Contractor Pay and Public Service Quality: Evidence from Court-Appointed Defense Attorneys

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Abstract

Governments increasingly outsource public services to private actors, but these contracted workers often face a "public-work pay disparity," earning less for their public-service work than for work in the private market. This paper focuses on the public provision of defense counsel outsourced to private attorneys and explores how changes in attorney pay affect defendant case outcomes and recidivism. In North Carolina, private attorneys provide indigent criminal defense at a set hourly rate. A 2011 reform cut that rate by 27 percent for misdemeanors, 20 percent for lower-level felonies, and 7 percent for higher-level felonies, while leaving in-house public defenders' salaries unchanged. Defendants assigned court-appointed counsel after the cut were 5.5 percentage points less likely to have their misdemeanor case dismissed and 4.7 percentage points more likely to be incarcerated, with smaller and statistically insignificant effects for felony cases. I estimate the value-added of court-appointed attorneys to better understand the size of this effect and find that the pay reduction effect on case outcomes is equivalent to a 1.23 standard deviation decrease in attorney case dismissal quality. Approximately 80 percent of the effect on misdemeanor case outcomes remains after including attorney fixed effects, suggesting the pay reduction primarily reduced attorney effort rather than changing which attorneys participate. My results raise concerns about equity for low-income defendants and suggest that the lack of financial resources may exacerbate broader social and economic inequalities.

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In recent years, the US and many OECD countries have increasingly outsourced the provision of public services such as waste collection, prisons, and legal defense to private contractors (The Council of State Governments, 2003; OECD, 2023).[1] For many of these contracted services, the government sets the price the contractors receive for providing public services, which is often less than the pay the contractors would earn for comparable work in the private market.[2] Consequently, outsourcing public services raises a first-order question for the effectiveness of government: how does the pay of contractors of public services affect the quality of public services? The answer to this question may be particularly important for addressing broader social and economic inequalities.

The importance of pay for private contractors may be most evident in the public provision of defense counsel. The 1963 landmark case *Gideon v. Wainwright* established the right to legal representation and required states to provide public counsel to low-income individuals in criminal cases (i.e., indigent defense counsel). Indigent defense services have become a vital public service, with nearly 80% of felony defendants relying on public counsel because they lack the financial resources to hire a private defense attorney (Harlow, 2000). In most of the country, the guaranteed right to counsel is outsourced to private attorneys who are paid by the government by the hour or the case, also known as the court-appointed system.[3] However, the significant gap between public and private defense pay has led to concerns about the quality of public defense for low-income individuals. Yet, little is known about how the price paid to contracted attorneys affects the quality of public defense.

In this paper, I examine how the compensation of publicly contracted private attorneys affects the outcomes of criminal cases and defendants' future interactions with the criminal

[1] Appendix Figure A1 shows the increasing share of US expenditures devoted to outsourced public goods and services provided by private suppliers between 1970 and 2020.

[2] This disparity is part of the broader conversation about compensation for public sector workers. In 2024, the Federal Salary Council estimated a 28% pay disparity between government and private sector workers (Federal Salary Council, 2024). The Economic Policy Institute recently reported that the public-sector pay disparity has widened since the COVID-19 pandemic (Morrissey and Sherer, 2024). Gittleman and Pierce (2012) show that the size of this disparity is sensitive to the inclusion of fringe benefits and the choice of controls in the analysis.

[3] The court-appointed system serves as the predominant or an auxiliary form of indigent defense in roughly 30 U.S. states (Beeman and Buetow, 2023).

justice system using administrative data on criminal court cases in North Carolina from 2008-2016. I exploit an unanticipated 2011 funding change that decreased the hourly compensation for court-appointed attorneys by 27% for misdemeanors, 20% for lower-level felonies, and 7% for higher-level felonies. At the same time, the funding change did not affect the salaries of public defenders, the state employees who also represent low-income defendants. Using the change in hourly pay, I implement a difference-in-differences strategy that compares case outcomes for defendants assigned court-appointed attorneys with those for defendants assigned public defenders.

I find that the pay cut adversely affected case outcomes. Across all case types, the likelihood of case dismissal decreased by 4.2 percentage points (7 percent), and the likelihood of incarceration rose by 3.7 percentage points (12 percent). The negative effect on case outcomes is concentrated among misdemeanor defendants. For misdemeanor defendants, whose attorneys faced the largest pay reduction, the probability of case dismissal fell by 5.5 percentage points (9 percent), and the probability of incarceration rose by 4.7 percentage points (21 percent).

When I investigate whether the less favorable case outcomes for defendants are from changes in attorney effort or compositional shift, I find that about 80 percent of the misdemeanor effect remains after including attorney fixed effects, suggesting that the majority of the adverse effects may be driven by changes in attorney effort. Importantly, the pay cut may have affected how attorneys practice more generally, such as requiring some attorneys to eliminate office staff. To investigate this pathway, I exploit the fact that many court-appointed attorneys maintain private clients besides their court-appointed cases to estimate the effect of the pay cut on appointed case outcomes using retained cases as the comparison group. I find that defendants assigned a court-appointed attorney experienced worse case outcomes after the pay cut, compared with private clients of the same attorney, suggesting that the pay cut changed how much effort attorneys exerted on their court-appointed cases. These effort changes are captured in changes to case-handling outcomes, where I find that

misdemeanor defendants were 5 percentage points (17 percent) more likely to plead guilty after the pay cut.

I present a simple conceptual framework to investigate how attorney compensation may affect attorney quality, incorporating the idea that wages affect effort. I then leverage the quasi-random assignment of attorneys to defendants to investigate how attorney quality affects case outcomes and how the wage effects differ by attorney quality. In most North Carolina counties, court-appointed attorneys are assigned to low-income defendant cases following a standard rotational procedure, creating randomness in case assignment. The randomness in case assignment overcomes issues with endogenous sorting and allows for the construction of attorney quality measures. Using value-added methods, I estimate measures of attorney quality based on the likelihood of case dismissal and incarceration.[4]

I find that quality, measured using case dismissal and incarceration, varies substantially among court-appointed attorneys and that it meaningfully affects case outcomes. For example, a one-standard-deviation increase in case dismissal quality increases the likelihood of a defendant's case being dismissed by 4.3 percentage points. I estimate attorney quality before and after the pay reduction, and find that both the dispersion of quality and the effect of a one-standard-deviation increase in quality are similar before and after the pay decrease.

Examining pay-cut effects by pre-cut attorney quality and private caseload reveals that the effort reduction was similar across the attorney quality distribution and across the distribution of private-practice size for misdemeanor cases. Scaling the effects of the pay cut by the attorney quality measures suggests the effect of the pay cut on misdemeanor case dismissals was equivalent to a 1.23 standard deviation decrease in attorney dismissal quality. Together, these results suggest that the pay cut resulted in lower-quality public defense for misdemeanor defendants.

