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House and Senate Elections

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Abstract and Keywords

Research on U.S. congressional elections has made remarkable progress in the past four decades. Scholars of congressional politics for years have studied elections in Congress and the Senate, focusing attention on questions ranging from incumbency advantage in Congress to why turnout voter is lower in midterm elections. However, there are still a number of unanswered questions concerning House and Senate elections. This article evaluates the state of research on U.S. House and Senate elections. It examines the central themes that have permeated the literature on congressional elections since the early 1970s. Prior to the 1970s, congressional elections were rarely mentioned in research on Congress. However, that trend changed dramatically when Mayhew noticed the decline in incumbent marginality in his research which examined House elections from 1956 to 1972. With the exponential growth on congressional elections scholarship, this article focuses on some of the more important questions in the field. It also discusses the implications for research on House and Senate elections.

Keywords: House elections, Senate elections, congressional elections, elections, congressional elections scholarship, research, U.S. congressional elections, Congress, Senate

RESEARCH on U.S. congressional elections, and House elections in particular, has made remarkable progress in the past four decades. Starting with Mayhew's (1974a) observation that members of Congress are "single-minded seekers of reelection," students of congressional politics have spent countless hours investigating a wide variety of puzzles generated by elections to the House and Senate. A considerable amount of attention has focused on questions ranging from the sources of the incumbency advantage in Congress to why voter turnout is so much lower in midterm elections. Although much has been learned in the past few decades from this research on congressional elections, there are still a number of unanswered questions that, in our view, have received insufficient attention to date. This is unfortunate, as elections go to

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the very heart of accountability and representation, and an insufficient understanding of how they are conducted—as well as of their direct and indirect effects—limits our ability to generalize from them.

This chapter seeks to assess the state of research on U.S. House and Senate elections, most notably by examining a few central themes that have permeated the literature on congressional elections since the early 1970s. Prior to that time, congressional elections were rarely mentioned in research on Congress, which instead was largely focused on congressional institutions. However, that changed quite dramatically approximately forty years ago, when Mayhew (1974b) first noticed the decline in (p. 142) incumbent marginality in his research examining House elections from 1956 to 1972. In light of the exponential growth in the amount of scholarship on congressional elections during the past few decades, it would be impossible to focus on all facets of this research in a single chapter. Instead, we seek to examine what we have determined to be some of the more important questions in the field to provide the necessary context in evaluating where we are in answering them. From there, we conclude by discussing the implications for research on House and Senate elections in terms of where we should go from here.

The electoral connection

Since it was first published in 1974, David Mayhew's book, *Congress: The Electoral Connection*, has had a profound impact on the way congressional scholars have analyzed both legislative behavior and congressional organization.¹ In his book, Mayhew offers a theoretically rich, coherent, and provocative argument as to why members of Congress—House members in particular—behave the way they do. In attempting to build a general theory of congressional organization and behavior, Mayhew begins with a very simple assumption: members of Congress are “single-minded seekers of reelection” (1974a, 5). He explains that this assumption is largely motivated by his observations of legislators while working on Capitol Hill. Moreover, he explains that an emphasis on the reelection motivation is attractive for a number of reasons. First, he contends that it fits political reality well. Additionally, “it puts the spotlight directly on men rather than on parties and pressure groups, which in the past have often entered discussions of American politics as analytic phantoms” (6). Mayhew also believes that politics is best studied as a struggle between individuals to gain and maintain power. Finally, and perhaps most importantly to Mayhew, “the reelection quest establishes an accountability relationship with an electorate, and any serious thinking about democratic theory has to give a central place to the question of accountability” (6).

Mayhew contends that it is one thing to assume that legislators are single-minded seekers of reelection, but quite another to assume that they are in a position to necessarily achieve this goal. Indeed, Mayhew argues that members can and must constantly engage in three types of activities related to reelection. The first activity is *advertising*, in which legislators attempt to disseminate their name among constituents to create a favorable image (with little or no issue content) among the electorate (49). This is known as creating a brand name that constituents can identify with. Second, members engage in what Mayhew refers to as *credit claiming*, a practice (p. 143) whereby legislators try to convince constituents that they are responsible for causing something positive to occur.² Whether it involves providing distributive benefits or casework, voters become accustomed to the variety of services that legislators can provide on their behalf, thus increasing their chances of getting reelected. Finally, Mayhew argues that representatives engage in *position taking*, defined as a public statement on a subject of potential interest to the public (position taking can be in the form of speech, press release, or even a roll-call vote). Here the position is the political commodity; members act more as speakers than as doers.

In numerous ways, the distinctions between advertising, credit claiming, and position taking have proven invaluable for scholars seeking to build upon Mayhew's analysis, especially with regard to the electoral implications of member behavior. In exploring issues associated with the incumbency advantage, for instance, scholars such as Cover and Brumberg (1982) have explored whether incumbents received electoral benefits from increasing their levels of name recognition. Additionally, Fiorina (1977), Bickers and Stein

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(1997), Alvarez and Saving (1997), Sellers (1997), Lee (1998, 2003), and Bovitz (2002) have investigated credit claiming in order to seek a better understanding of the electoral rewards that accrue to members for providing distributive benefits to their constituents.³ Finally, Erikson (1971), Wright (1978), Arnold (1990), Jacobson (1993), Jones (2003), and Bovitz and Carson (2006), among others, have explored both the electoral calculus and the implications of position-taking for members of Congress.

Successive generations of scholars have been heavily influenced by Mayhew's work, as many of his ideas have been extended or reevaluated in light of recent events transpiring in the political arena. Despite the extensive literature that has developed in response to Mayhew's portrayal of Congress, in certain respects it has restricted the way in which scholars have analyzed institutional changes and legislative behavior. Although we agree that Mayhew's thought experiment about members acting as "single-minded" seekers of reelection explains a large amount of congressional behavior, the single-goal theories of politics that have emanated from it have been limiting. Assuming that members are primarily reelection seekers diverted attention from the complexity of congressional politics. Armed with Mayhew's statement that parties lacked theoretical importance in explaining congressional activity, legislative scholars largely ignored the resurgence of political parties in the U.S. House for nearly two decades (but see Rohde 1991). To this day, the literature on the role of parties in the U.S. House suffers from early attempts to develop theories and explanations of behavior that emanate from a reelection only motivation (Smith 2007).

(p. 144) It is also the case that the single-goal form of research has led to a disproportionate focus on House elections at the expense of Senate elections. Though the regularity of House elections, combined with the large number of cases, makes House elections an easier target of research, it is also true that institutional features of the House make it fit the single-goal reelection narrative better than the Senate does.⁴ The six-year term undoubtedly frees senators from constantly being in campaign mode; thus other goals—such as public policy and institutional influence—may play a larger role in their activities (on this point, see especially Fenno 1978). Unfortunately, students of legislative behavior have only recently begun focusing on these types of trends in research on the U.S. Senate (see, e.g., Lee 1998; Lee and Oppenheimer 1999; Monroe, Roberts, and Rohde 2008).

