Essays in Empirical Law and Economics

by

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Abstract

This thesis, which consists of three essays, uses empirical methods to study questions in criminal procedure and employment antidiscrimination law.

The first chapter measures the consequences for offenders of expanding constitutional criminal jury trial rights. I study the Supreme Court's landmark decision in *Apprendi v. New Jersey* (2000), which extended beyond-a-reasonable-doubt jury factfinding (and all the costs and complications it entails) to particular facts previously decided by judges using a preponderance-of-the-evidence standard. The limited holding of *Apprendi* and the calculations required by the U.S. Sentencing Guidelines allow me to compare changes in the sentence lengths of groups of offenders who were differentially affected by the decision. I find that this expansion of jury trial rights benefited criminal offenders, reducing the average sentence for those most affected by more than 5 percent.

The second chapter studies the prosecutorial charging response to the Supreme Court's *Apprendi* decision. Using federal arrest, charging, and sentencing data to evaluate prosecutorial behavior, I find evidence that prosecutors reacted to the higher costs of factfinding by reducing the number of counts filed against affected defendants by as much as 10 percent, presumably magnifying the sentence reduction that would have occurred had prosecutors not substituted charging resources toward unaffected defendants.

The third chapter, co-authored with Christine Jolls, examines the employment consequences of the American's with Disabilities Act's (ADA) two major features—the discrimination prohibition and the "reasonable accommodations" requirement. Several studies have suggested that the passage of the ADA might have reduced employment opportunities for individuals with disabilities, but which particular feature or features of the ADA, if any, caused this disemployment effect are unknown. Using state-level variation in pre-ADA legal regimes to separately estimate the employment effects of the ADA's two substantive provisions, we find strong evidence that the immediate post-enactment employment effects of the ADA are attributable to the reasonable accommodations mandate rather than the firing costs associated with the antidiscrimination provision. Moreover, the pattern of effects across states suggests, contrary to prior findings, that declining disabled employment after the immediate post-ADA period may reflect factors other than the ADA.

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Chapter 1

Measuring the Consequences of Criminal Jury Trial Protections

Abstract

The Sixth and Fourteenth Amendments to the U.S. Constitution guarantee criminal defendants the right to a jury trial and require that the elements of crimes be proved beyond a reasonable doubt. Academics, judges, and practitioners generally assume that these constitutional guarantees protect defendants. Recently, however, scholars and even members of the Supreme Court have suggested that expanding jury trial rights may actually work to the detriment of defendants given the existing rules and structure of the criminal justice system. Others have argued that procedural protections such as jury trial options and higher standards of proof are irrelevant in light of significant prosecutorial resources and discretion. In this paper, I seek to measure the consequences of expanding criminal jury trial rights. To do so, I study the Supreme Court's landmark criminal procedure decision in *Apprendi v. New Jersey* (2000), which held that a jury—not a judge—must decide beyond a reasonable doubt any fact that causes the penalty for a crime to exceed a prescribed statutory maximum. I use the limited reach of the *Apprendi* decision and the calculations required by the United States Sentencing Guidelines to create groups of offenders who were differentially affected by *Apprendi*'s expansion of jury rights but who were otherwise comparable. By comparing the change in sentence length of these groups pre- and post-*Apprendi*, I am able to identify and measure the effect of the broader jury trial rights (including the higher standard of proof) mandated by that case. I find that this expansion in jury trial rights substantially benefited criminal offenders, reducing the average sentence for those most affected by more than 5 percent.

1.1 Introduction

Over the past five years, the Supreme Court has decided a number of seminal cases delineating the scope of Sixth Amendment jury trial rights, and yet almost nothing is known about the real-world consequences of these protections. We are not completely in the dark. Legal scholars and members of the Supreme Court have speculated about the possible effects of expanding jury trial rights (e.g., Bibas 2001a, 2001b; King and Klein 2001b; Stuntz 2004). Unfortunately, their arguments come to inconsistent conclusions and are difficult to quantify. Empirical work on the behavior of judges and juries is also not entirely absent. For instance, the relative ability of juries and judges to arrive at accurate and predictable decisions in criminal cases has received substantial scholarly attention (e.g., Eisenberg et al. 2005).

Nevertheless, no study empirically examines the consequences of giving criminal defendants a jury trial "option." This omission is a drawback of existing empirical work, which typically compares the experimental, hypothetical, or selection-bias-corrected decisions of judges to those of juries (see MacCoun 1993, pp.138-47; Helland and Tabarrok 2000). To be sure, the across-the-board differences between judges and juries identified in the literature make up part of what a jury trial right is "worth." But comparisons of jury performance to judicial performance *inclusive* of cases in which a defendant would waive his jury trial right necessarily ignore option value. Existing empirical studies discount or wholly ignore the incentives and costs that shape the behavior of defendants and prosecutors, and, as a result, the existing literature cannot assess the full consequence of jury trial protections.

A jury trial right also includes more than a potential change in the identity of the factfinder. Under American law, an important aspect of the expansion of criminal jury trial rights has been the application of a higher standard of proof—proof beyond a reasonable doubt—to facts otherwise decided by a preponderance of evidence. A few studies have explored the differences between how these two standards are interpreted and applied by judges and juries using survey responses and laboratory experiments (Kagehiro and Stanton 1985; Simon and Mahan 1971; Kalven and Zeisel 1966), but we know nothing about the real-world consequences of combining better defendant access to a jury with a change in the standard of proof.

In this paper, I evaluate the practical consequences for defendants of jury trial rights by studying a rare and unexpected expansion of those rights. In June 2000, the Supreme Court decided *Apprendi v. New Jersey*, 530 U.S. 466 (2000), a landmark criminal procedure decision that expanded the scope of the Sixth Amendment's jury trial guarantee. The decision granted criminal defendants the right to have a jury determine beyond a reasonable doubt certain facts that, under preexisting law, had been submitted to a judge and decided by a preponderance of evidence.

Importantly, this expansion of jury trial rights did not necessarily benefit defendants. As I discuss more fully in Section 1.3, the Sixth Amendment guarantees criminal defendants a jury trial, but does not provide a right to *waive* a jury trial in favor of a bench trial. Therefore, it is constitutionally permissible (and, in fact, legal under current federal law) for prosecutors to "force" jury trials with respect to facts covered by the Sixth Amendment when juries are prosecutor-friendly. In addition, Leipold (2005) has suggested that judges may have become defendant-friendly, relative to juries, since the U.S. Sentencing Guidelines came into force. Under these conditions, it is possible for broader jury trial rights to worsen defendant outcomes. Bibas (2001a, pp.1157-60), for example, has argued that expanding jury trial rights to sentencing facts will hurt defendants by strengthening prosecutors' bargaining positions, and members of the Supreme Court have cited his work for that proposition.

To measure the consequences of *Apprendi*'s expansion of jury trial rights, I use the limited reach of the decision and the calculations required by the United States Sentencing Guidelines to create groups of offenders who were differentially affected by the expansion of jury rights under *Apprendi* but who were otherwise comparable. This difference-in-differences approach is superior to comparing offender outcomes pre- and post-*Apprendi* because simple before and after comparisons may capture much more than just the effect of expanding criminal jury trial rights, such as changes in resource availability or other secular trends in crime levels.

My empirical approach builds on three facts. First, *Apprendi* requires juries, rather than judges, to find facts only when the defendant's sentence may exceed the maximum statutory sentence for the charged offense. Second, because of the structure of the sentencing guidelines, offenders in higher criminal history categories are by construction "closer" to the relevant statutory maximum. Third, *Apprendi* did not affect criminal history factfinding—the decision exempted facts of prior conviction. Therefore, by comparing the change in the average sentence length of recidivist offenders pre- and post-*Apprendi* to the same change in the sentences of relatively new criminals (while at the same time controlling for observable differences between these groups), I am able to identify and measure the effect of the broader jury trial rights mandated by *Apprendi*. Importantly, as I show below, my results are unlikely to be driven by changes in the composition of my treatment and control groups because, in addition to *Apprendi*'s explicit exception for facts of prior conviction, it is not easy for criminal history determinations to be manipulated by prosecutors or judges.

I find that expanding jury trial rights substantially benefits defendants. Specifically, my results suggest that the expansion of jury trial rights under *Apprendi* reduced the sentences of those in higher criminal history categories by about six months or more than 5% of the mean sentence for those groups. For purposes of comparison, a recent paper studying racial and gender disparities in sentencing finds "large, persistent" effects in that context that are similar to the effect of expanded jury trial rights I estimate here (Schanzenbach 2005, p.84; see Schanzenbach and Tiller 2005, p.33).

Understanding the consequences of criminal jury trial rights has grown in importance as the scope of these rights has become more controversial and open to change. The holding of *Apprendi* was unexpected, and academics, practitioners, and judges uniformly perceived the Supreme Court's decision as opening a new era in criminal sentencing procedure. Consistent with this prediction, *Apprendi* was followed a few years later by *Blakely v. Washington*, 542 U.S. 296 (2004), which established that the Sixth Amendment gives criminal defendants the right to have *all* aggravating sentencing facts decided by a jury and proven beyond a reasonable doubt. Following *Blakely*, however, the Supreme Court decided *United States v. Booker*, 125 S.Ct. 738 (2005), which effectively undermined a defendant's access to a jury trial on aggravating sentencing facts (previously binding under the guidelines) by making the consequences of those facts advisory and therefore subject to judicial decision making.

Because of these decisions, Congress and state legislatures must now choose between determinate sentencing systems that provide defendants with jury trial rights and what are essentially indeterminate or advisory sentencing systems in which judges are free to evaluate sentencing facts by a preponderance of evidence. The real-world consequences of this policy decision are potentially very important, but—until now—entirely unknown. My results inform this choice by providing evidence that, relative to advisory systems in which judges by and large follow sentencing guidelines, determinate systems with jury trial rights will lead to lower average sentences.

The remainder of this paper motivates and presents my empirical work. In Section 1.2, I briefly describe how federal sentencing practice has evolved recently and explain the Supreme Court's *Apprendi* opinion (and the cases that followed) in more detail. In Section 1.3, I examine the academic debate over the likely consequences of expanding jury trial rights. Section 1.4 describes my data and explains my empirical approach. In Section 1.5, I describe my empirical model and report my basic results. Section 1.6 addresses robustness issues. Section 1.7 concludes.

1.2 Sentencing Guidelines and Criminal Factfinding

In crafting federal sentencing policy, Congress has historically relied on judges to evaluate the existence, relevance, and importance of sentencing "facts"—facts related to the crime, the victim, the criminal's course of conduct and likely future propensities (Stith and Cabranes 1998). Before the 1970s, this practice was uncontroversial. Congress typically enacted a statute that defined a crime in terms of key elements, appended a wide penalty range (e.g., ten to twenty years), and then left it to judicial discretion to set appropriate punishments within the minimum and maximum for a violation of the statute.

A political coalition favoring sentencing reform emerged in the 1970s, however. The alliance included groups concerned with racial and class discrimination in sentencing, as well as critics worried about the "soft on crime" tendencies they perceived in the sentencing practices of certain federal judges. Both groups were troubled by reports of substantial sentencing disparities, which they attributed to judicial abuse of discretion (see Anderson, Kling and Stith 1999). By the early 1980s, Senator Ted Kennedy and President Ronald Reagan led the movement for federal sentencing reform, and in 1984 the federal Sentencing Reform Act (SRA) was passed with bipartisan support (Stith and Cabranes 1998, pp.38-48).

The SRA was designed to reduce sentencing disparities by mandating that federal judges set penalties according to guidelines that explicitly and uniformly incorporated "relevant" mitigating and aggravating facts. The "guidelines" were promulgated by a semi-independent commission, the United States Sentencing Commission (which draws commissioners from the judiciary and elsewhere), and were extensive, complicated, and detailed from the outset. For approximately fifteen years, these guidelines mechanically "generated" the sentences federal criminals received: each aggravating fact increased a defendant's offense level by a fixed amount, which in turn translated into a fixed increase in the required sentencing range. If a judge found many aggravating facts, the mandatory guidelines sentence would substantially exceed the minimum statutory sentence for the underlying offense.

The practical consequence of sentencing under the mandatory guidelines was that judges, not juries, were determining facts that dramatically increased offender sentences (Schanzenbach and Tiller 2005, pp.9-10). At the same time, however, judges lost nearly all of their discretion to select a sentence anywhere within the minimum and maximum statutory bounds for a crime. The guidelines reduced the discretionary range to a few months, with the constraints set by a limited number of guidelines-relevant "facts." Judges did retain sentencing power in the sense that they could unlawfully manipulate their factfinding, but the SRA both required judges to record their factual conclusions and made judicial factfinding subject to appellate review.

Sentences have always been constrained by statutory maximums (see U.S.S.G. §§5G1.1, 5G1.2), but under the new guidelines system, *which* maximum applied could turn on a judge-found fact. Thus, a pre-*Apprendi* "statutory maximum" could potentially be *any* maximum that was provided for by the statute containing or related to an offense. Take, for example, a defendant convicted of a relatively minor drug crime under 21 U.S.C. §841 (which specifies many distinct drug offenses). If, as a predicate to convicting the defendant, the jury determined that the crime involved at least a small quantity of drugs, courts nevertheless considered it legal to sentence above the statutory maximum for the minor offense if the judge concluded by a preponderance of evidence that a very large quantity of drugs had actually been involved. This was considered permissible because larger quantities of drugs were dealt with elsewhere in the statute and because a larger quantity determination provided for a much higher maximum sentence. Put another way, pre-*Apprendi*, a judge-found fact could shift an offense from one statutory provision to another (with a higher maximum).

The Supreme Court was faced with the constitutionality of a state-level version of that sentencing situation in *Apprendi v. New Jersey*, 530 U.S. 466 (2000). The defendant in that case was convicted of a firearms crime that carried a statutory penalty range of five-to-ten years. At the defendant's sentencing, the prosecutor sought a "hate crime" enhancement, which the court subsequently granted based on the judge's conclusion, by the preponderance of the evidence, that the crime was racially motivated. The defendant was sentenced to a 12-year prison term, which was in excess of the statutory maximum for the crime of conviction. The Supreme Court concluded on June 26, 2000, that the 12-year sentence violated the defendant's Sixth Amendment right to a jury trial and his right to have the fact of his racial motivation proved beyond a reasonable doubt rather than by a preponderance of the evidence.

Importantly, however, the Supreme Court's holding did not extend to all sentencing facts. *Apprendi* does not require a jury to determine criminal history facts, which are both necessary and important in calculating a federal sentence under the sentencing guidelines. This feature of *Apprendi* originated in the Supreme Court's then-recent decision in *Almendarez-Torres v. United States*, 523 U.S. 224 (1998), which held that past offenses need not be proven to the jury for the purpose of applying a recidivist statute. The Court admitted that *Almendarez-Torres* appeared inconsistent with the underlying rationale of *Apprendi*, and some justices have continued to suggest that the Court reconsider the jury's role in determining facts of prior convictions (see, e.g., *Shepard v. United States*, 125 S.Ct. 1254, 1264 (2005)). The wording of the Court's holding in *Apprendi*, however, is precise about the types of facts that were covered: "[o]ther than the fact of a prior conviction, any fact that increases the penalty for a crime beyond the prescribed statutory maximum must be submitted to a jury, and proved beyond a reasonable doubt" (p.490).

Even excluding criminal history facts, *Apprendi*'s expansion of jury trial rights did not eliminate the judicial determination of aggravating facts. *Apprendi* specified a particular set of conditions under which a defendant had a right to have a jury, not a judge, determine sentence-enhancing facts, and until *Blakely* and *Booker* were decided (in June 2004 and January 2005, respectively) judges continued to decide many facts not covered by *Apprendi* just as they had prior to the decision. Facts implicated by *Apprendi*'s rule requiring beyond-a-reasonable-doubt jury factfinding shared three characteristics. First, the fact could not be one of prior conviction. Second, the fact, if found, had to increase a sentence. Third, the fact had to cause the sentence to "bump up" against the statutory maximum for the offense.

As I demonstrate in Section 1.4, *Apprendi*'s limited scope in these three respects makes it possible to construct a quasi-experiment in which sentencing data can be used to study the consequences of broader jury trial rights.

1.3 The Jury Trial Rights Debate

The word "right" itself suggests something of value. Yet significant debate has emerged in the legal literature over the consequences of expanding jury trial rights for defendants in the context of existing criminal procedure, especially given the dominance of plea bargaining. Legal scholars have made every possible prediction about the consequences of broader jury trial rights. *Apprendi* and *Blakely* may have raised the necessary standard of proof for certain facts and may have provided defendants with a jury trial "option," but these gains could be minuscule or offset by the interaction of broader rights with existing features of the criminal procedure landscape. In this section, I summarize the arguments in the legal literature over the likely consequences of broader jury trial rights and then organize those arguments around the predictions of a simple plea bargaining model (see Appendix).

In a provocative article in the Yale Law Journal, Bibas (2001a) asserts that *Apprendi*-like expansion of jury trial rights may work to the detriment of criminal offenders by allowing prosecutors to force hostile jury trials. In the federal system a jury trial right renders a jury the default factfinder, and prosecutors have the right to veto a jury trial waiver under Federal Rule of Criminal Procedure 23. Therefore, if judges are defendant friendly relative to juries or if judges can at times blunt prosecutorial or legislative excesses (Bowman and Heise 2001, 2002; Leipold 2005), a broader jury trial right may increase sentences because prosecutors will be free to force a jury trial when the jury is expected to favor the prosecution.

Bibas also suggests that defendants may be harmed by expanded jury trial rights because prosecutorial jury trial costs exhibit substantial scale economies. On this account, both the benefits to defendants of pleading guilty and the costs to prosecutors of a jury trial are so high that the only realistic consequence of adding a jury trial right to the determination of sentencing facts may be that defendants virtually always plead guilty to *Apprendi*-covered facts instead of litigating them—potentially successfully—before a judge. The intuition is that if a prosecutor is forced to take one fact to a jury, the additional cost of taking all other facts to a jury is relatively low. A jury trial as to all facts is highly risky for most defendants, and therefore the average defendant may prefer to concede *Apprendi*-covered facts rather than hazard a failed plea negotiation. Ultimately, however, Bibas admits that the consequences of broader jury trial rights under *Apprendi* must be empirically evaluated (p.1167).

King and Klein (2001b) disagree with the counterintuitive notion that expanding a defendant's jury trial rights might strengthen a prosecutor's bargaining position. They argue instead that broader jury trial rights should unambiguously benefit criminal offenders. If a sentencing-relevant fact is newly covered by *Apprendi* (i.e., the fact, if found, would increase the penalty for a crime beyond the prescribed statutory maximum), they note that the fact must be proved beyond a reasonable doubt. Moreover, as a practical matter, defendants are likely to have more influence over the identity of their factfinder. While prosecutors may technically be able to defeat a jury trial waiver, the cost of doing so is high, meaning that under many circumstances, defendants will have a choice between a jury and a judge. Structurally, broader jury trial rights may insulate defendants from overly aggressive prosecutors and judges.

To organize and synthesize these competing contentions, I present a simple model of factfinding with criminal jury trial rights and standards of proof in the Appendix. The model assumes that a defendant can try to waive a jury trial right, but that prosecutors can defeat that waiver at some cost, a move that is allowed under Federal Rule of Criminal Procedure 23. I take into account that the expected outcomes of bench trials and jury trials may differ, that jury trials are more expensive for prosecutors than bench trials, and that a prosecutor's decision to object to a jury trial waiver may be costly in other ways (e.g., irritating the judge). The model generates empirical predictions by allowing the comparison of a defendant's expected outcome with a post-*Apprendi* jury trial right to that under the prior judicial factfinding regime. Figure 1 depicts the policy change for sentencing facts.

It is straightforward to show, perhaps counterintuitively, that under certain conditions defendants are made worse off by an expansion of jury trial rights. Specifically, if there are many cases in which a judge using a "preponderance of evidence" standard is more defendant-friendly than a jury using a "beyond a reasonable doubt" standard (perhaps because judges view guidelines sentences as overly harsh), but objecting to a jury trial waiver is less costly to the prosecutor than allowing a bench trial, then a jury trial rights expansion can harm defendants on average (see Appendix for more detail). Whether these conditions are satisfied, and, if so, whether there are any significant consequences for defendants, are empirical questions.

Taking a different, non-bargaining approach, Stuntz (2004) argues that expanded criminal procedural protections, like *Apprendi*'s expansion of jury trial rights, will have no effect on defendant outcomes because "litigants in criminal cases do not bargain in the shadow of the law. Rather, they bargain in the shadow of prosecutors' preferences, budget constraints, and political trends" (p.2548). Stuntz asserts that because federal substantive law gives federal prosecutors almost unlimited power in charging a defendant, almost any federal crime can be framed and then prosecuted in a way that can plausibly jail a criminal almost indefinitely. His view thus suggests that an expansion of jury trial rights under *Apprendi* or later under *Blakely* should not matter (or not matter very much) to offenders' sentencing outcomes. Prosecutors will simply implement low-cost changes such as altering how criminals are charged (in particular, charging them with more offenses, thereby making sentencing facts irrelevant) to achieve the same results.

The arguments made by the authors above and other scholars (e.g., Lillquist 2004) have identified a number of provocative ideas and competing effects, from the potential benefits of strategic commitment, to the value of the jury option, to the effect of raising the standard of proof, to the idea that prosecutors make, in effect, all important decisions in criminal law. But analytical scholarship on the possible consequences of *Apprendi* (and, for that matter, empirical work that studies judge and jury decision making outside of the broader strategic environment in which criminal procedure operates) does too little to assist policy makers and judges in understanding the likely aggregate effects of expanding Sixth Amendment protections. In the remainder of this paper, I seek to contribute to this debate by empirically assessing the consequences—both the direction and the magnitude—of expanding jury trial rights.

1.4 Data and Empirical Strategy

Legal scholars have argued variously that *Apprendi*'s expansion of jury trial rights has hurt defendants, that it has helped them, and that procedural protections like jury trial rights simply do not matter. The important message from this dialogue in the legal literature, however, is simply that the consequences for offenders of jury trial rights cannot be known without looking to actual data. No jury rights scholars have sought to *measure* the effects of broader jury trial protections, even though understanding those consequences is necessary both for improving sentencing policy and broadly understanding how criminal procedure and its protections translate into real-world outcomes.

Below, I use sentencing data from the United States Sentencing Commission and the structure of the federal sentencing guidelines to measure the effect of *Apprendi*'s expansion of jury trial rights on defendant sentence length. To do this, I create a test that combines the limited holding of *Apprendi* with the method by which sentences were calculated under the then-mandatory federal guidelines. I use a difference-in-differences estimation approach, employing either simple means comparisons or a regression framework to calculate my estimates. Although not perfect, difference-in-differences techniques are quite robust, as I explain in more detail below (see Card and Krueger 1995; Angrist and Krueger 1998, 2001).

My empirical work has two key building blocks:

1. *Apprendi* held that a Sixth Amendment jury trial right applies to most sentencing facts that, if found, would generate a sentence that exceeds the statutory maximum.
2. Under the federal sentencing guidelines, recidivist offenders (i.e., those in higher criminal history categories) will be, holding all else even, closer to the applicable statutory maximum than offenders with little or no criminal history.

The first building block tells us that if broader jury trial rights "matter" to defendant sentences at all, the effect should be stronger (and thus potentially measurable) for categories of offenders who are closer, all else even, to the applicable statutory maximum. The second building block tells us that, under the guidelines, for any particular offense, criminal offenders in higher criminal history categories will be closer to the statutory maximum. Therefore, if sentencing outcomes change in some systematic way for offenders in high criminal history categories (relative to low criminal history categories) around the time

Apprendi was decided (and that change is not easily attributable to some other cause), then this change can be interpreted as evidence of the consequences of broader jury trial rights.

The approach I take estimates the "net" effect of the expansion of jury trial rights under *Apprendi*, meaning that it captures both (1) the underlying consequences of the new right to a jury trial for specific facts and of the higher standard of proof for those facts and (2) the effect of prosecutorial and judicial attempts to counteract (or magnify) the underlying effect through charging adjustments and factfinding manipulation. Given that such discretion is a permanent feature of the criminal justice system, this aggregate measure is the one of interest. The "net" effect calculated by my empirical approach is best interpreted as a lower bound on the consequences of expanding of jury trial rights generally. I am unable to compare the outcomes of a pure "control" group and a pure "treatment" group, but instead must compare a "less affected" group and a "more affected" group. *Apprendi* did not implicate high recidivists who were nowhere near the statutory maximum, but potentially did affect new criminals who had many aggravating offense facts. Moreover, *Apprendi* did not apply to all aggravating sentencing facts. Even after *Apprendi*, an aggravating sentencing fact continued to be decided by a judge by a preponderance of evidence so long as the fact in question did not push a defendant over the statutory maximum. In other words, *Apprendi* affected fewer offenders and covered fewer sentencing facts than would a jury trial right applicable to all aggravating sentencing facts.

My basic empirical approach might be problematic if *Apprendi* "changed" the composition of groups compared in the test described above. For example, if *Apprendi* made it more or less difficult to prove criminal history facts (these are sentencing facts, after all, which can cause a sentence to exceed the statutory maximum), then offenders who would previously have been placed in one criminal history category might instead be placed in another after *Apprendi*. If that were the case, then the effect measured would in part be an artifact of the shifting composition of the groups compared. But, as noted above, *Apprendi* *did not* change how criminal history facts were determined—the holding of the *Apprendi* decision *expressly excluded* facts of prior conviction or, more generally, criminal history determinations from its coverage.

My approach might also raise concerns if *Apprendi* caused or allowed prosecutors or judges to manipulate the determination of criminal history facts. But, under the guidelines, prosecutors were limited in their ability to manipulate basic criminal history determinations, and, more importantly, they had no incentive to *reduce* an offender's criminal history score, a move that would be necessary to account for my results, as I explain in more detail below. Judges were also limited in their ability to manipulate basic criminal history determinations, and, in any event, because judicial manipulation is likely to be visible, I am able to control for it statistically.

Therefore, neither the *Apprendi* decision nor the behavior of prosecutors or judges is likely to have altered the composition of criminal history groups. Moreover, my empirical approach controls for differences over time in observable demographic changes in composition. As a result, using sentencing data from the United States Sentencing Commission, which I describe below, I can evaluate the consequences of expanding jury trial rights by comparing the outcomes of individuals who are in different criminal history categories but are otherwise demographically comparable.

1.4.1 Sentencing Under the Federal Guidelines

My empirical approach makes use of the mechanical calculations required by the guidelines during my sample period (1998-2001). A judge calculating a presumptive guidelines sentence begins by determining the base offense (according to the indictment, information, or plea agreement), and identifies a base offense level. If the offender pled guilty to robbery, for example, the 2004 version of the guidelines (U.S.S.G. §2B3.1) specifies a mandatory base offense level of 20. Next, a judge considers the applicability of specific offense characteristics. For a robbery offense, there are seven different characteristics the judge must address, such as whether and how a firearm was used in the commission of the crime (§2B3.1(b)(2)) and how much was lost as a result of the crime (§2B3.1(b)(7)). If the robbery occurred during a carjacking, for example, the judge would add two levels to the already-earned 20. Additional offense characteristics would add to the total, since, in general, offense characteristics are not mutually exclusive.

Once the base sentencing offense level and additional level increases due to offense characteristics have been totaled, the guidelines direct that a judge make further adjustments (see U.S.S.G. §1B1.1(c)). In the robbery context, a judge would be required to ask, among many other possibilities, whether the offense was a hate crime (§3A1.1), whether the offender played an aggravated role in the crime (§3B1.1), whether a minor was used to commit the crime (§3B1.4), and whether the offender subsequently obstructed justice (§3C1.1). There are more than ten additional non-exclusive adjustments that can be made, and each adjustment can add from one to four levels to the total offense level. Next, the judge repeats the earlier steps for other offenses, if there are other offenses, and then applies a set of rules (§3D1.1 to §3D1.5) to combine all offense calculations into one final offense level.

The judge's next task is calculating an offender's criminal history category. The judge does not use the indictment or the plea agreement to establish criminal history, but instead relies on an agent of the court, a probation officer, to draft a presentence report (§6A1.1). The defendant is not entitled to waive the preparation of the report (§6A1.1(b)). The probation officer is not a member of the prosecutor's team, but is directed by and reports to the sentencing judge (Cohen and Fields 2004) and plays a fairly central and independent role in determining sentencing outcomes (Bunzel 1995). A probation officer does receive information from the prosecution, so it is conceivable that prosecutors, post-*Apprendi*, may be able to affect probation officers' criminal history determinations by altering the types of information shared.

As a general matter, however, it is no easy task for prosecutors to influence an offender's criminal history category because probation officers and the reports they submit to the court are at least somewhat insulated from manipulation. Consider a recent description of the probation officer's task:

> By order of the court, the officer makes a thorough investigation – a presentence investigation – into the circumstances of the offense and the offender's criminal background and characteristics. The officer gathers information in two ways: by conducting interviews and by reviewing documents. The cornerstone of the investigation is the interview with the offender, during which the officer inquires about such things as the offender's family, education, employment, finances, physical and mental health, and alcohol or drug abuse. The officer also conducts a home visit to assess the offender's living conditions, family relationships, and community ties and to detect alcohol or drugs in the home. Besides interviewing the offender, the officer interviews other persons who can provide pertinent information about the offender and the offense, including the defense counsel, the prosecutor, law enforcement agents, victims, the offender's family and associates, employers, school officials, doctors, and counselors. The officer also reviews various records and reports, including court records, financial records, criminal history transcripts, probation/parole/pretrial services records, birth/marriage/divorce records, school records, employment records, military service records, school records [sic], med-

> ical records, and counseling and treatment records. The officer verifies the information gathered, interprets and evaluates it, and presents it to the court in an organized, objective report called the presentence report (Federal Corrections and Supervision Division 2000, p.1).

This description is surely aspirational, but it does indicate that probation officers do not generally rely entirely on prosecutors for their information. Furthermore, because prosecutors and defense attorneys can object to material included or missing in the report as well as to the preliminary guidelines calculations made by the probation officers (see U.S.S.G. §§6A1.2, 6A1.3), it seems highly unlikely that probation officers would systematically depend on prosecutorial information without some attempt to verify the information they receive.

That does not mean that prosecutorial manipulation of criminal history facts does not occur, however. In particular, when a plea agreement includes a stipulation under Federal Rule of Criminal Procedure 11(c)(1)(C) as to criminal history, that stipulation may be accepted by the court even when it is not entirely accurate. But manipulation of criminal histories in plea agreements, if it were common and if it had any effect, would affect only plea-bargained cases. Empirical analysis of only those cases that went to trial provides a partial check on this, although there is the possibility that *Apprendi*-induced manipulation might have a selection effect, causing a different set of cases to be resolved through plea bargaining. Below I find that broader jury trial rights had effects on offender sentences both in jury *and* plea-bargained cases (see Tables A1 and A2), indicating that prosecutorial manipulation of criminal history categories through plea agreements cannot alone account for my results.

The broader possibility that prosecutorial manipulation might affect my results seems remote. The numbers of offenders in each criminal history category pre- and post-*Apprendi* remain remarkably stable (see Figures 3 and 4). More importantly, it is difficult to construct a plausible story capable of explaining my results in which a prosecutor would find it newly worthwhile—i.e., on account of *Apprendi*—to stipulate to an inaccurate criminal history category or to alter the type of criminal history information provided to a probation officer.

One might think, for example, that a prosecutor could benefit post-*Apprendi* from manipulating criminal history information downward if a certain sentencing fact would cause the guidelines sentence for an offense to exceed the statutory maximum with, but only with, the inclusion of a prior conviction. But criminal history information essentially increases an offender's sentence with certainty. More importantly, under *Apprendi* a prosecutor was free to have a judge determine sentencing facts so long as the sentence was capped by the applicable statutory maximum (Bibas 2001a, pp.1158-59). As a practical matter, this means that a prosecutor's decision to reduce an offender's criminal history score could never have increased an offender's statutory exposure and would have more likely than not reduced it.

Alternatively, consider whether prosecutors might have found it newly worthwhile to force an offender's criminal history score upwards. Doing so would worsen the *Apprendi* problem for those offenders, but since criminal history facts are perhaps easier to prove than other facts—especially in light of the *Apprendi* exception—manipulating criminal history facts in this way might have freed up additional resources to fund newly required jury trials as to certain sentencing facts. To see how this incentive might have affected the composition of my comparison groups, note that there is both a substitution and income effect to an increase in the cost of proving only certain sentencing facts, at least assuming prosecutors have limited resources. Because the price of proving offense facts, but not criminal history facts, rose with *Apprendi*, the rule in that case should have led to an unambiguous decrease in a prosecutor's willingness to use offense-related sentencing facts and an ambiguous effect on the attractiveness of criminal history facts because income and substitution effects would have worked against each other.

But even if the substitution effect dominated, this prosecutorial manipulation story runs against the results I find below, which show a reduction in sentences for high criminal history types. To generate that effect, prosecutors would have had to push low-sentence offenders into higher criminal history categories. This is at odds with the substitution theory identified above because the increase in the cost of prosecuting offense-related sentencing facts would have applied only to those "close" to the statutory maximum—in other words, high-sentence offenders. As a result, if it were to have any effect at all, this sort of prosecutorial manipulation would weaken my findings.

Once a probation officer has collected all the information relevant to an offender's criminal history, he translates that data into criminal history points using guideline §4A1.1. The guideline directs the probation officer (and judge) to apply certain point scores for each of an offender's previous convictions, where the number of points assigned to each earlier crime turns on the seriousness of the offense and how recently the crime was committed (§§4A1.1, 4A1.2).

During the period I study, judges were required by law to incorporate all information covered by the guidelines into the calculation of an offender's sentence, since provisions of the sentencing guidelines were mandatory unless specifically stated otherwise (18 U.S.C. §3553(b)(1)). Schanzenbach and Tiller (2005) have argued that sentencing judges use their factfinding and legal decision making discretion to pursue their political preferences and will "manipulate the rules and structure of the Sentencing Guidelines to the extent possible" (p.4). Yet accounting for judicial manipulation of offense-level factfinding and the strategic use of offense-level departures is important to an accurate measurement of the aggregate consequences of jury trial rights. If judges can reduce, magnify, or completely eliminate the benefits or burdens of Sixth Amendment rights, the effects of this behavior should be included in the final analysis of the value of those rights.

The results of my empirical work might be misleading if judicial manipulation extended to altering criminal history determinations, however, because I would not be comparing the same types of offenders pre- and post-*Apprendi*. But there is nothing to suggest that judges might have manipulated criminal history factfinding determinations in response to *Apprendi*. Judges typically accept probation officers' presentencing recommendations and are required by law to include in their sentencing calculations criminal history information of which they are aware. Moreover, the trends in the numbers of offenders in each criminal history category remain remarkably stable around the time of the decision (see Figures 3 and 4).

In Section 1.6 below, I address the fact that a judge can legally "depart" from a criminal history determination under U.S.S.G §4A1.3 if the judge believes that an offender's criminal history score does not accurately reflect his actual history. Departures do not drive my results. As Schanzenbach and Tiller (2005) note, "the Criminal History Category is more or less set by past judicial determinations and is not as easily manipulated as the adjustments to the offense level calculations" (p.7 n.7). And unlike other sentencing adjustments, criminal history issues are rarely litigated. All of this goes to show that, like an offender's basic demographic information, criminal history scoring was unaffected by the ruling in *Apprendi* and can therefore be used to create comparison groups.

After the court settles on a criminal history category, the judge turns to Chapter 5, Table A, of the applicable sentencing guidelines manual (see Figure 2). The judge finds the correct sentencing guidelines range by locating the cell where the offender's "offense" level and "criminal history category" intersect. For example, if an offender had a clear record and was convicted of robbery (a base offense level of 20) with no offense characteristics or other upward adjustments, he would receive a sentence of between 33 and 41 months. Prior to *Blakely*, unless a judge exercised his departure power, this range was mandatory under the guidelines.

1.4.2 U.S.S.C. Sentencing Data

The Sentencing Commission has collected data on federal sentencing outcomes for years as required by law under the SRA.[1] The U.S.S.C. collects this data primarily for use in evaluating whether its sentencing guidelines are effectuating their purposes such as, most importantly, reducing federal sentencing disparities (see U.S.S.C. 2004a). The quality of the Commission's data, at least with respect to individual (as opposed to organizational offenders), appears to be very high. The Sentencing Commission recently described the data collection process in a publication reviewing studies of the SRA's effectiveness:

> Each federal court is required by law to transmit several sentencing-related documents to the Commission. Presentence reports, judgment of conviction forms, statements of reasons, and plea agreements are received for the vast majority of felony and serious misdemeanor cases. Staff in the Commission's Monitoring Unit assign each case a unique identifier and enter information on over 200 variables involving guideline applications, offender characteristics, and case processing factors. Expansion of the dataset has added elements through the years (U.S.S.C. 2004a, p.D-1).

U.S.S.C. analysts and criminologists interested in studying rehabilitation and recidivism as well as race- and sex-based sentencing disparities have used this data for years (see, e.g., Hofer, Blackwell and Ruback 1999). The variables collected are not uniform over all years, but generally include timing data (month of sentence), court data (district and circuit of the sentencing court), demographic data (race, sex, age, education, citizenship, residency status, and other information, such as income and number of dependents). Further, as the

[1]The Federal Justice Statistics Resource Center makes federal sentencing data available to the public, as does the University of Michigan's Inter-university Consortium for Political and Social Research. I use data from 1998 to 2001. Less detailed federal sentencing data is available back to the early 1990s.

above description notes, information is collected regarding the offender's sentence, including not only the final tally, but also how the sentence was actually calculated (e.g., offense level, criminal history category, offense characteristics, and adjustments).

Legal scholars and economists have recently begun to use these data (see, e.g., LaCasse and Payne 1999; Mustard 2001; Schanzenbach 2005; U.S.S.C. 2004a, App.A), though for many important questions this data set alone is insufficient to study the causal relationships of interest. The data include a number of important demographic and court variables (the most significant omission is probably the identity of the sentencing judge), which can plausibly be treated as exogenous, but almost all of the remaining variables collected are ultimately jointly determined outcomes.

Accordingly, the quasi-experiment generated by the Supreme Court's unanticipated decision in *Apprendi* is a rare opportunity to study the consequences of a significant change in jury trial rights, as well as a major shift in federal sentencing policy.[2] My analysis uses three years of sentencing data: FY 1999, FY 2000, and FY 2001. *Apprendi* was decided on June 26, 2000,[3] so I am able analyze 21 months of pre-*Apprendi* sentencing and 15 months of post-*Apprendi* sentencing.[4] The total number of observations is large, as suggested by the Sentencing Commission's description of the collection procedure above.

Only those offenders who were convicted (and therefore received some sort of sentence) are observed. If *Apprendi* had an effect on the likelihood that an accused offender was ultimately convicted, my results may not capture the full effect of the change. But no one has suggested that *Apprendi* would affect offenders on the guilt/innocence dimension (nor is it clear exactly how this might happen); rather, *Apprendi* works at the margin of sentences, on the facts that raise a sentence from a base level upon conviction. Relatedly,

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[2] *Apprendi* was preceded by *Jones v. United States*, 526 U.S. 277 (1999). The *Apprendi* Court described *Jones* as "foreshadowing" the holding in *Apprendi* (p.476), but the ruling in *Apprendi* was in sharp contrast to existing precedent and is generally viewed as a dramatic departure from previous law. *Apprendi* has been described variously as the start of a "revolution" (Greenhouse 2002) and as having sent a "shock wave" (Lane 2002) throughout the legal community. King and Klein (2000) also discuss the case's likely implications.

[3] Because I only observe the month that an offender was sentenced, I treat all sentencing in the month of June as occurring pre-*Apprendi*.

[4] The Supreme Court decided a few other federal criminal law cases around the same time as *Apprendi*. See, e.g., *Carter v. United States*, 530 U.S. 255 (June 12, 2000) and *Castillo v. United States*, 530 U.S. 120 (June 5, 2000). But these cases dealt with the definition of particular crimes, and there is no evidence to suggest that the crimes in question would be correlated with criminal history categories.

only individuals who are arrested and prosecuted can be convicted, so if *Apprendi* influenced the probability that a criminal was arrested or prosecuted, my results may not be entirely accurate. However, it seems even less likely that a change in a procedural rule would work along the arrest or decision to prosecute margin.

The original data set was trimmed to eliminate certain aberrational cases: an observation was dropped if the offender was being resentenced; if the offender had a missing total prison sentence; if a non-prison type of sentence was imposed; if the offender was sentenced to community confinement; or if the offender was sentenced to more than a day but less than a month in prison or to time served.[5] The outcome variable, length of sentence in months, ranges from 0 to 990 months. Life sentences were recoded as the maximum recorded sentence. Schanzenbach (2005, p.68) excludes life sentences, finding they matter very little. I do not do this, since many sentencing facts can lead to the imposition of life sentences, especially for drug crimes (see King and Klein 2000, App.A).[6]

Table 1 displays descriptive statistics of the data I use for my analysis, with the data set divided into 6-month intervals. The column of means for the third and fourth quarters of fiscal year 2000 includes all offenders who were sentenced for the three month periods before and after *Apprendi*. Therefore, comparing the column containing the first two quarters of fiscal year 2000 and the first two quarters of fiscal 2001 offer an initial picture of whether *Apprendi* had any effect on the types of offenders progressing through the criminal justice system. The only notable change is the decline in the number of offenders receiving a jury trial, though this may be part of a long-term trend toward more prevalent plea bargaining. I discuss this trend in more detail below.

1.5 Empirical Model and Basic Results

The limited scope of *Apprendi*'s holding, when combined with the guidelines' grid approach to calculating sentences, makes the decision almost ideally suited for studying the conse-

[5]This process removed a total of 13,805 cases (or 2,920 cases, 1,914 cases, 2,630 cases, 4,096 cases, and 2.245 cases, respectively). Mustard (2001, p.298) and Schanzenbach (2005, p.68) note similar reductions in their samples after eliminating missing or irrelevant data.

[6]If I omit life sentences, the magnitude of my coefficients drops by about half (they remain easily statistically significant), suggesting that guaranteeing a federal jury trial option reduces a prosecutor's ability to win a life sentence. presumably following a jury trial.

quences of broader jury trial protections. *Apprendi* expanded jury trial rights in a very precise way—to cover, in addition to traditional elements of a crime, sentencing facts that would cause a sentence to exceed the applicable statutory maximum. Therefore, if there exists a group of offenders whose sentences are "closer" to the statutory maximum in a predetermined way, then it is possible to identify the direction of any effect and measure the potential magnitude of applying jury trial rights to sentencing facts. In this section, I begin to evaluate the effect of *Apprendi*'s expansion of jury trial rights on sentence length.

1.5.1 Graphical and Means Analysis

Figures 5 and 6 show the evolution of sentence means by criminal history types over my sample period. As Figure 2 shows and the discussion above notes, guidelines sentences are based on an offender's placement in one of six basic categories (I-VI). For ease of comparison, I have combined the six categories into three larger groups (low-level (I & II), mid-level (III & IV), and high-level (V & VI)) with each group containing two categories (Figure 5) and two even larger groups (low-level (I, II, III) and high-level (IV, V, VI)) with each group containing three categories (Figure 6). Both figures reveal a narrowing in average sentence length between criminal history types around the time *Apprendi* was decided, offering preliminary evidence that broader jury trial rights under *Apprendi* benefited criminal offenders.

Table 2 presents a similar pre- and post-comparison (using three groups) looking directly at the differences in mean sentences. Pre- and post-*Apprendi* offenders in any particular criminal history group are very similar demographically. (Similarly, Figures 3 and 4 show the evolution of the number of offenders in two different criminal history groupings, revealing that the groups appear markedly stable around the time of *Apprendi*.) The table suggests only a few slight differences over time, and there is no evidence of a shifting composition for any of the groups—something we might expect to see if, for example, prosecutors or judges began to manipulate criminal history findings or if other unobserved changes or trends were at work.

There is one exception: the percentage of offenders pleading guilty has grown over the period coinciding with *Apprendi* and that growth seems slightly larger for the high criminal history group.[7] This growth, however, may be the product of the long-lived and continuing trend toward fewer and fewer jury trials (Fisher 2003; Mnookin 2005). The Sentencing Commission recently concluded that "[i]t is clear from the data that plea bargaining has continued, and even expanded, in the guidelines era. Guilty plea rates steadily increased from 87 percent in the years preceding the guidelines to 96.6 percent in 2001" (U.S.S.C. 2004a, p.30).[8] Moreover, higher plea bargaining rates alone, however interesting, cannot be interpreted as an outcome that is necessarily pro-defendant or pro-prosecutor—the evolution of plea rates tells us little about the overall consequences for offenders over time without knowing the sentences that result from the agreements.[9]

Across criminal history groups, there are some interesting differences: repeat offenders appear to offend earlier and are more likely to be male, non-white, and less educated, findings which are consistent with the Sentencing Commission's Recidivism Project (U.S.S.C. 2004b, pp.11-15). But these differences do not change over time. This suggests that pre-*Apprendi* offenders can serve as a control for evaluating differences in outcomes in the post-*Apprendi* period. Other than a differential rise in plea rates for higher criminal history types, the only notable difference across the three groups pre- and post-*Apprendi* is an outcome—average sentence length.

The differences in average sentences calculated in the far-right column of Table 2 suggest preliminarily that broader jury trial rights benefit defendants. Sentences for offenders in the low criminal history group changed only slightly, if at all. Compare this "no change" result to the criminal history categories in the two lower panels. For offenders in the middle category (categories III and IV), simple calculations indicate that *Apprendi* (and its requirement that certain facts be proved to a jury and beyond a reasonable doubt) reduced the average sentence by over 4 months (a 7% reduction in the mean sentence) relative to those in the low criminal history category. The effect of *Apprendi* appears significant in

[7]In results not reported here, I have preliminarily explored whether there is a relationship between *Apprendi* and plea bargaining. I find that offenders more likely to be covered by *Apprendi* were slightly more likely to plead guilty, though my results are not particularly robust.

