Essays in Empirical Law and Economics

by

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Abstract

This dissertation, which is a collection of three essays, uses empirical methods to study questions at the intersection of law and economics. The first chapter, co-authored with Joshua Fischman, explores how supervision by an administrative appeals court, the Board of Immigration Appeals (BIA), influences the exercise of discretion of lower court immigration judges in asylum cases. The second chapter studies whether career concerns influence judicial decision-making within the context of asylum adjudication in immigration courts. The final chapter investigates how expansions in the right to counsel impacted criminal defendants, with particular focus on the Supreme Court's 1963 decision in *Gideon vs. Wainwright.*

Thesis Supervisor: David Autor
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I dedicate this dissertation to my parents, Kelley and Shirley Chou, and my husband, Steve Lem. There are no words that can possibly express the debt I owe to them for their love and for their sacrifice.

Contents

Chapter 1

Appellate Supervision of Lower Court Discretion: Evidence from Asylum Adjudication

with Joshua Fischman

Abstract

This paper explores how supervision by an administrative appeals court, the Board of Immigration Appeals (BIA), influences the exercise of discretion of lower court immigration judges in asylum cases. We exploit as a natural experiment the BIA streamlining regulations of 2002 and 2003. These regulations resulted in more cursory review by a Board with fewer, more conservative members. Using a dataset that contains the universe of asylum decisions between 1994 and 2004, we explore how the level and dispersion of asylum grant rates across immigration judges changed before and after BIA streamlining. To identify judge effects, we rely on the fact that, at least within a particular immigration court, asylum cases are randomly assigned across judges. Our results, estimated from a random effects probit model, indicate that both the level and variation in asylum grant rates fell coincident with BIA streamlining. These patterns cannot be attributed solely to other times trends such as changes in country conditions, the September 11th terrorist attacks, or changes in the composition of immigration judges.

1.1 Introduction

In 2004, an immigration judge deciding Chinese asylum cases in New York City immigration court granted asylum in over 60 percent of the cases he heard. That same year, his colleague, also deciding Chinese asylum cases in New York City, granted asylum in less than 40 percent of cases. Both judges were 50-year-old males appointed during the Clinton administration whose most significant prior experience was as an attorney in private practice. Such wide disparities in asylum grant rates which cannot be explained by differences in the judges' caseloads or demographic characteristics, have been well-documented by lawyers, academics and the broader media. That an asylum decision depends on the random draw of a judge has been questioned as contrary to a system based on the consistent application of the rule of law.

Asylum applicants must demonstrate a "well-founded fear of persecution based on race, religion, nationality, membership in a social group, or political opinion." Immigration law contains no comprehensive definition of "persecution" and the likelihood that an applicant will suffer persecution is defined by an inexact "reasonable possibility" standard. Immigration judges apply these indeterminate legal standards to highly fact-specific individual circumstances. Often, when the only available evidence is the applicant's own testimony, the decision to grant or deny asylum is based almost entirely on an immigration judge's assessment of the applicant's credibility.

The asylum legal standard leaves immigration judges a tremendous amount of discretion in deciding outcomes in particular cases. Judicial discretion can be a double-edged sword. On the one hand, discretion gives a decision-maker flexibility to take into account and weigh various types of evidence, tailoring a decision to the unique circumstances of a given case. On the other hand, discretion may also lead to unwarranted disparity in outcomes such that two different judges may reach different conclusions in otherwise similar cases. Ramji-Nogales, Schoenholtz and Schrag (2007)—building on previous efforts by Asylumlaw.org and the Transactional Records Access Clearinghouse, and using the same data on which this paper is based—have thoroughly documented disparities among immigration judges. Figure 1, adapted from statistics reported by Ramji-Nogales et al., illustrates the nature of inter-judge disparity in asylum grant rates. The figure shows that among judges deciding Indian cases in San Francisco, the

mean grant rate overall was 51 percent, while individual judge grant rates ranged from 3 to 84 percent. Similar patterns are observed in other courts for other countries. In Los Angeles, the mean grant rate for Chinese asylum seekers was 37 percent, but individual grant rates among judges ranged from 9 to 81 percent.

What determines this disparity and what factors might operate to constrain it? First, immigration scholars including Ramji-Nogales et al. (2007), Taylor (2007), and Legomsky (2007) have contemplated the creation of more detailed and determinate legal standards. These reforms range from more precise statutes and regulations defining the meaning of the terms "persecution" and "well-founded," to the use of rigid quota systems confining asylum grant rates within a specific range. In the 1980s, similar concerns over unwarranted disparity in the sentencing of criminal offenders by federal district court judges led to the adoption of the Federal Sentencing Guidelines. The Guidelines were intended to restrict the exercise of judicial discretion to a narrow sentencing range and to limit judicial departures from that range. Anderson, Kling and Stith (1999) found that following the introduction of the Guidelines, the expected difference between two typical judges in the average sentence length fell from 5 to 4 months.

The second broad category of reform immigration scholars have contemplated involves greater investments and resources in immigration courts and judges. Currently, over 200 judges sit in over 50 immigration courts nationwide. Unlike federal court Article III judges, immigration judges are executive branch civil service employees who do not enjoy life tenure. They work under significant time pressure to decide heavy caseloads, with little support staff, in a substantive area of the law that is complex and dynamic. Asylum applicants often appear in court without the assistance of legal counsel, and in these cases, the immigration judge may bear additional responsibility to identify the relevant legal and factual issues. Infusing trial-level immigration courts with more judges, more support staff, better screening, better training, and increased access to attorney representation are reforms that have been repeatedly advanced.

The third broad category of reform—and the focus of this paper—involves the role of appellate supervision in constraining the exercise of discretion. Legomsky (2007) hypothesizes

that an appellate tribunal, and the procedures under which it operates, can affect the level of consistency among judges in a lower court. He posits that the total number of judges on a higher court, the size of the panels in which they deliberate, and whether or not they produce written, reasoned opinions are institutional design features that not only affect, horizontally, the consistency of the appellate tribunal itself, but also affect, vertically, the consistency of the lower, trial-level tribunal. A large law and economics and political science literature has explored similar ideas relating to hierarchical court systems and the role of appellate supervision in shaping the decision-making of a lower court (Shavell (1995, 2006); McCubbins, Noll, and Weingast (1995); Kornhauser (1995)).

This paper picks up where existing theoretical literature leaves off. It tests, empirically, the effect of appellate supervision on the exercise of discretion of judges in a lower court. In the immigration context, the decisions of immigration judges are reviewed in the first instance by an administrative appeals body, the Bureau of Immigration Appeals (BIA). In 2002 and 2003, the BIA implemented dramatic changes to its appellate procedure, with the stated goal of eliminating its existing backlog and disposing of cases more quickly. Prior to March 2002, asylum appeals to the BIA were heard in three-member panels, and decisions were accompanied by a written, reasoned opinion. Beginning in March 2002, however, the default procedure for asylum appeals was changed to single-member review with single-sentence affirmances without further opinion. In addition, the standard of review for factual determinations was changed from de novo to clear error, and the BIA was downsized from 23 to 11 authorized members. Together, BIA streamlining—as these reforms are referred to—led to less intense appellate scrutiny by a smaller, more ideologically conservative Board. These reforms provide a natural experiment for studying how judges on a lower court respond to changes in appellate supervision, and in turn, how changes in lower court decision-making impact the overall level of inter-judge disparity.

Using data on the universe of asylum decisions issued by immigration judges between 1994 and 2004, we examine how changes in appellate procedure at the BIA affected the decision-making of lower court immigration judges. The dataset identifies the judge who decided the case, the decision reached, the immigration court where the case was heard, the country of origin of the applicant and other basic characteristics such as whether the applicant was represented by

an attorney. Immigration judges must decide a large volume of asylum cases, and indeed, the dataset contains over 600,000 asylum filings over a ten year period. This allows us to observe a large number of decisions for any single judge and in turn, allows us to examine relatively narrow time periods, for example a 6 month window. In addition, random assignment of asylum cases, at least within a particular immigration court, allows for meaningful comparisons of judge grant rates that are not driven solely by differences in the judges' underlying caseloads.

The predicted effects of the BIA streamlining reforms depends on assumptions about the underlying model of judicial behavior describing how judges on a lower court respond to appellate supervision. Appellate review can have both retrospective error-correcting functions as well as prospective behavior-inducing functions. In an "independent judge" model, lower court judges simply interpret and apply the law as they see fit, and the presence or absence of an appellate court does not change a lower court's application of the law. In a "principal-agent" model, appellate supervision functions to monitor lower court judges, constraining the exercise of their discretion and aligning the decisions of the lower court judges to the preferences of the supervising court. Finally, in a "team model," the lower and higher court have aligned preferences but imperfect information, and appeals serve to correct errors. Under this approach, when a higher court abdicates its responsibility to sufficiently review lower court decisions, the lower court may expend more of its own effort to reach a correct decision in the first instance.

Conceptually and methodologically, this paper is most closely related to Anderson, Kling and Stith (1999). As mentioned above, these authors study criminal sentencing disparity among federal district court judges before and after the introduction of the Federal Sentencing Guidelines in 1987. Like Anderson et al., this paper employs an interrupted time-series approach to measure the effect of a major legal reform on inter-judge disparity, relying on random case assignment within a given court to identify judge effects. Inter-judge disparity in Anderson et al. is measured using a binomial random effects model appropriate for count data, and similarly, inter-judge disparity in this paper is measured using a probit random effects model more appropriate for binary data. Unlike Anderson et al. which studies a change in the substantive legal standard as a policy instrument to constrain the exercise of judicial discretion, this paper examines a change in appellate supervision.

More broadly, this paper builds on the existing literature in a variety of ways. First, a series of empirical law and economics papers—including Anderson et al. (1999); Waldfogel (1998); Abrams, Bertrand and Mullainathan (2007); Schanzenbach (2005); and Schanzenbach and Tiller (2007)—study criminal sentencing decisions made by state and federal judges. This paper extends the study of judicial decision-making to a different type of decision-maker applying legal rules from a different substantive area of the law, namely immigration judges in an administrative setting adjudicating asylum cases. Previous scholarship from the legal academy has also focused on decision-making in the administrative setting. The most well-known is Mashaw's 1983 study of the Social Security system.

Moreover, this paper builds on the empirically-oriented literature focusing on various aspects of the immigration bureaucracy in the both the United States and Europe. Ramji-Nogales et al., as already mentioned, study inter-judge disparity among immigration judges in the United States, and similarly, Holzer, Schneider, and Widmer (2000) as well as Neumayer (2005) study the same in European asylum systems. In a series of empirical papers, Gilboy has studied bail setting in deportation cases, administrative review of bond determination hearings, and the decision-making of federal inspectors.

Inter-judge consistency is not the only desirable property of a legal system based on the rule of law. Accuracy, too, is a desirable characteristic that merits close study. The scope of this paper, however, does not extend to an examination of how BIA streamlining may have affected the accuracy of immigration judge decisions. This is because the measurement of accuracy, as opposed to consistency, is relatively more difficult for two reasons, one conceptual and the other practical. First, while a judge's decision to grant or deny is readily observable, whether a judge's decision is right or wrong is more difficult to quantify. Indeed, the difficulty in measuring accuracy relates back to the reason for why there may be inconsistency in the first place—given two sets of identical facts, reasonable minds may disagree as to what outcome is "right," and therefore, whether a decision is correct or incorrect may be an inherently unanswerable question. Second, even if non-controversial measures of accuracy could be formulated, there exists a practical, data-collection hurdle. Measuring accuracy requires a content analysis of the case file and record. This is time-intensive not only because it requires reading through documentary and testimonial evidence, but also because, the files themselves, rather than being

maintained in a central repository, are dispersed across 50 different immigration courts across the country.

The remainder of the paper is organized as follows. Section 1.2 provides institutional background on asylum adjudication, immigration courts and judges, and the BIA streamlining reforms of 2002 and 2003. Section 1.3 develops alternative models of judicial behavior which serve as the starting point for predicting the effects of BIA streamlining. Section 1.4 describes the data upon which this paper is based and provides basic descriptive statistics. Section 1.5 presents the empirical methodology, results, and interpretation of the data, and Section 1.6 concludes.

1.2 Institutional Background

1.2.1 Asylum Process

Asylum applicants must demonstrate a "well-founded fear of persecution on account of race, religion, nationality, membership in a particular social group, or political opinion". Individuals displaced due to civil war, ethnic strife or natural disaster do not necessarily satisfy this legal definition unless there are instances of persecution targeted specifically at the individual. The legal standard is highly fact specific, and if no corroborating evidence is available, asylum can be granted on the applicant's testimony alone. Thus, decisions often turn on a judge's determination of the applicant's credibility.

Figure 2 illustrates the steps in the asylum process. Removal proceedings are initiated when the Department of Homeland Security (DHS), through its subagency Immigration and Customs Enforcement (ICE), issues a charging document with one of the over 50 immigration courts spread across the country. Individuals may request asylum as a defense against removal, and through an adversarial hearing, an immigration judge decides whether to grant or deny the request. Either party, the applicant or DHS-ICE, may appeal an immigration judge's decision to the Bureau of Immigration Appeals (BIA), an administrative appeal body[1]. Applicants[2],

[1] 8 C.F.R. 1003.3, 1003.38.
[2] 8 U.S.C. 1252.

but not DHS-ICE[3], may appeal an adverse BIA decision to the federal circuit courts of appeal.

In addition to asserting asylum as a defense in removal proceedings, applicants can also request asylum through an affirmative application process. This process is available to individuals who are physically present in the United States, either legally or illegally, and who are not already in removal proceedings. The individual files an application with the United States Citizenship and Immigration Services (USCIS), also a component of the Department of Homeland Security. A USCIS asylum officer then decides the affirmative asylum application in a non-adversarial, informal interview. If the asylum officer denies the affirmative asylum claim, it is then referred to an immigration judge for further consideration, at which point it is treated as a defensive asylum claim that can be appealed to the BIA and the federal circuit courts.

Several executive branch agencies are involved in the asylum process, as is the federal judiciary. Both ICE and USCIS are components of the Department of Homeland Security (DHS), while the immigration courts and the BIA are components of the Executive Office of Immigration Review (EOIR) within the Department of Justice (DOJ). As such, prosecution and adjudication functions are vested in separate agencies. Thus, the ICE agents who issue charging documents for removal and the attorneys who prosecute such cases are employees of DHS, while the immigration judges and BIA members who adjudicate such cases are employees of DOJ. Ultimately, however, both DHS and DOJ answer to the Attorney General[4].

Asylees are eligible to adjust to legal permanent resident (LPR) status one year after receiving a grant of asylum. Previously, the number of asylees who could adjust to LPR status was limited by an annual cap of 10,000, but this quota was eliminated by the 2005 Real ID Act. Asylees comprised 2% of new legal permanent residents[5] in 2005, while the bulk of new LPRs came from family and employment sponsored preferences[6]. Despite the fact that asylees do not constitute a major source of legal immigration, asylum cases constitute a significant volume

[3]Footnote 203, page 35, Palmer, Yale-Loehr and Cronin., Why are so many people challenging BIA appeals decisions in federal court?

[4]There have been frequent proposals to make the immigration courts and the BIA more independent and separate from Attorney General. Palmer, Yale-Loehr, Cronin footnote 54, page 15.

[5]2005 Yearbook of Immigration Statistics, Table 6, shows 1.1 million new LPRs, Table 16, shows 25,000 asylum grants.

[6]2005 Yearbook of Immigration Statistics, Table 6.

of the work of the immigration courts. In recent years, removal proceedings have constituted about 85% of the workload of the immigration judges, with the remainder devoted to hearings on motions and bonds[7]. Individuals request relief from removal in about one-third of removal proceedings[8], with asylum being the most common form of relief requested[9].

1.2.2 Immigration Courts and Judges

Immigration judges are non-Article III, civil service employees of the Department of Justice, Executive Office of Immigration Review. Currently, there are over 200 judges sitting in over 50 immigration courts across the country. The professional backgrounds of immigration judges vary, but many have prior legal experience as prosecuting trial attorneys in ICE and its predecessor the INS. Immigration judges have also served as lawyers with non-profit and pro bono legal services organizations, as public defenders, or as attorneys in private practice. One-third of immigration judges are female. Generally, the selection and hiring of immigration judges is less politicized than the selection process for federal judges[10]. Unlike federal judges, immigration judges do not enjoy life tenure and can be fired or disciplined for misconduct or poor performance[11].

Immigration judges must decide a large number of fact-intensive cases, with minimal resources, applying substantive legal standards that are complex and often in flux. In fiscal year 2006, immigration judges presided over 300,000 individual matters[12]. Some immigration courts are assisted by law clerks and staff attorneys, but many are not[13]. In addition, immigrants often appear in court without legal representation, and in these situations, the immigration judge bears additional responsibility to help the immigrant identify the relevant legal and factual issues. The day-to-day work has been described by an immigration judge as follows:

[7] EOIR Statistical Yearbook FY 2005, page B5.
[8] EOIR Statistical Yearbook FY 2005, page N1.
[9] AILA-EOIR liaison meeting 10/18/06.
[10] AILA-EOIR Liaison meeting notes, October 17, 2005 (paragrah 15). But see, Immigration Judges Often Picked Based on GOP Ties, Amy Goldstein and Dan Eggen, The Washington Post, June 11, 2007, page A01.
[11] AILA-EOIR liason meeting notes, September 25, 2003 (paragraph 5), October 18, 2006 (paragraph 3).
[12] EOIR 2006 Statistical Yearbook, page B2.
[13] AILA-EOIR Liaison meeting notes, October 18, 2006 (paragraph 4).

> Immigration judges routinely have four full hearings scheduled each day to determine the merits of a claim for relief from deportation, such as asylum, and are expected to render oral decisions from the bench, often with little time for reflection. We are charged with applying a complicated and frequently amended governing statute which has repeatedly been acknowledged as second only to the tax code in its legal complexity.[14]

The performance of immigration judges has come under recent scrutiny by both the Attorney General and the federal judiciary. In January 2006 Attorney General Alberto Gonzales opened an investigation into the immigration courts, eventually calling for the creation of a code of conduct for judges, competency testing and performance evaluations to detect high reversal rates, frequent complaints or unusual backlogs[15]. Judge Richard Posner of the Seventh Circuit has also been a frequent critic of immigration judges, warning that adjudication has "fallen below the minimum standards of legal justice."[16]

1.2.3 Bureau of Immigration Appeals (BIA) and Streamlining

The Board of Immigration Appeals (BIA) is an administrative review body with responsibility to adjudicate appeals of immigration judge decisions. The BIA exists solely through the Attorney General's regulations and has never been recognized by statute. Furthermore, the Attorney General has the authority to eliminate any member of the BIA for any cause. As mentioned above, both the immigrant and the government may appeal a decision of the immigration judge to the BIA. Although the number of authorized BIA members has fluctuated over time, the BIA is currently composed of 15 members[17].

Beginning in October 1999, the BIA implemented "pilot" streamlining regulations to address a growing backlog of cases. These "pilot" regulations would eventually be expanded into a "comprehensive" streamlining effort beginning in 2002. BIA streamlining, summarized in

[14] House of Representatives, Committe on House Government Reform, Subcommittee on Federal Workforce and Agency Organization, Testimony of Denise Slavin, President, National Association of Immigration Judges, May 16, 2006.

[15] Attorney General Gonzales Outlines Reforms for Immigration Courts, Board of Immigration Appeals, U.S. Department of Justice press release, August 9, 2006.

[16] Benslimane v. Gonzales, 430 F.3d 828 (7th Cir. 2005).

[17] 71 FR 70855.

Figure 3a, is well-documented in an American Bar Association report authored by the law firm Dorsey and Whitney (2003). BIA streamlining involved changing important appellate procedures and downsizing the number of members, with the goal of deciding more cases in less time.

Prior to BIA streamlining, appeals at the BIA were decided in three-member panels whose decisions were accompanied by a written opinion. The "pilot" streamlining regulations adopted in October 1999 authorized the BIA Chairman to designate "certain categories" of cases for single member affirmance without opinion (AWO). This meant that an appeal would be heard by a single member who could decide the case without giving any written opinion or reasoning other than a one-sentence statement of affirmance. During 2000 and 2001, single member AWO was applied to only a narrow subset of immigration appeals. Significantly, the BIA Chairman in March 2002 extended the single member AWO procedure to asylum appeals as well.

At around the same time, in February 2002, proposed regulations for "comprehensive" streamlining were announced. These comprehensive streamlining regulations codified many of the changes adopted under the pilot streamlining initiative. It made single member AWO the default procedure for all immigration appeals. In addition, the number of BIA members would be downsized from 23 to 11, and the standard of review for factual issues would be more deferential, changing from de novo to clear error. Comprehensive streamlining was announced in February 2002 and was officially implemented seven months later in September 2002. The BIA downsizing was officially implemented six months later in March 2003.

As summarized in Figure 3b, BIA streamlining, as it applied to asylum cases, occurred at different points in time. First, single member affirmance without opinion was applied to asylum cases beginning in March 2002. Prior to that time, asylum appeals were heard by three judge panels whose decisions were accompanied by reasoned opinions. Second, a more deferential, clear error standard of review for factual issues was announced in February 2002 and was implemented for all appeals in September 2002. Third, BIA downsizing was announced in February 2002 and was officially implemented in March 2003, one year after single member AWO was applied to asylum cases. Finally, as far as we are aware, no other major change in asylum procedural or substantive law took place between 2000 and 2003. Importantly, however,

the September 11, 2001 terrorist attacks occurred 5 months before single member AWO was applied to asylum appeals. The events of September 11th could potentially confound the effects of BIA streamlining, and we discuss this more fully below.

The conventional wisdom on the effects of single member AWO is that it led to rubber-stamping of immigration judge decisions as fewer errors were identified and corrected. This is supported by evidence of a flood of appeals to the federal circuit courts beginning in March 2002 after single member AWO was applied to asylum appeals. Palmer, Yale-Loehr, and Cronin (2005) explain that as the BIA began to deny a larger proportion of appeals, immigrants were increasingly dissatisfied with the BIA's review and began to seek further redress in the federal circuit courts. More frequent and/or intense review by the federal circuits may offset relaxed review at the BIA.

In addition, the conventional wisdom on the effects of BIA downsizing is that liberal pro-asylum members were purged. Levinson (2004) supports this argument by examining the voting patterns of BIA members in closely divided en banc precedent decisions. Although statistical data on the voting patterns of the BIA members is unavailable because the BIA does not maintain a database of appeals and their outcomes, some limited information is available through data collected by the law firm of Jones Day. This data supports the view that those members who were fired or reassigned were the more liberal members.

1.3 Models of Judicial Behavior

We draw upon the theoretical literature on judicial behavior to develop testable hypotheses regarding the impact of BIA streamlining. The two primary models of judicial hierarchy, the "principal-agent" model and the "team" model, generate different predictions which we can compare.

Under the "principal-agent" model, e.g. Shavell (2006), McNollgast (1995), the higher court is viewed as a principal that monitors the lower court's compliance with doctrine. Due to the inability to review every case, the higher court cannot ensure strict compliance, and must allow the lower court some latitude in deciding cases.

There are two primary ways in which the BIA streamlining could have affected the decision-making of the immigration judges in the principal-agent model. First, the provisions allowing affirmance without opinion would result in less scrutiny for the immigration judges, allowing more latitude However, given that asylum claimants are more likely to appeal than the INS, this increase in latitude may be greater in the conservative direction. Second, the more conservative bent of the BIA following its reduction in size might lead to more conservative rulings on the part of the immigration judge.

In the "team" model of adjudication, e.g. Shavell (1995), Kornhauser (1995), judges have aligned preferences but imperfect information, and appeals serve to correct errors. Under this approach, we hypothesize that immigration judges would expend more effort to avoid errors, on the theory that the appeals court would apply less scrutiny to appeals. Given that most cases subject to affirmance without opinion will be denials of asylum, we also hypothesize that streamlining might lead immigration judges to be more generous with asylum, on the theory that the BIA would be less likely to catch their mistakes if they deny asylum.

A third possibility, which we refer to as the "independent judge" model, assumes that immigration judges will be completely indifferent to the BIA. Under this assumption, the immigration judges decide cases in accord with their best interpretation of what the law requires, and pay no attention to strategic considerations involving the appellate process. If immigration judges are truly independent in this sense, we should expect no change in their behavior as a result of streamlining.

Note that these models are not necessarily exclusive. There may be elements of both the "principal-agent" model and the "team" model influencing the behavior of immigration judges. However, evidence that immigration judges are becoming more conservative and that disparities are increasing following streamlining would tend to support the "principal-agent" model; evidence that immigration judges are granting asylum more freely and that disparities are decreasing would tend to support the "team" model. No change in behavior would support the "independent judge" model; however, no change could also result from the above effects cancelling each other.

1.4 Data Description

The original dataset contains the universe of over 600,000 asylum cases filed in immigration courts between 1994 and 2004. It contains information on the decision in each case, the identity of the judge, the court in which the case was heard, the date the case was decided, and the country of origin of the asylum applicant. Additional information includes whether the applicant was represented by an attorney, whether the case was originally filed as an affirmative or defensive claim, and whether the case was decided in absentia. Neither the age nor the gender of the asylum applicant is included in the data.

The data was obtained through a Freedom of Information Act (FOIA) request initiated by the journalist Frederic Tulsky. His analysis of the data was the basis of an article published in 2000 by the *San Jose Mercury News*[18]. A subsequent FOIA request filed by attorney David Berten of asylumlaw.org updated and extended Tulsky's dataset through the year 2004. We are indebted to both Tulsky and Berten for their efforts in obtaining this data which would otherwise not be publicly available. Comparisons of basic statistics calculated from the FOIA dataset with those published in the EOIR Statistical Yearbooks are generally consistent[19], and thus, there is little reason to doubt the reliability of the data.

Data on asylum cases was supplemented with demographic information on immigration judges including gender, age cohort, date of appointment, and six categories of employment history including INS/DHS, military, non-governmental organization such as public defender or legal aid society, private law practice, academia, and government service. The demographic variables are constructed using definitions similar to those in Ramji-Nogales et al. (appendix

[18] "Asylum seekers face capricious legal system." Frederic N. Tulsky. San Jose Mercury News, page 1A, October 18, 2000.