One notable exception to the House-centric research on the electoral connection is more recent investigations into how the adoption of direct election in 1913 affected both the Senate as an institution and the type of representation among senators. For several decades, conventional wisdom suggested that passage of the 17th Amendment had little or no impact on the Senate institution. More recently, however, scholars have found evidence that adoption of direct election affected the composition of the Senate chamber (Crook and Hibbing 1997), changes in voting patterns among senators (Bernhard and Sala 2006), and representational styles (Schiller 2006; Gailmard and Jenkins 2009). Additionally, Meinke (2008) suggests that direct election intensified existing electoral

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incentives in the upper chamber, leading to increased patterns of bill sponsorship and participation by senators.

The incumbency advantage

In the congressional elections literature, no single subject has drawn the attention of scholars more than trying to explain and measure the advantages accruing to incumbent members of Congress. Beginning with the work of scholars first recognizing the apparent advantages of incumbency (see Erikson 1971; Mayhew 1974b; Ferejohn 1977; Cover 1977; Fiorina 1977), a variety of explanations were offered to account for the high rates at which incumbent legislators get reelected in the contemporary era. Initially, the incumbency effect was attributed to a wide range of institutional features such as legislative casework (Fiorina 1977), legislative activism (Johannes and McAdams 1981), advertising (Cover and Brumberg 1982), replacement among members (Born 1979; Alford and Hibbing 1981), and redistricting (Erikson 1972; (p. 145) Cover 1977).⁵ Some scholars believe that the advantage can be explained by legislators' personal home styles in their districts (Fenno 1978), rational entry and exit decisions by strategic candidates (Jacobson and Kernell 1981; Krasno 1994; Cox and Katz 1996), a growing 'personal' vote (Cain, Ferejohn, and Fiorina 1987), and a greater emphasis on television appearances in a candidate-centered electoral era (Prior 2007).⁶ Still others place greater emphasis on the role of donations and money in contributing to the incumbency advantage, especially in terms of the increasing costs of House and Senate campaigns (Abramowitz 1989, 1991) and on the growing disparity between incumbents and challengers in terms of their fundraising capabilities (i.e. the "strategic money" thesis as discussed by Jacobson and Kernell 1981).⁷

In their critique of the existing scholarly literature on the incumbency advantage, Gelman and King (1990) rigorously show that the most commonly used measures of the incumbency advantage—such as sophomore surge and retirement slump—produce biased and/or inconsistent estimates of the advantage.⁸ As a result, Gelman and King develop a technique that, they argue, corrects for most of the inherent problems in measuring the incumbency advantage, especially those that rely on traditional measures such as the retirement slump or the sophomore surge. As such, they find evidence of a vote-denominated incumbency advantage as early as the beginning of the twentieth century, and evidence of a large increase in the advantage accruing to incumbents in the mid-1960s.

In drawing upon this new estimation technique established by Gelman and King (1990), Cox and Katz (1996) re-examine why the incumbency advantage grew so sharply in the mid-1960s. Their model of the incumbency advantage is a refined version of the Gelman-King model that allows them to account for the differences in candidate quality between the political parties and for the possibility that incumbents "scare off" high-quality challengers, who might otherwise reduce their margins of victory. While they consider alternative explanations for the growth in the incumbency advantage, they ultimately conclude that the dramatic growth was driven almost entirely by an increase in the quality effect (i.e. the advantage that incumbent legislators received from not facing an

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experienced challenger).⁹ In discussing the (p. 146) implications of their findings, they noted that the challenge for future scholars was to discover why the quality effect had steadily increased over time.

In more recent work, Cox and Katz (2002) provide one of the most comprehensive accounts of the incumbency advantage in the literature to date. Unlike in other treatments of this subject, Cox and Katz find that the regularity of redistricting that resulted from the Supreme Court decision in *Wesberry v Sanders* (1964) drove the large 1960s increase in the incumbency advantage. They find convincing evidence that the combination of liberal-leaning judges and Democratically controlled state legislatures drew new district boundaries, which efficiently spread Democratic voters across districts in a way that maximized the share of seats Democrats could expect to control in the House. At the same time, this also created extremely safe seats for the remaining Republican members.¹⁰

In terms of the incumbency advantage, Cox and Katz offer two distinct conclusions, one short-term and the other long-term. For the former, they hold that the mid-1960s redistricting created a larger incumbency advantage for Republicans, which explains the explosive growth of the incumbency advantage in the 1960s. In terms of the latter, they maintain that the regularity of redistricting following *Wes - berry* created an electoral environment whereby quality challengers and incumbents avoided facing each other as much as possible. In particular, Cox and Katz attribute this decline in the candidate “collision rate” to the strategic entry and exit decisions of incumbents and experienced challengers. These challengers recognize the difficulty inherent in defeating a sitting incumbent, so they wait until she decides not to seek reelection before they decide to run. Likewise, incumbents are apt readers of the proverbial tea-leaves, often opting for retirement when their odds of winning reelection are reduced by scandal, poor fund raising, or the emergence of a high- quality challenger to run against them.

In another innovative analysis of the incumbency advantage, Ansolabehere, Snyder, and Stewart (2000) offer valuable insights into the sources of such advantages, which incumbents retain over non-incumbents. In their study, they employ the quasi- experiment associated with congressional redistricting in the U.S. to determine the extent to which the advantage incumbents enjoy stems from a personal vote (the vote incumbents receive as a result of the “homestyle” legislators maintain with their constituents). More specifically, they compare legislative vote percentages in the old and new territory within redrawn congressional districts controlling for the political inclinations of both sets of voters. All else being equal, they hold that incumbents should do better within the counties of the district that they have represented before, since voters are already familiar with them and their policies, which positively shapes their personal vote (2000, 18). As expected, the authors find that a significant portion of the advantage incumbents retain stems from a legislator's personal vote as opposed to other factors such as candidate quality and an incumbent's ability to (p. 147) scare off experienced challengers. Indeed, Ansolabehere, Snyder, and Stewart conclude that the personal vote comprises anywhere from one-half to two-thirds of the overall incumbency advantage on average.

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This finding was important in that it demonstrated a personal vote for members of the House long before television, easy travel, large staffs, and the widespread use of the franking privilege. In doing so, this paper convincingly demonstrated that these factors could not have been the necessary conditions for the apparent modern-day growth in the incumbency advantage.

