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The Role of the U.S. Courts of Appeals in Legal Development: An Empirical Analysis

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by

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ABSTRACT OF THE DISSERTATION

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What are the causes and consequences of legal development? In recent years courts scholars have begun to address these broad and challenging questions, yet there is still much work to be done. The intermediate level of the federal court system (a.k.a., circuit courts) provides an institutional context replete with opportunities to extend our theoretical and empirical understanding of legal development. My dissertation takes advantage of these opportunities in three ways. First, I explore legal constraint by comparing citation to and treatment of circuit court precedents. A precedent is binding in its own circuit, but merely persuasive in other circuits. Consequently, if law constrains judges the effect of ideology on how a precedent is treated should be significantly less when it is considered in its own circuit than when considered by a sister circuit. Second, I investigate the nuances of a circuit's citation to its own binding precedent to determine how it is influenced by strategic anticipation of whether a case will be reviewed and overturned by the entire circuit. Third, I examine the impact of federal courts on state policy diffusion, positing that both adoption and content of a policy will be influenced by federal court rulings on the constitutionality of a previously adopted statute.

Chapter 1

Introduction

For decades, political scientists who study courts have focused on the effect personal ideology has on judicial decisionmaking (*e.g.,* Segal and Spaeth, 2002; Brace, Langer and Hall, 2000; Segal and Cover, 1989; Pritchett, 1968; Schubert, 1965). However, more recently, increasing attention has been paid to incorporating the effect of law into models of judicial behavior (*e.g.,* Bartels, 2009; Hansford and Spriggs, 2006; Lindquist and Klein, 2006; Richards and Kritzer, 2002). These efforts to "take law seriously"[1] involve exploring both what drives legal development and the changes effected by it. This project takes up these important questions in the context of the U.S. Courts of Appeals.

Although the U.S. Supreme Court receives the lion's share of attention from political scientists, the vast majority of precedential federal case law is produced by the intermediate courts of appeals (Cross, 2007; Hettinger, Lindquist and Martinek, 2006; Klein, 2002; Songer, Sheehan and Haire, 2000). If taking law seriously is important, then taking the federal circuit courts seriously is crucial as well. In addition

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[1]Friedman (2006) criticizes political scientists for not taking law seriously.

to being an essential object of study in their own right, the institutional structure of circuit courts is particularly conducive to exploring legal development. Both the development of different legal rules in different circuits and the geographic grouping of contiguous states into federal circuits offer opportunities to gain leverage on interesting questions.

The U.S. Courts of Appeals, like many entities, produce a prodigious amount of textual data every year. The bulk of empirical scholarship on courts has focused on the outcomes of cases rather than the content of the opinions judges write. Although this standard approach has resulted in many important discoveries, several scholars have urged moving beyond this focus solely on case outcomes (Hume, 2006, 2009a,b; Friedman, 2006; Edwards, 1998). Technology is increasingly making this move possible by enabling automated extraction of information from the content of opinions which is complemented by human coding rather than solely relying on the latter (*e.g., see* Corley, Collins and Calvin, 2011; Corley, 2008). This dissertation takes advantage of such technology to glean important information about legal development from the text of relevant documents.

In the following three chapters I explore the causes and consequences of legal development in three specific areas: the incorporation of non-binding out-of-circuit precedent into the law of a sister circuit, the anticipation of en banc review within a circuit, and the adoption and drafting of state legislation. The first of these topics offers the opportunity to provide empirical evidence of legal constraint on judges' decisions[2] by contrasting the effect of non-binding precedent (from another circuit) with the effect of binding precedent. The second topic sheds light on the role of

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[2]To date, such evidence has proved to be quite elusive (Lax and Rader, 2010; Friedman and Martin, 2011), although some clever approaches have been developed (Bartels, 2009; Black and Owens, 2009; Bailey and Maltzman, 2008; Hansford and Spriggs, 2006; Lindquist and Klein, 2006; Richards and Kritzer, 2002).

strategic behavior of a panel vis-a-vis the ideological makeup of the whole circuit. While the broader topic of strategic judicial behavior has been widely explored (*e.g.,* Scott, 2006; Klein and Hume, 2003; Songer, Ginn and Sarver, 2003; Maltzman, Spriggs and Wahlbeck, 2000; Epstein and Knight, 1998), a circuit panel's anticipation of en banc review has yet to be examined in detail. Finally, the third topic builds on the literature exploring policy diffusion. Scholars have long noted that in deciding whether to adopt a new policy, states tend to be influenced by their neighboring states (Allen, Pettus and Haider-Markel, 2004; Andrews, 2000; Mooney and Lee, 1995; Berry and Berry, 1990; Gray, 1973; Walker, 1969). This study suggests a mechanism that helps explain such a phenomenon by positing that states consider how the federal courts–especially in their own circuit–have treated policies adopted by other states.

Chapter 2

Legal Constraint

Backgrounds will vary, attitudes will differ, environments will change, but the law remains the alpha and omega of judicial decisionmaking.

-Sisk, Heise and Morriss, 1998: 1500

To most people, the claim that law is central to judicial decisionmaking hardly seems controversial. The concept that the primary responsibility of judges is to neutrally apply existing law to specific factual situations is foundational in our legal system. Judges are expected to create new rules only where necessary. Once a new rule is established, the doctrine of stare decisis constrains future judges from the same court and lower courts to follow that precedent (Aldisert, 1989; Boyd and Spriggs, 2009). Yet this picture of how law develops is somewhat at odds with the picture painted by existing empirical scholarship. Within the body of empirical work on judicial behavior, one of the most consistently observed factors has not been law, but a judge's ideology. Nevertheless, the considerable evidence that judges act ideologi-

cally does not necessarily mean that law does not matter. This chapter will provide evidence of both factors working in tandem.

Finding unequivocal evidence of legal constraint is not a simple task. Differentiating between conduct based on ideology and legal doctrine in a field which entails exercise of considerable discretion is difficult (Cross et al., 2010; Kim, 2006). The challenges are further compounded when searching for legal constraint in the U.S. Supreme Court because that is the institutional context where it is least likely to exist (Friedman and Martin, 2011). Supreme Court justices face few disincentives for acting ideologically, and the nature of their caseload (composed of both novel and important legal issues) lends itself to ideological decisionmaking (Cross et al., 2010).

Although the U.S. Supreme Court receives the lion's share of attention from political scientists who study courts, the vast majority of federal case law is produced by the intermediate courts of appeals (Boyd and Spriggs, 2009; Cross, 2007; Hettinger, Lindquist and Martinek, 2006; Klein, 2002; Songer, Sheehan and Haire, 2000). Cross (2003) has noted that circuit court decisions "are probably the decisions of greatest importance for the development of the law in the United States" (Cross, 2003: 1459). Since these courts both exercise broad power and are held responsible by a higher level of judicial hierarchy, they present an excellent forum for examining the extent to which legal constraint influences the development of law. There is reason to expect that judges might be constrained by stare decisis, and learning about such constraint is important for understanding a set of courts which has a substantial impact on the legal landscape.

While the circuit courts are ripe for examination of the impact of law on judicial decisionmaking, the challenges inherent in such a task remain. One difficulty lies in objectively identifying what the law is or what result the law requires in order to compare judges' behavior to that standard. Another serious difficulty is addressing

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the endogeneity concerns raised since the law in question is usually judge-made law. Distinguishing between the effect of law and the effect of factors influencing judges similarly at two different times is not a simple matter. While scholars have crafted insightful and creative techniques designed to tease out empirical evidence of legal doctrine influencing judges, much work remains to be done in this area.

My contribution in this paper is identifying an opportunity provided by the institutional structure of federal circuit courts which ameliorates endogeneity concerns in a novel way. Circuit courts dispose of the vast majority of their caseload using panels composed of three judges (Collins and Martinek, 2011; Kim, 2009).[1] In these courts a published[2] opinion (hereinafter "precedent") is binding for panels within its own circuit, but only persuasive[3] in other circuits (Cross, 2003; Klein, 2002; Landes, Lessig and Solimine, 1998; Merritt, 1990). If judges are constrained by the doctrine of stare decisis, ideology should have a reduced impact on whether they cite a binding precedent compared to a persuasive precedent. Furthermore, the decision to negatively or positively treat a cited precedent should also be influenced by whether the precedent is binding. The fact that every precedent is binding in some circumstances and not in others mitigates concerns traditionally associated with empirical tests of legal constraint.

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[1]After a panel rules, the losing party may request that the entire circuit rehear the case in an en banc proceeding, but such petitions are granted only rarely (Choi, Gulati and Posner, 2012; Kim, 2009; Giles et al., 2007; Law, 2004; George and Solimine, 2001; Van Winkle, 1997; Posner, 2000).

[2]Because federal litigants have a right to appeal at least once (Boyd and Spriggs, 2009), federal circuit courts resolve a considerable number of routine cases each year in addition to their important policymaking role. For the sake of efficiency, many of these cases are dispatched with "unpublished" opinions. Although the name is a bit of a misnomer since such opinions are available online now, the distinction is important because most circuits do not recognize unpublished precedents as binding.

[3]"[J]udges are under no obligation to follow persuasive authority. . . ." (Aldisert, 1989: 632)

Many scholars have noted that citation analysis can be an invaluable tool to explore a variety of interesting questions (Caldeira, 1985; Hume, 2009*b*; Landes, Lessig and Solimine, 1998; Posner, 1999). This paper continues that tradition by utilizing citation patterns in the U.S. Courts of Appeals to scrutinize the effect of stare decisis on judicial behavior. I have constructed an expansive new dataset containing all search and seizure precedents published by circuit courts from 1953 to 2010. Using this comprehensive list of possible cases a judge might cite, I analyze the citation and treatment decisions of judges from 2000 to 2010 and provide evidence that the doctrine of stare decisis does constrain judicial behavior. Judges are significantly less ideological when deciding whether to cite binding precedents than when deciding to cite persuasive precedents, and they are consistently less likely to negatively treat binding precedents.

2.1 Ubiquitous yet Elusive: The Paradox of Law

How does law influence judges? This basic question is at the heart of studying judicial behavior and decisionmaking. The classical view that judges apply the law to facts in an objective and impartial manner has been called into question since the advent of the Legal Realist movement in the 1920's. The Legal Realists' attribution of judicial decisions to judge-specific factors other than law (such as a judge's culinary choices) launched the examination of extralegal factors in decisionmaking (Bailey and Maltzman, 2011; Duxbury, 1995). For decades, the field of political science has focused on the effect personal ideology (and, later, strategic pursuit of ideological preferences) has on judicial decisionmaking (*e.g.,* Brace, Langer and Hall, 2000; Goldman, 1966, 1975; Pritchett, 1941, 1968; Revesz, 1997; Rowland and Carp, 1980; Schubert, 1965; Segal and Cover, 1989; Segal and Spaeth, 2002). More recently,

judicial scholars have turned their attention to the complexity of judges' behavior, recognizing that it is most likely influenced by different types of factors, including, at least under some circumstances, the existing state of the law (Cross, 2003; George and Epstein, 1992; Gibson, 1983; Kim, 2006). Consequently, scholars have paid increasing attention to incorporating the effect of law into models of judicial behavior (Bartels, 2009; Bailey and Maltzman, 2008, 2011; Black and Owens, 2009; Hansford and Spriggs, 2006; Kritzer and Richards, 2003, 2005; Lindquist and Klein, 2006; Luse et al., 2009; Richards and Kritzer, 2002; Richards, Smith and Kritzer, 2006; Songer and Sheehan, 1992).

Scholars have developed a variety of methods to examine the question of whether law influences judicial decisionmaking. Since legal rules involve mapping case facts to appropriate outcomes, one of the earliest approaches was fact pattern analysis. There is evidence that different facts lead to different outcomes in search and seizure (Segal, 1984) and death penalty cases (George and Epstein, 1992; Traut and Emmert, 1998). Segal and Spaeth examine the voting patterns of justices who dissented in landmark cases and point to their disinclination to change votes in later similar cases as evidence that law does not constrain (Segal and Spaeth, 1996, 2002). The work of Richards and Kritzer operates by identifying a key regime change in the Supreme Court and evaluating judicial behavior before and after the change (Richards and Kritzer, 2002; Richards, Smith and Kritzer, 2006; Kritzer and Richards, 2005, 2003). They find that the changes in legal rules they study do affect case outcomes (Richards and Kritzer, 2002; Richards, Smith and Kritzer, 2006; Kritzer and Richards, 2005, 2003). Luse, et. al. (2009) find that circuit judges similarly respond to new jurisprudential regimes established by the Supreme Court. Bailey and Maltzman (2011) approach the question of legal constraint by comparing actions of Supreme Court justices to those of political actors from other branches who are unhampered by the doctrine of

stare decisis. They also find some evidence of legal constraint (Bailey and Maltzman, 2011). Other scholars have pointed to different outcomes in cases which apply different legal standards (Bartels, 2009; Cross, 2003), higher win rates in cases with better quality legal arguments (Lindquist and Klein, 2006), and reliance on jurisprudential considerations in agenda-setting decisions (Black and Owens, 2009) as evidence that judges are influenced by law.

Many of these studies, while ingenious and innovative, are also subject to important critiques or limitations. For example, the work of Richards and Kritzer has been called into question on methodological grounds and only some of their findings hold up under scrutiny (Pang et al., 2012; Lax and Rader, 2010). The broader concern with studies of legal constraint, however, is the endogeneity of measures of law since law is often created by judges. Consequently, it is particularly challenging to distinguish the effect of law itself from the effect of some unobserved factor influencing both the judges who originally formulated the law and later judges. As a result of this conundrum, solid empirical evidence of law constraining judicial behavior has proved elusive.

The extant work on legal constraint is somewhat limited by the fact that most of it has been done at the Supreme Court level and on relatively small groups of cases. While this does not diminish the importance of the insights these studies offer, it does raise obstacles to extending both the conclusions and research techniques to other forums. Nevertheless, the results are suggestive since the Supreme Court is the forum where law would be least likely to constrain judges. Judges with a life appointment to a court of last resort face significantly less pressure than their counterparts who face reversal and whose rulings may have career implications (Boyd and Spriggs, 2009; Caminker, 1994; Friedman, 2006; Hettinger, Lindquist and Martinek, 2006). The institutional characteristics of the Supreme Court may very well explain the difficulty

scholars have had identifying clear empirical evidence of the impact of law on judicial behavior (Friedman and Martin, 2011). As Friedman (2006) points out, "if constraint is the issue, all the important action might be in the lower courts" (Friedman, 2006: 265).

Within the federal court system, the effect of law is probably most constraining for trial court judges. Litigants have a right to appeal which makes the possibility of reversal ever-present (Haire, Lindquist and Songer, 2003; Randazzo, 2008). In addition, the possibility of a judge being promoted to a higher court is quite realistic which provides additional incentive to conform with norms regarding neutral application of law (Haire, Lindquist and Songer, 2003; Morriss, Heise and Sisk, 2005; Sisk, Heise and Morriss, 1998). However, while these judges have considerable influence over numerous litigants and their individual disputes, broader policymaking power is typically reserved to the appellate courts (Kim, 2006). Federal trial judges' opinions do not constitute binding precedent,[4] and a considerable proportion of their efforts are devoted to matters which are case-specific (Rowland and Carp, 1996).

In between the two extremes of the Supreme Court and trial courts lie the intermediate courts of appeals. The federal circuit courts dispose of many routine matters, but they are also responsible for settling a wide range of legal disputes which have broad policy implications, yet may never be addressed by the Supreme Court (Cross, 2003, 2000; George, 1997; Hettinger, Lindquist and Martinek, 2006). These significant rulings are set forth in published opinions which are legally binding across all states in the circuit. This broad policy power is balanced by the possibility of reversal by the circuit as a whole (en banc) or by the Supreme Court (Hellman, 2007;

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[4]In the federal court system, the opinions of district court judges are not binding even within the same district. While the reasoning may be persuasive, legal doctrine does not require subsequent judges to apply such precedent *See* 18-134 *Moore's Federal Practice - Civil* §134.02(d).

Revesz, 1997). The threat of reversal–although small–may still result in the doctrine of stare decisis circumscribing judges' behavior. Moreover, judges may act within the constraints of legal doctrine for other reasons such as preserving legitimacy or normative beliefs regarding appropriate judicial conduct (Cross et al., 2010; Hansford and Spriggs, 2006; Kim, 2006; McNollgast, 1995). Studies have shown a high degree of compliance with Supreme Court doctrine by federal circuit courts (Benesh and Reddick, 2002; Gruhl, 1980; Songer and Haire, 1992; Johnson, 1987; Klein and Hume, 2003; Songer, Segal and Cameron, 1994). In sum, the circuit courts offer a context where law may very well operate as a constraint on the behavior of judges who are significant policymakers.

However, the extent, and even existence, of the effect of law in the circuit courts is far from certain. The possibility of reversal either en banc or by the Supreme Court is quite remote in any given case (Cross and Tiller, 1998). Circuit judges have lifetime tenure and a very low probability of promotion. Klein and Hume (2003) found that a greater possibility of reversal by the Supreme Court was actually related to *lower* levels of circuit court compliance. Just as with Supreme Court justices, there is considerable empirical evidence that circuit judges' ideology influences their decision-making (Goldman, 1975, 1966; Haire, Lindquist and Songer, 2003; Haire, Hartley and Lindquist, 1999; Songer and Haire, 1992; Hettinger, Lindquist and Martinek, 2004; Revesz, 1997; Schubert, 1965; Segal and Spaeth, 2002; Songer, Sheehan and Haire, 1999; Songer, Segal and Cameron, 1994; Songer and Davis, 1990; Sunstein, 2006; Sunstein, Schkade and Ellman, 2004). The challenge taken up here is to examine whether, how, and under what circumstances the legal doctrine of stare decisis moderates the well-documented ideological effect.

2.2 Disentangling Law and Ideology

Searching for convincing evidence that judges' behavior is shaped by legal principles is complicated. There is a strong professional and societal expectation that judges rely on the law to make their decisions, so judges have a powerful motivation to write their opinions and describe their behavior as if they are doing exactly that (Cross, 2003; Gillman, 2001). Moreover, determining what the law "requires" (as an objective matter) is virtually impossible. Consequently, judges' actions cannot simply be compared to the legally "correct" result in a particular case. The problem of observational equivalence between different motives creates further complications. A liberal judge might reach a liberal result for either ideological or legal reasons (or some combination). As a result, judicial scholars are faced with the challenge of identifying creative ways to examine whether, and to what extent, law constrains judges.

One way to overcome these challenges is to identify a situation where quasi-counterfactual data are available. In the courts of appeals, institutional rules and the legal doctrine of stare decisis provide the opportunity to examine counterfactual-type data with respect to citation behavior. Published circuit court opinions are binding in their own circuit,[5] but only persuasive in other circuits (Cross, 2003; Klein, 2002; Landes, Lessig and Solimine, 1998; Lee III, 2003; Lindquist and Cross, 2005; Merritt, 1990; O'Hara, 1993). As a result, a particular precedent is binding in some circumstances, but not in others. A judge considering a precedent must make two decisions: first, should the precedent be cited at all?, and second, if it is cited should it be treated negatively, neutrally, or positively?[6] To the extent that legal doctrine influ-

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[5]As a general rule, three-judge panels are not permitted to overturn previous circuit decisions (Hellman, 2007).

[6]Following Hansford and Spriggs (2006) I refer to the case where a judge is making these decisions as a treatment case to distinguish it from a precedent.

ences judges' behavior, the answers to both of those questions should be conditional on whether the precedent is binding.

The novel contribution of studying citation to circuit precedents lies in the fact that each precedent is binding for some judges and persuasive for others. Comparing citation to two separate groups of precedent, one which is always binding and another which is always persuasive, would raise endogeneity concerns about unmeasured differences between the two types of cases. Such concerns are substantially ameliorated when we can observe how judges treat every precedent under two different conditions. For the set of judges in the same circuit we have data to answer the question, How will a judge cite/treat this precedent when required by law to follow it? For the set of judges in other circuits we have data to answer the question, How will a judge cite/treat this precedent when its authority is only persuasive? The discrepancy between these two responses provides insight into the extent to which law constrains circuit judges' citation behavior (assuming other relevant factors are appropriately controlled).

Citation analysis has the potential to shed important light on how law develops (Boyd and Spriggs, 2009; Choi, Gulati and Posner, 2012; Cross et al., 2010; Hansford and Spriggs, 2006; Landes, Lessig and Solimine, 1998; Posner, 1999; Spriggs and Hansford, 2002; Walsh, 1997; Wedeking, 2012). Looking at citations is a step towards focusing on both the content of judicial opinions and the dynamic nature of law. Although a focus on case outcomes has driven courts scholars for several decades and has resulted in many important discoveries, there is additional information available to us as well. Many scholars have urged moving beyond this focus solely on case outcomes (Edwards, 1998; Friedman, 2006; Hume, 2006, 2009a,b; Lax, 2012). Technology is increasingly making this move possible by enabling automated extraction of information from the content of opinions which is complemented by human coding

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rather than solely relying on the latter (*e.g., see* Corley, Collins and Calvin, 2011; Corley, 2008). One essential piece of information from the text of a judicial opinion is which legal precedents are cited and how they are treated. This information is especially important when the question of interest is how judges are influenced by law. As a result, citation analysis in the circuit courts is a fruitful new area to study, not only because institutional rules provide a convenient counterfactual, but because this mode of analysis also allows us to get at the heart of judicial lawmaking.

2.3   Exploring Citation Behavior

In the course of crafting an opinion, a judge must make two decisions about each precedent which might potentially be included. First, will the precedent be cited? Second, if the precedent is cited, will it be treated negatively, neutrally,[7] or positively (Hazelton, Hinkle and Spriggs, 2011)? Previous research has established that several factors influence such decisions including the ideological similarity between the judge and a precedent (Benjamin and Desmarais, 2012; Hansford and Spriggs, 2006; Hazelton, Hinkle and Spriggs, 2011; Johnson, 1987; Spriggs and Hansford, 2002; Westerland et al., 2010). This paper builds on existing citation research by introducing the additional variable of whether a precedent is binding or persuasive and examining how the effect of ideology is conditioned by the doctrinal status of a precedent.

The doctrine of stare decisis requires citation and application of relevant binding precedents while no such requirement exists for persuasive precedents. Consequently, binding precedents should be more likely to be cited than persuasive precedents, and

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[7]As a practical matter a neutral citation to a precedent can be understood as a soft positive treatment. While the signal may not be quite as emphatic as an explicitly positive treatment, it is in the same direction.

cited binding precedents should be more likely to be treated positively. However, such patterns do not necessarily indicate compliance with legal doctrine. There are also other reasons why binding precedents might be cited or positively treated more. Policy-minded judges may prefer to discuss binding precedents since they provide an opportunity to more directly shape the law of the circuit. Furthermore, there is often a lower cost associated with incorporating binding precedents in an opinion. Cases from other circuits are less likely to be previously known to a judge or brought to their attention by counsel. With computerized legal research, finding cases from other circuits is certainly possible, but it takes time and effort to locate such cases.[8]

Consider these factors in terms of the utility a judge gains from citing a case.

$$u(Citation) = Legal\ Constraint + Policy - Cost$$

Each of the factors on the right side potentially has a different impact on the utility of citation depending on whether a precedent is binding or persuasive. As discussed above, judges gain more utility from shaping the law in their own circuit by discussing their own cases.

$$Policy_B > Policy_P$$

The subscript denotes whether a precedent is binding or persuasive. The cost of becoming aware of a precedent is higher when it is from another circuit.

$$Cost_B < Cost_P$$

If judges are constrained by stare decisis that effect would increase the utility of citing a binding precedent, but would have no similar effect for persuasive precedents. So a higher probability of citing binding precedents is a necessary condition for finding

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[8]Paradoxically, the considerable growth of the number of cases available electronically can ultimately increase the amount of effort needed to sort through the available opinions.

evidence of legal constraint. However, both *Policy* and *Cost* also increase the utility of citing a binding precedent relative to citing a persuasive precedent. This means that it is possible to observe the expected pattern of more frequent citation to binding precedents even if legal constraint has zero effect. As a result, a higher rate of citation to binding precedents is a necessary, but not sufficient, condition for legal constraint.

A very similar analysis applies to the utility of positively treating a cited precedent with the minor exception that the *Cost* factor is no longer different. Once a precedent is cited, the cost of treating that precedent should be similar regardless of whether it is from that same circuit or a different circuit. The remainder of the analysis and the conclusion remain the same; a higher rate of positive treatment of binding precedents is a necessary, but not sufficient, condition for establishing the existence of legal constraint.

The role of legal constraint in the decision to negatively treat a cited precedent is markedly different even though the utility function is similar.

$$u(NegativeTreatment) = Legal\ Constraint + Policy - Cost$$

As discussed regarding positive treatment, the *Cost* factor should not differ for binding and persuasive precedents. However, I still expect judges to gain more utility from discussing precedents from their own circuit, even when the discussion constitutes negative treatment because it shapes the development of the law. The key difference stems from the different expectations regarding legal constraint. If law constrains judges, they will gain *less* utility from negatively treating a binding precedent.[9] The observable effect of legal constraint would be to decrease the probability

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[9]I do not claim that legal doctrine precludes all negative treatment. A judge can cite a similar case and distinguish it from the case at bar without violating stare decisis. Therefore, a certain baseline amount of negative treatment is expected even where judges are constrained by law (Lee III, 2003). However, when no such constraint exists, judges are free to negatively treat a precedent on

of negative treatment. Maximizing policy influence would result in the opposite pattern. Consequently, a lower rate of negative treatment of binding precedents is both a necessary and sufficient condition for evidence of legal constraint.