Back-of-the-envelope estimates suggest that the pay decrease shifted state spending on court-appointed attorneys to county spending on longer jail sentences. The state saved at

[4]Examples of value-added methods include Rockoff (2004); Jacob, Lefgren and Sims (2010); Chetty, Friedman and Rockoff (2014*b*); Petek and Pope (2023).

least $3.7 million a year from the lower hourly rate, while county jail costs rose by roughly $2 million, leaving net savings of approximately $34 per case. Counties spent roughly 54 cents on jail for every dollar the state saved. These estimates do not take into account the adverse effect of case outcomes on defendants. I estimate that the pay reduction lowered social welfare if a lost dismissal cost a defendant more than $22,862, using a lower bound for the social cost of crime, or more than $84,269 at an upper bound.

Using earnings penalties estimated for criminal records in comparable settings suggests that dismissal costs are below the lower bound, suggesting that the pay reduction may have raised social welfare. At the same time, the costs fell disproportionately on low-income defendants, with larger effects for Black and Hispanic defendants than for white defendants. These results raise concerns about equity for low-income defendants and suggest that the lack of financial resources for public defense may exacerbate broader social and economic inequalities.

This paper's main contribution is to estimate how the price of an outsourced public service affects its quality. A large literature examines whether governments should provide public services in-house or contract them out (Hart, Shleifer and Vishny, 1997; Levin and Tadelis, 2010), comparing contracted and public provision in education (Abdulkadiroğlu, Pathak and Walters, 2018), prisons (Mukherjee, 2021), juvenile correctional facilities (Bayer and Pozen, 2005), and employment services (Bennmarker, Grönqvist and Öckert, 2013). In public defense, defendants assigned public defenders receive more favorable case outcomes than defendants assigned court-appointed attorneys (Anderson and Heaton, 2012; Roach, 2014; Shem-Tov, 2022). This public defense focuses on differences in how to provide public defense, but not on the response of quality to the price the government pays. Prior work on attorney compensation has found that attorneys exert less effort when paid a flat rate than an hourly rate (Schwall, 2016; Lee, 2021). In the study most comparable to this paper, Roach (2017) finds that case outcomes improved in higher-poverty New York counties after an increase in the hourly rate, using county poverty as a proxy for assignment to public

counsel. In contrast to Roach, I observe the type of counsel on every case, allowing me to better understand how the compensation of contracted attorneys affects the quality of public defense.

My results also separate the effect of pay on the effort of public-service workers from its effect on the composition of workers who provide the service. Studies of public-sector pay find that higher wages change the pool of applicants for public jobs (Dal Bo, Finan and Rossi, 2013; Deserranno, 2019). Evidence on the effort of incumbent workers is more mixed. De Ree et al. (2018) find that doubling teachers' unconditional salaries did not improve student learning. In contrast, Mas (2006) finds a decline in police performance after arbitration awards below police unions' demands. In public defense, Agan, Freedman and Owens (2021) shows that attorneys obtain worse outcomes for their assigned clients than for their retained clients, driven by moral hazard rather than the selection of worse attorneys into assigned counsel. I show that a change in the price of appointed work changes the outcomes of the attorneys' appointed cases. Most of the effect remains after including attorney fixed effects, and appointed cases worsen relative to the same attorneys' retained cases. Attorney turnover explains little of the effect for misdemeanor cases, suggesting that the hourly price affects quality mainly through the decreased effort of the attorney who continues to provide public service.

Lastly, I measure the quality of court-appointed attorneys and use these measures to express the effect of pay in units of attorney quality. A growing literature estimates the effects of judges and prosecutors on case outcomes (Mueller-Smith, 2015; Leslie and Pope, 2017; Dobbie, Goldin and Yang, 2018; Bhuller et al., 2020; Yang, 2015; Agan, Doleac and Harvey, 2023), and Abrams and Yoon (2007) estimate differences in quality among public defenders in Las Vegas. Building on value-added methods from education (Kane and Staiger, 2008; Chetty, Friedman and Rockoff, 2014*a*; Petek and Pope, 2023), I provide estimates for court-appointed attorneys, whose training, oversight, and pay differ from those of salaried public defenders. A recent working paper estimates the quality of court-appointed attorneys

in a Texas county to understand racial disparities in the criminal justice system (Mikdash, Hoekstra and Oh, 2026). Importantly, my paper measures the dispersion of attorney quality and how a policy shifts it. My paper finds that the pay cut lowered the quality of misdemeanor defense by an amount equal to a 1.23 standard deviation decrease in attorney dismissal quality. In other words, the state's choice of hourly rate mattered more for the outcomes of the average misdemeanor defendant than drawing an attorney one standard deviation better, suggesting the hourly pay of contracted providers of public defense can be a meaningful lever to influence the quality of public defense.

I. Setting

The landmark 1963 case *Gideon v. Wainwright* established the right to legal representation for low-income defendants in state criminal cases. Public defense has since become a vital public service, with the large majority of criminal defendants in the United States being represented by publicly funded counsel rather than a privately retained attorney.[5] While *Gideon* requires states to provide defense counsel, the provision of public defense varies across jurisdictions. In North Carolina, public defense is offered through two systems: public defenders, who are salaried government employees, and court-appointed attorneys, who are private attorneys compensated on a case-by-case basis. Because both systems operate simultaneously, North Carolina provides a natural setting to study the effect of compensation of contracted employees on the quality of public services. In this section, I describe the two different public defense systems in North Carolina, the 2011 pay reduction for court-appointed attorneys, and the rotational assignment rule used to assign court-appointed attorneys to cases.

I.A. Public Provision of Defense Counsel in North Carolina

North Carolina provides public defense to low-income defendants using both court-appointed attorneys and public defenders. In larger, more populous counties, North Carolina hires

[5]Bureau of Justice Statistics, *Defense Counsel in Criminal Cases*, reports that roughly 80 percent of felony defendants in the counties it studied had publicly financed counsel (Harlow, 2000).

salaried, in-house attorneys (public defenders) to represent low-income defendants. During the sample period, North Carolina maintained public defender offices that operated in 35 counties. When a defendant was financially unable to hire an attorney in these counties, their case would be assigned to the local public defender's office, where the chief public defender would assign a public defender to the case.

In almost all counties, including those with public defenders, North Carolina also relied on private attorneys who would register with the North Carolina Office of Indigent Defense Services (NC IDS) to represent indigent defendants on a case-by-case basis, known as court-appointed attorneys. Court-appointed attorneys received an hourly or flat rate for their work, and they were assigned cases following a strict rotational system. For counties without a public defender's office, court-appointed attorneys served as the primary form of public defense. In counties with a public defender's office, any conflict and overflow cases initially sent to the public defender's office were later assigned to court-appointed attorneys, similar to how assignment takes place in counties without a public defender's office. Because I rely on the procedure used to assign attorneys to cases to construct the attorney value-added estimates, I explain how cases are assigned to court-appointed attorneys in Section I.C.