[8]For an early assessment of the effect of the guidelines on plea bargaining rates, see Karle and Sager (1991) and Dunworth and Weisselberg (1992), who generally find mixed results. with reduced plea bargaining post-guidelines for some offenses.

[9]I focus here on the end-of-the-day outcome for defendants—the sentences they receive. Many have deplored the use of plea bargaining, regardless whether plea bargained sentences mimic those that would have been produced by jury trials (e.g., Alschuler 1983).

the bottom panel also—offenders in the highest criminal history category (V and VI) had sentences that were on average almost 6 months shorter (approximately a 6% reduction).

Importantly, the accuracy and interpretation of the estimates in Table 2 do not depend on whether offenders in the three criminal history groups are similar to each other; in fact, we know that they are not: offenders in the different groups have noticeable differences in their observable demographic characteristics. A simple comparison of means is nonetheless valid so long as the characteristics of offenders within a criminal history category do not change differentially across groups around the time that *Apprendi* was decided. If there were a uniform change in offender characteristics or offense severity over time (due to a "get tough on crime" campaign, for example), then all that would have to be shown in order for the unadjusted difference-in-differences estimates in Table 2 to hold is that the change was not correlated with or specific to any of the particular criminal history categories.

On the other hand, if prosecutors were able to manipulate offenders' criminal history groups and for some reason began moving offenders with less serious crimes into higher criminal history categories around the same time as *Apprendi* was decided, the differences displayed in Table 2 might be misleading. Alternatively, if individuals in higher criminal history groups began around the time of *Apprendi* to commit less serious offenses (or commit them with fewer aggravating circumstances), then the results above again might be inaccurate. But the pre- and post-*Apprendi* averages listed in Table 2 offer no evidence of any change in group composition, nor do the numbers of offenders in each group change much over time (see Figures 3 and 4). As I noted above, it is also difficult to devise a prosecutorial or judicial manipulation hypothesis that both makes sense and could generate the results in Table 2. Nevertheless, to further reduce the possibility that alternative explanations account for the differential changes in sentence length calculated above, the next subsection employs a regression framework.

1.5.2 Regression Analysis

Although the differences in group means in Table 2 are suggestive, I am able to control for other potentially confounding factors by using regression analysis. I identify the relationship between the expansion in jury trial rights introduced by *Apprendi* and average offender sentence length by estimating the following equation using ordinary least squares:

$$\begin{aligned} Sentence_{ijt} = \; & \alpha + \beta_1 Race_{ijt} + \beta_2 Education_{ijt} + \beta_3 Age_{ijt} + \beta_4 Sex_{ijt} \\ & + \beta_5 NumDep_{ijt} + \beta_6 GuidelinesYear_{ijt} + \beta_7 Apprendi_t \\ & + \beta_8 CrimHist_{ijt} + \beta_9 Apprendi_t \times CrimHist_{ijt} + \epsilon_{ijt} \end{aligned} \tag{1.1}$$

where i indexes criminals, j indexes districts, and t indexes month of sentencing. The dependant variable is sentence length in months.

To account for disparate sentencing treatment on the basis of demographic differences, I employ the following controls: *Race* is a vector that includes three dummy variables (for black, white, and Hispanic, with other omitted); *Education* includes five dummies for different levels of attainment (high school, vocational or military training, some college, college degree, and some graduate training, with no high school diploma omitted); *Age* includes dummies for different age groups (18-29, 30-44, 45-60, and greater than 60, with less than 18 omitted); *Sex* is one if the offender is a male; $NumDep$ is the number of dependants; and $GuidelinesYear$ is a vector of dummy variables to control for any amendments or other changes over time in the Guidelines Manuals used to sentence defendants.[10]

The variable *Apprendi* is a dummy variable equal to zero for months before and including June 2000 and equal to one thereafter. $CrimHist$ is a vector of varying dimension, made up of either two (high/low), three (high/mid/low), or six (I-VI) dummy variables, designed to capture the direct effect of criminal history on offender sentences. If there were no other trends or changes during my sample period, the coefficient estimated on *Apprendi* would capture the effect, and only the effect, of the decision's expansion of jury trial rights *on all groups*, but since the timing of the decision might well be correlated with other developments, a simple calculation of the difference between the pre- and post-period is unlikely, in general, to be very reliable. However, because it is possible to identify groups in the pre- and post-period that are differentially affected by *Apprendi*, yet otherwise similar, one can use the regression equivalent of the difference-in-differences estimator applied in Table 2 to identify the consequences of *Apprendi*'s expansion of jury trial rights.

[10] I do not report or discuss at any length the direct relationships between these demographic variables and sentence length because at least some of them have been studied by others (see, e.g., Schanzenbach 2005; Mustard 2001; Hofer, Blackwell and Ruback 1999; U.S.S.C. 2004a, App.A). My results are consistent with these researchers' findings and intuition.

The variables of interest here are the interactions *Apprendi* $\times$ *CrimHist*. The rule of *Apprendi* did not affect all types of offenders equally: *Apprendi* only bites as an offender's sentence for a particular offense approaches the statutory maximum. Since those offenders in a higher criminal history category are more likely to be near the relevant statutory maximum, *Apprendi* $\times$ *CrimHist* represents at least part of the effect of *Apprendi* on defendant sentence length. Alternatively, *Apprendi* $\times$ *CrimHist* may be interpreted as the disproportionate effect of expanding jury trial rights on those more likely to be affected by the decision. As noted above, unlike in more standard difference-in-differences applications, well-defined groups do not exist in this quasi-experiment. The sentence for an individual with no criminal history points *can* be constrained (because of offense characteristics and upward adjustments) by the statutory maximum. My estimates should thus be interpreted as lower bounds on the effects of *Apprendi*'s expansion of the jury trial right.

More formally, one can interpret this approach to measuring the consequences of *Apprendi*'s broadening of jury trial rights as a reduced-form substitute for an instrumental variables model in which the causal variable of interest is "the probability that *Apprendi* binds" and where the instruments are criminal history categories. Ideally, I would observe the probability that *Apprendi* binds, and then instrument for that probability with the criminal history categories (because *Apprendi*'s binding is endogenous to sentence length). This implies that the OLS coefficients on *Apprendi* $\times$ *CrimHist* are scaled by the difference between criminal history groups in the proportion of cases where *Apprendi* binds. For example, if *Apprendi* never applied to low criminal history types, but applied only 50% of the time to recidivists, then the effect of expanding jury trial rights would be double the estimate since there is only a 50% difference in *Apprendi* coverage.[11]

I begin my analysis using only two criminal history categories (high and low). The results are recorded in Table 3. I find that *Apprendi*'s broadening of jury trial rights reduced the average sentence of offenders in the high criminal history category (relative to those in the low criminal history category) by approximately 6 months, which is over 10% of the mean sentence of all offenders and more than 5% of the mean sentence for high criminal history offenders. Including demographic controls thus does not change the gist of

[11]In subsection 1.6.1 below, I explore this issue more fully by limiting my sample to offenders who are systematically more likely to be affected by *Apprendi*. As expected, the magnitude of the estimates I calculate are much larger.

the simple differences in means presented in Table 2. For offenders more likely to be affected by *Apprendi*'s expansion of jury trial rights, sentences dropped by around 6 months relative to the post-*Apprendi* sentences received by other types of federal offenders.

Table 3 also reports coefficient estimates on selected demographic variables described above. These estimates (reported and unreported) are straightforward in their interpretation: college-educated, older, white, and female offenders are likely to receive lower sentences, all else even. These results should be interpreted descriptively, not causally. Education and race are also correlated with income, which obviously affects legal representation and much more. I do not control for underlying offense characteristics here, so it may be that shorter sentences of, say, women may be attributable to less severe offenses.

The first column of Table 3 presents results from the estimation of equation (1.1) described above. In the remaining columns, I add further controls to reduce the possibility that an omitted variable may bias my results. In the second column, I control separately for citizenship, residency status, number of dependents, and the year of the sentencing guidelines manual that was applied to the case. I introduce the first two because non-U.S. citizens or residents are not distributed uniformly across criminal history categories (perhaps as a result of deportation, which may prevent them from being repeat offenders) and the treatment of non-citizens or residents may have changed over time.

In the third, fourth, and fifth columns, I add fixed effects to control for district and month effects. The inclusion of *District*, *Circuit*, *Month*, and *Circuit* × *Month* are designed to ensure that my results are not driven by differences between districts (or circuits) or by any trends specific to a particular circuit or set of circuits.[12] *Apprendi* applied uniformly to all districts (and all appellate circuits), and there was no preexisting district or circuit legal variation (all circuits had previously adhered to the approach invalidated in *Apprendi*). But the geographic distribution of offenders in different criminal history categories might not be uniform—repeat offenders, for example, could plausibly be concentrated in particular districts—and districts may have been on different pre-*Apprendi* trends. Therefore, it is important to control for such trends when possible. Relatedly, Schanzenbach

[12]For example, when district effects are introduced, inter-district variation is eliminated; instead, offenders within a particular district are compared, and the effects are then aggregated. The month fixed effects control for time trends that are common to all offenders, regardless of their criminal history status. As a consequence of including month effects, I cannot separately estimate the coefficient on *Apprendi*.

and Tiller (2005) argue that a district judge's use of discretion might turn on the political leanings of the circuit court judges reviewing his decisions. The inclusion of circuit and district controls can protect against such effects biasing my estimates. Importantly, when included, none of these controls eliminates or much reduces the magnitude or significance of the estimated coefficient on $Apprendi \times CrimHist$. The broader jury trial rights established by *Apprendi* reduced offender sentence length by approximately 6 months.[13]

To ensure that aggregation of the criminal history categories is not the source of the findings in Table 3, I report in Table 4 and Table 5 results for three and six criminal history categories, respectively. If anything, the two category grouping depressed the estimated effect of *Apprendi*. In Table 4, I find that the middle-level (III-IV) criminal history offenders received a lower sentence by a magnitude of between 4 and 5 months. The effect on high-level (V-VI) criminal history offenders was more substantial—sentences were reduced by almost 7 months.

In Table 5, with the first criminal history group serving as the base line, the estimates show that offenders in successively higher criminal history categories typically experience ever-greater sentence reductions following *Apprendi*. Four of the five coefficients increase steadily in magnitude with the highest criminal history offenders faring the best of all. The fact that the five coefficients are not quite monotonically increasing is a bit of a puzzle, given my identification strategy. My estimates suggest that offenders in criminal history category IV do particularly well (or category V offenders do particularly poorly) post-*Apprendi*, almost as well as those in the highest category (VI). Importantly, the standard errors on the estimates cannot rule out a monotonically increasing set of coefficients even as they do rule out the possibility that high criminal history offenders did not see a significant post-*Apprendi* decline in their sentences relative to low criminal history types.

One further way to explore whether the aggregation of offenders into criminal history categories affects my results is to re-estimate equation (1.1) using criminal history points

[13]The outcome variable in my data—prison sentence in total months—is capped at 990 months. More than 95% of the offenders observed received a lower sentence, but those who received life sentences or received a sentence of more than 990 months are not correctly coded, and this fact may affect my results. I address this problem by re-estimating my basic equation using a tobit framework. My results are reported in Table A3, and are consistent with the results reported in Table 3. In the second through fifth columns of Table A3, I report estimates from fixed-effects tobit specification. Estimating nonlinear models in the presence of fixed effects can generate significant bias because of incidental parameters, but Greene (2004) showed using Monte Carlo methods that with tobit, any such bias may be small. Even if any bias is potentially large, the estimated fixed-effects results are broadly consistent with the ordinary tobit results and with the OLS results.

rather than criminal history categories. In Figure 7, I plot the point estimates of $Apprendi \times CrimHistPts$ (the interaction of *Apprendi* with an offender's total number of criminal history points) against the difference in the average sentence of the pre- and post-*Apprendi* periods. The results are striking—the evidence strongly suggests that expanding jury trial rights substantially favors offenders.

Figure 7 is useful at demonstrating the robustness of the pattern in the data, but it should be viewed with some caution for two reasons. First, criminal history points themselves are not particularly relevant to sentencing once an offender tops out at 13 points (see Figure 2), and so it may be that criminal history point determinations are less reliable at very high levels. Second, the regression underlying the figure generates many estimated coefficients (one for every criminal history point total received, which, for some observations in my data set, exceeds fifty) and substantially reduces the precision of my estimates, especially at higher numbers of criminal history points. The number of offenders in each criminal history point group drops below 500 after the 20-point threshold (the cutoff for Figure 7), and the scatter becomes more dispersed and less informative (with significant outliers in both directions).

1.6 Robustness Checks

In this section, I demonstrate the robustness of my basic finding that broader criminal rights mandated by *Apprendi* substantially benefited criminal offenders. I use two criminal history groups (I-III versus IV-VI), as in Table 3, for the remainder of the paper, though different criminal history groupings arrive at similar results. As noted elsewhere, to the extent that *Apprendi* affects Category II or Category III offenders, my estimates will *understate* the effect of *Apprendi*'s expansion of jury trial rights.

1.6.1 Verifying the Intuition

My empirical approach makes use of the fact that *Apprendi* only binds when an offender's sentence approaches the statutory maximum. As I described in Section 1.2, the phrase "statutory maximum" has a very particular meaning in the *Apprendi* context. Before

Apprendi, a judge-found fact could essentially increase the statutory maximum for a crime by moving the locus of the offense to a different provision, thereby allowing offenders with high criminal history determinations or otherwise aggravated offenses to receive a higher sentence. After *Apprendi*, this was no longer possible in some cases. A jury had to pass on any fact that would shift the offense to a higher statutory maximum *if* the offender's sentence would fall above the original statutory cap.

Therefore, if the sentence reductions I estimate are truly a consequence of broader jury trial rights and not some secular trend or correlation, the effect of broader jury trial rights I measure should be much larger for those offenses that contained "nested" maximums or "add-on" provisions, in which *Apprendi* would more likely apply, relative to offenses in which *Apprendi* could have played no role. King and Klein describe these two categories as follows:

> "[A]dd-on" statutes impose a higher maximum for any offense (or for a large subset of crimes) following proof at sentencing of a specified aggravating fact. Statutes that add prison time to what would otherwise be a statutory maximum if a firearm was used, or if there was injury to a victim, or if the crime was committed while on pretrial release, are additional examples of "add-on" statutes that are subject to the *Apprendi* rule.... "Nested" statutes are those that include provisions that define a core offense, but peg higher sentence ceilings to the presence of aggravating facts as determined by the sentencing judge. The carjacking provision examined by the Court in *Jones v. United States* and the firearms offense interpreted in *United States v. Castillo* are examples of such "nested" statutes, as are theft statutes that set the sentence maximum using the sentencing judge's determination of the value of the item stolen, and drug statutes that boost maximum sentences for increasing quantities of drugs (King and Klein 2000, p.331)

I verify that my estimates of *Apprendi*'s consequences are in fact stronger where *Apprendi* should matter more in two ways. First, many have suggested that *Apprendi*'s effects, if they were to be felt at all, would affect those charged with drug crimes—in particular, crimes under provisions containing nested penalties, such as 21 U.S.C. §§841-846 (Lillquist 2004, p.706; Lewis 2001, p.618). Therefore, I re-estimate equation (1.1) using a sample that includes only offenders convicted of crimes classified as being primarily drug-related.[14] Table 6 presents my results. When I restrict my analysis to drug-related cases only, the estimated effects of *Apprendi*'s expansion of jury trial rights are substantially higher in magnitude (at a minimum, 10% higher) than the basic results I report in Table 3.

[14] As I describe in more detail below, the propriety of comparing offenders sentenced under drug statutes before and after *Apprendi* depends on whether prosecutorial charge manipulation alters the composition of offenders sentenced under those provisions.

Second, using a set of federal statutes collected by King and Klein (2000, App.A) that are either "nested" or susceptible to "add-ons," I re-estimate equation (1.1) on a sample containing only offenders whose primary offense fell under at least one of the statutes that King and Klein identify. Estimated coefficients are reported in Table 7, and are strongly corroborative of the broader jury rights interpretation of the effects in Table 3. I estimate that high criminal history types convicted under these statutes received sentences as much as 10 months shorter because of *Apprendi*.[15] To dismiss these estimates as unrelated to the expansion of jury trial rights, a plausible alternative explanation would need to explain why recidivist offenders sentenced under "nested" federal drug statutes, for example, seemed to do remarkably better post-*Apprendi* than all other groups.[16]

1.6.2 Ruling Out Judicial Manipulation

As I noted above, an offender's criminal history status is largely predetermined. Judges rely on probation officers to calculate a criminal history score, and there is rarely litigation over facts of prior conviction. Because criminal history determinations are basically "fixed," I am able to compare the sentencing trends of different criminal history categories to estimate the consequences of broader jury trial rights. But judges are not required to be passive in the determination of criminal history scores. In fact, under U.S.S.G. §4A1.3, judges are given the power to depart from a preliminary criminal history determination.[17] A change in judicial departure practice around the time of *Apprendi*—or even in response to it—might confound my results if judges began to shift certain offenders into different criminal history categories.

[15] If I run equation (1.1) on a data set omitting all sentences premised on *Apprendi*-relevant statutes, my estimates drop dramatically. Under all specifications, I can reject the hypothesis that the coefficients on *Apprendi* $\times$ *CrimHist* from the two sample sets are the same. Still, the estimates using data without *Apprendi*-relevant offenses are not zero.

[16] *Apprendi*'s holding may have systematically affected all criminal prosecutions, not just those to which the rule literally applied. For example, if broader jury trial rights raised the costs of prosecuting certain crimes and prosecutorial resources are limited, we would expect to see consequences of the decision in all post-*Apprendi* sentencing outcomes. Presumably, a systematic effect would also alter low criminal history offender prosecutions, causing my results to underestimate the true consequences of expanding jury trial rights.

[17] Judges are given the power under 18 U.S.C. §3553(b) to depart from the guidelines range if there exists an "aggravating or mitigating circumstance of a kind, or to a degree, not adequately taken into consideration by the Sentencing Commission in formulating the Sentencing Guidelines that should result in a sentence different from that described." For the commission's departure policy statement, see U.S.S.G. §5K.2.0.

Table 2 and Figures 3 and 4 have already offered evidence against this possibility by showing that the differences in offender composition across criminal history categories did not change after *Apprendi*. To further rule out judicial departure behavior as a source of my results, I re-estimate equation (1.1) after dropping all offenders with sentence calculations that included a departure related to criminal history.[18] The estimates, contained in Table 8, are almost identical to those reported in Table 3. In part, this consistency can be attributed to the fact that departures related to criminal history are rare. Nevertheless, judicial departure behavior with respect to criminal history does not appear to underlie the empirical evidence that expanding jury trial rights benefit defendants by reducing their sentences.

To further explore the effect of departures on my results, I also re-estimate equation (1.1) on a sample that excluded all cases involving departures of any kind. The results, reported in Table 9, suggest that, if anything, judicial departure behavior may have compensated for or masked the effects of expanding jury trial rights. If departures are removed entirely, I find that offenders in the high criminal history category experience an average reduction in their sentences of almost 10 months, which is approximately 10% of the mean sentence for high criminal history types. Care must be taken when interpreting these numbers, however, because cases in which departures occur are different from those in which a judge opts for a presumptive guidelines sentence. The differences in estimates may thus be due to non-departure differences (in unobserved characteristics) between the two sets of observations.

1.6.3 Controlling for Offense Types

Sentencing ranges are offense-specific. My empirical approach thus runs the risk of capturing changes over time in the types of crimes that high and low criminal history offenders commit instead of the effect of *Apprendi*'s expansion of jury trial rights. For example, if for some unknown reason recidivists became more likely on average to commit less serious crimes around the time of *Apprendi*, my empirical approach would incorrectly attribute the lower average sentence for offenders with significant criminal histories to *Apprendi*'s expansion of jury trial rights.

[18]Specifically, I omit all observations in which a judge gave one of the following three reasons for departing from the presumptive guideline range: "General adequacy of criminal history," "Other adequacy of criminal history," or "Criminal history overrepresents involvement." 3,945 observations are dropped.

By using information on the types of offenses for which offenders are convicted, I can significantly reduce the likelihood that my results are driven by a shift in the kinds of crimes committed around the time of *Apprendi.*[19] Controlling for offense types (as opposed to the crime committed) is an imperfect solution to this problem.[20] Other than demographic and geographic variables and the rare exception of criminal history scores, sentencing outcomes are all jointly determined, meaning that the sentence a defendant receives *and* the statute under which he receives it may shift in response to a procedural innovation like *Apprendi.*

But this concern is minor when offenses are grouped, since there is only so much room for manipulating a charge (fraud cannot be turned into murder, for example). Changes in charging practices might have included bringing more or fewer charges, or shifting charges to a related statutory provision, but switching to an entirely different offense type would have been uncommon.[21] Therefore, I re-estimate equation (1.1) adding a series of dummy variables to control for differences in offense type. Table 10 reports my results. Although the magnitude of the estimates drops slightly, I continue to find that *Apprendi*'s expansion of jury trial rights benefited offenders: average sentence length for offenders with substantial criminal history records dropped by over 5 months.[22]

1.6.4 Anticipation and Adjustment Concerns

The proper definition of pre- and post-periods is central to empirical work using a quasi-experimental framework. If comparison periods are poorly defined, it can be difficult to know exactly what effect the researcher has captured. Fortunately, in this study, the precise date of the legal change is well-known—*Apprendi* became law on the day the Supreme

[19]Ideally, I would like to compare the sentencing trends of offenders *who committed the same crime* and who were otherwise identical except for their falling into different criminal history categories.

[20]Schanzenbach (2005) attempts to account for offense "seriousness" by using cells of the sentencing guidelines table as control variables. Given the nature of my project (in which, by hypothesis, *Apprendi* may put downward or upward pressure on the offense level), I cannot use this approach.

[21]The drawback of using a broad categorization of offenses, of course, is that the larger the group, the less the researcher is able to avoid comparing apples and oranges—i.e., offenders who committed different crimes with different statutory ranges.

[22]In future work, I plan to link offender sentencing records with arrest and investigation data. Because this sort of data is collected at an early stage in the criminal justice process and relies on the decisions of a different set of actors, it is more likely to be predetermined and not open to manipulation by prosecutors and judges. As a result, I can use this data either directly or by way of an instrumental variables framework to control for biases due to offense-type trends without fear of introducing an endogeneity bias.

Court announced the ruling, June 26, 2000, and the case's holding was both significant and well-publicized (e.g., Greenhouse 2000). Moreover, because I have monthly (as opposed to yearly) data, I am able to draw a clean line between the time periods I compare.

Nevertheless, if prosecutors or defendants, anticipating *Apprendi*'s outcome, altered their behavior prior to the policy's implementation, then my estimates may understate the true magnitude of any effect. This seems unlikely, however. The ruling in *Apprendi* was unexpected, even after the Court granted certiorari. A pre-decision leak of a Supreme Court ruling seems highly unlikely given the procedures and norms of the Court. In any event, the leaked holding would have to have been both broadly disseminated and credible in order to prompt prosecutors and defendants to invest in altering their behavior prematurely. There is no evidence to suggest that this story is a reasonable possibility.

Another possible "anticipation" scenario, though also unlikely, is that once the case was before the Supreme Court defense lawyers sought to delay sentencing or the resolution of an appeal for those cases in which there were plausible *Apprendi* arguments. On this account, *Apprendi*-friendly cases (for example, cases with high criminal history scores or many aggravating facts that were particularly close to the statutory maximum) would have been shifted to the post-period. This strategy appears inconsistent with my findings, however, since offenders close to statutory maximums also typically receive higher sentences. If anticipation effects were present, we would expect to see sentences rise post-*Apprendi*, when in fact they fell. Shifting of this sort is also inconsistent with Figures 3 and 4, which show a steady flow of sentences around *Apprendi*.

Another set of concerns may arise if prosecutors or defendants were slow to understand or adjust to a broader interpretation of the Sixth Amendment. In that case, including the months immediately following *Apprendi* in my sample might skew my results. In the sentencing context, plausible reasons for slow adjustment include cognitive failures on the part of overworked prosecutors and criminal defense attorneys and the simple information problem of attorneys not fully appreciating how *Apprendi* affected their practice. Prosecutors and defense attorneys may also have been overwhelmed by the fallout of the decision itself. For example, in the wake of *Apprendi*, federal prosecutors faced the burden of filing thousands of superseding indictments to account for the change in the law. Moreover, sentencing under the guidelines is complicated, and the Supreme Court's announcement of a new rule does nothing to ensure that relevant players (including probation officers) interpret and apply the rule correctly from the outset. The threat of appellate review and the adversarial environment may have provided incentives to learn quickly, but nevertheless the immediate post-*Apprendi* period may not have been representative of behavior under broader jury trial rights generally.

I treat these issues by re-estimating equation (1.1) on data that excludes the six months prior to and following *Apprendi*. The twelve-month window ought to eliminate any anticipation effects generated by the Court's granting of certiorari as well as accord prosecutors and defense counsel half a year to adjust to *Apprendi*'s new requirements. In Table 11, I report my results, which demonstrate the robustness of Table 3's baseline estimates. Repeating this exercise using a number of other variations does not change the outcome—omitting the five months, four months, and three months before and after *Apprendi*, as well as removing the first nine months of my sample, makes no difference. In all cases, *Apprendi* systematically reduced the sentences of high criminal history category offenders by approximately 6 months.

1.6.5 Controlling for Pre-existing Sentencing Trends

Comparisons of outcomes from pre- and post-periods must control for trends that might cause period "averages" to misrepresent the real behavior of an outcome variable over time. For example, although the simple means in Table 2 are suggestive of what happened before and after *Apprendi* was decided, the higher "average" for high criminal history types in the pre-period may mask a downward trend. It is possible, in other words, that at the end of the pre-*Apprendi* period, immediately before *Apprendi*, the actual average sentence length was much lower than the pre-period's overall average. Similarly, it is possible that the lower "average" for the same group in the post-*Apprendi* period masks a higher starting point and a lower ending point—i.e., a downward trend that is correlated with *Apprendi*.

Figures 5 and 6 (which plot the average prison sentence for each of three criminal history groups using a three-month running average) allow an initial assessment of these concerns: neither figure shows a trend for any of the criminal history categories pre-*Apprendi*. Around the time of *Apprendi*, however, the average sentences for offenders in the higher criminal history groups appear to drop, especially in the first six months, and, in the case of Figure

6, especially for the highest criminal history type. On the whole, nothing indicates that the regression results are capturing some other confounding trend.

To further reduce the likelihood that my estimates capture a pre-existing trend in sentencing, I re-estimated equation (1.1), adding a linear time-trend and trend-squared. I report the results in Table 12. Adding a trend does not alter the basic message of my results—in fact, the results are almost identical to those reported in Table 3.

In Table 13, I present estimates from a regression similar to equation (1.1) but where *Apprendi* and $Apprendi \times CrimHist$ had been replaced with three-month period dummies and interaction of those dummies with $CrimHist$. If $Apprendi \times CrimHist$ were capturing the consequences of an unrelated trend or some other change during the sample period (say, 10 months before *Apprendi*), we would see negative estimates emerge that much earlier. Instead, the pattern of the coefficients supports the interpretation that it is *Apprendi*'s expansion of jury trial rights that reduced offenders' sentences. There is one exception: two periods before *Apprendi* was decided, the estimate of the interaction is negative. But this unexpected negative coefficient occurs during the period in which the Supreme Court granted certiorari for *Apprendi*, a finding which is indicative of anticipation effects (which are in turn controlled for in subsection 1.6.4, where I omit that period). In sum, I find nothing in the data to indicate that my results are a spurious consequence of non-*Apprendi* dynamics in sentencing.

1.7 Conclusion

This paper studies the consequences for defendants of expanding Sixth Amendment criminal jury trial rights. I use the limited reach of the Supreme Court's holding in *Apprendi v. New Jersey*, the calculations required by the United States Sentencing Guidelines, and individual-level sentencing data to create groups of offenders who were differentially affected by *Apprendi*'s expansion of jury rights but who were otherwise similar. By comparing the change in sentence length of these groups before and after *Apprendi*, I am able to identify and measure the effect of the broader jury trial rights mandated by that decision. For those most affected, I find that broader jury trial rights reduced sentences by over 6 months or more than 5 percent.

My finding that expanding Sixth Amendment jury trial rights reduces offender sentences has important and immediate policy relevance. The recent string of decisions in the Supreme Court has left the future of federal sentencing policy unclear (Rosen 2005). *Blakely*, decided last year, extended the logic of *Apprendi* to aggravating sentencing facts generally (not only facts that cause an offender's sentence to "bump up" against the statutory maximum), and therefore expanded jury trial rights much further than *Apprendi* did. *Blakely* did not expressly address sentencing under the federal guidelines, but if it had, my results suggest that *Blakely* would likely have substantially reduced the sentences of many federal defendants.

When the Supreme Court applied the logic of *Blakely* to the federal sentencing guidelines early this year in *Booker*, however, it put the scope of jury trial rights firmly in the hands of Congress. *Booker* ruled in effect that aggravating facts under the federal sentencing guidelines must be decided by juries and proved beyond a reasonable doubt *only if* those facts have mandatory consequences. If aggravating facts have only *discretionary* consequences—that is, if the sentencing judge need not impose a higher sentence if the fact is found—then the fact can be determined by a judge and proved by a preponderance of the evidence. Thus, the Court's decision forces Congress to choose between "mandatory" guidelines (beyond a reasonable doubt jury factfinding) or "discretionary" guidelines (preponderance of the evidence judicial factfinding and discretion). My results imply that the path selected by Congress and the states will have serious consequences for offenders.

My empirical work also speaks to larger questions about the real-world effects of criminal procedure. First and foremost, it suggests that procedural protections meaningfully influence actual outcomes, presumably by constraining powerful prosecutors. The debate over whether the "shadow of the law" is shrinking in the criminal procedure context is a case in point: the answer, at least according to the results of this study, is that prosecutors are significantly constrained by jury protections and heightened proof requirements. My conclusion that expanding jury trial rights reduces average sentence length also supports the notion that changes in criminal procedure will affect substantive criminal law. The expansion of jury trial rights has in effect reduced criminal penalties, and one would expect that to influence the ability of criminal law to deter potential offenders.

In future work, I plan to address the "how" question that emerges from my findings. What role do prosecutors and judges play in animating—or undermining—jury trial protections? Do offenders with broader jury trial rights benefit through, for example, less severe charges, bargaining concessions, or acquittals on additional counts? In other words, do prosecutors use the same strategies, but simply fail more often? Or do they change their approach in an attempt to "undo" procedural protections? This study and its results indicate that, as commentators at the time of the decision believed, *Apprendi* changed the legal landscape of criminal procedure. It thus provides a rare and useful tool to disentangle how players in the criminal justice system behave, and perhaps more importantly, what goals they seek to achieve.

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1.9 Appendix: Plea Bargaining with Jury Trial Rights

Jury trial rights, when they exist, are part of a complicated bargaining-oriented litigation environment in which the differences between judge and jury decision making and the cost of factfinding play important roles in determining outcomes and strategy (Bibas 2001a, 2001b). And, since almost all federal criminal cases are resolved through plea bargains, prosecutorial and defendant responses at stages *prior to* the actual application of broader jury trial rights must be taken into account when evaluating possible consequences. In this Appendix, I build a simple plea-bargaining model that incorporates (1) differences in judge and jury decision making, (2) prosecutorial factfinding costs, and (3) the sequential structure of criminal prosecution. I conclude that under certain conditions, expanding jury trial rights may worsen offender outcomes.

1.9.1 Model Preliminaries

I model federal factfinding as a four-stage bargaining game (see Baker and Mezzetti 2001; Reinganum 1988, 2000; Grossman and Katz 1983). I make a number of simplifying assumptions to make the model tractable and capable of offering testable predictions. First, I assume that defendants maximize their utility by minimizing their sentence. Second, prosecutors seek to maximize the *cost-adjusted* sentence imposed (meaning that prosecutors price sentences and trade off the benefits of a longer sentence with the costs of achieving that sentence). I do not explicitly include the cost of defense for criminal defendants, both to simplify the game and because in a large percentage of cases the government pays for criminal defense.[23]

Following Reinganum (2000), I assume that a defendant commits an offense with severity x before he enters the formal legal system. The defendant knows the true value of x, but the prosecutor only knows that x is a random variable X, which is distributed according to the cumulative distribution function $F(\cdot)$, with density $f(\cdot)$ and support $[\underline{x}, \bar{x}]$. If the defendant is innocent, severity (x) is close to zero. The prosecutor charges the defendant with having committed an offense with severity $\bar{x}$. The court has the same information that the prosecutor does—in particular, the court knows $F(\cdot)$ and that charges above $\bar{x}$ cannot be supported by evidence. Consequently, prosecutors do not charge higher severity levels, since a (law-abiding) court would unilaterally throw out or reduce the charges. I also assume that prosecutors charge the maximum possible severity even though it may sometimes be in their interest to charge less.

Once the crime (if any) has been committed and the prosecutor has charged the highest possible severity level, the "game" between the prosecutor and the defendant begins. The purpose of the game is for a court of law to determine the severity of the crime, either through a plea bargain, a jury trial, or a bench trial. The severity of the crime (once

[23] Also, to avoid unnecessary formalism, I largely gloss over two very important earlier stages: (1) the decision to commit a crime and (2) the decision to bring charges.

determined) translates into a sentence that increases in length with severity (through a judge exercising discretion or mechanically applying a set of mandatory guidelines).

As Figure A1 shows, the prosecutor and defendant alternate in making decisions, each moving twice. First, the prosecutor makes a take-it-or-leave-it plea bargain offer (x_p) to the defendant. Second, the defendant chooses whether to accept that offer. If the defendant accepts the offer, the game ends.

If the defendant rejects the plea offer, the game enters the third stage: the defendant must choose whether to accept a jury as a factfinder or "waive" that right by requesting a bench trial. A federal jury trial right works by setting a jury trial as the default, meaning that a jury will be used *unless* the defendant moves to substitute a judge. If the defendant selects a jury, the jury determines the severity, and the defendant is sentenced on the basis of that determination.

If the defendant waives the jury, the game enters the fourth stage, at least under federal law (and the law of many states): the prosecutor must choose whether to allow the waiver or to object, thereby *forcing* a jury trial (see Fed. R. Crim. P. 23).[24] This last stage may be practically unimportant for a number of reasons (Leipold 2005, pp.21-22), but Bibas (2001a, p.1158) places particular importance on its possible consequences when arguing that broader jury trial rights have the potential to burden, not benefit, criminal offenders.

1.9.2 Factfinding Outcomes and Prosecutorial Costs

So far I have set out the aims of prosecutors and defendants, what they know, and how and when they are allowed to make decisions. To resolve how defendants and prosecutors will behave, what outcomes will result, and under what conditions, two more features of the bargaining environment must be defined: (1) how factfinders (judges and juries) determine severity, and (2) what costs prosecutors and defendants face.

I define the *jury* factfinding outcome (or severity determination) as $O_{jr}(\sigma, \pi, \bar{x}, \omega)$ and the *judge* factfinding outcome as $O_{jd}(\sigma, \pi, \bar{x})$. The parameter σ represents the standard of proof ($\sigma \in \{\sigma_{brd}, \sigma_{pe}\}$, with σ_{brd} = "beyond a reasonable doubt" and σ_{pe} = "preponderance of the evidence"). A higher standard of proof applied to the same evidence results in a lower determination of severity, all else even. Formally, I define σ such that $\sigma_{brd} > \sigma_{pe}$ and $\partial O / \partial \sigma < 0$. The parameter π is a signal of fact severity such that $E(\pi) = x$, with $\pi \backsim G(\cdot)$ on the support $[\underline{\pi}, \bar{\pi}]$. π is *positively correlated* with actual severity, meaning that a higher π is more likely to come from a defendant who committed a more severe crime. The parameter $\bar{x}$ is the fact severity charged, and $\omega \in$ {objection, no objection}, captures the effect on the outcome, if any, of a prosecutor's having objected to a jury trial waiver (see the discussion of prosecutorial costs below).

[24]For cases involving this rule, see *Patton v. United States*, 281 U.S. 276, 312 (1930); *United States v. Duarte-Higareda*, 113 F.3d 1000, 1002 (9th Cir. 1997). Fisher (2000, p.1072) and Kurland (1993, pp.321-23 & nn.39, 40, 42, 43 & 45) discuss this rule and its state-level counterparts.

The key feature of these definitions relates to the signal of severity, π. x and π are positively correlated and only the defendant knows x, so the defendant has different (better) information about the likely outcome of any trial. Because the information sets of the prosecutor and defendant differ, it is possible in equilibrium for plea offers to be rejected by defendants.[25]

Costs are important in this bargaining environment because, as noted above, prosecutors prefer a higher "cost-adjusted" sentence, all else even. In the last stage, the prosecutor has to decide whether to object to a jury trial waiver if the defendant attempts to waive his jury trial right. But objecting is not free. I assume that objecting to a bench trial insults or annoys the judge (Klein 2004, p.1353 n.20), who can punish the prosecutor by manipulating his procedural and evidentiary rulings. Prosecutors regularly cite this "cost" to justify their decision not to object to a jury waiver (Leipold 2005, p.22 n.57). Therefore, $O_{jr}(\sigma, \pi, \bar{x}|no\ waiver) > O_{jr}(\sigma, \pi, \bar{x}|waiver)$. That is, a jury trial by default does not produce the same outcome (in fact, it always produces a higher sentence) as a forced jury trial. Once a defendant waives his jury trial right, the prosecutor may still prefer a jury to a judge (because a jury trial with a judge hostile to the prosecutor may still result in higher expected sentence than a bench trial), and he may decide to force a jury trial, although doing so is costly (above and beyond any extra resources made necessary by a jury trial). Define the resulting difference in sentences as $\Delta_w(\bar{x})$, where $\Delta_w(\bar{x}) = O_{jr}(\sigma, \pi, \bar{x}|no\ waiver) - O_{jr}(\sigma, \pi, \bar{x}|waiver) > 0$.

A prosecutor will not exercise his ability to "undo" a defendant's decision to waive a jury trial, even if a lower sentence will result, if the costs of objecting to that waiver are sufficiently high. Unless Δ_w is very small, however, the illusory "option" still has some value above and beyond a jury-only scheme, because even an attempt to waive the jury trial right has the defendant-friendly consequence of forcing the prosecutor to annoy the judge.

There is a second cost the prosecutor must take into account when a defendant has a jury trial right: a jury trial requires more prosecutorial resources than a bench trial because of the additional time and resources required to explain law and evidence to a lay jury and/or because the rules of evidence can exclude the least expensive methods of demonstrating necessary facts.[26] I define $C_{jr}(\bar{x})$ and $C_{jd}(\bar{x})$, as the total cost (in sentence-length units)

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[25] I assume that x is not correlated with the difference, if any, in the expected outcomes of judges and juries. If a jury is defendant-friendly, not only is it defendant friendly for all levels of x, but the difference between a judge and jury outcome is constant. If this condition does not hold, the game becomes more complicated because the defendant would reveal usable information to the prosecutor at both the plea bargaining stage and at the waiver stage. For example, when the prosecutor makes a plea offer on the basis of $F(x)$, if the defendant accepts, the prosecutor updates his prior understanding of the severity distribution to $F(x|\text{plea accepted})$. In other words, the prosecutor learns of a higher lower bound for actual severity. If the assumption above were not made, this would affect his later decision of whether to object to a waiver.

[26] The government often pays (some of) a defendant's legal fees, and so I do not consider separately the increased cost to a defendant of a jury trial. A more complicated model would consider the possibility that defendants might be resource constrained. If so, there is likely to be a strategic incentive for prosecutors to force a jury trial in order to raise their rivals' costs. The cost parameter can alternatively be taken to represent the *difference between* the incremental costs for prosecutors and defendants. Because a prosecutor

of trying the facts charged $\bar{x}$ to a jury and judge, respectively. I also define the additional cost of a jury trial as $\Delta_c(\bar{x}) = C_{jr}(\bar{x}) - C_{jd}(\bar{x})$, again where $\bar{x}$ represents the severity level the prosecutor seeks to demonstrate to a particular factfinder.

I assume that all cost information is known to both prosecutors and defendants and that it costs the prosecutor (weakly) more to prove a fact to a jury than to a judge, i.e., that $\forall\, \bar{x}$, $\Delta_c \geq 0$. This difference—which represents the additional cost for a prosecutor to prove facts in a system with jury trial rights—is one reason why some commentators have argued that expanding jury trial rights will benefit defendants.

1.9.3 Solving for Equilibrium Strategies and Outcomes

I can now assemble all of these pieces and describe how expanding jury trial rights may affect the outcomes for prosecutors and defendants. I can also state the empirical conditions under which defendants, according to the model, will be better or worse off in terms of expected sentences under broader jury trial rights. To do this, I begin with the last stage of the game, the prosecutor's decision whether to object to a waiver, and then work backward toward the plea bargaining stage.

If the defendant attempts to waive his jury trial right, the prosecutor will object if the judge is relatively defendant-friendly *and* the costs of forcing a jury trial are sufficiently low. More precisely, the prosecutor will object if:

$$E[O_{jd}(\sigma, \pi, \bar{x})] < E[O_{jr}(\sigma_{brd}, \pi, \bar{x}|waiver)] - \Delta_c(\bar{x})$$

Using the definition of Δ_w, the conditions under which a prosecutor will object can be rewritten to make explicit the costs a prosecutor faces:

$$E[O_{jr}(\sigma_{brd}, \pi, \bar{x}|no\ waiver)] - E[O_{jd}(\sigma, \pi, \bar{x})] > \Delta_c(\bar{x}) + \Delta_w(\bar{x})$$

This *Objection Condition* has a simple interpretation: A prosecutor will not exercise his ability to "undo" a defendant's decision to waive a jury trial, even if a lower sentence is expected to result, if the costs of objecting to that waiver are sufficiently high. In the limit, as $\Delta_c(\bar{x}) + \Delta_w(\bar{x}) \rightarrow \infty$, defendants have a *full jury trial option*, since prosecutors will not object to a waiver no matter how defendant-friendly a judge may be. On the other hand, as $\Delta_c(\bar{x}) + \Delta_w(\bar{x}) \rightarrow 0$, the prosecutor will always force a jury trial if the prosecutor expects that a jury will produce a higher sentence. If juries are always prosecutor friendly, then there is no jury trial "option" at all—the defendant has no choice but to have facts determined by a hostile jury.

Given the prosecutor's response, when will a defendant choose to "attempt" a waiver? If we assume that $\Delta_w(\bar{x}) \geq 0$ (i.e., objecting to a waiver is costly to a prosecutor), the

bears the burden of proof, it is at least plausible that it is disproportionately more expensive for prosecutors to put facts to a jury than it is for a defendant to defend a jury trial.

defendant will waive his jury trial option if:

$$E[O_{jd}(\sigma, \pi, \bar{x})] < E[O_{jr}(\sigma_{brd}, \pi, \bar{x}|no\ waiver)]$$

This *Waiver Condition's* interpretation is even more straightforward. The defendant will only waive his jury trial right if he believes the judge is defendant-friendly relative to a jury.[27]

Importantly, the defendant prefers to waive his jury trial right if the Waiver Condition holds even if he knows the prosecutor will object. This is true by assumption, since $\Delta_w(\bar{x}) \geq 0$. If $\Delta_w(\bar{x})$ is low (but non-negative) such that $E[O_{jd}(\sigma, \pi, \bar{x})] < E[O_{jr}(\sigma_{brd}, \pi, \bar{x}|waiver)] < E[O_{jr}(\sigma_{brd}, \pi, \bar{x}|nowaiver)]$, the prosecutor will force a jury trial, but the defendant prefers this forced trial to not waiving because the judge punishes the prosecutor. If $\Delta_w(\bar{x})$ is high, such that $E[O_{jd}(\sigma, \pi, \bar{x})] < E[O_{jr}(\sigma_{brd}, \pi, \bar{x}|\ nowaiver)] < E[O_{jr}(\sigma_{brd}, \pi, \bar{x}|waiver)]$, the defendant will waive and hope for an objection, but the prosecutor will prefer judicial factfinding (unless, for some reason, $\Delta_c(\bar{x})$ is sufficiently negative).

For any set of cost parameters and judge/jury disparities, the Objection Condition and the Waiver Condition ensure that one of three possible outcomes emerge, if the plea bargain offer is rejected:

$$\text{(no waiver, N/A)} \Longrightarrow \text{default jury trial}$$

$$\text{(waiver, objection)} \Longrightarrow \text{forced jury trial}$$

$$\text{(waiver, no objection)} \Longrightarrow \text{bench trial}$$

The equilibrium that results depends on the relationship of the expected jury outcome to a judge outcome, the cost (in terms of expected outcome) to the prosecutor of objecting to a waiver, and the additional cost to the prosecutor (in financial terms) of a jury trial, relative to a bench trial. Very shortly, I will examine how these possible outcomes compare to what a defendant would face *without* a jury trial right.

Before studying the possible consequences of expanding jury trial rights, it is important to account for the fact that very few criminal prosecutions actually end in either a bench or jury trial—over 90% of criminal prosecutions are resolved through plea bargaining. The goal of this paper is to study the *real-world consequences* of broader jury trial rights, and, in the existing criminal justice system, the response of plea bargaining behavior to legal changes is central to any empirical sentencing project. Therefore, it is necessary to ask how a prosecutor's plea offer (and a defendant's decision whether to accept) is determined in a factfinding system that includes a jury trial right.

[27] My notation assumes that a judge does not punish a defendant for exercising his jury trial rights, or, put differently, a judge does not reward the defendant for waiving his jury trial right. Some have suggested otherwise, noting that defendants may be rewarded by a judge at sentencing for waiving their jury trial rights (see Bibas 2001a, p.1155 n.346). This possibility can be easily incorporated into the model by assuming that a judge is systematically more friendly in a jury rights system than in a judicial factfinding system.

Which of the three possible outcomes the prosecutor anticipates *absent* a plea bargain will determine the plea offer he makes to the defendant in stage one of the game. The prosecutor offers a plea x_p that trades off the costs of taking the case to trial and the loss (in terms of a lower sentence) of offering an overly lenient plea bargain. The defendant will accept the bargain if the plea offer is lower than the sentence he expects to receive if he rejects the offer.