Scholars have demonstrated that ideological distance between a court and a precedent influences citation and treatment of that precedent (Hansford and Spriggs, 2006; Johnson, 1987; Spriggs and Hansford, 2002). This relationship is attributed to the long-recognized impact ideological preferences have on judicial decisionmaking. While the impact of ideology on citation and treatment may be different in size, the expectations are similar in terms of direction. An authoring judge is more likely to cite and positively treat a precedent that is ideologically proximate and more likely not to cite or negatively treat a precedent that is ideologically distant. Consequently, I hypothesize that an increase in ideological distance should decrease the probability of both citation and positive treatment while increasing the probability of negative treatment.

In addition to the effects of ideology and the doctrinal status of a precedent being interesting in their own right, the interaction between the two provides key insight. The idea that law constrains judges simply means that they act differently–i.e., less ideologically–than they would in the absence of legal doctrine mandating adherence to binding precedents. Legal constraint does not mean that the effect of ideology must be wiped out completely or that there will be perfect compliance (Lee III, 2003). Judging is a process which necessarily includes discretion (Cross et al., 2010; Kim, 2006). It is impractical to expect such discretion will be exercised without being influenced (however subconsciously) by ideology. Legal constraint need not be absolute in order to be important and interesting. Simply put, "[t]aking legal rules seriously need

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ideological grounds as well as legal grounds which would result in a higher overall incidence of negative treatment.

not require jettisoning notions of ideological preferences ..." (Lax, 2007: 600). I hypothesize that, to the extent law constrains judges, ideology will have a reduced impact on citation and treatment decisions when a precedent is binding. Figure 2.1 depicts this and the forgoing hypotheses graphically.

While the central focus of this paper is examining the extent to which the doctrinal status of a precedent constrains citation and treatment decisions, it is, of course, important to account for other factors which might also influence these decisions and may be correlated with the primary variables of interest. Some of these factors influence both citation and treatment, while others primarily bear on the decision regarding whether to cite a case. For both the citation and treatment decisions, I control for case similarity, whether the precedent was written by the authoring judge in the treatment case, whether a precedent had a dissenting opinion, whether a precedent was signed, whether a precedent was decided en banc, vitality of a precedent, the number of previous citations to a precedent (by a circuit court), the number of previous treatments of a precedent (by a circuit court), age of a precedent, length of a precedent (logged number of words), length of the treatment case (logged number of words), and the quality of the legal analysis in a precedent. The control variables which only effect citation, and not treatment, are the number of available binding precedents (logged) and the workload of the deciding court.

2.4   Data and Research Design

Since my focus is to model the decisions of whether and how precedents are cited, it is necessary to identify not only a set of treatment cases to study, but also the relevant choice set of potentially applicable precedents which a panel may choose to cite in a treatment case. Nibblett (2010) conducts such an analysis by isolating
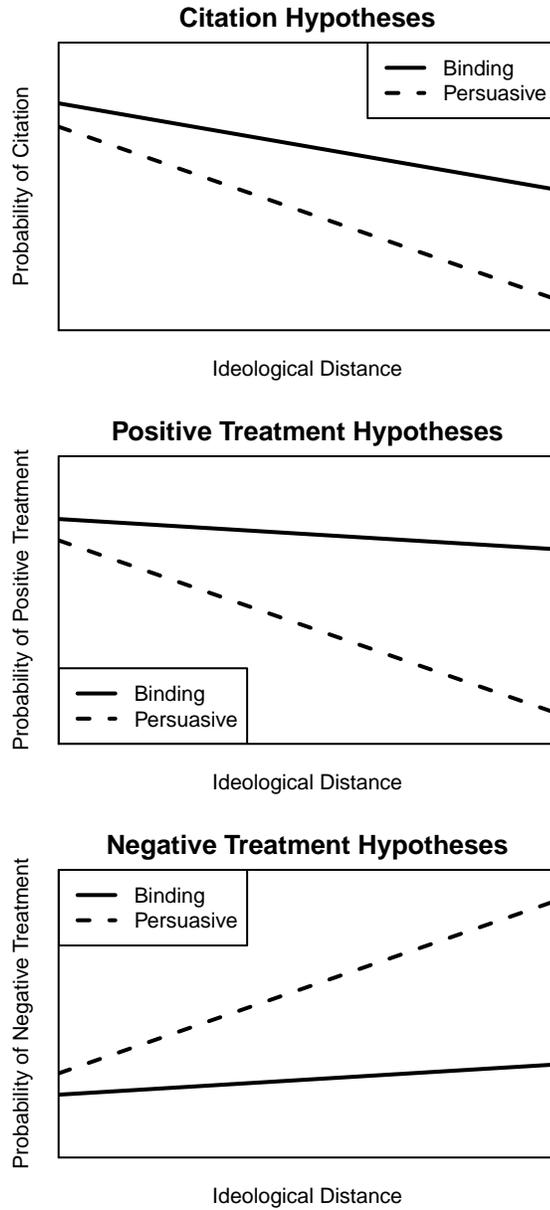
Figure 2.1: Illustration of Hypotheses: These graphs depict the general expectations of what the empirical results should look like if the legal doctrine of stare decisis constrains circuit judges.

all California appeals cases which ruled on the issue of the unconscionability of an arbitration clause in a form contract. I take a similar approach, but cast a wider net. The federal circuit courts generate a prodigious number of published opinions

each year, especially compared to the Supreme Court. My strategy is to focus on a subset of cases by selecting one particular issue area: Fourth Amendment search and seizure cases. This topic is well-suited to this context because it incorporates a discrete set of legal issues which are routinely raised in litigation.[10] Moreover, the bulk of search and seizure cases has been litigated since the early 1950's. This pattern is illustrated in Figure 2.2 by plotting the appearance of the phrase "Fourth Amendment" in Supreme Court cases. In the circuit courts there is a similar dearth of Fourth Amendment cases before the 1950's. Such timing is key because the measure of judicial ideology employed here is not widely available for cases decided before 1953. Therefore, search and seizure provides an area of law where it is possible to incorporate almost the entire body of circuit precedent within the scope of the study.

**Number of Times 'Fourth Amendment'
Appears in S.Ct. Cases by Year (1791–2005)**
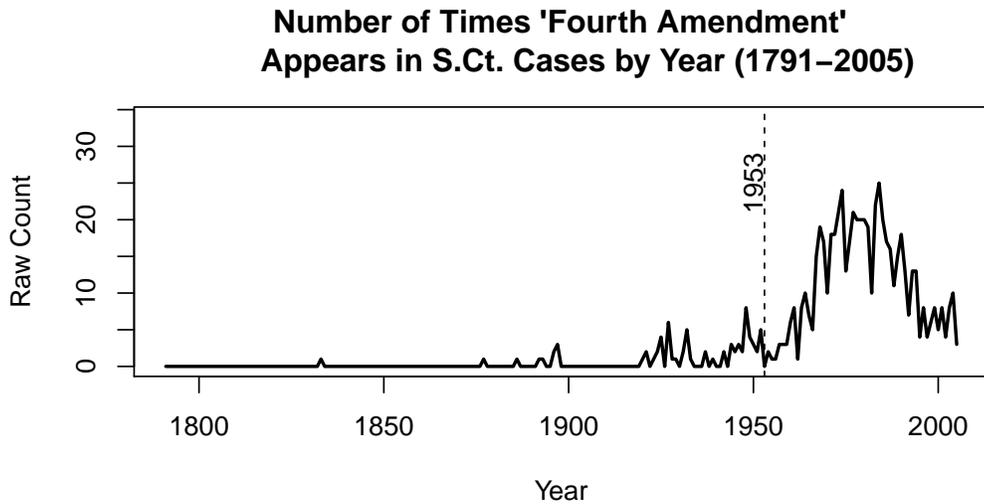
Figure 2.2: The Emergence of Fourth Amendment Litigation: This plot tracks the raw number of time the phrase "Fourth Amendment" appears in U.S. Supreme Court cases each year. The data were obtained from www.legallanguageexplorer.com.

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[10]Although the Supreme Court rules on such cases on a fairly regular basis, there are so many possible factual scenarios that significant litigation at the circuit level continually develops.

I selected search and seizure cases from the circuit courts by using Lexis to identify every published[11] circuit case from 1953 to 2010 which cites the Fourth Amendment of the United States Constitution.[12] Then all cases on this list which were not decided on the merits (e.g., ruling on a motion for rehearing en banc) were excluded. The resulting dataset contains 15,517 cases. This selection method is likely to be somewhat over-inclusive. However, the objective is to build a choice set of all potential precedents which may be cited with respect to a search and seizure issue. In this context, the primary concern is under-inclusion, and that danger has been ameliorated. It is difficult to imagine a federal court ruling on a search and seizure issue (or even discussing it) without citing the Fourth Amendment.

I analyze citation patterns in search and seizure panel opinions from 2000 to 2010. En banc cases are not analyzed as treatment cases because a circuit sitting en banc has the authority (not held by a panel) to overturn a previous rulings from their own circuit (Hellman, 2007). The cases in my dataset from 1953 to 1999 and all en banc cases are used only as potential precedents which may be cited. For each treatment case, the choice set includes every precedent from 1953 to the day before the opinion in the treatment case was issued. The unit of analysis is a treatment case-precedent pair. There is an observation for each pair between a treatment case and every precedent in that treatment case's choice set.

The first stage decision of whether to cite a precedent in the choice set is modeled using a probit model. Since each precedent appears in the dataset every time it

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[11]Unpublished opinions are excluded both because they are never binding precedent and because they are unavailable for some circuits until quite recently (Merritt, 1990). Furthermore, any citation analysis involving unpublished opinions would be significantly complicated by the presence of rules in many circuits banning or limiting citation to such cases prior to the enactment of Federal Rule of Appellate Procedure 32.1 on January 1, 2007.

[12]Cases are obtained from the eleven numbered geographical circuits and the D.C. Circuit. Intermediate appellate courts with subject-specific jurisdiction were not included in this study.

is paired with a treatment case, I estimate robust standard errors clustered on the precedent. For the treatment case-precedent pairs where the precedent was cited, I use a probit model to model whether the treatment was negative and another to model whether it was positive. A two-stage decisionmaking process such as this sometimes raises the problem of correlated residuals between the two stages biasing the estimates at the second stage (Heckman, 1979). However, recent research indicates that estimation of selection models can cause bigger problems than they purport to fix (Brandt and Schneider, 2007) and that selection models with categorical outcomes in the second stage are particularly problematic (Freedman and Sekhon, 2010). Estimation of a Heckman-type selection model for positive treatment is not possible (using the "heckman" command in Stata produce results that are not concave even after several dozens of iterations). The results of modeling negative treatment with this type of model produces results which are substantially similar to those presented and do not materially alter any of the central substantive conclusions (see Table 2.5 in the Appendix).

I obtain the data on citation and treatment of precedents from *Shepard's Citations*, a legal publication which both lists all citations to a case and classifies the nature of those citations. Since some treatment categories in *Shepard's* can be both ambiguous and heterogenous, I follow the advice of Spriggs and Hansford (2000), and only utilize treatment categories which clearly indicate either positive or negative treatment. (Spriggs and Hansford, 2000). I employ their classification of which treatments are positive and negative and extend that categorization slightly to accommodate treatments *Shepard's* has added over time. 'Followed' is the only *Shepard's* treatment classified as positive while the negative treatments are the following: 'Distinguished,' 'Criticized,' 'Limited,' 'Questioned,' 'Overruled,' 'Abrogated,' 'Superseded,' and 'Dis-

approved.' All citations which were not treated positively or negatively I classify as neutral treatment. The most common neutral treatment is simply 'Cited.'

The first key explanatory variable is the ideological distance between a treatment case and a precedent. For the treatment case, I use a measure of the ideology of the authoring judge since citation decisions are largely (although not entirely) under the discretion of the author (Cross et al., 2010; King, 2007). While details in an opinion, including use of citations, are dictated primarily by the author, the overall outcome and import of a circuit court opinion is more likely to reflect the ideology of all the judges. Since an author in a treatment case considers a precedent more or less in its entirety, the ideological location of the precedent is most accurately reflected by the median judge. Consequently, the ideological location of each precedent is measured using the median Judicial Common Space ("JCS") score of the judges.

JCS scores are based on the ideology of the political elites who appointed a judge and are located on a scale from -1 (liberal) to 1 (conservative) (Epstein et al., 2007; Giles, Hettinger and Peppers, 2001; Poole, 1998). The variable *Ideological Distance* is the absolute value of the difference between the JCS score of the authoring judge in the treatment case and the median JCS score of the judges in the precedent case. This variable has a theoretical range from zero to two and higher values indicate greater iedological disparity. The second key explanatory variable is whether a precedent is binding or not. This variable, *Binding Precedent* takes a value of one if the precedent is from the same circuit as the treatment case and zero otherwise.[13] The interaction between *Ideological Distance* and *Binding Precedent* rounds out the key theoretical explanatory variables.

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[13]When the Fifth Circuit was split in 1981, the judges agreed that all existing precedents from the old Fifth Circuit would be binding in the newly created Eleventh Circuit as well (Barrow and Walker, 1988: 245). Consequently, I code *Binding Precedent* as one for all precedents from the old Fifth Circuit when present in the choice set of a case from either the Fifth or Eleventh Circuit.

Control variables for both the citation and treatment stages of the models include a number of characteristics of the precedent that have been identified as important: whether it was written by the author in the treatment case, whether there was a dissenting opinion, whether it was a per curiam opinion, whether it was decided en banc, its vitality, the total number of times it has been previously cited by circuit courts, the total number of times it has been previously treated by circuit courts, its age, its length (logged number of words), the quality of the legal analysis, and its similarity to the treatment case (Black and Spriggs, 2008; Hansford and Spriggs, 2006; Hazelton, Hinkle and Spriggs, 2011; Johnson, 1987; Spriggs and Hansford, 2002). The length of the treatment case (logged number of words) is also important to include in both models (Black and Spriggs, 2008). In addition, in the citation model I control for the logged number of available binding precedents and the workload of the deciding court.

Determining whether a precedent was written by the author in the treatment case, had a dissent, and was per curiam or en banc and calculating its age and total cites and treatments at the time of the treatment case are straightforward (Corley, 2009). Vitality of a precedent is measured as the number of positive treatments by a published circuit court case minus the number of negative treatments by a published circuit court case (at the time of the treatment case) (Hansford and Spriggs, 2006).[14] The circuit caseload variable is the average number of cases terminated per active

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[14]Total citations, total treatments, and vitality of a precedent are measured using all citations/treatments of a case by another published circuit court case since 1953, even if the case was treated by a different circuit.

judge in the circuit and year of the treatment case. These data were obtained from the Federal Court Management Statistics.[15]

Both legal quality of a precedent and similarity between a treatment case and precedent are important to control for, but elusive concepts to operationalize. For legal quality, I take an approach similar to Hume (2009) and look to the extent quoted language is employed in the opinion (Hume, 2009a,b). Arguably, more extensive use of direct quotations indicates that an opinion is more solidly rooted in the relevant body of law. Using a series of regular expressions written in Python, I identify all quoted text in each opinion and generate a measure of the proportion of words in the opinion which are quoted. The length of the precedent may also be viewed as a rough measure of legal quality.

In order to formulate a measure of case similarity that is feasible to implement on a large scale, I turn to the field of machine learning. State-of-the-art information retrieval systems utilize cosine similarity scores as a normalized metric of the similarity of the text of two documents. I calculate this measure for every treatment case-precedent pair in the dataset and scale it from zero to one hundred.[16] Cosine similarity scores do not take into account the order of words, but are generated based on the number and importance of words which occur in both documents. Words which appear in fewer documents within the entire corpus are given a higher weight since they carry more information. For example, the appearance of the word "curtilage" in two documents would increase the cosine similarity score more than the appearance

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[15]The Federal Court Management Statistics are available online at `http://www.uscourts.gov/Statistics/FederalCourtManagementStatistics/FederalCourtManagementStatistics_Archive.aspx`.

[16]Before calculating cosine similarity scores, I processed the text of each opinion by dropping citations and stopwords, lemmatizing and stemming the text, and then dropping all tokens which appeared fewer than ten times in the corpus.

of the word "defendant" in both documents. Two search and seizure cases which discuss curtilage are more likely to be similar cases than two search and seizure cases which involve a defendant.

In the dataset the actual values of the cosine similarity measure range from one to fifty and the distribution has a substantial right-hand skew. In order to identify the most relevant precedents while avoiding estimation problems caused by the sparsity of data points in the upper end of the range, I create a dummy variable which equals one if the cosine similarity score is in the top one percent (i.e., greater than 11) and zero otherwise. This threshold identifies a group of the most similar precedents which is small enough that a judge might actually consider them all. Clearly a judge will not actively consider ten thousand potential precedents, but she may very well evaluate a hundred or so of the most relevant cases, at least in a cursory fashion. This measurement strategy brings the considerable experience and knowledge of computer science and computational linguistics to bear on a challenging problem.[17]

A summary of the dataset is provided in Table 2.1. Since the unit of analysis is every pairwise combination of a treatment case and every precedent in its choice set, the size of the dataset is quite large, over 60 million. Table 2.1 provides the mean, standard deviation, and median of the continuous variables and a percentage breakdown of each of the dichotomous variables. The word length of both the precedent and the treatment case as well as the size of the choice set of binding precedents are all transformed by taking their natural log. In about 18% of the cited cases the precedent was positively treated, while the precedent was negatively treated only around

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[17]Since cosine similarity is calculated using the text of a treatment case, there may be some endogeneity in this measure. While it would be preferable to use text generated prior to the citation/treatment decision (such as the district court ruling being appealed or legal briefs submitted by the parties) this type of information is not readily available for most cases in the dataset. Moreover, endogeneity of the cosine similarity metric is not unduly troubling in this context since it is only used as a control variable.

7% of the time. The median precedent is 17 years old, has been cited three times, has not been treated (and, thus, has a vitality score of zero), and contains 8% quoted language. About 10% of the time a precedent is binding (i.e., from the same circuit as the treatment case). Less than a quarter of a percent of precedents were written by the same author as the treatment case, approximately 15% of precedents have a dissent, almost 10% were per curiam, and 2.4% were decided en banc.

Continuous Variables	Mean	Std. Dev.	Med.
Ideological Distance	0.38	0.26	0.34
Vitality	0.13	1.13	0
Total Citations	4.86	6.35	3
Total Treatments	0.85	1.56	0
Age	18.25	11.94	17
Length (prec.)	7.96	0.77	8.01
Proportion Quoted (prec.)	0.09	0.07	0.08
Length (treat.)	8.21	0.64	8.2
Size of Binding Choice Set	7.09	0.42	7.12
Caseload	449.51	155.80	415
Dichotomous Variables	**1**	**0**	
Citation	0.05%	99.95%	
Positive Treatment	18.03%	81.97%	
Negative Treatment	6.57%	93.43%	
Binding Precedent	10.11%	89.89%	
Same Author	0.23%	99.77%	
Dissent (prec.)	14.90%	85.10%	
Per Curiam (prec.)	9.51%	90.49%	
En Banc (prec.)	2.38%	97.62%	

Table 2.1: Summary Statistics: The length of both the precedent and the treatment case are measured as the natural log of the word count. The size of the binding choice set is also transformed by taking the natural log of the raw count. The percentages reported for positive and negative treatment are conditional on a precedent being cited. All other summary statistics are calculated using all 60,695,465 treatment case-precedent pairs in the dataset.

2.5 Results

The first decision analyzed is simply which precedents to cite. Figure 2.3 presents the results of the citation model. Since this dataset contains over 60 million observations, it is worth noting at the outset that a large N does not change the probability of Type I error, but it does facilitate uncovering very small effects. Consequently, one must be particularly careful to bear in the mind the distinction between statistical and substantive significance. Figure 2.3 shows that all three of the main explanatory variables of interest are statistically significant[18] and in the expected direction. The negative coefficient for *Ideological Distance* demonstrates that the farther away a judge is ideologically from a persuasive precedent, the less likely she is to cite it in her opinion. Furthermore, when a judge is ideologically aligned with a precedent, he is considerably more likely to cite binding precedents than persuasive precedents. Finally, the interaction between these two is positive which indicates that when a precedent is binding the effect of ideology is dampened. These findings support the hypothesis that judges' decisions about which precedents to cite are constrained by the doctrinal status of a precedent (as binding or persuasive). In other words, a judge's ideological proximity to a precedent has less impact on the citation decision when the precedent is binding than when it is persuasive.

Figure 2.4 shows the predicted probability of citation for binding (top panel) and persuasive (bottom panel) precedents at different values of *Ideological Distance* (holding all other variables constant at their mean or mode as applicable) with accompanying 95% confidence intervals.[19] Since citation to a binding precedent is much

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[18]All discussion of statistical significance is at the 0.05 level.

[19]I generated these predicted probabilities and associated confidence intervals (and all similar quantities of interest presented throughout this dissertation) using stochastic simulations (Brambor, Clark and Golder, 2006; King, Tomz and Wittenberg, 2000).
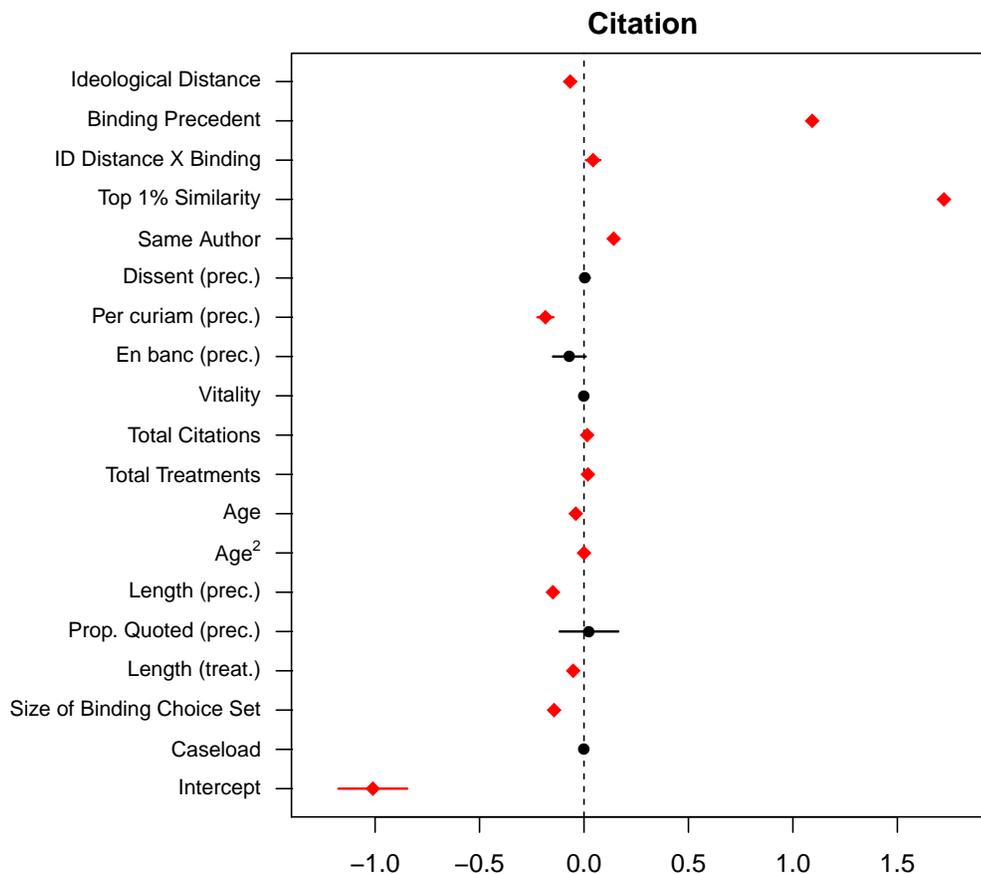
Citation

Figure 2.3: Citation Model: Probit regression estimates of the effect of *Ideological Distance*, whether a precedent is binding, their interaction, and a range of control variables on the decision of whether to cite a precedent. Dots and diamonds indicate point estimates. Bars indicate 95% confidence intervals. Diamonds (instead of dots) and bars in red denote that a coefficient has a p-value less than 0.05. The full regression table is presented in the Appendix.

more likely than citing a persuasive precedent, the results are presented on different scales to provide a more useful visual comparison. For binding precedents, the effect of *Ideological Distance* is also negative and statistically significant.[20]

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[20]The p-value is 0.006
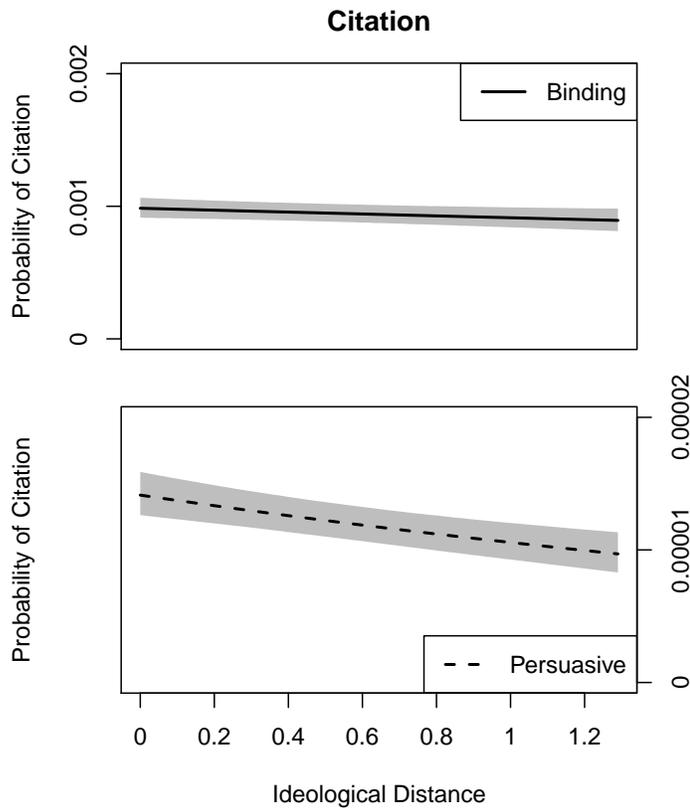
Figure 2.4: Effect of *Ideological Distance* on Citation: This graph provides the predicted probability of citation and 95% confidence intervals for each type of precedent (binding and persuasive) at different values of *Ideological Distance*. All other variables are held at their mean or mode as applicable.

