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The U.S. Voting Rights Act of 1965: a study of the politics of structural changes in voter turnout

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The U.S. Voting Rights Act of 1965: 
A Study of the Politics of Structural Changes in Voter Turnout

A dissertation submitted in partial satisfaction of the Requirements for the degree Doctor of Philosophy

in

Political Science

by

William Charles Terry

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2010
The Dissertation of William Charles Terry is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

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University of California, San Diego

2010
DEDICATION

This dissertation is dedicated to my grandparents.
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Last but not least, I owe a huge debt of gratitude to my family and friends, particularly my grandparents, to whom this dissertation is dedicated, with love.
VITA

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PUBLICATION
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This dissertation studies the effects of the Voting Rights Act of 1965 from both an historical and a comparative perspective. By an historical perspective, I mean that I consider the context of southern politics and the specific debates that surround the act—e.g., concerning the existence and magnitude of white backlash and countermobilization. By a comparative perspective, I mean that I consider the VRA as a particular instance of a structural increase in turnout—i.e., an increase due to legal changes that increase the
incentives, or decrease the disincentives to participate in elections. The comparative perspective suggests that the VRA might be useful for culling broader insights, for example, regarding the ongoing scholarly debate about whether or not turnout “matters” (cf., Lijphart 1997).

My empirical analysis of the VRA is centered specifically on a ubiquitous puzzle noted in existing studies of southern politics, namely, the reactionary, ostensibly anti-Downsian policy repositioning of southern members of Congress in the late 1960s and 70s (cf., Glazer, Grofman, Owen 1998). Scholars find the post-VRA voting patterns of the South’s politicians puzzling precisely because the Downsian theory on which scholarly expectations are based would seem to suggest that policy should have moved left when large numbers of African-Americans entered the effective electorate after 1965.

Recognizing the difficulties in employing a strict Downsian model in the 1960s South, I develop a “two front war” theory of electoral competition that better approximates the post-VRA environment, and show that the model is capable of rationalizing the observed policy patterns. To test the theory, I implement a number of substantial conceptual and methodological improvements on existing empirical studies. My regression analysis finds broad support for the thesis—where post-VRA dynamics precipitated a larger threat on the incumbent’s right, she responded by moving in a conservative direction. Conversely, when the influx of African-Americans opened up a more dangerous front on the left, she responded by becoming more liberal.
Chapter 1.
Introduction: Does Structurally Low Turnout Favor the Right?

I. The Central Question

The question of how voter turnout influences political outcomes has been a subject of debate for some time. A frequently voiced and plausible argument is that, because the pool of non-voters in most democracies disproportionately consists of citizens who are less educated, poor, working-class, or members of minority groups, political outcomes are likely to reflect the preferences of the relatively advantaged members of society—precisely because they are empirically more likely to show up at the polls (Merriam & Gosnell 1923; Tingsten 1937; Galbraith 1986; Burnham 1987; Lijphart 1997). Thus, as the argument goes, the candidates and policies of the left, which are presumed to favor the disadvantaged, should benefit when turnout is high.¹

Despite the common-sense appeal of this argument, many empirical studies of the relationship between turnout and the political fortunes of the left have failed to support it. For instance, Highton & Wolfinger (2001; p. 179) assert that “[s]imply put, outcomes would not change if everyone voted.” Citrin, Schickler, & Sides (2003; p. 88) conclude that, “The lack of competitiveness of most Senate (and House) elections means that there are unlikely to be many races where even universal turnout would change the outcome.” So numerous are the studies concluding low turnout shouldn’t substantively affect political outcomes that Lutz & Marsh (2007; p. 539) summarize the literature as follows: “The various articles, despite using different approaches, looking at different countries

¹ This argument is also made by Mills (1956), Schattschneider (1970), and Hill (2006).
and different types of election, all show that any bias in election outcomes is typically rather small and it is not in a specific direction: sometimes the left would benefit from higher turnout, sometimes other parties.” The authors conclude that “the concerns about potential bias consequent on low turnout are generally misplaced.”

This study seeks to contribute to our understanding of whether or not and in what sense voter participation rates matter. For the sake of clarity, I will impose some structure on the inquiry from the outset. In particular it is important to specify precisely what claims are being made. When a scholar or pundit asserts that “higher turnout would benefit the left” one must ask, “What factors precipitated the change in turnout?”; “How much more turnout are we talking about?”; and, “In what sense is the left benefited?”.

Consider the first question: “What factors precipitated the change in turnout?” One possible answer is that turnout changed because the net structural incentives to participate in elections changed. By structural incentives to participate I mean those sourced in law. Structural incentives can be negative—e.g., when voters must pay poll taxes in order to vote—or positive—e.g., when voting is compulsory. Another possible answer is that turnout changes for non-structural reasons—as when one party mounts an unusually large mobilization effort. While one might expect a structural increase in turnout to benefit the left (under certain circumstances), it is hard to see why an increase in turnout produced by mobilization campaigns launched by the right should do so. In my dissertation, I focus on structural changes in turnout.
Now consider the second question: How large a counterfactual change are we entertaining? Are we imagining relatively marginal increases in participation rates, say, of the order one would expect were the Democrats to spend an additional hundred thousand dollars mobilizing voters in New York City? Or, much larger changes in turnout, say, of the order we would expect were strict compulsory voting laws implemented in the U.S. or in all of Switzerland’s cantons? Small increases in participation rates may have little impact, but larger increases may have profound political implications. In what follows, in both my review of the literature and in my own work, I will be careful to note the scope of the change imagined in any given claim.

Finally, consider the third question: “In what sense might the left benefit when turnout is increased?” Scholars have answered this question in one of two ways. When Kousser (1980), for example, argues that the low turnout induced by the emergence of Jim Crow laws hurt African-Americans in turn-of-the-century North Carolina, he is referring to educational expenditure per pupil—a specific policy outcome. When Citrin, Schickler, & Sides (2003, p. 83) claim that only “four of the 91 races [in the 1994, 1996, and 1998 U.S. Senate elections] would have had a different winner if everyone eligible had voted,” they are referring to who wins—an electoral outcome. Because there are at least two potentially meaningful senses in which the left may do well or not, deciding whether or not the Left “did well in Election X” depends to some extent on whether the dependent variable of interest is the number of seats the Left picked up, or the equilibrium policy subsequently enacted by the government. In my dissertation I attempt to discern turnout’s implications for both electoral and policy outcomes.
I begin my project in this chapter by reviewing the literature, proceeding as follows. First I review the arguments that turnout “does not matter.” Section II summarizes the authors who have focused on electoral outcomes—i.e., vote-shares—as the left’s performance metric and section III reviews the studies focusing instead on policy outcomes. Section IV presents the corresponding “turnout matters” arguments for the vote-share and policy metrics, respectively. In section V I discuss how political actors of the right and left have viewed changes to the structural voting regime and I conclude this introductory chapter in section VI by motivating my specific research program and describing the theoretical and empirical studies that follow.

To preview the broader project, in Chapter 2 I present a simple strategic model of electoral competition in an environment with variable turnout. I develop a broadly Downsan perspective that frames the subsequent analysis and I derive a number of formal propositions that add precision to specific arguments in the turnout debate.2 Having developed some theoretical guideposts, I present empirical studies of the political and economic impact of the Voting Rights Act of 1965 in the U.S. South—encompassing chapters 3 and 4—as a specific case study of the implications of structural changes in the turnout regime. The VRA chapters are as follows. In Chapter 3 I provide background on the South’s distinctive politics, highlighting the features of the southern case that depart from the Downsian framework. I discuss at some length the ostensibly anti-Downsan

2 Harold Hotelling (1929) and Duncan Black (1948a,b) also deserve credit for developing the theory I build on. For the sake of brevity I cite only Downs (1957).
policy responses widely-noted by scholars of the 1960s South. Chapter 3 also discusses how one ought to think about the major features of Jim Crow and post-VRA southern politics in terms of modifications to Downs’s model, and motivates my empirical work.

Next, in chapter 4, I offer some novel empirical analysis addressing the southern puzzle laid out in chapter 3. My central objective in chapter 4 is to probe the anti-Downsian findings and throw the puzzle into starker relief. Towards this end, I make a number of substantial methodological and conceptual improvements on existing studies, including looking at the broader electoral context (which included candidate exit and entry) and employing a much stronger measurement of the Downsian mechanism in the South (the ratio of racial registration rates, as opposed to the relative size of the black voting-aged population). The chapter focuses on the VRA’s proximal policy consequences in the U.S. House and also includes some analysis of the U.S. Senate, for its own sake and as a revealing point of comparison for the impact in the House.

II. Arguments High Turnout Does Not Favor the Left

A. Turnout and the Left’s Vote Share

There are two types of argument that high turnout does not systematically improve the left’s electoral fortunes, by which I mean the vote share of leftist parties. One argument is theoretical, the other empirical. The theoretical argument denies the premise that non-voters would choose any differently than voters were they to show up at the polls. The empirical argument is based on direct observation of the correlation between participation rates and partisan vote shares.
Wolfinger & Rosenstone (1980) made the seminal theoretical argument that turnout doesn’t matter. Their work framed the terms of the debate by introducing a popular research design and first presented the empirical evidence that has been confirmed numerous times since, across polities and over time. The authors’ analysis of survey data drawn from the U.S. electorate found that people who are white, well-educated, well-to-do, married, Northerners, government employees, and residentially stable account for a proportion of voters larger than their share of the population. Wolfinger & Rosenstone thus documented the well-corroborated empirical regularity that the disadvantaged are underrepresented at the polls. Their analysis of the 1972 National Election Survey (N.E.S.) nevertheless led them to conclude that, with respect to most important political issues, “voters are virtually a carbon copy of the citizen population. Those most likely to be underrepresented are people who lack opinions…as long as attitudes on issues are so weakly related to social class and race, the poor and minorities will find enough allies to avoid political weakness in proportion to their own voting rates” (p. 109-11).

Numerous other studies of American survey data have reached similar conclusions. Prominent examples are Highton & Wolfinger (2001); Bennet & Resnick (1990); Gant & Lyons (1993); Teixeira (1992); and Shaffer (1982). In the context of the U.S., Ellcessor & Leighly (2001, p. 127) conclude that “one of the least contested conclusions in the study of political behavior is that voters’ political attitudes and policy positions are fairly representative of non-voters.” It’s worth noting that the “low turnout doesn’t matter” arguments of this type are implicitly predicated on a counterfactual story of the following
form: “Suppose turnout was increased by selecting non-voters at random and sending
them to the polls, then (in expectation) the outcome of the election would not change.”
On the other hand, strategic mobilization by one party or the other would presumably
have non-neutral consequences.

Though countries outside the U.S. have received considerably less attention with
regards to how similar are voters and non-voters, some scholars have argued that
Wolfinger & Rosenstone’s conclusions are generally valid elsewhere. Highton (2001) for
example cites “G. Bingham Powell’s (1986) comparative analysis that finds that at the
individual level within countries, age, not education, is the strongest demographic
predictor of turnout” as suggesting non-voters’ preferences do not differ substantially
from voters’ (p. 16,332). In a study of political attitudes in the U.K., Studlar & Welch
(1986; p. 139) conclude that “nonvoters are much like voters in specific issue opinions
and in social class. They do not have a strong ideology or a markedly different attitudinal
structure than their fellow citizens.” Tóka’s (2004; p. 47) analysis of survey data for 18
democracies in the Comparative Study of Electoral Systems (CSES) dataset leads him to
conclude that “social differences in both turnout and political knowledge may lead to the
hypothesized political inequalities but their size is remarkably modest.”

There exist other claims, similar in spirit to Wolfinger & Rosenstone, but taking a
softer position regarding the question of whether voters and non-voters prefer similar

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3 Lutz & Marsh’s (2007) introduction to the Electoral Studies symposium on voter
turnout has more cites along these lines.
political outcomes. Like Wolfinger & Rosenstone, these studies conclude that increased turnout would not substantively change electoral outcomes. The logic at work here, in Highton’s words, is that “if the number of nonvoters in an election is small compared to the number of voters, then even if nonvoters have a very different set of preferences, their small number will limit the potential for impact on the aggregate preferences of voters” (2001; p. 16,332).\(^4\) Citrin, Schickler, & Sides (2003; henceforth CSS) is one argument in this vein. CSS estimate a vote choice equation for U.S. Senate races using demographic data on voters surveyed in exit polls. CSS then take the parameters from the choice equation they estimate and use these to construct a predicted vote for all the respondents of the Current Population Survey (CPS), which is presumed to reasonably sample the universe of eligible voters. By comparing the predicted outcome under various counterfactual turnout scenarios against the observed outcome of some actual elections, CSS conclude that, while randomly bringing non-voters to the polls would generally have boosted Democratic vote shares, very few elections were actually close enough for an increase in turnout to have changed the winner. Similarly, Brunell & DiNardo (2004) conclude that of the 13 presidential elections between 1952 and 2000, full turnout would not have made a difference in the outcome (with the possible exceptions of the 1980 and 2000 elections).

Lutz (2007) employs a research design similar to CSS to study turnout’s impact on 144 referenda put before Swiss voters between 1981 and 1999. Interestingly, he

\(^4\) See also Tingsten’s (1937) “law of dispersion” for a theoretical argument along these lines (cf., Hajnal 2009; p. 6).
includes in his analysis a measure of survey respondents’ level of political knowledge. In general, Lutz concludes that there is only a small turnout bias in the outcome of these votes, and that the bias is attenuated in more substantively important votes.

Counterintuitively, Lutz finds that in those contests “where turnout and information did matter, higher levels of turnout tended to work in favour of the right-wing parties, whereas higher levels of information tended to work to favour outcomes supported by left-wing parties.” Lutz’s conclusion regarding citizen’s political sophistication is an interesting one: “Non-voters tend to have less information and are more right-wing in their views. Hence higher turnout might benefit the right. However, more informed voters are more left wing and, arguably, higher turnout could only be achieved (short of compulsory voting) by raising interest and information. If so, higher turnout might then benefit the right much less and perhaps the left rather more than might be expected on a simple comparison of voters and non-voters” (p. 546). The causality in Lutz’s study is difficult to discern; obtaining a clearer picture of the casual relationships would presumably help one speculate on the likely consequences of structural changes.

The second type of argument that high turnout does not help the left electorally is based on observational studies of turnout and the distribution of partisan vote shares. The preponderance of evidence here suggests that turnout has only a negligible impact on leftist vote shares, if it has any impact at all. Examples of this species include Key (1958); Burnham (1965); DeNardo (1980); Tucker & Valditz (1986); Erikson (1995); Nagel & McNulty (1996; 2000). Work outside the U.S. includes Pacek & Radcliff (1995), who find an insignificant relationship between turnout and partisan vote shares
cross-nationally. Radcliff (1994) is remarkable for being the single frequently-cited study finding some correlation between turnout and partisan vote shares—he finds a statistically significant positive relationship between state-level turnout and the Democrats’ presidential vote share, though his methodology has been sharply criticized by Erikson (1995).

It is critical for what follows to point out that these studies are merely correlational. In a recent working paper, Gomez & Hansford (2008) raise a serious argument against this conventional evidence by pointing out a particular source of potential endogeneity between turnout and partisan vote shares. Drawing informally on a Downsian (1957, p. 39) rational abstention model, Gomez & Hansford identify the following theoretical problem: “The problem with [the standard] approach is that it assumes that levels of voter turnout are exogenous to partisan vote shares. Yet it is very likely that voter turnout is actually endogenous [because]...vote choice and the decision to vote are both a function of the spatial locations of the voter and the candidates” (p. 9). The authors are in fact arguing for a certain model of “policy-mobilized” turnout which has developed independently in the theoretical literature—e.g., by Hinich & Ordeshook (1969); Adams, Dow, & Merrill (2005); and Peress (2007)—holding that citizens base their turnout decision in part on the policies at stake in the election.

In the electoral environment imagined by Gomez & Hansford (2008), Downs (1957), and, more formally, by Hinich & Ordeshook (1969) and others, each candidate first chooses a policy platform (typically unobserved by the researcher) and then presents
this platform to citizens in the electorate. In turn each registered citizens decides (a.) which candidate is preferable and (b.) whether or not s/he would find it worthwhile to turn out. To the extent that a rational candidate will keep both (a.) and (b.) in mind simultaneously when choosing a utility-maximizing platform, turnout will be related to the error term in a partisan vote share regression, potentially leading to endogeneity bias (cf., Gujarati 2003; chapter 18). More specifically, in a regression context, the presence of endogeneity implies that one or more of the independent variables is correlated with the error term, violating the well-known Gauss-Markov assumptions and generally rendering identification impossible using conventional single-equation regression techniques.

Gomez & Hansford make an extremely compelling argument, yet their story raises a number of questions. One might wonder for instance how much policy platforms affect turnout empirically.

Nevertheless, what Gomez & Hansford’s argument does point out is that the frequently-cited single-equation research designs looking only at the correlation between

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5 Gomez & Hansford suggest another source of endogeneity: “A second way in which Downs’ logic suggests endogeneity is via the perceived closeness of the election. As the perceived probability of a close election (i.e., the closeness of the likely vote shares of the candidates) increases, the instrumental value of voting also increases and turnout levels should rise. Moreover, if it appears that an election will be close, candidates and parties also have a greater incentive to mobilize voters, another possible source of higher turnout (Cox & Munger 1989). Consequently, this could cause high turnout to appear to cause close elections (i.e., vote shares approaching 50%) when in fact the opposite relationship is perhaps more likely.”
partisan vote shares and turnout do not account for the larger strategic environment generating the observed data, limiting the usefulness of existing findings (cf., Pearl 2000). Indeed, Gomez and Hansford present preliminary empirical evidence (based on an instrumental variable approach to estimating a simultaneous two-equation turnout/vote share model) suggesting that the standard turnout/vote share correlational studies may have drawn incorrect conclusions about the relationship between U.S. turnout and the Democratic vote share. Specifically, Gomez & Hansford (p. 3) find evidence that “(1.) increases in turnout increase the vote share of Democratic candidates, and (2.) this partisan effect is conditioned by the partisan composition of the electorate.” As I discuss below, another sensible argument, an alternative to the specific point Gomez & Hansford make, suggests precisely that observational studies of turnout and left vote share should show little relationship even if turnout “matters.”

**B. Turnout and Leftist Policies**

As evidenced in Lutz & Marsh’s (2007) literature review, arguments that high turnout does not favor leftist policy are not common! To the extent that those who argue “turnout doesn’t matter” address policy outcomes—as opposed to electoral outcomes—the evidence they marshal is essentially the same survey evidence discussed above, namely, the well-documented similarities in the stated preferences of voting and non-voting survey respondents.
III. Evidence Supporting the “Turnout Matters” Thesis

A. Turnout and the Left’s Vote Share

The argument that high turnout should favor the left electorally is (1.) the poor are underrepresented at the polls, and (2.) the poor have views about issues and parties that differ markedly from other people’s, hence, (3.) if higher turnout meant a dramatic increase in the voting poor, voters as a group would have different—presumably more liberal—views than they do at present. The conclusion drawn is that the vote share of the leftist candidate would increase following a structural increase in turnout.

The first premise of this argument—that the disadvantaged are less likely to vote—is a widely reported empirical regularity. In America for instance, one can adduce numerous studies documenting low participation rates among the poor, less educated, and members of most racial, and ethnic minority groups (e.g., Macedo et al 2005; Verba, Schlozman, & Brady 1995; Rosenstone & Hansen 1993; Verba & Nie 1972). That Wolfinger & Rosenstone and numerous other scholars seem to have disproved premise (2.)—by presenting survey evidence that non-voters and voters have similar preferences—is not viewed as a death knell for those inclined to believe that the left’s vote share would increase if more citizens consistently voted. To wit, the conventional Wolfinger-Rosenstone finding that voters and non-voters express similar preferences is countered by an argument that says: “The debate over whether or not non-voters’ preferences are the same as voters’ is somewhat misleading because non-voters don’t have true preferences in the same sense that voters do.” This line of thought is embodied, for instance, in Lijphart’s (1997) presidential address to the American Political Science
Nonvoters who are asked their opinions on policy and partisan preferences in surveys are typically citizens who have not given these questions much thought, who have not been politically mobilized, and who, in terms of social class, have not developed class consciousness. It is highly likely that, if they were mobilized to vote, their votes would be quite different from their responses in opinion polls.

Three remarks are in order here. The first is that, as Lijphart argues, citizens’ preferences are theoretically malleable and may be influenced by the activities of political elites. This observation resonates with a large body of empirical evidence including the seminal work by La Palombara & Weiner (1966) and more cutting-edge theoretical work on partisan persuasion expenditures, e.g., Iaryczower & Mattozzi (2009; p. 8). The second point is that, if, as seems to be the case, Lijphart is imagining that the left’s vote share would increase following a large increase in turnout, there is a compelling theoretical reason to believe he is not correct. I explain this momentarily.

My final remark is that Lijphart is implicitly making an important conceptual point that is worth highlighting. He is imagining a counterfactual world following what I will refer to as a structural change in turnout—a nontrivial flux in the institutions that governs a country’s electoral process. Examples of structural changes include the extension of the franchise to women or ethnic minorities or repealing impediments to registration or voting like the removal of poll taxes or (as an example of a smaller change) implementing a Motor-Voter program.\(^6\) I distinguish structural turnout changes

\(^6\) Variation in the electoral incentives induced by first-past-the-post versus proportional electoral rules is another source of structural variation in the turnout regime (c.f., Jusko 2008; Cox 2007; Iversen & Soskice 2006; Blais & Carty 1990).
from changes that occur for other reasons, in constant legal regimes. I refer to these types of turnout fluctuations—like those that might take place when parties rally their bases in swing states, or when the weather is particularly pleasant on election day—as non-structural variation in turnout.

**B. Turnout and Equilibrium Policy**

A second rejoinder to the claim that turnout doesn’t matter focuses on the policy implications of increased turnout rather than electoral outcomes. Piven and Cloward (1988), for example, contend that:

> [T]he opinions elicited by surveys reflect the underdevelopment of political attitudes resulting from the historic exclusion of low-income groups from active political participation. In other words, what survey data cannot reveal is the dynamic dimension of politics. Political attitudes would inevitably change over time if the allegiance of voters from the bottom became the object of partisan competition, for then politicians would be prodded to identify and articulate the grievances and aspirations of lower-income voters in order to win their support, thus helping to give form and voice to a distinctive class politics.

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Piven & Cloward echo Lijphart’s argument that structural increases in turnout would incentivize political parties to acculturate or politicize the previously underrepresented. They also suggest a distinct argument: parties will adopt new equilibrium policies to court the new voters.

> Although Piven & Cloward do not refer to it, a natural way to think about their claim is in terms of the classic Downsian model (Downs 1957). If politics is reasonably

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7 Emphasis is mine.
8 Hotelling (1929) and Black (1948a,b) also helped formulate the model.
approximated as competition along a single policy dimension—representing, for instance, the progressivity of the tax code (cf., Meltzer & Richard 1981)—the Downsian prediction is that the equilibrium policies of party Left and party Right will converge at the most preferred policy of the median voter. A natural extension to the Downsian model suggests that, when higher turnout is expected to increase the relative number of voters with ideal points to the left (respectively right) of the current median, the equilibrium policies of both parties will shift left (respectively right). A logical necessity of the empirical imbalance in participation rates is that full turnout would introduce a population that is disproportionately poor, hence, if the Downsian model is correct, informed, self-interested voting should lead to a leftward policy shift following a large structural increase in turnout.

With respect to the kinds of research designs usually brought to bear on the question of how turnout changes affect policy outcomes, it is worth making two points here. In a Downsian equilibrium, once both candidates locate at the expected median, all voters are indifferent between them. Thus, (1.) Downs’ model is consistent with survey evidence that voters and nonvoters look similar in terms of their preferred candidate (even if non-voters are disproportionately poor). (2.) Since a Downsian model suggests both parties would reposition at the new median—and still garner roughly half the vote—it follows that structural changes in turnout should be fully anticipated by parties and thus a regression of L’s vote share on turnout—as utilized by DeNardo (1980) and Nagel & McNulty (1996) for example—is useless for assessing the impact of structural changes.
These observations allow me to make a more direct critique of the turnout/vote-share correlational studies than the one raised by Gomez & Hansford (2008).

**Proposition 1.1:** To the extent that candidates’ policy platforms track the location of the median voter, increased turnout will not be correlated with partisan vote shares.⁹

The result is a consequence of Proposition 2.1, developed in the next chapter. Notice, this Downsian argument also runs counter to Lijphart’s prediction that higher turnout would lead to a higher vote share for the left.

Generally speaking, empirical studies of turnout’s effect on equilibrium policy outcomes—as opposed to electoral outcomes—have been more supportive of the claim that higher turnout favors the left, though the picture appears decidedly complex. Sometimes the world seems to operate essentially as the Downsian model would predict. For example, in the U.S. context, Hajnal & Trounstine (2008) have found that higher turnout rates among minority groups increase the incidence of redistributive budget items in U.S. cities, and Martin (2003) documents a positive correlation between U.S. counties’ overall turnout and their receipt of targetable federal funds.

Other studies suggest that the relationship between turnout and policy is more nuanced however. One particular issue that has not been fully resolved is how parties and

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⁹ This proposition holds not just for the Downsian model but also for Wittman-esque models in which the candidates diverge symmetrically around the median.
partisan competition mediate the relationship between turnout and policy outcomes.

With regards to this question, in the American context, Hill & Leighly (1992) and subsequently Hill et al (1995) have looked at cross-state variation in AFDC generosity as a function of voting rates among the poor. Hill et al (1995, p. 83-85) conclude that, conditional on a state having vigorous partisan competition, politicians seem to respond to the mere threat of the lower-class vote—whether it tends to materialize or not:

States with higher levels of party competition, first, provide higher levels of welfare benefits. Competitiveness in the party system induces both parties to respond to the interests of the lower class, regardless of the level of mobilization…thus competitiveness enhances a generalized form of party responsiveness to the potential, not actual, participation of the lower class.10

In fact, Hill et al. find evidence of a second operative causal channel by which “mobilization”—i.e., higher voter turnout amongst the poor—affects welfare policy in their favor:

At the same time, the ideology and competitiveness of the relatively liberal party combine to effect a second path of linkage. The importance of lower-class mobilization for policy is entirely mediated by these attributes of the Democratic party—indicating a particularized form of party responsiveness to

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10 For Hill et al “mobilization” means “higher voter turnout among the poor.” In the past the terms “mobilization” and “turnout” have been used interchangeably, a possible source of confusion. To avoid confusion between two distinct but related concepts, I will reserve the term mobilization to refer to the act of encouraging someone to vote, not turnout itself.
actual political mobilization. Thus lower-class-voter mobilization is related to state policy by two discrete channels of opinion-policy linkage.\(^{11}\)

The conclusions of Hill and his colleagues—that turnout amongst the poor does affect redistributive policy (if somewhat indirectly)—sits uneasily with research by Radcliff & Saiz suggesting that turnout at the state level has no effect on left-right policy (Radcliff & Saiz 1998), and that, surprisingly, increases in minority turnout relative to white turnout are actually associated with “white backlash” and less spending on minority-preferred outcomes in the U.S. (Radcliff & Saiz 1995). This finding is consistent with numerous studies concluding that the “share of spending on productive public goods—education, roads, sewers and trash pickup—in U.S. cities (metro areas/urban counties) are inversely related to the city’s (metro area’s/county’s) ethnic fragmentation, even after controlling for other socioeconomic and demographic determinants” (Alesina, Baqir, & Easterly 1999). In other work, Alesina & Glaeser (2004) stress that parties politicize racial divisions in the U.S.—unlike Western Europe—leading to the observed differences in redistributive expenditures between the two regions in the postwar era.\(^{12}\)

The evidence from outside the U.S. paints a similarly complicated picture of the turnout/policy relationship. On one hand, Mahler’s (2008) empirical study of the first-order correlation between turnout and various measures of tax and expenditure

\(^{11}\) See also, Ringquist et al (1997).