[19] Comparisons of the annual number of grants reported in the EOIR 2005 Statistical Yearbook (table 16, page 43) with the number of grants calculated from the FOIA dataset are as follows:

	Yearbook	FOIA	% difference
2001	10,001	9,944	0.57
2002	10,977	11,471	-4.50
2003	13,376	13,572	-1.47
2004	13,015	12,797	1.67

The minor discrepancies may be due to the fact that the Statistical Yearbook numbers are based on a fiscal year beginning in October, whereas the calculations generated from the FOIA dataset are based on the calendar year.

pages 97-98)[20]. The primary source of biographical information are press reports issued by the Executive Office of Immigration Review (EOIR) at the time a judge is appointed[21]. Of the 280 judges appearing in the original asylum dataset, biographical information was available for all but 29 judges. The relationship between observable judge characteristics and grant rates is discussed more fully below.

To form the working dataset upon which the results are based, the original is altered in a variety of ways. First, some judge names that had multiple variants were altered to a single, consistent form. Second, some observations were dropped due to missing or incomplete information[22]. Third, judges who decided less than 500 cases during the ten year period between 1994 and 2004 were eliminated[23], as were low volume immigration courts with less than 1,000 cases filed during the decade[24].

Finally, like Ramji-Nogales et al., we retained only one observation from the same "family" of cases since decisions in these cases are likely to be correlated. For example, a husband and wife often seek asylum at the same time. Their cases are typically assigned to the same immigration judge who then considers the family's application as a unit. This is similar to the problem of correlated sentences among co-defendants in Abrams et al. (2007) and Anderson

[20]Like Ramji-Nogales et al., gender was determined from the pronouns used in the EOIR press releases. While Ramji-Nogales et al. calculate age at the time of particular asylum decision based on the year of the judge's undergraduate degree, assuming the judge was 22 years old when the degree was awarded, we use a simpler, fixed age cohort variable based on the year the judge obtained a JD degree. Work experience was divided into six binary variables: INS/DHS, military, non-governmental organization, private law practice, academia, and government service. Work experience is similar to Ramji-Nogales et al., except that military experience includes all experience including the Reserves and National Guard, whether or not prior to or after law school; and academic expericence is a broader category including teaching faculty, whether tenured, visitor, lecturer, instructor or adjunct, and deans, but not administrative staff or research assistants and associates. Experience as an intern or short-term judicial law clerk was not considered. Unlike Ramji-Nogales et al. who use the date of first appointment, we use the date of the most recent appointment, which is the date reported in the first sentence of the press release. Only a handful of judges leave the bench and are subsequently reappointed.

[21]These press reports have been collected by the Transactional Records Access Clearinghouse and are available on their website at http://trac.syr.edu/immigration/reports/judgereports/. Additional biographical information not available from the EOIR press reports has been collected by Ramji-Nogales et al (2007) and is detailed in their data appendix.. We thank Ramji-Nogales et al for graciously sharing this additional data.

[22]A total of 4,111, or less than 1% of the observations were dropped due to missing or incomplete information. Observations were dropped in the following order, for the following reasons: (1) decision missing [4]; (2) immigration judge name missing or not specifically identified, e.g. identified as only "visiting judge" [1462]; (3) absentia missing [689]; (4) country of origin missing or not specifically identified, e.g. identified as "no nationality" or applicant "unable to name a country" [1081]; (5) defensive/affirmative missing [875].

[23]This resulted in 58 judges being dropped, for a total of 13,146 observations.

[24]Of the 52 immigration courts in the original dataset, 15 courts were dropped due to a low volume of case filings. This resulted in 4,615 observations being dropped.

et al. (1999). The asylum data set does not separately identify these "families," but they can be inferred when observations have identical information, i.e. the same country of origin, decision, judge, date, court, legal representation status, and type of claim whether defensive or affirmative. After identifying these "families," we kept one observation and eliminated the remainder[25]. The working dataset consists of 442,437 observations, or about 70% of the original 629,416 observations.

1.4.1 Descriptive Statistics

Although the working dataset contains over 400,000 cases filed, many are never adjudicated on the merits. Rather, they are abandoned, withdrawn, or disposed of in other ways such as through a change of venue. Of the cases that do reach a merits hearing, a majority are denied asylum. Figure 4a shows the number of asylum cases filed, decided and granted between 1994 and 2004. Over this decade, about 50% of cases filed reached a merits hearing, and of these cases, 30% were ultimately granted asylum. Figure 4b shows how the grant rate has fluctuated year-to-year, rising in the mid-to-late 1990s and leveling off beginning in 2000. Interestingly, there is a drop in the overall grant rate in 2002 and 2003, coincident with BIA streamlining.

Currently, over 200 judges are spread across 50 immigration courts. Tables 1a and 1b show the total number of judges and their entry and exit into and out of the working dataset. Since 1998, the total number of judges has been stable at around 180 judges, and there has been relatively little turnover. The largest increases in the number of judges occurred in 1995 and 1997 when there were net gains of 53 and 24 judges, respectively. To the extent that judge turnover was low during the years immediately preceding and following BIA streamlining in 2002 and 2003, changes in grant rate disparity are unlikely to be driven by changes in the composition of judges. Table 1b, however, shows that the average number of decisions per judge increased 15% in both 2001 and 2002. Increased caseloads may be a confounding factor that influences the pattern of grant rates independent from the effects of BIA streamlining.

[25] Using this method, we identifed 88,236 "families." Fifty-nine percent of these families were clusters of two observations (e.g. husband and wife, or parent and child). Ninety-five percent were clusters of 5 observations or less (e.g. two parents and three children). This method of inferring family units is overinclusive since the largest family identified was a cluster of 69 observations. Such large clusters, however, were not common in the dataset. Altogether, 165,107 observations were dropped.

These and other potential confounds are addressed more fully in the last section of the paper.

Figure 5 shows the distribution of filings across immigration courts. The working dataset consists of 37 courts spread across the country. Los Angeles, New York, Miami and San Francisco are the largest immigration courts. Together, these four courts receive 64% of all asylum filings. The remaining courts are small in comparison.

A total of 220 countries are represented in the data. Table 2 shows the top fifteen countries ranked by the number of asylum filings. Together, these fifteen countries comprise over half of the observations in the dataset. China is the largest country by any measure including filings, decisions and grants. It accounts for 14% of filings, 20% of decisions and 25% of grants. Mexico is the second largest country by filings, but the percent of filings that are decided on the merits–thirteen percent–is the lowest of all the top fifteen countries. This is because many Mexicans voluntarily entered the affirmative asylum process in order to be placed into deportation proceedings. Once in deportation proceedings before an immigration judge, they withdrew their asylum claims and requested other forms of relief available only as a defense to deportation. Figure 6 shows a wide variation in the asylum grant rate for these top fifteen countries. Applicants from Russia win asylum in 58% of cases, while applicants from El Salvador win in only 6% of cases.

Tables 3a and 3b show how filings from the top fifteen countries are distributed across the four largest immigration courts–Los Angeles, New York, Miami and San Francisco–and the other courts combined. Clearly, filings from any particular country are not evenly distributed across courts. Rather, applicants from a given country tend to cluster in certain immigration courts. For example, 67% of Chinese asylum cases are filed in New York, 52% of Indian asylum cases are filed in San Francisco, and 55% of Colombian cases are filed in Miami.

1.4.2 Grant Rates

Monthly asylum grant rates, disaggregated by country and by court, are graphed in Figures 7 and 8. Panel (a) in Figure 7 shows the monthly grant rate for all observations. Panels (b)-(f) in Figure 7 show the monthly grant rate for various groups of countries. Coincident with the implementation of BIA streamlining, grant rates are lower post-2002 relative to pre-2002,

although the magnitude of the differences varies across the different country groupings.

A similar pattern of falling grant rates post-2002 is depicted in panels (a) and (b) in Figure 8, which graph the grant rates for the top ten immigration courts and the non-top-ten courts, respectively. This pattern, however is not uniform across the four largest immigration courts. Panels (c)-(f) in Figure 8 show that while New York and San Francisco experienced falling grant rates post-2002, grant rates in Los Angeles and Miami remained relatively stable. San Francisco experienced the most dramatic fall in grant rates post-2002. Grant rates fell to their lowest level of 28% in March 2003, the month that BIA downsizing was implemented, from a high of 70% in August 2001, the month prior to the terrorist attacks of September 11th.

Panel (e) of Figure 7 graphs the grant rate for the 24 Muslim countries targeted in the government's National Security Entry-Exit Registration System (NSEERS). Initiated in 2002 in response to the September 11th terrorist attacks, the NSSERS program required non-citizens from certain countries to register in person with the government to be photographed, finger-printed and interviewed. The 24 Muslim countries targeted by NSEERS are listed below:

> Afghanistan, Algeria, Bahrain, Bangladesh, Egypt, Eritrea, Indonesia, Iran, Iraq, Jordan, Kuwait, Lebanon, Libya, Morocco, Oman, Pakistan, Qatar, Saudi Arabia, Somalia, Sudan, Syria, Tunisia, United Arab Emirates, and Yemen.

To the extent that immigration judges were concerned that potential terrorists could use the asylum system as a point of entry into the United States, the events of 9/11 may have an independent effect on asylum grant rates separate from the effects of BIA streamlining. Panel (e) in Figure 7 shows that grant rates for the 24 Muslim NSEERS countries began to fall dramatically beginning in late 2001, early 2002. In the month prior to 9/11, the grant rate for NSEERS countries was over 50% and by the end of 2004, the grant rate had fallen to 35%.

Rather than plot the average grant rate as in Figures 7 and 8, Figures 9 and 10 plot the distribution of grant rates. Each panel in Figures 9 and 10 plots three separate times series representing the grant rate of the 25th, 50th and 75th percentile judge. Generally, the patterns in the individual percentiles mirrors the patterns in the average grant rates. A wide variation in grant rates is apparent in these graphs. For example, Figure 9 panel (a) shows that the 75th percentile judge in 2004 granted around 50-60% of cases, while the 25th percentile judge

granted around 10%. These figures also show that grant rates, particularly for the median and 75th percentiles judges, were lower in 2002 and 2003 compared to the periods immediately preceding 2002. This is suggestive evidence that BIA streamlining affected the decision-making of immigration judges.

1.4.3 Judge and Case Characteristics

Table 4 explores how judge and case characteristics affect asylum grant rates. The table reports coefficients and average partial effects from a random effects probit model. The dependent variable is the binary decision to grant or deny asylum. The independent variables include: (1) judge characteristics including age cohort, gender, prior employment, and whether the judge was appointed during the term of a Democratic or Republican president; (2) case characteristics including whether the case was initially filed as an affirmative or defensive claim, whether the applicant was represented by an attorney, and whether the case was decided in absentia; and (3) dummy variables for immigration court, year, and applicant's country of origin. The estimation is based on 174,294 observations resulting from limiting the working dataset to cases that reached a merits hearing.

The signs of the coefficients are generally plausible. For example, a judge who is younger, female, or appointed during the term of a Democratic president is relatively more likely to grant asylum than a judge who is older, male, or appointed during the term of a Republican president. Moreover, applicants who are represented by an attorney are more likely to win asylum, while applicants whose claims are filed defensively have a lower chance of winning asylum. A natural presumption regarding the effect of prior experience as an employee of DHS and its predecessor INS, whose primary function is prosecution and enforcement, is that a judge would be less sympathetic to an applicant and less likely to grant asylum. Surprisingly, however, the sign of the coefficient on prior employment with INS-DHS is positive. Nevertheless, the magnitude of the coefficient is small and is statistically insignificant.

The average partial effect reported in the right hand column of Table 4 is the marginal effect of a particular case or judge characteristic on the probability that an applicant wins asylum, holding all other characteristics constant. For example, relative to their male counterparts, female judges who are otherwise observationally identical are, on average, approximately 10%

more likely to grant asylum. A likelihood ratio test of joint significance indicates that taken together, the judge characteristics in the model are statistically significant, with a p-value on the order of magnitude of minus ten. Although the judge characteristics are jointly statistically significant, they explain a relatively small percentage of the variation in the probability of granting asylum. The pseudo R-squared is 0.2567 for the restricted model excluding judge characteristics, and 0.2570 for the unrestricted model including judge characteristics. In sum, while statistically significant, the observable judge characteristics lack strong predictive value.

1.5 Empirical Methodology and Results

The goal of this paper is to examine whether or not changes at the BIA influenced the decision making of lower court immigration judges. In March 2002, the appellate procedure for asylum cases was altered from a default three-member review with written opinion to a default procedure of single member review without written opinion. In addition, in March 2003, the number of BIA members was downsized from 23 members to 11. There is evidence to suggest that this downsizing was a purge of the more pro-asylum BIA members. Other than BIA streamlining, we are not aware of any major procedural or substantive changes in asylum law.

Immigration cases, at least within a given immigration court, are randomly assigned across judges. This is important since it allows us to make a meaningful comparison of grant rates across judges, without having grant rates driven solely by differences in the characteristics of a judge's underlying case load. We also observe the identity of each individual judge and the decisions made in each case. The decision in asylum cases, a simple grant or deny, is easily observed and quantifiable. Overall, the judges decide a high volume of cases, and this allows us to examine relatively narrow time periods, for example a 3 month window.

Since single member affirmance without opinion was adopted one year prior to the BIA downsizing, we can potentially observe separate effects of these two distinct changes. Nevertheless, to the extent that immigration judges anticipated the effects of BIA downsizing when it was announced in February 2002, it may be difficult to separate the two effects. In addition, BIA streamlining and downsizing may have signaled to the immigration judges that the Attorney General was adopting a more anti-asylum approach focused on deciding cases quickly.

This overall change in policy may also influence the decision-making of immigration judges. At least, even if we are unable to separately identify the individual components of BIA streamlining from the overall change in policy goals, we will be able to estimate the cumulative effects of BIA streamlining taken as a whole.

Finally, to the extent that the terrorist attacks of 9/11 affected the decision-making of immigration judges, the effects of BIA streamlining may be confounded with changes driven by 9/11. Plausibly, however, 9/11 may have had a differential impact on cases from Muslim countries compared to cases from non-Muslim countries. Looking at the relative time trend in grant rates of Muslim versus non-Muslim countries may be one strategy to separate the effects of streamlining from the effects of 9/11.

Since we are interested primarily in the change in overall grant rates and the change in interjudge disparity resulting from the streamlining procedures, we estimate parameters characterizing the *distribution* of judge effects, rather than the judge effects themselves, using a random-effects probit model. Let x_i be a vector of characteristics for case i. This will typically include dummy variables for whether the claimant had legal representation and whether the claim was brought affirmatively or defensively. Let judge j have a "judge effect" θ_j, which characterizes how lenient or harsh this judge is in deciding asylum claims. Then our model determines that the judge will rule in favor of the claimant if $y^* > 0$, where

$$y^* = x_i\beta + \theta_j + \varepsilon_{ij}$$

where β is a vector of parameters to be estimated, and ε_{ij} is a normally distributed error term. In this specification, a judge with a higher θ_j would be more likely to grant relief to the asylum claimant, while a judge with a lower θ_j would be less likely. Our goal is to understand how these judge effects very over time as a result of the procedural changes.

Most empirical studies of judicial voting behavior use one of two strategies to estimate the judge effects. The most common practice is to proxy for these effects using the characteristics of the judge, such as party of the appointing president, race, gender, and past work experience. While these characteristics are available in the data, there are two problems with this approach. Although interjudge disparities are quite evident in the data, they correlate only weakly with

the variables traditionally used to measure judicial "ideology." The second problem with the proxy variable approach is that the correlation between these variables and judicial propensities may change over time. Therefore, a change in the estimated impact of gender, for example, cannot be conclusively linked to a change in interjudge disparity.

An alternative way to model judicial ideology is to treat the judge effect θ_j as a dummy variable to be estimated for each individual judge. Once estimates $\hat{\theta}_1, \ldots \hat{\theta}_J$ are derived for each judge, a measure of dispersion could be derived from these estimates. The problem with this approach is that measures of dispersion based on the *estimates* of judge fixed effects will generally be upwardly biased measures of the dispersion of the *true* judge effects. The easiest way to see this is to suppose that there were no disparities among the judges, so that $\theta_1 = \cdots = \theta_j$. Then any measure of disparity (such as variance or interquartile range) based on the true parameters must be zero. However, the estimates $\hat{\theta}_1, \ldots \hat{\theta}_J$ will never be precisely equal, so a measure of disparity based on the estimates will be strictly positive.

Instead, we incorporate the judge effects directly into our statistical model, and estimate the parameters that characterize their distribution. Our approach most closely resembles the method employed by Anderson et al. (1999) in their study of the impact of the sentencing guidelines.

Specifically, we assume that $\theta_j \sim N\left(\mu, \sigma^2\right)$ and estimate the mean μ and the standard deviation σ of the judge effects. This results in a random-effects probit specification

$$y^* = x_i\beta + \mu + (\theta_j - \mu) + \varepsilon_{ij}$$

where μ is a constant term and $(\theta_j - \mu)$ is a normally distributed random effect with mean 0 and variance σ^2.

Note that with the estimates of the distributional parameters $\hat{\mu}$ and $\hat{\sigma}$, we can easily estimate the grant rates corresponding to the median judge as well as various quantiles of the judge distribution. For example, since the median judge corresponds to $\theta_j = \mu$, the estimated grant rate for a claimant with characteristics x_i appearing before the median judge would be

$$\hat{g}_{0.5} = \Phi\left(x_i\hat{\beta} + \hat{\mu}\right)$$

The judges at the 25th and 75th percentiles of the distribution would have corresponding grant rates

$$\begin{aligned}\hat{g}_{0.25} &= \Phi\left(x_i\hat{\beta} + \hat{\mu} + \hat{\sigma}\Phi^{-1}(0.25)\right) \\ \hat{g}_{0.75} &= \Phi\left(x_i\hat{\beta} + \hat{\mu} + \hat{\sigma}\Phi^{-1}(0.75)\right)\end{aligned}$$

A simple way to measure interjudge disparity in this model is to use an estimate of the interquartile range of the judges' grant rates

$$\hat{g}_{IQ} = \hat{g}_{0.75} - \hat{g}_{0.25}$$

which measures the change in grant rates between the judge at the 75th percentile of the judge distribution and the judge at the 25th percentile.

In order to estimate the change in grant rates and interjudge disparity over time, we estimate $\hat{\mu}_t$ and $\hat{\sigma}_t$ for each three-month period in the data, where t indexes the time period. We can similarly derive the grant rate quantiles and interquartile range for each period to track the changes in the distribution of judge grant rates over time.

Since we are not only interested in the *absolute* grant rates over time, but also the *changes* in the grant rates, we also estimate a two-period model with correlated random effects. Since the judge effect θ_j is likely to be moderately stable over time, the judge effects in two consecutive time are likely to be highly correlated. Following Anderson et al. (1999), we model the judge effects in consecutive periods as being bivariate normal random variables with $\theta_{jt} \sim N\left(\mu_t, \sigma_t^2\right)$, $\theta_{j,t+1} \sim N\left(\mu_{t+1}, \sigma_{t+1}^2\right)$, and $\text{Corr}\left(\theta_{jt}, \theta_{j,t+1}\right) = \rho_t$, where ρ_t is an additional parameter to be estimated. Because the estimated correlation is in fact very high (greater than 90%) in most periods, the standard errors on the estimates of the *changes* in grant rates and interquartile range are typically smaller than the standard errors on the same estimates derived from the one-period model above.

Figures 11a and 11b report the results of the estimation. The estimates are derived from the random-effects probit model, estimated in six-month periods, where the random effect models the heterogeneity in judge-specific grant rates. The estimation controls for region of origin,

office of adjudication, and claimant-specific characteristics. The results are based on the time period from 1997–2004 since the years between 1994 and 1996 experienced significant hiring of new immigration judges. Moreover, the results control for the changing composition of the immigration judges by excluding all judges who were not active for the entire period from 1997 – 2004. The estimates are based on 102,433 adjudications by 110 judges from the 15 immigration courts with the largest caseloads.

Figure 11a plots the estimated grant rates of the 25th, median and 75th percentile judges. Consistent with the overall patterns observed in the descriptive statistics, Figure 11a shows a rise in overall grant rates prior to 2002 and then a reversal of that trend beginning in 2002 as grant rates began to fall. These patterns are observed at various points in the distribution of grant rates. Figure 11b plots the estimated interquartile range, bounded by a 95 percent confidence interval. It shows that the disparity in asylum grant rates was increasing prior to 2002 and that this trend reversed in early 2002 as the disparity began to fall. Figures 12 and 13 present the change in the predicted grant rates of the 25th, median, and 75th percentile judge, as well as the change in the interquartile range. They are generally consistent with Figure 11, showing that grant rates were falling coincident with BIA streamlining.

1.6 Conclusion

In 2002 and 2003, the Bureau of Immigration Appeals instituted dramatic changes to its appellate procedure and member composition. In March 2002, the default appellate procedure for asylum cases was changed from three member panels to single member review, and from decisions with written opinions to affirmances without opinion. In addition, in March 2003, the number of BIA members was downsized from 23 to 11, and there is evidence to suggest that the members who were dismissed were relatively more pro-asylum. The goal of this paper has been to examine what effects these changes at the BIA had on the decision-making of the lower court immigration judges.

Our results suggest that a change in the trend of asylum grants in early 2002, coincident with the timing of BIA streamlining. During this time, the level of asylum grant rates decreased as did the overall disparity in grant rates. This change in the grant rate cannot be attributed solely to changes in country conditions, changes in the composition of immigration judges, nor to the effects of September 11th.

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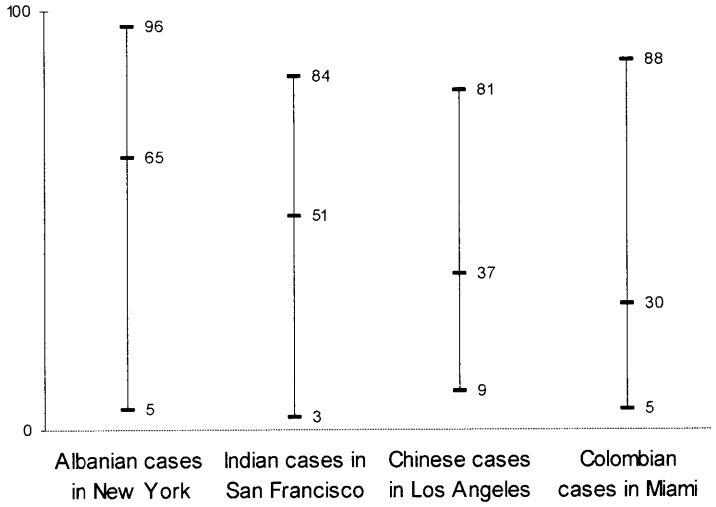
Figure 1. Inter-judge Disparity in Asylum Grant Rates (Mean and Range)



Note: This figure is adapted from Ramji-Nogales et al. (2007), Figures 25-28. It plots the mean, maximum, and minimum grant rates for immigration judges in the four largest immigration courts, deciding cases from countries with the largest number of filings. The figure is based on judges who decided at least fifty cases from the country in question between January 2000 and August 2004.

