Public Perceptions of Judicial Unanimity and Dissent:
The Impact of Divided Court Decisions on the Mass Media and Public Opinion

By

Michael Frank Salamone

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Political Science

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Gordon Silverstein, Chair
Professor Jack Citrin
Professor Robert Kagan
Professor Kevin Quinn

Fall 2011

Public Perceptions of Judicial Unanimity and Dissent:
The Impact of Divided Court Decisions on the Mass Media and Public Opinion

© 2011
By Michael Frank Salamone

Abstract

Public Perceptions of Judicial Unanimity and Dissent:
The Impact of Divided Court Decisions on the Mass Media and Public Opinion

By

Michael Frank Salamone

Doctor of Philosophy in Political Science

University of California, Berkeley

Professor Gordon Silverstein, Chair

Do judicial dissents affect mass politics? Many people, including judges, scholars, political commentators, and public officials claim that they do. The conventional wisdom is that unanimous rulings boost support for court decisions, while judicial division fuels popular opposition. As such, it has been suggested that courts present a united front on controversial cases as a strategy for garnering support and quelling resistance. However, empirical analysis of the public perception of judicial unanimity and dissent is sparse, incomplete, and inconsistent.

This dissertation is broadly guided by the question of whether unity/division among judges can in fact influence media coverage of and popular attitudes toward court decisions. In doing so, I consider both the role of the news media as an intermediary between the courts and the public and direct public reaction to information about court unity. Using a combination of existing and original data, I analyze newspaper coverage patterns to determine if judicial consensus has an independent effect on the visibility and favorability of Supreme Court coverage. While previous work has assumed that the correlation between coverage and division is the result of the most newsworthy cases producing the most divided outcomes, I find that dissent on the Supreme Court generates press coverage independent of other factors associated with a case's newsworthiness. Moreover, this dissertation is the first study to find that narrower Supreme Court majorities attract more critical coverage.

In addition, using a series of original survey experiments from a nationally representative sample, I expand and improve upon existing research of the direct popular reaction to judicial unanimity and dissent. Though most previous work on this subject had found no link between judicial consensus and public opinion, recently published findings have suggested that unanimity does indeed boost agreement with Supreme Court decisions across a variety of issue areas. Breaking with this, I find that popular reaction to judicial consensus is

1

highly dependent on the ideological salience of the issue involved and that, contrary to the conventional wisdom, dissent can actually foster acceptance of rulings among the Court's opponents by suggesting evidence of procedural justice. However, this effect of majority size on public opinion appears limited to the Supreme Court: I find no evidence of a similar effect at the lower court level.

This dissertation improves upon the existing body of knowledge regarding the judiciary's role in the political world. Not only does it reveal potential evidence of a news media bias toward judicial conflict, its public opinion findings buck both the conventional wisdom and the extant literature. Rather than suggesting that unanimity generally leads to a more supportive populace, as the conventional wisdom argues, or that there is no connection at all, as much of the scholarly literature claims, my findings show that the relationship between judicial consensus is more nuanced and is frequently the opposite of what the conventional wisdom suggests.

For Katie, Rita, and Lena

Table of Contents

List of Figures

List of Figures (cont.)

List of Tables

List of Tables (cont.)

Acknowledgments

I came to Berkeley when I was twenty-two years old, fresh out of college, and had little understanding of what I was getting myself into. I soon discovered that dissertation writing can, at times, be a fairly lonesome endeavor, which in turn can lead to bouts of discouragement. Fortunately, I've been surrounded by a network of unrelenting support that has carried me through to the completion of this project.

I am indebted to the Berkeley Empirical Legal Studies Program and the Institute for Governmental Studies for their generous support. Not only did they contribute funding that made my dissertation research possible, but they each provided a unique and stimulating intellectual environment that encouraged me in the development of my ideas. My year attending the weekly BELS colloquia exposed me to a variety of multidisciplinary approaches to understanding the interactions between law, politics, and society that I may not have encountered otherwise, and it challenged me to make my own work accessible to those outside the world of political science. IGS provided a home away from home – a place for both serious scholarly conversations and memorable social experiences; the intellectual community it housed will be the standard by which I will measure all others I may encounter in the future.

Before I came to Berkeley, Ray LaRaja, a Cal grad and one of my undergrad mentors at UMass, advised me that the biggest component of a graduate education comes from the colleagues that surround you. Indeed he was right. I am fortunate to have spent the last seven years of my life surrounded by the smartest group of twenty- and thirty-somethings I could imagine. I particularly want to thank Josh Green, John Hanley, Peter Hanson, and Matthew Wright for having been great classmates, collaborators, and friends.

It goes without saying that the faculty at Berkeley has played a monumental role in my scholarly development. It has been an extraordinary experience to learn from so many great minds. I am forever grateful to the collection of mentors that have served on my dissertation committee. Jack Citrin not only provided honest, constructive feedback on my work, but it was his suggestion that I co-author a chapter in his anthology, *Public Opinion and Constitutional Controversy*, that inspired my dissertation topic. Moreover, as a fierce advocate for his students, he was instrumental in helping me secure my research funding. Kevin Quinn, having only been at Berkeley for a couple of years, joined my committee after my research was already underway. However, in that time he has had a profound impact on the direction and quality of my research, and I owe much of my success to him. Bob Kagan is rightfully known as one of the greatest mentors in the field of public law. Bob has mastered the art of combining uncompromising criticism with cheerful encouragement, without either of which my

Introduction

       In May of 1954, the Supreme Court, led by Chief Justice Earl Warren, announced its unanimous decision in *Brown v. Board of Education* that racial segregation in public schools was unconstitutional. Although the justices were initially deeply divided on the issue in this case, Warren, fearing that divisions on the Court would feed public resistance to the ruling, artfully crafted and negotiated a unanimous opinion (Klarman 2004, 292-302). In other words, the Court in *Brown* believed that the extent to which it was unified mattered to the public's perception of the decision.

       This is just one example of a prevalent assumption regarding the relationship between the courts and the public: dissenting opinions are somehow damaging to a court's public image and political power. Following this logic, we would likely assume that a unanimous decision will meet less resistance from the public than a divided one. Additionally, courts that more often present unified fronts may maintain a higher degree of public trust than those that are wrought with division. Versions of these assumptions appear frequently in discussions of the court's standing with the public at large. Other jurists have expressed this sentiment both in their judicial opinions (*Bush v. Gore* 2000, *Pollock v. Farmers' Loan & Trust Co.* 1895) and in their writings outside the Court (Hand 1958, 72; Hughes 1928, 67). Similarly, this assumption has made it into the public debate on the politics of the Supreme Court; the presumed public importance of judicial unity has permeated news commentary (e.g., Douthat 2009; Pannick 2008) and political debate (e.g., U.S. Congress. Senate. Judiciary Committee 2010; hereafter Judiciary Committee). In fact, there even exists evidence that the public believes that divided decisions do not carry the same legal weight as their unanimous counterparts (Jamieson and Hennessy 2007).

       Furthermore, several studies in political science have alluded to this assumed relationship (Rathjen 1974, 394; Ulmer 1971, 702; Wahlbeck, Spriggs, and Maltzman 1999, 490-91; Walker, Epstein, and Dixon 1988, 387), but only a handful have made any attempt to evaluate it empirically (Gibson, Caldeira, and Spence 2005; Marshall 1987; 1989; Peterson 1981; Zink, Spriggs, and Scott 2009). Therefore, the question remains: *Is there any evidence to support this widespread, conventional wisdom?* This dissertation will use empirical and experimental evidence to explore the importance of judicial consensus and evaluate the degree to which judicial conflict and unanimity may affect popular political debates.

       My approach to this is three-fold. I first situate the debate over judicial unity and public opinion within the larger political science and judicial behavior literatures in order to establish the plausibility of such a far-reaching effect. This exploration also allows me to generate specific hypotheses regarding the nature of a unanimity or division effect. Second, I consider the role of the news media as an intermediary between the courts and the public. Third, I use a series of experimental public opinion surveys to expand and improve upon existing research of the direct popular reaction to judicial unanimity and dissent. Thus, using these methods, the subsequent

chapters will evaluate several hypotheses that stem from the notion that dissent negatively impacts the public's perception of the judicial branch. This dissertation, through new empirical and experimental study, challenges previous assumptions regarding the public consequences of judicial dissent. Ultimately, I find that, though dissent can impact mass politics, its effect is limited and often manifests itself in a way contrary to the conventional wisdom: dissent can sometimes increase acceptance of the Court's action by demonstrating evidence of procedural fairness to its public opponents.

Theoretical Importance of Public Perception of the Judiciary

Understanding the relationship between courts and public opinion is vitally important to our understanding of our system of government. Precisely because the Court is perceived to play an anti-majoritarian role in American democracy, fears about its power has been of central concern for elites and the general public alike from the start. In the late eighteenth century, when Americans were debating the ratification of a new constitution, anti-federalists, such as Robert Yates in *Brutus XI*, worried that an unelected, unaccountable Supreme Court that had the power to declare legislation void would ultimately usurp power from the democratically elected arms of government. Alexander Hamilton insisted this concern was unfounded. In *The Federalist no. 78* Hamilton argued that the Court would exercise self-control since it would rely upon public acceptance for its own power and influence. Unlike the elected branches of government, the judiciary was designed without formal enforcement mechanisms, making it an inherently weak institution. Describing this situation, Hamilton wrote:

> The Executive not only dispenses the honors, but holds the sword of the community. The legislature not only commands the purse, but prescribes the rules by which the duties and rights of every citizen are to be regulated. The judiciary, on the contrary, has no influence over either the sword or the purse; no direction either of the strength or of the wealth of the society; and can take no active resolution whatever. It may truly be said to have neither FORCE nor WILL, but merely judgment; and must ultimately depend upon the aid of the executive arm even for the efficacy of its judgments.

That is, for judicial decisions to carry any weight, they must gain acceptance of the elected branches whose responsibility it is to enforce and fund them. Because these branches are accountable to voters, they have no incentive to follow the Court's judgment if there is substantial public opposition. Hence, even though federal judges are not elected and are not subject to removal from office, they must rely on other institutions that are accountable to public opinion and therefore are not fully insulated from the policy preferences of the populace at large.

Given the fact that, unless the public is indifferent or ignorant of a particular case, the courts' only real source of power is their ability to persuade the public that their rulings ought to be followed, it is not surprising that the early Supreme Court saw its role to be that of a civic educator. During the first decade under the Constitution, the Supreme Court would take the

opportunity when riding circuit (travelling from district to district) to promote appreciation for the newly formed justice system (Lerner 1967). The need for popular understanding of the Court continues to be necessary. Muir (1967) provides a twentieth century example with the public reaction to *Abington Township School District v. Schempp* (1963), a Supreme Court decision prohibiting public school sponsored Bible readings. Although this decision was unpopular in many communities, Muir finds that the presence of well-spoken lawyers and civic leaders, who can educate the public and implementing officials of the ruling's importance, was a key factor in assuring its compliance. Thus, Muir shows that if the Court is able to persuade the broader legal community, that may in turn translate into acceptance among the citizenry.

As popular acceptance of the courts has long been considered to be paramount to their institutional efficacy, scholars have specified two different ways in which the public expresses support for the judiciary. The first, known as specific support, can be thought of as approval for particular decisions or actions of the institution. On the other hand, diffuse support is a more general expression of the institution's legitimacy as a whole (Caldeira and Gibson 1992; Easton 1975; Jaros and Roper 1980). While research has shown that, under certain circumstances, individual Supreme Court opinions *can* cause structural and/or aggregate changes in public opinion on specific policy issues[1] (Brickman and Peterson 2006; Clawson, Kegler, and Waltenburg 2001; Franklin and Kosaki 1989; Hoekstra 1995, 2003; Stoutenborough, Haider-Markel, and Allen 2006), diffuse support is far more difficult to move (Gibson, Caldeira, and Spence 2003).

This distinction has important implications for the examination of court majority size as a factor in public perception of the judiciary. The little direct research on this topic that exists has primarily concerned itself with measures of specific support (Gibson, Caldeira, and Spence 2005; Marshall 1987; 1989; Peterson 1981; Zink, Spriggs, and Scott 2009). This makes sense when one considers the short- and long-term interactions between the judiciary and the public. Because individual case decisions may have consequences at the level of specific support but they are not likely to impact diffuse support, we would expect that the Justices would be likely to adopt different public relation strategies for each. To maintain a sufficient level of diffuse support, the Court may wish to ensure that its decisions on the whole do not stray too far from public opinion. That is, while individual decisions may not impact institutional legitimacy, such capital may deteriorate over time. As such, some research has found that the Court is likely to follow public attitudes as if it were an elected branch of government as a means of preserving public esteem (McGuire and Stimson 2004; Mishler and Sheehan 1993, 1996). However, in the short term, judges are likely to be more concerned with specific support for their decisions. As a result, they can be expected to do what they can in order to sway the public to agree with – or at least accept – the reasoning of their judgment. Thus, given the assumptions regarding unanimity and dissent, presenting a united front is one plausible way of doing this.

---

[1] It is important to note that the Court's ability to persuade the public considered by many to be highly conditional and is not often realized (see, Johnson and Martin 1998; Persily, Citrin, and Egan 2008).

This behavior is actually consistent with a prominent theory of political behavior. According to Zaller (1992), elite opinion shapes public opinion, and polarization in the former translates into polarization in the latter. Following this theory, Persily (2008) notes that, if we view the Supreme Court as a set of elites that may influence the public, unanimous opinions are the most likely to move public attitudes in the direction of the decision. This is due to the fact that they offer a clear, unambiguous signal to the public while divided decisions may carry the conflict from the Court to the masses. Thus, the assumption of jurists like Warren is consistent with political science theory.

The Role of the News Media in Public Attitudes toward the Courts

As mentioned, this dissertation examines media reactions in addition to public opinion reactions to varying levels of court unity. While Americans may get their information on political events from a variety of sources, including political parties, interest groups, and family members, the news media are a vital source of public knowledge and play a key role in how the public formulates its opinions (Iyengar and Kinder 1987). Information regarding the judiciary and its decisions is, of course, no exception to this.

Thus, we can think of the media as an intermediary that relays information about the courts and their rulings to the public that enables them to formulate opinions. As will be discussed in Chapter 2, the media's role in disseminating information to the public is the key to the media's capacity to actually shape and even manipulate public opinion. Therefore, if there is validity to the assumption that dissents negatively affect the public image of courts, it is logical to assume that the mechanism by which this happens includes the news media. An illustration of how this works is represented in Figure 0.1. In the first row, we see the situation that Warren foresaw in *Brown*: once the Court announces its decision and, along with it, the number of justices who joined with the majority, the media then report that information. Through these reports, the public becomes informed of the decision and formulates an opinion on the Court's action. While a variety of factors are no doubt at play in the formulation of public opinion, Warren's assumption implies that a court's level of division is part of that calculus. Thus, because the media included this information in the report, the public can then react based upon the court's degree of consensus.

However, the second row presents a different scenario. While the court makes its decision regardless of what happens next, it is possible that the media may not include the court's majority size in their reports. If this is this case, the public may still react to the decision, but the extent to which the court was united can no longer play a role in opinion formulation. Moreover, the third row presents a situation where the media do not report the decision at all. In this instance, the public cannot react to the decision at all since they have received no information about it. Therefore, majority size in the absence of news reporting is irrelevant to public opinion. Thus, from this schematic, we can see the importance of adequate news coverage in realizing a relationship between court unity and public opinion.

Figure 0.1. Path of information from the Court to the public.
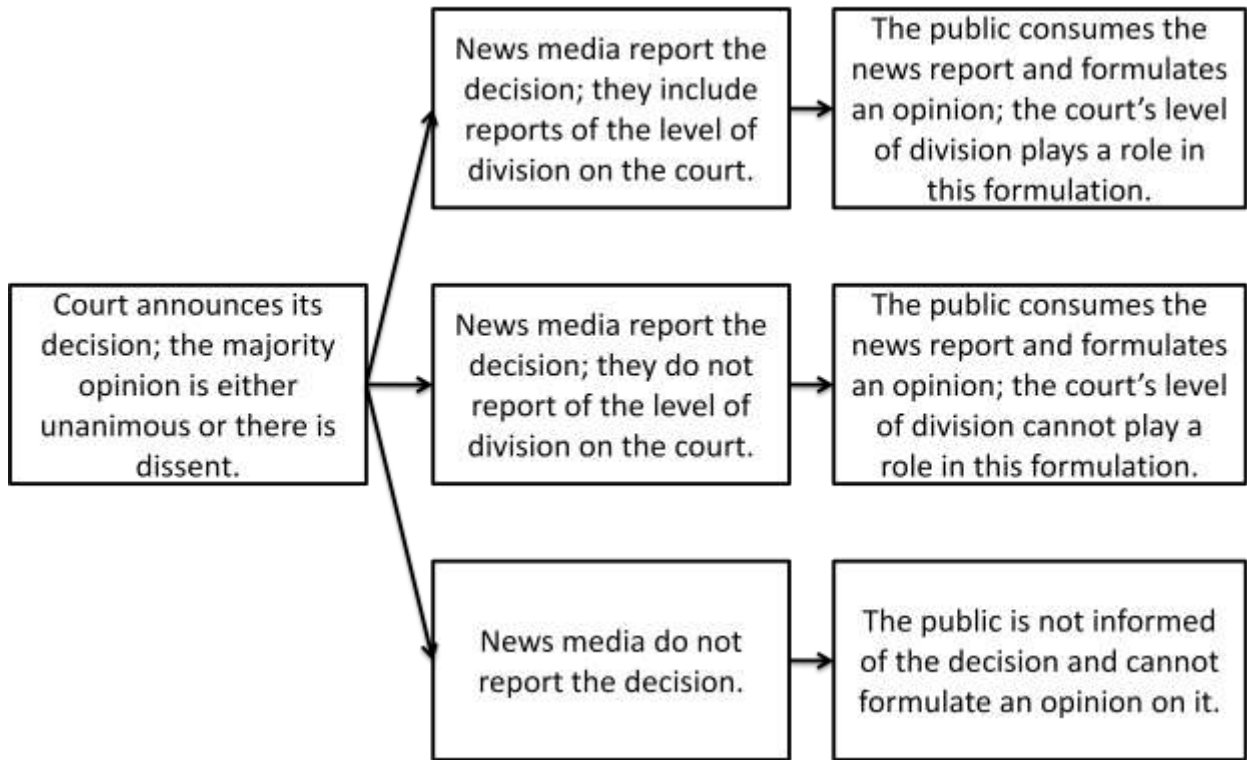
Unfortunately, a comprehensive study of this process is absent from the literature. There has, however, been some discussion of evidence that indicates a bias toward coverage of divided decisions; that is, reporters tend to disproportionately cover cases that are closely divided (Epstein and Segal 2000; Ho and Quinn 2008). Little has been written about the nature of this bias. In general, it has been assumed that this discrepancy in coverage between unanimous and divided rulings can be explained by the relative salience of the cases; that is, unanimous cases are thought to be uninteresting cases that are foregone conclusions and, thus, not very newsworthy (Pritchett 1941). However, much like the Warren assumption, there has been little scholarly consideration of whether or not this explanation is empirically true.

Sequence of Chapters

While it seems clear that public perception of the degree of unanimity or dissent in Supreme Court opinions might be a significant factor in understanding the Court's role as a political institution, and its relationship to the public, surprisingly little research addresses this topic. This dissertation addresses this gap in the literature through a variety of methods.

Chapter 1 elaborates the theoretical foundations for this work. The first part of the chapter examines the development and prevalence of the assumption that court division is a factor in the formulation of public opinion. To establish just how commonplace this assumption may be, this dissertation examines closely the writings and statements of jurists, news commentators, and public officials. A close look at the relevant scholarly literature concerning the public impact of unanimity and dissent demonstrates the degree to which these assumptions about the effect of unanimity and dissent is an underexplored theme in the fields of judicial behavior, public opinion, and the politics of the media.

Chapter 2 addresses the issue of public awareness of court unity and whether the level of division on the Supreme Court has an independent effect on the quantity and favorability of the news coverage its decisions receive. As discussed above, media attention to the courts is a prerequisite for any effect that they, or their level of unity, may have. Hence, the question underlying this chapter is, what information regarding judicial unanimity and dissent is made widely available to the public through the mass media? I address this question with an examination of newspaper coverage of Supreme Court cases. Using data collected by Epstein and Segal (2000) and Ho and Quinn (2008), I investigate the correlation between newspaper coverage and court majority size. Specifically, I determine that an inverse relationship between the degree of coverage and majority size remains even when controlling for a number of potentially confounding characteristics. That is, this chapter provides evidence supporting the hypothesis that, all else being equal, a decision's visibility in the news media (as measured by front page coverage in the *New York Times* and appearances in newspaper editorials across the country) increases with the number of dissenting votes. Moreover Ho and Quinn's data on newspaper editorials generates further evidence to support Chief Justice Warren's assumption about the relationship between unanimity and public perception. If the Court is interested in shaping public understanding, then mere reporting is probably not sufficient: Focusing on the editorial pages and the degree of favorable editorial coverage adds an important dimension to the raw coverage and to movement in public opinion polls. Indeed, the evidence suggests that all else being equal, as newspaper editorial coverage becomes more negative as the number of dissenting votes goes up.

While this establishes the degree to which the public is exposed to information of the judiciary's level of unity, the subsequent two chapters examine the ability of this information to sway the public's attitudes. To capture the causal effect of judicial unity on public attitudes accurately, both Chapters 3 and 4 employ experimental survey designs. This is a common technique used in the study of popular reaction to court decisions (Clawson, Kegler, and Waltenburg 2001; Hoekstra 1995; Mondak 1990, 1991, 1992) and has recently been used to gain leverage on public response to judicial dissent (Gibson, Caldeira, and Spence 2005; Zink, Spriggs, and Scott 2009).

Chapter 3's design is a modification of that used by Zink, Spriggs, and Scott (2009), which has been the most successful to date. Consistent with their model, I present subjects with vignettes describing Supreme Court decisions where the majority size has been manipulated as the experimental treatment. However, unlike their design – or any other design

to date – this chapter makes a distinction between cases decided with a minimum winning coalition (i.e., five votes to four) and those with an overwhelming majority (i.e., eight votes to one). Previous work on this subject has only made the distinction between unanimous and divided decisions. By including two levels of division, this experiment allows me to officiate between two versions of the popular anti-dissent assumption: the first assumes that unanimity is always preferred and that any dissent is damaging to the Court; the second argues that closely divided decisions harm the Court and, thus, any large majority (including unanimity) should be sought. The results demonstrate that the effect of unanimity and dissent is dependent on the ideological salience of the issue being decided and that, in certain instances, dissent is actually preferred to unanimity by the significant segments of the public.

Of course, one might wonder if the effects observed in Chapter 3 are unique to the Supreme Court. Thus, Chapter 4 presents a variation on Chapter 3's experimental design that tests the popular effect of unanimity and division in the federal Courts of Appeals. However, by moving the venue from the Supreme Court to the lower courts, the experiment introduces a variable that may interact with judicial unity: the deciding court's ideological reputation. Unlike the Supreme Court, of which there is only one, the U.S. Courts of Appeals are plentiful and diverse, particularly in the perceived political leanings of their judges. For instance, the Court of Appeals for the Ninth Circuit is often considered the most liberal federal court, while the Fourth Circuit is generally considered to be very conservative. This may play a significant role in the effect that unanimity and dissent may have: to a politically conservative citizen, a court ruling, even if it is decided unanimously, is unlikely to win her over if she perceives that court to have a liberal bias in general. Thus, including ideological reputation in conjunction with decisional unity is essential in an analysis of the lower courts. Consequently, in the vignettes utilized in this experiment, the respondents will be told of a decision, which will be either unanimous or divided, by a court that is either generally liberal or generally conservative (both of these variables will be assigned at random). Hence, this experiment has the potential to tell us if any effect of unanimity or dissent is altered depending on whether deciding court is ideologically opposed or in alignment with the individual. Regrettably, however, the results indicate no evidence that dissent in these lower courts has the same effect that it does at the Supreme Court level.

Following these chapters, I provide some concluding remarks summarizing these findings, discussing their implications, and suggesting avenues for future research.

Chapter 1

Conventional Perceptions:

The Widespread Concern about Dissent and its Place in the Study of Judicial Politics

Is judicial unanimity important? Many scholars, journalists, and ordinary citizens believe it is. They think it provides an important boost to judicial legitimacy and compliance with court rulings. Whether or not this belief is correct, however, lends itself to careful empirical evaluation. Before launching into such an investigation, this chapter highlights the relevance of judicial unanimity and dissent to wide range of work in political science since there is a widespread assumption and broad conventional wisdom, shared by political scientists and practitioners alike, that there is an important link between judicial unity and public attitudes. This chapter will explore that common assumption and demonstrate just how widely shared it is.

Judicial dissents raise a host of interesting questions, many of which are outside the scope of this study. When people think of Supreme Court dissents, for example, they often think first of the great iconic moments in the Court's history when a thunderous dissenting opinion was handed down – one which, years later, would gain widespread acceptance, certifying the dissent-writer as among the great principled leaders in our history. No constitutional law casebook would be complete without Justice John Marshall Harlan's dissent in *Plessy v. Ferguson* (1896) or Justice Oliver Wendell Holmes' dissent in *Lochner v. New York* (1905). These opinions have been a key element to the Court's rich history and have been of central importance to subsequent legal scholars arguing against existing Supreme Court precedent. This, of course, is a lagging effect, one that can take years to take hold (Harlan's dissent in *Plessy* only *starts* to triumph in 1954 – 58 years after the dissent was filed). This dissertation focuses on more immediate effects, in part because the interest here is not in how dissents might shape public policy or future court decisions, but in whether or not unanimity has the effect that Chief Justice Warren, and other Justices, assume that it does. The concern expressed by Warren and others was not in regard to future generations' understanding of the Court, but rather it was driven by concerns about the immediate efficacy of a Court ruling (and the degree to which dissension on the Court might encourage resistance by those unhappy with Court rulings). Thus, what follows focuses on the role of dissents as they relate to the political world beyond the Court, exploring the practice of opinion writing, the evolution of the dissent, and the relationship of dissent to institutional judicial behavior and mass political behavior.

Perspectives on Judicial Dissent

History of Dissent

The practice of Supreme Court justices writing separate, dissenting opinions has a rich history. Initially, the Court continued the English tradition of writing *seriatim* opinions; that is, each justice would write an individual opinion for each case. However, in 1801, when John Marshall became the Court's fourth Chief Justice, he discontinued this practice in favor of writing a single, typically unanimous opinion of the Court. Thomas Jefferson, a political adversary of Marshall's, was highly critical of this new approach and suggested that *seriatim* opinion writing be required so that Congress could maintain oversight over the behavior individual judges (Moorhead 1952). Nevertheless, Marshall's new custom of a singular opinion for each case dominated the Court's rulings throughout his tenure, thereby strengthening the institutional position of the third branch.

In fact, Marshall's unanimity norm continued on the Court well into the twentieth century. Though justices did occasionally write separate opinions to dissent from the majority even as early as the Marshall Court, this was a relatively unusual occurrence. Until 1941, the Court averaged 8.5 dissents per 100 opinions and rarely exceeded 25 per 100 opinions in a given year. However, after 1941, this rate rose to an average 73 dissents per 100 opinions and rarely dropped below 60 (Walker, Epstein, and Dixon 1988). This shift was initially attributed to a change in institutional factors, particularly the Court's lightened and more discretionary workload brought about by the Judiciary Act of 1925, which allowed the Court to bypass "easy" cases and focus on the most controversial disputes (Halpern and Vines 1977; Pritchett 1941). However, more recent scholars find that this increase in dissensus is the result of a transformation in the Court's internal consensus norms (Epstein, Segal, and Spaeth 2001). The leading theory attributes this change in norms to a change in the leadership style of twentieth century chief justices, particularly Harlan Fiske Stone, who valued unanimity less than their predecessors did (Caldeira and Zorn 1998; Epstein, Segal, and Spaeth 2001; Haynie 1992).

In spite of this, the rate of dissent on the lower federal courts has remained comparatively low. Hettinger, Lindquist, and Martinek (2006) note that, between 1960 and 1996, judges on the United States Court of Appeals wrote separate opinions in only 13 percent of the cases decided. The circuit courts' low rate of dissent when viewed against that of the Supreme Court is not surprising given the differences between the two, especially when it comes to case discretion, judicial organization, and level of authority. While some have suggested that the three-judge rotating panel method of hearing cases masks the true level of disagreement on these courts (e.g., Atkins and Green 1976), the fact remains that the use of dissent as a public signal of that disagreement is nonetheless used infrequently. Additionally, the norms of consensus are not consistent across all circuits, making dissent a more acceptable activity on some courts than on others (see, Hettinger, Lindquist, and Martinek 2006; Howard 1981; Sickels 1965).

Concerns of Legal Scholars and Practitioners

Given Chief Justice Marshall's norm of avoiding dissent, it is not surprising that a number of practicing judges have viewed separate opinion writing negatively. Interestingly, many of their arguments point to external motives for judicial dissent. For instance, between his two Supreme Court appointments, soon-to-be Chief Justice Charles Even Hughes (1928, 67) wrote:

> There are some who think it desirable that dissents should not be disclosed as they detract from the force of judgment. Undoubtedly, they do. When unanimity can be obtained without sacrifice of conviction, it strongly commends the decision to public confidence. But unanimity which is merely formal, which is recorded at the expense of strong, conflicting views, is not desirable in a court of last resort, whatever may be the effect upon public opinion at the time.

Hughes' statement appears to acknowledge the notion that judges are aware of, and concerned about the external force of public opinion when determining their vote. Hughes is clearly critical of Marshall's norm of unanimity; in fact, some even point to Hughes' tenure as Chief Justice as a starting point for a new norm of dissensus on the Court (Haynie 1992). However, he believes that unanimity influences the degree of public support Court rulings might garner. He differs from Marshall by arguing that only true, and not contrived, unanimity is ultimately a court's best tool for legitimating their opinions to the masses, but this nonetheless concedes that the justices are aware of how their unity, or lack thereof, conveys to a public audience.

Similarly, Learned Hand (1958, 72), while serving as a judge on the U.S. Court of Appeals for the Second Circuit, wrote:

> [T]he difficulty of securing unanimity ... is disastrous because disunity cancels the impact of monolithic solidarity on which the authority of a bench of judges so largely depends. People become aware that the answer to the controversy is uncertain, even to those best qualified, and they feel free, unless especially docile, to ignore it if they are reasonably sure that they will not be caught.

That is, Hand suspects that unanimity is essential for public confidence in and compliance with legal decisions. Supreme Court Justice Edward White also expressed this point in his dissenting opinion in *Pollack v. Farmers' Loan & Trust Co.* (1895): "The only purpose which an elaborate dissent can accomplish, if any, is to weaken the effect of the opinion of the majority, and thus engender want of confidence in the conclusions of courts of last resort." Thus, just as Marshall believed that solidifying the Court's rulings into singular, rather than *seriatim*, opinions would strengthen the judiciary's institutional position in the American system, legal minds throughout the nineteenth and into the twentieth century continued to share this regard for unanimity.

Chief Justice Earl Warren's quest for unanimity in the landmark *Brown v. Board of Education* (1954) is perhaps the most prominent example of this judicial concern. Although this case came into being over a decade after the Supreme Court's norm of consensus had begun to

break down, the delicate nature of the case's subject matter seemed to demand a Marshall-esque approach. The Court was very narrowly in favor of upholding segregation after first hearing the case in 1952; however, Chief Justice Fred M. Vinson, whose conference vote sided with the school board, died before the rehearing and was replaced by Warren, who held the opposite position. In spite of the fact that Warren's presence on the Court ensured at least a five-to-four victory for Brown, the chief justice actively sought a unanimous opinion out of fear that a dissenting opinion would fuel resistance from white southerners (Klarman 2004, 292-302). Although Hutchinson (1979) argues that Warren's role in crafting unanimity has been overstated, he does find that the Court did believe that unanimity would increase the acceptability of desegregation decisions.

Much more recently, Justice Stephen Breyer lamented the sharp divisions on the Court in the controversial case of *Bush v. Gore* (2000). In his dissenting opinion, Breyer writes,

> And above all, in this highly politicized matter, the appearance of a split decision runs the risk of undermining the public's confidence in the Court itself. That confidence is a public treasure. It has been built slowly over many years... But we do risk a self-inflicted wound—a wound that may harm not just the Court, but the Nation.

Thus, in spite of the fact that he is leading the dissent in this case, he notes that the Court's division, particularly in this highly salient ruling, may cost it much of its political capital.

Moreover, as noted by Wahlbeck, Spriggs, and Maltzman (1999, 488-89), Canon 19 of the American Bar Association's Judicial Canons of Ethics explicitly attempts to persuade judges to refrain from writing dissents:

> It is of high importance that judges constituting a court of last resort should use effort and self-restraint to promote solidarity of conclusion and the consequent influence of judicial decision. A judge should not yield to pride of opinion or value more highly his individual reputation than that of the court to which he should be loyal. Except in case of conscientious difference of opinion on fundamental principle, dissenting opinions should be discouraged in courts of last resort.

This illustrates the fact that the above sentiments were not merely the isolated philosophical opinions of a few legal minds. Rather, this guideline indicates that the duty to avoid dissent was, at least at one time, considered somewhat of a professional obligation.

Popular and Political Concerns

Public voices expressing concern over the Court's division do not only arise from the judiciary itself. Recently, a number of figures in the news media have taken up the issue. However, unlike the above concerns, which chiefly warn against the dangers of any dissent whatsoever, media commentators are particularly troubled by the Supreme Court's apparent level of sharp, partisan divisions. For instance, in an op-ed column for the *New York Times*, Ross

Douthat (2009) lamented that "settling so many vexing controversies with 5-to-4 votes ... is an awfully poor way to run a republic." That is, Douthat seems to be concerned about the apparent anti-democratic nature of a single person, in an already very small governmental body, determining the direction of important public policy decisions. Though, like that of the judges mentioned above, this worry regards division among the judiciary, his concern is that legitimacy is sacrificed when complicated policy questions, which affect millions of people, come down to the judgment of just one person.

From abroad, David Pannick (2008) argued in an op-ed for the *Times* of London that Americans should elect Barack Obama because of the consequences that McCain's nominations would have on the partisan and closely divided American Supreme Court. This illustrates another aspect of the perceived legitimacy dilemma caused by five-to-four decisions: when the Court is so closely divided on a number of issues, vacancies on the Court may become opportunities to shift the ideological direction of its decisions. Following this line of thinking, the process of nominating a new Supreme Court justice is undeniably political, and the determining factors guiding the decisions in controversial cases are ideological whims and not consistent legal principles.

Naturally, such skepticism of the legitimacy of narrowly divided decisions is found within government as well. In fact, during the recent Supreme Court confirmation hearings for Justice Elena Kagan, the nominee and Senator Jeff Sessions engaged in an exchange that exemplifies the perceived apprehension surrounding closely divided rulings (Judiciary Committee 2010):

SESSIONS: Have you expressed any opinions previously on *Lopez* and *Morrison*? They were very controversial at the time. And do you agree with those five-to-four decisions?

KAGAN: Gosh, I don't think that I've expressed any views in my academic writing or anything I can think of on *Lopez* or *Morrison*. You know, I've given a lot of speeches in my life, but, you know, I can't think of any place where I specifically addressed those issues. I think that they are settled law, that they are part of the jurisprudence of the commerce clause going forward.

SESSIONS: Can I ask you about that? You've said that it's settled law with regard to the gun case, *Chicago* [sic], *McDonald* and *Heller*. Those were five-to-four cases. Does your definition of settled law mean anything more than the normal precedent you would give to any of those kind of five-to-four cases?

KAGAN: I think I've actually used that phrase with respect to a number of cases which people have asked me about. Those are a couple, but—

SESSIONS: If I was going to use the phrase interchangeably, "precedent," which has a certain amount of power, and then you've thrown out "settled law," to the layman seems to be a more firm acknowledgement of the power of that ruling. But I want to know, do you mean any difference when you use those two phrases?

KAGAN: I don't mean any difference. What I mean to say when I use those phrases is: these are decisions of the Court, they are decisions of the Court that are entitled to all the weight that any decision of the Court has as precedent going forward, that I have no thought, no agenda, no purpose, no, you know, remotely no plan to think about reversing any of them, that these are cases I accept as decisions of the Court going forward.

SESSIONS: All right. Well, Justice Sotomayor said a similar thing about the *Heller* case, and it didn't bother her one bit to be in the dissent in the *McDonald* case Monday. So you're not saying that you're binding yourself to be a six-to-three vote with now six members of the Supreme Court on the gun cases, and you're not binding yourself and suggesting you feel bound by *Lopez* and *Morrison*, are you?

It is clear that Sessions goes out of his way to emphasize the narrowness with which these cases, whose rulings he supports, were decided. The implication is not only that personnel changes on the Court may have consequences for the policies that come out of it, but also that narrowly decided precedents are the ones that hold the least doctrinal weight among their ideological opponents.

It turns out that this sentiment is not limited to the discussions of political commentators and government officials. Survey data suggests that the public view of five-to-four decisions is similar to what Sessions describes above. An Annenberg Public Policy Center poll revealed that less than half of Americans believe that five-four decisions carry the same legal weight as their unanimous counterparts (Jamieson and Hennessy 2007). Thus, in spite of the fact that Court rulings are legally the same regardless of the size of the majority that decided them, there is widespread belief that those decided by a single vote are less binding than others.

Unanimity, Dissent, and the Study of Judicial Politics

Judicial Behavior

The consensus norm John Marshall insisted upon clearly is gone. However, it is because of its longstanding practice that dissenting opinions are so conspicuous: had the Court never transitioned from the *seriatim* norm, separate opinion writing would hardly raise an eyebrow. Yet, it is not so clear just exactly what compels a Justice to invest the time and energy needed to prepare and write a (legally non-binding) statement of disagreement. This has generated real interest among those who study judicial behavior. Many who have done so focus on personal motives for dissent, dwelling on the personal characteristics of the judges themselves. For instance some have argued that sociological differences within the judiciary can account for dissensus. Ulmer (1970) suggests that elements of a justice's social background play a part in his or her propensity to dissent. Similarly, in a study of state supreme court judges, Patterson

and Rathjen (1976) argue that the political, social, and legal diversity of a court increases its likelihood to produce dissenting opinions.

Meanwhile, others have looked at judicial dissent as a behavior motivated more by ideology than by personal experience. Both Rathjen (1974) and Brenner and Spaeth (1988) rely heavily on ideological variables in their analyses of separate opinion writing. Additionally, the attitudinal model of judicial behavior views justices' votes largely as an expression of policy preference, making the decision to dissent little more than ideological opposition to the Court's majority (Segal and Spaeth 2002).

Competing with the attitudinalist view, Wahlbeck, Spriggs, and Maltzman (1999) find that Supreme Court justices use dissent as a tool to strategically pursue their ideological goals within the confines of the Court's institutional constraints; as such, a strategic view of judicial behavior sees the decision to dissent as more complex than a simple ideological calculation (see also, Epstein and Knight 1998). Brace and Hall (1993) find similar results for state supreme courts. In addition, Cross and Tiller (1998) argue that the dissenter on a three-judge panel in the Federal Court of Appeals is in a unique strategic position to prevent the majority judges from disregarding legal doctrine. However, Hettinger, Lindquist, and Martinek (2004) find evidence that suggests Court of Appeals judges are driven to dissent more by ideology than by strategy. Moreover, Baum (1997, 98-100) suggests that a judge's need for personal satisfaction may influence him or her to dissent sincerely rather than strategically.

While these works posit a number of reasonable hypotheses, they all primarily focus on the court's internal structure and dynamics. However, the Justices and the Court itself function within a far wider world – they are a part of the national government, and they are political players on a national stage. This is particularly salient, one would think, for those who advocate a strategic view of judicial behavior: what are the stakes in this game with an external factor like public respect? Clearly, jurists like Warren, Hand, and Breyer believed that judicial voting behavior has consequences beyond the simple policy outcome of the case. Whether or not they are correct, their perception that it does could well be a significant factor – even if not a dominant one – in understanding judicial behavior.

Public Opinion and the Courts

Of course, whether or not court dissents may affect public response to their decisions is highly dependent on the court's ability to confer legitimacy on its rulings. For quite some time, scholars have speculated that the Court does indeed possess this power. Alexander Hamilton argued that judges would restrain themselves because doing otherwise would jeopardize the legitimacy of their rulings, invite non-compliance, and thus undercut, erode, or even destroy their own power. In *The Federalist no. 78,* Hamilton famously reminded his readers that the judicial branch has "neither force nor will, but merely judgment," meaning that securing compliance would hinge not on any formal enforcement mechanism, but instead on the judges' reasoning. As discussed before, the early Supreme Court was quite aware of this. "Riding

circuit," or traveling from district to district and hearing cases, was among the early Court's responsibilities. Lerner (1967) illustrates that the justices used this as an opportunity to act as civic educators, interacting with the people directly and demonstrating the merits of their decisions. Moreover, many argue this need has not abated in the modern era, and that concerns about the legitimacy of their rulings are still paramount for many on the modern Court (Adamany 1973; Casper 1976; Dahl 1957; Funston 1975).