The very small baseline probability of a case being cited complicates the assessment of substantive significance. The choice set for each treatment case contains ten to fifteen thousand cases while each opinion actually cites only seven cases from the choice set on average. As a result, the baseline probability of citation is quite low simply by virtue of the research design.[21] In this context it is more helpful to con-

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[21]The only way to remedy this situation would be to construct a much more narrowly tailored choice set. However, doing so without creating worse problems in terms of issues such as subjective coding is a major roadblock. In the context of Supreme Court research it is possible to extract citations from legal briefs or lower court decisions to build a narrower choice set (see, e.g., Spriggs and Hansford, 2002), but such documents are not widely available for circuit court cases.

sider effect size in relative, rather than absolute, terms. Holding everything else in the model at its mean or mode, a binding precedent is seventy-five times more likely to be cited than a persuasive precedent. The relative effect of *Ideological Distance* can be assessed in terms of the percentage increase in the probability of citation from one standard deviation above the mean of *Ideological Distance* (farther away) to one standard deviation below the mean (closer). For persuasive precedents, where ideology has a larger effect, this change results in a 16% increase in the probability of citation. A binding precedent with *Ideological Distance* one standard deviation below the mean is only 4% more likely to be cited than a binding precedent with *Ideological Distance* one standard deviation above the mean. Using this metric, the effect of ideology is four times larger when a judge is not constrained by the doctrine of stare decisis.

In the citation model several of the control variables perform as expected. Precedents which are more similar to the treatment case, precedents authored by the same judge as the treatment case, and precedents which have been cited or treated more frequently are more likely to be cited. Per curiam precedents are less likely to be cited. When a judge has a a greater number of binding precedents to choose from the probability of any given case being cited decreases. Finally, the curvilinear effect of age follows the anticipated pattern. However, results for two of the control variables are curious. Greater length for both the treatment case and precedent actually decrease the probability of citation.

The second stage of a judge's decision process is to determine whether and how to treat those precedents which she has chosen to cite (which reduces the size of the dataset to 33,263). Since the expectations for positive treatment are similar to those for citation, I will begin by presenting the results for the positive treatment model before turning to the negative treatment model. The results of the probit model

used to explore positive treatment decisions are set forth in Figure 2.5. When the authoring judge is ideologically aligned with the precedent, he is more likely to cite binding precedents than persuasive precedents. However, neither *Ideological Distance* nor the interaction terms are statistically significant. One possible explanation for no finding of an ideological effect is the similarity between positive and neutral treatment.
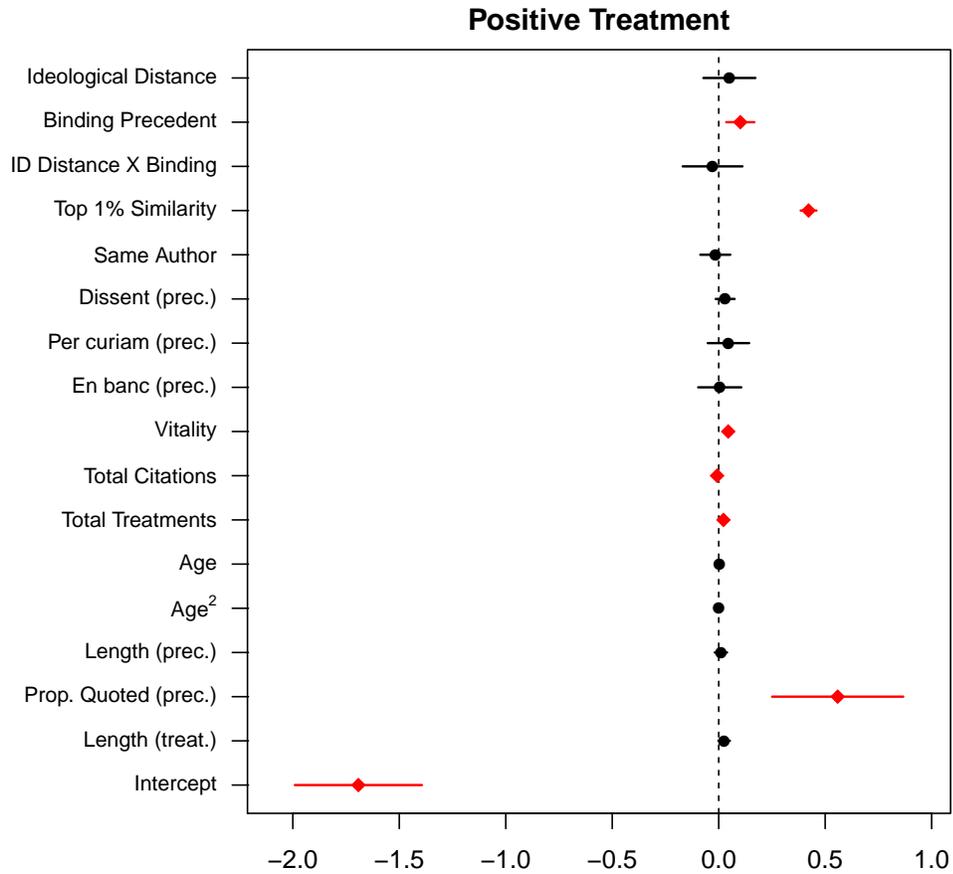
Figure 2.5: Positive Treatment Model: Probit regression estimates of the effect of *Ideological Distance*, whether a precedent is binding, their interaction, and a range of control variables on the decision to treat a cited precedent positively (or not). Dots and diamonds indicate point estimates. Bars indicate 95% confidence intervals. Diamonds (instead of dots) and bars in red denote that a coefficient has a p-value less than 0.05. The full regression table is presented in the Appendix.

As discussed previously, increased positive treatment of binding precedents without accompanying ideological dampening is consistent with legal constraint, but not sufficient to exclude other explanations. Moreover, the difference in positive treatment rates for binding and persuasive precedents is not statistically significant for all possible values of *Ideological Distance*. Figure 2.6 plots the difference between the probability of citing a binding precedent and the probability of citing a persuasive precedent over the entire range of *Ideological Distance* observed in the dataset. The 95% confidence interval includes zero for all values of *Ideological Distance* greater than 0.83. However, for nearly 95% of the data *Ideological Distance* falls below this threshold.

While most of the control variables are not statistically significant in the positive treatment model, those which are largely perform as expected. Cited precedents which have a cosine similarity score in the top one percent, have greater vitality, and have been treated more frequently are all more likely to be positively treated. A precedent being cited more frequently actually results in a slightly lower rate of positive treatment. Finally, one of the variables designed to roughly approximate the legal quality of a precedent, proportion of text which is quoted, increases the probability of positive treatment as expected.

Figure 2.7 contains the results of the negative treatment model. As expected, *Ideological Distance* increases the probability of negative treatment. The predicted probability graph in Figure 2.8 illustrates that this effect is statistically significant for binding precedents as well as persuasive precedents. Judges are more likely to treat a precedent negatively when it is more ideologically distant. As hypothesized, negative treatment of binding precedents is *less* likely than negative treatment of persuasive precedents. Figure 2.9 shows that this difference is statistically significant for all values of *Ideological Distance* in the data.

**Difference in Probability of Positive Treatment of
Binding and Persuasive Precedent**
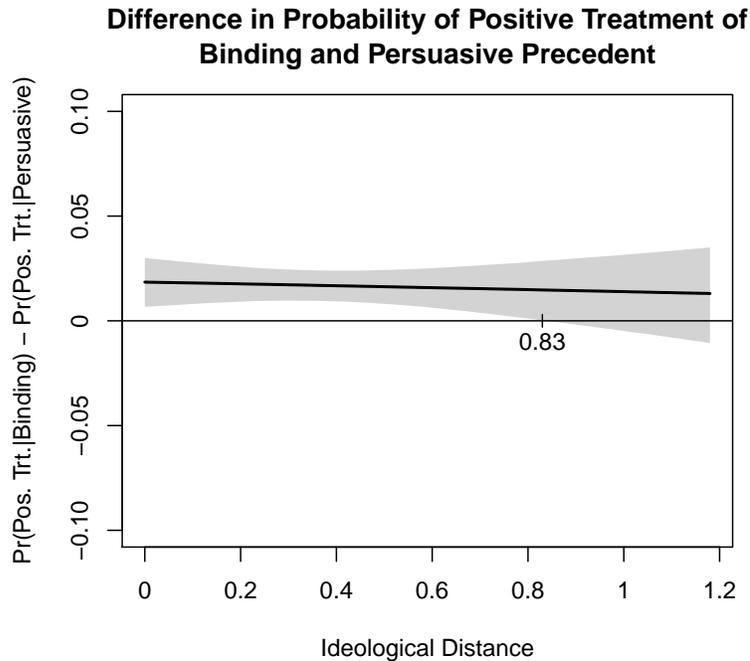
Figure 2.6: Effect of *Ideological Distance* on Difference Between Positive Treatment of Binding and Persuasive Precedents: This graph provides the difference in the predicted probability of positively treating a binding and persuasive precedent (and the associated 95% confidence interval) at different values of *Ideological Distance*. All other variables are held at their mean or mode conditional on a precedent being cited.

The interaction term is not statistically significant. The similar slope for binding and persuasive precedents in Figure 2.8 illustrates that the effect of ideology does not appear to be dampened for binding precedents. However, for negative treatment the significantly different predicted probabilities for binding and persuasive precedents provides evidence of legal constraint. Stare decisis is the only logical explanation for judges consistently showing more restraint in negatively treating binding precedents and being more free with negative treatment of persuasive precedents. Moreover, the gap is relatively substantial, given the infrequent nature of negative treatment. Over the range of *Ideological Distance* (and holding other variables constant at the mean
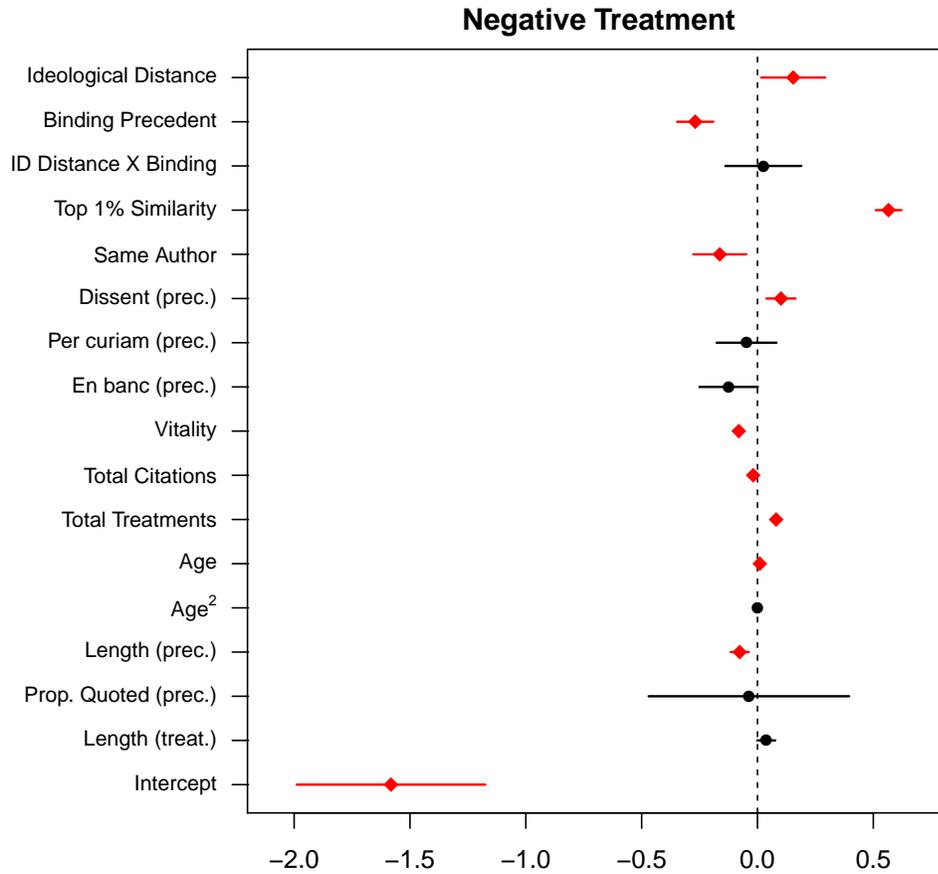
Figure 2.7: Negative Treatment Model: Probit regression estimates of the effect of *Ideological Distance*, whether a precedent is binding, their interaction, and a range of control variables on the decision to treat a cited precedent negatively (or not). Dots and diamonds indicate point estimates. Bars indicate 95% confidence intervals. Diamonds (instead of dots) and bars in red denote that a coefficient has a p-value less than 0.05. The full regression table is presented in the Appendix.

or mode) the absolute rate of negative treatment for binding precedents is between 1.6% and 2% lower than for persuasive precedents. Relatively speaking, the average persuasive precedent is 80% more likely to be treated negatively than the average binding precedent.

Negative Treatment
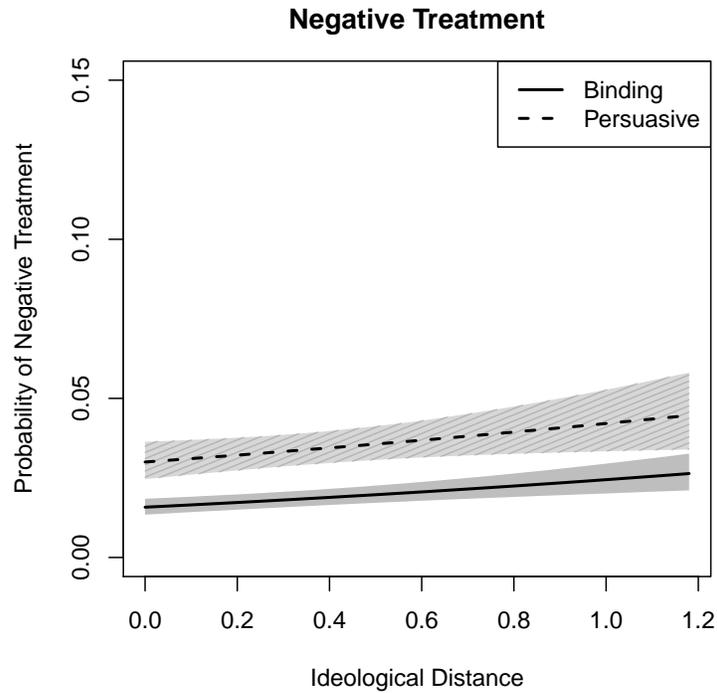
Figure 2.8: Effect of *Ideological Distance* on Negative Treatment: This graph provides the predicted probability and 95% confidence intervals of negatively treating a cited precedent for each type of precedent (binding and persuasive) at different values of *Ideological Distance*. All other variables are held at their mean or modal value conditional on a precedent being cited.

In the negative treatment model, most of the control variables are statistically significant. Notably, treatment case-precedent pairs with similarity scores in the top 1% are more likely to be treated negatively, just as they are more likely to be treated positively. As one might expect, precedents with a dissent and older precedents are more likely to be treated negatively. Precedents written by the same author, precedents with greater *Vitality*, more heavily cited precedents, and longer precedents are all less likely to be negatively treated. Interestingly, after controlling for vitality, precedents which have been treated more frequently are more likely to be treated negatively. Perhaps increased discussion of a case leads to chipping away at the rule set forth in the case. The remaining control variables are not statistically significant.

**Difference in Probability of Negative Treatment of
Binding and Persuasive Precedent**
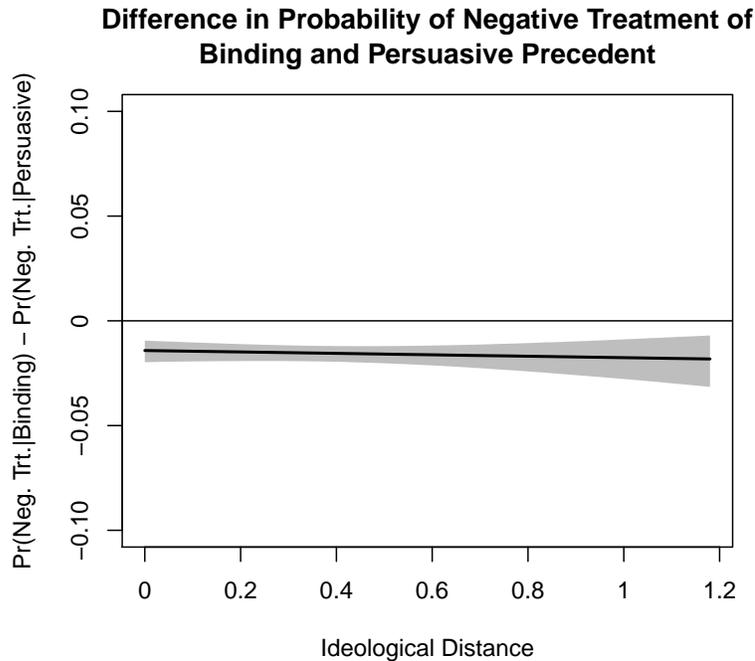
Figure 2.9: Effect of *Ideological Distance* on Difference Between Negative Treatment of Binding and Persuasive Precedents: This graph provides the difference in the predicted probability of negatively treating a binding and persuasive precedent (and the associated 95% confidence interval) at different values of *Ideological Distance*. All other variables are held at their mean or mode conditional on a precedent being cited.

2.6   Discussion

Examining circuit court citation practices with regard to their own published precedents provides insight into when and how law constrains judicial behavior and also highlights the need for continued investigation. The extensive data collected for this project facilitates close examination of the choices circuit judges make when deciding which circuit court precedents to incorporate into their opinions. As expected, ideology plays a role in the selection of precedents.

Remarkably, the effect of ideology on deciding whether to cite a case is significantly lessened when the precedent is from the same circuit. This pattern provides evidence

that the legal doctrine of stare decisis constrains judges' behavior. That is, judges act less ideologically when considering precedents which are binding as a matter of legal doctrine. On one hand, law dictates that published precedents from one's own circuit have virtually the same legal status as U.S. Supreme Court opinions (as long as there is no conflicting Supreme Court case). On the other hand, published precedents from other circuits are simply persuasive. Similar to a law review article, judges may refer to such a source if they find the reasoning persuasive or helpful, but have no obligation to do so under the law. The data provide evidence that these rules have an effect on judges. Judges are not only less ideological about the decision to cite binding precedents, they are also much more likely to cite binding precedents overall.

Since there is evidence that law constrains judges' determination of which cases to cite, one would also expect to find similar influence over decisions of how to treat cited cases. Since treatment decisions are clearly visible within the four corners of an opinion without consulting outside resources, it would make sense for judges to be even more careful about complying with legal doctrine. However, there is no evidence of ideological dampening for either positive or negative treatment. In fact, there is no evidence that ideology is a significant factor in the positive treatment decision at all. Most likely the lack of traction in the positive treatment model is due to similarity between neutral and positive treatments. In practice, merely citing a precedent is a type of soft positive treatment. Within the context of legal writing a citation to a precedent is understood to tacitly indicate acknowledgment and applicability. The fact that most of the control variables in the positive treatment model also fail to reach statistical significance further supports this conclusion.

While there is no evidence of ideological dampening on the decision to negatively treat a cited precedent, there is evidence that this decision is constrained by stare decisis. Judges are about 80% more likely to negatively treat a persuasive precedent

than a binding precedent. There can certainly be purely legal reasons to criticize or distinguish a precedent. But judges may have ideological reasons for doing so as well. The consistently lower rate of such negative treatment of binding precedents suggests judges refrain, at least to some extent, from negatively treating for ideological reasons when a precedent is binding.

The evidence of legal constraint this study provides overcomes a significant challenge in the study of how law influences judges. The institutional structure of the U.S. Courts of Appeals as twelve separate entities combines with the legal doctrine that in-circuit and out-of-circuit precedents have a different legal status to create a unique research opportunity. Each precedent in the dataset is considered by some judges as a binding precedent and by others as merely persuasive. The utility and novelty of this research design lies in the fact that it obviates concerns about unmeasured differences between two groups of cases, one consisting of binding precedents and the other of persuasive precedents. This approach illustrates the utility of extracting and studying information from the text of court opinions. Looking at how circuit court judges use precedent provides an opportunity to gain more nuanced insights into how and when judges are constrained by legal doctrine in a way which mitigates endogeneity concerns.

2.7 Appendix

	Coefficient	Standard Error	p-value
Intercept	-1.011^*	0.084	0.000
Ideological Distance	-0.066^*	0.014	0.000
Binding Precedent	1.092^*	0.010	0.000
ID X Binding	0.043^*	0.018	0.015
Top 1% Similarity Score	1.723^*	0.009	0.000
Same Author	0.142^*	0.013	0.000
Dissent	0.005	0.012	0.671
Per Curiam	-0.185^*	0.020	0.000
En Banc	-0.070	0.040	0.079
Vitality	0.000	0.006	1.000
Total Cites	0.015^*	0.003	0.000
Total Treatments	0.019^*	0.009	0.034
Age	-0.039^*	0.002	0.000
Age^2	0.000^*	0.000	0.000
Precedent Length	-0.149^*	0.007	0.000
Proportion Quoted	0.024	0.072	0.739
Treatment case length	-0.052^*	0.004	0.000
Binding Choice Set	-0.143^*	0.008	0.000
Caseload	0.000	0.000	0.221
N		60,695,465	

Table 2.2: Citation Model: Probit regression estimates of the effect of *Ideological Distance*, whether a precedent is binding, their interaction, and a range of control variables on the decision of whether to cite a precedent. The reported standard errors are robust standard errors which are clustered on the precedent and * denotes a p-value less than 0.05.

	Coefficient	Standard Error	p-value
Intercept	-1.692^*	0.152	0.000
Ideological Distance	0.050	0.062	0.420
Binding Precedent	0.102^*	0.034	0.003
ID Distance X Binding	-0.029	0.072	0.688
Top 1% Similarity Score	0.422^*	0.019	0.000
Same Author	-0.016	0.036	0.661
Dissent (prec.)	0.030	0.023	0.181
Per Curiam (prec.)	0.046	0.050	0.356
En Banc (prec.)	0.005	0.052	0.924
Vitality	0.045^*	0.006	0.000
Total Cites	-0.008^*	0.002	0.000
Total Treatments	0.022^*	0.006	0.000
Age	0.003	0.003	0.408
Age^2	0.000	0.000	0.664
Precedent Length	0.010	0.015	0.481
Proportion Quoted (prec.)	0.559^*	0.157	0.000
Treatment case length	0.026	0.014	0.065
N		33,263	

Table 2.3: Positive Treatment Model: Probit regression estimates of the effect of *Ideological Distance*, whether a precedent is binding, their interaction, and a range of control variables on the decision to treat a cited precedent positively (or not). The reported standard errors are robust standard errors which are clustered on the precedent and * denotes a p-value less than 0.05.

	Coefficient	Standard Error	p-value
Intercept	-1.542^*	0.208	0.000
Ideological Distance	0.168^*	0.071	0.018
Binding Precedent	-0.252^*	0.040	0.000
ID Distance X Binding	0.006	0.084	0.944
Top 1% Similarity Score	0.552^*	0.028	0.000
Same Author	-0.173^*	0.059	0.003
Dissent (prec.)	0.099^*	0.031	0.001
Per Curiam (prec.)	-0.022	0.066	0.737
En Banc (prec.)	-0.071	0.065	0.272
Vitality	-0.070^*	0.007	0.000
Total Cites	-0.010^*	0.003	0.001
Total Treatments	0.069^*	0.008	0.000
Age	0.008	0.005	0.092
Age^2	0.000	0.000	0.995
Precedent Length	-0.069^*	0.020	0.001
Proportion Quoted (prec.)	-0.067	0.219	0.758
Treatment case length	0.028	0.020	0.155
N		33,263	

Table 2.4: Negative Treatment Model: Probit regression estimates of the effect of *Ideological Distance*, whether a precedent is binding, their interaction, and a range of control variables on the decision to treat a cited precedent negatively (or not). The reported standard errors are robust standard errors which are clustered on the precedent and * denotes a p-value less than 0.05.