Figure 2. Flow Chart of Asylum Process

DHS-USCIS: Department of Homeland Security, United States Immigration and Citizenship Services
DHS-ICE: Department of Homeland Security, Immigration and Customs Enforcement
DOJ-EOIR: Department of Justice, Executive Office of Immigration Review

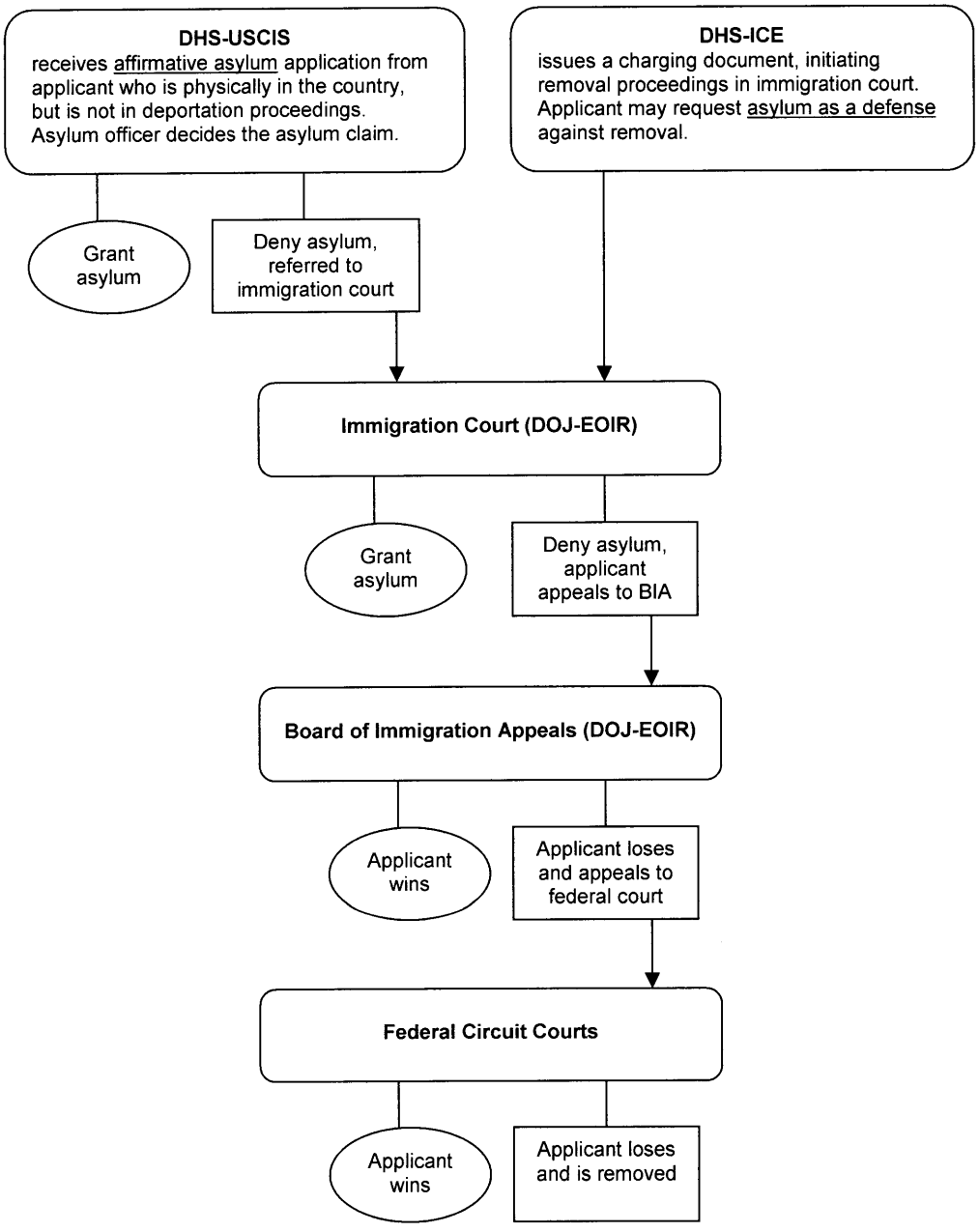


Figure 3a. Timeline of BIA Streamlining Regulations

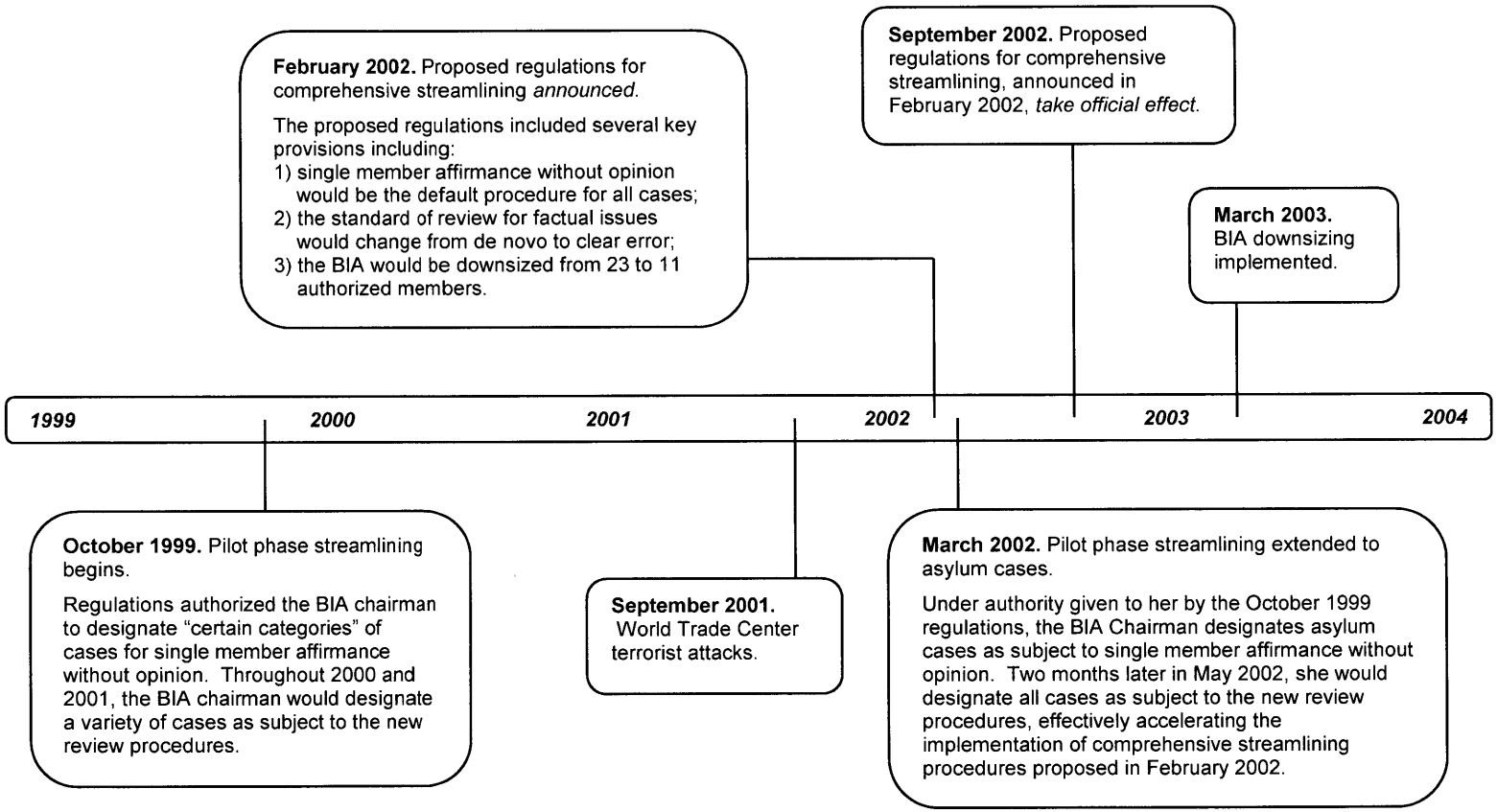


Figure 3b. Timeline of BIA Streamlining Regulations as Applied to Asylum Appeals

Announcement	Implementation	From	To
October 1999	March 2002	Three member review	Single member review
		Written opinion	Affirmance without opinion
February 2002	September 2002	De novo factual review	Clear error factual review
February 2002	March 2003	23 BIA members	11 BIA members (liberal purge)

Figure 4a. Total Number of Asylum Filings, Decisions and Grants, by Year

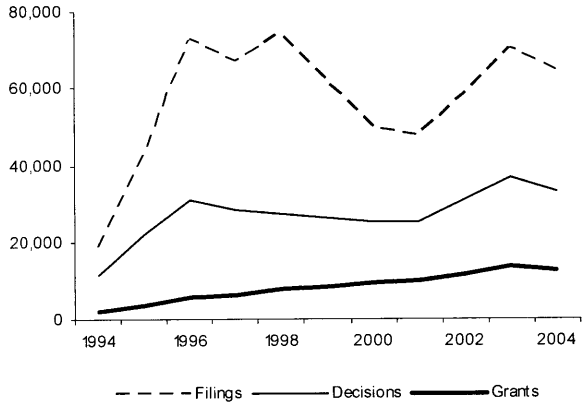


Figure 4b. Asylum Grant Rate, by Year

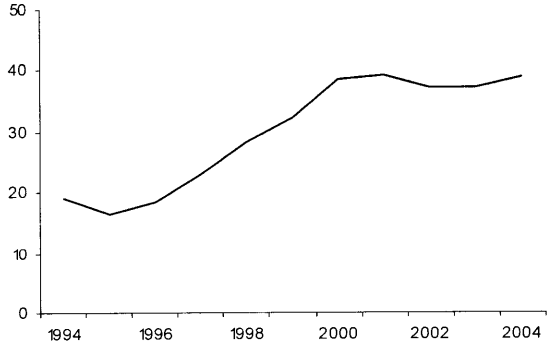


Table 1a. Immigration Judge Entry and Exit

Year of Entry/Exit	Judges freq	Judges %	Judges cum%	Decisions freq	Decisions %	Decisions cum%
entry prior to 1994, no exit	55	25.0	25.0	68,500	31.2	31.2
entry in 1994, no exit	14	6.4	31.4	18,912	8.6	39.8
entry in 1995, no exit	49	22.3	53.6	64,620	29.4	69.3
entry in 1996, no exit	7	3.2	56.8	6,516	3.0	72.2
entry in 1997, no exit	23	10.5	67.3	22,365	10.2	82.4
entry in 1998, no exit	9	4.1	71.4	9,609	4.4	86.8
entry in 1999, no exit	4	1.8	73.2	2,332	1.1	87.9
entry in 2000, no exit	2	0.9	74.1	1,038	0.5	88.3
entry in 2001, no exit	4	1.8	75.9	2,493	1.1	89.5
entry in 2002, no exit	9	4.1	80.0	2,958	1.3	90.8
entry in 2003, no exit	6	2.7	82.7	1,067	0.5	91.3
sporadic	7	3.2	85.9	1,333	0.6	91.9
exit prior to 2004, various entry	31	14.1	100.0	17,753	8.1	100.0
TOTAL	220	100.0		219,496	100.0	

Notes: This table shows the entry and exit of judges into and out of the dataset. For example, 55 judges were appointed to the bench prior to 1994 and continued to decide asylum cases through 2004. These 55 judges decided 68,500 cases between 1994 and 2004. Similarly, the table indicates that 14 judges were appointed to the bench in 1994. These 14 judges did not exit the dataset, continuing to decide cases through 2004. Together, the 1994 cohort of judges decided 18,912 cases between 1994 and 2004. Seven judges decided cases sporadically, alternating years of no decisions with years of non-zero decisions. Thirty-one judges exited the dataset prior to 2004. These judges entered the dataset at various times.

Table 1b. Immigration Judge Entry, Exit and Net Change

Year	Total Number of Judges	Entry A	Entry B	Exit B	Net Change	Total Number of Decisions	Decisions Per Judge	% Change in Decisions Per Judge
1994	89	69	20	0	n/a	8,673	97	n/a
1995	142	49	4	1	53	16,493	116	19
1996	150	7	2	1	8	23,077	154	32
1997	174	23	2	3	24	20,512	118	-23
1998	180	9	0	2	6	19,821	110	-7
1999	182	4	0	5	2	19,964	110	0
2000	179	2	0	3	-3	19,361	108	-1
2001	182	4	2	5	3	19,297	106	-2
2002	187	9	1	6	5	22,704	121	15
2003	187	6	0	5	0	26,026	139	15
2004	182	0	0	0	-5	23,568	129	-7
TOTAL		182	31	31		219,496	131	

Notes: This table shows, for each year from 1994 through 2004, the number of judges that entered and exited the dataset. The column labeled "Entry A" refers to the 182 judges that did not exit the dataset once they entered. The columns "Entry B" and "Exit B" refer to the 31 judges that exited the dataset prior to 2004. This table does not include the seven judges that decided cases sporadically. The "Total Number of Judges" in year n is calculated as follows: $TOTAL_n = TOTAL_{n-1} + EntryA_n + EntryB_n - ExitB_{n-1}$.

Figure 5. Percent of Filings by Immigration Court

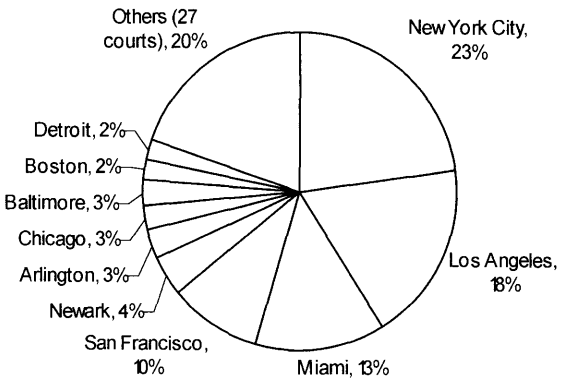


Table 2. Top Fifteen Countries (ranked by the number of filings)

	Country	Number of Filings	Number of Decisions	Percent Decided	Number of Grants	Grant Rate	Filings as a % of all	Decisions as a % of all	Grants as a % of all
1	China	61,557	43,722	71	17,035	39	14	20	25
2	Mexico	38,821	5,113	13	278	5	9	2	0
3	Guatemala	37,929	15,664	41	1,436	9	9	7	2
4	El Salvador	35,473	12,943	36	753	6	8	6	1
5	Haiti	29,903	18,592	62	2,791	15	7	8	4
6	India	19,805	10,841	55	3,598	33	4	5	5
7	Colombia	13,192	8,032	61	2,445	30	3	4	4
8	Honduras	10,790	3,539	33	302	9	2	2	0
9	Nicaragua	10,510	2,929	28	427	15	2	1	1
10	Pakistan	8,867	4,447	50	1,108	25	2	2	2
11	Cuba	7,772	2,518	32	960	38	2	1	1
12	Albania	7,487	5,188	69	2,383	46	2	2	4
13	Russia	7,346	3,800	52	2,091	55	2	2	3
14	Peru	6,636	3,022	46	884	29	1	1	1
15	Somalia	6,621	4,195	63	2,149	51	1	2	3
	All Top Fifteen	302,709	144,545	48	38,640	27	68	66	58
	All Countries	442,437	219,496	50	67,112	31	100	100	100

Figure 6. Grant Rates for Top Fifteen Countries (ranked by the number of filings)

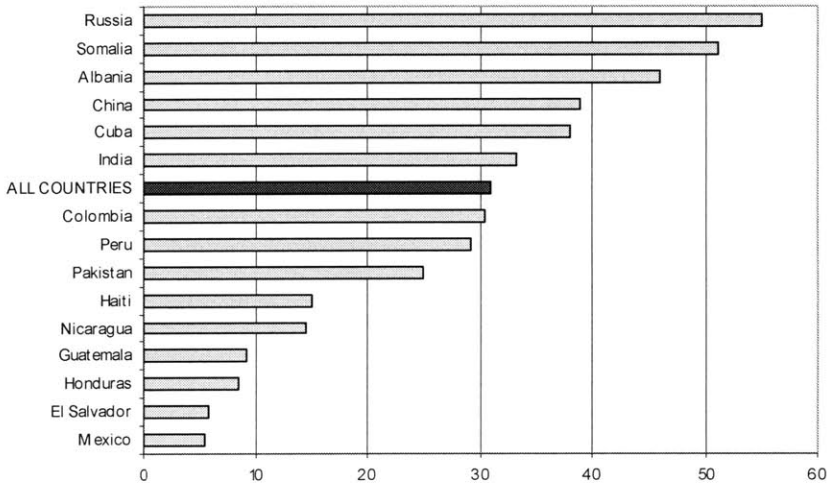


Table 3a. Distribution of Countries *Across* Immigration Courts

Row	Country	New York	Los Angeles	Miami	San Francisco	Others	TOTAL
0	Other	25	14	3	9	49	100
1	China	**66**	10	1	3	20	100
2	Mexico	1	**59**	5	19	16	100
3	Guatemala	4	32	13	8	**43**	100
4	El Salvador	12	26	6	12	**44**	100
5	Haiti	5	0	**76**	0	19	100
6	India	20	8	1	**52**	20	100
7	Colombia	6	4	**55**	1	34	100
8	Honduras	8	29	17	5	**42**	100
9	Nicaragua	2	20	**54**	7	17	100
10	Pakistan	**43**	12	2	8	36	100
11	Cuba	1	2	**62**	2	33	100
12	Albania	47	0	4	0	**48**	100
	Russia	**40**	14	3	7	36	100
14	Peru	5	24	**34**	12	25	100
15	Somalia	1	7	2	7	**83**	100

Notes: This table is read horizontally, across the rows. For example, row 1 shows how asylum filings from China are distributed across the four largest immigration courts--New York, Los Angeles, Miami and San Francisco--and the other courts combined. Sixty-six percent of filings from China are filed in New York City immigration court. Similarly, row 6 shows that 52% of asylum filings from India are filed in San Francisco immigration court.

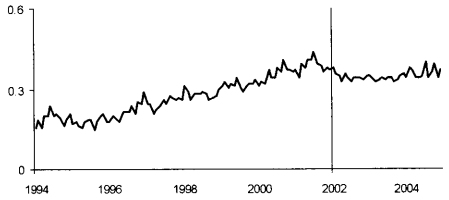
Table 3b. Country Composition *Within* Immigration Courts

Row	Country	New York	Los Angeles	Miami	San Francisco	Others
0	Other	35	24	8	28	43
1	China	**41**	7	1	5	8
2	Mexico	1	**28**	3	17	4
3	Guatemala	2	15	8	7	10
4	El Salvador	4	11	3	10	10
5	Haiti	1	0	**38**	0	4
6	India	4	2	0	**24**	3
7	Colombia	1	1	12	0	3
8	Honduras	1	4	3	1	3
9	Nicaragua	0	3	10	2	1
10	Pakistan	4	1	0	2	2
11	Cuba	0	0	8	0	2
12	Albania	4	0	1	0	2
13	Russia	3	1	0	1	2
14	Peru	0	2	4	2	1
15	Somalia	0	1	0	1	3
	TOTAL	100	100	100	100	100

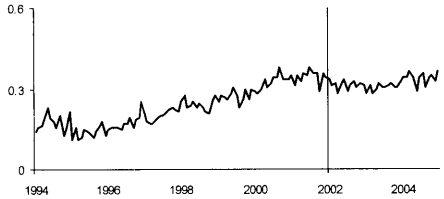
Notes: This table is read vertically, down the columns. For example, the column labeled "New York" shows the country composition of cases filed in the New York City immigration court. Asylum cases from China constitute 41% of all cases filed in New York. Similarly, asylum cases from India constitute 24% of all cases filed in San Francisco.

Figure 7. Monthly Asylum Grant Rates, by Country

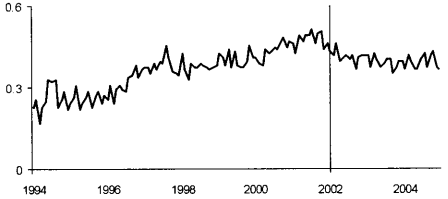
(a) All Countries, All Courts



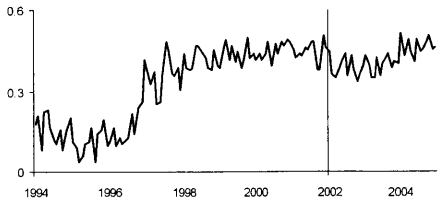
(b) Top Fifteen Countries



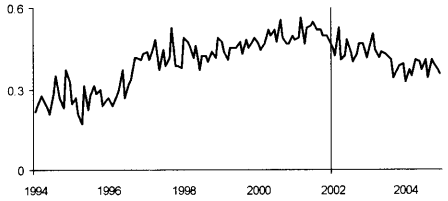
(c) Non-Top-Fifteen Countries



(d) China



(e) NSEERS Countries



(f) Non-China, Non-NSEERS Countries

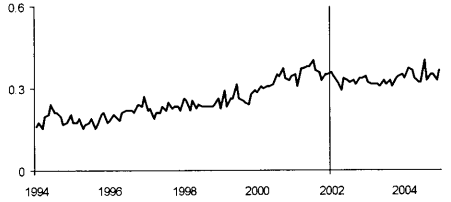


Figure 8. Monthly Asylum Grant Rates, by Court

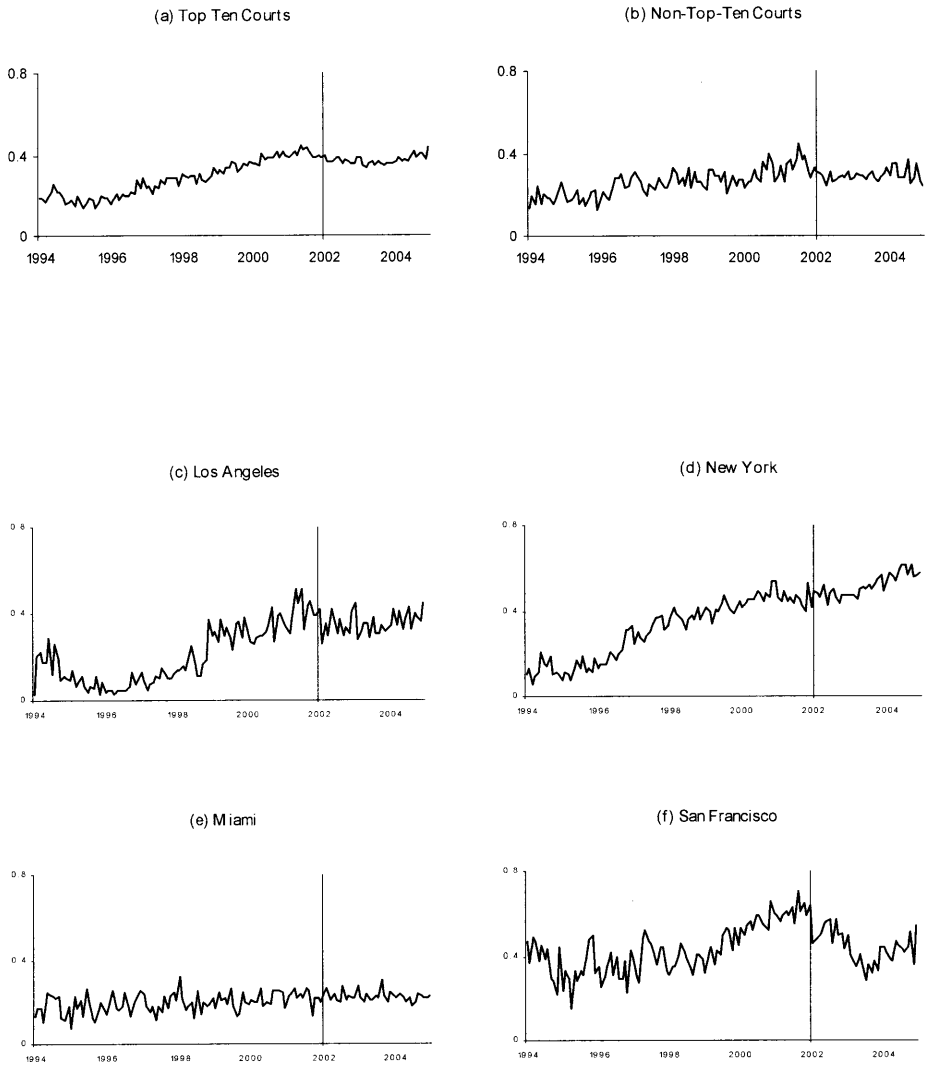
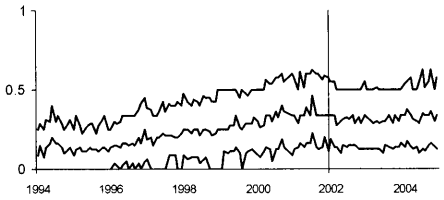
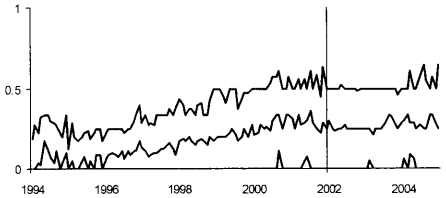


Figure 9. Distribution of Monthly Asylum Grant Rates, by Country
Each panel below graphs the grant rate of the 25th, median, and 75th percentile judge

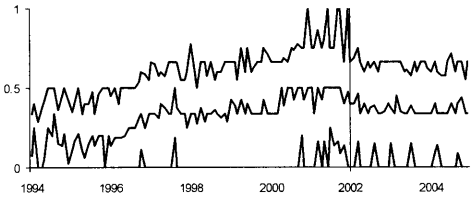
(a) All Countries, All Courts



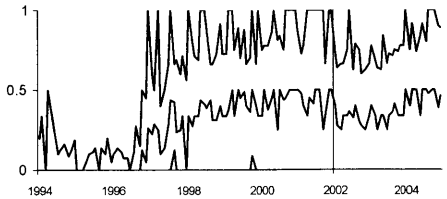
(b) Top Fifteen Countries



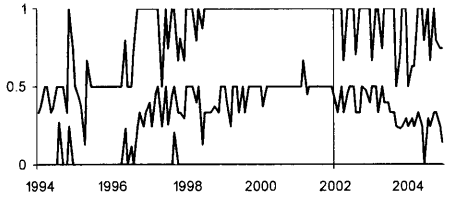
(c) Non-Top-Fifteen Countries



(d) China



(e) NSEERS Countries



(f) Non-China, Non-NSEERS Countries

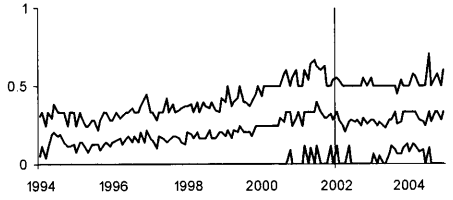


Figure 10. Distribution of Monthly Asylum Grant Rates, by Court
Each panel below graphs the grant rate of the 25th, median, and 75th percentile judge

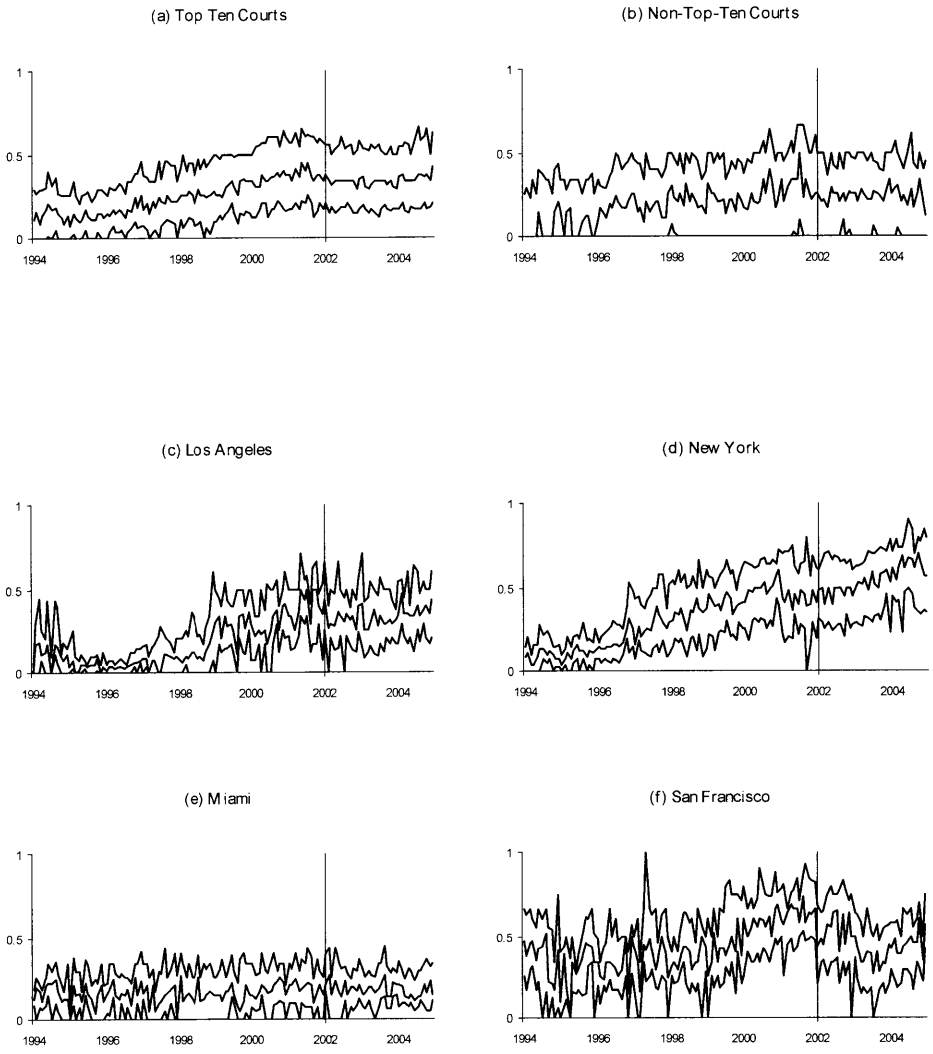


Table 4. Effects of Judge and Case Characteristics on Asylum Grant Rates

Judge and Case Characteristics	Coefficients		Average Partial Effects	
	estimate	SE	estimate	SE
Year of JD minus 1942	-0.008	0.001	-0.004	0.000
Female	0.360	0.010	0.097	0.003
INS-DHS employment	0.015	0.010	0.004	0.003
Military employment	-0.207	0.015	-0.053	0.004
NGO employment	0.038	0.012	0.010	0.003
Private practice employment	0.112	0.010	0.029	0.003
Government employment	-0.125	0.010	-0.033	0.003
Academic employment	0.185	0.013	0.049	0.004
Democratic President	0.117	0.011	0.030	0.003
Defensive	-0.119	0.009	-0.031	0.002
Represented	0.601	0.017	0.143	0.004
Absentia	-2.307	0.042	-0.324	0.002

This table reports coefficients and average partial effects from a random effects probit model. The dependent variable is the binary decision to grant or deny. The independent variables include: (1) judge characteristics, (2) case characteristics, and (3) dummy variables for court, year, and applicant's country of origin. The average partial effect is the average marginal effect of a particular case or judge characteristic on the probability that an applicant wins asylum, holding all other characteristics constant. For example, relative to their male counterparts, female judges who are otherwise observationally identical are, on average, approximately 10% more likely to grant asylum.