Building from this, several studies have sought to test empirically the limits of the Court's legitimacy as measured by various public opinion variables. One popular approach has been to look at diffuse support of the Court as an institution. Although Grosskopf and Mondak (1998) have shown that controversial Court decisions may negatively impact overall confidence in the institution, Gibson, Caldeira, and Spence (2003a) note that confidence is not entirely interchangeable with diffuse support, which they find to be fairly unshakeable.

Meanwhile, others have examined popular legitimacy as an incentive for the Court to follow the will of the majority. Although Congress and the president have obvious electoral incentives to follow public opinion, the federal judiciary, whose members enjoy lifetime appointments, have no such formal need to align themselves with the masses. Nevertheless, some argue that, because the judicial branch's efficacy depends on their ability to preserve good standing with those who are subject to their opinions, judges may check their own power against popular trends. Following this line of reasoning, several studies have found that the Supreme Court actually follows public opinion trends (Barnum 1985; McGuire and Stimson 2004; Mishler and Sheehan 1993, 1996). However, some may counter that this is not evidence that judges concern themselves with public opinion; instead this may merely be an effect of the judicial selection process: as old judges, appointed and confirmed by old elected political majorities, leave the bench, they are replaced by new judges, appointed and confirmed by the current political powers (Dahl 1957; Norpoth and Segal 1994).

Another popular course of research has been to evaluate the Court's ability to legitimate policy by measuring change in public opinion in response to judicial decisions. That is, if the Court does indeed hold a reserve of legitimacy with the public, it should be able to transfer that good faith into popular acceptance for its rulings. However, many studies have failed to find evidence that the Court has indeed had such sway over public opinion (e.g., Baas and Thomas 1984; Persily, Citrin, and Egan 2008). Even such salient rulings as that in *Roe v. Wade* (1973) have been shown to have no effect on aggregate public opinion (Blake 1977; Uslaner and Weber 1979).

In light of these null results, Franklin and Kosaki (1989) offer an alternative model for the Court's impact on the public. They note that, while the Court may have a legitimating effect among some segments of the population, its decisions may create backlash among other groups. In the case of *Roe*, they argue, there exists no aggregate opinion change on abortion not because the Court failed to move the public but because the various subgroup effects canceled out each other. Johnson and Martin (1998) expand upon this notion of structural response to opinion change by offering evidence that it is likely to occur in the aftermath of

landmark decisions on controversial issues, but this response is conditional on the Court taking on the issue for the first time. However, Brickman and Peterson (2006) find that this type of reaction to judicial opinions may not be as limited as Johnson and Martin suggest. Thus, these studies show a line of research indicating that the Court may indeed influence public opinion, but in a way other than increasing overall popular legitimacy of its decisions.

However, the legitimation effect is far from considered dead in the literature on courts and the public. In fact, several experimental studies have found evidence supporting the legitimation hypothesis (Clawson, Kegler, and Waltenburg 2001; Hoekstra 1995; Mondak 1990, 1991, 1992, 1994). Moreover, Hoekstra and Segal (1996) argue that studies searching for a legitimation effect at the national level are overlooking the fact that most cases are not particularly salient nationwide; however, they find that in the local communities from which these cases arise, where the media coverage and awareness of the decision is more widespread, there is indeed a legitimation effect (see also, Hoekstra 2000, 2003). Furthermore, Stoutenborough, Haider-Markel, and Allen (2006) find the legitimation hypothesis to hold with salient, landmark cases pertaining to gay rights, and Hanley, Salamone, and Wright (2011) find there in fact does exist evidence that the Court persuaded the general public with its decision in *Roe v Wade*. Thus, it appears that, given the right conditions for receiving information (whether it be local or national salience of a case or a laboratory situation in which one is given information), the Court can indeed move public attitudes in the direction of its ruling.

Public Reaction to Judicial (Dis)unity

With the legitimation hypothesis viable, the notion that judicial unity, or lack thereof, may affect public opinion is indeed a possibility. In fact, Stoutenborough and his colleagues partially explain their finding that *Lawrence v. Texas* (2003) caused a drop in support for gay rights (an opinion shift in the opposite direction of the Court's ruling) by arguing that the public was swayed by Scalia's bitter dissent in the case, which received considerable news coverage. As mentioned earlier, Persily (2008) notes that Zaller's (1992) work on elite signaling may justify the notion that judicial unanimity may improve popular reaction: dissenting opinions may obscure the clarity of the Court's message to the mass public, thereby minimizing the institution's legitimation effect. However, this aspect of the legitimation theory is not directly tested in his book, likely due to data limitations.

A number of scholars consider the public impact of judicial unity in passing, but few directly focus on this potential effect. For instance, Ulmer (1971, 702) speculatively writes regarding Warren's opinion in *Brown*, "[O]ne supposes that the unanimity of the Court enhanced the acceptability of the decision. Had there been dissents, it is possible that dissidents in the concerned publics might have rallied around the dissenters." However, there is little direct evidence that the *Brown* opinion itself was able to sway public opinion (Murakami 2008), and the evidence of the political and social efficacy of the decision is highly debated (Klarman 1994; McCann 1992; Rosenberg 1991). However, as Ulmer raises this supposition, we

cannot help but wonder how much less effective the *Brown* decision may have been if it were decided by a divided Court.

Ulmer was not alone among political scientists in his speculation that dissent matters to the political world outside beyond the Court. For instance, Rathjen (1974, 394) argues:

> On yet another level an individual justice is likely to consider the fact that his separate opinion writing may be costly to the Court's authority. Each justice recognizes the tenuous status of the Court's power and authority; a power and authority which is based, to a certain extent, upon the degree to which the court's decisions are viewed as decisions of the court as a whole.

Thus, like Chief Justice Marshall, he sees the need for judicial unity to be the logical result of Hamilton's argument in *Federalist 78*. Wahlbeck, Spriggs, and Maltzman (1999, 490-91) echo this sentiment:

> Separate opinions play a key role in shaping the law and determining the role of the Supreme Court in the broader polity. Lacking failproof means of enforcing its decisions, the power of the Supreme Court depends in part on the legitimacy the public affords its rulings... Any signals suggesting disagreement on the Court possibly weaken the standing of the Court as an institution.

Similarly, Walker, Epstein, and Dixon (1988, 387) state that separate opinion writing "may shake public confidence in the judiciary by bringing into question the certainty of the law." However, while each of these publications point, in passing, to the notion that judicial disunity is likely to be a factor in affecting public support for the Court, they all do so in the absence of empirical evidence indicating this phenomenon.[2] Thus, it appears that tendency to assume this relationship between the public and the courts extends beyond jurists, public officials, and political commentators and is prevalent in academic circles as well.

In spite of this recognition that judicial unanimity and dissent is potentially a key factor in the judiciary's image to the masses, only a handful of studies have taken on the topic directly. Peterson's (1981) review of the literature on the causes and consequences of judicial dissent points out that the few existing studies of public opinion and the judiciary limited the plausibility of the argument that judicial dissent may weaken the court's perceived authority. He reasons that research (e.g., Lehne and Reynolds 1978; Murphy, Tanenhaus, and Kastner 1973) has shown that people are largely unaware of the court's activities, making them unlikely to be affected by any particular judicial action. Moreover, he also argues that political elites are much more aware of the court's business, and yet they are largely unmoved by the level of judicial unity. As such, Peterson states that there is "indirect disconfirmation" for the hypothesis that dissent negatively affects perceptions of the institution's authority.

---

[2] Zink, Spriggs, and Scott (2009) were the first to find results supporting this claim; however, their findings were published ten years after the most recent of the examples listed above.

Though his reasoning is sound, research on the interaction between the judiciary and the public has developed greatly since the publication of his review three decades ago. As discussed above, research over this time period has demonstrated that there is support for a number of ways that the court may indeed influence public opinion. In addition, several of these works have built upon the field of social psychology to explain how courts are able to influence public attitudes (see, Brickman and Peterson 2006; Johnson and Martin 1998). Thus, while Peterson's conclusion is not incorrect given the information available to him, it was premature.

Marshall (1987) was the first to tackle directly the issue of whether or not unanimity and dissent affect the public's reaction to judicial opinions. He did this within a larger study of the legitimacy-conferring power. His research design utilized 18 Supreme Court cases for which opinion polls existed both before and after the decision. In his analysis of the shifts in opinion before and after the Court's rulings, Marshall controlled for a number of factors thought to impact public reaction, including whether or not the opinion was unanimous. He found this variable to be statistically insignificant in explaining the opinion movement. However, it should be noted that only two of the decisions were unanimous. It should also be noted that this very small number of cases was not randomly selected either; his analysis was restricted to the meager number of cases for which sufficient data existed. Although this study marks a great first step in empirically analyzing the effects of judicial unity on the public, the small sample size, nonrandom selection of cases, and lack of variance on the variable of interest make it hard to generalize these results.

Nearly two decades later, Gibson, Caldeira, and Spence (2005) examine the role of the level of division as part of a larger study in the sources of the Court's legitimacy. In their experimental survey design, the researchers gave their respondents vignettes about a fictitious case dealing with the treatment of Florida ballots in the wake of the 2000 presidential election controversy. Among the manipulated variations in the vignettes was a phrase regarding the degree of division in the decision making: the court was either characterized as "nearly unanimous" or "divided along party lines." Their findings showed no significant difference in public acceptance of the decision between those given reports of a nearly unanimous decision and those told the decision was divided. While this is a more sound causal model than that presented by Marshall, it also suffers from generalization problems, since their study drew from the 2000 election controversy, an unusual and polarizing case that was still fresh in people's minds; thus, attitudes toward the controversy itself may be the real driving force behind their results (Zink, Spriggs, and Scott 2009).

In spite of this criticism, Zink, Spriggs, and Scott do agree that an experimental design is a valuable tool to untangle the effects of court unity on public opinion. As such, they have provided the most thorough and direct investigation of the topic to date. Their experiment provided subjects with doctored newspaper articles on three different (fabricated) Supreme Court cases, each covering a different policy area (abortion, school prayer, and bankruptcy). Prior to exposure of these articles, the authors had the subjects complete questionnaires to determine their *ex ante* positions on those issues. The treatment articles were manipulated in

three ways: the direction of the decision (liberal or conservative), level of division (unanimous or divided), and treatment of precedent (overturning or upholding prior cases) was randomly varied. After exposure to each treatment article, subjects were then asked if they agreed with the decision and if they accepted it. Their findings indicate that unity is indeed a factor that boosts agreement and acceptance of court decisions, particularly among those who agree with the position of the Court ex ante.

While this article provides valuable insight into the interaction between Court majority size and public opinion, there are a few ways in which one can build upon its results. First, its 2x2x2 design does not consider the Court's level of unity independent of its treatment of precedent. Given the absence of evidence that these two variables are independent of one another, we should consider the possibility that an interaction exists between them, thus making it difficult to say that the observed results are due to Court majority size alone.

Second, their experiment treats division as a dichotomous variable – by their specification, a decision is either "unanimous" or "divided." Of course, the Supreme Court has nine justices, and a divided decision can have as little as one and as many as four dissenters. This distinction is important due to the popular attention given to five-to-four decisions, as noted above. While Earl Warren and other legal minds expressed concern over the damage done to the Court's image by any form of non-unanimity, popular and political discourse has instead focused only on the harm of very narrowly decisions. By giving the level of division only a binary value, Zink and his colleagues conflate these two concerns and allow no leverage for distinguishing between the two.

Public Information and the Courts

Zink and his colleagues' experiments represent a promising first step in understanding the *potential* effect that dissenting opinions may have on public opinion. I emphasize that this impact is not necessarily realized because much is still unknown about what information the public actually receives regarding judicial unanimity and dissent. Above, I note that studies that have had the most success in finding results that support the legitimation hypothesis (i.e., courts can persuade the public overall in the direction of their decisions) have utilized experimental designs (Clawson, Kegler, and Waltenburg 2001; Hoekstra 1995; Mondak 1990, 1991, 1992, 1994) or have been limited to local issues and opinions (Hoekstra 2000, 2003; Hoekstra and Segal 1996). Moreover, Stoutenborough and his colleagues (2006) find evidence of legitimation in cases that receive plenty of media coverage but not in cases that receive little. On top of this, although the opinion change they find is structural and not an aggregate legitimation effect, Franklin and Kosaki (1989) are careful to distinguish between those who had heard of the *Roe* decision and those who had not. Thus, one may surmise that when people are made to be aware of a judicial decision, either because they are paid to read about it as part of a study, because it is relevant to their community, or because the decision has received a high degree of national media attention, they are often persuaded by the court. However,

nationally, it may be the case that few people do pay close enough attention to courts for their decisions to sway mass opinion overall. Thus, the effects found in experiments like that conducted by Zink and his colleagues only hold up inasmuch as the information given to their subjects mirrors the actual information the public receives.

Regrettably, there is only a sparse literature on news coverage of the judiciary, a very small fraction of which addresses the issue of disagreement among the judges. Much of the existing literature has focused on the degree to which the media has promoted public understanding and awareness of the judiciary and its activities. While it is understood that media coverage of the courts relay the importance of their decisions (Iyengar and Kinder 1987), public knowledge of the courts has generally been considered to be low (Murphy and Tanenhaus 1968a, 1968a, 1968b; Murphy, Tanenhaus, and Kastner 1973). Perhaps contributing to this problem, Slotnick and Segal (1998) argue television coverage of Supreme Court decisions is often superficial and reports only a small proportion of cases. Moreover, Obbie (2007) notes that reporters have become more concerned with the outcomes of court cases while ignoring important legal details.

Nevertheless, some evidence points to a sunnier view of court coverage. While relatively few cases gain national attention, Hoekstra (2003) finds that many cases receive much more attention in the communities from which they arise. In addition, Franklin and Kosaki (1995) contend that, although coverage of the Supreme Court is lower than that of Congress and the president, certain cases do become highly visible and public awareness of them can get quite high. Furthermore, they find that awareness of the Court is a key component in the public's evaluation of the institution.

However, none of this addresses the role that the level of unity may play in the transmission of information about the judiciary to the public. On this note, Ho and Quinn (2008) observe that newspapers run editorials on Supreme Court decision more often when the number of justices that are in the majority is lower. Moreover, Epstein and Segal (2000) find that unanimous decisions are less frequently reported on the front page of the *New York Times* than their divided counterparts; they also note a correlation between the size of the majority and the frequency of front-page coverage. These data may suggest that those cases that are more divided are also more difficult or controversial, and thus more newsworthy. However, there is an alternative explanation: journalists may be more drawn to stories that show a court to be in conflict, as the dramatic narrative of two clearly delineated sides on the court is appealing. Research that officiates between these two possibilities is currently absent from the literature.

Summary

In this chapter, I have shown the nature and prevalence of the idea that dissent is harmful to the judiciary's image as well as the (underdeveloped) role of that idea within the scholarly literature. Jurists, public officials, political commentators, and scholars, have all

speculated as to both the specific and diffuse support implications of unanimity and dissent. Without pointing to specific evidence, the popular wisdom is to accept the notion that there is a relationship between judicial consensus and public acceptance.

In spite of this, empirical support for this position was entirely absent from the literature until very recently. The very few studies conducted on this question prior to 2009 had all found results contrary to the conventional wisdom. Moreover, the fact there have been fewer than half a dozen publications on the topic despite its relevance to the extant judicial politics literature is a scholarly trend in need of correction. Thus, the subsequent chapters begin an empirical analysis on the role of judicial unity within mass politics.

Chapter 2

Media Perceptions:

Newspaper Coverage of Supreme Court Decisions

As noted before, both Chief Justice Warren and Justice Breyer displayed concern for the degree of consensus on the Court in *Brown v. Board* and *Bush v. Gore*, respectively. Warren's quest to secure unanimity was primarily directed at an effort to maintain order in the immediate aftermath of the decision, while Breyer's statement was somewhat more of a concern for the Court's institutional legitimacy, or diffuse support, as a result of the case's unusually political nature. In either case, both of their concerns were focused on how the nation would react to the Court's decision: each had an underlying assumption that the public would be made well aware of the level of unity (or lack thereof) among the justices when they learned of the ruling.

Indeed, they were correct – the public was well informed of the Court's level of unity in each case. On May 18, 1954, the headline for the top story in the *New York Times* read, "High Court Bans School Segregation; 9-0 Decision Grants Time to Comply." In like fashion, on December 13, 2000, the paper ran the headline, "By Single Vote, Justices End Recount, Blocking Gore After 5-Week Struggle." In each, the vote on the Court was highlighted to readers. Thus, in these two particularly highly salient cases, their concerns regarding public reaction to the Court's vote is at least made plausible by the publicity it received.

However, there is reason to believe that the media's coverage of Supreme Court cases is not uniform across levels of division. If this is indeed the case, it would have serious implications for the concern about the level of judicial division. Asymmetries in the level of information one receives on divided opinions versus those that are more unified will affect the way she reacts to the Court. In fact, our current understanding of media reports of Supreme Court cases with varying levels of division does indicate that such a bias exists: the data show that there is more coverage of closely divided cases than those that are more unified (Epstein and Segal 2000; Ho and Quinn 2008). Given the potential significance of this bias to the way in which the public understands judicial consensus, a deeper investigation of these data is needed to determine if it really is division that drives the quantity of coverage. Moreover, these data say nothing regarding the favorability of this coverage; asymmetries in *how* – and not just *how much* – these rulings are covered may too be relevant to the Court's impact.

This chapter explores this unchartered territory. I begin with a discussion of the limited existing findings regarding news coverage of unanimity and dissent on the Supreme Court. From there, I review the data necessary to address the question of whether or not the level of unity on the Court has an independent effect on its coverage in the media. Subsequently, I present a series of regression analyses that support the claim that this independent effect does

indeed exist. After discussing the quantitative results, I expand on this analysis with a pair of anecdotal examples that further illustrate the dynamic between Court majority size and coverage.

Reports of Divided and Unanimous Supreme Court Decisions

As noted in Chapter 1, Ho and Quinn and Epstein and Segal both present data indicating that newspapers cover decisions more frequently as their level of dissensus increases. Specifically, Ho and Quinn (2008, 368) observe that newspapers run editorials on Supreme Court decision more often when the number of justices that are in the majority is lower. Although their study focuses on calculating the political ideology of various news outlets, in their discussion of their model, they state, "[W]e look at the decision to write an editorial as a function of the size of the court majority. Here we find that while decisions with all margins of victory are written about, 5-4 decisions are most likely to appear on the editorial page followed by 6-3 decisions, 7-2 decisions, and 8-1 decisions." This correlation is illustrated graphically in Figure 2.1. (Their exclusion of 9-0 decisions will be discussed below.) While they note that this simple correlation exists, a more robust analysis of it was beyond the scope of their study, which deals with quantifying the ideological ideal points of various news outlets. In other words, though they make this observation, the authors do not investigate whether editorial behavior is a direct or indirect function of majority size.

Likewise, Epstein and Segal (2000, 79) find that unanimous decisions are disproportionately underreported on the front page of the *New York Times*. At the same time, they note that decisions are increasingly more likely to reach this level of coverage as the number of dissents increase. This trend is illustrated in Figure 2.2.[3] This graph shows that there is a clear gap in coverage between those cases decided with a bare majority of votes and those decided unanimously, particularly since the late 1960s.

Epstein and Segal see the above trend as a support for an argument made by Pritchett (1941, 890): "In [unanimous] cases, presumably the facts and the law are so clear that no opportunity is allowed for the autobiographies of the justices to lead them to opposing conclusions." In other words, cases that have high levels of consensus are simply easier to resolve. As such, they are the least interesting and the least newsworthy. Thus, they are covered less frequently than the heavily divided cases, which are more difficult and therefore more interesting and newsworthy.

---

[3] Because there is a bit more overlap between majority sizes in this graph than in Figure 2.1, for the ease of the reader, only the extremes are shown. In general, however, this trend does hold up across all majority sizes.

Figure 2.1. Editorial coverage of Supreme Court rulings by term, 1994-2004 terms.
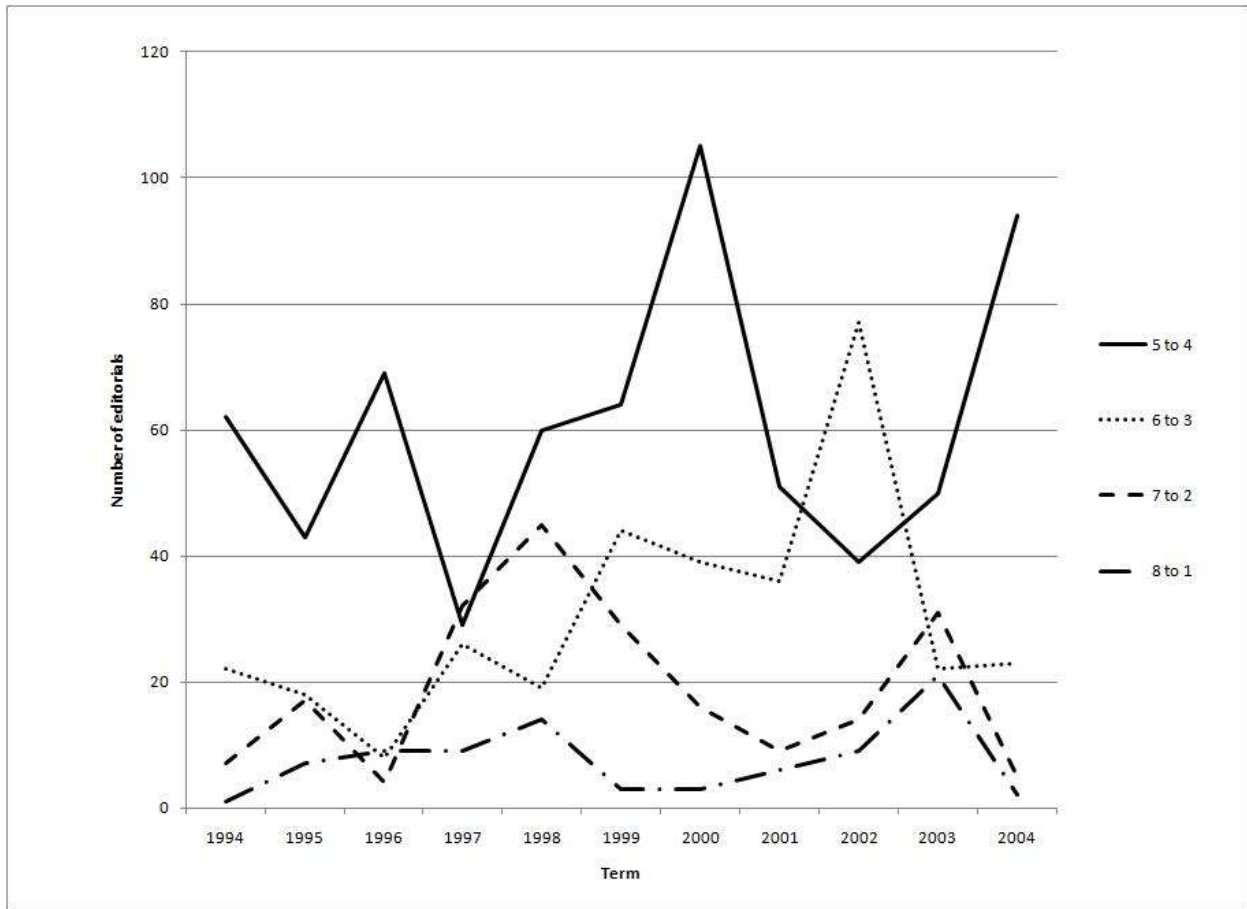
Source: Ho and Quinn (2008)

These data may indeed suggest that those cases that are more divided are also more difficult, controversial, or important. However, there is an alternative explanation: journalists may be more drawn to stories that show a court in conflict, offering a dramatic narrative of two clearly delineated and clashing views on the Court. If that is the case, then we ought to be able to observe an effect of majority size on news coverage independent of characteristics that are typically considered to make a case newsworthy. Research that officiates between these two possibilities is currently absent from the literature. Therefore, based on these peripheral findings, we can consider the hypothesis that, *all else being equal, the more divided the Supreme Court is in its ruling, the more media attention that decision will get.*

Figure 2.2. Front-page *New York Times* coverage by term, 1946-1995 terms.
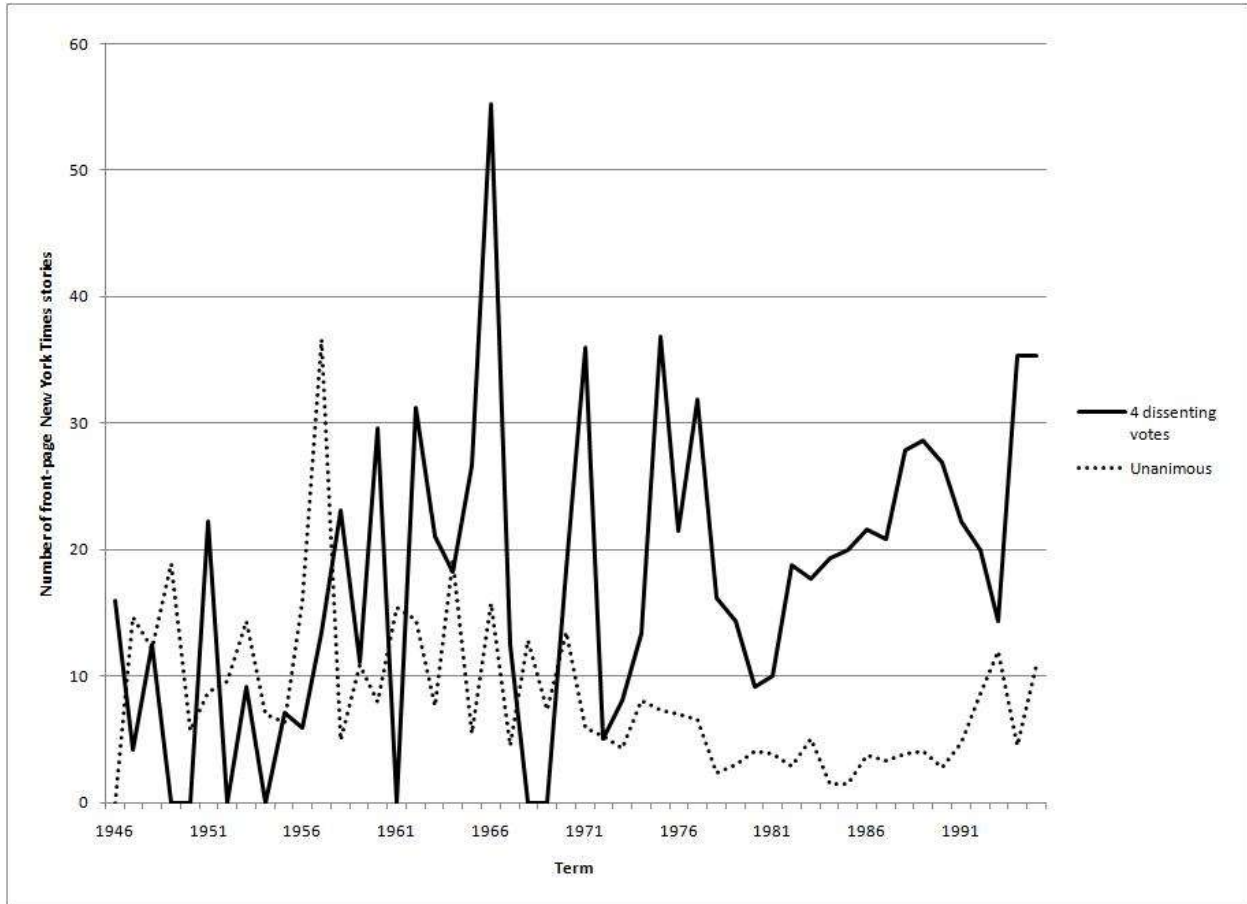
Source: Epstein and Segal (2000)

Moreover, there may also be reason to believe that the content of news coverage, in addition to its quantity, may vary with the size of the majority. As noted, there has long existed widespread concern that divisiveness on the courts may lead to negative reactions to the judiciary and its rulings. As such, we might expect to see this come from journalists as well as from the public at large. Furthermore, research on the news media's role in politics has argued that journalists tie, or index, their stories to the range of elite opinion (Bennett 1990). That is, they merely capture the degree of elite debate. Extending the indexing hypothesis to the Court therefore implies that news coverage of unanimous rulings will be more favorable than that of divided rulings, since they represent a narrow range of debate among the justices. Thus, I also hypothesize that, *all else being equal, more divided decisions will receive more negative media attention, and more unified decision will receive more positive coverage.*

Data

Dependent variables

　　Given that, this chapter deals with two dependent variables: media attention and positive/negative media response. I operationalize the first of these with two different measures of post-decision coverage. The first measure of this is whether or not a decision is covered on the front page of the *New York Times* immediately following the case*.* While this is far from an aggregate measure of all media coverage a ruling receives, it is an indication of the case's visibility within the news media. As Epstein and Segal note, the *New York Times*, particularly its front page, is tailored toward a general, national audience. Thus, a decision's appearance there is a good indication that it is a nationally visible event. As noted in Table 2.1, this variable is gleaned from Epstein and Segal's data, which covers the 1953 through 1995 Supreme Court terms.

　　The second measure that I utilize for this variable is the case's appearance in newspaper editorials across the country in the week following the case. While this measure includes *New York Times* editorials, it also includes those from other papers in other regions, thereby reaching a variety of readerships across the country. As such, it measures national visibility in a much different way. While the *New York Times* measure assesses a ruling's ability to receive noteworthy attention from a prominent national news outlet, the editorial measure demonstrates a ruling's ability to stir debate in a variety of different venues – with different regional and ideological biases – throughout the United States. Also noted in Table 2.1, this variable is constructed from the data compiled by Ho and Quinn, which cover the 1994 through 2004 terms.[4] It should be emphasized that this dependent variable measure and the previous measure have only one term of overlap. As such, the purpose of one is not to shed light on the other; rather, they act as essentially independent sources of evidence, and thus any similarities in the findings across these two analyses should act to strengthen the validity of the results.

　　This measure does come with a caveat: the data do not contain information on the coverage of unanimous cases in newspaper editorials. Although these cases are missing, it does still capture a wide range of majority sizes, from a minimum winning coalition to nearly (but not fully) unanimous. However, research on a random sample of unanimous cases show that editorial coverage of cases without dissenting votes is extremely rare. Thus, the trend Ho and Quinn observe likely extends to unanimous cases

---

[4] The newspapers included in this index are the *Arizona Republic, Atlanta Constitution, Atlanta Journal, Atlanta Journal-Constitution, Boston Globe, Chicago Sun-Times, Chicago Tribune, Cleveland Plain Dealer, Dallas Morning News, Detroit Free Press, Houston Chronicle, Investor's Business Daily, Los Angeles Times, Miami Herald, Minneapolis Star Tribune, New York Post, New York Times, Philadelphia Inquirer, Rocky Mountain News, San Diego Union Tribune, San Francisco Chronicle, USA Today, Wall Street Journal, Washington Post,* and *Washington Times.*

as well. By not including one extreme in the majority size, this variable is, if anything, biasing the results against a significant finding.

The second dependent variable – positivity and negativity of coverage – can be measured in three different ways using Ho and Quinn's data: the number of editorials favoring the majority's decision (i.e., total positive coverage), the number opposing (i.e., total negative coverage), and the proportion of total editorials on the decision that agree with the majority. As this is constructed from the same editorial dataset as above, it carries the same caveat.

Independent variables

The independent variable of interest across all these dependent variable measurements is the size of the Court's majority. This is best measured as a proportion of the justices on the Court that vote with the majority. Utilizing this as a proportion rather than the total number of majority or minority votes avoids confusion in those cases where one or more justices do not participate in a decision (either due to a recusal or vacancy). As noted in Table 2.1, this variable is constructed using data from the Supreme Court Database (available at scdb.wustle.edu) and ranges in value from .5 (where an odd number of justices do not participate in the case and those who do are evenly split) and 1 (where the vote is unanimous).

However, to isolate the effect that Court majority size has on coverage, it is necessary to control for other factors that could influence a case's newsworthiness and may also be associated with the level of judicial division. In other words, the model must take into account how interesting a decision is. This presents a challenge since this is inherently a subjective and intangible quality of a Court ruling. However, there are a number of characteristics of a Court decision that both the extant literature and the conventional wisdom suggest will make it more appealing to the news media.

A number of these characteristics, like the size of the majority, are revealed at the moment when the Court announces its decision. For instance, the ideological direction of a ruling is also worth consideration. Newspapers and Supreme Court Justices alike have been accused of having degrees of political bias. Consequently, a conservative ruling may not be able to unify a Court with liberal justices. Similarly, a liberal newspaper may have different coverage habits toward conservative decisions. Thus, I control for liberal decisions as categorized by the Supreme Court Database.

Table 2.1. Descriptions of variables.

Variable	Source	Coding
Dependent variables		
Front page *New York Times* coverage	Epstein & Segal (2000)	1 if covered
Total editorial coverage	Ho & Quinn (2008)	# of editorials
Pro-majority editorial coverage	Ho & Quinn (2008)	# of favorable editorials
Anti-majority editorial coverage	Ho & Quinn (2008)	# of unfavorable editorials
Proportion of pro-majority coverage	Ho & Quinn (2008)	# of favorable editorials divided by total # of editorials
Independent variables		
Majority size	Supreme Court Database	# of justices in the majority divided # of justices participating in the case
Ideological direction	Supreme Court Database	1 if liberal decision
Declaration of constitutionality	Supreme Court Database	1 if policy is declared unconstitutional
Alteration of precedent	Supreme Court Database	1 if precedent is altered
Self-assigned majority opinion	Supreme Court Database	1 if self-assigned opinion
End of term	Supreme Court Database	1 if decided in June or July
Constitutional authority	Supreme Court Database	1 if constitutional case
Pre-decision coverage	Proquest	1 if oral arguments covered in the *New York Times*
Civil rights	Supreme Court Database	1 if case deals with civil rights
First Amendment	Supreme Court Database	1 if case deals with the First Amendment
Privacy	Supreme Court Database	1 if case deals with privacy rights
Criminal procedure	Supreme Court Database	1 if case deals with criminal procedure
Economic activity	Supreme Court Database	1 if case deals with economic activity
Median justice ideal point	Martin & Quinn (2002)	Higher scores are more conservative, lower scores are more liberal
Warren Court	Supreme Court Database	1 if decided by the Warren Court
Burger Court	Supreme Court Database	1 if decided by the Burger Court

Rulings that declare a law unconstitutional are typically considered to be newsworthy. By definition, these rulings change the legal status quo and nullify policies passed by democratically elected officials. As such, the policy transformation and political conflict that naturally surrounds these types of decisions are inherently likely to capture the attention of journalists. Similarly, the controversial practice of judicial review itself may also be a factor in creating division among the justices, as differing judicial philosophies can lead jurists to have differing opinions on the appropriateness of the courts interfering in the democratic process.

Furthermore, when the senior-most justice in the majority coalition in a particular case assigns the opinion writing to his or herself, it may also be an indication of that ruling's broader significance. The responsibility of deciding who writes the opinion of the Court falls upon the Chief Justice or, if the Chief Justice is dissenting, the most senior associate justice voting with the majority. Given this fact, some have suggested that justices in such a position reserve the most important, interesting, and controversial opinions for themselves while leaving the less groundbreaking opinions to their junior colleagues (Danelski 1989; Epstein and Segal 2000).

Moreover, conventional wisdom says that the Court often holds off on deciding its most difficult and controversial cases until the end of the term, typically in June, though sometimes stretching into July. Not surprisingly, Franklin and Kosaki (1995) note that newspaper coverage of the Curt is at its highest during this time. Thus, the timing of a ruling could potentially have an impact on the dependent variables, which are themselves measures of media output. Moreover, to the extent that the conventional wisdom is true, these may indeed be the sorts of cases that are most likely to have sharply divided opinions. Thus, this variable is included in the analysis.

Additionally, there are characteristics of a case that are observable prior to the announcement of the ruling that may also indicate the level of interest one can expect the decision to attract. For instance, while one cannot easily predict whether or not the Court will declare a policy unconstitutional, whether or not a case involves a constitutional issue is known well before the ruling. The involvement of constitutional authority, rather than the interpretation of statutes alone, may be an indication of both its controversy among the justices and its newsworthiness. Although both matters of constitutional and statutory interpretation may hold profound legal consequences, there may be more controversy and visibility surrounding constitutional cases due to their finality: while cases that only interpret federal statutes may potentially be overridden by Congress, constitutional rulings can only be changed by the passage of a constitutional amendment or reversal from a future Court ruling, each of which presents a formidable challenge. As such, reporters may be drawn to these cases, and, for the same reasons, they may lead to strong disagreements among the justices.

Coverage of a case prior to its decision is probably the best indicator of the level of interest in a legal battle independent of the outcome. As such, I control for a case's coverage in the *New York Times* at the oral argument stage. This is typically the last major public event in a case's life before the announcement of its decision. It stands to reason that if a case has the attention of reporters at this point, it will likely also have their attention when the Court hands

down the ruling. Thus, many of the intangible characteristics of a case that make it interesting and are not captured by the other covariates are likely to be captured by this measure. As noted in Table 2.1, I gathered data on oral argument coverage by way of a Proquest search.[5]

Also known prior to the decision itself, the substantive issues involved in a case may also confound the relationship between the Court's vote and its coverage. Naturally, based on the public's interest, the media may hone in on certain issues more than others, and, perhaps as a result, the justices may have a more diverse range of opinions on those issues. The Supreme Court Database identifies a number of issue areas, including civil rights, the First Amendment, privacy, criminal procedure, and economic activity. As explained in Table 2.1, I include a dummy variable for each of these areas.

In addition to the above characteristics of the cases themselves, we may also want to consider some characteristics of the Court making these decisions. It is possible that newspapers could have different attitudes toward differently composed Courts: perhaps journalists will comment more on the decisions they like, or on those they do not. This is not necessarily the case, but, if true, the Court's composition will have a profound effect on its ability to reach a consensus. Thus, it is important to control for this potential effect of the Court's overall ideology. One key characteristic of any Court is the position of its median justice. Following the Median Voter Theorem, scholars of judicial politics note that a Court is often swayed in the preferred direction of its own median voter (Martin, Quinn, and Epstein 2005). One measure of identifying a justice's ideal point has been computed by Martin and Quinn (2002), and their scores for the median justice are thus included in the analysis.

Also, because the leadership style of the Chief Justice is thought to be a major factor in determining a Court's ability to find consensus in decisions (Caldeira and Zorn 1998; Epstein, Segal, and Spaeth 2001; Haynie 1992), I have included dummy variables accounting for each Chief justice that served during the time period under analysis. Cases decided during the Warren and Burger Courts are, as indicated in Table 2.1, coded 1, respectively, and the Rehnquist Court is treated as the baseline.[6]

---

[5] I searched for *New York Times* articles that include the phrase "Supreme Court" and either "oral arguments" or "heard arguments" in their text. I then read through the articles produced by the search and eliminated false positives.

[6] Because the editorial data span only the 1994 through 2004 terms, when the composition of the Court did not change, the Court specific variables are only included when front-page coverage in the *New York Times* is treated as the dependent variable.

Analysis

The first hypothesis, that, all else being equal, a case's media visibility increases with its level of division, gains strong support when using the *New York Times* measure. As we can see in Table 2.2, using a logistic regression, the negative relationship between the majority size and a ruling's coverage on the front page of the *Times* remains statistically significant under the most stringent significance tests. That is, even when controlling for a variety of other factors, opinions with smaller majorities (i.e., more divided decisions) are more likely to be on the front page of the *New York Times* than those with larger majorities (i.e., more unified decisions).

In fact, nearly every covariate in the model also yielded a statistically significant result. Perhaps justifying the concerns of those who claim the *New York Times* is politically biased, controlling for other factors, liberal rulings make the front page more often than their conservative counterparts. Also, rulings that have constitutional authority are also more likely to win a prestigious spot on the front page than those that only settle the meaning of statues. In addition, cases that declare unconstitutionality are significantly associated with front-page coverage compared to those that uphold policies; however, the relationship is only significant under a relaxed significance test (*p*-value = .053). Moreover, rulings that alter legal precedent are likely to gain more coverage than those that uphold the status quo. Similarly, self-assigned majority opinions are associated with higher levels of coverage than are those penned by more junior members of the Court, giving credence to the theory that senior justices prefer to maintain control over the most interesting cases.

