that parties will be ambiguous about their positions on controversial issues (Shepsle 1972), will incorporate seemingly incompatible positions into their platforms (see Hinich and Munger 1994, 77–79), and will seek to avoid long-run solutions to problems in order to maximize their present electoral fortunes (Tufte 1978; Hibbs 1987). This economic conception rendered normative arguments such as those of the APSA report somewhat moot; the fault, if there was one, lay with the median voter himself, and no amount of exhortation by an elite cadre of political scientists would sway the parties from their course.

In a laudatory review of Downs’s book in 1958, Charles Lindblom hailed the economic theory as a cure to what ailed political science:

While economists have made the most of a seriously defective system, political scientists have permitted a kind of perfectionism to inhibit serious, explicit system-building. In talking with political scientists I am often struck by their dissatisfaction with theoretical proposals that do not promise a rough fit to the phenomena to be explained, while economists have happily elaborated, to take one example, a theory of the firm that is still a caricature of the phenomena described. (Lindblom 1958, 241)

The division between Lindblom’s economists and political scientists still stands within the discipline. Rational choice modeling of electoral competition has, despite debate over its relevance to discussing actual elections, become an established part of political science and a standard shorthand for describing ideological conflict. The history of applications of these models to actual elections, however, is short, a circumstance which has been one of the chief complaints of Lindblom’s political scientists. The median voter
model is presently both one of the most widely accepted frameworks for inquiry among rational choice theorists and one of the most criticized by empirical scholars. The median voter model is under attack on three fronts. Outside the academy, “median voterism” is held in disdain by virtually all political observers and activists. Within political science, it is criticized for its “lack of fit to the phenomena to be explained,” particularly in Green and Shapiro’s (1994) broadside against rational choice theory. Among less quantitative social scientists, its assumption of a relatively atomized, self-interested public often draws fire. That is, it draws both normative criticism and two types of empirical criticism.

On a normative level, one need only open up a newspaper to sense the public dissatisfaction with the perceived ambiguity and lack of distance between America’s two parties. From the right, one need only consider the attacks by the Club for Growth and other right-leaning groups on “RINOs” (“Republicans in Name Only”). From the left, liberal frustration with the Democratic Party is frequently in evidence, most recently in presidential candidate Howard Dean’s claim to speak for “the Democratic wing of the Democratic Party.” Such views may be commonplace among those with strong ideological views, but clearly they are a symptom of widespread sentiment among observers of politics that the two parties do not stand for anything, that they have abandoned their core beliefs, and that this abandonment is not beneficial to the nation. There continues to be conflict between the twin norms of representativeness and choice, but this conflict may have moved out of the academy. Public opinion polls seem to constantly top themselves in showing high levels of public disinterest in the issues of the day, public disgust with elected officials, and public cynicism about politics in general. If the median voter has triumphed within the public discourse, it has been at the expense of the public.

On a second, more applied level, quantitative political scientists have eschewed directly criticizing the median voter model’s normative implications. The idea that politicians do behave instrumentally, at some minimal level, is fairly uncontroversial within political science, but why, the critics ask, have applications of economic models been so scarce? The median voter model was intended to be a general theory of elections, yet it has been successfully applied only to isolated, often anomalous elections, or to voting in elite bodies such as legislative committees. Despite the praise of those in political science who would be system builders, many contemporary researchers were skeptical. Stokes (1963) was among the first critics, arguing that candidates’ issue positions play a relatively minor role in voter decisions. Other criticisms rooted in behavioral studies have been added since; the informational aspects of the model, the ideological mobility of parties, the unidimensionality of the
model, and the lack of enforcement mechanisms are just a handful of the problems identified in Green and Shapiro’s (1994) survey. Formal modelers have defended the theory by pointing to general studies such as Fenno (1978), Mayhew (1974), and Page (1978), but these studies, while they have been influential in guiding empirical research, are primarily qualitative, “soak and poke” research, not the highly mathematical papers at which the critics of spatial modeling have taken aim. Some formal theorists, most notably Sartori (1976) and Robertson (1976), have sought to correct some of the institutional factors in the Downs model, allowing for the uneven playing field to which I have referred, but their alternate proposals have, like the Downs model, suffered from limited empirical applications.

A third critique, one which blurs the lines between the descriptive and the normative, comes from theorists of political power. The median voter model rests upon assumptions about the nature of government and the nature of majority rule not particularly distant from the work of pluralist theorists such as Dahl (1961) and Lindblom (1977). As befits a model that aspires to a relatively uncomplicated assessment of the quest to capture majority preferences, the Downsian conception of majority rule is relatively straightforward: “Control over government decisions is shared so that the preferences of no one citizen are weighted more heavily than the preferences of any other one citizen” (Downs 1957, 32). Should this crucial presumption actually prove to be true, spatial modeling would be far more applicable to empirical reality than it has proven to be. Where this is not the case, however, numerous alternate candidate strategies and goals can be hypothesized.