\(^{12}\) Mebane (1994) theorizes, and finds empirical support for the notion that politicians have electoral incentives during election years to shift benefits from recipients of means-tested programs (who are less likely to vote) to non-recipients (who are more likely to vote).
redistribution in developed democracies supports the Downsian notion that higher turnout should lead to more leftist policies. But, while Mahler’s conclusions (2008) are consistent with the logic of the Downsian spatial model, his regressions incorporate no control variables of any kind for nations’ political institutions or level of partisan competition. More detailed cross-national analyses of the relationship between turnout and policy—notably Hicks & Swank (1992); Meuller & Stratmann (2003); and Brady (2003)—find that the relationship between turnout and policy is at least partially channeled through political institutions. Brady for example concludes that “left political institutions are essential to explanations of the comparative historical variation in poverty” (p. 557).

One remarkable counterexample to the natural theory that high turnout should engender pro-poor policy is India. According to Keefer & Khemani (2004), “India exhibits a large reliance on targeted transfer payments and subsidies,” and simultaneously, “significant underprovision of social services such as education.” For Keefer & Khemani, the concomitantness of these is “puzzling because the poor in India, who vote in large numbers, would benefit from more of the latter and less of the former” (p. 935). Keefer & Khemani’s explanation for the unexpected fact pattern is that “inadequate social services and excessive targeted transfers can be explained as a consequence of the incomplete information of voters, lack of credibility of political promises, and social polarisation.” Keefer & Khemani—like Hill et al (1995) in the

13 Peter Lindert (1986, 1994) deserves to be cited here.
American context—emphasize the role of party structure in mediating the Indian turnout/transfers relationship.

Note that I have now discussed two empirical studies where the assumptions of Downs’ model did not obtain, and the policy outcome did not conform to the model’s prediction. First, Downs assumes competitive parties. If the party system is not competitive, then parties may be less responsive to the poor (cf., Hill et al 1995). Second, Downs assumes credible parties. If parties are not credible, then politics becomes more porcine and policy trends right (cf. Keefer & Khemani 2004). Below I describe other instances of political settings in which Downsian assumptions are violated, and the Downsian prediction do not obtain. Below, I will highlight such data points when I come across them in order to develop a more useful model of the political consequences of structural changes in turnout.

IV. Partisan Preferences Over Structural Increases in Turnout

A. The View from the Democratization Literature

Academic arguments aside, what has been the actual response of elites faced with possible legal changes affecting the structural turnout regime? The general conclusion of the democratization literature—that the right opposes increases in structural turnout—is another argument that turnout matters. For the present purposes, by the democratization literature I mean the economic history and political science scholarship attempting to explain the incidence of large democratizing structural changes, like extensions of the franchise. One common conception of democratization processes is that the economic
“haves” extend the right to vote to the “have-nots” when the cost of repression becomes larger than the losses associated with extending the franchise to mollify some critical subset of the currently disenfranchised. For instance, looking effectively at the universe of democratizations occurring before 2000, Przeworski (2008) provides evidence that is both qualitative—he documents what the rich said when faced with the prospect of extending the franchise—and quantitative—suggesting that most historical franchise extensions were made under duress.

The broad empirical literature makes clear that there have been a number of specific historical exigencies leading to franchise extension, but while specifics vary, the general theme is consistent with Przeworski’s characterization. Acemoglu & Robinson (2007) for instance describe temporary shocks whereby the lower classes gained a temporary upper-hand against the ruling classes, and ossified these temporary gains in the form of constitutional changes to the voting regime. The Przeworskian account also comports with numerous other studies from the economic history and democratization literatures, notably North (1981); Lindert (1986, 1994); Engerman & Sokoloff (2005); and Tilly (1990, 1999).14

In contrast to the conception of franchise extensions as something made in the shadow of violence—a view most empirical studies advance—many famous political theoreticians have considered franchise extension as a dynamic “endogenous” democratic

14 Consult Therborn (1977); Rueschemeyer, Stephens, & Stephens (1992); and Collier (1999) for a review of the literature.
process, a sort of Pandora’s box leading to further and further democratic extensions of the franchise. For instance, Alexis de Tocqueville wrote that “[t]here is no more invariable rule in the history of society. The further electoral rights are extended, the greater the need for extending them; for after each concession, the strength of democracy increases, and its demands increase with its strength.” (1886, vol. 1 ch 4). John Adams (1776) echoed Plato when he warned, “Depend upon it, Sir, it is dangerous to open so fruitful a source of controversy and altercation as would be opened by attempting to alter the qualifications of voters; there will be no end of it. New claims will arise; women will demand the vote; lads from twelve to twenty-one will think their rights not enough attended to; and every man who has not a farthing, will demand an equal voice with any other, in all acts of state. It tends to confound and destroy all distinctions, and prostrate all ranks to one common level.” These early “endogenous” democratization theorists focused on sociological forces, arguing that a chain-reaction was the natural consequence of democratizing structural changes (and therefore possibly something to fear).  

More recently, formal theorists like Conley & Temimi (1990; 1999), Bolton & Roland (1997); Alesina & Spolaore (1997, 2003); and Jack & Lagunoff (2003) have considered other endogenous processes. These papers are unlike the modal empirical account in the sense that the decisive decision maker is the median of the currently enfranchised, not a representative member of the elite, and, unlike the older theories of “endogenous” democratization, these theorists stress economic motivations rather than

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15 See Popper (1962) for a discussion of Plato’s views on suffrage extensions.
16 Bolton & Roland (1997) could also be similarly characterized. Boix & Stokes (2003) use the term “endogenous” democratization to refer to a very different phenomenon.
sociological ones. Still, the formal structure is familiar in the sense that there is a decisive unitary actor—in this case, a forward-looking median voter. So the question in the political-economic perspective is, under what conditions will the median voter support structural changes relocating the next period’s median, hence, presumably, equilibrium policy?

One straightforward insight from the literature is that the effect of franchise extension on economic redistribution (in a restrictive one-dimensional policy setting, more on which below) can be understood in Downisan terms, along the lines of the well-known model developed independently by Romer (1975) and Roberts (1977) and extended by Meltzer & Richard (1981).\footnote{Duncan Foley (1967) developed another early version of the basic model.} Thus, while one can find many diverse stories in the literature pertaining to the specific costs and benefits of real or hypothetical extensions of the franchise, the next proposition, a close adaptation of the Romer-Meltzer-Richard result, conveys much of the theoretical insight.

Assume taxes are linear, redistributive (positive) transfers are lump sum, voters have single-peaked preferences over the one-dimensional tax rate space, and assume political institutions are such that the median voter is decisive. In such an environment—as Meltzer-Richard point out—the equilibrium tax policy depends on the position of the mean income relative to the income of the decisive voter’s, i.e., the median’s. If the mean income increases relative to the median voter’s income, the median voter would benefit from a more redistributive tax regime. In this setting it’s easy to see how a majority of
voters would feel about further extensions of the franchise. Proposition 1.2 describes what the political effect of leftist franchise extensions would be—it follows from a straightforward application of the Downsian result for political equilibrium in a unidimensional policy space (provided formally in Proposition 2.1 below).

**Proposition 1.2: (Romer-Roberts-Meltzer-Richard)** A structural change introducing voters whose incomes fall below the current mean income would generate a political push for economic redistribution financed by (net) taxes on incomes that are (relatively) high.\(^\text{18}\)

Real fiscal redistribution is far more complex than the unidimensional model assumes, involving an assortment of government tax and transfer programs whose net benefits generally vary non-monotonically across the income scale—so Downs could be problematic in practice. Nevertheless, Proposition 1.2 provides a useful benchmark for a simple tax and transfer technology: political pressure for redistribution depends on how the current median in the income distribution stands in relation to the mean income post-structural change.

I will make two unrelated points here. First, scholars do not find a strong empirical connection between the proximity of the mean and median incomes and the push for political redistribution (e.g., McCarty, Poole, Rosenthal 2006). Second, the VRA plausibly represents a third type of franchise extension—neither the duress model, nor the endogenous democratization model fit the southern case well. In the South,

formal suffrage extension, and the liberalization of southern society writ large, stemmed to a large extent from the efforts of an external third-party, the U.S. federal government. In this sense the passage of the VRA was clearly not a purely endogenous processes that took place in the shadow of local violence or otherwise.

B. The View from the Trenches

The picture authors like Keyssar (2000) and Piven & Cloward (2000) paint of the nineteenth-century battles over election law in the U.S. provides a thesis about parties’ reaction to structural changes impinging on turnout. Piven & Cloward write, “[s]o long as the local parties were dependent on voter constituencies at risk of being disfranchised by the new laws, they had ample motive to resist, amend, or circumvent the new registration procedures when they faced electoral challenge. And so long as the local parties were organizationally strong, they also had the capacity to resist amend, or circumvent them. In other words, the impact of laws and procedures cannot be assessed without taking into account the motives and capacities of the political parties involved in implementing the rules.”19 Empirically, the fact is that parties do seem to have cared very much about a wide variety of structural changes impinging on the turnout regime.

Witness for instance the partisan machinations animating the U.S. Supreme Court case Crawford v. Marion County, Board of Elections (2009). The local Democratic Party and other interests brought the suit against the Board of Elections in Marion County, Indiana on the grounds that the election laws employed in the county placed an undo

19 The quote is found on page 34. Italics in original.
burden on particular voters (who were “likely” Democrats). The 2005 state law
governing Marion County elections involved comparatively strict voter identification
standards, requiring that registered voters present a state- or federally-issued photo id
before casting a ballot. Of legal interest, it was the case that Indiana’s minority
population, largely black, did not possess valid photo id at the same rates as whites
(Barreto, Nuño, & Sanchez 2009). The court ultimately decided 6-3 in favor of the Board
of Elections on the grounds that the law was apparently disfranchising few (few potential
voters complained that they were turned away at the polls) but provided a potentially
useful deterrent to vote fraud. The court’s decision left open the door for future cases that
would define which partisan structural changes are constitutional.20 There are two
interesting observations to make here about parties and turnout.

One obvious point about the case is that it elicited considerable partisan interest,
suggesting the two parties believed that the structural turnout at stake could have affected
outcomes they cared about. Justice Stevens, for example, noted in oral argument that,
“[w]hile it is true that Indiana’s law mainly disenfranchises a group that primarily votes
Democratic, [thus potentially] violating the Fourteenth Amendment, it is also important
to take notice of the partisanship under which this law was passed, [because], in
Anderson v. Celebrezze, this Court contested a statute on the grounds that it was partisan
in intent. [Likewise, in Crawford v. Marion County], with the Democratic Party presented
as one of the parties to the case, and the Republican Party submitting an amicus curiae

20 The court also failed to offer a rubric for weighing the burden on individuals to
produce photo id against the state’s right to fight election fraud.
brief in support of the Marion County Election Board, *it is apparent that this is a partisan issue.*”

The expert statistical evidence presented in *Crawford v. Marion County* raises a second issue of interest here, related to parties’ role in encouraging turnout. Note that the specific empirical evidence actually brought to bear on how voter id laws affect turnout in the U.S. suggests that there is essentially no difference in voting rates across the gamut of empirically observed voter id regimes (Alvarez, Bailey, & Katz 2008)—and, while there are some recent structural innovations that do seem to have a discernable impact on turnout—e.g., portable voter registration and election day registration laws (McDonald 2008, Brians & Grofman 1999, Nagler & Leighly 2010)—the effect of lowered barriers to voting in recent decades is typically small and confined to certain subsets of the electorate. If common sense suggests that structural barriers depress turnout, why do scholars in the U.S. typically find no appreciable corresponding increase when specific structural barriers fall?

On this topic, it’s interesting to note the conclusion of studies like Karp & Banducci’s (2000) analysis of Oregon’s structural changes, which finds that “[v]oting only by mail is likely to increase turnout among those who are already predisposed to vote, such as those with higher socioeconomic status...the [new] voters will be limited most likely to those already inclined to vote but find it inconvenient to go to the polling place” (p. 223). Likewise, Highton (2004) comes to the conclusion that “[t]here is now little room for enhancing turnout further by making registration easier. It is still possible
to lower voting costs by extending polling place hours and mailing sample ballots and polling place information to registrants...yet continued non-voting by substantial numbers of citizens suggests that for many people, voting remains an activity for which there is virtually no gratification—instrumental, expressive or otherwise” (p. 512).

What role do political parties play? Brians & Grofman (1999, 2001) conclude that, “[m]ore than anything else, we believe that our research reinforces the enduring intuition that political participation requires political mobilization and education. Absent these prerequisites simply reducing citizens’ administrative costs [only] modestly affects the likelihood they will vote” (1999; p. 171). Below, in the next chapter I develop some theory that will allow me say more about the relationship between structural voting costs and partisan incentives to mobilize citizens.

Another piece of evidence of partisans’ interest in structural turnout is the position of Alabama’s Republican Party in 2003 on an effort to expedite the process by which the state’s ex-convicts are allowed to renew their voter registration. Marty Connors, chairman of the state party, was candid regarding his party’s motivation, “There’s no more anti-Republican bill than this. As frank as I can be, we’re opposed to it because
felons don’t tend to vote Republican.” The GOP’s frequent clashes with the now defunct organization ACORN is yet another example from the U.S.

So, how do these anecdotes square with the Downsian prediction that increases in turnout should have policy, but not partisan, implications? *Prima facie*, not well. In a strictly Downsian world where politicians lack personal preferences over policy and can instantaneously and costlessly adopt new platforms, they should care not a whit how high turnout is! Thus, we have a third failure of the Downsian model. Again we might ask if the Downsian prediction is erroneous because reality departs from the model’s assumptions. Two possible resolutions suggest themselves: (1.) Parties do in fact care about policy, not just winning election, or (2.) it may take time for parties to move to the new equilibrium—possibly because the voters must be convinced they would actually implement their new platforms if elected—hence the party least tethered to the current status quo (in the minds’ of voters) is temporarily advantaged following structural changes.

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21 *New York Times* (2008a). Voter registration laws are obviously not the only electoral parameters contested over by parties in the U.S., others include, e.g., district boundaries (Cox & Katz 2002) and at-large districting (Trebbi, Aghion, & Alesina 2007).

22 More evidence that parties care about the structural turnout regime comes from Australia and Belgium where it is clear that politicians believe abolishing compulsory voting in their countries would have an effect on election outcomes in the direction of helping the parties of the right: rightist parties favor, and leftist parties oppose the abolition of mandatory voting for this reason (Kris Deshouwer).
V. Towards a Resolution

A. A Critical Summary of the Literature

Considered as a whole, the most damning critiques of existing studies dealing with the impact of turnout is that they don’t distinguish between structural and non-structural changes and they fail to take full advantage of insights drawn from strategic models. As I noted above, Gomez & Hansford (2008) have made this second point. I made another specific argument along these lines, a different Downisan argument.

To summarize, recall the two main arguments that turnout “doesn’t matter”: (1.) turnout rates show very little correlation with the left’s electoral fortunes, measured in terms of the left party’s vote share, (hence low turnout does not favor the right), and, (2.) in terms of their stated policy preferences, if not their demographics, non-voting survey respondents look very much like voters (hence low turnout does not favor the right). Regarding argument (1.), I argue that the policy consequences of increased turnout, not the distribution of vote shares, is generally the more substantively important political variable. Moreover, as I formally demonstrate in the next chapter, the simple Downsian model predicts not only that turnout “matters” with respect to equilibrium policy—policy should shift towards the median ideal point of the newly turned out—but also predicts the low correlation between turnout and the left’s vote share that has been one of the bedrock findings of the literature (e.g. Nagel & McNulty, 2000; Pacek & Radcliff, 1995). In the next chapter I also rebut argument (2), along the lines of Lijphart (1997) and Piven & Cloward (1988), by constructing a natural model that accounts for the possibility that citizens’ preferences are malleable and could be altered as a consequence of partisan
responses to structural increases in turnout (resulting ultimately in policy changes consistent with the Downsian prediction).

To push further on my critique of the literature, even among analyses contextualized in terms of a Downsian model (explicitly or implicitly), there is often very little effort made to explore how one might explain unexpected outcomes in terms of modifications to Downs’ (possibly too) parsimonious model. For instance, among those studies that do employ a spatial logic, there are many debates—Did suffrage extension lead to Meltzer-Richards redistribution of income? (Clearly not.) Did suffrage extension lead to better welfare for the poor in the form of public schooling, social insurance, etc.? (There are arguments about the timing, but clearly yes “eventually.”) Those who purport to show that suffrage extensions failed to lead to the policy change predicted by the simple spatial model do not fully engage the model—so, it’s unclear how/if violations of the Downsian model’s assumptions explain their findings.

I propose a deeper test of the spatial logic by analyzing in detail the political consequences of the U.S. Voting Rights of 1965 (and related legislation) which removed structural impediments to voting in the U.S. South by, among other things, removing literacy tests and poll taxes (in local elections), and providing measures for federal oversight in localities where these obstacles were used to disfranchise large segments of the voting-age population. The dissertation attempts to understand turnout’s implications theoretically and empirically. I conclude this introductory chapter by laying out the two components of my research agenda. I first discuss my theoretical efforts to understand the
likely implications of structural changes in voter turnout, then preview my empirical work.

**B. Formalizing the terms of the debate**

In Chapter 2 I recast the important ideas from the literature in formal terms. I begin with the basic spatial model (Downs 1957) and add extensions useful for understanding my study of the VRA. Specifically, in chapter 2 I consider the impact of structural turnout on the equilibrium distribution of post-election, group-targetable government transfers. To do this, I employ a general model in the spirit of Cox, McCubbins & Sullivan (1984); Dixit & Londregan (1996) and Bartels (1998). I also study the comparative statics of parties’ equilibrium campaign expenditures following structural turnout changes (cf., Cox 2007; Dunning & Stokes 2010). Next, I draw out the implications of straightforward models of political persuasion to see how such structurally-induced reallocations of campaign resources could affect the distribution of mass preferences (hence equilibrium policy) in the long run. I conclude chapter 2 by discussing the current theoretical limitations in simultaneously modeling candidates’ public policy and transfer decisions.

**C. The Voting Rights Act as a case study**

On August 6, 1965, at a ceremony in the White House attended by an ebullient crowd that included Dr. Martin Luther King, Jr. and many other giants of the civil rights movement, President Lyndon B. Johnson signed the Voting Rights Act of 1965 (VRA).
By outlawing literacy tests (in the states that still employed them) and poll taxes (in local and state elections), and increasing federal oversight of local registration procedures, the VRA consolidated and expanded the gains of grassroots campaigns and federal efforts to assist African-Americans exercise their rights as citizens. These efforts had dramatic effect. Black voting rates in the South climbed from 3 percent in 1940 (Davidson 1994; 29), to 24.9 percent in 1956, to 43.1 percent in 1964. Shortly after the VRA, black voting obtained rough parity with Southern whites after the VRA—in 1968 for example approximately 62 percent of the eligible black population voted southwide (Stanley 1987; p. 97).  

My VRA research deals with the perennial question of how democratic outcomes reflect social preferences—in a society that is particularly interesting for its extreme polarity. In this regard, the first point to make about the southern case is that—stifled though politics was during much of the twentieth century—there is some evidence politicians were, in the end, still beholden to those citizens who could exercise their political rights. This point is plausibly illustrated by the career of George Wallace, four-term governor of Alabama. Wallace credited an early loss in a gubernatorial bid (in the 1958 Democratic primary to John Patterson), to his unpopular race position vis-à-vis his opponent. Having learned his lesson well, Wallace won a number of subsequent

23 Below, my frequent reference to the “Voting Rights Act” is somewhat imprecise in the sense that the focus of the study is on the impact of the increase in black voting rates, not the VRA’s impact *per se*. It should be noted that, though VRA is correctly viewed as the single most important piece of legislation securing the franchise of blacks, as I discuss below, it was not the only factor increasing black voting rates, particularly outside the Deep South. For the sake of convenience I refer to the numerous voting rights initiatives by the name of the federal legislation that solidified their gains.
elections (when blacks could not vote in large numbers) by vocally supporting segregationist policies. In colorful language he recounts his early experiences: “You know, I tried to talk about good roads and good schools and all these things that have been part of my career, and nobody listened. And then I began talking about niggers, and they stomped the floor” (PBS 2000). After African-American registration votes surged post-VRA however, by many accounts, George Wallace substantially moderated his anti-black positions (PBS 2000).

A second notable case is Strom Thurmond of South Carolina. As the presidential candidate of the conservative States’ Rights Party in 1948, Thurmond argued that “[t]here’s not enough troops in the Army to force the southern people to admit the Negro race into our theaters, into our swimming pools, and into our churches” (Caro 2002; p. 104). Thurmond subsequently played a critical role in the success of Nixon’s southern strategy, by stumping hard for the candidate in the 1968 GOP primary (apparently in exchange for the right to vet Nixon’s running mate). Still later, in 1971, Thurmond became the first southern senator to appoint a black staff aide and the first to sponsor an African-American for a judgeship (Kuzenski, p. 30). Thurmond’s office was also famous for its constituency service, which apparently extended to black constituents.

This dissertation asks, how common were responses like the one attributed to Wallace and Thurmond? Numerous aspects of southern politics frustrate a straightforward application of the Downsian logic. These include: (1.) a debile Republican Party; (2.) legislative cartels (that could gerrymander or exert negative
agenda control, for instance); (3.) party primaries; (4.) the multiplicity of purposive actors in the federal system; (5.) “white backlash” in the electorate; (6.) unprecedented demographic changes—e.g., black emigration/white immigration into the region and the urbanization of the southern populace; (7.) and the “leaky bucket” phenomena that occurs in any polity when laws are passed from the legislature to the government apparatus that implements them. The objective of my research is to provide a cleaner picture of how the simple Downsian logic played out in the extremely complex setting of the post-VRA South.

After Chapter 3—which provides background on the South’s politics and modifies the general predictions of Chapter 2 to suit the southern case—I begin a program of process-tracing the VRA’s political consequences. In Chapter 4 I look at the proximal political consequences of the civil rights era. One important piece of evidence in chapter 4 consists of the scaled policy positions of the South’s legislators (Poole & Rosenthal 2000). Another piece of evidence comes from the career decisions made by southern legislators in the wake of the civil rights movement. In Chapter 5 I conclude.

Finally, I note that my VRA research adds to numerous existing Downsian studies, many of which find the Downisan model wanting (e.g., Rodriguez 1999, 2004; Gerber & Lewis 2002; Gilens 2005; McCarty, Poole & Rosenthal 2006, pp. 124-138;

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24 Scaled Nominate positions give researchers a summary measure of large numbers of individual votes, as do ADA or LCCR interest group scores for example (cf., Poole & Rosenthal 2000). Below I analyze both Nominate scores and interest groups scores, though the former is generally preferred for the reasons laid out in Herron (2001).
Bartels 2008). One distinguishing feature of my case study is that I look for a Downsian effect following a particularly large, well-publicized exogenous shock—a la Kousser, Lewis, & Masket 2007—thereby strengthening my research design relative to studies exploring less extreme longitudinal variation, or cross-sectional variation in district electorates (cf., Trochim 2001). The next chapter argues that the VRA is a strong case study because it was (a.) large, (b.) exogenous, and (c.) preferences were highly polarized, so the Downsian prediction is unambiguous. A second distinguishing feature of my analysis is that I contextualize the spatial model more explicitly than is typical in studies of the U.S. South, an extremely interesting historical data point. In particular I consider the role of parties and partisan competition, redistricting, and other possibly confounding trends specific to the South in the 1960s—e.g., the countermobilization of white voters.
Chapter 2.  
The Theoretical Implications of Structural Turnout

I. Introduction

In this chapter I develop the theory that frames the rest of my discussion, by considering the simplest models of electoral competition that are sufficiently rich to discuss the main issues identified in the literature review. I derive three central propositions about the likely consequences of structural changes in turnout and a fourth ancillary proposition.

Regarding the framework I exposit below, one commonly finds in the literature three conceptualizations of politicians’ objectives when they compete in majoritarian elections. Variously, candidates are treated as though they attempt to maximize (1.) the number of votes they receive; (2.) their plurality; or (3.) the probability of winning. A second, orthogonal, distinction between existing models of electoral competition pertains to how one conceives of the policy motivations of the candidates. Some models presume that candidates care only about winning election in order to enjoy the private perks of office (e.g., Downs 1957), others make the more realistic assumption that the competitors also care about the public policy that is ultimately implemented (e.g., Wittman 1973; Cox 1984).

Yet another independent dimension along which models differ is whether or not voters are assumed to base their decisions in whole or in part on non-policy “valence”
differences between the candidates. It’s worth noting that theorists have considered a variety of specific reasons for the existence of candidate valence, including reputational advantages (e.g., Bernhardt & Ingberman 1985); unknown, differential capacities to provide constituency service (Londregan & Romer 1993); persuasive campaign expenditures (Baron 1989); the electorate’s past experiences with the candidates (Cox et al., 1984) and partisan attachments (Adams 1998). General versions of the valence-advantaged-candidate model have been studied by Ansolabehere & Snyder (2000); Groseclose (2001); Aragones & Palfrey (2002); and Hummel (2010). The various theoretical findings make clear that the valence assumption is a non-trivial departure from the Downsian model—for example, in the environments imagined by Bernhardt & Ingberman (1985) and Londregan & Romer (1993), pure policy-motivated candidates diverge in equilibrium, on either side of the median. In the chapters that follow, I will attempt to incorporate candidate valence when it furthers my empirical objectives.

For expositional purposes, in this chapter I will focus on a case that makes the mathematics as simple as possible without losing essential elements of the story. I assume (a.) that each party’s objective is to maximize its plurality, and (b.) candidates are interested in winning election exclusively for the sake of enjoying the perks of office-holding. I believe some version of the main results also obtains in less simplified models, an issue I return to in the conclusion of this chapter and in the chapters that follow.

The most substantively restrictive assumptions that I make are two. First, I assume that there is two-party competition (something that will need revisiting when I
turn to the case of the U.S. South). Second, I consider two polar cases, one in which parties choose unidimensional platforms (as in the classic Downsian model) and one in which parties are exogenously assigned public policy platforms and optimize instead over the vectors of feasible private transfers (as in the Dixit-Londregan model). Ideally, one would like a strategic model in which both the public policy and the transfers components of candidates’ electoral platforms are jointly determined (cf., Jackson & Moselle 2002). I return to this issue in the final section of the chapter.

The first two results I describe are widely assumed in the literature, if not typically formalized. The third and fourth results that I develop are more novel, however, I do not dwell on them here as they turn out to play a limited role in my VRA case study.