Figure 11a. Estimated Grant Rates, by Percentile
25th, median, and 75th percentiles

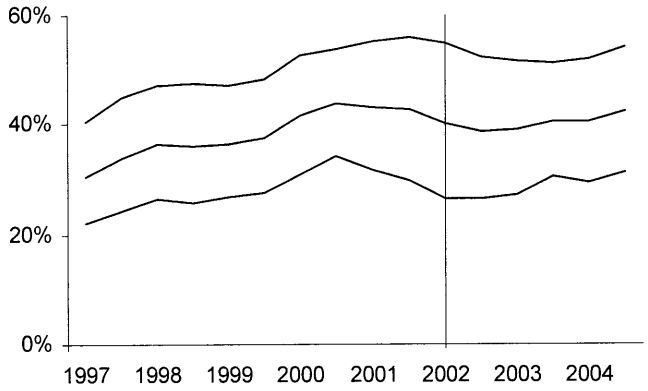


Figure 11b. Estimated Interquartile Range in Grant Rates (with 95% Confidence Intervals)

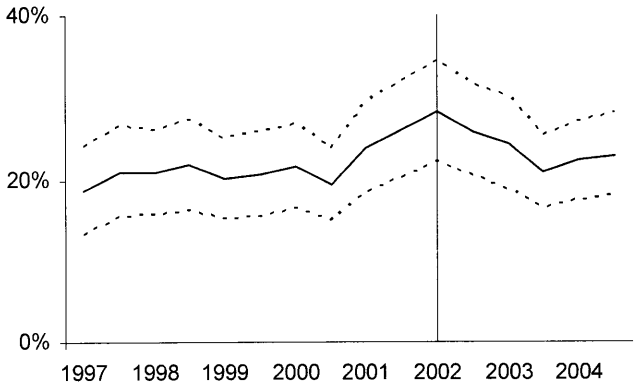
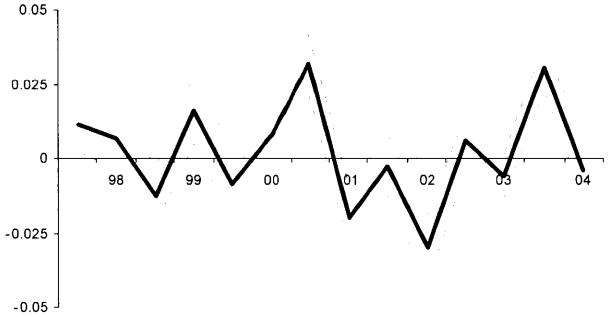
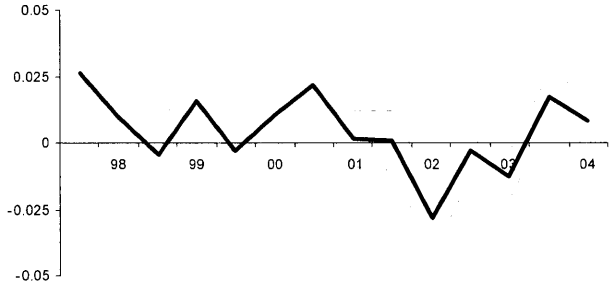


Figure 12. Change in Predicted Grant Rate of X-percentile Judge, with 95% Confidence Interval

(a) 25th percentile judge



(b) median judge



(c) 75th percentile judge

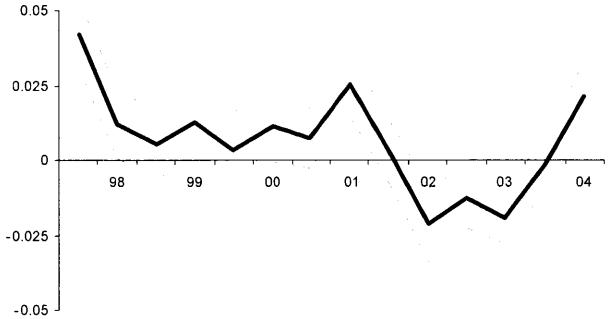
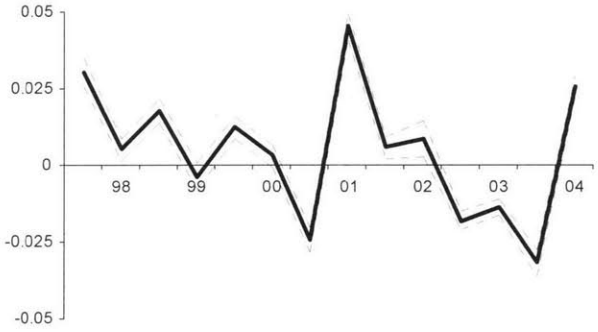


Figure 13. Change in predicted interquartile range, with 95% confidence interval



Chapter 2

The Politics of Judging: Evidence from Immigration Courts

Abstract

This paper explores how career concerns influence judicial decision-making within the context of asylum adjudication in immigration courts. Beginning in 2002, Attorney General John Ashcroft implemented a series of "streamlining" reforms at the Bureau of Immigration Appeals (BIA) that touched off a period of increasing politicization marked by hostility towards BIA members and immigration judges with liberal, pro-immigrant stances. Among the BIA reforms, Attorney General Ashcroft reassigned and demoted the most liberal members of the BIA and signaled to lower-court immigration judges that they too could face adverse employment consequences if they did not decide cases consistent with the new, more conservative approach. Based on data containing the universe of asylum decisions between 1994 and 2004 and obtained from a Freedom of Information Act (FOIA) request, this paper provides evidence that relative to their conservative colleagues, liberal immigration judges, in the face of political pressure and threats to their job security, moderated the rate at which they granted asylum applications following the BIA streamlining reforms in 2002.

2.1 Introduction

Beginning in March 2002, Attorney General John Ashcroft instituted a series of "streamlining" reforms at the Bureau of Immigration Appeals (BIA), the administrative body that adjudicates appeals of asylum decisions made by lower-court immigration judges. Among those reforms, he downsized the BIA from 23 to 11 members. He targeted for removal from the BIA the members that held liberal, pro-immigrant stances. Those members were in some instances demoted to less desirable positions within the immigration bureaucracy. Simultaneously, Attorney General Ashcroft signaled to lower court immigration judges that they too could face adverse consequences based on their liberal decision-making.

These events provide an opportunity to study the impact that politics has on judicial decision-making. Using a unique dataset that would not be publicly available but for a Freedom of Information Act (FOIA) request, this paper examines whether "liberal" immigration judges, relative to their more conservative colleagues, moderated the rate at which they granted asylum applications in response to the increased politicization of the immigration courts. It finds that indeed, liberal judges, concerned about maintaining their jobs, reacted to BIA streamlining and increased politicization by reducing the number of asylum applications they granted.

This paper contributes to the long-standing debate over the proper balance between political oversight and judicial independence. Immigration judges are administrative adjudicators who, on the one hand, serve to carry out executive branch priorities in the enforcement of statutes, and on the other hand, are expected to follow and apply the law with an even hand.

2.2 Background

An extensive law and economics literature has explored the incentives and motives that drive judicial decision-making. At one end of the spectrum is the idea of the "independent" judge, one that interprets, applies, and follows the law, often through an ideological lens. At the other end of the spectrum is the idea of a "political" judge whose decision-making is driven not by the judge's own ideology, but by the underlying preferences of the constituency that selects, retains, and monitors them. Rather than vote their underlying beliefs, these judges, driven by career concerns to keep their jobs, vote based on political or partisan concerns.

The literature on independent judges has generally focused on federal Article III judges with life tenure. Particular emphasis has been placed on U.S. Supreme Court justices and circuit court appellate judges. Many such studies have come from the political science literature spawned from the "attitudinal" model developed in Segal and Spaeth (1993) (see e.g., McCubbins, Noll, and Weingast (1995); Cross and Tiller (1998); Sunstein, Schkade, and Ellman (2004)). These studies examine the relationship between a judge's ideology, generally proxied by the political party of the president who appointed the judge, and how a judge votes in a broad range of cases including employment discrimination and criminal appeals.

In contrast to the literature on independent judges, the literature on political judges has focused on state court judges who typically serve without life tenure and are often selected and retained in partisan elections. These studies exploit cross-state variation in judicial selection methods to study how judicial voting varies depending on whether judges are appointed or elected (see e.g., Helland and Tabarrok (2002); Huber and Gordon (2004); Shepard (2007)). They find that the method of selection affects judicial decision making, with more political pressure for judges facing partisan elections. A recent paper by Choi, Gulati and Posner (2007), however, questions whether appointed judges are truly more independent than elected judges.

The tension between judicial independence on the one hand and political accountability on the other is perhaps greatest in the context of adjudication of cases within an executive branch agency. Margaret Taylor (2007) has summarized this tension as follows:

> Adjudication of [executive agency cases] rests on a premise that is inconsistent with the norm of judicial independence embodied in our Article III courts. In most administrative contexts, the adjudicators—those individuals who decide whether to grant or deny a benefit, or to impose a civil penalty under a particular statute—are employees of the very agency whose caseload they adjudicate. They are, in other words, potentially subject to the supervision and control of one of the interested parties. This is because administrative adjudication, traditionally conceived, is not simply about deciding individual cases; it is a means to effectuate the statutes enacted by Congress in accordance with the priorities of the executive branch.

One type of agency adjudicator, the administrative law judge, enjoys a particularly high degree of judicial independence. These are judges employed in a number of federal executive branch agencies and serve according to provisions of the Administrative Procedure Act. Under the Act, administrative law judges (ALJs) are insulated from career pressures since employing agencies have little control over the hiring, firing, and managerial supervision of ALJs. An agency that employs ALJs cannot hire candidates directly, but rather, candidates are screened through procedures established by the Office of Personnel Management and selected according to rigid statutory criteria. Once hired, an ALJ cannot be supervised by anyone who performs investigative or prosecuting functions within the agency, they are exempt from annual performance reviews, and they can be removed only for "good cause" after a hearing before the Merit Systems Protection Board. While these measures provide ALJs with a certain degree of judicial independence, an elaborate, multi-tiered system of appellate review within the agency allows for a certain degree of political accountability. Jerry Mashaw's 1983 work, Bureaucratic Justice, which examines ALJs deciding Social Security disability claims, remains a classic for scholars studying administrative law.

Immigration judges, while they also serve in the executive branch and function as agency adjudicators, do not serve under the Administrative Procedure Act and do not enjoy the same degree of independence as ALJs. Until 1983, immigration judges were employed by the Immigration and Naturalization Service, the same agency that had enforcement and prosecution functions. In January 1983, adjudication and enforcement functions were separated when immigration judges were moved to the Executive Office of Immigration Review (EOIR) within the Department of Justice. See Rawitz (1988). This gave immigration judges a greater degree of judicial independence, but today, because immigration judges ultimately report to the Attorney General, scholars, lawyers, and immigration judges themselves have repeatedly called for the creation of an immigration court separate from the Department of Justice. See Ramji-Nogales et al. (2007); National Association of Immigration Judges (2002).

Calls for reforms to enhance judicial independence have intensified in recent years in response to the perceived politicization of immigration courts under the stewardship of Attorney General John Ashcroft. Beginning in March 2002, Attorney General Ashcroft announced a series of measures that altered the size, composition, and operations of the Bureau of Immigration

Appeals (BIA), the administrative review body that decides appeals of immigration judge decisions. In addition to implementing "streamlining" measures that replaced review by a three-member panel with review by a single member, Ashcroft also reduced the size of the BIA from 23 to 11 members. Levinson (2004) provides empirical evidence that BIA members who were involuntarily removed and reassigned to lower-level immigration judge positions or to non-adjudicative staff positions were those members who were more liberal and pro-immigrant.

In the published final rule that announced the BIA streamlining measures (67 Fed. Reg. at 54893 (Aug. 26, 2002)), Ashcroft responded to concerns that the selective reassignments of BIA members might be perceived as a way for the Attorney General to eliminate liberal members with whom he disagreed. Ashcroft explained:

> Each Board member is a Department of Justice attorney who is appointed by, and may be removed or reassigned by, the Attorney General. All attorneys in the Department are excepted employees, subject to removal by the Attorney General, and may be transferred from and to assignments as necessary to fulfill the Department's mission.

As noted in Legomsky (2006), the Attorney General's reference to "all attorneys" and not just BIA members, coupled with his explicit claim of power not only to reassign but to remove, sent a signal to immigration judges that they too could face adverse employment actions. Significantly, Ashcroft's statement concerning removal contained no qualifying language that would require a finding of misconduct. Indeed, the Attorney General's streamlining efforts at the BIA and his reassignment of liberal BIA members foreshadowed what some have characterized as a politicization of the lower immigration courts. Beginning in 2003 and continuing through 2004, the Attorney General began to take a more direct role in the hiring of immigration judges, a function that the Attorney General had traditionally delegated to EOIR and the Chief Immigration Judge. As described in a 2008 report by the Office of the Inspector General, the Attorney General began to make direct appointments occurring outside of EOIR's normal interview and selection process, and these direct appointments were given to candidates with little relevant immigration experience, but strong political ties.

These events provide an opportunity to empirically test whether career concerns influence judge decision making. The BIA streamlining measures, along with the allegedly more politicized atmosphere against which streamlining took place, provide a focal event to test whether immigration judges are "political" judges with career concerns. The hypothesis of this paper is that relative to their "conservative" colleagues, "liberal" judges—whose pro-immigrant views under Attorney General Ashcroft were allegedly disfavored and whose jobs were allegedly more at risk—reacted in a different way to BIA streamlining reforms. In particular, the paper posits that asylum grant rates of liberal judges declined more than grant rates of their conservative colleagues following BIA streamlining measures beginning in March 2002. This paper not only provides an examination of the effects of Attorney General Ashcroft's immigration court reforms, but also contributes to the broader literature contrasting independent and political conceptions of judicial decision-making.

2.3 Data Description

The original dataset contains the universe of over 600,000 asylum cases filed in immigration courts between 1994 and 2004. It contains information on the decision in each case, the identity of the judge, the court where the case was heard, the date the case was decided, and the country of origin of the asylum applicant. Additional information includes whether the applicant was represented by an attorney, whether the case was originally filed as an affirmative or defensive claim, and whether the case was decided in absentia. Neither the age nor the gender of the asylum applicant is included in the data.[1]

The data were obtained through a Freedom of Information Act (FOIA) request initiated by the journalist Frederic Tulsky. His analysis of the data was the basis of an article published in

[1] Comparisons of the annual number of grants reported in the EOIR 2005 Statistical Yearbook (table 16, page 43) with the number of grants calculated from the FOIA dataset are as follows:

Year	Yearbook	FOIA Dataset	% difference
2001	10,001	9,944	0.57
2002	10,977	11,471	-4.50
2003	13,376	13,572	-1.47
2004	13,015	12,797	1.67

The minor discrepancies may be due to the fact that the Statistical Yearbook numbers are based on a fiscal year beginning in October, whereas the calculations generated from the FOIA dataset are based on the calendar year.

2000 by the San Jose Mercury News. A subsequent FOIA request filed by attorney David Berten of asylumlaw.org updated and extended Tulsky's dataset through the year 2004. I am indebted to both Tulsky and Berten for their efforts in obtaining these data which would otherwise not be publicly available. Comparisons of basic statistics calculated from the FOIA dataset with those published in the EOIR Statistical Yearbooks are generally consistent, and thus, there is little reason to doubt the reliability of the data .

Data on asylum cases was supplemented with demographic information on immigration judges including gender, age cohort, date of appointment, and six categories of employment history including INS/DHS, military, non-governmental organization such as public defender or legal aid society, private law practice, academia, and government service. The demographic variables are constructed using definitions similar to those in Ramji-Nogales et al. (2007) (appendix pages 97-98). The primary source of biographical information is press reports issued by the Executive Office of Immigration Review (EOIR) at the time a judge is appointed. Of the 280 judges appearing in the original asylum dataset, biographical information was available for all but 29 judges.

To form the working dataset upon which the results are based, the original is cleaned in a variety of ways. First, some judge names that had multiple variants were altered to a single, consistent form. Second, some observations were dropped due to missing or incomplete information. Third, judges who decided fewer than 500 cases during the ten year period between 1994 and 2004 were eliminated, as were low volume immigration courts with less than 1,000 cases filed during the decade.

Like Ramji-Nogales et al. (2007), we retained only one observation from the same "family" of cases since decisions in these cases are likely to be correlated. For example, a husband and wife often seek asylum at the same time. Their cases are typically assigned to the same immigration judge who then considers the family's application as a unit. This is similar to the problem of correlated sentences among co-defendants in Abrams et al. (2007) and Anderson et al. (1999). The asylum dataset does not separately identify these "families," but they can be inferred when observations have identical information, i.e. the same country of origin, decision, judge, date, court, legal representation status, and type of claim whether defensive

or affirmative. After identifying these "families," we kept one observation and eliminated the remainder. The working dataset consists of 442,437 observations, or about 70% of the original 629,416 observations.

Currently, there are over 200 judges sitting in over 50 immigration courts across the country. The working dataset consists of 37 courts spread across the country. Los Angeles, New York, Miami and San Francisco are the largest immigration courts, adjudicating 64% of all asylum filings (see Figure 1). The remaining courts are small in comparison. Filings from any particular country are not evenly distributed across courts. Rather, applicants from a given country tend to cluster in certain immigration courts (see Tables 1a and 1b). Differences in the pattern of asylum grant rates across courts, therefore, reflect not only differences in the composition of the judges, but also differences in the composition of cases heard. Turnover among judges was low during the years immediately preceding and following BIA streamlining in 2002 and 2003 (see Tables 2a and 2b), and consequently, changes in asylum grant rates over time are unlikely to be driven by changes in the composition of judges.

Monthly asylum grant rates are depicted in Figure 2. Coincident with the implementation of BIA streamlining, grant rates are lower post-2002 relative to pre-2002. A similar pattern of falling grant rates post-2002 is depicted in panels (a) and (b) of Figure 3, which graph the grant rates for the top ten immigration courts and the non-top-ten courts, respectively. This pattern, however is not uniform across the four largest immigration courts. Panels (c)-(f) of Figure 3 show that while New York and San Francisco experienced falling grant rates post-2002, grant rates in Los Angeles and Miami remained relatively stable. San Francisco experienced the most dramatic fall in grant rates post-2002. Grant rates fell to their lowest level of 28% in March 2003, the month that BIA downsizing was implemented, from a high of 70% in August 2001.

2.4 Empirical Methodology and Results

The hypothesis posed in this paper is that relative to their conservative colleagues, liberal immigration judges reacted in a different way to politically-motivated reforms which began in March 2002 with the dismissal of pro-immigrant members of the Bureau of Immigration Appeals. In particular, the paper posits that the grant rates of liberal judges declined more than

the grant rates of conservative judges following BIA streamlining measures beginning in March 2002. This is because, concerned about their careers and possible dismissal or reassignment, more liberal immigration judges moderated their judicial decision-making to be more in line with a conservative, anti-immigrant Attorney General.

Immigration cases, at least within a given immigration court, are randomly assigned across judges. This random assignment is important since it allows meaningful comparisons across judges, without having grant rants driven solely by differences in the characteristics of a judge's underlying caseload.

In this paper, the "liberalness" of a judge is a measured by the judge's average asylum grant rate in the pre-period before BIA streamlining measures began to be implemented in March 2002. The higher a judge's average grant rate, the more liberal the judge is presumed to be, and therefore, the more likely a judge is to alter her decision-making in response to the streamlining measures and increased hostility toward pro-immigrant judges. Following the literature that studies federal Article III judges, an alternative measure of "liberalness" for immigration judges might be the political party of the president serving at the time the judge was appointed. A priori, however, because immigration judges, unlike Article III judges, are appointed subject to civil service law and policy that explicitly prohibit discrimination based on politics, there is less reason to believe that political party of the president is a good proxy for "liberalness." Moreover, as discussed below, empirical results show that the political party of the president is a statistically insignificant predictor for a judge's propensity to grant asylum.

For all courts, using a long, three-year window around March 2002, Figure 4 plots the raw pre- and post-period grant rates (in black) along with smoothed y-values resulting from a locally weighted regression (in pink) and a 45-degree line (in blue). Table 3 reports the results of a simple linear regression of grant rates in the post-period (the left-hand side variable) against grant rates in the pre-period (the right-hand side variable). Regressions in Panel A use the same three-year window used in Figure 4, while regressions in Panel B use a shorter seven-month window around March 2002.

Differences between liberal and conservative judges, pre- and post- March 2002, are not so dramatic that they can be eyeballed in the scatterplot depicted in Figure 4. All the coefficients

reported in Table 3, however, are less than one, except for New York and Miami. This is consistent with the fact that, as illustrated in Figure 3 which plots monthly grant rates by court, grant rates in New York were actually rising even after March 2002, and in Miami, grant rates appear to be rising very gradually over time. Los Angeles and particularly San Francisco experienced the most dramatic declines post March 2002. These simple regressions provide preliminary evidence to show that at least for judges on some courts, concerns over keeping their jobs in a more anti-immigrant political climate caused more liberal judges to moderate the rate at which they granted asylum applications.

To more formally test whether liberal judges granted fewer asylum applications following the March 2002 reforms, the following regression is used:

$$\begin{aligned} grantrate_{it} &= \alpha + \beta_1 propensity_i + \beta_2 propensity_i * postMarch2002 \\ &\quad + judge_i + month_t + \varepsilon_{it} \end{aligned}$$

In this regression, i indexes individual judges and t indexes time measured in months. The variable *propensity* is a measure of the "liberalness" of a judge, and as already discussed above, is a constant represented by the average grant rate of judge i in the pre-period, i.e. the 38-month period between January 1999 and February 2002. The variable *postMarch2002* is a dummy variable that equals 1 if t is in the 33-month period between April 2002 and December 2004, and equals 0 otherwise. The coefficient of interest is β_2 and its expected sign is negative.

Table 4 reports coefficients from the basic regression model described above, as well as variations on the basic model. Coefficients from the basic model, with and without judge fixed effects, are given in columns 1 and 2. In both columns, the coefficient β_2 is negative as expected and is statistically significant at the 95% level. The negative sign of β_2 indicates that compared to their conservative colleagues, the grant rates of liberal judges declined more after BIA streamlining measures were implemented in March 2002.

The regressions reported in columns 3 through 6 of Table 4 are similar to the basic regressions in columns 1 and 2, except that additional time periods are added. Columns 3 and 4 divide the data into six month increments while columns 5 and 6 divide the data into one year increments. Figure 5 plots of coefficients on the interaction of propensity with time, as well as a 95%

confidence interval. The vertical line marks the month of March 2002 when BIA streamlining measures were first implemented. Although many of the coefficients are not statistically different from zero, the general trend of negative coefficients after March 2002 is consistent with more liberal judges granting less asylum applicants coincident with the timing of BIA streamlining.

Immigration judges vary in the number of asylum applications they decide. Consequently, because the decisions of some judges are observed more frequently than others, the propensity variable that measures "liberalness" may be more accurately measured for some judges than others. To address this issue of heteroskedasticity, Table 5 and Figure 6 replicate the basic regressions presented in Table 4 and Figure 5, respectively, except that the regressions are weighted by the number of observations for each judge in each monthly time period. While the unweighted regressions capture the judge level average treatment effect, the weighted regressions capture the case level treatment effect, that is, the effect on the average immigrant appearing before the court.