	Coefficient	Standard Error	p-value
Negative Treatment			
Intercept	0.133*	0.032	0.000
Ideological Distance	0.029*	0.013	0.023
Binding Precedent	−0.051*	0.008	0.000
ID X Binding	−0.009	0.014	0.491
Top 1% Similarity Score	0.027*	0.009	0.004
Same Author	−0.018*	0.005	0.000
Dissent	0.013*	0.005	0.005
Per Curiam	−0.003	0.008	0.753
En Banc	−0.017*	0.008	0.033
Vitality	−0.013*	0.002	0.000
Total Cites	−0.002*	0.000	0.000
Total Treatments	0.011*	0.001	0.000
Age	0.002*	0.001	0.012
Age^2	0.000	0.000	0.965
Precedent Length	−0.008*	0.003	0.003
Proportion Quoted (prec.)	−0.008	0.027	0.770
Treatment case length	0.005*	0.002	0.035
N		33,263	
Citation			
Intercept	−0.935*	0.081	0.000
Ideological Distance	−0.067*	0.014	0.000
Binding Precedent	1.094*	0.010	0.000
ID X Binding	0.038*	0.018	0.033
Top 1% Similarity Score	1.716*	0.009	0.000
Same Author	0.144*	0.013	0.000
Dissent	0.003	0.011	0.806
Per Curiam	−0.197*	0.019	0.000
En Banc	0.043	0.028	0.123
Vitality	−0.020*	0.007	0.003
Total Cites	0.033*	0.002	0.000
Total Treatments	0.021*	0.005	0.000
Age	−0.032*	0.001	0.000
Age^2	0.000*	0.000	0.000
Precedent Length	−0.134*	0.006	0.000
Proportion Quoted (prec.)	−0.118	0.066	0.073
Treatment case length	−0.057*	0.004	0.000
Binding Choice Set	−0.171*	0.008	0.000
Caseload	0.000	0.000	0.720
N		60,695,465	
ρ	−0.080	0.026	
σ	0.245	0.003	

Table 2.5: Heckman Negative Treatment Model: Heckman selection model regression estimates of the effect of *Ideological Distance*, whether a precedent is from the same circuit, their interaction, and a range of control variables on a judge's two-stage decision of whether to cite a precedent and, if cited, whether to treat it negatively. The selection equation (citation) is a probit model and the outcome equation (negative treatment) is a linear probability model with standard errors adjusted to account for the correlation between the residuals of the two equations. The reported standard errors are robust standard errors which are clustered on the precedent and * denotes a p-value less than 0.05.

Chapter 3

Strategic Anticipation

of En Banc Review

The previous chapter provides evidence that federal circuit judges are constrained by the norm of stare decisis. The existing state of the law is one of the factors which influences how judges write the opinions which dynamically shape the contours of the law. This observation raises the question of *why* judges' act in a constrained fashion. Political scientists often cite the fear of reversal and desire for promotion as primary motivations, framing the judicial hierarchy as a typical principal-agent relationship (Boyd and Spriggs, 2009; Choi, Gulati and Posner, 2012; Epstein and Jacobi, 2010; Randazzo, 2008). This framework posits that judges follow the law only when institutional features combine to create disincentives for abandoning established doctrine. However, the quest for empirical evidence of strategic judicial behavior has produced mixed results (Collins and Martinek, 2011; Bowie and Songer, 2009; Blackstone and Smelcer, 2008; Giles et al., 2007; Cross, 2003; George, 1997). In this chapter I examine whether judges' compliance with stare decisis is strategic.

Specifically, I investigate the impact of institutional context on citation practices in the U.S. Courts of Appeals.

Federal circuit judges have very little chance of promotion. Consequently, in the context of a principal-agent model, fear of reversal is the primary explanation for why a policy-motivated circuit judge would follow the doctrine of stare decisis (Cameron, Segal and Songer, 2000; Kim, 2006). Three-judge panels in the U.S. Courts of Appeals face oversight from two sources, their own circuit rehearing a case in an en banc proceeding and the U.S. Supreme Court (Boyd and Spriggs, 2009; Kim, 2006). This chapter focuses on the former source of oversight. I explore whether circuit court panels adjust their level of adherence to legal doctrine in strategic anticipation of en banc review. In this context, strategic action is particularly feasible because actors consider the possibility of review by colleagues who are very well known to them from regular interactions (Bowie and Songer, 2009; Giles et al., 2007).

This chapter empirically tests for evidence of strategic anticipation of en banc review using a subset of the citation data employed in Chapter 2. The choice set for citation analysis is composed only of binding precedents (i.e., those originating within the same circuit as the treatment case). If there is strategic behavior, the relative location of the author of a panel opinion, the precedent, and the circuit will all influence citation and treatment decisions. The results show that the ideological preferences of the entire circuit influence citation and treatment decisions both directly and indirectly. Decisions regarding citation and negative treatment are directly influenced by circuit preferences, and the extent to which an author's ideology influences positive treatment decisions is contingent upon the ideology of the entire circuit.

These results provide insight into the role of strategy in the often overlooked hierarchical relationship between circuit court panels and their circuit. Exploring this dynamic is key to understanding issues such as the interplay between stare decisis and

ideological shifts brought about by personnel changes within circuits. Scholars have increasingly recognized that both law and ideology play important roles in judicial decisionmaking and the nuances of when, how, why, and under what conditions these factors matter present fascinating and challenging questions. This study furthers this tradition by offering insight into the relationship between respect for the doctrine of stare decisis and ideological expediency.

3.1 Strategic Behavior in the U.S. Courts of Appeals

The vast majority of federal case law is established by the U.S. Courts of Appeals rather than the U.S. Supreme Court. The resulting importance of the policymaking function of the intermediate appellate courts is reflected by the fact that cases are not resolved by an individual judge, but by a group of judges working together. This institutional structure results from the idea that multiple judges are more likely to produce legally sound decisions and less likely to make mistakes (Boyd and Spriggs, 2009; Hettinger, Lindquist and Martinek, 2006). The impracticability of determining objectively what constitutes the "correct" or "best" result in any given case renders this proposition largely unfalsifiable (Cross, 2005). Nevertheless, the fact that circuit judges make decisions as part of a group rather than individually is a significant institutional feature and its implications can be (and are) explored empirically.

Scholars have long recognized that actors working in such a group context may act strategically by taking into account their colleagues' anticipated actions when determining what actions will be most successful to obtain their own ideological preferences in the long run (Atkins, 1972; Boucher and Segal, 1995; Bowie and Songer,

2009; Cameron, Segal and Songer, 2000; Collins and Martinek, 2011; George, 1997; King, 2007; Law, 2004; Lindquist, Martinek and Hettinger, 2007; Maltzman, Spriggs and Wahlbeck, 2000). Strategy comes into play in a hierarchical context as well. A principal-agent framework is often employed to examine how lower court judges, as agents, modify their behavior based on their estimation of how higher court judges, as principals, will exercise oversight (Boyd and Spriggs, 2009; Choi, Gulati and Posner, 2012; Epstein and Jacobi, 2010; Randazzo, 2008). The federal circuit courts provide a context within which all the judges in a circuit alternatively function as both colleagues and superiors to one another.

For bulk of their caseload, federal circuit judges decide cases in randomly assigned panels of three (Collins and Martinek, 2011; Kim, 2009). Consequently, each judge in a circuit repeatedly works in conjunction with different combinations of her circuit colleagues. In any given case, the assigned three-judge panel may or may not reflect the ideological composition of the circuit as a whole. As a matter of law, any published opinion issued by a panel is binding on future panels in the circuit (Barnett, 2002). However, if a panel's ruling is too extreme, the circuit can choose to re-hear the case en banc with all of the active judges in the circuit participating.[1] This arrangement puts interesting dynamics into play (Atkins, 1972).

The possibility of en banc review creates an institutional configuration which appears ripe to produce strategic action (Haire, Lindquist and Songer, 2003; Hettinger, Lindquist and Martinek, 2004; Kim, 2009; Lindquist, Martinek and Hettinger, 2007; Spitzer and Talley, 2000; Van Winkle, 1997). Judges who are in the ideological minority in their circuit will have the opportunity at some point to sit on a panel with

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[1]The exception is the Ninth Circuit which hears en banc cases using a subset of eleven judges because bringing all the judges on the circuit together to rule on such cases is too unwieldy (Posner, 2000).

one or more similarly-minded colleagues. This offers the opportunity to shape the law of their circuit in a manner which is consistent with their own preferences. There is evidence that judges take advantage of such opportunities (Atkins, 1972; Van Winkle, 1997). Yet judges may still be mindful of the possibility of en banc review. While very few cases are re-heard en banc (George and Solimine, 2001), the possibility still poses an extant threat to discourage rulings which are manifestly at odds with the majority of the circuit. Evidence indicates that this reality tempers the extent to which ideology influences judges' votes (Blackstone and Smelcer, 2008; Van Winkle, 1997). Van Winkle (1997) finds that judges who are in the ideological minority in a circuit are increasingly likely to vote according to their ideology when the have more allies in their circuit. In a similar vein, Blackstone and Smelcer (2008) present evidence that circuit judges in the minority of their circuit act less ideologically than their counterparts in the majority even when sitting on a panel with another similarly-minded judge. However, Hettinger et. al. (2004) fail to find evidence of circuit judges strategically anticipating en banc review.

There are multiple reasons why a circuit judge might want to avoid en banc review that must be weighed against the probability of such rehearing.[2] First, the possibility (if the en banc petition is granted) of the panel ruling being reversed and the law of the circuit being materially altered is against the judge's own interests. He might very well choose to temper the language and moderate the content of a legal policy in order to avoid the circuit instituting a more undesirable policy en banc. Second,

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[2]There are no corollary reasons for panel judges to seek out en banc review. Panels have the authority to make binding law in their circuit. Any small increase in the prestige of an en banc ruling over a normal decision is heavily outweighed by the resources necessary to rule on a case en banc. This is an important distinction between en banc review and Supreme Court review. A circuit panel can benefit substantially from having the Supreme Court affirm its decision because such a ruling would substantially expand both the importance and geographic impact of the case. There is no similar benefit to being affirmed en banc.

unlike review by a higher court, en banc review adds a considerable additional burden to the workload of the panel judges regardless of the outcome of the review since they are included in the rehearing of the case. In fact, the mere filing of a petition for rehearing en banc adds to a judge's workload. At a minimum the judge has to consider the petition.[3] In some circuits once a judge requests a vote on an en banc petition standard practice permits/expects the author of the panel opinion to write a memo in response to the petition. The third reason a panel judge is incentivized to avoid en banc rehearing is that rehearing creates additional work for one's colleagues. In a collegial environment where judges work with a relatively small handful of colleagues on a regular basis, long-term interests in not aggravating those colleagues may militate in favor of taking pains to avoid unnecessary en banc situations.

As many scholars have discussed, circuit judges may also act strategically with respect to the Supreme Court (Bowie and Songer, 2009; Cross, 2003; George, 1997; Haire, Lindquist and Songer, 2003). For example, a judge in the minority of the circuit might be more inclined to write dissenting opinions, thus signaling the Supreme Court to grant certiorari, when ideologically aligned with the Supreme Court (Caldeira, Wright and Zorn, 1999; Caldeira and Wright, 1988; Hettinger, Lindquist and Martinek, 2004). The evidence of this type of strategic behavior is mixed (Bowie and Songer, 2009; Cross, 2003; Kim, 2009). Haire et. al. (2003) find that circuits reverse district court rulings more often when the ruling under scrutiny is contrary to the Supreme Court's preferences. Westerland et. al. (2010) present evidence that circuit judges show less deference to Supreme Court precedents when they diverge from the

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[3]A formal vote is only conducted if at least one judge requests a vote. Federal Rules of Appellate Procedure 35(f). Consequently, in practice the majority of en banc petitions languish without even a vote. The is somewhat analogous to the agenda-setting process in the Supreme Court. Requesting a vote on a petition is similar in effect to putting a cert petition on the discuss list.

preferences of the current Supreme Court. Both of these findings are suggestive of strategic behavior.

If anything, strategic behavior might be even more likely within the circuit than with respect to the Supreme Court. The more immediate incentives to avoid en banc review are coupled with greater information about the relevant players which may enable circuit judges to be more efficiently strategic in the circuit context. Because each judge works with all of her colleagues in turn, circuit judges have the opportunity to develop extensive knowledge about the ideological preferences of the people who will make the decision about which cases to rehear en banc (Bowie and Songer, 2009; Giles et al., 2007). This chapter focuses on strategic anticipation of en banc review and leaves the complication of dual principals for future research.

Many studies looking for evidence of strategy focus on case outcomes or judges' votes. The difficulty in finding consistent evidence of strategic anticipation of en banc review may be attributable to circuit judges acting strategically in ways which are difficult to detect rather than to an absence of strategy. One of the challenges faced by judicial scholars is finding more nuanced modes of analysis which can detect subtler aspects of judicial behavior than votes. I go beyond votes and case outcomes to look for strategic behavior in the citation patterns of circuit court opinions. Both a judge's decision about which cases to cite and the discussion of the cited cases may be strategic with respect to the possibility of en banc review. While scholars have utilized citation analysis to study the Supreme Court (Hansford and Spriggs, 2006; Spriggs and Hansford, 2002; Walsh, 1997), to examine how circuit courts interact with Supreme Court precedent (Benesh and Reddick, 2002; Corley, 2009; Johnson, 1987; Niblett and Yoon, 2012; Westerland et al., 2010; Yoon, 2012) and to look for strategic anticipation in the actions of federal district judges (Boyd and Spriggs,

2009), deploying such analysis to investigate the impact of institutional structure on the inner-workings of circuit decisionmaking remains largely uncharted territory.

While all published in-circuit precedents are equally binding as a matter of legal doctrine (assuming they are applicable to the facts of the case) (Barnett, 2002), whether such equality is maintained in the face of strategic incentives to the contrary remains an open question. Aldisert (1989) opined that "[t]oo many advocates and commentators assume that all precedents are equivalent, that all are precedents *fortissimo*" (Aldisert, 1989: 630). Other observers have frequently pointed out the potential for judges to selectively cite and interpret precedents which suit their own purposes (Barnett, 2002; Cross et al., 2010; Hansford and Spriggs, 2006; Johnson, 1987; Niblett, 2010; Spriggs and Hansford, 2002). Not only are judges more likely to cite precedents which are closer to them ideologically, but there is also evidence that judges are more likely to comply with binding precedent when there is a greater chance their decision would be reviewed and overturned by a higher court (Collins, 2010; Westerland et al., 2010).

If strategic behavior is at work in the circuit courts, a panel would be most likely to strictly adhere to legal doctrine regarding the binding nature of precedent when it anticipates that en banc review is particularly likely. Conversely, if a panel anticipates a lack of oversight by the full circuit, there is little (or at least less) reason to act within the constraints of a binding precedent the panel finds unpalatable. Examining the characteristics of precedents that are not cited, those that are cited, and the uses of cited precedent provides an opportunity to look for these manifestations of strategy.

3.2 Modeling Citations: Strategy and Ideology

A simple model of strategic citation decisions facilitates the formation of hypotheses. Consider a judge who is crafting a panel opinion and deciding whether and how to cite a precedent. Three key factors come into play; the ideological location of the precedent, P; the panel author, A;[4] and the circuit median, C. Each of these points can be placed in one-dimensional space as illustrated in the top panel of Figure 3.1. Boyd and Spriggs (2009) construct a model for an analogous situation with three moving parts designed to investigate the impact of judicial hierarchy on citation behavior. I adapt their framework to the similar situation at hand.

In Chapter 2, evidence revealed that even when judges are constrained by law, their own ideology continues to play a role. This result is consistent with the long line of research which demonstrates the important role of ideology in judicial decisionmaking more broadly. Accordingly, the first quantity of interest is the distance between an authoring judge and a precedent, $|A - P|$, which I refer to as *Ideological Distance* and is illustrated in the second panel of Figure 3.1. When an author is closer to a precedent she is more likely to cite or positively treat a precedent and less likely to negatively treat a cited precedent.

The primary focus of this chapter is investigating whether circuit judges writing panel opinions strategically anticipate en banc review. This type of strategy can manifest in two ways. First, the location of the circuit can directly impact citation-related decisions. Second, the location of the circuit may moderate the effect of an author's ideology on such decisions. In other words, authors of panel opinions may strategically reign in their own ideology to a greater extent when circuit preferences

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[4]While the other two judges on the panel might occasionally request changes to a draft, the author has the most direct control over citation and treatment decisions.
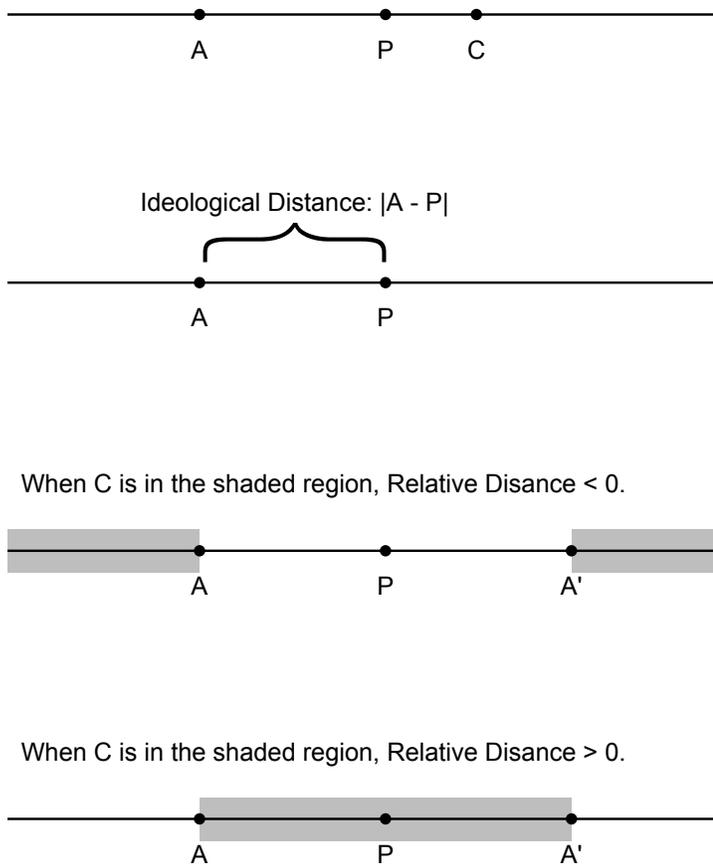
Figure 3.1: Illustration of Components of the Strategic Anticipation Model: The ideological location of the author of a panel opinion is denoted by A while P is the location of the precedent median and C is the relevant circuit median. *Relative Distance* $= |A - P| - |C - P|$.

make en banc review more likely. A strategic judge will not just take the circuit's location itself into account, but the location of the circuit in relationship to both the precedent and herself. I incorporate the concept developed by Boyd and Spriggs (2009) of measuring the *Relative Distance* of the author to a precedent relative to the distance between the circuit and the precedent. Formally, this quantity is the

distance between the author and precedent minus the distance between the circuit and the precedent, $|A - P| - |C - P|$.

The bottom two panels in Figure 3.1 depict two general scenarios which may occur. First, the circuit median, C, may be farther away from the precedent, P, than the author is. This is the case if C is located anywhere along the shaded region of the line in the third panel, i.e., if C is less than A or greater than A' (the reflection around P of the author's location). When this is the case *Relative Distance* will necessarily have a negative value. Smaller values of *Relative Distance* indicate that the author is increasingly closer to the precedent relative to the circuit median. On the other hand, if C is in the shaded region of the line depicted in the bottom panel $(A < C < A')$, the circuit median is closer to the precedent than the author of the panel opinion is. Under these circumstances, *Relative Distance* will be greater than zero. As *Relative Distance* gets larger, the author is farther away from the precedent relative to the location of the circuit. If the author and circuit are equidistant from the precedent, *Relative Distance* equal zero.

Holding the location of an author (A) and precedent (P) constant, as the circuit gets closer to the precedent, *Relative Distance* increases. The closer the circuit is to the precedent, the more likely it is to punish departures from the doctrine of stare decisis because such departures are contrary to the circuit's preferences. Although stare decisis leaves considerable room for judges to exercise discretion (Kim, 2006), greater compliance with stare decisis should manifest as an overall higher likelihood of citation and positive treatment and a lower rate of negative treatment. Therefore, I hypothesize that as *Relative Distance* increases, the panel is more likely to cite and positively treat and less likely to negatively treat a precedent.

The second manifestation of strategic anticipation of en banc review is how the location of the circuit has an impact on how the author's ideology influences citation

and treatment decisions. This dynamic would reveal itself as an interaction effect between *Ideological Distance* and *Relative Distance*. An author who is closer to a precedent relative to the circuit may rely more on ideology without as much concern about en banc review as an author who is more distant from a precedent relative to the circuit. Under extreme circumstances, when an author is very far from a precedent and the circuit is closely aligned with the precedent, the direction of the effect of *Ideological Distance* might even flip. Such an author may go out of his way to show deference to precedents farther from his own ideal point in anticipation that the circuit will be applying a higher level of scrutiny to his actions. I hypothesize that *Ideological Distance* will have a larger effect size when *Relative Distance* is small and that the effect size will decrease, and perhaps even flip signs, as *Relative Distance* increases. These and the forgoing hypotheses are summarized in Table 3.1.

Variable	Citation	Positive Treatment	Negative Treatment
Ideological Distance	-	-	+
Relative Distance	+	+	-
ID X Relative Distance	+	+	-

Table 3.1: Hypothesized sign for key explanatory variables.

3.3 Data and Research Design

The original database of published circuit court search and seizure cases which I introduced in Chapter 2 is also used to empirically test the hypotheses in this chapter. The sole focus here is on variation in the use and treatment of binding precedents, so the choice set for each treatment case includes only the available binding precedents (i.e., those from within the same circuit as the treatment case). Since the choice sets are smaller, it is practical to widen the scope of treatment cases under examination.

I model the citation and treatment decisions in 8,514 cases from 1990 to 2010. There is an observation for each pair between a treatment case and every precedent in that treatment case's choice set which results in a dataset containing over nine million observations. The outcome variables of citation, positive treatment, and negative treatment are also the same as in Chapter 2, and I use a probit model to test each.[5]

The models presented here also include a set of control variables similar to those employed in Chapter 2. The control variables in all three models include similarity between the precedent and the treatment case, whether the treatment case and precedent were written by the same author, whether there was a dissenting opinion in the precedent, whether the precedent was a per curiam opinion, whether the precedent was decided en banc, its vitality, the total number of times it has been previously treated by circuit courts, its age, the length of both opinions (logged number of words), and the proportion of quoted text in the precedent. The citation model also has additional control variables for the logged number of available precedents and the workload of the deciding court. The measures of how a precedent has been treated by circuit courts in the past (*Vitality*, *Total Citations*, and *Total Treatments*) are calculated based on all past references to a precedent in a published circuit court case from 1953 to the year before the treatment case. Citations and treatments from all circuits are included, not just those from the same circuit as the treatment case.

A summary of the dataset is provided in Table 3.2 which includes the mean, standard deviation, and median of the continuous variables and a percentage breakdown of each of the dichotomous variables. The word length of both the precedent and the treatment case as well as the size of the choice set are all transformed by taking their natural log. In about 15% of the cited cases the precedent was positively treated,

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[5]Once again, alternative specification using selection models either will not run or do not materially alter my results.

while a cited precedent was negatively treated only around 5% of the time. The median precedent is 15 years old, has been cited 3 times but not treated (and, thus, has a vitality score of zero), and contains 7% quoted language. About 2% of precedents were written by the same author as the treatment case, approximately 14% have a dissent, 10.5% were decided per curiam, and 2% were decided en banc.

Continuous Variables	Mean	Std. Dev.	Med.
Ideological Distance	0.35	0.27	0.32
Relative Distance	0.07	0.28	0.05
Vitality	0.08	1.09	0
Total Citations	4.71	6.24	3
Total Treatments	0.77	1.51	0
Age	16.66	11.29	15
Length (prec.)	7.87	0.77	7.92
Proportion Quoted (prec.)	0.09	0.08	0.07
Length (treat.)	8.12	0.67	8.14
Size of Choice Set	7.10	0.42	7.17
Caseload	465.58	148.88	445
Dichotomous Variables	**1**	**0**	
Citation	0.41%	99.59%	
Positive Treatment	15.10%	84.90%	
Negative Treatment	5.33%	94.67%	
Same Author	2.23%	97.77%	
Dissent (prec.)	14.20%	85.80%	
Per Curiam (prec.)	10.49%	89.51%	
En Banc (prec.)	2.18%	97.82%	

Table 3.2: Summary Statistics: The length of both the precedent and the treatment case are measured as the natural log of the word count. The size of the choice set is also transformed by taking the natural log of the raw count. The percentages reported for positive and negative treatment are conditional on a precedent being cited. All other summary statistics are calculated using all 9,607,742 treatment case-precedent pairs in the dataset.