II. Structural turnout in a Downsian world

Imagine that citizens may either turn out to vote, or abstain. If they turn out, they are assumed to vote for the candidate whose public policy platform brings them the highest utility. With respect to their preferences over public policy, citizens are assumed to have a continuous, single-peaked utility function defined over the unidimensional policy space $X$ (cf., Downs 1957), which might represent policy pertaining to society’s (non-economic) treatment of minorities, for instance. Denote voter $i$’s ideal policy $v_i$ and assume the distribution of voter ideal points is non-degenerate. Below, citizens’ turnout decisions will be determined in part by the actions of the candidates. At this point however, it is convenient to treat voters’ turnout rates as exogenous—i.e., beyond the control of the candidates. The first result is a formal statement of Piven & Cloward’s
prediction that increasing turnout among left-of-center groups would lead the candidates to adopt more leftist platforms.

**Proposition 2.1:** When structural changes in turnout increase the relative number of voters with ideal points left of the current mean/median, equilibrium policy shifts left.

**Proof:** The result follows trivially from the well-known Downsian model, of which there are two variants: one for settings in which, from the candidates’ perspective, voters’ ideal points are deterministic, another for settings in which they are probabilistic. In both versions, the two parties locating at the mean/median (and splitting the vote) is the unique Nash equilibrium.

That both parties adopting the appropriate centrist policy is an equilibrium is demonstrated by considering a unilateral movement by either party away from the center: Such a move would result in the (stationary) opponent garnering more than half the vote (in a deterministic world) or more than half the expected vote (in a probabilistic world), hence would not take place (in equilibrium). A similar thought experiment demonstrates that the equilibrium is unique: should one or both parties locate at a non-centrist position the other party would defeat her (with certainty, or in expectation) by moving to the center. (For a more extensive discussion of spatial models of elections see, for example, Enelow & Hinich 1984)

It follows obviously from the Downsian result(s) that, should structural changes in
turnout precipitate a leftward shift of the median voter (in a deterministic world) or the mean voter (in a probabilistic world); both parties would relocate to a new, more leftist, mean/median. Q.E.D.

Generically, the equilibrium described in Proposition 2.1 will involve a change in policy, in the direction of the new voters’ median. In fact, if one relaxes the assumption of non-degenerate voter preferences—a situation we might think of in some settings as perfect bloc voting—one can rationalize a non-movement in policy. For instance, suppose the population of voters is comprised of black and white voters with population mass .4 and .6, respectively, whose ideal points are stacked at $\mu_B=0$ and $\mu_W=1$, respectively. In this degenerate setting, no structural increases in the black proportion of the electorate $\beta \in [0, .1)$ will change the equilibrium policy from $x^*=1$, because the median wouldn’t change. The point to make is that the magnitude of the policy change is a real-valued function of the distribution of voter ideal points—and unlikely to be linear, for instance (cf., Barreto & Grofman 2010).

More generally, speculating on the likely policy implications of structural turnout shocks in an actual election, say the next presidential election in the U.S., forces one to conjecture how higher turnout would affect the location of the median voter. Recall from chapter 1 that we have reason to believe that the newly turned-out would shift the national median left—in the modal democracy, non-voters are more disadvantaged as a

\[25\] In a perfect Downsian model, an unrealistically frictionless benchmark, the generic result is that policy will change. In the real world, barriers to candidate entry may affect the actual response—I return to this issue below in my empirical VRA analysis.
group—and reason to believe they would not—non-voters express the same policy preferences as voters.\(^{26}\) Regardless of how structural changes would affect the median in the short run, in Proposition 2.3, I present a formal argument, along the lines of Lijphart (1997), that higher turnout should eventually precipitate a leftward shift of the median voter (hence a leftward shift in equilibrium policy). Before doing so, I point out another intuitive implication of models like the present one, namely, a group’s turnout rate affects its receipt of targetable government expenditures.

**III. Structural turnout and private transfers**

To see this, let us switch gears from a purely Downsian model to a model in which each candidate seeks office by making promises of private transfers to the various (well-defined) groups in the electorate, conditional on already (exogenously) possessing a public policy platform. In this model, citizens evaluate candidates according to both the private transfers the candidates (credibly) promise and the public policy component of their platforms.

Without loss of generality, imagine there exist transfer technologies that define the candidates’ ability to distribute a finite sum of post-election consumption transfers across a partition of society into \( G \) groups (each of which may contain as few as one member). Denote the size of the \( g \)th group as \( N_g \). Denote by \( c_L \equiv (c_{L1}, \ldots, c_{LG}) \) the vector of consumption transfers Left (credibly) commits to make conditional on winning the

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\(^{26}\) A third possibility is that reactionary, non-structural countermobilization amongst current voters—as witnessed to some extent in the post-VRA South—actually leads to a net increase in the number of voters right of the current median.
election. Denote the budget constraint on total transfers as $C$.

Let $U_g(c_{Lg}) - U_g(c_{Rg})$ be the utility difference a voter of group $g$ feels with regard to the transfers offered by the two parties and note that such a voter is indifferent between the candidates precisely when his extra utility of consumption should Left win rather than Right equals his ideological preference for Right. Denote this ‘indifference cutpoint’ $X_g(c_{Lg}) = U_g(c_{Lg}) - U_g(c_{Rg})$ and assume it follows a group-specific cdf denoted $L_g(X_g(c_{Lg}))$, which represents the probability that a member of group $g$ supports Left—when she promises $c_{Lg}$.

I assume that each candidate chooses transfers so as maximize her expected plurality. Formally, Left’s choice problem is as follows (Right’s is defined analogously):

$$\max_{c_L} E[V_L(c_L) - V_R(c_L)] = \sum_{g=1}^G N_g T_g [2 L_g(X_g(c_{Lg})) - 1] \quad \text{s.t.} \quad C \geq \sum_{g=1}^G c_{Lg};$$

where $T_g$ is group $g$’s exogenous turnout rate. The next proposition is an analog to a result in Cox, McCubbins, & Sullivan (1984; p. 236) who proved a similar result when the politician’s maximand is her raw vote tally.

**Proposition 2.2:** Should the poor (or any other targetable group $g$) turn out in higher numbers (i.e., should $T_g$ increase) they would receive a larger proportion of government expenditures.

**Proof:** First, note that the theoretical framework is standard. I present a modified version of Dixit & Londregan’s (1996) exposition, the differences being that (1.) I incorporate
the more realistic assumption that candidates maximize their expected plurality, rather
than the number of votes they receive, (2.) I drop DL’s assumption that the pool of
available transfers is a sum that must be collected through taxation—because it adds little
to the analysis—and instead make the simplifying assumption that the government’s
allocateable budget is exogenous, and (3.) I generalize DL to consider the case of variable
turnout. Conditions guaranteeing the existence of an equilibrium (via Glicksburg’s
Theorem) are discussed in DL (p. 1149). In the present model I will follow DL and
assume an interior solution.

Consider groups g and h that are identical in all respects except \( T_g > T_h \). Denote
\( L_g = L_h = L \); \( U_g = U_h = U \); \( N_g = N_h = N \); and \( c_{Rg} = c_{Rh} = c_R \). Note that the first order
conditions for a maximum \( c_L^* \) require that \( L_j' (X_j(c_L^*)) U_j' (c_L^*) [N_j / \lambda] = 1/T_j \) for each
\( j \in \{1, \ldots, G\} \). Thus it follows from \( T_g > T_h \) that \( L_h' (X_h(c_L^*)) U_h' (c_L^*) [N_h / \lambda] = 1/T_h > L_g' (X_g(c_L^*)) U_g' (c_L^*) [N_g / \lambda] \), equivalently, \( L'(U(c_L^*) - U(c_{Rh})) U'(c_L^*) < L'(U(c_L^*) - U(c_{Rh})) U'(c_L^*) \). There are two exhaustive cases to consider, \( L''(X_g(c_L^*)) < 0 \) and \( L''(X_h(c_L^*)) > 0 \). Consider case 1. Since, in this case, the marginal return to \( c_{Lg} \)
decrees monotonically with \( c_{Lg} \) it follows that \( c_{Lg}^* > c_{Lh}^* \). Thus we turn to case 2, and note
that \( \delta^2 \mathbb{E} [V_L - V_R] / \partial c_{Lg}^2 = 2N_g T_g [U' L'' U' + L' U''] \). Note that if \( U' L'' U' < -L' U'' \) then the
conclusion obtains. We make that assumption at this point and note that it is satisfied for
a large set of reasonable distributions \( L \).\(^{27}\) Q.E.D.

My framework shares general features with a number of well-known and obscure choice theoretic models of the electoral determinants of private government transfers (cf., Cox et al 1984, 1986; Coughlin 1986; Lindbeck & Weibull 1987; Myerson 1993; Dixit & Londregan 1996, 1998; Bartels 1998). The model is also thematically similar to Jackson & Moselle’s (2002) model of legislative bargaining in a richer social choice setting wherein the public policy and distribution of private goods are jointly determined. Despite the theoretical shortcoming of my approach vis-à-vis a model like Jackson & Moselle’s, I note that my result is consistent with the findings of Martin (2004); Hill & Leighly (1992); and Hill et al (1995).

**IV. Structural turnout and equilibrium campaign expenditures**

In this section, I focus on how candidates would reallocate campaign expenditures following a structural change in voter turnout (cf., Cox 2004; Nichter 2008; Stokes & Dunning 2010). I suppress the distributive aspect of politics by setting \( C=0 \) and imagine that every citizen in group \( g \) has a structural voting cost \( \sigma_g \), normalized in such a way that positive values of the variable (“costs for voting”) decrease the likelihood of voting and negative values (“costs for not voting”) increase the likelihood of voting.

Imagine further that Left and Right can affect citizens’ probability of turning out by

\(^{27}\) For example, if \( U(c) = \ln(c) \), the condition is satisfied whenever \( L \) is near uniform.
making mobilization expenditures, $m_L$ and $m_R$, that increase the likelihood that a citizen from a targeted group will turn out. For the sake of convenience, assume that the partition of citizens into mobilizable groups is the same as that which defined the groups of targetable-benefits recipients. We can interpret the mobilization expenditures that candidates direct toward the $g$th group as subsidizing its members’ trip to the poll.  

Denote Left’s mobilization expenditures as $m_L = (m_{L1}, \ldots, m_{LG})$.

I assume that citizens who do turn out vote for the candidate that brings them the highest utility. As has been discussed, one campaign variable politicians manipulate in their efforts to woo voters is public policy; another is distributive policy. A third activity candidates engage in is expending effort to shape citizens’ preferences directly (cf. La Palombara & Weiner 1966). I will refer to such efforts “persuasion expenditures.”

Persuasion expenditures will be thought of as increasing the valence of the candidate on whose behalf they are spent, e.g. by providing testimonials to competence, or by reducing uncertainty about the candidate’s policy position. Let $p_L = (p_{L1}, \ldots, p_{LG})$ denote the vector of persuasion expenditures Left directs towards the $G$ groups.

Suppose that, prior to an election, Left (similarly Right) must decide how to allocate a vector of campaign expenditures $B_L=(B_{L1}, \ldots, B_{LG})$ across the two competing uses just

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28 Formal theorists have considered a distinct type of mobilization, which we might call “policy-mobilization,” wherein citizens’ turnout decisions depend on the policies at stake in the election. Examples include Hinich & Ordeshook 1(969); Bartels (1998); Adams, Dow, & Merrill (2005); and Peress (2007). See section II of chapter 1.

29 Momentarily, we will consider a specific behavioral model of citizens’ responsiveness to political persuasion in order to explore the long-term versus short-term consequences of structural changes in voting.
described. Candidate Left can expend resources \((a.)\) persuading voters—by making persuasion expenditures \(p_L\)—and \((b.)\) mobilizing voters—by making mobilization expenditures \(m_L\). The notion is that Left has already decided the total expenditures it will target towards each group \(g\) and now asks how to divide that expenditure between mobilization and persuasion. So, how does Left choose an optimal allocation \((p_L^*, m_L^*)\)?

Recall that Left’s objective is to maximize her expected plurality of voters, denoted \(V_L - V_R\). Let \(t_{jg}\) be a random variable equal to 1 if individual \(j\) of group \(g\) turns out and zero otherwise. Let \(\alpha^L_g\) and \(\alpha^T_g\) represent all the group-specific factors affecting \(g\)-members’ preference for Left and their propensity to turn out, respectively. Let \(T_g(m_{Lg} | \sigma_g ; \alpha^T_g) = \Pr[t_{jg} = 1]\) be a function representing the probability that a randomly sampled member \(j\) of group \(g\) turns out, and let \(L_g(p_{Lg} | \alpha^L_g)\) be a function representing the probability that such a citizen supports Left, conditional on having decided to turnout. Note that \(T_g(\bullet)\) is independent of \(p_{Lg}\) and \(L_g(\bullet)\) is independent of \(m_{Lg}\). Thus, in the terms just defined, Left’s choice problem is as follows.

\[
\max_{p_L, m_L} E[ V_L(p_L, m_L) - V_R(p_L, m_L) ] = \sum_{g=1}^{G} N_g T_g(m_{Lg} | \sigma_g ; \alpha^T_g) \left[ 2L_g(p_{Lg} | \alpha^L_g) - 1 \right]
\]

such that \(B_{Lg} \geq p_{Lg} + m_{Lg}\) for each \(g\).

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\(^{30}\) In addition to \((p_{Lg}, m_{Lg})\), it is reasonable to assume that Left’s total expenditure on \(g\) (viz., \(B_{Lg}\)) is endogenous as well, and depends on Right’s budget for \(g\) (among other things). Hence, one would want to study the candidates’ equilibrium expenditures on \(g\), \((B^*_L, B^*_R)\), game-theoretically.
Recall that when the net structural incentives to vote are such that $\sigma_g > 0$, I refer to these as a “cost” to voting—i.e., the structural inducements depress turnout relative to the baseline turnout rate. Likewise, if $\sigma_g < 0$ the net structural incentives are a “subsidy” which increase participation relative to the baseline rate. Thus, if we let $m^*_{Lg}(\sigma_g)$ denote the optimal mobilization expenditure, given $\sigma_g$, then $\partial m^*_{Lg} / \partial \sigma_g > 0$ means that a decrease in $g$’s structural costs of voting—i.e., an increase in $g$’s incentives to vote—decreases $m^*_{Lg}$, the expenditures Left makes mobilizing members of group $g$. Let $L_g(p_{Lg} | \alpha^L_g ) = F_{Lg} (\alpha^L_g + p_{Lg})$ where $F_{Lg}(\bullet)$ is a cdf with corresponding pdf $f_{Lg}(\bullet)$. Likewise, let $T_g(m_{Lg} | \sigma_g, \alpha^T_g ) = F_{Tg}(\alpha^T_g + m_{Lg} - \sigma_g)$ where $F_{Tg}(\bullet)$ is a cdf with corresponding pdf $f_{Tg}(\bullet)$. Making use of the budget constraint we can substitute $p_{Lg} = B_{Lg} - m_{Lg}$ into Left’s maximand and restate it as follows,

$$\max_{p_{L}, m_L} E [ V_L(p_{L}, m_L) - V_R(p_{L}, m_L) ] = \sum_{g=1}^{G} N_g F_{Tg}(\alpha^T_g + m_{Lg} - \sigma_g) [2F_{Lg}(B_{Lg} - m_{Lg} + \alpha^L_g) - 1].$$

Lemma 1 gives one obvious condition under which Left’s optimal mobilization expenditure on group $g$ will be zero. Lemma 1 is formally proven in the appendix following this chapter.

**Lemma 1:** If $[2F_{Lg}(B_{Lg} + \alpha^L_g) - 1] < 0$, $m^*_{Lg}(\sigma_g) = 0$. In words, Left will not choose to mobilize its opponent’s supporters.

With respect to formalizing the main arguments of the “turnout matters” camp, Propositions 2.1 and 2.2 are the most central. The next claim, Proposition 2.3, is a more
novel insight suggested by the formalism just developed. The intuition behind this result is simply that, from a candidate’s perspective, when a group’s exogenous turnout rate increases (viz., when structural changes increase its members propensity to vote), a marginal dollar of expenditure is better spent persuading voters, rather than providing mobilizational subsidies (for citizens who are likely to turn out anyways). The appendix following this chapter contains a formal proof of Proposition 2.3.

**Proposition 2.3:** Suppose $F_{Lg}$ and $F_{Tg}$ are symmetric about their medians and that $F_{Tg}(\alpha^T_g - \sigma_g) > .5$. When a group’s structural voting costs ($\sigma_g$) decline, raising its voting rate, Left (similarly Right) devotes no smaller a proportion of the expenditures earmarked for that group (viz., $B_{Lg}$ or $B_{Rg}$) trying to persuade its members. That is, $\partial m^{*}_{Lg} / \partial \sigma_g \geq 0$.

V. Long-term versus short-term consequences of structural changes

What are the most likely long-term consequences of a sustained structural change in turnout? First my model suggests that (under certain reasonable circumstances) higher turnout will lead candidates to spend more money on political persuasion. The natural way to think about the long-term effect persuasion expenditures is to start with the very general model of voting behavior with valence-advantaged candidates—the seminal papers are Ansolabehere-Snyder (2000) and Groseclose (2001). In this framework, one can imagine that the relative partisan valence is a function of the historical persuasion expenditures of the parties. If one pushes a model with these microfoundations one can

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31 Iaryczower & Mattozzi (2009; p. 8) for instance, develop a model along these lines.
arrive at another ancillary proposition relevant for theorizing about the long-term implications of structural changes.

The notion in the next proposition is that after the structural increase, Left will shift a larger portion of its expenditures to persuasion than will Right. Once the change in equilibrium expenditure patterns has had time to influence voters there may be a larger leftward policy shift than might occur in the very short term (when the newly turned out will tend to have more centrist preferences). Proposition 2.4 thus ties the theory embodied in Proposition 2.3 to Arend Lijphart’s (1997) ideas regarding the politicization of non-voters, see chapter 1. The appendix to this chapter formalizes a version of these ideas and contains a formal proof of Proposition 2.4.

**Proposition 2.4:** Suppose a structural change increases turnout and (per Proposition 2.3) causes parties to spend more on persuasion. Suppose further that parties’ persuasion efforts have cumulative effects on voters’ preferences and exhibit diminishing marginal returns. Then “naturally” leftist voters will eventually prefer left-of-center public policy following a structural increase in turnout.

If Wolfinger & Rosentstone’s (1980) evidence is appropriate, electoral results and equilibrium policy would not change much in the short run—structural changes would introduce persons who, while relatively disadvantaged on average, would fall more or less evenly across the ideological spectrum. Nevertheless, in the long run, if preferences are influenced by the parties’ cumulative persuasion efforts, policy preferences (hence
equilibrium policies) are likely to change. I note that empirical studies of how persuasion expenditures impacts mass preference include Green & Krasno (1988); Kenny & McBurnett (1994); Gerber (1998); Mueller & Stratmann (1994); Stratmann (2009).

The theory has some bearing on a normative problem commonly alluded to in empirical studies of the U.S. and elsewhere, namely, the existence of groups who chronically under-vote and are less inclined to engage in quantifiable political behaviors generally (see e.g., Piven & Cloward 1988; Bachrach & Baratz 1962). In the international context, Huber & Stephens (2001), for example, note a “group of long-term unemployed, mostly people with low skills...[which] lacks organization and power and is thus acted upon rather than being an actor in shaping the welfare state” (p. 18).

Recall from chapter 1 (section IV) the robust empirical finding that lowered structural barriers in recent decades seem to have only increased voting propensities among citizens who were already marginally inclined to vote, generally white middle-class voters. Thus, what one finds in the literature is the widespread view that (1.) a sizeable group of politically disengaged non-voters exist for whom “voting has no instrumental or expressive value” and for whom “a pessimistic conclusion cannot be avoided” (Highton 2004; p. 512), and (2.) conventional structural changes are generally insufficient to make these despondent citizens politically active.

Cf., Iaryczower & Mattozzi (2009).
Another view is that partisan mobilization and politicization are the only obvious means (short of legal compulsion) to substantially increase turnout beyond its current levels—this view is found for instance in Brians & Grofman (1999, p. 171). The theory presented here adds a more nuanced version of how that type of mobilization might take place—sustained changes lowering structural barriers may have secondary effects on turnout rates among very inactive groups if they incentivize political parties to re-direct persuasion and/or mobilization efforts towards the disaffected.\footnote{Green & Gerber (2004) discuss the efficacy of partisan get-out-the-vote drives. \textit{Alvarez, Bailey, & Katz} (2008); McDonald (2008); and \textit{Karp & Banducci} (2000) have studied the effect of structural incentives to participate. Shachar & Nalebuff (1999) and Zeng (1998) have studied the strategic incentives of parties to mobilize and Gay (2009) studies the effects that derive from one’s embeddedness in particular social contexts.}

**VI. Robustness of the propositions**

In this chapter I’ve formalized two common arguments about turnout’s effect on equilibrium policy and two less common claims about the implications of structural turnout on partisan outcomes. The main theoretical conclusions—Propositions 2.1 and 2.2—suggest that policy should respond to benefit the new voters. Proposition 2.3 holds that structural increases in turnout will generally induce parties to deemphasize mobilization in favor of persuasion (cf., Cox 2004), and Proposition 2.4 advances an argument about the long-term implications of structural increases, suggesting that structural inducements will lead parties to politicize previous non-voters, thereby changing the long-run equilibrium distribution of voter preferences.\footnote{There is a developing literature that studies aspects of political persuasion—see e.g., Cox (2006) and Iaryczower & Mattozzi (2009).}
Are the central claims robust to natural modifications of the underlying assumptions? Recall that the suspect Downsian assumptions are: (1.) Competitive parties (cf. Hill et al 1995, Frymer 1999); (2.) policy-neutral parties (cf., Witman 1973; Cox 1984); (3.) unidimensional politics (cf., Jeong, Miller & Sened 2008); and (4.) credible parties (cf., Keefer & Khemani 2004). I return to departure (3.) below. Since the assumption of credible parties is not of particular concern presently, I will pause to consider generalizations to (1.) and (2.) only.

Note first that one obtains a result analogous to Proposition 2.1 when one discards the restrictive Downsian assumption of policy-neutral parties—in an environment a la Wittman (1973), Cox (1984), and Calvert, henceforth WCC, policy favors groups that turn out because parties’ equilibrium policies track the median. Though the basic insight of the Downisan model is unchanged in a WCC model, there are two sensible reasons for incorporating the additional complexity.

Consider a situation that bears some semblance to the post-VRA South, as depicted in Figure 2.1. Suppose that the two plurality-maximizing candidates Left and Right have preferences over policy outcomes which lead them to adopt equilibrium policies $L_t$ and $R_t$ in period $t$, prior to a change in the structural turnout regime. In a WCC environment like this, the equilibrium platforms locate the candidates on either side of the median voter and the winner is determined, effectively, by a coin-flip. Now imagine that a structural change in voter participation rates leads to an influx of leftist voters in period $t+1$, shifting the median from $\mu_t$ to $\mu_{t+1}$. The increased turnout induces the candidates to
re-equilibrate to $L_{t+1}$ and $R_{t+1}$, left of their old platforms.

![Diagram of policy move in a Wittman environment](diagram)

**A Rightward Policy Move in a Wittman Environment**

**Figure 2.1**

Of relevance to the post-VRA U.S. South, where the partisan valence differential shifted in favor of the GOP, if we imagine that Left wins the coin-flip in period $t$ and Right wins in the next period after the structural change, a naïve analysis would suggest that the Downsian model is inoperative. The introduction of left-of-center voters apparently generated a rightward shift in policy! For this reason I will attempt to control for partisan forces in my econometric analysis of the Downsian mechanisms.

Still another reason for preferring a more general model that considers policy-oriented candidates is that it generates the following intuitive prediction, which is consistent with anecdotal evidence presented in Chapter 1.\(^{35}\)

**Proposition 2.5:** Suppose (à la Wittman-Cox-Calvert) that parties care about policy and winning election. Then Right will oppose structural increases in turnout and Left will favor them.

\(^{35}\) Generalizing the framework also puts the model in line with the abundant evidence of policy-oriented parties (e.g., Ansolabehere, Snyder, & Stewart 2000, Burden 2004, Levitt 1999).
Finally, the Downsian assumption that elections are competitive—in the sense that neither party enjoys an electoral advantage beyond that conferred by their respective public policy platforms—will demand some attention. In this respect, the one-party South was very unlike the world Downs imagines. As I discuss at length in the next chapter, beginning more or less with the resolution of the contested presidential election of 1876 (and certainly by the time the Bourbon Democrats put down the Populist uprising of 1896) until the partisan realignment of the 1960s, southern politics were famously uncompetitive, particularly with respect to inter-party competition (Key 1948). When it becomes necessary to think about the effects of partisan valence advantage, I will turn to models like Ansolabehere & Snyder’s (2000) and Groseclose’s (2001) for theoretical guidance.

VII. Pure public policy versus targetable government expenditures

For all their simplicity, the Downisan model and derivative models—e.g., the Witman-Cox-Calvert policy-motivated candidate models, the Romer-Richard-Meltzer redistribution models, and the Shepsle-Bernhardt-Ingberman valence-advantaged candidates models—only consider a unidimensional public policy space. Thus, another difficulty in using the Downsonian model (and its derivatives) to frame richer environments like the post-VRA South is that it does not allow analysts to consider political competition in more than one public policy dimension (cf., Jeong, Miller, & Sened 2009), or the electoral determinants of the many privately-valued services real-world governments fund—e.g., trash collection, neighborhood recreation facilities, local
education or infrastructure expenditures, or private government contracts. I discuss the issue of multidimensionality in the public policy space in chapter 4.

Regarding the distribution of targetable government expenditures, real world politicians typically have some discretion over the division of these private transfers, subject to a finite budget and the available technologies for targeting such expenditures. If there are $G$ distinct groups, a fully accurate equilibrium model of electoral politics would thus specify at least a $G$-dimensional strategy space for each candidate—wherein s/he vies for office by choosing a public policy platform (specifying a position in each distinct public policy dimension) and simultaneously optimizes over the $G$-1 independent transfer decisions.

The seminal theory pertaining to the electoral determinants of government expenditure patterns is due to Cox et al, (1984, 1986); Coughlin (1986); Lindbeck & Weibull (1987); Myerson (1993); and Dixit & Londregan (1996, 1998). A weakness of these first generation models—and the model I developed above, which follows them—is that candidates only choose how to distribute a private good, i.e., choose a feasible vector of government expenditures. In each of these models public policy is taken to be exogenous, as though it were chosen prior to the start of the distribution game. This is an intuitively restrictive assumption.

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36 See Persson & Tabellini (2000) for a review of these models. Bartels’ (1998) model is technically different but has a similar flavor—see the festschrift for Stan Kelley.
Equilibrium models of electoral competition wherein vote buying with private transfers and Downsian spatial competition are jointly determined are very few—Roemer’s (2001) model is the only one I am aware of. What Roemer’s work seems to point out is that restrictive (potentially ad hoc) assumptions are necessary to find a solution. Roemer himself posits a particular intra-party bargaining game that generates his equilibrium.

In a related context—a Baron-Ferejohn legislature—Jackson & Moselle (2002) have extended the standard Rubenstien bargaining framework to consider “a legislature that must make a decision about both an ideological (or public good) dimension, over which legislators have single peaked preferences, and a purely distributive (or private good) dimension for which each legislator prefers to have a larger amount allocated to his or her constituency” (p. 50). Jackson & Moselle’s findings suggest that in equilibrium “[t]he outcome will generally not consist of a median decision in the [public policy dimension] and separate bargaining over the distribution of spending” (p. 51).37 Evidently, neither Roemer’s or Jackson & Moselle’s model features a variable electorate. For now, I can only note that more realistic models incorporating both public and private goods might generate more nuanced predictions than the propositions I have advanced.