To qualify for public defense, defendants must demonstrate financial hardship that limits their ability to hire a private attorney. In practice, almost all criminal defendants in North Carolina who complete an indigency-determination form and show some evidence of financial difficulty are assigned public counsel.[6] The right to an attorney extends to all critical stages of the court process, and public counsel is assigned within 48 hours of arrest to ensure attorney representation before the defendant's first court appearance.

[6]North Carolina does not use a precise indigency threshold, though state regulations require that factors such as employment, income, and assets be considered. Conversations with IDS staff suggest that almost all individuals who request public counsel qualify. The form informs the defendant that they will owe a $60 indigent-defense fee if their case is not dismissed, and that they may be required to repay the cost of their public counsel as part of a sentence if convicted. Although defendants may still owe attorney fees, the financial cost is far below that of hiring private counsel.

I.B. 2011 Court-Appointed Pay Reduction

Before the second half of 2011, court-appointed attorneys were paid $75 per hour for any case they represented, except in two counties where a flat, per-case rate was used.[7] In an unexpected decision, North Carolina cut the funds it appropriated to compensate court-appointed attorneys by nearly 20 percent, effective for cases assigned on or after May 1, 2011.[8] In response, the NC IDS cut attorney pay and differentiated attorney pay by case type: the hourly rate fell to $55 for misdemeanor cases, $60 for lower-level felonies resolved in Superior Court, and $70 for higher-level felonies. While substantially decreasing court-appointed pay, the cut to funds left public defender salaries unchanged. A survey by NC IDS found that the pre-cut rate was only marginally sufficient to cover attorneys' costs from public-defense work, and attorneys indicated that the cut imposed substantial financial strain, prompting some attorneys to consider withdrawing from the court-appointed system (North Carolina Office of Indigent Defense Services, 2015).

Two features of the pay reduction for court-appointed attorneys support using the setting to understand the role of compensation in the quality of contracted public services. First, the pay cut was largely unanticipated. It arose from a mid-year state appropriations shortfall, and some attorneys were assigned cases before learning they would be paid the new hourly pay rates. Second, the pay decrease affected only the pay of court-appointed attorneys and left public defender salaries untouched, with no other contemporaneous policy change differentially affecting the two public defense systems. Consequently, I use public defenders as a never-treated comparison group to estimate the effect of court-appointed pay on case outcomes in a difference-in-differences framework. Section IV examines the parallel-trends

[7] I exclude cases in counties that use a flat rate. Additionally, 18 counties in North Carolina contract with individual attorneys or firms hired to represent all indigent defendants for a fixed period, such as one year. Because there is more flexibility in how these cases are assigned within a firm, my analysis excludes these attorneys and focuses on the 80 counties that use a list-based court-appointed system.

[8] Cases assigned before May 2011 continued to receive the pre-reduction rate even if resolved afterward. The difference-in-differences specification assigns the post-period indicator to the half-year of attorney assignment, with cases from May and June 2011 assignments in the omitted base period in the main specification. However, the effects are robust to alternative specifications that omit cases in May and June from the analysis.

and timing assumptions directly.

I.C. Case Assignment to Court-Appointed Attorneys

When a defendant who qualifies for public defense is assigned to a court-appointed attorney, NC IDS regulations require attorney appointments to follow a strict rotational order. Each court-appointed county maintains three lists of court-appointed attorneys, one each for misdemeanors, lower-level felonies, and higher-level felonies. The rotation is designed to spread cases evenly across attorneys. For cases with both misdemeanor and felony charges, the most serious charge fixes which attorney list is used.

The rotational assignment system creates randomness in case assignment and eliminates concerns that attorneys might select cases based on their preferences, expertise, or defendant characteristics. Within a county, list, and month-year, the defendant's attorney is uncorrelated with defendant and case characteristics such that any difference in outcomes between defendants who drew one attorney and defendants who drew another measures the effect of the attorney. I exploit this rotation to estimate attorney quality measures, which I use to scale the pay cut effect and to understand the distribution of attorney quality before and after the compensation change. Section V expands on the construction of the attorney quality measures.

II. Conceptual Framework

From the perspective of a defendant, the defense attorney may be their sole advocate throughout the court process. Their defense counsel advises on the strength of the prosecution's evidence, negotiates with the prosecutor, and may represent them in front of a jury. Consequently, the defense attorney's quality may significantly affect whether a defendant receives a favorable case outcome.[9]

[9]Throughout the paper, I consider the outcomes from the defendant's perspective: a higher dismissal rate is better for the defendant, and higher conviction and incarceration rates are worse. Later in the paper, I establish the causal effect of a court-appointed attorney on case outcomes.

At the same time, an attorney receives compensation from the state regardless of whether a defendant receives a favorable case outcome. Additionally, there are few systems in place to establish whether an attorney provided quality defense for their client. Importantly, the hourly wage, w, is the price of each additional hour an attorney spends on an appointed case, and each hour has an opportunity cost, whether a private client at market rates or time away from work. For attorneys who also serve private clients, w is also a price relative to the rates retained clients pay, and changes to it shift how they allocate working hours between private and public cases. Consequently, the hourly wage set by the state might affect at least two margins for an attorney: 1) whether to participate in the court-appointed system and 2) how much effort to exert in court-appointed cases. I consider both the extensive and intensive margins below.

II.A. Extensive and Intensive Margins

Attorneys determine whether to participate in the court-appointed system and can renew their participation annually. Let $v_j(h)$ be attorney j's revenue from h hours of private practice, with a declining marginal return. Denote the return to j's marginal private hour by $\hat{h}_j = v_j'(h_j)$. Registration on list ℓ carries a fixed cost F_j of qualifying and remaining listed but delivers n_ℓ appointed hours over the year. Attorney j registers on list ℓ if and only if

$$w_\ell \;\geq\; \hat{h}_j + \frac{F_j}{n_\ell}. \tag{1}$$

where w_ℓ is the court-appointed wage for list ℓ.

In this framework, declining marginal returns make appointed work attractive. Because a local client base of finite size leaves the next private hour worth less than the last, appointed work absorbs hours of low private return. Contrary to the idea that only low-quality attorneys participate in public defense work, the framework suggests that participation might also

be governed by slack in private demand. Consequently, a cut in w_ℓ removes the attorneys for whom (1) no longer holds. However, it is less clear how the quality of public defense would change. If the most able attorneys have the strongest private practices, the cut is adversely selective. Alternatively, appointed work may fill idle hours for attorneys with weak private demand, and the cut may screen on private demand rather than on quality. Consequently, it is unclear whether the quality of public defense would be affected by changes to the pool of attorneys providing contracted public defense.