3.4   Results

The first place I look for evidence of strategic anticipation of en banc review is a panel opinion author's decision regarding which precedents to cite and which to ignore. Figure 3.2 presents the results of the citation model. The coefficients for *Ideological Distance* and *Relative Distance* are both statistically significant and in the expected direction, but the interaction between the two is not significant. The negative coefficient for *Ideological Distance* indicates that when the panel author and the circuit median are the same distance away from a precedent, the author's ideology has the expected influence on citation. The farther away an author is from the precedent, the less likely she is to cite it. The positive coefficient for *Relative Distance* indicates that when an author is aligned with a precedent he is more likely to cite the precedent when the circuit median is closer to the precedent.

Since the model includes an interaction between two continuous variables, the information which can be gleaned solely from the regression results presented in Figure 3.2 is limited. Plotting marginal effects and predicted probabilities provides important additional insight. These graphs are presented in Figures 3.3 and 3.4. Figure 3.3 shows the marginal effect of each variable while the other (and all control variables) are set at their median. Marginal effects are the change in predicted probability which occurs when the constituent variable in question is increased by one standard deviation. The panel on the left demonstrates that the marginal effect of *Ideological Distance* is negative and statistically significant for all observed values of *Ideological Distance*. This finding holds true for values of *Relative Distance* greater than or equal to -0.22 (which includes 87% of the data).

The predicted probability of citation over the range of *Ideological Distance* is depicted in the left panel of Figure 3.4. While the substantive effect of *Ideological*
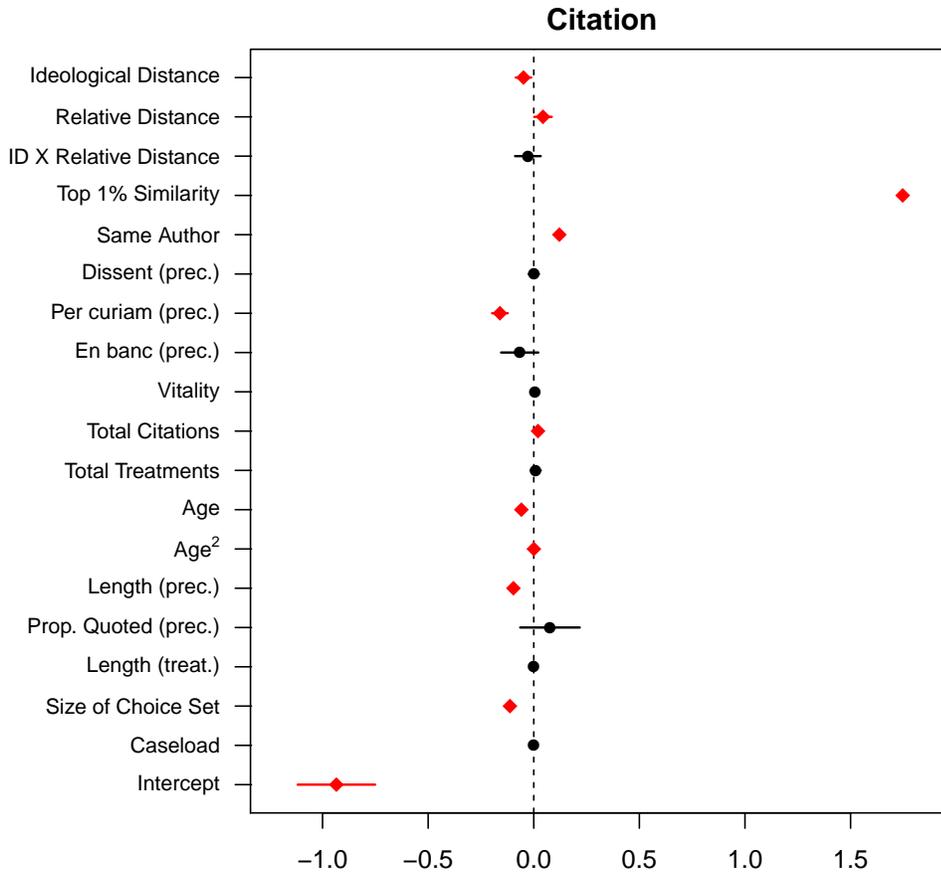
Citation

Figure 3.2: Citation Model: Probit regression estimates of the effect of *Ideological Distance*, *Relative Distance*, their interaction, and a range of control variables on the decision of whether to cite a precedent. Dots and diamonds indicate point estimates. Bars indicate 95% confidence intervals. Diamonds (instead of dots) and bars in red denote that a coefficient has a p-value less than 0.05. The full regression table is presented in the Appendix.

Distance is quite small in absolute terms, the very small baseline probability of citing any given case in the choice set makes relative changes the more appropriate tool for considering substantive significance. An authoring judge who is perfectly aligned with a precedent is 23% more likely to cite the precedent than a judge who is the maximum distance away from a precedent. Even a less dramatic shift from one standard deviation above the mean of *Ideological Distance* to one standard deviation

Figure 3.3: Marginal Effects of *Ideological Distance* and *Relative Distance* on Citation: These graphs provide the marginal effect of a one standard deviation increase in *Ideological Distance* and *Relative Distance* in turn while holding all other variables at their median. Marginal effects are calculated by estimating the predicted probability of citation at a baseline value of the variable in question and subtracting that from the predicted probability of citation when the value of the variable is one standard deviation higher. The shaded regions around each line delineate the 95% confidence intervals.

below the mean (i.e., closer to the precedent) increases the predicted probability of citation by 10%.

The right panel of Figure 3.3 shows the marginal effect of *Relative Distance* when *Ideological Distance* is set at its median. Although the 95% confidence interval comes very close to zero, it does not actually include zero for any value of *Relative Distance*. Moreover, further exploration reveals that the marginal effects of all values of *Relative Distance* are statistically significant for values of *Ideological Distance* equal to or less than 0.33 which includes for 52% of observations. The predicted probabilities plotted in the right panel of Figure 3.4 show that the size of this impact is similar to that of *Ideological Distance*. When the author's *Relative Distance* to the precedent is one
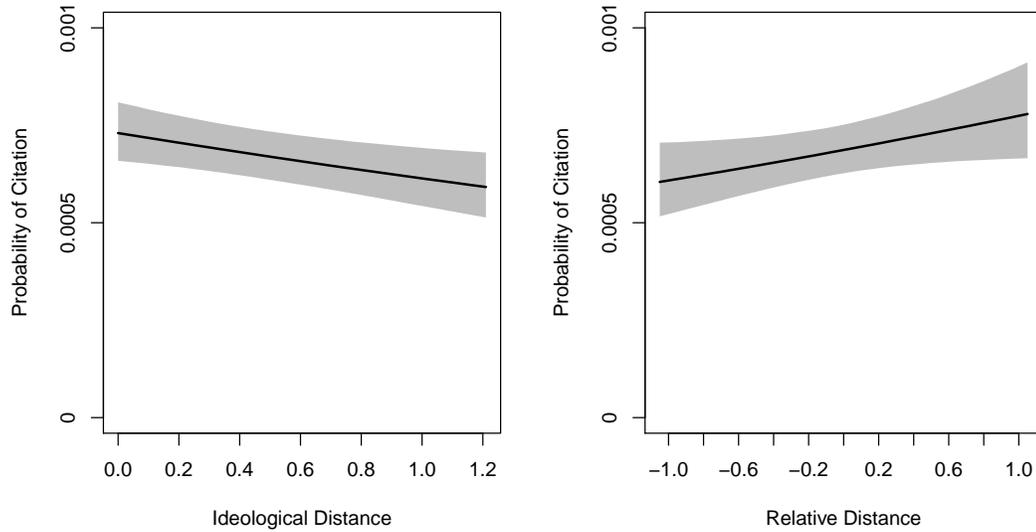
Figure 3.4: Effect of *Ideological Distance* and *Relative Distance* on Citation: This graph provides the predicted probability of citation and 95% confidence intervals at different values of *Ideological Distance* and *Relative Distance* in turn. All other variables are held at their median.

standard deviation above the mean, they are 7% more likely to cite the precedent than when *Relative Distance* is one standard deviation below the mean.

The control variables perform very similarly in the citation model here as they do in the analogous citation model in Chapter 2. Precedents which are more similar to the treatment case, precedents authored by the same judge as the treatment case, and precedents which have been cited more frequently are more likely to be cited. Per curiam precedents are less likely to be cited. When a judge has a greater number of precedents to choose from the probability of any given case being cited decreases. Finally, the curvilinear effect of age follows the anticipated pattern. One curious result observed in Chapter 2 persists. Greater length of the precedent actually decreases the probability of citation.

The next model explores the decision regarding whether to positively treat a cited precedent. These results are presented in Figure 3.5. Although the coefficients

for each of the constitutive terms are not statistically significant when the other equals zero, the interaction term is both statistically significant and in the expected direction. The marginal effects in Figure 3.6 show that both *Ideological Distance* and *Relative Distance* also fail to gain statistical significance when the other (and all control variables) are set at their median value.
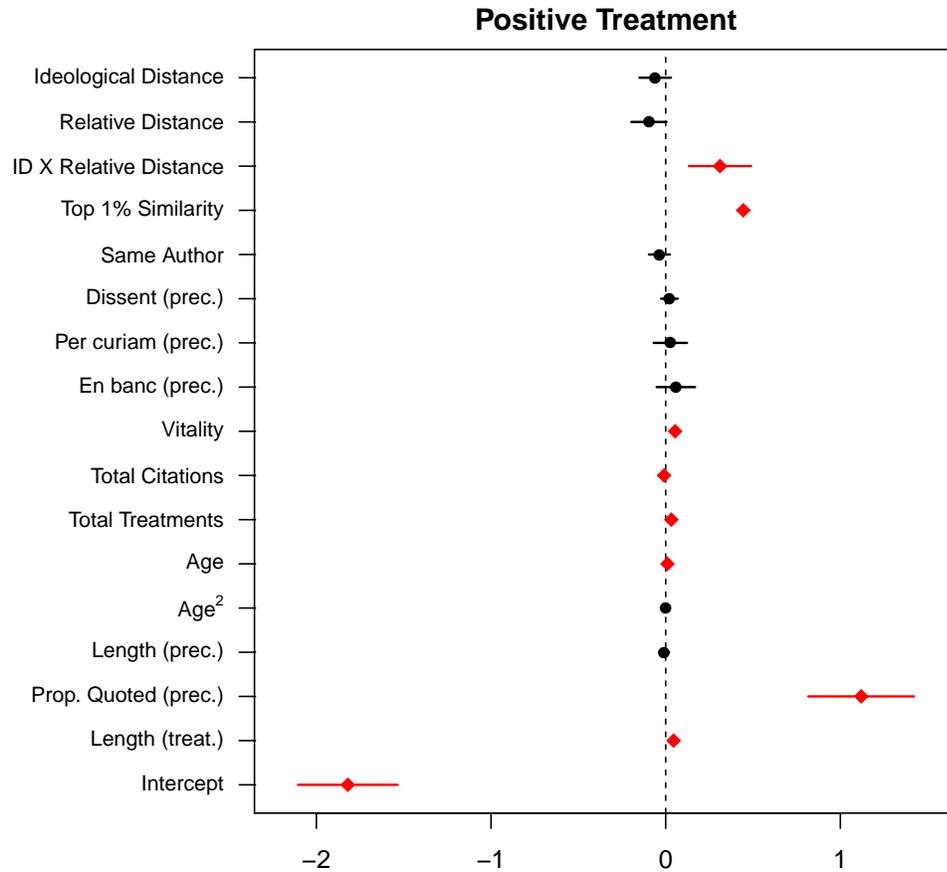
Figure 3.5: Positive Treatment Model: Probit regression estimates of the effect of *Ideological Distance*, *Relative Distance*, their interaction, and a range of control variables on the decision to treat a cited precedent positively (or not). Dots and diamonds indicate point estimates. Bars indicate 95% confidence intervals. Diamonds (instead of dots) and bars in red denote that a coefficient has a p-value less than 0.05. The full regression table is presented in the Appendix.

Marginal Effect of a 1 S.D. increase in Ideological Distance

Marginal Effect of a 1 S.D. increase in Relative Distance
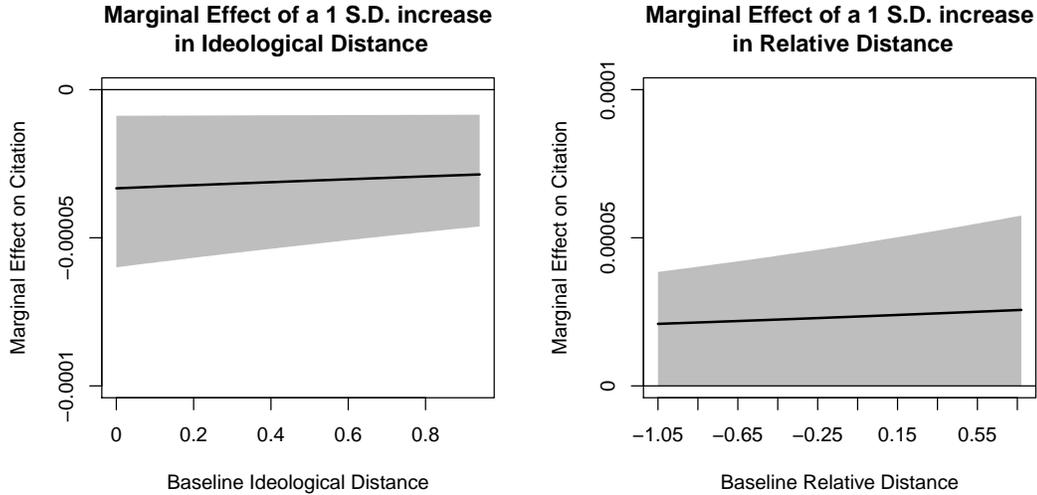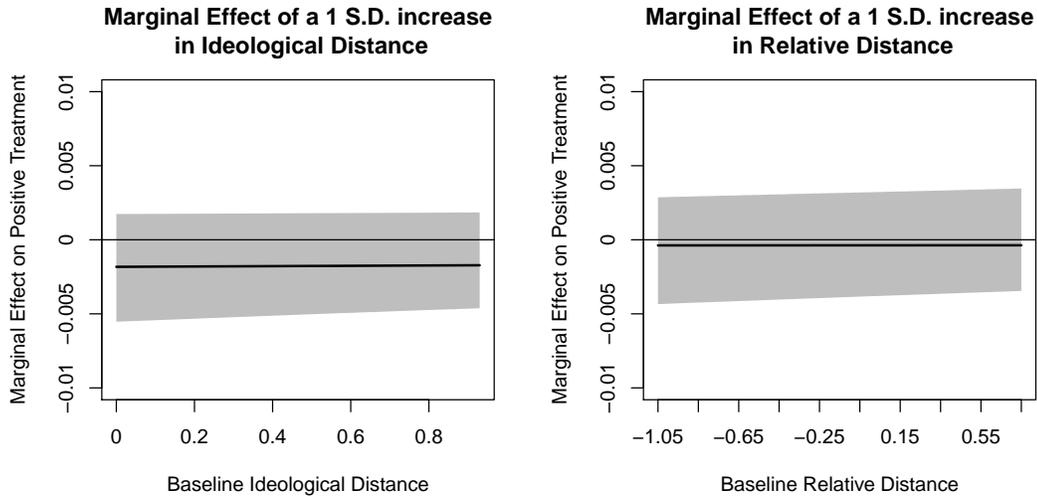
Figure 3.6: Marginal Effects of *Ideological Distance* and *Relative Distance* on Positive Treatment: These graphs provide the marginal effect of a one standard deviation increase in *Ideological Distance* and *Relative Distance* in turn while holding all other variables at their median. Marginal effects are calculated by estimating the predicted probability of positive treatment at a baseline value of the variable in question and subtracting that from the predicted probability of positive treatment when the value of the variable is one standard deviation higher. The shaded regions around each line delineate the 95% confidence intervals.

Figure 3.7 depicts the predicted probabilities of positive treatment over the entire range of both *Ideological Distance* and *Relative Distance* from two slightly different angles. The interaction effect causes the effect of Ideological Distance to change signs as *Relative Distance* increases. When an author is closer to a precedent relative to the circuit (towards the front of the figure) *Ideological Distance* has the usual negative effect. Conversely, when the author is more distant from the precedent relative to the circuit (towards the back of the figure), the author is more likely to positively treat a precedent the farther away it is from their own ideal point. For further illustration of the interaction effect, I display the change in predicted probability created by a change in one variable from one standard deviation below the median to one standard

deviation above the median for all values of the other variable (and the associated 95% confidence intervals). Figure 3.8 graphs the predicted probability of positive treatment at the high value minus the predicted probability of positive treatment at the low value for each variable over the entire range of the other constitutive term. The results reveal the ranges over which a two standard deviation change is statistically different from zero.

The left panel of Figure 3.8 demonstrates that when *Ideological Distance* is greater than or equal to 0.6, *Relative Distance* has a significant, positive impact on positive treatment. This region includes 21% of the data. The information in the right panel of Figure 3.8 is particularly revealing. As expected when judges strategically anticipate en banc review, the effect of *Ideological Distance* is substantially influenced by the relative position of the circuit. For values of *Relative Distance* less than or equal to -0.14, the effect of *Ideological Distance* is negative and has a bigger effect as *Relative Distance* decreases. This range of the data accounts for 18% of observations. For mid-range values of *Relative Distance*, the author's distance to the precedent does not have a statistically significant impact on the decision to positively treat a cited precedent. Once the value of *Relative Distance* is equal to or greater than 0.55, the effect of *Ideological Distance* is significant again, but has a positive effect. Within this segment of the data (8% of all observations), an author who is farther from a precedent is actually more likely to positively treat it. This effect size gets larger as *Relative Distance* increases.

Since the options a judge faces in the treatment models include only cited cases, it is feasible to consider effect size in absolute terms as well as relative terms. When an author is the maximum distance from a precedent, the change in *Relative Distance* from one standard deviation below to above the median increases the predicted probability of positive treatment from 6.1% to 8.4%. Similarly, at the maximum value
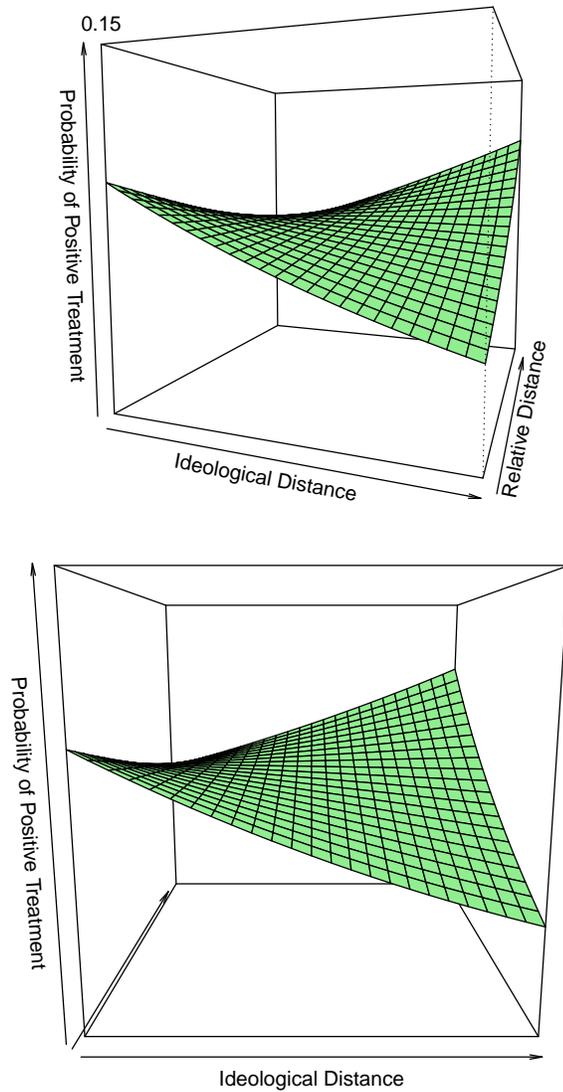
Figure 3.7: Three Dimensional Predicted Probabilities-Positive Treatment: This graph provide the predicted probability of positive treatment at different values of *Ideological Distance* and *Relative Distance* presented at two slightly different viewing angles. All other variables are held at their median.

of *Relative Distance*, a change in *Ideological Distance* from one standard deviation below to above the median results in an increase in the predicted probability from 6.6% to 8.7%. At the other end of the scale, the same change in *Ideological Distance* results in a decrease in the probability of positive treatment from 9.6% to 6.4%.
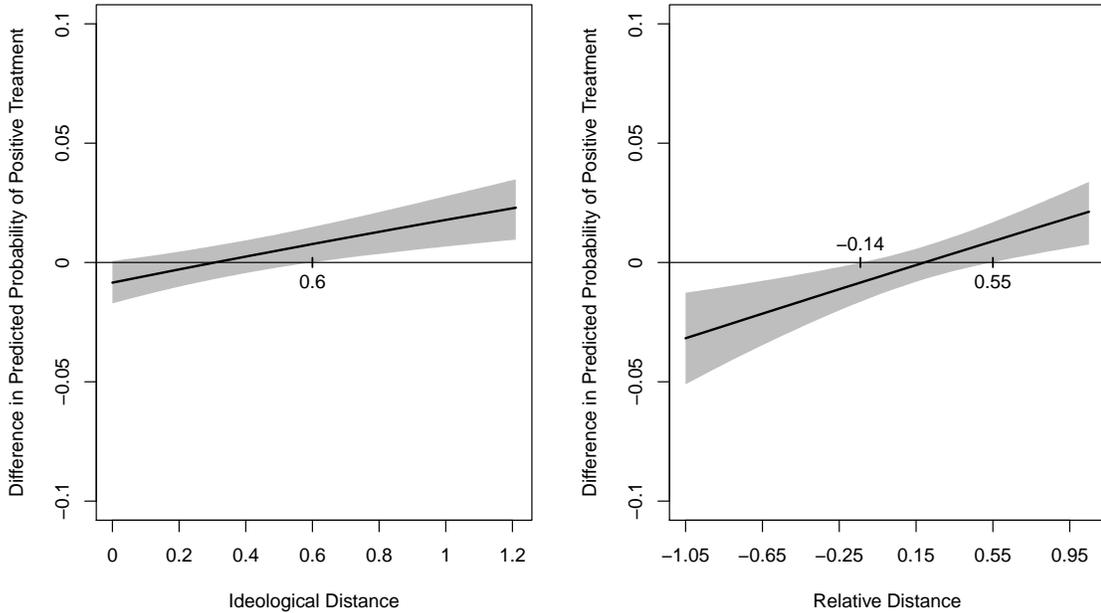
Figure 3.8: Difference in Predicted Probability of Positive Treatment: The left panel provides the difference in predicted probability of positive treatment when *Relative Distance* is one standard deviation above the median and one standard deviation below the median for all possible values of *Ideological Distance*. The right panel provides the difference in predicted probability of positive treatment when *Ideological Distance* is one standard deviation above and below the median for all possible values of *Relative Distance*. The shaded regions depict the 95% confidence intervals. All control variables are held at their median.

The control variables in the positive treatment model perform much the same as they do in the analogous model in Chapter 2. While several are not statistically significant, those which are tend to perform as expected. Cited precedents which have a similarity score in the top one percent, have greater vitality, have been treated more frequently, and are older are all more likely to be positively treated. A precedent being cited more frequently actually decreases the probability of positive treatment slightly. When the opinion in a treatment case is longer, positive treatment is more likely. Most notably, an indicator of opinion quality, the proportion of text in the precedent

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which is quoted, increases the probability of positive treatment. This suggests that authoring judges are more willing to expand the scope of higher quality precedents.

Now I move from examining the decision to expand the scope of a precedent to the decision to restrict or narrow the scope of a precedent. Figure 3.9 contains the starting point for this analysis, the model results for the negative treatment model. Both *Ideological Distance* and *Relative Distance* are statistically significant and in the expected direction, but the interaction term is not significant. When the author and circuit are equidistant from a precedent, *Ideological Distance* has a positive effect on negative treatment. Authoring judges who are more distant from a precedent are more likely to treat it negatively. When an author is perfectly aligned with a precedent, she is less likely to negatively treat a precedent when the circuit is closer to it. Since neither of these situations is terribly common, I proceed to examine the marginal effects and predicted probabilities of each of the constitutive terms when the other is held at its median.

The marginal effects plotted in Figure 3.10 show that the effects of both constitutive terms observed when the other equals zero also hold true then the other is set at its median. In fact, both effects are consistent over most of the range of the data. The marginal effect of a one standard deviation increase in *Ideological Distance* is positive and significant for all values of *Relative Distance* which are greater than or equal to -0.38 which includes 93% of the data. *Relative Distance* is negative and significant for all values of *Ideological Distance* equal to or less than 0.55 which describes 75% of the data.