Formally, he will accept a plea offer of x_p if $x_p \leq E_\pi[O(\pi)|x]$ or if $x_p \leq \int_{\underline{\pi}}^{\bar{\pi}} O(\pi) g(\pi|x)d\pi$. Because the prosecutor only knows the distribution of x, from the prosecutor's perspective the probability that the defendant will accept the plea offer is $B(x_p) = P(x_p \leq \int_{\underline{\pi}}^{\bar{\pi}} O(\pi)g(\pi|x)d\pi) = P(x_p \leq E_\pi[O(\pi)|x])$. Note that $B'(x_p) < 0$, which has the intuitive interpretation that the probability that the defendant will find the plea bargain attractive (given x) declines as x_p increases.

The prosecutor will make a plea offer that maximizes the expected "cost-adjusted" sentence S. Thus, the prosecutor selects x_p to maximize $B(x_p) \cdot (x_p) + (1 - B(x_p)) \cdot (E_\pi[O(\pi)] - C)$. The first order condition with respect to x_p is $B'(x_p) \cdot x_p + B(x_p) - (E_\pi[O(\pi)] - C) \cdot B'(x_p) = 0$. Solving for x_p yields:

$$x_p^\star = (E_\pi[O(\pi)] - C) + \left(-\frac{B(x_p)}{B'(x_p)}\right)$$

The first term is the prosecutor's expected "cost-adjusted" trial outcome. The second term $-\frac{B(x_p)}{B'(x_p)}$, which is positive since $B'(x_p) < 0$, represents the prosecutor's share of the surplus created by the bargain. Therefore, the prosecutor offers a discount from the expected trial outcome $E[O(\pi)]$, which is designed to increase the defendant's likelihood of accepting the plea (thereby saving the prosecutor factfinding trial costs) without giving away too much.

Notice that $B'(x_p) < 0$ and $\frac{\partial x_p^\star}{\partial C} < 0$, which has the straightforward interpretation that the length of plea offers will decline the more it costs a prosecutor to put on the trial. Since jury trials are more expensive for the prosecutor than bench trials, equilibrium plea bargained sentences will drop as a system moves more toward jury trials, even if judges and juries produce identical outcomes.

Notice also that $\frac{\partial x_p^\star}{\partial E[O(\pi)]} > 0$. This condition captures the classic idea that plea bargains will mimic appropriately discounted trial outcomes, or that pleas will be negotiated in the "shadow" of the trial.[28] The higher the expected sentence at trial, the more aggressive the prosecutor will bargain, by offering a higher x_p. The defendant accepts the bargain x_p if he believes (conditional on his knowledge of x) that his expected trial sentence is higher. In equilibrium, defendants will both accept and reject plea offers, because defendants have superior knowledge regarding what is likely to happen at trial.

[28]This model assumes that prosecutors care about costs by assuming that they will trade off a higher sentence to save resources. This is not necessarily the case, as Stuntz (2004) makes clear. Therefore, if empirical analysis were to show that there was no effect of expanded jury trial rights, it might mean that the assumptions of the model need modification.

1.9.4 Possible Consequences of Expanding Jury Trial Rights

The foregoing analysis aims to make specific and concrete the important features of a jury trial right. But to evaluate the theoretical *consequences* of giving defendants jury trial rights, I must compare the outcomes of factfinding with a jury trial right to the outcomes of a regime that lacks this feature. Prior to the *Apprendi* line of cases, defendants had no jury trial right as to sentencing facts—all such facts were decided by judges. Therefore, the appropriate baselines for comparison are the outcomes that emerge from judicial factfinding.

Figure A2 depicts a factfinding game with only judicial factfinding (i.e., without a jury trial right). The judicial factfinding game is simple. If the defendant rejects the plea offer, the outcome is $O_{jd}(\sigma_{brd}, \pi, \bar{x})$.[29] If the defendant accepts the bargain, the sentencing is x_p. The problem the prosecutor solves in choosing the plea offer is identical to the game with the federal jury trial right above. The key difference is that, if bargaining fails, the two jury trial outcomes (with waiver and objection or without waiver) are not available. The introduction of these possibilities (and their effect on plea bargaining) generate the "consequences" of a jury trial right. We can organize these consequences into three propositions.

First, if on average judges are defendant-friendly relative to juries, but not so friendly that it pays for the prosecutor to object to a jury waiver, then the defendant's position has not changed, and a jury trial right is irrelevant. This occurs if the Waiver Condition is satisfied, but the Objection Condition is not, so defendants and prosecutors both prefer judicial factfinding. There are a number of ways this can happen. For example, it might be that judges are very defendant-friendly, but the costs to the prosecutor of objecting are extremely high. Alternatively, the costs to objecting might be very low, but the benefit to the defendant of choosing a judge or a jury might be even more slight, though positive. For the set of cases in this category, nothing has changed: the prosecutor's plea offer x_p should be identical, since the consequence of the defendant's refusal is judicial factfinding in either scenario.

Second, if judges are defendant-friendly on average, but objecting to a jury trial waiver is less expensive to the prosecutor than allowing a bench trial, then the defendant is worse off, and a jury trial right harms the defendant. This occurs if the Waiver Condition and the Objection Condition are both satisfied, so the defendant prefers a judge, but the prosecutor is willing to force a jury trial. The defendant, in other words, is incapable of exercising any jury trial "option." This may be because objecting is inexpensive to the prosecutor, or because the benefit to the defendant of a bench trial is very high. The defendant also suffers at the plea bargaining stage. The Objection Condition is satisfied by assumption, so $E[O_{jd}(\sigma, \pi, \bar{x})] < E[O_{jr}(\sigma_{brd}, \pi, \bar{x}|waiver)] - \Delta_c(\bar{x})$, which can be rewritten $E[O_{jr}(\sigma_{brd}, \pi, \bar{x}|waiver)] - C_{jr}(\bar{x}) > E[O_{jd}(\sigma, \pi, \bar{x})] - C_{jd}(\bar{x})$, which means that the $E[O(\pi)] - C$ term in the prosecutor's offer equation increases. Differentiating $x_p^\star$ with respect to $E[O(\pi)] - C$ generates a positive derivative, which tells us that the prosecutor's offer will be less lenient.

[29]The *Apprendi* line also raised the standard of proof for sentencing facts from "preponderance of evidence" to "beyond a reasonable doubt." Incorporating that change into the analysis is straightforward, as I describe below.

Third, if juries are defendant-friendly on average, then the defendant is better off, and a jury trial right benefits a defendant. This occurs because the Waiver Condition is not satisfied, so the defendant prefers the jury trial and will not attempt to waive. Although costs matter to plea bargaining outcomes, whether the Objection Condition is satisfied is itself irrelevant. In these cases, the defendant is unambiguously better off with a federal jury trial right, because the jury is defendant friendly *and* it is more expensive for the prosecutor to prosecute. If the case goes to trial, the defendant will wind up better off with a more defendant-friendly judge. The trial will also cost more to prosecute to a jury. During plea bargaining, the prosecutor will take both of these effects into account and, since $\frac{\partial x_p^*}{\partial O(\pi)} > 0$, $\Delta_c(\bar{x}) > 0$, and $\frac{\partial x_p^*}{\partial C(\bar{x})} > 0$, he will reduce the plea offer.[30]

The conclusion of this analysis is that the consequences of expanding federal jury trial rights (in which a prosecutor can object to a jury trial waiver) turn on the relative defendant friendliness of judges and juries and the particulars of prosecutorial costs. An important conclusion is that if (1) forcing a jury trial is fairly inexpensive to a prosecutor *and* (2) judges are relatively more defendant-friendly than juries, then defendants will be worse off, both in the terms of trial outcomes and plea bargains. Another important finding is that if prosecutorial costs are high, the defendant has a true jury trial option, and is therefore unambiguously better off. Which of these scenarios accurately characterizes the effects of expanding jury trial rights is the empirical question that this paper seeks to answer in Sections 1.4 through 1.6 above.

1.9.5 Raising the Standard of Proof

In this section, I incorporate the fact that the *Apprendi* line of cases did more than expand jury trial rights as I describe above. Those cases also required that all facts covered by a jury trial right be proved beyond a reasonable doubt (see Bibas 2001a, pp.1156-58). The model itself is sufficiently general to account for the change in standard of proof required under *Apprendi*: the standard of proof was already included in the definition of how judges and juries arrived at severity determinations. I assumed above that $O_{jr}(\sigma_{brd}, \pi, \bar{x}, \omega) < O_{jr}(\sigma_{pe}, \pi, \bar{x}, \omega)$ and $O_{jd}(\sigma_{brd}, \pi, \bar{x}, \omega) < O_{jd}(\sigma_{pe}, \pi, \bar{x}, \omega)$, where σ represents the standard of proof. This assumption means that for both judges and juries, a higher standard of proof always moves the severity determination in favor of the defendant, at least weakly. This shift will also be reflected in plea bargains, because $\frac{\partial x_p^*}{\partial O(\pi)} > 0$. Scholars have suggested that judges and juries might interpret and apply standards of proof differently (Kalven and Zeisel 1966; Eisenberg et. al 2005, p.7 n.31) and therefore a *change* in standards might have different consequences for judge and jury determinations, but those differences too are captured in the post-*Apprendi* judge and jury outcomes, as I have already defined them. Otherwise, the mechanics of the game are unchanged.

[30] If one expands the game to include a prosecutor's charging decisions, it is possible to show that, for cases in this category, the prosecutor would charge fewer defendants or charge each defendant with lesser crimes. This is because proving facts has become more expensive, and so, at the margin, it is optimal for the prosecutor to be less aggressive.

The change in the standard of proof must be incorporated, however, because it further limits the circumstances under which expanding jury trial rights might harm defendants.[31] For defendants to be worse off with broader jury trial rights, the satisfaction of both the Waiver Condition and the Objection Condition is necessary, but not sufficient: it is possible that the defendant prefers a judge to a jury for a given standard of proof, but would prefer a jury with a higher standard of proof to a judge with a lower standard of proof. More precisely, for the set of cases in which the Objection Condition is satisfied, and $O_{jr}(\sigma_{brd}, \pi, \bar{x}, \omega) < O_{jd}(\sigma_{pe}, \pi, \bar{x}, \omega) < O_{jr}(\sigma_{pe}, \pi, \bar{x}, \omega)$, defendants would have been harmed by expanding jury trial rights *but for* the benefits of the higher standard of proof.

Since the "beyond a reasonable doubt" standard is a central feature of the constitutional jury trial rights afforded defendants in the United States, I amend the second proposition in subsection 1.9.4 to account for this feature: *if a judge using a "preponderance of evidence" standard is more defendant-friendly than a jury using a "beyond a reasonable doubt" standard, but objecting to a jury trial waiver is less expensive to the prosecutor than allowing a bench trial, then the defendant is worse off, and a jury trial right harms the defendant.*

1.9.6 Introducing versus Expanding Jury Trial Rights

The *Apprendi* line of cases did not *introduce* criminal jury trial rights, but rather *expanded* existing jury trial rights to cover sentencing facts. Offense facts (or elements) were already covered by the Sixth and Fourteenth Amendments. An important question is whether the existing application of jury trial rights to offense facts has any significance for assessing the possible consequences of expanding jury trial rights to sentencing facts. By appending to the model above a preceding set of factfinding stages (with jury trial rights) in which offense facts are determined, it can be shown that the consequences of existing jury trial rights turn on the particular contours of the prosecutorial costs detailed above—the additional cost of prosecuting a trial to a jury as opposed to a judge, and the cost of objecting to a jury trial waiver—as well as on how judges and juries make their factfinding decisions.

The introduction of a preceding set of offense factfinding stages generates effects that run in both directions between offense factfinding and sentencing factfinding—all outcomes are determined endogenously. First, behavior and outcomes in the offense factfinding portion of the game affect the initial conditions of the sentencing factfinding game. For example, if prosecutorial jury trial costs display significant scale economies, a prosecutor may face a much lower Δ_c during sentence factfinding if offense facts were presented to a jury, which might cause a prosecutor to object to a jury trial waiver or reduce his plea offer, if a jury trial would follow, since jury trial costs figure into those decisions. Second, precisely because behavior and outcomes in the offense factfinding stage have effects at the sentencing factfinding stage, prosecutors and defendants will adjust their behavior to account for the consequences of their decisions at later stages. For instance, a prosecutor may be able

[31] It also magnifies the benefits for those defendants whose position was already improved by the extension of jury trial rights.

to commit to forcing a jury trial (and therefore may exact a better plea bargain) at the sentencing stage by having presented offense facts to the jury. The prosecutor will take this positive externality into account when deciding whether to object to a jury trial waiver.

A detailed model of the differences between "expanding" and "introducing" jury trial rights is beyond the scope of this paper, but it is worth noting how an expanded model might make a difference in a defendant's position relative to the simple model above. Below, I briefly discuss how an expanded model might play out under a number of different conditions.[32] The analysis is informal, and is organized around a defendant's preferences over judges and juries for offense and sentencing factfinding. Although a model in which jury trial rights are "expanded" rather than "introduced" is more realistic in its complexity, understanding the consequences of broadening jury trial rights requires empirical analysis. To determine the existence, direction, and magnitude of any effect, data are necessary.

There are two plausible ways to model "broader" jury trial rights. One way is to view pre-*Apprendi* defendants as having a jury trial right as to some facts, but not as to others, and treat all facts as if they are negotiated and determined simultaneously. On this approach, the prosecutors offer a plea bargain knowing that, if the offer is rejected, some of the facts will be determined by a judge while the defendant will have a jury trial right as to the others. The *Apprendi*-line simply extended jury trial rights as to all facts. A second way to interpret "broader" jury trial rights is to view the prosecutor and defendant as bargaining over a longer sequential game, with a game to determine offense facts followed by a similar, if not identical, game to determine sentencing facts. I take the second approach here.

Case #1: Assume that a defendant prefers a jury for both offense and sentencing facts. In the simple model, I showed that providing a jury trial right always improves a defendant's position if a jury is defendant-friendly relative to the judge.

Now consider an expanded model in which jury trial rights already exist for offense facts. In the pre-period, if plea bargaining fails, the defendant selects a jury for offense factfinding, but will be legally forced to rely on judicial factfinding for sentencing facts. In the post-period, because the defendant always prefers a jury, the defendant will exercise his jury right for sentencing facts. If prosecutorial jury trial costs display significant scale economies, then, because plea bargaining fails with positive probability at the sentencing stage, the prosecutor will offer a more severe sentence x_p at the offense stage because jury trial costs as to offense facts drop, but not by as much as his sentencing fact plea offer will drop, as a consequence of the defendant's credible jury trial threat.

Conclusion: The defendant will usually be better off with broader jury trial rights, but to a lesser extent than in the simple model. The defendant might be worse off if the scale economies of a jury trial are so great that the cost of trying all facts to a jury falls below the

[32] I refer to the period before the *Apprendi*-line of cases, when there were jury trial rights only for offense facts, as the pre-period, and the period after jury trial rights are extended to all facts as the post-period. For simplicity, I ignore the consequences of change in the standard of proof, since its role is no different than in the simple model.

total cost of trying offense facts to a jury and sentencing facts to a judge. In the expanded model, the prosecutor can mitigate some of the consequences of the broader jury trial rights by changing his behavior at the offense stage.

Case #2: Assume the defendant prefers a jury for offense facts but a judge for sentencing facts. In the simple model, I showed that the defendant is in the same position with or without a jury trial right if he prefers a judge, unless the costs to the prosecutor of forcing a jury trial are sufficiently low.

Now consider an expanded model. In the pre-period, if plea bargaining fails, the defendant selects a jury for offense facts and enjoys the required judicial factfinding at the sentencing stage. In the post-period, if the costs of objecting to a jury trial waiver at the sentencing stage are very high, the prosecutor has an incentive (at the margin) to raise his offense fact plea offer to the defendant, although not by enough to compensate for the lower plea offer he must make at the sentencing stage. But if the costs of objecting are fairly low, and if prosecutorial jury trial costs display significant scale economies, then the prosecutor can do more: he might find it pays to significantly lower his plea offer at the offense stage, raising the chances of a jury trial, which in turn lowers the costs of objecting to a jury trial waiver to the point where the prosecutor will prefer to force a jury trial at the sentencing stage. Note that the defendant will also adjust his behavior at the offense stage. The prosecutor may make his plea offer worse at the sentencing stage to force a jury trial, but the defendant will find it optimal to accept a worse plea bargain, considering the strategic advantage that declining the offer gives to the prosecutor.

Conclusion: The defendant may be better or worse off with broader jury trial rights. In the expanded model, the prosecutor will alter his behavior during the offense stage (which the defendant will be only partially able to undo) at least at the margin, and may do so dramatically of the prosecutor can commit to forcing a jury trial at the sentencing stage.

Case #3: Assume that a defendant prefers a judge for offense facts but a jury for sentencing facts (the reverse of case #2). In the simple model, I showed that a preference for a jury rights means that a jury trial right benefits a defendant.

Now consider an expanded model. In the pre-period, if plea bargaining fails, the defendant waives his jury trial right (if the prosecutor does not object) as to offense facts and is constrained by the judicial factfinding at the sentencing stage. In the post-period, now that he is given a choice, the defendant will always select a jury at sentencing, but he may wish he could commit to waiving his right. The reason is that the prosecutor, in anticipation of being required to put on a jury trial at the sentencing stage, may find it optimal to force a jury trial at the offense stage as well. If the costs to the prosecutor of objecting to a jury trial waiver are very high, then the prosecutor does not change his behavior at the offense stage (if he raised his plea offer, and it was rejected, the defendant would waive his jury trial right and the prosecutor would allow it, given the prosecutor no cost savings later). If prosecutorial jury trial costs display significant scale economies, however, then the fact that a jury trial will happen with positive probability at the sentencing stage will lower the costs of objecting to a jury trial waiver, perhaps to the point where it becomes optimal for the prosecutor to object to the waiver and force a jury trial. If the defendant gains less from having a jury at the sentencing stage than he loses from a forced jury trial at the offense stage, then he is worse off with broader jury trial rights.

Conclusion: The defendant may be better or worse off with a broadening of jury trial rights. In the expanded model, it is possible that the inability of the defendant to commit to waiving a jury trial at the sentencing stage will lead a prosecutor to force a jury trial at the offense stage.

Case #4: Assume that a defendant prefers a judge for both offense and sentence facts. In the simple model, I showed that a jury trial right will worsen a defendant's position if the costs of objecting to a jury trial waiver are low.

Now consider an expanded model. In the pre-period, the defendant will attempt to waive his jury trial right as to offense facts, and will enjoy obligatory judicial factfinding. In the post-period, the prosecutor has a new option of forcing a jury trial right as to sentencing facts. If a judge is sufficiently defendant-friendly during sentencing (or the costs of objecting to a jury trial waiver are sufficiently low), then the prosecutor will object and force a jury trial, making the defendant worse off. But, in addition, if prosecutorial jury trial costs display significant scale economies, then the costs of objecting to a jury trial waiver decline at the offense stage. This decline in jury trial costs will not affect the offense stage plea offer or judicial determination, unless the decline is so great that it becomes optimal for the prosecutor to object to a jury trial waiver, in which case the defendant is hit twice—a worse factfinder if the plea bargaining fails, and a worse plea offer. Note that if a forced jury trial at the offense stage reduces the costs of objecting enough so that the prosecutor prefers a jury trial at sentencing, then these effects are mutually reenforcing.

Conclusion: The defendant is always worse off under broader jury trial rights. In the expanded model, scale economies of jury trial rights may allow a prosecutor to force a jury trial as to both sentencing facts and offense facts, whereas, prior to the expansion, judges would find all facts.

Figure 1: Federal Jury Trial Rights Expansion

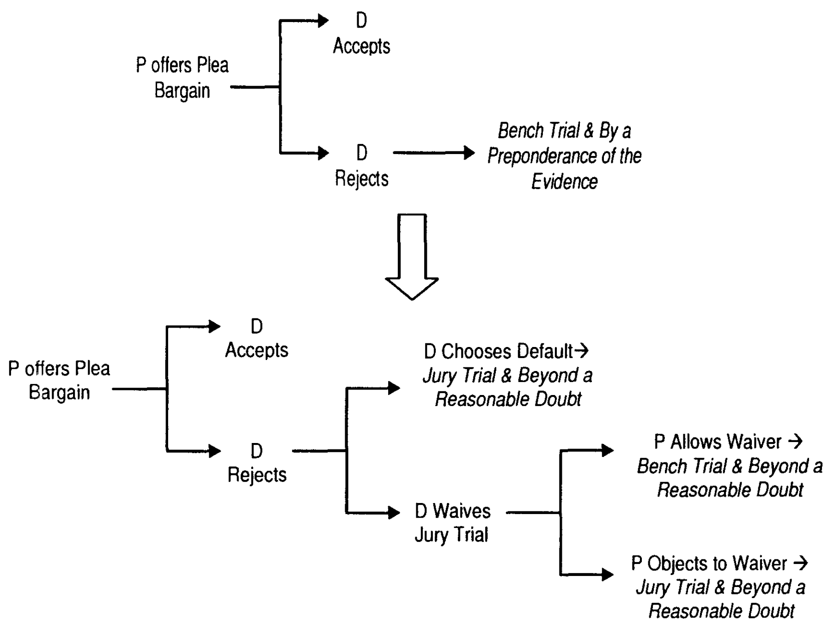


Figure 2: 2004 Sentencing Guidelines Grid

	Offense Level	Criminal History Category (Criminal History Points) I (0 or 1)	II (2 or 3)	III (4, 5, 6)	IV (7, 8, 9)	V (10, 11, 12)	VI (13 or more)
	1	0-6	0-6	0-6	0-6	0-6	0-6
	2	0-6	0-6	0-6	0-6	0-6	1-7
	3	0-6	0-6	0-6	0-6	2-8	3-9
	4	0-6	0-6	0-6	2-8	4-10	6-12
	5	0-6	0-6	1-7	4-10	6-12	9-15
	6	0-6	1-7	2-8	6-12	9-15	12-18
	7	0-6	2-8	4-10	8-14	12-18	15-21
Zone A	8	0-6	4-10	6-12	10-16	15-21	18-24
	9	4-10	6-12	8-14	12-18	18-24	21-27
Zone B	10	6-12	8-14	10-16	15-21	21-27	24-30
	11	8-14	10-16	12-18	18-24	24-30	27-33
Zone C	12	10-16	12-18	15-21	21-27	27-33	30-37
	13	12-18	15-21	18-24	24-30	30-37	33-41
	14	15-21	18-24	21-27	27-33	33-41	37-46
	15	18-24	21-27	24-30	30-37	37-46	41-51
	16	21-27	24-30	27-33	33-41	41-51	46-57
	17	24-30	27-33	30-37	37-46	46-57	51-63
	18	27-33	30-37	33-41	41-51	51-63	57-71
	19	30-37	33-41	37-46	46-57	57-71	63-78
	20	33-41	37-46	41-51	51-63	63-78	70-87
	21	37-46	41-51	46-57	57-71	70-87	77-96
	22	41-51	46-57	51-63	63-78	77-96	84-105
	23	46-57	51-63	57-71	70-87	84-105	92-115
	24	51-63	57-71	63-78	77-96	92-115	100-125
	25	57-71	63-78	70-87	84-105	100-125	110-137
	26	63-78	70-87	78-97	92-115	110-137	120-150
	27	70-87	78-97	87-108	100-125	120-150	130-162
	28	78-97	87-108	97-121	110-137	130-162	140-175
	29	87-108	97-121	108-135	121-151	140-175	151-188
	30	97-121	108-135	121-151	135-168	151-188	168-210
	31	108-135	121-151	135-168	151-188	168-210	188-235
	32	121-151	135-168	151-188	168-210	188-235	210-262
	33	135-168	151-188	168-210	188-235	210-262	235-293
	34	151-188	168-210	188-235	210-262	235-293	262-327
	35	168-210	188-235	210-262	235-293	262-327	292-365
	36	188-235	210-262	235-293	262-327	292-365	324-405
	37	210-262	235-293	262-327	292-365	324-405	360-life
	38	235-293	262-327	292-365	324-405	360-life	360-life
	39	262-327	292-365	324-405	360-life	360-life	360-life
	40	292-365	324-405	360-life	360-life	360-life	360-life
	41	324-405	360-life	360-life	360-life	360-life	360-life
Zone D	42	360-life	360-life	360-life	360-life	360-life	360-life
	43	life	life	life	life	life	life

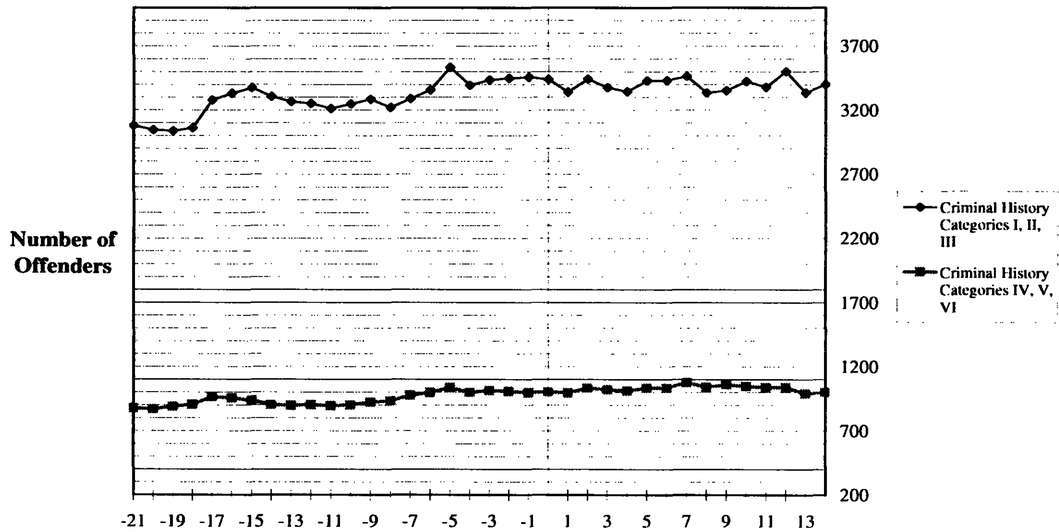
Application Notes:

1. The Offense Level (1-43) forms the vertical axis of the Sentencing Table. The Criminal History Category (I-VI) forms the horizontal axis of the Table. The intersection of the Offense Level and Criminal History Category displays the Guideline Range in months of imprisonment. "Life" means life imprisonment. For example, the guideline range applicable to a defendant with an Offense Level of 15 and a Criminal History Category of III is 24-30 months of imprisonment.

2. In rare cases, a total offense level of less than 1 or more than 43 may result from application of the guidelines. A total offense level of less than 1 is to be treated as an offense level of 1. An offense level of more than 43 is to be treated as an offense level of 43.

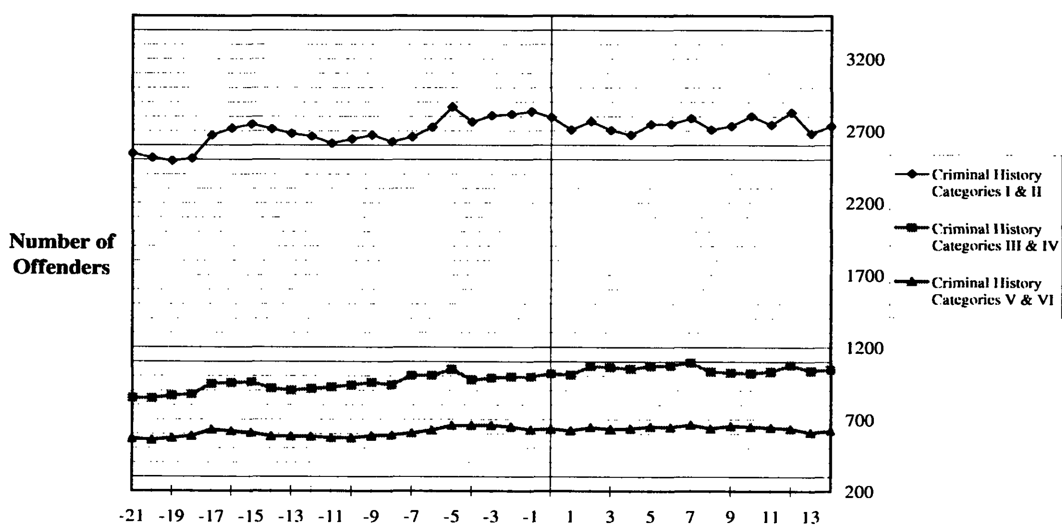
3. The Criminal History Category is determined by the total criminal history points from Chapter Four, Part A, except as provided in §§4B1.1 (Career Offender) and 4B1.4 (Armed Career Criminal). The total criminal history points associated with each Criminal History Category are shown under each Criminal History Category in the Sentencing Table.

Figure 3: Number of Cases By Month
(High and Low Criminal History Grouping)



Notes: Figure plots running three-month weighted average of offender count by criminal history group. The low criminal history group consists of offenders in categories I, II, and III; the high-level group consists of offenders in categories IV, V, and VI. Month "0" is the first month during which *Apprendi* applied.

Figure 4: Number of Cases By Month
(High, Mid, and Low Criminal History Grouping)



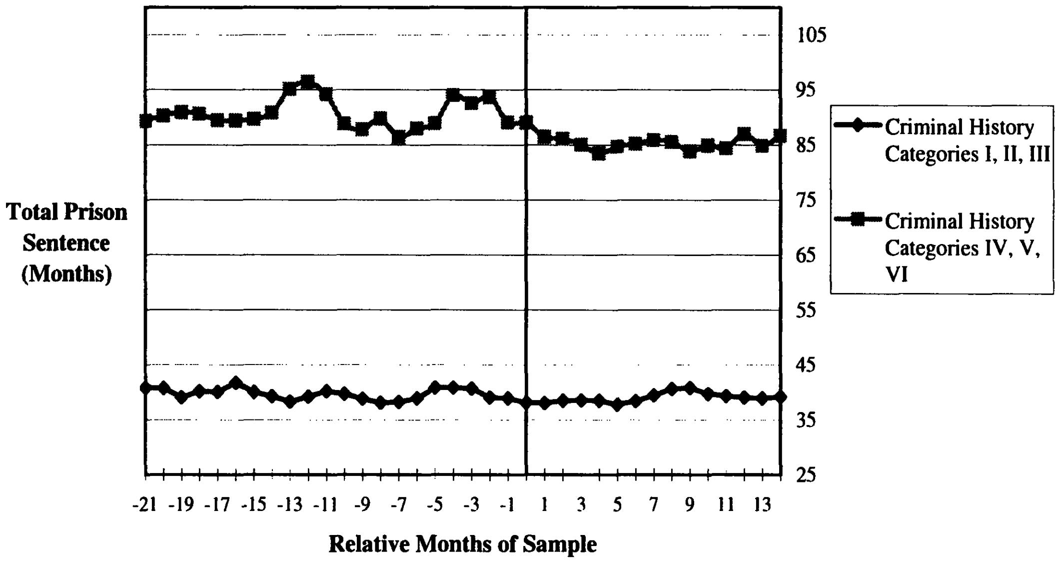
Notes: Figure plots running three-month weighted average of offender count by criminal history group. The low criminal history group consists of offenders in categories I & II; the mid-level group consists of offenders in Categories III & IV; the high-level group consists of offenders in Categories V & VI. Month "0" is the first month during which *Apprendi* applied.

Table 1: Descriptive Statistics—All Criminal History Levels

	FY 1999 Q1-2	FY 1999 Q3-4	FY 2000 Q1-2	FY 2000 Q3-4	FY 2001 Q1-2	FY 2001 Q3-4
Age (Years)	34.196	34.087	34.250	34.210	34.162	34.015
	(0.069)	(0.068)	(0.066)	(0.066)	(0.066)	(0.067)
White (Proportion)	0.649	0.657	0.677	0.694	0.668	0.669
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Male (Proportion)	0.846	0.850	0.853	0.860	0.854	0.854
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Jury Trial (Proportion)	0.054	0.053	0.048	0.043	0.037	0.030
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
U.S. Citizen (Proportion)	0.670	0.663	0.663	0.651	0.670	0.683
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
High School (Proportion)	0.194	0.194	0.197	0.193	0.200	0.196
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
College (Proportion)	0.046	0.044	0.046	0.044	0.047	0.044
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Sentence (Months)	51.626	51.294	50.792	50.302	49.794	50.002
	(0.606)	(0.594)	(0.538)	(0.539)	(0.506)	(0.516)
Obs.	24,259	24,715	25,991	26,287	26,524	26,215

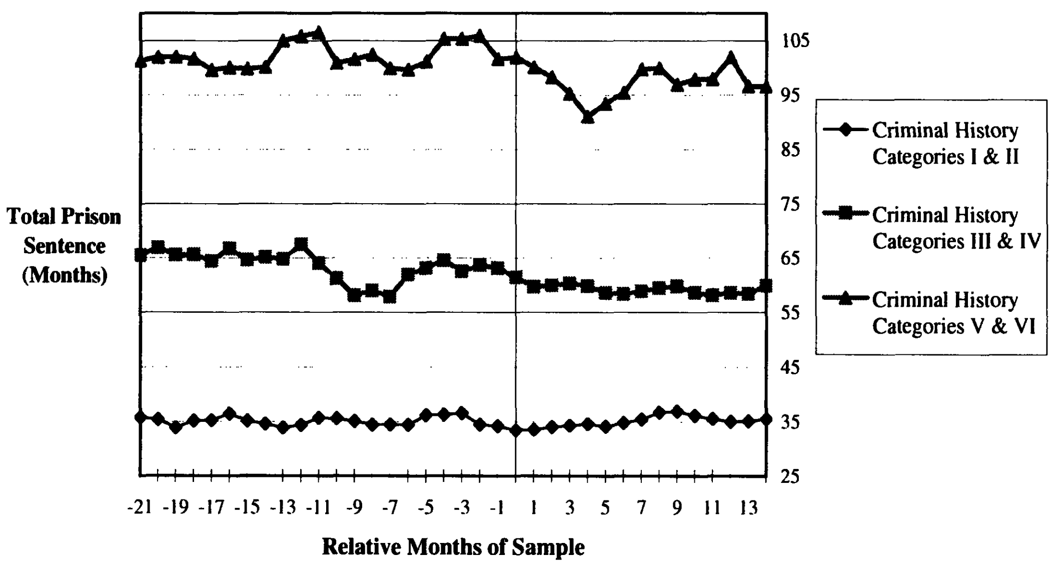
Notes: Data are from the United States Sentencing Commission. Standard errors are reported in parentheses below estimates.

Figure 5: Average Sentence Length
(High and Low Criminal History Grouping)



Notes: Figure plots running three-month weighted average sentence in months by criminal history group. The low criminal history group consists of offenders in categories I, II, and III; the high-level group consists of offenders in categories IV, V, and VI. Month "0" is the first month during which *Apprendi* applied.

Figure 6: Average Sentence Length
(High, Mid, and Low Criminal History Grouping)



Notes: Figure plots running three-month weighted average sentence in months by criminal history group. The low criminal history group consists of offenders in categories I & II; the mid-level group consists of offenders in Categories III & IV; the high-level group consists of offenders in Categories V & VI. Month "0" is the first month during which *Apprendi* applied.

Table 2: Pre-*Apprendi*/Post-*Apprendi* Differences

Low-Level Criminal History	**10/1998 - 6/2000**	**7/2000 - 9/2001**	**Difference**
Age	34.903	34.810	-0.093
	(0.049)	(0.057)	(0.075)
White (Proportion)	0.704	0.717	0.013
	(0.002)	(0.002)	(0.003)
Male (Proportion)	0.801	0.804	0.003
	(0.002)	(0.002)	(0.003)
Jury Trial (Proportion)	0.049	0.034	-0.015
	(0.001)	(0.001)	(0.001)
High School (Proportion)	0.221	0.222	0.001
	(0.002)	(0.002)	(0.003)
Sentence (Months)	35.170	34.994	-0.176
	(0.283)	(0.318)	(0.425)
Obs.	56,121	40,753	

Mid-Level Criminal History	**10/1998 - 6/2000**	**7/2000 - 9/2001**	**Difference**
Age	32.413	32.347	-0.066
	(0.067)	(0.075)	(0.101)
White (Proportion)	0.617	0.621	0.004
	(0.003)	(0.004)	(0.005)
Male (Proportion)	0.929	0.926	-0.003
	(0.002)	(0.002)	(0.003)
Jury Trial (Proportion)	0.047	0.032	-0.015
	(0.002)	(0.001)	(0.002)
High School (Proportion)	0.167	0.166	-0.001
	(0.003)	(0.003)	(0.004)
Sentence (Months)	63.941	59.382	-4.560
	(0.727)	(0.703)	(1.011)
Obs.	19,643	15,494	

High-Level Criminal History	**10/1998 - 6/2000**	**7/2000 - 9/2001**	**Difference**
Age	33.870	33.824	-0.046
	(0.074)	(0.085)	(0.113)
White (Proportion)	0.578	0.571	-0.007
	(0.004)	(0.005)	(0.007)
Male (Proportion)	0.956	0.958	0.002
	(0.002)	(0.002)	(0.003)
Jury Trial (Proportion)	0.063	0.043	-0.019
	(0.002)	(0.002)	(0.003)
High School (Proportion)	0.124	0.133	0.010
	(0.003)	(0.003)	(0.005)
Sentence (Months)	103.449	97.562	-5.887
	(1.222)	(1.209)	(1.719)
Obs.	12,520	9,460	

Notes: Data are from the United States Sentencing Commission. Standard errors are reported in parentheses below estimates.

Table 3: Basic Regression Results—High and Low Criminal History

	(1)	(2)	(3)	(4)	(5)
Apprendi	1.130	0.866	1.211		
	(0.558)*	(0.556)	(0.551)*	—	—
	[0.834]	[0.791]	[0.819]		
Apprendi × *CrimHist*	-6.183	-6.522	-6.385	-6.400	-6.393
	(1.347)**	(1.339)**	(1.314)**	(1.314)**	(1.323)**
	[1.493]**	[1.456]**	[1.460]**	[1.107]**	[1.128]**
CrimHist	42.983	44.580	44.258	44.255	44.249
	(0.975)**	(0.980)**	(0.970)**	(0.970)**	(0.970)**
	[1.170]**	[1.151]**	[1.122]**	[0.902]**	[0.907]**
College	-20.572	-21.630	-22.738	-22.806	-22.767
	(0.778)**	(0.780)**	(0.792)**	(0.794)**	(0.797)**
	[0.799]**	[0.800]**	[0.805]**	[0.810]**	[0.812]**
Age (>60)	-11.165	-11.126	-9.969	-10.051	-10.141
	(4.846)*	(4.827)*	(4.799)*	(4.807)*	(4.802)*
	[4.472]*	[4.489]*	[4.497]*	[4.493]*	[4.461]*
Black	26.913	24.544	18.091	18.089	18.160
	(1.032)**	(1.035)**	(1.138)**	(1.139)**	(1.144)**
	[1.404]**	[1.250]**	[1.279]**	[1.277]**	[1.275]**
Male	27.401	29.266	28.547	28.539	28.514
	(0.405)**	(0.412)**	(0.408)**	(0.409)**	(0.409)**
	[0.686]**	[0.760]**	[0.714]**	[0.718]**	[0.722]**
Additional Controls					
U.S. Citizenship/Residency		X	X	X	X
District Effects			X	X	X
Month Effects				X	X
Circuit and Month × Circuit Effects					X
Obs.	148,569	148,569	148,569	148,569	148,569

Notes: The dependent variable is sentence length in months. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%; ** represents significance at 1%. Circuit and Month×Circuit effects were used in place of Month×District effects for computational ease. In columns (4) and (5), the *Apprendi* main effect is absorbed by the month effects, but *Apprendi* ×*CrimHist* is identified.

Table 4: Basic Regression Results—High, Mid, and Low Criminal History

	(1)	(2)	(3)	(4)	(5)
Apprendi	1.716	1.452	1.730		
	(0.563)**	(0.562)**	(0.558)**	—	—
	[0.873]	[0.832]	[0.864]*		
Apprendi × *CrimHistMID*	-4.890	-5.026	-4.524	-4.534	-4.550
	(1.103)**	(1.097)**	(1.081)**	(1.082)**	(1.091)**
	[1.218]**	[1.200]**	[1.180]**	[0.811]**	[0.870]**
Apprendi × *CrimHistHIGH*	-6.576	-6.880	-6.758	-6.745	-6.701
	(1.781)**	(1.769)**	(1.734)**	(1.735)**	(1.742)**
	[1.957]**	[1.937]**	[1.942]**	[1.630]**	[1.649]**
CrimHistMID	58.538	60.426	60.061	60.044	60.032
	(1.272)**	(1.275)**	(1.260)**	(1.260)**	(1.261)**
	[1.217]**	[1.212]**	[1.190]**	[1.025]**	[1.025]**
CrimHistHIGH	20.921	22.461	22.621	22.628	22.599
	(0.796)**	(0.800)**	(0.795)**	(0.796)**	(0.797)**
	[0.999]**	[0.992]**	[0.924]**	[0.706]**	[0.717]**
Additional Controls					
U.S. Citizenship/Residency		X	X	X	X
District Effects			X	X	X
Month Effects				X	X
Circuit and Month × Circuit Effects					X
Obs.	148,569	148,569	148,569	148,569	148,569

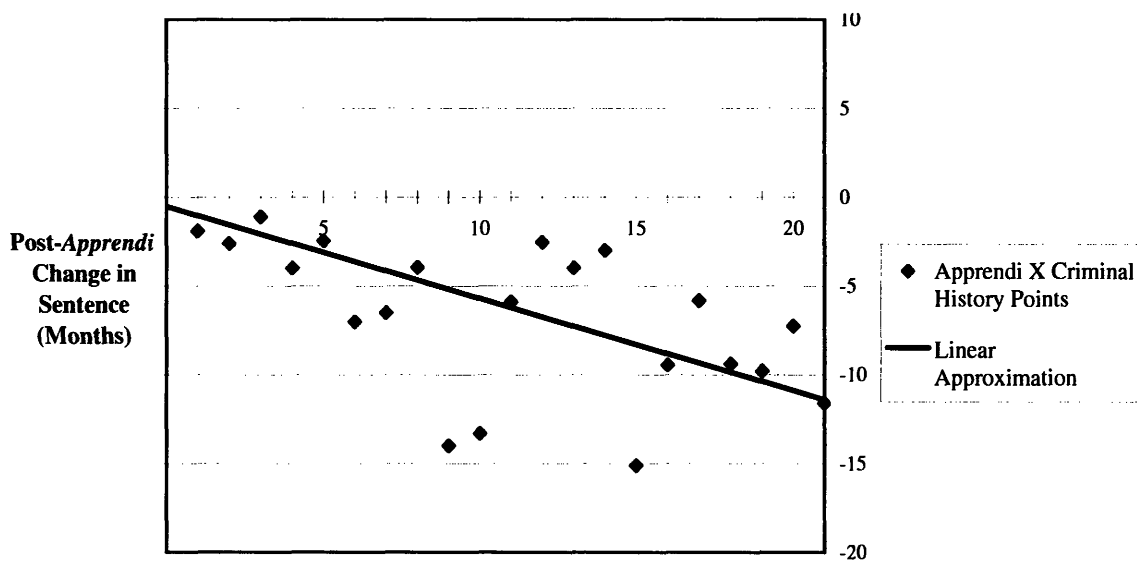
Notes: The dependent variable is sentence length in months. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHistMID* equals one for a post-*Apprendi* offender with a criminal history category of III or IV (and equals zero if otherwise). *Apprendi* × *CrimHistHIGH* equals one for a post-*Apprendi* offender with a criminal history category of V or VI (and zero otherwise) The symbol * represents statistical significance at 5%; ** represents significance at 1%. Circuit and Month×Circuit effects were used in place of Month×District effects for computational ease. In columns (4) and (5), the *Apprendi* main effect is absorbed by the month effects, but *Apprendi* ×*CrimHistMID* and *Apprendi* ×*CrimHistMID* are identified.

Table 5: Basic Regression Results—Six Criminal History Categories

	(1)	(2)	(3)	(4)	(5)
Apprendi	1.941 (0.569)** [0.788]*	1.647 (0.568)** [0.753]*	1.910 (0.565)** [0.765]*	—	—
Apprendi × *CrimHist2*	-1.832 (1.316) [1.631]	-1.587 (1.310) [1.622]	-1.304 (1.292) [1.653]	-1.340 (1.290) [1.339]	-1.596 (1.298) [1.367]
Apprendi × *CrimHist3*	-4.801 (1.312)** [1.441]**	-4.751 (1.305)** [1.427]**	-4.173 (1.285)** [1.404]**	-4.165 (1.286)** [1.083]**	-4.163 (1.295)** [1.146]**
Apprendi × *CrimHist4*	-5.984 (1.828)** [1.909]**	-6.356 (1.816)** [1.879]**	-5.923 (1.788)** [1.837]**	-5.979 (1.790)** [1.767]**	-6.121 (1.795)** [1.771]**
Apprendi × *CrimHist5*	-4.100 (2.146) [2.507]	-4.541 (2.132)* [2.480]	-4.784 (2.095)* [2.426]*	-4.740 (2.095)* [2.287]*	-5.054 (2.112)* [2.297]*
Apprendi × *CrimHist6*	-7.366 (2.409)** [2.580]**	-7.559 (2.394)** [2.545]**	-7.218 (2.347)** [2.530]**	-7.233 (2.348)** [2.301]**	-7.056 (2.349)** [2.311]**
CrimHist2	13.161 (0.902)** [1.287]**	13.465 (0.901)** [1.276]**	13.541 (0.890)** [1.266]**	13.530 (0.889)** [1.067]**	13.642 (0.893)** [1.069]**
CrimHist4	29.925 (1.352)** [1.531]**	32.442 (1.352)** [1.507]**	32.961 (1.337)** [1.470]**	32.982 (1.338)** [1.336]**	33.010 (1.335)** [1.326]**
CrimHist6	76.622 (1.695)** [1.433]**	78.232 (1.691)** [1.424]**	77.315 (1.665)** [1.378]**	77.289 (1.665)** [1.295]**	77.222 (1.662)** [1.322]**
Additional Controls					
U.S. Citizenship/Residency		X	X	X	X
District Effects			X	X	X
Month Effects				X	X
Circuit and Month × Circuit Effects					X
Obs.	148,569	148,569	148,569	148,569	148,569

Notes: The dependent variable is sentence length in months. Robust standard errors are reported in parentheses: robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHISTX* equals one for a post-*Apprendi* offender with a criminal history category of X (and equals zero if otherwise), where X is a number between 2 and 6. The symbol * represents statistical significance at 5%; ** represents significance at 1%. Circuit and Month×Circuit effects were used in place of Month×District effects for computational ease. In columns (4) and (5), the *Apprendi* main effect is absorbed by the month effects, but *Apprendi* ×*CrimHISTX* is identified.