For some w_ℓ that satisfies equation (1), an attorney then determines how much effort to contribute to a defendant's case on list ℓ. For one of the three court-appointed lists, let attorney quality be denoted as $\theta_j(w)$ for that list's wage w. Then,

$$\theta_j(w) \equiv q_j + e_j(w), \tag{2}$$

where q_j represents the stable component of attorney j's quality, and $e_j(w)$ represents the effort the attorney devotes to a publicly assigned case at an hourly rate w. A lower w reduces the return to each hour on an appointed case, so effort might fall for every attorney, including attorneys with no private clients to turn to. Assuming effort enters additively, a common decline in effort lowers every attorney's outcomes by a similar amount and leaves the ranking of attorneys by q_j unchanged. For attorneys with strong private practices, the cut might also shift hours toward private clients, and effort on appointed cases might fall further. This second decline would differ by quality only if the most able attorneys hold the strongest private practices. However, a potential client has few ways to discern an attorney's quality, because a case outcome reflects the strength of the evidence as well as the attorney's work. If clients cannot observe quality, private demand might be uncorrelated with q_j. Consequently, the decline in effort might be unrelated to attorney quality. I identify effort changes using within-attorney estimates of the pay effect on case outcomes.

The conceptual framework suggests that the pay cut lowers effort on appointed cases,

while its effect on the pool of attorneys is ambiguous. It also leaves open which attorneys respond the most, because the decline in effort would differ by quality only if the most able attorneys hold the strongest private practices. In what follows, I estimate the effect of the pay cut on defendant outcomes and public defense quality empirically.

III. Data

The core data come from the North Carolina Administrative Office of the Courts (AOC) and consist of charge-level criminal court records assigned an attorney between 2008 and 2016, covering the universe of criminal charges disposed in the state's district and superior courts. These records include detailed defendant demographics and information on charges, case outcomes, and punishments, along with attorney-specific data such as the attorney's name, bar number, and representation type (public defender, court-appointed attorney, or privately retained). I use the AOC records to define the assignment cells, the counsel type, and every case outcome. I augment these data with a second source, the North Carolina State Bar roster, which supplies attorney gender and bar admission date. I also use the bar roster to construct attorney experience, measured as the number of years between admission to the North Carolina bar and assignment to a criminal case.

I perform two linkages across observations to create a panel dataset. First, to capture each defendant's case history, I link cases across time using the defendant's name and date of birth. This linkage lets me construct criminal-history controls, such as the number of prior convictions and whether the defendant is a first-time offender. I also use this linkage to identify future crime spells for each defendant. Second, to attach roster characteristics to counsel, I link the attorney named on the case to the State Bar roster on bar number, falling back on name-based matching where the bar number is absent from the court record.

The final dataset is one observation per defendant-disposition. I use defendant-disposition as the observation because an attorney assigned to a defendant is responsible for all of that defendant's cases and charges resolved on a given disposition date. The most serious charge

determines which list is used to assign counsel; for defendants with multiple cases resolved on the same date, I identify the most serious charge across all cases using North Carolina's offense-grading system, restrict to that charge, but use information from all charges to determine case outcomes. I describe the data construction in more detail in Appendix D.

Descriptive statistics for cases represented by court-appointed attorneys and public defenders are reported in Table 1. The resulting dataset contains just under 750,000 defendant-disposition observations. I compare case outcomes for defendants represented by court-appointed attorneys and public defenders, separately by misdemeanor and felony charges. All of the defendant case outcomes are indicators equal to one if the outcome occurs, except incarceration and probation sentences, which are measured in days. A case is considered dismissed if all charges are dismissed.[10] Conviction is defined as any guilty disposition, and incarceration, probation, and fines-only are subsets of conviction.

Comparison across the columns reveals that defendants represented by court-appointed attorneys differ demographically from those represented by public defenders. For example, court-appointed attorneys more often represent white defendants, while public defenders more often represent Black defendants. These differences reflect that public defender offices are predominantly found in urban areas. Additionally, the summary statistics show that dismissal is the most frequent outcome across both attorney types for misdemeanors. This observation highlights that the police act as the charging agency in North Carolina. As a result, nearly all arrested individuals are charged by police, some of whom prosecutors may not have charged had they been the charging agency. I observe 3,321 court-appointed attorneys handling misdemeanor cases and 2,508 handling felony cases. I also capture the outcomes for the 1,102 public defenders handling misdemeanor cases and the 793 handling felony cases.

[10] AOC disposition codes for a dismissed charge include dismissal by the court, lack of probable cause, dismissal for a speedy trial, and voluntary dismissal.

IV. The 2011 Pay Cut

IV.A. Design and Identification

To estimate the effect of the May 2011 court-appointed attorney pay reduction on case outcomes, I compare the case outcomes of court-appointed attorneys with those of public defenders. Importantly, the pay decrease did not affect the pay for public defenders, allowing me to estimate the effect of compensation on the quality of public defense. In the main analysis, I exclude cases assigned to court-appointed attorneys in counties that use public defenders.

The effect of the 2011 pay reduction is estimated using the following specification:

$$Y_{ict} = \beta \text{PayDecrease}_{ct} + \psi X_{ict} + \gamma_c + \lambda_t + \epsilon_{ict} \tag{3}$$

where Y_{ict} represents the case outcome for defendant i in county c at half-year t. The term PayDecrease_{ct} is an indicator equal to one after the first half of 2011 in court-appointed attorney counties c. X_{ict} is a vector of demographic and criminal history controls. The terms γ_c and λ_t are county and half-year fixed effects. The coefficient of interest, β, captures the impact of the pay reduction on case outcomes for defendants represented by court-appointed attorneys. Standard errors are clustered at the county level.

The main identifying assumption is that, in the absence of the pay reduction, case outcomes for defendants in counties relying on court-appointed attorneys as their primary public defense system would have followed the same trajectory as those in counties using public defenders. Additionally, no other policy differentially affecting cases handled by court-appointed attorneys and public defenders should have occurred at the same time as the pay reduction. The Justice Reinvestment Act was introduced in December 2011 as a significant reform to North Carolina's criminal justice system, with its primary focus on limiting courts' authority to revoke probation to reduce correctional facility costs (Rose, 2021). Importantly,

these reforms applied uniformly to all cases, regardless of attorney type. While the counterfactual parallel trends assumption can not be observed, Figure 1 shows little evidence of pre-trends before the 2011 pay reduction, supporting the validity of the parallel trends assumption.