In some ways, research on the incumbency advantage in Congress is like the Energizer Bunny—it just keeps going and going. As early as 1982, Doug Arnold declared that scholarship on the incumbency advantage was an “overtilled” field of American politics. Despite this pronouncement, work on the incumbency advantage has continued unabated. Why has this proven to be the case? We believe there are several plausible reasons for this trend. First, the incumbency advantage is an important theoretical concept. The idea that incumbent office-holders have an advantage over their would-be challengers runs afoul of many tenets of democratic theory. From a normative perspective, elections are supposed to be fair and reflect the will of the voters, thus determining why incumbents apparently do so well at the polls is inherently important. If incumbent legislators are gaining an electoral advantage due to the rules of the game being skewed in their favor, then this would be especially problematic and would require some form of change. However, if legislators are securing high rates of reelection as a result of being exemplary representatives, then we should applaud them and the voters.

Second, since congressional elections occur every two years, new data in the form of electoral outcomes are constantly being generated. As such, our election data-sets become even richer, and we are in a position to develop more accurate measurements of advantages that appear to be accruing to incumbents. Moreover, events occur in subsequent elections that allow critical tests of concepts related to incumbency. The House banking scandal in the early 1990s, for instance, is a prominent example, which allowed legislative scholars to examine how charges of corruption affected a member's reelection chances (Groseclose and Krehbiel 1994; Jacobson and Dimock 1994). The first two years of the Clinton presidency also provided an opportunity to examine the effects of voting against the view of one's congressional district and the impact such behavior had on members' electoral fortunes (CanesWrone, Brady, and Cogan 2002).

Finally, research on the incumbency advantage continues to develop new insights and interpretations, often leading us to reevaluate existing explanations for this advantage. In their analysis of state and federal offices from 1942 to 2000, Ansolabehere and Snyder (2002) show that the incumbency advantage increased for other offices at about the same time as it did for House elections, which undermines redistricting as a potential explanation for why incumbents are more likely to get re-elected. Through their use of a regression discontinuity design, Friedman and Holden (2009) demonstrate that, contrary to conventional wisdom, changes in redistricting (p. 148) have actually reduced the likelihood that incumbents will be reelected over time. Lee (2008) employs a similar estimation technique in his analysis of House elections to illustrate the difference between having an incumbent *party* advantage and having an incumbent *legislator* advantage in congressional races. Furthermore, recent historical work by Carson,

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Engstrom, and Roberts (2007) suggests that the incumbency advantage and the personal vote are not simply modern phenomena, but actually extend as far back as the late nineteenth century.

One additional argument pertaining to the potential advantages accruing to incumbents merits attention. A recent book by Jeffrey Stonecash (2008) makes the stunning claim that much of the apparent growth in the incumbency advantage is actually an illusion. Stonecash maintains that most of the increase in incumbent vote margins is an artifact of how uncontested elections are treated by analysts. Most scholars have omitted uncontested elections when studying the incumbency advantage, but included elections that are contested, yet not very close. Stonecash (2008) argues that, around the late 1960s, the number of uncontested races declined sharply, to be replaced by races in which the incumbent won handily. This change, he maintains, explains much of the increase in incumbent vote margins over time in contrast to many of the existing explanations popular today.

Stonecash's book demonstrates that the progress of science is not always linear. If we buy his argument that the incumbency advantage has not really grown in the past fifty years, then we have come full circle as a scholarly community. Stonecash's point about non-contested seats is well taken; nevertheless, a number of his other arguments are likely to be disputed by students of elections and congressional politics. For example, Stonecash finds fault with scholars using open seat elections as a reference point in determining how large the incumbency advantage is in incumbent held seats, or whether there is such an advantage. Yet we think that the analytical leverage provided by open seats is the key to making accurate empirical inferences about the incumbency advantage. Similarly, his claims about the role of realignment in the growth of the incumbency advantage are undermined by the growing literature that discounts much of the conventional wisdom regarding electoral realignment (Mayhew 2004).

Despite the wealth of articles and books on the incumbency advantage, we think this field is far from overtilled and we expect to see many new works on this theme in the future. Although much of the work to date has focused on House elections, the findings are often equally applicable to Senate elections as well. That being said, we would like to see more work explicitly examining the differences between House and Senate incumbent reelection rates over time, especially since Senate elections have traditionally been somewhat more competitive than House races (Krasno 1994). Additionally, and perhaps most importantly, we believe that research examining *multiple* explanations for the incumbency advantage (as opposed to yet another study arguing that one *single* factor is almost entirely responsible for this increase) is poised to make the largest contributions to the study of congressional elections in the coming years.

(p. 149) **Strategic politicians and candidate competition**

Over time, research on candidate competition in congressional elections has come to the conclusion that focusing on the incumbent side of the equation can only tell us so much about electoral outcomes. Beginning with the work by Hinckley (1980a), but especially with Jacobson and Kernell's (1981) important work on the role of strategic politicians, students of congressional politics began to stop putting so much emphasis on what congressional incumbents were doing right, and to focus instead on what challengers were doing wrong or not at all. In this and much of the subsequent research on congressional elections, attention has begun to shift exclusively away from the incumbent, to include also the challenger in the explanation of electoral outcomes. According to Hinckley (1980a), variation in electoral success is as much explained by the quality of the challenger as it is by any advantages inherent to incumbents. In many ways, this argument represented an important step in the congressional elections literature, as scholars since then have regularly examined the effect of challenger quality on election outcomes.¹¹

Scholars interested in the subject of candidate competition in congressional elections have spent considerable energy examining the issue in question and have approached the topic from a variety of perspectives. While several early studies redirected attention away from an exclusive emphasis on the incumbent to focus on the role of the challenger in explaining election outcomes (see e.g. Mann 1978; Mann and Wolfinger 1980; Hinckley 1980a, 1980b), Kazee (1980, 1983) was among the first to study various conditions under which candidates chose to emerge in House races.¹² Drawing upon interviews with individual candidates, Kazee (1980) found that many candidates ran for Congress simply for the experience or joy of running for office. In his subsequent work, Kazee (1983) confirmed that incumbency regularly served as an effective deterrent to potential challengers from emerging in congressional races.

In their now classic study of challenger emergence in congressional elections, Jacobson and Kernell (1981) examine whether political candidates exhibit strategic behavior in deciding whether or not to seek office. Through an examination of aggregate patterns of candidates' career decisions, the authors speculate as to the underlying motivations for the politicians' behavior. As their theory is premised on rational calculations, they argue that experienced candidates (those who have previously run for, and won, an elected office) are more likely to run for the House when national and partisan conditions are more favorable in terms of their likelihood of success.