Not only are the marginal effects of the constitutive terms statistically significant in most cases, but the effect sizes are substantively significant as well. Figure 3.11 graphs the predicted probability of negative treatment over the range of each constitutive term while holding the other (and all control variables) at their median. An
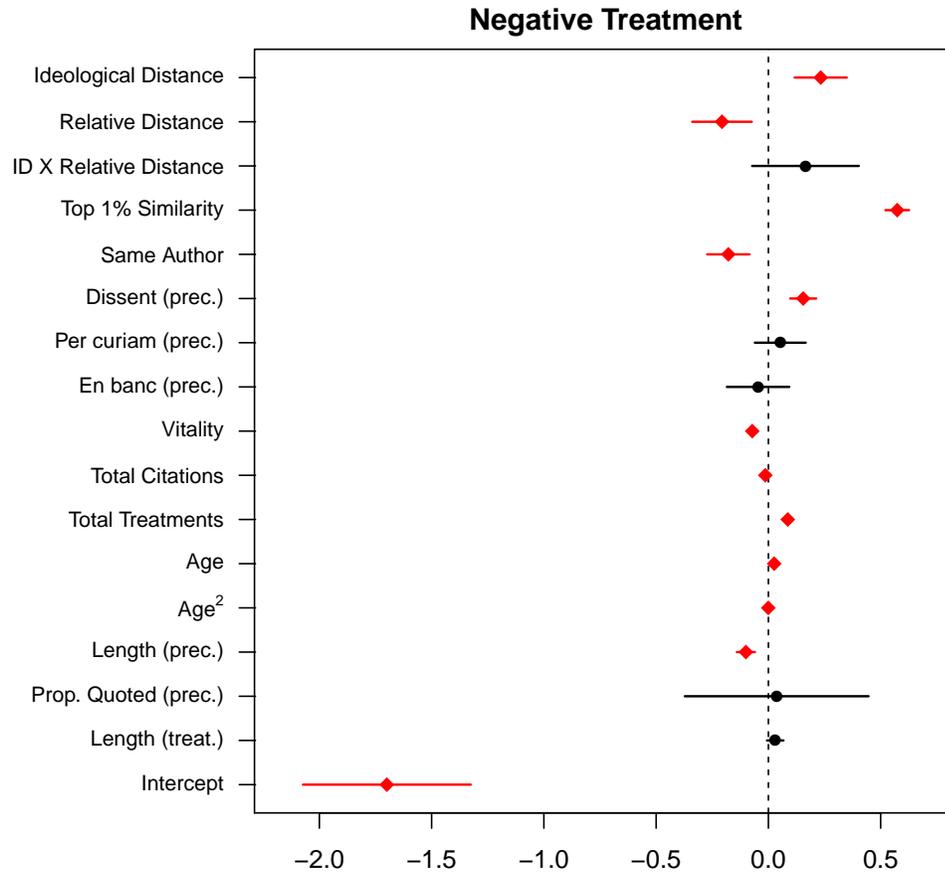
Figure 3.9: Negative Treatment Model: Probit regression estimates of the effect of *Ideological Distance*, *Relative Distance*, their interaction, and a range of control variables on the decision to treat a cited precedent negatively (or not). Dots and diamonds indicate point estimates. Bars indicate 95% confidence intervals. Diamonds (instead of dots) and bars in red denote that a coefficient has a p-value less than 0.05. The full regression table is presented in the Appendix.

authoring judge who is aligned with a precedent only negatively treats a precedent 1.3% of the time, while an author the maximum distance from a precedent more than doubles that rate, resorting to negative treatment for 2.7% of cited precedents. *Relative Distance* has a similar size effect. At the minimum value of *Relative Distance* the

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Marginal Effect of a 1 S.D. Increase in Ideological Distance

Marginal Effect of a 1 S.D. Increase in Relative Distance
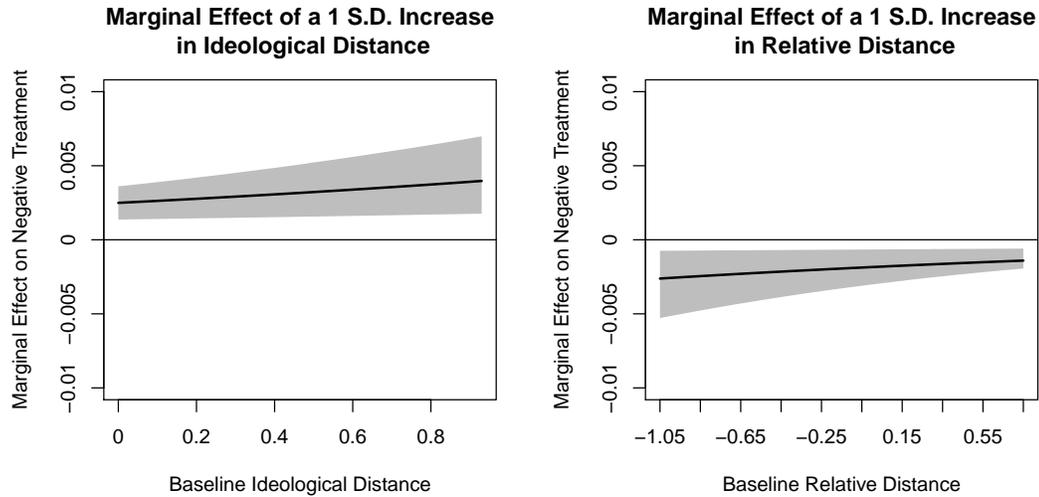
Figure 3.10: Marginal Effects of *Ideological Distance* and *Relative Distance* on Negative Treatment: These graphs provide the marginal effect of a one standard deviation increase in *Ideological Distance* and *Relative Distance* in turn while holding all other variables at their median. Marginal effects are calculated by estimating the predicted probability of negative treatment at a baseline value of the variable in question and subtracting that from the predicted probability of negative treatment when the value of the variable is one standard deviation higher. The shaded regions around each line delineate the 95% confidence intervals.

predicted probability is 2.4% and it drops to 1.1% at the maximum value of *Relative Distance*.

In the negative treatment model most of the control variables are statistically significant, just as they were in the negative treatment model in Chapter 2. Treatment case-precedent pairs which are very similar are more likely to be treated negatively, just as they are more likely to be treated positively. Also, precedents with a dissent, precedents which have been treated more frequently, and older precedents are more likely to be treated negatively. Precedents written by the same author, precedents with greater *Vitality*, more heavily cited precedents, and longer precedents are all less likely to be negatively treated.
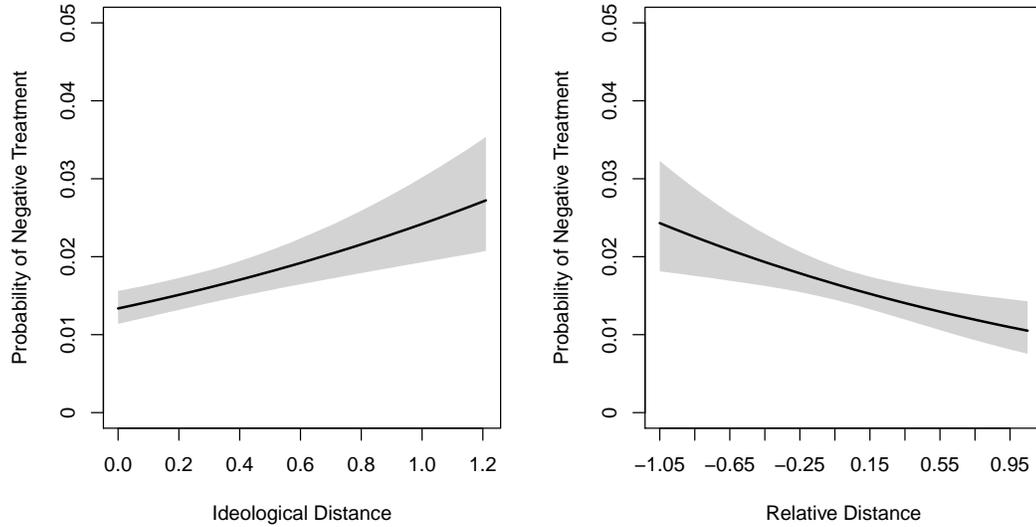
Figure 3.11: Effect of *Ideological Distance* and *Relative Distance* on Negative Treatment: This graph provides the predicted probability of negative treatment and 95% confidence intervals at different values of *Ideological Distance* and *Relative Distance* in turn. All other variables are held at their median.

3.5 Discussion

This study of the selection and use of binding circuit precedents in the U.S. Courts of Appeals reveals that the ideological preferences of the entire circuit influence the actions of panel opinion authors. For the decision of whether to positively treat a cited precedent, the impact of an author's ideology is conditioned on the location of the circuit median. When the circuit is farther away from a precedent than the author (*Relative Distance* ≤ -0.14), authors are less likely to positively treat precedents which are farther away from them and this ideological effect is amplified when the circuit is more distant. Conversely, when the author is far away from a precedent relative to the circuit (*Relative Distance* ≥ .55), greater distance between the author and a precedent actually increases the predicted probability of positive treatment. The latter finding is consistent with authors expecting their circuit to apply heightened scrutiny where there is greater ideological extremism. In other words, authors are

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particularly careful to establish their adherence to stare decisis by positively treating precedents which are farthest away from them when the circuit is most protective (i.e., closer to the precedent). These patterns are consistent with the hypothesis that judges strategically anticipate en banc review by moderating the extent to which they rely on their own ideology when a circuit is more likely to be protective of a precedent (because it reflects the circuit's own preferences).

A more straightforward form strategic behavior occurs when the author of a panel opinion makes a decision which is directly influenced by the location of the circuit as a whole. The author's citation-related decisions vary based on circuit preference even when the overall impact of an author's ideology may not depend on the location of the circuit median. In all three models, the marginal effect of *Relative Distance* has such an influence on authoring judges for at least some values of *Ideological Distance.*

When an author is deciding which precedents to cite, the marginal effect of *Relative Distance* is significant and positive when the author is an average distance away from the precedent or closer (*Ideological Distance* ≤ 0.33). Under these circumstances, a judge is more likely to cite a case when the circuit is closer to the precedent (holding the judge's distance to the precedent constant). For the decision to positively treat, *Relative Distance* also has a significant positive effect, but only when the author is fairly far away from the precedent (*Ideological Distance* ≥ 0.6). Finally, for almost all values of *Ideological Distance* (those less than or equal to 0.55), *Relative Distance* has a negative marginal effect on negative treatment. Authors are less likely to negatively treat precedents with which the circuit is more closely aligned.

The influential role of the circuit median in panel citation and treatment decisions throws important light on the evidence of legal compliance uncovered in the previous chapter. Taken together, these findings indicate what while circuit judges do show respect for the doctrine of stare decisis, that respect is more pronounced when the

circuit as a whole is ideologically motivated to enforce that norm by exercising its power of en banc review. Examining the use of citations has provided leverage on these important questions. Turning to more granular decisions than case outcomes has provided evidence of strategic behavior even though the topic has generated mixed results in the past.

It is important to bear in mind that evidence of strategic considerations playing a role in citation and treatment of precedent does not mean that strategy is the *only* driving force behind legal constraint. As scholars have noted, given the extent of professional socialization, judges may very well obtain significant utility from following the law independent of the policy outcomes generated (Cross et al., 2010; Kim, 2006). While this study looks for, and finds, evidence of strategic anticipation of en banc review in the way circuit judges use precedent, other possible explanations for legal constraint may very well play a role as well.

3.6   Appendix

	Coefficient	Standard Error	p-value
Intercept	-0.934^*	0.094	0.000
Ideological Distance	-0.049^*	0.019	0.009
Relative Distance	0.044^*	0.021	0.040
ID X RD	-0.028	0.031	0.371
Top 1% Similarity Score	1.747^*	0.009	0.000
Same Author	0.121^*	0.011	0.000
Dissent	0.000	0.012	0.983
Per Curiam	-0.160^*	0.019	0.000
En Banc	-0.067	0.045	0.139
Vitality	0.006	0.006	0.333
Total Cites	0.020^*	0.005	0.000
Total Treatments	0.009	0.012	0.438
Age	-0.059^*	0.003	0.000
Age^2	0.001^*	0.000	0.000
Precedent Length	-0.096^*	0.008	0.000
Proportion Quoted	0.077	0.072	0.286
Treatment case length	-0.001	0.004	0.818
Size of Choice Set	-0.113^*	0.010	0.000
Caseload	0.000	0.000	0.320
N		9,607,742	

Table 3.3: Citation Model: Probit regression estimates of the effect of *Ideological Distance*, *Relative Distance*, their interaction, and a range of control variables on the decision of whether to cite a precedent. The reported standard errors are robust standard errors which are clustered on the precedent and * denotes a p-value less than 0.05.

	Coefficient	Standard Error	p-value
Intercept	-1.820^*	0.146	0.000
Ideological Distance	-0.060	0.046	0.193
Relative Distance	-0.096	0.051	0.063
ID X RD	0.311^*	0.091	0.001
Top 1% Similarity Score	0.445^*	0.018	0.000
Same Author	-0.036	0.031	0.239
Dissent	0.021	0.024	0.387
Per Curiam	0.027	0.049	0.584
En Banc	0.058	0.056	0.299
Vitality	0.054^*	0.006	0.000
Total Cites	-0.009^*	0.001	0.000
Total Treatments	0.032^*	0.006	0.000
Age	0.010^*	0.004	0.004
Age^2	0.000	0.000	0.978
Precedent Length	-0.009	0.015	0.531
Proportion Quoted	1.119^*	0.154	0.000
Treatment case length	0.045^*	0.014	0.001
N		39,498	

Table 3.4: Positive Treatment Model: Probit regression estimates of the effect of *Ideological Distance*, *Relative Distance*, their interaction, and a range of control variables on the decision to treat a cited precedent positively (or not). The reported standard errors are robust standard errors which are clustered on the precedent and * denotes a p-value less than 0.05.

	Coefficient	Standard Error	p-value
Intercept	-1.699^*	0.191	0.000
Ideological Distance	0.233^*	0.059	0.000
Relative Distance	-0.207^*	0.068	0.002
ID X RD	0.165	0.121	0.173
Top 1% Similarity Score	0.574^*	0.027	0.000
Same Author	-0.178^*	0.048	0.000
Dissent	0.155^*	0.030	0.000
Per Curiam	0.053	0.058	0.360
En Banc	-0.046	0.071	0.518
Vitality	-0.072^*	0.010	0.000
Total Cites	-0.014^*	0.004	0.002
Total Treatments	0.086^*	0.012	0.000
Age	0.026^*	0.006	0.000
Age^2	0.000^*	0.000	0.047
Precedent Length	-0.100^*	0.021	0.000
Proportion Quoted	0.037	0.209	0.860
Treatment case length	0.031	0.019	0.103
N		39,498	

Table 3.5: Negative Treatment Model: Probit regression estimates of the effect of *Ideological Distance*, *Relative Distance*, their interaction, and a range of control variables on the decision to treat a cited precedent negatively (or not). The reported standard errors are robust standard errors which are clustered on the precedent and * denotes a p-value less than 0.05.

Chapter 4

The Impact of Federal Courts on State Policy Diffusion

The influence U.S. Courts of Appeals exercise over the development of law is not only widespread, but varied. In this chapter I turn away from the direct examination of how circuit judges impact and are influenced by circuit case law to consider the role both circuit courts and the U.S. Supreme Court play in the formation and diffusion of state policies. The federal constitution sets minimum requirements for protections all state governments must provide. Federal judges are typically the final arbiters of whether state legislation complies with the U.S. Constitution. I explore the extent to which state legislators react to such rulings when considering the adoption and formulation of policies.

There is evidence that the federal government influences state policy diffusion (Allen, Pettus and Haider-Markel, 2004; Berry and Berry, 1990; Grossback, Nicholson-Crotty and Peterson, 2004; Karch, 2012; Roh and Haider-Markel, 2003; Welch and Thompson, 1980) and also that both state supreme courts (Langer and Brace, 2005)

and the U.S. Supreme Court (Patton, 2007) play a role in this process. These findings indicate that the influence of the federal government may also be brought to bear through the circuit courts. States face considerable federal restrictions on the policies they are permitted to implement (Welch and Thompson, 1980: 717). There is an appeal as a matter of right to the federal circuit courts (Giles et al., 2007: 451), so any challenge to a state policy in federal court has a substantial chance of being ultimately resolved by a circuit court. Since there are multiple states in each federal circuit, a state may learn valuable information from the rulings on a policy from another state in its own circuit. Consequently, a thorough examination of the effect of federal courts on state policy diffusion requires looking at circuit courts as well as the Supreme Court.

Important methodological advances in recent policy diffusion literature enable more nuanced analysis of the mechanisms behind policy diffusion (Boehmke, 2009a; Karch, 2007b). The empirical analysis in this chapter utilizes a dyadic approach first deployed in the policy diffusion context by Volden (2006). The unit of analysis is a dyad of states in each year with the dyad composed of a potential adopter, $State_i$, and a previous adopter, $State_j$. This approach makes it possible to model not only the characteristics of the potential adopter, but also characteristics of each previous adopter, characteristics of the previously adopted policy, and aspects of the relationship between the two states. When a previously adopted policy has been examined by the federal courts, the results provide potential adopters with valuable information about risks associated with adopting such a policy. The dyadic data structure allows me to evaluate the impact of such information while accounting for the source and type of any relevant federal court rulings. The source of rulings is important because Supreme Court rulings should have a similar strong effect nationwide while circuit rulings provide the most information to states located within that circuit. Whether

a policy is ruled constitutional or unconstitutional is similarly crucial to how a ruling influences policy diffusion.

In addition to taking a dyadic approach, I follow the example of Brinks and Coppedge (2006) and perform a two stage analysis to examine the influence of federal court rulings on both the adoption and content of a policy. First, I explore a potential adopter's decision regarding whether to adopt or amend a policy previously adopted by $State_j$ in a given year, t. Second, if $State_i$ does legislate in year t, I model the extent to which the text of its new statute overlaps with the text of previously adopted policies. A particularly efficient way for states to utilize the information in court rulings is to directly adopt statutory language the courts have approved or explicitly avoid language which has been struck down. I find evidence that Supreme Court rulings that a previously adopted statute is constitutional influence the adoption of a policy, and circuit court rulings that a previously adopted policy is unconstitutional can significantly decrease the extent of policy overlap.

This chapter builds on recent policy diffusion work examining the mechanisms behind diffusion by looking beyond the effects of the mere existence of preexisting policies to evaluate the impact of such policies conditional on their success in federal courts. While measuring the success of public policies is often a very difficult task, how well a policy fares in terms of federal constitutional challenges lends itself to empirical examination which helps us better understand the role of an important multi-jurisdictional actor, the federal government. In addition to shedding light on the role federal courts play in how and why state policies diffuse, this study illustrates a technique for evaluating similarity in the content of policies which can be applied to answer a wide range of other questions about the mechanisms behind policy diffusion. This novel measure of policy content overlap holds particular promise because it

provides a more direct measurement of the influence of previous adopters than simply looking at the binary choice over whether to adopt a policy.

4.1 State Policy Diffusion and Federal Courts

The states have long been described as laboratories of democracy where innovative public policies can be developed and tested (see, e.g. Boeckelman, 1992). For decades scholars have worked towards developing a better understanding of the forces which shape and drive this process (Walker, 1969; Gray, 1973; Berry and Berry, 1990; Allen, Pettus and Haider-Markel, 2004; Berry and Berry, 2007; Karch, 2007b). In 1969 Walker posited that states have an important influence on each other in the process of policy innovation resulting in a pattern of proliferation of policies across states. Since then many scholars have presented evidence that diffusion plays a role in state policy innovation (Allen, Pettus and Haider-Markel, 2004; Berry and Berry, 1990; Bouché and Volden, 2011; Gray, 1973; Mooney and Lee, 1995). Recent scholarly work has increasingly investigated the underlying mechanisms which drive observed patterns of policy diffusion including learning, competition, socialization, and coercion (Boehmke and Witmer, 2004; Glick, 2011; Graham, Shipan and Volden, 2008; Karch, 2007b; Pacheco, 2012; Shipan and Volden, 2008).

One of the foundational observations of early policy diffusion work was that a policy is more likely to diffuse to neighboring[1] states (Walker, 1969). For years scholars uncovered evidence that previous adoptions of several policy types increase the likelihood nearby states would adopt similar policies (Walker, 1969; Gray, 1973 (education, welfare, and civil rights); Berry and Berry, 1990 (lotteries); Mooney and Lee,

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[1] "Neighboring" states are typically operationalized as those which are geographically contiguous (Berry and Berry, 2007: 244).

1995 (abortion regulation); Andrews, 2000 (electricity deregulation); Allen, Pettus and Haider-Markel, 2004 (partial birth abortion bans, truth-in-sentencing laws, and hate crime laws)). One theoretical reason for such neighbor effects is that states learn from one another's experiences (Berry and Berry, 2007: 225). The risk of policy failure is one of the obstacles to innovation, and if there are other similar states that have enacted similar policies, that provides information which reduces the risk of innovation (Mooney and Lee, 1995: 605). Available examples of a policy's impact in another state are a relatively low-cost way of ascertaining information sufficient to get a policy enacted (Berry and Berry, 1990: 84). While this information could be obtained from any previous adopter, scholars explain neighbor effects by theorizing that state policymakers are more likely to be aware of the details of policy implementation in their neighbor states. Moreover, the experience of a neighboring state in enacting and implementing a policy might very well provide more relevant information than the experience of a state in a different part of the country.

Current work on policy diffusion has taken important steps toward understanding both when and why policymakers might be influenced by their neighbors rather than simply treating neighbor effects as single unconditional force (Baybeck, Berry and Siegel, 2011; Boehmke and Witmer, 2004; Bouché and Volden, 2011; Gilardi, Füglister and Luyet, 2009). Much of this progress has been made possible due to methodological advancements. Volden (2006) incorporated a dyadic approach used in the study of international relations to study diffusion by looking at pairs of states rather than just the decision of a single state. This groundbreaking approach paved the way to study policy convergence between two states while accounting for features of both states individually and their relationship.

Studying dyads of states has enabled scholars to investigate a range of new questions generated by the traditional understanding of the reasons for neighbor effects

(Gilardi and Füglister, 2008; Strebel and Widmer, 2012). The theory that learning drives policy diffusion prompted scholars to incorporate measures of policy success into their analyses. As expected, policies which are more successful are more likely to be emulated (Gilardi, Füglister and Luyet, 2009; Gilardi and Füglister, 2008; Shipan and Volden, 2010; Volden, 2006). Also, states are more likely to adopt policies which converge with the policy in another state when the two states are more similar ideologically (Baybeck, Berry and Siegel, 2011; Gilardi, 2010; Grossback, Nicholson-Crotty and Peterson, 2004). While both of these findings are suggested by some of the original explanations for neighbor effects, neither exclusively predicts that only neighbors will have such an effect.

This chapter focuses on another aspect of policy diffusion which may contribute to the appearance of neighbor effects-the influence of federal courts. While the effect neighbors have on state policy diffusion has received considerable attention in the scholarly literature, the impact of courts in general has received less attention. The impact of federal circuit courts in particular has not been addressed. I examine the influence of circuit courts as well as the Supreme Court. Incorporating circuit courts into the analysis is important for two reasons. First, while the circuit courts are studied considerably less than the Supreme Court, they exert an important influence due to the large number of cases in which they have the final word (Boyd and Spriggs, 2009; Cross, 2007; Hettinger, Lindquist and Martinek, 2006; Klein, 2002; Songer, Sheehan and Haire, 2000). Considering the extent to which this significant, but often overlooked, force might be influencing state legislators is important to a more complete and nuanced understanding of the way state governments operate within the context of federalism. The second reason including circuit courts is important arises due to the fact that they are arranged by geographical jurisdiction over groups of con-

tiguous states as illustrated in Figure 4.1. Because of this configuration, observations of regional diffusion of policy potentially conflate neighbor effects and circuit effects.
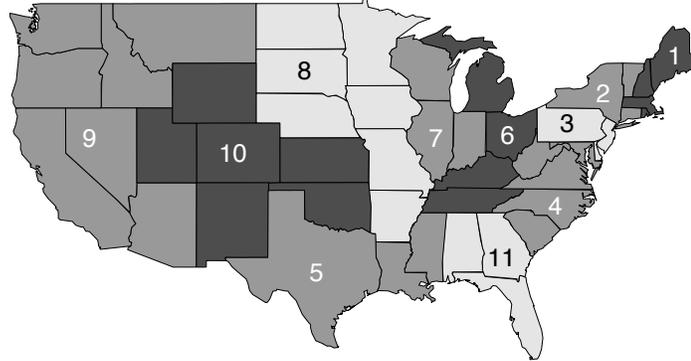
Figure 4.1: Map of the U.S. Courts of Appeals

Scholars have examined the role of various multi-jurisdictional actors in policy diffusion including the role of the federal government (Karch, 2012; Strebel, 2011; Welch and Thompson, 1980). Various branches of the federal government and various levels of those branches may exert such an influence (Berry and Berry, 1990; Boushey, 2012; Grossback, Nicholson-Crotty and Peterson, 2004; Karch, 2012, 2007b; Roh and Haider-Markel, 2003). The primary source of influence the federal government exerts over states is the Supremacy Clause of the United States Constitution. No state has the power to pass laws which contravene either the federal constitution or federal statutes or treaties. Both the power and scope of the federal government have expanded over the course of the nation's history with the result that today states face considerable federal restrictions on the policies they are permitted to implement (Welch and Thompson, 1980: 717). Moreover, the influence of the federal government extends beyond the effect of the compulsory supremacy of federal law. Even in those remaining areas where the federal government does not directly restrict state

policymaking, it is still able to effectively influence state policy (Allen, Pettus and Haider-Markel, 2004). Welch and Thompson (1980) examine this sort of indirect influence by the federal government and find evidence that "incentives provided by the federal government do stimulate the diffusion of policies through the states" (727). While there are some policy areas the states control largely without interference by the federal government (Grossback, Nicholson-Crotty and Peterson, 2004), the number of ways the federal government can and does interject itself into matters of state policy lends considerable significance to questions related to federal influences over state policy.