Furthermore, rulings announced at the end of the Court's term are also more likely to make the front page than those decided at other times of the year, which is not surprising given the extra attention media outlets give to the Court during those weeks. Also, there is a very strong association between front-page coverage of a ruling and that case's prior appearance in the *Times* during the oral argument phase. This is potentially due to the influence of the journalist assigned to the case: after covering it at the oral argument phase, she may frequently push her editors to highlight her follow-up story on the final ruling.

Most of the issue area categories included in the regression also successfully predict front-page coverage. Decisions pertaining to civil rights, the First Amendment, privacy rights, and criminal procedure are all more likely to receive this high level of media visibility than those cases dealing with other issues not categorized in this model. Rulings on economic activity, however, are no more or less likely to receive this type of coverage than the remaining body of cases.

Interestingly, the position of the median justice also has no significant relationship with this form of news coverage. However, it does appear to matter who the Chief Justice is. The results indicate that cases decided during Earl Warren's tenure were more likely to make the front page of the *Times* than those decided during the Rehnquist Court terms included in this analysis. Additionally, cases decided during Warren Burger's tenure were less likely to receive this level of attention when compared to those after his retirement. However, whether this is a

result of the Chief Justices' leadership or changes in the public's interest in the Court's activities across different periods in time is hard to say given only these data.

Table 2.2. Logistic regression: Front-page coverage of Supreme Court decisions in the *New York Times,* 1953-1995 terms.

	Coeff.	S.E.
Simultaneous with decision		
Majority size	-1.504 ***	.242
Liberal decision	.238 **	.086
Declaration of unconstitutionality	.261 †	.135
Alteration of precedent	1.288 ***	.200
Self-assigned majority opinion	.271 **	.104
Decided at end of term	.256 **	.083
Prior to decision		
Constitutional authority	.642 ***	.093
Coverage of oral arguments	2.254 ***	.143
Issue area		
Civil rights	1.071 ***	.118
First Amendment	1.075 ***	.142
Privacy	1.429 ***	.342
Criminal procedure	.296 *	.122
Economic activity	-.043	.133
Court characteristics		
Ideal point of median justice	-.145	.095
Warren Court	.444 **	.138
Burger Court	-.325 **	.112
Constant	-1.622 ***	.248
N	5726	
LR χ^2	893.71***	
Pseudo R^2	.173	

† $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$

By turning to the predicted probability of front-page coverage, we can get a better picture of the magnitude of the effect that Supreme Court division has on this measure of media visibility. Figure 2.3 shows that, with the other variables held constant, a five-to-four decision has about a predicted 18 percent chance of becoming front page news. Thus, when the Court is unanimous, the predicted probability that a case will make the front page drops dramatically to 10 percent, just over half as likely as when the justices were very narrowly divided.

Figure 2.3. Predicted probabilities of front-page *New York Times* coverage by Court majority size.
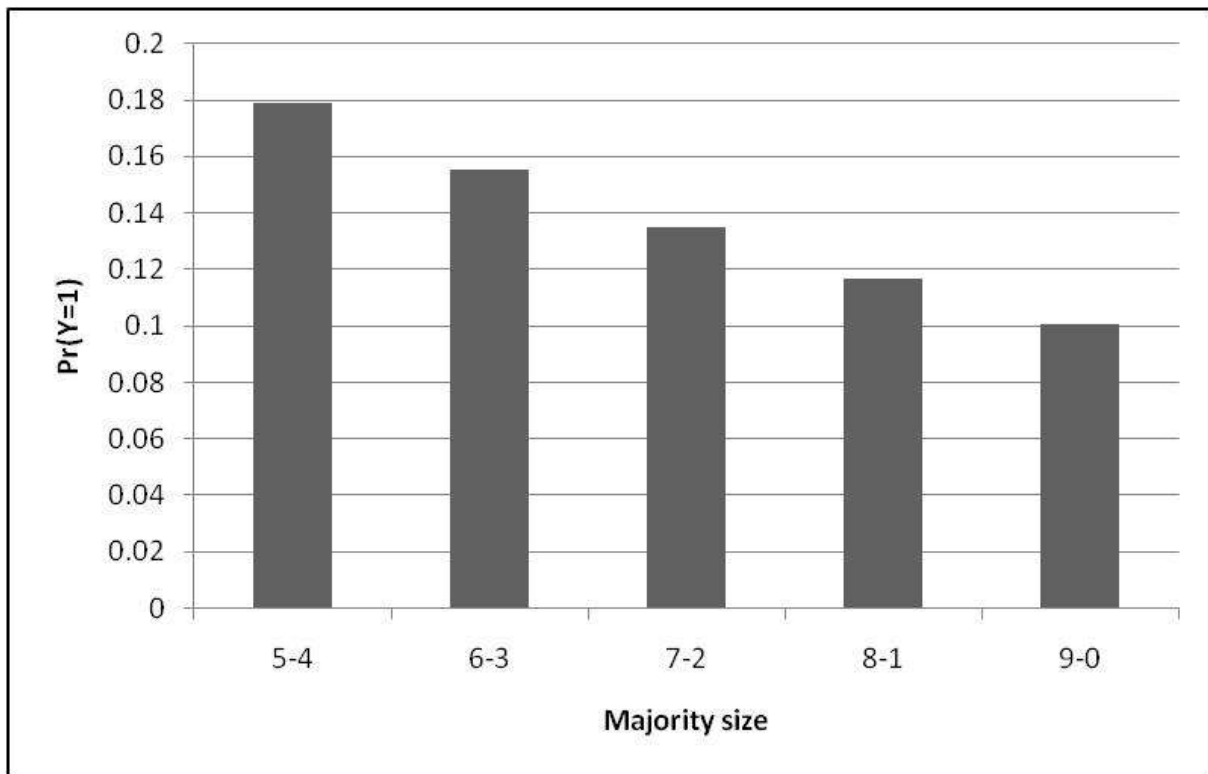
Turning to our other measure of visibility, we can see similar results in the regression analysis of decisions that are covered in newspaper editorials throughout the country. Given the high proportion of decisions that received no editorial coverage (58 percent of cases were not covered), I employ a zero-inflated negative binomial regression model to estimate the effects of the independent variables.[7] Table 2.3 shows that majority size does not seem to have

---

[7] I use a logit for the inflation stage of the model.

a significant impact on the specific number of editorials that a ruling receives, as indicated by the statistically insignificant coefficient at the count stage of the model; however, it does seem to be associated with whether or not the ruling receives *any* editorial coverage. The coefficient for majority size is positive and significant in the inflation stage of the model, indicating that larger majority sizes are associated with zero editorial coverage. Put otherwise, more closely divided decisions are more likely to receive some degree of editorial coverage, even when controlling for a variety of other factors.

Table 2.3. Zero-inflated negative binomial regression: Editorial coverage of non-unanimous Supreme Court decisions, 1994-2004 terms.

	Count		Zero-inflation	
	Coeff.	*S.E.*	*Coeff.*	*S.E.*
Simultaneous with decision				
Majority size	-.658	.678	-2.486*	1.208
Liberal decision	.283*	.142	-.171	.292
Declaration of unconstitutionality	.006	.194	-1.217	1.970
Alteration of precedent	.506	.327	-1.104	1.388
Self-assigned majority opinion	.583***	.150	.278	.319
Decided at end of term	.341*	.135	-.630*	.300
Prior to decision				
Constitutional authority	.473*	.185	-1.916***	.383
Coverage of oral arguments	.422**	.152	-2.388***	.735
Issue area				
Civil rights	.454*	.217	-.399	.408
First Amendment	.542*	.214	-2.165*	.909
Privacy	.721*	.332	-1.112	.986
Criminal procedure	-.208	.202	.114	.393
Economic activity	.269	.305	.329	.516
Constant	.710	.493	-.447	.919
N		628		
LR χ^2		86.54***		
Vuong test		5.16***		

† $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$

Several of the other independent variables were successful in this regression as well. Liberal decisions appear to be significantly associated with higher numbers of editorials that cover the ruling; however, liberal decisions are no more or less likely to receive any coverage at all than are conservative majority opinions. A similar relationship holds true between the dependent variable and whether or not the opinion of the Court was written by the senior-most justice in the majority coalition.

On the other hand, whether or not the Court exercises judicial review or alters precedent appear not to be significantly related to either the number of editorials written about the case or if any such editorials were written at all. Nevertheless, rulings that had constitutional authority are both more likely to receive editorial coverage and to attract higher numbers of editorials than statutory cases are. Likewise, cases decided at the end of the term and those that were covered at the oral argument stage are associated with both higher levels of coverage and fewer absences from the editorial pages.

Several issue areas are also significant predictors of editorial coverage. Decisions that rule on civil rights, the First Amendment, and privacy are all likely to gain the attention of larger numbers of newspaper editorial writers. However, only civil rights cases are more likely than the uncategorized body of cases to receive any editorial coverage. Moreover, criminal procedure and economic activity cases are not significantly associated with either the specific number of editorials or the mere presence of such a story.

Turning to two of our measures of the favorability of news coverage – the number of editorials that agree with the Court's majority and the number of editorials that disagree with that position – further illuminates the relationship between majority size and news commentary on the Court. Table 2.4 shows that there is no statistically significant relationship between majority size and pro-Court majority editorial coverage at either the count or inflation stage of the model. However, Table 2.5 shows that there is indeed a significant relationship between majority size and anti-majority editorial coverage, which is driving the relationship observed in Table 2.3. The coefficient at the inflation stage of this model clearly shows that more narrowly decided decisions are far more likely to attract at least one newspaper editorial criticizing the ruling. Furthermore, using a relaxed significance test, more divided rulings are also more likely to lead to higher numbers of critically newspaper commentary than more unified decisions are (p-value = .094).

In addition, these models show that liberal decisions, all else being equal, are more likely to receive favorable editorial coverage and can expect a larger number of editorials commenting favorably on the case. On the other hand, while liberal decisions are less likely to receive any negative feedback from newspaper editorial boards, there is no significant relationship between the ideological direction of a decision and the number of unfavorable columns a ruling receives.

Meanwhile, rulings that declare a law unconstitutional have no relationship with pro-majority editorial coverage, but they can expect significantly fewer anti-majority editorial

opinions. In either case, the exercise of judicial review is not a good predictor of whether or not a case receives editorial coverage at all. However, a case with constitutional authority is more likely to receive editorial coverage of any kind and can expect higher numbers of both positive and negative coverage. The alteration of precedent, on the other hand, does not accurately predict whether or not a ruling receives any favorable or unfavorable coverage, although it is associated with larger quantities of pro-majority editorials when the rulings are discussed on the opinion page.

Table 2.4. Zero-inflated negative binomial regression: Pro-majority editorial coverage of non-unanimous Supreme Court decisions, 1994-2004 terms.

	Count		Zero-inflation	
	Coeff.	*S.E.*	*Coeff.*	*S.E.*
Simultaneous with decision				
Majority size	.246	.681	.623	1.177
Liberal decision	.407 **	.150	-.805 **	.282
Declaration of unconstitutionality	.200	.192	-.741	.711
Alteration of precedent	.824 *	.376	.842	1.843
Self-assigned majority opinion	.728 ***	.155	.421	.314
Decided at end of term	.387 ***	.146	-.503 †	.284
Prior to decision				
Constitutional authority	.356 †	.207	-1.570 ***	.349
Coverage of oral arguments	.239	.153	-2.334 ***	.583
Issue area				
Civil rights	.714 ***	.247	-.284	.405
First Amendment	.626 **	.216	-1.441 *	.594
Privacy	.557 †	.327	-1.855 †	1.100
Criminal procedure	.085	.230	.174	.386
Economic activity	.430	.312	.026	.523
Constant	-.369	.537	1.488	.938
N			628	
LR χ^2			76.75 ***	
Vuong test			5.31 ***	

† *p* < .1, * *p* < .05, ** *p* < .01, *** *p* < .001

Table 2.5. Zero-inflated negative binomial regression: Anti-majority editorial coverage of non-unanimous Supreme Court decisions, 1994-2004 terms.

	Count		Zero-inflation	
	Coeff.	*S.E.*	*Coeff.*	*S.E.*
Simultaneous with decision				
Majority size	1.460†	.873	4.750**	1.452
Liberal decision	.001	.179	1.082**	.344
Declaration of unconstitutionality	-.542*	.230	-.391	.710
Alteration of precedent	.009	.325	-2.770	1.704
Self-assigned majority opinion	.521**	.174	.271	.365
Decided at end of term	-.115	.164	-1.185**	.372
Prior to decision				
Constitutional authority	.534*	.233	-1.375***	.370
Coverage of oral arguments	.408*	.182	-1.326**	.442
Issue area				
Civil rights	-.206	.255	-.881*	.443
First Amendment	-.252	.250	-3.690**	1.218
Privacy	.682†	.389	-.222	.781
Criminal procedure	.755**	.254	-.060	.432
Economic activity	.053	.449	.781	.609
Constant	1.443*	.622	-1.461	1.070
N			628	
LR χ^2			39.95 ***	
Vuong test			4.93 ***	

† *p* < .1, * *p* < .05, ** *p* < .01, *** *p* < .001

Interestingly, self-assigned opinions of the Court do not predict whether or not a decision receives either form of editorial coverage, but these opinions yield larger numbers of newspaper criticisms and praises than their junior-authored counterparts. Also, those cases that were covered when their arguments were heard are more likely to receive commentary from both sides of the spectrum. Still, oral argument coverage is not a good predictor of the specific number of favorable editorials; although, cases covered at this phase are likely to expect higher numbers of unfavorable editorials, perhaps suggesting that newspaper opinion writers begin bracing themselves for undesirable rulings well before the Court has officially announced its stance. Meanwhile, the reverse is true for those cases decided at the end of the

term: these decisions are more likely to receive some number of either form of editorial coverage; however, a statistically significant association between the number of editorials and being decided in June exists only for favorable coverage. This possibly indicates an enthusiasm for the Court among editorial writers who closely follow the justices' work during their busiest season.

Civil rights, First Amendment, and privacy rights decisions can expect larger numbers of favorable editorials than can cases dealing with other subjects, while only privacy cases can expect a larger number of favorable commentaries. Meanwhile, criminal procedure cases can expect significantly fewer editorials that disagree with the Court's holding. Civil rights decisions are more likely than other cases to receive at least one editorial opposing the Court's majority, while privacy cases can expect at least one in favor. Moreover, First Amendment cases can expect at least one editorial opinion from each side of the debate.

Figure 2.4. Predicted rates of editorial coverage by majority size.
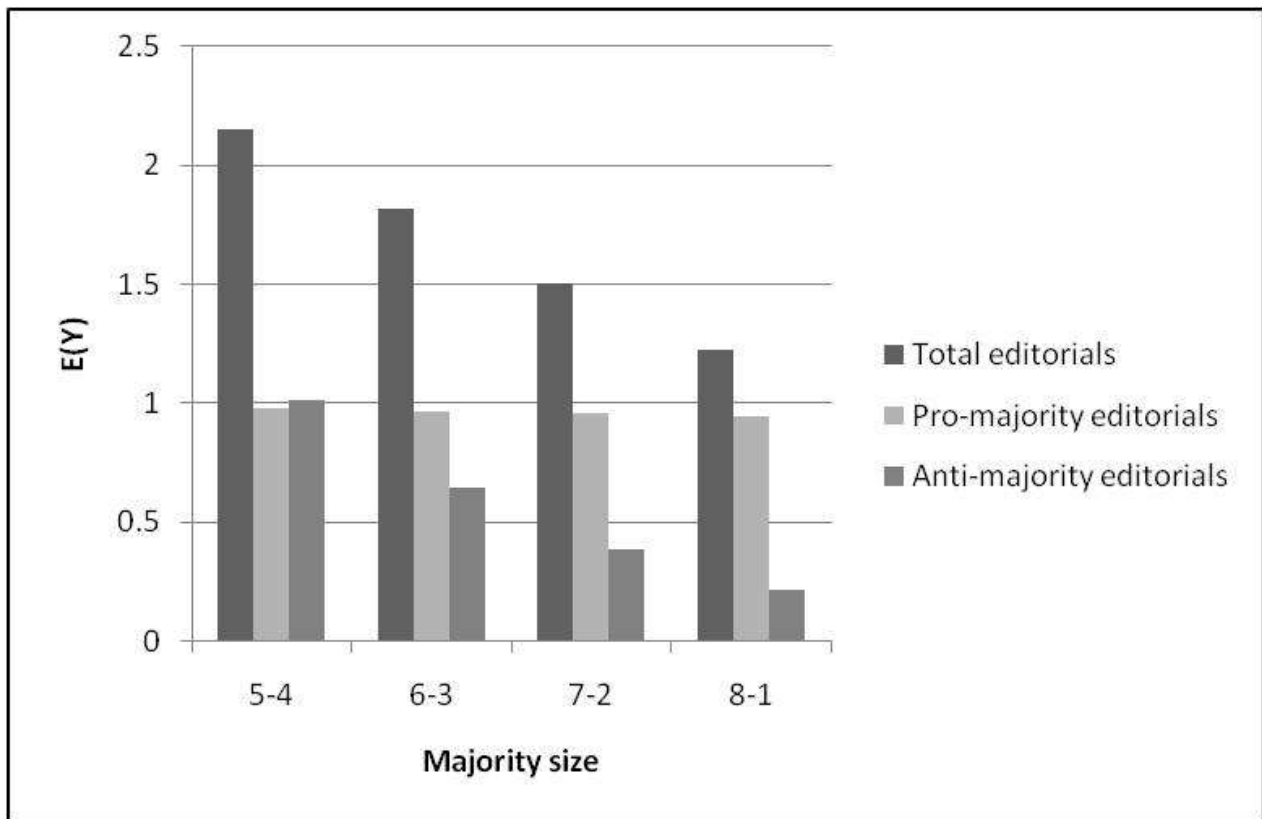
Returning to the effect of majority size, we can again get a more substantive understanding of its impact on editorial coverage by looking at these models' predicted rates of coverage. As seen in Figure 2.4, the predicted rate of total editorial coverage declines steadily

as the majority size on the Court increases. Holding all other factors constant, the expected number of total editorials covering a five-to-four decision is 2.15. However, this value is almost cut in half for an eight-to-one decision, which can expect only 1.22 editorials. The decline is even steeper looking at only anti-majority editorials; eight-to-one opinions can expect roughly one-fifth of this type of coverage that a five-to-four decision receives. On the contrary, expected pro-majority coverage across majority size does decline modestly, but the difference between each level of unity is negligible and statistically insignificant.

Table 2.6. OLS regression: Proportion of pro-majority editorials covering non-unanimous Supreme Court decisions, 1994-2004 terms.

	Coeff.	S.E.
Simultaneous with decision		
Majority size	.536**	.159
Liberal decision	.146**	.035
Declaration of unconstitutionality	.030	.046
Alteration of precedent	.019	.100
Self-assigned majority opinion	.030	.037
Decided at end of term	-.014	.034
Prior to decision		
Constitutional authority	-.010	.044
Coverage of oral arguments	-.040	.038
Issue area		
Civil rights	.075	.053
First Amendment	-.022	.053
Privacy	-.035	.082
Criminal procedure	.093†	.051
Economic activity	.074	.072
Constant	.306*	.120
N	209	
F	3.83***	
R^2	.204	

$†\ p < .1,\ *\ p < .05,\ **\ p < .01,\ ***\ p < .001$

Among non-unanimous cases in this period that *did* receive at least one editorial favoring or opposing the majority, there is a relationship between the size of the majority and

the proportion of favorable editorials. In the linear regression model shown in Table 2.6, the relationship between majority size and our measure of editorial positivity (i.e., the number of favorable editorials divided by the total number of editorials) is statistically significant and above zero. It is worth noting that, given the dependent variable used in this regression analysis, *the population of cases evaluated here are only those that are covered by at least one paper* (hence the drop from 628 cases in the previous regressions to 209 here). Thus, as the majority grows, so does the proportion of favorable editorials covering the case.

The ideological direction of a decision appears to have a similar, albeit smaller, effect. The coefficient here is positive as well, meaning that the percentage of editorials that are favorable toward the majority is higher for liberal cases than it is for those that are conservative. Criminal procedure rulings are also slightly more likely than other cases to receive a higher proportion of favorable coverage. However, no other covariate in this regression model produces a significant result.

Discussion of Quantitative Results

The hypothesis that Supreme Court decisions with many dissents are more visible than those with fewer – or no – dissents is supported by the results above, as is the hypothesis that decisions with higher levels of consensus are covered more favorably. Cases with smaller majority sizes are significantly related to coverage on the front page of the *New York Times* and in newspaper editorials across the country. Further, they are also associated with higher numbers of articles that editorialize against the Court's majority, while rulings decided with higher levels of unity receive a higher percentage of favorable news commentary when they are covered. All of these results are found after controlling for a variety of factors that can impact both the newsworthiness and legal controversy of a case. In other words, the correlations that Ho and Quinn and Epstein and Segal note in passing withstand at least a certain degree of scrutiny.

One can easily imagine a causal scenario consistent with these results. It is possible that reporters view division among the justices as a cue that the case is newsworthy and thereby give it more prominent coverage. Meanwhile, they may read a unanimous or nearly unanimous vote as the opposite cue: that this case was a foregone conclusion and is not groundbreaking news. In other words, reporters may use majority size as a heuristic for the substantive importance of a ruling *even when it shouldn't be taken as such*. Additionally, it is conceivable that reporters and editors are drawn to heavily divided cases because the conflict between the majority and the dissenters makes for an engaging story. Whichever the case may be, given the fact that the relationship between majority size and media visibility and favorability is robust against a litany of potential confounding variables supports the claim that this is not simply a spurious correlation and that the media's behavior toward division among the justices is more complex than meets the eye.

A Tale of Two Cases

If indeed the news media are independently attracted to division in Supreme Court cases, as the quantitative analysis above suggests, then we ought to be able to point to examples of cases where reporters expressed little interest until the Court announced its narrowly divided vote. Conversely, there should also be examples of cases that did grab the media's attention as they were working their way up toward the final decision, but then caused journalists to lose interest when they learned of the high level of consensus on the Court. *Kelo v. City of New London* (2005) provides an example of the former phenomenon, while *Branch v. Smith* (2003) illustrates the latter.

Kelo v. City of New London

The Court announced its ruling in *Kelo*, an eminent domain case, on June 23, 2005. A narrow majority led by Justice Stevens ruled that the local government could allow private corporations take private property, with just compensation, for the purpose of economic development. The five-to-four decision immediately made headlines. Looking at measures utilized in the analyses above, *Kelo* certainly received the type of attention predicted for a bitterly divided decision: the ruling was covered on the front page of the *New York Times* and was editorialized in 15 of the 23 papers indexed by Ho and Quinn (the average ruling is covered by 2.4 newspaper editorials). The decision also received the type of negative editorial coverage that the model above predicts for close rulings: 13 of the 15 editorials were critical of the Court's majority. Furthermore, *Kelo* sparked a wave of political debate and activity: ballot measures limiting eminent domain and asserting the rights of property owners were approved by voters in eight states in 2006 (Pristin 2006).

However, the dispute between the displaced New London residents and their municipal government did not appear to be a national new story in waiting. Prior to the Court's ruling, the case received little attention. Unlike a number of other cases decided that term, the oral arguments in this case were not covered by the *New York Times*. Though the case was not absent from the *Times*' agenda, it was primarily covered as a local issue in the "Metro" section of the paper (Peterson 2005; Pristin 2004). Thus, the case could be considered moderately salient at best. Furthermore, most legal scholars, viewing this case as the predictable outcome of prior imminent domain case law, foresaw the ruling in favor of the city (Nadler, Diamond, and Patton 2008). In other words, the case not only flew below the radar of the national media, but its outcome was also a foregone conclusion.

After the decision, media visibility of the case and the surrounding issue greatly increased. Not only did the ruling itself receive plenty of national press and backlash as mentioned above, but it also coincided with an uptick in journalistic interest in eminent domain policy. As shown in Figure 2.5, eminent domain was an issue discussed in several news media outlets in the portion of 2005 leading up to the *Kelo* ruling. However, after the ruling, there appeared to be a remarkable increase in the number of stories discussing eminent domain for

the remainder of the year. It should be noted that June 24, 2005, is excluded from the graph below; this is the day after the *Kelo* decision was announced and thus when most newspapers would run stories covering the Court's action in the case.

Figure 2.5. News stories covering eminent domain before and after *Kelo v. City of New London*.
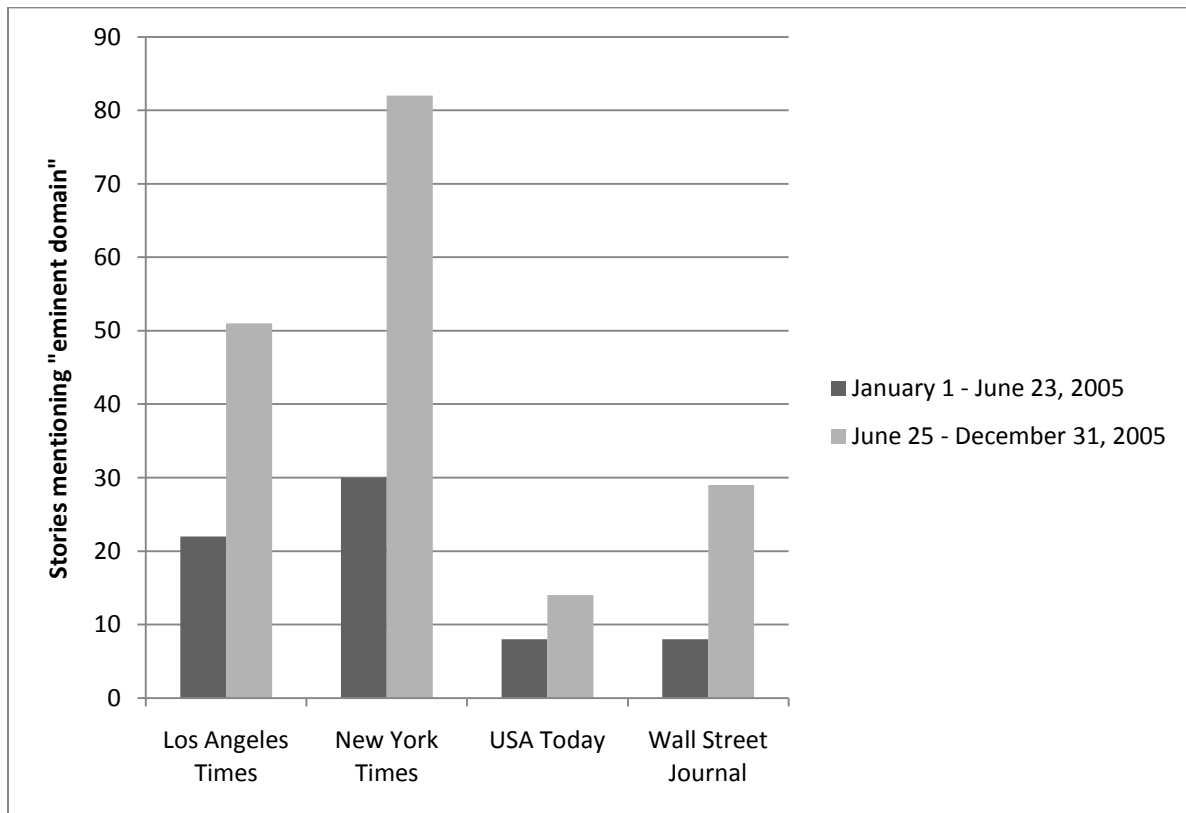
Sources: Proquest, Lexis-Nexis

Branch v. Smith

The news coverage of *Branch v. Smith* tells a somewhat different story. When the case was heard by the Supreme Court on December 10, 2002, the *New York Times* ran a story calling it "the most highly political case to reach the court since Bush v. Gore was decided two years ago" (Rosenbaum 2002). The dispute in this case arose when the state of Mississippi failed to pass a congressional redistricting plan after the 2000 census. Mississippi lost a seat in the House of Representatives, meaning that any redistricting plan would cost an incumbent member of Congress his seat. In response, a battle for the right to draw the state's district map, and ultimately settle who would continue to serve in Congress, broke out between state and federal

judges. Ultimately, the Supreme Court, with a whopping seven-vote majority, ruled in favor of the federal judges.

Although the *Times* article covering the oral arguments noted that this case was rife with political consequences, the largely unified decision barely received a mention from the paper when the ruling was announced: after the decision, an article covering the oral arguments of a case, argued the same day that *Branch* was decided, closes with two sentences summarizing the Court's ruling in the redistricting case (Greenhouse 2003). Thus, as the degree of controversy surrounding the case that was initially observed at the oral argument phase failed to stir similarly lively debate among the justices, the case soon became an afterthought for Supreme Court reporters. As such, this ruling, in spite of its electoral consequences, also failed to attract any editorial coverage from the papers indexed by Ho and Quinn. Moreover, as Table 2.6 shows, the case is not associated with any change in the salience of the issue involved as *Kelo* was.

Figure 2.6. News stories covering redistricting before and after *Branch v. Smith.*
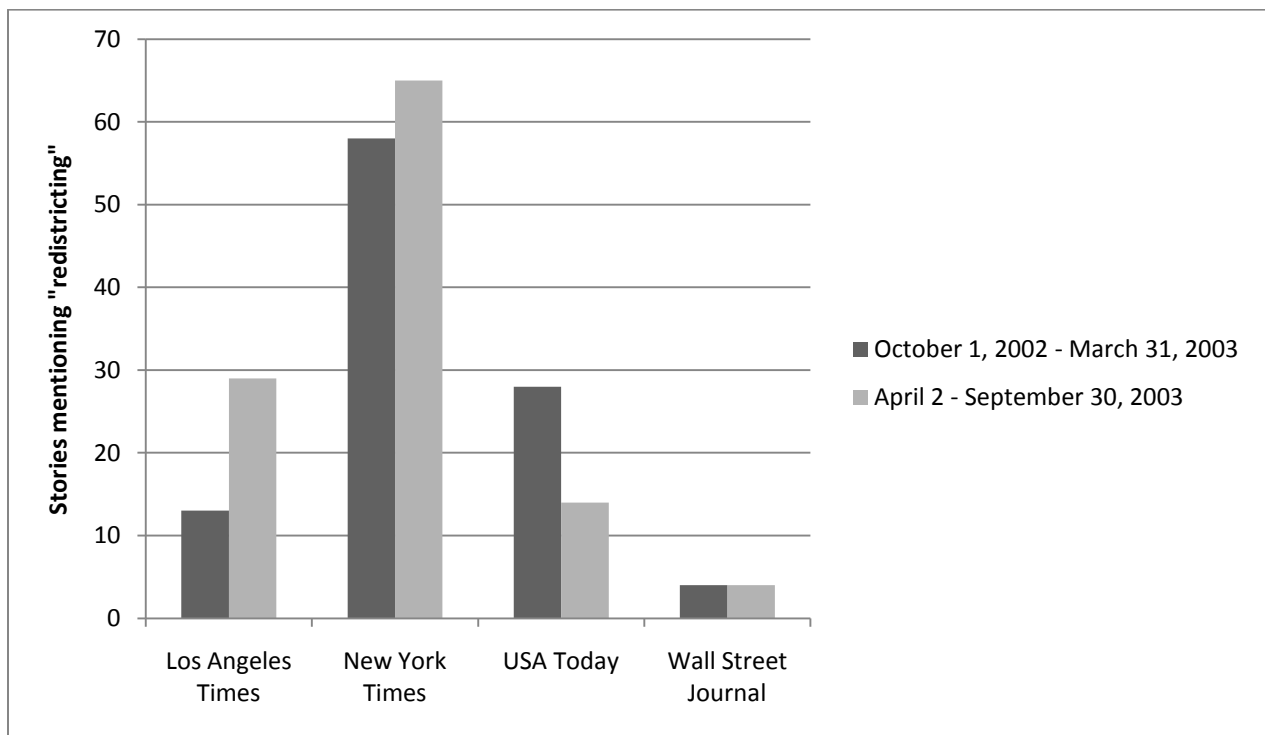
Sources: Proquest, Lexis-Nexis.

Summary

While these anecdotal examples do not conclusively prove the phenomena that the statistical analysis suggests, they are consistent with such a story and therefore add credence to the notion that majority size, independent of all the other factors for which majority size may be a proxy, is a key factor in predicting the visibility and favorability of news coverage of a Supreme Court ruling. While it is important to note that the analysis in this chapter demonstrates that the level of judicial division is just one of several factors that can affect a decision's news presence, it is equally important understand that these cases do not gather more media attention *because* of these other factors. Division itself, somehow, is compelling to journalists beyond its role in the other characteristics of the case.

As such, these results imply that the general public is disproportionately informed about cases that stir divisiveness on the Court. That is, most of what people are likely to hear about the Court portrays it as being internally conflicted, and many of these reports will be at least somewhat critical. However, this alone does not necessarily suggest that this forces a negative view of the Court upon the public. By establishing that division affects media behavior, it opens the door to the question of how public information about judicial unanimity and dissent affects mass behavior. Chapter 3 and 4 focus on this issue.

Chapter 3

Conditional Perceptions:

Public Opinion of Large and Small Majorities by Issue Salience

Assessing media behavior is a necessary though insufficient component of understanding the public's perception of judicial unanimity and dissent. Although we now know that the news media has a proclivity toward covering and criticizing divided cases, the question remains: *How do individuals digest and internalize information about judicial unity once they receive it?* As previously noted, the little scholarly research addressing this question has varied in its answer. For 25 years, this slow-moving literature's response was that public attitudes toward the Supreme Court and its rulings are unaffected by majority size (Gibson, Caldeira, and Spence 2005; Marshall 1987, 1989; Peterson 1981). However, the most recent such study, which bears the most thoughtful research design, found that larger court majorities can in fact bolster public opinion of court decisions (Zink, Spriggs, and Scott 2009). Furthermore, Zink and his colleagues find this effect to hold true across cases with varying levels of salience.

Although Zink, Spriggs, and Scott's work is truly groundbreaking, this chapter argues that the subtle flaws in their research design and sample population mask the true effect that unanimity and dissent have on public opinion. While this chapter finds evidence that majority size has an impact on public attitudes toward judicial decisions, the results show that this effect varies – in both its magnitude and direction – with the salience of the issue involved. Specifically, I find that the public is unmoved by the majority size in highly salient decisions, that those predisposed to oppose the court are more receptive to divided cases that are of medium salience, and that large majorities in cases with low salience can move public attitudes in the direction of the decision.

I begin with a thorough account of the prior work on this topic, particularly that which utilizes experimental research designs to gain leverage on the question. From there, I respond to the gaps in this stream of literature with a refined set of hypotheses and an improved experimental design. Following that, I present the experimental results, discuss statistical manipulation of those results, and present the findings of the statistical analysis.

Prior Experimental Study of the Response to Judicial Consensus

As noted in Chapter 1, Gibson, Caldeira, and Spence (2005) were the first to introduce an experimental research design to the study court majority size. However, the scope of their study was much broader than merely establishing the impact of judicial unanimity and dissent. Their paper broadly asked why individuals accept policies with which they disagree. In doing so, they surveyed a random sample of respondents about their reaction to policies regarding how to dispose of the ballots cast in Florida during the disputed 2000 presidential election. If a

respondent favored the destruction of the ballots, she was told, "Suppose that later this year, a group of citizens comes forward demanding that a special governmental panel be established to take custody of the ballots from Florida and make them available to be counted. ... The ballots are then made available to be counted." Meanwhile, respondents who favored the preservation of the ballots were told, "Suppose that later this year, a group of citizens comes forward demanding that a special governmental panel be established to take custody of the ballots from Florida and prevent them from being counted. ... The ballots are then impounded so that they cannot be counted" (Gibson, Caldeira, and Spence 2005, 188).

Also included in these vignettes were experimental manipulations designed to assess hypotheses regarding institutional legitimacy, partisanship in the decision making process, the rule of law, and the ultimate consequences of the decision. The authors' operationalization of decisional partisanship involved telling some respondents, at random, that the policy regarding the ballots was reached with near unanimity (i.e., almost every judge agreed with the decision) or that it was split along party lines. They, like Chief Justice Warren and others, hypothesized that unity among the judges would lead to higher levels of acquiescence to the decision. Although they found evidence supporting many of their hypotheses, the authors came up empty on their partisanship hypothesis: following their statistical analysis of the experimental survey results, the authors conclude, "With some exceptions, whether a decision is consensual or divided by partisanship seems to have few consequences for acquiescence. Many scholars believe that sharp splits in Court decisions substantially delegitimized those outcomes. We find little evidence for that proposition" (Gibson, Caldeira, and Spence 2005, 197). As such, their results maintained consistency with Peterson's (1981) survey of the literature and Marshall's (1987) empirical findings.

Unconvinced, Zink, Spriggs, and Scott (2009, 912) directly and critically respond to this finding:

> [Gibson, Caldeira, and Spence] conducted their experimental study of a controversial decision regarding a Presidential election immediately in the wake of *Bush v. Gore* (2000) and used factual circumstances in their experimental vignettes nearly identical to those of that case. The content and timing of their study raises the possibility that residual, polarized attitudes surrounding the controversial 2000 presidential election might contaminate the effects of any experimental manipulations in their hypothetical scenario.

Thus, Zink and his colleagues conducted a study of public reaction to the Court using a series of more general, less time-sensitive, hypothetical issues: abortion, school prayer, and bankruptcy. The authors chose these three issues because they believe they represent a range of ideological salience, with abortion being the most salient issue and bankruptcy the least.

They designed their experiment as follows. Respondents were asked a series of questions to establish their *ex ante* positions on the issues listed above. They were then presented with a series of vignettes, which were randomly manipulated to report that the court

was either divided or unanimous in reaching its ruling on the issue. In addition, the ideological direction of the decision was also varied at random. Moreover, their study sought to understand the effect of the treatment of precedent on public opinion as well, so the vignettes also reported that the Court either followed or overruled precedent when deciding the case (this was also varied at random). Following each vignette, the respondents were asked whether they agreed or disagreed with the Court's ruling. They were also asked whether they accepted the ruling as the final word on the matter or felt that it ought to be challenged.

Their findings with regard to the level of consensus on the Court break from prior work. Generally, they find that, among *ex ante* supporters of the policy decisions (i.e., those who, prior to treatment, held a position in line with the Court's ruling), unanimity bolstered support. In other words, when one is already inclined to agree with the Court's ruling, unanimity increases the strength of their agreement. This relationship is not observed among *ex ante* opponents of the decision. It is, however, present in each issue area studied, suggesting that the presence of this effect is not dependent on the political salience of the case.

Though groundbreaking, Zink, Spriggs, and Scott's experiments fall short in several ways. First, the respondents were exclusively undergraduate political science students at the University of California, Davis. While resource restrictions often dictate undergraduate samples, the lack of diversity in age, education, political interest, and geography hurts the external validity of such studies. Clearly, a more representative sample should be attained whenever possible. Second, by combining hypotheses regarding both majority coalition size and treatment of legal precedent into a single experiment, the researchers run the risk of testing not the effect of each element of judicial behavior individually but instead the interaction between the two. Zink, Spriggs, and Scott's conclusions about the effect of coalition size can only be applied independently of the use of precedent if the two considerations are additive not interactive. That is, applying these conclusions to majority size alone assumes that respondents, who were presented with information about the Court's level of unity and treatment of precedent simultaneously, psychologically process each of these treatments independently of one another. Without any evidence pointing to the fact that coalition size does not interact with treatment of precedent when effecting public opinion, this is not necessarily a safe assumption.

Third, by dichotomizing the majority size variable, the researchers overlook an important nuance in the popular understanding of how judicial division affects public attitudes. Of course, the Supreme Court has nine justices, and a divided decision can have as little as one and as many as four dissenters. This distinction is important due to the popular attention given to five-to-four decisions. As noted in Chapter 2, popular concerns regarding judicial dissensus has largely been focused on those cases where a single vote controls the ruling. However, this is not entirely the same as the legal truism considered by Warren in *Brown*; after all, Warren was determined to make *Brown* a unanimous decision, not one with simply a large majority. In other words, an eight-to-one decision would not have satisfied Warren, though it would satisfy the many contemporary public commentators who lament the sharp, narrow division on the Court. By failing to distinguish between a minimum winning coalition and a large but non-

unanimous majority, Zink and his colleagues conflate these two lines of conventional wisdom and thus fail to officiate between the two. As such, a key element in how majority size may manipulate public opinion is unobservable in their analysis.

Hypotheses and Experimental Design

This chapter addresses all three of the concerns mentioned above. First, respondents are drawn from a nationally representative sample instead of from an exclusively undergraduate population (see Appendix A for a detailed description of the sample population).[8] Second, majority size is explicitly considered independently of other factors. Third, respondents are randomly assigned to one of four, rather than two, treatment groups based on majority size: unanimous, eight-to-one, five-to-four, and a baseline group where no information is given regarding majority size. (Details of the treatment specifications and survey instruments are described below.)