Figure 7: Change in Average Sentence Length Post-*Apprendi* by Criminal History Points



Criminal History Points

Notes: Figure plots unweighted coefficients estimated on *Apprendi*×*CrimHistPts* in the basic model with controls for U.S. Citizenship, U.S. Residency, Number of Dependents. District, Month, and Circuit×Month effects included.

Table 6: Drug Offenders Only

	(1)	(2)	(3)	(4)	(5)
Apprendi	-0.572	-0.686	-1.033		
	(0.879)	(0.879)	(0.854)	—	—
	[1.22]	[1.215]	[1.097]		
Apprendi × *CrimHist*	-7.775	-7.751	-7.400	-7.535	-6.786
	(2.733)**	(2.733)**	(2.646)**	(2.649)**	(2.650)*
	[2.978]*	[2.982]*	[2.779]**	[2.418]**	[2.362]**
CrimHist	63.762	62.705	60.617	60.675	60.333
	(2.003)**	(2.009)**	(1.948)**	(1.950)**	(1.933)**
	[2.383]**	[2.385]**	[2.245]**	[1.920]**	[1.871]**
Additional Controls					
U.S. Citizenship/Residency		X	X	X	X
District Effects			X	X	X
Month Effects				X	X
Circuit and Month × Circuit Effects					X
Obs.	65,415	65,415	65,415	65,415	65,415

Notes: Sample includes only offenders classified as having committed a drug-related offense. The dependent variable is sentence length in months. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%; ** represents significance at 1%. Circuit and Month×Circuit effects were used in place of Month×District effects for computational ease. In columns (4) and (5), the *Apprendi* main effect is absorbed by the month effects, but *Apprendi* ×*CrimHist* is identified.

Table 7: "Add On" And "Nested" Statutes Only

	(1)	(2)	(3)	(4)	(5)
Apprendi	-0.678	-0.702	-0.219		
	(0.978)	(0.979)	(0.963)	—	—
	[1.200]	[1.203]	[1.105]		
Apprendi × *CrimHist*	-10.037	-10.064	-9.924	-9.996	-8.892
	(2.896)**	(2.897)**	(2.818)**	(2.821)**	(2.831)**
	[2.631]**	[2.635]**	[2.516]**	[2.175]**	[2.270]**
CrimHist	68.422	68.039	64.966	64.999	64.590
	(2.149)**	(2.153)**	(2.092)**	(2.094)**	(2.084)**
	[2.110]**	[2.102]**	[2.047]**	[1.797]**	[1.843]**
Additional Controls					
U.S. Citizenship/Residency		X	X	X	X
District Effects			X	X	X
Month Effects				X	X
Circuit and Month × Circuit Effects					X
Obs.	53,929	53,929	53,929	53,929	53,929

Notes: Sample includes only those offenders sentenced under one of the selected "nested" or "add on" statutes collected in King & Klein (2000 Appendix A). The dependent variable is sentence length in months. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%; ** represents significance at 1%. Circuit and Month×Circuit effects were used in place of Month×District effects for computational ease. In columns (4) and (5), the *Apprendi* main effect is absorbed by the month effects, but *Apprendi* ×*CrimHist* is identified.

Table 8: Cases Without Criminal History Departures

	(1)	(2)	(3)	(4)	(5)
Apprendi	1.274	1.004	1.327		
	(0.565)*	(0.563	(0.558)*	—	—
	[0.834]	[0.790]	[0.816]		
Apprendi × *CrimHist*	-6.261	-6.598	-6.520	-6.543	-6.574
	(1.416)**	(1.407)**	(1.381)**	(1.381)**	(1.390)**
	[1.574]**	[1.540]**	[1.547]**	[1.178]**	[1.197]**
CrimHist	44.021	45.612	45.273	45.275	45.303
	(1.023)**	(1.028)**	(1.017)**	(1.017)**	(1.017)**
	[1.246]**	[1.226]**	[1.195]**	[0.965]**	[0.965]**
Additional Controls					
U.S. Citizenship/Residency		X	X	X	X
District Effects			X	X	X
Month Effects				X	X
Circuit and Month × Circuit Effects					X
Obs.	144,775	144,775	144,775	144,775	144,775

Notes: Sample excludes those offenders whose sentencing included a depature related to criminal history. The dependent variable is sentence length in months. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%; ** represents significance at 1%. Circuit and Month×Circuit effects were used in place of Month×District effects for computational ease. In columns (4) and (5), the *Apprendi* main effect is absorbed by the month effects, but *Apprendi* ×*CrimHist* is identified.

Table 9: Cases Without Guidelines Departures

	(1)	(2)	(3)	(4)	(5)
Apprendi	1.681	1.410	1.598		
	(0.831)*	(0.828)	(0.822)	—	—
	[1.240]	[1.211]	[1.224]		
Apprendi ×*CrimHist*	-9.248	-9.678	-9.746	-9.773	-9.990
	(2.045)**	(2.032)**	(1.997)**	(1.997)**	(2.008)**
	[2.228]**	[2.192]**	[2.173]**	[1.661]**	[1.682]**
CrimHist	50.436	52.215	51.910	51.907	52.062
	(1.475)**	(1.479)**	(1.468)**	(1.468)**	(1.467)**
	[1.824]**	[1.810]**	[1.719]**	[1.360]**	[1.357]**
Additional Controls					
U.S. Citizenship/Residency		X	X	X	X
District Effects			X	X	X
Month Effects				X	X
Circuit and Month × Circuit Effects					X
Obs.	91,520	91,520	91,520	91,520	91,520

Notes: Sample excludes those offenders whose sentencing included a guidelines depature of any kind. The dependent variable is sentence length in months. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%; ** represents significance at 1%. Circuit and Month×Circuit effects were used in place of Month×District effects for computational ease. In columns (4) and (5), the *Apprendi* main effect is absorbed by the month effects, but *Apprendi* ×*CrimHist* is identified.

Table 10: Controlling for Offense Type

	(1)	(2)	(3)	(4)	(5)
Apprendi	0.776	0.684	0.896		
	(0.517)	(0.517)	(0.512)	—	—
	[0.631]	[0.629]	[0.627]		
Apprendi × *CrimHist*	-5.686	-5.685	-5.529	-5.534	-5.539
	(1.248)**	(1.248)**	(1.225)**	(1.226)**	(1.234)**
	[1.349]**	[1.346]**	[1.358]**	[1.094]**	[1.106]**
CrimHist	48.650	48.184	46.989	46.970	46.961
	(0.947)**	(0.945)**	(0.933)**	(0.934)**	(0.933)**
	[1.133]**	[1.125]**	[1.104]**	[0.917]**	[0.918]**
Additional Controls					
U.S. Citizenship/Residency		X	X	X	X
District Effects			X	X	X
Month Effects				X	X
Circuit and Month × Circuit Effects					X
Obs.	148,517	148,517	148,517	148,517	148,517

Notes: Dummies for offense type are included as independent variables. The dependent variable is sentence length in months. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%; ** represents significance at 1%. Circuit and Month×Circuit effects were used in place of Month×District effects for computational ease. In columns (4) and (5), the *Apprendi* main effect is absorbed by the month effects, but *Apprendi* ×*CrimHist* is identified.

Table 11: Omitting Twelve-Month Period Around *Apprendi*

	(1)	(2)	(3)	(4)	(5)
Apprendi	4.603	4.368	5.011	—	—
	(1.111)**	(1.112)**	(1.103)**		
	[1.351]**	[1.368]**	[1.387]**		
Apprendi × *CrimHist*	-6.409	-6.386	-6.049	-6.056	-6.065
	(1.577)**	(1.577)**	(1.548)**	(1.549)**	(1.559)**
	[1.515]**	[1.512]**	[1.556]**	[1.271]**	[1.290]**
CrimHist	48.906	48.416	47.260	47.245	47.230
	(1.161)**	(1.160)**	(1.147)**	(1.147)**	(1.145)**
	[1.134]**	[1.130]**	[1.084]**	[0.961]**	[0.948]**
Additional Controls					
U.S. Citizenship/Residency		X	X	X	X
District Effects			X	X	X
Month Effects				X	X
Circuit and Month × Circuit Effects					X
Obs.	97,665	97,665	97,665	97,665	97,665

Notes: Sample excludes offenders sentenced in the six months before and after *Apprendi*. The dependent variable is sentence length in months. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%; ** represents significance at 1%. Circuit and Month×Circuit effects were used in place of Month×District effects for computational ease. In columns (4) and (5), the *Apprendi* main effect is absorbed by the month effects, but *Apprendi* ×*CrimHist* is identified.

Table 12: High and Low Criminal History with Linear Trend

	(1)	(2)	(3)	(4)	(5)
Apprendi	-2.304 (0.884)** [1.167]	-2.176 (0.880)* [1.147]	-2.594 (0.870)** [1.114]*	—	—
Apprendi × *CrimHist*	-6.157 (1.347)** [1.461]**	-6.498 (1.339)** [1.434]**	-6.355 (1.314)** [1.417]**	-6.400 (1.314)** [1.107]**	-6.393 (1.323)** [1.128]**
CrimHist	42.968 (0.975)** [1.151]**	44.568 (0.980)** [1.130]**	44.247 (0.970)** [1.098]**	44.255 (0.970)** [0.902]**	44.249 (0.970)** [0.907]**
Additional Controls					
U.S. Citizenship/Residency		X	X	X	X
District Effects			X	X	X
Month Effects				X	X
Circuit and Month × Circuit Effects					X
Obs.	148,569	148,569	148,569	148,569	148,569

Notes: A linear trend and trend squared have been included as independent variables. The dependent variable is sentence length in months. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%; ** represents significance at 1%. Circuit and Month×Circuit effects were used in place of Month×District effects for computational ease. In columns (4) and (5), the *Apprendi* main effect is absorbed by the month effects, but *Apprendi* ×*CrimHist* is identified.

Table 13: Three-Month Interaction Dummies

	(1)	(2)	(3)	(4)	(5)
5 Periods Before × *CrimHist*	2.449	1.956	2.246	2.238	1.783
	(3.742)	(3.713)	(3.643)	(3.644)	(3.662)
	[2.487]	[2.607]	[2.043]	[1.467]	[1.502]
4 Periods Before × *CrimHist*	5.856	5.026	5.706	5.687	5.384
	(3.900)	(3.869)	(3.801)	(3.797)	(3.829)
	[3.751]	[3.616]	[3.576]	[3.122]	[3.077]
3 Periods Before × *CrimHist*	1.623	1.250	1.199	1.150	0.634
	(3.681)	(3.652)	(3.592)	(3.592)	(3.609)
	[1.722]	[1.832]	[1.505]	[1.267]	[1.337]
2 Periods Before × *CrimHist*	-2.176	-2.537	-2.273	-2.248	-2.604
	(3.404)	(3.378)	(3.314)	(3.313)	(3.330)
	[4.118]	[4.132]	[3.452]	[2.040]	[2.091]
1 Period Before × *CrimHist*	5.115	4.846	5.091	5.112	4.940
	(3.714)	(3.684)	(3.623)	(3.624)	(3.646)
	[2.980]	[3.149]	[3.234]	[2.826]	[2.952]
1 Period After × *CrimHist*	-2.069	-2.674	-2.405	-2.416	-2.797
	(3.415)	(3.392)	(3.332)	(3.332)	(3.351)
	[2.104]	[2.114]	[1.933]	[1.254]	[1.128]*
2 Periods After × *CrimHist*	-5.598	-6.050	-6.098	-6.122	-6.299
	(3.283)	(3.255)	(3.206)	(3.206)	(3.236)
	[2.085]**	[2.049]**	[1.515]**	[1.277]**	[1.394]**
3 Periods After × *CrimHist*	-3.691	-3.968	-3.307	-3.447	-3.563
	(3.371)	(3.345)	(3.290)	(3.292)	(3.322)
	[2.612]	[2.601]	[2.692]	[1.213]**	[1.147]**
4 Periods After × *CrimHist*	-5.229	-6.117	-5.631	-5.623	-5.848
	(3.386)	(3.359)	(3.303)	(3.304)	(3.333)
	[2.654]	[2.675]*	[2.447]*	[2.073]**	[2.277]*
Additional Controls					
U.S. Citizenship/Residency		X	X	X	X
District Effects			X	X	X
Month Effects				X	X
Circuit and Month × Circuit Effects					X
Obs.	148,569	148,569	148,569	148,569	148,569

Notes: The reported coefficients are interactions between criminal history (IV, V, and VI) and three-month period dummies. The variable *X Periods Before(After)* ×*CrimHist* equals one for an offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III) sentenced X three-month periods before(after) *Apprendi* was announced. The dependent variable is sentence length in months. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. The symbol * represents statistical significance at 5%; ** represents significance at 1%. Circuit and Month×Circuit effects were used in place of Month×District effects for computational ease.

Table A1: Jury Trial Cases Only

	(1)	(2)	(3)	(4)	(5)
Apprendi	-12.630	-12.195	-9.814		
	(7.771)	(7.762)	(7.784)	—	—
	[7.886]	[7.869]	[8.542]		
Apprendi × *CrimHist*	-32.142	-30.536	-29.611	-30.758	-33.577
	(15.415)*	(15.307)*	(14.950)*	(14.927)*	(15.416)*
	[16.203]	[16.014]	[16.803]	[13.126]*	[12.402]**
CrimHist	116.715	115.049	115.436	116.315	116.483
	(10.243)**	(10.208)**	(10.058)**	(10.036)**	(10.305)**
	[12.377]**	[12.008]**	[12.555]**	[9.887]**	[9.734]**
Additional Controls					
U.S. Citizenship/Residency		X	X	X	X
District Effects			X	X	X
Month Effects				X	X
Circuit and Month × Circuit Effects					X
Obs.	6,573	6,573	6,573	6,573	6,573

Notes: The sample includes only offenders convicted by a jury. The dependent variable is sentence length in months. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%; ** represents significance at 1%. Circuit and Month×Circuit effects were used in place of Month×District effects for computational ease. In columns (4) and (5), the *Apprendi* main effect is absorbed by the month effects, but *Apprendi* ×*CrimHist* is identified.

Table A2: Plea Bargained Cases Only

	(1)	(2)	(3)	(4)	(5)
Apprendi	2.660	2.434	2.532		
	(0.443)**	(0.440)**	(0.435)**	—	—
	[0.718]**	[0.672]**	[0.731]**		
Apprendi × *CrimHist*	-2.338	-2.661	-2.564	-2.560	-2.492
	(1.068)*	(1.060)*	(1.038)*	(1.038)*	(1.045)*
	[1.178]	[1.125]*	[1.119]*	[0.664]**	[0.677]**
CrimHist	36.771	38.104	37.716	37.694	37.629
	(0.735)**	(0.737)**	(0.725)**	(0.725)**	(0.726)**
	[0.781]**	[0.752]**	[0.710]**	[0.486]**	[0.477]**
Additional Controls					
U.S. Citizenship/Residency		X	X	X	X
District Effects			X	X	X
Month Effects				X	X
Circuit and Month × Circuit Effects					X
Obs.	141,407	141,407	141,407	141,407	141,407

Notes: The sample includes only offenders who pleaded guilty. The dependent variable is sentence length in months. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%; ** represents significance at 1%. Circuit and Month×Circuit effects were used in place of Month×District effects for computational ease. In columns (4) and (5), the *Apprendi* main effect is absorbed by the month effects, but *Apprendi* ×*CrimHist* is identified.

Table A3: Tobit Results—Correcting for Right-Hand Censoring

	(1)	(2)	(3)	(4)	(5)
Apprendi	1.130	0.866	1.210	—	—
	(0.488)*	(0.485)	(0.488)*		
Apprendi × *CrimHist*	-6.190	-6.528	-6.397	-6.412	
	(0.651)**	(0.648)**	(0.657)**	(0.663)**	
CrimHist	42.995	44.593	44.279	44.276	
	(0.424)**	(0.424)**	(0.435)**	(0.440)**	
Additional Controls					
U.S. Citizenship/Residency		X	X	X	X
District Effects			X	X	X
Month Effects				X	X
Circuit and Month × Circuit Effects					X
Obs.	148,569	148,569	148,569	148,569	148,569

Notes: The dependent variable is sentence length in months. Standard errors are reported in parentheses. *Apprendi* × *CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%; ** represents significance at 1%. Circuit and Month×Circuit effects were used in place of Month×District effects for computational ease. In columns (4) and (5), the *Apprendi* main effect is absorbed by the month effects, but *Apprendi* ×*CrimHist* is identified.

Figure A1: "Federal" Jury Trial Right

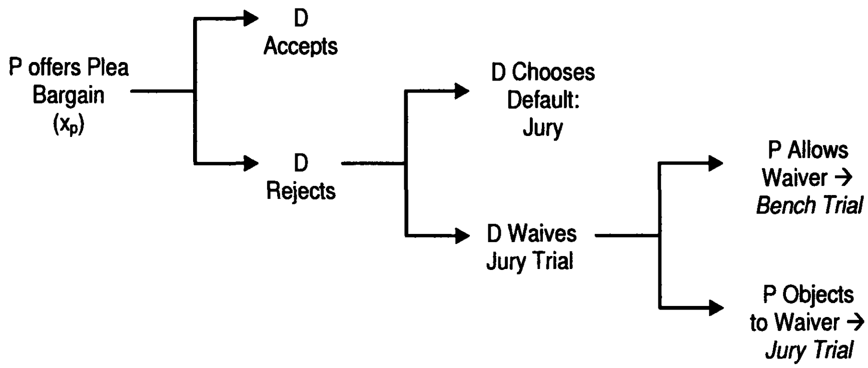


Figure A2: Pre-*Apprendi* Sentencing

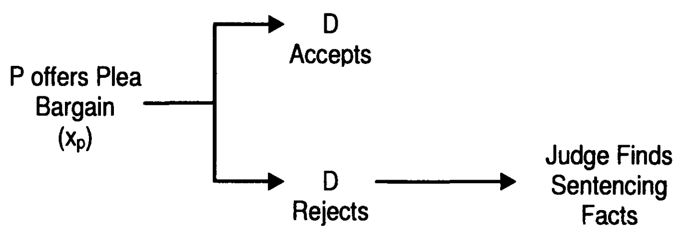


Figure A3: Possible Consequences of Broader Jury Trial Rights

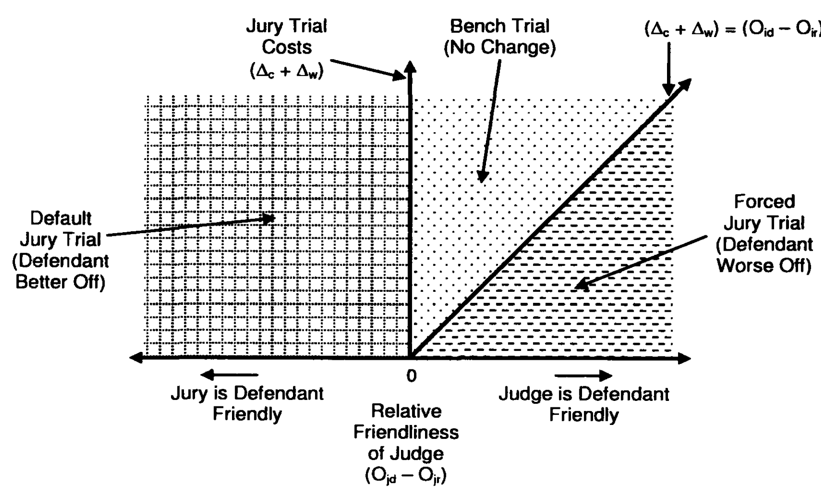
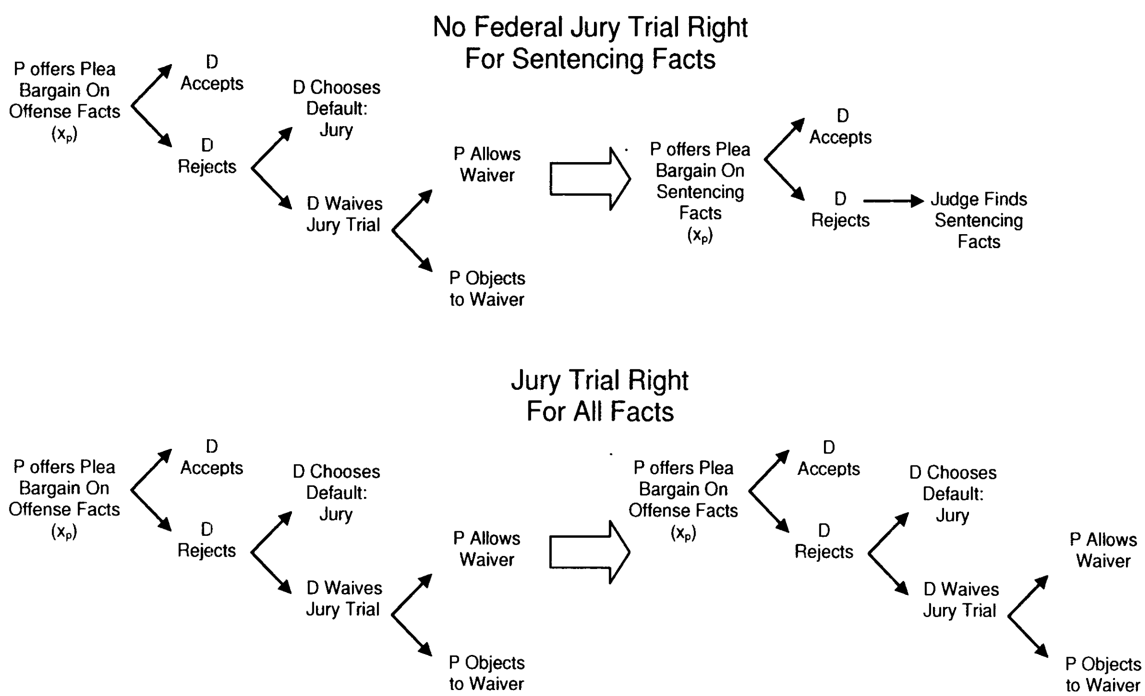


Figure A4: "Broadening" Jury Trial Rights



Chapter 2

Identifying Prosecutorial Charging Manipulation

Abstract

Prosecutors are private individuals as well as public agents. They are instructed to bring all appropriate charges against alleged offenders in an even-handed way, but limited budget and personnel resources make extensive prosecutorial discretion necessary, and prosecutors may have preferences that are inconsistent with their public mandate. A growing empirical literature seeks to identify what, precisely, prosecutors "maximize" when allowed the necessary freedom to operate in a resource-constrained environment. There is some evidence that prosecutors value career advancement and preserving the status quo, for example, but much less is known about what instruments prosecutors use and how freely they can pursue these ends. In this paper, I examine whether (and how and by how much) prosecutorial charging decisions respond to a pro-defendant tightening of procedural requirements. I use federal arrest, charging, and sentencing data to evaluate the charging response of prosecutors to a Supreme Court decision that affected groups of offenders differently. I find some evidence that prosecutors reacted by reducing the total number of counts filed against affected defendants by as much as 10%, presumably magnifying the sentence reduction that would have occurred had prosecutors not substituted charging resources toward unaffected defendants.

2.1 Introduction

Prosecutors have a general rule to determine whether they ought to bring a charge against a defendant: prosecute all crimes that can reasonably be proven, but otherwise leave the policy decisions and the scope of procedural protections to others. Students of prosecutorial practice have long argued, however, that prosecutors, as a practical matter, play by different rules. In order to maximize their visibility, success, and effectiveness as well as to preserve their limited resources, prosecutors may manipulate the charges they bring in order to induce easy plea bargains or to assure a target sentence is reached.[1] Prosecutors may also use their charging discretion to preserve the status quo when policymakers or courts attempt to reform the criminal justice system either substantively or procedurally.

In this paper, I study how prosecutorial charging behavior responded to a shift in constitutional criminal procedure. In June 2000, the U.S. Supreme Court decided in *Apprendi v. New Jersey* that the Sixth Amendment applied to certain sentencing facts,[2] meaning the newly covered facts had to be proven to a jury instead of a judge (unless waived), and had to be proved beyond a reasonable doubt rather than by the lower preponderance of the evidence standard. This procedural shift had the effect, under certain circumstances, of increasing the cost and difficulty for prosecutors of proving these facts.[3] Despite the legal and practical significance of *Apprendi* for litigants and the criminal justice system generally, nothing systematic is known about whether or how prosecutors reacted to the new requirements.

Understanding how prosecutors respond to procedural change is key to understanding how these public agents allocate their limited resources and what they seek to accomplish with their decision making. For example, prosecutors solely focused on proving that a

[1] Some of this manipulation may be related to "charge bargaining," in which prosecutors adjust the charges they bring as a result of plea negotiation to assure a guilty plea from the defendant. But charge manipulation is potentially much more general. Prosecutors may alter the way they charge a defendant without making any assumptions about whether a plea bargain will be struck, or even when a prosecutor prefers to go to trial.

[2] The text of the case can be found in the U.S. Reporter at 530 U.S. 466 (2000).

[3] Under the United States Sentencing Guidelines, which were mandatory during the time of this study, a defendant's sentence is calculated using a complicated combination of proven offenses and sentencing facts. See Campbell and Bemporad (2003) for more detail on how federal offenders were sentenced during this period.

crime occurred and that particular sentencing facts were applicable should have asked only whether *Apprendi* applied to the circumstances of the case and, if so, whether there was sufficient evidence to meet the new proof requirement before a jury (or whether there were sufficient resources to satisfy the higher burden) for the relevant sentencing facts. If not or if the question were close, then the prosecutor might understandably decide not to argue the sentencing facts in question. But the new constitutional rule did not change the procedural requirements with respect to base offenses, and so, in theory, a prosecutor should not have altered the set of basic charges brought against a defendant.[4]

If, however, prosecutors also seek other goals,[5] then the higher cost of proving sentencing facts may lead to a broader set of responses—but only if prosecutors have the instruments and freedom (i.e., the discretion) necessary to vary their response. For example, if prosecutors are able to alter how they charge a defendant easily, a prosecutor may try to "undo" some of the consequences of the procedural innovation by substituting away from more expensive sentencing enhancements toward additional offense counts.[6] Alternatively, a prosecutor may view the prosecution of an *Apprendi*-affected defendant as more expensive overall, and so decide to substitute resources toward other defendants. This latter sort of prosecutorial reaction would essentially magnify the consequences of the policy change.

I use federal arrest, charging, and sentencing data to examine how prosecutors use their discretion over base-offense charging decisions to respond to an increase in the cost of proving sentencing enhancements. Although my empirical approach cannot identify ideal treatment and control groups, I find evidence that prosecutors reduce the number of counts for those affected by the price increase by approximately 10%, relative to other offenders who were not affected by the decision in *Apprendi*.

If valid, this conclusion has a number of implications. First, prosecutors appear to have significant discretion in their charging decisions *and* appear willing to use that discretion

[4]Put differently, from a legal perspective, base offenses and sentencing facts are generally not substitutes for one another. They are separate inquiries.

[5]Alternative goals might include maximizing the sentence for a defendant, minimizing resource use when achieving a particular sentence, or avoiding cross-defendant sentencing disparity.

[6]The U.S. Sentencing Guidelines dictate that, in most cases, counts related to the same set of events run concurrently, and so additional proven counts may not always increase a sentence. On the other hand, there are many exceptions, and if the count relates to a different set of events, the sentences might be "stacked." See generally USSG §3D1.1-5. In addition, evidence suggests that additional counts on average lead to a higher overall sentence (Wilmot and Spohn 2004).

to respond to policy changes. Second, prosecutors may view base-offense counts as complements for sentencing facts, in line with their specific legal definitions and functions. Third, the data indicate that prosecutors may magnify the consequences of procedural innovations by reallocating their resources across defendants in response to price changes. Finally, the reduction in the number of counts documented here may explain in part the reduction in sentence length that Prescott (2006) shows was experienced by *Apprendi*-affected offenders. It is not possible to distinguish the reduction in sentence length caused directly by the procedural change from the reduction caused indirectly by charging manipulation, but the positive correlation between counts and sentence length suggests changes in charging behavior played a role.

The remainder of this paper motivates and presents my empirical work. In Section 2.2, I review the empirical literature on prosecutorial incentives and behavior. In Section 2.3, I use a simple model of prosecutorial behavior to develop a set of testable hypotheses. Section 2.4 describes my data and explains my empirical approach. In Section 2.5, I present my empirical model and report my results. Section 2.6 concludes.

2.2 Prosecutorial Behavior and Discretion

Prosecutors are public officials directed to carry out specific public tasks, and yet they exercise "largely uncontrolled discretion" in deciding whether and how to prosecute someone charged with a crime (LaFave 1970, p.532).[7] In the sentencing guidelines era (in which judges are precluded in large part from checking the charging and bargaining behavior of prosecutors), concern over prosecutorial behavior has become even more pointed (see, e.g., Richman 2003, pp.750-51). There is a considerable legal literature discussing the extent of prosecutorial discretion (and its accompanying theoretical costs and benefits), but less empirical work exists on the scope of discretion and whether or how prosecutors exercise it (O'Neill 2005).

As a legal matter, prosecutors are essentially free to prosecute who they want, when they want, and how they want (Brown 2004, pp. 331-33). Courts and legislatures have cir-

[7]Various reasons have been offered for the existence of this discretion: overcriminalization of primary activity, prosecutorial resource constraints, and the need for individualized justice (see LaFave 2000, pp.533-35; NDAA 1991, pp.127-28).

cumscribed prosecutorial behavioral only to a very limited extent (e.g., strictly prohibiting racially motivated prosecutions).[8] In part, this "hands-off" legal approach has its source in "separation of powers" ideas, but it is also unavoidable when funds are limited and when important, difficult, and costly-to-review decisions are necessarily and regularly made by many agents.

Professional organizations have established norms to improve the likelihood that discretion is used to further criminal justice aims.[9] Prosecutors are also regulated by their respective executive. The U.S. Department of Justice, for instance, publishes the "Principles of Federal Prosecution" in its U.S. Attorneys' Manual.[10] The federal government also issues memoranda and guidelines to its prosecutors from time to time to limit the inappropriate exercise of charging discretion.[11] This guidance, if effective, implies that while prosecutors require sufficient discretion to exercise their judgment in a particular case, they are ultimately public agents who will (generally) aim to maximize social welfare.

[8]One possible counterexample is the application of real offense sentencing in the U.S. Sentencing Guidelines, which was intended to generate sentences based on what actually happened, as opposed to how a prosecutor chooses to charge someone. The fact that prosecutors control much of the information a court sees is an obvious limitation to this technique.

[9]For example, the National District Attorneys Association ("NDAA") has established guidelines that prosecutors "should file only those charges [they] reasonably believe[] can be substantiated by admissible evidence at trial" (Standard 43.3) and that prosecutors "should not attempt to utilize the charging decision only as a leverage device in obtaining guilty please to lesser charges." (Standard 43.4). Standard 42.4 states that a prosecutor should not consider his or her "rate of conviction" or the "personal" or "political" advantages that result from a particular prosecution.

[10]On the surface these rules appear to significantly limit a prosecutor's discretion. For example, federal prosecutors "should charge...the most serious offense that is consistent with the nature of the defendant's conduct, and that is likely to result in a sustainable conviction" (Principle 9-27.300) and should file additional charges only when they are "necessary to ensure that the information or indictment [a]dequately reflects the nature and extent of the criminal conduct involved, and provides the basis for an appropriate sentence under all the circumstances of the case; or [w]ill significantly enhance the strength of the government's case against the defendant or a codefendant" (Principle 9-27.320). But commentary on the principles signals that prosecutors retain substantial discretion, allowing prosecutors to weigh various factors so as to best carry out the public purposes of criminal law (see also Rabin 1972, p.1042). The rules are precise about forbidden goals, however: "In determining whether to commence or recommend prosecution or take other action against a person, the attorney for the government should not be influenced by: (1) [t]he person's race, religion, sex, national origin, or political association, activities or beliefs; (2) [t]he attorney's own personal feelings concerning the person, the person's associates, or the victim; or (3) [t]he possible effect of the decision on the attorney's own professional or personal circumstances" (Principle 9-27.260).

[11]For example, in 1987, after the federal sentencing guidelines went into affect, the Department of Justice published the "the Redbook," which addressed prosecutorial discretion and charging policy under the guidelines. The Redbook admits the necessity of discretion, but "underscores the impropriety of dismissing provable charges and excludes charge bargains based on factors other than the weakness of the case, such as caseload pressure" (Nagel and Schulhofer 1992, p.507). Nagel and Schulhofer (1992) offer (a now-dated) discussion of DOJ directives to U.S. Attorneys.

For years, however, scholars have questioned that assumption, generating an empirical literature interested in discovering what prosecutors are actually trying to accomplish when they exercise their charging discretion. Early attempts to understand the use of prosecutorial discretion focused on the effects of legitimate defendant and offense characteristics or resource constraints. Rabin (1972), for example, argues that prosecutors use their discretion to improve their conviction rate (p.1046), given various considerations (e.g., caseload, type of offense, special characteristics of the defendant, adequacy of the case, and equality of treatment).[12] The charging consequences of "extra legal factors," such as gender, race, and ethnicity, have also received attention (see O'Neill 2005, pp.8-18).

Recent empirical work has expanded this domain to include other possible prosecutorial goals, focusing most notably on career concerns. Boylan (2005) finds that the length of prison sentences, but not the conviction rate, is positively related to later career success for prosecutors. Boylan and Long (1999) present evidence indicating that when monitoring is poor (either very small or very large U.S. Attorneys' offices), federal prosecutors typically plea bargain less often. They interpret this as evidence that prosecutors, all else equal, seek trial experience to better their future careers.[13] Glaeser et. al (2000) conclude that federal prosecutors choose to pursue cases with more "career-enhancing" features, like a wealthy or well-known defendant or the use of a private defense attorney, who might be a useful contact in later jobs.

More directly relevant to this paper are studies of prosecutorial responses to public policy innovations. Bjerk (2005) examines state-level three-strikes laws implemented throughout the 1990s, and finds that "prosecutors become significantly more likely to lower a defendant's charge to a misdemeanor when conviction for an initial felony arrest charge would lead to sentencing under a three-strikes law" (p.591). He interprets this result as indicating that prosecutors are circumventing these laws "because of their own preferences and resource constraints" (p.623).[14] Nagel and Schulhofer (1992) examine the response of U.S. Attorneys

[12] Rabin's analysis was based on interviews with prosecutors, and so it is not surprising that prohibited considerations (career concerns, etc.) were not mentioned. Ramseyer and Rasmusen (2001) and O'Neill (2003) are more recent studies in a similar vein.

[13] Similarly, Boylan and Long (2005) show that U.S. Attorneys are more likely to leave their positions in districts with high private-sector salaries, and that, in those districts, prosecutors are more likely to take their cases to trial to gain experience.

[14] Bjerk hints that this prosecutorial resistance may have its source in the inflexibility and severity of these

to the implementation of the federal sentencing guidelines, and find that a majority of prosecutors did not circumvent the guidelines through charging decisions, but a "substantial minority" did—for example, by dismissing § 924 weapons charges to reach a plea bargain (pp.551-52)—and did so because they did not like the guidelines, empathized with the defendant, or were not appropriately trained or monitored (pp.556-57).

Kessler and Piehl (1998) ask directly what prosecutors seek to accomplish with their discretion by evaluating how prosecutors responded to California's Proposition 8, which imposed mandatory minimums on repeat-offenders of particular crimes.[15] Their data show that the imposition of mandatory minimums raised the sentence length not only for affected offenders, but also for offenders convicted of factually similar crimes. They interpret this "spillover effect" to mean that prosecutors use their discretion neither to realize social preferences nor to circumvent legal changes, but instead to maximize some more complicated set of (unknown) preferences.

Miethe (1987) investigates the exercise of prosecutorial discretion by testing for a hypothesized "hydraulic" effect after Minnesota's sentencing guidelines were put in place in 1980 (p.155). A hydraulic effect occurs when a mandated reduction of discretion in one part of the system generates greater use of discretion elsewhere, typically undermining the goals of the reform. Miethe finds that the implementation of sentencing guidelines (which limited judicial discretion at sentencing) did not lead to significant changes in prosecutorial charging or plea bargaining practices that could not be explained by case or offense attributes. He concludes that, if prosecutors have significant discretion, they choose not to exercise it to circumvent the reform, perhaps because a number of internal (norms) and external (relationships) constraints.

This paper contributes to this literature in at least three ways. First, it examines the prosecutorial response to a pro-defendant policy change, not a pro-prosecution or neutral policy change (e.g., typically sentencing reforms). Second, it studies the response to a *constitutional* and *procedural* reform. Procedural reforms may ultimately translate into

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laws, but his empirical work does not speak to the question of prosecutorial motivation.

[15]Kessler and Piehl hypothesize, first, that prosecutors may use their discretion to carry-out social preferences, in which case later legal changes just formalize those preferences, and, second, that prosecutors' preferences may differ from society's, and thus prosecutors may seek to undermine changes in criminal justice policy.

substantive changes in outcomes, but prosecutors may react to them differently. Further, given its constitutional basis, prosecutors may not view the legal change as coming from out-of-touch or politically driven policymakers. Third, the paper uses federal data to study how *federal* prosecutors use their discretion to respond to changes in their legal environment.[16]

In the next part, I describe the prosecutor's basic problem, and evaluate the possible responses to the procedural change studied below. As I show, the prosecutor is essentially a consumer with a budget constraint, and the Supreme Court decision can be interpreted as increasing the price for one of the prosecutor's "goods" (in this case, the ability to use sentencing facts to increase a defendant's sentence length). As an empirical matter, we know relatively little about a prosecutor's preferences, but by making reasonable assumptions about the structure of those preferences, it is possible to explore their content using the results presented in Section 2.5 below.

2.3 Prosecutorial Behavior and Testable Hypotheses

In this part, I develop three empirical hypotheses of prosecutorial decision making using a straightforward model of prosecutorial behavior. I assume that the prosecutor makes his charging and sentencing-related decisions to maximize his overall utility, given the cost and expected outcome (in sentence length) of each decision, and total available resources. There are two basic decisions a prosecutor must make when optimizing. First, the prosecutor must determine how to allocate a fixed set of resources to the prosecution of a single offender, where the offender's sentence is calculated using the sum of an offense-related base sentence and an additional sentence enhancement due to sentencing factors.[17] Second, the prosecutor must decide how to allocate resources *across* multiple criminal defendants.

By imposing reasonable assumptions, the structure of this problem can be analyzed using a two-stage budgeting approach (see Deaton and Muellbauer 1980, pp.122-27). I begin with the prosecutor's problem, which, for defendants $i = 1...n$, is:

$$Max\ U_p(x_o^1, x_s^1, x_o^2, x_s^2, \cdots, x_o^n, x_s^n) \quad s.t. \quad \Sigma_1^n\ x_o^i \cdot p_o^i + x_s^i \cdot p_s^i \leq M, \tag{2.1}$$

[16]Federal prosecutors and state prosecutors practice in very different environments, and none of the work focusing on prosecutorial responses to policy changes studies federal prosecutors.

[17]See Campbell and Bemporad (2003) for an explanation of these calculations.

where x_o^i is the sentence length attributable to base offenses, x_s^i is sentence length attributable to sentencing enhancements, p_j^i is the cost to the prosecutor of x_j^i, all for offender i, and M represents the prosecutor's total resources.

I assume that prosecutors care only about the total sentence length an individual offender receives, not its component parts.[18] Therefore, if prosecutors were constrained only by prices and total resources, x_o and x_s would be perfect substitutes, and a difference in price would generate a corner solution. Proving any particular set of sentencing facts, however, requires an offense on which to base those facts, so substitutability is, at a minimum, not perfect. Therefore, I take a prosecutor's preferences to be weakly separable:

$$\begin{aligned} &Max\ U_p\,(d_1(x_o^1, x_s^1), d_1(x_o^2, x_s^2), \cdots, d_n(x_o^n, x_s^n)) \\ &\qquad\qquad s.t. \quad \Sigma_1^n\ x_o^i \cdot p_o^i + x_s^i \cdot p_s^i \leq M, \end{aligned} \tag{2.2}$$

where U_p is increasing in $d_i(x_o^i, x_s^i)$, the total sentence for offender i.[19] Prosecutors clearly make tradeoffs across defendants, but they are complex, and I do not assume any particular structure for U_p (see, e.g., Kessler and Piehl 1998).

Two-stage budgeting is a natural approach to thinking about prosecutorial behavior. Prosecutors must prove different offense facts and different sentencing facts for each offender, but prosecutors clearly group these choices at the offender level. So, if it becomes more difficult to prove a particular fact for an offender, and, as a consequence, it becomes more difficult to raise the overall sentence for that offender, a prosecutor may change his allocation of resources across some or all offenders. For a particular initial allocation. it is possible to analyze the various consequence of a change in p_s for some subset of defendants.

For any particular offender i, and a given budget constraint m, a prosecutor will maximize $d(x_o, x_s)$. If sentence length attributable to base offenses and sentencing enhancements

[18]Boylan (2005) recently offered evidence that prosecutors seek to maximize sentence length rather than conviction certainty, and so I use that assumption here. This is a common assumption in the economics literature (see Landes 1971, Reingenaum 2000). An alternative assumption would be that prosecutors have a target sentence, but the analysis would then be transformed into the dual of this problem, with a prosecutor minimizing his expenditure to achieve a given sentence.

[19]For some values of x_o and x_s, $d(x_o, x_s)$ is presumably approximately $x_o + x_s$, but because there can be no enhancements without an appropriate base offense, d must equal 0 when $x_o = 0$. Additional otherwise possible combinations are similarly ruled out by the mechanics of the sentencing guidelines and criminal codes.

are both normal goods, then $\partial x_s^*(p_o, p_s, m)/\partial p_s < 0$, but the consequence for $x_o^*(p_o, p_s, m)$ is indeterminate, because the substitution and income effects work against each other. Thus, if the cost of proving sentencing enhancements increases, it is unclear whether a prosecutor will charge a defendant with more or less.

In allocating resources *across* defendants, an increase in the p_s for defendant i will increase the composite price per unit of sentencing length charged.[20] If sentence length is a normal good, the sentence length sought against defendant i will drop, while the change in sentence length sought against other defendants, for example, defendant j, is indeterminate. For example, if the price increase generates a strong substitution effect, the total sentence length charged against j will increase and, conditional on the shape of $d^*(p_o, p_s)$, both sentencing enhancements sought and base offense counts alleged should increase. If, on the other hand, the income effect dominates, the total sentence length charged against j will drop, typically both x_o^{j*} and x_s^{j*}, but not by as much as it drops for i.

Charged sentence length may be an inferior good, however. For example, prosecutors may find, for political or other reasons, that they cannot reduce the sentence length they charge for certain defendants. Alternatively, charged sentence length for defendants i and j may be complements. For instance, career or other prospects may be better advanced by having low variance in sentence outcomes across defendants. Two forty-year sentences may be preferable to a 100-year sentence and a one-year sentence, even though total sentence length would be higher with the latter option. Therefore, under some circumstances, a rise in p_s^i may generate a reduction in charged sentence length for j that exceeds the reduction in charged sentence length for i. These relationships generate three testable hypotheses:

Hypothesis #0. If the price of sentencing enhancements p_s increases for offender i, but prosecutors are not budget constrained or they are unable to raise or lower x_o^i or x_o^j, then charged sentence length for offender i should not change relative to charged sentence length for offender j.

Hypothesis #1. If the price of sentencing enhancements p_s increases for offender i and $d_i(x_o^i, x_s^i)$ and $d_j(x_o^j, x_s^j)$ are normal goods, then charged sentence length for offender i should drop relative to charged sentence length for offender j.

[20] Above I assumed that prosecutors only care about overall sentences, but because of the legal relationship between sentence length due to sentencing enhancements and sentence length due to base offenses, the two cannot be perfect substitutes. Accordingly, the composite price for prosecuting most offenders will rise.

Hypothesis #2. If the price of sentencing enhancements p_s increases for offender i and $d_i(x_o^i, x_s^i)$ is an inferior good or if $d_i(x_o^i, x_s^i)$ and $d_j(x_o^i, x_s^j)$ are complements, then charged sentence length for offender i should rise relative to charged sentence length for offender j.

The latter two hypotheses assume that prosecutors have the ability to respond to a change in price. If prosecutorial discretion is tightly circumscribed by law or by professional norms, and charging behavior is therefore disassociated from cost (Hypothesis #0), the above model may properly characterize prosecutorial incentives, while mischaracterizing the constraints under which prosecutors operate. If a price change generates no visible change in prosecutorial behavior, it may be because the reaction was small or difficult to detect with this data, or because prosecutors were unable to respond, despite their interest in doing so.

2.4 How Prosecutors Respond: Data and Empirical Strategy

My empirical work makes use of five different sources of data. The first three are individual-level arrest, charging, and sentencing data. The fourth and fifth are the sentencing guidelines framework and a 2000 United States Supreme Court case, *Apprendi v. New Jersey*. Combining these sources allows me to examine how prosecutors respond to a pro-defendant change in constitutional criminal procedure.