IV.B. Effect of Hourly Pay Decrease on Case Outcomes and Recidivism

Figure 1 plots the dynamic treatment effect of the 2011 pay reduction on the five primary case outcomes: the likelihood of dismissal, incarceration, probation, incarceration sentence, and probation sentence. The pre-period estimates for each outcome suggest that case outcomes in counties solely using court-appointed attorneys would have followed the same trajectory as those using public defenders, supporting the parallel trends assumption. The figure shows an adverse effect of the pay decrease on case outcomes appears after 2011 and stabilizes roughly two years after the pay reduction. Two years after the pay decrease, the likelihood of case dismissal for defendants assigned to court-appointed attorneys decreases by 4 percentage points. Likewise, defendants assigned court-appointed attorneys after the pay decrease were 4 percentage points more likely to be incarcerated, with no statistically significant effect on probation or incarceration and probation sentences.

Table 2 presents estimates from equation (3), separately by misdemeanor, lower-level, and higher-level felony cases. The size of the pay reduction varied by case severity, corresponding to the three attorney lists. The estimates in column 1 report the average effect of the pay reduction for the outcomes listed in each row. They average the post-treatment effects and are, in all cases, smaller than the effects observed in later time periods in Figure 1.

The table reveals that the overall impact is primarily driven by attorneys handling misdemeanor cases, who received the largest pay cut. Specifically, the likelihood of case dismissal decreases by 5.5 percentage points for misdemeanor cases. Additionally, for misdemeanor cases, the likelihood of incarceration increases by 4.7 percentage points, and the likelihood of receiving fines increases by 0.4 percentage points. Incarceration sentences, primarily in

jail, also increased by about one day.

For lower-level felony cases, the only notable impact is on probation sentences. Defendants in lower-level felony cases assigned a court-appointed attorney after the pay reduction are assigned an average of 17 additional days of probation. Defendants charged with a higher-level felony were 2.7 percentage points less likely to receive a probation sentence after the pay reduction. The signs of the effects for higher-level felony cases suggest that defendants were more likely to receive favorable outcomes after the pay cut, though the effects are generally not statistically significant. The one exception is that defendants charged with a higher-level felony were 2.7 percentage points less likely to receive probation after the pay reduction.

Table 3 reports the effect of the pay reduction on whether a defendant reoffended within one and three years. Within one year of disposition, defendants assigned a court-appointed attorney after the pay reduction were 2.3 percentage points more likely to be charged with a new offense (6 percent). To understand how these effects might differ by crime type, I estimate the effect on the likelihood of a new charge by the new crime type. I find that the pooled effect is concentrated among new drug charges, which increased by 1.7 percentage points (24 percent). I also find that the pay reduction decreased new violent charges by 0.7 percentage points (6 percent) within three years. Misdemeanor and lower-level felony reoffenses look similar, with the likelihood of recidivism for drug charges increasing. For higher-level felony charges, I find that the pay reduction increased the likelihood that court-appointed defendants are charged with a new violent crime by 1.7 percentage points (13 percent). I observe a similar pattern when I consider three-year recidivism, with some exceptions. For misdemeanor charges, the new violent crime offense decreased by 0.8 percentage points. The effects for higher-level felonies also become statistically insignificant[11]

[11]The event study figures are in Appendix Figure A3 through A8.

IV.C. Case Type, Attorney Characteristics, and Additional Heterogeneity

The pay reduction may have differentially affected defendants. Tables A1 and A2 report the effect of the pay cut by defendant demographic and case characteristics. Table A1 reports the pay cut effects by defendant race, sex, and prior record. In general, I find that the point estimates across defendant demographic characteristics follow the pooled estimates, suggesting minimal heterogeneity, especially by defendant sex and prior record. However, there are a few point estimates by defendant race that stand out. For misdemeanor charges, I find that the pay cut decreased the likelihood of dismissal by 6.3 percentage points for Black defendants and 6.4 percentage points for Hispanic defendants. For white defendants, the point estimate is 4.4 percentage points, suggesting differential effects by race. I also find that the likelihood of incarceration increased by 3.8 and 3.9 percentage points for White and Hispanic defendants, respectively, and 4.6 percentage points for Black defendants. For higher-level felony cases, the pooled estimates hide that Black defendants are 6 percentage points less likely to be convicted or incarcerated, a positive outcome for these defendants.

Table A2 reports the effect of the pay cut by crime type. I find that the adverse case outcome effects for misdemeanor cases appear for nearly all case types, with stronger adverse effects for property and traffic/public order charges. For example, the likelihood of dismissal falls by 3.6 percentage points for defendants charged with violent crimes and by 7.6 percentage points for defendants charged with property crimes. This suggests that defendants charged with less severe misdemeanor crimes are receiving greater punishments than defendants charged with the same crime before the pay reduction. The effects for lower- and higher-level felonies are nearly all statistically insignificant, with the exception of financial crimes. Defendants charged with a lower-level financial felony are 4.8 percentage points less likely to have their case dismissed.

IV.D. Changes to Attorney Effort vs. Compositional Shifts

The pay reduction for court-appointed attorneys adversely affected case outcomes, especially for misdemeanor cases. However, it is still unclear whether changes to attorney effort or compositional change drive the adverse effects. To disentangle these two channels, I estimate several within-attorney specifications to identify within-attorney pay cut effects on defendant case outcomes by adding an attorney fixed effect to equation (3).

Table 4 reports the results of this exercise. Column 1 provides the difference-in-differences estimates restricted to the within-attorney sample. Column 2 reports the within-attorney pay cut effect. Column 3 differences column 2 from column 1. Assuming the total effect is composed of only the effort and composition effects, column 3 reports the implied effect of compositional changes. Column 4 acts as a placebo by estimating the difference-in-differences and within-attorney difference-in-differences specifications on a sample of public attorneys who remained in the public defense system before and after the pay cut (i.e., "stayers"). In a sample of stayers, compositional change should contribute zero percent of any effect of the pay cut by definition. Column 5 is the fraction of the difference-in-differences effect that remains after including attorney fixed effects. The fraction represents an estimate of the total effect that might be attributed to changes in attorney effort.

The results of Table 4 reveal that including an attorney fixed effect slightly decreases the effect size of the pay cut on case outcomes, and the effects remain statistically significant. Comparing the composition decompositions in columns 3 and 4 reveals that the compositional piece of stayers is smaller than that for the full sample, and closer to zero, for all outcomes. For misdemeanor cases, the estimates suggest that between 79 and 84% of the effect of the pay cut on defendant case outcomes remains after including attorney fixed effects, suggesting that the bulk of the observed adverse effects may be attributed to changes in attorney effort. For felony cases, I find smaller fractions, which still suggest that a sizable share of the total effect comes from changes in attorney effort. However, the effects are not statistically significant.