Hypotheses

The conventional wisdoms that have emerged separately from the legal community and from the world of media pundits, political commentators, and public officials essentially form two competing hypotheses. We can label these the "pro-unanimity" hypothesis and the "anti-division" hypothesis, respectively.

The pro-unanimity hypothesis is modeled after the behavior and sentiments of Earl Warren and likeminded jurists. Stated simply, the pro-unanimity hypothesis argues that, *all else being equal, unanimity will increase support for Supreme Court decisions*. The corollary according to this hypothesis is that *any dissent can be harmful to the perception of the Court's decision*. While it would stand to reason that more dissent is even more harmful, allowing even a lone dissenter to voice her opinion can fuel resistance to the majority's ruling. Thus, if we were to imagine an individual's baseline level of support for a judicial decision (i.e., a level of support for a decision independent of how the justices divide themselves), support for a unanimous ruling would be greater than the baseline level, while all levels of division would be lower than it. These predicted levels of support, relative to one another, are represented graphically in Figure 3.1.

---

[8] The survey data utilized in this dissertation was compiled by YouGov Polimetrix, a professional survey research company, via a nationwide internet survey.

Figure 3.1. Predicted relative level of support for Supreme Court decisions by majority size: pro-unanimity hypothesis.

Less supportive   More Supportive

5-4   8-1   baseline   unanimous

The anti-division hypothesis, on the other hand, makes a slightly different prediction. It argues that, *all else being equal, close division will decrease support for Supreme Court decisions*. This view, put forth primarily by media pundits and political commentators, is particularly concerned about closely divided decisions because they indicate that public policy outcomes may hang on the political whims of the median justice. However, *these concerns would be assuaged by larger non-unanimous majorities*. That is, while the previous perspective views the lone dissent in an eight-to-one decision as a potential rallying cry for Court opponents, this hypothesis suggests that such a decision is almost as good as unanimous and represents a near consensus on the Court. Thus, according to this hypothesis, all large majorities would attain above-baseline support while only narrowly divided cases would lead to more negative public opinion. Figure 3.2 graphs these predicted effects.

Figure 3.2. Predicted relative level of support for Supreme Court decisions by majority size: anti-division hypothesis.

Less supportive   More Supportive

5-4   baseline   8-1   unanimous

Of course, other hypotheses are plausible as well. For instance, reaction to the level of consensus on the Court may be a direct reflection of one's political attitudes toward the issue at hand. That is, *those who are predisposed to agree with the Court's decision may feel more favorably toward the ruling if the majority is large*, as this indicates a Court largely in sync with their views. On the other hand, *those who are predisposed to oppose the Court's ruling may have more favorable attitudes toward a case rife with dissent*, since this shows them that, even though their side lost, their voice was heard and that at least some of the decision makers took their side. In other words, under this scenario dissent may represent evidence of procedural

fairness and therefore make the decision more palatable to the Court's opponents (Lind and Tyler 1988). We can label this the "political agreement" hypothesis. Given Zink, Spriggs, and Scott's findings – that unanimity boosts support among those predisposed to agree with the Court – this hypothesis is highly plausible.

However, with respect to the relative position of the baseline level of support, we can think of two versions of the political agreement hypothesis based on the previous two hypotheses. In other words, we can imagine a unanimity-based political agreement hypothesis, where only unanimity boosts the support for the decision among *ex ante* supporters and only unanimity hurts support among opponents, and a division-based political agreement hypothesis, where only narrow division on the Court hurts the ruling's standing among supporters but helps among opponents. These hypotheses are represented graphically in Figures 3.3 and 3.4.

Figure 3.3. Predicted relative support for Supreme Court decisions by majority size: political agreement hypothesis (unanimity-based).
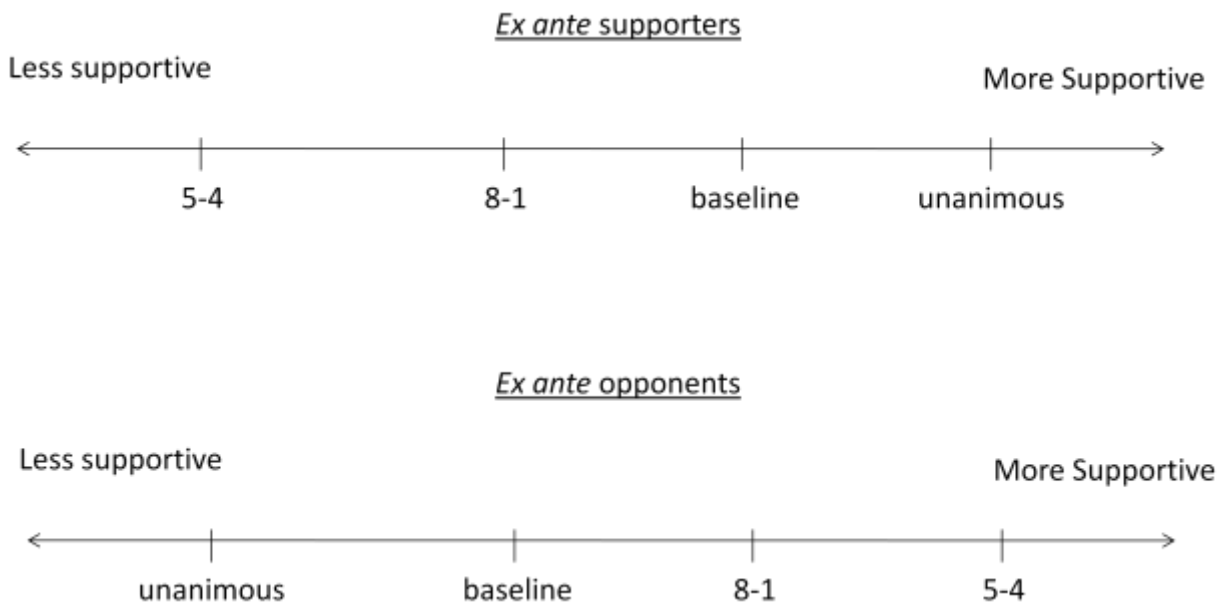
Figure 3.4. Predicted relative support for Supreme Court decisions by majority size: political agreement hypothesis (division-based).

Ex ante supporters

Less supportive   More Supportive

←————————+————————+————————+————————+————————→
                 5-4   baseline   8-1   unanimous

Ex ante opponents

Less supportive   More Supportive

←————————+————————+————————+————————+————————→
             unanimous   8-1   baseline   5-4
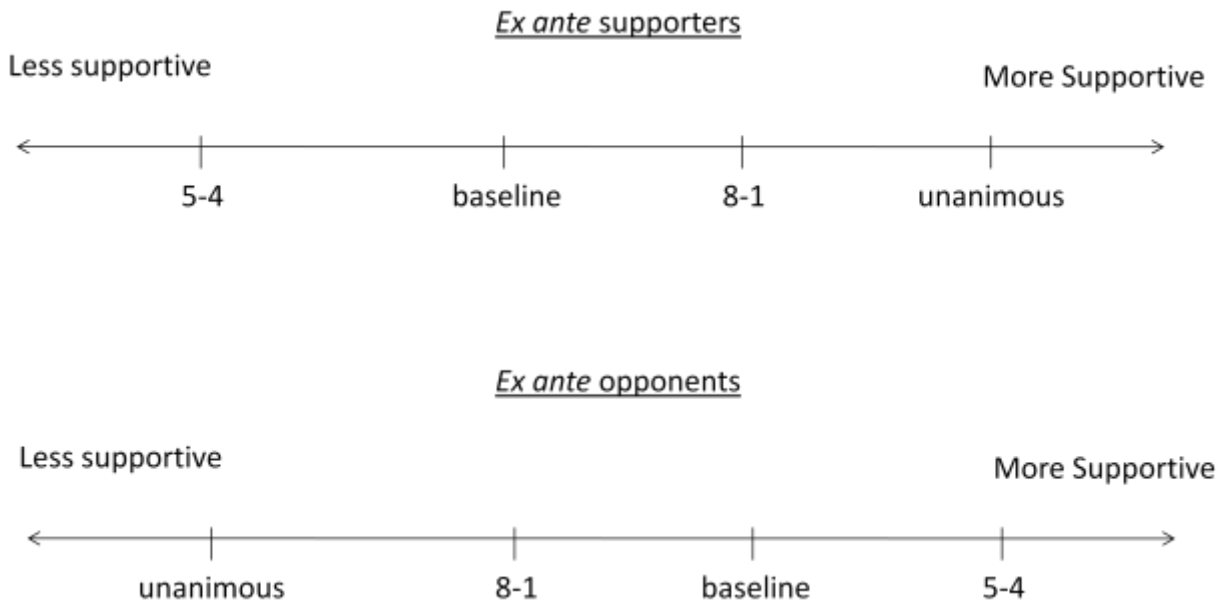
Experimental Design

        Much like the experimental design conducted by Zink and his colleagues, the experimental survey instrument used here consisted of three parts: a pre-treatment questionnaire, a series of vignettes describing (fabricated) Supreme Court cases, and a post-treatment questionnaire following each vignette. As mentioned above, unlike that used by Zink, Spriggs, and Scott, the treatment groups are designed to focus only on the majority size of the court and are specified in such a way to gain leverage on the hypotheses described above. Furthermore, like Zink and his colleagues, this experiment utilizes three issue areas that represent a range of ideological salience. Whereas, Zink, Spriggs, and Scott chose abortion (high salience), school prayer (medium salience), and bankruptcy (low salience), respondents in this experiment received vignettes about gay rights (high salience), employee privacy (medium salience), and contract dispute resolution (low salience). Including issues that capture varying levels of salience is important not just because it allows for a complete replication of Zink and his colleagues' work but because studies of social psychology (Petty and Cacioppo 1986), political behavior (Zaller 1992), and public attitudes toward courts (Brickman and Peterson 2006; Johnson and Martin 1998) have found that the ones ability to be persuaded (by a set of judges, for example) is dependent on how crystallized their prior attitudes are. Specifically, these findings suggest that individuals are difficult to persuade when they are entrenched in their opinions on a topic but are persuadable when they are only moderately engaged in the topic.

The experimental survey was completed by 600 respondents from across the country via the internet. To establish their *ex ante* positions on the above issues, they were first asked to complete a questionnaire that included questions about their positions on those issues. However, to avoid priming the respondents, the questionnaire also included a number of "distracter" questions asking for their position on issues that were not to be covered in the experiment. (See Appendix B for questionnaire wording.)

In addition to the questions on their policy preferences, the respondents were asked to answer a series of questions regarding their institutional support for the Supreme Court. These questions are designed to capture the respondents' diffuse support for the Court (see, Caldeira and Gibson 1992; Gibson, Caldeira, and Spence 2003a) and are commonly used in the literature (Gibson, Caldeira, and Baird 1998; Gibson, Caldeira, and Spence 2005; Zink, Spriggs, and Scott 2009). Because individuals are most easily persuaded by those people and institutions that they find to be credible (Chaiken 1980; Petty, Cacioppo, and Goldman 1981), diffuse support is an important consideration in this analysis: those with little faith in the institution are unlikely to change their attitudes based on what the Court decides, regardless of how strong the vote among the justices is.

After completing the questionnaire, the respondents were then exposed to a series of short vignettes about a Supreme Court decision. Though each vignette was presented as a factual statement about the recent activity of the Court, they were, in fact, fabricated (though loosely based upon actual and potential cases).[9] The vignettes were also manipulated to state that the Court was unanimous, divided eight-to-one, or divided five-to-four in its decision; furthermore, some respondents received a vignette that contained no information regarding the majority size (to be used as the baseline treatment group). In addition to randomly varying the majority size of the decision, the experiment also randomly varied the direction of ruling to ensure that the results are not driven by an ideological preference for or against the decision. Thus, examples of the vignettes include, "By a vote of 8 to 1, the United States Supreme Court recently ruled that states may ban same sex unions, which grant homosexual couples many of the rights given to married couples," "By a unanimous vote, the United States Supreme Court recently ruled that employers may not read text messages on their employees' company issued phones," and, "The United States Supreme Court recently ruled that people disputing contracts must pursue mediation or arbitration before resorting to lawsuits."[10] A complete list of vignettes is available in Appendix B.

Following each vignette, respondents were then asked if they agreed with the Court's ruling in the case. If they did not agree, they were asked if they accepted the ruling and felt it

---

[9] Following the experiment, the respondents were debriefed about the factual inaccuracy of the vignettes.

[10] The order or the vignettes was also randomized to ensure that receiving one particular vignette before another did not color the results.

ought to be considered the final word on the matter or if they instead thought that there ought to be an effort to challenge the ruling and get it changed.[11]

Experimental Results

The survey results reveal several fascinating trends. First and foremost, the respondents corroborate the research design's assumption regarding the relative salience of each issue. Looking at Table 3.1, we can see that among both *ex ante* opponents and supporters, the average strength of the respondents' *ex ante* position on the topic declines along with the level of salience. It is worth remembering that, since the outcome of the Court decision was randomly varied from respondent to respondent, *ex ante* support and opposition does not denote a specific ideology or policy preference; it only indicates whether the position of the respondent prior to treatment was the same as that described in the vignette randomly assigned to her. More than two-thirds of those predisposed to agree and nearly three-fourths of those predisposed to disagree with the Court's decision on same-sex unions were strongly predisposed to do so. Meanwhile just more than half of both *ex ante* supporters and opponents of the Court's decision on employee privacy held a strong prior position, and this figure drops to about one-fourth for the issue of contract dispute resolution.

Table 3.1. Percentage of respondents holding strong *ex ante* positions.

Issue	*Ex ante* supporters	*Ex ante* opponents
Same-sex unions	68.75%	74.62%
Employee privacy	51.88%	53.67%
Contract dispute resolution	25.79%	23.48%

Looking at the rate of agreement with and acceptance of the decisions among the respondents, it appears the most marked difference is not between treatment groups but between those who support the Court's decision *ex ante* and those who oppose it. As illustrated in Figure 3.5, the percentage of *ex ante* supportive respondents who report to agree with the Court's decision on same-sex unions varies very little by treatment group; the least support comes from those assigned to an eight-to-one group (76 percent of respondents agree

---

[11] It is assumed that those who agree with the decision also accept it.

with the Court's ruling), while the most support comes from the baseline treatment group (83 percent of respondents agree). There similarly little variation in agreement by treatment groups among ex ante opponents: support ranges from 10 percent agreement (five-to-four) to 19 percent (unanimous). Thus, while the treatment groups appear to be clustered around the same levels of agreement, there is a noticeable difference between *ex ante* supporters and opponents, supporting the widely understood belief that prior opinion on an issue is a key factor in determining positive feelings toward judicial rulings.

Turning to acceptance, rather than agreement, as a measure of support of the Court's rulings, we can observe similar trends. Again, as shown in Figure 3.6, *ex ante* supportive and opposing groups differ strongly from one another in their level of support for the Court's decision, but the treatment groups within each subsample have very similar acceptance rates. Among *ex ante* supporters, acceptance rates range from 85 (baseline) to 92 percent (unanimous), while they range from 25 (eight-to-one) to 30 percent (five-to-four) among *ex ante* opponents.

Figure 3.5. Agreement with Court decision by treatment group: same-sex unions.
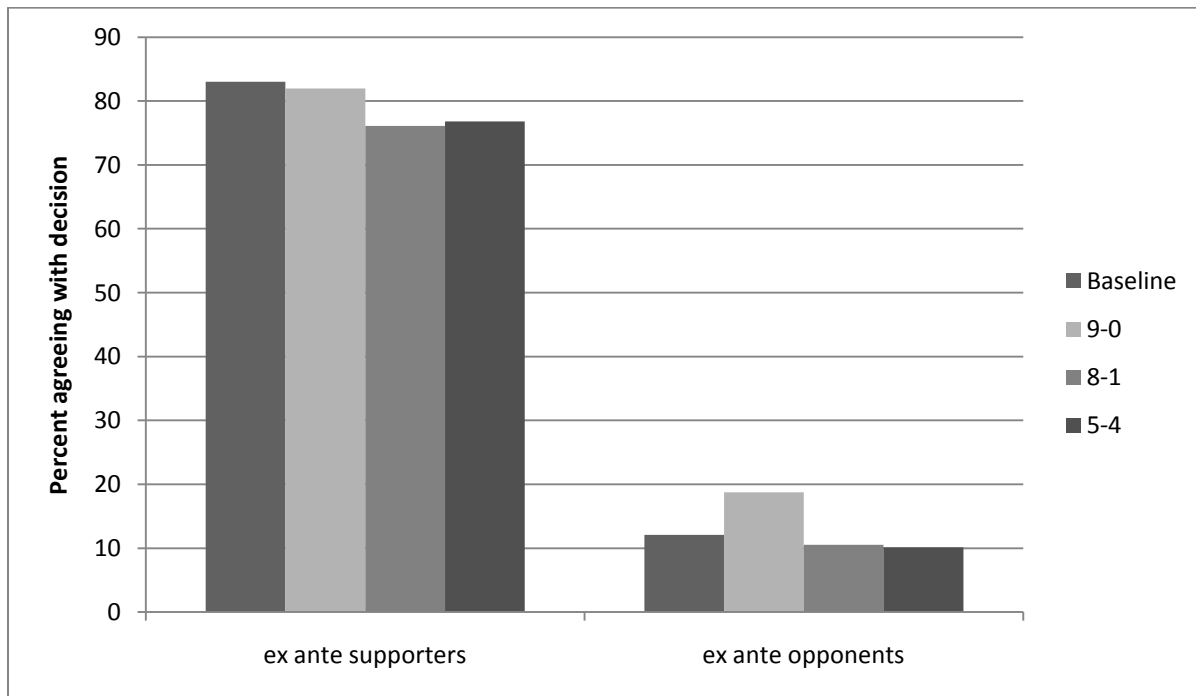
Figure 3.6. Acceptance of Court decision by treatment group: same-sex unions.
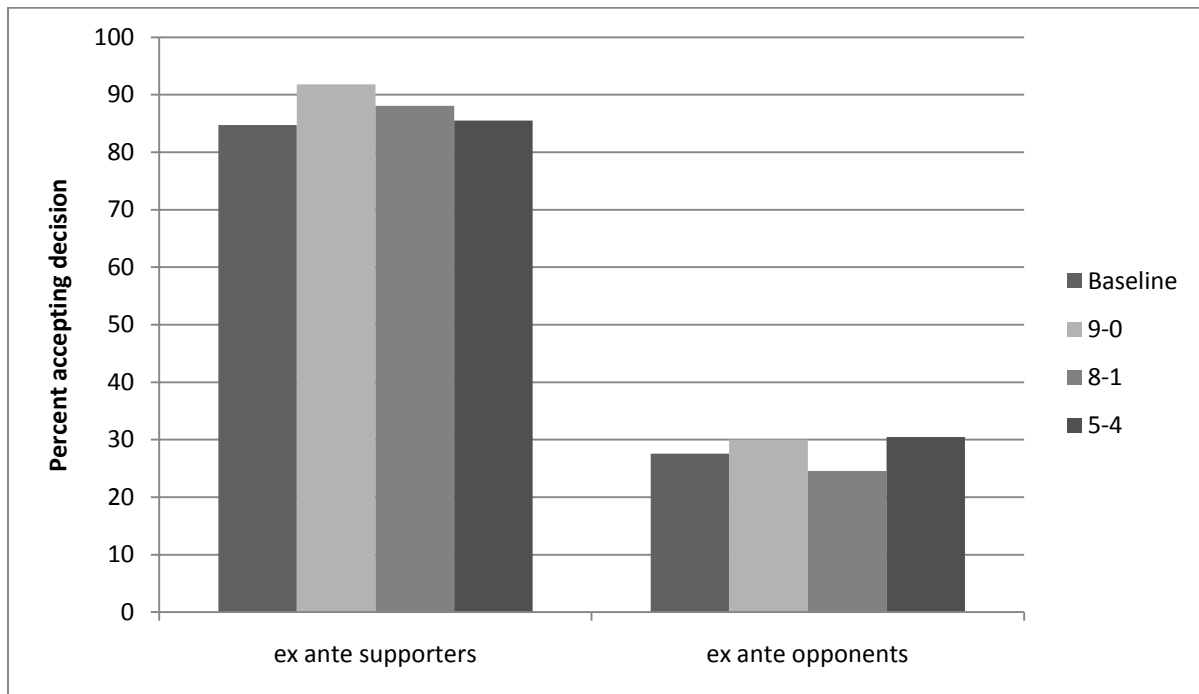
Table 3.2 shows the differences in the mean level of agreement for each treatment group after subtracting the baseline group's mean level of support.[12] As can also be seen in Figure 3.5, the baseline group of *ex ante* supporters is the most likely to agree with the Court's decision – something that is not predicted by any of the hypotheses above. However, none of these differences are significantly different from zero (using a two-tailed *t*-test), indicating that majority size had no measurable effect on agreement with the decision. The differences among *ex ante* opponents are consistent with the pro-unanimity hypothesis – the difference between the unanimous treatment group and the baseline is positive while the other differences are negative – but they too are statistically insignificant.

Substituting acceptance for agreement produces results equally unsupportive of the hypotheses above. Table 3.3 (along with Figure 3.6) shows the odd result that *ex ante* supporters in the baseline group have the lowest acceptance; however, these differences again are statistically insignificant. Likewise, the strange pattern among *ex ante* opponents – a positive difference for unanimous and closely divided outcomes but a negative difference when

---

[12] Agreement and acceptance are treated as dichotomous variables, where those who indicate that they agree with the decision (either strongly or somewhat) or accept it as the final word on the matter are coded 1, respectively and all others – whether they disagree/do not accept or fail to respond – are coded 0.

there is only a single dissent – fails the significance test. Thus, so far the experiment reveals no evidence that majority size has any bearing on public response to the Court.

Table 3.2. Differences in rates of agreement with Court decision: same-sex unions.

Treatment	Ex ante supporters			Ex ante opponents		
	Difference	S.E.	n	Difference	S.E.	n
9-0	-.011	.070	61	.067	.063	80
8-1	-.069	.073	67	-.015	.060	57
5-4	-.062	.072	69	-.019	.056	59
	(baseline *n* = 59)			(baseline *n* = 58)		

Table 3.3. Differences in rates of acceptance of Court decision: same-sex unions.

Treatment	Ex ante supporters			Ex ante opponents		
	Difference	S.E.	n	Difference	S.E.	n
9-0	.071	.059	61	.024	.079	80
8-1	.033	.061	67	-.030	.083	57
5-4	.008	.064	69	.028	.082	59
	(baseline *n* = 59)			(baseline *n* = 58)		

The findings are similar when looking at the respondents' attitudes toward employee privacy. Again, as seen in Figure 3.7, the responses of *ex ante* supporters and opponents are quite different from one another, though the treatment groups within each subsample remain clustered. As the graph shows, the agreement rate among *ex ante* supporters ranges from 87 percent (baseline) to 97 percent (eight-to-one) – an overall range only somewhat larger than that observed with the highly salient case though not drastically so. Among *ex ante* opponents, agreement ranges from 17 percent (eight-to-one) to 25 percent (five-to-four), which is also in line with the same-sex union example.

Figure 3.7. Agreement with Court decision by treatment group: employee privacy.
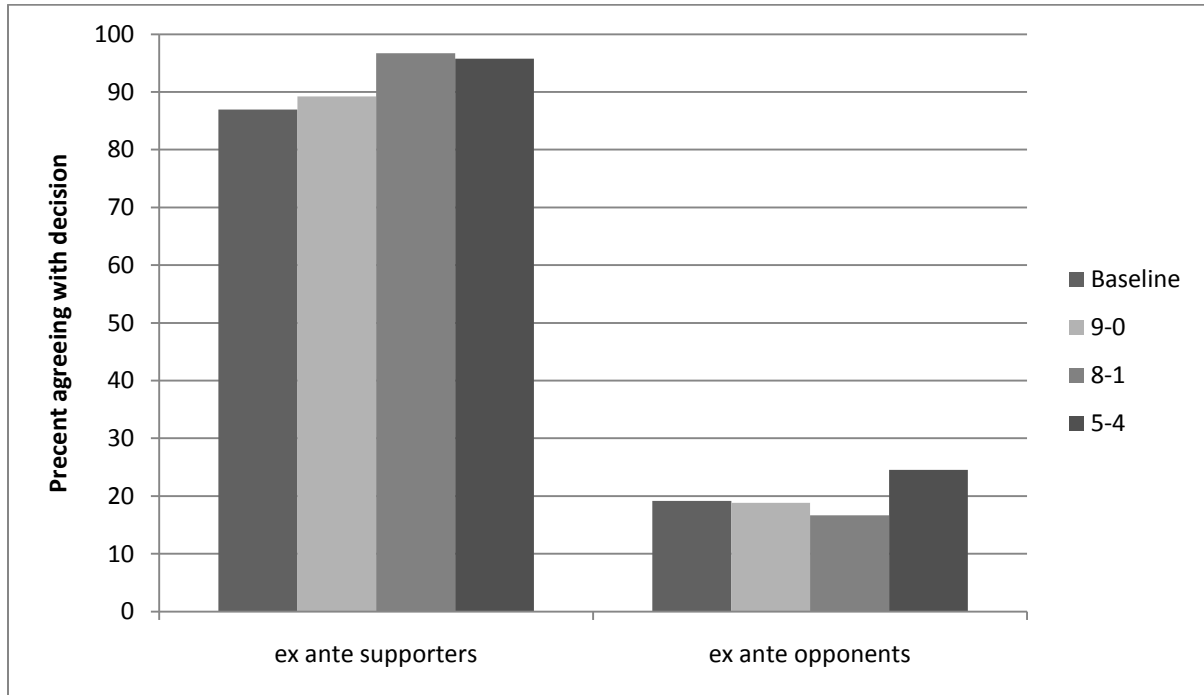
Figure 3.8. Acceptance of Court decision by treatment group: employee privacy.
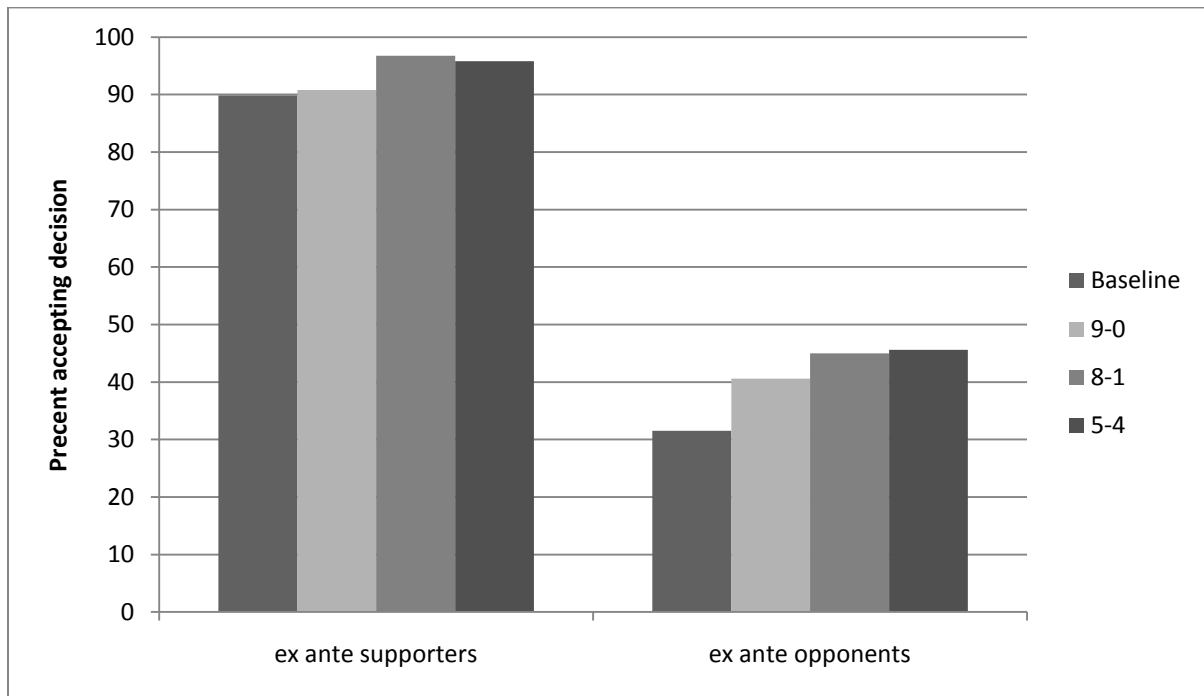
Again, when we turn to the rate of acceptance, seen in Figure 3.8, the gap between *ex ante* supporters and opponents is somewhat smaller, though still dramatic. The treatment groups among supporters remain clustered, ranging from 90 percent (baseline) to 97 percent (eight-to-one). The groups of opponents are clustered as well, but exhibit a larger range, stretching from 32 percent (baseline) to 46 percent (five-to-four).

As with same-sex unions, few of the differences among treatment groups on their attitudes toward the employee privacy decisions are statistically significant. However, Table 3.4 does show that, among *ex ante* supporters, the eight-to-one group has a significantly higher rate of agreement with the Court than the baseline group. None of the other groups produced a significant effect, leaving us with a strange result. This finding indicates that, when looking at those predisposed to agree with the Court's ultimate ruling on the issue of employee privacy, a single dissent from the Court boosts the degree to which they agree with the decision, but neither unanimity nor narrow division have any effect on the public response at all. There is no readily available theoretical reason for this finding, which suggests that it may simply be a false positive. Regardless, it merits further analysis, which is discussed below.

Table 3.4. Differences in rates of agreement with Court decision: employee privacy.

Treatment	*Ex ante* supporters			*Ex ante* opponents		
	Difference	*S.E.*	*n*	Difference	*S.E.*	*n*
9-0	.023	.056	65	-.003	.066	69
8-1	.098*	.048	61	-.025	.067	60
5-4	.088	.047	71	.054	.073	57
	(baseline *n* = 69)			(baseline *n* = 73)		

* *p* < .05

Aside from this bizarre finding, none of the other treatment groups for this issue area using either measure of support are statistically different from the baseline. That is, *ex ante* opponents from one treatment group of the Court's decision on employee privacy are no more likely to agree with the Court *ex post* than any other group, nor are any respondents more likely to accept the decision depending on the majority size to which they were exposed. Even the relatively large differences between acceptance rates in both the five-four and eight-one treatment groups and the baseline group fails to muster statistical significance in this analysis (see Table 3.5).

Table 3.5. Differences in rates of acceptance of Court decision: employee privacy.

Treatment	*Ex ante* supporters			*Ex ante* opponents		
	Difference	*S.E.*	*n*	Difference	*S.E.*	*n*
9-0	.009	.052	65	.091	.081	69
8-1	.069	.044	61	.135	.084	60
5-4	.059	.044	71	.141	.085	57
	(baseline *n* = 69)			(baseline *n* = 73)		

When looking at the less salient decision on contract dispute resolution, we still see that agreement (Figure 3.9) and acceptance (Figure 3.10) rates are largely driven by the respondents' *ex ante* position: with each dependent variable, the treatment groups are again tightly clustered by their predisposition to the Court's ruling. This is particularly true of the *ex ante* supporters. The range in agreement rates is quite small, with 83 percent (five-to-four) on the lower end and 86 percent (unanimous) at the upper end. The variation in treatment group response is slightly tighter when looking at acceptance, ranging from 88 percent (five-to-four) to 90 percent (baseline and eight-to-one).

Although the *ex ante* opponents are clustered as well, they are not nearly as tightly grouped as the supporters are. The agreement rates range from 20 percent (five-to-four) to 33 percent (unanimous), and the acceptance rates range from 29 percent (baseline) to 46 percent (eight-to-one). Furthermore, another pattern is clear across both agreement and acceptance measures: the large majority groups (unanimous and eight-to-one) have noticeably more favorable responses to the Court than do the other treatment groups. This is supportive of the anti-division hypothesis, at least among *ex ante* opponents on this issue of little ideological salience.

Not surprisingly, as seen in Table 3.6, the small differences among the agreement rates of *ex ante* supporters are not statistically significant. In fact, the eight-to-one group produced a rate of agreement identical to the baseline. However, the more noticeable differences between the large majority groups and the baseline are also insignificant. Thus, these results do not allow us to say with confidence that large majorities lead to attitude change.

Figure 3.9. Agreement with Court decision by treatment group: contract disputes.
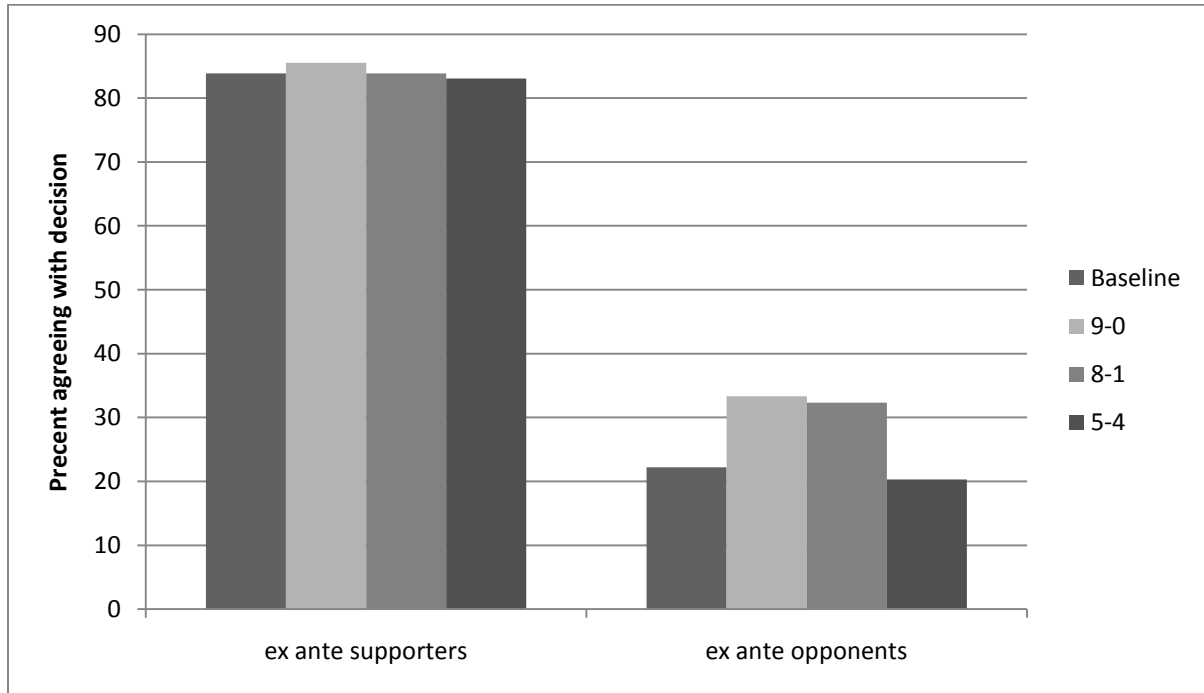
Figure 3.10. Acceptance of Court decision by treatment group: contract disputes.
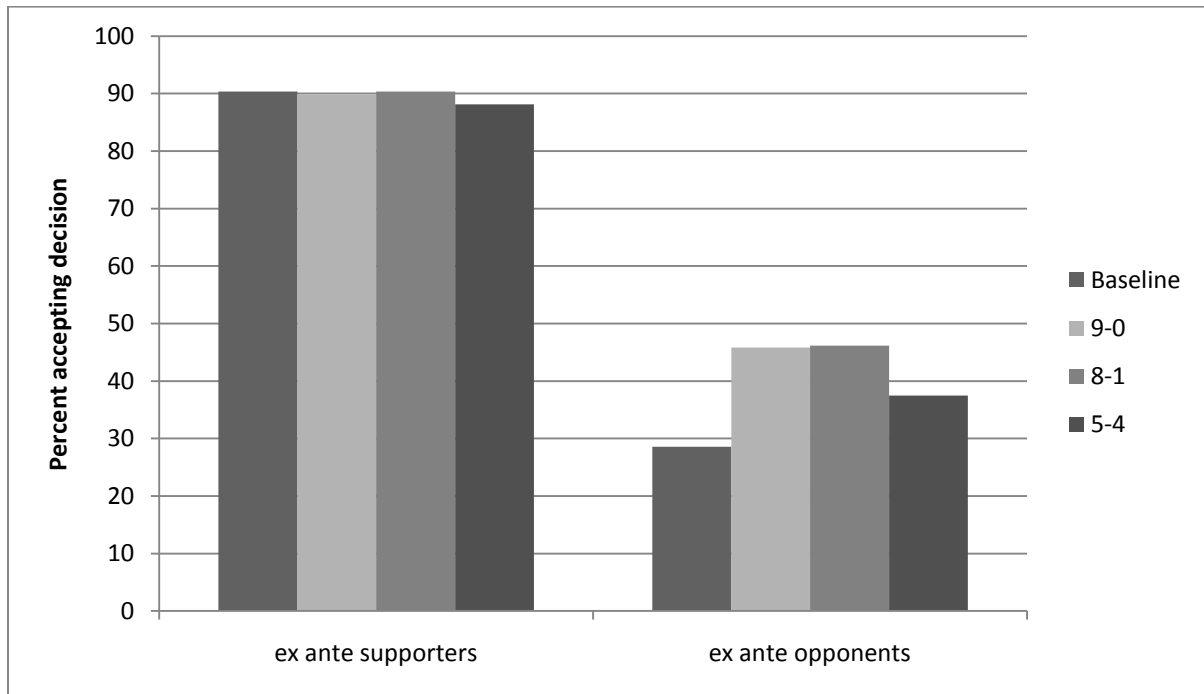
On the other hand, while the acceptance rate differences among the groups *ex ante* supporters are similarly insignificant, the opponent groups receiving vignettes about unanimous or eight-to-one decisions do report significantly higher levels of acceptance of the Court's ruling (see Table 3.7). In other words, while this experiment does not produce solid evidence of the Court's ability to persuade *ex ante* opponents on the issue of contract dispute resolution itself, it does appear to have the ability – at least with this non-salient issue – to lower the level of resistance among their ideological opponents in the public. Moreover, these results show that the Court does not have to be unanimous to have such an effect – it needs only an overwhelming majority, as predicted by the anti-division hypothesis.

Table 3.6. Differences in rates of agreement with Court decision: contract disputes.

Treatment	*Ex ante* supporters			*Ex ante* opponents		
	Difference	*S.E.*	*n*	Difference	*S.E.*	*n*
9-0	.016	.063	69	.111	.077	72
8-1	0	1	62	.101	.079	65
5-4	-.008	.068	59	.019	.073	64
	(baseline *n* = 62)			(baseline *n* = 63)		

Table 3.7. Differences in rates of acceptance of Court decision: contract disputes.

Treatment	*Ex ante* supporters			*Ex ante* opponents		
	Difference	*S.E.*	*n*	Difference	*S.E.*	*n*
9-0	-.005	.053	69	.173*	.083	72
8-1	0	1	62	.176*	.085	65
5-4	-.022	.057	59	.089	.084	64
	(baseline *n* = 62)			(baseline *n* = 63)		

* $p < .05$

Post-Experimental Manipulation

Matching as a Correction for Statistical Imbalance

Of course, the validity of these experimental results is dependent upon covariate balance among the treatment groups. That is, each group of respondents should be essentially identical to one another on every characteristic save for their treatment group assignment. Randomization typically takes care of this requirement: by randomly assigning respondents to treatment groups, there is a strong probability that the groups will be statistically indistinguishable from one another on both observed and unobserved characteristics.

Unfortunately, there exists the possibility, especially when using sampling methods that are not based on purely random chance, that such balance will not be achieved. Given the unusual finding above – that a large but not unanimous majority boosts agreement among *ex ante* supporters in the medium salience case – it is necessary to review the balance produced by the experimental randomization. If such covariate imbalance did occur here, it is necessary to correct for this problem in order to insure the validity of the results.

Because the experimental design utilized above is explicitly tailored to the principles of causal inference, I employ a statistical technique, genetic matching, that was developed with the experimental ideal in mind. As a method, matching compares two subsamples (i.e., treatment and control groups), compares the statistical similarity between those groups on specified characteristics, and assigns weights to each data point in order to maximize the statistical balance between the treatment groups. Genetic matching, or GenMatch, uses an evolutionary search algorithm to assign weights that will produce optimal balance between treatment and control (Sekhon, forthcoming). Matching has become an increasingly common method of causal inference in the social sciences, including the study of judicial politics (Boyd, Epstein, and Martin 2010; Epstein, Ho, King, and Segal 2005; Gordon and Huber 2007; Hanley, Salamone, and Wright 2011).