2.4.1 Individual-Level Federal Data

All individual-level data were obtained from the Federal Justice Statistics Resource Center ("FJSRC").[21] For arrest information, I rely on the U.S. Marshals Service's Prisoner Tracking System, which follows all offenders arrested for federal offenses and contains offender and offense-related variables. For charging information, I construct defendant-level data using charge-level records derived from the Executive Office for U.S. Attorney's Central Charge files. For sentencing information, I use data collected for the U.S. Sentencing Commission's monitoring database, which contains demographic and sentencing information for each offender sentenced pursuant to the Sentencing Reform Act of 1984, which is a large majority of federal offenders.

[21] See http://fjsrc.urban.org/.

The FJSRC makes available "linking data" that allows a researcher to follow a defendant from arrest through sentencing.[22] Using this linking file, I assembled a data set of defendant-level records that includes arrest, charging, and sentencing information.[23] As I explain below, the outcome variable I use—total number of counts—uses charging data and is constructed by summing the total number of counts across all charges brought against each defendant. The sentencing data report a criminal history score for each offender, which determines whether the *Apprendi* decision was more or less likely to affect that person. I use the arrest data to control for the possibility of offense manipulation on the part of prosecutors.

I examine those offenders who were sentenced between January 1998 and December 2002. *Apprendi* was decided at the end of June 2000 (and took effect immediately), which leaves me with 30 months of pre-*Apprendi* data and 30 months of post-*Apprendi* data. To eliminate anomalous cases, observations were excluded if the record indicated that the offender was arrested after he was sentenced;[24] if the offender had a missing total prison sentence (including a sentence of time served); if a non-prison type of sentence was imposed; if the offender was being resentenced; if the sentence was clearly incorrectly coded; if the defendant was sentenced to death; or if the offender was sentenced to community confinement.[25] My basic results are not sensitive to the loss of these observations.

I calculate the total number of counts brought against a defendant by adding up the number of counts for those charges (by indictment or information) filed against a defendant. I exclude observations where the record has an erroneous or missing charge date; where the

[22] As the FJSRC's website puts it, "[b]y joining the sequential record numbers from the linking file with the sequential record numbers in the appropriate [analysis files], it is possible to track the course of individual defendant-cases from arrest to prosecution, adjudication, sentencing, and corrections."

[23] I verified that the linking of the data sets succeeded by comparing fixed demographic variables (e.g., sex, race, and date of birth). This process indicated that some mismatches might have occurred, but FJSRC was kind enough to verify that these cases were correctly matched using the offender's name and other non-public data, and determined that these cases were miscoded. I exclude these cases. I also eliminate a small number of offenders (26) where the total number of counts was over 100, but including them does not significantly affect my results, as I show below. In addition, the data suffer from broken links, in which, for example, there was no arrest or charging record for someone who was sentenced. I drop all cases in which I did not have a full arrest, charging, and sentencing record. According to the FJSRC, this link failure is typically due to missing or different identifying information (name, court docket number, etc.). Also, many arrest and charging observations are lost because of normal attrition. Some arrestees are not ultimately charged or sentenced. Similarly, some charged defendants are acquitted.

[24] According to conversations with FJSRC, if an offender was rearrested, the new date could overwrite the original arrest date.

[25] This process removed a total of 16,876 cases (or 4,678 cases, 2,910 cases, 2,384 cases, 2,474 cases, 18 cases, 1 case, and 4,411 cases, respectively).

number of counts for the charge was recorded as 0, 99, or was missing; where the ultimate disposition was missing, because charges without dispositions often appear to be repeats of other charges; and where the charge was for a misdemeanor. Basic descriptive statistics for the five years of data (in six ten-month increments) are displayed in Table 1.

I use the "total number of counts" to gauge the prosecutorial response to the procedural change introduced by *Apprendi*. This measure has been used in other work studying prosecutorial behavior (see Wilmot and Spohn 2004), and it makes sense to focus on the number of counts when prosecutors have two basic tools with which to seek a particular sentence (basic offenses and sentencing facts) and one of those tools has been affected by a legal change. Wilmot and Spohn (2004) find that charging decisions (holding actual offenses constant) significantly influence final sentences: "decisions made by prosecutors at charging have an independent effect on sentence severity" (p.333).[26] Moreover, using "counts charged" is similar in approach to Schanzenbach and Tiller's (2005) study of judicial behavior. They model judges as having two "instruments" of discretion—factual determinations and legal departures, and find that when one of these becomes more difficult to use because of the expected response of the appeals court, judges substitute toward the other.

2.4.2 *Apprendi* and the Sentencing Guidelines

To measure the prosecutorial response to the expansion of constitutional jury trial rights for sentencing facts, I use *Apprendi* and the then-mandatory calculations required by the federal sentencing guidelines.[27] The *Apprendi* decision established that "[o]ther than the fact of a prior conviction, any fact that increases the penalty for a crime beyond the prescribed statutory maximum must be submitted to a jury, and proved beyond a reasonable doubt" (p.490).[28] The holding expressly omitted "facts of prior conviction" (criminal history),

[26]Charging behavior can also affect sentences through its influence on a judge's view of a case. Wilmot and Spohn (2004) show that "defendants charged with more than one count receive a smaller discount for substantial assistance departures than defendants charged with only one count" (p.336).

[27]Prescott (2006) found that the broader jury trial rights imposed by *Apprendi* had the net effect of reducing sentences, but the study was unable to determine whether or how prosecutors responded to the decision. Lower sentences could have emerged through a number of mechanisms, with prosecutorial charging decisions being just one possibility.

[28]See Prescott (2006) for a detailed explanation of the history of this case, and the jury trial rights debate that has surrounded it and the cases that have followed. *Apprendi* was preceded by *Jones v. United States*, 526 U.S. 277 (1999), but was nevertheless uniformly viewed as unexpected.

and because the sentencing guidelines use offense characteristics and an offender's criminal history to determine a sentence, an offender's pre-determined criminal history score can be used to identify the effects of the price increase imposed by the broader rights mandated by *Apprendi*.

To see this, notice that under *Apprendi*'s limited holding, the Sixth Amendment jury trial right only applies to sentencing facts (other than criminal history) that, if found, would cause a sentence to *exceed* the applicable statutory maximum. Combine with this the fact that under the federal sentencing guidelines, recidivist offenders will be, holding all else even, closer to the applicable statutory maximum than will offenders with little or no criminal history. Figure 1 reproduces the basic sentencing table, and illustrates how, for a given offense level, an offender with a higher history score will be closer to any applicable statutory maximum. Therefore, if prosecutors respond to the higher cost of proving sentencing facts, that response should be different for recidivist offenders (relative to new criminals) after *Apprendi*.[29]

2.5 Empirical Model and Results

Apprendi was an unexpected procedural shift that, in effect, raised the price of using sentencing facts to increase sentences for a particular pre-determined group of offenders. But it is unknown whether prosecutors reacted in their charging behavior, and, if so, how they reacted. By combining *Apprendi*'s holding, the guidelines' grid approach, and arrest, charging, and sentencing data, I am able to study the prosecutorial response to this pro-defendant change in procedure.

2.5.1 Regression Analysis

As I described above, *Apprendi* raised the price for prosecutors of sentencing facts *more* for defendants in high criminal history categories than for defendants in low criminal history

[29]This strategy would be problematic if *Apprendi* made it more or less difficult to prove criminal history facts. In that case, offenders might switch criminal history categories after *Apprendi*. But Prescott (2006) shows that neither the *Apprendi* decision nor the behavior of prosecutors or judges is likely to have altered the composition of criminal history groups. Moreover, my empirical approach controls for differences over time in observable demographic changes in composition.

categories. Therefore, I begin by looking at basic changes in charging behavior across different history groups. For each criminal history group, I estimate the following equation by OLS:

$$\begin{aligned} Total\ Counts_{ijt} = \ & \alpha + \beta_1 Race_{ijt} + \beta_2 Education_{ijt} + \beta_3 Age_{ijt} + \beta_4 Sex_{ijt} \\ & + \beta_5 NumDep_{ijt} + \beta_6 GuidelinesYear_{ijt} + \beta_7 Citizen_{ijt} \\ & + \beta_8 Apprendi_t + \beta_9 L_t + \beta_{10} L_t^2 + \epsilon_{ijt}, \end{aligned} \quad (2.3)$$

where i indexes offenders, j indexes districts, and t indexes the month the offender was first charged.[30] The dependant variable is total number of counts lodged against an offender across all charges filed.

To account for the possibility of disparate charging on the basis of a defendant's personal characteristics, I include a number of offender demographic variables: *Race* is a vector that includes three dummy variables (for black, white, and Hispanic, with other omitted); *Education* includes five dummies for different levels of attainment (high school, vocational or military training, some college, college degree, and some graduate training, with no high school diploma omitted); *Age* includes dummies for different age groups (18-29, 30-44, 45-60, and greater than 60, with less than 18 omitted); *Sex* is one if the offender is a male; and *NumDep* is the number of dependants. Because immigration crimes account for a significant portion of the federal criminal docket, *Citizen* contains a dummy for whether the offender is a U.S. citizen and a dummy for whether the offender is a U.S. resident. I also include some simple trend controls and *GuidelinesYear*, which contains dummy variables indicating the Guidelines Manual under which the defendant was sentenced, to control for any amendments or other changes in the manuals introduced over time.

The variable *Apprendi* is a dummy variable equal to zero if the defendant was first charged before or in June 2000 and equal to one thereafter. Because equation (2.3) controls for simple time trends, the coefficient estimated on *Apprendi* provides a first-pass measure of the prosecutorial reaction to the Supreme Court's decision. As explained in Section 2.4, if *Apprendi* had *any* effect on charging behavior, the change should be more evident for higher as opposed to lower criminal history groups. We would expect, therefore, that if prosecutors responded to *Apprendi* by altering their charging decisions, estimates of β_8

[30] Plausibly, prosecutorial manipulation may include filing additional charges at a later time, but most offenders have all their charges filed in the same month.

would be positive or negative for high criminal history types, but close to zero for low criminal history types.

For this reason, the results of estimating equation (2.3), shown in Table 2, are puzzling. Although none of the estimated coefficients on *Apprendi* is statistically significant, the magnitudes of the estimates (calculated separately for various criminal history category groupings) suggest that the number of counts for low criminal history types *increases* post-*Apprendi*, while the number of counts for high criminal history types remains essentially constant or falls only slightly.[31] These numbers are consistent with Figure 2, which graphs the average total number of counts by month for high and low criminal history types. Around the time of *Apprendi* (or a few months before, a possibility I address below), the difference in the number of counts between low and high criminal history types begins to increase, with the number of counts for high criminal history offenders remaining constant or dropping slowly and the number for those with low criminal history scores increasing (or increasing at a faster relative rate) post-*Apprendi*. Furthermore, the aggregate total number of counts rises after *Apprendi*, at least until the last few months of the sample.[32]

This pattern seems a bit surprising because *Apprendi* raised a prosecutor's costs for a particular group of offenders—and yet the prosecutorial demand for counts appears not to have changed for those affected and to have increased for those who were not affected. The model in Section 2.3, however, offers a plausible explanation. Loosely, if prosecutors view sentences for different defendants as substitutes, and sentencing factors and base-offense counts for a given defendant as weak complements, then the aggregate total number of counts should rise (if the cross-defendant substitution effect outweighs the cross-defendant income effect) and the number of counts for non-affected defendants should rise relative to those affected by *Apprendi*.[33]

[31] This pattern is clear in the top two panels (which break down criminal history into two or three groupings). A similar pattern is also evident in the bottom panel, but in general the estimated coefficients on *Apprendi* across the six criminal history groupings vary a lot in sign.

[32] The pattern during the last few months of the sample raises some question about the long-term affect of *Apprendi* or may indicate some anticipation of a broadening of the *Apprendi* rule to all criminal history groups, which occurred in June 2004 with the decision in the *Blakely v. Washington* case, not long after my sample ends. Alternatively, the change may be due entirely to selection effects. In order to be included in my sample, the offender must have been sentenced by the end of fiscal year 2003, 10 months after my sample ends. More complicated or otherwise different cases may take longer than a year to proceed to sentencing, thus potentially skewing the results at the end of the sample period.

[33] Another possible explanation is that prosecutors faced a looser budget constraint over time. More resources, combined with shifting prices, would also generate the pattern in Figure 2.

For instance, as sentencing facts became more expensive for high criminal history types post-*Apprendi*, prosecutors might have found it optimal to move resources from sentencing facts for high criminal history types to base-offenses for low criminal history types. This reallocation would generate the pattern of higher aggregate count levels and of increasing disparity in count totals between high- and low-history offenders visible in Table 2 and Figure 2.[34] Thus, although initially appearing difficult to explain, the fact that the post-*Apprendi* change in charging behavior appears conspicuously present for those less affected by *Apprendi* (and absent for those more likely to be covered by the decision) is entirely compatible with a model in which optimizing prosecutors can substitute resources across defendants.

Figure 2 also suggests a different concern. As mentioned above, around the time of *Apprendi*, one could argue that the number of counts for high criminal history types does not appear to change or, at most, drops slowly, and looks to remain essentially on a zero growth path. The number of counts for low criminal history types, however, appears to have been on a different path—pre-*Apprendi*, the number of counts may have been increasing, but after *Apprendi*, the rate of growth seems to slow or even stop. Thus, if the trends were removed, Figure 2 hints that *Apprendi* might have led to a relative *increase* in the number of counts for high criminal history offenders (relative to the counterfactual, in which the low criminal history counts would have continued to grow). As a consequence, Table 2 and the results below control for linear and quadratic time trends (as well as many other independent variables that may generate the trends of the two groups visible in Figure 2), and I do not find evidence supporting this interpretation. Nevertheless, some caution is warranted in interpreting the results presented above and below.

As is clear by now, one of the most prominent features of Table 2 and Figure 2 is the increasing difference, starting around the time of *Apprendi*, in the total number of counts between high and low criminal history types. This development suggests that prosecutors may have responded to *Apprendi* by putting relatively less effort into counts against high

[34]In sum, because only charged offense counts exist in the data, and not the combination of offenses and enhancements, a rise in counts following an enhancement cost increase is fully consistent with prosecutorial maximization as described in Section 2.3. Ideally, I would like to study the number of enhancements charged, since the one clear empirical prediction of Section 2.3 is that the number of enhancements sought for high criminal history types should drop post-*Apprendi*. Unfortunately, these data do not exist and, even if they did, they would be difficult to quantify. Furthermore, sentencing facts are in theory "determined" post-conviction by a judge with the help of a probation officer, and are not charged by a prosecutor.

criminal history offenders than into those against low criminal history offenders. To explore this possibility, I use a difference-in-differences approach. One important problem with taking this route, as I explain below, is that *Apprendi* did not apply to clearly defined treatment and control groups prosecuted by separate attorneys under independent budget constraints. Thus, what I use as my control group, may also have been treated, meaning that the model below may not be identified. With that caveat, I estimate the following equation:

$$\begin{aligned} Total\ Counts_{ijt} = \ & \alpha + \beta_1 Race_{ijt} + \beta_2 Education_{ijt} + \beta_3 Age_{ijt} + \beta_4 Sex_{ijt} \\ & + \beta_5 NumDep_{ijt} + \beta_6 Guidelines Year_{ijt} + \beta_7 Citizen_{ijt} \\ & + \beta_8 Apprendi_t + \beta_9 L_t + \beta_{10} L_t^2 + \beta_{11} CrimHist_{ijt} \\ & + \beta_{12} Apprendi_t \times CrimHist_{ijt} + \epsilon_{ijt}, \end{aligned} \tag{2.4}$$

where the variables previously defined for equation (2.3) are identical to those above. $CrimHist$ is comprised of two (high/low), three (high/mid/low), or six (I-VI) dummy variables, and captures the direct effect of criminal history on the total number of counts filed against an offender.

The interactions *Apprendi* $\times$ $CrimHist$ in equation (2.4) represent the prosecutorial response to *Apprendi* in terms of additional counts charged, and should be interpreted as the *differential* prosecutorial reaction to the Supreme Court's pro-defendant procedural innovation. The magnitude of the calculated response is a lower bound estimate, because in this experiment pure "control" and "treatment" groups do not exist.[35] Moreover, the OLS coefficients on *Apprendi* $\times$ $CrimHist$ must be scaled by the difference between criminal history groups in the proportion of cases where *Apprendi* matters, something which I do not observe.[36]

The first column in Table 3 presents my baseline results of estimating equation (2.4) using only two criminal history categories. The data indicate that prosecutors *did* respond to the increase in the price of proving sentencing facts, and that they responded by substituting away from (charging fewer counts against) those protected by the procedural innovation.

[35] *Apprendi* was not directly relevant to recidivists who were not close to the statutory maximum, but it did affect new criminals who had many aggravating offense facts. In addition, *Apprendi* did not apply to all aggravating sentencing facts. Such facts were decided by a judge by a preponderance of evidence even post-*Apprendi* unless the fact put the defendant's sentence at risk of exceeding the statutory maximum.

[36] If it were possible, I would use the probability that *Apprendi* binds as my independent variable, and instrument for it with criminal history categories.

The data show that those most affected by *Apprendi* were charged with between 0.27 and 0.31 fewer counts after the decision, relative to less affected offenders. The mean number of counts during most of the sample period is between between 2 and 3 counts per offender, implying that prosecutors reduced the counts charged against those protected by *Apprendi* by approximately 10 percent.

Overall, these results are consistent with Hypothesis #1, and provide some evidence against Hypotheses #0 and #2. Prosecutors appear to have substituted their resources toward non-affected offenders. This may be because base-offense charges and sentencing facts are weak complements under the relevant circumstances, or because prosecutors view sentences across defendants as strong substitutes, or both. This interpretation accords with Prescott's (2006) finding *Apprendi* lowered sentences for high criminal history offenders relative to those with low criminal history scores. Although the decline in sentence length shown in that paper may be driven by many distinct *Apprendi*-related mechanisms, these results raise the possibility that prosecutorial charging behavior lies behind some of the decline.

These numbers also have potentially important implications for our understanding of prosecutorial goals and behavior. The estimates imply that after *Apprendi*, unlike after the three-strikes laws studied by Bjerk (2005), prosecutors *did not* behave in ways that effectively undermined the policy change. Instead, their actions potentially *magnified* the consequences for offenders.[37] The differences between the policy innovations studied may explain the different conclusions. Bjerk examined the response of state-level prosecutors to a change in substantive criminal law that many prosecutors found objectionable on multiple grounds. *Apprendi* was put in "rights" terms and decided by the U.S. Supreme Court on a constitutional basis.

In the remainder of Table 3, I control for possible previously omitted variables. In the second column, I include *Arrest*, which is a vector of arresting-agency recorded offense dummies, to reduce the possibility that prosecutors responded by *changing* the type of crime charged rather than the number of counts. In the third and fourth columns, even

[37]The results are also consistent with the claim made occasionally in the legal literature that prosecutors dislike the severity of federal sentences, but are at least partially constrained in exercising their discretion for a defendant's benefit (see Bowman and Heise 2001, 2002). On this story, *Apprendi* might have provided prosecutors with cover for not seeking as many counts against affected defendants.

though *Apprendi* applied uniformly to all districts, I add district effects and month effects to ensure that my estimates are not driven by fixed differences in offenders or prosecutorial tactics. For example, if over time different districts prosecuted offenders with very different criminal history scores on average, and districts used different prosecutorial approaches, then conceivably the data might generate the results I list in the first column. The new estimates remain roughly the same, with offenders affected by *Apprendi* receiving 10% fewer charges on average.

In Tables 4 and 5, I disaggregate the two criminal history groups used in Table 3 into three and six criminal history groups to verify that how the offenders are combined for analysis does not explain my results. Table 4 reports the estimates using three history groups (low, mid, and high). If *Apprendi* binds more for offenders in higher criminal history groups and is the source of changes in charging behavior, then the prosecutorial response should be greater for groups with higher history scores. The estimated coefficients correspond exactly to this prediction. In every column, the estimate for high-level criminal history types exceeds in magnitude the estimate for mid-level criminal history types, and both are substantially larger than the estimate for those offenders with low-level history scores.[38] The same is true in Table 5, where the estimated coefficients on the six groups grow monotonically with the history score, consistent with the identification strategy and with Hypothesis #1.

2.5.2 Robustness Checks

Tables 3 through 5 suggest that prosecutors responded to *Apprendi*'s pro-defendant extension of jury trial rights to sentencing facts by *reducing* the relative number of base-offense counts charged against those defendants who were formally affected by the decision. But to increase the confidence in the conclusion that it was *Apprendi* behind these results rather than some other contemporaneous change in the criminal justice system not already controlled for, I have re-estimated equation (2.4) making a number of changes to the underlying sample and specification. My results are broadly robust to these tests. I also check to confirm that an *Apprendi*-related change in conviction rates is not at the root of my basic findings.

[38]The difference between the high- and mid-level estimates is statistically significant at the 1% level in the fourth column of Table 4.

In Table 6, I include 26 additional observations with counts of over 100, which I omitted in earlier regressions. These are extreme outliers, with some observations reporting many hundreds of counts. Nevertheless, Bollinger and Chandra (2005) have shown that omitting data in such circumstances can have perverse consequences. Given the small number observations, it is not surprising that adding these observations does not much affect my results. If anything, the magnitudes of my estimates are somewhat larger with the outliers than without them.

In Table 7, I add additional years of charging data to my analysis. My baseline sample includes defendants who were charged between January 1998 and December 2002. Charging information exists for 1997 and 1996, but these data do not appear to be reported or coded in the same way as in later years, and so they were excluded. Data also exists for at least ten months after December 2002. I omitted these observations because, to be included in my sample, an offender has to be both charged *and sentenced* before October 2003. Therefore, if I were to incorporate offenders charged in October 2003, for instance, I would only capture those charged and sentenced in that month. These cases are on average unrepresentative, with high plea bargain rates and presumably more straightforward or less serious issues. Nevertheless, if I include all of these observations, my estimates drop in magnitude by at most 20%, and remain highly statistically significant.

In Table 8, I present estimates from a log-linear version of equation (2.4), in which the log of total counts replaces total counts, in case the relationship between total counts and the independent variables of interest is nonlinear. The estimates are consistent in direction and statistical significance with those in Table 3, and suggest a percentage change of comparable magnitude.

In Table 9, I begin to explore the timing of the *Apprendi* decision. Although the rule announced in *Apprendi* was unexpected, prosecutors may have altered (or postponed) their charging decisions as soon as the Supreme Court granted certiorari in the case (November 29, 1999). Furthermore, in the immediate aftermath of the decision, it may have taken prosecutors some time to understand the holding and deal with its administrative consequences. Thus, because the months immediately surrounding the decision may have been unrepresentative, I re-ran equation (2.4) on the original sample *minus* the six months before and after the decision. The estimated coefficients are reported in Table 9, and show that, if anything, anticipation and a slow response may have masked some of the effect attributable to *Apprendi*.

This interpretation is borne out by Figure 2 and Figure 3, which show the average total number of counts by criminal history group by month and the total number of offenders charged by criminal history group by month, respectively. In Figure 2, a few months *before* *Apprendi* is decided, but after certiorari was granted, the difference in average total counts between high and low history offenders begins to grow. This may imply that a cause other than *Apprendi* led to the changes in charging behavior reported in Table 3, but it may also indicate that prosecutors were watching and perhaps anticipating the *Apprendi* decision.[39] Figure 3 provides some evidence that prosecutors may have postponed charging decisions until the outcome in (and implications of) *Apprendi* became clear, and that prosecutors were occupied with the case's consequences for a short period after the opinion was released. In the months immediately surrounding *Apprendi*'s announcement, the number of offenders charged per month drops substantially. This reduction begins prior to the decision and continues until months afterward. Although there is clear seasonality to charging numbers throughout the duration of the sample, the decline in the number of charged offenders is most noticeable during the *Apprendi* period.

To further examine the possibility and consequences of anticipation effects, Table 10 shows estimates from an analogue to equation (2.4) where I have substituted three-month period dummies (for one year pre- and two years post-*Apprendi*) and interaction of those dummies with $CrimHist$ for the variables *Apprendi* and *Apprendi*$\times CrimHist$. The results seem to show some *Apprendi*-like effect beginning three to six months before the decision is released. Thus, some caution should be taken in interpreting the causal relationship between *Apprendi* and the relative decline in counts for those more likely to be affected by *Apprendi*—an unrelated trend or some other policy change may be producing the effects I have attributed to *Apprendi*. But it also suggests, along with Figures 2 and 3, that prosecutors may have anticipated the decision after certiorari was granted about 7 months before the decision. In fact, this interpretation is bolstered by the fact that the *Apprendi* effect becomes much stronger after the decision is handed down.

[39]The pattern is particularly noticeable beginning three or four months before *Apprendi* was decided in June 2000. The case was argued in March 2000, and questions posed by the Justices may have signaled to Court watchers that the holding in *Apprendi* was a possible outcome.

Another potential problem with my empirical approach is that my strategy, by design, requires a criminal history score, which is only calculated at sentencing, to identify the effect of *Apprendi*. As a consequence, I am forced to omit from my analysis defendants who are charged, but who are not ultimately convicted (or whose outcomes at the charging and sentencing stage cannot be linked). Therefore, if *Apprendi* had the effect of changing the rate of conviction for defendants, then any skewing in the distribution of offenders could confound my empirical results. For example, equation (2.4) would generate my findings with no change in charging decisions if *Apprendi* caused high criminal history defendants with many counts to be acquitted at a higher rate than low criminal history defendants with many counts.

Nothing in the legal literature suggests that this might have happened, and scenarios in which a higher standard of proof for a sentencing fact might cause a jury to acquit on all charges or cause a prosecutor to dismiss all of his base offense counts are unlikely.[40] More importantly, the data do not provide evidence favoring a selection explanation. Using individual-level arrest data, I have calculated a "predicted" distribution of offender criminal history scores for defendants *charged* post-*Apprendi* that can be compared to the actual distribution of criminal history scores of those *sentenced* post-*Apprendi*. To predict the likely criminal history scores of charged defendants, I ran a multinomial logit model on pre-*Apprendi* arrest data using an offender's criminal history category as the dependent variable and a host of offender arrest characteristics (e.g., gender, race, citizenship, marital status, age, offense, arresting agency, and district of arrest). I then used the estimated coefficients and post-*Apprendi* data to predict criminal history scores for each offender who was charged, but not necessarily convicted.

Table 11 presents the results of this comparison. For each criminal history group, I show the difference between the actual percentage of offenders and the predicted percentage of offenders receiving that criminal history score (first column) for each of six five-month periods (covering the entire 30-month post-*Apprendi* period). Although the distributions do differ slightly, they are very close overall.[41] For criminal history category six, the last two

[40]Furthermore, Figure 3, which plots the number of offenders in the high and low criminal history groups, does not show a disproportionate change in the relative number of high criminal history types post-*Apprendi*.

[41]This illustrative comparison does not rule out the possibility that different offenders were convicted. Even if the same number of offenders in a given history category post-*Apprendi* are convicted, it is possible,

periods indicate that there are fewer offenders than there "should" be, but criminal history category four, also a high score, has in theory too *many* offenders in the second-to-last period.[42] Furthermore, although the results are not statistically significant, there seems to be too few offenders in the first criminal history category, meaning that there may be too few offenders in both very high and very low criminal history categories. The conclusion to be drawn from Table 11 is that, although there are some significant differences between the actual and predicted criminal history distributions, there is no strong evidence that changes in rates of conviction might be biasing my results.

Together, these robustness checks indicate that my basic finding—showing that prosecutors reacted to the price increase imposed by *Apprendi* by reducing the relative number of counts against those affected—appears real and substantial.

2.6 Conclusion

When writing about the significance of research into prosecutorial discretion in a 1972 article, Robert Rabin said, "A prerequisite to any system of effective controls is an understanding of the behavior to be constrained" (p.1036). This paper studies the charging behavior of prosecutors, and attempts to identify the response of these public agents to an innovation in criminal procedure that had the practical effect of raising the price prosecutors had to pay to prove sentencing facts. I begin with a simple model of prosecutorial behavior in which prosecutors allocate their limited budgets across multiple defendants and, for each defendant, across two types of facts. The model generates straightforward empirical hypotheses that allow inferences about prosecutorial preferences to be drawn from the empirical response prosecutors make to *Apprendi v. New Jersey*'s sentencing fact price increase.

To calculate this response, I use federal arrest, charging, and sentencing data, the limited holding of the *Apprendi*, and the structure of the U.S. Sentencing Guidelines to create

although probably unlikely, that a different high criminal history defendant was convicted after *Apprendi* than would otherwise have been convicted.

[42]To ensure that these last two periods are not behind my basic results, I recalculated the results presented in Table 3 using a sample that omitted the last ten months of post-*Apprendi* data. The results did not change appreciably.

comparison groups of offenders that were differentially affected by the Supreme Court's ruling. Because in theory *Apprendi*'s ruling may have affected all federal defendants, I am not able to identify treatment and control groups precisely. But with that caveat, I find some evidence that prosecutors reduced the *relative* number of counts they charged against defendants more likely to be affected by *Apprendi* by as much as 10 percent. These results, when viewed through the model's hypotheses, imply that prosecutors consider criminal defendants to be substitutes for one another, and that sentencing facts and offense facts may be weak complements.

If the results in this paper are accurate, this reduction in counts has clear practical significance for offenders. Because the total number of counts charged against a defendant, at least in my data, is positively correlated with the sentence that a defendant ultimately receives, defendants do better on average if they are charged with fewer crimes. Furthermore, Prescott (2006) found that the *Apprendi* ruling led to a substantial reduction (of approximately six months on average or around 5% of the total average sentence) in sentence length for those most likely to be covered by the decision. This study complements that paper by suggesting that at least one of the ways that sentences may have been reduced is by prosecutors reducing the relative number of counts for affected individuals.[43]

For policymakers, these results impart important lessons. Prosecutors appear willing to exercise their substantial charging discretion in ways that are not obviously beneficial to the public. In *Apprendi*'s case, the charging choices of prosecutors may have unwittingly magnified the effect of the Supreme Court's decision on affected defendants. The results of this paper also point to the fact that, even though *Apprendi*'s broadening of Sixth Amendment jury trial rights did not formally apply to certain defendants. cross-defendant substitution of counts by prosecutors may nevertheless have made the decision relevant to all federal defendants charged with crimes. The possibility of a similar effect should be considered whenever a policy change thought to affect a limited number of defendants may actually influence a central component of a prosecutor's basic resource allocation problem.

[43]More precisely, the reduction in sentences for high criminal history types may be at least partially attributable to the fact that their absolute number of counts did not increase in response to *Apprendi* and sentencing facts were less likely to be found because they become more expensive to prove.

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Table 1: Descriptive Statistics—All Criminal History Levels

	-30 to -20 M	-20 to -10 M	-10 to 0 M	0 to 10 M	10 to 20 M	20 to 30 M
Age	33.432	33.320	33.348	33.270	33.294	33.461
	(0.087)	(0.079)	(0.077)	(0.081)	(0.078)	(0.068)
White (Proportion)	0.713	0.733	0.747	0.703	0.729	0.752
	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Male (Proportion)	0.845	0.853	0.861	0.855	0.843	0.856
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Jury Trial (Proportion)	0.040	0.031	0.023	0.020	0.019	0.018
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
U.S. Citizen (Proportion)	0.655	0.604	0.597	0.662	0.633	0.614
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
High School (Proportion)	0.194	0.179	0.177	0.189	0.183	0.176
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
College (Proportion)	0.039	0.034	0.040	0.037	0.039	0.035
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Total Number of Counts	2.368	2.581	2.631	2.803	2.659	2.586
	(0.026)	(0.029)	(0.032)	(0.032)	(0.032)	(0.027)
Number of Charges	1.720	1.729	1.678	1.807	1.750	1.712
	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.008)
Obs.	14,555	17,417	18,359	16,702	17,617	22,947

Notes: Means are presented for all five years (60 months) of the base-line sample, which runs from January 1998 to December 2002. *Apprendi v. New Jersey* was decided in June 2000. Data are from the Executive Office of U.S. Attorneys and the U.S. Sentencing Commission. Standard errors are reported in parentheses below estimates.

Figure 1: 2004 Sentencing Guidelines Grid

	Offense Level	Criminal History Category (Criminal History Points) I (0 or 1)	II (2 or 3)	III (4, 5, 6)	IV (7, 8, 9)	V (10, 11, 12)	VI (13 or more)
	1	0-6	0-6	0-6	0-6	0-6	0-6
	2	0-6	0-6	0-6	0-6	0-6	1-7
	3	0-6	0-6	0-6	0-6	2-8	3-9
	4	0-6	0-6	0-6	2-8	4-10	6-12
	5	0-6	0-6	1-7	4-10	6-12	9-15
	6	0-6	1-7	2-8	6-12	9-15	12-18
	7	0-6	2-8	4-10	8-14	12-18	15-21
Zone A	8	0-6	4-10	6-12	10-16	15-21	18-24
	9	4-10	6-12	8-14	12-18	18-24	21-27
Zone B	10	6-12	8-14	10-16	15-21	21-27	24-30
	11	8-14	10-16	12-18	18-24	24-30	27-33
Zone C	12	10-16	12-18	15-21	21-27	27-33	30-37
	13	12-18	15-21	18-24	24-30	30-37	33-41
	14	15-21	18-24	21-27	27-33	33-41	37-46
	15	18-24	21-27	24-30	30-37	37-46	41-51
	16	21-27	24-30	27-33	33-41	41-51	46-57
	17	24-30	27-33	30-37	37-46	46-57	51-63
	18	27-33	30-37	33-41	41-51	51-63	57-71
	19	30-37	33-41	37-46	46-57	57-71	63-78
	20	33-41	37-46	41-51	51-63	63-78	70-87
	21	37-46	41-51	46-57	57-71	70-87	77-96
	22	41-51	46-57	51-63	63-78	77-96	84-105
	23	46-57	51-63	57-71	70-87	84-105	92-115
	24	51-63	57-71	63-78	77-96	92-115	100-125
	25	57-71	63-78	70-87	84-105	100-125	110-137
	26	63-78	70-87	78-97	92-115	110-137	120-150
	27	70-87	78-97	87-108	100-125	120-150	130-162
	28	78-97	87-108	97-121	110-137	130-162	140-175
	29	87-108	97-121	108-135	121-151	140-175	151-188
	30	97-121	108-135	121-151	135-168	151-188	168-210
	31	108-135	121-151	135-168	151-188	168-210	188-235
	32	121-151	135-168	151-188	168-210	188-235	210-262
	33	135-168	151-188	168-210	188-235	210-262	235-293
	34	151-188	168-210	188-235	210-262	235-293	262-327
	35	168-210	188-235	210-262	235-293	262-327	292-365
	36	188-235	210-262	235-293	262-327	292-365	324-405
	37	210-262	235-293	262-327	292-365	324-405	360-life
	38	235-293	262-327	292-365	324-405	360-life	360-life
	39	262-327	292-365	324-405	360-life	360-life	360-life
	40	292-365	324-405	360-life	360-life	360-life	360-life
	41	324-405	360-life	360-life	360-life	360-life	360-life
Zone D	42	360-life	360-life	360-life	360-life	360-life	360-life
	43	life	life	life	life	life	life

Application Notes:

1.The Offense Level (1-43) forms the vertical axis of the Sentencing Table. The Criminal History Category (I-VI) forms the horizontal axis of the Table. The intersection of the Offense Level and Criminal History Category displays the Guideline Range in months of imprisonment. "Life" means life imprisonment. For example, the guideline range applicable to a defendant with an Offense Level of 15 and a Criminal History Category of III is 24-30 months of imprisonment.

2. In rare cases, a total offense level of less than 1 or more than 43 may result from application of the guidelines. A total offense level of less than 1 is to be treated as an offense level of 1. An offense level of more than 43 is to be treated as an offense level of 43.

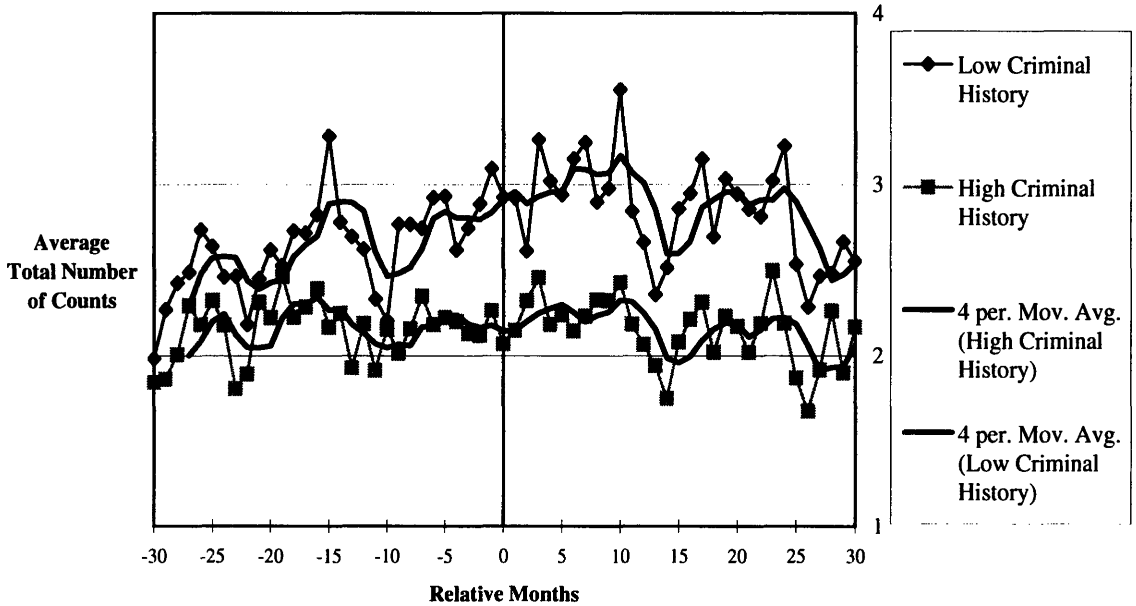
3. The Criminal History Category is determined by the total criminal history points from Chapter Four, Part A, except as provided in §§4B1.1 (Career Offender) and 4B1.4 (Armed Career Criminal). The total criminal history points associated with each Criminal History Category are shown under each Criminal History Category in the Sentencing Table.

Table 2: Difference Regression Results

	(1)	(2)	(3)	Obs
Apprendi (High: 4, 5, 6)	0.063 (0.138) [0.139]	0.041 (0.134) [0.112]	-0.025 (0.134) [0.116]	24,142
Apprendi (Low: 1, 2, 3)	0.215 (0.138) [0.252]	0.195 (0.138) [0.226]	0.148 (0.137) [0.216]	77,692
Apprendi (High: 5, 6)	-0.022 (0.179) [0.157]	-0.011 (0.173) [0.139]	-0.087 (0.172) [0.135]	14,913
Apprendi (Mid: 3, 4)	0.021 (0.162) [0.178]	-0.044 (0.158) [0.147]	-0.094 (0.158) [0.152]	25,065
Apprendi (Low: 1, 2)	0.285 (0.162) [0.278]	0.274 (0.161) [0.257]	0.235 (0.161) [0.245]	61,856
Apprendi (Category 6)	0.107 (0.242) [0.248]	0.150 (0.235) [0.229]	0.110 (0.232) [0.233]	9,386
Apprendi (Category 5)	-0.257 (0.248) [0.258]	-0.259 (0.241) [0.261]	-0.313 (0.247) [0.261]	5,527
Apprendi (Category 4)	0.186 (0.208) [0.204]	0.109 (0.205) [0.176]	0.050 (0.211) [0.179]	9,229
Apprendi (Category 3)	-0.085 (0.225) [0.239]	-0.145 (0.217) [0.195]	-0.210 (0.218) [0.204]	15,836
Apprendi (Category 2)	0.157 (0.395) [0.589]	0.112 (0.39) [0.568]	0.007 (0.386) [0.548]	11,262
Apprendi (Category 1)	0.304 (0.178) [0.278]	0.308 (0.178) [0.264]	0.283 (0.177) [0.252]	50,594
Additional Controls				
Crime of Arrest Effects		X	X	
District Sentenced Effects			X	

Notes: The dependent variable is total number of counts. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* equals one for all charges filed after *Apprendi v. New Jersey* was decided. The symbol * represents statistical significance at 5%, ** represents significance at 1%.

Figure 2: Average Total Counts By History Group



Notes: Figure plots running total number of counts by criminal history group for five years of the sample (month of first charge). The low criminal history group consists of offenders in categories I, II, and III; the high-level group consists of offenders in categories IV, V, and VI. Month "0" is the first month during which *Apprendi* applied.

Table 3: Basic Regression Results—High and Low Criminal History

	(1)	(2)	(3)	(4)
Apprendi	0.234	0.228	0.174	
	(0.115)*	(0.114)*	(0.113)	—
	[0.207]	[0.189]	[0.181]	
Apprendi × *CrimHist*	-0.271	-0.309	-0.294	-0.298
	(0.049)**	(0.048)**	(0.048)**	(0.048)**
	[0.071]**	[0.068]**	[0.068]**	[0.045]**
CrimHist	-0.261	0.119	0.094	0.092
	(0.037)**	(0.038)**	(0.038)*	(0.038)*
	[0.054]**	[0.051]*	[0.051]	[0.042]*
High School Graduate	0.120	0.010	-0.023	-0.023
	(0.035)**	(0.035)	(0.035)	(0.035)
	[0.037]**	[0.037]	[0.036]	[0.035]
College Graduate	0.695	0.239	0.205	0.206
	(0.111)**	(0.111)*	(0.111)	(0.111)
	[0.119]**	[0.120]*	[0.120]	[0.120]
Aged 18-29 years	0.026	0.112	-0.111	-0.181
	(0.498)	(0.598)	(0.665)	(0.678)
	[0.506]	[0.598]	[0.669]	[0.688]
Black	0.445	0.303	0.079	0.089
	(0.095)**	(0.111)**	(0.133)	(0.133)
	[0.135]**	[0.156]	[0.169]	[0.168]
Male	-0.263	-0.068	-0.022	-0.023
	(0.045)**	(0.046)	(0.046)	(0.046)
	[0.055]**	[0.055]	[0.053]	[0.052]
Additional Controls				
Crime of Arrest Effects		X	X	X
District Sentenced Effects			X	X
Month of Charging Effects				X
No. of Obs.	101,834	101,834	101,834	101,834

Notes: The dependent variable is total number of counts. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%, ** represents significance at 1%. In column (4), the *Apprendi* main effect is absorbed by the month effects but *Apprendi* ×*CrimHist* is identified.

Table 4: High, Mid, and Low Criminal History

	(1)	(2)	(3)	(4)
Apprendi	0.287	0.276	0.216	
	(0.117)*	(0.116)*	(0.116)	—
	[0.212]	[0.193]	[0.185]	
Apprendi ×*CrimHistMID*	-0.262	-0.260	-0.230	-0.227
	(0.054)**	(0.054)**	(0.053)**	(0.053)**
	[0.063]**	[0.060]**	[0.060]**	[0.056]**
Apprendi ×*CrimHistHIGH*	-0.368	-0.399	-0.384	-0.388
	(0.062)**	(0.061)**	(0.060)**	(0.060)**
	[0.081]**	[0.079]**	[0.079]**	[0.056]**
CrimHistMID	-0.279	0.107	0.085	0.080
	(0.041)**	(0.042)*	(0.042)*	(0.042)
	[0.047]**	[0.046]*	[0.047]	[0.046]
CrimHistHIGH	-0.269	0.193	0.168	0.167
	(0.047)**	(0.049)**	(0.049)**	(0.049)**
	[0.059]**	[0.058]**	[0.059]**	[0.051]**
Aged 18-29 years	0.043	0.109	-0.114	-0.184
	(0.530)	(0.590)	(0.663)	(0.675)
	[0.539]	[0.590]	[0.667]	[0.686]
Black	0.474	0.300	0.075	0.085
	(0.095)**	(0.112)**	(0.133)	(0.133)
	[0.135]**	[0.157]	[0.169]	[0.169]
Male	-0.226	-0.069	-0.023	-0.024
	(0.046)**	(0.046)	(0.046)	(0.046)
	[0.055]**	[0.055]	[0.053]	[0.053]
Additional Controls				
Crime of Arrest Effects		X	X	X
District Sentenced Effects			X	X
Month of Charging Effects				X
No. of Obs.	101,834	101,834	101,834	101,834

Notes: The dependent variable is total number of counts. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHistMID* equals one for a post-*Apprendi* offender with a criminal history category of III or IV (and equals zero if otherwise). *Apprendi* ×*CrimHistHIGH* equals one for a post-*Apprendi* offender with a criminal history category of V or VI (and equals zero if otherwise). The symbol * represents statistical significance at 5%, ** represents significance at 1%. In column (4), the *Apprendi* main effect is absorbed by the month effects but *Apprendi* ×*CrimHist* is identified.