Federal courts are frequently the mechanism through which the supremacy of federal law is established. Moreover, research shows that the impact of courts is not limited to the direct impact of specific court decisions after a policy has been enacted, challenged, and litigated (Langer and Brace, 2005: 317). There is evidence that courts play a role at the stage of policy consideration and adoption; state policymakers take into account the possibility that a policy might be stuck down. Patton (2007) studied the effect of the U.S. Supreme Court and found evidence that policies which the Court had indicated were on shaky constitutional ground were less likely to be adopted by states than policies the Court had not addressed at all or had ruled constitutional. Another study found evidence that state policymakers rely on knowledge about the ideological makeup of their state supreme court and the likelihood a statute will be challenged in that forum to inform their decision about whether or not to adopt a particular policy (Langer and Brace, 2005: 318).

There are good reasons why state policymakers pay attention to the proclivities of the courts which might rule on a policy after it is adopted. The most obvious is the possibility that a policy will be stuck down. Whether state legislators' motivations for enacting a policy are substantive (they truly believe that the policy is what is

best for the state) or political (they believe supporting the policy will help get them re-elected), neither goal is served very effectively if the policy is struck down by the courts. Enacting policies takes considerable expenditure of limited political resources (Boushey, 2010; Karch, 2012). Therefore, legislators have incentive to avoid adopting policies that are at a high risk of being struck down.[2] In order to husband scarce resources, state policymakers are motivated to anticipate how the courts are likely to treat a potential policy (Langer and Brace, 2005: 319).

State legislators should have just as much motivation to pay attention to the potential actions of their federal circuit court as they do the U.S. Supreme Court or their state supreme court. All the concerns about a policy being overturned remain the same, plus there are other factors at work in the dynamic between states and federal circuit courts which provide additional incentives. If there is a potential challenge to a policy based on federal law, the chances that a challenge will make it all the way to the U.S. Supreme Court are fairly low. But if any person or entity challenges the policy in federal court, there is an appeal as of right to the federal circuit court (Giles et al., 2007: 451) which means there is a very good chance the federal circuit court will weigh in on the issue. Langer and Brace (2005) show in their work on state supreme courts that the attention state policymakers pay to a court is greater if it is more likely the court will rule on a policy. Consequently, it makes sense to expect that state policymakers pay attention to their federal circuit court's likely treatment of a policy.

The institutional structure of the circuit courts provides a unique opportunity for state legislators to gain information about how the court would rule on a policy before they adopt it. This dynamic arises from the fact that each federal circuit

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[2]This is not to say that there are never equally or more powerful incentives from other quarters which would militate in favor of adopting policies in spite of a high risk of being stuck down.

contains multiple states. A ruling issued by a federal circuit court is binding on all future cases that arise in that circuit (Cross, 2003; Klein, 2002; Landes, Lessig and Solimine, 1998; Lee III, 2003; Lindquist and Cross, 2005; Merritt, 1990; O'Hara, 1993). Once a policy has been adopted by one state in a federal circuit, any ruling on that policy will be applicable to the other states in that circuit (it they adopt a sufficiently similar policy). This affects the cost of adopting a policy. After a policy has been adopted by another state in the same circuit, any resulting litigation will reduce the risk and uncertainty for subsequent adopters. This dynamic is not present when policymakers consider how their state supreme court would rule because they do not have the option to sit back and wait to see how the court would rule on a particular type of policy.

4.2   A Theory of Federal Court Effects

The idea that political subdivisions within a federal system of government act as policy laboratories suggests that the effects of implementing different kinds of polices in one context provides valuable information for future policy decisions (Karch, 2007a; Volden, 2006). While scholars have frequently opined that states learn from each other's experiences, factoring actual success or failure of previously adopted policies into empirical analysis of policy diffusion has been undertaken in only a handful of recent studies (Gilardi, Füglister and Luyet, 2009; Gilardi and Füglister, 2008; Neumayer and Plümper, 2012; Volden, 2006). This element is key to understanding learning as a mechanism of policy diffusion. The mere adoption of a policy in another state is not necessarily enough to expect other states to adopt it as well. Valuable information comes from observing how well a policy works.

The federal courts have the potential to effect the success or failure of certain state policies in a unique way which is inherently related to, although not synonymous with, the policy's ultimate success in terms of policymakers' goals. When a federal court upholds a state statute as constitutional, the law meets a critical threshold which is a necessary precondition for the policy to be a success down the road. In other words, the policy's survival (of federal scrutiny) is key to there even being a possibility of success. On the other hand, a court ruling that a state law is unconstitutional is a direct indication of policy failure. Even if the court strikes down only a part or particular application of a law, that is a concrete indication that policymakers' intentions and efforts were thwarted.

When a potential adopter, $State_i$, is considering the information available about a policy previously enacted by $State_j$, federal court rulings either declaring the statute, $Policy_j$, constitutional or unconstitutional are important signals. The relative importance of these signals will depend on the identity of the federal court issuing the ruling. Such rulings may come from the U.S. Supreme Court or the U.S. Courts of Appeals.[3] If $State_i$ and $State_j$ are both in the same federal circuit, circuit court rulings are more important because they are binding as a matter of law[4] whereas a ruling on a policy from a state in a different circuit would not be binding in $Circuit_i$ if $State_i$ chose to adopt such a policy. Since federal circuits are arranged geographically, this dynamic may contribute to observed neighbor effects.

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[3]I do not consider rulings issued by U.S. District Courts for two reasons. First, any ruling regarding the constitutionality of $State_j$'s policy issued at the trial court level is not binding in any other state. Second, since there is an appeal as of right and cases involving the constitutionality of statutes are inherently high stakes cases, most litigation from the district courts will be appealed to the circuit level.

[4]Only published opinions issued in a federal circuit are binding under the doctrine of stare decisis. However, as a practical matter when a court rules on the constitutionality (or unconstitutionality) of a state law the opinion is virtually always deemed important enough to warrant publishing the opinion.

Current research using directed dyadic analysis for studying policy diffusion has emphasized its utility for modeling characteristics of the relationship between a potential and previous adopter and characteristics of each state (Gilardi and Füglister, 2008; Gilardi, 2010; Volden, 2006). I extend this approach a step further to explicitly address characteristics of the previously adopted policy itself. Specifically, I utilize the dyadic framework to develop a theory of how the treatment of $Policy_j$ in federal courts influences $State_i$'s policy decisions. I consider both how different types of court rulings influence the decision to pass legislation and, if such legislation is passed, how much the text directly reflects the content of $State_j$'s previously enacted law.

This two stage approach addresses the need, discussed by several scholars, to look beyond the binary decision to adopt a policy and incorporate an evaluation of the content of adopted policies as well (Boehmke and Witmer, 2004; Boehmke, 2009a; Brinks and Coppedge, 2006; Glick, 2011; Karch, 2012, 2007b; Shipan and Volden, 2012; Strebel and Widmer, 2012; Taylor et al., 2012; Volden, 2006). Simply adopting the same kind of policy enacted in another state does not guarantee that the policy is exactly the same (Karch, 2007b). Nor does it necessarily indicate that $State_j$ was the source of the idea or information behind $State_i$'s decision (Volden, Ting and Carpenter, 2008). Going beyond the binary decision to adopt a policy to examine the extent to which it overlaps with the text of $Policy_j$ provides important additional insight. The nature of language is such that an idea can be expressed in a nearly infinite number of ways. As a result, the appearance of the same phrases in two policies is not likely to happen coincidentally. Overlap in the actual text used in legislation helps trace the flow of ideas in a more definitive way than just two states adopting the same policy.

When $State_i$ decides whether to adopt a policy with federal implications that has previously been adopted by $State_j$, federal court treatments are likely to influence

that decision. The extent and direction of the influence will depend on the source and type of any court rulings. Rulings that $Policy_j$ is constitutional handed down by either the Supreme Court or the circuit in which $State_i$ is located should increase the probability that $State_i$ will adopt[5] the same kind of policy as $State_j$.[6] Constitutional rulings from a different circuit may have a smaller positive effect or no effect at all.

The effect of unconstitutional rulings on the decision to adopt is somewhat ambiguous. At first glance it may appear that such decisions should decrease the chance of $State_i$ adopting the policy in place in $State_j$. However, a court decision ruling a statute unconstitutional will often provide substantial information about the particular defects of the statute under consideration including a discussion of how to cure such defects. The availability of this kind of information may increase the ability of $State_i$ to adopt a policy which can survive constitutional review. On the other hand, the mere fact of a policy being struck down may discourage $State_i$ from passing a similar law. These two types of effects also might work at cross-purposes, perhaps resulting in a net appearance of no effect. Due to these ambiguities, I do not have any theoretical expectations regarding the effect of unconstitutional rulings on the decision to adopt.

The ambiguity about how unconstitutional rulings might influence the decision to adopt a policy highlights another benefit of examining the extent of policy convergence in the actual content of the adopted policy. For the extent of policy overlap, I expect rulings that $Policy_j$ is constitutional or unconstitutional to have opposing effects. When a particular statute has been declared constitutional and another state

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[5]I use the term "adopt" to denote any legislation in a given policy area. In other words, I use it broadly to refer to both initial adoptions and subsequent amendments.

[6]This, and other, hypothesized federal court effects may be dampened to the extent that potential adopters have no interest in passing a particular type of policy. Nevertheless, the anticipated patterns should emerge in the aggregate.

decides to adopt the same policy, borrowing the text of the approved statute is an efficient means to reduce the risk of future federal court interference. Conversely, un-constitutional rulings are a clear signal for subsequent adopters to avoid (or at least alter in significant ways) the text of the preexisting statute. I expect the effect size to be most notable for Supreme Court rulings or those from the same circuit as $State_i$. These and the forgoing hypotheses are summarized in Table 4.1.[7]

	Decision to Adopt	Extent of Policy Overlap
Policy$_j$ Ruled Constitutional		
Supreme Court	+	+
Same Circuit	+	+
Different Circuit	+ or n.s.	+ or n.s.
Policy$_j$ Ruled Unconstitutional		
Supreme Court	+ or - or n.s.	-
Same Circuit	+ or - or n.s.	-
Different Circuit	+ or - or n.s.	- or n.s.

Table 4.1: Hypothesized sign for key explanatory variables.

4.3   Data and Research Design

I test the forgoing hypotheses using data from two broad issue areas, abortion and election law. Both of these issues provide contexts with substantial federal con-stitutional implications, while offering variation on the important dimension of issue

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[7]One may wonder if the hypothesized patterns could also be generated by outside forces influ-encing both federal courts and state policymakers. In addition to controlling for such factors to the extent possible in my empirical models, there is further reason to expect that such endogeneity is not a problem. The time which necessarily elapses between implementation of a statute and a court's final ruling on its constitutionality means that each policy is typically considered by poten-tial adopters before a court ruling as well as after any such rulings. Consequently, any change in behavior by potential adopters is not likely to be due to unmeasured differences between policies which courts rule on and those which they do not.

salience. There is evidence that issue salience affects the process of policy diffusion (Nicholson-Crotty, 2009), so looking at both types of policies enables the comparison of the high salience issue of abortion regulation with the less salient topic of election law. In addition to directly testing my hypotheses about the effects of federal courts, I also conduct a supplemental analysis of policy diffusion in family law, an area traditionally left almost exclusively to state governments, to provide a point of comparison to the diffusion of policies with federal implications.

Studies of policy diffusion have long broken broad topic areas down into subtopics which cover comparable policies. Following previous research on abortion policies I gather data on six distinct types of policies: (1) bans of partial-birth abortions, (2) requirement of parental consent for a minor's abortion, (3) requirement of parental notice for a minor's abortion, (4) restriction on use of public funding for abortions, (5) imposition of a waiting period between informed consent and an abortion procedure, and (6) restrictions on post-viability abortions. For each subtopic I collected every applicable statute in all 50 states from 1973 to 2010. Within the issue areas of election and family law I collected one subtopic each due to the challenges involved with sufficiently isolating discrete, readily identifiable, subtopics. The data include all laws regarding voter qualifications and grounds for divorce passed from 1990 to 2010.[8] Table 4.2 provides the number of each type of policy enacted during the relevant time frame.

Abortion policies compose a substantial portion of my data because it is an issue area which works well for evaluating the role of federal court effects on state policy diffusion. First, the role of the federal government is prominent. Since *Roe v.*

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[8]While 1973 provides a natural starting point for collecting abortion legislation (since it marks the beginning of federal involvement), there is no similar obvious starting point for the election and family law policies. Twenty years proved to be a sufficient time frame for these topics to include an average of at least 1.5 policies per state.

Topic	Subtopic	Number of Laws
Abortion	Partial Birth	35
	Parental Consent	36
	Parental Notice	31
	Public Funding	27
	Waiting Period	29
	Post-Viability	20
Election Law	Voter Qualifications	119
Family Law	Grounds for Divorce	75

Table 4.2: Breakdown of Policies by Topic and Subtopic

Wade was decided in 1973, state abortion regulations are clearly restricted by federal constitutional law. When a state passes a law regulating abortion, interference by the federal courts is a very real possibility. Second, the field of abortion regulation is sufficiently complex that there is frequently uncertainty about which abortion-related policies will be deemed unconstitutional by the federal courts and which will be upheld. The Supreme Court has not hastened to clear up every possible issue and even the concerns they have addressed often take considerable time before there is a definitive ruling. As a result, a variety of legal issues related to state abortion policy are decided by federal circuit courts and either never make it to the Supreme Court or take some time getting there. These features combine to provide sufficient variability in the data to test my hypotheses. Furthermore, the inclusion of voter qualification laws in the study helps mitigate concerns about the ability to generalize the results here to other issue areas.

I examine two aspects of the policy process. The first step is looking at what drives the decision to adopt or amend a policy. This analysis is conducted using a conditional dyadic event history model (Boehmke, 2009b; Shipan and Volden, 2010). Modeling the decision of a state regarding whether to adopt a policy in a given year has been the standard approach to policy diffusion studies since Berry and Berry's watershed

1990 article analyzing the diffusion of state lotteries (Jones and Branton, 2005; Karch, 2007b; Graham, Shipan and Volden, 2008).[9] A dyadic event history model is a more recent development which models a dyad of two states in a given year (Boehmke, 2009b; Gilardi, 2010; Gilardi and Füglister, 2008; Shipan and Volden, 2010; Volden, 2006). Using this approach, I model whether a potential adopter, $State_i$, decides to adopt a policy currently enacted by a previous adopter, $State_j$, in a given year. This enables the use of explanatory variables for attributes of $State_i$, $State_j$, the law previously adopted by $State_j$, and the relationship between the two states.

The second step of my analysis focuses on states which choose to adopt or amend a policy and examines what factors influence the content of the new policy. One of the key limits of policy diffusion studies to date has been the lack of attention to the content of legislation (Brinks and Coppedge, 2006). Scholars have increasingly worked towards finding ways to incorporate more information about the nature of policies into quantitative models. Advances in computational text analysis offer the opportunity for further progress in this vein. Specifically, plagiarism software has been used to quantify the overlap in two documents (Corley, Collins and Calvin, 2011; Corley, 2008). I utilize this tool to generate a form of outcome variable new to policy diffusion research, the percentage of text in a law adopted by $State_i$ which overlaps with a statute previously enacted by $State_j$.[10] As in the first step of my analysis, I use a dyadic model.

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[9]For more extensive discussion of the use of event history models in state policy diffusion research, see Box-Steffensmeier and Jones, 2004, 1997.

[10]This outcome variable is generated using WCopyfind 4.1.1 software with the same settings employed by Corley (2008). The software counts all phrases in $Policy_i$ which are at least six words long and match a phrase in $Policy_j$ with no more than two imperfections. The matching process disregards numbers, case, and outer punctuation.

The unit of analysis for both stages is the $State_i$-$State_j$ dyad in year t. For the first stage I incorporate observations for every $State_i$ in year t paired with every $State_j$ which had the relevant policy type in place in year $t - 1$. Such dyads are included for each of the policy subtopics. I include only comparisons to previous adopters for both methodological and substantive reasons. Boehmke (2009) warns that including all dyad-year combinations may bias the results. Moreover, the question I am investigating is how variation in federal court treatment of previous legislation influences subsequent policy decisions. The impact of another state's legislative inaction is not the focus of my inquiry. The second stage of analysis includes all dyad-year observations from the first stage in which $State_i$ adopted a new policy in year t.

Since the outcome variable in the first stage is the binary determination of whether $State_i$ has taken legislative action on a policy previously adopted by $State_j$, I use a probit model.[11] The outcome in the second stage is the percentage of text in $State_i$'s new legislation which overlaps with the text in $State_j$'s policy. Although this continuous variable is theoretically bounded by zero and one hundred, I model it using a linear model.[12] One challenge created by dyadic analysis is that it introduces complex interdependencies into the data. In order to address this concern, I estimate robust standard errors clustered on $State_i$ and year t (Bouché and Volden, 2011; Makse and Volden, 2011).[13] Another potential concern is that a two-stage decisionmaking process sometimes raises the problem of correlated residuals between the two stages biasing the estimates at the second stage (Heckman, 1979). In this case, estimation of

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[11]Alternative functional forms including logit, complementary log-log, and rare events logit all lead to the same substantive conclusions.

[12]Model diagnostics indicate that this approach is not particularly problematic, and only 0.6% of the fitted values fall outside of the zero to one hundred interval.

[13]Clustering only on $State_i$ and including either year dummies or variables for time and time squared both lead to substantially similar results.

these two stages in a Heckman selection model does not reveal significant correlation between the two stages (p-value of ρ is 0.173).

The key explanatory variables (and most of the control variables) are the same for both the adoption and policy overlap models. I incorporate six variables which contain counts of the two types of court treatments, constitutional and unconstitutional, from the three possible types of courts, the Supreme Court, the same circuit as $State_i$, and a different circuit from $State_i$. These data were compiled using information obtained from *Shepard's Citations*. While this legal publication is more commonly used by political science scholars to track subsequent treatment of case law, *Shepard's* provides analogous information for statutes as well. For each policy in my dataset I obtained a list of all Supreme Court and circuit court citations to the statute in question and further narrowed this list to contain only specific rulings that a statute was constitutional or unconstitutional.[14]

Using the year of citation, court, and type of treatment obtained from *Shepard's*, I compiled counts of the six types of court treatments for each dyad-year in my data. For example, the variable *Supreme Court - Constitutional* is a count of the number of times the U.S. Supreme Court had ruled that $Policy_j$ was constitutional between the year $State_j$ adopted the law and year $t-1$. The circuit court counts follow the same overall pattern with the exception that a circuit court treatment is dropped out of the count after the Supreme Court has ruled on the same statute. For example, if $State_j$ adopted a policy in 1995, the circuit ruled it unconstitutional in 1997, and the Supreme Court affirmed that the law is unconstitutional in 1999, the circuit treatment is only counted for dyads in the years 1998 and 1999. Once there is a Supreme Court ruling on the statute, that treatment overrides previous circuit rulings.

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[14]All relevant circuit court rulings are published.

In addition to testing the effect of federal court treatment, I also control for a variety of other factors which may influence the process of policy diffusion. Since I model both the initial adoption of policies and subsequent amendments, I include a control for whether $State_i$ had a particular type of policy in existence in year $t-1$. When *Existing Statute$_i$* equals one, any legislation adopted by $State_i$ in year t amends or replaces another policy rather than instituting it for the first time. Another important feature to account for is whether the policy in question is one of the policies related to abortion regulation.[15]

There is extensive literature addressed to the impact of the internal characteristics of a state on its policy innovation. Berry and Berry (1990) point out that research on diffusion must take such factors into account in order to make any valid claims about diffusion of policy. Consequently, I include controls for a number of characteristics of $State_i$ including state government ideology, the level of legislative professionalism, whether it is an election year, the state population, and its per capita income.[16] I rescale both *Population$_i$* and *Per Capita Income$_i$* by dividing them by 10,000.

The dyadic nature of the models makes it possible to also account for the impact of a number of important features of the relationship between $State_i$ and $State_j$. The most immediately interesting such features are whether the two states in the dayd are in the same circuit and whether they are neighbors. While I do not necessarily expect states in the same circuit to exercise additional influence beyond that explained by

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[15]Including separate dummy variables for all but one of the specific policy subtypes does not substantially change the results.

[16]The government ideology scores are Berry, Ringquist, Fording and Hanson's NOMINATE measure of state government ideology which are available at: `http://rcfording.wordpress.com/state-ideology-data/` (accessed March 10, 2013). I use the Squire index to measure legislative professionalism (Squire, 2007). The data for the remaining variables were obtained from datasets made available by Carl Klarner at: `http://www.indstate.edu/polisci/klarnerpolitics.htm` (accessed March 10, 2013).

actual court treatments, it is still important to account for this possibility. I also control for the similarity between $State_i$ and $State_j$ in terms of government ideology, size, and income. *Government ID Difference* captures the absolute difference between the ideology scores for the state governments of $State_i$ and $State_j$. I use the ratio of population and per capita income in $State_i$ to that in $State_j$ to track demographic similarities. When these ratios are greater than one, that reflects that the numbers are larger for $State_i$ than for $State_j$.

Finally, there are attributes of previously adopted laws which are important to take into account. The age of a preexisting law may very well influence how much $State_i$ will pay attention to it. Therefore, the age of $Policy_j$ is included in both stages of the analysis. Also, the measurement of the percentage of overlapping text in the second model may be somewhat dependent on the length of both the law adopted by $State_i$ and $State_j$'s previously adopted law. Consequently, I include controls for the natural log of the word count of both documents in the second stage of the analysis.

Table 4.3 shows a summary of the outcome variables and the federal court treatment explanatory variables for the policies which have federal implications. There are a total of 146,412 dyads in the first stage model. $State_i$ chose to adopt the relevant policy type in a little over three percent of those cases. In those 4,832 cases, the average amount of the text in the adopted policy which overlaps the text from $Policy_j$ is about 12%. The counts for the federal court treatment variables range from zero to two, and Table 4.3 shows the distribution of these values for the full dataset used in the first stage of the model.

	0	**1**	**2**
Policy Adopted	141,580	4,832	n.a.
S.Ct. - Const.	140,189	5,243	980
S.Ct. - Unconst.	140,434	4,949	1,029
Same Cir. - Const.	145,869	543	0
Same Cir. - Unconst.	145,623	600	189
Diff. Cir. - Const.	139,801	6,611	0
Diff. Cir. - Unconst.	138,185	6,456	1,771
	Mean	**Std. Dev.**	**Med.**
Percentage Overlap	12.34	20.75	1

Table 4.3: Summary Statistics for Policies with Federal Implications

4.4 Results

My analysis begins with an examination of abortion and election law policies.[17] The first model is presented in Figure 4.2 and examines the effect of federal court rulings regarding previously adopted state policies on the decision of other states to adopt the same type of policy. The only type of court rulings which have a statistically significant impact are rulings of the U.S. Supreme Court declaring a state law constitutional. When a previously adopted statute has survived scrutiny from the highest court in the federal judicial hierarchy other states are more likely to adopt the same kind of policy. When there are no federal rulings on the constitutionality of $State_j$'s previously adopted policy (and all other variables are held at their median) the predicted probability of $State_i$ legislating in the same policy area is 1.4%. This increases to 1.6% with a constitutional ruling by the Supreme Court. Although the increase is only a fraction of a percent, it is a 14.3% change which is substantively interesting.

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[17]Because federal litigation is relatively rare, data scarcity undercuts the utility of evaluating these two policy areas separately. Nevertheless, while estimates from modeling abortion and election law policies separately are less precise, some of the same types of patterns emerge as in the pooled analysis discussed here.

Figure 4.2: Adoption Model, Abortion and Election Law: Probit regression estimates of the effect of various types of federal court treatment and a range of control variables on $State_i$'s decision of whether to adopt a policy similar to $State_j$. Dots and diamonds indicate point estimates. Bars indicate 95% confidence intervals. Diamonds (instead of dots) and bars in red denote that a coefficient has a p-value less than 0.05. The full regression table is presented in the Appendix.

Results from the control variables shed light on other factors which influence states' decisions about whether to adopt legislation in a policy area with federal implications. Perhaps most notably, the often observed neighbor effect plays a role. $State_i$ is more likely to adopt a policy previously adopted by $State_j$ when the two states are neighbors. This effect emerges even after controlling for whether the two

states are in the same federal circuit, any rulings handed down by the circuit courts, and ideological and demographic similarities between the two states.