Table 4 presents evidence that the effect runs through changes to attorney effort. However, it is less clear how the pay cut may have affected how attorneys practice more generally. For example, the lower pay rate may have required some attorneys to eliminate office staff or services that might negatively affect defendant case outcomes. To investigate this and similar pathways, I exploit the fact that court-appointed attorneys maintain private clients besides their court-appointed cases to estimate the effect of the pay cut on appointed case outcomes using retained cases as the comparison group. I estimate the following equation:

$$Y_{ijct} = \alpha \text{Appointed}_{ij} + \phi \text{PayDecrease}_t + \beta(\text{Appointed}_{ij} \times \text{PayDecrease}_t) + \psi X_{ijct} + \delta_j + \gamma_{ct} + \omega_o + \epsilon_{ijct} \quad (4)$$

where Y_{ijct} represents the case outcome for defendant i represented by attorney j in county c at half-year t. The term Appointed_{ij} is an indicator equal to one if defendant i's case with attorney j was court-appointed rather than privately retained, and PayDecrease_t is an indicator equal to one after the first half of 2011. X_{ijct} is a vector of demographic and criminal history controls, along with the attorney's cumulative prior caseload and the running share of their cases that are appointed. The terms δ_j, γ_{ct}, and ω_o are attorney, county-by-half-year, and offense-code fixed effects, where o indexes the offense code of the case. The coefficient of interest, β, is identified within-attorney and captures the change in the appointed-versus-retained gap for the same attorney after the pay reduction. Any change in how the attorney practices that affects both retained and court-appointed defendants equally, such as the loss of office staff or support services, is differenced out, so β isolates the effect specific to appointed work. Standard errors are two-way clustered on attorney and county.

Table 5 reports the results from estimating equation (4). Using retained cases as the comparison group, court-appointed defendants experienced worse case outcomes after the pay cut compared with private clients of the same attorney. Court-appointed defendants were 3 percentage points less likely to have their case dismissed and 2.7 percentage points

more likely to be incarcerated. The effects for misdemeanor cases are similar to the pooled results. For lower-level felony cases, court-appointed defendants were 4.2 percentage points less likely to have their case dismissed and 3.9 percentage points more likely to be incarcerated. These results suggest that the pay cut changed how much effort attorneys exerted on court-appointed cases after the pay reduction.

IV.E. Case-Handling Outcomes

The results from the previous two exercises provide evidence that the majority of the adverse effects of the pay cut on court-appointed case outcomes are through a change in attorney effort and not a shift in attorney composition. However, it remains unclear how changes in attorney effort resulted in adverse case outcomes for defendants with court-appointed attorneys. To understand how attorneys altered their practices for court-appointed cases, I estimate the effect of the pay cut on case-handling outcomes using equation (3). I consider the time between the attorney assignment and the end of a case, plea negotiation, and other case-handling outcomes.

Table 6 reports the effect of the pay decrease on court processing outcomes. Panel A reports the effect on the length of time between attorney assignment and when a case outcome is decided. I find that the length of time between attorney assignment and disposition increased by 8 days, though this effect is not statistically significant for any specific case type.

Panel B reports the results for changes in plea negotiations. Pooling across all case types, I find that defendants assigned a court-appointed attorney after the pay decrease are 4 percentage points more likely to enter a plea, and 3 percentage points more likely to plead guilty. These effects are concentrated among misdemeanor cases, where defendants are 4.3 percentage points more likely to enter any plea, but 5 percentage points more likely to plead guilty. Panel C considers additional case-handling outcomes, and I find no statistically significant effect on the number of motions filed or whether charges were consolidated. These

results reveal that court-appointed attorneys handled cases differently after the pay cut, in ways that were detrimental to defendant case outcomes. In particular, cases took longer to resolve and were more likely to end in a guilty plea.

V. Attorney Quality and the Scale of the Pay Cut Effect

The pay cut to court-appointed attorneys adversely affected defendant case outcomes, decreasing the likelihood of dismissal by 5.5 percentage points (9 percent) and increasing the likelihood of incarceration by 4.7 percentage points (21 percent) for misdemeanor defendants. About 80 percent of both effects remain after including attorney fixed effects, suggesting that most of the pay-cut effect comes through the intensive margin, with attorneys putting less effort into each case. In the conceptual framework, attorneys choose effort given the wage, and effort enters attorney j's quality, $\theta_j(w)$. Consequently, the pay cut may have lowered attorney quality directly and may also have changed the dispersion of quality across attorneys. Additionally, the difference-in-differences estimates do not reveal how large the pay-cut effect is relative to the differences in quality across attorneys. In this section, I estimate the quality of court-appointed attorneys with conventional value-added methods, both pooled across the cut and separately under the pre-cut wage, w_0, and the post-cut wage, w_1. I first measure the dispersion of attorney quality and its effect on case outcomes and recidivism. I then use the quality measures to express the pay-cut effect in standard deviations of attorney quality and to examine how effort changes differed by attorney quality.

To construct attorney quality measures, I implement a residualized, leave-out procedure that compares case outcomes for defendants who could have been assigned to the same set of attorneys.[12] In North Carolina, the set of potential attorneys a defendant may be assigned to is determined by the highest charge on the case, the county where the charge is filed, and the month and year of the attorney assignment. To ensure that comparisons are made only

[12]My approach follows the teacher value-added literature (Kane and Staiger, 2008; Chetty, Friedman and Rockoff, 2014*b*; Jacob, Lefgren and Sims, 2010) while incorporating the leave-out measures used for legal decision-makers (Dobbie, Goldin and Yang, 2018; Leslie and Pope, 2017).

between defendants who could have been assigned to the same group of attorneys, all models include county-list-by-time fixed effects. Although the rotational case assignment mechanism eliminates many concerns about selection bias, using residualized measures yields a more precise measure of attorney quality by controlling for variation in observable characteristics. Additionally, conditioning on demographic and criminal history characteristics alleviates any lingering concern about sorting along these observable dimensions.

V.A. Measuring Attorney Quality

Let Y_{ict} be a case outcome for defendant i in case c, where defendant i is assigned attorney j in month-year t.[13] The residual case outcome is given by:

$$Y^*_{ict} = Y_{ict} - \beta X_{ict} = \theta_j + \epsilon_{ict}. \tag{5}$$

where X_{ict} contains defendant demographic and criminal history controls and county-list-by-time fixed effects. I include the same defendant demographic and criminal history controls used in equation (3). I assume that the residual outcome, Y^*_{ict}, is an additively separable function of attorney quality θ_j and a defendant-case idiosyncratic error term ϵ_{ict}.

For each case, I use the residual outcome, Y^*_{ict}, to construct a leave-out mean case outcome for defendants assigned to attorney j. However, attorneys may be assigned to the same defendant multiple times. Consequently, I build attorney quality by averaging Y^*_{ict} over defendant i's dispositions with attorney j to give $\bar{Y}^*_{ij}$, so that a defendant represented by the same attorney repeatedly contributes only one observation.