Relative to the unweighted regressions, the coefficients in the weighted regressions follow the same general patterns, but the magnitude of the effects is smaller and estimated with less precision. The differences between the unweighted and weighted regressions can be attributed to the fact that the weighted regressions place more weight on Miami and New York, which both experienced increasing grant rates after March 2002, and less weight on San Francisco, whose judges on average decided the fewest number of cases and which saw the most dramatic decreases in asylum grant rates following the BIA streamlining measures (See Figure 3). Table 6 and Figure 7 report regressions for the four largest immigration courts—New York, Los Angeles, Miami and San Francisco. These estimates are generally very imprecise.

The coefficients presented and Tables 4 and 5, particularly those in columns 1 and 2, are not only statistically significant, but they also imply a considerable magnitude of the predicted effect. Tables 7a and 7b illustrate the magnitudes. For example, Table 7b shows that the change in asylum grant rates before and after March 2002 for a conservative judge whose 10 percent grant rate placed her at the 10th percentile, compared to a judge whose 50 percent asylum grant rate placed her in the 90th percentile, is about 6 percentage points (based on column 1 of the unweighted regressions). In 2004, there were nearly 200 immigration judges that decided

nearly 24,000 asylum cases, which translates to an average of 120 decisions per judge per year. The estimates imply that in 2004, 720 cases were decided in the opposite direction as a result of BIA streamlining measures and the allegedly anti-immigrant policies adopted by Attorney General Ashcroft.[2]

Table 8 explores additional relationships based on judge characteristics, interacting the *post-March2002* dummy variable with each of the demographic variables available in the dataset including past employment history, gender, and the political party of the president at the time the immigration judge was appointed (note that immigration judges are formally appointed by the Attorney General, not the president, but the Attorney General has traditionally delegated this appointment authority to EOIR). The results reported in Table 8 confirm the other estimates and bolster the conclusion that liberal judges moderated their asylum grant rates in reaction to increasing political, anti-immigration pressures. The interaction of *propensity* with *postMarch2002* is statistically significant and relatively stable across all specifications and is not sensitive to the inclusion of other judge characteristics. Moreover, the interaction of *post-March2002* with the political party of the president is small in comparison to the interaction of *postMarch2002* with the *propensity* variable and is statistically insignificant. These results indicate that the propensity of a judge to grant or deny asylum is not simply a reflection of a judge's underlying demographic characteristics.

It is unlikely that the above results are driven by simple mean reversion. That is, it unlikely that judges with extreme decision-making, whether pro- or anti-immigrant, are simply moderating their behavior over time. Rather than being a general feature of the data, the results indicate that moderation does not begin until March 2002, coincident with start of BIA streamlining reforms (see columns 3 through 9 of Table 4, as already discussed above). In order to more formally distinguish mean reversion from the cause and effect relationship posited in this paper and suggested by the results above, i.e. that perceived politicization of the immigration courts caused more liberal judges to moderate their asylum grant rates, the following regression

[2] Using the 6 percent difference between the 90th and 10th percentile judges in asylum grant rates before and after March 2002, and assuming there are 100 "conservative," 10th percentile, and 100 "liberal," 90th percentile, judges each deciding 120 cases per year, the annual difference in the absolute number of cases that would have been decided differently as a result of BIA streamlining can be expressed as 100 * 120 * 0.06 = 720. Using the 3.5 percent difference between the 75th and 25th percentile judges yields 420 cases that would have been decided differently.

is used:

$$\begin{aligned} grantrate_{it} &= \alpha + \beta_1 propensity_i \\ +\beta_2 propensity_i * (propensity_i &> median\_grantrate) * postMarch2002 \\ +\beta_3 propensity_i * (propensity_i &\leq median\_grantrate) * postMarch2002 \\ &+ judge_i + month_t + \varepsilon_{it} \end{aligned}$$

In this regression, the variable *median_grantrate* is the grant rate of the median judge, which as shown in Table 7a, is 0.250. If β_3 is positive and β_2 is negative then this would suggest mean reversion rather than political behavior of judges in response to court reforms. Table 9 reports the results from this regression. While β_3 is positive, it is not statistically significant, suggesting that the results are not a statistical artifact driven by mean reversion.

2.5 Conclusion

Exploiting a series of immigration reforms that allegedly signaled to immigration judges that the Attorney General was taking a more conservative, anti-immigrant policy stance, this paper has tested the hypothesis that judges reacted to the changing political climate in order to protect their jobs. Following the liberal "purge" at the BIA, immigration judges, particularly those with liberal, pro-immigrant track records, had good reason to be concerned about possible dismissal or reassignment. This concern was reflected in their judicial decision-making. The results of the paper show that liberal immigration judges, relative to their conservative colleagues, reduced the rate at which they granted asylum applications.

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Table 1a. Distribution of Countries Across Immigration Courts

Row	Country	New York	Los Angeles	Miami	San Francisco	Others	TOTAL
0	Other	25	14	3	9	49	100
1	China	**66**	10	1	3	20	100
2	Mexico	1	**59**	5	19	16	100
3	Guatemala	4	32	13	8	**43**	100
4	El Salvador	12	26	6	12	**44**	100
5	Haiti	5	0	**76**	0	19	100
6	India	20	8	1	**52**	20	100
7	Colombia	6	4	**55**	1	34	100
8	Honduras	8	29	17	5	**42**	100
9	Nicaragua	2	20	**54**	7	17	100
10	Pakistan	**43**	12	2	8	36	100
11	Cuba	1	2	**62**	2	33	100
12	Albania	47	0	4	0	**48**	100
	Russia	**40**	14	3	7	36	100
14	Peru	5	24	**34**	12	25	100
15	Somalia	1	7	2	7	**83**	100

Notes: This table is read horizontally, across the rows. For example, row 1 shows how asylum filings from China are distributed across the four largest immigration courts--New York, Los Angeles, Miami and San Francisco--and the other courts combined. Sixty-six percent of filings from China are filed in New York City immigration court. Similarly, row 6 shows that 52% of asylum filings from India are filed in San Francisco immigration court.

Table 1b. Country Composition Within Immigration Courts

Row	Country	New York	Los Angeles	Miami	San Francisco	Others
0	Other	35	24	8	28	43
1	China	**41**	7	1	5	8
2	Mexico	1	**28**	3	17	4
3	Guatemala	2	15	8	7	10
4	El Salvador	4	11	3	10	10
5	Haiti	1	0	**38**	0	4
6	India	4	2	0	**24**	3
7	Colombia	1	1	12	0	3
8	Honduras	1	4	3	1	3
9	Nicaragua	0	3	10	2	1
10	Pakistan	4	1	0	2	2
11	Cuba	0	0	8	0	2
12	Albania	4	0	1	0	2
13	Russia	3	1	0	1	2
14	Peru	0	2	4	2	1
15	Somalia	0	1	0	1	3
	TOTAL	100	100	100	100	100

Notes: This table is read vertically, down the columns. For example, the column labeled "New York" shows the country composition of cases filed in the New York City immigration court. Asylum cases from China constitute 41% of all cases filed in New York. Similarly, asylum cases from India constitute 24% of all cases filed in San Francisco.

Table 2a. Immigration Judge Entry and Exit

Year of Entry/Exit	Judges freq	Judges %	Judges cum%	Decisions freq	Decisions %	Decisions cum%
entry prior to 1994, no exit	55	25.0	25.0	68,500	31.2	31.2
entry in 1994, no exit	14	6.4	31.4	18,912	8.6	39.8
entry in 1995, no exit	49	22.3	53.6	64,620	29.4	69.3
entry in 1996, no exit	7	3.2	56.8	6,516	3.0	72.2
entry in 1997, no exit	23	10.5	67.3	22,365	10.2	82.4
entry in 1998, no exit	9	4.1	71.4	9,609	4.4	86.8
entry in 1999, no exit	4	1.8	73.2	2,332	1.1	87.9
entry in 2000, no exit	2	0.9	74.1	1,038	0.5	88.3
entry in 2001, no exit	4	1.8	75.9	2,493	1.1	89.5
entry in 2002, no exit	9	4.1	80.0	2,958	1.3	90.8
entry in 2003, no exit	6	2.7	82.7	1,067	0.5	91.3
sporadic	7	3.2	85.9	1,333	0.6	91.9
exit prior to 2004, various entry	31	14.1	100.0	17,753	8.1	100.0
TOTAL	220	100.0		219,496	100.0	

Notes: This table shows the entry and exit of judges into and out of the dataset. For example, 55 judges were appointed to the bench prior to 1994 and continued to decide asylum cases through 2004. These 55 judges decided 68,500 cases between 1994 and 2004. Similarly, the table indicates that 14 judges were appointed to the bench in 1994. These 14 judges did not exit the dataset, continuing to decide cases through 2004. Together, the 1994 cohort of judges decided 18,912 cases between 1994 and 2004. Seven judges decided cases sporadically, alternating between years of no decisions and years with non-zero decisions. Thirty-one judges exited the dataset prior to 2004. These judges entered the dataset at various times.

Table 2b. Immigration Judge Entry, Exit and Net Change

Year	Total Number of Judges	Entry A	Entry B	Exit B	Net Change	Total Number of Decisions	Decisions Per Judge	% Change in Decisions Per Judge
1994	89	69	20	0	n/a	8,673	97	n/a
1995	142	49	4	1	53	16,493	116	19
1996	150	7	2	1	8	23,077	154	32
1997	174	23	2	3	24	20,512	118	-23
1998	180	9	0	2	6	19,821	110	-7
1999	182	4	0	5	2	19,964	110	0
2000	179	2	0	3	-3	19,361	108	-1
2001	182	4	2	5	3	19,297	106	-2
2002	187	9	1	6	5	22,704	121	15
2003	187	6	0	5	0	26,026	139	15
2004	182	0	0	0	-5	23,568	129	-7
TOTAL		182	31	31		219,496	131	

Notes: This table shows, for each year from 1994 through 2004, the number of judges that entered and exited the dataset. The column labeled "Entry A" refers to the 182 judges that did not exit the dataset once they entered. The columns "Entry B" and "Exit B" refer to the 31 judges that exited the dataset prior to 2004. This table does not include the seven judges that decided cases sporadically. The "Total number of judges" in year n is calculated as follows: $TOTAL_n = TOTAL_{n-1} + EntryA_n + EntryB_n - ExitB_{n-1}$.

Figure 2. Asylum Grant Rate, by Year

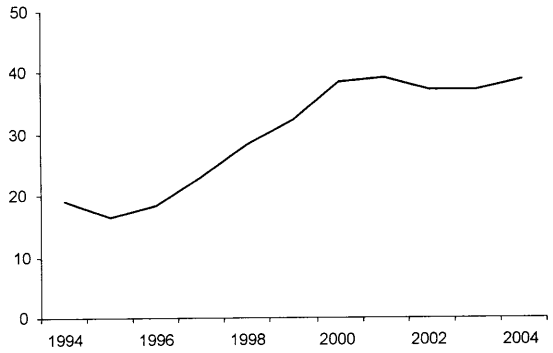


Figure 3. Monthly Asylum Grant Rates, by Court

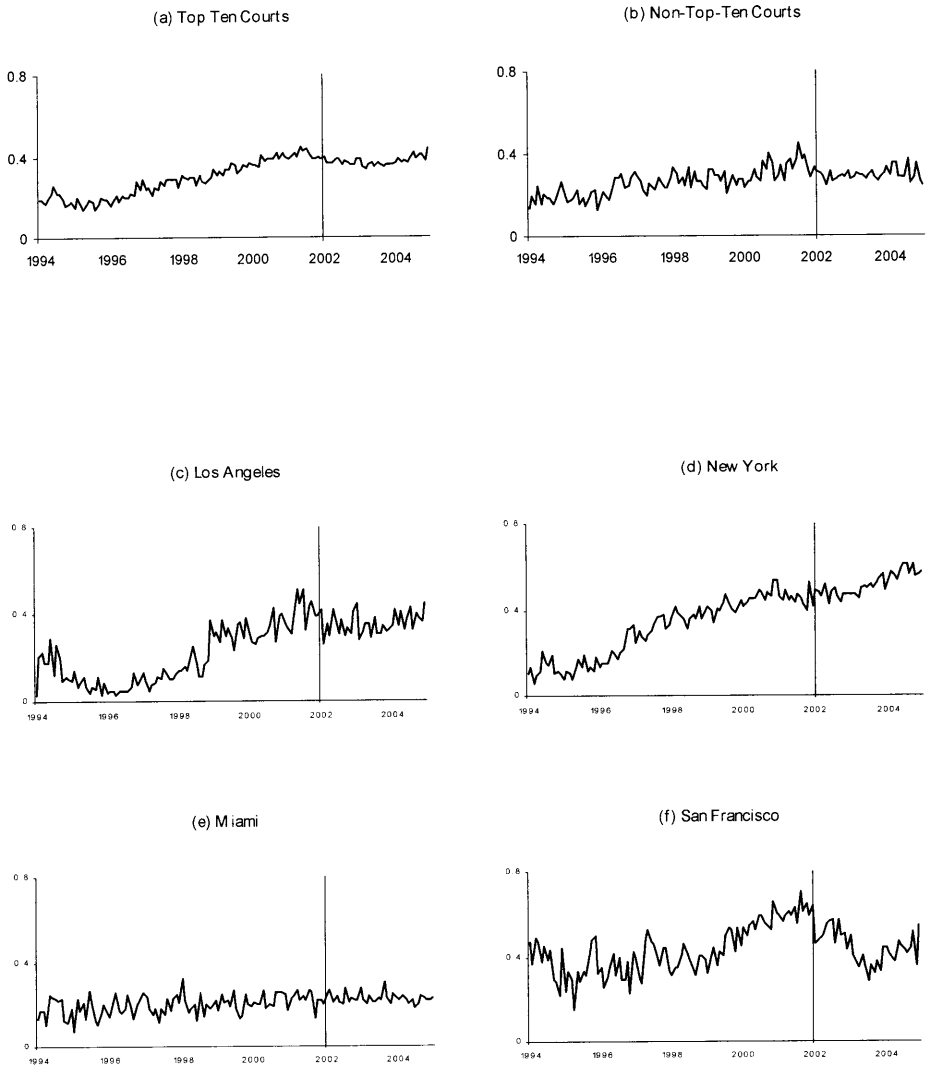


Figure 4. Scatterplot of Individual Judge Grant Rates, Before and After March 2002, With Smoothed y-Values from Locally Weighted Regression

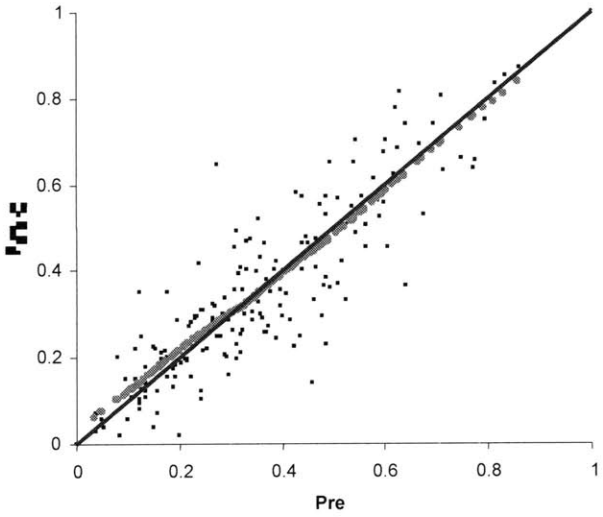


Table 3. Coefficients from a Regression of Grant Rates in the Post-Period Against Grant Rates in the Pre-Period

Court	Panel A Long time period (1/1999 through 12/2004, three-year window)		Panel B Short time period (8/2001 through 10/2002, seven-month window)	
	Coeff.	Std. Dev.	Coeff.	Std. Dev.
All Courts	0.915	(0.039)	0.866	(0.038)
New York	1.026	(0.083)	1.060	(0.081)
San Francisco	0.863	(0.116)	0.937	(0.116)
Los Angeles	0.750	(0.105)	0.567	(0.123)
Miami	1.117	(0.162)	1.045	(0.151)
Other Courts	0.837	(0.063)	0.746	(0.056)

Table 4. Coefficients from Regressions Based on the Model:

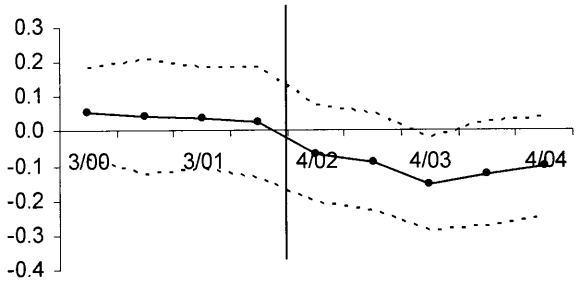
$grantrate_{it} = \alpha + \beta_1\, propensity_i + \beta_2 prosensity_i * postMarch2002 + judge_i + month_t + \varepsilon_{it}$

variable	1	2	3	4	5	6
constant	0.119	0.289	0.060	0.267	0.056	0.267
	(0.024)	(0.021)	(0.025)	(0.035)	(0.023)	(0.020)
propensity	1.056	omitted	1.050	omitted	.0975	omitted
	(0.039)		(0.065)		(0.047)	
propensity * postMar2002	-0.158	-0.103				
	(0.073)	(0.033)				
propensity * Mar2002 - 5			omitted	omitted		
propensity * Mar2002 - 4			0.069	0.053		
			(0.064)	(0.066)		
propensity * Mar2002 - 3			0.058	0.043	omitted	omitted
			(0.080)	(0.083)		
propensity * Mar2002 - 2			0.059	0.036	0.143	0.115
			(0.073)	(0.073)	(0.054)	(0.056)
propensity * Mar2002 - 1			0.047	0.023	0.132	0.099
			(0.079)	(0.080)	(0.059)	(0.054)
propensity * Mar2002 + 1			-0.099	-0.069	-0.340	-0.010
			(0.095)	(0.070)	(0.083)	(0.053)
propensity * Mar2002 + 2			-0.130	-0.090	-0.125	-0.078
			(0.101)	(0.070)	(0.085)	(0.058)
propensity * Mar2002 + 3			-0.215	-0.156		
			(0.098)	(0.068)		
propensity * Mar2002 + 4			-0.189	-0.126		
			(0.102)	(0.075)		
propensity * Mar2002 + 5			-0.152	-0.105		
			(0.102)	(0.072)		
month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
judge fixed effects	No	Yes	No	Yes	No	Yes
number of observations	12,418	12,418	10,330	10,330	10,356	10,356
number of judges (clusters)	199 (398)	199 (398)	198 (396)	198 (396)	199 (398)	199 (398)
time period	1/99 - 12/04	1/99 - 12/04	9/99 - 9/04 (six month increments)	9/99 - 9/04 (six month increments)	3/99 - 3/04 (one year increments)	3/99 - 3/04 (one year increments)

Figure 5. Coefficients (with 95% Confidence Interval) from Regressions Based on the Model:

$grantrate_{it} = \alpha + \beta_1\, propensity_i + \beta_2 prosensity_i\ *\ postMarch2002 + judge_i + month_t + \varepsilon_{it}$

Six-month intervals (Table 2, column 4)



One-year intervals (Table 2, column 6)

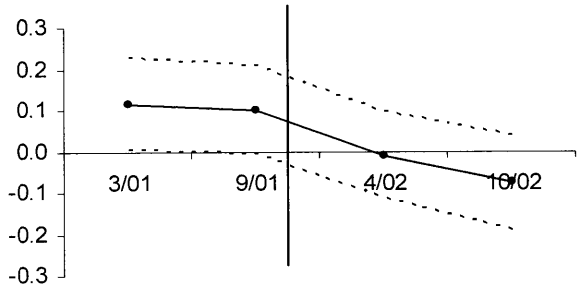


Table 5. Coefficients from *Weighted* Regressions Based on the Model:

$grantrate_{it} = \alpha + \beta_1\ propensity_i + \beta_2 prosensity_i * postMar2002 + judge_i + month_t + \varepsilon_{it}$

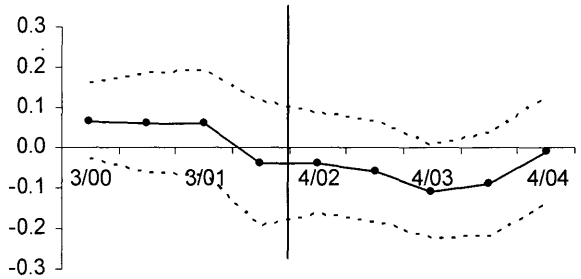
variable	1	2	3	4	5	6
constant	0.081	0.294	0.048	0.260	0.045	0.229
	(0.022)	(0.024)	(0.023)	(0.041)	(0.021)	(0.021)
propensity	1.090	omitted	1.092	omitted	1.029	omitted
	(0.044)		(0.060)		(0.049)	
propensity * postMar2002	-0.092	-0.060				
	(0.080)	(0.031)				
propensity * Mar2002 - 5			omitted	omitted		
propensity * Mar2002 - 4			0.077	0.065		
			(0.046)	(0.047)		
propensity * Mar2002 - 3			0.064	0.061	omitted	omitted
			(0.065)	(0.063)		
propensity * Mar2002 - 2			0.070	0.060	0.136	0.113
			(0.068)	(0.065)	(0.048)	(0.045)
propensity * Mar2002 - 1			-0.042	-0.041	0.086	0.070
			(0.081)	(0.077)	(0.068)	(0.066)
propensity * Mar2002 + 1			-0.077	-0.041	-0.021	0.005
			(0.096)	(0.062)	(0.089)	(0.053)
propensity * Mar2002 + 2			-0.093	-0.061	-0.075	-0.053
			(0.103)	0.064	(0.084)	(0.053)
propensity * Mar2002 + 3			-0.148	-0.111		
			(0.090)	(0.058)		
propensity * Mar2002 + 4			-0.126	-0.092		
			(0.100)	(0.063)		
propensity * Mar2002 + 5			-0.042	-0.008		
			(0.100)	(0.066)		
month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
judge fixed effects	No	Yes	No	Yes	No	Yes
number of observations	12,418	12,418	10,330	10,330	10,356	10,356
number of clusters (judges)	199 (398)	199 (398)	198 (396)	198 (396)	199 (398)	199 (398)
time period	1/99 - 12/04	1/99 - 12/04	9/99 - 9/04 (six month increments)	9/99 - 9/04 (six month increments)	3/99 - 3/04 (one year increments)	3/99 - 3/04 (one year increments)

Note: Regressions weighted by the number of observations, per judge, per month.

Figure 6. Coefficients (with 95% Confidence Interval)
From *Weighted* Regressions Based on the Model:

$grantrate_{it} = \alpha + \beta_1\, propensity_i + \beta_2 prosensity_i * postMarch2002 + judge_i + month_t + \varepsilon_{it}$

Six-month intervals (Table 3, column 4)



One-year intervals (Table 3, column 6)

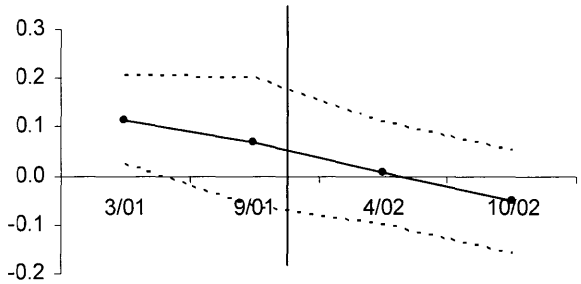


Table 6. Regression Coefficients, by Court (NY, SF, LA, and Miami)

	New York		San Francisco		Los Angeles		Miami	
variable	1	2	3	4	5	6	7	8
propensity * postMar2002	0.024		-0.214		-0.122		0.267	
	(0.045)		(0.070)		(0.135)		(0.111)	
propensity * Mar2002 - 5		omitted		omitted		omitted		omitted
propensity * Mar2002 - 4		-0.216		-0.120		-0.419		0.453
		(0.122)		(0.194)		(0.397)		(0.379)
propensity * Mar2002 - 3		-0.166		-0.368		-0.102		0.348
		(0.125)		(0.116)		(0.464)		(0.413)
propensity * Mar2002 - 2		-0.174		0.053		-0.012		0.329
		(0.124)		(0.131)		(0.341)		(0.130)
propensity * Mar2002 - 1		-0.252		0.089		-0.177		0.555
		(0.124)		(0.128)		(0.284)		(0.297)
propensity * Mar2002 + 1		-0.088		-0.127		-0.499		0.809
		(0.112)		(0.153)		(0.316)		(0.320)
propensity * Mar2002 + 2		-0.228		0.011		-0.459		0.663
		(0.121)		(0.150)		(0.304)		(0.314)
propensity * Mar2002 + 3		-0.201		-0.249		-0.048		0.649
		(0.104)		(0.124)		(0.315)		(0.314)
propensity * Mar2002 + 4		-0.217		-0.426		-0.171		0.289
		(0.111)		(0.222)		(0.349)		(0.311)
propensity * Mar2002 + 5		-0.094		-0.448		0.072		0.596
		(0.124)		(0.182)		(0.343)		(0.320)
month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
judge fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
number of observations	1,760	1,466	1,073	898	1,510	1,259	1,701	1,423
number of clusters (judges)	50 (25)	50 (25)	32 (16)	32 (16)	46 (23)	46 (23)	50 (25)	50 (25)
time period	1/99 - 12/04	9/99 - 9/04 (6-month incrmnt.)	1/99 - 12/04	9/99 - 9/04 (6-month incrmnt.)	1/99 - 12/04	9/99 - 9/04 (6-month incrmnt.)	1/99 - 12/04	9/99 - 9/04 (6-month incrmnt.)

Figure 7.