(p. 150) Jacobson and Kernell (1981) test their theory of strategic behavior on data from the 1974, 1980, and 1982 congressional elections and find convincing evidence in support of their hypotheses concerning strategic politicians. Not only do they conclude that

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experienced challengers wait until circumstances are optimal before they decide to run, they also find that strategic politicians play a pivotal role in determining both the results of district-level elections and the overall partisan composition of Congress.¹³

Jacobson (1989) offers additional support for the strategic politicians theory by testing it with congressional elections data from 1946 to 1986. Through his examination of elections data during this forty-year period, he finds that experienced challengers do not emerge randomly. Rather, their likelihood of running varies with their perceived chance of winning (1989, 775). Indeed, Jacobson concludes that a greater proportion of experienced or quality candidates emerge when prospects appear favorable to their party. As a result, he argues that strategic decisions by congressional candidates, based on factors such as likelihood of victory, value of the seat, and opportunity costs, both reflect and enhance national partisan tides. In support of his contention that experienced politicians act strategically, he recognizes that quality challengers are more likely to emerge when a seat is uncontested, and they rely increasingly on an incumbent's prior margin of victory as an important cue in deciding whether or not to run (778).

In an attempt to further discern challengers' motivations in running for Congress, Banks and Kiewiet (1989) examine an interesting puzzle regarding the behavior of non-experienced or weak candidates who emerge to challenge incumbents. While they agree with Jacobson and Kernell (1981) concerning the deterrent effects of incumbency with respect to the emergence of experienced challengers, they seek to understand why incumbency does not have the same effect on weak challengers; as they point out, nearly all incumbents are challenged from one election to the next, usually by candidates lacking electoral or political experience. Through their analysis of congressional primary data from 1980 to 1984, Banks and Kiewiet conclude that weak challengers run against incumbents for the same reason that strong challengers are more likely to run in open seat contests—to maximize their probability of getting elected to Congress (1989, 1002).¹⁴ Even though their chances of defeat in the fall election are high, political amateurs recognize that running against an incumbent affords them the best opportunity to win their party's nomination, especially since more experienced challengers are likely to stay out of the race in the absence of favorable national or partisan conditions.

(p. 151) To date, the literature on the role of candidate quality in House elections has generated a number of good insights. Jacobson's and Kernell's measure of challenger quality is parsimonious, fairly easy to find (for most candidates), and a reliable predictor of electoral success. Despite numerous attempts to develop more detailed codings of challenger quality (see Bond, Covington, and Fleisher 1985; Krasno and Green 1988), the simple dichotomy has typically proven just as reliable a predictor of a competitive House election. There are no doubt a few candidates each year who go on to be successful without having held previous office (by this measure, Hillary Clinton was a “non-quality” Senate candidate; on this point, see also Canon 1993), and there are some previous office holders who are incapable of mounting a competitive campaign for a House seat. Nevertheless, for all the reasons mentioned above, we believe that trying to come up with

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yet another alternative measure of candidate quality represents an area where further research is clearly unwarranted.

In contrast to research on strategic politicians in the House, very little attention has been given to the same phenomenon in the Senate, and nothing much has been offered in the way of cross-chamber comparisons, either (but see Carson 2005). The one notable exception has been Lublin's (1994) article, which examines the impact of strategic politicians in Senate elections from 1952 to 1990. On the basis of his analysis of these election years, Lublin finds that, among all the quality challengers running in Senate races, U.S. House members gain a much higher percentage of the vote than other elected officials. Moreover, he finds that, like their counterparts running in House races, potential Senate challengers take into account economic, local, and national conditions when deciding whether or not to emerge. Furthermore, Lublin maintains that, even when incumbent senators do not lose to quality challengers, they are often weakened in the long run, since "past margins of victory relate directly to future electoral prospects. A small victory margin further endangers an incumbent's reelection indirectly by spurring strong challengers in the future" (1994, 239).

To understand the impact of strategic politicians further, we would like to see more work exploring the initial decision to seek elected office. It is fairly clear why experienced candidates wait until conditions are ideal until they decide to run for elected office, since running and losing could affect their career prospects adversely. At the same time, it could be the case that many more House and Senate elections would actually become competitive if a significantly higher proportion of experienced candidates decided to emerge. To date, the innovative work by Maisel and Stone (1997) and Stone and Maisel (2003) has gone a long way toward filling this void in the literature by way of their challenger emergence project. In particular, they have shown that a greater number of potential quality challengers choose not to emerge because they do not want to give up their current elected position or they believe the economic or political costs of running are too high (on this point, see also Maestas, et al. 2006).¹⁵ We believe that additional studies building upon this research agenda would (p. 152) be especially beneficial in helping to better understand the factors promoting greater democratic accountability and competition in both House and Senate elections.

Money in congressional elections

As a result of Jacobson and Kernell's (1981) pathbreaking study, congressional scholars have regularly begun controlling for challenger quality in models explaining election outcomes. Even before this important work became widely known, however, Jacobson (1978, 1980) had made an earlier theoretical discovery in his analysis of the effects of campaign spending in congressional elections. In examining the independent effects of both incumbent and challenger spending on electoral outcomes, Jacobson came up with a rather counterintuitive finding. While Jacobson found that increased spending by a challenger increased both levels of name recognition and the challenger's chances of winning, he found the opposite effect for incumbents. Indeed, the more money spent by an incumbent, the worse he or she did.

Jacobson contends that, although surprising at first, this anomaly can best be understood in terms of the quality of the challenger and of the perceived closeness of the race. Incumbents who run against a quality challenger in an election often have to raise and spend additional money in their campaign; but additional spending does not yield much in the way of further advantages since incumbents often start off with relatively high levels of name recognition. Moreover, high-quality or experienced candidates are usually adept at raising money themselves, which makes it more challenging for an incumbent to defeat them. Incumbents who perceive themselves to be in electoral danger can, and do, typically raise and spend more money than incumbents who perceive themselves to be relatively safe in a particular election. Therefore, on average, incumbents who win by a large electoral margin spend relatively small amounts of money when compared to incumbents in close races. Thus, Jacobson concludes that increased spending can definitely help the challenger, but it is almost always associated with lower vote shares for the incumbent.