There are several covariates that, if imbalanced, may seriously bring the above results into question. For one, we should expect balance in the strength of the respondents' *ex ante* positions on the issue in question. After all, it would certainly be problematic if, when comparing two treatment groups, one group contained significantly more respondents with strong – rather than mild – preferences on the policy at hand; that group would most definitely be less mobile in their post-treatment attitudes, regardless of any other factor. This is coded as a dummy variable where 1 indicates a strong *ex ante* position (i.e., the respondent indicated that they strongly agree or disagree with the policy position, as opposed to somewhat agreeing or disagreeing with it).

Similarly, it is important to have statistical balance on the overall ideological position of the relevant policy issue. Because the ideological direction of the Court decisions described in the treatment vignette is varied at random, this variable is essential if we are to avoid the assumption that mobility on the issue attitude is symmetrical. That is, it is possible that, say, opponents of same-sex unions are more likely to adjust their opinions in light of a Court

decision than supporters are. Thus, balance on the direction of the respondents' prior positions will assure that such an asymmetry, if it exists, will not bias the results. These variables are coded on a 0 to 1 scale where higher values represent attitudes that are more favorable to same-sex unions, employee privacy, and the use of mediation to settle contract disputes, respectively.

Institutional support for the Supreme Court is also likely to color one's survey responses. Regardless of one's prior opinion or the majority size of the Court, those who hold deep respect for the Court are more likely to be persuaded by its ruling than those who think little of the institution. As noted above, the respondents were asked to answer several questions, common to the extant literature, targeting their diffuse support of the Supreme Court. Like Zink, Spriggs, and Scott, I take a factor analysis of the responses to these questions, which produces two underlying factors of diffuse support for the United States Supreme Court. Thus, I look at the statistical balance of both of these factors (which are coded such that higher scores are more supportive of the Court).

Likewise, one's level of civic engagement may affect her ability to internalize and be persuaded by the Court's decisions. After all, individuals are more persuadable when they feel their opinions have important consequences (e.g., Chaiken 1980; Petty and Cacioppo 1979; Zimbardo 1960), and politically active and engaged individuals may be more likely to feel the importance of their political preferences. As a heuristic for political engagement, I look at balance on both the respondents' interest in current events and their voter registration.[13] News interest is coded on a 0 to 1 scale, with higher values indicating more interest, and voter registration is coded as a dummy variable, where 1 indicates that the respondent is registered to vote.

In addition, we ought to ensure balance on the political ideology and partisan identification of the respondents. Again, it is important to avoid the assumption that the ability to be swayed by the Supreme Court is symmetrical across the political spectrum. In fact, some have found evidence that, in the wake of *Bush v. Gore*, individuals may indeed view the Court differently according to their party identification and ideology (Mate and Wright 2008). These variables are both coded on a 0 to 1 scale, with higher values indicating a respondent is more conservative and Republican, respectively.

It is also important to factor in the several demographic characteristics of the respondents. A number of studies examining public opinion of the Supreme Court have found it necessary to control for such population characteristics (e.g., Franklin and Kosaki 1989; Gibson, Caldeira, and Spence 2003b; Johnson and Martin 1998; Mate and Wright 2008). I thus factor in variables for income, age, sex, race, religiosity, and geography. Sex, race, and geography are

---

[13] The experimental survey itself did not contain questions regarding these nor several other of the matching covariates. However, they were contained in the respondents' profiles provided by YouGov Polimetrix, the survey company hired to execute the experimental survey.

coded as dummy variables where 1 indicates that a respondent is male, nonwhite, and from the American South, respectively. Each respondent's age is simply coded as her age in years. Religiosity is taken as a factor analysis of several questions regarding church attendance, frequency of prayer, and the importance of religion in the respondents' life; the variable is coded such that higher values indicate higher levels of religiosity. Income is coded based on the respondents' self-placement into one of fourteen annual income categories, with higher values representing higher categories of income.[14]

Given this, I compare each vote majority treatment group to the baseline group (which is treated as a control group) and employ GenMatch to improve balance on the covariates listed above (as well as on the propensity of being assigned to the treatment group). Moreover, I compare the balance on the responses "distracter" questions, which are not specified in the match. These variables include the respondents' attitudes on several variables irrelevant to the direct question at hand; specifically they are the respondents' opinion of replacing the income tax with a national sales tax, their attitude toward the principle of universal healthcare, their opinion on the right to own a handgun, and their willingness to allow prayer in the public schools.

Comparing the pre- and post-match balance of unmatched covariates serves as a placebo test for the matching procedure: if the matching procedure succeeds in optimizing overall balance between treatment and control groups, then it ought to improve balance across groups on all characteristics – both matched and unmatched – except for, perhaps, the dependent variable. In sum, matching not only serves as a robustness test for the experimental results described in the previous section, but also, by ensuring optimal balance between treatment and control groups, serves to correct any errors in the randomization process that may have caused any Type I or Type II errors.

Covariate Balance

To evaluate the covariate balance between the baseline and each treatment group, Tables 3.8 through 3.25 report three indicators of balance: the difference in means between the baseline and the compared treatment group,[15] the *t*-test *p*-value, and the *p*-value of the bootstrapped Kolmogorov-Smirnov (KS) test.[16] Since any comparison on the values of the dependent variables are valid only if the values of the covariates across treatment groups are

---

[14] The income categories are (1) less than $10,000, (2) $10,000 to $14,999, (3) $15,000 to $19,999, (4) $20,000 to $24,999, (5) $25,000 to $29,999, (6) $30,000 to $39,999, (7) $40,000 to $49,999, (8) $50,000 to $59,999, (9) $60,000 to $69,999, (10) $70,000 to $79,000, (11) $80,000 to $99,999, (12) $100,000 to $119,999, (13) $120,000 to $149,999, and (14) $150,000 or more.

[15] The difference is taken as the treatment group (9-0, 8-1, or 5-4) minus the baseline group.

[16] The KS test is not applicable to dummy variables.

sufficiently similar, mean differences close to 0 are preferable. Likewise, the closer to 1 the *p*-values of the *t*-test, which compares the two means based on a Student's *t* distribution, and the KS test, which nonparametrically compares the two distributions, are the better. In particular, *p*-values that fall below .05, the level at which the difference between the two groups is conventionally considered to be statistically significant, are strong indicators of covariate imbalance.

Looking at Table 3.8, which compares the baseline and unanimous treatment groups of *ex ante* supporters, we can see that matching improved the balance on nearly every matched covariate. In fact, it even three of the four unmatched covariates' balance improved after matching, indicating an overall improvement in statistical balance beyond the specified characteristics. Prior to matching, balance on political ideology is particularly bad: the unanimous treatment group respondent pool is more conservative than the baseline. Although this variable's balance remains worse than the other covariates after matching, it is much improved and no longer falls in the conventional range of statistical significance. Moreover, party identification, a variable closely associated with ideology, achieves virtually perfect balance after matching.

Table 3.9 shows another comparison among *ex ante* supporters of the same-sex unions decision, only the comparison here is between the baseline and the eight-to-one group. Again, matching appears to improve the balance for nearly all the matched and unmatched covariates. Most notably, matching repairs the severe imbalance between the treatment groups on the news interest variable (the eight-to-one group is significantly more interested in current events than is the baseline) and the moderate imbalance on age, sex, and school prayer (the eight-to-one group is debatably older, more male, and more supportive of prayer in public schools). Furthermore, nearly perfect post-matching balance is achieved on the measure of political ideology.

Table 3.10 shows the covariate balance between the *ex ante* supporters in the five-to-four and baseline groups. Again, most matched and, in this case, all unmatched covariates experience some form of balance improvement. Although the *t*-test *p*-value for the news interest variable remains below the .05 level, it is a marked improvement from the pre-match balance on that variable, and the two groups are statistically indistinguishable after matching according to the KS test. Moreover, matching also appears to repair the low *p*-values associated with the age, race, and gun rights opinion variables. In fact, matching achieves perfect racial balance between the two groups. On the other hand, the matching procedure does sacrifice some balance on the second diffuse support factor – a potentially key characteristic in explaining movement on the dependent variable. However, the difference between the matched treatment groups on this factor remains statistically insignificant, particularly when measured by the KS test.

Table 3.8. Covariate balance between baseline and unanimous groups (*ex ante* supporters): same-sex unions.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.088	.314		**-.049**	.256	
Gay rights	-.086	.273	.364	**-.020**	**.653**	**.932**
Diffuse support (1)	-.081	.613	.740	**.001**	**.989**	.118
Diffuse support (2)	-.042	.293	.446	**.021**	**.477**	**.596**
News interest	.152	.009	.022	**.022**	**.394**	**.212**
Voter registration	.072	.277		**.016**	**.565**	
Political ideology	.090	.069	.010	**.033**	**.169**	.060
Party identification	.062	.329	.574	**0**	**1**	**.976**
Income	.390	.568	.402	**.131**	**.825**	**.794**
Age	2.733	.342	.276	**-.262**	**.869**	**.282**
Sex	-.102	.264		**.033**	**.565**	
Race	-.044	.609		**-.033**	.480	
Religiosity	-.012	.922	.612	.015	.908	**.638**
South	.022	.798		.049	.565	
Unmatched covariates						
National sales tax	.085	.163	.136	**-.004**	**.941**	**.722**
Universal healthcare	-.072	.320	.480	**-.037**	**.432**	.398
Gun rights	.066	.347	.368	.074	.302	.340
School prayer	.073	.324	.272	**.016**	**.791**	**.570**

Post-matching improvements are in **bold**.

Table 3.9. Covariate balance between baseline and eight-to-one groups (*ex ante* supporters): same-sex unions.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.022	.794		-.045	.406	
Gay rights	-.069	.377	.484	**.004**	**.919**	**.930**
Diffuse support (1)	.033	.825	.210	-.057	.498	**.274**
Diffuse support (2)	.008	.836	.724	**.002**	**.934**	.502
News interest	.164	.003	.006	**.050**	**.103**	**.268**
Voter registration	.097	.121		**.045**	**.366**	
Political ideology	.041	.371	.218	**0**	**1**	**.680**
Party identification	.033	.603	.274	**.012**	**.668**	**.684**
Income	.645	.338	.526	**-.179**	**.633**	**.610**
Age	5.274	.055	.126	**.473**	**.827**	**.278**
Sex	-.147	.098		**.015**	**.835**	
Race	-.055	.507		**-.030**	**.528**	
Religiosity	-.009	.946	.828	.056	.397	**.896**
South	.051	.551		-.060	.349	
Unmatched covariates						
National sales tax	.100	.103	.258	**.082**	**.138**	.192
Universal healthcare	-.061	.377	.556	**-.060**	.166	.430
Gun rights	.030	.661	.872	.093	.163	.374
School prayer	.131	.065	.044	**.056**	**.309**	**.070**

Post-matching improvements are in **bold**.

Table 3.10. Covariate balance between baseline and five-to-four groups (*ex ante* supporters): same-sex unions.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.030	.715		**-.029**	.156	
Gay rights	-.034	.659	.836	**.022**	**.742**	**.984**
Diffuse support (1)	-.078	.597	.312	.094	.105	**.408**
Diffuse support (2)	-.002	.956	.942	.037	.094	.294
News interest	.168	.002	.016	**.053**	**.032**	**.128**
Voter registration	.042	.534		**.014**	**.782**	
Political ideology	.018	.703	.448	-.022	.585	.178
Party identification	.046	.458	.548	**-.041**	**.497**	**.726**
Income	.232	.716	.600	.884	.111	.408
Age	6.193	.031	.178	**-.131**	**.912**	**.394**
Sex	-.132	.137		**.014**	**.656**	
Race	-.136	.088		**0**	**1**	
Religiosity	-.119	.299	.070	**.010**	**.884**	**.596**
South	.127	.142		**-.043**	**.578**	
Unmatched covariates						
National sales tax	.067	.270	.338	**.007**	**.906**	**.614**
Universal healthcare	-.050	.465	.516	**.040**	**.543**	.374
Gun rights	.123	.056	.066	**-.015**	**.791**	**.680**
School prayer	.087	.236	.270	**-.007**	**.894**	**.490**

Post-matching improvements are in **bold**.

Turning to the *ex ante* opponents of the same-sex unions case, Table 3.11 shows the comparison between the unanimous and baseline treatment groups. Here, matching improves balance on most of the matched covariates but only one of the four measured unmatched covariates. In fact, matching seems to throw the group values on gun control opinion out of balance: the post-matching unanimous treatment group appears to be less supportive of gun rights than the baseline is. However, the respondents in that group are not more politically conservative or Republican than those in the baseline, as the *p*-values for both tests (particularly the *t*-test) on those variables are quite high. Thus, it is unlikely that the failure to achieve balance on this one isolated policy preference will skew the final results. On the other hand, the matching procedure achieves excellent balance on the income variable, whose pre-matching KS test *p*-value falls below the .05 threshold. Moreover, prior to matching, the two groups are arguably not balanced on their rate of voter registration, but are sufficiently balanced afterwards.

Covariate balance between the baseline and eight-to-one treatment groups improved in most of the matched variables and all of the unmatched variables, as seen in Table 3.12. Although some balance is lost on race, the post-matching difference between the treatment groups is still statistically insignificant at the .05 level, though it does come close. Meanwhile, virtually perfect balance is achieved on the strength of the respondents' *ex ante* position, their directional opinion on gay rights, and their rate of voter registration (which had the lowest *p*-value prior to matching).

Finishing out the treatment group comparisons on the issue of same-sex unions, Table 3.13 shows strong balance improvement between the baseline and five-to-four treatment groups of *ex ante* opponents as well. Again, balance is improved in some form on all the unmatched covariates and most of the matched covariates. Although voter registration rates differ significantly between treatment groups prior to matching, balance on this variable is repaired after matching. Furthermore, gay rights opinion and political ideology are of questionable balance in the unmatched sample according to the *t*-test (the treatment group is more liberal than the baseline on both counts), but the differences between the two groups post-matching are definitively insignificant.

Table 3.11. Covariate balance between baseline and unanimous groups (*ex ante* opponents): same-sex unions.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.026	.736		.063	.130	
Gay rights	-.022	.771	.918	**-.009**	.663	**.936**
Diffuse support (1)	.066	.631	.842	-.099	.210	.588
Diffuse support (2)	.036	.319	.132	**.032**	.074	**.262**
News interest	-.013	.772	.814	-.025	.273	.384
Voter registration	-.086	.080		**.013**	**.317**	
Political ideology	.029	.520	.450	**.003**	**.903**	**.604**
Party identification	.019	.764	.920	**-.002**	**.968**	.754
Income	-.990	.109	.040	**-.013**	**.969**	**.724**
Age	1.401	.597	.278	**-.776**	.541	.196
Sex	-.028	.747		**.013**	.740	
Race	.029	.709		.063	.297	
Religiosity	.058	.553	.298	-.085	.282	**.428**
South	-.062	.450		**.013**	**.764**	
Unmatched covariates						
National sales tax	-.037	.488	.562	-.044	.229	.378
Universal healthcare	.056	.419	.404	.075	.124	.224
Gun rights	-.068	.243	.286	-.091	.049	.068
School prayer	.059	.345	.268	**.003**	**.948**	**.754**

Post-matching improvements are in **bold**.

Table 3.12. Covariate balance between baseline and eight-to-one groups (*ex ante* opponents): same-sex unions.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.065	.418		**0**	**1**	
Gay rights	.019	.815	.834	**0**	**1**	**.994**
Diffuse support (1)	.083	.578	.730	**-.080**	.377	.388
Diffuse support (2)	-.016	.679	.658	.021	.408	.122
News interest	.012	.798	.852	-.029	.353	.838
Voter registration	-.089	.110		**0**	**1**	
Political ideology	-.065	.196	.576	**.004**	**.885**	**.652**
Party identification	.019	.771	.454	.061	.213	**.470**
Income	.340	.985	.558	.684	.100	.530
Age	1.405	.636	.624	**-.088**	**.970**	**.818**
Sex	-.080	.393		**.018**	**.707**	
Race	.110	.208		.053	.081	
Religiosity	.084	.442	.242	**-.023**	**.814**	**.466**
South	.024	.793		.123	.124	
Unmatched covariates						
National sales tax	.014	.812	.960	**.013**	.793	.672
Universal healthcare	.090	.215	.342	**-.004**	**.921**	.282
Gun rights	-.013	.822	.800	.013	.754	**.978**
School prayer	-.016	.808	.554	**.009**	**.876**	.122

Post-matching improvements are in **bold**.

Table 3.13. Covariate balance between baseline and five-to-four groups (*ex ante* opponents): same-sex unions.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	0	.995		.029	.638	
Gay rights	.125	.093	.126	**.014**	**.652**	**.894**
Diffuse support (1)	-.090	.555	.384	**.078**	.550	.296
Diffuse support (2)	.037	.296	.756	**-.013**	**.663**	.406
News interest	-.017	.724	.830	.048	.130	.260
Voter registration	-.108	.045		**-.014**	**.317**	
Political ideology	-.085	.088	.336	**-.011**	**.549**	**.596**
Party identification	-.018	.766	.806	.022	.351	**.918**
Income	-.295	.644	.094	**-.036**	**.906**	**.396**
Age	-2.187	.427	.154	**1.522**	**.307**	**.634**
Sex	.042	.643		**.029**	**.656**	
Race	.031	.697		**-.014**	.317	
Religiosity	-.039	.727	.076	-.040	.721	**.296**
South	.087	.322		.087	.200	
Unmatched covariates						
National sales tax	-.018	.754	.726	**.011**	.493	.148
Universal healthcare	.107	.121	.162	**.004**	**.919**	**.578**
Gun rights	-.061	.325	.270	**.004**	**.937**	**.502**
School prayer	-.100	.149	.076	**-.004**	**.953**	**.786**

Post-matching improvements are in **bold**.

Matching produces similar balance improvements in the employee privacy example as well. In Table 3.14, which shows the covariate balance between *ex ante* supporters of this decision in the baseline and unanimous treatment groups, we can see improvement on nearly every matched covariate and every unmatched covariate. Although some balance between the two groups appears to be lost on the proportion of respondent from the South, the post-matching difference is still insignificant by conventional standards, and seems to be a fair trade-off for the overall improvement.

Likewise, Table 3.15, which compares the characteristics of the eight-to-one *ex ante* supporters to the baseline, shows nearly across-the-board improvement. Again, nearly every matched and unmatched covariate sees its balance improved after matching. Additionally, matching solves some imbalance on the unmatched gun rights opinion variable as well as on the diffuse support factors. It is worth noting that, according to the *t*-test, the first factor of diffuse support for the Supreme Court is significantly higher among those in the eight-to-one treatment group than it is for those in the baseline group. In other words, prior to matching, the treatment group has more reverence for the Court as institution than the control group has. This may potentially explain the unusual finding in Table 3.4, which showed the eight-to-one group to be significantly more likely to agree with the Court's decision than any other treatment group. Fortunately, matching balances this covariate, and the post-matching results should not be affected by this asymmetry between treatment groups.

Similar results are found for the balance between *ex ante* supporters in the five-to-four treatment group and the baseline (shown in Table 3.16). Prior to matching, the two groups are imbalanced on several covariates. In particular, the sample lacks balance on the strength of the *ex ante* position (the five-to-four treatment group held stronger prior positions on employee privacy than the baseline did), sex (the treatment group contains significantly more males), opinion toward universal healthcare (the treatment group is less favorable toward such a policy), and attitudes about gun rights (the treatment group is more favorable toward them). Additionally, the balance of several variables is questionable. Though not meeting the standards of statistical significance, the *t*-test *p*-values for the first diffuse support factor, political ideology, party identification, geographic region, and national sales tax opinion are quite low (suggesting that the treatment group is more supportive of the Court, more politically conservative, more Republican, more Southern, and more supportive of a national sales tax). Fortunately, matching improves balance on all of these covariates.

Table 3.14. Covariate balance between baseline and unanimous groups (*ex ante* supporters): employee privacy.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.087	.318		**.015**	.317	
Employee privacy rights	-.014	.825	.766	**.012**	**.406**	**1**
Diffuse support (1)	-.105	.480	.566	**.015**	**.906**	**.674**
Diffuse support (2)	-.047	.200	.178	**-.009**	**.769**	**.636**
News interest	.026	.617	.288	**0**	**1**	.268
Voter registration	.006	.930		**0**	**1**	
Political ideology	.046	.342	.270	**-.004**	**.870**	**.698**
Party identification	.091	.098	.156	**.016**	**.416**	**.846**
Income	-.280	.641	.312	**-.139**	**.831**	**.330**
Age	-.892	.753	.774	1.692	.415	**.918**
Sex	.008	.927		.031	.480	
Race	-.026	.753		**-.015**	.706	
Religiosity	.057	.602	.466	**.047**	.491	.394
South	.094	.249		.123	.071	
Unmatched covariates						
National sales tax	.051	.342	.476	**.023**	**.663**	**.734**
Universal healthcare	-.041	.529	.632	**.027**	**.585**	.208
Gun rights	.129	.030	.046	**.012**	**.840**	**.986**
School prayer	.030	.632	.202	**-.054**	**.329**	**.228**

Post-matching improvements are in **bold**.

Table 3.15. Covariate balance between baseline and eight-to-one groups (*ex ante* supporters): employee privacy.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.184	.036		**.016**	**.317**	
Employee privacy rights	.019	.781	.382	**-.012**	**.836**	**.982**
Diffuse support (1)	.299	.042	.146	**-.022**	**.826**	**.780**
Diffuse support (2)	-.034	.351	.040	**.001**	**.963**	**.442**
News interest	-.042	.398	.314	**-.022**	**.466**	**.734**
Voter registration	.077	.751		**.016**	**.656**	
Political ideology	.016	.733	.916	.021	.493	.564
Party identification	.050	.484	.446	**.033**	.285	**.690**
Income	.615	.314	.258	**-.328**	**.424**	**.448**
Age	-3.016	.285	.606	**2.132**	**.372**	.060
Sex	.115	.190		**.033**	.156	
Race	-.087	.275		**0**	**1**	
Religiosity	-.014	.896	.644	.079	.486	.374
South	.085	.302		**.066**	.100	
Unmatched covariates						
National sales tax	.056	.283	.662	.070	.111	.368
Universal healthcare	-.043	.519	.604	**-.025**	**.618**	**.918**
Gun rights	.132	.029	.042	**.066**	**.175**	**.530**
School prayer	-.050	.427	.372	**-.045**	**.432**	.350

Post-matching improvements are in **bold**.

Table 3.16. Covariate balance between baseline and five-to-four groups (*ex ante* supporters): employee privacy.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.186	.028		**.028**	**.156**	
Employee privacy rights	.059	.369	.288	**-.007**	**.901**	**1**
Diffuse support (1)	.248	.077	.104	**.005**	**.950**	**.414**
Diffuse support (2)	-.058	.104	.318	**.007**	**.805**	**.414**
News interest	-.010	.842	.976	**-.005**	**.862**	.894
Voter registration	.061	.287		**0**	**1**	
Political ideology	.088	.062	.132	**.014**	**.618**	**.940**
Party identification	.116	.053	.248	**.026**	**.340**	**.640**
Income	.097	.878	.990	-.577	.157	.388
Age	-3.725	.156	.344	**-.930**	**.645**	**.812**
Sex	.201	.017		**.070**	**.130**	
Race	-.080	.304		**-.028**	**.528**	
Religiosity	-.106	.333	.166	**-.007**	**.915**	**.256**
South	.133	.098		**0**	**1**	
Unmatched covariates						
National sales tax	.100	.066	.122	**.092**	**.077**	.118
Universal healthcare	-.143	.030	.048	**-.060**	**.187**	**.406**
Gun rights	.163	.006	.010	**.081**	**.099**	**.032**
School prayer	-.053	.382	.314	-.067	.206	.166

Post-matching improvements are in **bold**.

Turning to *ex ante* opponents of the employee privacy case, Table 3.17 shows fewer improvements, particularly among the specified covariates, after matching the unanimous treatment group to the baseline. Nevertheless, those variables that are already balanced prior to matching remain statistically indistinguishable across treatment groups after matching (even though only half of the matched covariates actually improve in balance). More importantly, however, is that the matching procedure produces excellent balance on the religiosity variable, which is imbalanced in the unmatched sample (the unanimous group is less religious than the baseline). Furthermore, matching also improves balance in three of the four unmatched covariates, demonstrating its ability to equalize the characteristics of treatment and control beyond those that are specified.

The covariate balance improvement in the comparison of *ex ante* opponents in the eight-to-one group and the baseline is more obvious. Table 3.18 shows that matching improves the statistical balance on nearly all of the specified and unspecified variables. Prior to matching, the balance between treatment and control is quite poor. The eight-to-one group contains more respondents holding a strong *ex ante* position, is more interested in current events, has a larger average income, is more supportive of a national sales tax, and is more favorable toward prayer in the public schools. Also, if we are to relax the standard tests of statistical significance, the pre-matched treatment group is also more male and less white. However, after matching all of these statistical imbalances are resolved.

Matching also produces many improvements in the final treatment group comparison for this issue area. From Table 3.19, which shows the covariate balance between *ex ante* opponents in the baseline and five-to-four groups, we can see that nearly all the matched and half of the unmatched variables improve in their statistical balance. Matching even corrects for a statistically significant imbalance on race (in the unmatched sample, the baseline group is significantly whiter than those assigned the five-to-four treatment) by producing perfect balance on this variable. Unfortunately, the matching specification appears to sacrifice balance on the gender variable (the matched treatment group is slightly more male than the baseline). However, given the widespread improvement across the other covariates, this is an acceptable trade-off.

Table 3.17. Covariate balance between baseline and unanimous groups (*ex ante* opponents): employee privacy.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	0	.996		.015	.317	
Employee privacy rights	-.027	.679	.840	**.011**	**.826**	**.958**
Diffuse support (1)	.067	.606	.382	-.077	.390	.294
Diffuse support (2)	-.010	.784	.414	-.025	.421	.396
News interest	.019	.675	.810	**.010**	**.716**	.732
Voter registration	-.023	.710		-.029	.156	
Political ideology	.020	.629	.554	.051	.185	**.624**
Party identification	.031	.577	.838	**-.027**	.325	.566
Income	-.473	.414	.470	**-.420**	.152	.376
Age	.614	.823	.754	-.725	.687	.372
Sex	.053	.530		**.043**	.178	
Race	-.107	.187		**0**	**1**	
Religiosity	-.284	.004	.066	**.008**	**.891**	**.972**
South	.025	.770		.101	.222	
Unmatched covariates						
National sales tax	.071	.194	.050	**.029**	**.611**	.048
Universal healthcare	-.015	.817	.528	-.022	.661	.354
Gun rights	.002	.977	.682	.062	.244	**.734**
School prayer	-.069	.267	.328	**.029**	**.600**	**.784**

Post-matching improvements are in **bold**.

Table 3.18. Covariate balance between baseline and eight-to-one groups (*ex ante* opponents): employee privacy.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.193	.023		**.017**	**.317**	
Employee privacy rights	.020	.674	.188	**-.029**	.438	**.812**
Diffuse support (1)	.088	.567	.832	**.018**	**.913**	**.860**
Diffuse support (2)	-.012	.758	.760	-.026	.376	.316
News interest	.105	.018	.082	**.039**	**.160**	**.494**
Voter registration	.067	.227		**0**	**1**	
Political ideology	-.074	.116	.090	**-.021**	**.501**	**.378**
Party identification	-.053	.381	.562	**-.003**	**.932**	**.764**
Income	1.243	.039	.094	**.350**	**.446**	**.584**
Age	2.544	.378	.710	-2.917	.246	.086
Sex	.156	.075		**.017**	**.317**	
Race	-.144	.080		**.017**	**.706**	
Religiosity	-.124	.293	.090	**.075**	**.594**	**.698**
South	-.025	.776		.067	.248	
Unmatched covariates						
National sales tax	.127	.037	.018	**.079**	**.112**	**.052**
Universal healthcare	-.041	.566	.608	-.075	.188	.364
Gun rights	-.041	.502	.544	**-.004**	**.947**	**.840**
School prayer	-.163	.016	.044	**-.067**	**.351**	**.318**

Post-matching improvements are in **bold**.

Table 3.19. Covariate balance between baseline and five-to-four groups (*ex ante* opponents): employee privacy.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.068	.443		**.053**	**.603**	
Employee privacy rights	.010	.781	.394	**-.083**	.227	.086
Diffuse support (1)	-.123	.369	.720	**.010**	**.926**	.546
Diffuse support (2)	.009	.788	.456	**.002**	**.891**	.098
News interest	.005	.928	.660	.035	.317	.306
Voter registration	.010	.869		**0**	**1**	
Political ideology	-.002	.966	.388	-.039	.249	.052
Party identification	.052	.408	.546	**-.009**	**.843**	.500
Income	-.144	.813	.852	**.053**	**.897**	.276
Age	2.165	.465	.784	**-.562**	**.795**	**.948**
Sex	.098	.271		.123	.049	
Race	-.168	.045		**0**	**1**	
Religiosity	.014	.894	.994	-.147	.100	.304
South	-.109	.203		**.035**	**.317**	
Unmatched covariates						
National sales tax	.098	.089	.108	**.061**	**.283**	.074
Universal healthcare	.002	.980	.946	.026	.616	.824
Gun rights	-.053	.377	.276	-.066	.218	.056
School prayer	-.100	.131	.148	**.022**	**.748**	**.286**

Post-matching improvements are in **bold**.

Turning to the final issue area, contract dispute resolution, matching continues to resolve many problems with the covariate balance. When comparing the *ex ante* supporters in the unanimous treatment group with those in the baseline, shown in Table 3.20, we can see a lack of balance on the second factor of diffuse support (the treatment group is more supportive of the Court), political ideology (the treatment group is more conservative), and gun rights opinion (the treatment group is more favorable toward them). The *t*-test *p*-value is just above the .05 cutoff on both income and geographic region – suggesting the possibility that the treatment group is richer and more Southern. Furthermore, although the difference between the groups' opinion on a national sales in statistically insignificant according to the KS test, it is significant according to the *t*-test, which suggests that the treatment group is more supportive of the policy. Matching improves balance on nearly all of these covariates. Unfortunately, matching does not improve the *t*-test *p*-value of the gun rights variable, and, while both *p*-values improve on the diffuse support variable (second factor), the variable does not achieve balance according to the KS test (but it is balanced when measured by the *t*-test). Moreover, according to the *t*-test, some balance is lost on age, making the treatment group younger than the baseline. However, most variables, both matched and unmatched, do see improvement, and the majority of the pre-matching balance issues are resolved.

The match-up between the eight-to-one treatment group *ex ante* supporters and their baseline counterparts reveals fewer imbalances in need of repair. However, Table 3.21 does show that, according the second factor of diffuse support, the treatment group is indisputably more supportive of the Court. Matching resolves this imbalance, and improves the balance on many of the other matched covariates and on all but one of the unmatched covariates. These improvements include a perfect match on the respondents' opinion toward mediation as a means of contract dispute resolution, the issue at hand in the experimental treatment. Unfortunately, these improvements are not without a cost: some balance is lost on income – according to the *t*-test (but not the KS test) *p*-value, the treatment group earns more on average than the baseline. Again, this is an acceptable trade-off given the other improvements.

Table 3.22 compares the *ex ante* supporters of the contracts decision in the five-to-four and baseline groups. Prior to matching, there are a few variables that are out of balance. Most seriously is that the treatment group prefers mediation over lawsuits significantly more strongly than the baseline group does. In addition to this, the treatment group contains fewer nonwhites. Moreover, the *t*-test *p*-value is just shy of the .05 mark on the religiosity variable, suggesting the baseline group may be more religious (though the KS test shows that the groups are sufficiently balanced). Matching makes significant improvements after matching, including universal improvement among the unmatched covariates. Unfortunately, the KS test *p*-values drop below .05 on religiosity and diffuse support (second factor), but the *t*-test *p*-values indicate comfortable balance.

Table 3.20. Covariate balance between baseline and unanimous groups (*ex ante* supporters): contract disputes.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.051	.493		.097	.164	
Mediation over lawsuits	.067	.126	.372	**-.011**	**.656**	**.932**
Diffuse support (1)	.176	.217	.534	**.117**	.162	.092
Diffuse support (2)	.086	.019	.014	**.027**	**.187**	**.042**
News interest	.055	.250	.192	.029	.179	.086
Voter registration	-.001	.981		-.043	.256	
Political ideology	.109	.032	.060	**.018**	**.554**	**.714**
Party identification	.044	.476	.346	**.026**	**.578**	**.726**
Income	1.132	.054	.210	**.449**	**.315**	**.480**
Age	1.245	.659	.138	-4.464	.044	.128
Sex	.043	.627		**.014**	**.656**	
Race	0	.995		.029	.317	
Religiosity	.001	.994	.720	-.099	.235	.468
South	.161	.055		**.014**	**.656**	
Unmatched covariates						
National sales tax	.105	.046	.146	.112	.041	.090
Universal healthcare	-.036	.621	.654	**-.011**	**.857**	.640
Gun rights	.122	.045	.056	**.065**	**.188**	**.594**
School prayer	.106	.120	.214	**-.080**	**.142**	**.358**

Post-matching improvements are in **bold**.

Table 3.21. Covariate balance between baseline and eight-to-one groups (*ex ante* supporters): contract disputes.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.048	.528		**.032**	.156	
Mediation over lawsuits	.044	.318	.662	**0**	**1**	**1**
Diffuse support (1)	.039	.800	.768	**-.013**	**.929**	.442
Diffuse support (2)	.122	.001	.012	**.029**	**.230**	**.076**
News interest	.005	.913	.958	.022	.415	.582
Voter registration	-.016	.796		-.016	.317	
Political ideology	.048	.345	.692	**.036**	**.376**	.690
Party identification	.005	.933	.518	.019	.670	.204
Income	.645	.280	426	1.032	.020	.134
Age	-.145	.962	.866	-2.436	.311	.448
Sex	.081	.373		**.032**	**.528**	
Race	-.032	.690		**-.016**	.317	
Religiosity	.004	.971	.306	-.074	.276	**.354**
South	.048	.560		**.016**	**.317**	
Unmatched covariates						
National sales tax	.101	.084	.068	**.036**	**.544**	**.574**
Universal healthcare	.060	.391	.446	**.036**	**.502**	**.862**
Gun rights	.105	.082	.214	**-.020**	**.703**	**.484**
School prayer	.036	.595	.588	-.081	.180	.190

Post-matching improvements are in **bold**.

Table 3.22. Covariate balance between baseline and five-to-four groups (*ex ante* supporters): contract disputes.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.095	.234		**.017**	**.819**	
Mediation over lawsuits	.111	.016	.006	**-.017**	**.494**	**.732**
Diffuse support (1)	.211	.151	.280	**-.027**	**.700**	**.318**
Diffuse support (2)	.044	.275	.576	**.036**	.230	.046
News interest	.007	.880	.420	**.006**	**.882**	**.956**
Voter registration	.061	.261		**-.017**	**.317**	
Political ideology	-.021	.683	.894	**-.017**	.480	**1**
Party identification	-.050	.447	.612	**.008**	**.751**	.476
Income	1.178	.084	.032	**.949**	**.135**	**.046**
Age	2.992	.327	.480	-1.559	**.517**	.190
Sex	.005	.955		.085	.130	
Race	-.155	.037		**-.017**	**.565**	
Religiosity	-.217	.053	.118	**-.090**	**.222**	.010
South	.116	.180		**.017**	**.317**	
Unmatched covariates						
National sales tax	.068	.235	.312	**-.008**	**.873**	**.950**
Universal healthcare	.065	.374	.500	**-.004**	**.935**	**.908**
Gun rights	.033	.611	.866	**.008**	**.885**	**.942**
School prayer	.045	.520	.730	**.004**	**.935**	**.934**

Post-matching improvements are in **bold**.

Turning to the *ex ante* opponents, Table 3.23 compares the covariate balance between the unanimous treatment group and the baseline. Again, matching produces widespread improvement. This includes a resolution of the clear imbalance on the groups' level of interest in current events (the treatment group reports to be more interested in the news than the baseline did) and a potential imbalance on age (the treatment group is older than the baseline). In addition, matching also achieves perfect or near perfect balance on two of the four unmatched variables – respondents' opinions on gun rights and school prayer.

Looking at the equivalent match-up with the eight-to-one treatment group, shown in Table 3.24, we see similarly high levels of improvement after matching. Although the covariate means are reasonably balanced for the most part prior to matching, the two groups are seriously imbalanced on the strength of their *ex ante* opposition: the baseline group is more strongly opposed to the Court's ultimate position. Matching corrects for this and strengthens the balance on most other covariates as well, including all of the unmatched variables. However, in the process, some balance is sacrificed on the news interest variable, particularly according to the *t*-test: after matching, those in the treatment group are arguably more interested in the news than their baseline counterparts. However, given the ambiguity of this imbalance (the KS test *p*-value is low but outside the standards for statistical significance) and the other improvements made by this specification, this again appears to be an acceptable loss.

The final comparison, between the five-to-four treatment and the baseline, shown in Table 3.25, also shows many improvements in covariate balance. Unlike many of the other pairings, however, covariate balance is satisfactory across the board before matching. Nonetheless, matching still improves the balance on most variables, thereby optimizing the overall statistical balance between the two groups.

Table 3.23. Covariate balance between baseline and unanimous groups (*ex ante* opponents): contract disputes.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.155	.046		**0**	**1**	
Mediation over lawsuits	.028	.508	.494	**-.007**	**.746**	**.810**
Diffuse support (1)	.137	.319	.260	**.041**	**.491**	**.314**
Diffuse support (2)	.006	.876	.390	-.010	.453	.108
News interest	.103	.037	.010	**.019**	**.480**	**.262**
Voter registration	.075	.187		**.014**	**.317**	
Political ideology	.028	.545	.698	**-.003**	**.925**	**.986**
Party identification	.021	.730	.538	**-.019**	.726	**.858**
Income	-.036	.954	.912	.167	.647	.486
Age	4.786	.080	.076	1.472	.380	.290
Sex	.054	.538		**.014**	.317	
Race	.052	.508		.097	.106	
Religiosity	.089	.402	.702	**.046**	.361	.298
South	-.004	.962		-.153	.084	
Unmatched covariates						
National sales tax	-.030	.613	.962	-.042	.464	.202
Universal healthcare	-.008	.907	.550	-.017	.748	.188
Gun rights	-.012	.835	.902	**0**	**1**	.834
School prayer	.046	.490	.786	**0**	**1**	**1**

Post-matching improvements are in **bold**.

Table 3.24. Covariate balance between baseline and eight-to-one groups (*ex ante* opponents): contract disputes.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.180	.020		**-.031**	**.565**	
Mediation over lawsuits	-.041	.385	.472	-.050	.110	.406
Diffuse support (1)	.104	.467	.884	**.077**	.232	.154
Diffuse support (2)	-.046	.237	.422	**-.002**	**.927**	**.512**
News interest	.084	.074	.172	**.072**	.014	.088
Voter registration	.082	.155		**.015**	**.317**	
Political ideology	-.028	.568	.938	-.046	.062	.850
Party identification	-.083	.165	.164	**-.008**	**.784**	**.928**
Income	.874	.334	.600	**.369**	**.414**	**.606**
Age	1.960	.476	.738	**-.246**	**.877**	.726
Sex	.047	.595		**.031**	.156	
Race	.038	.630		.062	.433	
Religiosity	-.020	.853	.700	**.008**	**.927**	.334
South	.081	.354		**-.031**	**.638**	
Unmatched covariates						
National sales tax	-.051	.395	.652	**-.008**	**.878**	**.872**
Universal healthcare	.042	.539	.516	**-.015**	**.743**	**.806**
Gun rights	-.064	.312	.446	**-.054**	**.320**	.168
School prayer	-.026	.695	.592	-.050	.407	**.618**

Post-matching improvements are in **bold**.

Table 3.25. Covariate balance between baseline and five-to-four groups (*ex ante* opponents): contract disputes.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.115	.157		**.016**	**.706**	
Mediation over lawsuits	.003	.948	.534	-.012	.591	**.840**
Diffuse support (1)	.098	.491	.250	.106	.363	.060
Diffuse support (2)	-.033	.361	.514	**.025**	**.362**	.362
News interest	.037	.468	.228	**.026**	**.297**	.082
Voter registration	.002	.970		.031	.528	
Political ideology	0	.993	.878	-.004	.908	**.986**
Party identification	-.020	.735	.752	**.010**	**.787**	.286
Income	.610	.335	.822	**.375**	**.250**	.582
Age	-.934	.731	.508	-1.937	.492	**.754**
Sex	.055	.537		**.031**	.317	
Race	.090	.273		.047	.081	
Religiosity	.079	.464	.690	**.045**	**.665**	**.972**
South	.057	.516		**-.016**	**.848**	
Unmatched covariates						
National sales tax	-.040	.505	.764	**.012**	**.833**	**.884**
Universal healthcare	-.006	.927	.984	-.043	.395	.510
Gun rights	.012	.834	.754	**.008**	**.876**	.404
School prayer	.030	.665	.746	.145	.033	.108

Post-matching improvements are in **bold**.