37 Volden & Wiseman (2007, 2008) have studied a similar extension of Baron-Ferejohn.
Chapter 3.
The Downsian Logic and the Peculiarities of Southern Politics

I. Introduction

For years, securing African-Americans’ right to vote was a special objective of the civil rights movement. The brutal repression by Dallas County sheriffs and Alabama state troopers of the Selma-to-Montgomery march on March 7, 1965—an event meant to promote voter registration in a county where less than one percent of voting-aged African-Americans were registered, and 80% of them lived below the poverty line—galvanized the forces of African-Americans and racially liberal elites (mostly northern members of the Democratic party) to obtain de facto suffrage for black southerners. On August 3, 1965 the resolution that would become the Voting Rights Act passed the U.S. House along very regional lines, by a vote of 328 to 74, and an identical bill passed the Senate by a vote of 79 to 18 the following day. On August 6, 1965 President Johnson signed the Voting Rights Act into law, effectively eliminating the South’s ability to employ its most commonly used methods of preventing its black population from participating in elections.¹

In the economic realm, the evidence strongly suggests that the civil rights legislation preceding the VRA (and precursors to the civil rights program broadly construed) increased African-American social welfare. For instance, by the ‘60s black per capita income was converging on white levels (cf., Donahue & Heckman 1991), as

¹ This chapter also describes other important structural changes to southern elections, notably the 24th Amendment, the Civil Rights Act of 1964 and the Supreme Court case Smith v. Allwright.
were African-American infant mortality rates (cf., Almond, Chay & Greenstone 2007). In the realm of education, in terms of pupil-teacher ratios, average term length, and teacher pay, black schools had progressed to near-equality with white schools already by the late 1950s (cf. Card & Krueger 1992). Donahue, Heckman & Todd (2002) for example document significant improvements in both primary and secondary schooling and average educational attainment for southern blacks, and Heckman & Payner (1989) convincingly tie these improvements to social activism in the first half of the twentieth century, specifically interventions on the part of philanthropic foundations and litigation by the NAACP.

Black matriculation in institutions of higher education also increased in the 1960s, also apparently independent of the VRA and the other structural changes affecting the southern voting regime (cf., Wright 2003). According to Gavin Wright, Titles VII and VI of the Civil Rights Act of 1964 (outlawing employment discrimination) increased the returns to schooling for blacks and is therefore responsible for the documented increase in black enrollment in institutions of higher education. Wright concludes that “[i]n essence the Civil Rights revolution of the 1960s launched a rapid upgrading of black education in the South, and these gains have been largely maintained if not extended over the intervening years” (p. 12).

In the political realm, the consequences of the civil rights program are more puzzling. To wit, political scientists have explored various aspects of politics in the post-VRA South. Specifically, (1.) congressional scholars have studied the important role the
region played in national coalition politics before, during, and after the tumultuous 1960s; (2.) scholars of political behavior have studied the changing tenor of partisan competition in the postwar South, particularly how it was affected by changes in the electorate’s partisan preferences (the changes first manifested themselves in presidential elections, then trickled down the political hierarchy, bringing the Republican party from virtual nonexistence to a coequal, if not dominant, position in the region by the mid-1990s). (3.) More recently, some political scientists have attempted to cast the southern case as an instance of democratization following a long period of effective one-party rule, driven fundamentally by economic and demographic changes.  

Clearly, the civil rights program (and the VRA in particular) had a substantial impact on African-American voter registration and turnout rates—for instance, southwide, black registration rates increased 37 percentage points between 1956 and 1968 (cf., Stanley 1987). Yet, regarding certain basic issues of political representation, existing scholarship suggests that the VRA produced unexpected outcomes.

Indeed, some straightforward aggregate statistics on congressional voting behavior—as presented in Rohde (1991) and Black & Black (2002), for instance—find evidence of a conservative—rather than liberal—shift among southern politicians post-VRA. More direct studies of individual House voting records finds a similar anti-

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Downsian response, e.g., Van Der Silk (1969); Feagin (1972); Combs, Hibbing, & Welch (1984); Whitby (1985, 1987); Stern (1982); Bullock (1981); Whitby & Gilliam (1991); Nye & Bullock (1982); Fleisher (1993); Grofman, Griffin, & Glazer (1992). Summarily, in Glazer, Grofman & Owen’s (1998) view, extant findings are “inconsistent with the usual Downsian framework in which the addition of liberal (black) voters to the electorate (which moves the median to the left) should also shift the location of the winning candidate (here, in the time period in question, almost certainly a Democrat) to the left...[T]he puzzle remains: since black voters are much more liberal than most southern white Democrats, we would expect that, in general, the more black Democratic voters in a constituency, the more liberal should any Democratic representative be. But, until the 1990s, this expectation is not confirmed by the data” (p. 24).

What explains these surprising, ostensibly anti-Downsian findings? In this chapter I begin my study of the structural turnout changes in the ‘60s South—conveniently defined here as the 11 states of the former Confederacy—by describing the historical context and walking through the presumptive causal channels. I will point out the most critical issues for process-tracing the impact of the VRA’s structural changes. In the empirical studies that follow, I attempt to resolve the puzzle developed in this chapter.

Regarding my analytical framework, I choose to structure my study in terms of a broadly Downsian logic. It is important to recognize however that conditions in the South deviated from Downs’ model in a number of potentially important ways. Thus, a central

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40 Epstein et al (2007) study the South Carolina state senate.
objective of this chapter is to (1.) systematically discuss the deviations that are most
critical for explaining southern politics post-VRA, and (2.) justify my Downsian
approach. The chapter is organized around the logic represented in figure 3.1, a depiction
of the mapping between social preferences in the southern electorate and equilibrium
policy outcomes.

Figure 3.1: Tracing the VRA’s Political Consequences

Following the introduction I begin by discussing the factors affecting the
distribution of ideal points in the South’s voting-aged population and in the effective
electorate (variously defined), pre- and post-VRA. Next, in section III, I describe how the
distribution of ideal points was processed through primary and general elections. Section
IV discusses the variety of countermeasures southern conservatives could have deployed
in response to new Downsian pressures. For example, I discuss southern political elites’
role in shaping mass preferences, the influence of negative agenda control in southern legislatures, and institutional manipulation of structural parameters in local elections. I focus in particular on the impact of redistricting, a potentially important confound for my overarching Downsian analysis. I conclude the chapter in section V with a discussion of equilibrium models that incorporate both public policy and targetable government expenditures. As I explain, enriching the basic spatial model by including private transfers might have implications for the expected empirical results.

The punch line of this chapter is that the anti-Downsian puzzle survives: Despite the fact that the South was more complex than the simple spatial model suggests, all things considered, one would still expect that the post-VRA electoral dynamics should have pushed politics left, yet, policy apparently moved right, if it moved at all. In the chapters that follow, I attempt to discover whether the inconsistency points to a deficiency of the spatial model or shortcomings in previous attempts to measure the Downsian effect.

II. The Distribution of Ideal Points in the Southern Electorate

A. The pre-VRA electoral connection

After the Civil War, prior to Jim Crow, African-Americans voted in large numbers and appear to have reaped the corresponding benefits predicted by the Downsian model (cf., Woodward 1955, chapter 2; Kousser 1974). Peter Irons’ account of educational expenditures in Reconstruction-era Mississippi gives some idea of the policy repercussions of black participation: “[i]n Mississippi, for example, the legislature—
controlled by black members and their white Republican allies—established a school system in 1870 that enrolled 127,000 black children the following year, 39 percent of the school-age population” (2000; p. 7). Morgan Kousser’s (1980) studies of local tax-expenditure progressivity in pre-turn-of-the-century North Carolina is another piece of evidence that the Downsian electoral channels were operable in the decades immediately following the civil war. The fact that every U.S. Congress but one between 1869 and 1901 had an African-American member from the South is more (indirect) testimonial to the influence of African-American voting power pre-Jim Crow (Woodward 1955; p. 54).

The (ostensibly) Downsian rewards African-Americans enjoyed after the reconstruction amendments enfranchised them were fleeting. The nadir of black voting occurred in the first decade of the twentieth century following a violent racial demobilization campaign launched by the Democratic Party (Horowitz & Terry 2010). Briefly, the story is that the Republicans pulled out of the South as a condition of the “Compromise of 1877,” an unwritten agreement between the GOP and southern Democrats that settled the disputed presidential election of 1876 in favor of the GOP’s candidate, Rutherford B. Hayes. In exchange for the South’s decisive votes in the ’76 election, Hayes and his Republican predecessor, Ulysses S. Grant, pulled federal troops out of Florida, Louisiana, and South Carolina and abruptly discontinued the GOP’s southern reconstruction program. This allowed the Redeemer Democrats to establish a solid one-party region and the Jim Crow sociopolitical regime.

41 W.E.B. Du Bois (1935), John Hope Franklin, Kenneth Stampp, and Eric Foner are among the scholars who have contributed to the sometimes contentious study of Reconstruction-era politics.
During Jim Crow, the general pattern was that black registration and voting rates slowly climbed over the decades as a consequence of legal campaigns on blacks’ behalf and increasing levels of local civil rights agitation. Certain regions of the Deep South were exceptional, but in the years immediately preceding the VRA, southwide, African-American voting rates had returned to roughly 50% of their gender-adjusted reconstruction levels. Nevertheless, African-Americans were not participating at rates approaching whites until the end of the 1960s—when the black and white turnout rates in presidential elections were approximately 60 and 68% respectively.

Overlaid on the temporal pattern (a sharp decline in black participation at the fin-de-siècle followed by slow growth in the ‘30s, ‘40s, and ‘50s) was a strong cross-sectional pattern commonly referred to as the “black belt” or “racial threat” phenomenon. The scholarly consensus here is that the relationship between the size of the black population and local conservatism in the South was characterized by a robust positive relationship. This widely reported correlation dates back at least to the first decade of the twentieth century and is commonly referred to as the “black belt” hypothesis among southern scholars following V.O. Key (1949), who observed particularly conservative policy in the South’s black belt counties. In the sociological literature, scholars tend to call the theory that white racial animosity towards blacks should be positively related to the relative size of the black population (in a “curvilinear fashion”) as the “racial threat hypothesis,” following Blalock’s (1967) theory on the determinants of antiblack
The black belt/racial threat hypothesis has been tested many times in the U.S. using various kinds of data and looking at numerous different operationalizations of white backlash. In different literatures the dependent variables have included county-level African-American lynching rates (e.g., Tolnay & Beck 1995); the proportion of the white vote for Wallace in the 1964 presidential election (Wright 1977); and the level of welfare expenditures ostensibly preferred by minorities (Radcliff & Saiz 1995). Looking at the legislative voting behavior of the southern congressional delegation in the 1950s and ‘60s, Bullock observes an inverse relationship between the size of the African-American population in a member of Congress’s district and the liberalism of his voting record (1981; p. 663). Other studies have considered, for example, cross-state variation in the GOP vote share as a function of the black proportion of the state population (Black & Black 1992, p. 91), and cross-parish variation in the vote for David Duke in his 1990 senatorial and 1991 gubernatorial bids (Voss 1996). In the appendix I review a large number of studies looking at some variant of the “black belt” thesis.

B. The structural changes contained in the VRA

The appendix following this chapter gives a lengthy description of the VRA’s text and its evolving implications since 1965. For present purposes, the VRA did three things.

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42 Gary Becker’s (1957) equilibrium wage model and Spilerman’s (1970) theoretically informed empirical study of black riot patterns in the 1960s, also relates racial disparities in outcomes to the size of the black population, but has not received any attention in the political science literature.
Effective August 1965, the VRA (1.) lowered structural costs that theoretically burdened all voting-aged citizens in the South, but were especially burdensome on African-Americans; (2.) necessitated federal “pre-clearance” for any change in electoral law throughout large parts of the South; and (3.) temporarily put federal registrars and election observers in particularly problematic southern counties.\(^\text{43}\)

After 1965, the VRA was reaffirmed then expanded (in a sense) by the U.S. Supreme Court and various U.S. presidents. In *South Carolina v. Katzenbach* (1966) the Warren Court upheld the constitutionality of the act (despite the fact that the law was “inventive” in the Chief Justice’s words) and rejected the arguments of southern states that it constituted an illegal transgression of states’ rights. In *Allen v. State Board of Elections* (1969) the Court (1.) established that minimizing minority the dilution of minority voting power was an objective of the federal civil rights legislation and (2.) established specific conditions—called “tests” or “devices”—for determining whether or not structural characteristics of the voting regime resulted in minority vote dilution. The decision identified specific tactics used to counteract the VRA—e.g., the use of at-large elections (as opposed to single-member districts); racial gerrymandering; “single-shot” voting in multimember districts; and majority runoff provisions—that could be challenged by the Justice Department under Section 5 of the VRA.

### C. The VRA’s impact on the effective electorate

\(^\text{43}\) See, for instance, Davidson & Grofman (1994); Grofman, Handley, & Neimi (1992); Polsby & Popper (1993); and Kousser (1999) for details of the VRA’s provisions and subsequent legal interpretations.
There are two critical variables of interest to a Downsian analyst: voter turnout and registration rates. The best estimates of racial turnout rates come from survey data in the National Election Survey (NES), since 1952, and the Current Population Survey (CPS), since 1972. With respect to studying relatively local phenomenon—e.g., U.S. congressional races—both series suffer from the problems characteristic of surveys with a national frame. The CPS in particular also suffers from a number of quality control problems in its early years.

Figure 3.3 gives the best available picture of black voting rates southwide during 1880-1986 using the NES, CPS, and other data sources. I constructed the trend lines by taking the unweighted average of statewide rates, for every state with published voting rates by race. The general pattern is that southern turnout rates were trending upwards during the latter half of Jim Crow. Pre-VRA growth was encouraged by mobilization campaigns like the Voter Education Project (VEP) and the Supreme Court’s ruling in Smith v. Allwright (1944). However, among all the non-structural and structural factors, the VRA had the greatest impact on African-American participation. Black voting rates climbed immediately after the VRA from 43.1% in 1964 to 62% in 1968, the largest increase in any comparable period.

Another conceptualization of the Downsian dynamic—the one suggested by Hill et al’s (1995) findings regarding political responsiveness in the U.S. House—suggests

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44 See the list of pros and cons of registration and turnout data in Stanley (1987; p. 151).
45 For example, the CPS data series for the 1960s was written over and the 1978 data set does not contain state identifiers.
that we should also be interested in voter registration rates. Recall from chapter 1 Hill et al.’s (1995) finding that, when partisan competition is high, parties respond to the mere possibility of high turnout among the poor, “competitiveness enhances a generalized form of party responsiveness to the potential, not actual, participation of the lower class” (1995; p. 83-85). Findings like these motivate our interest in registration rates—an operationalization of the “potential” or “effective” electorate (cf., Stanley 1987; p. 151).

From a practical standpoint, as I discuss at length in the next chapter, data turnout rates by race is virtually non-existent at the congressional district level, whereas some racial registration was accumulated in the 1960s by the U.S. Civil Rights Commission and the Urban League.

Figure 3.3 gives some idea of the changes in black electoral power at the state level—as measured by black registrants/total registrants—from reconstruction through the mid-1980s. The figure displays black and white registration rates separately as well. The trends in registration rates track the racial turnout rates—the general pattern is that blacks were participating at low but slowly increasing levels for six decades. The VRA precipitated a very dramatic surge in African-American registration.

In the immediate aftermath of the VRA, black registration rates grew nearly 20 percentage points from 43.1% in 1964 to 62% in 1968. Regarding the impact on relative registration rates (the Downsian variable of interest), summarily, in the average southern county, whites were overrepresented in the population of registered voters by a ratio of almost four to one in 1964. This was cut by more than half between 1965 and 1967 (Alt
By 1964 black registration rates had climbed over 700% from their Jim Crow nadir. The effect of the VRA was particularly acute in the deep South, where the structural regime in some counties had not changed since the early years of Jim Crow.

To fully appreciate the political consequences of the structural changes in southern elections, it is critical to note that white participation (both registration and turnout rates) also increased after the VRA. Indeed, the 1960s witnessed an increase not only in the voting rates of blacks (the citizens primarily targeted by VRA), from roughly 30 percent in 1960 to roughly 60 percent in 1970, the number of white voters also increased over that period (from 61 to 69 percent). In fact, by some accounts, though the proportional increase was smaller among whites, the absolute size of the increase in white turnout was larger (Stanley 1987). That is, 8% of the total white population in the South exceeded 30% of the total black population. The secular increase in white participation has been a flashpoint for academic debate, with conflicting claims about the uptick (e.g., Lublin 2004, ch 1). I turn to this issue now.

D. Evidence of “countermobilization” and “backlash”

To be clear about the causal mechanisms in the Downsian story, I will maintain a strict distinction between two conceptually different forms of reactionary white response to the civil rights program. First, structural increases in black voting rates (could have

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46 This fact casts doubt on that view that, “[u]ntil the mid-sixties, white majorities did not have to worry about the black vote in the South; only with the Voting Rights Act of 1965 were blacks really a block [sic.] to reckon with electorally” (Trebbi, Aghion, & Alesina 2005; p. 17). In some areas outside of the deep South, blacks had an electoral presence before 1965 (Klarman 2001).
potentially) mobilized new anti-black votes from the large segment of (disproportionately poor and rural) southern whites who had not previously participated in elections—I refer to this concept as white “countermobilization.” A logically distinct form of response among white voters (potentially) was a change in ideal points—e.g., existing white voters becoming more conservative. Note that reactionary increases in white turnout or rightward preference shifts would both militate against new black votes, on a Downsian account.

What evidence is there of antiblack countermobilization? As far as I am aware, Alt’s (1994) study is the most sophisticated analysis of racial mobilization and countermobilization post-VRA. Alt estimates a simultaneous equations model of white and black registration rates at the county level using the best existing data, (more on which below). He finds that, “[w]here blacks were more concentrated, greater proportions of whites registered [after the VRA].” As a consequence, “other things equal, higher proportions of blacks—now able to register—responded by doing so. But rising black registration rates, as distinct from heavy black population concentrations, did not...affect white registration rates [directly]” (p. 370).

This fact pattern leads Alt to conclude that, “because white registration rates were...positively related to black population concentration...and black registration rates [responded to] white [registration] rates...it is hard to avoid the interpretation that registration politics in these years was characterized by white mobilization and black countermobilization” (p. 370-71). Alt’s conclusion does not seem to emphasize that
heavily black regions (viz., the black belt counties) had the lowest black voting rates pre-1965, hence the most room for increases post-1965.

Other scholars view the concomitant increase in white turnout as stemming less from racial factors, and stress instead the demographic and economic changes that introduced myriad democratizing influences in the post-war era (e.g., Lublin 2004; Shafer & Johnston 2006). Harold Stanley represents the perspective of these scholars: “[n]o doubt some whites took an interest in voting in an attempt to counteract new black voters…[on the other hand] some whites began participating in politics because of changes that facilitated both registration and voting: Interparty political competition increased in the South; Southern social and economic development meant that more southerners acquired characteristics associated with a greater likelihood of voting; and whites as well as blacks benefited from the lowering of registration barriers” (1987; p. 9).

Thus, while scholars widely agree that racial considerations caused some fraction of the explanandum, some contend that non-racial policy concerns were at least as important in mobilizing the white voters. In particular scholars have pointed to specific social issues, e.g., abortion (cf. Adams 1997), and a general philosophical aversion to taxes and social welfare programs that became acute in the South after the 1960s for

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47 The recently ascendant economic explanations of post-war southern politics are consistent with historians’ accounts of why Democrats erected structural barriers in the first place, namely, to undercut the pre-turn-of-the-century biracial coalitions of poor white and black voters (cf., Kousser 1974; Keyssar 2000; Perman 2001).

What is the evidence regarding “white backlash?” There is certainly prima facie evidence of reactionary voting patterns. Recall for instance Wright’s (1977) finding that the probability that a white survey respondent voted for Wallace in the 1968 presidential election (between Humphrey, Wallace, and Nixon) was increasing in the percentage of African-Americans in the respondent’s state. But is there specific evidence that pre-’65 voters shifted right post-VRA? Apparently, yes, in a number of contexts.48

Radcliff & Saiz’s (1995) study is a well-cited example. The study concludes that increases in minority’s relative voting rates (i.e., in black turnout rate/white turnout rate) were counterintuitively associated with less spending on minority-preferred outcomes at the state level (specifically, on measures of per capita welfare transfers that state governments control, and two scholarly indices of general liberalism). There are a number of observations to make about this study: (1.) RS exclude the South in their regression analysis. (2.) The research design is a little confusing, but RS find evidence of a statistically significant effect—the ratio of black to white participation rates is

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48 Fossett & Kiecolt (1989), Giles & Buckner (1993), Giles & Hertz (1994), and Whitby & Gilliam (1991) offer evidence of southern white backlash—see also Huckfeldt & Kohfeld (1989; p. 84). Mendelberg (2001) points out that white backlash in the 1960s was not confined to the South, “Wallace contested LBJ’s reelection in 1964 in three northern Democratic primaries, garnering large majorities of white working-class precincts that previously had been firmly in the liberal camp and winning well over a third of the total primary vote in those states. He did this with almost no financial support and despite well-organized opposition.”
negatively correlated with per capita welfare transfers. (3.) The finding is not obviously anti-Downisan, because the authors are trying to explain cross-sectional variation using the ratio of blacks and white turnout rates, though the Downsian mechanism would seem to work more directly through the proportion of voters African-American. (4.) Such as it is, the expected effect of moving from a (non-southern) state where blacks participated at a low level relative to whites, to a (non-southern) state where blacks participated at high rates relative to whites (e.g., CT to CA), is not much larger than a unit change in the authors’ “interval operationalization of Elazar’s (1966) concept of political culture.”

The authors explain their counterintuitive findings as evincing white “backlash,” which they defined (somewhat tautologically) as a situation in which “heavy black turnout, compared to whites, tends to produce a measurable ‘backlash’ such that public polices become more conservative as blacks vote at higher rates” because “as black political demands increase, the system compensates by producing outputs that become increasingly contrary to the interests of the black community” (p. 788-89). Though the authors make no explicit reference to the spatial model, it’s possible to contextualize their explanation in Downsian terms. Indeed, theory suggests that a leftist electoral shock like the VRA could have been neutralized at either the electoral level (white preferences could have changed) or at the legislative level (politicians may have simply refused to give in to pressures for more liberal policy).

In the electoral arena, the first question for Downs is, did the % registrants black increase or decrease in the focal district? In a non-trivial number of cases the proportion
of blacks in the effective electorate decreased between elections. In these cases, white countermobilization implied a net increase of whites and, per Downs, a rightward shift is to be expected.

In many, if not most, elections, however, the Downsian prediction would be that policy should have moved left to track the median. Let’s consider a counterintuitive district where the electorate shifted left but policy did not. How much white backlash is necessary to explain this observed outcome in Downsian terms?

Assume voters have single-peaked preferences over the policy space $\mathbb{R}$. Denote the distribution of white and black ideal points in the effective electorate as $w(.)$ and $b(.)$ respectively. The three-step sequence of events to consider is depicted in the figure below: In period $t=0$ the Downsian equilibrium platform is located at the median of the pre-shock distribution of preferences in the general electorate, denoted $g_0(x)=\lambda b(x) + (1-\lambda)w(x)$. In period $t=1$ a structural change introduces new African-American citizens into the effective electorate. Assume the new voters are identical in distribution to the blacks participating prior to the shock (cf., Wolfinger & Rosentone 1980). Denote the increase in the black proportion of the electorate $\beta \in (0,1)$. To generate a counterintuitive rightward policy shift, per the Downsian model, the distribution of white preferences must shift right in period $t=2$. Denote the post-backlash white preferences with the pdf $\hat{w}(.)$. In the picture, the $t=0$ distributions are black, the $t=1$ distributions are blue, and the $t=2$ post-backlash white distribution, $\hat{w}(.)$, is red.
A Downsian conception of white backlash

Figure 3.4

It’s straightforward to represent the sequence of events depicted in the figure algebraically. Consider the cdfs $W(.)$ and $B(.)$ that correspond to the pdfs of black and white ideal points respectively. Note, one can represent the $t=0$ distribution of ideal points in the general effective electorate, $G_0(.)$, as a linear combination of the groups’ independent distributions. In particular, there are weights $\lambda, 1-\lambda$ in $(0,1)$ satisfying

$$G_0(x)=\lambda B(x) + (1-\lambda)W(x).$$

In period $t=1$, after a structural shock increases the black proportion of the effective electorate by $\beta \in (0,1)$, the Downsian equilibrium is located at the median of the cumulative density function $G_1(x|\beta)=(\lambda+\beta)B(x) + (1-\lambda-\beta)W(x)$.

In the third stage, period $t=2$, I imagine that a variance-preserving rightward shift in the distribution of white ideal points takes place, $\Delta>0$. $\Delta$ is the shift in the mean that operationalizes “white backlash” in my thought experiment. The distribution of ideal points in the general electorate in period $t=2$ is thus given by $G_2(x|\beta,\Delta)=(\lambda+\beta)B(x) + (1-\lambda-\beta)W(x)$.
\(\lambda - \beta) \hat{W}(x)\), where \(\hat{W}(\cdot)\) is the distribution of white ideal points post-backlash. Since backlash is conceived as a shift in the first moment of the distribution, \(\hat{W}(x) = W(x - \Delta)\).

Note that, if the white backlash perfectly offsets the influx of African-American voters, the Downsian equilibrium implies \(G_0(\mu) = G_2(\mu)\). It’s straightforward to exploit this equality to solve for the amount of backlash \(\Delta\) that just neutralizes the increase in black voting power. \(^{49}\) This calculation is a useful benchmark in the sense that, were one to observe a rightward policy shift after the structural shock, the implied backlash must have been larger.

**Proposition 3.1:** Suppose structural changes increase the African-American proportion of the effective electorate by \(\beta \in (0,1)\), generating leftward Downsian pressure. Per Downs, the amount of white backlash (i.e., increase in the first moment of the white ideal point distribution) necessary to offset this structural shock is \(\Delta = \mu - W^{-1}[(1-\lambda-\beta)^{-1}((1-\lambda)W(\mu) - \beta B(\mu))]\).

For instance, if the black proportion of the population was initially \(\lambda = .2\), the increase in the black proportion of registrants was \(\beta = .05\), and \(W(.)\) and \(B(.)\) were normal cdfs with variance equal to 1, and means equal 0 and -1 respectively, the size of the

\[^{49}\] .5 = G_0(\mu) = G_2(\mu) = \lambda B(\mu) + (1-\lambda)W(\mu) = (\lambda + \beta)B(\mu) + (1-\lambda-\beta)W(\mu) = (\lambda + \beta)B(\mu) + (1-\lambda-\beta)W(\mu - \Delta).\) Thus, \(\Delta\) is given by Proposition 3.1.
rightward shift in white preferences is $\Delta = 0.063$. In this example, the implied backlash would increase the difference of means between white and black ideal points by roughly 6.3%.