Let D_j denote the number of distinct defendants that attorney j represents. Then,

$$\widehat{\mu}_{j,-i} = \frac{1}{D_j - 1} \sum_{i' \neq i} \bar{Y}^*_{i'j}, \tag{6}$$

is the leave-out mean of the average defendant residuals for each attorney j. This provides

[13] I construct attorney quality before and after the 2011 pay cut, and pooled across all years.

an estimate of the attorney's quality, which is a measure of their ability to affect defendant case outcomes. It is an unbiased estimate of θ_j if there is conditional random assignment of defendants to attorneys (i.e., $E[\epsilon_{ict}|j, X_{ict}] = 0$ for every defendant in j's leave-out set). This is likely to hold in this situation because, within a county-by-list-by-month assignment cell, predetermined defendant and case characteristics are orthogonal to attorney assignment under the rotational list assignment mechanism. Residualizing on X_{ict} also alleviates concerns related to any potential sorting on observable characteristics while providing a more precise measure of attorney quality.

I then construct an empirical Bayes estimate, $\tilde{\mu}_{j,-i}$, that accounts for the reliability of the estimated attorney quality by incorporating the number of defendants handled by each attorney (Chetty, Friedman and Rockoff, 2014*b*; Kane and Staiger, 2008). Specifically,

$$\tilde{\mu}_{j,-i} = \bar{Y}^* + \lambda_j\big(\widehat{\mu}_{j,-i} - \bar{Y}^*\big) \tag{7}$$

where $\bar{Y}^*$ is the mean of $\bar{Y}^*_{ij}$ across all attorney–defendant pairs. The shrinkage factor, λ_j, adjusts the leave-out mean $\widehat{\mu}_{j,-i}$ by reducing the influence of noisy estimates for attorneys who worked with fewer defendants by "shrinking" their estimates toward the overall mean, resulting in a more reliable measure of attorney quality. The shrinkage factor is defined as

$$\lambda_j = \frac{\hat{\sigma}^2_\theta}{\hat{\sigma}^2_\theta + \frac{\hat{\sigma}^2_\epsilon}{(D_j - 1)}}, \tag{8}$$

where $\hat{\sigma}^2_\epsilon$ is the within-attorney variance of the defendant-level residuals, estimated as a single value pooled across all attorneys, separately for misdemeanor and felony cases. The variance $\hat{\sigma}^2_\theta$ is the variance of θ_j across attorneys.[14]

[14]The signal variance $\hat{\sigma}^2_\theta$ comes from the leave-out covariance of $\bar{Y}^*_{ij}$ and $\widehat{\mu}_{j,-i}$. Both are built from disjoint sets of defendants, their errors are independent, and their covariance recovers the variance of θ_j. Thus,

$$\widehat{\sigma}^2_\theta = \frac{1}{N}\sum_{D_j \geq 2}\big(\bar{Y}^*_{ij} - \bar{Y}^*\big)\big(\widehat{\mu}_{j,-i} - \bar{Y}^*\big) \tag{9}$$

is unbiased for the signal variance, where N is the number of attorney–defendant pairs with $D_j \geq 2$. The

The constructed attorney quality measure, $\tilde{\mu}_{j,-i}$, is a predictor of true attorney quality such that $\theta_j = \tilde{\mu}_{j,-i} + \eta_j$. Because $\tilde{\mu}_{j,-i}$ is shrunk toward the mean, it is not an unbiased estimate of θ_j. Consequently, I need the predictor to exhibit forecast unbiasedness (Chetty, Friedman and Rockoff, 2014*b*; Petek and Pope, 2023). This would require that a one-unit increase in $\tilde{\mu}_{j,-i}$ corresponds, on average, to a one-unit increase in θ_j, or equivalently, η_j should be uncorrelated with $\tilde{\mu}_{j,-i}$. This condition is weaker than conditional random assignment, since it requires only that unobserved determinants of case outcomes be uncorrelated with estimated quality rather than balanced for every attorney.[15] I check for forecast bias and examine the effect of attorney quality on predicted outcomes as a placebo test in Section V.D to alleviate any concerns about sorting based on unobservable characteristics that might affect case outcomes.

I construct measures of attorney quality using case dismissal and incarceration. From the perspective of a defendant, case dismissal and incarceration are clear indicators of attorney quality. A better attorney increases the likelihood of case dismissal and decreases the likelihood of incarceration. I estimate the variance components separately for misdemeanor and felony cases. I also estimate attorney quality measures before and after the 2011 pay reduction, pooling cases from 2008–2010 for pre-2011 attorney quality measures and cases from 2012–2016 for post-2011 measures.[16] In Appendix C, I construct annual attorney quality measures that allow quality to change over time. Because annual caseloads are small, these measures are much noisier. Empirical Bayes shrinkage corrects the resulting attenuation bias but pulls most estimates toward the mean, leaving little variation and wide confidence in-

estimator remains unbiased with heteroskedastic errors and unequal caseloads (Kline, Saggio and Sølvsten, 2020, Example 1). It follows the approach of Kane and Staiger (2008) and Chetty, Friedman and Rockoff (2014*a*), who estimate the variance of teacher quality from the covariance of a teacher's average residuals across classrooms, but takes the covariance between each defendant and the attorney's other defendants. Excluding from each leave-out mean the attorney's other defendants whose cases were disposed of on the same day, which removes shocks shared within a court session, lowers $\hat{\sigma}_\theta$ by about 1 percent for misdemeanors and 4 percent for felonies.

[15]Conditional random assignment implies forecast unbiasedness. If only forecast unbiasedness holds, coefficients on $\tilde{\mu}_{j,-i}$ still recover the effect of attorney quality, but individual attorneys' estimates may be biased by sorting that offsets their true quality (Chetty, Friedman and Rockoff, 2014*b*). In that case, rankings of specific attorneys are unreliable and $\hat{\sigma}^2_\theta$ understates the true dispersion of attorney quality.

[16]Pooling across years would assume that attorneys' relative quality is stable across the wage cut.

tervals. I therefore use the pooled measure throughout and report the attorney-year version in Appendix C.

V.B. The Effect of Attorney Quality on Case Outcomes and Recidivism

Table 7 reports the standard deviation of attorney quality, $\widehat{\sigma}_\theta$, measured by case dismissal and incarceration avoidance, separately for misdemeanor and felony cases. The first estimate pools all defendants between 2008 and 2016. The last columns report the standard deviation estimated using only pre-2011 defendants and only post-2011 defendants, respectively.

The table reveals that attorneys differ meaningfully in their effects on case outcomes. For example, two court-appointed attorneys, one standard deviation apart in quality, differ by 4.1 percentage points in the share of their misdemeanor defendants whose cases are dismissed. The table also reveals that the estimated dispersion does not meaningfully change across the pay regime change. Before the pay cut, the dispersion of misdemeanor dismissal quality, $\widehat{\sigma}_\theta(w_0)$, was 4.4 percentage points, while the post-cut dispersion, $\widehat{\sigma}_\theta(w_1)$, was 4.3 percentage points. This pattern holds across both quality measures and case types in three of the four cells; felony incarceration avoidance falls from 3.2 to 2.8 percentage points. However, the difference is not statistically significant.