Given Jacobson's rather counterintuitive findings regarding the effects of challenger and incumbent spending, subsequent research has attempted to offer alternative perspectives on the role of money in congressional elections. Green and Krasno (1988), for instance, reexamine the 1978 House elections, arguing that Jacobson's findings are subject to three specific forms of model misspecification. In particular, they claim that his failure to control for challenger quality, include interaction effects, and adequately deal with the issue of reciprocal causation leads to a variety of erroneous conclusions. More specifically, Green and Krasno rely on the lagged value of incumbent spending as an instrument for the endogenous spending levels. When these biases are corrected, the marginal effect of incumbent spending is found to be substantial and, under certain circumstances, on par with the effect of challenger spending (Green and Krasno 1988, 884). Moreover, they conclude that the quality of the challenger appears to be an important predictor of the overall vote, especially when large sums of money are spent in the race against the incumbent.¹⁶

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Other students of elections seeking to build upon Green and Krasno's (1988) critique of Jacobson's findings have searched for alternative and presumably better instruments for dealing with the endogenous nature of campaign spending in congressional elections. Abramowitz (1991), for instance, includes a measure predicting the closeness of the election and finds that challenger spending continues to outmatch spending by the incumbent, which is consistent with Jacobson's original conclusion. Kenny and McBurnett (1992) find similar evidence for challenger spending using a lag of contributions and a research design that employs a dynamic model (but see Ansolabehere and Gerber 1994). Alternatively, Erikson and Palfrey (1998) estimate simultaneous equations to identify three separate equations they seek to estimate. They find that incumbent spending matters a great deal in House elections and is actually a major source of the incumbency advantage. Moreover, they conclude that incumbent spending has an enduring effect on incumbent success, which is one of the main reasons why challengers have such a difficult time gaining traction in races contested by incumbents.

Extending research on campaign spending in the House, Gerber (1998) analyzes Senate elections from 1974 through to 1992, in an attempt to gain additional leverage on the debate over spending effects. One of Gerber's principal criticisms of the prior literature is that many of the instruments that have been used for challenger or incumbent spending have been inappropriate, since they may themselves be endogenous. Recognizing that both challenger and incumbent spending are related to the expected closeness of the election, Gerber relies upon levels of candidate wealth as his instruments. In his analysis of Senate races, he finds roughly equivalent marginal effects of incumbent and challenger spending on candidate vote share. Beyond acknowledging the potential policy consequences of his results for campaign finance reform, Gerber also takes issue with prior research on campaign spending—especially with respect to Jacobson's somewhat controversial findings about spending effects in congressional races.

Given the complications associated with evaluating the independent effects of money in congressional elections, Goldstein and Freedman (2000) pursue an alternative estimation strategy for evaluating these effects in the context of Senate races. More specifically, they rely on a new data-set targeting political advertising during the 1996 Senate elections, which they merged with the 1996 National Election Study in order to develop a unique measure of political advertising. With this measure, they find that both incumbent and challenger advertising had an independent effect on individual vote choice during the election. Goldstein and Freedman argue that, (p. 154) since a substantial amount of campaign spending goes to political advertising, their approach offers a cleaner measure of campaign effects in Senate races.

In another innovative analysis of campaign spending effects, Moon (2006) considers a possible explanation as to why incumbent spending is less effective than challenger spending in congressional elections. In particular, Moon maintains that the overall efficiency of incumbent spending depends on the marginality of individual seats (i.e. spending by “safe” incumbents is much less effective than spending by marginal incumbents, given the disparity in the number of swing voters). Through an analysis of

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U.S. Senate elections from 1974 to 2000, Moon demonstrates that, while safe incumbent spending is much less effective than spending by challengers, spending by marginal incumbents is not. Additionally, Moon argues that the reason why this finding has not previously been identified in research on spending effects in congressional elections is due to not disaggregating marginal and safe races.

Regardless of the divergent findings discussed above, nearly everyone agrees that money clearly shapes election outcomes and is a critical component of electoral success. Furthermore, there is little doubt that congressional elections, like presidential elections, have become much more expensive in recent decades. Growing disparities in spending patterns by incumbents and challengers raise obvious concerns about representation and democratic accountability in the context of House and Senate elections. As noted earlier, some scholars view the lack of financial competitiveness in congressional races as a major component of the incumbency advantage and of the decline in relative levels of electoral competition during the post-World War II era (see, e.g., Abramowitz 1989, 1991; Jacobson and Kernell 1981). In light of existing disagreements in the elections literature concerning the evaluation of spending effects in congressional races, we hope to see additional, innovative studies, which attempt to resolve this debate in order to evaluate better both how and why electoral competition may be affected.

In a related discussion of the role of money in elections, students of congressional politics often believe that incumbents accumulate large sums of money in their war chests in an attempt to “scare off” potential challengers from running in a given election (Epstein and Zemsky 1995; Jacobson 1990, 2009). Indeed, using a variety of different measures, a number of scholars have found that the amount of money raised by an incumbent does serve as a potential deterrent to candidate entry in congressional races (see Box-Steffensmeier 1996; Carson 2003; Goidel and Gross 1994; Goldenberg, Traugott, and Baumgartner 1986; Hersch and McDougall 1994). Nevertheless, not everyone who studies congressional elections agrees with this conclusion about the deterrent effect of money. Several different students of legislative politics (see Krasno and Green 1988; Epstein and Zemsky 1995; Milyo 1998; Ansolabehere and Snyder 2000; Goodliffe 2001) have all found that the amount of money raised by an incumbent does *not* serve as an effective deterrent to challenger entry in House races. In his follow-up analysis of the deterrent effect of incumbent war chests in Senate elections, Goodliffe (2007) finds similar evidence in the upper chamber.

In light of the aforementioned controversy over the deterrent effects of incumbent war chests on candidate entry decisions, we believe additional research is necessary (p. 155) in an attempt to try to resolve this debate. Although both sides present compelling arguments for their findings, it is theoretically unclear why large war chests would not deter strong challengers from running against incumbents. From the perspective of rational choice theory, this argument has a certain intuitive appeal. If we assume that legislators are rational actors and we know that they dislike raising large sums of money, it is unclear why they would continue to repeat this cycle each election, if money had little or no measurable impact on strategic challenger entry. On the other hand, if we

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assume that legislators do not habitually squander their limited time or scarce resources, then it seems reasonable to conclude that they are making rational calculations when raising money for their reelection campaigns, since this seems like a prudent thing to do. After all, raising money may not necessarily deter challengers, but it will most certainly help incumbents fund their campaign, should a strong challenger emerge.