One problem with my measure of backlash, an assumption I could weaken, is the presumption that the new voters are identical in distribution to their co-racials who were voting prior to the structural shock. In support of this assumption, one could point to the numerous well-known studies suggesting that non-voters tend to look much like voters in terms of their partisan and policy preferences (Wolfinger & Rosenstone 1980). On the other hand, there is no loss of generality in my setup from not explicitly describing white countermobilization, because the change in the % of registrants African-American, $\beta$, is what ultimately matters in the Downsian model, not the raw numbers.

Why have I bothered developing Proposition 3.1? The point is that white preferences would have had to change substantially in order to counter-balance, or reverse, the consequences of the influx of African-Americans. Moreover, if we believe that preferences did change, we would expect to see certain patterns in the data, e.g., a positive difference-in-differences between the mean policy position of southern and non-

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50 First solve for $\mu$: $G_0(x) = \lambda B(x) + (1-\lambda)W(x) = .2N(x|-1,1)+.8N(x|0,1)=N(x|-0.2,0.04)+N(x|0,0.64)=N(x|-0.2,0.68)$, so $\mu = -0.2$. Thus, $\Delta = -0.2 - \Phi^{-1}[(4/3)(.8*0.68)-0.05*0.68)]= -0.2 - \Phi^{-1}[0.3962] = 0.0631$. To check that this is the value of $\Delta$ that just neutralizes the influx of black voters, one can verify that $(\lambda+\beta)B(\mu) + (1-\lambda-\beta)W(\mu-\Delta)=.5$, as hypothesized.
southern NES respondents before and after the VRA.\footnote{One could make a complementary test by producing an estimate of \( \Delta \) using registration data like those I employ in the next chapter.} I will not attempt to conduct such a test here, merely point out that the white backlash hypothesis could be tested and would seem to require big effects, if it were the whole reason that the seemingly anti-Downisan results occurred.\footnote{Cf., Lowndes (2009).}

III. How the distribution of ideal points was processed

A. Southern primaries

The impact of structural turnout increases in the South was filtered through both primary and general elections; the consequences also varied with the institutional countermeasures deployed across the South’s multi-tiered political system. Primary elections played a particularly important role in southern political outcomes during Jim Crow and afterwards in the civil rights era.

Nationally, in the modal case, party primaries materialized in the early years of the twentieth-century as a so-called good government reform to address undemocratic nominating practices of the machine-era—the idea simply being that the institution would transfer nominating power from the party bosses to the party’s rank and file. According to Kousser however, the first direct statewide primary in U.S. history occurred in a gubernatorial race in Louisiana in 1892, when the Democratic candidates agreed to hold a nominating (all white) primary to prevent a Republican or Populist victory in the general election against a divided Democratic Party. The “white primary” diffused across the
South (as was characteristic of southern defranchising innovations) and was employed in each of the former Confederate states, save Tennessee, at least once during Jim Crow (Kousser 1974; p. 74).

The Democrats’ white primary had long-term and short-term consequences. In the short term, it increased the party’s odds against its erstwhile competitors—variously, Republican, Populist, Independent, Greenback, and fusion candidates—by lessening the probability that two competing Democrats would divide the party’s vote in a general election. In this way the primary system helped usher in Jim Crow and one-partyism.

In the short and long term—i.e., after the threat of inter-party competition had completely evaporated, typically in the first decade of the twentieth century—the Democratic primary became an important means of circumventing the 15th Amendment. The reconstruction amendment that extended the franchise to blacks was generally interpreted as protecting black voting rights in general elections only, but not party primaries, as these were deemed private affairs in the eyes of southern courts, hence beyond the purview of the Constitution. For many decades the U.S. Supreme Court was silent here. The effect of the white primary on depressing voter turnout was impressive—see, for example, Woodward (1955), Fairclough (1995) or Kousser (1974, p. 72-82).53

53 Gunnar Myrdal’s (1944) well-cited report offers an historical account of black turnout after the white primary was abolished. Also see Klarman (2001, section IV) who argues that “Smith v. Allwright, inaugurated a political revolution in the urban South.”
Though the NAACP and other civil rights organizations began challenging the white primary as early as the 1920s, they did not get much traction until a particularly egregious state law in Texas explicitly banned blacks from voting in Democratic primaries. In 1940, Lonnie E. Smith, an African-American from Harris County, TX, sued for the right to participate in Democratic primaries on the grounds that the law prohibiting him from doing so essentially disfranchised him, because the party’s nominating convention was the only meaningful election in his jurisdiction. The case found its way to the U.S. Supreme Court and the court decided in 1944 that the Texas statute violated the 15th Amendment—when “primaries become a part of the machinery for choosing officials, state and federal, the same tests to determine the character of discrimination or abridgement should be applied to the primary as are applied to the general election” (U.S. Supreme Court, 321 U.S. 649). In the final analysis, Smith v. Allwright was meaningful in the sense that the voting rights legislation of the 1960s would have been theoretically meaningless had Smith v. Allwright not established blacks’ right to participate in party primaries.

How should primaries have affected policy before and after Smith v. Allwright?

The Aldrich (1983)-Baron (1984) framework is a natural way to model primary dynamics—see figure 3.5. Let $\mu$ denote the location of the median voter in the general

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54 Thurgood Marshall considered Smith v. Allwright his most significant victory as chief counsel for the NAACP (cf., Marshall 2009). Justice Marshall later recalled the effect on registration rates, “in South Carolina, when we won the right to vote [in primaries] there and if I remember correctly, we put something like eighty thousand Negroes on the books in a ten-day period. And they don’t have that many now. I remember, in Charleston, a story a newspaper boy told me, the Negroes all dressed up to register, to vote in the primary. They put on their Sunday clothes. It was a big day.”
electorate, let $\mu_D$ denote the median in the Democratic primary, and suppose that candidates choose their campaign platforms prior to the party primaries. In a simple spatial model, it’s natural to assume that if the general election is negligible, then the two Democrats (whose platforms are denoted $D_1$ and $D_2$) would track the location of the median Democratic voter, $\mu_D$. If the general election is binding, then the two Democrats should track the district median, $\mu$. Does this intuition hold up to reasonable modeling assumptions?

A la Ansolabehere-Snyder-Groseclose, suppose that the probability a candidate, x, defeats her primary or general election opponent, y, is a real-valued function of the valence difference between the candidates and the difference between their policy positions, i.e., $\Pr[x \text{ defeats } y] = f(\text{valence component} + \text{spatial component})$. For example, let $\lambda$ represent the valence differential between the two Democratic candidates—labeled 1 and 2—and let $\theta$ represent the valence advantage both Democrats enjoy over the Republican candidate. The following is a restatement of an informal proposition made by Glazer, Grofman & Owen (1998; p. 28) that should obtain in the Aldrich-Baron framework.
Proposition 3.2: (Glazer, Grofman, & Owen 1998) The introduction of liberal (black) voters to the Democratic electorate would shift the Democratic Party median left, leading to (a.) the nomination of candidates by the Democratic Party who are more liberal, causing (b.) voters on the rightward end of the spectrum to defect to the Republican Party.\(^{55}\)

Glazer, Grofman, & Owen propose that the GOP would have taken aboard right-leaning Democrats fleeing the party. Proper microfoundations would specify precisely why conservative former Democrats switched parties. Did the GOP’s non-policy valence increase among such voters? Or, is it necessary to develop a model where voters prefer to join the party whose primary median is nearest their own ideal policy, as Glazer et al propose?

\(^{55}\) One could use Clubb, Flanigan, & Zingale’s (2006) dataset to get a more precise picture of the magnitude of Smith v. Allwright impact on policy by linking their turnout data to roll call voting records.
The general framework described in this section will be useful for interpreting the empirical results I present in the next chapter. Specifically, I will rationalize the statistical patterns I document in terms of a “two-front war” analogy: After the VRA, incumbent Democrats sought to balance the leftward “constituency pull,” generated by refranchised blacks, against the rightward “opponent push” generated by the resurrected GOP (cf., Hook, Kidd, & Morris 1999). See chapter 4, section III.

B. General elections

The next filter in the mapping between social preferences and policy outcomes is the general election. There are two facts worth highlighting about inter-party competition in the twentieth-century South. First, during Jim Crow, Democrats held complete control of southern politics but became the weaker party in the region by the 1990s in many important elections. The GOP’s resurgence in the South occurred precisely during the civil rights era and was inextricably linked with the federal voting rights legislation of the 1960s. Second, the two parties flipped positions on race policy.

The southern partisan realignment is widely viewed as the most significant trend in twentieth-century American politics (cf., Sundquist 1983; Carmines & Stimson 1989). Prior to the civil rights era, Republicans occupied virtually no important elected offices in the South. Indeed, during Jim Crow, the South was an informal confederation of one-party states represented at the national level by a famously coordinated team and political competition, such as it was, consisted exclusively of internecine fighting within the Democratic Party’s state caucuses, and varied southwide (Key 1949). After the civil
Rights era, the GOP ultimately climbed to regional prominence, building momentum in
the ‘70s and ‘80s and passing a milestone in 1994 when the success of its regional
candidates propelled the party to a majority in the U.S. House. Figure 3.6 at the end of
the chapter depicts the southern realignment as it manifested itself in the partisan
composition of the region’s state and federal representation from 1952-1998.

The second important issue, related to southern partisan realignment, is the
reversal of the parties’ race platforms (Sundquist 1983, p. 352-375). From the
Redemption era (in the 1890s) until the advent of the civil rights era (in the 1950s) the
Democratic Party was unequivocally the party of racial conservatism in the South (cf.,
Cash 1941; Woodward 1955). The partisan reversal on race was multi-faceted. Prior to
the civil rights era, the South was pivotal in national politics and reaped tremendous
benefits from this fact and the fact that the absence of competitive politics meant that
southern politicians, virtually all Democratic, could return to Washington year after year
and rise steadily through the seniority-based committee system. For their part, fiscally
liberal and (increasingly racially progressive) Democrats from the North caucused with
the southerners in order to gain control of the House and the Senate more elections than
not (at the expense of northern Republicans). In return for their decisive votes,
southerners received from their northern co-partisans a free pass on Jim Crow and New
Deal payoffs, a particularly valuable political commodity in the agricultural South after
the Great Depression.
In the re-equilibration period that preceded and followed the VRA, the mantle of racial conservatism ultimately passed from the Democrats to the GOP. Beginning with Truman’s civil rights plank in the Fair Deal (1948) and Senator Humphrey’s (MN) call at the Party’s national convention of the same year for the party “to get out of the shadow of states’ rights and walk forthrightly into the bright sunshine of human rights,” southern Democrats felt increasingly threatened by what might be viewed as a leftward shift of the median voter in the national Democratic caucus. Richard Russell, as de facto leader of the southern senators, hence the region’s head gatekeeper against the “Second Reconstruction,” characterized Truman’s proposals as the “most outrageous affront to the people of our section that we have had to face since Reconstruction...an opening wedge in the fight to stop all segregation,” which would, in Russell’s view, mean that blacks and whites would one day “attend the same schools, swim in the same pools, eat together and, eventually, intermarry” (Black & Black 2002; p. 53).

Southern fears were amplified in 1957 when Eisenhower used federal troops in Little Rock, Arkansas to enforce the Supreme Court’s decision in Brown v. Board (1954), and again in 1963 when Kennedy used similar force to integrate the University of Alabama, prompting Alabama governor George Wallace to call for “segregation now, segregation tomorrow, segregation forever.”

When the Party’s national caucus shifted left, the South’s conservative elite had a number of possible institutional, mobilizational, and personal responses, as I discuss at length in coming chapters. For racially conservative voters (newly mobilized or
otherwise) three important factors increased the GOP’s partisan valence during the civil rights era: (1.) The Democratic median was moving rapidly left. (2.) Because northern Democrats were pushing a liberal race agenda, newly-mobilized African-Americans were entering the southern electorate overwhelmingly as Democrats, driving away racial conservatives who had an aversion to sharing a party with blacks (cf., Grofman, Griffin, & Glazer 1992), and (3.) beginning with the Goldwater and Nixon campaigns, the GOP’s candidates strategically targeted racial conservatives.

There were at least two important factors retarding Republican growth in the South. First, as the civil rights era wound down, the southern bloc continued to hold a privileged position in national coalitional politics, and powerful leadership positions in the Democratic Party. The immense power southern politicians derived from their seniority in the majority party prevented many southerners from leaving the Party in the ‘60s, though some politicians elected under the old order—famously Strom Thurmond, in late 1964—did transition to the GOP in the midst of the civil rights fray. A second factor slowing GOP growth was the vestigial reticence of southern whites to support the party of Lincoln—a consequence of the Democrats’ highly effective Redemption-era propaganda campaigns (Woodward 1955). This important electoral consideration also discouraged many southern incumbents from defecting to the GOP, even as the Democratic Party became less hospitable.

Eventually however, as the electorate’s partisan preferences began to realign, districts that had been staunchly Democratic for 80 years or more began to elect
Republicans. Through retirement, party-switching, and electoral defeat, the GOP ultimately emerged as the party of racial conservatism and a coequal in regional partisan politics. In many instances, GOP growth was slow, at first, but steady. The trend began to manifest itself first at the level of presidential politics before moving down to the lower levels of the political strata, and the story of the presidential contests in the South gives some idea of the GOP’s means of ascent.

In the 1964 presidential election, Senator Goldwater’s (R-AZ) vote against the Civil Rights Act (1964) helped him carry the South in his presidential bid against LBJ, a Democrat from Texas. Following in Goldwater’s footsteps, Nixon’s “southern strategy” appealed to racially conservative southerners and helped him forge a winning coalition in 1968 and ‘72 (cf., Hillygus & Shields, 2008; ch. 5). Carter (D-GA) defeated Ford in 1976 and returned the presidency to the Democrats, but subsequently lost to Reagan (R-CA) who successfully carried the South following a general election campaign that began, controversially, at the Neshoba County fairgrounds in Mississippi, in a county made infamous when three freedom riders were murdered there 15 years prior for encouraging blacks to vote (cf., Edsall & Edsall 1991; p. 148-214).

The tendency of the GOP’s presidential candidates to adopt the more conservative position on issues broadly construed as racial was present in the first Bush campaign (Mendelberg 2001) and, the case has been made, was also evident in the 2008 presidential election, wherein a number of well-placed observers saw echoes of the southern strategy in the McCain-Palin campaign, comparing the candidates’ tactics to
one-time segregationist George Wallace’s (Lewis 2008; Rymer 2008). Regarding the electoral implications of the partisan reversal on race, LBJ’s famous quote proved prescient (if overly optimistic)—when he signed the Civil Rights Act of 1964 he allegedly remarked to his co-partisans that “[w]e have lost the South for a generation.” House minority leader Dirksen and his Republican colleagues apparently welcomed the bill’s passage precisely for this reason (Rodriguez & Weingast 2003).

C. Variance across political subunits

The impact of the VRA was not uniform across the political strata. In particular, the ability of the powers-that-be to manage the black influx was different in presidential, gubernatorial, U.S. and state congressional elections, and the various kinds of local races. Here I discuss a number of institutional countermeasures that could have occurred in elections below the state level. I will focus in particular on theory pertaining to at-large districting and racial gerrymandering, for reasons that will become clear.

The first observation to make is that the objective of a rational, conservative elite is African-American “vote dilution,” i.e., limiting the ability of (presumably leftist) black voters to influence political outcomes. From Redemption until the VRA, vote dilution was largely unnecessary because the Democratic elite could effectively maintain the sociopolitical order by setting blacks’ (and poor whites’) structural voting costs prohibitively high.56 After federal legislation increased black turnout and federal

56 See Kousser (1976) and Woodward (1971) regarding the genesis of the Jim Crow structural voting regime and its relationship with southern one-partyism.
oversight of southern elections, the conservative elite turned to more sophisticated institutional tinkering, especially racial gerrymandering (strategic manipulation of district boundaries), at-large districting (in which all representatives are elected at the city level, leaving minorities vulnerable to bloc voting), “anti-single-shot” vote counting rules (used to magnify the effect of at-large districting), and majority runoff provisions (which helped white voters coordinate against undesirable candidates). In other instances, municipalities employed selective annexation or secession to manipulate black voting power in the resulting jurisdictions.

Reactionary institutional countermeasures such as these were particularly common in the 1960s, before a series of legal decisions established the prevention of vote dilution as a specific legal objective of the federal voting rights legislation, and empowered the U.S. Attorney General to sue on behalf of affected citizens under various provisions of the VRA and other voting rights legislation. The use of at-large districting in local election is a useful case study of the strategic considerations at play, first, because at-large districting schemes were ubiquitous in school board, board of supervisors and other types of local elections. For instance, in 1960, 60% of cities in the U.S. with a population above 10,000 elected their councilmen at-large (cf., Banfield & Wilson 1964). The scheme was even used once at the state level, to elect Alabama’s delegation to the U.S. House in the’62 elections. Second, at-large districting is of interest
because it was a particularly pernicious form of African-American vote dilution after 1965 (cf., Hajnal & Trounstine 2005).\textsuperscript{57}

To see how a conservative city council member would have felt about at-large districting, consider the model developed by Trebbi, Aghion, & Alesina (2008). The Trebbi-Aghion-Alesina framework is rich enough to produce a number of nuanced propositions about the choice of citywide districting regimes, and the theory receives empirical support from the authors’ study of 214 southern cities between 1930 and 2000. The next proposition conveys the main intuition of the authors’ results. The insight is that, when the city contains relatively few blacks, “right-of-median city councils can leverage the sheer size of conservative white voters to capture all the available seats in at-large elections, instead of running the risk that some minority-favored representatives will be elected from minority-controlled districts.” On the other hand, as the size of the city’s African-American population becomes larger (closer to a fifty-fifty split), the possibility of losing the whole city induces a conservative to prefer single-member districts, which allow minority vote dilution through standard district packing schemes.

\textit{Proposition 3.3: (Trebbi, Aghion, & Alesina 2005)} Faced with structural increases in turnout, conservative city councils should respond with at-large districting (ALD) when the city has few minorities, and single-member districts (SMD) packed with minorities when the black population is sizable.

\textsuperscript{57} The landmark U.S. Supreme Court case \textit{White v. Regester} (1973) and the cases it spawned have defined the conditions when at-large elections are permissible. In the South and the nation as a whole the incidence of at-large districting has dropped precipitously since the ‘60s and ‘70s (Davidson & Korbel 1981).
Obviously at-large districting would not have been a means of diluting a leftward electoral shock in statewide races, as there is only one district at stake. A second countermeasure operable only in sub-state level elections was strategic manipulation of district boundaries. Empirically, racial and partisan-motivated gerrymandering became common and highly contentious after the 1960s, as I discuss in the next section.

D. Redistricting

The impact of redistricting is critical for Downsian explanations of the VRA’s impact across districts, hence the equilibrium policy outcomes determined in the legislature (cf., Lublin & Voss 2003). This section presents the case that strategic manipulation of district boundaries should have produced predictable patterns in the data. Regarding the historical context, the important point to make is that four U.S. Supreme Court cases of the early ‘60s spurred the so-called “reapportionment revolution” in the U.S.—these were Gomillion v. Lightfoot (1960), Baker v. Carr (1962), Wesberry v. Sanders (1964), and Reynolds v. Sims (1964). These legal decisions turned on issues dealing with African-American civil rights issues and on unrelated issues (Keyssar 2000; p. 284-298).^58

The literature contains numerous formal and informal propositions about the impact of redistricting on various types of political outcomes. For instance, Swain (1993); Sherstyuk (1995); Thernstrom & Thernstrom (1997); Cameron, Epstein & O’Halloran (1996); Lublin (1999); Cox & Katz (1998; 2002), Gilligan & Matsusaka (1999); Shotts

(2002); and Coate & Knight (2007) each advance a theoretical perspective about the impact of strategic redistricting. I focus on the literature culminating in studies like Friedman & Holden (2008), because I believe they imply the proposition that follows.

The seminal theory on optimal gerrymandering (i.e., optimal from the standpoint of a conservative, instrumentally rational mapmaker) comes from Owen & Grofman (1988). The lesson from this study is that—in an extremely stylized model—if a partisan mapmaker is concerned with the long-run risk of electoral defeat, optimal districting schemes will make use of two basic strategies—colloquially, the “stack” and “crack” strategies. The first technique concentrates unlikely supporters, sacrificing the resulting “stacked” district, but keeping the opponent’s supporters out of more competitive districts. The second canonical strategy is to disperse opponents across districts, “cracking” their voting power.

There are many obvious shortcomings of Owen & Grofman’s seminal model (cf., Sherstyuk 1995). For instance it does not incorporate geographic and legal constraints faced by real-world mapmakers. As it pertains to the South, another shortcoming is that the model focuses on partisan redistricting—wherein the mapmakers’ objective is to maximize some measure of a party’s strength in the legislature—though straightforward partisan redistricting may not adequately characterize the complex relationship between race, party, and the urban-rural divide in the South. Cox & Katz (2002) for example “believe that southern redistricting before and even in the 1960s was much more a matter
of factional infighting within the dominant party than partisan gerrymandering fought out between parties” (p. 8).

Gilligan & Matsusaka (1999) and Friedman & Holden (2005) address still other shortcomings of the seminal work. As usual, in these papers the mapmaker’s assumed maximand is the number of districts she wins (presumably this derives from her desire to maximize her power in the legislature). Both papers generalize Owen & Grofman’s (1998) model by assuming that the mapmaker receives a noisy signal about voter preferences, as opposed to knowing voter types with certainty. The generalization has significant theoretical implications, Friedman & Holden (2005), for instance, overturn the prevailing conventional wisdom regarding the utility of Owen & Grofman’s “cracking” strategy.

How would one characterize an optimal district map (from the conservative mapmaker’s perspective) that minimizes the median of district medians? Imagine that there are $N$ congressional districts to draw and $kN$ voters to put in districts. For the sake of simplicity assume that both $N$ and $k$ are odd. Define a districting plan as a partition of equally-populated districts \( \{D_1, \ldots, D_N\} \) each containing $k$ voters. Assume (1.) every citizen’s preferences over public policy are represented by a single-peaked utility function defined over the real line; (2.) legislators choose policy so as to maximize their chance of re-election by locating their own policy position at their district median, and (3.) regional policy is set by the median legislator of the South’s delegation.
**Proposition 3.4**: Strategic re-districting by elites interested in mitigating the public policy consequences of higher African-American turnout (by minimizing the median voter of the median Southern district) should have led to greater variance among the Nominate scores of the South’s representatives.

I’ve spent so much time discussing redistricting because of its significance in the empirical results I present in the next chapter. To quantify the dilutive effect of redistricting at the system level, one would want to focus first on the theoretical impact in a race-neutral districting scheme—i.e., a plan that is neutral with respect to the seats-votes ratio. (cf. Cox & Katz 2002; McCarty, Poole, & Rosenthal 2006). So, I imagine there was a leftward shift in a state’s median voter. In a race-neutral scheme, the Downsian model leads to the expectation of a leftward shift in each district proportional to shift in its median voter (assuming the electoral connection was the same in districts across the state). So the race-neutral prediction is that the median of the states’ delegations should have shifted left.

For completeness, I’ll note first that there are some pointed empirical debates about the effect of optimal districting schemes across districts—e.g., between Cameron et al. (1996) versus Lublin (1999), and versus Swain (1993) and Thernstrom & Thernstrom (1997). There is also research looking at the impact of post-VRA majority-minority districts on minority turnout: Barreto, Segura, & Woods (2004), for instance, find that being in districts where minorities are a majority of the electorate (or near-majority) has a net positive effect on Latino turnout propensities. Claudine Gay’s (2001) analysis of
precinct-level data in eight states suggests that residing in a majority-minority district with an African-American representative does not affect black turnout rates, but it does depress white turnout rates.

Another point to make is that, Proposition 3.4 may provide a theoretical justification for the well-known, but controversial theory explaining the policy polarization noted by scholars in the post-war House—e.g., by Jacobson (2000) and McCarty, Poole, & Rosenthal (2006). The idea that gerrymandering has contributed to polarization in the House is commonly discussed, but rarely derived in formal terms—see for example the exposition of the thesis in Carson et al (2007) and McCarty, Poole, & Rosenthal (2009). It’s an open question whether the proposition that strategic gerrymandering increases polarization, actually obtains in a scenario Friedman & Holden’s.59

Finally, the contemporary scope of strategic districting in the U.S. is as follows. The Supreme Court has tended to look askance at partisan gerrymandering—e.g., in Vieth v. Jabelirer (2004)—but clearly not outlawed the practice. Some strictures, like the equality of district populations, have been very rigidly enforced (sometimes comically so), while the court has been more ambiguous on other issues, like the acceptable shape of districts (cf., Chambers & Miller 2010). Obviously, computers and large-scale data

59 I haven’t run across evidence suggesting that real-world district makers explicitly optimize as the theoretical literature suggests.
collection have dramatically increased mapmakers’ capacity to analyze various districting scenarios since the 1960s.

Critics of the American districting system exist and have made efforts to reform the system but have so far met with insurmountable resistance at the federal level from both parities. A notable example is the case of Rep. John Tanner, a moderate blue dog Democrat from Tennessee’s 8th district. His yearly efforts to push through a bill that would compel independent boards to draw up district boundaries met with zero success congress after congress (Politico 2009a). At the state level, a small number of states use independent commissions to design their decennial redistricting plans and an increasing number of states are evidently considering such a reform (Friedman & Holden 2008).

IV. Conservative mobilization and negative agenda control

The role of elites from the “black belt” counties in dictating regional policy and mobilizing reactionary sentiment was apparently great. The moniker “black belt” refers to the 300-mile crescent-shaped swath of rich black soil running from eastern Texas to Virginia’s Tidewater region, and somewhat further north along the eastern seaboard of the U.S. (Washington 1901). The rich soil in this region fostered intensive agricultural and high concentrations of African slaves. It was in these counties (containing large

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60 Indeed, some see Tanner’s early retirement after the 111th congress as evidence that the current districting system and the broader electoral regime is systematically biased against moderates like him (Politico 2009b).
proportions of African-Americans) where racial views were typically most conservative and southern political power was disproportionately concentrated. The inordinate power of rural black belt areas was due in part to differential demographic growth rates between urban and rural areas, and the fact that southern legislatures had generally not reapportioned for decades prior to the reapportionment revolution, in the early ‘60s (cf., Ansolabehere, Gerber & Snyder, 2002).

On the political significance of the black belt counties, Key (1949; p. 5) for instance writes that “[i]t is far from the truth to paint a picture of southern politics as being chiefly concerned with the maintenance of the supremacy of white over black. [White over black] dominance is an outcome, but the observer must look more closely to determine which whites and which blacks give southern politics its individuality. The hard core of the political South—and the backbone of southern political unity—is made up of those counties and sections of the southern states in which Negroes constitute a substantial proportion of the population. In these areas, a real problem of politics, broadly considered, is the maintenance of control by a white minority.”

The South’s “massive resistance” movement is an interesting case in point. In response to the Brown v. Board of Education of Topeka decision in ‘54, U.S. Senator Harry F. Byrd, Sr. launched a campaign of interposition in early 1956 to unite white political elite in Virginia and across the South behind a campaign for new state and local policies to prevent public school desegregation. Initially, the extreme resistance to the federal courts, embodied in the Southern Manifesto (1956)—a document drafted by
Senators Thurmond and Russell pledging resistance to the court-ordered integration, signed by a majority of the southern representatives to the U.S. Congress and Senate—drowned out the advocates of more moderate policy.