Post-Matching Results

Having achieved optimal covariate balance, we can now estimate the effects of the treatment variations on the dependent variables. Table 3.26, however, confirms the results of pre-matching analysis in Table 3.2: there is no apparent effect of majority size on whether one agrees with the Court's ruling on same-sex unions. Table 3.27 also confirms the lack of evidence for an effect when the dependent variable is measured by acceptance of the Court's decision instead of agreement.

Table 3.26. Post-matching estimates for effect of majority size on agreement with Court decision: same-sex unions.

Treatment	*Ex ante* supporters		*Ex ante* opponents	
	Estimate	Abadie-Imbens Standard Error	Estimate	Abadie-Imbens Standard Error
9-0	.082	.084	.088	.077
8-1	.030	.086	.053	.060
5-4	-.087	.093	-.014	.090

Table 3.27. Post-matching estimates for effect of majority size on acceptance of Court decision: same-sex unions.

Treatment	*Ex ante* supporters		*Ex ante* opponents	
	Estimate	Abadie-Imbens Standard Error	Estimate	Abadie-Imbens Standard Error
9-0	.066	.075	.013	.103
8-1	.060	.085	.035	.102
5-4	-.058	.080	.014	.109

Turning to the medium salience issue of employee privacy, Table 3.28 also shows no evidence that majority size affects agreement with the Court. As suspected, matching appears to have corrected the theoretically inexplicable pre-matched finding in Table 3.4 – that an eight-to-one, but not a five-to-four or unanimous, decision increases the rate of agreement with the Court. Having attained statistical balance on the covariates, it now seems quite likely that result was due to imbalance on the diffuse support variable.

Table 3.28. Post-matching estimates for effect of majority size on agreement with Court decision: employee privacy.

	Ex ante supporters		*Ex ante* opponents	
Treatment	Estimate	Abadie-Imbens Standard Error	Estimate	Abadie-Imbens Standard Error
9-0	-.015	.067	0	.084
8-1	.066	.060	.100	.083
5-4	.014	.049	.140	.093

Table 3.29. Post-matching estimates for effect of majority size on acceptance of Court decision: employee privacy.

	Ex ante supporters		*Ex ante* opponents	
Treatment	Estimate	Abadie-Imbens Standard Error	Estimate	Abadie-Imbens Standard Error
9-0	-.015	.062	.058	.109
8-1	.049	.056	.300**	.112
5-4	.014	.049	.298**	.101

** $p < .01$

Looking at the estimates for acceptance of the Court's decision on employee privacy tells a much different story. Although, among *ex ante* supporters, there is no significant difference between the baseline group and any of the treatment groups, Table 3.29 does show statistically significant estimates among *ex ante* opponents. For those respondents, assignment to the eight-to-one and five-to-four treatment groups both produce estimates for the rate of decisional acceptance that are significantly higher than the baseline. Hence, it appears that any degree of dissent, whether the vote is eight-to-one or five-to-four, makes *ex ante* opponents more receptive to the Court's ruling. This is consistent with the unanimity-based political agreement hypothesis described above. Furthermore, it is worth noting that the analogous pre-matching analysis, presented in Table 3.5, did not find such a result. Meanwhile, the relevant covariate balance analyses, particularly that shown in Tables 3.18, corrected for a number of asymmetries between the treatment and control groups. Thus, it seems that the true effect of majority size in this instance was disguised by a failure of the experimental randomization to achieve optimum statistical balance.

Meanwhile, the rate of agreement with the Court's decision on contract dispute resolution, as shown in Table 3.30, is unaffected by majority size. This again corroborates the pre-matched results displayed in Table 3.6. However, Table 3.31 also replicates the significant findings of Table 3.7: *ex ante* opponents exposed to large majorities are more accepting of the

Court's decision on this issue. This again supports the anti-division hypothesis, at least among those predisposed to have hostile attitudes toward the Court's ruling – although these results do not show narrow division to hurt *ex ante* opponents' specific support for the Court ruling, large majorities, both unanimous and non-unanimous, do appear to boost it, at least where this issue of low ideological salience is concerned.

Table 3.30. Post-matching estimates for effect of majority size on agreement with Court decision: contract disputes.

Treatment	*Ex ante* supporters		*Ex ante* opponents	
	Estimate	Abadie-Imbens Standard Error	Estimate	Abadie-Imbens Standard Error
9-0	-.029	.076	.153	.101
8-1	-.081	.077	.200	.103
5-4	0	.082	0	.085

Table 3.31. Post-matching estimates for effect of majority size on acceptance of Court decision: contract disputes.

Treatment	*Ex ante* supporters		*Ex ante* opponents	
	Estimate	Abadie-Imbens Standard Error	Estimate	Abadie-Imbens Standard Error
9-0	-.014	.067	.222*	.106
8-1	-.048	.064	.262*	.112
5-4	-.017	.071	.156	.095

* *p* < .05

Summary

The findings presented in this chapter give us a much different and far more nuanced view of the interaction of judicial majority size and public opinion than has been described previously in the scholarly literature. This chapter finds no evidence that majority size affects individuals' level of agreement with Court decisions, nor does it find any evidence that those who already agree with the ultimate outcome of the decision can be moved by the number of justices casting dissenting votes. This alone shows a failure to replicate Zink, Spriggs, and Scott's result, which up to this point stood alone in finding that majority size can manipulate public

attitudes. Instead, this chapter finds that *ex ante* opponents of the Court's policy may be persuaded to accept judicial decisions with which they disagree.

The presence and dynamics of this effect, however, are contingent on the policy under review. The results show no evidence that the level of Supreme Court unity has any impact on public opinion of highly salient same-sex unions rulings. On the other hand, the results do show that, among *ex ante* opponents, any level of division on the Court leads to higher acceptance rates of the somewhat salient employee privacy decisions. Meanwhile, unanimity and near unanimity are associated with higher acceptance rates among the decisions regarding contract dispute resolution, which is of low salience. Together, this suggests that the way in which court division affects public opinion is highly dependent on the salience of the issue at hand.

If we are to generalize these results, it appears that an individual's attitudes are likely to be too crystallized on issues of high salience for the level of unity on the Court to make any significant difference. However, on somewhat less salient issues, individuals may be moved by the level of division, although this appears to happen in a way that reflects support for the dissidents on the Court. In other words, the higher acceptance levels may reflect warmer feelings toward a Court that has members receptive to the respondent's position. While this shows a lack of strategic thinking on the part of the respondent, it is consistent with theories of public perceptions of procedural justice (Lind and Tyler 1988). When it comes to issues on which individuals are not likely to have very strong opinions, large majorities are likely to persuade *ex ante* opponents to accept adverse opinions.

Given these results, it is possible that, with the proper publicity, the Court could build its political capital with the public over time by standing united on issues of low salience and representing a diversity of opinions on those of somewhat higher salience. While it is difficult to assess this type of long term effect, particularly in a short term study such as this, these results do suggest that such a long term view of the Court ought to be considered in future research. Regardless, the implications of these findings are that a Court strategically trying to build its reserve of public confidence is not best off acting unanimously as much as possible; rather it is better off acting unanimously selectively.

Chapter 4

Ideological Perceptions:

Public Opinion of Unity in the Lower Courts

The previous chapter established that 1.) judicial division does affect the public's acceptance of Supreme Court decisions and 2.) the nature and direction of this effect is highly dependent on the salience of the policy issue involved. At the same time, this opens the door to other questions about the public's response to judicial dissent and unanimity. For one, *is this phenomenon unique to reactions to the Supreme Court, or can we find evidence of it in response to lower appellate courts?* Although the Supreme Court is undoubtedly the nation's single most visible judicial body, the federal Courts of Appeals also provide an environment that could potentially be open to the unity effects observed in Chapter 3. After all, these courts are of fairly high visibility in which panels of judges vote on rulings that have the potential to have an enormous impact on public policy. However, in spite of their policy importance and potential for conflict, they are still secondary to the Supreme Court: not only are Court of Appeals judges obliged to uphold Supreme Court precedent, their decisions are reviewable by the high court, making reversal a potential concern for them. As such, these courts may not bear nearly the same weight with the public. Thus, it is reasonable to ask if the weaker visibility of these courts – as well as the conflict among their judges – is enough to erase the effect that unity and dissent have on public attitudes.

Of course, unlike the Supreme Court, which is a single body of nine justices whose composition only changes slowly over time, the United States Courts of Appeals constitute a far larger network of courts and judges, consisting of a dozen individual regional courts featuring rotating three-judge combinations. Further complicating things is the fact that many of these courts have different ideological reputations from one another. For instance, the Court of Appeals for the Ninth Circuit, which includes California, is often considered to be the most politically liberal, while the Fourth Circuit Court of Appeals, located in the South, is often ranked among the most conservative. This in turn raises the question, *how does the perceived ideology of a court interact with the effect of unity and dissent on public reaction?*

One might expect the effect of perceived ideology to matter a great deal when considering the effects of unanimity and dissent. For instance, imagine that Ninth Circuit Court of Appeals hands down a unanimous ruling. A staunch conservative, aware of the court's liberal reputation, may be dismissive of the consensus among the judges, since she knows that she is likely going to disagree with many – or perhaps all – of the judges on that court anyway. Likewise, a liberal citizen may view a unanimous Fourth Circuit decision similarly. From this perspective, an individual's ideological perception of the court is intrinsically linked to any affect that unanimity or division might have.

The importance of this question is also apparent when considering the sources of public support for judicial institutions. Tyler and Rasinski (1991) argue that public support for judicial behavior is rooted in perceptions of procedural fairness employed by the system. At odds with this theory, Gibson (1989; 1991) argues that this support derives from overall attitudes toward courts as legitimate institutions with large reserves of political capital. In either case, however, an opinion that a court is politically biased would damage this support: belief that a court is ideologically biased could conceivably damage perceptions of its procedural fairness and cost the court political capital. Therefore, in both scenarios, perceived ideological bias is harmful to the public support for the court.

This chapter, like the previous chapter, presents and analyzes the results of an experimental survey. In this case, the survey seeks to understand the public's response to unanimous and divided decisions from appeals courts with different ideological reputations. In the subsequent section, I expand on the hypotheses of this chapter and explain its experimental design. Following that, I present the experimental results. Next, I further analyze these data to verify the robustness of the results, which ultimately suggest that the Supreme Court is unique in its ability to foster popular reactions to its decision based on majority size. Finally, I discuss these results and offer some suggestions for future research.

Hypotheses and Experimental Design

This chapter presents an experimental design modified from that presented in Chapter 3. First and foremost, the vignettes given to the respondents now discuss decisions from a "federal appeals court" rather than from the Supreme Court. However, given the lack of exploration of public reaction to decisions from courts other than the Supreme Court, it is important to revisit this dissertation's underlying question: *Can the degree of judicial unity impact public reaction to court decisions?* That is, we should not necessarily expect to be able to replicate the results of the previous chapter if we are simply to change the venue from Supreme Court to circuit court. As noted in Chapter 1, the general consensus regarding the Supreme Court's ability to move public opinion is that it is able to translate public support for the institution into positive attitudes toward the policies it promotes. It is an open question whether or not the lower courts also hold a strong enough level of public confidence to allow for this legitimacy transfer. Clearly, such an ability is a precondition for any effect that court unity may have on the public. Thus, the presence or nonexistence of a detectable effect of majority size on the public will help us understand if the effect of unanimity and dissent on public opinion might be universal across all courts or if this attention to the level of unity is peculiar to the Supreme Court.

This experiment also differs from the previous one in that it only gives two levels of unity: unanimous and divided. While this was a point of criticism in the previous chapter's discussion of Zink, Spriggs, and Scott's (2009) work regarding public attitudes toward Supreme Court decisions, it is, in fact, appropriate here. With the exception of *en banc* decisions, the

United States Courts of Appeals hear and decide cases in panels of three judges, thereby requiring a two-judge majority for a ruling. Thus, there really are only two levels of division: unanimous (three votes to none) and divided (two votes to one). Hence, division need not be treated as a nuanced phenomenon.

In the event that the lower courts are able to have a similar effect on popular opinion, it is worth examining whether ideological congruence with the court trumps any effect that division or unanimity might have. As such, the vignettes not only vary in the level of division, but in the ideological reputation of the court handing down the ruling. The vignettes therefore discuss either a "generally liberal" or "generally conservative" federal court. Thus, by randomizing both the court's level of unity (divided or unanimous) and its ideological leaning (liberal or conservative), the experimental treatment is the interaction between division and ideology.

Since the data include the respondents' self-identified political ideology, the experiment produces four treatment groups: those who are exposed to a unanimous decision from a court that shares the respondent's ideological preference, those who also receive a unanimous decision but from a court with an opposing ideology, those who are presented with a divided ruling from a court that shares their ideology, and those who receive a divided ruling from an ideologically opposed court. Thus, using these groups, we may hypothesize the relationship between unity, ideological congruence, and public attitudes toward the court's decision.

For instance, we may hypothesize that both ideological agreement and unity affect public opinion, but one effect is stronger than the other. Illustrating an example of this, Figure 4.1 shows the relative arrangement of treatment groups by attitudes toward the court decision if a.) both unity and ideology matter, b.) unity is a stronger factor in determining attitudes than ideology is, and c.) the respondent is more persuaded by unanimous decisions than divided decisions.[17] The diagram shows that support for the decision is clustered primarily based on the level of the court's level of unity, but ideological similarity drives support within each cluster. In other words, according to this model, ideological agreement with the Court matters to the respondent, but unanimous rulings are universally favored to divided rulings.

On the other hand, Figure 4.2 represents a similar scenario, except that ideological is a stronger determinant of public opinion. As with the previous figure, unanimity is preferred to division; however, support is clustered by ideological agreement. That is, courts that are ideologically congruent with the respondents always yield higher levels of support than those that are opposed; however, the level of division affects the level of support within each ideological category.

---

[17] Given the results in Chapter 3, it is plausible, if not likely, that in certain instances, the opposite of the third condition may be true.

Figure 4.1. Predicted level of decision support by treatment group with judicial unanimity as the preferred and dominant characteristic.

Less supportive   More Supportive

←———————|————————————|————————————|————————————|———————→

Divided &   Divided &   Unanimous &   Unanimous &
Ideologically   Ideologically   Ideologically   Ideologically
opposite   similar   opposite   similar
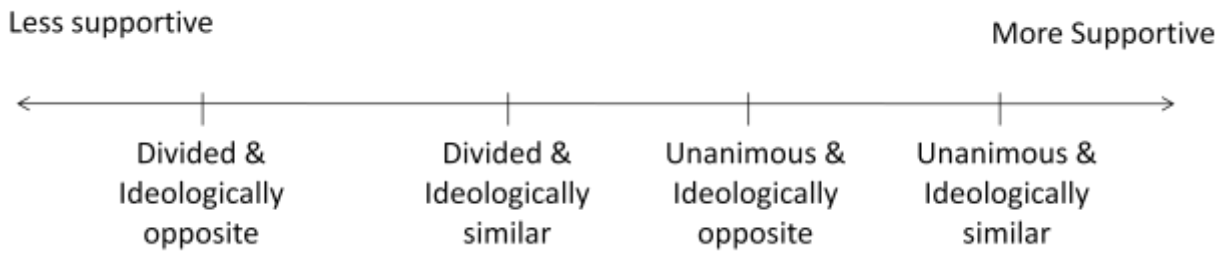
Figure 4.2. Predicted level of decision support by treatment group with ideological agreement as dominant characteristic.

Less supportive   More Supportive

←———————|————————————|————————————|————————————|———————→

Divided &   Unanimous &   Divided &   Unanimous &
Ideologically   Ideologically   Ideologically   Ideologically
opposite   opposite   similar   similar
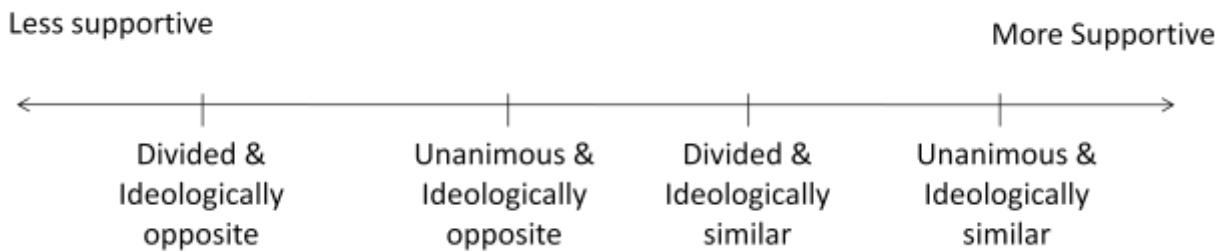
      Keeping in line with the previous experiment's results, this experiment also uses three issue areas of differing ideological salience in the experimental stimuli. These issue areas are affirmative action (high salience), the rights of the accused (medium salience), and employer liability (low salience). Again, respondents identified their attitudes on these issues prior to treatment exposure, and the stimuli are randomly varied in the direction of the decision, allowing the categorization of respondents into *ex ante* supporters and opponents. Thus, examples of these vignettes include, "By a unanimous vote, a generally liberal federal appeals court recently ruled that universities may not consider race in admissions in order to correct for past discrimination," "By a vote of 2 to 1, a generally conservative federal appeals court recently ruled that police officers may question suspected criminals without a lawyer present unless they explicitly request one," and, "By a unanimous vote, a generally liberal federal appeals court recently ruled that employees may not sue their employers for emotional distress after being exposed to toxic chemicals at work if this exposure did not result in any illness or injury" (see Appendix B for a complete list of experimental vignettes). As was the case in the previous chapter, following each vignette, the respondents are asked if they agree with the court decision and, if they do not agree with it, if they accept the decision. The responses on these questions, again, serve as the dependent variables.

Experimental Results

Based on the strength of the respondents' *ex ante* policy preferences, it is clear that, yet again, the respondents' attitudes corroborate the experiment's assumption regarding the ideological salience of each issue. Table 4.1 shows that the proportion of respondents strongly agreeing or disagreeing (as opposed to somewhat agreeing or disagreeing or expressing no opinion) with the court's decision *ex ante* steadily declines with the assumed level of ideological salience. Roughly two-thirds of respondents feel strongly about their position on affirmative action, while roughly half hold strong opinions regarding suspect rights, and less than half have strong attitudes regarding employer liability. Again, the direction of the decision in the experimental vignette is randomized, so the table below is not an indication of the direction of the respondents' opinions prior to treatment.

Table 4.1. Percentage of respondents holding strong *ex ante* positions.

Issue	*Ex ante* supporters	*Ex ante* opponents
Affirmative action	64.03%	67.81%
Suspect rights	54.31%	52.60%
Employer liability	37.05%	42.45%

Much like that presented in the previous chapter, the survey results show a noticeable cleavage between *ex ante* supporters and opponents in their rates of agreement with and acceptance of the courts' decisions. Figure 4.3, which depicts the respondents' rate of agreement with the court decisions on affirmative action by treatment group, shows a vast difference between those inclined to agree with the court and those inclined to disagree. As one would probably expect, the rate of agreement with the court is quite high among *ex ante* supporters – ranging from 84 percent among those who received vignettes describing a unanimous decision from a court of an opposing ideological reputation up to 92 percent among respondents who were exposed to divided courts from ideologically opposing courts. Likewise, support is consistently low among *ex ante* opponents: agreement with the decision ranges from 10 percent among the unanimous-same ideology group to 13 percent among both the unanimous-opposite ideology and divided-same ideology groups.

Figure 4.4 shows similar a similar pattern on the respondents' willingness to accept the court's decision. Acceptance among *ex ante* supporters is even higher than their agreement: acceptance rates among treatment groups range from 92 percent (divided-opposite ideology) to 96 percent (unanimous-same ideology). Acceptance among *ex ante* opponents is

97

considerably lower than that of the supporters, but the range is larger: 19 percent accept the decision in the divided-opposite ideology group, while 38 percent accept it in the unanimous-opposite ideology group. Moreover, among *ex ante* opponents, there appears to be a split in the rate of acceptance according to the level of division: both treatment groups that received a vignette describing a unanimous court have acceptance rates above 35 percent, while both treatment groups that received a divided court are below 25 percent.

Figure 4.3. Agreement with court decision by treatment group: affirmative action.
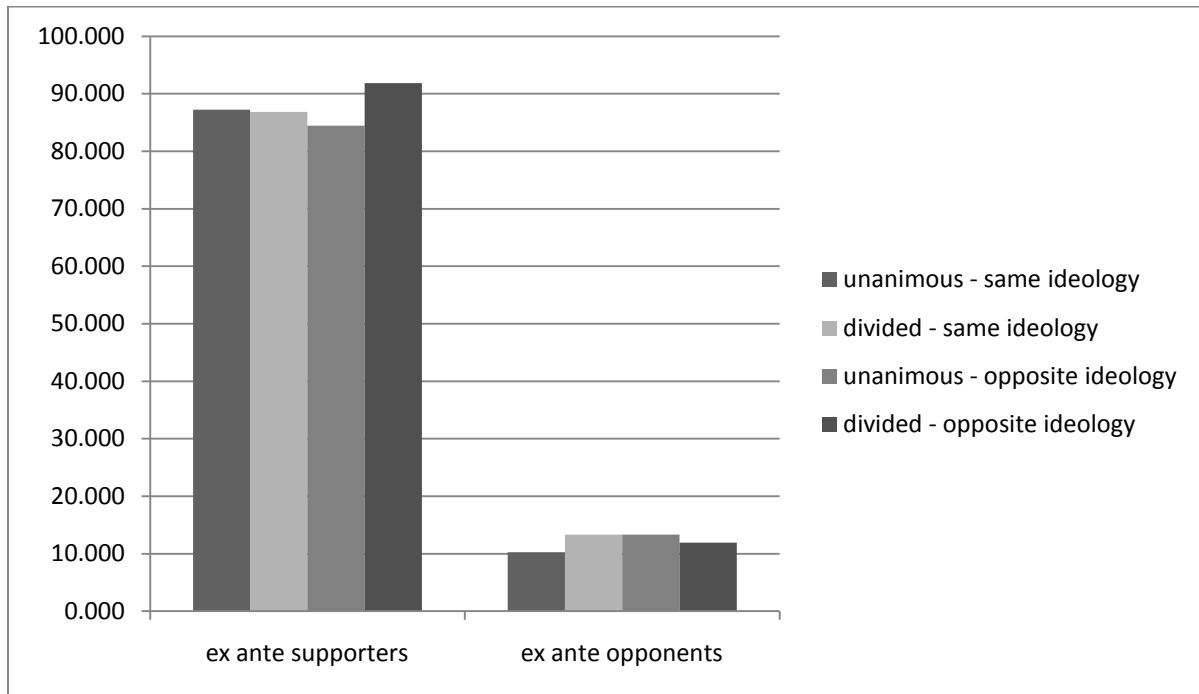
Figure 4.4. Acceptance of court decision by treatment group: affirmative action.
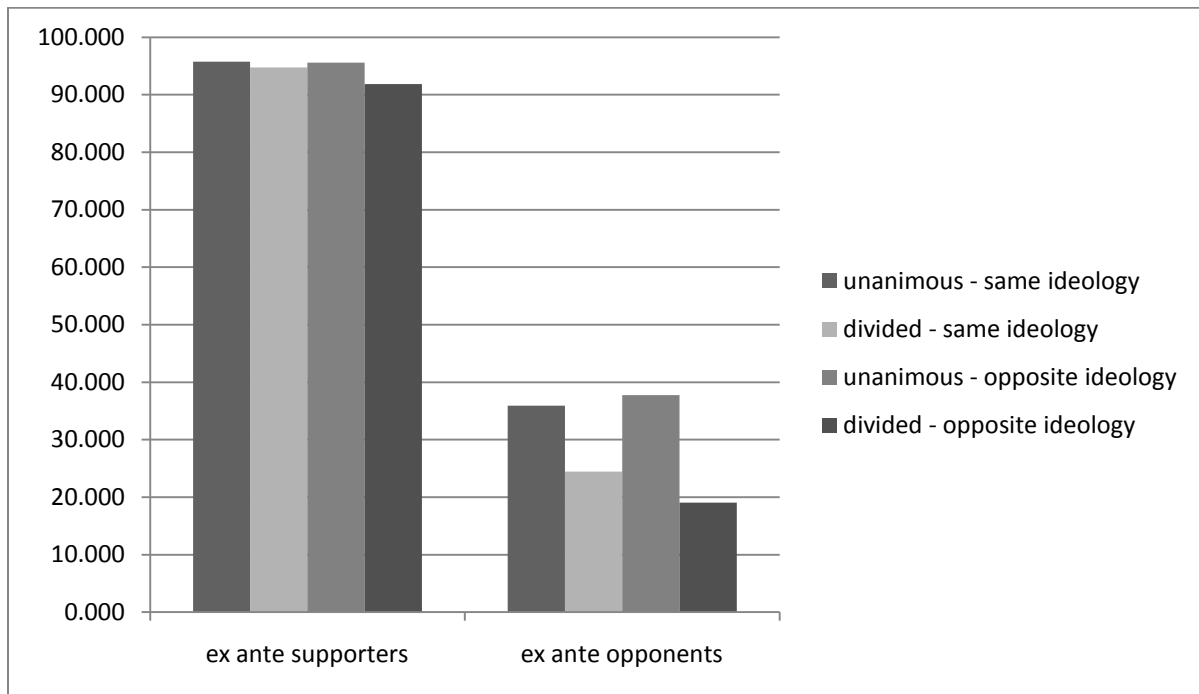
However, the differences between these groups are not statistically significant. Tables 4.2 and 4.3 show significance tests for the mean differences between each treatment group and the unanimous-same ideology group. This group is treated as a baseline group due to its hypothesized placement at one of the extremes in Figures 4.1 and 4.2. Given the very small differences between treatment groups in Figure 4.3, the results presented in Table 4.2 are not surprising. The same can be said of the results for *ex ante* supporters in Table 4.3. However, in spite of the hint of support for a pro-unanimity hypothesis among *ex ante* opponents, Table 4.3 shows these differences unable to pass standard statistical significance tests. It should also be noted that the largest difference in this set of treatment groups, that between the unanimous-opposite ideology group and the divided-opposite ideology group, is not shown in the table; however, this difference two falls short of conventional measures of statistical significance, but by a much closer margin ($p = .055$). Thus, it may be unfair to shut the door completely on the hypothesized relationship between majority size and decision acceptance among *ex ante* opponents, but the evidence found here is weak at best.

Table 4.2. Differences in rates of agreement with court decision: affirmative action.

Treatment	Ex ante supporters			Ex ante opponents		
	Difference	S.E.	n	Difference	S.E.	n
Divided-same	-.004	.074	38	.031	.072	45
Unan. – Opp.	-.028	.073	45	.031	.072	45
Divided – Opp.	.046	.063	49	.016	.071	42
	(Unan. – Same n = 47)			(Unan. – Same n = 39)		

Table 4.3. Differences in rates of acceptance of court decision: affirmative action.

Treatment	Ex ante supporters			Ex ante opponents		
	Difference	S.E.	n	Difference	S.E.	n
Divided-same	-.010	.047	38	-.115	.100	45
Unan. – Opp.	-.002	.043	45	.019	.107	45
Divided – Opp.	-.039	.050	49	-.168	.098	42
	(Unan. – Same n = 47)			(Unan. – Same n = 39)		

Turning from the case of racial considerations to that of police interrogations, we can see some similarities in the patterns across treatment groups. Figure 4.5 again shows a clear divide between *ex ante* supporters and opponents. However, there is a bit more variation within each preference group by treatment assignment. Among supporters, there is a considerable range in the level of agreement with the decision: 95 percent of those in the unanimous-same ideology group agree with the court's ruling, but only 79 percent of those in the divided-same ideology group do. Meanwhile, among opponents, 36 percent of those in the dived – same ideology group agree with the ruling, compared to 25 percent of those in the divided-opposite ideology group. Although the variation across treatment groups appears to be stronger than it was with the previous issue, there does not appear to be any consistent cleavage across either the ideology or unity dimensions, making these between-group

differences difficult to reconcile with the theoretical understanding of these elements' role in public opinion formulation.

Figure 4.5. Agreement with court decision by treatment group: suspect rights.
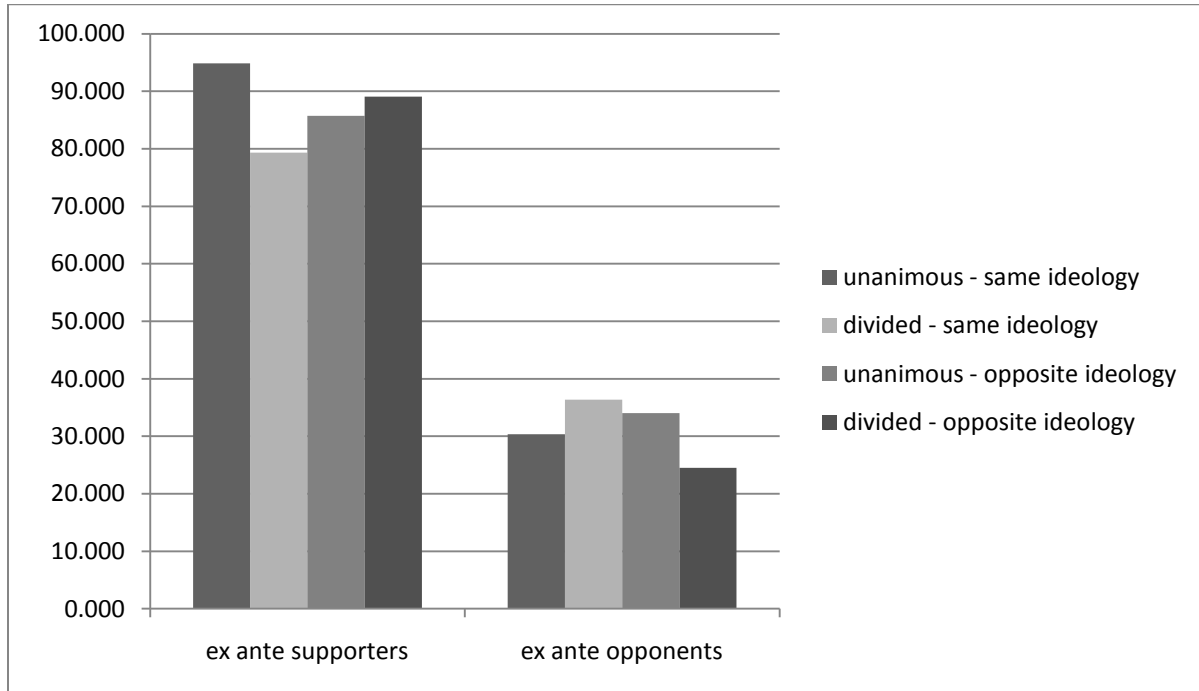
The distribution of acceptance rates across treatment groups is similar. The gap between supporters and opponents, however, is somewhat less dramatic, though it is clearly present. As with the previous measure, there is a rather large difference between the acceptance rate of the unanimous-same ideology (97 percent) and the divided-same ideology (79 percent) groups of *ex ante* supporters. While the two unanimous treatment groups in this subset of respondents have the two highest acceptance rates, the difference between the unanimous-opposite ideology and divided-opposite ideology group is too small to clearly suggest a trend that fits with a pro-unanimity hypothesis. There likewise exists variation in acceptance of the decision among the *ex ante* opponents: the divided-same ideology treatment group is the most accepting of the decision at 55 percent, while only 38 percent of the divided-opposite ideology accept the ruling. Again, though these differences are interesting, neither of these distributions match the theorized relationship between ideology, unity, and public attitudes.

Figure 4.6. Acceptance of court decision by treatment group: suspect rights.
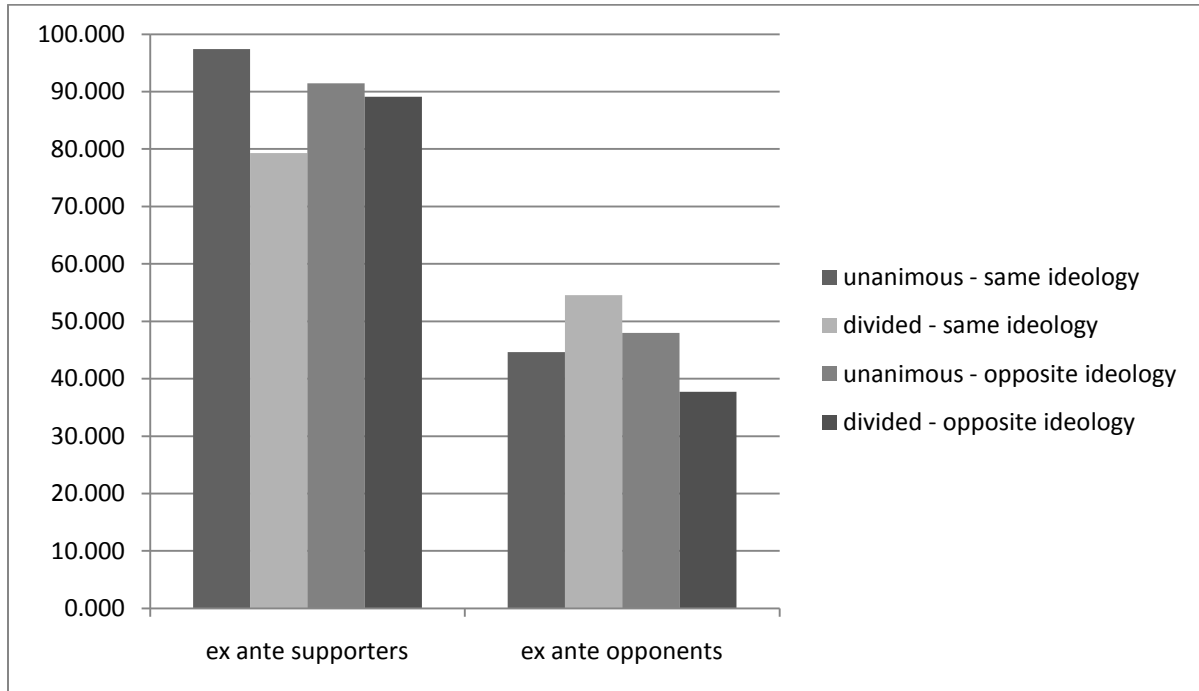
For the most part, however, the differences between these groups are not statistically significant. Regardless, the largest difference among *ex ante* supporters for both measures – between the unanimous-same ideology group and the divided-same ideology group – is significant at the .05 level, as evident in Tables 4.4 and 4.5. The largest difference among *ex ante* opponents – that between the divided-same ideology and divided-opposite ideology groups – is not captured when using the unanimous-same ideology group as a baseline; however, it is worth noting that the difference between these two groups rates of agreement and acceptance are insignificant, yielding *p*-values of .23 and .10, respectively. Nonetheless, a significant difference between these two groups and none of the others is unexplained by any of the theoretical explanations offered above.

Table 4.4. Differences in rates of agreement with court decision: suspect rights.

Treatment	Ex ante supporters			Ex ante opponents		
	Difference	S.E.	n	Difference	S.E.	n
Divided-same	-.156*	.078	29	.060	.095	44
Unan. – Opp.	-.092	.068	35	.036	.092	50
Divided – Opp.	-.058	.059	35	-.058	.086	53
	(Unan. – Same n = 39)			(Unan. – Same n = 56)		

* $p < .05$

Table 4.5. Differences in rates of agreement with court decision: suspect rights.

Treatment	Ex ante supporters			Ex ante opponents		
	Difference	S.E.	n	Difference	S.E.	n
Divided-same	-.181*	.072	29	.099	.101	44
Unan. – Opp.	-.060	.053	35	.034	.098	50
Divided – Opp.	-.083	.055	35	-.069	.095	53
	(Unan. – Same n = 39)			(Unan. – Same n = 56)		

* $p < .05$

On the issue of employer liability, the results more clearly resemble that of affirmative action than that of suspect rights in their relative lack of variation in agreement and acceptance rates by treatment group. While Figure 4.7 shows a clear divided in the agreement rates between *ex ante* supporters and opponents of the employer liability case, the range within each support group is fairly small. Among supporters, the unanimous-opposite ideology group is the most favorable toward the decision at an agreement rate of 86 percent, but this is not much higher than the unanimous-same ideology group, which has the least favorable rate at 80 percent. Likewise, the agreement rates among *ex ante* opponents range from 12 percent (divided-same ideology) to 19 percent (unanimous-same ideology). Figure 4.8 shows a very similar pattern for the acceptance rates of this case, though with a slightly narrower gap between supporters and opponents. Still, treatment groups of the *ex ante* supporters of the employer liability decision range from 82 percent (unanimous-same ideology) to 89 percent

(unanimous-opposite ideology) accepting of the ruling, while opponents range from 28 percent (divided-same ideology) to 34 percent (unanimous-same ideology) accepting.

Figure 4.7. Agreement with court decision by treatment group: employer liability.
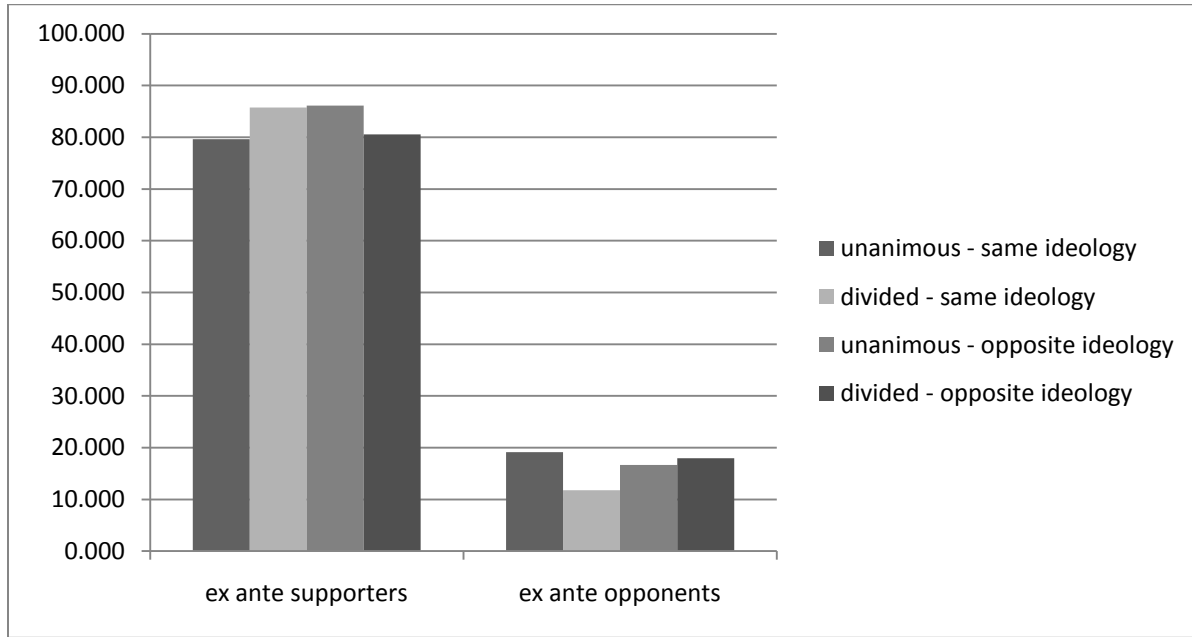
Figure 4.8. Acceptance of court decision by treatment group: employer liability.
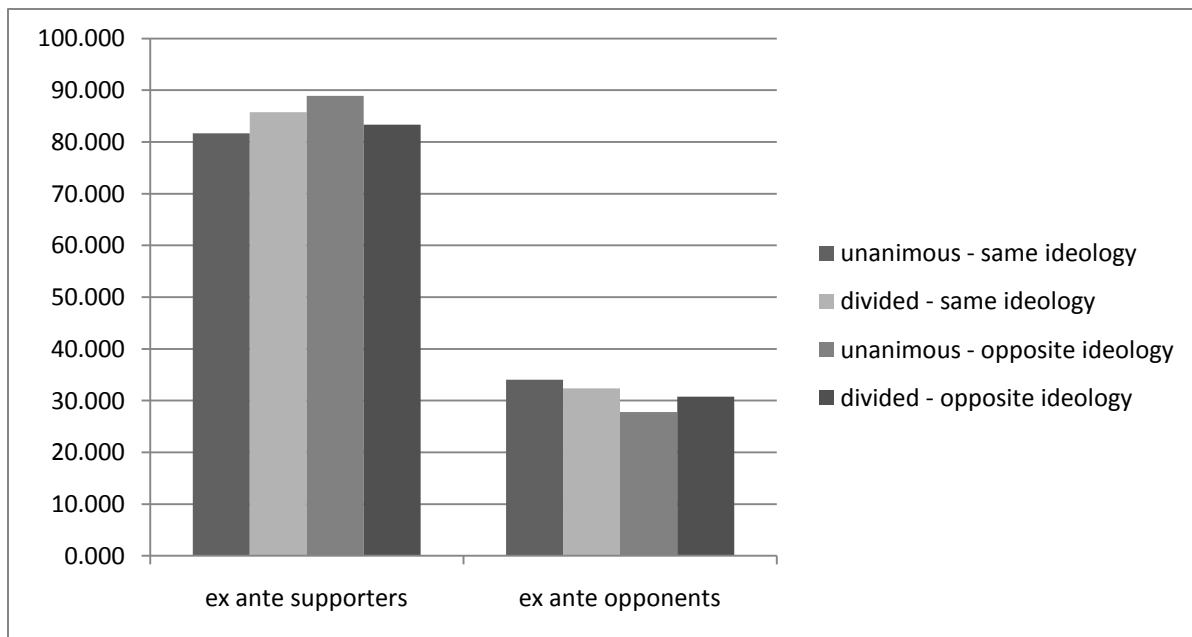
Not surprisingly, the differences between treatment groups on this issue are not statistically significant. The results in Tables 4.6 and 4.7 show there is insufficient evidence to argue that any of the treatment groups are different from the unanimous-same ideology group in either their rate of agreement with or acceptance of the employer liability case. It is also worth noting that treating this group as the baseline does indeed capture the largest between-group differences. Thus, like with the affirmative action example, judicial unity and ideology do not appear to have any effect on the public attitudes toward the decision.