Surge and decline

In addition to focusing on individual elections and patterns in electoral outcomes, some students of congressional politics have sought to explore more general patterns of aggregate seat change, especially those that occur during mid-term congressional elections. V.O. Key (1958, 615) was among the first to recognize the mid-term loss phenomenon in congressional elections when he wrote: “The president's party, whether it basks in public favor or is declining in public esteem, ordinarily loses House strength at mid-term—a pattern that, save for one exception, has prevailed since the Civil War.” Two years later, Angus Campbell (1960) proposed a general theory of “surge and decline” that examines how many congressional seats the party of the president loses at the mid-term election. According to the theory, the more seats the party of the president picks up during a presidential election, the more seats are at risk during a mid-term election. Without the president on the national ballot at the mid-term, individual congressional candidates become more vulnerable to local conditions and also potentially suffer if the economy or national conditions have declined in the months preceding the election. Thus the number of seats the president's party loses at the mid-term (the decline) is a function of the number of seats gained in the previous presidential election (the surge).¹⁷

Building on the work of Angus Campbell (1960), several scholars have offered various offshoots of the surge and decline theory to account for why the party of the president tends to lose seats during mid-term elections. Tufte (1975), for instance, (p. 156) frames the loss of seats in terms of the referenda theory of mid-term elections. In particular, he states:

Because there are no other targets available at the midterm, it is not unreasonable to expect that some voters opposed to the president might take out their dissatisfaction with the incumbent's administration on the congressional candidates of the president's party. (Tufte 1975, 813)

Following up on this work, Kernell (1977) offers a slightly different perspective on the referenda theory, which places a greater amount of emphasis on negative evaluations. Most notably, Kernell maintains that negative impressions about the current administration (especially those pertaining to economic considerations) tend to be much more salient to voters than positive ones, which leads to greater bias against the members of the president's party in Congress. Kernell's findings are similar to those

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reported by Erikson (1988) in his analysis of mid-term loss and fit remarkably well with the retrospective voting model discussed by Fiorina (1981).

Oppenheimer, Waterman, and Stimson (1986) offer a different version of the referenda theory—what they term the *exposure thesis*—to account for the loss of seats at the mid-term by the president's party. In short, their theory evaluates the extent to which a party's pre-election seat holdings exceed or fall below that party's normal share of seats. On the basis of their analysis of House elections since 1938, Oppenheimer and colleagues determine that the average number of seats held by the Democrats during this period was 254. When the number of Democratic seats falls below 254, the party is considered to be underexposed and is most likely to pick up additional seats in the upcoming election. In contrast, increases beyond 254 seats would leave the party overexposed and at greater risk of losing seats in the next midterm election. Despite the accuracy of the exposure thesis in predicting seat loss from 1938 to 1984, its explanatory power has been undermined to a certain extent in more recent elections.¹⁸

On the whole, research on surge and decline and the referenda theories of midterm loss have held up remarkably well over time. If one updates Key's earlier quotation about mid-term loss, the president's party has lost House seats in all but three mid-term elections since the Civil War. Somewhat interestingly, two recent mid-term elections—in 1998 and 2002—have challenged the conventional view associated with surge and decline theory, as the president's party actually gained a few additional seats in each of these mid-term elections. Whether these recent elections are simply anomalies or harbingers of aggregate electoral change is as yet unclear.¹⁹ For now, those scholars interested in predicting aggregate outcomes in congressional elections would be best served by offering greater comparisons of seat loss in *both* House and Senate races, since this is an area that definitely could use additional research.

(p. 157) **Implications for future research**

In her discussion of the state of legislative elections research in the mid-1990s, Ragsdale (1994) is critical of a lack both of theoretical and of comparative focus in this context, especially as it pertains to questions concerning voters' choices and legislative decisions.²⁰ Although some progress along these lines has occurred during the past fifteen years, we recognize that there is still room for improvement in the literature. In terms of specific suggestions for where we think the literature should go in the next ten years, we offer several additional ideas in the remainder of this chapter—beyond those discussed in the preceding pages. To be clear, we do not believe the following suggestions are exhaustive in terms of where the research should go next. Rather, they should be viewed as a starting point for research and for scholars interested in making additional contributions to our understanding of House and Senate elections.

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Our first suggestion for future work on congressional elections is to encourage scholars working in this area to recognize that congressional elections did not begin in 1946. Most individuals who study elections naturally rely upon Gary Jacobson's House elections data extending back to World War II, but an unfortunate consequence of this reliance is that almost all research on elections has focused exclusively on the modern era. In fact, and until recently, most of our knowledge about nineteenth- and early twentieth-century elections was based largely on anecdotal or historical accounts from that period. Moreover, almost no one thought it worthwhile to examine House elections prior to 1946, given the enormous costs associated with collecting data from earlier periods. Nevertheless, as more of these historical data have become available (see Dubin 1998; Carson, Engstrom, and Roberts 2007), it is much easier to analyze House elections in a comparative context by examining changes across time.

On a related point, we would like to see additional research focusing on Senate elections over a greater period of history. Surprisingly little research has focused on Senate races outside of the modern era, for instance. Lublin's (1994) analysis of the impact of strategic politicians in the Senate only considers elections extending back to 1952. Although Highton (2000) examines Senate elections from 1920 to 1994, his focus is more limited on the influence of certain variables on predicting election outcomes. As noted earlier, several recent studies have examined how the adoption of direct election in 1913 influenced the institution of the Senate as well as the behavior of its members (see Bernhard and Sala 2006; Gailmard and Jenkins 2009; Meinke 2008; Schiller 2006). Nevertheless, this line of research represents only a drop in the bucket in terms of the types of research questions that are awaiting to be addressed in the context of the Senate.

Analyzing House and Senate elections over a longer time span offers us a number of distinct advantages in terms of understanding congressional elections. First, extending the study of elections further back in time offers us the opportunity to generate (p. 158) more dynamic theories of elections and of legislative behavior. For instance, take analyses seeking to better understand the mid-term loss phenomenon. As is generally known, the president's party almost always suffers a loss of seats in the House during a mid-term election. Two recent exceptions to this rule occurred in 1998 and 2002, as noted previously, where the party of the president actually picked up seats at the midterm. If we wanted to study these unique elections in a larger historical context, we would have to go back to 1934, since this was the last time such an event occurred at a mid-term. By turning to the past, we can begin to search for alternative explanations that may account for this rather unusual electoral phenomenon.

A second advantage of studying House and Senate elections across time is that it can lead to greater generalizability, which stems from increased variation in outcomes. Consider again the preceding example concerning instances when the president's party actually gained seats during a mid-term election. With only three instances of this occurrence since the Civil War, it could prove challenging to try to derive a reasonable explanation for this phenomenon. However, if we extend our analysis back in time to include all mid-

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term elections during the nineteenth century, we have a much larger sample of elections to draw upon. This, in turn, has the added benefit of increasing the accuracy of our theoretical explanation, since there presumably will be much more variation across election outcomes during the past 200+ years.

A third advantage of studying congressional elections across time is greater institutional variation. The Australian or secret ballot gained regular usage in the United States beginning in the mid-1890s; thus it is impossible to discern the full effect of this change using data from 1946 to the present. Similarly, the direct primary system of choosing congressional candidates emerged in the early decades of the twentieth century and replaced the system by which House candidates were selected by party bosses. To truly capture the effects of these and other institutional changes on elections and election outcomes, scholars should employ data that pre-date and post-date the changes they seek to study.