In Lassiter & Lewis’s (1998) view, “[t]he story of how massive resistance, interposition, and school-closing laws came to dominate the white southern response to Brown is largely a political one. The success of those who championed defiance had much to do with the disproportionate power wielded by rural Black Belt whites in state politics and the corresponding circumspection and political weakness of urban moderates who supported local solutions rather than rigid statewide defense of de jure segregation...In the aftermath of Brown, the charged political climate surrounding issues of race and education muffled the voices of many who dissented from the defiant course chosen by the region’s political leaders...But as the recklessness and futility of massive resistance policies became clearer, white moderates—especially middle-class parents concentrated in urban areas and in counties where blacks constituted a small percentage of the population—became increasingly vocal in their opposition to policies designed to protect absolute segregation at the expense of public education” (p. 1).

Another likely form of elite response would have taken place in regional and national legislatures where strategic management of the agenda could have been a potential weapon in the hands of a reactionary legislative blocs inclined to stymie socio-political change. Following a leftward electoral shock, rational conservative politicians would have done everything in their power to keep undesirable policy changes off the
plenary schedule (cf., Cox & McCubbins 2008). In stark contrast, in the Downsian model, policy is set by the median legislator in a uni-cameral legislature governed by a simple majoritarian decision rule. Critically, in a frictionless account of legislative policy-making, *legislators are free to introduce a change to any point in the policy space.* Thus, among the model’s dubious oversimplifications is the fact that it does not account for negative agenda control, or strategic agenda setting more generally (cf., McKelvey 1977). It’s worth noting that the exercise of negative agenda control could frustrate many standard techniques for quantifying legislators’ policy positions using legislative voting records (e.g., Poole & Rosenthal 2000). I return to this issue in the next chapter.

**V. Conclusion**

The main conclusions of the chapter are these. (1.) First and foremost, in the 1960s there was a dramatic increase in southern voter registration rates in the South, consisting of both white and black influx. Both structural and non-structural factors contributed to the uptick. (2.) Following the turnout shocks were counterintuitive rightward policy trends among southern politicians, e.g., among the South’s delegation to the U.S. House. (3.) From a theoretical perspective, the filtering of the VRA’s impact through primary and general elections would have warped the impact of the electoral shock, but does not nullify the basic Downsian expectation that policy should have moved left when the median did. (4.) The ubiquity of reactionary countermeasures—e.g.,

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61 For more empirical evidence of the phenomenon see Gailmard & Jenkins (2007).
62 Canon (1999; p. 191) argues that examining the introduction and co-sponsorship of legislation would address the “censored sample” problem inherent in measuring policy adaptation using roll call data. I’ll consider the issue later (cf., Adler & Wilkerson 2005).
strategic gerrymandering and at-large districting at the electoral level, and the use of negative agenda control in the legislature—show that there was a problem to be managed.

Going forward, the main message is that the VRA’s anti-Downsian puzzle survives; the rest of the dissertation attempts to discover why. One obvious possibility is that the Downsian model is operating exactly as expected but the premise that the median voter in the southern districts moved left is false, because of “white backlash” (i.e., existing white voters shifting their ideal points, hence the district median to the right), or white counter-mobilization (i.e., the introduction of right-of-median white voters mobilized during the civil rights era).

Another possible explanation for the ostensible disjuncture between the Downsian expectation and the southern experience is that the model is wrong. It’s possible that, if one were to construct a more accurate model, one would no longer expect that a leftward movement of the district median would necessarily induce southern candidates to move policy left. In fact, as I’ve pointed out, the simple spatial model departs from the southern reality in a number of potentially important ways. For instance, the South was a one-party region just beginning to transition toward the sort of two-party competition Downs envisioned. Moreover, though commitment to racial conservatism varied (contrast Senators Thurmond and Helms, for example) southern candidates and parties generally did care about the policies that were adopted, not just obtaining office (Wittman 1973; Cox 1984).
Alternatively, as I’ve discussed, Downsian dynamics might have been operable at the district level only to be countervailed by any of a number of structural countermeasures deployed by entrenched conservatives, so that the influx of black voters failed to produce policy change (though it may have affected the behavior of some individual legislators). For example, in this chapter I focused on negative agenda control and strategic re-districting. As I’ve argued at length in this chapter, neither strategic agenda manipulation, racial gerrymandering (or any of the other reactionary countermeasures discussed in this chapter) are directly relevant for assessing the central Downsian claims within any given district. So I’ll focus especially on understanding what happened within individual districts.

There are also important party effects to consider here. The standard model is that legislators’ votes are influenced by both constituency and party (cf., Levitt 1996). Assuming this is also the case in post-VRA South, recall the lesson from Figure 2.1: in a setting with a large turnout increase and policy-motivated candidates (a la Wittman), when the party of the victorious candidates alternates policy may shift right, though both parties have shifted their platforms left. So if partisan valence changed at the same time as the VRA, then the apparently conservative behavior of Southern legislators could have been the net result of a leftward constituency pull and a rightward party pull.63

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63 In fact, there are existing arguments suggesting something along these lines—see, for example, Cox & McCubbins’ (1993) “broken promise” story.
Recall from the final section of chapter 2 that another difficulty with using Downs’s model is that it does not include transferable goods, though the distribution of group-specific government transfers was an integral part of southern politics, as it is in any democracy (cf., Button 1989; Keech 1968). In the next chapter I focus on the public policy component of post-VRA electoral competition. In chapter 5 I return to the role of pork and private consumption. The view that I want to advance here is that it is not obvious that amending the bare-bones spatial model to accommodate these or any other obvious objection would lead one to expect anything other than a leftward public policy move (at the district level) following a leftward shift of the (effective) electorate.\(^6^4\)

Where do I go from here? In the next chapter I look for evidence of two kinds of forces operating on policy at the district-level, as measured by members’ DW-Nominate scores (cf., Poole & Rosenthal 2000)—I refer to these as “constituency-pull” and “opponent-push,” following Hood, Kidd, & Morris (1999, 2001). The logic behind the notion of “constituency-pull” is simply that southern politicians would have responded to the structural turnout shocks by “changing their stripes,” in Downsian fashion, to track the median—where the VRA increased the ratio of black to white voters, rational politicians should have adapted by adopting more moderate race positions, like George Wallace and Strom Thurmond allegedly did. Further, I argue in the next chapter that the Downisan logic leads us to predict that politicians who were unwilling or unable to alter their old policies were removed from their positions at higher than average rates through

\(^6^4\) Formal models like Jackson & Moselle’s (2002) might generate more nuanced predictions, once appropriately modified to model electoral competition (rather than legislative bargaining), see chapter 2, section VI.
(i.) retirement, (ii.) electoral defeat (in primary or general elections), or (iii.) by switching parties.

I also want to quantify other—sometimes counteractive, sometimes amplifying—forces that derived from the resurrection of the southern GOP. Collectively, I’ll refer to these forces as the “opponent push” (per Hood, Kidd & Morris 1999, 2001). On one hand, one finds sensible stories suggesting that the equilibrium impact of a viable GOP candidate should have resulted in the representative Democrat moving right—i.e., to protect his right flank against GOP candidates who were more conservative than the national Democratic median on many important issues of the day—e.g., labor relations, military escalation in Vietnam, and, increasingly, race. In this account, it’s the fear of partisan defection that sends the Democrat in a more conservative direction.

Alternatively, it’s possible that strategic Democratic candidates would have hewn very closely to the median in the primary electorate, abandoning the front on the right and withdrawing to more a liberal position. Recall that Glazer, Grofman & Owen (1998) offer an observation that would have amplified the effect—they argue that the emergent GOP picked off conservative former Democrats who couldn’t tolerate the new farther-left Democratic coalition, resulting in the Democratic median drifting still further left. In

\[65\] Indeed, there is a robust finding that Democrats in the post-civil rights era were more liberal than their Republican colleagues with the same geographic constituency (e.g., Brady & Lynn 1973; Jackson 1967; Fiorina 1974; Grofman, Griffin & Glazer 1990; Shapiro et al. 1990). Glaser (2001) has a qualitative account of the impact of post-VRA party defections. The time-variant property of the DW-Nominate policy estimates will obviously come in handy when I analyze post-VRA in the next chapter.
the next chapter I seek to quantify the influence of both constituency “pull” and opponent “push.”
Table 3.1: VRA Section 5 pre-clearance jurisdictions since 1965

<table>
<thead>
<tr>
<th>States covered as a whole</th>
<th>commencing in</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alabama</td>
<td>1965</td>
</tr>
<tr>
<td>Georgia</td>
<td>1965</td>
</tr>
<tr>
<td>Louisiana</td>
<td>1965</td>
</tr>
<tr>
<td>Mississippi</td>
<td>1965</td>
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<tr>
<td>South Carolina</td>
<td>1965</td>
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<tr>
<td>Virginia</td>
<td>1965</td>
</tr>
<tr>
<td>Alaska</td>
<td>1975</td>
</tr>
<tr>
<td>Arizona</td>
<td>1975</td>
</tr>
<tr>
<td>Texas</td>
<td>1975</td>
</tr>
</tbody>
</table>

Covered counties in states not covered as a whole:

**California**

**Florida**

**New York**

**North Carolina**

**South Dakota**
(1976) Shannon, Todd.

There exist covered townships not in covered counties or states. There were two in Michigan, commencing in 1976, and ten in New Hampshire, commencing in 1974 (see,

Figure 3.2: Locations requiring Section 5 pre-clearance, 2006-2031
Figure 3.3: African-American electoral power, 1940-1980

The red, blue, and green trend lines represent the percentage of voting aged whites that were registered to vote southwide, the percentage of African-Americans registered, and the percentage of the registered electorate African-American, respectively.
Figure 3.6: Southern realignment and GOP seat shares, 1954-2002

In terms of its proportion of the southern U.S. House delegation and its mean seat share in southern state legislatures, the GOP saw its fortune improve after the war.
Figure 3.7: Conservative backlash among southern House Democrats, 1952-1998

Note: The figure essentially replicates Black & Black (2002). The y-axis denotes percentages of the southern delegation to the U.S. House. The number of southern members did not fluctuate between the 1950 and 1990 reapportionments. The South had 106 representatives during this period.

The figure shows a sharp increase in the percentage of conservative-voting legislators in the South’s delegation. Conservative Democrats are defined as those voting with the Democratic congressional caucus less than 60% of the time, per Black & Black (p. 35).

I. Introduction
The social, political, and economic currents that coalesced in the 1960’s have been the subjects of a voluminous literature, some of which I described in the previous chapter. Two things I noted were the large structural and non-structural increases in black and white turnout rates and the dramatic resurgence of the GOP, arguably the most profound trend in twentieth century American politics (Carmines & Stimson 1989; Rohde 1991). I also noted that the civil rights era presents a puzzle for political scientists and the workhorse model of electoral competition: despite the large influx of presumably left-of-center black voters, southern legislators apparently moved right in their post-VRA policies, if they moved at all.

Explaining the ostensibly anti-Downsian policy response is the primary objective of this chapter. To frame the analysis, consider a simple simultaneous equation model of politics in a representative southern district, where \(x_t\) denotes the incumbent’s optimal post-shock policy platform:

\[
\begin{align*}
(1.) & \textit{incumbent exits}_t = f(\omega_t, c(x_t, x_{t-1}), \textit{opposition strength}_t); \\
(2.) & x_t = g(y_t, c(. , x_{t-1}), \textit{opposition strength}_t);
\end{align*}
\]

Equation (1.) represents the incumbent’s reelection outcome. I assume her likelihood of exit depends on factors within her control—e.g., whether or not she decides to run and what platform to adopt if she does run—and factors beyond her control—e.g.,
the strength of the opposition. The outcome thus depends on the utility value of her outside option \((\omega_t)\), the costs she expects to accrue in re-optimizing her policy to compete in the new environment, \(c(x_t, x_{t-1})\), and the strength of the opposing candidates. In equation (2.), the incumbent’s optimal policy also depends on the strength of the opposition and her repositioning costs, and it depends on the ideological location of her rivals—represented by the vector \(y_t\). In practice, efforts to estimate these equations in isolation are likely to be hampered by a variety of identification issues stemming from the rational expectations equilibrium behavior of forward-looking incumbents, more on which below.

Regarding the anti-Downsian puzzle, logically, the ostensibly anti-Downsian results reported in the literature represent either a failure of the basic spatial model or a failure of previous empirical efforts to measure the impact. In this chapter I develop a new formal model— informed by the simultaneous equations approach—that better approximates the southern environment. To test the theory, I analyze (1.) politicians’ career outcomes—e.g., their propensity to retire from office or lose elections—and (2.) their legislative voting records. The chapter makes a number of conceptual and methodological improvements on existing empirical studies and finds broad support for the theory. My work focuses primarily on the behavior of the southern delegation to the U.S. House and U.S. Senators (to a lesser extent).

Critically, the model I advance is extra-Downsian in the sense that Downs assumes instant and costless organization of candidates. I on the other hand recognize
that, empirically, there was substantial variation across southern districts in the strength of the factors inhibiting or encouraging viable challengers. Moreover, I note that the VRA typically opened up a “two-front war” for southern incumbents: When the left flank appeared, it materialized in the Democratic primary where the addition of re-enfranchised blacks increased the likelihood of strong leftist challengers. On the right, the battle was pitched in the general election against the resurrected GOP. The theory I advance simply suggests that politicians should have moved right when the influx of African-Americans precipitated a larger threat on the right, and moved left when the opposite case obtained.

In the next section I formally develop my “two front war” thesis. To see how the theory works, consider four archetypical cases. Suppose districts vary with respect to: (a.) the size of the increase in the African-American registrants, and (b.) the amount of conservatism in the district. A reasonable proxy for (b.) might be the focal district’s G.O.P. vote share in the previous presidential election, for example. For expositional purposes, suppose each district has either a large or small influx of black voters and a high or low level of conservatism. Refer to districts with low conservatism as “liberal.” What is the prediction in each of the possible cases?

In a conservative district with a large black influx the prediction is that policy would have moved in a conservative direction (provided the newly enfranchised African-Americans did not constitute a majority of the effective electorate). Empirically, these districts were found in the South’s black belt. Likewise in conservative districts with a small black influx, the natural prediction is a rightward shift post-VRA. These are
districts where the reactionary response to the civil right was presumably and the newly
enfranchised constituted a small electoral base. The number of districts fitting this
description was presumably much smaller (cf., Key 1949).

Though there few if any liberal districts witnessed large black influxes—generally
speaking, the large post-VRA influxes occurred in the Deep South—the unambiguous
prediction would be a leftward policy shift. Likewise, in a liberal district with a small
black influx the natural prediction is that policy should have shifted left. This case
plausibly represents the South’s urban areas—which were historically at odds with the
black belt—and the rural Appalachian counties where few blacks lived and politics
tended in a more liberal direction, as witnessed by the success of Populist and Fusion
candidates at the in the early years of the twentieth century for instance (Woodward
1951).

II. The implications of the Democrats’ two-front war

A. When rightward moves are optimal responses to leftward entrants

As a practical matter, politicians could attempt to extend their political careers
after the VRA—knowing this might entail costly policy re-positioning or defeat in the
primary or general election—or they could choose to exit politics. Assume for the time
being that the incumbent finds running worthwhile. How should this mobile-at-a-cost
candidate have responded to passage of the VRA? Should he have moved left—\(x_t < x_{t-1}\),
moved right — $x_t > x_{t-1}$, or stayed put — $x_t = x_{t-1}$? In general, staying in exactly the same position is unlikely, but either a leftward or rightward move is possible.

I assume the sequence of events is such that the incumbent observes the change in the electorate prior to the primary in election cycle $t$ and determines the binding campaign platform $x_t$ that would maximize her chances of reelection. Suppose further that the Democratic incumbent previously won election in period $t-1$ by adopting a position $x_{t-1}$ that maximized her ex ante probability of obtaining office, denoted $P(x_{t-1} \mid EP_{t-1}) \cdot G(x_{t-1} \mid EG_{t-1})$. Here $P(.)$ and $G(.)$ represent the probability of victory in the primary and general elections, respectively, $EP_{t-1}$ represents the primary electorate in $t-1$, and $EG_{t-1}$ denotes the general electorate. For example, $EG_{t-1}$ might be a vector containing the ideal points of all the voters general electorate. It is easily verified that the optimal position lies somewhere between the median voter in the primary electorate, $\mu_{p_{t-1}}$, and the median voter in the general electorate, $\mu_{g_{t-1}}$ (cf., Cox 1984). I will assume that the incumbent compiles a voting record in Congress that is consistent with $x_{t-1}$. In other words, his/her DW-Nominate score in $t-1$ accurately tracks $x_{t-1}$.

**Proposition 4.1:** If $P(\cdot \mid EP_t)$ is less sensitive than $P(\cdot \mid EP_{t-1})$ and $G(\cdot \mid EG_t)$ is more sensitive than $G(\cdot \mid EG_{t-1})$, then the optimal location $x_t$ should shift right.\(^{66}\)

\(^{66}\) My set-up is only choice theoretic at this point. Both Agranov (2010) and Hummel (2009) develop a proper equilibrium model. Both models are also more general in the sense that candidates have some capacity to change their policy platform after the party primary.
In practice, what determines an incumbent’s probability of success in an election is the relative strength of the opponent. So, in formal terms, we can assume the VRA increases the black proportion of the registered electorate in a particular district—$\beta \in (0,1)$—which affects the probability the incumbent will face a viable challenger in the primary and general elections—denoted $\pi(\beta| x_{t-1})$ and $\gamma(\beta| x_{t-1})$, respectively. If the incumbent is fully optimized prior to the influx of black voters, $\pi(\beta| x_{t-1}) = \gamma(\beta| x_{t-1})$ in t-1. The optimal post-shock response thus depends on the relative size of the change in $\pi(\beta| x_{t-1})$ and $\gamma(\beta| x_{t-1})$. If $\Delta \pi(\beta| x_{t-1})$ exceeds $\Delta \gamma(\beta| x_{t-1})$, the strategic Democrat moves left, if the reverse inequality obtains, she shifts right. If the influx affects both probabilities equally, there is no change in the equilibrium policy—see figure 4.1.

The equilibrium policy consequences of the “two front war”

Figure 4.1
So, the question becomes, when would a Democratic incumbent have worried more about his primary competition relative to his general competition? And when should these relative concerns have shifted more decisively in favor of worrying about winning the general rather than the primary?

For the South, there are a number of specific factors to consider. The most prominent theories explicitly linking the influx of black voters to the rise of strong conservative candidates focus on the behavior of white voters and its relation to the rise of Southern Republicans. As I discussed in chapter 3, Glazer, Grofman, Owen (1998) make a theoretical argument that the introduction of new black voters to the Party’s primary electorate resulted in the defection of conservative white Democrats to the GOP. Hood, Kidd, & Morris (2004) develop a very similar story in their “relative-advantage” theory. Scholars like Polsby (2005) argue that the G.O.P. ranks were swelled by the immigration of non-southern whites to the region’s urban areas.

To the list of factors affecting $\Delta \pi(\beta| x_{t-1})$ and $\Delta \gamma(\beta| x_{t-1})$, I add the following (a.) ideological (as opposed to partisan) white backlash; (b.) the relative barriers to entry for Democratic and GOP challengers, and (c.) the relative organizational capacity and financial resources of the forces of the left and right in the focal election. Note that in House races strategic redistricting was another variable that would have affected policy outcomes through its influence on (a)-(c.).
How might this dynamic have worked in practice? Consider for instance the relevance of the local organizational capacity of the right versus left (cf., Lipset & Rokkan 1967)—if the African-American community failed to organize and finance a viable candidate but the GOP succeeded in doing so, the incumbent faces no pressure to move left as he will win the primary. Likewise, if the incumbent possessed a firm grasp on the nomination proceedings in his or her primary while the G.O.P.’s primary offered lower barriers to entry, a rightward policy shift would seem more likely. In contrast, if the Democratic electoral machine was capable of keeping Republican nominees off the ballot but had less control over the outcome of the Democratic primary, a leftward shift seems more plausible.

B. When exit is the optimal response to leftist entrants

Regarding congressional career outcomes, the Downsian model focuses on parties that can costlessly re-position themselves. In the present context I argue it’s better to focus instead on candidates, who expect to pay reputational costs if they change their ideological location. Why candidates rather than parties? Because in the U.S., individual candidates largely determine their own policy positions, rather than being constrained to adopt a centrally chosen platform. Why costly rather than costless mobility? Because there is substantial evidence that candidates who change policy positions pay costs for doing so, for example, in terms of voters’ ability to identify their true policy positions (cf., Bernhardt & Ingberman 1985; Bartels 1986, p. 709).67

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So what where the theoretical expectations regarding congressional career outcomes? Suppose again that the incumbent—whose current policy position is \(x_{t-1}\)—is faced with the prospect of structural changes that will relocate the median of the general electorate in her district from its current location, \(\mu_{G,t-1}\), to a new location in election cycle \(t\), such that \(\mu_{Gt} < \mu_{G,t-1}\). The potential candidate must decide on a new position for a campaign in \(t\), denoted \(x_t\). Suppose the incumbent expects to incur a cost \(c(x_t ; x_{t-1})\) from such a move. This re-positioning cost may consist of a psychic cost from substantively representing what one believes to be an inferior group, for instance, or a monetary cost, like an advertising fee for marketing one’s new platform, or a payroll addition for hiring new African-American staff members. I assume \(c(.)\) is increasing in the magnitude of the incumbent’s policy re-positioning.

If one makes standard technical assumptions about the electoral environment, the incumbent legislator faces a well-defined maximization problem. For example, assume a simple model wherein the potential candidate chooses a policy position \(x_t\) to maximize \(P(x_t, \mu_{Pt}) G(x_t, \mu_{Gt}) B - c(x_t, x_{t-1})\), where \(P(.)\) and \(G(.)\) represent the probability of successful re-election and the benefits from returning to office, respectively. We might specify that \(P(x_t, \mu_{Pt}) G(x_t, \mu_{Gt}) = \Phi(\alpha_{Pt} - | x_t - \mu_{Pt}|)\Phi(\alpha_{Gt} - | x_t - \mu_{Gt}|)\) so that the probability or reelection in period \(t\) is a function of election specific fixed-effects— \(\alpha_{Pt}\) and \(\alpha_{Gt}\). I will further specify a quadratic cost function \(c(x_t ; x_{t-1}) = (x_t - x_{t-1})^2\), so that the incumbent’s cost of repositioning is increasing in the magnitude of the change.
As in a natural Bayesian model à la Shepsle (1972; p. 1976); and Bernhardt & Ingberman (1985), we can assume voters prefer candidates who are ideologically closer in expectation and about whom there is less uncertainty, ceteris paribus. Denote the politician’s optimal platform \( x^* \), and note that in such a model her re-positioning puts her nearer to the new district median \( \mu_t \) (thereby improving her electoral prospects) but also increases the electorate’s uncertainty about the candidate’s policy position (which tends to decrease her attractiveness to risk-averse voters). This structure leads to the following predictions about incumbent \( i \)'s career outcomes.

**Proposition 4.2:** Structural changes that lead to increases in either the cost of optimal repositioning, \( c(x^*; x_t) \), or the candidate’s optimal distance from the median, \( |x^* - \mu_t| \), lead to an increase in each of the following: \( Pr[i \text{ retires}] \); \( Pr[i \text{ seeks another office}] \); and \( Pr[i \text{ loses } | i \text{ seeks re-election}] \).

In the next section (and appendix 4.2) I present a number of research designs to make the most of the available data. The idea is that I want to identify the VRA-related factors that increased the likelihood southern legislators were forced out of what (I presume) is their preferred career path, i.e., extending their congressional careers.

V. The VRA’s impact on southern congressional exit rates

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\(^{68}\) One could adduce numerous such theoretical studies assuming that risk-averse voters should punish ambiguous candidates, under various conditions—e.g., Alesina & Cukierman (1990); Aragones & Neeman (2000); Meirowitz (2005); Callander & Wilson (2008). For empirical evidence, see, e.g., Bartels (1986; p. 709) and Brady & Ansolabehere (1989). For contrary findings see Tomz & Van Houweling (2009).
A critical point that has been overlooked in the literature is that the career outcomes of incumbent legislators is a theoretically important aspect of post-VRA politics, and one that is not well addressed in the standard spatial framework. For instance, given Keith Poole’s (1998; p. 3) evidence that politicians don’t change ideologically speaking, but “die in their boots,” our prior might be that southern politicians were only pushed out of office when their policies became too out of sync with the altered electorate—they didn’t adapt to increase their fitness in the new electoral environment. Indeed, there is speculative evidence for this point of view: David Lublin (2004; pp. 14-15), for instance, suggests that, “[i]n contrast to more dramatic revolutions, the Civil Rights Movement did not remove much of the old elite from public office. Existing officials continued to occupy their offices and could wield substantial resources to maintain them even in the face of the expanded franchise.”

The analysis in this section addresses the question econometrically. My results suggest that there was a perceptible—if not overwhelming—“Downsian” effect on congressional career outcomes, but the relationship is strongly mediated by whether or not the incumbent had her district re-drawn prior to the election.

It is useful to briefly point out first how bad the data problem is for House races in the ‘60s: Between the ‘52 and ‘72 decennial reapportionments there were 106 southern congressional districts, suggesting that there could have been as many as 530 elections of interest for congresses seated in the 1960s—in fact, I am interested in the change between elections (which reduces the sample size by 106), moreover, some incumbents
weren’t eligible for re-election, so the conceptual universe is smaller still (N=418). As far as I am aware, the only existing estimates of the relevant racial registration rates below the state-level come from U.S. Civil Rights Commission and Urban League reports for select counties, for the 1960s, but not prior. These county-level data were compiled incrementally by Matthews & Prothro (1963) and Alt (1994), then aggregated by me for use at the U.S. congressional district level.

The unit of analysis is an incumbent member from the South who was eligible for reelection in 1962-1970, and the Downsian “treatment” is the inter-election change in the percent of the district’s registered voters black, \( \Delta\%\text{registrants black}_i \). The probit regressions I estimate are meant to model the probability of forced exit—by which I mean that the focal politician either chooses not to run for reelection, or suffers defeat in a general or primary election. The outcome variable is a random variable \( E_i \) that takes the value 0 if incumbent \( i \) successfully defends his seat in election \( t \), and 1 if he exits office. I model candidate \( i \)’s probability of exit as

\[
\Pr[E_i=1] = \Phi[\delta_0 + \delta_1 \text{ previous vote share}_i + \delta_2 \text{ GOP}_i + \delta_3 \Delta\%\text{registrants black}_i + \delta_4 \Delta\% \text{ blacks registered}_i + \delta_5 \text{ redistrict}_i + \delta_6 \text{ redistrict*previous vote share}_i + \delta_7 \text{ redistrict*GOP}_i + \delta_8 \text{ redistrict*\Delta\%registrants black}_i + \delta_9 \text{ redistrict*\Delta\%blacks registered}_i].
\]

See the maximum-likelihood parameter estimates in Table 4.2: 42% of the member-congresses for which I have sufficient data to include in the regression are members who were running for a redistricted seat (N=92). Among these redistricted
politicians, the effect of increases in $\Delta%\text{reg black}_i$—the most straightforward operationalization of the Downsian mechanism appears to have been an increased probability of exit, though the coefficient is not significant at conventional standards ($\hat{\delta}_8 = 8.24; p = .14$)—see model I of Table 4.2. A related variable, the inter-election change in the percentage of African-Americans who were registered $-\Delta%\text{blacks registered}_i$—produces a statistically significant effect. Among redistricted incumbents (see model II), increases in $\Delta%\text{blacks registered}_i$ increased the likelihood of exit ($\hat{\delta}_9 = 9.03; p = .09$).