$State_i$'s decision to adopt is also influenced by the ideology of the state government, whether it is an election year, its per capita income relative to that of $State_j$, the age of $State_j$'s policy, and whether the policy in question relates to the regulation of abortion. Adoption of a policy is less likely when the state government is more liberal, it is an election year for $State_i$, or the policy is an abortion policy. $State_i$ is also less likely to adopt $State_j$'s policy when the preexisting policy is older. Finally, as the per capita income ratio between $State_i$ and $State_j$ gets larger, the probability of adoption decreases. In other words, as $State_i$ is increasingly richer than $State_j$, the likelihood $State_i$ will adopt the policy of $State_j$ decreases.

Even when $State_i$ chooses to adopt legislation in the same policy area as $State_j$, there is variation in the level of similarity between those two policies. Consequently, I go beyond the adoption decision to model the extent to which the text in $State_i$'s policy overlaps the text in $State_j$'s preexisting policy. While this variable only measures one specific type of similarity, it represents an important step towards more extensive evaluation of policy content. The results of this model for abortion and election law policies are set forth in Figure 4.3.

As with the decision to adopt, constitutional rulings by the Supreme Court have a positive effect on the extent to which the text of $Policy_i$ overlaps with the text of $Policy_j$. However, the effect does not quite reach statistical significance. Circuit court rulings that $Policy_j$ is unconstitutional can also effect the way states craft legislation. When $State_i$ and $State_j$ are in different circuits, a circuit ruling that $Policy_j$ is unconstitutional significantly decreases the amount of policy overlap.[18] Four percent

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[18]Although this finding is consistent with my hypothesis, there is no evidence that court rulings from the same circuit have a more important impact as I also hypothesize. Most likely this is due
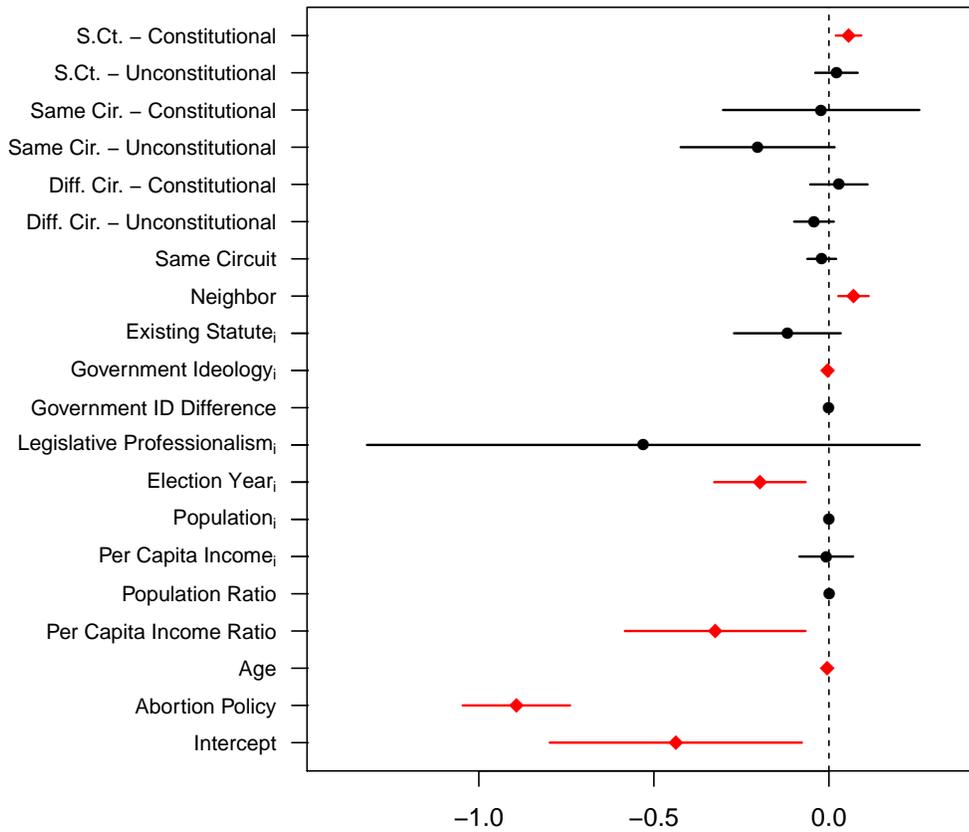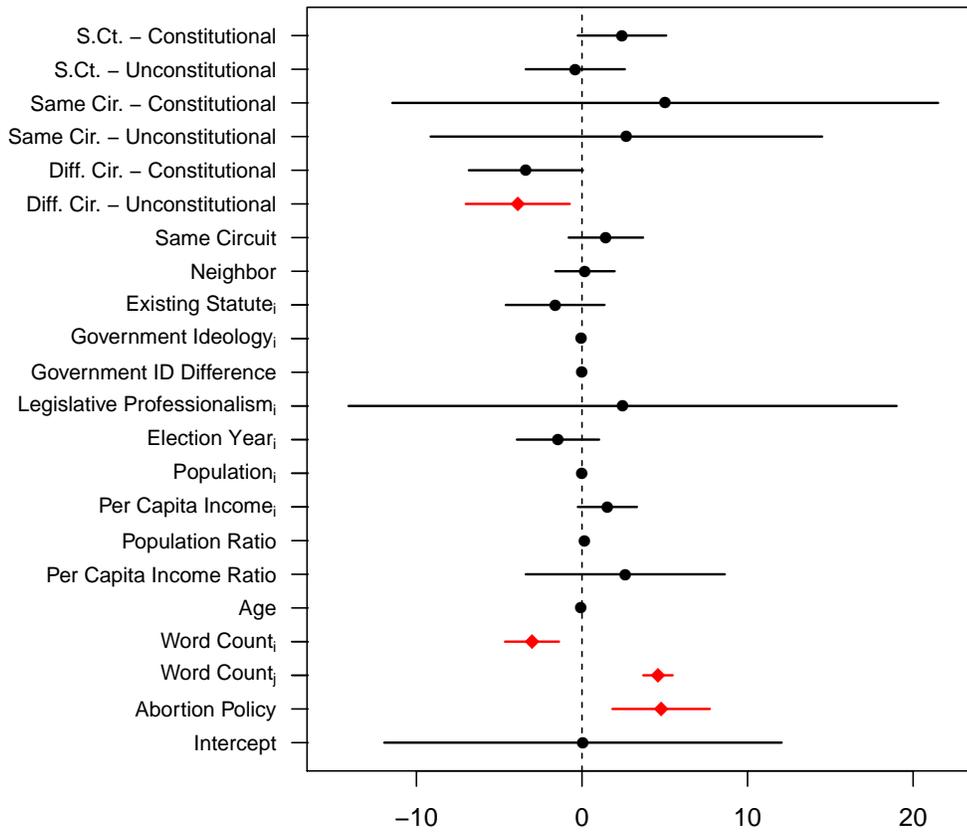
Figure 4.3: Policy Overlap Model, Abortion and Election Law: Linear regression estimates of the effect of various types of federal court treatment and a range of control variables on the percentage of the text of a policy adopted by $State_i$ which overlaps with the existing statute from $State_j$. Dots and diamonds indicate point estimates. Bars indicate 95% confidence intervals. Diamonds (instead of dots) and bars in red denote that a coefficient has a p-value less than 0.05. The full regression table is presented in the Appendix.

less of $State_i$'s policy overlaps the text of $Policy_j$. When all variables are held at their median or mode (as applicable), the model predicts that 10% of $State_i$'s policy will

to the small amount of available data rather than the absence of such effect. $State_i$ only has circuit court rulings from its own circuit to consider in a handful of cases.

match phrases from $Policy_j$. A ruling of unconstitutionality from a different circuit results in a roughly 40% reduction in the amount of borrowed text.

Many of the control variables are not statistically significant in the policy content model. Most notably, there is no evidence that the status of $State_i$ and $State_j$ as neighbors has an impact on the extent to which $State_i$ borrows language from the text of $State_j$'s policy. The only control variables which do have an impact are the length of the respective statutes and the policy type. $State_i$ borrows more text for abortion-related polices than for voter qualification statutes. Also, as expected, the more text that is available in $Policy_j$, the larger the percentage of $State_i$'s policy which is drawn from that source. Conversely, when $State_i$'s policy is more brief, a smaller percentage is borrowed from $State_j$'s preexisting statute.

Federal courts can only be expected to play a role in the diffusion of state policies which have potential federal constitutional implications. To highlight this point, I briefly present and discuss models of adoption and policy overlap for a policy area traditionally left exclusively to the states, family law. The model results for policies outlining the grounds for obtaining a divorce are presented in Figures 4.4 and 4.5. Since, as expected, there are no federal court rulings on the constitutionality of any of the policies in these data, the federal court ruling variables are not included. There is no theoretical reason to expect that $State_i$ and $State_j$ being located in the same federal circuit would influence either adoption or the content of a policy, and there is no evidence of such a pattern.

Interestingly, the *Neighbor* variable has no statistically significant effect on the adoption of a family law policy, but it does have a significant *negative* effect on the extent to which an adopted policy overlaps with the text of $Policy_j$. When $State_i$ and $State_j$ are neighbors, $State_i$ borrows about 2% less text from $State_j$ than when the two states are not neighbors. This effect is all the more notable because it is the

Figure 4.4: Adoption Model, Family Law: Probit regression estimates of the effect of $State_i$ and $State_j$ being in the same circuit and a range of control variables on $State_i$'s decision of whether to adopt a policy similar to $State_j$. Dots and diamonds indicate point estimates. Bars indicate 95% confidence intervals. Diamonds (instead of dots) and bars in red denote that a coefficient has a p-value less than 0.05. The full regression table is presented in the Appendix.

only significant variable in the policy overlap model other than the length of $State_j$'s policy and $State_i$'s per capita income. The adoption model provides a few more insights. As with state policies which have federal implications, adoption is less likely in election years. However, states are more likely to adopt when $Policy_j$ is older. Less populous states and states with a higher level of legislative professionalism are

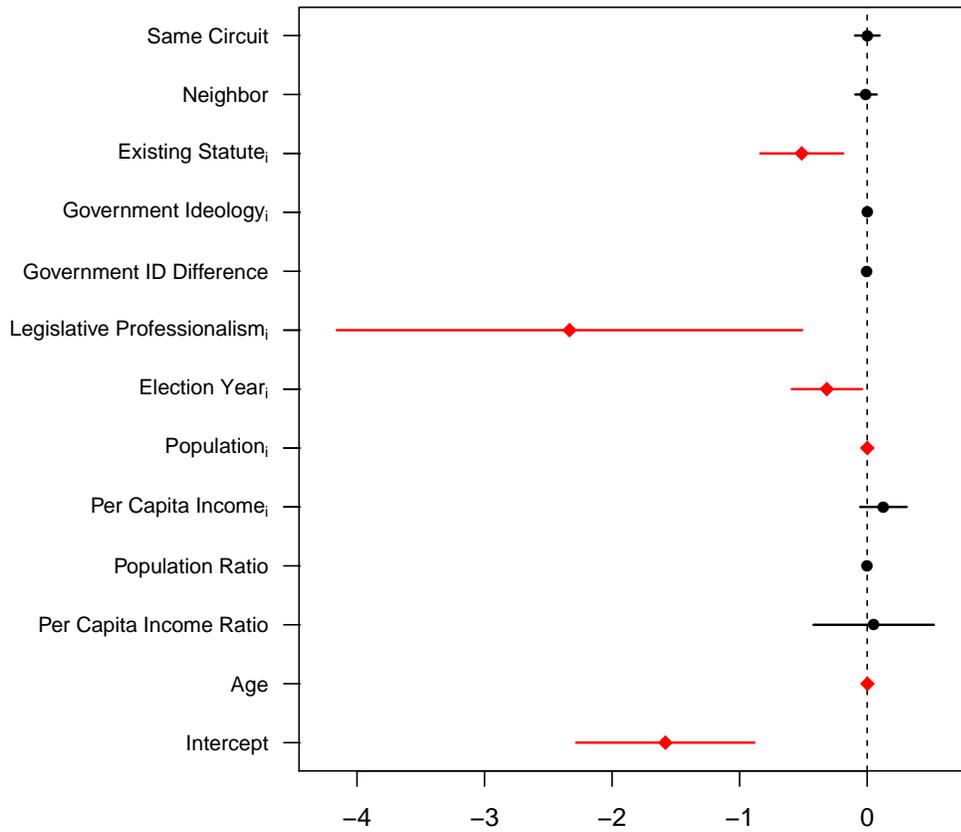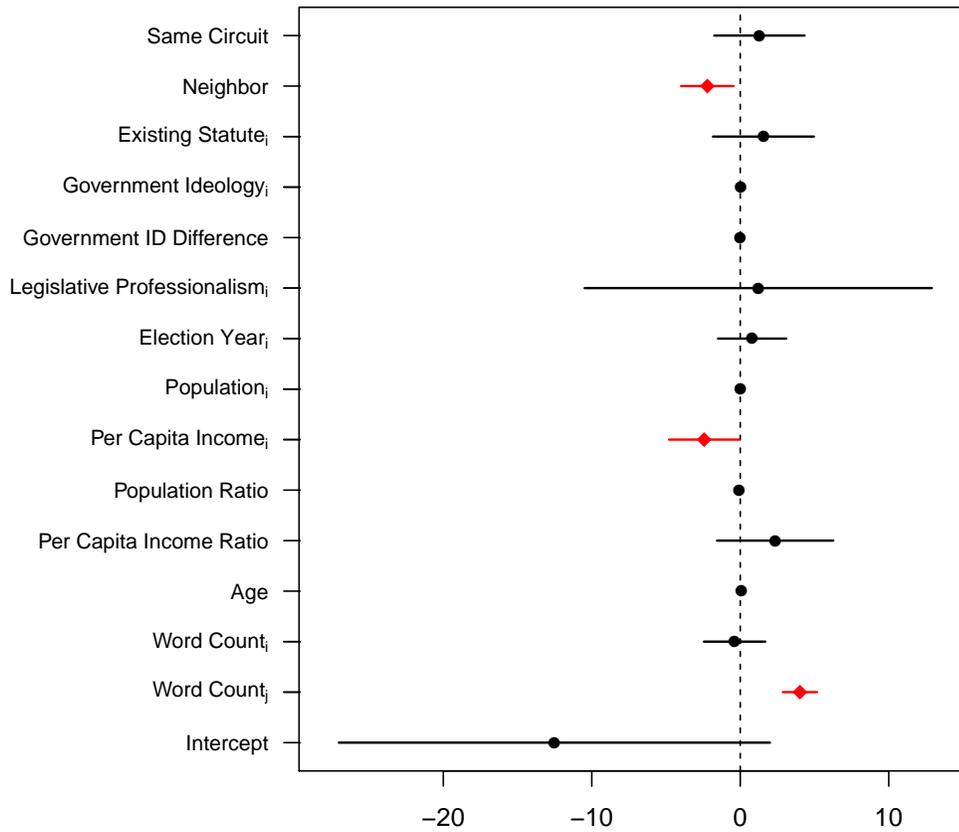Figure 4.5: Policy Overlap Model, Family Law: Linear regression estimates of the effect of $State_i$ and $State_j$ being in the same circuit and a range of control variables on the percentage of the text of a policy adopted by $State_i$ which overlaps with the existing statute from $State_j$. Dots and diamonds indicate point estimates. Bars indicate 95% confidence intervals. Diamonds (instead of dots) and bars in red denote that a coefficient has a p-value less than 0.05. The full regression table is presented in the Appendix.

less likely to adopt as are states which already have a preexisting statute of the same policy type in effect.

4.5 Discussion

This study provides evidence that federal courts influence state policy diffusion for policies which have federal constitutional implications. While the U.S. Supreme Court predictably plays an influential role, the results indicate that it is important to account for the actions of the U.S. Courts of Appeals as well. Moreover, the impact of these two different court levels is confined to different types of rulings. The opinions of the Supreme Court which declare a state policy constitutional influence the adoption and perhaps the content of other states' policies, whereas circuit court rulings from a different circuit striking down state policies as unconstitutional serve as a warning to subsequent adopters to avoid similar statutory language. Although there are theoretical reasons to expect the effects of constitutional and unconstitutional rulings to be asymmetrical, the operation of each at a different level of the judicial hierarchy is curious and suggests the need for further study.

The insights provided here about the role of federal courts in state policy diffusion are made possible by relying on and further extending two important trends in the field of policy diffusion research. The first is Shipan's (2006) application of dyadic analysis to the study of policy diffusion. The ability to model features of a previously adopted policy opens up a host of interesting theoretical questions. The influence of federal court treatment on how a policy affects other state's legislative choices is only one of many such questions. The second trend this chapter builds on is the burgeoning attention to policy content. The results reveal that different factors appear to be at work in choosing content than in the decision to adopt. While many familiar control variables operate as expected in the adoption model, they shed comparatively little light on the decision to borrow language directly from another state's policy. This indicates that further work is needed focusing on this aspect of policy formation

to supplement what we know about the decision to adopt a policy. Traditionally the difficulty and high cost of quantitatively analyzing the content of legislation has created a substantial barrier. The increasing availability of computational methods of text analysis is changing the landscape. The method employed here of calculating the overlap between two policies can be easily deployed to study a range of questions related to policy diffusion.

Being able to measure policy overlap is not just important to study decisions about policy content separately from policy adoption. This measure is central to the findings here because it gets directly at the underlying theoretical mechanism. When the federal courts rule on a state policy, subsequent states which choose to enact a similar policy take that decision into account. Borrowing text from policies which have been upheld and avoiding use of text from those which have been struck down directly reduces both cost and risks. The differential amount of overlap based on federal court treatment indicates that state legislatures pay attention to federal courts and act based on what they see. There is no other logical explanation for the observed patterns of behavior. This chapter demonstrates that federal courts have some influence over state policy diffusion and that measuring policy overlap combined with dyadic analysis is a viable and vibrant new option for continued research exploring the mechanisms behind policy diffusion.

4.6 Appendix

	Coefficient	Standard Error	p-value
Intercept	-0.437^*	0.184	0.017
S.Ct. - Constitutional	0.056^*	0.019	0.003
S.Ct. - Unconstitutional	0.021	0.031	0.489
Same Cir. - Constitutional	-0.022	0.143	0.877
Same Cir. - Unconstitutional	-0.204	0.112	0.069
Diff. Cir. - Constitutional	0.028	0.042	0.498
Diff. Cir. - Unconstitutional	-0.043	0.029	0.140
Same Circuit	-0.020	0.021	0.329
Neighbor	0.070^*	0.022	0.001
Existing Statute$_i$	-0.119	0.078	0.127
Government Ideology$_i$	-0.004^*	0.002	0.014
Government ID Difference	-0.001	0.001	0.428
Legislative Professionalism$_i$	-0.530	0.403	0.188
Election Year$_i$	-0.197^*	0.067	0.003
Population$_i$	0.000	0.000	0.606
Per Capita Income$_i$	-0.008	0.039	0.844
Population Ratio	0.001	0.001	0.182
Per Capita Income Ratio	-0.325^*	0.132	0.014
Age of Policy$_j$	-0.005^*	0.002	0.007
Abortion Policy	-0.893^*	0.078	0.000
N		146,412	

Table 4.4: Adoption Model, Abortion and Election Law: Probit regression estimates of the effect of various types of federal court treatment and a range of control variables on $State_i$'s decision of whether to adopt a policy similar to $State_j$. The reported standard errors are robust standard errors which are clustered on $State_i$ and year t and * denotes a p-value less than 0.05.

	Coefficient	Standard Error	p-value
Intercept	0.060	6.091	0.992
S.Ct. - Constitutional	2.417	1.356	0.076
S.Ct. - Unconstitutional	-0.404	1.519	0.790
Same Cir. - Constitutional	5.028	8.366	0.548
Same Cir. - Unconstitutional	2.682	6.001	0.655
Diff. Cir. - Constitutional	-3.391	1.748	0.054
Diff. Cir. - Unconstitutional	-3.878^*	1.594	0.016
Same Circuit	1.444	1.141	0.207
Neighbor	0.182	0.909	0.841
Existing Statute$_i$	-1.618	1.512	0.286
Government Ideology$_i$	-0.043	0.031	0.157
Government ID Difference	-0.009	0.021	0.662
Legislative Professionalism$_i$	2.451	8.402	0.771
Election Year$_i$	-1.447	1.261	0.252
Population$_i$	-0.003	0.002	0.105
Per Capita Income$_i$	1.533	0.906	0.092
Population Ratio	0.159	0.102	0.121
Per Capita Income Ratio	2.614	3.050	0.392
Age of Policy$_j$	-0.065	0.054	0.231
Word Count$_i$	-3.019^*	0.828	0.000
Word Count$_j$	4.587^*	0.450	0.000
Abortion Policy	4.781^*	1.493	0.002
N		4,832	

Table 4.5: Policy Overlap Model, Abortion and Election Law: Linear regression estimates of the effect of various types of federal court treatment and a range of control variables on the percentage of the text of a policy adopted by $State_i$ which overlaps with the existing statute from $State_j$. The reported standard errors are robust standard errors which are clustered on $State_i$ and year t and * denotes a p-value less than 0.05.

	Coefficient	Standard Error	p-value
Intercept	-1.582^*	0.356	0.000
Same Circuit	0.001	0.049	0.985
Neighbor	-0.010	0.043	0.820
Existing Statute$_i$	-0.513^*	0.166	0.002
Government Ideology$_i$	0.001	0.003	0.682
Government ID Difference	-0.002	0.001	0.119
Legislative Professionalism$_i$	-2.334^*	0.931	0.012
Election Year$_i$	-0.316^*	0.141	0.025
Population$_i$	0.001^*	0.000	0.000
Per Capita Income$_i$	0.128	0.093	0.172
Population Ratio	0.000	0.001	0.840
Per Capita Income Ratio	0.051	0.241	0.832
Age of Policy$_j$	0.002^*	0.001	0.029
N		28,861	

Table 4.6: Adoption Model, Family Law: Probit regression estimates of the effect of $State_i$ and $State_j$ being in the same circuit and a range of control variables on $State_i$'s decision of whether to adopt a policy similar to $State_j$. The reported standard errors are robust standard errors which are clustered on $State_i$ and year t and * denotes a p-value less than 0.05.

	Coefficient	Standard Error	p-value
Intercept	-12.517	7.233	0.090
Same Circuit	1.293	1.516	0.398
Neighbor	-2.219^*	0.882	0.015
Existing Statute$_i$	1.563	1.694	0.360
Government Ideology$_i$	0.037	0.028	0.181
Government ID Difference	-0.004	0.020	0.843
Legislative Professionalism$_i$	1.211	5.824	0.836
Election Year$_i$	0.800	1.144	0.487
Population$_i$	0.000	0.001	0.796
Per Capita Income$_i$	-2.435^*	1.176	0.044
Population Ratio	-0.072	0.045	0.115
Per Capita Income Ratio	2.351	1.950	0.234
Age of Policy$_j$	0.070	0.041	0.094
Word Count$_i$	-0.382	1.031	0.712
Word Count$_j$	4.022^*	0.573	0.000
N		1,433	

Table 4.7: Policy Overlap Model, Family Law: Linear regression estimates of the effect of $State_i$ and $State_j$ being in the same circuit and a range of control variables on the percentage of the text of a policy adopted by $State_i$ which overlaps with the existing statute from $State_j$. The reported standard errors are robust standard errors which are clustered on $State_i$ and year t and * denotes a p-value less than 0.05.

Chapter 5

Conclusion

This dissertation makes two kinds of contributions. First, the application of technologically advanced techniques for data-gathering and analysis illustrates how the application of new tools can help shed light on old questions. Partially automating data collection processes made it possible to amass data on a scale which would have been impossible to achieve otherwise. Using existing tools in a new context allows me to quantify important measures such as the similarity between two cases and the percentage overlap between two statutes. The use of advanced technology also opened up the ability to focus on the use of precedent within circuit courts, a task left largely unexamined by scholars, most likely due to the difficulties involved.

This project illustrates how using advanced tools facilitates gaining substantive insights, which is the second kind of contribution offered here. My novel datasets have provided evidence of several key observations about the role of federal circuit courts in legal development. In Chapter 2 I provide evidence that the legal doctrine of stare decisis constrains judges' decisions regarding which published circuit court precedents to cite and which of the cited precedents should be treated negatively.

Next, in Chapter 3 I turn to an examination of whether strategic anticipation of en banc review helps explain when and why judges act in a constrained fashion. The results of my empirical analysis indicate that circuit judges' citation and treatment decisions are sensitive to the ideology of the whole circuit. Finally, in Chapter 4 I shift gears to examine the role of federal courts in state policy diffusion and provide some preliminary indications that states consider federal court rulings both when deciding whether to adopt a policy and when choosing the actual wording of new legislation.

The analyses in the forgoing chapters confirm both the importance of the U.S. Courts of Appeals and the utility of exploring the content of court opinions and legislation to investigate legal development. While such textual materials hold considerable promise for the continued examination of judicial politics, the sheer vastness of available content poses challenges as well as opportunities. This is particularly true in the context of the federal circuit courts which produce tens of thousands of published opinions every year. The datasets I compiled for this dissertation are only the tip of the iceberg. The possibilities for future research will continue to expand as technological tools and computational resources develop.

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