One final advantage of studying congressional elections across time is that this gives us the chance to correct historical inaccuracies in the conventional wisdom. Consider, for instance, our understanding of strategic politicians. Most observers of contemporary politics would argue that the impact of experienced candidates should be greatest in the modern era of candidate-centered elections. However, Carson and Roberts (2005) find that, regardless of the era, candidates with prior electoral experience were more likely to run for office when national and local conditions favored their candidacies. Similarly, incumbents during the late nineteenth century responded to changes in their district boundaries brought on by redistricting, running in favorable districts, and bailing out when they were given an unfriendly congressional district (on this point, see Carson, Engstrom, and Roberts 2006; Engstrom 2006).

Furthermore, recent evidence suggests the presence of an incumbency advantage as far back as the late nineteenth century, well before the onset of careerism (Ansolabehere, Snyder, and Stewart 2000; Carson, Engstrom, and Roberts 2007).

(p. 159) While we would argue that finding that there was an incumbency advantage in the nineteenth century is interesting in and of itself, this type of research presents new contexts for understanding research by employing historical data. For example, given that we know there was an incumbency advantage in Congress as early as the 1870s, we can definitively claim that factors such as the Australian ballot, direct primary, frequent travel, large staff support, court-ordered redistricting and the rise of television are not *necessary* conditions for an incumbency advantage to exist. As each of these examples illustrates, our understanding of both modern and historical elections can change considerably when these phenomena are analyzed in a broader, historical context.

Beyond examining past elections, we would also encourage students of House and Senate elections to place more emphasis on congressional primary elections. For the past few decades, and with very few exceptions, nearly all the research on congressional elections has focused exclusively on the general election stage.²¹ Although this emphasis has helped us understand general election outcomes to a much greater extent, it has also

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failed to illuminate a number of potentially interesting puzzles and questions about primaries that remain unaddressed. For instance, under what conditions will candidates decide to emerge in congressional primaries? Are state legislators more likely to be successful in these types of races than other experienced candidates are? If so, why might this be the case? Is greater familiarity with voters in statewide constituencies a significant advantage for state legislative candidates who decide to pursue higher office? What role are national party organizations playing in encouraging and deterring candidates from running in primaries?

In addition to the above questions concerning congressional primaries, we also know very little about the effects of primaries on general election outcomes. To what extent are general election outcomes significantly influenced by what occurs at the earlier primary stage of the electoral process? Are otherwise strong or experienced candidates deterred from emerging in congressional primaries by incumbent's war chests or by national party leaders? If so, what are the implications for electoral accountability and representation? How difficult is it for House and Senate candidates to raise sufficient sums of money to wage a competitive primary campaign, in comparison to what is necessary to win during the general election? Where do candidates running in congressional primaries obtain a majority of their campaign funds from? How do positions taken in primary campaigns translate into general election positions and positions taken within the chamber? We believe that addressing these and a variety of related questions will go a long way in illuminating a number of previously unexplored puzzles in the context of congressional elections research.

A third avenue for making an independent contribution with respect to our understanding of House and Senate elections is to focus more carefully on interactions between congressional behavior, campaigns, and election outcomes. To date, most (p. 160) research on congressional elections bypasses the details of congressional actions (exceptions being aggregate voting statistics and interest group ratings) and election campaigns (other than the quality of the candidate emerging and how much money is spent) in favor of what happens when voters go to the polls on Election Day (but see Sellers 1998). This is unfortunate, however, as it overlooks a variety of important factors that might actually shape election outcomes and behavior inside the beltway. Most followers of congressional elections are familiar with anecdotal accounts of a vote or a scandal by a legislator causing them electoral harm, but what is missing is the mechanism by which the harm occurs. Are some incumbents better at explaining away controversial votes or behavior? Are some challengers better at exploiting these issues than others? Are some electorates more or less responsive to campaign messages? Additional case studies and analyses focusing on these specific questions would be a welcome addition to the literature on elections to the U.S. House and Senate.

On a related point, consider again the role of candidate quality in elections. We know that experienced candidates are more likely to defeat incumbents than are non-quality candidates or political amateurs. Are these candidates more likely to win because they have greater name recognition than political amateurs, or are they simply better at the

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art of electioneering? While some might contend that it is actually a combination of both factors that determines victory in congressional races, we simply do not know if each of these related explanations *independently* contributes to electoral success. Indeed, it could be the case that a candidate's connection with voters is far more valuable than electioneering in determining who wins an election, especially if both candidates running for a particular seat in Congress have roughly the same financial resources. Nevertheless, without greater attention to what candidates actually do during the campaign, we cannot sufficiently answer this question.

Furthermore, we would like to see additional research investigating how candidates utilize money in congressional campaigns and where those funds come from. Beyond focusing on the effects of challenger and incumbent spending in determining election outcomes (see Jacobson 1980, 1990), we know considerably less about candidates' allocation strategies as well as the effects of such strategies on their congressional campaigns. At the same time, only limited attention has been given to identifying sources of candidates' campaign funds. Larson (2004) was among the first to analyze incumbent contributions to congressional campaign committees. More recently, Cann (2008) has focused on member-to-member contributions as well as the increasing role of leadership PACs in influencing election outcomes. Additionally, Gimpel, Lee, and Kaminski (2006) and Gimpel, Lee, and Pearson-Merkowitz (2008) have begun examining the geographic component of campaign contributions, particularly as it relates to interdistrict funding flows in House races. More research on fundraising in this vein would help fill a noticeable gap in the congressional campaigns literature.

We believe that future work that examines elections across *both* the House and Senate would be especially valuable as well. For too long, research on Congress has focused almost exclusively on the House with little or no attention paid to the Senate. Although this has begun to change in recent years, there is still an overwhelming (p. 161) amount of work focusing on House rather than Senate races (for two notable exceptions, see Gronke 2000; Krasno 1994). By analyzing elections across both chambers, we can gain a better understanding of how differences in lengths of terms, chamber rules and procedures, and individual legislator goals can directly impact election outcomes. Moreover, more cross-chamber comparisons can further illustrate how legislative outcomes in a bicameral context may be influenced directly or indirectly by the underlying electoral context in individual races.

An additional area in need of more development in the congressional campaigns literature involves the linkage between campaigns and legislative representation. Sulkin (2005) and Sulkin and Swigger (2008) have made an important contribution along these lines in their work on issue politics, agendas, and campaign advertisements, but more research is necessary. For instance, to what extent do candidates shape their campaign message in terms of the quality of the challenger they face in an election? Do legislators seek to offset controversial positions with more positive campaign messages? How effective are candidates' communication strategies in reaching out to potential voters?