In contrast, in the remaining 125 cases where the incumbent was not re-districted, the estimated effect is small and statistically insignificant, for both $\Delta%\text{registrants black}_i$ (\hat{\delta}_3 = 3.12; p = 0.32) and $\Delta%\text{blacks registered}_i$ (\hat{\delta}_4 = -0.12; p = .96). Note the fact that the overall model fit is much better redrawn districts, and compare this to the multinomial logit results. Succinctly, there is some evidence of “Downsian” exit rate patterns, but only among redistricted incumbents.

Why would electoral shocks produce the expected outcome only in elections where the incumbent was redistricted? An obvious rationale (that complements the two front war thesis) derives from Fenno’s (1978) famous insights regarding the concentric circles of constituent influence (cf., Buena de Mesquita et al 2003).\(^69\) Incumbents whose district were affected by the wave of redistricting spawned by the early 1960s’ court decisions were probably being targeted by the state legislatures that drew district

\(^69\) See Christine LeVeaux-Sharpe’s work on congressional responsiveness to redistricting-induced constituency change in Kuzenski et al, (2001).
boundaries in such a way that African-Americans were important components of these
district’s selectorate (hence a bon fide electoral risk factor) but were less essential for
reelection in stable districts (on average). Note this is something more than a Downsian
model of politics! There is nothing in the Downsian model suggesting that the policy
response (hence, by extension, the career response) to a structural shock should depend
on redistricting one way or another.

How does this analysis relate to the literature? At the macro level, Gilmour &
Rothstein (1994, 1996) have studied the steady state properties of a legislature’s partisan
composition under various retirement regimes. My study and studies like it, analyze the
microfoundations generating the aggregate behavior Gilmour & Rothstein explore.
Explicitly or implicitly, these microfoundational studies take a broadly Downsian
perspective, in the sense that the propensity of an individual member to depart the
legislature is linked to characteristics of her district’s electorate.

The short synopsis of these “Downsian” studies of House career patterns is that
there was an upsurge in retirements that occurred in the 1970s (cf. Cooper & West 1981;
Frantzich 1978; Hibbing 1982), a subsequent decline in the ‘80s (cf., Moore & Hibbing
1991), and record levels of retirement in the early ‘90s, particularly after the eventful 93rd
congress (cf., Jacobson & Dimock 1994; Groseclose & Krehbiel 1994), and the
Republican Revolution of ’94.70

70 Need cites for 1994.
Regarding my research design, like Kiewiet & Zeng (1993)—and unlike other leading studies—I also control for redistricting and find that it has a significant mediating effect on career outcomes. My second study is also unique because it focuses on the racial composition of House electorates (as opposed to the size of the district’s minority population), and it looks at the 1960s South. The scarcity of registration data prevents me from employing a more general choice model like Kiewiet & Zeng (1993), so I use a conventional bivariate model to analyze those data here (as do, e.g., Jacobson & Dimock 1994; Groseclose & Krehbiel 1994). In section 4.2 of the appendix I exploit a coarser operationalization that allows me to use a more general multinomial logit model. The results are complementary.

How strong is the evidence? The most obvious problem with the House analysis is the lack of racial registration data for many elections in the ‘60s. U.S. secretaries of state are required to tally total turnout, not turnout by race (cf. Clubb et al 1986) and it is low total turnout, not low racial turnout, that triggers Section 5 of the VRA. While NES, CPS, and other surveys allow scholars to reliably estimate statewide racial turnout rates, beginning in the 1960s, the Alt-Matthews-Prothro registration dataset is apparently the only one of its kind below the state-level. Thus, my data is apparently the most comprehensive congressional scholars are likely to get.

While the coverage is limited, I have no reason to believe the excluded elections differ systematically, threatening the scope of my conclusions in the broader South. I also don’t control for member’s age here, as some other studies do (with mixed results:
Jacobson & Dimock 1994 find no effect, Kiewiet & Zeng 1993 find an effect). As I have no reason to believe that the age is systematically correlated with any of the independent variables I do include, I do not believe the omission biases the parameter estimates I report.

IV. The impact on southern policy

To quantify the Downsian effect, consider a linear regression model of the change in policy in district i between congresses t-1 and t, and assume policy changes are captured in the first dimension of the DW-Nominate score. Specifically, I define an indicator variable r and partition the sample into stable districts (r=0, N=119) and redrawn districts (r=1, N=89), then estimate the following regression equation separately for each subsample.

\[
\Delta \text{dwnom}_1 | r = \lambda_{r,0} + \lambda_{r,1} \text{previous vote share}_it + \lambda_{r,2} \text{lag Nominate}^* \Delta \% \text{registrants black}_it + \lambda_{r,3} \text{GOP}_it + \lambda_{r,4} \Delta \% \text{registrants black}_it + \lambda_{r,5} \text{GOP}^* \Delta \% \text{registrants black}_it + \lambda_{r,6} \Delta \% \text{blackregrate}_it + \lambda_{r,7} \text{GOP}^* \Delta \% \text{blackregrate}_it + \epsilon_{it} \quad \text{for } r = 0, 1. \]

Per Downs, the treatment variable is the change in the percent of district registrants black, \( \Delta \% \text{registrants black}_it \). The research design here is simple, but solid in the sense that it controls for all fixed idiosyncrasies of the focal congressional district, as the comparison being made is between the voting behavior of the district’s representative before and after an interelection change in the racial composition of his/her electorate.

The fact that identification is based on the difference in district policy scores sets my

\[ \text{Note this specification is equivalent to regressing } \text{dwnom}_it \text{ on } \text{dwnom}_i|t-1 \text{ and the given covariates, with the coefficient on } \text{dwnom}_i|t-1 \text{ constrained to equal 1.} \]
design apart from all previous studies of the ‘60s House, each of which regresses some policy measure on a set of covariates in a given session without including district fixed effects.

The policy regression results appear in Table 4.1. The estimates suggest that in *stable districts*, in contrast to Downs, an increase in the African-American proportion of the registered electorate led to a statistically significant *rightward* shift in the first dimension of its representative’s policy position. In such a district, when the incumbent was not too conservative in the previous Congress—i.e. *lag Nominate* was small—an increase in the black proportion of the effective electorate actually led to rightward shifts in the subsequent DW-Nominate score. This is true both when Democrats won office ( \( \hat{\lambda}_{0.0} = 0.495; p=0.14 \)) and when Republicans won office ( \( \hat{\lambda}_{0.5} = 1.60; p=0.14 \)). Note the estimate for \( \lambda_{0.2} \) is significant and highly negative ( \( \hat{\lambda}_{0.2} = -5.89; p=0.007 \)) however, which implies that as the incumbent’s policy position in the previous congress became counterfactually more conservative, the impulse to move left to accommodate newly enfranchised African-Americans was more likely to dominate the countervailing rightward pressures.

---

Would data permit, one could alternatively use a random effects model that allows the Downsian effect to vary by district—in these cases, the first coefficient becomes \( \lambda_d = (\hat{\lambda}_d + \theta_d) \), where \( \theta_d \) is the vector of parameters capturing the district-specific effects. As far as I am aware, no existing study of the 1960s House controls for district fixed effects (or random effects), though some studies of the Senate do (e.g., Wilson & Butler 2007).
In redrawn districts on the other hand, changes in $\Delta\%\text{registrants black}_a$ were not significantly associated with movement in Democrats’ DW-Nominate scores ($\hat{\lambda}_{1.4} = -0.229; p=0.35$), though positive changes in this variable precipitated a large, statistically significant rightward shift among House Republicans ($\hat{\lambda}_{1.5} = 5.96; p=0.002$). Thus, Democratic politicians in redrawn seats did not respond to the ostensibly left-of-median electoral shock by shifting their policies right—as they sometimes did in stable districts—but they clearly didn’t move policy left either, as the simple spatial model predicts—and Republicans in redrawn districts always responded to greater black electoral strength by moving right.

What technical contributions have I made to this literature? For House races, I aggregated scarce county-level data on the % of registrants African-American for use at the U.S. congressional district level—rather than relying, as all previous studies have, on the readily available, but theoretically problematic alternative, % of the district’s VAP African-American. (It goes without saying that the first variable changed dramatically relative to the second in the 1960s.) Looking at registration data allows me to control for white counter-mobilization and it allows me to adopt certain desirable elements of the leading Senate studies, where the availability of statewide NES and CPS registration estimates has permitted scholars to implement more sophisticated research design than has previously been possible for analysis of the post-VRA House.
Unlike existing studies, I also employed scaled roll-call agreement scores rather than House interest group scores and leveraged the panel structure of the data more fully than existing studies of the House and Senate—all of which use either inefficient longitudinal, or, cross-sectional designs that, while valuable, rank relatively low in the conventional hierarchy of research designs (cf. Trochim 2001; Wilson & Butler 2007). All told, my analysis of policy change in the U.S. House incorporates large methodological improvements relative to existing research designs. My contributions to post-VRA Senate scholarship are more modest, though I am able to report a few new findings for the Senate as well—e.g., concerning the effect of seniority and percentage of the state African-American on Senate policy scores, see appendix 4.1.

VI. Summary of empirical findings

In this chapter I developed a novel theory of post-VRA southern politics—the “two front war” thesis—and presented some new empirical studies of congressional career outcomes and voting patterns in the 1960s. Two themes emerged. The first is that evidence supporting the unsophisticated Downisan expectation that policy should have shifted left post-VRA is difficult to find, particularly in elections where conservatives had the opportunity to mitigate leftward electoral pressure through other means—e.g., strategic redistricting—or where the GOP posed a legitimate threat on the Democrat’s right.

Epstein, Herron, O’Halloran, & Park (2007) use Bayesian MCMC scaled roll-call voting scores in their study of South Carolina state senate, but they use black VAP rather than racial registration data, and they don’t study the U.S. House.
The fact pattern I document is entirely consistent with my theory. In terms of career patterns, there is some evidence that the ostensible Downsian mechanism was working as theory predicts. The main findings are as follows.

- There is potentially interesting regional variation in national congressional career patterns in the immediate post-VRA era: Multinomial logit analysis of U.S. congressional seats reveals that districts in the post-VRA South (1966-68) were more likely to see their incumbent relinquish her seat in search of another political office. As this exit path accounts for over a third of exits during the period, the VRA had a substantial (if not overwhelming) effect—see appendix 4.2.

- Using a new dataset to provide a more precise look at legislators in southern districts, I find that chbr was a significant positive predictor of southern exit rates in the House when an incumbent was running for a redistricted seat. The effect of $\Delta$%registrants black produced a similar “Downsian” effect that was not quite significant and conventional levels.

- Among non-redistricted incumbents, leftward shocks to the racial composition of the registered electorate did not significantly increase exit rates.

With regards to the policy positions adopted by southern legislators, the outcomes look convincingly anti-Downsian in the House and moderately Downsian in the U.S. Senate.

- The policy response in the House was mediated by redistricting and anti-Downsian. Contra Downs, the inter-election change in the percentage of the registered electorate African-American produced a substantial, statistically significant rightward policy shift in districts that were not re-drawn. Among redistricted seats there was no statistically policy effect from increases in the proportion of black registrants. This finding is novel.

- With respect to the policy impact in the Senate, note (a.) redistricting was not an issue and (b.) increases in the state proportion of registrant’s African-American produced a statistically significant leftward shift in southern Democrats’ ADA and DW-Nominate scores (cf., Hood, Kidd, & Morris 1999).
I argued that my two front war theory explains the difference between the House and Senate policy responses, provided one believes that the civil rights leadership was better prepared to organize a political campaign at the state rather than congressional district level.

My study differs from previous work first, because I use racial registration rates in my analysis of the House. Second, I employ scaled coordinates of roll call votes rather than interest group scores, the previous standard in House and Senate studies of the ‘60s. Third, I exploit the panel structure more efficiently—per Wilson & Butler (2007)—by looking at the change in policy following a change in the racial composition of the district electorate. Fourth, in my House studies I account for redistricting. Fifth, I develop a novel theory that rationalizes the fact pattern.

VII. Concluding remarks

Regarding the current status of the anti-Downsian puzzle, it is important to point out some shortcomings of my analysis, beyond the obvious issues related to the quantity and quality of the registration data.

One limitation of my design (and all previous studies of the House and Senate) is that it assumes the relationship between the treatment variable (in my case, $\Delta{\%regblack}_{it}$) and the outcome variable (Adwnom1_{it}) is linear. Theoretically, the Downsian policy response is a real-valued function of the distribution voter ideal points, though it is unlikely to be linear (cf., Barreto & Grofman 2010). By way of example, suppose as in section I of chapter 2, that the population of voters is comprised of blacks and whites
with population mass .4 and .6, respectively, whose ideal points are stacked at $\mu_B=0$ and $\mu_W=1$, respectively. No structural increases in the black proportion of the electorate $\beta \in [0, 1)$ will change the equilibrium policy in this degenerate setting, because the median wouldn’t change—locally, $dx^*(\mu(\beta))/d\beta=0$ according to Downs. It speaks to the robustness of my results that I found significant effects with an imprecise linear regression model.

Another shortcoming is that the DW-Nominate scores can only recover preferences in latent policy dimensions that were affected by the set of bills that actually reached the floor (cf., Gailmard & Jenkins 2009). In the case of the South, one would want to consider the possibility that the divergent scores post-’65 (precipitated by an apparent rightward shift of southern members) are not just picking up changes in the legislative agenda.\footnote{Also recall Cox & McCubbins’s (2008) “broken promise” story and the phenomenon of the southern Boll Weevils (cf., Lublin & Voss 2003).}

Another potential problem with my study is that my operationalization of policy change—viz., a change in the first dimension of the focal representative’s DW-Nominate score—potentially obscures the multidimensional bargaining that generated the roll call data underlying the measure. Indeed, Jeong, Miller & Sened’s (2009) analysis of Senate voting records on four hallmark civil rights bills—i.e., the Civil Rights Acts of 1957 and 1964, the Voting Rights Act, and the 1974 reauthorization of the Education Act of 1965—finds that, (a.) politics was two-dimensional in 3 of these 4 civil rights bills, and,
(b.) contrary to the conventional wisdom, President Johnson and the northern Democratic leadership engaged in horse-trading to guarantee passage. In fact, the authors advocate for interpreting the bargaining process that produced the landmark civil rights legislation in terms of the uncovered set—i.e., a more general multidimensional solution concept (cf., Cox 1987).

It is an interesting question whether the civil rights bills Jeong, Miller & Sened study are representative of the broader post-VRA policy dynamics captured in the DW-Nominate scores I study—but the findings certainly raise flags for a unidimensional regression model like mine.75

Finally, one could dispel the validity threat that southern legislators only appear more conservative (artifactually) because northerners were becoming more liberal, by appealing to the technical arguments underlying DW-Nominate scores. Intuitively, the policy space is identified using knowledge of the policy positions of legislators who served prior to the introduction of the structural break under the assumption that these legislators had stable preferences (cf., Poole & Rosenthal 2005).

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75 My analysis of the Downsian impact on $\Delta_{dwnom2}$ did not produce statistically significant results. An alternative approach is to look at the scaled policy position computed exclusively on roll call votes where black voters had unambiguous policy preferences—e.g., civil rights or social welfare issues (cf. Wilcox & Clausen, 1991). I could also look at LCCR scores a la Hood, Kidd, & Morris (2002).
Table 4.1: Probit Analysis of Southern House Exit Rates, 1960-1970

<table>
<thead>
<tr>
<th>Covariate</th>
<th>Stable Districts (1.)</th>
<th>Re-drawn Districts (2.)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Model I</td>
<td>Model II</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Model I</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Model II</td>
</tr>
<tr>
<td>Vote Share</td>
<td>0.01†</td>
<td>-0.147*</td>
</tr>
<tr>
<td>in Previous Election</td>
<td>(0.14)</td>
<td>(0.081)</td>
</tr>
<tr>
<td>Member of GOP</td>
<td>0.87</td>
<td>-0.911</td>
</tr>
<tr>
<td></td>
<td>(0.09)</td>
<td>(0.081)</td>
</tr>
<tr>
<td>Change in Registrants</td>
<td></td>
<td>3.12</td>
</tr>
<tr>
<td>African-American</td>
<td></td>
<td>(0.32)</td>
</tr>
<tr>
<td>(percentage points)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Af.-Am. Registration</td>
<td>-0.12</td>
<td>9.03†</td>
</tr>
<tr>
<td>Rates</td>
<td>(0.96)</td>
<td>(0.09)</td>
</tr>
<tr>
<td>% District Black</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>-2.42***</td>
<td>7.1†</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.131)</td>
</tr>
<tr>
<td>N</td>
<td>125</td>
<td>91</td>
</tr>
<tr>
<td>Pseudo R²</td>
<td>0.04</td>
<td>0.52</td>
</tr>
<tr>
<td>LR χ²</td>
<td>2.95</td>
<td>17.2***</td>
</tr>
<tr>
<td>χ² p-value</td>
<td>0.39</td>
<td>0.0006</td>
</tr>
</tbody>
</table>

Notes:
The columns report maximum likelihood estimates for a probit regression modelling "forced exit" from the U.S. House. The dependent variable takes the value 1 if the incumbent did not return to office because s/he either chose not to seek re-election or lost in the primary or general election.

The covariates were interacted to produce estimates for incumbents in (1.) stable, and (2.) redrawn districts separately. The differences between models I and II are described in the text.

The sample includes House incumbents from the 11 ex-Confederate states who where not expelled or deceased before announcing their career plans and for whom sufficient registration data is available. See the text for a discussion of the data limitations.

p-values reported in parantheses.
***, **, * represent statistical significance at the .01, .05, and .1 confidence levels, respectively.
† denotes significance at the .15 level.
Table 4.2: Policy Responsiveness in the U.S. House, 1960-1970

<table>
<thead>
<tr>
<th></th>
<th>Stable (1)</th>
<th>Redrawn (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Δdwnom1</td>
<td>Δdwnom1</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Incumbent Vote Share in</strong></td>
<td>0.002***</td>
<td>-0.0004</td>
</tr>
<tr>
<td><strong>Previous Election</strong></td>
<td>(0.005)</td>
<td>(0.68)</td>
</tr>
<tr>
<td>Lag Nominate*Δ % Reg Black</td>
<td>-5.89***</td>
<td>-0.539</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.36)</td>
</tr>
<tr>
<td>GOP indicator</td>
<td>0.327***</td>
<td>-0.007</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.93)</td>
</tr>
<tr>
<td>Intercept</td>
<td>-0.234***</td>
<td>0.054</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.50)</td>
</tr>
<tr>
<td>Dem</td>
<td>0.495†</td>
<td>1.60†</td>
</tr>
<tr>
<td>GOP</td>
<td>(0.14)</td>
<td>(0.14)</td>
</tr>
<tr>
<td>Change in % of District Registrants Black (%) pts</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Dem</td>
<td>-0.344†</td>
<td>-0.521</td>
</tr>
<tr>
<td>GOP</td>
<td>(0.15)</td>
<td>(0.52)</td>
</tr>
<tr>
<td>Change in Black Registration Rates (%) pts</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Dem</td>
<td>0.36</td>
<td></td>
</tr>
<tr>
<td>GOP</td>
<td>0.26</td>
<td></td>
</tr>
</tbody>
</table>

Notes:
The columns report maximum likelihood OLS regression estimates for the change in the first dimension of southern congressional districts’ DW-Nominate scores between consecutive congresses. By a district’s Nominate score, I mean the score of its congressional representative during the focal Congress (Poole & Rosenthal 2000). A negative regression coefficient suggests the covariate produces leftward policy changes. A positive coefficient suggests the covariate produces rightward shifts.

The sample includes each of the 105 U.S. House districts in the former confederacy for which there was sufficient registration data. See the text for a discussion of the data limitations.

I ran separate regressions for (1.) districts that were not redrawn between consecutive elections, and (2.) districts that were. Gary Jacobson produced the redistricting coding.

p-values reported in parentheses.

***, **, * represent statistical significance at the .01, .05, and .1 confidence levels, respectively.
† represents significance at the .15 level.

The DW-Nominate scores are designed to scale policy positions into a common space across time, hence the fact that the coefficient estimate on the intercept term for stable districts (-0.234***) is significant and negative means that, ceteris paribus, unredistricted Democrats were trending left. There was no such trend among members of the GOP.
Chapter 5.
Lessons from the VRA

I. Introduction

Beyond addressing specific puzzles in southern political scholarship, the central rationale for my intensive VRA study has been to draw out broader conclusions about the substantive implications of structural turnout. Towards this end I developed a number of straightforward theoretical benchmarks (in chapter 2) and endeavored to test these in my VRA case study. Specifically, I revisited the strict Downsian model of spatial competition and employed some natural extensions to the basic model to develop the argument that structural turnout “matters.” It matters in the sense that electoral considerations induce politicians to reward groups with higher structural turnout rates—immediately, or “eventually”—with more favorable public policy and a larger allocation of targetable government expenditures.

While the general comparative perspective and the Downsian model work well in some respects, there are also a number of obvious ways in which the South is peculiar. Given the dominance of the Democrats for instance, there was little inter-party competition, (one reason to be skeptical of a too-facile appeal to the median voter theorem) and the “left” party espoused some very conservative policies, especially as regards the issues of race and labor relations (a second reason).
As a practical matter, the challenge for understanding the VRA’s impact is to disentangle the Downsian effects precipitated by the influx of black voters from several confounding factors, including the Downsian effect of white counter-mobilization and white backlash (change of ideal points) and “non-Downsan” effects—e.g., changes in partisan valence, redistricting, and candidate entry and exit.

II. Why didn’t higher structural turnout favor the Left?

So what to make of the decidedly anti-Downsian results I document in the U.S. House? Recall that, unlike the modal franchise extension, the VRA was very much externally imposed. Thus, to the extent that the VRA posed a threat to the status quo, the powers that be would be expected to have exerted great effort to mitigate its impact (this is the lesson of Proposition 2.5). Indeed, in phenomena like the Massive Resistance movement there is ample empirical evidence of elite resistance. I also made a theoretical argument that strategic gerrymandering could have been a useful tool in the hands of reactionary southern legislatures inclined to stymie political change. In fact, for the first time, I have documented a mediating effect of redistricting in the House response to the VRA.

Another factor I stress is the possibility that the organizational capacity of the forces on the left and the forces on the right differed widely—to wit, it may have been the case that the civil rights movement was optimized to advanced its agenda at the national and state levels but less capable of developing the grass-roots campaigns needed to win
House elections. I argue that this could explain why the anti-Downsian outcomes are present in House, but not in the Senate (cf., Hood, Kidd, Morris 1999).

Apropos this explanation, consider the skepticism expressed by Barack Obama when he argued that an overemphasis on the courts as an engine of political change has historically distracted liberals from the arduous task of winning elections. Obama claimed that, “one of the tragedies of the civil rights movement, was because the civil rights movement became so court-focused—I think there was a tendency to lose track of the political and community-organizing activities on the ground that are able to put together the actual coalitions of power through which you bring about redistributive change. And in some ways we still suffer from that” (PBS 2001).

Later, in 2006 Obama explained his reluctance to oppose President Bush’s judicial nominees in the following terms: “I wondered if, in our reliance on the courts to vindicate not only our rights but also our values, progressives had lost too much faith in democracy,” he wrote. “Elections ultimately meant something…Instead of relying on Senate procedures, there was one way to ensure that judges on the bench reflected our values, and that was to win at the polls” (Obama 2006).

III. Directions for future research

The question of how turnout influences political outcomes is critical for illuminating the meaning and consequence of elections. Despite (or perhaps because of) the positive and normative importance of the issue, central aspects of how turnout affects
democratic outcomes remain contested. Thus, I argue that understanding why the Downsian expectation was not met in the post-VRA South warrants more scholarly attention.

Regarding the VRA’s legislative consequences, I made a number of improvements on existing studies of the U.S. House and offered a theory rationalizing the observed patterns in congressional legislative voting—i.e., the “two-front war” hypothesis. The theory notes that the incumbent faced challenges on both the left and the right, and posits simply that she should have responded to whichever challenge was greater. While the theory offers a plausible rationale for the fact pattern I documented in chapter 4, it clearly lacks definitive empirical testing.

In addition to the puzzling political outcomes I addressed are vexing economic trends that lack a clear explanation—specifically, despite the South’s rapid economic development in the latter half of the twentieth-century, African-Americans have systemically lagged behind. Fairclough (1990), for instance, writes that “[w]ith a few exceptions, historians and political scientists are more likely to stress what [the civil rights movement] has failed to achieve. School desegregation did not yield the social and educational dividends envisaged by its supporters, who often erased segregation de jure only to see it transmuted to segregation de facto. The integration of public accommodations has been far less significant that once thought. The enfranchisement of southern blacks has not upset white domination of state politics. A distressing number of
blacks suffer from poverty, crime, drugs, and family breakdown” (pp. 997-98).¹

In fact, the extent to which we can trace the puzzling post-VRA economic outcomes to characteristics of the voting regime has concerned scholars of the South for some time. For instance, without the benefit of much hindsight Keech (1968, p. 1) asked, “[d]oes the vote have an influence over the distribution of advantages that is separate from the influence of those social, economic and political factors that are associated with Negroes getting the vote in the first place?” Gavin Wright’s (1999; p. 274) presidential address to the Economic History Association some 30 years later suggests that scholars had made little progress in elucidating the causal relationships, “[t]he surge in black voter registration raises a topic that should get more attention from economic historians: what is the connection between voting and economic advancement?”²

Regarding this project, I note that it stands to reason that the VRA could have paid meaningful dividends to the newly enfranchised that may not be apparent in the legislative behaviors scholars typically study—e.g., congressional roll call voting. I propose therefore that an interesting research questions is (1.) how post-VRA changes in turnout patterns affected the calculus of targetable governmental expenditures, and (2.) the empirical effect such transfers had on African-American material wellbeing—e.g., in terms of per capita earnings, infant mortality, or mean life expectancy. Such a study

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² Viewed in a certain light, the question is fundamentally about the relationship between democratization and economic development (cf., Przeworski, Alvarez, Cheibub, Limongi 2000).
would add to the few existing analyses in the U.S. attempting to link variation in material outcomes of local constituencies to political characteristics of the district’s electorate (cf., Levitt & Poterba 1999).

There are two reasons to explore the economic consequences of the VRA in greater detail. One reason is to resolve the puzzling political and economic outcomes that postdate the VRA. Another reason is to develop a simultaneous model of the public policy and targetable expenditure implications of voter turnout (cf., Jackson & Moselle 2002). A more comprehensive approach along these lines would provide a stronger basis for understanding other historical structural turnout changes and formulating hypotheses about the likely consequences of unrealized changes.
APPENDIX

2.1: Proof of Lemma 1

Note that \( \frac{\partial E[V_L - V_R]}{\partial m_{Lg}} = N_g [f_{Tg}(\alpha_g^T + m_{Lg} - \sigma_g) [2F_{Lg}(B_g - m_{Lg} + \alpha_g^L) - 1] - 2 F_{Tg}(\alpha_g^T + m_{Lg} - \sigma_g) f_{Lg}(B_g - m_{Lg} + \alpha_g^L)] \). If \( 2 F_{Lg}(B_g + \alpha_g^L) - 1 < 0 \), then \( \frac{\partial E[V_L - V_R]}{\partial m_{Lg}} < 0 \) for all \( m_{Lg} \), hence \( m_{Lg}^*(\alpha_g) = 0 \), a corner solution.