This criticism challenges the very framework of the median-voter model in that it disputes the model’s definition of the parties as office-seeking teams. In the economic model, the relationship between the parties is quite simple—one party has power, the other wants it. Bachrach and Baratz (1962) and Gaventa (1980) dispute this assertion. In a representative government, no party can be said to be completely without power. One party may hold more power than the other, but neither is completely without a voice. There may be a degree of collusion between the parties to keep certain issues, issues which fit into neither party’s ideology, off of the agenda. Or (a more mild formulation), because both parties are successful in some parts of the country, or with some constituencies, they may be sufficiently risk-averse to shy away from formulating new issues (Klingemann, Hofferbert, and Budge 1994, 11–12, 28). It is only the desperation of almost certain impending defeat, the chaos of social upheaval (see Page 1978, 62–107), or a lack of more risk-averse voices within the party that bring about new issue stances by parties. In comparison to these events,
the pluralist approach comes off as particularly staid. In the pluralist one-dimensional approach, argues Gaventa, all grievances are recognized and acted upon, and all may participate in decision making. In this conception, even centrist voters may not be given choices which represent their “true” ideology. There is no vehicle for protest against the status quo.

These theoretical arguments about the assumptions of the median voter model ultimately have significant import for the study of ideological competition in elections. If one is to dispute the model on empirical grounds, one’s argument must necessarily fall into one of two camps: first, that voters do not choose candidates as the model presumes them to, based upon their own ideological proximity to the candidates; or second, that candidates do not take the positions we would expect them to, based upon their knowledge of or beliefs about the electorate. It is difficult to disaggregate these arguments, and attempts to do so will inevitably take on a “chicken or the egg” quality: if voters do not vote ideologically, and candidates know this, candidates will not take the positions they would otherwise take. If candidates do not take these positions, however, it becomes difficult to ensure that voters are not choosing based upon ideological positions, because we do not have the convergence that would otherwise be predicted. What matters here is not how voters make their choices, but how candidates believe voters make their choices. Regardless of whether voters will consider ideology in making their voting choices, candidates must still take ideological positions. The relevant question, then, is whether candidates do have incentives to take a median position; and if not, what other incentives exist for the candidates. How do disparities in the electoral environment such as those I documented earlier in this chapter affect candidate incentives?

Are Candidates Rational Choice Theorists (Or, Again, What Is a Challenger to Do?)

The goal of this book, then, is to ask what a particular class of candidates for office—congressional challengers and nonincumbents—are thinking when they make strategic decisions regarding their ideological pronouncements or issue positions. On a basic level, the question is not whether candidates make mathematical calculations about where the median voter lies. The question is whether candidates are behaving “rationally,” are making logical decisions given the various factors surrounding their campaigns that are out of their control. It would seem preposterous to argue that they are doing nothing of the sort, despite their often extreme or unusual issue
positions. Many of the basic tenets of the theories we have about how ideological competition proceeds in elections seem logical, and the internal consistency of these theories has been worked out in great detail. Furthermore, the sheer comprehensiveness of these theories is unrivaled by any other theory of competition. Why is it, then, that the rough-and-tumble world of election politics seems so far removed from this stylized model? Are these two directions in our study of elections, the descriptive and the theoretical, mutually incompatible?

In the next chapter I argue that there is much we can learn from basic theoretical notions of issue competition that can shed light on the dynamics of issue competition in the context of congressional elections that betray a pronounced pro-incumbent bias. I argue that an overriding desire for political expression is the only explanation for the nature of ideological competition in such campaigns. In the remainder of this chapter, however, let us briefly try to assess why congressional challengers fall between the cracks of both of the streams of literature I have discussed here and why it is important to find a place for them within both literatures. In the introduction to this book, I argued that congressional challengers are an important subject of study because of what their campaigns can tell us about the state of American democracy. Here, since I have recapped the absence of serious analysis within political science of how congressional challengers campaign, let us briefly consider what a knowledge of the logic behind the challengers campaign can add to political science beyond merely filling a gap in our existing literature.

One crude, but enduring, distinction made in political science is between the study of political behavior and the study of governmental institutions. Elections are one of many areas in which citizens’ political activities influence governing institutions, yet at times we are slow to incorporate concepts within the study of political behavior into our study of the actions of those who are part of, or who aspire to be part of, our government. The candidates I study here, precisely because of their low probability of victory, can thus be understood as being similar to other nongovernmental political activists and as representing something, even if what they represent is not the viewpoint of the majority.