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Addressing these and other related questions will help raise awareness of an area of congressional elections research that has largely been overlooked to date.

One final area where additional research could significantly enhance our understanding of congressional elections is in terms of analyzing voter behavior and turnout in elections. The recent development of the Cooperative Congressional Elections Study (CCES) across a large number of universities is an important step in better understanding voter behavior across individual states and congressional districts. Through individual-level surveys conducted via this study, we can seek to systematically examine why turnout varies so dramatically across districts and states, whether voters feel as though they influence policy outcomes, and how representation is viewed by individual voters. With such a large sample of voters spread out across the country, analyses that employ these individual-level surveys should be more accurate and generalizable in terms of the conclusions they draw about voter attitudes and behavior.

We believe that greater attention to legislator–constituency linkages as perceived by the voter will help us address a number of research questions that have been largely overlooked. For instance, how do different styles of representation affect voters' perceptions of incumbents? Do voters regularly punish legislators for behavior that is perceived to be too ideological or too partisan?²² Are newly elected legislators constrained by the “homestyle” of the previous House member, as Fenno (1978) suggests, or do they have the opportunity to establish their own unique style of representation? When and under what conditions will legislators be perceived as being out of touch with their constituents? After an incumbent's congressional district is redrawn, how do legislators adapt to their new constituencies consisting of a mix of “old” and “new” voters? Greater attention to these types of questions, as (p. 162) well as to others, will serve to further enrich our understanding of House and Senate elections for many years to come.

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Notes:

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(1) Although Mayhew (1974a) focuses exclusively on the modern era in his discussion, recent evidence (see e.g. Bianco, Spence, and Wilkerson 1996; Carson and Engstrom 2005) suggests that his argument may extend back to earlier political eras as well.

(2) The notion of credit claiming is central to Mayhew's argument concerning the importance of committees in the congressional arena. As Mayhew claims, members

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serving on constituency-oriented committees can use the influence of their positions to procure pork barrel projects for their district, thereby fostering an opportunity to accept the credit for these particularized benefits.

(3) Mayhew's notion of credit claiming has also extended into the debate in the congressional organization literature regarding the relevance of the distributive perspective with respect to committee outliers. This perspective is often contrasted with the partisan theory advanced by Aldrich and Rohde (2000) and Cox and McCubbins (2005, 2007) and with the informational perspective advanced by Krehbiel (1991).

(4) Although Mayhew (1974) appears to focus principally on the House in his discussion of the electoral connection, occasionally he does single out the Senate when elaborating on a specific aspect of his argument. This is certainly the case in conjunction with position taking (67), an activity Mayhew believes has become far more common among candidates running for elected office (179). For more recent evidence of this behavior, see Jones (2003).

(5) Critics of congressional redistricting as a possible explanation for the incumbency advantage quickly pointed out that, since incumbent senators were also reelected at very high rates, redistricting could not be a contributing factor, given that state boundaries do not change every ten years. On this point, see especially Tufte (1973).

(6) See also Jacobson (1987) for a discussion suggesting that incumbents are no safer than they had been in the past. For a critique of this argument, see Bauer and Hibbing (1989) and Ansolabehere, Brady, and Fiorina (1992).

(7) For a more recent discussion of the extent to which incumbents outraise challengers, see Herrnsen (2007). He also elaborates on the logic by which access-oriented contributions go to incumbents rather than to challengers.

(8) Although Erikson (1971) was among the first to note that traditional measures of the incumbency advantage such as the retirement slump or the sophomore surge could result in biased estimates, Gelman and King (1990) formally demonstrated the nature of this bias in their work.

(9) For an alternative explanation for the increase in the incumbency advantage, see Levitt and Wolfram (1997). For evidence demonstrating that redistricting can be used to estimate incumbents' personal vote by treating redistricted voters as a control group, see also Ansolabehere, Snyder, and Stewart (2000) and Desposato and Petrocik (2003).

(10) This is analogous to Jacobson's (2009) work showing that Republican voters are more efficiently distributed across congressional districts than Democrat voters.

(11) To be fair, Hinckley was not the first congressional scholar to discuss the impact of challenger quality on election outcomes. Indeed both Erikson (1971) and Jacobson (1978)

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mention it as a potential variable in explaining electoral outcomes. However, we believe she was the first to explore it systematically in the context of a given election.

(12) For a general discussion of ambition theory that examines the motivations for individuals to choose to run for office or to make a career out of politics, see Fowler and McClure (1989); Rohde (1979); and Schlesinger (1966).

(13) For critiques and extensions of the Jacobson–Kernell strategic politicians hypothesis in the context of House races, see Bianco (1984); Bond, Covington, and Fleisher (1985); and Born (1986). For more recent evidence confirming the hypothesis in the context of both House and Senate elections, see Carson (2005).

(14) For an alternative perspective regarding political amateurs' motivation for running for Congress, see Canon (1990, 1993). In brief, Canon argues that amateurs would be better off running in open seat contests, since they actually do *not* maximize their chances of winning by running against incumbents. Canon (1993: 1130–8) adds that to fully understand why inexperienced candidates challenge 'safe' incumbents, it is necessary for scholars to distinguish between experience-seeking amateurs and ambitious amateurs.

(15) For evidence of why a greater proportion of women decide not to run for office, see Lawless and Fox (2005).

(16) See also Thomas (1989) for a critique of Jacobson's conclusions about the effects of incumbent spending in congressional races. For a response to Green and Krasno's (1988) critique, see Jacobson (1990). Green and Krasno (1990) follow up with a rebuttal immediately after Jacobson's response.

(17) For more recent extensions of Angus Campbell's work on surge and decline, see Campbell (1991, 1997). For a specific extension to Senate races, see Abramowitz and Segal (1986). The arguments presented in this context are not all that dissimilar from work examining presidential coat-tails in House and Senate races (see e.g. Ferejohn and Calvert 1984; Campbell and Sumners 1990).

(18) Waterman, Oppenheimer, and Stimson (1991) extend their analysis of the exposure thesis to an earlier era (1896–1928) in their follow-up work. For an application of the exposure thesis to both the House and Senate, see Waterman (1990).

(19) For a recent analysis that considers seat change in light of these two mid-term elections, see Finocchiaro (2003).

(20) See also Squire (1995) for a summary of the state of congressional elections research in the mid-1990s.

(21) For a few notable exceptions of research examining congressional primaries, see Ansolabehere, Hansen, et al. (2007); Banks and Kiewiet (1989); Fox and Lawless (2005); Galderisi, Ezra, and Lyons (2001); and Maisel and Stone (2003).

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(22) For an extended discussion of this particular question in the context of House races over time, see Carson, Koger, Lebo, and Young (2010).

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