Table 4.6. Differences in rates of agreement with court decision: employer liability.

Treatment	Ex ante supporters			Ex ante opponents		
	Difference	S.E.	n	Difference	S.E.	n
Divided-same	.061	.086	35	-.074	.083	34
Unan. – Opp.	.065	.084	36	-.025	.086	36
Divided – Opp.	.010	.089	36	-.012	.085	39
	(Unan. – Same n = 49)			(Unan. – Same n = 47)		

Table 4.7. Differences in rates of acceptance of court decision: employer liability.

Treatment	Ex ante supporters			Ex ante opponents		
	Difference	S.E.	n	Difference	S.E.	n
Divided-same	.041	.083	35	-.017	.107	34
Unan. – Opp.	.073	.080	36	-.063	.104	36
Divided – Opp.	.017	.085	36	-.033	.103	39
	(Unan. – Same n = 49)			(Unan. – Same n = 47)		

Post-Experimental Manipulation

Covariate Balance

As discussed in the previous chapter, the findings of randomized experiments are limited to the extent that the treatment groups are in balance with one another. Therefore, it is important to check against any imbalances that may either be masking or distorting any treatment effects. Following the example of Chapter 3, I match the treatment groups on the respondents' strength of their *ex ante* policy preference, directional attitude on the issue before them, diffuse support for the Supreme Court, interest in current events, rate of voter registration, political ideology, party identification, income, age, sex, race, religiosity, and geography. I also verify the matching procedure my examining the treatment groups' balance on the respondents' attitudes toward a national sales tax, universal healthcare, gun ownership rights, and public school prayer. (See Chapter 3 for a discussion of each matched and unmatched covariate). In addition, whereas the previous chapter's experiment had a natural baseline group to which all other treatment groups were matched, the treatment groups in this chapter are matched to the unanimous-same ideology group due to its hypothesized extreme position as illustrated in Figures 4.1 and 4.2.

Right away, Table 4.8 reveals balance problems between the unanimous-same ideology and divided-same ideology groups of *ex ante* supporters of the affirmative action case. The most obvious problem is that, according to both the *t* and KS tests, the unanimous group has a higher average interest in the news that is statistically significant. Additionally, the *t*-test shows the unanimous group to have a significantly higher average income, and the KS test also shows that group to be older and less supportive of the Supreme Court (moreover, in the case of income and age, the *p*-value of the other test is not far above the .05 threshold). Matching, however, appears to remedy the balance problems that plague this comparison. The balance is improved on nearly every matched covariate and on all the unmatched variables.

When shifting the comparison to the unanimous-same ideology and unanimous-opposite ideology groups, there are fewer areas that are problematic to the covariate balance, but there is still plenty of room for improvement. As seen in Table 4.9, there no variables for which both *p*-values fall below .05, though both of those for the news interest variable hover just barely above that threshold. Moreover, the KS test *p*-value for school prayer attitudes is within the range of statistical significance, and the *t*-test *p*-value for the age variable is very close to the standard significance cutoff. Fortunately, after matching, the balance nearly every variable is improved once again. Regrettably, though, the KS test *p*-value worsens after matching, falling to .018; however, the *t*-test for this variable is sufficiently high, making a loss of balance on this particular test of this covariate an acceptable trade-off given the vast improvements across the other characteristics.

Turning to the last comparison of *ex ante* supporters on the affirmative action example, the pre-matched treatment groups are mostly in balance, though there are a few instances that are cause for concern. As shown in Table 4.10, the unanimous-same ideology group and the

divided-opposite ideology group are clearly off-balance when it comes to the respondents' age – the former is significantly older on average than the latter. Moreover, according to the *t*-test, the two groups are also imbalanced on their average income, suggesting that the unanimous-same ideology group is the richer of the two. Luckily, the matching procedure remedies these imbalances and improves the balance statistics for nearly every covariate.

Table 4.8. Covariate balance between unanimous-same ideology and divided-same ideology groups (*ex ante* supporters): affirmative action.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.145	.096		**0**	**1**	
Affirmative action	-.101	.171	.086	**.053**	**.285**	**.554**
Diffuse support (1)	-.269	.171	.030	**.043**	**.639**	**.330**
Diffuse support (2)	.079	.098	.084	**-.022**	**.542**	**.668**
News interest	-.172	.007	.036	**-.026**	**.468**	**.558**
Voter registration	-.089	.163		**-.053**	**.415**	
Political ideology	.068	.347	.528	**-.013**	**.837**	**.998**
Party identification	-.035	.650	.684	-.053	.387	**.816**
Income	-1.705	.020	.072	**-.158**	**.670**	**.530**
Age	-5.865	.070	.036	**-1.552**	**.514**	**.182**
Sex	-.142	.193		**-.079**	**.468**	
Race	.098	.303		**.026**	**.317**	
Religiosity	-.028	.820	.874	-.105	.317	.736
South	-.067	.523		.079	.317	
Unmatched covariates						
National sales tax	.010	.892	.272	-.013	.839	**.374**
Universal healthcare	.009	.924	.668	.020	.800	**.752**
Gun rights	.120	.099	.142	**.017**	**.927**	**.936**
School prayer	.065	.410	.322	**0**	**1**	**.948**

Post-matching improvements are in **bold**.

Table 4.9. Covariate balance between unanimous-same ideology and unanimous-opposite ideology groups (*ex ante* supporters): affirmative action.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.123	.216		**.022**	**.317**	
Affirmative action	-.062	.358	.426	**-.050**	.264	.328
Diffuse support (1)	.076	.694	.762	**-.058**	.635	.582
Diffuse support (2)	.029	.513	.804	**.025**	.429	.450
News interest	-.113	.057	.050	**-.044**	**.273**	.018
Voter registration	-.069	.224		**-.044**	.155	
Political ideology	-.036	.615	.660	**.017**	**.707**	**.852**
Party identification	-.110	.146	.392	**.019**	**.656**	**.968**
Income	-.374	.593	.848	**.267**	.471	.246
Age	-6.481	.051	.134	**-2.778**	**.221**	**.238**
Sex	-.022	.837		.044	.619	
Race	.053	.544		.089	.155	
Religiosity	-.061	.630	.212	-.082	.470	**.416**
South	-.005	.960		.022	.317	
Unmatched covariates						
National sales tax	-.060	.375	.388	**-.006**	**.931**	**.792**
Universal healthcare	.135	.129	.132	**.044**	**.548**	**.416**
Gun rights	-.074	.330	.294	**-.006**	**.935**	**.342**
School prayer	-.132	.095	.028	**-.022**	**.757**	**.800**

Post-matching improvements are in **bold**.

Table 4.10. Covariate balance between unanimous-same ideology and divided-opposite ideology groups (*ex ante* supporters): affirmative action.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.111	.252		**-.041**	**.481**	
Affirmative action	-.010	.890	.576	**0**	**1**	**.956**
Diffuse support (1)	.107	.532	.202	**-.040**	**.685**	**.628**
Diffuse support (2)	.004	.933	.500	-.026	.356	.628
News interest	-.060	.262	.552	**-.014**	**.716**	.280
Voter registration	-.080	.157		**-.020**	**.317**	
Political ideology	.028	.685	.764	**.026**	.369	**.852**
Party identification	.023	.755	.928	**0**	**1**	**.962**
Income	-1.427	.032	.108	**-.184**	**.652**	**.214**
Age	-6.844	.020	.032	**.061**	**.973**	**.912**
Sex	-.082	.426		**-.020**	**.809**	
Race	.053	.531		**.041**	.317	
Religiosity	.156	.516	.360	**-.052**	**.847**	**.560**
South	.005	.962		.041	.317	
Unmatched covariates						
National sales tax	.020	.759	.758	**-.010**	**.850**	.292
Universal healthcare	.019	.827	.880	.026	.679	.780
Gun rights	.063	.369	.204	**.031**	**.585**	**.546**
School prayer	-.037	.644	.866	-.097	.185	.236

Post-matching improvements are in **bold**.

Looking at the *ex ante* opponents for the affirmative action case, we actually see that the covariates are quite well balanced between the unanimous-same ideology and divided-same ideology groups. Table 4.11 shows that no variable is statistically distinguishable across the two groups, though the KS test *p*-values for diffuse support (second factor) and news interest do come close, as do both balance measures for opinion toward universal healthcare. Regardless, matching improves the balance statistics for the vast majority of the variables. Unfortunately, it does appear that some balance is lost rather than restored to the healthcare opinion variable. However, the fact that matching also produces four virtually perfectly balanced covariates (news interest, voter registration, race, and geography) makes the adjustment appear to be successful overall.

Covariate balance is also fairly good when comparing the unanimous-same ideology group to the unanimous-opposite ideology group. Table 4.12 shows again that there are no statistically significant differences on any variable across these two groups. However, it is worth noting that the *t*-test *p*-value for the respondents' attitudes toward affirmative action, the issue involved in the vignette, is still relatively low. Matching solves this potential imbalance and increases the *p*-values on several other covariates as well, thereby improving the overall balance between the two treatment groups.

The results are similar in the final covariate comparison of the affirmative action example. Table 4.13 shows all the covariates to be sufficiently balanced, though the KS test *p*-value for the first diffuse support factor is a little low. Regardless, matching improves the balance statistics for all but one of the matched covariates. Matching does not do as well to improve the balance of the unmatched covariates, though: only the school prayer variable shows any improvement at all, and the *p*-values for gun ownership rights opinion drop dangerously close to statistical significance. Still, the widespread and strong improvements overall outweigh this loss of balance on a single covariate.

Table 4.11. Covariate balance between unanimous-same ideology and divided-same ideology groups (*ex ante* opponents): affirmative action.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.032	.742		**-.022**	**.783**	
Affirmative action	-.047	.527	.766	**.017**	**.623**	**.930**
Diffuse support (1)	.096	.612	.418	**-.079**	**.681**	**.780**
Diffuse support (2)	.046	.309	.076	**.011**	**.610**	**.908**
News interest	.067	.304	.088	**0**	**1**	**.128**
Voter registration	-.002	.983		**0**	**1**	
Political ideology	.011	.883	.816	-.011	.764	**.822**
Party identification	-.022	.794	.714	**.011**	.757	.698
Income	.308	.694	.694	**.044**	**.947**	.298
Age	-1.285	.729	.964	-1.400	.587	.710
Sex	.050	.655		**.022**	.565	
Race	-.031	.737		**0**	**1**	
Religiosity	.016	.911	.692	**-.007**	**.954**	.370
South	-.009	.927		**0**	**1**	
Unmatched covariates						
National sales tax	-.004	.955	.590	-.089	.210	.500
Universal healthcare	-.157	.093	.086	-.161	.022	.052
Gun rights	.054	.503	.124	**.039**	.443	**.162**
School prayer	-.106	.248	.120	-.117	.105	**.374**

Post-matching improvements are in **bold**.

Table 4.12. Covariate balance between unanimous-same ideology and unanimous-opposite ideology groups (*ex ante* opponents): affirmative action.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.010	.916		**0**	**1**	
Affirmative action	-.120	.080	.210	**-.011**	**.565**	**.980**
Diffuse support (1)	.057	.752	.780	.128	.255	.606
Diffuse support (2)	.002	.973	.446	.031	.465	.304
News interest	.037	.585	.350	-.044	.273	.212
Voter registration	-.024	.772		-.067	.256	
Political ideology	-.006	.933	.544	.011	.656	.134
Party identification	.037	.649	.902	**-.033**	.488	.862
Income	-.981	.262	.232	**-.422**	**.500**	**.608**
Age	.670	.854	.744	2.044	.475	.374
Sex	.072	.517		**.022**	**.707**	
Race	-.031	.737		**.022**	.317	
Religiosity	-.035	.815	.660	-.042	.758	.402
South	.147	.146		**.111**	**.297**	
Unmatched covariates						
National sales tax	-.115	.086	.414	**-.072**	.252	.290
Universal healthcare	-.024	.794	.602	**-.011**	**.869**	.464
Gun rights	.021	.796	.334	**-.017**	.754	**.756**
School prayer	.061	.487	.446	.139	.094	.130

Post-matching improvements are in **bold**.

Table 4.13. Covariate balance between unanimous-same ideology and divided-opposite ideology groups (*ex ante* opponents): affirmative action.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.148	.159		**-.024**	**.783**	
Affirmative action	-.046	.524	.578	**.006**	**.891**	**.906**
Diffuse support (1)	.286	.141	.070	**.186**	**.251**	**.580**
Diffuse support (2)	.041	.397	.486	**-.003**	**.917**	**.520**
News interest	.016	.830	.772	-.024	.493	**.976**
Voter registration	.035	.654		**-.024**	.317	
Political ideology	-.057	.450	.562	**-.024**	**.539**	**.678**
Party identification	-.053	.529	.818	**-.004**	**.936**	**.970**
Income	-.320	.692	.796	-.429	.306	.410
Age	-4.307	.258	.202	**-1.023**	**.698**	**.678**
Sex	.062	.581		.119	.275	
Race	.079	.431		**.024**	.317	
Religiosity	-.083	.581	.610	**-.074**	.473	.596
South	.126	.216		**.071**	**.317**	
Unmatched covariates						
National sales tax	-.014	.848	.432	-.036	.621	.306
Universal healthcare	-.051	.597	.792	-.113	.124	.478
Gun rights	.032	.694	.356	.137	.061	.076
School prayer	.056	.513	.250	.089	.251	**.298**

Post-matching improvements are in **bold**.

We see a similar mix of serious and minor balance problems when examining treatment group comparisons on their reactions to the police interrogation case. Looking at the court's *ex ante* supporters, the comparison between the unanimous-same ideology and the divided-same ideology groups, depicted in Table 4.14, shows few variables that are severely imbalanced. Still, there are a few that are cause for concern. Respondents in the divided group appear to be significantly more likely to be from the South than those in the unanimous group. Additionally, according to the KS test, the unanimous group is more supportive of suspect rights (though this variable is balanced according to the *t*-test). Fortunately, matching improves the balance on both these covariates (the *p*-value for geography remains fairly low, though not below the significance level). It also improves the balance statistics for all but two of the variables listed. Thus, matching very clearly improves the overall balance of this comparison.

The comparison between the unanimous-same ideology group and the unanimous-opposite ideology group is even better balanced prior to matching. In fact, Table 4.15 shows that not a single variable statistically differs between the two treatment groups. Even so, matching improves the balance statistics for a majority of the matched covariates and for half of the unmatched variables. Matching even achieves perfect balance on two variables: voter registration and race. In other words, matching, though not entirely necessary for this comparison, further increases overall balance.

Looking at the last comparison of *ex ante* supporters on this issue – that between the unanimous-same ideology and the divided-opposite ideology groups – we again see very little cause for alarm in the pre-matched analysis. Table 4.16 shows that the lowest *p*-value prior to matching is .08, which is that for the KS test of the suspect rights attitude variable. Although this is not considered statistically significant by conventional standards, and the *t*-test *p*-value shows good balance between the means of each treatment group on this covariate, the respondents' opinions toward the issue involved in the vignette are potentially a very strong factor in determining their response, so higher balance on this measure would be preferred. Luckily, matching rectifies this minor concern and improves the balance statistics for most of the other covariates as well.

Table 4.14. Covariate balance between unanimous-same ideology and divided-same ideology groups (*ex ante* supporters): suspect rights.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.193	.120		**-.034**	.783	
Suspect rights	-.119	.206	.034	**-.043**	**.657**	**.270**
Diffuse support (1)	-.135	.557	.686	.215	.107	**.710**
Diffuse support (2)	.059	.237	.310	**.002**	**.947**	.306
News interest	-.074	.267	.248	**-.023**	**.640**	**.994**
Voter registration	.007	.941		-.034	.318	
Political ideology	.100	.245	.450	**.043**	**.477**	.178
Party identification	.063	.508	.676	**.040**	**.444**	**.984**
Income	-.256	.754	.440	**-.207**	**.719**	.108
Age	-.913	.823	.824	-1.724	.611	.092
Sex	.038	.759		**.034**	**.783**	
Race	-.187	.082		**-.034**	**.741**	
Religiosity	-.112	.444	.848	-.170	.165	.430
South	.261	.031		**.103**	**.078**	
Unmatched covariates						
National sales tax	.092	.261	.260	**.086**	**.382**	**.672**
Universal healthcare	-.104	.307	.412	**-.086**	**.318**	**.604**
Gun rights	-.007	.940	.672	-.034	.614	**.958**
School prayer	.007	.942	.666	-.043	.531	**.824**

Post-matching improvements are in **bold**.

Table 4.15. Covariate balance between unanimous-same ideology and unanimous-opposite ideology groups (*ex ante* supporters): suspect rights.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.016	.887		.029	.657	
Suspect rights	-.005	.954	.622	-.036	.494	.392
Diffuse support (1)	.051	.805	.838	-.097	.490	.812
Diffuse support (2)	.055	.301	.172	**.036**	**.472**	**.398**
News interest	0	.997	.922	.019	.733	.602
Voter registration	.008	.929		**0**	**1**	
Political ideology	.014	.860	.970	-.029	.529	.660
Party identification	-.009	.920	.772	.019	.635	**.820**
Income	-1.028	.174	.354	**.400**	**.555**	**.804**
Age	-5.222	.150	.138	**-2.571**	**.369**	**.208**
Sex	.075	.521		-.271	.317	
Race	-.073	.507		**0**	**1**	
Religiosity	-.019	.897	.916	.097	.407	.126
South	.086	.426		**.057**	**.566**	
Unmatched covariates						
National sales tax	-.035	.661	.746	.057	.420	.340
Universal healthcare	-.059	.538	.400	-.114	.109	.136
Gun rights	-.052	.553	.542	-.064	.415	**.680**
School prayer	-.032	.740	.508	-.036	.598	**.586**

Post-matching improvements are in **bold**.

Table 4.16. Covariate balance between unanimous-same ideology and divided-opposite ideology groups (*ex ante* supporters): suspect rights.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.150	.150		**-.073**	**.372**	
Suspect rights	-.041	.596	.080	**-.009**	**.819**	**.220**
Diffuse support (1)	.124	.476	.182	**-.072**	.465	.120
Diffuse support (2)	.084	.098	.178	**-.018**	**.567**	**.716**
News interest	-.061	.321	.378	**.012**	**.733**	**.774**
Voter registration	.089	.233		**-.018**	**.565**	
Political ideology	-.010	.894	.674	-.059	.147	**.714**
Party identification	-.048	.592	.830	**.029**	.501	.512
Income	.198	.783	.484	**.127**	**.805**	.074
Age	-.760	.817	.960	-1.854	.186	.254
Sex	.117	.267		**.073**	.155	
Race	-.050	.619		**0**	**1**	
Religiosity	.112	.381	.422	**.079**	.438	.130
South	.053	.580		**.036**	.481	
Unmatched covariates						
National sales tax	.053	.490	.720	.073	.227	.546
Universal healthcare	-.019	.824	.730	-.064	.196	.276
Gun rights	-.076	.333	.170	**-.032**	**.584**	**.494**
School prayer	.027	.761	.816	.041	.520	.542

Post-matching improvements are in **bold**.

The balance problems for the suspect rights vignettes tend to be pronounced among the *ex ante* opponents. Table 4.17 shows that, in the comparison between the unanimous-same ideology and divided-same ideology groups, there are no covariates that are significantly different from one another at the .05 level, but there are several with *p*-values that come awfully close: the *t*-test *p*-values for *ex ante* position strength, income, race, geography, and preference for a national sales tax, and the KS test *p*-value for attitudes toward universal healthcare are all below .1. Not surprisingly, matching rectifies the dubious balance on all these covariates (including achieving perfect balance on *ex ante* position strength and race) and on most other covariates – matched and unmatched – as well.

The comparison between the unanimous-same ideology and unanimous-opposite ideology groups tells a somewhat different story. Here, as seen in Table 4.18, most covariates are very well balanced prior to matching except for income. We can see that the difference between the two treatment groups' average income is undeniably statistically significant: the same ideology group is simply richer than the opposite ideology group is. Matching corrects for this imbalance, boosting both this variables *p*-values from 0 to .377 (*t*) and .784 (KS). Improvement goes far beyond this single variable: all but two matched and half of the unmatched covariates also have improved balance statistics after matching.

Income is again a problematic variable in the final comparison of this issue area. Though the *p*-values are not as dramatically low as they were in the previous comparison, Table 4.19 shows that the unanimous-same ideology group clearly has a higher average income than the divided-opposite ideology group does. Moreover, the two groups are imbalanced on race: the divided-opposite ideology group contains significantly more nonwhites than the unanimous-same ideology group does. Matching resolves both of these imbalances and, again, yields widespread balance improvement across the remaining covariates.

Table 4.17. Covariate balance between unanimous-same ideology and divided-same ideology groups (*ex ante* opponents): suspect rights.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.172	.087		**0**	**1**	
Suspect rights	.007	.918	.378	-.011	.839	**1**
Diffuse support (1)	-.060	.728	.494	.090	.531	.396
Diffuse support (2)	.029	.514	.436	**.022**	**.522**	**.556**
News interest	-.057	.316	.150	**-.038**	**.366**	**.450**
Voter registration	-.110	.110		**-.045**	**.155**	
Political ideology	-.004	.958	.920	.011	.824	**1**
Party identification	-.066	.369	.404	**.015**	**.775**	**.956**
Income	-1.029	.099	.106	**.318**	**.491**	**.400**
Age	2.315	.517	.302	**1.818**	.296	**.382**
Sex	.119	.237		**-.023**	**.764**	
Race	.148	.073		**0**	**1**	
Religiosity	-.020	.878	.954	-.086	.399	.526
South	.182	.060		**.023**	**.317**	
Unmatched covariates						
National sales tax	-.126	.073	.194	-.136	**.104**	**.210**
Universal healthcare	.122	.139	.052	**.028**	**.717**	**.102**
Gun rights	-.050	.491	.790	-.108	.117	.148
School prayer	.051	.531	.296	**-.040**	**.616**	**.594**

Post-matching improvements are in **bold**.

Table 4.18. Covariate balance between unanimous-same ideology and unanimous-opposite ideology groups (*ex ante* opponents): suspect rights.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.044	.650		**-.020**	**.707**	
Suspect rights	.103	.136	.308	**.015**	**.687**	**.844**
Diffuse support (1)	-.175	.345	.638	**.058**	**.746**	**.926**
Diffuse support (2)	-.001	.973	.982	.018	.467	.662
News interest	-.062	.291	.202	**-.033**	**.436**	**.842**
Voter registration	-.049	.404		**0**	**1**	
Political ideology	.006	.929	.574	.020	.684	**.952**
Party identification	-.014	.852	.880	**-.013**	.709	.478
Income	-2.484	.000	.000	**-.380**	**.377**	**.784**
Age	.389	.910	.760	-1.440	.465	.476
Sex	-.078	.428		**-.020**	**.707**	
Race	.055	.439		**.040**	.155	
Religiosity	-.155	.227	.396	**-.088**	**.262**	**.428**
South	.030	.730		.060	.256	
Unmatched covariates						
National sales tax	-.108	.117	.248	-.110	.113	**.256**
Universal healthcare	-.036	.671	.628	-.100	.129	.110
Gun rights	-.014	.825	.856	-.105	.082	.282
School prayer	.083	.260	.308	**.060**	**.438**	.258

Post-matching improvements are in **bold**.

Table 4.19. Covariate balance between unanimous-same ideology and divided-opposite ideology groups (*ex ante* opponents): suspect rights.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.011	.906		-.038	.317	
Suspect rights	-.010	.888	.952	-.028	.503	.888
Diffuse support (1)	-.025	.890	.786	.050	.668	.390
Diffuse support (2)	.063	.119	.094	**-.009**	**.797**	**.276**
News interest	-.070	.204	.058	**.044**	**.413**	**.736**
Voter registration	-.004	.936		-.038	.317	
Political ideology	.045	.478	.524	**-.014**	**.782**	**.978**
Party identification	0	.996	.980	.038	.516	.498
Income	-1.719	.010	.042	**-.283**	**.432**	**.726**
Age	.486	.874	.956	**.378**	.801	.342
Sex	-.103	.287		**-.094**	.275	
Race	.158	.042		**.019**	**.317**	
Religiosity	-.061	.613	.612	**-.047**	.585	.584
South	.090	.310		**0**	**1**	
Unmatched covariates						
National sales tax	-.011	.863	.530	-.042	.492	.262
Universal healthcare	.065	.433	.440	**-.024**	**.689**	.352
Gun rights	-.052	.443	.538	-.099	.095	.146
School prayer	.055	.459	.462	**.009**	**.886**	**.782**

Post-matching improvements are in **bold**.

Turning to the final issue area tested, employer liability, we again see the benefits of covariate matching. Table 4.20 compares the *ex ante* supporters in the unanimous-same ideology and divided-same ideology treatment groups. While most covariates are sufficiently balanced prior to matching, two key variables are not: the divided group is statistically less likely to support awarding employees damages and more likely to hold a strong opinion on the matter prior to the experiment. Matching corrects the imbalance on these two variables and even achieves perfect balance on *ex ante* position strength. It also improves statistical balance for most of the other covariates, as well, including two of the four unmatched covariates.

The pre-matched balance is not nearly as problematic for the comparison of the unanimous-same ideology group and the unanimous-opposite ideology group. Table 4.21 shows that no variable is statistically different across these groups, though a couple, ideology (p = .068) and religiosity (p = .058) come very close to the .05 threshold. Regardless, matching creates virtually universal balance improvement. Not only are three variables perfectly balanced after matching, all but one unmatched covariate have improved balance statistics. Thus, while this comparison is not in dire need of adjustment, matching indisputably improves our ability to draw inferences from this comparison.

The final comparison of *ex ante* supporters also shows little need for post-experimental adjustment. However, as seen in Table 4.22, the experimental randomization does fail to achieve balance on the income: according to the *t*-test, the divided-opposite ideology group earns more on average than the unanimous-same ideology group (however, the two are balanced according to the KS test). Matching achieves virtually perfect balance on this covariate and, again, leads to widespread balance improvement: most matched covariates and all but one unmatched covariate see some form of improvement after matching.

Table 4.20. Covariate balance between unanimous-same ideology and divided-same ideology groups (*ex ante* supporters): employer liability.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.212	.049		**0**	**1**	
Employee damages	-.153	.046	.040	**-.086**	**.204**	**.656**
Diffuse support (1)	.054	.780	.288	-.147	.305	.248
Diffuse support (2)	-.058	.208	.372	**.011**	**.804**	**.390**
News interest	.007	.918	.598	-.038	.372	.380
Voter registration	-.090	.241		**0**	**1**	
Political ideology	.027	.725	.678	.079	.268	.242
Party identification	-.046	.583	.952	**-.024**	.467	**.994**
Income	.482	.532	.744	**-.172**	**.732**	.462
Age	2.788	.458	.420	**-1.000**	**.754**	**.390**
Sex	-.094	.403		**0**	**1**	
Race	.004	.965		-.029	.657	
Religiosity	-.008	.954	.746	-.018	.883	.734
South	-.045	.678		-.057	.566	
Unmatched covariates						
National sales tax	.026	.732	.670	.064	.381	**.688**
Universal healthcare	.020	.832	.436	.064	.418	**.476**
Gun rights	-.087	.254	.632	.164	.023	.144
School prayer	.004	.963	.924	-.064	.447	.850

Post-matching improvements are in **bold**.

Table 4.21. Covariate balance between unanimous-same ideology and unanimous-opposite ideology groups (*ex ante* supporters): employer liability.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.144	.167		**.028**	**.566**	
Employee damages	-.058	.455	.458	**.007**	**.893**	**1**
Diffuse support (1)	.085	.652	.700	.086	**.659**	.432
Diffuse support (2)	.032	.487	.390	**-.008**	**.852**	**.276**
News interest	.033	.526	.390	**-.009**	**.783**	**.860**
Voter registration	-.029	.658		**0**	**1**	
Political ideology	.121	.068	.160	**.028**	**.434**	**.900**
Party identification	.039	.639	.506	**.014**	**.760**	**.942**
Income	.391	.519	.826	**-.222**	**.654**	**.960**
Age	2.015	.580	.942	**.250**	**.936**	**.996**
Sex	-.107	.338		**-.028**	**.566**	
Race	.053	.583		**0**	**1**	
Religiosity	.274	.058	.292	**.028**	**.819**	**.934**
South	-.138	.179		**0**	**1**	
Unmatched covariates						
National sales tax	.109	.122	.164	**.097**	**.155**	.134
Universal healthcare	-.067	.470	.726	**.028**	**.664**	.682
Gun rights	-.036	.622	.338	-.056	.434	.250
School prayer	.091	.242	.454	**-.021**	**.752**	**.972**

Post-matching improvements are in **bold**.

Table 4.22. Covariate balance between unanimous-same ideology and divided-opposite ideology groups (*ex ante* supporters): employer liability.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.172	.103		**.083**	**.256**	
Employee damages	-.079	.311	.292	**.007**	**.889**	**.942**
Diffuse support (1)	-.033	.872	.802	-.242	.161	.634
Diffuse support (2)	-.062	.163	.140	**-.016**	**.528**	**.828**
News interest	.005	.934	.692	.009	.810	.570
Voter registration	-.002	.978		**0**	**1**	
Political ideology	-.103	.156	.288	**-.069**	**.210**	**.668**
Party identification	-.053	.533	.484	-.088	.203	.412
Income	1.613	.045	.142	**0**	**1**	**.966**
Age	.542	.870	.964	-1.028	.643	.622
Sex	-.051	.647		**.028**	.317	
Race	.053	.583		.111	.204	
Religiosity	-.037	.804	.532	**.031**	.693	**.838**
South	-.110	.290		**0**	**1**	
Unmatched covariates						
National sales tax	-.009	.905	.770	-.035	.637	.504
Universal healthcare	-.018	.844	.848	-.049	.536	**.868**
Gun rights	-.113	.131	.240	**-.111**	**.186**	**.324**
School prayer	-.110	.209	.228	**-.090**	**.273**	**.560**

Post-matching improvements are in **bold**.

Looking at the *ex ante* opponents, we can see several problems in the covariate balance. For instance, in the comparison between the unanimous-same ideology and divided-same ideology groups, shown in Figure 4.23, the respondents are not balanced on their opinion of gun ownership rights: the unanimous group is less favorable toward such a policy than the divided group is. In addition, the *t*-test *p*-values for *ex ante* position strength and the second factor of diffuse support are both very close to statistical significance. These imbalances are resolved after matching, even in spite of the fact that one of these variables – gun rights – is an unmatched covariate. Furthermore, improves balance on nearly variable listed.

In the comparison between the unanimous-same ideology and unanimous-opposite ideology groups, there are even more balance concerns prior to matching. Table 4.24 shows that the two groups clearly differ from one another on race: the opposite ideology group contains significantly more whites than the same ideology group. Additionally, according to the *t*-test, the two groups are imbalanced on both factors of diffuse support for the Supreme Court, with the same ideology group more supportive of the institution. Matching solves these imbalances and improves for most covariates, but it does introduce a few new balance problems. For instance, in spite of the fact that the mean difference between the two groups' sex is slightly improved, the weights produced by GenMatch cause a decrease in the *p*-value, making the difference statistically significant. In addition, the post-matching difference in national sales tax opinion is also statistically significant according to the *t*-test, though it remains balance when measured by the KS test. Likewise, income and religiosity fail the KS test after matching, though they remain sufficiently balanced on the *t*-test. Thus, matching does not produce a perfect solution to the differences between these two treatment groups. However, due to the theoretical importance of diffuse support and the number of variables that are in fact improved after matching, the trade-off seems worthwhile on the whole.

Finally, the comparison of *ex ante* opponents, between the unanimous-same ideology and divided-opposite ideology groups, reveals fewer balance problems. While most covariates are well balanced in this comparison, the divided-opposite ideology group is significantly more supportive of gun rights as seen in Table 4.25. Furthermore, the *t*-test *p*-value for diffuse support for the Supreme Court (first factor) is uncomfortably close to statistical significance. These balance concerns are resolved after matching and, once again, improvement is seen across a vast majority of the other variables as well.

Table 4.23. Covariate balance between unanimous-same ideology and divided-same ideology groups (*ex ante* opponents): employer liability.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.208	.062		**.029**	**.657**	
Employee damages	-.028	.742	.326	.037	.458	**.926**
Diffuse support (1)	-.205	.295	.546	**-.164**	.282	.372
Diffuse support (2)	-.080	.067	.132	**.012**	**.582**	**.410**
News interest	-.014	.848	.536	**.010**	.765	**.998**
Voter registration	-.049	.557		**-.029**	.317	
Political ideology	.083	.272	.550	**.022**	**.317**	**.558**
Party identification	.039	.656	.746	**-.025**	.598	**.892**
Income	-.397	.573	.446	**-.235**	.463	**.550**
Age	5.340	.168	.170	**.471**	**.854**	**.758**
Sex	-.022	.845		.029	.765	
Race	-.148	.155		**.029**	**.741**	
Religiosity	.056	.664	.230	**-.028**	**.785**	.184
South	.093	.402		.118	.285	
Unmatched covariates						
National sales tax	.011	.880	.898	.088	.289	.890
Universal healthcare	-.020	.825	.904	**.015**	.779	.544
Gun rights	.182	.013	.036	**.051**	**.371**	**.060**
School prayer	.101	.229	.396	**.051**	**.444**	.114

Post-matching improvements are in **bold**.

Table 4.24. Covariate balance between unanimous-same ideology and unanimous-opposite ideology groups (*ex ante* opponents): employer liability.

	Before matching			After matching		
	Mean diff.	*t*-test *p*-value	KS test *p*-value	Mean diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	-.115	.303		**-.111**	.099	
Employee damages	.032	.709	.794	**-.028**	.634	**.816**
Diffuse support (1)	-.376	.047	.074	**0**	**.999**	**.278**
Diffuse support (2)	-.137	.012	.154	**-.034**	**.376**	.150
News interest	.041	.573	.488	-.074	.168	.348
Voter registration	-.150	.101		**-.056**	**.155**	
Political ideology	.068	.379	.250	**.049**	**.496**	**.616**
Party identification	.088	.264	.322	-.093	.159	.070
Income	-1.303	.126	.276	**-1.083**	.077	.018
Age	-.327	.930	.488	-1.889	.422	.402
Sex	.179	.109		**.167**	.030	
Race	-.355	.000		**0**	**1**	
Religiosity	-.104	.446	.154	-.158	.339	.018
South	-.069	.493		**.028**	**.783**	
Unmatched covariates						
National sales tax	.024	.761	.840	.181	.034	.206
Universal healthcare	-.108	.221	.472	**-.056**	**.420**	.362
Gun rights	.101	.205	.172	**.014**	**.824**	.100
School prayer	.035	.701	.784	.063	.554	.362

Post-matching improvements are in **bold**.

Table 4.25. Covariate balance between unanimous-same ideology and divided-opposite ideology groups (*ex ante* opponents): employer liability.

	Before matching			After matching		
	diff.	*t*-test *p*-value	KS test *p*-value	diff.	*t*-test *p*-value	KS test *p*-value
Matched covariates						
Strong *ex ante* position	.007	.952		-.051	.565	
Employee damages	-.088	.305	.472	**-.051**	.180	.360
Diffuse support (1)	-.307	.078	.222	**.004**	**.970**	**.684**
Diffuse support (2)	-.053	.317	.186	**-.038**	**.397**	.182
News interest	-.031	.624	.460	**-.026**	.366	**.622**
Voter registration	-.001	.994		-.051	.317	
Political ideology	.111	.202	.320	**.001**	.481	**.902**
Party identification	.018	.816	.932	**.017**	.588	.840
Income	-1.448	.062	.242	**.205**	**.667**	.178
Age	1.314	.751	.444	-1.667	.493	.300
Sex	.160	.143		**.077**	**.468**	
Race	-.127	.213		**-.026**	**.317**	
Religiosity	-.075	.592	.202	-.159	.174	.128
South	.142	.184		**.051**	**.595**	
Unmatched covariates						
National sales tax	.028	.718	.624	-.051	.548	.570
Universal healthcare	.014	.873	.530	.051	.276	.188
Gun rights	.152	.048	.044	**.103**	**.092**	**.122**
School prayer	.057	.513	.670	**-.006**	**.934**	1

Post-matching improvements are in **bold**.

Post-Matching Results

Having optimized the balance between treatment groups, we can now better assess the effect that ideology and unity may – or may not – have on agreement and acceptance of court decisions. Tables 4.26 and 4.27 confirm the results found in Tables 4.2 and 4.3: the experimental treatments have no measurable effect on either measure of the dependent variable. As such, matching has increased our confidence in the null result produced by the raw experimental data. Thus, it appears that neither unity nor division have any bearing on an individual's agreement with or acceptance of the appeals court's decision on racial preferences regardless of that individuals prior position on the issue.

Table 4.26. Post-matching estimates for treatment group effects on agreement with court decision: affirmative action.

	Ex ante supporters		*Ex ante* opponents	
Treatment	Estimate	Abadie-Imbens Standard Error	Estimate	Abadie-Imbens Standard Error
Div. - Same	.053	.107	.044	.087
Unan. - Opp.	-.111	.088	0	.086
Div. - Opp.	.020	.071	.024	.090

Table 4.27. Post-matching estimates for treatment group effects on acceptance of court decision: affirmative action.

	Ex ante supporters		*Ex ante* opponents	
Treatment	Estimate	Abadie-Imbens Standard Error	Estimate	Abadie-Imbens Standard Error
Div. - Same	.105	.093	-.022	.120
Unan. - Opp.	0	.060	.067	.138
Div. - Opp.	-.041	.065	-.143	.109

Table 4.28 also shows no effect for any of the treatment groups on agreement with the court's decision on police interrogations. These results differ from the unmatched results in Table 4.4 in that, among *ex ante* supporters, the effect of the divided-same ideology group no longer statistically differs from the unanimous-same ideology group. Since this effect was puzzling given the theory, the fact that it was due to an imbalance in the distribution of the

covariates across treatment groups is reassuring. However, as a result, I find no evidence that ideology or unity has any effect on the degree to which one agrees with the court in this case.

Table 4.28. Post-matching estimates for treatment group effects on agreement with court decision: suspect rights.

Treatment	Ex ante supporters		Ex ante opponents	
	Estimate	Abadie-Imbens Standard Error	Estimate	Abadie-Imbens Standard Error
Div. - Same	-.172	.112	.045	.119
Unan. - Opp.	-.086	.095	.120	.112
Div. - Opp.	-.018	.087	-.113	.112

Table 4.29 presents a more complicated story. Like with the agreement rates in this case, Table 4.5 also revealed an unexpected an unexplained difference in the acceptance rates between the divided-same ideology group and its unanimous counterpart. While matching allowed us to chalk up this bizarre result for the agreement measure to a statistical imbalance, Table 4.29 shows that the effect remains after matching. However, given that there is no readily available explanation as to why there would be a difference between these two treatment groups and none of the others, it would be unwise to rule out the possibility of a false positive. After all, given the large volume of statistical comparisons involved in this dissertation, it would be strange not to come across at least one. Of course, further study would be needed to confirm this.

Table 4.29. Post-matching estimates for treatment group effects on acceptance of court decision: suspect rights.