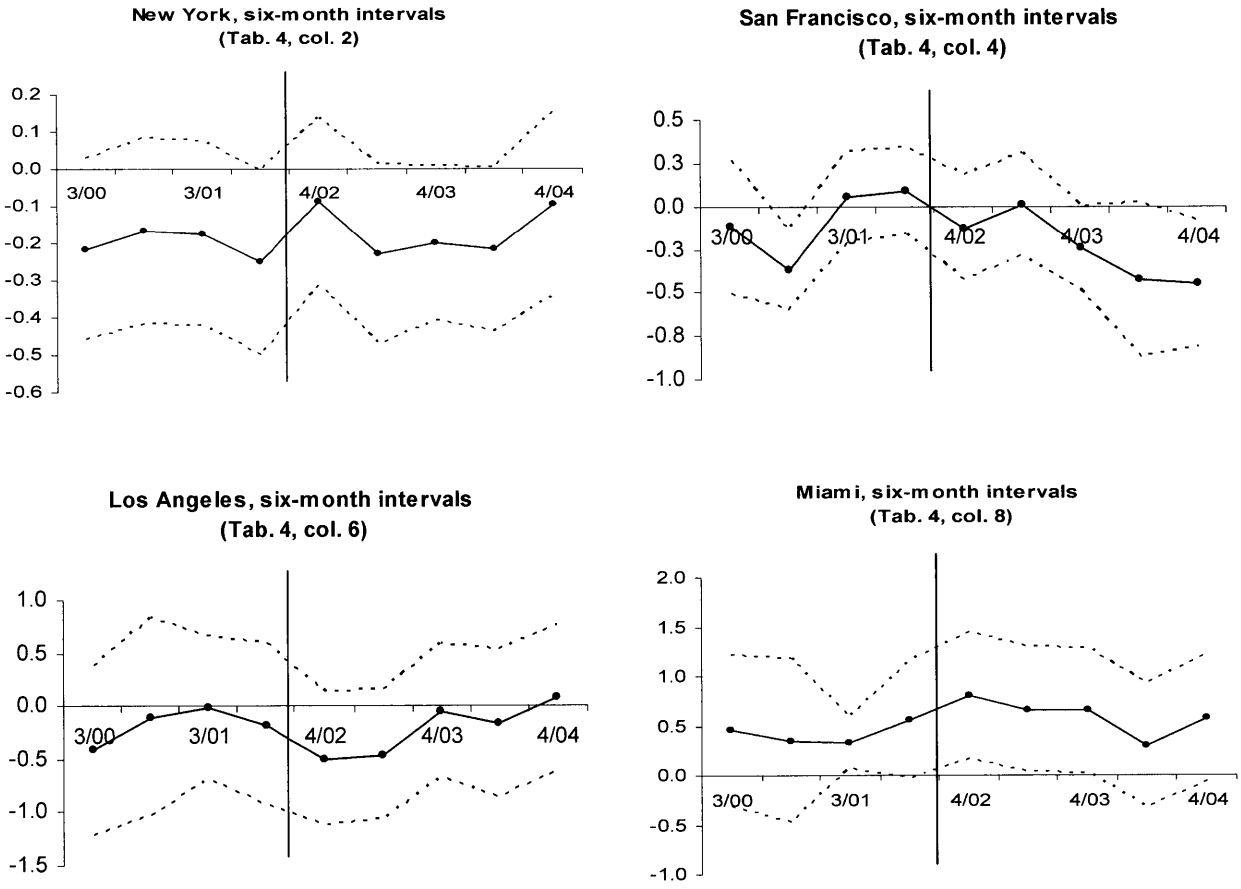

New York, six-month intervals
(Tab. 4, col. 2)
0.2
0.1
0.0
-0.1
-0.2
-0.3
-0.4
-0.5
-0.6
3/00
3/01
4/02
4/03
4/04
San Francisco, six-month intervals
(Tab. 4, col. 4)
0.5
0.3
0.0
-0.3
-0.5
-0.8
-1.0
3/00
3/01
4/02
4/03
4/04
Los Angeles, six-month intervals
(Tab. 4, col. 6)
1.0
0.5
0.0
-0.5
-1.0
-1.5
3/00
3/01
4/02
4/03
4/04
Miami, six-month intervals
(Tab. 4, col. 8)
2.0
1.5
1.0
0.5
0.0
-0.5
-1.0
3/00
3/01
4/02
4/03
4/04

Table 7a. Summary Statistics for "Propensity" Variable

Mean	0.275
standard deviation	0.151
10th percentile	0.105
25th percentile	0.160
Median	0.250
75th percentile	0.380
90th percentile	0.480

Table 7b. Magnitude of Predicted Effect of BIA Streamlining

	table 3, unweighted regressions		table 4, weighted regressions	
	column 1	column 2	column 1	column 2
mean - (1 SD below mean)	-0.024	-0.016	-0.014	-0.009
90th - 10th	-0.059	-0.039	-0.035	-0.023
75th - 25th	-0.035	-0.023	-0.020	-0.013
median - 10th	-0.023	-0.015	-0.013	-0.009
median - 25th	-0.014	-0.009	-0.008	-0.005
median - 75th	0.021	0.013	0.012	0.008
median - 90th	0.036	0.024	0.021	0.014

Table 8. Relationship Between Demographic Variables and Propensity, Before and After March 2002

	Propnsty * PostMar02	INS-DHS emplymt. * PostMar02	Military emplymt * PostMar02	NGO emplymt. * PostMar02	Private practice emplymt. * PostMar02	Government emplymt. * PostMar02	Academic emplymt. * PostMar02	Female * PostMar02	Appt. when President is a Democrat * PostMar02
1		0.001							
		(0.011)							
2	-0.103	-0.006							
	(0.036)	(0.011)							
3			0.021						
			(0.014)						
4	-0.095		0.017						
	(0.034)		(0.014)						
5				-0.031					
				(0.016)					
6	-0.083			-0.020					
	(0.035)			(0.016)					
7					0.027				
					(0.011)				
8	-0.103				0.029				
	(0.033)				(0.011)				
9						-0.001			
						(0.011)			
10	-0.099					-0.000			
	(0.034)					(0.011)			
11							-0.003		
							(0.016)		
12	-0.105						0.010		
	(0.035)						(0.017)		
13								-0.006	
								(0.012)	
14	-0.105							0.005	
	(0.036)							(0.013)	
15									0.007
									(0.011)
16	-0.103								0.012
	(0.033)								(0.011)
17		0.002	0.020	-0.028	0.029	-0.000	0.005	-0.000	0.010
		(0.012)	(0.015)	(0.016)	(0.016)	(0.011)	(0.017)	(0.013)	(0.010)
18	-0.109	-0.002	0.022	-0.021	0.028	-0.001	0.016	0.010	0.014
	(0.039)	(0.012)	(0.015)	(0.016)	(0.011)	(0.011)	(0.017)	(0.014)	(0.010)

Notes: Like the regression reported in column 2 of Tables 4 and 5, the regressions reported here contain month and judge fixed effects and are clustered by judges, pre- and post-March 2002. The time period is January 1999 through December 2004. Because biographical data for some judges is missing, the number of observations---12,249---is slightly less than the number reported in Tables 4 and 5.

Table 9. Distinguishing Moderation Resulting from Streamlining Reforms from Moderation Due to Mean Reversion

propensity	omitted
propensity * (propensity > median_grantrate) * postMar02	-0.067 (0.040)
propensity * (propensity <= median_grantrate) * postMar02	0.022 (0.097)

Notes: The regression contains month and judge fixed effects. The error term is clustered by judges, pre- and post-March 2002. The time period is January 1999 through December 2004.

Chapter 3

The Effect of Expansions in the Right to Counsel on Felony Incarcerations

Abstract

The Supreme Court's 1963 decision in *Gideon v. Wainwright* is considered one of the most important decisions in constitutional law and criminal procedure. It held that as a matter of the Sixth and Fourteenth Amendments to the U.S. Constitution, felony defendants were entitled to a right to counsel and states were obligated to appoint attorneys to those defendants too poor to hire one on their own. Prior to the Supreme Court's 1963 decision, 37 states already guaranteed a right to counsel as a matter of state law, whereas 13 states did not. This cross-state variation provides a natural experiment from which to study the impact of mandating a right to counsel. Measuring the impact of *Gideon* within a differences-in-difference framework, I use the rate of new felony prison admissions as a proxy for the probability of conviction and sentence lengths. The difference-in-difference estimates do not support an inference that *Gideon* had differential impacts in treatment and control states. Indeed, the estimates across various regression specifications are generally statistically insignifcant from zero.

3.1 Introduction

The Sixth Amendment of the U.S. Constitution guarantees that a criminal defendant shall have "the Assistance of Counsel for his defense." In *Gideon v. Wainwright*, 372 U.S. 335 (1963), the Supreme Court held that under the due process clause of the Fourteenth Amendment, criminal defendants in state courts, like their federal counterparts, were entitled to a Sixth Amendment right to counsel. In so holding, the Supreme Court overturned its earlier decision in *Betts v. Brady*, 316 U.S. 455 (1942) in which it declined to require states to appoint counsel to indigent defendants. In its narrowest holding, *Gideon* obligated the states to appoint counsel to indigent felony defendants only, but the Supreme Court in *Argersinger v. Hamlin*, 407 U.S. 25 (1972), later expanded the right to include misdemeanor defendants as well. Today, the federal Constitution requires state governments to provide counsel for any indigent felony or misdemeanor criminal defendant who faces actual incarceration.

Empirically-oriented law and economics scholars have examined the real-world consequences of changes in criminal procedural protections including the impact of Miranda rights on police clearance rates (Cassell 1996; Schulhofer 1996; Cassell and Fowles 1998; Donohue 1998); the impact of the exclusionary rule on crime (Atkins and Rubin 2003); and the impact of jury trial rights on sentence lengths (Prescott 2006). However, relatively less is known about the real-world consequences of expansions in the right to counsel, even though *Gideon v. Wainwright* is considered one of the most important decisions in constitutional and criminal law and even though the right to counsel is one of the most fundamental protections afforded to criminal defendants.

The goal of this paper is to examine whether criminal defendants benefited in an observable, measurable way, from expansions in the right to counsel. At the time *Gideon v. Wainwright* was decided in 1963, 37 states already guaranteed indigent felony defendants a right to counsel as a matter of state law. The remaining 13 states did not guarantee the right. Using a difference-in-differences framework, I exploit this cross-state variation, hypothesizing that expansions in the right to counsel benefited criminal defendants by lowering the probability of conviction and/or reducing sentence lengths.

Because historical state-level measures of conviction rates and sentence lengths are unavailable, I use as a proxy data on felony admissions to state prisons. I compare changes in felony prison admissions before and after 1963 for states where the rights announced in *Gideon* were an innovation with the same change in prison populations for states where *Gideon* was not an innovation. If the right to counsel benefitted criminal defendants in a measurable way, then the expected sign of this difference is negative.

Theoretically, attorney representation may, or may not matter, for a variety of reasons. Attorneys may serve a watchdog function over judges and prosecutors, monitoring the exercise of judicial discretion and constraining overzealous prosecution. Alternatively, even if attorneys do not function to constrain the behavior of judges and prosecutors, they may still assist the judicial system by simply doing good lawyering—framing the relevant legal issues and identifying facts needed to resolve those legal issues. In an adversarial legal system, attorney advocacy assists in minimizing legal and factual errors. Finally, to the extent that attorney representation is a guarantee that criminal defendants are aware of, defendants may be more aggressive in asserting that right before cooperating with law enforcement. This may lead, for example, to fewer voluntary confessions.

Empirically, the question of whether attorneys matter for their clients, and why, is an important one that has been addressed in a variety of contexts, using a variety of empirical methods. Perhaps the most convincing evidence of the effect of legal representation is based on randomized field studies, for example, in the context of juvenile courts (Stapleton and Teitelbaum 1974) and eviction proceedings (Seron et al 2001). Although a well-executed randomized field study can provide the most convincing evidence of the effect of attorney representation, they also demand a large amount of time and resources, and in the criminal context, is no longer constitutionally feasible. In the absence of experimental evidence, other researchers have examined the effects of attorney representation using observational data. For example, Lederman and Hrung 2006 examine data on tax court litigation outcomes, using tax protester status as an instrument to address the non-random matching of clients and attorneys.

This is not the first paper to exploit state level variation in right to counsel laws that existed prior to *Gideon v. Wainwright.* Atkins and Rubin 2003 also use this empirical strategy to

examine the effects of several Supreme Court decisions handed down in the 1960s. They focus their study on the Supreme Court's 1961 ruling in *Mapp v. Ohio*, 367 U.S. 643, excluding from trial evidence obtained in violation of the Fourth Amendment's protection against unreasonable searches and seizures, but also examine the effects of the *Gideon* decision. They find a significant increase in crime rates in states which had not already adopted a so-called "exclusionary" rule prior to the Supreme Court's imposition of such a rule via *Mapp v. Ohio*. Similarly, with respect to the *Gideon* decision, Atkins and Rubin find significant increases in crime in states that had not already adopted a right to counsel law. They attribute this result to the fact that *Gideon* was applied retroactively, so that criminals incarcerated without the benefit of counsel were entitled to a new trial.

This paper builds and expands upon Atkins and Rubin. First, rather than using crime rates as the outcome variable, this paper uses felony prison admissions as a proxy for conviction rates and sentence lengths. Second, the paper modifies the classification of state legal regimes, as some states laws were mis-classified in the Atkins and Rubin paper. Finally, this paper uses a variety of alternative regression specifications not used by Atkins and Rubin.

The following section describes the data used, including the coding of state right to counsel laws and data on felony prison admissions. The paper continues with the differences-in-differences empirical framework and presents the empirical results. The final section interprets the results and provides concluding remarks.

3.2 Data

3.2.1 State Legal Regimes

Tables 1 summarizes, for each state, the scope of the right to counsel for indigent felony defendants as it existed in 1962, the year prior to the *Gideon* decision. At the time, 37 states (36 excluding Alaska) required the appointment of counsel to indigent felony defendants as a matter of state law. Of these 37 states, 31 had adopted such laws by the time the U.S. Supreme Court in 1942 decided *Betts v. Brady*, 316 U.S. 455, in which the Court declined to find a federal constitutional right to counsel. An additional 6 states—Michigan, Virginia, Alaska, Massachusetts, Texas, and Colorado—adopted right to counsel laws between 1942 and 1962. Thus,

when the Supreme Court decided *Gideon v. Wainwright* and overruled *Betts v. Brady* in 1962, there were still 13 states (12 states excluding Hawaii) that did not mandate the appointment of counsel to indigent felony defendants. For some of these states, however, counsel was routinely appointed as a matter of informal practice. Figure 1 illustrates the timeline of the adoption of right to counsel laws. Note that under *Powell v. Alabama*, 287 U.S. 45 (1932), all states in 1962 were constitutionally required to provide free counsel to indigent criminal defendants facing capital charges punishable by death.

The categorization in Table 1 is based on a combination of secondary and primary legal sources. The starting point for legal research into historical state right to counsel laws is a variety of secondary sources including journal articles and legal briefs. For the vast majority of states, these secondary sources are consistent in their classification of state right to counsel laws, and no additional legal research was conducted for these states. Where the sources disagree on the classification, I have conducted original research into primary legal materials, including state constitutions, judicial decisions, statutes and court rules. The appendix discusses the secondary sources in more detail and summarizes the results of the additional research based on primary sources.

There are two major limitations to the variable coding the legal regime. First, the variable captures the law as it was written on books, not as it was implemented or enforced in practice. Thus, even in states where the law mandated the appointment of counsel, the actual implementation of the right may have been circumscribed by a variety of factors including 1) insufficient, or the complete absence of, funding for appointed attorneys; 2) waiver of the right to counsel, perhaps encouraged or permitted by judges; 3) the stage at which appointment was mandatory, either early on at a preliminary hearing or later during arraignment prior to the entry of a plea; and 4) the standards used to determine whether a defendant was indigent and qualified for the appointment of counsel.

Conversely, in states where the formal law did not mandate the appointment of counsel, the actual practice of appointing counsel may have been consistent, fully funded, and well-functioning. For example, Rhode Island implemented a state-wide public defender system as early as 1941. While the courts exercised discretion in deciding when to appoint indigent defense

counsel, the public defender system was centralized at the state level and was funded by the public. Stapleton and Teitelbaum's randomized field study of legal representation in juvenile courts found that institutional factors beyond an attorney's control can frustrate an attorney's efforts to advocate on behalf of a client. In the authors' two-site study, attorneys in one city mattered for case disposition, e.g. dismissal, probation, or detention in a corrections facility, but they did not matter in another. The authors attributed the difference to institutional factors such as the fact that attorneys in one city fewer days to prepare for dispositive merits hearings.

The second limitation of the variable coding legal regimes is that the level of observation is the state, not the city, county, court or judge. Anecdotal evidence suggests significant within state variation in the implementation of right to counsel laws. For example, even in states that mandated the appointment of counsel, urban areas tended to be better funded than rural areas and also tended to have relatively larger concentrations of attorneys who specialized in criminal law.

3.2.2 Prison Admissions

The ideal outcome variable is a measure of the probability of conviction and sentence length, holding constant the crime committed by the defendant. Unfortunately, however, comprehensive state-level data on conviction rates and sentence lengths is unavailable. Some states such as Texas have maintained a relatively long time series of court statistics through a state agency such as the Judicial Council. These court statistics, collected at the county level, document the number of defendants charged, whether the defendant was convicted, and what sentence the defendant received. Other states have similar statistical data, but the time coverage varies, the information collected differs, and the variable definitions are not consistent across states.

Parallel with efforts to create statistical records of state prisons populations, there was an effort in the 1930s to standardize and centralize the collection of state court records related to the processing and sentencing of criminal defendants. This national effort is documented in a 1932 publication by Leon C. Marshall entitled Judicial Criminal Statistics. See also, Bettman, Alfred, Leon C. Marshall, et al., Ohio Criminal Statistics: An Experiment in Methods and Techniques of State Reporting, 1931. Unfortunately, this effort did not materialize and to this

day, there are no nationally standardized, readily available statistical data concerning state courts. In contrast, data on the processing and sentencing of criminal defendants in federal court is readily available through the Federal Sentencing Commission, national crime statistics are available through the Federal Bureau of Prison crime reports, and incarceration statistics are available through the National Prisoner Statistics and the Prison and Jail Censuses.

In the absence of historical state-level data on conviction rates and sentence lengths, I use data on felony admissions to prisons as a proxy. Data on prison admissions were gathered from the National Prisoner Statistics, an annual survey of prison populations conducted by the U.S. government since 1925. For each state in each year, the National Prisoner Statistics collects data on the total year-end count of the prison population, including the number of prisoners admitted and the number released. Admissions are disaggregated by new admissions from court and re-admissions of escapees and parole violators. The data collection is managed at the federal level and a standard form using standard definitions is distributed to state agencies. The National Prisoner Statistics has been used extensively in prior research, for example, by Marvell and Moody (1994) who utilize the total year-end count of the prison population. This paper uses the data on flows measured by new admissions into prisons from court.

Use of the National Prisoner Statistics is subject to several caveats. First, the data generally cover prisons only, but some states (Alaska, Connecticut, Delaware, Hawaii, Rhode Island, and Vermont) have combined prison and jail systems. For these states, data on felons and misdemeanants are combined into a single number. Second, although the series has been collected since 1925, it was not until 1945 that data were available for all states. Prior to that time, some states participated in the data collection, while others did not. The working dataset upon which the results of this paper are based, therefore, begins in 1945. Third, between 1968 and 1970, data for some years are not reported for seven states including Arkansas, Delaware, Maryland, Massachusetts, Minnesota, North Carolina, and Rhode Island. Moreover, between 1971 and 1973, data are unavailable for all states because responsibility for administering the data collection was transferred from the U.S. Bureau of Prisons to the U.S. Department of Justice.

It is plausible that expansions in the right to counsel had different effects for different demographic groups. For example, it is likely that poor defendants or black defendants may have benefited more from *Gideon* than others. To the best of my knowledge, however, while some measures of prison population, disaggregated by race and income, are available at the national level, no measures exist at the state level.

3.2.3 State Demographics and Crime

To control for other demographic trends, state level demographic data are collected from the U.S. Statistical Abstracts. These variables include percent black, and percent urban, and the share of population in various age groups (under 15, 15-34, 35-64, over 65). State population is available annually, while percent black and percent urban are available from the decennial census. For percent black and percent urban, inter-census years have been extrapolated based on a linear time trend.

Data on crime trends are reported in the Federal Bureau of Investigation Uniform Crime Reports (UCR). Crime data from 1967 through 1983 are borrowed from UCR data collected by Thomas Marvell[1]. Crime data from 1945 through 1966 are collected from published, hard copy UCR reports. Use of UCR data are subject to a variety of well-known caveats. See, e.g., Maltz and Targonski (2004). Most importantly, the data capture only crimes reported to law enforcement agencies and the number of jurisdictions reporting crimes to the FBI has generally increased over time.

3.3 Descriptive Statistics

Figure 2 graphs the national time series of felony prison admissions per 100,000 people between 1945 and 1983. The vertical line marks the year 1963 when the U.S. Supreme Court decided *Gideon v. Wainwright.* Between 1945 and 1960, the number of prison admissions per 100,000 people was relatively stable between a low of 30 and a high of 45. During this time period, there is a steady overall increase. However, this steady upward trend reverses beginning in the early 1960s such that by the mid to late 1960s, the rate of admissions ranges between 33 and

[1]Publicly available at http://cemood.people.wm.edu/criminology.data.

36. This period of relatively fewer prison admissions ends in the early 1970s and thereafter begins a dramatic increase. By 1983, the rate of prison admissions per 100,000 people had increased to over 70, more than twice the rate of admissions at the beginning of the time series in 1945.

Figure 3a graphs the same time series of felony prison admissions, but disaggregates the series by two groups of states. Group 1 represents the group of 36 states of early adopters which mandated a right to counsel by 1963 when the U.S. Supreme Court decided *Gideon v. Wainwright.* Group 2 refers to the group of 12 remaining states that had not adopted counsel laws by 1963. The date of adoption of counsel laws appears to correlate with the level of incarceration, with earlier adopters having a lower rate of incarceration than late adopters. That the early and late adopters have different levels of incarceration rates makes the former an imperfect control group for the latter. Figure 3 also shows that the decline in the rate of admissions beginning in the early 1960s is consistent across both groups of states, and that the rate of decline is generally similar. Figure 3b is similar to Figure 3a, but graphs the natural log of prison admissions for early and late adopters.

Table 2 presents descriptive statistics for the key variables: prison admissions, violent crime, and property crime. The table gives means and standard deviations, by early- and late-adopter states and by time periods (2-year averages over 2-year intervals from 1958-1961 and from 1965-1968, with one 1-year averages from 1962-1964, near the *Gideon* decision). The table shows declining per capita prison admissions for both early- and late-adopting cohorts.

3.4 Empirical Framework and Results

The regression analysis employs a straightforward difference-in-difference framework. It compares the difference in per capita felony prison admissions before and after the 1963 *Gideon* decision for states where counsel laws had already been adopted as a matter of state law, with the same difference in prison admissions for states where counsel laws had not yet been adopted by the time of the *Gideon* decision. This difference-in-difference may be represented

algebraically as,

$$(P_{T,post-1963} - P_{T,pre-1963}) - (P_{C,post-1963} - P_{C,pre-1963})$$

where P denotes prison admissions, T denotes "treatment" states which had not adopted counsel laws by the time of the *Gideon* decision, and C denotes "control" states which had adopted counsel laws by the time of the *Gideon* decision. The differences-in-differences framework does not assume that *Gideon* had no impact on the control states. Even for control states, *Gideon* may have provided an impetus to reevaluate existing public defender programs, shore up their funding, and reinforce their applicability. Instead, the differences-in-differences framework detects differential effects across the treatment and control states.

The regressions take the following basic form,

$$\begin{aligned} P_{st} &= \alpha + \beta_1 * Post1963_t + \beta_2 * Gideon_s + \\ &\quad \beta_3 * Post1963_t * Gideon_s + \beta_4 X_{st} + \epsilon_{st} \end{aligned}$$

where P represents prison admissions in state s in year t; α is a constant; $Post1963$ is a dummy variable equal to 1 in years 1963 and later; *Gideon* is a dummy variable equal to 1 if state s had not adopted a mandatory counsel law by the time *Gideon* was decided, i.e. the state is a "treatment," late-adopter state; and X is a vector of demographic characteristics for state s in year t and includes percent black, percent urban, and state population by age brackets. In this regression framework, the coefficient β_3 captures the relevant difference-in-difference, and its expected sign is negative. With the inclusion of state and year fixed effects, the regression becomes:

$$P_{st} = \alpha + Year_t + State_s + \beta_1 * Post1963_t * Gideon_s + \beta_2 X_{st} + \epsilon_{st}$$

Finally, an alternate specification includes the variable $Post1963_t * Gideon_s * Yrs\,\mathrm{Si}\,nce1963_t$. This variable is appropriate if it takes some years for the *Gideon* effect to be felt.

Tables 3a and 3b present the basic regression results, first for a ten year time period between 1958 through 1968 (Table 3a), and second, for a longer time period from 1945 through 1983 (Table 3b), the time period covered by the full sample. Each column in the tables represents

a separate regression. For each regression, the dependent variable is the natural log of felony prison admissions. The coefficient of interest is given in the third row of the table and is labeled $Post1963_t * Gideon_s$. Whether or not state and year fixed effects are included in the regressions is indicated in the table. In all of the regressions shown in Tables 3a and 3b, the coefficient of interest is not statistically different from zero. Moreover, the coefficient is not robust to changes in specification, with positive signs in some specifications and negative signs in others. These results indicate that the data do not show significant relative differences in the way *Gideon* impacted control and treatment states.

Table 4 shows results of regressions that exploit two sources of variation. The first source of variation is generated prior to the adoption of *Gideon* when states adopted counsel laws as a matter of state law. The second source of variation is generated from the imposition of *Gideon* in 1963 on states that had not already adopted counsel laws. The regression in column 1 of Table 4 uses only the first source of variation, and accordingly, the time period is 1945 through 1962, before the Supreme Court decided *Gideon*. The regressions in columns 2 and 3 of Table 4 exploit both sources of variation, and use data from the time period 1945 through 1983. The results shown in column 2 of Table 4, which includes state and year fixed effects, are from the following regression:

$$\begin{aligned} P_{st} &= \alpha + Year_t + State_s + \beta_1 * CounselLaw_{st} + \\ &\quad \beta_2 * Post1963_t * Gideon_s + \beta_3 X_{st} + \epsilon_{st} \end{aligned}$$

The variable $CounselLaw_{st}$ becomes 1 for every state in 1963 when *Gideon* is decided. As in Tables 3a and 3b, the results in Table 4 are also statistically insignificant, and are sensitive to the specification.