Table 5: Six Criminal History Categories

	(1)	(2)	(3)	(4)
Apprendi	0.326 (0.118)** [0.179]	0.310 (0.118)** [0.171]	0.249 (0.117)* [0.163]	—
Apprendi ×*CrimHist2*	-0.233 (0.085)** [0.116]*	-0.198 (0.084)* [0.112]	-0.193 (0.083)* [0.109]	-0.188 (0.083)* [0.087]*
Apprendi ×*CrimHist3*	-0.307 (0.069)** [0.092]**	-0.277 (0.068)** [0.089]**	-0.245 (0.067)** [0.088]**	-0.240 (0.067)** [0.061]**
Apprendi ×*CrimHist4*	-0.306 (0.069)** [0.087]**	-0.324 (0.068)** [0.084]**	-0.294 (0.068)** [0.083]**	-0.294 (0.068)** [0.065]**
Apprendi ×*CrimHist5*	-0.324 (0.088)** [0.111]**	-0.335 (0.087)** [0.109]**	-0.329 (0.086)** [0.111]**	-0.338 (0.086)** [0.093]**
Apprendi ×*CrimHist6*	-0.464 (0.076)** [0.089]**	-0.491 (0.075)** [0.087]**	-0.468 (0.074)** [0.087]**	-0.468 (0.074)** [0.068]**
CrimHist2	-0.142 (0.063)* [0.089]	0.142 (0.063)* [0.089]	0.156 (0.063)* [0.086]	0.150 (0.063)* [0.071]*
CrimHist3	-0.239 (0.052)** [0.067]**	0.154 (0.054)** [0.066]*	0.142 (0.054)** [0.066]*	0.136 (0.054)* [0.053]*
CrimHist4	-0.455 (0.050)** [0.063]**	0.092 (0.053) [0.063]	0.065 (0.053) [0.062]	0.058 (0.053) [0.057]
CrimHist5	-0.447 (0.061)** [0.080]**	0.107 (0.063) [0.080]	0.091 (0.063) [0.081]	0.087 (0.063) [0.082]
CrimHist6	-0.233 (0.060)** [0.068]**	0.281 (0.062)** [0.068]**	0.259 (0.063)** [0.069]**	0.256 (0.063)** [0.060]**
Additional Controls				
Crime of Arrest Effects		X	X	X
District Sentenced Effects			X	X
Month of Charging Effects				X
No. of Obs.	101,834	101,834	101,834	101,834

Notes: The dependent variable is total number of counts. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* ×*CrimHIST* X equals one for a post-*Apprendi* offender with a criminal history category of X (and equals zero if otherwise), where X is a number between 2 and 6. The symbol * represents statistical significance at 5%, ** represents significance at 1%. In column (4), the *Apprendi* main effect is absorbed by the month effects but *Apprendi* ×*CrimHist* is identified.

Table 6: Outlier Observations Included

	(1)	(2)	(3)	(4)
Apprendi	0.336 (0.139)* [0.222]	0.329 (0.138)* [0.207]	0.270 (0.138) [0.200]	—
Apprendi × *CrimHist*	-0.295 (0.053)** [0.081]**	-0.335 (0.052)** [0.077]**	-0.325 (0.052)** [0.078]**	-0.329 (0.052)** [0.050]**
CrimHist	-0.272 (0.038)** [0.058]**	0.116 (0.039)** [0.055]*	0.097 (0.039)* [0.054]	0.096 (0.039)* [0.047]*
High School Graduate	0.117 (0.038)** [0.036]**	0.004 (0.039) [0.038]	-0.031 (0.038) [0.035]	-0.033 (0.038) [0.035]
College Graduate	0.787 (0.130)** [0.153]**	0.311 (0.130)* [0.150]*	0.278 (0.129)* [0.150]	0.282 (0.129)* [0.150]
Aged 18-29 years	0.024 (0.501) [0.509]	0.093 (0.606) [0.606]	-0.145 (0.676) [0.681]	-0.235 (0.689) [0.699]
Black	0.542 (0.098)** [0.147]**	0.417 (0.116)** [0.173]*	0.146 (0.135) [0.177]	0.156 (0.135) [0.176]
Male	-0.299 (0.054)** [0.065]**	-0.093 (0.055) [0.063]	-0.047 (0.055) [0.060]	-0.046 (0.055) [0.059]
Additional Controls				
Crime of Arrest Effects		X	X	X
District Sentenced Effects			X	X
Month of Charging Effects				X
No. of Obs.	101,860	101,860	101,860	101,860

Notes: The dependent variable is total number of counts. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* × *CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%, ** represents significance at 1%. In column (4), the *Apprendi* main effect is absorbed by the month effects but *Apprendi* ×*CrimHist* is identified.

Table 7: Longer Sample

	(1)	(2)	(3)	(4)
Apprendi	0.203	0.137	0.125	
	(0.110)	(0.109)	(0.109)	—
	[0.191]	[0.171]	[0.166]	
Apprendi ×CrimHist	-0.267	-0.254	-0.239	-0.252
	(0.046)**	(0.046)**	(0.045)**	(0.045)**
	[0.114]*	[0.111]*	[0.109]*	[0.045]**
CrimHist	-0.242	0.100	0.097	0.096
	(0.036)**	(0.037)**	(0.037)**	(0.037)**
	[0.102]*	[0.102]	[0.100]	[0.043]*
High School Graduate	0.094	-0.005	-0.052	-0.038
	(0.031)**	(0.031)	(0.031)	(0.031)
	[0.033]**	[0.033]	[0.033]	[0.032]
College Graduate	0.580	0.182	0.097	0.133
	(0.089)**	(0.090)*	(0.089)	(0.088)
	[0.100]**	[0.097]	[0.097]	[0.096]
Aged 18-29 years	0.416	0.698	0.599	0.389
	(0.584)	(0.599)	(0.612)	(0.554)
	[0.580]	[0.602]	[0.628]	[0.582]
Black	0.428	0.314	0.086	0.115
	(0.080)**	(0.096)**	(0.115)	(0.113)
	[0.106]**	[0.124]*	[0.133]	[0.133]
Male	-0.184	-0.044	-0.005	-0.018
	(0.039)**	(0.040)	(0.040)	(0.039)
	[0.048]**	[0.047]	[0.046]	[0.045]
Additional Controls				
Crime of Arrest Effects		X	X	X
District Sentenced Effects			X	X
Month of Charging Effects				X
No. of Obs.	140,145	140,145	140,145	140,145

Notes: The dependent variable is total number of counts. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi ×CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%, ** represents significance at 1%. In column (4), the *Apprendi* main effect is absorbed by the month effects but *Apprendi ×CrimHist* is identified.

Table 8: Log-Linear Specification

	(1)	(2)	(3)	(4)
Apprendi	0.073	0.074	0.059	
	(0.016)**	(0.015)**	(0.015)**	—
	(0.036)*	(0.033)*	(0.029)*	
Apprendi × *CrimHist*	-0.061	-0.072	-0.066	-0.067
	(0.010)**	(0.009)**	(0.009)**	(0.009)**
	(0.017)**	(0.015)**	(0.015)**	(0.009)**
CrimHist	-0.104	0.032	0.023	0.023
	(0.007)**	(0.007)**	(0.007)**	(0.007)**
	(0.013)**	(0.012)**	(0.011)*	(0.008)**
High School Graduate	0.007	-0.008	-0.018	-0.018
	(0.006)	(0.006)	(0.006)**	(0.006)**
	(0.007)	(0.006)	(0.006)**	(0.006)**
College Graduate	0.028	-0.012	-0.024	-0.023
	(0.015)	(0.015)	(0.014)	(0.014)
	(0.015)	(0.016)	(0.015)	(0.015)
Aged 18-29 years	-0.079	0.008	-0.060	-0.074
	(0.226)	(0.271)	(0.291)	(0.302)
	(0.229)	(0.271)	(0.290)	(0.303)
Black	0.160	0.123	0.062	0.065
	(0.015)**	(0.017)**	(0.019)**	(0.019)**
	(0.019)**	(0.023)**	(0.023)**	(0.023)**
Male	-0.028	-0.019	0.000	-0.001
	(0.007)**	(0.007)**	(0.007)	(0.007)
	(0.008)**	(0.008)*	(0.007)	(0.007)
Additional Controls				
Crime of Arrest Effects		X	X	X
District Sentenced Effects			X	X
Month of Charging Effects				X
No. of Obs.	101,834	101,834	101,834	101,834

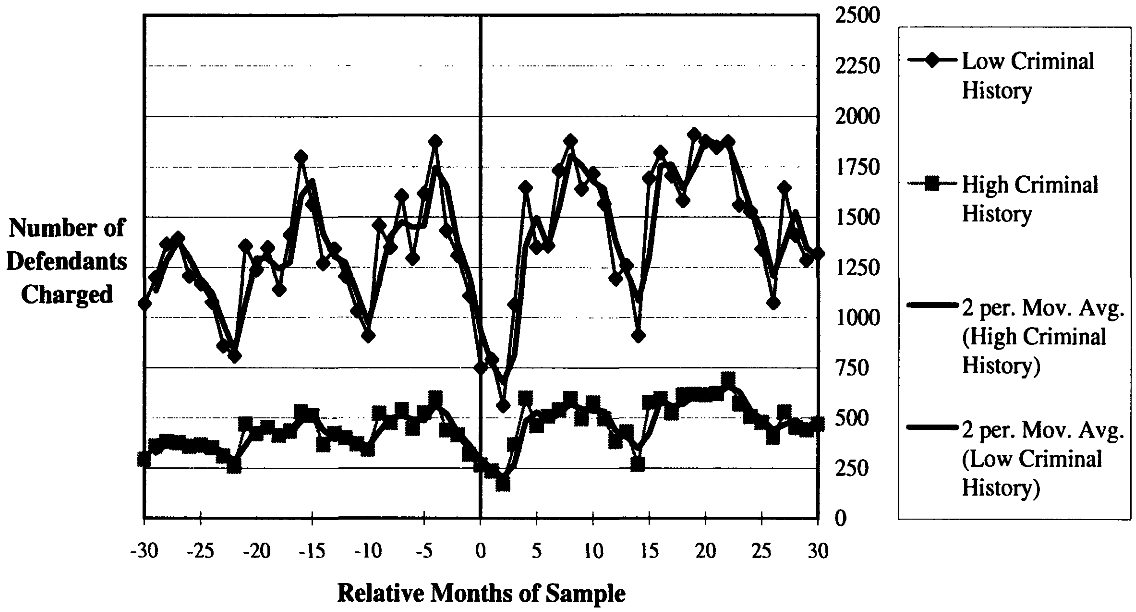
Notes: The dependant variable is total number of counts. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* × *CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%, ** represents significance at 1%. In column (4), the *Apprendi* main effect is absorbed by the month effects but *Apprendi* ×*CrimHist* is identified.

Table 9: Omitting Twelve-Month Period Around *Apprendi*

	(1)	(2)	(3)	(4)
Apprendi	1.894	1.636	1.479	—
	(0.258)**	(0.255)**	(0.253)**	
	[0.383]**	[0.369]**	[0.367]**	
Apprendi × *CrimHist*	-0.320	-0.356	-0.332	-0.334
	(0.053)**	(0.052)**	(0.052)**	(0.051)**
	[0.080]**	[0.077]**	[0.076]**	[0.046]**
CrimHist	-0.204	0.167	0.147	0.143
	(0.040)**	(0.041)**	(0.041)**	(0.041)**
	[0.060]**	[0.058]**	[0.056]*	[0.045]**
High School Graduate	0.121	0.018	-0.024	-0.024
	(0.038)**	(0.038)	(0.038)	(0.038)
	[0.041]**	[0.041]	[0.039]	[0.039]
College Graduate	0.493	0.085	0.036	0.040
	(0.109)**	(0.110)	(0.109)	(0.109)
	[0.113]**	[0.122]	[0.121]	[0.121]
Aged 18-29 years	-0.140	-0.189	-0.398	-0.466
	(0.546)	(0.579)	(0.657)	(0.685)
	[0.561]	[0.588]	[0.663]	[0.695]
Black	0.522	0.393	0.188	0.196
	(0.095)**	(0.111)**	(0.126)	(0.126)
	[0.133]**	[0.154]*	[0.168]	[0.168]
Male	-0.269	-0.094	-0.048	-0.049
	(0.049)**	(0.050)	(0.049)	(0.049)
	[0.062]**	[0.061]	[0.059]	[0.058]
Additional Controls				
Crime of Arrest Effects		X	X	X
District Sentenced Effects			X	X
Month of Charging Effects				X
No. of Obs.	83,659	83,659	83,659	83,659

Notes: The dependent variable is total number of counts. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* × *CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%, ** represents significance at 1%. In column (4), the *Apprendi* main effect is absorbed by the month effects but *Apprendi* ×*CrimHist* is identified.

Figure 3: Number of Offenders Charged by Criminal History



Notes: Figure plots running total number of defendants charged by criminal history group. The low criminal history group consists of offenders in categories I, II, and III; the high-level group consists of offenders in categories IV, V, and VI. Month "0" is the first month during which *Apprendi* applied.

Table 10: Three Month Interaction Dummies

	(1)	(2)	(3)	(4)
4 Periods Before × *CrimHist*	0.100	0.164	0.122	0.126
	(0.126)	(0.124)	(0.124)	(0.123)
	[0.139]	[0.128]	[0.121]	[0.075]
3 Periods Before × *CrimHist*	-0.096	-0.066	-0.092	-0.094
	(0.103)	(0.100)	(0.100)	(0.100)
	[0.107]	[0.108]	[0.102]	[0.075]
2 Periods Before × *CrimHist*	-0.253	-0.219	-0.244	-0.249
	(0.111)*	(0.109)*	(0.109)*	(0.109)*
	[0.113]*	[0.117]	[0.128]	[0.109]*
1 Period Before × *CrimHist*	-0.287	-0.251	-0.242	-0.236
	(0.152)	(0.149)	(0.148)	(0.148)
	[0.148]	[0.145]	[0.149]	[0.094]*
1 Period After × *CrimHist*	-0.215	-0.206	-0.289	-0.294
	(0.152)	(0.149)	(0.147)*	(0.147)*
	[0.127]	[0.123]	[0.137]*	[0.129]*
2 Periods After × *CrimHist*	-0.353	-0.366	-0.414	-0.418
	(0.111)**	(0.109)**	(0.109)**	(0.109)**
	[0.073]**	[0.071]**	[0.083]**	[0.057]**
3 Periods After × *CrimHist*	-0.359	-0.370	-0.344	-0.351
	(0.100)**	(0.098)**	(0.097)**	(0.097)**
	[0.090]**	[0.092]**	[0.094]**	[0.084]**
4 Periods After × *CrimHist*	-0.404	-0.414	-0.410	-0.414
	(0.115)**	(0.112)**	(0.112)**	(0.112)**
	[0.092]**	[0.096]**	[0.091]**	[0.084]**
5 Periods After × *CrimHist*	-0.077	-0.085	-0.052	-0.048
	(0.098)	(0.096)	(0.096)	(0.096)
	[0.094]	[0.101]	[0.102]	[0.065]
6 Periods After × *CrimHist*	-0.347	-0.369	-0.330	-0.327
	(0.119)**	(0.117)**	(0.116)**	(0.116)**
	[0.108]**	[0.093]**	[0.098]**	[0.047]**
Additional Controls				
Crime of Arrest Effects		X	X	X
District Sentenced Effects			X	X
Month of Charging Effects				X
No. of Obs.	101,834	101,834	101,834	101,834

Notes: The dependent variable is total number of counts. Robust standard errors are reported in parentheses; robust standard errors clustered on *CrimHist* ×Month are reported in square brackets. *Apprendi* × *CrimHist* equals one for a post-*Apprendi* offender with a criminal history category of IV, V, or VI (and equals zero if I, II, or III). The symbol * represents statistical significance at 5%, ** represents significance at 1%. In column (4), the *Apprendi* main effect is absorbed by the month effects but *Apprendi* ×*CrimHist* is identified.

Table 11: Actual and Predicted Criminal History Composition

	History Category 1			History Category 2			History Category 3		
Period 1	0.488	0.488	0.000	0.109	0.107	0.002	0.152	0.154	-0.002
	(0.006)	(0.003)	(0.007)	(0.004)	(0.000)	(0.004)	(0.004)	(0.001)	(0.004)
	6,946	7,342		6,946	7,342		6,946	7,342	
Period 2	0.490	0.496	-0.006	0.111	0.106	0.005	0.156	0.152	0.004
	(0.005)	(0.002)	(0.005)	(0.003)	(0.000)	(0.003)	(0.003)	(0.001)	(0.004)
	10,900	11,510		10,900	11,510		10,900	11,510	
Period 3	0.498	0.498	0.001	0.109	0.105	0.005	0.155	0.152	0.003
	(0.005)	(0.003)	(0.006)	(0.003)	(0.000)	(0.003)	(0.004)	(0.001)	(0.004)
	9,078	9,720		9,078	9,720		9,078	9,720	
Period 4	0.488	0.497	-0.009	0.109	0.106	0.003	0.155	0.152	0.003
	(0.005)	(0.002)	(0.005)	(0.003)	(0.000)	(0.003)	(0.003)	(0.001)	(0.003)
	11,889	12,591		11,889	12,591		11,889	12,591	
Period 5	0.478	0.480	-0.002	0.112	0.107	0.004	0.159	0.156	0.003
	(0.005)	(0.002)	(0.005)	(0.003)	(0.000)	(0.003)	(0.003)	(0.001)	(0.003)
	11,897	12,743		11,897	12,743		11,897	12,743	
Period 6	0.463	0.464	-0.001	0.123	0.108	0.015	0.168	0.160	0.008
	(0.005)	(0.003)	(0.006)	(0.003)	(0.000)	(0.003)**	(0.004)	(0.001)	(0.004)
	9,266	10,190		9,266	10,190		9,266	10,190	
	History Category 4			**History Category 5**			**History Category 6**		
Period 1	0.093	0.091	0.002	0.059	0.058	0.000	0.099	0.102	-0.003
	(0.003)	(0.001)	(0.004)	(0.003)	(0.001)	(0.003)	(0.004)	(0.001)	(0.004)
	6,946	7,342		6,946	7,342		6,946	7,342	
Period 2	0.092	0.089	0.003	0.056	0.057	-0.001	0.094	0.100	-0.006
	(0.003)	(0.001)	(0.003)	(0.002)	(0.000)	(0.002)	(0.003)	(0.001)	(0.003)*
	10,900	11,510		10,900	11,510		10,900	11,510	
Period 3	0.091	0.091	0.000	0.053	0.058	-0.005	0.093	0.097	-0.004
	(0.003)	(0.001)	(0.003)	(0.002)	(0.001)	(0.002)*	(0.003)	(0.001)	(0.003)
	9,078	9,720		9,078	9,720		9,078	9,720	
Period 4	0.094	0.089	0.004	0.057	0.057	0.000	0.097	0.099	-0.002
	(0.003)	(0.001)	(0.003)	(0.002)	(0.000)	(0.002)	(0.003)	(0.001)	(0.003)
	11,889	12,591		11,889	12,591		11,889	12,591	
Period 5	0.102	0.094	0.008	0.057	0.060	-0.003	0.092	0.101	-0.010
	(0.003)	(0.001)	(0.003)**	(0.002)	(0.000)	(0.002)	(0.003)	(0.001)	(0.003)**
	11,897	12,743		11,897	12,743		11,897	12,743	
Period 6	0.101	0.099	0.001	0.059	0.065	-0.006	0.086	0.104	-0.018
	(0.003)	(0.001)	(0.003)	(0.002)	(0.001)	(0.003)*	(0.003)	(0.001)	(0.003)**
	9,266	10,190		9,266	10,190		9,266	10,190	

Notes: The first sub-column for each criminal history group contains the actual post-*Apprendi* fraction of the total number of offenders made up by offenders in that group. The second sub-column shows the "predicted" fraction of that group, where the criminal history category of each offender is predicted using coefficients estimated on pre-*Apprendi* data using the multinomial logit model described in the text. The third sub-column shows the difference between the actual and predicted fractions. Standard errors are reported in parentheses. The symbols * and ** represent statistical significance at 5% and 1%, respectively.

Chapter 3

Disaggregating Employment Protection: The Case of Disability Discrimination

Joint with Christine Jolls

Abstract

Studies of the effects of employment protection frequently examine protective legislation as a whole. From a policy reform perspective, however, it is often critical to know which particular aspect of the legislation is responsible for its observed effects. The American with Disabilities Act (ADA), a 1990 federal law covering over 40 million Americans, is a clear case in point. Several empirical studies have suggested that the passage of the ADA reduced rather than increased employment opportunities for individuals with disabilities. To the extent this is true, it is crucial to credibly disentangle the different features of this complex and multi-faceted law. Separately evaluating the distinct aspects of the ADA is important not only for determining how the law might best be reformed if some aspects of it produce negative employment effects, but also for improving our understanding of the potential consequences of ADA-like provisions in race and other civil rights laws. This paper exploits state-level variation in pre-ADA legal regimes governing disability discrimination to separately estimate the employment effects of each of the ADA's two primary substantive provisions. We find strong evidence that the immediate post-enactment employment effects of the ADA are attributable to its requirement of "reasonable accommodations" for disabled employees rather than to its potential imposition of firing costs for such employees. Moreover, the pattern of the ADA's effects across states suggests, contrary to widely-discussed prior findings based on national-level data, that declining disabled employment after the immediate post-ADA period may reflect other factors rather than the ADA itself.

3.1 Introduction

A large literature examines the effects of employment protection on employment levels and other labor market outcomes for protected workers. In much of this literature, "employment protection" is taken to be a simple unitary measure,[1] and the effects of such "protection" are identified from a single change in the legal regime. Thus, for instance, Oyer and Schaefer (2000) study the effects of employment protection on employee outcomes in the United States by examining the consequences of a multifaceted antidiscrimination law, the Civil Rights Act of 1991. But relying on a complex, one-time legal innovation to identify employment effects means that there is no separate source of variation to identify which particular components of the "employment protection" law at issue are responsible for the observed effects. Such a limitation is unfortunate because, without a more precise understanding of the specific cause of the labor market consequences detected, it is difficult to design or evaluate potential policy reforms of multi-dimensional employment protection laws.

The growing literature on the employment effects of the Americans with Disabilities Act of 1990 (ADA) is a clear case in point. Several recent empirical studies have suggested that the ADA, a law that broadly regulates the treatment of individuals with disabilities in the workplace and elsewhere, has reduced the employment prospects of those individuals (DeLeire 2000, 2003; Acemoglu and Angrist 2001). To the extent this is true—a closely debated question to which we will return—it is critical from a policy perspective to determine which specific features of the ADA may be responsible. While DeLeire (2003, pp.259-60) suggests that we "should reconsider their support of the ADA as the vehicle for achieving that goal," policy reform targeted to improve the efficacy of the ADA—a law passed virtually unanimously by Congress and signed with enthusiasm by a Republican president—appears far more promising as a means of helping individuals with disabilities. To be policy relevant, therefore, empirical work that studies the consequences of the ADA must determine the specific source of the observed labor market effects.

Despite the ample literature on the employment effects of the ADA, the question of why the ADA might have a negative effect on disabled employment has received surpris-

[1] Similarly, Nickell (1997), Blanchard and Wolfers (2000), and Besley and Burgess (2004), in studying the effects of employment protection on European and Asian unemployment, measure the level of protection or labor regulation using single-dimension measures from OECD or other data sources.

ingly little systematic empirical attention. If the ADA's provisions render individuals with disabilities more costly to employ but—because of the difficulty of enforcing prohibitions on discrimination in hiring (Donohue and Heckman 1991)—the law cannot effectively prevent employers from refusing to hire these individuals in the first place, then it is unremarkable that the ADA could be found to reduce disabled employment. But existing empirical work has not resolved the question of just why the ADA might increase the costs of, and thereby cause the disemployment of, individuals with disabilities.

Two central provisions of the ADA seem most likely to increase the cost of employing disabled individuals. First, the ADA mandates that employers provide "reasonable accommodations" to individuals with disabilities—such as purchasing special equipment or altering workplace structures or procedures—unless such accommodations would create "undue hardship" for the employer. Such mandated accommodations impose obvious costs, though the precise magnitude of these costs may be uncertain (Blanck 1996). Second, by prohibiting discriminatory discharge on the basis of disability, the ADA creates "firing costs" associated with the employment of individuals with disabilities. These costs reflect the anticipated expenses (litigation and otherwise) of terminating disabled employees even for lawful reasons; such costs arise because the legal system must now be convinced that any termination was not discriminatory.[2]

The ADA, in potentially generating firing costs, parallels other civil rights statutes, such as Title VII of the Civil Rights Act of 1964 and the Age Discrimination in Employment Act of 1967. The literature on Title VII's effects contains competing evidence on whether the law has increased (contrary to the firing costs prediction) or decreased (consistent with the prediction) employment levels of protected persons (Chay 1998; Heckman and Payner 1989; Donohue and Siegelman 1991). In light of the empirical uncertainty about the validity of the firing costs account, an empirical investigation of the role of firing costs in the ADA context is an important next step in this literature.

Because the ADA imposed both a reasonable accommodations requirement and potential firing costs upon its initial enactment, existing studies comparing disabled employment levels before and after the ADA—including the studies by DeLeire and by Acemoglu and

[2]Unlike prohibitions on discriminatory failure to hire, prohibitions on discriminatory termination are likely to give rise to a significant amount of litigation by employees (Donohue and Heckman 1991).

Angrist as well as more recent studies by Kruse and Schur (2003), Hotchkiss (2004), and Houtenville and Burkhauser (2004)—are not well-suited to separating out the effects of the ADA's reasonable accommodations requirement and its potential imposition of firing costs.[3]

In this study, we seek to isolate and evaluate the two distinct explanations for reduced disabled employment after the ADA by exploiting the substantial state-level variation in disability discrimination regimes that existed prior to the ADA's enactment. During the pre-ADA period, some states' disability discrimination regimes tracked the ADA in both requiring reasonable accommodations for disabled workers and subjecting employers to a "traditional antidiscrimination prohibition" (forbidding discrimination on the basis of disability in hiring, firing, and terms and conditions of employment), with its associated firing costs. During the same period, other states imposed traditional antidiscrimination prohibitions but departed from the eventual approach of the ADA in not requiring employers to make reasonable accommodations. Finally, a third group of states imposed no limits whatsoever on private employers' treatment of disabled workers in the pre-ADA period. By separately evaluating the effects of the ADA—with its dual imposition of a reasonable accommodations requirement and a traditional antidiscrimination prohibition with its accompanying firing costs—on disabled employment in each of these three distinct state groups, we are able to provide a measure of the relative importance of the ADA's reasonable accommodations requirement and its traditional antidiscrimination prohibition in driving ADA-related disabled employment effects.

We estimate that in the years just after its enactment the ADA produced approximately a 10% decline in disabled employment in states in which the law's reasonable accommodations requirement was an innovation, compared to states in which a similar requirement existed at the state level prior to the ADA's enactment. By contrast, we consistently find little to no effect of the ADA's enactment on disabled employment in states in which

[3]Acemoglu and Angrist briefly attempt to examine the issue of the relative role of the two distinct types of legal requirements under the ADA by testing whether "separation rates" for disabled workers fell during the post-ADA period; they find no discernible effect on separation rates and therefore tentatively suggest that negative effects of the ADA may result primarily from the law's reasonable accommodations requirement. However, as Acemoglu and Angrist emphasize, the separation rate information is "plagued by considerable measurement error," and this noise may explain their failure to find an effect of the ADA on separation rates. In contrast to Acemoglu and Angrist, Baldwin and Schumacher (2002) find that the relative rate of involuntary job changes for disabled compared to nondisabled workers fell between 1990 and 1993, although again accuracy of measurement may be affecting these results. Overall, separation rate data does not seem to be a reliable way to disaggregate the employment effects of the ADA's distinct provisions.

the law's traditional antidiscrimination prohibition, with its associated firing costs, was an innovation—although for reasons described below we are not able to measure the effect of the ADA's traditional antidiscrimination prohibition as confidently as the effect of the law's reasonable accommodations requirement.[4]

The state-law variation in pre-ADA disability discrimination regimes not only allows us to disaggregate the relationship between the ADA's enactment and post-ADA employment patterns, but also provides a valuable source of variation for probing the robustness of the causal relationship, if any, between the ADA and the employment trends observed over the 1990s. As the significant scholarly debate over the ADA's employment effects, culminating in a recent book-length treatment by David Stapleton and Richard Burkhauser (2003), clearly illustrates, a perennial concern with any study of outcomes before and after the implementation of a new federal program is that concurrent unmeasured changes other than the passage of the new law—including shifts in the economic, social, and technological environment—may be the actual causes of the observed changes in outcomes. Our research, by separately studying the effects of the ADA in those states that had similar regimes in place prior to the ADA's enactment, in those states in which the ADA was an innovation only with respect to imposing a reasonable accommodations requirement, and in those states in which the ADA was a complete innovation, provides an important new lens on the ADA's effect on disabled employment over the 1990s (see DeLeire 2000, 2003; Acemoglu and Angrist 2001; Stapleton, Houtenville, and Goodman 2001; Bound and Waidmann 2002; Kruse and Schur 2003; Stapleton and Burkhauser 2003; Hotchkiss 2004; Houtenville and Burkhauser 2004).

While other authors have briefly mentioned pre-ADA state-level regimes in their analyses of the ADA, our primary focus on employing state-law variation to disaggregate the effects of the ADA's provisions leads naturally to a more comprehensive treatment of the law's differential effects across state groups.[5] Our findings support the existence of a causal

[4]Our finding of a clear near-term employment effect of the ADA's reasonable accommodations requirement contrasts with the more inconclusive findings of Beegle and Stock (2003), who use Census data to explore the employment consequences of the initial enactment of state laws requiring reasonable accommodations and who find no effects; we discuss their study at length in subsection 3.3.3 below.

[5]In the existing literature, Acemoglu and Angrist (2001) briefly employ state-law information as an instrument in their empirical analysis and, in so doing, restrict attention to a limited number of pre-ADA state-law regimes that provided for "misdemeanor charges or civil penalties" in the event of an employer violation. We do not characterize state regimes along the dimension of whether or not such sanctions were available because

relationship between the ADA and declines in disabled employment in the years immediately following the law's enactment, but beyond that period our results—contrary to the existing work by DeLeire and by Acemoglu and Angrist—provide some evidence that disabled employment declines may not be causally linked to the ADA. In particular, although relative disabled employment was lower in those later years than in the period immediately before the ADA's enactment, we find no difference in the employment reduction between states in which the ADA was and was not an innovation. We explore below various reasons why the ADA, through its reasonable accommodations requirement, might have a short-term but not a longer-term effect on the level of disabled employment.

The remainder of the paper is organized as follows. Section 3.2 describes the data used in our empirical analysis. Section 3.3 presents our basic approach and results. Section 3.4 describes a variety of robustness checks. Section 3.5 concludes.

3.2 Data

3.2.1 Pre-ADA State-Law Regimes

Tables 1, 2, A1, and A2 report the results of our detailed legal research into state disability discrimination regimes prior to the ADA. We rely on primary sources (the actual published text of statutes and judicial decisions) and, as described in the tables, have traced statutory provisions through all of their pre-ADA amendments and code sections. We have also read the pre-ADA reported case law, which provides judicial interpretations of states' statutory provisions, plus unreported case law available on Westlaw. Judicial opinions are a crucial data source because a number of states imposed reasonable accommodations requirements by judicial decision rather than by statutory provision, and because in a few states (most notably Michigan) case law holdings significantly illuminate the meaning of ambiguous or even conflicting statutory provisions that would otherwise have been read differently.

As Tables 1 and 2 reveal, states in the pre-ADA period had varying statutory and judicial regimes governing private employers' treatment of disabled workers. The largest

the ordinary set of sanctions—money damages along with nonmonetary relief such as reinstatement—did not vary significantly across the states with pre-ADA disability discrimination regimes. Hotchkiss (2004) also makes some use of certain information about pre-ADA state-level regimes; see subsection 3.3.3 below for further discussion.

group of states tracked the ADA in mandating some form of traditional antidiscrimination prohibition (forbidding discrimination on the basis of disability in hiring, firing, and terms and conditions of employment), with its associated firing costs, but differed from the ADA in not imposing a reasonable accommodations requirement; these states are listed in Table 1, and we refer to them as "protection without accommodation" states. A second group of states, listed in Table 2, imposed substantive requirements parallel to those ultimately imposed by the ADA; we refer to these states as "ADA-like" states. Finally, a third group of states (consisting of Alabama, Arkansas, and Mississippi),[6] which we term "no protection" states, set no limits whatsoever on private employers' treatment of disabled workers prior to the ADA's enactment.[7]

3.2.2 Disability Status and Other Individual Data

For the disability status of individuals—as well as for other variables such as employment levels and various demographic and other controls—we draw on the March Current Population Survey (CPS). Throughout, we refer to data by its year of observation (the year preceding the March survey), and we focus our attention on all individuals aged 21 through 58, not just those in the labor force. Variables and summary statistics for the years 1988 to 1998 are reported in Table 3.

The CPS variable for disability requires some discussion.[8] The CPS definition of disability comes from the March income supplement and reflects the subject's answer to the question, "Does [respondent] have a health problem or a disability which prevents him/her from working or which limits the kind or amount of work he/she can do?" Under the ADA,

[6]We address at length in subsection 3.3.3 below concerns that all of these states are from the southern United States. We also explain how, even with those concerns, our estimated effect of imposing the ADA's reasonable accommodations requirement—as distinguished from our estimated effect of imposing its traditional antidiscrimination prohibition—is wholly independent of the composition of the "no protection" state group.

[7]All three of the "no protection" states did prohibit disability discrimination by public employers (akin to the employment provisions of the federal Rehabilitation Act of 1973), but they did not prohibit such discrimination by private employers. Naturally, given our interest in this paper in examining the effects of the ADA, we focus on the pre-ADA state-law regimes governing private employers.

[8]Burkhauser and Daly (2002, pp.219-20) describe varying approaches to the definition of disability. As Acemoglu and Angrist (2001) note, while the CPS disability question seems to refer to the individual's status at the time of the March survey, the question actually serves as a lead-in question for a series of questions about disability income in the preceding year.

meanwhile, an individual is disabled if he or she has a physical or mental impairment that substantially limits a major life activity, has a record of such an impairment, or is regarded as having such an impairment. An affirmative answer to the CPS question certainly does not map perfectly or even that closely onto the ADA's definition of disability (Schwochau and Blanck 2000, pp.299-300).

The reasons that the CPS and ADA definitions may diverge are several. First, individuals who answer the CPS question affirmatively may be incorrectly reporting health conditions or impairments that limit work—perhaps because they are unable to find work-and may not in fact be truly impaired (see Kreider and Pepper 2002). Second, only certain types of genuine impairments that may limit work have been deemed by the Supreme Court to be covered by the ADA (*Sutton v. United Airlines*, 527 U.S. 471 (1999)). Third, alongside these respects in which the CPS measure may be broader than the ADA measure, the CPS measure may be narrower in not including those with a record of impairment or who are regarded as impaired, but who are not actually impaired.[9]

While the CPS measure would clearly provide a poor basis for some empirical conclusions—such as the absolute number of people protected by the ADA at a given point of time—the estimates we report are not vulnerable on this ground because our approach uses the CPS measure to assess *changes* in employment levels after the ADA's enactment. Burkhauser, Daly, Houtenville, and Nargis (2002) provide evidence from National Health Interview Survey (NHIS) data that employment changes over time for populations defined by work limitations (as under the CPS) are not significantly different from employment changes over time for populations defined by impairments (closer to the ADA's approach). Thus, we think that the CPS disability question has sufficient overlap with the definition of disability under the ADA that studying how those who answer "yes" to the survey question were affected by the ADA in terms of their employment levels allows one to learn something important about the effects of the law on disabled employment.[10]

[9]The Equal Employment Opportunity Commission (EEOC) claims data (available at http://www.eeoc.gov/stats/ada.html) suggest that about 15% of EEOC claims under the ADA involve the "record" and "regarded as" prongs of the ADA's disability definition.

[10]In the future, the CPS is likely to include disability-related questions that map more closely onto the ADA's definition (see Kruse and Hale 2003, pp.6-9). No matter how precise this information, however, it will obviously not be available for either the time period in which the ADA was passed or for the years prior to the ADA's enactment. In this light, use of the CPS measure from the pre-ADA and immediate post-ADA periods is a reasonable step in seeking to measure the effects of the ADA on disabled employment.

The most important potential concern with the use of the CPS measure of disability or, indeed, the use of any survey-based measure of disability for purposes of examining pre- and post-ADA employment levels of individuals with disabilities is that the ADA's enactment could itself have altered the composition of the group responding "yes" to the disability survey question. Kruse and Schur (2003) describe several routes by which the passage of the ADA could alter the nature of the group of individuals answering "yes" to a CPS-like disability survey question. If such changes occurred, then apparent disemployment effects of the ADA could actually be effects of the law on the nature of the population being counted as disabled. In Section 3.4 below, we closely examine the time trend in affirmative answers to the CPS disability question, and, consistent with prior work on the prospect of such composition bias (Acemoglu and Angrist 2001, p.935; Beegle and Stock 2003, pp.855-56), we do not find evidence that compositional changes are driving our findings.[11]

3.3 Empirical Approach and Results

The ADA was enacted in July of 1990, so throughout the empirical analysis we compare employment levels in two-year periods starting in 1990 to employment levels in the two-year period immediately preceding 1990. Because the ADA did not go into effect until two years after its enactment, it is possible that effects lagged behind the 1990 enactment date. Alternatively, the time immediately following enactment might have witnessed the largest employment effects as employers in "protection without accommodation" and "no protection" states—while facing new potential costs of employing disabled workers down the road—were not yet restricted by ADA (or ADA-like) provisions and thus could, for instance, discharge or refuse to hire someone because of the person's need for reasonable accommodations without incurring any legal risk. Extensive enactment-period media coverage of the ADA suggests that many managerial employees learned of the ADA when it was enacted.[12] With respect to our "pre-ADA" period of 1988-1989, because the ADA actually received widespread media coverage as early as the latter half of 1989—when the law was

[11] The CPS was redesigned between the 1993 and 1994 surveys, corresponding to observation years 1992 and 1993. Acemoglu and Angrist (2001, pp.925, 951) offer analysis suggesting that the redesign does not materially affect an understanding of the ADA's employment effects.

[12] A search of the Lexis-Nexis News Group File for mentions of the ADA in 1990 yielded 965 hits.

already widely anticipated—our use of 1988-1989 as the period against which to measure the law's effects will, if anything, tend to bias our estimates against finding an effect of the ADA, as employers' behavior conceivably could have been affected as early as the second half of 1989.[13]

3.3.1 Univariate Results

Mean employment levels across our three state groups provide a first view of the basic effect of the ADA's reasonable accommodations requirement and the law's traditional antidiscrimination prohibition, with its associated firing costs, on employment of people with disabilities. Table 4 reports the mean employment levels in weeks per year for disabled and nondisabled people, before (1988-1989) and after (all subsequent pairs of years) the passage of the ADA, separately for each of our three state groups: the "protection without accommodation" group, containing states with traditional antidiscrimination prohibitions but no reasonable accommodations requirements prior to the ADA's enactment; the "ADA-like" group, with states that had both traditional antidiscrimination prohibitions and reasonable accommodations requirement prior to the ADA's enactment; and the "no protection" group, containing states that imposed no restrictions on private employers' treatment of disabled workers prior to the ADA's enactment. For ease of presentation, we use two-year windows before and after the change in the legal setting (similar to Katz 1998 and Autor, Donohue, and Schwab 2002), though our results are robust to windows of varying length.

Table 4a compares disabled versus nondisabled employment levels before and after the ADA in "protection without accommodation" states with disabled versus nondisabled em-

[13]During the latter half of 1989, media sources frequently referred to the certain or virtually certain passage of the ADA the following year. In the legal literature, for instance, Chatoff (1989) stated that the ADA "inevitably will" become law, while Gardner (1989) wrote that Congress "seems almost certain to enact" the ADA "in the very foreseeable future." In the popular media, Shapiro (1989) stated of the ADA that "President Bush . . . guaranteed the bill's passage with his support," while Calkins (1989) quoted a disability advocate's confident declaration that "for the first time ever, people with disabilities will have civil rights protection under federal law equal to the protection already afforded to members of minority groups and to women." Of particular interest are industry periodicals targeted to employers and their managerial employees; in this category, Romeo (1989) reports in an article in Nation's Restaurant News that at a meeting of the National Restaurant Association the "Americans with Disabilities Act was mentioned several times"; that a member of the Association's Human Resources Committee stated that the law "will affect us in the very near future"; and that another Association official stated that the "ADA seems certain to pass." Similarly, an editorial entitled "Accommodating Disabled Workers in the Construction Industry," published in October of 1989 in the Engineering News-Record, stated that passage of the ADA "seems certain to follow."

ployment levels before and after the ADA in "ADA-like" states. Because the absence or presence of a pre-ADA reasonable accommodations requirement is the dimension along which the two state groups differ, this first comparison provides a measure of the effect of the ADA's imposition of a reasonable accommodations requirement. As Table 4a shows, in "protection without accommodation" states, where the ADA's reasonable accommodations requirement was an innovation, disabled employment declined by 1.35 weeks per year in 1990-1991 compared to 1988-1989, while nondisabled employment showed a far smaller decline of 0.23 weeks per year; by contrast, in "ADA-like" states, in which the substantive requirements of the pre-ADA state-level regimes tracked those of the ADA, disabled employment actually increased by 0.83 weeks per year in 1990-1991 compared to 1988-1989, while nondisabled employment was virtually unchanged (a decline of 0.03 weeks per year).

Taking the difference between the two within-state-group differences for 1990-1991 compared to 1988-1989, the mean-based difference-in-difference-in-difference (DDD) estimate for the change in disabled employment generated by the imposition of a reasonable accommodations requirement is -1.98 weeks per year (the third column of Table 4a). Given the base number of weeks employed for disabled persons prior to the ADA's enactment—16.25 in "protection without accommodation" states and 18.22 in "ADA-like" states—the drop of 1.98 weeks represents over a 10% decline in disabled employment. The evidence of declining relative disabled employment in "protection without accommodation" states compared to "ADA-like" states continues in 1991-1992 and 1992-1993, and then disappears in 1993-1994 and subsequent pairs of years—the first suggestion of a near-term but not long-term effect of the ADA's reasonable accommodations requirement. We discuss this timing pattern in further detail below. Notice the reassuring fact that in all of the near-term post-ADA comparisons, nondisabled employment—in contrast to disabled employment—is relatively stable between the pre- and post-ADA periods in both "protection without accommodation" and "ADA-like" states.

Table 4b compares "no protection" states, with no pre-ADA legal restrictions on private employers' treatment of disabled workers, to "protection without accommodation" states, with only traditional antidiscrimination prohibitions prior to the ADA's enactment. This comparison thus provides a measure of the effect of imposing an antidiscrimination prohibition (an innovation in "no protection" states but not in "protection without accommodation" states). Our mean-based DDD estimate for imposing such a prohibition is unstable over the 1990-1991 through 1997-1998 "after" periods and is always small in magnitude and statistically indistinguishable from zero. Because this measure is identified based on the experience of three small southern states (the "no protection" states, in which the ADA's traditional antidiscrimination prohibition was an innovation), however, we explore a variety of robustness checks on this finding in the regression analysis below.

3.3.2 Regression Framework

Our regression analysis employs a straightforward DDD specification like that used, for example, in Gruber (1994) and Collins (2003). All regressions take the form (for each specified two-year post-ADA period in Tables 5-6 and A3-A5):

$$\begin{aligned} Y_{ijt} = {} & \beta_0 + \beta_1 X_{ijt} + \beta_2 ADA_t + \beta_3 DIS_i + \beta_4 LP_j + \beta_5 NP_j + \beta_6 ADA_t \times DIS_i \\ & + \beta_7 ADA_t \times LP_j + \beta_8 ADA_t \times NP_j + \beta_9 DIS_i \times LP_j + \beta_{10} DIS_i \times NP_j \\ & + \beta_{11} ADA_t \times DIS_i \times LP_j + \beta_{12} ADA_t \times DIS_i \times NP_j + \epsilon_{ijt} \end{aligned} \tag{3.1}$$

where Y is weeks worked; i indexes individuals, j indexes states, and t indexes years; X is a vector of demographic and state-level economic characteristics; ADA is a dummy variable equal to one in the post-ADA period; DIS is a dummy variable equal to one for disabled individuals; LP is a dummy variable equal to one for states offering limited protection in the form of a traditional antidiscrimination prohibition prior to the ADA's enactment ("protection without accommodation" states); and NP is a dummy variable equal to one for "no protection" states. The LP and NP dummy variables measure effects relative to those in the "ADA-like" state group.[14]

The coefficients of interest in equation (3.1) are the coefficients on the triple interaction terms, $ADA_t \times DIS_i \times LP_j$ and $ADA_t \times DIS_i \times NP_j$. The coefficient β_{11} on the first

[14] Instead of looking at such relative effects, one could examine separate effects within each of the three state groups. In that model the coefficients of interest would be $ADA_t \times DIS_i \times LP_j$, $ADA_t \times DIS_i \times NP_j$, and $ADA_t \times DIS_i \times AD_j$, where AD_j is a dummy variable equal to 1 for states in the "ADA-like" group. Table A3 reports results from specifications that omit $ADA_t \times DIS_t$ and instead estimate the coefficient on $ADA_t \times DIS_i \times AD_j$. In these specifications, the coefficients on $ADA_t \times DIS_i \times LP_j$ and $ADA_t \times DIS_i \times NP_j$ measure overall effects rather than effects relative to the "ADA-like" states. Returning to the original specification reflected in equation (3.1), because there are three (nonoverlapping) groups of states in our study, several of the interactions between the dummy variables are always zero (in particular, $LP_j \times NP_j$, $ADA_t \times LP_j \times NP_j$, $DIS_i \times LP_j \times NP_j$, and $ADA_t \times DIS_i \times LP_j \times NP_j$), and thus these drop out of equation (3.1).

of these terms measures the change between the pre- and post-ADA periods in disabled versus nondisabled outcomes in "protection without accommodation" states (those with traditional antidiscrimination prohibitions but no reasonable accommodations requirements prior to the ADA) relative to this same change in "ADA-like" states. In other words, β_{11} tells us how relative disabled outcomes changed in states in which the ADA's reasonable accommodations requirement, but not its traditional antidiscrimination prohibition, was new (the "protection without accommodation" group) compared to how these outcomes changed in states in which neither substantive requirement of the ADA was new (the "ADA-like" group).

Our approach here does not assume that the enactment of the ADA made no difference at all in states that had substantively comparable pre-ADA protections (the "ADA-like" states); among other possibilities, the enactment of the federal statute made available Equal Employment Opportunity Commission (EEOC) enforcement and altered other procedural aspects of pre-existing disability discrimination law, such as the availability of federal court adjudication (see generally Neuborne 1977), and these changes may have influenced disabled outcomes. Effects of the federal regime that are identical across states are permissible within a triple differences framework, although such effects, if they exist, cannot be identified.