2.2: Proof of Proposition 2.3

Let \( Q(\alpha_g) = f_{Tg}(\alpha_g^T + m_{Lg}^*(\alpha_g) - \sigma_g) [2F_{Lg}(B_g - m_{Lg}^*(\alpha_g) + \alpha_g^L) - 1] - 2 F_{Tg}(\alpha_g^T + m_{Lg}^*(\alpha_g) - \sigma_g) f_{Lg}(B_g - m_{Lg}^*(\alpha_g) + \alpha_g^L) \). We know from the proof to Lemma 1 that \( Q(\alpha_g) = 0 \) for all \( \alpha_g \) in the set of values yielding an interior solution. Dropping the arguments for convenience and differentiating \( Q(\alpha_g) \) with respect to \( \alpha_g \), we get \( (2F_{Lg} - 1) f_{Tg}(\frac{\partial m_{Lg}^*}{\partial \sigma_g} - 1) \)

\[ -2F_{Tg} f_{Lg} \frac{\partial m_{Lg}^*}{\partial \sigma_g} + 2F_{Tg} f_{Lg} \frac{\partial m_{Lg}^*}{\partial \sigma_g} - 2F_{Tg} f_{Lg} (\frac{\partial m_{Lg}^*}{\partial \sigma_g} - 1) = 0. \]

Some algebra shows that \( \frac{\partial m_{Lg}^*}{\partial \sigma_g} = \frac{(2F_{Lg} - 1) f_{Tg}^/ - 2 f_{Lg}^/}{(2F_{Lg} - 1) f_{Tg}^/ - 2 f_{Lg}^/ - 2 f_{Tg} f_{Lg}^/ + 2 F_{Tg} f_{Lg}^/}. \) We know from Lemma 1 that, if there is an interior solution, then \( 2F_{Lg} - 1 > 0 \). Because \( F_{Lg} \) is symmetric about its median by assumption, it follows that \( f_{Lg}^/ < 0 \). Similarly, because \( F_{Tg}(\alpha_g^T - \sigma_g) > .5 \), it follows that \( f_{Tg}^/ < 0 \). Thus, all told, \( \frac{\partial m_{Lg}^*}{\partial \sigma_g} > 0 \).

2.3: Intuition of Proposition 2.4

One way to incorporate the idea that candidates’ efforts to persuade voters have a cumulative effect on citizens’ predispositions is to suppose that each citizen \( i \) has an “effective” ideal point given by \( v_i = v_i^f(p_{Li}^h, p_{Ri}^h) \), where \( v_i \) is \( i \)’s “natural” ideal point, and \( p_{Li}^h \) and \( p_{Ri}^h \) represent the historical efforts of the parties to shape \( i \)’s preferences.

3.1: Synopsis of the Voting Rights Act of 1965

By 1965, a number of landmark Supreme Court cases had eroded some major structural barriers depressing southern African-American voting rates—recall, e.g., Smith
v. Allwright (1944). Still other obstacles were removed by federal legislation, notably the Civil Rights Act (enacted in 1964) and the 24th Amendment (ratified in 1964). The VRA dissolved the racially motivated structural barriers that remained in August of 1965, and by all accounts, precipitated the largest increase in voter turnout of the civil rights era. See, e.g., Kousser (1974); Lawson (1976); and Ogden (1958) for a detailed historical account of the structural voting regime the VRA replaced.

The text of the VRA begins with a broad reassertion of the principle embedded in the reconstruction amendments—the 14th and 15th—namely, that no party could “deny or abridge the right of any citizen of the United States to vote on account of race or color.” This is the crux of Sections 1 and 2 of the VRA. After this, the act imposes legal constraints on states (and in some cases, counties) where voter participation was particularly low in the presidential election of November 1, 1964. Specifically, areas where less than 50 percent of the voting age residents were registered to vote, or less than 50 percent had voted in the 1964 presidential elections, became known as “covered” or “pre-clearance” jurisdictions and were subject to federal scrutiny of virtually all legal aspects of elections held there. In 1965 the covered states were Alabama, Georgia, Louisiana, Mississippi, North Carolina (in 34 counties), South Carolina, and Virginia. Logically, it was in these covered districts that the VRA had the largest structural effects.

Section 5 of the VRA outlines the strict requirements in covered “pre-clearance” jurisdictions—including tacit approval of virtually any change to the structural voting regime, from moving a polling place across the street, to large-scale re-districting—by either a three judge panel drawn from the U.S. District Court for the District of Columbia, or the U.S. Attorney General. In Section 6, the bill authorized federal registrars to supervise voter registration in covered counties, more on which below.

A year prior to the VRA, the Civil Rights Act established that wherever literacy tests were used as a qualification for voting in federal elections, they must be administered (1.) wholly in writing, and (2.) only to persons who had not completed six years of formal education. Section 4 of the VRA suspended the use of literacy tests in all “covered” states or political subdivisions. The VRA also deemed illegal the requirement of English fluency for voting eligibility (apparently as a concession to a well-placed Puerto Rican lobby in New York City).

The 24th Amendment, ratified in 1964, abolished the poll tax as a qualification for all federal elections. The tax remained a voting qualification however for state and local

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3 The specific pre-clearance trigger was chosen to induce oversight in the areas that were still recalcitrant in 1965—the southern regions that did not fall under Section 5 coverage avoided federal intrusion by liberalizing their voting regimes in the years leading up to 1965.

4 In 1965, the VRA suspended the literacy test in each former Confederate state save Arkansas, Florida, and Texas, which did not have a literacy test at the time. Confirm that the VRA didn’t outlaw literacy tests everywhere.
elections in Alabama, Arkansas, Mississippi, Texas, and Virginia. In Section 10 of the VRA, Congress directed the Attorney General (Nicholas Katzenbach, at the time) to challenge the poll tax as a voting prerequisite in these five states. Katzenbach did so, and the U.S. Supreme Court case Harper v. State Board of Elections (1966) consequently proscribed the poll tax in all elections, federal, state, and local.

Some of the VRA’s temporary provisions—none involving the prohibition of poll taxes or literacy tests, on which the ban is permanent—periodically expire. These temporary provisions have been renewed in 1970, 1975, 1982, and 2006. Figures 3.1 and 3.2 are maps of the counties covered by Section 5 of the VRA (i.e., covered jurisdictions) when the VRA was passed in ‘65, and as of the most recent extension in 2006, respectively. President Bush signed the latest renewal of the VRA amid speculation about his allegedly partisan motives (cf., Swain 1993; 2010).

The VRA also mobilized federal election examiners and registrars whose impact was large but short-lived (cf., Alt 1994; Black p. 135). Federal actors played a central role in black reintegration into southern political life prior to the VRA and after the act passed. After the VRA was passed, its potency was determined largely by the Supreme Court—which found that the act was constitutional in South Carolina v. Katzenbach (1966)—by the Attorney General, and a three judge panel drawn from the United States District Court for Washington, DC, one of which had to “pre-clear” structural voting changes in covered regions. Various U.S. presidents have signed periodic renewals of the act’s temporary provisions.

Under sections 6 and 7 of the VRA, the Attorney General was given the power to enlist U.S. marshals and other federal officers as examiners to ensure that African Americans and members of other minority groups could register to vote “without delay or harassment.” VRA examiners were authorized to register qualified voters directly.

Section 8 of the VRA made provisions for the Attorney General to send federal observers to oversee elections in any political subdivision where federal registrars had been employed. When the Attorney General saw fit to activate the examiners, these officials could “enter and attend any place for holding an election in such subdivisions [where federal registrars were employed] for the purpose of observing whether persons entitled to vote” are (1.) “being permitted to vote,” and (2.) “being properly tabulated.”

According to Earl Black (1987), “between 1965 and 1980, federal registrars [hence observers] entered no more than 60 of the 533 southern counties covered by the Voting Rights Act of 1965. Forceful and vigorous implementation of the act was primarily limited to portions of three Deep South states—Mississippi, Alabama, and Louisiana. South Carolina’s earlier attitude of blatant resistance had begun to soften prior to the act, and Georgia was largely undisturbed by the Justice Department [in 1965]. Neither Virginia nor the North Carolina counties covered by the law received any federal examiners. Virtually all instances (97 percent) of federal intervention to register blacks, as Mack Jones has pointed out, occurred within the first two years of the act. For all
practical purposes, the registration assistance portion of the VRA came to an end in 1967” (p. 135).

Though their tenure was brief, Alt (1994) notes the dramatic but fleeting effect of the VRA Section 8 election officials: “[t]he presence of federal examiners mattered a great deal for black registration, at least in the transitional period up to 1968. While only 28 percent (16 of 57) black majority counties without examiners achieved black-majority electorates, 60 percent (19 of 32) of black-majority counties with federal examiners did so during this time. In fact, in Alabama, Georgia, Louisiana, and Mississippi, every county in which a majority-black electorate was registered in 1967 or 1968 either had a federal examiner or was geographically adjacent to one or more counties that did” (p. 368).

Alt also stresses that the effect of federal election officials very quickly: “The role of federal examiners in securing durable black majority electorates is less clear. On this point there is too little data for firm conclusions. In the three states for which county-level data in 1967 and 1971 are available—Louisiana and the Carolinas—8 out of 9 majority black counties in 1967 no longer had black electoral majorities four years later…Econometric analyses of all southern counties for which we have sufficient data show that as early as 1972 the independent impact of federal registrars had entirely dissipated” (p. 369).

3.2: Literature Review Regarding the “Black Belt Hypothesis”

For future reference I present a list of citations addressing the so-called “black belt” hypothesis and related theories of the racial context on majority group behavior (cf., Key 1949; Blalock 1969). Research on this question has been hampered by identification issues stemming from residential self-selection (cf., Manski 1995) and, more generally, the lack of well-matched treatment and control groups—large, strictly exogenous “context” changes have not occurred frequently in the past.

Studies of racial context include: Blumer (1958); Matthews & Prothro (1963); Keech (1969); Fossett & Kielcolt (1989); Brewer & Miller (1988); Fitzpatrick & Hwang (1992); Ellison & Powers (1944); Giles & Buckner (1993, 1996); Sigelman & Welch (1993); Giles & Hertz (1994); Glaser (1994); Carsey (1995); Quillian (1995); Bobo & Hutchings (1996); Taylor (1998); Oliver & Mendelberg (2000); Welch, Sigelman, Bledsoe, & Combs (2001); Oliver & Wong (2003); Gay (2004); Bafumi & Herron

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5 Harold Stanley (1987) provides interesting anecdotal accounts of the registrars’ experience in the South.
6 I will consider whether or not Alt’s fears were justified by including an indicator for the presence of a VRA registrar. This particular analysis complements a growing literature assessing the residual impact of temporary structural changes in democratizing polities, e.g., Hyde (2007) and Bhavnani (2009).
7 See Horowitz & Terry (2009) for a relevant review of the southern lynching literature.
4.1: Policy Responsiveness in the U.S. Senate 1960-1996

To gain some perspective on the VRA’s impact in House elections, it’s telling to look at the Senate, which differed from the House in a number of potentially important ways. For example, in Senate races, manipulation of district boundaries was a non-factor and, in the modal case, the organizational capacity of the left was presumably far greater than it was in the typical House race. Fortuitously, because reliable state-level registration estimates for the 1960s exist from the CPS and other sources, it is possible to look directly at the preferred Downsian variable in Senate races—i.e., the ratio of black to white registrants in the state.\(^8\)

Table 4.6 contains regression results for the policy response in the U.S. Senate. I implement a few modest technical changes to the research design of Hood, Kidd, & Morris (1999) and corroborate their core finding, namely, “as black electoral strength grew relative to that of white voters, Democratic senators became more sensitive to issues relevant to this new constituency.” Column III reports the authors’ published analysis of ADA scores in the Senate, suggesting that net increases in the ratio of black to white registrants precipitated leftward shifts in Democratic Senators’ voting behavior. (Hood, Kidd, & Morris 1998). This difference in the post-VRA response in the House and Senate has not previously been noted.\(^9\) I also use a random effects model that allows the Downsian effect to vary by district—in these cases, the first coefficient becomes \(\lambda_i \equiv (\lambda_0 + \theta_i)\), where \(\theta_i\) is the vector of parameters capturing the district-specific effects (e.g., Wilson & Butler 2007).

An interesting point in light of the House findings, is that increases in the proportion of registrants black did not induce GOP candidates to adopt more leftist behavior in the Senate. I merely reinforce these findings by using a random effects model to exploit the panel structure of the data more efficiently (cf., Wilson & Butler 2007), and incorporate DW-Nominate scores (Poole & Rosenthal 2005).\(^10\)

4.2: National congressional career patterns 1960-1970

In this section I exploit regional and temporal variation in the data to get a broad overview of national career outcomes in the U.S. House during the 1960s. I note that the VRA (and preemptive local changes intended to avoid triggering Section 5 covered status) precipitated vastly larger structural turnout changes in southern races. In places

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\(^8\) CPS series P-20.
\(^9\) Hood, Kidd, & Morris (2010) erroneously claim the impact was the same in the House and Senate. Studies of the House do not discuss the Senate at all.
\(^10\) I haven’t finished the Senate DW-Nominate analysis.
like Wyoming or Vermont, for instance, the Downsian effect would have been essentially nil, because very few blacks lived there. The natural comparison I make now is thus between a control group (non-southerners) and a treatment group (southerners), before and after the treatment is administered. The sample consists of all incumbent members of the House who were eligible for reelection in the 1950-70 elections.\footnote{11}

I note two caveats. First, the five southern states that weren’t Section 5 jurisdictions weren’t covered because they lowered structural barriers in the years immediately preceding the VRA. Second, the VRA and its extensions did affect non-southern states after 1968: the maximum number of states that have been covered in whole or part by special provisions of the act is twenty-two, in the 1980s (Davidson & Grofman 1994). In 2009, there were 11 states covered (in whole or in part) per Section 5 of the VRA.

The data allow me to employ a relatively sophisticated multinomial logit regression model (relative to existing research designs in the literature).\footnote{12} The obvious drawback with the approach is that it relies on a crude measure of the ostensible Downsian mechanisms. However, the approach has the clear advantage that the data are available for the universe of elections in the ‘60s—more on which follows.

The multinomial logit models the probability that a random variable \(Y_{it}\) takes each possible outcome in a set of feasible outcomes. Define \(Y_{it}\) to represent the career option chosen by incumbent \(i\) in election cycle \(t\), conditional on a vector of covariates \(x_{it}\). Let \(Y_{it}=1\), if \(i\) seeks re-election in period \(t\); \(Y_{it}=2\), if \(i\) retires; and \(Y_{it}=3\), if \(i\) decides to seek another office. The classic derivation of the multinomial logit regression equation proceeds from a straightforward random utility model of a decision maker’s preferences over a set of available alternatives. For a rigorous derivation, see Maddala’s original work (p. 59-71). Analogous microfoundations imply the probability that incumbent \(i\) chooses career path \(j\) in election \(t\) is:

\[
\Pr[Y_{it} = j] = e^{\beta_j x_{it}} \left[ \sum_{k=1}^{3} e^{\beta_k x_{it}} \right]^{-1} \quad \text{for } j \in \{1,2,3\}.
\]

One well-known characteristic of the multinomial logit is that it does not satisfy the independence of irrelevant alternatives criterion, which is problematic when any subset of choices being modeled are sufficiently similar (cf., Debreu 1960). I am not concerned that this feature of the model affects my results, as the three outcomes are self-evidently dissimilar.

\footnote{11} The sample excludes members who died in office or were expelled. 1960-70 covers the 87th-91st U.S. congresses.

\footnote{12} On could also employ a sequence of binary logits that follow the logic of the candidates’ extensive form game through the primary and general (a novel research design).
Table 4.3 reports the maximum likelihood estimates of the multinomial logit regression parameters. For each career outcome \( j = 1,2,3 \), I estimate a vector of parameters \( \hat{\beta}_j \) such that:

\[
\hat{\beta}_j x_{it} = \hat{\beta}_{j,0} + \hat{\beta}_{j,1} \text{previous vote share}_{it} + \hat{\beta}_{j,2} \text{GOP}_{it} + \hat{\beta}_{j,3} \text{south}_{it} + \hat{\beta}_{j,4} \text{post-VRA}_{it} + \hat{\beta}_{j,5} \text{south} \times \text{post-VRA}_{it} + \hat{\beta}_{j,6} \text{redistrict}_{it} + \hat{\beta}_{j,7} \text{redistrict} \times \text{previous vote share}_{it} + \hat{\beta}_{j,8} \text{redistrict} \times \text{GOP}_{it} + \hat{\beta}_{j,9} \text{redistrict} \times \text{south}_{it} + \hat{\beta}_{j,10} \text{redistrict} \times \text{post-VRA}_{it} + \hat{\beta}_{j,11} \text{redistrict} \times \text{south} \times \text{post-VRA}_{it}.
\]

The coefficients indicate the estimated effect of the covariates in the vector \( x_{it} \) on the probability of retiring from politics (\( Y_{it}=2 \)), and seeking another political office (\( Y_{it}=3 \)), respectively, relative to the baseline, seeking re-election (\( Y_{it}=1 \)). Thus, the fact that the coefficient for the GOP indicator in the “retirement” column is positive for redistricted incumbents (\( \hat{\beta}_{1,2} = 1.06, p = .004 \)), for instance, suggests that being a member of the GOP increased one’s odds of retiring—relative to staying in office—for politicians in stable districts.

Regarding the “Downsian” test, if one compares southerners after the VRA (in 1966, ’68, and ‘70) to southerners before (1950-64), one finds that the post-VRA legislators in stable districts were more likely to retire (\( \hat{\beta}_{2,5} = .56; p = .32 \)) and significantly more likely to seek another office (\( \hat{\beta}_{3,5} = 1.99; p = .005 \)), controlling for (1.) party; (2.) incumbent vote share in the previous election; (3.) party-specific election year fixed effects; and (4.) redistricting. Note that there was no discernable affect in redrawn districts.\(^{13}\)

To put this finding in context, note that instances where an incumbent chose to seek another political office constitute 34% of all cases when an otherwise eligible candidate decided not to run in 1960-70, a non-trivial percentage.\(^{14}\) So the national patterns show statistically significant regional variation in the data that could have non-trivial impact on the ideological make up of the South’s delegation. While these patterns tend in a broadly Downisan direction, this is hardly evidence of an overwhelmingly strong Downsonian impact. Problematically, however, the regressions relied on the simplistic assumption that the Downsonian treatment dosage was largest in the South after 1965—this is a natural but obviously limited operationalization of the Downsonian mechanism. The next set of regressions provides a cleaner test of the theory by using a

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\(^{13}\) I actually divided the sample between stable and redrawn districts. In future drafts I will saturate model, by interact redistricting. The larger N will improve precision of the estimates but cannot affect the substantive conclusions.

\(^{14}\) The other 66% of instances an incumbent chose not to run were retirements.
more defensible Downsian variable—i.e., the change in the proportion of registrants in the district who were African-American.¹⁵

¹⁵ County-level voting rates by race are not available during this period. As Harold Stanley (1988, p. 151) point out there are unavoidable difficulties with both registration and voting data.
### Table 4.3: Policy Responsiveness in the U.S. Senate 1950-1996

<table>
<thead>
<tr>
<th></th>
<th>ADA(1) Random Effects</th>
<th>ADA(2) Random Effects</th>
<th>ADA(3) HKM (1999)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Intercept</strong></td>
<td>41.028***</td>
<td>23.556***</td>
<td>39.430***</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.00)</td>
</tr>
<tr>
<td><strong>Lagged Legislator Policy Score</strong></td>
<td>0.385***</td>
<td>0.703***</td>
<td>0.551***</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.00)</td>
</tr>
<tr>
<td><strong>Lagged Seat Policy Score</strong></td>
<td></td>
<td>0.551***</td>
<td>0.551***</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.00)</td>
</tr>
<tr>
<td><strong>GOP Indicator</strong></td>
<td>-30.020 (0.195)</td>
<td>-26.197 (0.15)</td>
<td>-55.137***</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.00)</td>
</tr>
<tr>
<td><strong>GOP Strength Index (GSI)</strong></td>
<td>-0.440**</td>
<td>0.500 (0.30)</td>
<td>-0.318**</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.30)</td>
<td>(0.05)</td>
</tr>
<tr>
<td><strong>% Registrants</strong></td>
<td>-0.734***</td>
<td>-0.163 (0.419)</td>
<td>-0.182</td>
</tr>
<tr>
<td><strong>African-American (% pts)</strong></td>
<td>(0.00)</td>
<td>(0.41)</td>
<td>(0.39)</td>
</tr>
<tr>
<td></td>
<td>0.671 (0.52)</td>
<td>(0.66)</td>
<td>(0.61)</td>
</tr>
<tr>
<td><strong>GSI * %Registrants Black</strong></td>
<td>0.041***</td>
<td>0.021***</td>
<td>0.030***</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.04)</td>
<td>(0.00)</td>
</tr>
<tr>
<td><strong>General Election</strong></td>
<td>0.007 (0.88)</td>
<td>-0.024 (0.84)</td>
<td>-0.056</td>
</tr>
<tr>
<td></td>
<td>(0.59)</td>
<td>(0.84)</td>
<td>(0.21)</td>
</tr>
<tr>
<td><strong>Leadership Indicator</strong></td>
<td>0.957 (0.54)</td>
<td>0.205 (0.87)</td>
<td>0.679</td>
</tr>
<tr>
<td></td>
<td>-1.861 (0.58)</td>
<td>(0.87)</td>
<td>(0.68)</td>
</tr>
<tr>
<td><strong>Seniority (years served)</strong></td>
<td>0.022 (0.84)</td>
<td>-0.100 (0.42)</td>
<td>-0.268***</td>
</tr>
<tr>
<td></td>
<td>0.224 (0.32)</td>
<td>(0.42)</td>
<td>(0.00)</td>
</tr>
<tr>
<td><strong>% District Black</strong></td>
<td>-0.730***</td>
<td>-0.505***</td>
<td>-0.786***</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.16)</td>
<td>(0.00)</td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>740</td>
<td>740</td>
<td>792</td>
</tr>
<tr>
<td><strong>Adjusted R²</strong></td>
<td>0.70</td>
<td>0.73</td>
<td>0.68</td>
</tr>
<tr>
<td><strong>χ²</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Notes:**
- The table reports maximum likelihood estimates for OLS regression models of the ADA score of U.S. Senators’ from the 11 states of the former Confederacy. The sample covers 1960–1996. A positive coefficient suggests that the corresponding covariate produced more liberal policy—i.e., larger scores are more liberal. Negative coefficients suggest the corresponding covariates produce rightward policy shifts (as captured by the ADA score).
- p-values are reported in parentheses. ***, **, * represent statistical significance at the .1, .05, and .01 confidence levels, respectively. † denotes significance at the .15 level.
- Model (1.) is the preferred specification (cf., Wilson & Butler 2007); it employs a random effects model as implemented by Stata (cf., Rabe-Hesketh & Skrondal 2005). Blue font indicates that the corresponding parameter estimate in the random effects model leads to a substantively different statistical inference from Hood, Kidd & Morris’s (1999) published results—i.e., those found in model (3.).
- Model (2.) replicates the regression design in the current state of the art study—Hood, Kidd & Morris (1999), Table 1— but uses a corrected version of the lagged dependent variable. Red font denotes inferential “mistakes” due to the lagged ADA score miscoding—compare these cells with the corresponding cells in model (3.).
- Model (3.) reproduces Table 1 in HKM (1999) exactly. Red font denotes inferential “mistakes” due to the lagged ADA score miscoding—compare these cells with the corresponding cells in model (2.).
- The GOP strength index is a composite score, based on the vote share of Republican candidates in gubernatorial and congressional elections in the focal state, created by David (1972). The version is the data is Hood et al’s (1999)

Table 4.4: Multinomial logit analysis of U.S. House Career Paths, 1950-1970

<table>
<thead>
<tr>
<th>Covariate</th>
<th>Retires (^{(1.)}) Stable(^{(a.)})</th>
<th>Retries (^{(1.)}) Redistricted(^{(b.)})</th>
<th>Seeks another office (^{(2.)}) Stable(^{(a.)})</th>
<th>Seeks another office (^{(2.)}) Redistricted(^{(b.)})</th>
</tr>
</thead>
<tbody>
<tr>
<td>Vote Share in Previous Election</td>
<td>0.003 (0.45)</td>
<td>-0.10 (^{***}) (0.001)</td>
<td>0.01 (0.22)</td>
<td>-0.06 (^{†}) (0.12)</td>
</tr>
<tr>
<td>Member of GOP</td>
<td>1.06 (^{***}) (0.00)</td>
<td>0.99 (0.30)</td>
<td>0.56 (^{*}) (0.04)</td>
<td>-21.2 (0.99)</td>
</tr>
<tr>
<td>Southerner</td>
<td>0.48 (^{†}) (0.11)</td>
<td>1.53 (0.21)</td>
<td>-1.86 (^{***}) (0.001)</td>
<td>-34.14 (0.99)</td>
</tr>
<tr>
<td>Post-VRA</td>
<td>-0.86 (^{**}) (0.05)</td>
<td>0.0009 (0.999)</td>
<td>0.46 (^{†}) (0.133)</td>
<td>-0.37 (0.75)</td>
</tr>
<tr>
<td>Southerner*Post-VRA</td>
<td>0.56 (0.32)</td>
<td>-0.84 (0.54)</td>
<td>1.99 (^{***}) (0.005)</td>
<td>1.39 (0.99)</td>
</tr>
<tr>
<td>Constant</td>
<td>-4.05 (^{***}) (0.00)</td>
<td>1.69 (0.88)</td>
<td>-4.73 (^{***}) (0.00)</td>
<td>0.81 (0.74)</td>
</tr>
</tbody>
</table>

Cell N

<table>
<thead>
<tr>
<th>Stable</th>
<th>Redistricted</th>
</tr>
</thead>
<tbody>
<tr>
<td>N</td>
<td>3,782</td>
</tr>
<tr>
<td>Pseudo R(^2)</td>
<td>0.04</td>
</tr>
<tr>
<td>LR $\chi^2$</td>
<td>93.76 (^{***})</td>
</tr>
<tr>
<td>$\chi^2$ p-value</td>
<td>0.000</td>
</tr>
</tbody>
</table>

Notes:
The columns report maximum likelihood estimates of the multinomial logit coefficients modelling the probability an eligible House incumbent (1.) retired from politics or (2.) choose to run for another political office, respectively, rather than opting to run for reelection, the baseline outcome. The covariates were interacted to produce estimates for incumbents in (a.) stable, and (b.) redrawn districts separately.

Party-specific election year fixed effects were included in the regression but are not reported.

Of the X incumbents eligible for re-election, Y sought re-election, Z retired, N sought another office.

The analysis excludes incumbents who were expelled or died before announcing their career plans. The South is defined as the 11 states of the former Confederacy. 1950-70 covers the 81st through 91st U.S. Congresses.

\(p\)-values appear in parantheses.

\(^{***}\), \(^{*}\), \(^{**}\) represent statistical significance at the .01, .05, and .1 confidence levels, respectively.

\(^{†}\) denotes significance at the .15 level.
REFERENCES


Public Broadcasting System (2001). As quoted in the New Yorker, need citation.