Treatment	Ex ante supporters		Ex ante opponents	
	Estimate	Abadie-Imbens Standard Error	Estimate	Abadie-Imbens Standard Error
Div. - Same	-.207*	.099	.159	.125
Unan. - Opp.	-.086	.060	.160	.132
Div. - Opp.	-.091	.067	-.189	.135

* $p < .05$

Turning to the low salience issue of employer liability, the results are much the same as that of the affirmative action. Tables 4.30 and 4.31 are consistent with the findings of Tables 4.6 and 4.7: there is no significant difference in either the acceptance or agreement rates across treatment groups. Thus, the pre-matched experimental results were not sufficiently swayed by any covariate imbalances. Again, this indicates that neither those predisposed to agree with the court's ruling nor those who would be likely to disagree are persuaded by the reputational ideology of the court or the level of division with which it makes its decision.

Table 4.30. Post-matching estimates for treatment group effects on agreement with court decision: employer liability.

Treatment	*Ex ante* supporters		*Ex ante* opponents	
	Estimate	Abadie-Imbens Standard Error	Estimate	Abadie-Imbens Standard Error
Div. - Same	.086	.130	-.059	.092
Unan. - Opp.	.056	.107	0	.137
Div. - Opp.	.111	.101	0	.104

Table 4.31. Post-matching estimates for treatment group effects on acceptance of court decision: employer liability.

Treatment	*Ex ante* supporters		*Ex ante* opponents	
	Estimate	Abadie-Imbens Standard Error	Estimate	Abadie-Imbens Standard Error
Div. - Same	.057	.126	.088	.136
Unan. - Opp.	.056	.096	.083	.167
Div. - Opp.	.083	.097	.077	.126

Summary

The results of this chapter show virtually no evidence that court ideology interacted with unity has any effect on the individual's opinion of a ruling. Given the results of Chapter 3, it is not surprising that no effect was found for the highly salient case, since no effect was found there either, but the lack of any effect on the respondents' attitude toward the decisions of medium and low salience are inconsistent with the previous chapter's findings. Why should unity matter in these cases in Chapter 3 but unity along with ideology not matter in them in this chapter? The most likely reason for this is the change in venue. It is well established that the

United States Supreme Court has the potential to impact public opinion, and Chapter 3 demonstrates a specific way in which it does this. No such phenomenon has been demonstrated for any lower court. In other words, this chapter gives credence to the notion that the public opinion effects of judicial unity are unique to the nation's highest court.

We may speculate about the reasons for this. As noted earlier, the Supreme Court may simply have a deeper reserve of legitimacy than do other courts and is therefore more easily able to persuade the public – regardless of its level of unity – than any other legal institution can. Furthermore, the highly visible position of the Supreme Court may contribute to its unique ability to affect the public with its level of division. Because the public at large may, at least to some degree, be more familiar with the justices who serve on the Supreme Court than they are with the judges on any given appeals court, the division on the Supreme Court may be more meaningful: the public, and perhaps also the media, could be more likely to care about a conflict between a set of justices whose names they have heard or read in the news a few times than they are about a seemingly random set of nameless judges.

Whatever the reason for the Supreme Court division's uniqueness in its ability to shift public opinion, it would be interesting to investigate if an individual's attitudes are at all affected by her perception of the Court's overall ideology. While such an experiment was easy to assign in this chapter, since federal appeals courts are plentiful and diverse in their ideological reputations, the visibility of the Supreme Court makes it impossible to assign it an ideological reputation in an experimental setting. As such, future researchers would be wise to observe the public's opinion of the Supreme Court as liberal, moderate, or conservative when studying their reactions to its decisions.

It would also be enlightening to examine the root of the discrepancy between the high and lower courts' ability to influence popular attitudes. One possibility is that, because the Supreme Court is the nation's court of last resort, there is a higher expectation that it will rule "correctly", and thus its decisions carry a greater air of legitimacy than lower court rulings do. Work that examines this hypothesis and others that could explain the source of the Supreme Court's apparently unique legitimation effect would certainly contribute to our understanding of the conditions necessary for measurable public response to judicial behavior.

Conclusion

Recently, in response to the Supreme Court majority's decision in *Brown v. Plata* (2011), which ruled that the Eighth Amendment requires the state of California to reduce its bloated prison population by 37,000 inmates, Justice Antonin Scalia read an impassioned dissent from the bench. This rare act of reading a minority opinion aloud often captures the attention of journalists and is perhaps a way in which dissenting justices hope to communicate their position to the public (Johnson, Black, and Ringsmuth 2009). But did it matter? Is there any sort of relationship between the degree of consensus on a court and public perceptions of their decisions? This dissertation shows that:

1. Dissent and conflict on the United States Supreme Court generates press coverage. Regardless of other factors, such as prior coverage, issue area, and other characteristics associated with case visibility, the degree of division on the Court is independently correlated with higher levels of newspaper coverage.
2. Narrower Court majorities attract more critical coverage. That is, if you were to take two court decisions comparable in ideological salience, prior coverage, legal authority, etc., the one that attracted a smaller number of judges to its majority decision is more likely to receive editorial criticism.
3. Majority size is also a factor in determining citizen reaction to Supreme Court decisions. However, the presence and direction of this effect is conditional on the ideological salience of the issue involved. Division in rulings on issues of high ideological salience, like gay rights, makes no difference in public response – attitudes toward these decisions are governed primarily be people's prior opinion on the issue. Meanwhile, any degree of division in a ruling on an issue of medium ideological salience, such as employee privacy, may boost acceptance among those who may substantively disagree with the outcome. Contrarily, large majorities (regardless of unanimity) tend to boost the acceptance of decisions on issues of low ideological salience, like contract dispute resolution, among those who still would have preferred a different outcome in the case.
4. There appears to be no evidence that the majority-size effect on public opinion can be extended from Supreme Court decisions to those of lower appellate courts.

Given these findings, we might wonder whether Chief Justice Earl Warren was right to struggle so hard to achieve unanimity in *Brown v. Board of Education.* As noted in Chapter 1, one reason that Warren pursued a unanimous opinion was fear that a dissenting opinion would fuel backlash among those who would oppose the decision, chiefly Southern white segregationists. How does this concern hold up against the evidence? Although Chapter 2's results suggest that the average case can expect a lesser degree of media visibility if it is decided unanimously than if the vote were divided, *Brown*, of course, was not an average case: the regression analyses show that, in addition to majority size, a number of other characteristics are reliable predictors of visibility, many of them present in the *Brown* decision.

Thus, it was obvious that this ruling could not fly below the radar. By that same token, large majorities on average tend to decrease the likelihood of negative editorial coverage of a decision, but many other characteristics present in *Brown* – such as pre-decision coverage, self-assignment of the majority opinion, and relevance to the issue of civil rights – increase that likelihood. As such, the justices ought not to have expected a free pass from newspaper opinion writers. Thus, from the point of view of the news media, there is very little difference that we should expect between the actual reaction to *Brown* and the counterfactual situation where the Court is divided in its rejection of public school segregation.

Furthermore, while there may some legitimate reasons to ensure unanimity on a case as groundbreaking as this, persuading the public to accept the decision does not appear to be one of them. The evidence gives us little reason to expect that the public would have reacted differently to a divided ruling in *Brown*. Chapter 3 demonstrates that on issues of high ideological salience, as segregation certainly was in the 1950s, large majorities, whether unanimous or not, are an ineffective means of public persuasion. The real driver of individuals' inclinations to agree with or accept the Court's ruling is their policy preferences prior to the ruling. In other words, I find no evidence to suggest that white Southerners, who were *ex ante* opponents of the ruling in *Brown*, would have reacted to the Court any more harshly if it were a five-to-four ruling.

This of course begs the question, when, if ever, would unanimity or dissent actually matter? The answer, according to the evidence presented in this dissertation, is only under a narrow set of circumstances. An average case of medium ideological salience is not likely to be very visible in the news media, but the more divided that decision is, the better its chances of gaining attention. Yet, this division also makes the ruling more likely to receive public criticism. However, this division, which made the decision more likely to get noticed by the media, will help the Court win the acceptance of the ruling's opponents in the court of public opinion, in which case we can estimate that only about half the people reading about the case held very strong opinions on the issue involved from the start.

On the other hand, if the decision concerns an issue of low salience, the effect of division is slightly different. This case is even less likely receive notice from the media, but, again, increased divisiveness on the Court helps (while simultaneously increasing the likelihood of public criticism). However, the high levels dissent necessary to increase the chances of coverage are not found to have any effect on the level of support for the decision from the public. Indeed, large majorities will make the public more supportive of these rulings, but these large majorities also make the decision less likely to be visible in the first place. Thus, for Supreme Court cases dealing with issues of very little ideological interest, the results of this dissertation present a sort of catch-22: large majorities in these cases are persuasive to the public, but also make the case invisible to the public.

Thus, realistically, we only expect division to matter for those Supreme Court cases that are of medium ideological salience, that is, those cases that lie somewhere in between guaranteed visibility and obscurity. Moreover, for these cases, from the perspective of public

opinion and awareness, division seems to be a good thing: it promotes the visibility of these decisions and encourages criticism from the media while still satisfying the procedural justice demands of those who are likely to oppose the final outcome. In other words, a frequently divided court – one that is not prone to seeking consensus at every turn – may not be such a bad thing after all.

Of course, these conclusions speak only to the limited scope of this dissertation: the effects of unanimity on mass politics. It is absolutely possible that the consequences of judicial division are much different in other aspects of the political world. Internally, chronic divisiveness may brew bad professional relationships among the justices that could alter their future behavior. Externally, political elites may receive and understand unanimity and division differently than the public at large does, and opponents of the Court's decisions may use dissent as a model by which to shape their policy agendas. Perhaps dissent is an element in the decision behind legal mobilization movements, which have been demonstrated to have profound legal and political consequences (Epp 1998). Furthermore, judicial dissent may have long-term political consequences that are unlike the short-term consequences observed here: perhaps a small number of particularly profound dissenting opinions, such as Harlan's in *Plessy*, may "lie about like a loaded weapon" (to quote Jackson's dissent in *Korematsu*), waiting for a generation or two before sparking a popular political movement.

All these potential alternative consequences of dissent are, however, mere speculation. Future empirical research would be wise to evaluate the role of unanimity and dissent in these aspects of the larger political arena. However, given these findings here regarding the public visibility and persuasiveness of judicial unanimity and dissent, harsh criticism of the Court's divisive nature and calls for consensus seeking judges to be nominated to the high court should be met with some degree of skepticism.

References

Abington Township School District v. Schempp. 1963. 374 U.S. 203.

Adamany, David. 1973. "Legitimacy, Realigning Elections, and the Supreme Court." *Wisconsin Law Review* 1973(3): 790–846.

Atkins, Burton M., and Justin J. Green. 1976. "Consensus on the United States Courts of Appeals: Illusion or Reality?" *American Journal of Political Science* 20(4): 735–748.

Baas, Larry R., and Dan Thomas. 1984. "The Supreme Court and Policy Legitimation: Experimental Tests." *American Politics Research* 12(3): 335–360.

Barnum, David G. 1985. "The Supreme Court and Public Opinion: Judicial Decision Making in the Post- New Deal Period." *The Journal of Politics* 47(2): 652–666.

Baum, Lawrence. 1997. *The Puzzle of Judicial Behavior*. Ann Arbor, MI: University of Michigan Press.

Bennett, W. Lance. 1990. "Toward a Theory of Press-State Relations in the United States." *Journal of Communication* 40(2): 103-127.

Blake, Judith. 1977. "The Supreme Court's Abortion Decisions and Public Opinion in the United States." *Population and Development Review* 3(1/2): 45–62.

Boyd, Christina L, Lee Epstein, and Andrew D Martin. 2010. "Untangling the Causal Effects of Sex on Judging." *American Journal of Political Science* 54(2): 389-411.

Brace, Paul, and Melinda Gann Hall. 1993. "Integrated Models of Judicial Dissent." *The Journal of Politics* 55(4): 914–935.

Branch v. Smith. 2003. 538 U.S. 254.

Brenner, Saul, and Harold J. Spaeth. 1988. "Ideological Position as a Variable in the Authoring of Dissenting Opinions on the Warren and Burger Courts." *American Politics Research* 16(3): 317–328.

Brickman, Danette, and David Peterson. 2006. "Public Opinion Reaction to Repeated Events: Citizen Response to Multiple Supreme Court Abortion Decisions." *Political Behavior* 28(1): 87–112.

Brown v. Board of Education. 1954. 347 U.S. 483.

Brown v. Plata. 2011. 563 U.S. ___.

Bush v. Gore. 2000. 531 U.S. 98.

Caldeira, Gregory A., and James L. Gibson. 1992. "The Etiology of Public Support for the
       Supreme Court." *American Journal of Political Science* 36(3): 635–664.

Caldeira, Gregory A., and Christopher J. W. Zorn. 1998. "Of Time and Consensual Norms in the
       Supreme Court." *American Journal of Political Science* 42(3): 874–902.

Casper, Jonathan D. 1976. "The Supreme Court and National Policy Making." *The American
       Political Science Review* 70(1): 50–63.

Chaiken, Shelly. 1980. "Heuristic versus systematic information processing and the use of
       source versus message cues in persuasion." *Journal of Personality and Social Psychology*
       39(5): 752-766.

Clawson, Rosalee A., Elizabeth R. Kegler, and Eric N. Waltenburg. 2001. "The Legitimacy-
       conferring Authority of the U.S. Supreme Court: An Experimental Design." *American
       Politics Research* 29(6): 566–591.

Cross, Frank B., and Emerson H. Tiller. 1998. "Judicial Partisanship and Obedience to Legal
       Doctrine: Whistleblowing on the Federal Courts of Appeals." *The Yale Law Journal*
       107(7): 2155–2176.

Dahl, Robert A. 1957. "Decision-Making in a Democracy: The Supreme Court as a National
       Policy-Maker." *Journal of Public Law* 6(Fall): 279–295.

Danelski, David J. 1989. "The Influence of the Chief Justice in the Decisional Process of the
       Supreme Court." In *American Court Systems*, eds. Sheldon Goldman and Austin Sarat.
       New York: Longman.

Douthat, Ross. 2009. "Justices Gone Wild." *New York Times*: June 2.

Easton, David. 1975. "A Re-Assessment of the Concept of Political Support." *British Journal of
       Political Science* 5(4): 435–457.

Epp, Charles R. 1998. *The Rights Revolution: Lawyers, Activists, and Supreme Courts in
       Comparative Perspective*. University Of Chicago Press.

Epstein, Lee, and Jack Knight. 1998. *The Choices Justices Make*. Washington, DC: CQ Press.

Epstein, Lee, and Jeffrey A. Segal. 2000. "Measuring Issue Salience." *American Journal of Political Science* 44(1): 66–83.

Epstein, Lee, Daniel E. Ho, Gary King, and Jeffrey A. Segal. 2005. "The Supreme Court During Crisis: How War Affects Only Non-War Cases." *New York University Law Review* 80(1): 1-116.

Epstein, Lee, Jeffrey A. Segal, and Harold J. Spaeth. 2001. "The Norm of Consensus on the U.S. Supreme Court." *American Journal of Political Science* 45(2): 362–377.

Franklin, Charles H., and Liane C. Kosaki. 1995. "Media, Knowledge, and Public Evaluations of the Supreme Court." In *Contemplating Courts*, Washington, DC: CQ Press, p. 352–375.

———. 1989. "Republican Schoolmaster: The U.S. Supreme Court, Public Opinion, and Abortion." *The American Political Science Review* 83(3): 751–771.

Funston, Richard. 1975. "The Supreme Court and Critical Elections." *The American Political Science Review* 69(3): 795–811.

Gibson, James L. 1989. "Understandings of Justice: Institutional Legitimacy, Procedural Justice, and Political Tolerance." *Law & Society Review* 23: 469.

Gibson, James L. 1991. "Institutional Legitimacy, Procedural Justice, and Compliance with Supreme Court Decisions: A Question of Causality." *Law & Society Review* 25(3): 631–635.

Gibson, James L., Gregory A. Caldeira, and Vanessa A. Baird. 1998. "On the Legitimacy of National High Courts." *The American Political Science Review* 92(2): 343–358.

Gibson, James L., Gregory A. Caldeira, and Lester Kenyatta Spence. 2003a. "Measuring Attitudes toward the United States Supreme Court." *American Journal of Political Science* 47(2): 354–367.

———. 2003b. "The Supreme Court and the US Presidential Election of 2000: Wounds, Self-Inflicted or Otherwise?" *British Journal of Political Science* 33(04): 535–556.

———. 2005. "Why Do People Accept Public Policies They Oppose? Testing Legitimacy Theory with a Survey-Based Experiment." *Political Research Quarterly* 58(2): 187–201.

Gordon, Sanford C., and Gregory A. Huber. 2007. "The Effect of Electoral Competitiveness on Incumbent Behavior." *Quarterly Journal of Political Science* 2(2): 107-138.

Greenhouse, Linda. 2003. "Justices Hear Debate on Extending a Statute of Limitations." *New York Times,* April 1.

Grosskopf, Anke, and Jeffery J. Mondak. 1998. "Do Attitudes toward Specific Supreme Court Decisions Matter? The Impact of Webster and Texas v. Johnson on Public Confidence in the Supreme Court." *Political Research Quarterly* 51(3): 633–654.

Halpern, Stephen C., and Kenneth N. Vines. 1977. "Institutional Disunity, the Judges' Bill and the Role of the U.S. Supreme Court." *Political Research Quarterly* 30(4): 471–483.

Hand, Learned. 1958. *The Bill of Rights*. New York: Atheneum.

Hanley, John, Michael Salamone, and Matthew Wright. 2011. "Reviving the Schoolmaster: Reevaluating Public Opinion in the Wake of Roe v. Wade." *Political Research Quarterly*. http://prq.sagepub.com/content/early/2011/04/22/1065912911404564.abstract (Accessed April 29, 2011).

Haynie, Stacia L. 1992. "Leadership and Consensus on the U.S. Supreme Court." *The Journal of Politics* 54(4): 1158–1169.

Hettinger, Virginia A., Stefanie A. Lindquist, and Wendy L. Martinek. 2004. "Comparing Attitudinal and Strategic Accounts of Dissenting Behavior on the U.S. Courts of Appeals." *American Journal of Political Science* 48(1): 123–137.

———. 2006. *Judging on a Collegial Court: Influences on Federal Appellate Decision Making*. Charlottesville, VA: University of Virginia Press.

Ho, Daniel E., and Kevin M. Quinn. 2008. "Measuring Explicit Political Positions of Media." *Quarterly Journal of Political Science* 3(4): 353–377.

Hoekstra, Valerie J. 2003. *Public Reaction to Supreme Court Decisions*. New York: Cambridge University Press.

———. 2000. "The Supreme Court and Local Public Opinion." *The American Political Science Review* 94(1): 89–100.

———. 1995. "The Supreme Court and Opinion Change: An Experimental Study of the Court's Ability to Change Opinion." *American Politics Research* 23(1): 109–129.

Hoekstra, Valerie J., and Jeffrey A. Segal. 1996. "The Shepherding of Local Public Opinion: The Supreme Court and Lamb's Chapel." *The Journal of Politics* 58(4): 1079–1102.

Howard, J. Woodford. 1981. *Courts of Appeals in the Federal Judicial System*. Princeton, NJ: Princeton University Press.

Hughes, Charles Evans. 1928. *The Supreme Court of the United States: Its Foundation, Methods and Achievements*. New York: Columbia University Press.

Hutchinson, Dennis J. 1979. "Unanimity and Desegregation: Deicisionmaking in the Supreme Court, 1948-1958." *Georgetown Law Journal* 68: 1–96.

Iyengar, Shanto, and Donald R. Kinder. 1987. *News that Matters: Television and American Opinion*. University of Chicago Press.

Jamieson, Kathleen Hall, and Michael Hennessy. 2007. "Public Understanding of and Support for the Courts: Survey Results." *Georgetown Law Journal* 95: 899.

Jaros, Dean, and Robert Roper. 1980. "The U.S. Supreme Court: Myth, Diffuse Support, Specific Support, and Legitimacy." *American Politics Research* 8(1): 85–105.

Johnson, Timothy R., Ryan C. Black, and Eve M. Ringsmuth. 2009. "Hear Me Roar: What Provokes Supreme Court Justices to Dissent from the Bench?" *Minnesota Law Review* 93(5): 1560–1581.

Johnson, Timothy R., and Andrew D. Martin. 1998. "The Public's Conditional Response to Supreme Court Decisions." *The American Political Science Review* 92(2): 299–309.

Kelo v. City of New London. 2005. 545 U.S. 469.

Klarman, Michael J. 1994. "Brown, Racial Change, and the Civil Rights Movement." *Virginia Law Review* 80(1): 7–150.

———. 2004. *From Jim Crow to Civil Rights: The Supreme Court and the Struggle for Racial Equality*. New York: Oxford University Press.

Korematsu v. United States. 1944. 323 U.S. 214.

Lawrence v. Texas. 2003. 539 U.S. 558.

Lehne, Richard, and John Reynolds. 1978. "The Impact of Judicial Activism on Public Opinion." *American Journal of Political Science* 22(4): 896–904.

Lerner, Ralph. 1967. "The Supreme Court as a Republican Schoolmaster." *Supreme Court Review* 1967: 127–180.

Lind, Edgar Allan, and Tom R. Tyler. 1988. *The social psychology of procedural justice*. Springer.

Lochner v. New York. 1905. 198 U.S. 45.

Marshall, Thomas R. 1989. *Public Opinion and the Supreme Court*. Winchester, MA: Unwin Hyman.

———. 1987. "The Supreme Court as an Opinion Leader: Court Decisions and the Mass Public." *American Politics Research* 15(1): 147–168.

Martin, Andrew D., and Kevin M. Quinn. 2002. "Dynamic Ideal Point Estimation via Markov Chain Monte Carlo for the U.S. Supreme Court, 1953-1999." *Political Analysis* 10: 134–153.

Martin, Andrew D., Kevin M. Quinn, and Lee Epstein. 2005. "The Median Justice on the U.S. Supreme Court." *North Carolina Law Review* 83: 1275–1321.

Mate, Manoj, and Matthew Wright. 2008. "The 2000 Presidential Election Controversy." In *Public Opinion and Constitutional Controversy*, eds. Nathaniel Persily, Jack Citrin, and Patrick J. Egan. New York: Oxford University Press, p. 333–352.

McCann, Michael W. 1992. "Review: Reform Litigation on Trial." *Law & Social Inquiry* 17(4): 715–743.

McGuire, Kevin T., and James A. Stimson. 2004. "The Least Dangerous Branch Revisited: New Evidence on Supreme Court Responsiveness to Public Preferences." *The Journal of Politics* 66(04): 1018–1035.

Mishler, William, and Reginald S. Sheehan. 1996. "Public Opinion, the Attitudinal Model, and Supreme Court Decision Making: A Micro-Analytic Perspective." *The Journal of Politics* 58(1): 169–200.

———. 1993. "The Supreme Court as a Countermajoritarian Institution? The Impact of Public Opinion on Supreme Court Decisions." *The American Political Science Review* 87(1): 87–101.

Mondak, Jeffery J. 1992. "Institutional Legitimacy, Policy Legitimacy, and the Supreme Court."
  American Politics Research 20(4): 457–477.

———. 1990. "Perceived Legitimacy of Supreme Court Decisions: Three Functions of Source
  Credibility." *Political Behavior* 12(4): 363–384.

———. 1994. "Policy Legitimacy and the Supreme Court: The Sources and Contexts of
  Legitimation." *Political Research Quarterly* 47(3): 675–692.

———. 1991. "Substantive and Procedural Aspects of Supreme Court Decisions as
  Determinants of Institutional Approval." *American Politics Research* 19(2): 174–188.

Moorhead, R. Dean. 1952. "Concurring and Dissenting Opinions." *American Bar Association
  Journal* 38: 821–824, 884.

Murakami, Michael. 2008. "Desegregation." In *Public Opinion and Constitutional Controversy*,
  eds. Nathaniel Persily, Jack Citrin, and Patrick J. Egan. New York: Oxford University Press,
  p. 18–40.

Murphy, Walter F., and Joseph Tanenhaus. 1968a. "Public Opinion and Supreme Court: The
  Goldwater Campaign." *The Public Opinion Quarterly* 32(1): 31–50.

———. 1968b. "Public Opinion and the United States Supreme Court: Mapping of Some
  Prerequisites for Court Legitimation of Regime Changes." *Law & Society Review* 2(3):
  357–384.

Murphy, Walter F., Joseph Tanenhaus, and Daniel L. Kastner. 1973. *Public evaluations of
  constitutional courts, alternative explanations*. Thousand Oaks, CA: Sage Publications.

Nadler, Janice, Shari Seidman Diamond, and Matthew M. Patton. 2008. "Government Takings of
  Private Property." In *Public Opinion and Constitutional Controversy*, eds. Nathaniel
  Persily, Jack Citrin, and Patrick J. Egan. New York: Oxford University Press, p. 286-309.

Norpoth, Helmut, and Jeffrey A. Segal. 1994. "Popular Influence on Supreme Court Decisions:
  Comment." *The American Political Science Review* 88(3): 711–716.

Obbie, Mark. 2007. "Winners and Losers." In *Bench Press: The Collision of Courts, Politics, and
  the Media*, ed. Keith J. Bybee. Stanford, CA: Stanford University Press, p. 153–176.

Pannick, David. 2008. "McCain would set back constitutional law for a generation." *The Times*:
  October 30.

Patterson, John W., and Gregory J. Rathjen. 1976. "Background Diversity & State Supreme Court Dissent Behavior." *Polity* 8(4): 610–622.

Persily, Nathaniel. 2008. "Introduction." In *Public Opinion and Constitutional Controversy*, eds. Nathaniel Persily, Jack Citrin, and Patrick J. Egan. New York: Oxford University Press, p. 3–17.

Persily, Nathaniel, Jack Citrin, and Patrick J. Egan, eds. 2008. *Public Opinion and Constitutional Controversy*. New York: Oxford University Press.

Peterson, Iver. 2005. "As Land Goes to Revitalization, There Go the Old Neighbors." *New York Times*: January 30.

Peterson, Steven A. 1981. "Dissent in American Courts." *The Journal of Politics* 43(2): 412–434.

Petty, Richard E., John T. Cacioppo, and Rachel Goldman. 1981. "Personal involvement as a determinant of argument-based persuasion." *Journal of Personality and Social Psychology* 41(5): 847-855.

Petty, Richard E., and John T. Cacioppo. 1979. "Issue Involvement Can Increase or Decrease Persuasion by Enhancing Message-Relevant Cognitive Responses." *Journal of Personality and Social Psychology* 37(10): 1915–1926.

———. 1986. "The Elaboration Likelihood Model of Persuasion." In *Advances in Experimental Social Psychology*, ed. Leonard Berkowitz. San Diego, CA: Academic Press.

Plessy v. Ferguson. 1896. 163 U.S. 537.

Pollock v. Farmers' Loan & Trust Co. 1895. 157 U.S. 429.

Pristin, Terry. 2004. "Connecticut Homeowners Say Eminent Domain Isn't a Revenue-Raising Device." *New York Times*: September 8.

———. 2006. "Voters Back Limits on Eminent Domain." *New York Times*: November 15.

Pritchett, C. Herman. 1941. "Divisions of Opinion Among Justices of the U. S. Supreme Court, 1939-1941." *The American Political Science Review* 35(5): 890–898.

Rathjen, Gregory J. 1974. "An Analysis of Separate Opinion Writing Behavior as Dissonance Reduction." *American Politics Research* 2(4): 393–411.

Roe v. Wade. 1973. 410 U.S. 113.

Rosenbaum, David. 2002. "Supreme Court Takes Up Redistricting Case from Mississippi." *New York Times,* December 10.

Rosenberg, Gerald N. 1991. *The Hollow Hope: Can Courts Bring About Social Change?* Chicago: University Of Chicago Press.

Segal, Jeffrey A., and Harold J. Spaeth. 2002. *The Supreme Court and the Attitudinal Model Revisited*. New York: Cambridge University Press.

Sekhon, Jasjeet S. Forthcoming. "Multivariate and Propensity Score Matching Software with Automated Balance Optimization: The Matching package for R." *Journal of Statistical Software*.

Sickels, Robert J. 1965. "The Illusion of Judicial Consensus: Zoning Decisions in the Maryland Court of Appeals." *The American Political Science Review* 59(1): 100–104.

Slotnick, Elliot E., and Jennifer A. Segal. 1998. *Television News and the Supreme Court: All the News that's Fit to Air?* New York: Cambridge University Press.

Stoutenborough, James W., Donald P. Haider-Markel, and Mahalley D. Allen. 2006. "Reassessing the Impact of Supreme Court Decisions on Public Opinion: Gay Civil Rights Cases." *Political Research Quarterly* 59(3): 419–433.

Tyler, Tom R, and Kenneth Rasinski. 1991. "Procedural Justice, Institutional Legitimacy, and the Acceptance of Unpopular U.S. Supreme Court Decisions: A Reply to Gibson." *Law & Society Review* 25: 621.

U.S. Congress. Senate. 2010. *Continuation of the Nomination of Elena Kagan to be an Associate Justice of the Supreme Court of the United States*. 111th Congress, 2nd sess. June 30.

Ulmer, S. Sidney. 1970. "Dissent Behavior and the Social Background of Supreme Court Justices." *The Journal of Politics* 32(3): 580–598.

———. 1971. "Earl Warren and the Brown Decision." *The Journal of Politics* 33(3): 689–702.

Uslaner, Eric M., and Ronald E. Weber. 1979. "Public Support for Pro-Choice Abortion Policies in the Nation and States: Changes and Stability after the Roe and Doe Decisions." *Michigan Law Review* 77: 1772–1788.

Wahlbeck, Paul J., James F. Spriggs, and Forrest Maltzman. 1999. "The Politics of Dissents and Concurrences on the U.S. Supreme Court." *American Politics Research* 27(4): 488–514.

Walker, Thomas G., Lee Epstein, and William J. Dixon. 1988. "On the Mysterious Demise of Consensual Norms in the United States Supreme Court." *The Journal of Politics* 50(2): 361–389.

Zaller, John R. 1992. *The Nature and Origins of Mass Opinion*. New York: Cambridge University Press.

Zimbardo, Philip G. 1960. "Involvement and communication discrepancy as determinants of opinion conformity." *The Journal of Abnormal and Social Psychology* 60(1): 86-94.

Zink, James R., James F. Spriggs, and John T. Scott. 2009. "Courting the Public: The Influence of Decision Attributes on Individuals' Views of Court Opinions." *The Journal of Politics* 71(3): 909–925.

Appendix A

Sample Population

YouGov Polimetrix was hired to field the survey that provided the data for Chapters 3 and 4. Interviews were conducted online from January 19 to 24, 2011. YouGov Polimetrix provided the data for 600 respondents. Demographics of the sample population are as follows:

Age

18-34	26.33%
35-54	40.67%
55+	33.00%

Gender

Male	48.00%
Female	52.00%

Political Party

Democrat	37.17%
Republican	25.00%
Independent	29.17%
Other/NA	8.67%

Race

White	69.50%
Black	12.33%
Hispanic	12.17%
Asian	2.67%
Other	3.33%

Income

< $10,000	6.67%
$10,000 - $14,999	5.50%
$15,000 - $19,999	4.00%
$20,000 - $24,999	6.17%
$25,000 – $29,999	5.17%
$30,000 – $39,999	11.50%
$40,000 - $49,999	8.50%
$50,000 - $59,999	8.00%
$60,000 - $69,999	5.83%
$70,000 - $79,999	8.00%
$80,000 - $99,999	4.50%
$100,000 - $119,999	7.33%
$120,000 - $149,999	4.33%
$150,000+	3.83%
Prefer not to say	10.67%

Voter Registration

Registered	86.17%
Not registered	12.00%
Don't know	1.83%

Appendix B

Survey Materials

Pre-treatment questionnaire

Prior to treatment, respondents were to be asked the following. Each group of statements – the issue questions and the Court questions – were given in random order, but the issue questions always came before the Court questions.

Please indicate whether you agree or disagree with each statement:

1. Homosexual couples should be entitled to the same benefits as married heterosexual couples.
2. If an employer gives you a cell phone for work, he/she should be allowed to read all the text messages and emails you send and receive on that phone.
3. Contract disputes ought to be settled through arbitration or mediation, not lawsuits, whenever possible.
4. Universities should consider an applicant's race in order to achieve a more diverse student body.
5. Police should never be allowed to question a suspect without a lawyer present unless the suspect explicitly says he/she does not want a lawyer.
6. If an employee is exposed to a toxic substance at work but does not actually become ill, he/she should be able to sue his/her employer for emotional distress.
7. The income tax should be replaced by a national sales tax.
8. The government should ensure that every citizen receives adequate healthcare.
9. Law abiding individuals should have the right to own and carry handguns.
10. Public schools should schedule time when children may pray silently if they want to.

Please indicate whether you agree or disagree with each statement:

1. If the United States Supreme Court started making a lot of decisions that most people disagreed with, it might be better to do away with the Court altogether.
2. The United States Supreme Court can usually be trusted to make decisions that are right for the country as a whole
3. The decisions of the United States Supreme Court consistently favor some groups more than others.
4. The right of the United States Supreme Court to decide certain types of controversial issues should be reduced.

Treatments

Each respondent was given <u>one</u> vignette from each category below (i.e., one from Experiment #1 – High Salience, one from Experiment #1 – Medium Salience, etc.); thus, each respondent read six vignettes in total. The order of these vignettes was randomized.

Experiment #1 – High Salience (same-sex unions)

1. By a unanimous vote, the United States Supreme Court recently ruled that states may ban same sex unions, which grant homosexual couples many of the rights given to married couples.
2. By a unanimous vote, the United States Supreme Court recently ruled that states may not ban same sex unions, which grant homosexual couples many of the rights given to married couples.
3. By a vote of 8 to 1, the United States Supreme Court recently ruled that states may ban same sex unions, which grant homosexual couples many of the rights given to married couples.
4. By a vote of 8 to 1, the United States Supreme Court recently ruled that states may not ban same sex unions, which grant homosexual couples many of the rights given to married couples.
5. By a vote of 5 to 4, the United States Supreme Court recently ruled that states may ban same sex unions, which grant homosexual couples many of the rights given to married couples.
6. By a vote of 5 to 4, the United States Supreme Court recently ruled that states may not ban same sex unions, which grant homosexual couples many of the rights given to married couples.
7. The United States Supreme Court recently ruled that states may ban same sex unions, which grant homosexual couples many of the rights given to married couples.
8. The United States Supreme Court recently ruled that states may not ban same sex unions, which grant homosexual couples many of the rights given to married couples.

Experiment #1 – Medium Salience (employee privacy)

1. By a unanimous vote, the United States Supreme Court recently ruled that employers may read text messages on their employees' company issued phones.
2. By a unanimous vote, the United States Supreme Court recently ruled that employers may not read text messages on their employees' company issued phones.
3. By a vote of 8 to 1, the United States Supreme Court recently ruled that employers may read text messages on their employees' company issued phones.

4. By a vote of 8 to 1, the United States Supreme Court recently ruled that employers may not read text messages on their employees' company issued phones.
5. By a vote of 5 to 4, the United States Supreme Court recently ruled that employers may read text messages on their employees' company issued phones.
6. By a vote of 5 to 4, the United States Supreme Court recently ruled that employers may not read text messages on their employees' company issued phones.
7. The United States Supreme Court recently ruled that employers may read text messages on their employees' company issued phones.
8. The United States Supreme Court recently ruled that employers may not read text messages on their employees' company issued phones.

Experiment #1 – Low Salience (contract dispute resolution)

1. By a unanimous vote, the United States Supreme Court recently ruled that people disputing contracts must pursue mediation or arbitration before resorting to lawsuits.
2. By a unanimous vote, the United States Supreme Court recently ruled that people disputing contracts may sue in court without pursuing a resolution through mediation or arbitration first.
3. By a vote of 8 to 1, the United States Supreme Court recently ruled that people disputing contracts must pursue mediation or arbitration before resorting to lawsuits.
4. By a vote of 8 to 1, the United States Supreme Court recently ruled that people disputing contracts may sue in court without pursuing a resolution through mediation or arbitration first.
5. By a vote of 5 to 4, the United States Supreme Court recently ruled that people disputing contracts must pursue mediation or arbitration before resorting to lawsuits.
6. By a vote of 5 to 4, the United States Supreme Court recently ruled that people disputing contracts may sue in court without pursuing a resolution through mediation or arbitration first.
7. The United States Supreme Court recently ruled that people disputing contracts must pursue mediation or arbitration before resorting to lawsuits.
8. The United States Supreme Court recently ruled that people disputing contracts may sue in court without pursuing a resolution through mediation or arbitration first.

Experiment #2 – High Salience (affirmative action)

1. By a unanimous vote, a generally liberal federal appeals court recently ruled that universities may consider race in admissions in order to correct for past discrimination.

2. By a unanimous vote, a generally liberal federal appeals court recently ruled that universities may not consider race in admissions in order to correct for past discrimination.

3. By a unanimous vote, a generally conservative federal appeals court recently ruled that universities may consider race in admissions in order to correct for past discrimination.

4. By a unanimous vote, a generally conservative federal appeals court recently ruled that universities may not consider race in admissions in order to correct for past discrimination.

5. By a vote of 2 to 1, a generally liberal federal appeals court recently ruled that universities may consider race in admissions in order to correct for past discrimination.

6. By a vote of 2 to 1, a generally liberal federal appeals court recently ruled that universities may not consider race in admissions in order to correct for past discrimination.

7. By a vote of 2 to 1, a generally conservative federal appeals court recently ruled that universities may consider race in admissions in order to correct for past discrimination.

8. By a vote of 2 to 1, a generally conservative federal appeals court recently ruled that universities may not consider race in admissions in order to correct for past discrimination.

Experiment #2 – Medium Salience (police interrogation)

1. By a unanimous vote, a generally liberal federal appeals court recently ruled that police officers may question suspected criminals without a lawyer present unless they explicitly request one.

2. By a unanimous vote, a generally liberal federal appeals court recently ruled that police officers may not question suspected criminals without a lawyer present unless they explicitly refuse one.

3. By a unanimous vote, a generally conservative federal appeals court recently ruled that police officers may question suspected criminals without a lawyer present unless they explicitly request one.

4. By a unanimous vote, a generally conservative federal appeals court recently ruled that police officers may not question suspected criminals without a lawyer present unless they explicitly refuse one.

5. By a vote of 2 to 1, a generally liberal federal appeals court recently ruled that police officers may question suspected criminals without a lawyer present unless they explicitly request one.

6. By a vote of 2 to 1, a generally liberal federal appeals court recently ruled that police officers may not question suspected criminals without a lawyer present unless they explicitly refuse one.
7. By a vote of 2 to 1, a generally conservative federal appeals court recently ruled that police officers may question suspected criminals without a lawyer present unless they explicitly request one.
8. By a vote of 2 to 1, a generally conservative federal appeals court recently ruled that police officers may not question suspected criminals without a lawyer present unless they explicitly refuse one.

Experiment #2 – Low Salience (employer liability)

1. By a unanimous vote, a generally liberal federal appeals court recently ruled that employees may sue their employers for emotional distress after being exposed to toxic chemicals at work even if this exposure did not result in any illness or injury.
2. By a unanimous vote, a generally liberal federal appeals court recently ruled that employees may not sue their employers for emotional distress after being exposed to toxic chemicals at work if this exposure did not result in any illness or injury.
3. By a unanimous vote, a generally conservative federal appeals court recently ruled that employees may sue their employers for emotional distress after being exposed to toxic chemicals at work even if this exposure did not result in any illness or injury.
4. By a unanimous vote, a generally conservative federal appeals court recently ruled that employees may not sue their employers for emotional distress after being exposed to toxic chemicals at work if this exposure did not result in any illness or injury.
5. By a vote of 2 to 1, a generally liberal federal appeals court recently ruled that employees may sue their employers for emotional distress after being exposed to toxic chemicals at work even if this exposure did not result in any illness or injury.
6. By a vote of 2 to 1, a generally liberal federal appeals court recently ruled that employees may not sue their employers for emotional distress after being exposed to toxic chemicals at work if this exposure did not result in any illness or injury.
7. By a vote of 2 to 1, a generally conservative federal appeals court recently ruled that employees may sue their employers for emotional distress after being exposed to toxic chemicals at work even if this exposure did not result in any illness or injury.
8. By a vote of 2 to 1, a generally conservative federal appeals court recently ruled that employees may not sue their employers for emotional distress after being exposed to toxic chemicals at work if this exposure did not result in any illness or injury.

Post-treatment questionnaire

After each vignette, respondents we asked the following question:

Overall, do you agree or disagree with the Court's decision in this case?

If the respondent disagreed, s/he was then asked this question:

Do you think that the decision ought to be accepted and considered to be the final word on the matter or that there ought to be an effort to challenge the decision and get it changed?