Tables 5a and 5b explore whether *Gideon* had an effect on crime rates, putting either the natural log of violent crime (Table 5a) or the natural log of property crime (Table 5b) on the left hand side of the regression equation. A variety of specifications, similar to the ones used in the previous tables, are shown. For some specifications, for both violent and property crimes, there is a statistically significant rise in crime post-*Gideon* in states that did not have a pre-existing counsel law. One plausible explanation for these results is that *Gideon*, while it did

not necessarily lead to fewer prison sentences, may have reduced the length of sentences and thus, relaxed the deterrent effect of prison terms.

The previous regression results explored the impact of *Gideon*, separately, on prison admissions (Tables 3a, 3b, and 4) and crime rates (Table 5a and 5b). The next set of tables combines both outcome variables on the left-hand side simultaneously, examining the effect of *Gideon* on the natural log of prison admissions per violent crime (Table 6a), and per property crime (Table 6b). The time period in these regressions is 1958-1968, a ten-year window around the 1963 *Gideon* decision. The specifications used are identical to those in Tables 3a and 3b, and like those previous tables, the coefficients on the variable $Post1963_t * Gideon_s$ are statistically insignificant.

As an alternative to a fixed effects model with time and state dummies, Table 7 reports results of a first differences model. The regression equation, which regresses year-over-year changes in the dependent variable on treatment variables and year-over-year changes in the demographic control variables, is:

$$\begin{aligned} P_{st} - P_{s,t-1} &= Year_t + \beta_1 * Post1963_t * Gideon_s \\ &\quad + \beta_2 * Post1963_t * Gideon_s * Yrs\,Since1963_t \\ &\quad + \beta_3[X_{st} - X_{s,t-1}] + \epsilon_{st} \end{aligned}$$

The columns in Table 7 each represent a separate regression, with a different dependent variable similar to the dependent variable used in the previous Tables. First, the natural log of prison admissions and the natural log of crime, whether property or violent, are each used as the left-hand side variable. Then, prison admissions and crime are combined together. None of the coefficients of interest are statistically significant in any of the specifications.

3.5 Conclusion

John Donohue has observed that "the complex forces that shape major social phenomena do not tend to shift dramatically or quickly in response to a legal intervention." Donohue (1997-1998). He notes,

> Thus, if one looks at the degree of desegregation of southern schools in the wake of the momentous decision in *Brown v. Board of Education*, one sees almost no change in the percentage of black students attending all-black schools for well over a decade. Similarly, if one looks at birth rates before and after *Roe v. Wade*, it is surprisingly difficult to detect any impact of that important case, despite the substantial subsequent increase in the documented number of legal abortions. The point is not that legal interventions don't matter. Indeed, [Donohue] believes that the impact certainly of *Brown* and possibly of *Roe* has been enormous. But if one seeks to quantify this impact through seemingly plausible statistical measures, one frequently finds that the measurable consequences of Supreme Court commands, when identifiable, tend to be somewhat glacial.

Consistent with Donohue's observations that it is difficult to find measurable impacts of watershed Supreme Court decisions including, for example, *Brown v. Board of Education* and *Roe v. Wade*, this paper also fails to find measurable impacts of *Gideon v. Wainwright.* To be clear, the differences-in-dfferences estimates fail to support an inference that *Gideon* had differential impacts on treatment and control states. The hypothesis that *Gideon differentially* reduced the rate of prison admissions in treatment versus control states is not supported by the data.

Nevertheless, differences-in-differences estimates that are statistically insignificant from zero are not inconsistent with the theory that *Gideon* impacted all states in a similar manner—entitling defendants in treatment states to a new right to counsel, while at the same time providing an impetus to reinforce and strengthen existing rights in control states. A simple time series shows that in the years immediately following 1963, the rate of felony prison admissions declined in both treatment and control states. Declines in both treatment and control states that are similar in magnitude are consistent with differences-in-differences estimates that are statistically insignificant from zero.

3.6 References

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3.7 Appendix: Legal Research on Historical State Right to Counsel Laws

This appendix documents and summarizes the legal research I have conducted on historical state right to counsel laws. As noted previously, Table 1 summarizes the scope of the right to counsel for indigent felony defendants as it existed in 1962, the year prior to the *Gideon* decision. Figure 1 illustrates the timeline of the adoption of right to counsel laws. I used a variety of secondary sources as the starting point for legal research into historical state right to counsel laws. These include, in chronological order:

1. Beaney, William M., Jr., The Right to Counsel in American Courts, 1951, University of Michigan;
2. Kamisar, Yale, "The Right to Counsel and the Fourteenth Amendment: A Dialogue on 'The Most Pervasive Right' of an Accused", 30 University of Chicago Law Review 1, 1962;
3. American Civil Liberties Union and Florida Civil Liberties Union, Amici Curiae brief filed in Gideon v. Wainwright, 1962, 1962 WL 75208;
4. Silverstein, Lee, Defense of the Poor, American Bar Foundation, 1965;
5. Decker, John F. and Thomas J. Lorigan, "Right to Counsel: The Impact of Gideon v. Wainwright in the Fifty States," 3 Creighton Law Review 103, 1969; and
6. Atkins, Raymond and Paul Rubin, "Effects of Criminal Procedure on Crime Rates: Mapping Out the Consequences of the Exclusionary Rule", Journal of Law and Economics, Vol. XLVI, 2003.

Four of these sources—Kamisar in footnotes 74, 76 and 77; American and Florida Civil Liberties Union ("ACLU") in Appendix I; Decker & Lorigan ("D&L") in footnotes 13, 15 and 16; and Atkins & Rubin ("A&R") in Appendix A2—provide concise classifications of state right to counsel laws as they existed in 1962, the year prior to the *Gideon* decision. For 38 states, these four secondary sources are consistent in their classification of the state laws. For the remaining 12 states—Colorado, Connecticut, Delaware, Georgia, Hawaii, Maryland,

Massachusetts, Michigan, New Hampshire, Rhode Island, Vermont and Virginia—Table A1 below shows the inconsistencies among these four secondary sources in their classifications of right to counsel laws. In Table A1, the number "1" indicates that the secondary source classifies the state as a right-to-counsel state, and the number "0" indicates otherwise.

Table A1. Conflicts Among Four Secondary Sources

State	Kamisar	ACLU	A&R	D&L
Colorado	1	0	0	1
Connecticut	1	1	0	1
Delaware	0	0	0	1
Georgia	1	1	1	0
Hawaii	0	0	.	1
Maryland	0	0	0	1
Massachusetts	1	1	1	0
Michigan	1	0	1	1
New Hampshire	0	0	1	1
Rhode Island	0	0	0	1
Vermont	0	0	0	1
Virginia	1	1	0	1

Because historical state right to counsel laws are often defined by a complex, dynamic interaction between the state constitution, judicial decisions, statutes, and court rules, legal research is necessarily time intensive. Thus, rather than conducting original research on all 50 states, I have focused on researching the 12 states where the four secondary sources are in conflict. Except as explained below, no original research was conducted for the 38 states where all four secondary sources are in agreement. Below, I discuss the secondary sources in more detail and summarize the results of my original research on primary legal sources.

Kamisar cites two pre-Gideon U.S. Supreme Court decisions which catalog the scope of state right to counsel laws as they existed at the time of the respective decisions—*Betts v. Brady*, 316 U.S. 455 (1942), and *McNeal v. Culver*, 365 U.S. 109 (1961). In particular, Kamisar

relies on Justice Black's dissenting opinion in *Betts* and Justice Douglas' concurring opinion in *McNeal*. Based on an original review of the primary authorities cited in these opinions, Kamisar notes several mis-classifications. Ultimately, Kamisar identifies 30 states that mandated the appointment of counsel at the time *Betts* was decided in 1942 and an additional 7 states that adopted such laws between 1942 and 1962. Thus, according to Kamisar, at the time *Gideon* was decided in 1963, a total of 37 states guaranteed counsel as a matter of state law. The classification used in Table 1 conforms to the classification used in the Kamisar article, with one exception explained below.

New Mexico is classified in Justice Black's dissenting opinion in *Betts* as a state that did not have a law "clearly establishing" a right to counsel for felony defendants, but no citation to a primary legal source is given. In fact, however, New Mexico adopted a statute in 1937 which required the appointment of counsel for defendants charged with a capital offense or any offense punishable by imprisonment in the penitentiary. In his review of the primary sources cited in *Betts*, Kamisar does not note this mis-classification, perhaps because Kamisar only examined states in which a citation was given and none was given for New Mexico. Thus, while Kamisar identified 30 states that had adopted right to counsel laws at the time *Betts* was decided in 1942, there were actually 31 states including New Mexico. An additional six states would adopt right to counsel laws between 1942 and 1962, bringing to 37 the total number of states that mandated the appointment of counsel at the time *Gideon* was decided.

Like New Mexico, Justice Black's dissenting opinion in *Betts* classifies two other states as not "clearly establishing" a right to counsel for felony defendants, but no supporting authorities are cited. These states are North Carolina and Vermont. For both of these states, I have researched the primary legal sources and have confirmed that the classification in *Betts* is accurate. Neither North Carolina nor Vermont clearly mandated the appointment of counsel at the time *Betts* was decided in 1942, and this was still the case at the time *Gideon* was decided in 1963.

The ACLU *Gideon* amicus brief cites the *McNeal v. Culver* decision as well as the Kamisar article. Following the categorization in *McNeal*, the ACLU brief identifies 35 states as requiring the appointment of counsel, leaving out both Michigan and Colorado. However, as the Kamisar

article explains, Michigan adopted Court Rule 35A in 1947 and Colorado adopted Rule of Criminal Procedure 44 in 1961. Both of these rules required the appointment of counsel for felony defendants. In researching the history of these rules as well as the state laws which preceded them, I have verified that these rules mark the beginning of a mandate for appointed counsel in these states.

The Atkins & Rubin ("A&R") article cites the Kamisar article. No additional sources are cited. The categorization in A&R conforms to the Kamisar article, with the exception of three states—Connecticut, New Hampshire and Virginia. The A&R article lists both Connecticut and Virginia as states that "did not automatically grant counsel", when in fact, the Kamisar article lists both states as mandating the appointment of counsel. In contrast, the A&R article, as well as the Decker & Lorigan ("D&L") law review article, lists New Hampshire as mandating counsel, when in fact, the Kamisar article explains that New Hampshire required the appointment of counsel only for felony cases punishable by three or more years imprisonment. I have conducted my own legal research to verify that Kamisar's classification of these three states—Connecticut, New Hampshire and Virginia—is accurate.

It should also be noted that A&R groups Colorado with states that did not mandate counsel at the time *Gideon* was decided. A&R argue that Colorado's law became effective in November 1961, 16 months before *Gideon* was decided in March 1963, and that *Gideon* could "affect [the state] because of the possible retroactive effect of the decision." In other words, A&R argue that because *Gideon* was retroactive, prisoners in Colorado whose convictions were obtained prior to the November 1961 adoption of the law may have been released, retried and readmitted to prison for the same offense, or to the extent that former prisoners are likely to recidivate, tried and readmitted to prison for a new offense. This "revolving-door" scenario would affect the post-1963 time series of prison admissions in a "mechanical" way that is separate from the true effect of strengthening the right to counsel.

But by this reasoning, Massachusetts and Texas should also be grouped with the no-right-to-counsel states since their laws were passed in June 1958 and June 1959, respectively. Any possible retroactivity may have affected convicted felons who were serving sentences of 5 years or more in Massachusetts and 4 years or more in Texas. In fact, *Gideon's* retroactivity may

have affected any convicted felon serving a life or a relatively long sentence in states that did not mandate counsel at the time the sentence was imposed. This includes states such as Michigan, see Palmer, 371 Mich. 656 (1963) (holding that Gideon applies retroactively and granting writ of habeas corpus to petitioner sentenced, in 1942, to life imprisonment without benefit of counsel), and Virginia which did not adopt right to counsel laws until 1947 and 1948, respectively.

The practical effect of *Gideon's* retroactivity on the time series of prison admissions may have been attenuated by the fact that in successfully challenging a conviction obtained without the benefit of counsel, convicted criminals risked the possibility that on retrial, the new sentence would be even higher than the original sentence. Van Alstyne, William W., "In Gideon's Wake: Harsher Penalties and the 'Successful' Criminal Defendant", 74 Yale Law Journal 606, 1964; Krash, Abe, "The Right to a Lawyer: The Implications of Gideon v. Wainwright", 39 Notre Dame L. 150, 1963.

For the 7 remaining states which have not yet been discussed—Delaware, Georgia, Hawaii, Maryland, Massachusetts, Rhode Island and Vermont—all the sources are in agreement, except for the Decker & Lorigan ("D&L") law review article. D&L cite primarily to the 1962 American Bar Foundation survey conducted by Lee Silverstein, as well as to state statutes and court rules.

D&L characterize both Georgia and Massachusetts as providing counsel to felony defendants only in practice and not as a matter of state law (footnote 13; pages 113, 127). As explained more fully below, Georgia mandated the appointment of counsel through judicial decision at least as early as 1942, and Massachusetts did so in 1958 through the adoption of Rule 10.

D&L also characterize the following states as mandating the appointment of counsel: Hawaii (page 113, "The statutes of Hawaii, both before and after Gideon, have granted the right to appointed counsel to defendants charged with felonies"), Maryland (page 121, "Prior to Gideon, only felony charges necessitated appointment of counsel."), and Vermont (page 123, "prior to Gideon, Vermont courts appointed counsel for accused felons only"). My original research has revealed that neither Maryland nor Vermont mandated the appointment of counsel to all felons as D&L suggest. I have not conducted any independent research with respect to Hawaii.

For Delaware (page 126-127) and Rhode Island (page 117), D&L properly identify the relevant state statutes and court rules, but do not explicitly recognize that under these laws, appointment of counsel was permissive rather than mandatory.

To summarize, I have conducted original research into the primary legal materials of sixteen states. This includes five states—Michigan, Virginia, Massachusetts, Texas and Colorado—that adopted counsel laws between 1942, when *Betts v. Brady* was decided, and 1962, when *McNeal v. Culver* was decided. I have also researched states listed in Justice Black's dissenting opinion in *Betts* which did not have any citation to primary legal materials—New Mexico, North Carolina and Vermont—since it appears that Kamisar may not have verified the accuracy of classification for these states. I have also researched an additional six states—Delaware, Maryland, New Hampshire, Connecticut, Georgia and Rhode Island—since the classifications in either A&R or D&L were in conflict with the Kamisar classification. I have also researched an additional two states—California and Illinois. For these 16 states, more detail about the primary legal sources follows below.

California. In 1872, California enacted Penal Code § 987 which required the court to inform a defendant, upon arraignment, of his right to counsel and to appoint an attorney if the defendant desired, but was unable to afford an attorney. West's Annotated California Codes, as amended and supplemented through Ch. 82 of the 1985 portion of the 1985-1986 Regular Session (historical notes state that § 987 was enacted in 1872 and was amended for the first time in 1971); Deering's Annotated Codes and Statutes of California, as amended and in force at the close of the 26th session of the Legislature, 1885 (the 1885 version of the statute contains § 987). Case law and secondary sources confirm that § 987, unlike a separate statute dealing with the right to counsel at a preliminary hearing, was mandatory, not permissive. *People v. Crowley*, 13 Cal.App. 322, 324 (Cal.App. 3 Dist. 1910) (State statute "does not require the magistrate to appoint counsel on the request of defendant, at the preliminary examination, as is the case upon his arraignment (Pen. Code, sec. 987)"); "Right of Defendant in Criminal Action to Aid of Counsel Under California Law", 21 Cal. L. Rev. 479, 484-496, 1932 ("Under section 987 of the Penal Code, ... , the judge upon the arraignment is obligated to appoint counsel in aid of the defendant if the latter desires and is unable to employ counsel"; "[I]n contrast to the procedure upon arraignment, [the statutory code sections dealing with the right

to counsel upon preliminary examination], do not impose upon the magistrate a duty to ask the defendant if he desires counsel, nor do they obligate the court to appoint an attorney at the request of an indigent defendant").

Colorado. In 1961 Colorado adopted Rule of Criminal Procedure 44, requiring the appointment of counsel in any case involving an indigent felony defendant. 34 Rocky Mntn. L. Rev. 89, 90 1961 (Rule 44 "makes it mandatory for district courts (county courts do not have jurisdiction over felonies) to advise every criminal defendant of his right to counsel and to appoint counsel for an indigent criminal defendant in any felony case, at least where a request for counsel is made and indigency is proved"). Prior to 1961, the appointment of counsel to felony defendants was at the discretion of the court. *Id.* ("Prior to the adoption of Rule 44, "the appointment of counsel for an indigent criminal defendant, for all crimes, [was] discretionary with the court except where, upon an appraisal of the totality of the facts in each case, the refusal to appoint counsel or to advise the defendant concerning his rights thereto amount to a violation of due process of law"); Colorado Revised Statutes, 1953, s 39-7-29 ("In all indictments or informations for crimes or misdemeanors in any of the district courts of this state where by reason of the inability of the defendant to employ counsel, the court may assign him counsel for his defense") (emphasis added); *Kelley v. People*, 206 P.2d 337, 339-341 (Supreme Court of Colorado 1949) (holding that failure, upon arraignment, to inform defendant of right to counsel and to appoint counsel for defendant did not violate either federal or state law, including s 39-7-29 (at the time Kelley was decided, s 39-1-29 was numbered as Section 502, chapter 48, but the language was nearly identical to the later s 39-7-29)); *Vigil v. People*, 310 P.2d 552, 554 (Supreme Court of Colorado 1957) (reaffirming decision in *Kelley v. People*).

Connecticut. In 1917, Connecticut became the first state to establish a state-wide public defender system. In contrast with earlier assigned counsel systems in which the appointment of counsel was a matter of discretion for the courts, the newly created public defender system required the public defender to represent all indigent defendants. Public Acts 1917, ch. 225 (authorizing the judges of the superior court to appoint an attorney to act as public defender and requiring the public defender to represent any indigent person charged with a crime in superior court); Public Acts 1921, ch. 129 (broadening the duties of the public defender to mandate representation of indigent defendants in either superior court, the court of common

pleas, or the district court of Waterbury). As of 1958, these public acts were codified in the Connecticut statutes as §§ 54-80 and 54-81. General Statutes of Connecticut, Revision of 1958, §§ 54-80, 54-81 (Hildreth Press, Inc.); Mars, David, "Connecticut Public Defenders", Connecticut Bar Journal, Vol. 33, 1965, p. 297-314 (describing the history of the right to counsel in Connecticut); William W. Wilbourne, "The Developing Right to Counsel: Its Impact of the Connecticut Public Defender System", Connecticut Bar Journal, Vol. 39, 1965, p. 221-252 (describing the history of the right to counsel in Connecticut). Connecticut courts noted that under the statutory language, appointment of counsel to indigent defendants was mandatory. *State v. Reid*, 146 Conn. 227 (Supreme Court of Errors of Connecticut 1959) (Under sections 54-80 and 54-81 an "accused who lacks funds is assured of representation") (emphasis added); *Gipson v. Commissioner of Correction*, 54 Conn. App. 400 (Appellate Court of Connecticut 1999) ("Public Acts 1917, ch. 225, s 1 required the judges of the Superior Court to appoint an attorney" for indigent defendants) (emphasis added).

Delaware. Prior to *Gideon*, Delaware required the appointment of counsel to indigent defendants facing charges for "murder, manslaughter or any offense punishable by death, or the offense of being an accomplice or accessory to any such crime." Delaware Code Annotated, Title 11 § 5103(a) (West 1953). State law authorized the court, in its discretion, to appoint counsel for other non-capital defendants. Delaware Code Annotated, Title 11 §5103(b) (West 1953) ("The Court may assign counsel to any person in any criminal prosecution as provided by the Rules of Criminal Procedure for the Superior Court.") (emphasis added); Superior Court Rule 44 (West 1953) ("If the defendant appears in court without counsel, the court shall advise him of his right to counsel and, in every case in which the law requires or in any other case in which the court deems it appropriate, the court shall assign counsel to represent him at every stage of the proceeding unless he elects to proceed without counsel or is able to obtain counsel") (emphasis added).

Georgia. Georgia had no statutory provision requiring the appointment of counsel to indigent defendants. However, the Georgia Supreme Court construed the state constitutional provision that "every person charged with an offense ... shall have the privilege and benefit of counsel" (now Article 1, section 1, paragraph XIV; formerly 1976 Const., Art. I, § I, XI; 1945 Const., Art. I, § I, V; 1877 Const., Art. I, § I, V.), to mean that, if the accused desires,

but is unable to afford counsel, the court must appoint one to represent him and failure to do so violates his state constitutional right to counsel. *Bibb v. Hancock*, 211 Ga. 429, 435-437 (1955) (The U.S. Supreme Court in *Betts v. Brady* "reviews the decisions and statutes of most of the States, showing that in many States the courts have not held ... constitutional provisions [similar to the one in Georgia] to mean that the court must appoint counsel in order to satisfy the constitutional requirement that the accused must have counsel. It is clear that Georgia does, however.") (emphasis added); *Walker v. State*, 194 Ga. 727 (1942) (state constitution "guarantees [a defendant] who is unable to employ counsel the right to have counsel appointed for him by the court) (emphasis added); *Martin v. Georgia*, 51 Ga. 567 (1874) ("[T]he courts will appoint counsel for [an indigent defendant], without charge to the defendant") (emphasis added).

Illinois. In May 1948, Illinois adopted Supreme Court Rule 27A, requiring the appointment of counsel, upon arraignment, to indigent defendants facing charges punishable by imprisonment in the penitentiary. Rules of Practice and Procedure adopted by the Supreme Court of the State of Illinois, in effect as of May Term 1950. The adoption of the rule was a codification of earlier law announced by the Illinois Supreme Court. *Vise v. County of Hamilton*, 19 Ill. 78 (1857) ("[T]he court, in case of inability of the accused to obtain counsel, will appoint counsel for him, and may compel the counsel, as an officer of the court, subject to its authority, to defend the accused against unjust conviction") (emphasis added); *People v. Kurant*, 331 Ill. 470 (1928) ("A privilege most important to a person accused of crime, connected with his trial, is to be defended by counsel. ... The trial court's duty to assign competent counsel to defend an accused person who is unable to employ one is not intended to be a mere empty formality"); Illinois Annotated Statutes §101.26, Historical and Practice Notes by Jenner and Tone (West 1956) (noting that Rule 27A "is substantially a codification of the law of this State which has been vigorously announced by the Supreme Court of Illinois"); Deck, Jesse L., "Proposed Reforms in Illinois Criminal Law and Procedure", 12 J. Am. Inst. Crim. L. & Criminology 381, 384, May 1921 to February 1922 (noting that a court shall assign competent counsel to a defendant in a criminal case who is unable to employ counsel). Supreme Court Rule 27A was later incorporated into the state statutes as Supreme Court Rule § 101.26. Illinois Annotated Statutes (West 1956).

Maryland. In 1963, before *Gideon* was decided, Maryland Rules of Procedure, Criminal Causes, Rule 719 required the appointment of counsel, at arraignment, if the defendant was charged with an offense "for which the maximum statutory punishment is death or confinement in the penitentiary for five years or more." The court had discretion to appoint in any other case. Annotated Code of Maryland, 1957 (Michie 1963 replacement volume). Prior to 1963, this rule, numbered as Rule 723, required the appointment of counsel in "capital cases or other serious cases." Annotated Code of Maryland, 1957 (Michie 1958, 1962 Cumulative Supplement); *Hill v. State*, 218 Md. 120 (Court of Appeals of Maryland (high court) 1958) (reviewing the history of Rule 723). Following *Gideon*, Rule 719 was amended in August 1963 to extend the right to counsel to defendants charged with offenses "for which the maximum punishment is death or imprisonment for a period of six months or more, or a fine of $500,00 or more, or both." Annotated Code of Maryland 1957 (Michie 1971 replacement volume); *Manning v. State*, 237 Md. 349, 353 (Court of Appeals of Maryland 1965) (noting the 1963 amendments to Rule 719); *Johnson v. State*, 9 Md.App. 436, *439 (Court of Special Appeals of Maryland 1970) (noting the differences in the pre-1963 Rule 719 and the post-1963 Rule 719).

Massachusetts. In June of 1958, the Supreme Judicial Court adopted General Rule 10, requiring the appointment of counsel for non-capital felony defendants appearing in Superior Court. 337 Mass. 812. In 1964, Rule 10 was broadened in two ways. 347 Mass. 808. First, appointment of counsel was required for any defendant charged with a crime for which a sentence of imprisonment could be imposed. Second, appointment of counsel was required for such defendants appearing in any court, not just Superior Court. Prior to the adoption of Rule 10, appointment of counsel in non-capital cases was not mandatory. *Pugliese v. Commonwealth*, 335 Mass. 471 (Supreme Judicial Court of Massachusetts 1957) ("It must be regarded as settled ... that neither in its constitutional nor in its statutory provisions does the law of this Commonwealth require that a person charged with other than a capital offence be furnished counsel," although under *Betts v. Brady*, the 14th Amendment to the Federal Constitution may require appointment of counsel in non-capital cases where special circumstances, such as diminished mental capacity, exist); Martin, Ephraim and Robert U. Holden, "The Right to Counsel in Criminal Cases in Massachusetts", 37 B.U. L. Rev. 430, 1967.