Of course, if the ADA had *differential* effects across the "protection without accommodation" and "ADA-like" groups for reasons unrelated to the substantive legal provisions in effect in these groups, then our estimate of β_{11} would pick up those additional effects along with the effect of imposing a reasonable accommodations requirement. If, for example, states in the "ADA-like" group tended to be systematically more vigorous in accepting and enforcing civil rights claims brought by disabled individuals than states in the "protection without accommodation" group, then β_{11} would measure not only the effect of imposing a reasonable accommodations requirement but also the effect of supplementing moderate or limited enforcement of disability discrimination law in the "protection without accommodation" states with the more robust procedures provided by the ADA.[15]

[15]Any attempt to control directly for cross-state variation in pre-ADA enforcement behavior using the number of discrimination charges brought under the various state laws would be confounded by the significant endogeneity of charge rates and the employment level (our dependent variable). Acemoglu and Angrist (2001) address this endogeneity issue in their use of discrimination charge data by instrumenting for state charge rates with a variable for whether the state had a particular type of pre-ADA disability discrimination law (one providing for "misdemeanor charges or civil penalties"), but that approach is not open to us here given the role the state-law information already plays in our analysis.

Nonetheless, we think it is unlikely that our estimate of β_{11} will pick up enforcement differences that could confound the estimated effect on disabled employment of imposing a reasonable accommodations requirement. The timing of the state-law enactments suggests that the "ADA-like" states are not the systematically more aggressive, pro-disabled-worker states; in most cases the states that had reasonable accommodations requirements prior to the ADA's enactment were those that instituted disability discrimination regimes relatively *late* in the game, while the "protection without accommodation" states were those "early to the party" in protecting civil rights of disabled workers (see date columns in Tables 1 and 2). Thus, if anything, our estimate of β_{11} may *understate* the potential disemployment effect for disabled persons of imposing a reasonable accommodations requirement, given the most plausible direction of any pre-ADA enforcement disparities across states.

While β_{11} measures the effect of imposing a reasonable accommodations requirement, equation (3.1) also provides us with a direct measure of the effect of simultaneously imposing both a reasonable accommodations requirement and a traditional antidiscrimination prohibition. The coefficient β_{12} measures the change between the pre- and post-ADA periods in disabled versus nondisabled outcomes in "no protection" states relative to this same change in "ADA-like" states, and the difference in legal regimes between those two groups is the absence (in the former) versus the presence (in the latter) of both a reasonable accommodations requirement and a traditional antidiscrimination prohibition prior to the ADA. Thus the difference in the two groups' outcomes (β_{12}) is a measure of the effect of imposing both of these provisions; it then follows that the difference $\beta_{12} - \beta_{11}$, also reported in our tables, measures the effect of imposing only a traditional antidiscrimination prohibition.[16]

All of the regressions reported below contain controls for individual i's age, race, sex, educational attainment, marital status, and union membership; for individual or state-level disability benefits information; and for the state unemployment rate (except where precluded by the inclusion of state effects, as noted below) and the interaction of disability with the state unemployment rate. By the nature of the DDD methodology, our approach controls for national time trends in employment, the general effect of disability on employment, state-group specific employment effects, and interactions of each of these factors

[16]Because β_{12} depends on outcomes in the "no protection" states—all of which are in the South—we explore at length below whether region-specific factors could be affecting our results (and find evidence that they are not)

with the others. Many of our regressions also include state, year, and state-year interaction effects, although in those specifications we are unable to identify the effects of the state unemployment rate and of the ADA_t, LP_j, and NP_j variables from above and their interactions with each other. Importantly, because all of our regressions include the interaction of disability with the state unemployment rate, our approach controls for the possibility that individuals with disabilities may face especially poor employment prospects when unemployment rates are high—an important consideration given the early 1990s recession, which immediately followed the ADA's enactment (Kruse and Schur 2003) and thus might otherwise have generated effects similar to those estimated here.

3.3.3 Regression Results

The top panel of Table 5 reports the results of the basic specification in equation (3.1). Consistent with the findings in Table 4, the estimate for β_{11}, the effect of imposing a reasonable accommodations requirement, is clearly negative for the post-ADA year pairs 1990-1991, 1991-1992, and 1992-1993, with estimates ranging from -1.54 to -2.51 weeks per year. Thus, as before, imposing a reasonable accommodations requirement seems to produce in the neighborhood of a 10% decline in disabled employment in the near-term aftermath of the ADA's enactment. Meanwhile, again parallel to the results in Table 4, the estimate for $\beta_{12} - \beta_{11}$, the effect of imposing a traditional antidiscrimination prohibition, over the post-ADA years 1990-1991, 1991-1992, and 1992-1993 is small in magnitude, inconsistent in sign, and never statistically significant.[17]

Thus, our results indicate that the reasonable accommodations requirement of the ADA was the source of a short-term negative effect of the law on disabled employment and, more

[17]The dependent variable in our regressions, as noted above, is the number of weeks worked during the year. This is standard in the literature (see Acemoglu and Angrist 2001, p.925), and captures employment consequences resulting from discharges, temporary layoffs, and changes in seasonal employment. Another possible measure, however, is simply whether the individual was employed. To ensure that our findings are not driven by our choice of dependent variable (and linear specification), we report in Table A4 the estimated coefficients from a probit specification in which the dependant variable is equal to one if the individual was employed. The same pattern we saw in Table 5 emerges: a reasonable accommodations requirement appears to reduce the likelihood that an individual with a disability was employed, relative to a nondisabled individual, but we find no evidence that a traditional antidiscrimination requirement had any effect on the possibility of being employed. Again, consistent with Table 5, the reasonable accommodations effect is short-lived. Finally, in separate, unreported work, we find no evidence that either the accommodations requirement or the antidiscrimination mandate had any measurable effect on wages, which is consistent with earlier studies (see Acemoglu and Angrist 2001, p.932).

tentatively, that the law's traditional antidiscrimination prohibition may have had little effect in the years after the ADA's enactment. The second panel in Table 5 shows that our results are unchanged when state, year, and state×year effects are included, and the top panel of Table A5 verifies that the results are unchanged when state, state×year, and state×disability effects (to control for compositional changes) are included.

As we have suggested at several points, a potential issue with our measure of the effect of imposing the ADA's traditional antidiscrimination provision—although not with our measure of the effect of imposing the ADA's reasonable accommodations requirement—is the former effect is identified in part based on outcomes in the "no protection" state group, which consists of three southern states, Alabama, Arkansas, and Mississippi.[18] If, between the pre- and post-ADA periods, some unobserved shock occurred in the southern region of the country, and that shock differentially affected disabled and nondisabled persons, then our measure of the effect of imposing the ADA's antidiscrimination provision would capture this unobserved shock (because our measure of the effect of the ADA's traditional antidiscrimination prohibition depends on the experience of the "no protection" states) in addition to any ADA-related effect. Relatedly, it is possible that states with higher levels of disabled individuals—a group that includes states in the South—experienced unobserved labor-market changes differentially affecting disabled persons between the pre- and post-ADA periods; again our estimate of the effect of imposing the ADA's traditional antidiscrimination prohibition would capture these unobserved shifts because the "no protection" group consists exclusively of southern states.[19]

A straightforward strategy to alleviate these concerns about a possible trend differentially affecting states in the "no protection" group is simply to re-estimate equation (3.1) on just the southern states—with their more similar populations—from each of our three state groups. The "protection without accommodation" group in the regressions we report includes Georgia, Kentucky, South Carolina, and Tennessee, while the "ADA-like"

[18]By contrast, the "protection without accommodation" and "ADA-like" groups are large and well-balanced across the country, as shown in Tables 1 and 2; our estimate of the effect of the ADA's reasonable accommodations requirement will not be affected by the limited size and geographic diversity of the "no protection" group because that effect is identified solely from the comparison of employment changes in the "protection without accommodation" group with similar changes in the "ADA-like" group.

[19]In terms of the representation of disabled individuals in the population across states, the mean proportion of disabled individuals over 1988-1998 was .085 in the "no protection" states, compared to .067 in the "protection without accommodation" states and .068 in the "ADA-like" states. See Figure 1.

group contains Louisiana and North Carolina.[20] (The "no protection" group is, as before, Alabama, Arkansas, and Mississippi.) Using the number of disabled individuals in the population as a metric, these "southern-only" state groups are more comparable to the "no protection" group than were the groups in the original 50-state sample.[21] Although, not surprisingly, the precision of our estimates falls with the reduction in sample size, the top panel of Table A6 shows that the results of estimating the basic fixed-effects specification from the middle panel of Table 5 on the nine-state southern subsample follow the same basic pattern as the results from the full 50-state sample.

Our finding of a significant negative employment effect of imposing a reasonable accommodations requirement contrasts with the conclusions of a recent paper by Beegle and Stock (2003), whose results in some cases also point in a direction opposite that of DeLeire (2000, 2003) and Acemoglu and Angrist (2001). Beegle and Stock use Census data for 1970, 1980, and 1990 to study the effects of the enactment of state laws governing disability discrimination in the pre-ADA period (whereas we examine the effects of the ADA across different groups of states characterized by their varying pre-ADA legal regimes). Beegle and Stock find no significant effect of the enactment of reasonable accommodations requirements (in contrast with our finding) and, in specifications in which they include disability$\times$year fixed effects, no significant effect of the enactment of disability discrimination law in general (in contrast with the findings of DeLeire and of Acemoglu and Angrist). While, as just noted, incorporating disability$\times$year fixed effects has a significant impact on Beegle and Stock's results, the bottom panel of Table A5 shows that including these effects (along with the other fixed effects utilized by Beegle and Stock) has little effect on our results.

The differences between Beegle and Stock's results and ours may be attributable to various differences in econometric approach, and possibly also to the imprecision of some of the state-law information used by Beegle and Stock. With respect to econometric approach, Beegle and Stock's framework has some states promulgating a particular law during a given time period while others do not, and changes in outcomes across the two groups of

[20] Our conclusions are robust to the definition of the South. We experimented with a number of other southern state groupings, some of which included up to a half-dozen additional states, with similar results. Consequently, to be conservative, we report results based on a relatively narrow definition of the South.

[21] The mean proportion of disabled individuals over 1988-1998 is now .087 in the "protection without accommodation" group and .075 in the "ADA-like" group, far higher than in the original groups and much closer to the level (.085) observed in the "no protection" group.

states are then compared. In that setting, there is a risk that adjustments in the relative labor market outcomes in the different states reflect not the policy shifts under study but rather underlying state-level social or economic changes that simultaneously *caused*, or at least occurred contemporaneously with, the changes in the state laws (see Besley and Case 2000).[22] Examining the enactment of the ADA against a background of a well-established diversity of state law, by contrast, reduces the concern about this sort of omitted-variables bias because there is little reason to fear that the degree to which the ADA was an innovation in a given state is correlated with state-specific social or economic changes, given that virtually all of the state laws in question were enacted well before the passage of the ADA (see Tables 1 and 2).[23]

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[22]As Beegle and Stock note, "[i]f laws were disproportionately passed in states where the disabled were [already] faring better, we would expect the laws to have smaller effects and our empirical results to underestimate the negative impact of the legislation (relative to random assignment of the laws, including states where the negative impact of the laws would be larger)" (p.855).

[23]As mentioned, the difference between Beegle and Stock's results and ours may also stem in part from the imprecision of some of the state-law information used by Beegle and Stock. We obtain information exclusively from primary legal sources, while Beegle and Stock rely on secondary sources, which in some cases prove to be inaccurate and in any event do not allow them to identify the year of a law's enactment (just the year when the law is first referred to in the secondary source in question).

The imprecision in the information about state laws will affect Beegle and Stock's empirical analysis when the dating errors cross decade markers, as they do in a number of cases. For instance, Beegle and Stock, relying on a secondary source, state that Arkansas had a law, §20-14-303, prohibiting private sector disability discrimination by 1987. However, this statute did not, in 1987, cover *employment* discrimination, although it did cover private sector discrimination in other areas, such as access to restaurants and other public places. (The full text of the statute is available in the AR-STANN87 historical legislative database on Westlaw.) In alphabetical order, other states that are incorrectly classified by Beegle and Stock in terms of the decade in which private sector employment discrimination laws relating to disability were enacted include Colorado, Louisiana, Massachusetts, Missouri, North Carolina, Oregon, and Rhode Island. Of this group, Colorado and Louisiana are misclassified as to both the decade in which a traditional antidiscrimination prohibition was enacted and the decade in which a reasonable accommodations requirement was enacted; Missouri, North Carolina, and Rhode Island are misclassified as to the decade in which a traditional antidiscrimination prohibition was enacted; and Massachusetts and Oregon are incorrectly classified as to the decade in which a reasonable accommodations requirement was enacted. See Tables 1 and 2 below for state-law enactment information. This noise in the coding of the state-law explanatory variable will tend to bias estimates toward zero (see Autor, Donohue, and Schwab 2002, p.28), and this may help to explain the difference between Beegle and Stock's findings and ours.

Hotchkiss (2004) also makes some use of the pre-ADA state disability discrimination regimes, and differs from Beegle and Stock in relying on primary legislative materials rather than secondary sources. However, she nonetheless incorrectly categorizes a substantial number of states, in many instances because a state statute prior in time to the one she located also regulated disability discrimination in employment. States miscategorized by Hotchkiss, in terms of date of enactment, include Alaska, California, Colorado, Hawaii, Illinois, Massachusetts, New Jersey, North Carolina, South Carolina, South Dakota, Texas, and Virginia. (In the case of Alaska, California, and South Dakota, Hotchkiss explicitly notes that "exact original coverage [is] not available.") Some of the dating errors will not affect Hotchkiss's empirical results; she examines the effects of state laws enacted between 1981 and 1991, and, thus, a dating error outside this period will not affect her results. However, in the case of six states—Alaska, Hawaii, Massachusetts, North Carolina, South Carolina, and Texas—the dating error affects the classification of states over the 1981-1991 period.

Returning to our empirical results, the 93-94 through 97-98 columns of Table 5 show that in this period neither imposing a reasonable accommodations requirement nor imposing a traditional antidiscrimination prohibition had a statistically significant effect on disabled employment: estimates for β_{11}, the effect of imposing a reasonable accommodations requirement, range from -0.60 to 0.70, while those for $\beta_{12} - \beta_{11}$, the effect of imposing a traditional antidiscrimination prohibition, range from -0.25 to 0.87. In other words, beginning in 1993-1994 and going forward, the ADA's enactment had statistically indistinguishable effects across the three state groups.

What is especially striking about these later "post-ADA" years is that it is precisely in 1993-1994 that the estimated coefficient on the term $ADA_t \times DIS_i$, measuring the overall effect of the ADA as well as any other economic or social trends common across states, becomes negative and statistically significant. Therefore, from 1993-1994 forward, movements in relative disabled employment were downward *and similar in magnitude* across all three state-law regimes—a result at odds with the differential pattern, based on pre-ADA state legal regimes, in the early years after the ADA's enactment. The bottom panel of Table 5 underlines the point by showing that in a specification that omits state-law information (by setting $NP_j = LP_j = 0$ in equation (3.1)), the estimated coefficients on $ADA_t \times DIS_i$ in 1993-1994 and forward are similar to the ones in the top and middle panels, in which the separate effects of being in the "protection without accommodation" and "no protection" state groups are not constrained to be zero.

Thus, our results for 1993-1994 and forward, while consistent with existing findings of a persistent decline in disabled employment over the 1990s relative to the pre-ADA period, reveal the absence of any link between the degree of employment effects in this later period and the degree to which the ADA was actually a legal innovation relative to pre-ADA state law. The juxtaposition of a clear state-group pattern, in a predictable direction, in the period immediately following the ADA's enactment (through 1992-1993) and no state-group differences in the later years raises questions about the longevity of any negative employment effects of the ADA's two primary requirements. These questions, as well as possible interpretations of our results that are consistent with a longer-term negative effect of the ADA, are discussed further in the next subsection.

3.3.4 Discussion

Our empirical results indicate that in the near-term after the ADA's enactment, the law's reasonable accommodations requirement, but not its traditional antidiscrimination prohibition with its potential firing costs, had a measurable negative effect on disabled employment. The large negative effect of the reasonable accommodations requirement on disabled employment in the period just after the ADA's enactment may reflect the fact that many accommodations, including physical alterations to the workplace and modification of workplace policies, impose obvious but often one-time costs on employers—costs that may well have been exaggerated or particularly salient in employers' minds just after the ADA's passage.[24] Employers might naturally have responded to anticipated costs by curtailing their hiring of disabled workers, particularly in the period between the ADA's enactment and effective dates when curtailing hiring on account of accommodation costs was not illegal in states without an existing accommodations requirement.

Further reasons that the effect of the ADA's reasonable accommodations requirement on disabled employment might have been larger in the short term include the ADA's important symbolic effect and the resulting changes in attitudes over time; the possibility that reasonable accommodations could ultimately increase the flow of qualified disabled applicants following a short-term reduction as disabled individuals responded to the ADA by pursuing more education (see Jolls 2000); declining accommodation costs in response to technological changes and judicial refinements of the ADA's requirements; and enforcement of the ADA's prohibition on refusal to hire based on accommodation costs after the ADA's effective date.[25]

The possibility that the true employment effects of the ADA are short-term rather than longer-term effects is consistent with Kirchner (1986), who emphasizes that the con-

[24]Some observers have pointed to evidence that accommodation costs may often be modest (Blanck 1996), but measurement issues and skewed samples of accommodations suggest that relatively limited weight should be attached to such evidence (Stein 2000, p.1677). In any event, some legally mandated accommodations—for instance, the need to hire readers for blind employees, as specified by federal regulations (see 29 CFR §1630.2(o))—are clearly extremely costly for employers.

[25]A more positive account of the short-term negative employment effect of the ADA's reasonable accommodations requirement is that the effect was *itself* evidence of reasonable accommodations to the scheduling needs of disabled workers (see Tolin and Patwell 2003). Given that we observe declines in weeks worked, not hours worked per week, however, and that the decline is limited to the period immediately after the ADA was enacted, it is difficult to view the negative employment effects as the fact, rather than the consequence, of mandated accommodation.

sequences of laws such as the ADA may differ significantly over different time horizons. A potential interpretation of our findings—pointing against a causal role for the ADA in the longer-term employment trends of individuals with disabilities over the 1990s—is that the apparent negative effect of the ADA on disabled employment in 1993-1994 and subsequent years reflects not the impact of the ADA itself but, rather, other contemporaneous changes disproportionately affecting individuals with disabilities. Otherwise, it is not clear why the magnitude of the disabled disemployment effect in 1993-1994 and forward would have no relationship to the degree to which the ADA was a legal innovation, when such a relationship did appear to exist in the immediate post-enactment period.

Conceivably, the significant decline in disabled employment across all states in 1993-1994 and forward reflects a longer-term effect of aspects of the ADA that were innovations in *all* states. The ADA's enactment made available EEOC enforcement and federal court adjudication for disability discrimination claims, and thus one possible story is that such heightened enforcement generated firing costs sufficient to encourage disemployment. Under this theory, firing costs in states with traditional antidiscrimination prohibitions prior to the ADA were not large enough (because of limited enforcement) to discourage disabled employment, so effects of the ADA were observed across all states. In terms of timing, this story requires at least some lag between the ADA's enactment (July 1990) and effective (July 1992) dates, on the one hand, and the time at which the law affected disabled employment, on the other.[26] On balance, our findings do not by themselves rule out a continuing link between the ADA's enactment and disabled employment in 1993-94 and forward, but they do appear more consistent with an alternative, more short-term account of the ADA's effects on disabled employment.

3.4 Further Robustness Checks

This section further probes the robustness of our basic finding in Tables 4 and 5 that in the years just after the ADA's enactment, the imposition of the law's reasonable accommodations requirement, but not its traditional antidiscrimination prohibition, produced a

[26] A story of delayed effects seems less likely a priori with reasonable accommodations requirements because, compared to firing costs, the costs of making accommodations are more tangible and immediate.

significant decline in disabled employment. We also examine the possible role that composition effects or preexisting state-group specific employment trends may play in our analysis.

3.4.1 Robustness to the Timing of State-Law Enactment

The upper left-hand panel of Table 6 shows the results (for post-ADA years 1990-1991, 1991-1992, and 1992-1993) of estimating the basic fixed-effects specification from the middle panel of Table 5 on a subsample of observations from states in which the state-level pre-ADA disability discrimination regime was already in place prior to 1980. We perform this check on the theory that these early enactors—which, as noted earlier, are predominantly "protection without accommodation" states—may have differed systematically in their degrees of "civil rights orientation," and thus in their enforcement environments, from the later enactors. The fact that in two of the three regressions the estimated magnitude of β_{11} is even larger than in Table 5 suggests that the estimates based on the full 50-state sample may *understate* the effect of imposing an accommodations requirement.[27]

The robustness of our estimate of β_{11} in a sample of states with more uniform enactment dates also responds to the possible concern that, if there are either lags or bursts in state law effectiveness shortly after a state law is put on the books, then our 50-state results may be confounded by the different average enactment dates across the "protection without accommodation" and "ADA-like" state groups. Our estimate of $\beta_{12} - \beta_{11}$, the effect of imposing a traditional antidiscrimination prohibition, is also quite stable across the broader (all states) and narrower (pre-1980 enactors only) samples.

3.4.2 Robustness to Variation in Employer-Size Coverage Thresholds

Pre-ADA state legal regimes varied significantly in the numerical employer-size thresholds they established for coverage by the state legal regime (see Tables 1 and 2). To address any concerns of bias arising from these significantly varying thresholds, we re-estimated the basic fixed-effects specification from the middle panel of Table 5 using only observations from states with employer-size coverage thresholds of 15 employees (the ultimate ADA threshold)

[27]We view this possibility with a good deal of caution, however, because only three states in the "ADA-like" group (Colorado, Oregon, and Washington) had their pre-ADA regimes in place prior to 1980.

or higher.[28] Our results, reported in the upper right-hand panel of Table 6, show a negative effect of imposing an accommodations requirement and essentially no effect of imposing an antidiscrimination prohibition. The robustness of our findings to variation on employer-size coverage thresholds provides further support for our earlier suggestion that "protection without accommodation" and "ADA-like" states are not differentially affected by the ADA's enactment because of a difference in the general civil-rights orientation, as such a difference would probably correlate with the aggressiveness of small-employer coverage.

3.4.3 Robustness to Alternative Measures of Disability Benefits

Around the time the ADA was enacted, the generosity of federal disability benefits was increasing substantially (Bound and Waidmann 2002). Higher disability benefit levels provide an independent ground for reduced disabled employment because higher benefit levels reduce disabled individuals' need or perhaps desire for wage-based income. The increase in the number of disabled individuals receiving disability benefits, as well as the decrease in weeks worked for individuals with disabilities, over the 1990s are apparent from the summary statistics reported in Table 3.

While all of the regressions reported thus far contain controls for disability benefits, the shift in federal disability benefit levels is actually of less concern for our study than for prior studies that compare overall disabled employment outcomes before and after the ADA's enactment (DeLeire 2000, 2003; Acemoglu and Angrist 2001). Our DDD framework examines disabled employment levels in one group of states relative to disabled employment levels in other state groups, and thus changes in federal benefits levels are unlikely to matter for our results.

Still, changes in federal disability benefits could affect our analysis if for some reason the resulting changes in disabled individuals' need or desire for wage income (and thus their work incentives) differed systematically across our three state groups. Autor and Duggan (2003), for instance, note that work incentives depend on the relationship between disability benefit levels and wages, and thus states experiencing smaller wage increases (or larger wage declines) would tend to have more individuals receiving federal disability benefits at any

[28] Only one state, Delaware, had a threshold of greater than 15 employees.

given level of these benefits. If wage levels across states were for some reason correlated with pre-ADA state-law disability discrimination regimes, then the effects of changes in federal disability benefits generosity (mediated through the Autor-Duggan mechanism) might be captured by, and therefore bias, our results.

The regressions reported thus far control for disability benefits receipt using a dummy variable for whether individuals received federal disability benefits through either the Disability Insurance (DI) program or the Supplemental Security Income (SSI) program.[29] Because such individual disability benefit receipt may be a consequence as well as a cause of employment status, this approach raises potential endogeneity issues, although Acemoglu and Angrist (2001) find empirically that approaches taking account of these endogeneity issues yield results similar to those that do not. Nonetheless, we explore alternatives to the use of individual disability benefit receipt by including, in lieu of such individual information, state-level DI and SSI applications and receipts in the population from Social Security Administration records.

The lower panel of Table 6 reports the results of re-estimating the basic fixed-effects specification from the middle panel of Table 5 using, respectively, the percent of the state population receiving disability benefits interacted with the disability status dummy variable (left-hand panel) and the percent of the state population applying for disability benefits interacted with the disability status dummy variable (right-hand panel).[30] Our results for the effects of imposing a reasonable accommodations requirement versus a traditional antidiscrimination prohibition are, once again, consistent with our benchmark estimates from Table 5.

3.4.4 Robustness to Variation in Economic Environment

Our basic specification controls for variation in states' economic environments by including state unemployment rate and the interaction of disability status and state unemployment rate. However, because unobservable economic variation across states might be influencing

[29]The CPS provides information on receipt of benefits from the Old-Age, Survivors, and Disability Insurance (OASDI) and SSI programs. However, because our sample does not include individuals 59 and older, OASDI benefits should be exclusively from the DI program.

[30]Including state fixed effects means we cannot separately identify main effects of the new disability benefits variables.

our results, we re-estimated the basic fixed-effects specification from the middle panel of Table 5 without state unemployment rate information; if removing these controls does not affect our results, unobservable economic differences are unlikely to be playing an important role. The results, shown in the upper panel of Table A6, are again similar to those in the benchmark specification reported in Table 5.

3.4.5 Composition of the Disabled Group

With the use of a survey-based disability measure, such as the CPS measure used in our analysis, comes the possibility of law-driven changes in the composition of the group answering the survey question affirmatively. If the group of individuals identifying themselves as disabled in the CPS changed in shape or size as a result of the ADA's enactment, then measured changes in disabled employment levels between the pre- and post-ADA periods may capture differences in the composition of the group answering "yes" to the survey question rather than employer-side effects of the new legal regime. Kruse and Schur (2003, p.49), for instance, find evidence from Survey of Income and Program Participation data of higher numbers of individuals reporting severe limitations in 1993 than in 1991, and it would not be surprising for those with severe limitations to have lower chances of employment.

An important advantage of the framework we employ is that changes over time in the shape or size of the group of individuals identifying themselves as disabled in response to the CPS question will not affect our analysis unless these changes vary with the pre-ADA legal regime of the state in which an individual lives. While nationwide changes certainly seem plausible, state-varying changes are less likely. This is not to say that they are inconceivable, however; one obvious possibility is that legal reform may make disability more socially accepted and thus lead more people to identify themselves as disabled. If this were the case, then changes in disability identification with the ADA could be more substantial in states in which the ADA was a more significant innovation.

For state-group specific changes in individuals' identification as disabled to confound our results, one of three things would have to be the case. First, significant innovation through the ADA in the "protection without accommodation" and "no protection" states might for some reason have made individuals with worse employment prospects than those who identified as disabled prior to the ADA more likely to identify themselves as disabled. This might produce an apparent disemployment effect for disabled people in the states in which the ADA was a significant innovation. However, if anything it would be those closest to the line between disability and nondisability, and thus those with relatively good employment prospects, who would switch to identifying as disabled in a state in which the ADA was a significant innovation (say because the innovation made disability more socially acceptable). Put another way, it is difficult to tell a story in which those who are severely limited switch from answering the CPS question "no" to answering it "yes" when the ADA constitutes a significant innovation in their state, while those who are less severely limited do not exhibit a similar change.[31]

The second possibility for a confounding effect on our results is that individuals with reasonably good employment prospects became less likely in the wake of significant legal innovation through the ADA in "protection without accommodation" or "no protection" states to identify themselves as disabled, precisely because the reform helped them to obtain and retain jobs (see Kirchner 1986, p.83). Again, this type of compositional shift might produce an apparent disemployment effect for disabled persons in states in which the ADA was a significant innovation. If the legal reform did lead to a decline in the reporting of disability, then the proportion of disabled individuals should either shrink or grow more slowly in "protection without accommodation" and "no protection" states than in "ADA-like" states.

Figure 1 graphs the proportion of disabled individuals over time across our three state groups from 1988 to 1998, while Figure 2 presents corresponding fourth-order polynomial trend lines.[32] Over the 1988-1998 period, disability rates did not decline in states in which the ADA was a significant innovation relative to "ADA-like" states. Measuring changes between 1988 and the post-ADA years 1990-1993, Figure 1 shows that the proportion of individuals answering the CPS disability question affirmatively increased more in "no pro-

[31]Even if it is the case (as suggested by Kruse and Schur 2003, p.49) that the proportion of disabled individuals who report severe disabilities has increased on a nationwide basis in the post-ADA years, it is unclear how this effect could plausibly be correlated with the degree to which the ADA was a significant legal innovation in a given state.

[32]As suggested in Section 3.3 above, the difference between the "no protection" group and the other two state groups in terms of the absolute levels of disabled individuals seems to reflect in large part the higher concentration of individuals with disabilities in the southern states.

tection" states than in "ADA-like" states for year pairings 1988-1991 and 1988-1993, while this proportion increased more in "ADA-like" states than in "no protection" states for year pairings 1988-1990 and 1988-1992. Figure 1 also shows that over 1990 and 1991 (relative to 1988) the proportion of individuals with disabilities increased more in "ADA-like" states (implying slower growth in "protection without accommodation" states). But the same trend was apparent in 1989, even before the ADA was enacted. In 1992 and 1993 (the other years in which we find a negative effect of the ADA on disabled employment in "protection without accommodation" states relative to "ADA-like" states), by contrast, Figure 1 shows that disability rates are virtually identical across the two state groups, as they also were in the 1988 start year.

A final possible source of composition bias, suggested by Autor and Duggan (2003), is that states in which the ADA was a significant legal innovation also for some reason happened to be states that were experiencing smaller wage increases or larger wage declines around the time of the ADA's enactment; as described above, individuals in such states would both tend to be more likely to identify as disabled (to get federal disability benefits) and tend to be more likely not to be employed (again to be eligible for federal disability benefits). This effect could produce a spurious correlation between the ADA's enactment and employment effects across states if for some reason state-level wage changes were correlated with pre-ADA state disability discrimination regimes. The regression results reported above (Tables 5 and 6), however, show that our results are robust to a range of controls for disability benefits receipts and applications.

In sum, although we cannot entirely rule out an effect on our results of changes in the composition of the group responding affirmatively to the CPS disability question, our basic finding of a negative near-term effect of the ADA's reasonable accommodations requirement on disabled employment does not appear to be driven by such changes.

3.4.6 Preexisting State-Group Specific Trends in Disabled Employment

Our conclusion that declining disabled employment in "protection without accommodation" states relative to "ADA-like" states in the near-term post-ADA period reflects the ADA's imposition of a reasonable accommodations requirement rests on the premise that the observed pattern of effects did not predate the ADA "experiment." Figure 3 graphs disabled employment trends in the "protection without accommodation" and "ADA-like" states from 1987, the first observation year in which the general disability status question was asked in the CPS, to 1992, the last year in which disabled employment in the two state groups moved in significantly different ways.[33] These data paint a reassuring picture in which disabled employment moved roughly in tandem across the two state groups prior to 1990 and then diverged markedly in 1990-1992 (see Figure 3). A longer pre-ADA window would obviously be preferable, but the available data point to a genuine break in trend upon the ADA's enactment.

3.5 Conclusion

This paper uses pre-ADA state-law variation to disaggregate the disabled employment effects of the two central provisions of the ADA, its reasonable accommodations requirement and its traditional antidiscrimination prohibition with associated firing costs. Our effort to disaggregate the ADA's effects in this way reflects a desire to evaluate policy reforms more tailored and more politically realistic than the broadscale recommendation that the ADA be abandoned (see DeLeire 2003). However, our empirical approach ultimately yielded a more profound challenge to the existing literature that suggests ongoing negative employment effects of the ADA. Our results indicate that while the ADA's reasonable accommodations requirement had a significant negative effect on disabled employment in the near-term after the ADA's enactment, the law may well have had no causal link to the declines in disabled employment through much of the 1990s.

[33] As the results reported above suggest, convergence between the two state groups began in 1993. As the earlier results from Table 4 also show, nondisabled employment was relatively stable in both state groups over the relevant period. so Figure 3 focuses on disabled employment.

3.6 References

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Table 1: Pre-ADA State Laws Prohibiting Disability Discrimination – "Protection Without Accommodation" States

In the states listed in this table, pre-ADA statutory or judicial law imposed traditional antidiscrimination prohibitions but no reasonable accommodations requirements on private employers:

	Statutory Section(s)	Traditional Antidiscrimination Prohibition – Date Adopted	Reasonable Accommodations Requirement – Date Adopted	Employer-Size Threshold for Coverage[a]
Alaska	18.80.220(a)(1)*	1969†	n/a	1
California	Govt. 12940(a), 12994*	1973†	n/a‡	5
Connecticut	46a-60(a)(1)*	1973†	n/a	3
Florida	760.10(1)*	1977	n/a	15
Georgia	34-6A-4(a)	1981	n/a	15
Hawaii	378-2(1)	1975†	n/a	1
Illinois	68:1-103(Q), 2-102(A)*	1971	n/a	15
Indiana	22-9-1-3(l)	1975	n/a	6
Kansas	44-1009(a)(1)	1974	n/a	4
Kentucky	207.150(1)	1976	n/a	8
Maine	5:4572(1)(A)	1973	n/a	1
Maryland	49B:16(a)*	1974	n/a	15
Michigan	37.1102(2), 1202(1)	1976†	n/a‡	4
Missouri	213.055.1(1)*	1978	n/a	6
Montana	49-2-303(a), 49-4-101*	1974†	n/a	1
Nebraska	48-1104	1973	n/a	15
Nevada	613.330(1)	1971†	n/a	15
New Hampshire	354-A:8(I)	1975	n/a	6
New Jersey	10:5-4.1, -12(a), -29.1	1972†	n/a	1
New York	Exec. 296(1)(a)	1974	n/a	4
North Dakota	14-02.4-03	1983	n/a	10
Ohio	4112.02(A)	1976	n/a	4
Oklahoma	25:1302(A)	1981	n/a	15
South Carolina	43-33-530	1983	n/a	1
South Dakota	20-13-10, 23.7, 23.8	1986	n/a‡	1
Tennessee	8-50-103(a)	1976†	n/a	n/a[b]
Texas	Civ. Art. 5221k:5.01*	1975†	n/a‡	15
Utah	34-35-6(1)(a)(i)	1979	n/a	15
West Virginia	5-11-9(a)(1)*	1981	n/a	12

* Original statutory section differed from statutory section in effect immediately prior to the ADA: See Table A1 for details.
† Substantive amendment(s) subsequent to adoption: See Table A1 for details.
‡ Potential ambiguity over the existence of a reasonable accommodations requirement: See Table A2 for legal description and effect on our results of alternative characterizations.
[a] Number of employees, as of 1989. The column lists the number of individuals a firm had to employ before it was subject to coverage by the state's disability discrimination law.
[b] Neither statutory nor judicial law provided an employer-size threshold for coverage.

Table 2: Pre-ADA State Laws Prohibiting Disability Discrimination – "ADA-Like" States

In the states listed in this table, pre-ADA statutory or judicial law imposed both traditional antidiscrimination prohibitions and reasonable accommodations requirements on private employers:

	Statutory Section(s)	Traditional Antidiscrimination Prohibition – Date Adopted	Reasonable Accommodations Requirement – Date Adopted	Employer-Size Threshold for Coverage[a]
Arizona	41-1463(B)	1985	1985	15
Colorado	24-34-402(1)(a)*	1977	1977[b]	1
Delaware	19:723(b), 724(a), 724(e)(2)	1988	1988	20
Idaho	67-5909(1)	1988	1988	10
Iowa	601A.6(1)(a)*	1972	1987‡, [c]	4
Louisiana	46:2254(A), (C)	1980	1980	15
Massachusetts	151B:4(16)*	1972†	1983	6
Minnesota	363.03:1(2), (6)	1973	1983†, [d]	1
New Mexico	28-1-7(A), (J)*	1973†	1983	4
North Carolina	168A-4, 5(a)*	1973†	1985	15
Oregon	659.425(1)	1973†	1979	6
Pennsylvania	43:955(a), (b)	1974	1985[c]	4
Rhode Island	28-5-7(1)*	1973†	1986	4
Vermont	21:495(a)(1), 495d(6)*	1973	1981	1
Virginia	51.5-41(A), (C)*	1975	1985	n/a[e]
Washington	49.60.180	1973	1978[c]	8
Wisconsin	111.321, 322(1), 34(1)(b)*	1965†	1981‡	1
Wyoming	27-9-105(a), (d)	1985	1985	2

* Original statutory section differed from statutory section in effect immediately prior to the ADA: See Table A1 for details.
† Substantive amendment(s) subsequent to adoption: See Table A1 for details.
‡ Potential ambiguity over the timing of adoption of a reasonable accommodations requirement: See Table A2 for details.
[a] Number of employees, as of 1989. The column lists the number of individuals a firm had to employ before it was subject to coverage by the state's disability discrimination law.
[b] Statutory language is somewhat ambiguous but is clarified by an administrative regulation, 3 CCR 708-1, Rule 60.2(C).
[c] Judicial interpretation: Iowa—Cerro Gordo County Care Facility v. Iowa Civil Rights Comm'n, 401 N.W.2d 192 (Iowa 1987); Pennsylvania—Jenks v. Avco Corp., 940 A.2d 912 (Pa. Super. 1985); Washington—Holland v. Boeing Co., 583 P.2d 621 (Wash. 1978). In the case of Washington, the 1978 decision briefly mentions the existence of an administrative regulation requiring reasonable accommodations, but this regulation plays only a minor role in the court's opinion.
[d] Applicable only to employers with 50 or more employees.
[e] Neither statutory nor judicial law provided an employer-size threshold for coverage.

Table 3: Descriptive Statistics

Nondisabled	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998
Age	36.9	37.1	37.2	37.3	37.5	37.6	37.8	38.1	38.2	38.5	38.6
White	87.8%	87.1%	87.0%	86.7%	86.1%	85.5%	84.3%	86.4%	86.3%	86.2%	86.3%
Post-High School	44.5%	45.5%	45.8%	50.0%	51.8%	53.6%	54.3%	54.0%	54.4%	55.0%	55.6%
Working	87.5%	87.4%	87.2%	87.1%	86.8%	87.0%	87.3%	87.6%	88.0%	88.3%	88.4%
Weeks Worked	40.8	40.8	40.6	40.4	40.3	40.5	40.9	41.3	41.6	42.0	42.2
Weekly Wage	$397	$422	$433	$447	$462	$481	$508	$561	$572	$595	$625
SSI/DI	1.32%	1.39%	1.38%	1.31%	1.54%	1.61%	1.66%	1.59%	1.60%	1.43%	1.27%
Obs.	67,907	74,616	74,980	74,192	73,525	70,999	70,686	61,300	62,088	62,338	63,137

Disabled	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998
Age	42.5	42.5	42.2	42.2	42.1	42.6	43.0	43.2	43.2	43.8	44.0
White	83.7%	83.5%	82.6%	83.5%	83.6%	81.0%	79.5%	80.8%	80.9%	82.0%	80.6%
Post-High School	23.3%	23.6%	25.7%	28.5%	30.8%	31.3%	32.1%	32.9%	34.0%	34.9%	35.6%
Working	45.0%	45.8%	43.4%	43.2%	42.7%	40.8%	40.8%	39.7%	40.2%	37.4%	36.6%
Weeks Worked	16.4	17.2	15.9	15.9	15.8	15.1	14.7	14.8	15.0	14.0	13.8
Weekly Wage	$307	$308	$314	$335	$317	$347	$376	$435	$396	$366	$469
SSI/DI	32.98%	31.67%	34.33%	35.29%	37.66%	38.55%	38.10%	40.24%	42.41%	45.07%	44.68%
Obs.	4,396	4,884	5,025	5,100	5,311	5,307	5,336	4,680	4,775	4,655	4,579

Notes: Descriptive statistics are unweighted. Data are for individuals aged 21-58 and are reported by observation year (the year preceding the survey). "SSI/DI" reflects the percentage of individuals who received federal disability benefits through either the Disability Insurance (DI) program or the Supplemental Security Income (SSI) program.

Table 4a: Means Analysis by State, Time, and Disability Status: "Protection Without Accommodation" States versus "ADA-Like" States

	88-89	90-91	91-92	92-93	93-94	94-95	95-96	96-97	97-98
Protection Without Accommodation States									
Disabled workers	16.25	14.90	14.67	14.34	14.43	14.47	14.33	14.00	13.48
	(0.29)	(0.27)	(0.26)	(0.26)	(0.26)	(0.27)	(0.28)	(0.27)	(0.27)
	[5,680]	[6,101]	[6,298]	[6,486]	[6,521]	[6,058]	[5,677]	[5,741]	[5,684]
Time Diff.		-1.35	-1.57	-1.91	-1.82	-1.78	-1.92	-2.25	-2.77
		(0.39)	(0.39)	(0.39)	(0.39)	(0.39)	(0.40)	(0.40)	(0.40)
Nondisabled workers	40.68	40.45	40.23	40.19	40.48	40.94	41.26	41.64	42.02
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
	[88,630]	[93,449]	[92,335]	[89,407]	[86,473]	[80,671]	[75,509]	[75,872]	[76,201]
Time Diff.		-0.23	-0.46	-0.49	-0.20	0.25	0.58	0.96	1.34
		(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Group-Time Diff.		-1.12	-1.12	-1.42	-1.62	-2.03	-2.50	-3.21	-4.10
		(0.40)	(0.40)	(0.40)	(0.40)	(0.41)	(0.41)	(0.41)	(0.41)
ADA-Like States									
Disabled workers	18.22	19.05	19.26	17.79	16.51	15.79	15.97	15.98	14.65
	(0.43)	(0.42)	(0.42)	(0.42)	(0.40)	(0.40)	(0.42)	(0.43)	(0.43)
	[2,680]	[2,881]	[2,923]	[2,868]	[2,928]	[2,862]	[2,677]	[2,617]	[2,471]
Time Diff.		0.83	1.04	-0.43	-1.71	-2.43	-2.25	-2.24	-3.57
		(0.60)	(0.60)	(0.60)	(0.59)	(0.59)	(0.60)	(0.61)	(0.61)
Nondisabled workers	41.50	41.47	41.48	41.75	42.04	42.36	42.55	42.81	43.04
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.10)	(0.09)	(0.09)
	[40,849]	[40,727]	[40,238]	[38,892]	[39,111]	[36,821]	[33,504]	[33,818]	[34,207]
Time Diff.		-0.03	-0.02	0.25	0.54	0.86	1.05	1.31	1.54
		(0.13)	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)
Group-Time Diff.		0.86	1.05	-0.68	-2.25	-3.29	-3.30	-3.55	-5.11
		(0.61)	(0.61)	(0.61)	(0.60)	(0.60)	(0.61)	(0.62)	(0.62)
Group-Time-State Diff.		**-1.98**	**-2.17**	**-0.74**	**0.62**	**1.26**	**0.80**	**0.35**	**1.00**
		(0.73)	(0.73)	(0.73)	(0.72)	(0.72)	(0.74)	(0.74)	(0.74)

Notes: Means reflect average weeks worked by state group, disability status (disabled versus nondisabled), and time period. All estimates are weighted using CPS survey weights. Standard errors are in parentheses beneath mean estimates, and the numbers of observations in each state group-time period-disability status cell are in square brackets below mean estimates. See Tables 1 and 2 and the text for the states in each group.

Table 4b: Means Analysis by State, Time, and Disability Status: "No Protection" States versus "Protection Without Accommodation" States

	88-89	90-91	91-92	92-93	93-94	94-95	95-96	96-97	97-98
No Protection States									
Disabled workers	12.19 (0.88) [473]	10.40 (0.81) [533]	11.39 (0.85) [532]	11.11 (0.84) [524]	10.57 (0.86) [477]	11.58 (0.93) [450]	11.41 (0.90) [466]	10.64 (0.91) [450]	10.70 (0.93) [432]
Time Diff.		-1.79 (1.19)	-0.80 (1.23)	-1.08 (1.22)	-1.62 (1.23)	-0.61 (1.28)	-0.78 (1.26)	-1.55 (1.26)	-1.49 (1.28)
Nondisabled workers	39.89 (0.27) [5,222]	39.96 (0.26) [5,337]	40.22 (0.26) [5,289]	40.34 (0.27) [5,085]	40.75 (0.27) [4,816]	41.01 (0.28) [4,592]	41.55 (0.28) [4,499]	41.94 (0.28) [4,446]	42.03 (0.28) [4,309]
Time Diff.		0.07 (0.38)	0.32 (0.38)	0.45 (0.38)	0.85 (0.38)	1.12 (0.38)	1.66 (0.38)	2.05 (0.39)	2.14 (0.39)
Group-Time Diff.		-1.85 (1.25)	-1.12 (1.28)	-1.53 (1.27)	-2.47 (1.29)	-1.73 (1.33)	-2.44 (1.32)	-3.60 (1.32)	-3.63 (1.34)
Protection Without Accommodation States									
Disabled workers	16.25 (0.29) [5,680]	14.90 (0.27) [6,101]	14.67 (0.26) [6,298]	14.34 (0.26) [6,486]	14.43 (0.26) [6,521]	14.47 (0.27) [6,058]	14.33 (0.28) [5,677]	14.00 (0.27) [5,741]	13.48 (0.27) [5,684]
Time Diff.		-1.35 (0.39)	-1.57 (0.39)	-1.91 (0.39)	-1.82 (0.39)	-1.78 (0.39)	-1.92 (0.40)	-2.25 (0.40)	-2.77 (0.40)
Nondisabled workers	40.68 (0.06) [88,630]	40.45 (0.06) [93,449]	40.23 (0.06) [92,335]	40.19 (0.06) [89,407]	40.48 (0.07) [86,473]	40.94 (0.07) [80,671]	41.26 (0.07) [75,509]	41.64 (0.07) [75,872]	42.02 (0.07) [76,201]
Time Diff.		-0.23 (0.09)	-0.46 (0.09)	-0.49 (0.09)	-0.20 (0.09)	0.25 (0.09)	0.58 (0.09)	0.96 (0.09)	1.34 (0.09)
Group-Time Diff.		-1.12 (0.40)	-1.12 (0.40)	-1.42 (0.40)	-1.62 (0.40)	-2.03 (0.41)	-2.50 (0.41)	-3.21 (0.41)	-4.10 (0.41)
Group-Time-State Diff.		**-0.73** (1.31)	**-0.01** (1.34)	**-0.11** (1.33)	**-0.85** (1.35)	**0.30** (1.39)	**0.06** (1.38)	**-0.40** (1.38)	**0.48** (1.40)

Notes: Means reflect average weeks worked by state group, disability status (disabled versus nondisabled), and time period. All estimates are weighted using CPS survey weights. Standard errors are in parentheses beneath mean estimates, and the numbers of observations in each state group-time period-disability status cell are in square brackets below mean estimates. See Tables 1 and 2 and the text for the states in each group.