If we concede that the majority of congressional challengers know, at a minimum, that they face extremely long odds against winning the elections in which they run, we are, in a sense, freeing them from the dictates of what we expect politicians to do. Most challengers have no political experience (if we define experience as having held office) prior to their campaigns, and most will have no such experience after their campaigns. Literature on political participation rarely lists running for office as a form of participation, but
surely few students of political participation would deny that running for office is on the same continuum of political participation as voting, working for someone else’s campaign, contributing money to candidates, and so forth. Most importantly for the purposes of my argument here, however, few studies of political participation have limited their discussions of why people participate in politics to the likelihood of achieving one’s preferred outcome. That is, citizens work toward a wide range of hopeless causes in part because they wish to express their own beliefs about what is right. A desire to express one’s views has been held to be an important component of voting (see Schuessler 2000), contributing money to candidates (Brown, Powell, and Wilcox 1995, 45–48), running as an independent or minor-party candidate (Collet and Wattenberg 1999), and, in studies too numerous to mention, of more overtly expressive activities such as writing letters to members of Congress, participating in rallies, and so on. There is abundant evidence to suggest that such an expressive desire can take a backseat in the campaign behavior of elected officials, but there is no reason to exclude expressive behavior from studies of those who run for office as major-party candidates, particularly those who cannot realistically expect to win. And most importantly, one can agree with the findings of literature on congressional elections and one can agree with the basic tenets of rational choice theories of campaigning without denying the place of political expression within our system.

These candidates, then, can be seen as political participants. In addition, however, even if the candidates I study here are unlikely to ever become the elected representatives of their district, they are representatives in another sense. They do receive some votes, and thus the statements they make in their campaigns must be said to represent the views of some citizens. Above, I outlined one major criticism of the pluralist model of politics, a model from which, I contend, positive theories derive some inspiration. Critics of the pluralist conception of power have argued that ideas shared by significant minorities of the public tend to be left off of the agenda in American politics. Indeed, full candidate convergence would do just this; in a median voter scenario, it truly does not matter which party wins an election, or which voters—those on the left or those on the right—put the winning candidate in office. Policy views held by those on the extremes of the ideological spectrum tend to be left out of competitive campaigns. Again, rather paradoxically, the unbalanced nature of so many congressional campaigns means that those candidates who represent the preferences of particularly strong partisans are able to do so precisely because they have no rational expectation of attaining office.

In her study of conceptions of representation, Hanna Fenichel Pitkin describes just such a function. She notes that for Downs, representatives,
in the elected sense, are, to quote Downs, “specialists in discovering, transmitting, and analyzing public opinion” (Pitkin 1967, 83, quoting Downs 1957, 89). She goes on, however, to note that for a government to be representative of the public, it must contain a representative of “every worthwhile opinion.” The judgment of which opinions are “worthwhile” is, of course, subjective. The crucial distinction between elected representatives and others, those who are descriptively representative, is that elected representatives represent through activity. They deliberate over the enactment of laws, while descriptive representatives merely “stand for.” Yet the representation that occurs in these elections, no matter how fleeting, provides a link between contemporary congressional elections and the normative calls by theorists such as Shklar (1991) and Fishkin (1997, 44) for the inclusion of as many views as possible in political discourse.

Finally, the broader scope of ideological conflict that can occur in such races—or at least, might occur were these candidates to receive a fuller airing of their views—can be exciting. For voters, it has been shown that campaign dialogue strengthens voters’ sense of engagement in politics and increases their political knowledge (see Simon 2002). In addition, however, one should never underestimate the role of excitement or conflict even within dispassionate academic inquiry. Hannah Arendt’s *The Human Condition*, published one year after *An Economic Theory of Democracy*, decried the absence of political speech from contemporary American life. For Arendt (1958, 5, 323–24), the entire enterprise of quantitative social science had begun to prosper precisely because mass human behavior had become so predictable—and for her, predictable behavior was antithetical to political expression. The presence of forms of political expression that are not solely aimed at achieving electoral office would seem to be a dose of unpredictability to our political system. As I argue in the following chapters, such expression may be doomed to reside at the fringes of our system, but the fact that it exists at all (and that such expression is hardly irrational for some political actors) may provide some consolation to the normative theorist.

In the next chapter I explore the place of political expression within rational choice theories of campaigning—that is, I establish theoretical reasons to look for such behavior. In the subsequent chapters I describe the forms in which such behavior does exist in American congressional elections.