Michigan. In 1947, Michigan adopted Court Rule 35A, requiring the appointment of counsel to felony defendants upon arraignment. 318 Mich. xxxix. Rule 35A was codified in the Michigan statutes as Criminal Procedure Rule 785.3. Michigan Statutes Annotated (1963). Prior to 1947, the Supreme Court of Michigan had established that defendants did not have an affirmative right to counsel at public expense. *People v. Williams*, 225 Mich. 133, 137-138 (Mich. 1923) ("An accused is not entitled as of right, to have counsel assigned by the court to advise him relative to his plea. The state Constitution ... secures to an accused the right 'to have ... counsel for his defense' ... [but] [t]his does not mean he shall have counsel at public expense. ... State statute permits the court to appoint an attorney at public expense to conduct the defense of an accused when he is unable to procure counsel. This statute is permissive."); *People v. Crandell*, 270 Mich. 124, 129-120 (Mich. 1935) (reaffirming *People v. Williams*). According to Beaney (p. 128), Rule 35A was adopted in response to *People v. DeMeerleer*, 313 Mich. 548 (1946), in which the Michigan Supreme Court affirmed the conviction of a 17-year-old defendant for murder who was arraigned, tried and sentenced to life imprisonment in one day, without the assistance of counsel. In affirming the conviction, the Michigan Supreme Court relied on its precedents in *Williams* and *Crandell.* This decision was later reversed by the U.S. Supreme Court, *DeMeerleer v. People*, 329 U.S. 663 (1947).

New Hampshire. In 1955, New Hampshire statute § 604:1 and § 604:2 required the appointment of counsel in capital cases as well as cases involving non-capital offenses punishable by three or more years' imprisonment. New Hampshire Statutes Annotated, 1955; Fitzgibbons v. Hancock, 97 N.H. 162 (Supreme Court of New Hampshire 1951) (there is "no reasonable doubt that the Court within his discretion could allow the plaintiff to plead guilty without the advice of counsel."); State v. Herbert, 108 N.H. 332 (Supreme Court of New Hampshire 1967) (stating that § 604:2 authorized but did not require the court to appoint counsel at public expense). This statute governed the scope of the right counsel at the time Gideon was decided. Following Gideon in 1965, the statute was substantially revised and expanded to include all indigent defendants charged with felonies or misdemeanors other than petty offenses.

New Mexico. In 1937, the New Mexico Legislature adopted House Bill No. 138 requiring the appointment of counsel to indigent defendants charged with a capital offense or an offense punishable by imprisonment in the penitentiary. Laws of 1937, ch. 88. The exact language

of the bill appears in sections 42-1102 and 42-1103 in the New Mexico Statutes Annotated, 1941. This statute was later numbered as sections 41-11-2 and 41-11-13 and as 41-22-1 *et. seq.* The New Mexico Supreme Court recognized that the statute was mandatory, not permissive. *State v. Anaya*, 76 N.M. 572 (1966) ("When the offense with which the defendant is charged is punishable by imprisonment in the penitentiary, the court is required to assign counsel 'if the prison has not the financial means to procure counsel"') (emphasis added).

North Carolina. In *State v. Hedgebeth*, 45 S.E.2d 563 (1947), affirmed by *Hedgebeth v. North Carolina*, 334 U.S. 806 (1948), the North Carolina Supreme Court recognized that while the appointment of counsel in capital cases is mandatory, the appointment of counsel in felony cases is permissive, depending on whether counsel is requested and whether "circumstances are such, for financial or other reasons, as to show the apparent necessity of counsel for the protection of the defendant's right." The North Carolina Supreme Court reaffirmed *Hedgebeth* several times, *State v. Cruse*, 76 S.E.2d 320 (1953), *State v. Simpson*, 90 S.E.2d 708 (1956), *State v. Davis*, 103 S.E.2d 289 (1958), and it was the law in effect at the time *Gideon* was decided. See also Davis, Roy W., "Constitutional Law—Right of Counsel". 32 N.C. L. Rev. 331 (1953).

Rhode Island. Section 12-15-3 gives statutory authority for the creation of an office of the public defender and places a duty upon the public defender to represent indigent defendants referred to him or her from superior court (and later expanded to include cases referred from supreme and district courts as well). This statute was first enacted by Public Law 1941, chapter 1007, section 3. In *Lee v. Kindelan*, 80 R.I. 212, 218-219 (1953), the Rhode Island Supreme Court explained that while section 12-15-3 authorized the court to appoint counsel at public expense for an indigent prisoner, this authority was to be exercised in the sound judicial discretion of the court, and did not require the appointment of counsel in any particular case. See also *Ex parte Lee*, 123 F. Supp 439, (U.S. district court, D of Rhode Island 1954) (noting that the Rhode Island Supreme Court in *Lee v. Kindelan* interpreted Public Law 1941, chapter 1007 as permitting, but not requiring the appointment of counsel).

Texas. As early as 1925, Texas Code of Criminal Procedure art. 494 required the appointment of counsel to "the accused . . . [who] has no counsel and is too poor to employ [his own]

counsel." Baldwins' Complete Texas Statutes, 1925. The Texas courts interpreted this statute to require the appointment of counsel only in cases involving capital charges. *Gilley v. State*, 26 S.W.2d 1070, 1071 (Court of Criminal Appeals 1930) ("The law does not place upon the trial court the duty of appointing counsel for the accused on trial except in the case of a capital felony. Article 494, C. C. P."); *Brady v. State*, 54 S.W.2d 513, 514 (Court of Criminal Appeals 1932) (The defendant "was not charged in the indictment with an offense which could be punished by the infliction of the death penalty, and for that reason the court was not required by the law to appoint counsel for him. See article 494, C. C. P."). In 1959, however, the Texas Legislature amended art. 494, effective June 1959, to require the appointment of counsel in cases involving indigent felony defendants. 56th Leg., ch. 484.

Vermont. In *State v. Gomez*, 96 A. 190, 193 (Vt. 1915), the Supreme Court of Vermont stated that "there is no statute making it the duty of the court to assign counsel at the state's expense under any conditions." It noted that the only statute touching on the matter was a statute that authorized compensation for counsel appointed in capital cases but not in any other case.

Virginia. In 1948, the Virginia Legislature enacted s 53-278.1, requiring the court to appoint counsel to any felony defendant prior to accepting a plea. Code of Virginia, Session 1948, pt. 2 Amendatory Part. In the 1950 Code, s53-278.1 was reorganized as s19-214.1. Prior to 1948, Virginia case law left open the question of whether indigent defendants were guaranteed counsel at public expense, and if so, whether the guarantee applied to capital, felony or misdemeanor defendants. *Watkins v. Commonwealth*, 6 S.E.2d 670, 671-672 (Supreme Court of Appeals of Virginia 1940) (holding that defendant's right to counsel was not violated where the defendant was able to employ counsel but refused to do so; recognizing that the right to counsel is a fundamental one, perhaps guaranteed by the state bill of rights; leaving open the question of whether the right requires the appointment of counsel at public expense in felony cases).

Table 1. Did the state in 1962 require the appointment of counsel to indigent felony defendants?

YES. The state guaranteed counsel to indigent felony defendants by force of the state constitution, statute, court rule, or judicial decision.	37 states	[Alaska]^ Arizona Arkansas California Colorado^ Connecticut Georgia Idaho Illinois Indiana Iowa Kansas Kentucky	Louisiana Massachusetts^ Michigan^ Minnesota Missouri Montana Nebraska Nevada New Jersey New Mexico New York North Dakota	Ohio Oklahoma Oregon South Dakota Tennessee Texas^ Utah Virginia^ Washington West Virginia Wisconsin Wyoming
NO. The state did not guarantee counsel as a matter of state law.	13 states	Alabama Delaware* Florida [Hawaii]* Maine*	Maryland Mississippi New Hampshire* North Carolina	Pennsylvania* Rhode Island* South Carolina Vermont*

Sources:

See Appendix 1 for information on the sources used to create this table.

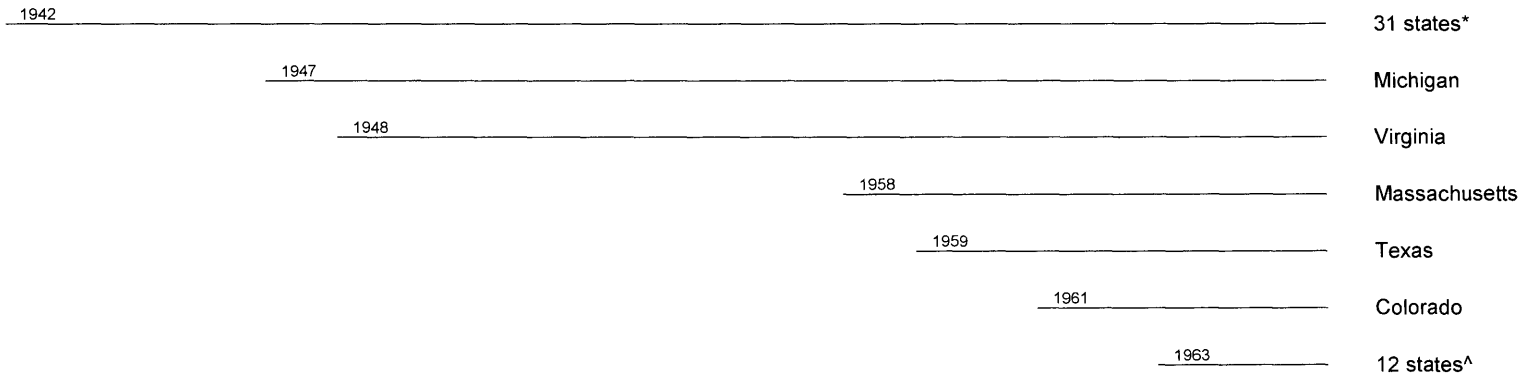
Notes:

*Although the seven states marked with an asterisk did not guarantee counsel to indigent felony defendants as a matter of law, there is evidence that they did provide counsel in many cases as a matter of informal practice.

^The six states marked with a carat are states that adopted a law mandating counsel between the years 1942 and 1962. The states and year of adoption are as follows: 1) Michigan 1947; 2) Virginia 1948; 2) Alaska ????; 4) Massachusetts 1958; 5) Texas 1959; 6) Colorado 1961. The remaining 31 states required the appointment of counsel at least as early as 1942, the year the Supreme Court decided *Betts v. Brady*.

[For completeness, Alaska and Hawaii are shown in this table. However, they are omitted from the regressions because data on prisoners was not collected for Hawaii until 1960 and for Alaska until 1974.]

Figure 1. Timeline of Adoption of Right to Counsel Laws

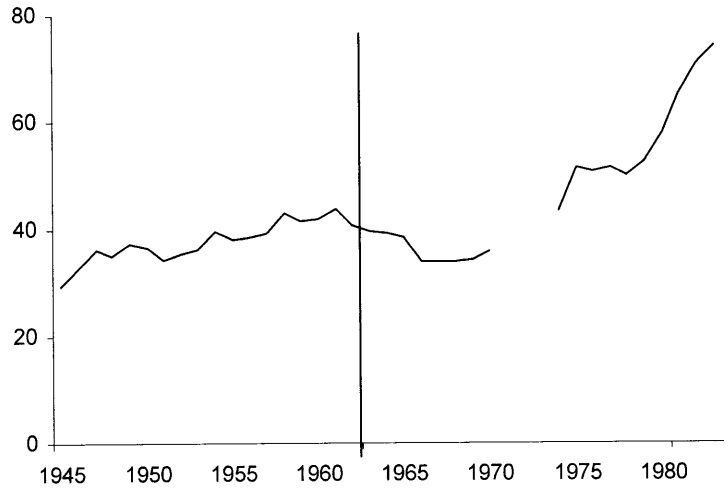


Notes:

* The 31 states that adopted right to counsel laws by 1942 include: Arizona, Arkansas, California, Connecticut, Georgia, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Minnesota, Missouri, Montana, Nebraska, Nevada, New Jersey, New Mexico, New York, North Dakota, Ohio, Oklahoma, Oregon, South Dakota, Tennessee, Utah, Washington, West Virginia, Wisconsin, and Wyoming.

^ The 12 states (excluding Hawaii) that had not adopted right to counsel laws by 1963 include: Alabama, Delaware, Florida, Maine, Maryland, Mississippi, New Hampshire, North Carolina, Pennsylvania, Rhode Island, South Carolina, and Vermont.

Figure 2. Prison Admissions Per 100,000 Population, All States, 1945-1983



Note: Data on prison admissions is missing for all states for the years 1971, 1972, and 1973.

Figure 3a. Prison Admissions Per 100,000 Population, States Grouped by Period of Counsel Law Adoption, 1945-1983

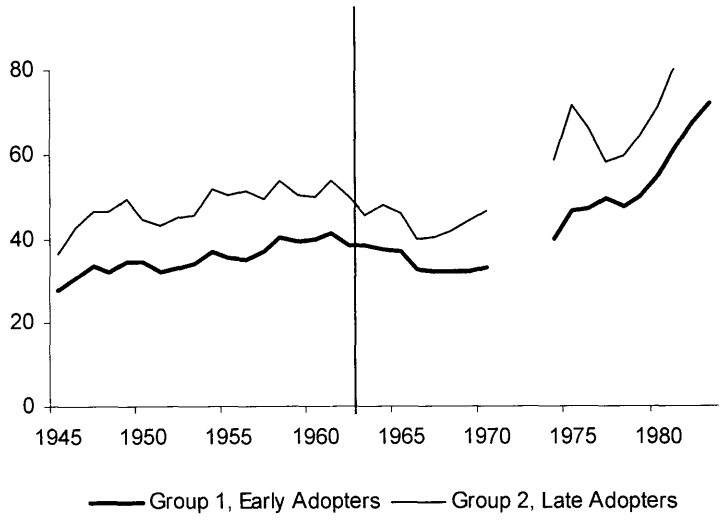


Figure 3b. Log Prison Admissions, States Grouped by Period of Counsel Law Adoption, 1945-1983

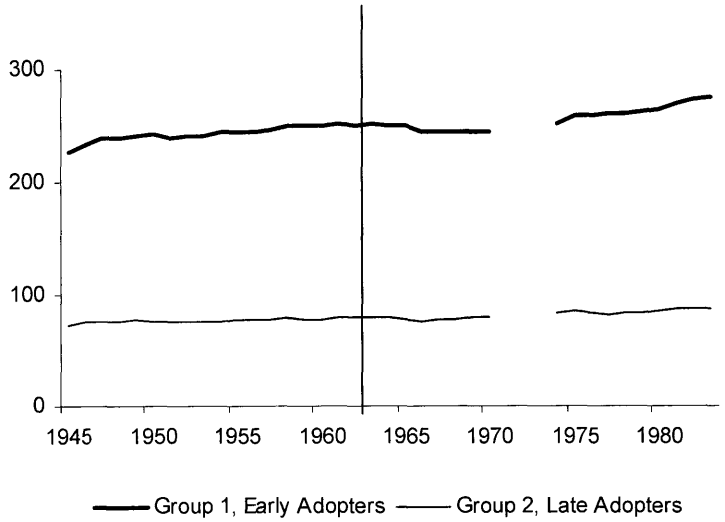


Table 2. Descriptive Statistics
Mean with standard deviation below in parentheses

	Time Period						
	1958/59	1960/61	1962	1963	1964	1965/66	1967/68
Per Capita Prison Admissions							
Group 1	52.7	53.6	50.4	47.3	49.1	42.4	42.1
(late adopters)	(32.8)	(34.7)	(33.5)	(29.0)	(31.6)	(29.8)	(27.0)
Group 2	45.1	45.3	43.3	43.2	41.2	38.2	34.7
(early adopters)	(15.8)	(15.5)	(15.6)	(15.6)	(15.2)	(14.6)	(11.9)
All States	47.0	47.3	45.1	44.2	43.2	39.2	36.6
	(21.4)	(22.0)	(21.3)	(19.6)	(20.4)	(19.4)	(17.1)
Per Capita Violent Crime							
Group 1	0.10	0.10	0.09	0.11	0.14	0.16	0.21
(late adopters)	(0.07)	(0.07)	(0.06)	(0.07)	(0.09)	(0.10)	(0.16)
Group 2	0.09	0.10	0.10	0.11	.014	0.15	0.19
(early adopters)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)	(0.11)
All States	0.09	0.10	0.10	0.11	0.14	0.15	0.20
	(0.06)	(0.07)	(0.07)	(0.07)	(0.08)	(0.09)	(0.12)
Per Capita Property Crime							
Group 1	0.63	0.69	0.73	0.80	0.91	1.00	1.31
(late adopters)	(0.26)	(0.30)	(0.28)	(0.34)	(0.39)	(0.43)	(0.65)
Group 2	0.75	0.85	0.92	1.00	1.10	1.21	1.57
(early adopters)	(0.29)	(0.34)	(0.38)	(0.43)	(0.42)	(0.45)	(0.59)
All States	0.72	0.81	0.87	0.95	1.05	1.16	1.51
	(0.29)	(0.33)	(0.37)	(0.42)	(0.41)	(0.45)	(0.61)

Table 3a. Regression Results
Dependent Variable is Ln of Prison Admissions
Time Period is 1958-1968

	1	2	3	4
Post1963	-0.094*			
	(0.024)			
Gideon (late adopters)	-0.322			
	(0.422)			
Post1963 * Gideon	0.021	0.021	-0.007	-0.027
	(0.063)	(0.067)	(0.057)	(0.054)
Post1963 * Gideon * YrsSince1963				0.010
				(0.015)
Percent Black			-0.059	-0.055
			(0.036)	(0.036)
Percent Urban			0.009	0.010
			(0.006)	(0.006)
Share of Pop. 15-24 Years			-0.070	-0.063
			(0.064)	(0.065)
Share of Pop. 25-34 Years			-0.044	-0.036
			(0.054)	(0.060)
Share of Pop. 35-64 Years			-0.172*	-0.168
			(0.043)	(0.043)
Share of Pop. Over 65 Years			-0.096	-0.089
			(0.067)	(0.066)
State & Year Fixed Effects	No	Yes	Yes	Yes
Observations	528	528	528	528

Notes: The dependent variable in each regression is the natural log of felony prison admissions. Each column represents a separate regression. Robust standard errors clustered on state are reported in parentheses. Coefficients with an asterisk are significant at the ten percent level or smaller.

Table 3b. Regression Results
Dependent Variable is Ln of Prison Admissions
Time Period is 1945-1983

	1	2	3	4
Post1963	0.359*			
	(0.047)			
Gideon (late adopters)	-0.275			
	(0.400)			
Post1963 * Gideon	0.043	0.043	0.044	-0.011
	(0.128)	(0.131)	(0.095)	(0.072)
Post1963 * Gideon * YrsSince1963				0.007
				(0.008)
Percent Black			-0.032	-0.032
			(0.021)	(0.021)
Percent Urban			0.021*	0.021*
			(0.004)	(0.004)
Share of Pop. 15-24 Years			-0.123*	-0.123*
			(0.050)	(0.050)
Share of Pop. 25-34 Years			-0.063*	-0.063*
			(0.024)	(0.024)
Share of Pop. 35-64 Years			-0.080*	-0.080*
			(0.029)	(0.029)
Share of Pop. Over 65 Years			-0.063*	-0.063*
			(0.046)	(0.046)
State & Year Fixed Effects	No	Yes	Yes	Yes
Observations	1680	1680	1680	1680

Notes: The dependent variable in each regression is the natural log of felony prison admissions. Each column represents a separate regression. Robust standard errors clustered on state are reported in parentheses. Coefficients with an asterisk are significant at the ten percent level or smaller.

Table 4. Regression Results

	1945-1962	1945-1983	
	1	2	3
Counsel Law	0.033	-0.578	0.067
	(0.082)	(0.354)	(0.160)
Gideon (late adopters)		-0.811	
		(0.497)	
Post1963 * Gideon		0.546	-0.051
		(0.350)	(0.183)
Post1963 * Gideon * YrsSince1963		0.003	0.003
		(0.009)	(0.009)
Year Fixed Effects		Yes	Yes
State Fixed Effects		No	Yes
Observations	864	1680	1680

Notes: "Counsel Law" is a dummy variable equal to 1 if state s requires the appointment of counsel in year t. This set of regressions exploits two sources of variation, one coming from pre-Gideon variation, as some states adopted counsel laws voluntarily prior to 1963, and the second coming from the imposition of Gideon on states that had not adopted counsel laws by 1963. Each column represents a separate regression. Robust standard errors clustered on state are reported in parentheses. Coefficients with an asterisk are significant at the ten percent level or smaller.

Table 5a. Regression Results
Dependent Variable is Natural Log of Violent Crimes

	1	2	3	4	5
Post1963	0.559*				
	(0.030)				
Gideon (late adopters)	-0.460		-1.460*		
	(0.596)		(0.070		
Post1963 * Gideon	0.019	0.019	1.088*	0.192	
	(0.065)	(0.068)	(0.474)	(0.170)	
Counsel Law			0.954*	0.014	0.004
			(0.487)	(0.093)	-0.091
Year Fixed Effects	No	Yes	Yes	Yes	Yes
State Fixed Effects	No	Yes	No	Yes	Yes
Observations	528	528	1680	1680	816
Time Period	1958-1968	1958-1968	1945-1983	1945-1983	1945-1962

Notes: Each column represents a separate regression. Robust standard errors clustered on state are reported in parentheses. Coefficients with an asterisk are significant at the ten percent level or smaller.

Table 5b. Regression Results
Dependent Variable is Natural Log of Property Crimes

	1	2	3	4	5
Post1963	0.496*				
	(0.025)				
Gideon (late adopters)	-0.545		-1.517*		
	(0.377)		(0.470)		
Post1963 * Gideon	-0.007	-0.007	1.054*	0.232*	
	(0.045)	(0.048)	(0.370)	(0.071)	
Counsel Law			-0.929*	-0.042	-0.089
			(0.388)	(0.052)	(0.056)
Year Fixed Effects	No	Yes	Yes	Yes	Yes
State Fixed Effects	No	Yes	No	Yes	Yes
Observations	528	528	1680	1680	816
Time Period	1958-1968	1958-1968	1945-1983	1945-1983	1945-1962

Notes: Each column represents a separate regression. Robust standard errors clustered on state are reported in parentheses. Coefficients with an asterisk are significant at the ten percent level or smaller.

Table 6a. Regression Results
Dependent Variable is Ln of Prison Admissions per Violent Crimes
Time Period is 1958-1968

	1	2	3	4
Post1963	-0.653*			
	(0.032)			
Gideon (late adopters)	0.137			
	(0.028)			
Post1963 * Gideon	0.002	0.002	-0.085	-0.082
	(0.070)	(0.075)	(0.074)	(0.065)
Post1963 * Gideon * YrsSince1963				-0.001
				(0.022)
Percent Black			-0.042	-0.042
			(0.041)	(0.043)
Percent Urban			0.005	0.005
			(0.009)	(0.009)
Share of Pop. 15-24 Years			-0.221*	-0.222
			(0.091)	(0.092)
Share of Pop. 25-34 Years			-0.014	-0.015
			(0.072)	(0.078)
Share of Pop. 35-64 Years			0.049	0.048
			(0.066)	(0.068)
Share of Pop. Over 65 Years			-0.128	-0.130
			(0.083)	(0.082)
State & Year Fixed Effects	No	Yes	Yes	Yes
Observations	528	528	528	528

Notes: Each column represents a separate regression. Robust standard errors clustered on state are reported in parentheses. Coefficients with an asterisk are significant at the ten percent level or smaller.

Table 6b. Regression Results
Dependent Variable is Ln of Prison Admissions per Property Crimes
Time Period is 1958-1968

	1	2	3	4
Post1963	-0.590*			
	(0.027)			
Gideon (late adopters)	0.222			
	(0.228)			
Post1963 * Gideon	0.028	0.028	-0.004	-0.025
	(0.068)	(0.072)	(0.070)	(0.062)
Post1963 * Gideon * YrsSince1963				0.010
				(0.021)
Percent Black			-0.026	-0.022
			(0.042)	(0.043)
Percent Urban			-0.004	-0.004
			(0.008)	(0.007)
Share of Pop. 15-24 Years			-0.076	-0.069
			(0.084)	(0.086)
Share of Pop. 25-34 Years			0.040	0.049
			(0.064)	(0.068)
Share of Pop. 35-64 Years			0.006	0.010
			(0.056)	(0.057)
Share of Pop. Over 65 Years			-0.095	-0.087
			(0.089)	(0.088)
State & Year Fixed Effects	No	Yes	Yes	Yes
Observations	1680	1680	1680	1680

Notes: Each column represents a separate regression. Robust standard errors clustered on state are reported in parentheses. Coefficients with an asterisk are significant at the ten percent level or smaller.

Table 7. First Differences Regression Results
Time Period is 1958-1968

	Dependent Variable				
	Ln Prison Admissions	Ln Property Crimes	Ln Violent Crimes	Ln Prison Admissions Per Property Crimes	Ln Prison Admissions Per Violent Crime
	1	2	3	4	5
Post1963 * Gideon	-0.011	0.012	0.016	-0.022	-0.025
	(0.032)	(0.016)	(0.037)	(0.037)	(0.056)
Post1963 * Gideon * YrsSince1963	0.011	-0.003	0.001	0.014	0.010
	(0.012)	(0.003)	(0.011)	(0.013)	(0.016)
Percent Black	-0.034	-0.011	-0.021	-0.023	-0.010
	(0.028)	(0.024)	(0.043)	(0.033)	(0.045)
Percent Urban	0.008*	0.008	0.007	0.001	0.005
	(0.003)	(0.005)	(0.006)	(0.006)	(0.008)
Share of Pop. 15-24 Years	-0.023	0.069	0.132	0.092	0.143
	(0.054)	(0.059)	(0.095)	(0.068)	(0.096)
Share of Pop. 25-34 Years	-0.024	-0.035	-0.070	0.011	0.055
	(0.026)	(0.024)	(0.056)	(0.030)	(0.055)
Share of Pop. 35-64 Years	-0.110*	-0.126*	-0.239*	0.015	0.102
	(0.039)	(0.031)	(0.063)	(0.051)	(0.071)
Share of Pop. Over 65 Years	-0.062	0.030	-0.062	-0.092	0.010
	(0.059)	(0.048)	(0.078)	(0.068)	(0.083)
Year Dummies	Yes	Yes	Yes	Yes	Yes
Observations	528	528	528	528	528

Notes: Each column represents a separate regression. Robust standard errors clustered on state are reported in parentheses. Coefficients with an asterisk are significant at the ten percent level or smaller.