Table 5: Basic Regression Results

Basic Specification	90-91	91-92	92-93	93-94	94-95	95-96	96-97	97-98
Coeff. on ADA*DIS	1.23	1.67	0.51	-0.99	-1.92	-1.46	-1.23	-2.67
	(0.63)	(0.78)	(0.51)	(0.52)	(0.49)	(0.61)	(0.70)	(0.58)
	[0.93]	[0.96]	[0.98]	[0.88]	[0.82]	[0.92]	[0.93]	[0.88]
Coeff. on ADA*DIS*LP	-2.14	-2.51	-1.54	-0.08	0.70	0.12	-0.60	0.02
	(0.69)	(0.73)	(0.68)	(0.76)	(0.68)	(0.79)	(0.88)	(0.74)
	[1.07]	[1.04]	[1.10]	[1.04]	[0.98]	[1.05]	[1.07]	[0.99]
Coeff. on ADA*DIS*NP	-2.63	-1.96	-1.12	-0.34	1.57	0.44	-0.56	0.24
	(0.86)	(0.94)	(0.87)	(1.22)	(1.44)	(1.19)	(0.87)	(0.68)
	[1.21]	[1.24]	[1.23]	[1.19]	[1.33]	[1.30]	[1.05]	[1.07]
Coeff. on ADA*DIS*NP – Coeff. on ADA*DIS*LP	-0.49	0.55	0.42	-0.25	0.87	0.32	0.04	0.22
	(0.71)	(0.76)	(0.96)	(1.24)	(1.38)	(1.11)	(0.80)	(0.68)
	[0.96]	[0.97]	[0.98]	[1.00]	[1.17]	[1.07]	[0.76]	[0.82]
Specification with State, Year, and State*Year Fixed Effects	**90-91**	**91-92**	**92-93**	**93-94**	**94-95**	**95-96**	**96-97**	**97-98**
Coeff. on ADA*DIS	1.33	1.72	0.55	-0.96	-1.89	-1.41	-1.19	-2.68
	(0.59)	(0.72)	(0.43)	(0.48)	(0.48)	(0.58)	(0.70)	(0.50)
	[0.77]	[0.82]	[0.80]	[0.69]	[0.67]	[0.77]	[0.79]	[0.74]
Coeff. on ADA*DIS*LP	-2.22	-2.56	-1.54	-0.06	0.76	0.14	-0.61	0.11
	(0.62)	(0.63)	(0.50)	(0.64)	(0.57)	(0.69)	(0.78)	(0.58)
	[0.84]	[0.85]	[0.88]	[0.81]	[0.79]	[0.88]	[0.89]	[0.81]
Coeff. on ADA*DIS*NP	-2.76	-2.03	-1.17	-0.36	1.59	0.42	-0.55	0.27
	(0.66)	(0.82)	(0.79)	(1.18)	(1.49)	(1.07)	(0.78)	(0.52)
	[0.96]	[1.07]	[1.05]	[1.06]	[1.22]	[1.21]	[0.90]	[0.90]
Coeff. on ADA*DIS*NP – Coeff. on ADA*DIS*LP	-0.53	0.53	0.37	-0.30	0.83	0.29	0.06	0.16
	(0.44)	(0.60)	(0.85)	(1.17)	(1.42)	(0.95)	(0.57)	(0.48)
	[0.69]	[0.82]	[0.88]	[0.94]	[1.11]	[1.05]	[0.65]	[0.68]
Specification Omitting State-Law Variables, but Including State, Year, and State*Year Fixed Effects	**90-91**	**91-92**	**92-93**	**93-94**	**94-95**	**95-96**	**96-97**	**97-98**
Coeff. on ADA*DIS	-0.14	0.24	-0.29	-0.94	-1.29	-1.29	-1.65	-2.65
	(0.37)	(0.53)	(0.46)	(0.36)	(0.28)	(0.32)	(0.33)	(0.33)
	[0.39]	[0.48]	[0.47]	[0.37]	[0.34]	[0.36]	[0.38]	[0.36]
No. of Observations	292,562	291,149	286,796	283,860	274,988	265,866	266,478	266,838

Notes: The dependent variable is weeks worked per year. The pre-ADA period is 1988-1989. The post-ADA period is as stated. Robust standard errors clustered on state-disability interactions are in parentheses below coefficient estimates, and robust standard errors clustered on state-disability-year interactions are in square brackets below coefficient estimates. All regressions are OLS regressions, employ CPS survey weights, and include the individual control variables listed in the text plus controls for state unemployment rate (in regressions without state, year, and state*year fixed effects) and the interaction of disability and state unemployment rate. See equation (3.1) for further details.

Table 6: Robustness Checks

(1) Sample Includes Only Observations from Pre-1980 Enactors	90-91	91-92	92-93	(2) Sample Includes Only Observations from High-Employer-Size-Threshold States	90-91	91-92	92-93
Coeff. on ADA*DIS	2.95	2.65	-0.17	**Coeff. on ADA*DIS**	0.48	1.14	0.99
	(0.74)	(1.99)	(1.00)		(1.25)	(0.97)	(0.70)
	[1.29]	[1.75]	[2.17]		[0.93]	[0.73]	[0.87]
Coeff. on ADA*DIS*LP	-3.80	-3.54	-1.07	**Coeff. on ADA*DIS*LP**	-1.40	-2.47	-1.93
	(0.85)	(2.02)	(1.12)		(1.39)	(1.10)	(1.11)
	[1.37]	[1.79]	[2.25]		[1.16]	[0.99]	[1.16]
Coeff. on ADA*DIS*NP	-4.38	-2.97	-0.43	**Coeff. on ADA*DIS*NP**	-1.93	-1.52	-1.62
	(0.79)	(2.03)	(1.20)		(1.30)	(1.07)	(0.98)
	[1.41]	[1.88]	[2.27]		[1.11]	[1.02]	[1.11]
Coeff. on ADA*DIS*NP – Coeff. on ADA*DIS*LP	-0.58	0.58	0.64	**Coeff. on ADA*DIS*NP – Coeff. on ADA*DIS*LP**	-0.53	0.95	0.31
	(0.48)	(0.66)	(0.96)		(0.71)	(0.75)	(1.11)
	[0.70]	[0.86]	[0.97]		[0.93]	[1.00]	[1.04]
No. of Observations	192,885	191,815	188,956	**No. of Observations**	93,566	92,864	91,464

(3) Specification with State-Level Disability Benefits Receipt Information	90-91	91-92	92-93	(4) Specification with State-Level Disability Benefits Application Information	90-91	91-92	92-93
Coeff. on ADA*DIS	1.00	1.28	0.68	**Coeff. on ADA*DIS**	1.45	1.95	1.23
	(0.70)	(0.81)	(0.59)		(0.69)	(0.81)	(0.68)
	[0.78]	[0.85]	[0.85]		[0.79]	[0.86]	[0.88]
Coeff. on ADA*DIS*LP	-2.03	-2.41	-1.43	**Coeff. on ADA*DIS*LP**	-1.78	-1.99	-0.92
	(0.76)	(0.74)	(0.54)		(0.76)	(0.74)	(0.62)
	[0.88]	[0.90]	[0.91]		[0.87]	[0.88]	[0.91]
Coeff. on ADA*DIS*NP	-2.54	-1.55	-0.01	**Coeff. on ADA*DIS*NP**	-1.82	-0.68	0.93
	(0.75)	(0.81)	(0.79)		(0.81)	(0.79)	(1.01)
	[1.61]	[1.67]	[1.38]		[1.87]	[1.89]	[1.67]
Coeff. on ADA*DIS*NP – Coeff. on ADA*DIS*LP	-0.50	0.86	1.42	**Coeff. on ADA*DIS*NP – Coeff. on ADA*DIS*LP**	-0.03	1.31	1.85
	(0.43)	(0.53)	(0.74)		(0.55)	(0.67)	(1.00)
	[1.46]	[1.52]	[1.23]		[1.73]	[1.77]	[1.57]
No. of Observations	292,562	291,149	286,796	**No. of Observations**	292,562	291,149	286,796

Notes: The dependent variable is weeks worked per year. The pre-ADA period is 1988-1989. The post-ADA period is as stated. Robust standard errors clustered on state-disability interactions are in parentheses below coefficient estimates, and robust standard errors clustered on state-disability-year interactions are in square brackets below coefficient estimates. All regressions are OLS regressions, employ CPS survey weights, and include state, year, and state*year fixed effects. Control variables are as stated in Table 5. In the upper panel of the present table, columns (1)-(3) use observations from states in which the state-level pre-ADA disability discrimination regime was in place prior to 1980; columns (4)-(6) use observations from states with ADA-like employer-size thresholds. See Tables 1 and 2 for details on employer-size thresholds; observations from Tennessee and Virginia, whose pre-ADA statutory and judicial law did not specify an employer-size threshold, are not included in the samples used in column (4)-(6). In the lower panel, columns (1)-(3) replace individual receipt of disability benefits with the percent of the state population receiving disability benefits interacted with disability status; columns (4)-(6) replace individual receipt of disability benefits with the percent of the state population applying for disability benefits interacted with disability status. Fixed effects preclude inclusion of the percent of the state population receiving or applying for disability benefits alone.

Figure 1: Percentage of Individuals Reporting Disability By State Group

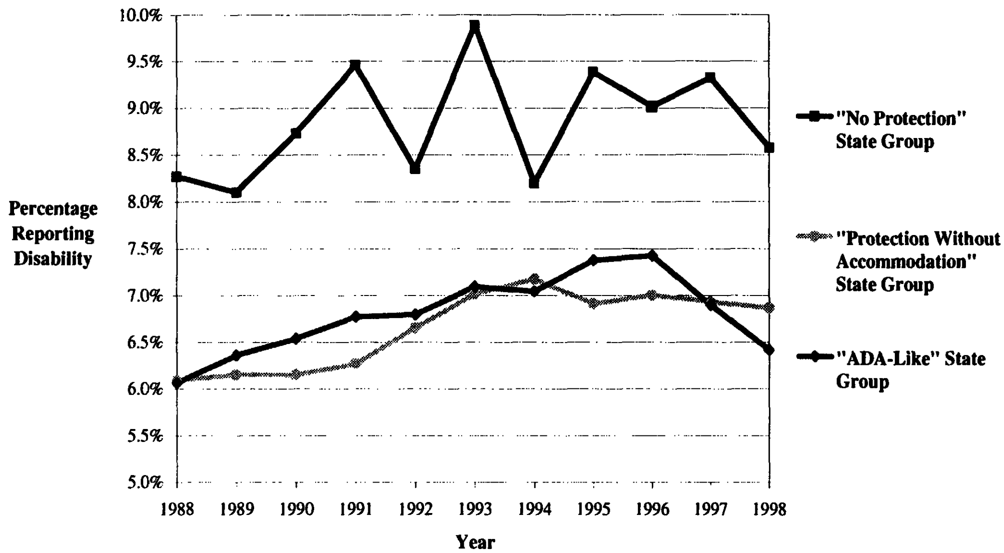
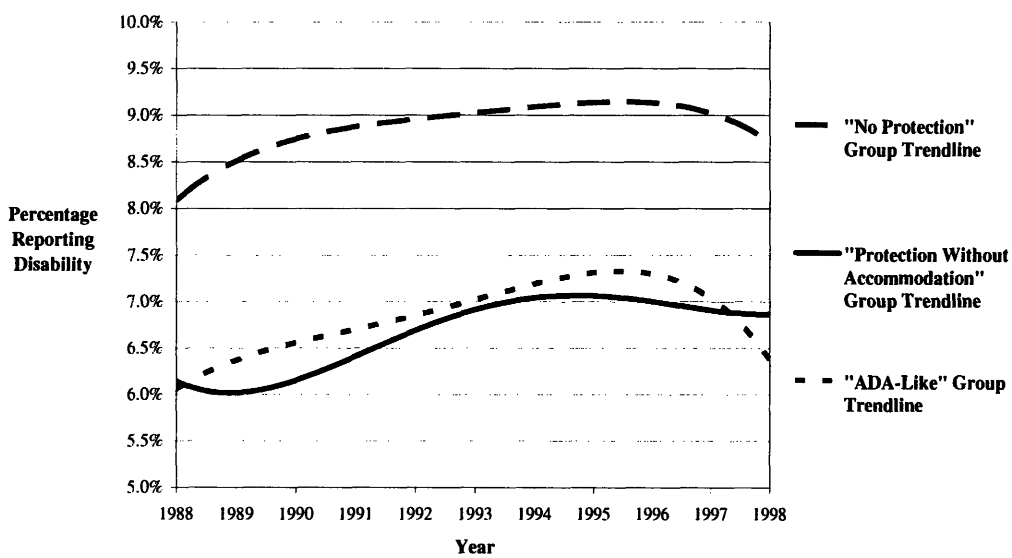


Figure 2: Percentage of Individuals Reporting Disability By State Group – Trendlines



Note: All trendlines are fourth-order polynomials.

Figure 3: "ADA-Like" and "Protection Without Accommodation" State Disabled Employment Trends

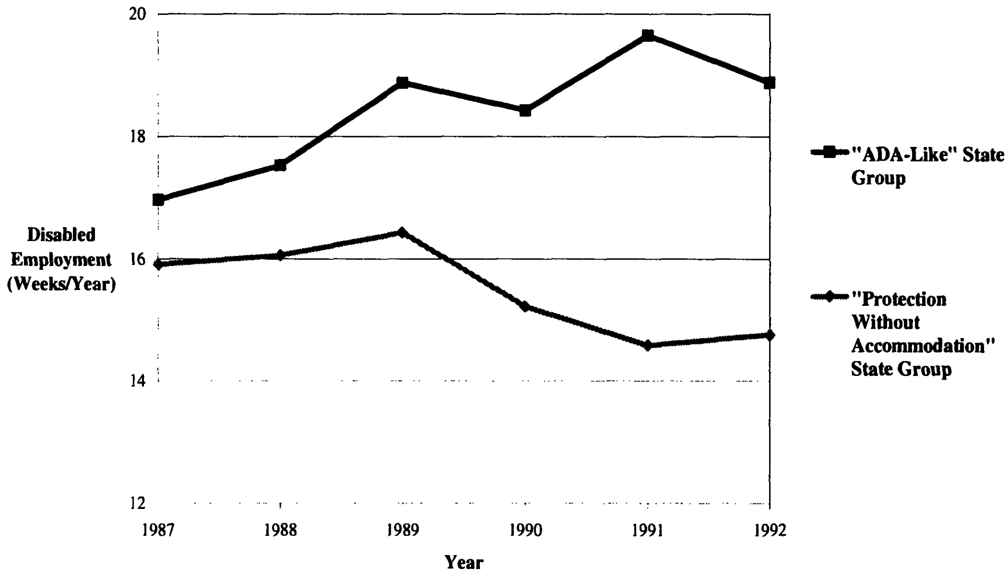
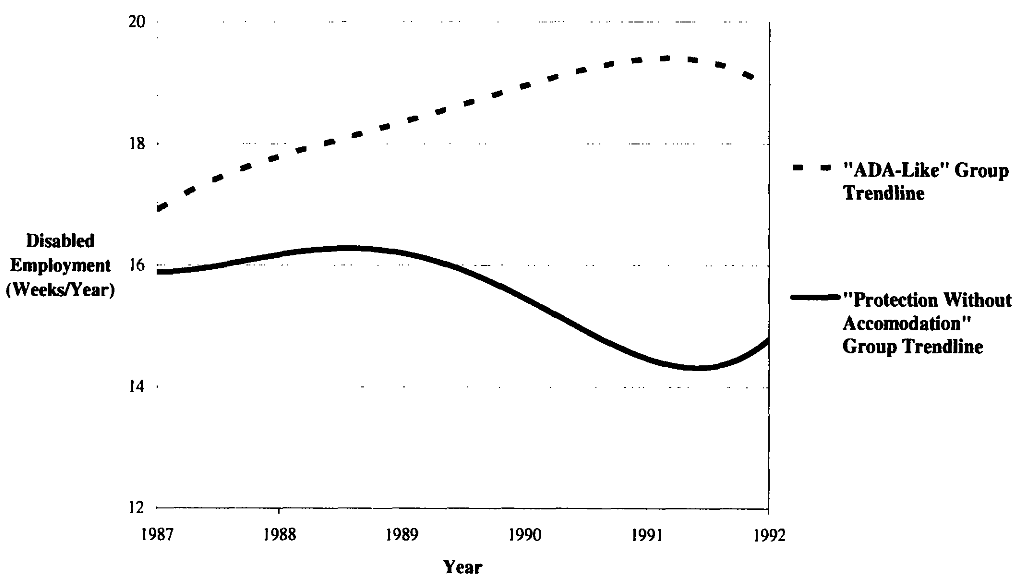


Figure 4: "ADA-Like" and "Protection Without Accommodation" State Disabled Employment Trendlines



Note: All trendlines are fourth-order polynomials.

Table A1: Pre-ADA State Laws Prohibiting Disability Discrimination by Private Employers – Original Statutory Sections and Substantive Pre-ADA Amendments

Protection Without Accommodation States	Original Statutory Section(s)	Substantive Pre-ADA Amendments	
		Date of Amendment(s)	Nature of Amendment(s)
Alaska	18.80.220(1)	1987	Broadened coverage to mental as well as physical disability
California	Lab. 1420(a)	1975	Broadened coverage to "medical conditions" as well as physical disability
Connecticut	31-126(a)	1978; 1979	Broadened coverage to mental retardation (1978) and mental disorders (1979) as well as physical disability
Florida	13-261(1) (later 23.167(1))	n/a	n/a
Hawaii	n/a	1981	Broadened scope of liability
Illinois	38:65-23(1) (later 48-853-3(a))	n/a	n/a
Maryland	49B:19(a)	n/a	n/a
Michigan	n/a	1980	Broadened scope of liability
Missouri	296.020(1)	n/a	n/a
Montana	1974 S.L. ch. 77, sec. 3 (later 64-306(a))	1975	Broadened coverage to mental as well as physical disability and broadened scope of liability
Nevada	n/a	1973; 1981	Added protection for use of guide dogs (1973); broadened coverage to "aural" as well as "physical" and "visual" handicaps (1981)
New Jersey	n/a	1978	Broadened coverage to mental as well as physical disability and broadened scope of liability
Tennessee	n/a	1986; 1987	Broadened scope of liability (1986); corrected omission of private employers from 1986 amendment (1987)
Texas	Art. 4419e(f) (later Hum. Res. Code 121.003(f))	1983	Broadened scope of liability
West Virginia	5-11-9(a)	n/a	n/a

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Table A1 (Continued): Pre-ADA State Laws Prohibiting Disability Discrimination by Private Employers – Original Statutory Sections and Substantive Pre-ADA Amendments

"ADA-Like" States	Original Statutory Section(s)	Substantive Pre-ADA Amendments	
		Date of Amendment(s)	Nature of Amendment(s)
Colorado	24-34-306(1)(a)	n/a	n/a
Iowa	105A.7(1)(a)	n/a	n/a
Massachusetts	149:24K	1983	Broadened scope of liability
Minnesota	n/a	1987; 1989	Altered definition of "undue hardship" (1987); refined definition of reasonable accommodations (1989)
New Mexico	4-33-7(A)	1987	Broadened coverage to "medical conditions" as well as physical and mental disabilities
North Carolina	168-6	1985	Made various revisions to liability provisions
Oregon	n/a	1979	Broadened scope of liability
Rhode Island	28-5-7(A)	1981	Broadened coverage to mental as well as physical disability
Vermont	21:498(a)	n/a	n/a
Virginia	40.1-28.7 (later 51.01-41(A),(C))	n/a	n/a
Wisconsin	111.32(5)(a), 111.36(4) (later 111.325)	1967; 1975; 1981	Rephrased and clarified prohibitions (1967, 1975, 1981)

Notes: The original statutory section often differs from the source reported in Tables 1 and 2 because states frequently renumbered their statutes in this period. The substantive amendments reported in this table are amendments to pre-ADA statutory sections imposing traditional antidiscrimination prohibitions or reasonable accommodations requirements, and do not reflect changes in other statutory sections of states' disability discrimination laws.

Table A2: Effects of Alternative Characterizations of Pre-ADA State Laws Prohibiting Disability Discrimination by Private Employers

State	Legal Description	Effect on Results for Post-ADA Years 90-91, 91-92, and 92-93
California	Prior to 1981, Cal. Govt. § 12994 expressly stated that accommodations were not required. In 1981, the section was amended to provide that an employer shall not be required "to make any accommodation for an employee who has a physical handicap that would produce undue hardship to the employer." There is no pre-ADA caselaw indicating whether the 1981 amendment was meant to impose affirmatively a requirement of reasonable accommodations unless such accommodations would be an undue hardship.	Categorizing California as an "ADA-like" state, rather than a "protection without accommodation" state, does not alter the basic pattern of our results. Our estimates of β_{11} (the effect of imposing a reasonable accommodations requirement) are somewhat smaller in magnitude and slightly less precise. Our estimates of $\beta_{12} - \beta_{11}$ (the effect of imposing a traditional antidiscrimination provision) remain insignificant in all years.
Delaware	Law prohibiting disability discrimination by private employers was not enacted until 1988.	Categorizing Delaware as a "no protection" state, rather than an "ADA-like" state, has virtually no effect on our estimates of β_{11} (the effect of imposing a reasonable accommodations requirement). Our estimates of $\beta_{12} - \beta_{11}$ (the effect of imposing a traditional antidiscrimination provision) remain insignificant in all years.
Idaho	Law prohibiting disability discrimination by private employers was not enacted until 1988, and the statutory language is somewhat ambiguous as to the existence of a reasonable accommodations requirement.	Categorizing Idaho as a "no protection" state or a "protection without accommodation" state, rather than an "ADA-like" state, has virtually no effect on our estimates of β_{11} (the effect of imposing a reasonable accommodations requirement). Our estimates of $\beta_{12} - \beta_{11}$ (the effect of imposing a traditional antidiscrimination provision) remain insignificant in all years.
Iowa	The Iowa Supreme Court adopted a reasonable accommodations requirement in 1987 as a freestanding interpretation of the statutory language, but earlier courts had mentioned and applied administrative regulations requiring reasonable accommodations.	This change cannot affect our results (because the timing of state law adoption does not enter into our categorization of states).
Michigan	Limited accommodation provision, not expressly requiring reasonable accommodations, was adopted in 1976. Administrative decisional law, summarized in Wardlow v. Great Lakes Express Co., 339 N.W.2d 670 (Mich. Ct. App. 1983), adopted a reasonable accommodations requirement, but in 1986 the Michigan Supreme Court, in Carr v. General Motors Corp., 389 N.W.2d 686 (Mich. 1986), adopted a conception of the Michigan statute inconsistent with the administrative decisional law's reasonable accommodations requirement.	Categorizing Michigan as an "ADA-like" state, rather than a "protection without accommodation" state, reduces the absolute magnitude of our estimates of β_{11} (the effect of imposing a reasonable accommodations requirement) by about 10%, while the precision of the estimates generally improves. Our estimates of $\beta_{12} - \beta_{11}$ (the effect of imposing a traditional antidiscrimination provision) remain insignificant in all years.

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Table A2 (continued): Effects of Alternative Characterizations of Pre-ADA State Laws Prohibiting Disability Discrimination by Private Employers

State	Legal Description	Effect on Results for Post-ADA Years 90-91, 91-92, and 92-93
Pennsylvania	Pennsylvania's reasonable accommodations requirement was imposed only by an intermediate court, rather than by the state's highest court.	Categorizing Pennsylvania as a "protection without accommodation" state, rather than an "ADA-like" state, does not alter the basic pattern of our results. Our estimates of β_{11} (the effect of imposing a reasonable accommodations requirement) are somewhat smaller in magnitude and slightly less precise. Our estimates of $\beta_{12} - \beta_{11}$ (the effect of imposing a traditional antidiscrimination provision) remain insignificant in all years.
South Dakota	Limited accommodation provision, not expressly requiring reasonable accommodations, was adopted in 1986.	Categorizing South Dakota as an "ADA-like" state, rather than a "protection without accommodation" state, improves the precision of our estimates of β_{11} (the effect of imposing a reasonable accommodations requirement), with little effect on their magnitudes. Our estimates of $\beta_{12} - \beta_{11}$ (the effect of imposing a traditional antidiscrimination provision) remain insignificant in all years.
Texas	Remedy provisions refer to reasonable accommodations (Civ. Art. 5221k:6.01(d) and 7.01(f)).	Categorizing Texas as an "ADA-like" state, rather than a "protection without accommodation" state, does not alter the basic pattern of our results. Our estimates of β_{11} (the effect of imposing a reasonable accommodations requirement) are somewhat smaller in magnitude and also somewhat less precise. Our estimates of $\beta_{12} - \beta_{11}$ (the effect of imposing a traditional antidiscrimination provision) remain insignificant in all years.
Wisconsin	The Wisconsin legislature adopted a reasonable accommodations requirement in 1981, but in the preceding period some Wisconsin lower courts judicially imposed a reasonable accommodations requirement (e.g., Teggatz v. Labor & Industry Review Comm'n, 1978 WL 3436 (Cir. Ct. Wisc.)), while others did not impose such a requirement (e.g., Samens v. Labor & Industry Review Comm'n, 1981 WL 11474 (Cir. Ct. Wisc.)).	This change cannot affect our results (because the timing of state law adoption does not enter into our categorization of states).

Table A3: Basic Regression Results – Overall State-Group Effects

Specification with ADA*DIS*AD	90-91	91-92	92-93	93-94	94-95	95-96	96-97	97-98
Coeff. on ADA*DIS*LP	-0.91	-0.84	-1.03	-1.08	-1.21	-1.34	-1.84	-2.64
	(0.41)	(0.55)	(0.65)	(0.60)	(0.46)	(0.51)	(0.53)	(0.51)
	[0.57]	[0.57]	[0.63]	[0.60]	[0.54]	[0.53]	[0.56]	[0.56]
Coeff. on ADA*DIS*NP	-1.40	-0.29	-0.61	-1.33	-0.34	-1.02	-1.80	-2.42
	(0.58)	(0.57)	(0.70)	(1.08)	(1.34)	(1.02)	(0.61)	(0.59)
	[0.77]	[0.80]	[0.74]	[0.79]	[1.05]	[0.96]	[0.62]	[0.75]
Coeff. on ADA*DIS*AD	1.23	1.67	0.51	-0.99	-1.92	-1.46	-1.23	-2.67
	(0.63)	(0.78)	(0.51)	(0.52)	(0.49)	(0.61)	(0.70)	(0.58)
	[0.93]	[0.96]	[0.98]	[0.88]	[0.82]	[0.92]	[0.93]	[0.88]
Specification with ADA*DIS*AD and State, Year, and State*Year Fixed Effects	**90-91**	**91-92**	**92-93**	**93-94**	**94-95**	**95-96**	**96-97**	**97-98**
Coeff. on ADA*DIS*LP	-0.90	-0.84	-0.99	-1.02	-1.13	-1.27	-1.79	-2.58
	(0.33)	(0.44)	(0.53)	(0.47)	(0.32)	(0.39)	(0.37)	(0.39)
	[0.39]	[0.46]	[0.54]	[0.47]	[0.41]	[0.42]	[0.44]	[0.40]
Coeff. on ADA*DIS*NP	-1.43	-0.31	-0.63	-1.32	-0.30	-0.99	-1.74	-2.41
	(0.29)	(0.44)	(0.65)	(1.07)	(1.40)	(0.89)	(0.46)	(0.44)
	[0.57]	[0.69]	[0.69]	[0.80]	[1.02]	[0.96]	[0.54]	[0.63]
Coeff. on ADA*DIS*AD	1.32	1.72	0.55	-0.96	-1.89	-1.41	-1.19	-2.68
	(0.59)	(0.72)	(0.43)	(0.48)	(0.48)	(0.58)	(0.70)	(0.50)
	[0.77]	[0.82]	[0.80]	[0.69]	[0.67]	[0.77]	[0.79]	[0.74]
No. of Observations	292,562	291,149	286,796	283,860	274,988	265,866	266,478	266,838

Notes: Results duplicate the first two panels of Table 5 except that state-group effects are overall effects for each state group rather than effects in the "protection without accommodation" and "no protection" groups relative to "ADA-like" states. ADA*DIS*AD replaces ADA*DIS in all regressions in this table. The dependent variable is weeks worked per year. The pre-ADA period is 1988-1989. The post-ADA period is as stated. Robust standard errors clustered on state-disability interactions are in parentheses below coefficient estimates, and robust standard errors clustered on state-disability-year interactions are in square brackets below coefficient estimates. All regressions are OLS regressions and employ CPS survey weights. Control variables are as stated in Table 5.

Table A4: Basic Regression Results, Probit Specification, Coefficients

Probit Specification	90-91	91-92	92-93	93-94	94-95	95-96	96-97	97-98
Coeff. on ADA*DIS	0.063	0.056	-0.035	-0.075	-0.107	-0.108	-0.108	-0.211
	(0.043)	(0.048)	(0.041)	(0.042)	(0.040)	(0.054)	(0.061)	(0.049)
	[0.069]	[0.070]	[0.075]	[0.071]	[0.070]	[0.076]	[0.079]	[0.076]
Coeff. on ADA*DIS*LP	-0.137	-0.134	-0.061	-0.012	0.017	0.015	0.001	0.045
	(0.052)	(0.060)	(0.061)	(0.065)	(0.056)	(0.070)	(0.081)	(0.066)
	[0.086]	[0.084]	[0.090]	[0.088]	[0.088]	[0.092]	[0.094]	[0.089]
Coeff. on ADA*DIS*NP	-0.202	-0.156	-0.062	-0.048	0.028	-0.002	-0.039	-0.014
	(0.069)	(0.048)	(0.109)	(0.145)	(0.115)	(0.098)	(0.125)	(0.129)
	[0.128]	[0.131]	[0.127]	[0.117]	[0.107]	[0.112]	[0.119]	[0.120]
Coeff. on ADA*DIS*NP – Coeff. on ADA*DIS*LP	-0.065	-0.022	0.000	-0.037	0.011	-0.017	0.040	-0.058
	(0.064)	(0.050)	(0.114)	(0.146)	(0.113)	(0.097)	(0.131)	(0.132)
	[0.120]	[0.124]	[0.117]	[0.107]	[0.095]	[0.100]	[0.106]	[0.106]
Probit Specification with State, Year, and State*Year Fixed Effects	**90-91**	**91-92**	**92-93**	**93-94**	**94-95**	**95-96**	**96-97**	**97-98**
Coeff. on ADA*DIS	0.068	0.059	-0.035	-0.076	-0.104	-0.103	-0.104	-0.211
	(0.041)	(0.047)	(0.033)	(0.032)	(0.038)	(0.054)	(0.064)	(0.039)
	[0.049]	[0.048]	[0.049]	[0.046]	[0.045]	[0.053]	[0.058]	[0.053]
Coeff. on ADA*DIS*LP	-0.144	-0.140	-0.059	-0.009	0.018	0.013	0.000	0.052
	(0.047)	(0.051)	(0.042)	(0.046)	(0.046)	(0.062)	(0.074)	(0.050)
	[0.057]	[0.055]	[0.058]	[0.057]	[0.055]	[0.062]	[0.066]	[0.059]
Coeff. on ADA*DIS*NP	-0.214	-0.163	-0.061	-0.040	0.035	-0.002	-0.038	-0.010
	(0.080)	(0.046)	(0.088)	(0.106)	(0.101)	(0.087)	(0.091)	(0.086)
	[0.090]	[0.095]	[0.087]	[0.080]	[0.075]	[0.077]	[0.078]	[0.080]
Coeff. on ADA*DIS*NP – Coeff. on ADA*DIS*LP	-0.070	-0.024	-0.002	-0.031	0.017	-0.015	-0.038	-0.062
	(0.074)	(0.035)	(0.089)	(0.105)	(0.095)	(0.076)	(0.080)	(0.084)
	[0.082]	[0.089]	[0.082]	[0.075]	[0.067]	[0.067]	[0.065]	[0.068]
Probit Specification Omitting State-Law Variables, but Including State, Year, and State*Year Fixed Effects	**90-91**	**91-92**	**92-93**	**93-94**	**94-95**	**95-96**	**96-97**	**97-98**
Coeff. on ADA*DIS	-0.029	-0.025	-0.067	-0.080	-0.090	-0.094	-0.106	-0.176
	(0.027)	(0.033)	(0.028)	(0.024)	(0.021)	(0.027)	(0.033)	(0.030)
	[0.027]	[0.031]	[0.031]	[0.026]	[0.025]	[0.027]	[0.028]	[0.027]
No. of Observations	292,562	291,149	286,796	283,860	274,988	265,866	266,478	266,838

Notes: The dependent variable is a dummy variable that equals one if the person was employed during the year (i.e., the number of weeks worked was greater than zero). The pre-ADA period is 1988-1989. The post-ADA period is as stated. Robust standard errors clustered on state-disability interactions are in parentheses below coefficient estimates, and robust standard errors clustered on state-disability-year interactions are in square brackets below coefficient estimates. All regressions are probit regressions, employ CPS survey weights, and include the individual control variables listed in the text plus controls for state unemployment rate (in regressions without state, year, and state*year fixed effects) and the interaction of disability and state unemployment rate. See equation (3.1) for further details.

Table A5: Alternative Fixed Effects Specifications

Specification with State, State*Year, and State*Disabled Fixed Effects	90-91	91-92	92-93	93-94	94-95	95-96	96-97	97-98
Coeff. on ADA*DIS	1.07	1.26	-0.06	-1.13	-1.84	-1.32	-1.02	-2.32
	(0.55)	(0.66)	(0.42)	(0.52)	(0.51)	(0.60)	(0.72)	(0.48)
	[0.46]	[0.51]	[0.59]	[0.50]	[0.51]	[0.56]	[0.54]	[0.50]
Coeff. on ADA*DIS*LP	-2.10	-2.53	-1.74	-0.21	0.63	0.05	-0.72	0.00
	(0.63)	(0.64)	(0.51)	(0.62)	(0.56)	(0.69)	(0.79)	(0.58)
	[0.53]	[0.55]	[0.67]	[0.60]	[0.59]	[0.63]	[0.62]	[0.55]
Coeff. on ADA*DIS*NP	-2.48	-1.65	-0.50	0.03	1.86	0.75	0.12	0.83
	(0.63)	(0.79)	(0.82)	(1.20)	(1.44)	(1.08)	(0.83)	(0.64)
	[0.72]	[0.80]	[0.89]	[0.90]	[0.94]	[1.01]	[0.75]	[0.77]
Coeff. on ADA*DIS*NP – Coeff. on ADA*DIS*LP	-0.38	0.89	1.24	0.24	1.23	0.70	0.84	0.83
	(0.44)	(0.60)	(0.88)	(1.14)	(1.32)	(0.88)	(0.53)	(0.47)
	[0.61]	[0.72]	[0.85]	[0.84]	[0.85]	[0.89]	[0.61]	[0.66]

Specification with State, Year, State*Year, and Year*Disabled Fixed Effects	90-91	91-92	92-93	93-94	94-95	95-96	96-97	97-98
Coeff. on ADA*DIS*LP	-2.69	-3.04	-1.98	-0.53	0.30	-0.35	-1.00	-0.25
	(0.81)	(0.81)	(0.64)	(0.64)	(0.55)	(0.78)	(0.89)	(0.70)
	[0.62]	[0.64]	[0.68]	[0.61]	[0.56]	[0.68]	[0.69]	[0.58]
Coeff. on ADA*DIS*NP	-4.66	-3.99	-3.12	-2.27	-0.18	-1.42	-2.17	-1.02
	(0.75)	(0.87)	(0.69)	(1.04)	(1.38)	(1.04)	(0.81)	(0.61)
	[0.75]	[0.84]	[0.75]	[0.90]	[1.09]	[1.10]	[0.77]	[0.66]
Coeff. on ADA*DIS*NP – Coeff. on ADA*DIS*LP	-1.96	-0.94	-1.14	-1.75	-0.48	-1.07	-1.17	-0.76
	(0.47)	(0.57)	(0.56)	(0.99)	(1.33)	(0.89)	(0.36)	(0.32)
	[0.57]	[0.67]	[0.61]	[0.85]	[1.04]	[0.99]	[0.55]	[0.48]
No. of Observations	292,562	291,149	286,796	283,860	274,988	265,866	266,478	266,838

Notes: The dependent variable is weeks worked per year. The pre-ADA period is 1988-1989. The post-ADA period is as stated. Robust standard errors clustered on state-disability interactions are in parentheses below coefficient estimates, and robust standard errors clustered on state-disability-year interactions are in square brackets below coefficient estimates. All regressions are OLS regressions, employ CPS survey weights, and include the fixed effects specified in the table. Control variables are stated in Table 5. In the upper panel of this table, the fixed effects preclude the estimation of the coefficients on ADA, LP, NP, ADA*LP, ADA*NP, DIS*LP, and DIS*NP from the basic specification in equation (1). In the lower panel, the fixed effects preclude the estimation of the coefficients on ADA, LP, NP, ADA*LP, ADA*NP, DIS*LP, DIS*NP, and ADA*DIS from this specification.

Table A6: Additional Robustness Checks

(1) Sample Includes Only Observations from Southern States	90-91	91-92	92-93	93-94	94-95	95-96	96-97	97-98
Coeff. on ADA*DIS	0.32	0.93	0.06	-0.81	-1.56	-0.74	-1.05	-4.08
	(1.02)	(0.69)	(0.57)	(0.56)	(0.49)	(0.83)	(0.98)	(1.05)
	[1.49]	[1.06]	[0.29]	[0.23]	[0.64]	[0.28]	[1.10]	[1.36]
Coeff. on ADA*DIS*LP	-1.33	-1.75	-1.78	-0.85	0.59	0.04	0.86	3.50
	(1.26)	(1.06)	(1.17)	(1.07)	(0.99)	(1.42)	(1.52)	(1.46)
	[1.59]	[1.18]	[0.93]	[1.29]	[0.73]	[0.85]	[1.35]	[1.49]
Coeff. on ADA*DIS*NP	-1.68	-1.21	-0.69	-0.43	1.69	-0.05	-0.79	1.70
	(1.18)	(0.99)	(0.90)	(0.97)	(1.03)	(1.28)	(1.01)	(1.15)
	[1.53]	[1.17]	[0.76]	[1.07]	[1.43]	[0.83]	[1.05]	[1.33]
Coeff. on ADA*DIS*NP – Coeff. on ADA*DIS*LP	-0.35	0.54	1.09	0.42	1.10	-0.10	-1.64	-1.81
	(0.94)	(1.05)	(1.22)	(1.21)	(1.30)	(1.47)	(1.29)	(1.21)
	[0.58]	[0.65]	[1.12]	[1.64]	[1.34]	[1.13]	[0.84]	[0.72]
No. of Observations	41,793	41,786	41,157	40,813	39,169	37,612	37,832	37,593

(2) Specification Omitting State-Level Unemployment Variables	90-91	91-92	92-93	93-94	94-95	95-96	96-97	97-98
Coeff. on ADA*DIS	1.34	1.72	0.57	-0.98	-1.91	-1.41	-1.14	-2.44
	(0.54)	(0.57)	(0.31)	(0.46)	(0.49)	(0.59)	(0.70)	(0.44)
	[0.75]	[0.76]	[0.76]	[0.68]	[0.68]	[0.77]	[0.78]	[0.72]
Coeff. on ADA*DIS*LP	-2.22	-2.56	-1.53	-0.08	0.73	0.13	-0.63	0.05
	(0.63)	(0.64)	(0.51)	(0.64)	(0.58)	(0.70)	(0.77)	(0.54)
	[0.85]	[0.85]	[0.88]	[0.81]	[0.79]	[0.87]	[0.89]	[0.81]
Coeff. on ADA*DIS*NP	-2.77	-2.03	-1.20	-0.30	1.76	0.55	-0.34	0.65
	(0.61)	(0.72)	(0.72)	(1.16)	(1.45)	(1.00)	(0.73)	(0.48)
	[0.95]	[1.03]	[1.03]	[1.05]	[1.20]	[1.19]	[0.89]	[0.90]
Coeff. on ADA*DIS*NP – Coeff. on ADA*DIS*LP	-0.55	-0.53	0.34	-0.22	1.02	0.42	0.29	0.60
	(0.43)	(0.53)	(0.76)	(1.15)	(1.40)	(0.90)	(0.42)	(0.38)
	[0.68]	[0.78]	[0.81]	[0.91]	[1.07]	[1.00]	[0.60]	[0.66]
No. of Observations	292,562	291,149	286,796	283,860	274,988	265,866	266,478	266,838

Notes: The dependent variable is weeks worked per year. The pre-ADA period is 1988-1989. The post-ADA period is as stated. Robust standard errors clustered on state-disability interactions are in parentheses below coefficient estimates, and robust standard errors clustered on state-disability-year interactions are in square brackets below coefficient estimates. All regressions are OLS regressions, employ CPS survey weights, and include state, year, and state*year fixed effects. Control variables are as stated in Table 5 minus state unemployment rate and the interaction of disability and state unemployment rate in the lower panel. The southern states used in the upper panel are as stated in the text.

Table A7: Fixed Effects Specifications with Time-Varying Covariates

Basic Specification	90-91	91-92	92-93	93-94	94-95	95-96	96-97	97-98
Coeff. on ADA*DIS	0.97	2.16	1.90	0.14	-0.51	-0.42	0.98	-0.08
	(1.97)	(2.14)	(1.81)	(1.99)	(1.78)	(1.68)	(1.91)	(1.97)
	[2.45]	[2.32]	[2.19]	[2.15]	[1.91]	[1.90]	[2.03]	[2.02]
Coeff. on ADA*DIS*LP	-2.31	-2.74	-1.71	-0.27	0.47	-0.19	-0.53	0.34
	(0.63)	(0.70)	(0.69)	(0.80)	(0.72)	(0.80)	(0.93)	(0.76)
	[0.95]	[0.96]	[1.05]	[1.02]	[0.92]	[1.01]	[1.03]	[0.94]
Coeff. on ADA*DIS*NP	-3.66	-3.39	-2.68	-1.86	0.14	-0.59	-1.14	-0.09
	(0.86)	(0.91)	(0.92)	(1.33)	(1.56)	(1.27)	(0.95)	(0.76)
	[1.21]	[1.20]	[1.15]	[1.18]	[1.37]	[1.35]	[1.12]	[1.06]
Coeff. on ADA*DIS*NP – Coeff. on ADA*DIS*LP	-1.35	-0.65	-0.97	-1.59	-0.33	0.40	-0.61	-0.43
	(0.74)	(0.72)	(0.93)	(1.27)	(1.40)	(1.09)	(0.66)	(0.61)
	[0.95]	[0.90]	[0.79]	[0.90]	[1.16]	[1.07]	[0.73]	[0.72]

Specification with State, Year, and State*Year Fixed Effects	90-91	91-92	92-93	93-94	94-95	95-96	96-97	97-98
Coeff. on ADA*DIS	-0.58	-0.01	-0.63	-1.96	-2.27	-2.53	-0.74	-0.79
	(1.76)	(1.91)	(1.59)	(1.60)	(1.39)	(1.50)	(1.76)	(1.70)
	[2.14]	[2.03]	[1.91]	[1.75]	[1.58]	[1.68]	[1.81]	[1.76]
Coeff. on ADA*DIS*LP	-2.39	-2.74	-1.67	-0.18	0.69	-0.02	-0.53	0.34
	(0.61)	(0.63)	(0.54)	(0.70)	(0.59)	(0.70)	(0.83)	(0.62)
	[0.83]	[0.84]	[0.89]	[0.84]	[0.79]	[0.88]	[0.90]	[0.81]
Coeff. on ADA*DIS*NP	-3.45	-2.62	-1.65	-0.76	1.41	0.15	-0.52	0.81
	(0.70)	(0.80)	(0.80)	(1.25)	(1.55)	(1.11)	(0.83)	(0.66)
	[0.99]	[1.05]	[0.99]	[1.10]	[1.27]	[1.27]	[1.01]	[0.92]
Coeff. on ADA*DIS*NP – Coeff. on ADA*DIS*LP	-1.06	0.12	0.03	-0.58	0.72	0.17	0.01	0.47
	(0.49)	(0.59)	(0.81)	(1.19)	(1.45)	(0.98)	(0.52)	(0.52)
	[0.76]	[0.82]	[0.76]	[0.95]	[1.14]	[1.09]	[0.73]	[0.66]
No. of Observations	292,562	291,149	286,796	283,860	274,988	265,866	266,478	266,838

Notes: The dependent variable is weeks worked per year. The pre-ADA period is 1988-1989. The post-ADA period is as stated. Robust standard errors clustered on state-disability interactions are in parentheses below coefficient estimates, and robust standard errors clustered on state-disability-year interactions are in square brackets below coefficient estimates. All regressions are OLS regressions, employ CPS survey weights, and include the fixed effects specified in the table. Control variables are stated in Table 5, but in each panel control variables are free to vary for each included year.