Tables 8, C4, and C5 report the effect of a one-standard-deviation increase in attorney quality on case outcomes and recidivism. Table 8 uses attorney quality measures constructed by pooling all defendants before and after the 2011 pay cut. Tables C4 and C5 use attorney quality measures constructed using only pre-2011 and post-2011 defendants, respectively. Each estimate is the coefficient on the unstandardized EB-shrunk leave-out quality measure multiplied by the respective standard deviation of attorney quality from Table 7 (i.e., $\hat{\beta}_{\text{raw}} \times \hat{\sigma}_\theta$).

Panel A of Table 8 reports the estimates for misdemeanor cases for both dismissal quality and incarceration avoidance quality. The σ_θ from Table 7 used to scale each row is reported next to the quality measure. Focusing on dismissal quality, column 1 reveals that

a one-standard-deviation increase in dismissal quality increases the likelihood of case dismissal by 4.3 percentage points. Under forecast unbiasedness, this is expected (i.e., a one-standard-deviation increase in attorney quality measured by case dismissal would increase case dismissal by one standard deviation). Columns 2 to 7 report the effect across other case outcomes. Defendants assigned a one-standard-deviation-higher dismissal quality attorney are 2.2 percentage points less likely to be incarcerated and 2.3 percentage points less likely to receive probation. Their incarceration sentences are 0.85 days shorter, and their probation sentences are 10 days shorter. Defendants assigned a one-standard-deviation-better attorney are 1.3 percentage points more likely to recidivate within one year and 1.1 percentage points more likely to recidivate within three years. These results suggest that better attorneys secure positive case outcomes for their clients, but also increase the likelihood that defendants have future interactions with the criminal justice system.

The effects of attorney quality on felony case outcomes and recidivism are reported in panel B of Table 8. Similar to Panel A, a one-standard-deviation increase in attorney dismissal quality increases case dismissals by 3.2 percentage points, as expected. Additionally, a one-standard-deviation increase in attorney dismissal quality decreases the likelihood of incarceration and probation by 2.8 and 2.5 percentage points, respectively, while decreasing the incarceration and probation sentences by 5 and 16 days, respectively. Higher-quality attorneys have no statistically significant effect on felony recidivism. Comparing the effects of attorney dismissal quality with those of incarceration avoidance quality reveals robust effects across the two measures.

Comparing the results of Table 8 to the results of Tables C4 and C5, which use attorney quality measures constructed using only pre-2011 and post-2011 defendants, respectively, reveals that the effects of a one-standard-deviation increase in attorney quality are robust to whether quality is measured before, after, or across the pay cut. The agreement across the three tables suggests that the effect of attorney quality on case outcomes and recidivism was not affected by the pay reduction. However, the agreement does not suggest that attorneys

remained in their relative position of attorney quality across the cut, or that the composition did not change. Rather, it suggests the return to quality is not affected by pay changes.

V.C. Pay Effects by Attorney Quality

The attorney quality measures allow me to understand how the effect of the pay reduction differed by attorney quality. Figure 2 plots the effects of the 2011 pay reduction on misdemeanor case outcomes by attorney quality. I split attorneys into terciles using their pre-2011 attorney case dismissal quality. The figure reveals similar effects across terciles. For each of the seven outcomes, I cannot reject that the effects are equal across terciles. A similar exercise in Figure A11 shows similar effects across terciles of attorneys' pre-cut retained caseloads. Taken together, these results suggest that attorneys reduced their effort by a similar amount regardless of their pre-cut quality or the caseloads in their private practices. In terms of the conceptual framework, I find little evidence that the wage acted as a relative price for attorneys with private clients. Instead, the wage appears to have acted as the price of each appointed hour, reducing effort by a similar amount for every attorney. Consequently, the pay cut lowered the quality of public defense for misdemeanor defendants regardless of which court-appointed attorney they were assigned.

Additionally, the attorney quality measures provide a way to scale the pay cut effects in terms of attorney quality. Table 9 reports the effects of the pay cut in terms of the pre-cut attorney quality measures. Column 1 repeats the effect of the pay cut on case dismissal and incarceration from Table 2. Column 2 reports the column 1 effect scaled by the standard deviation of the relevant attorney quality measure. For example, the pay reduction for court-appointed attorneys decreased the likelihood of case dismissal by 5.47 percentage points for misdemeanor cases. Scaling this effect by the estimated pre-cut standard deviation of attorney dismissal quality, 4.44 percentage points, shows that the effect of the pay cut on misdemeanor case dismissal was equivalent to a 1.23 standard deviation decrease in

attorney dismissal quality. The same exercise using incarceration avoidance[17] reveals that the pay decrease was equivalent to decreasing attorney incarceration avoidance quality by 1.74 standard deviations.

V.D. Checking for Bias in Case Outcome Effects

In this section, I test whether the effects of attorney quality presented in Table 8 are biased by the sorting of defendants to attorneys. For a causal effect of attorney quality on case outcomes, assignment of defendants to attorneys cannot differ in unobservables within a county-by-list-by-time assignment cell. The rotational assignment rule makes this plausible, but I conduct further tests to provide evidence that bias from sorting is limited.

Table 10 tests whether measured quality exhibits forecast unbiasedness. Each cell regresses a defendant's case outcome on the leave-out quality measure built from that outcome, with the controls and assignment-cell fixed effects used to construct the measure. Under forecast unbiasedness, a one-unit increase in measured quality raises the outcome by one unit on average, implying a coefficient of one. A correctly shrunk estimate should, by construction, return a coefficient of one in the sample used to build the measure (Chetty, Friedman and Rockoff, 2014*a*), suggesting this is a weaker test. The test does verify that the measures were correctly shrunk, and failing it would be problematic. Three of the four coefficients are statistically indistinguishable from one. The one exception is case dismissal, which is marginally statistically different from 1, where a coefficient is 1.07.

To test whether defendants with better potential case outcomes are sorted to higher-ability attorneys, I predict case outcomes based on predetermined characteristics excluded from the attorney quality measures. In particular, I consider predicting case outcomes using a quadratic of ACS neighborhood socioeconomic status, a leave-out-defendant mean outcome for the charge statute from court-appointed cases, process-service code, citation, warrant, and complaint indicators, a quadratic of days from offense to attorney assignment, and

[17]To use the incarceration avoidance measure of attorney quality with the pay cut effects, I use the negative of the incarceration effects reported in Table 2.

charge code.[18] Column 2 of Table 10 regresses the predicted outcome on quality with the same controls and fixed effects as column 1. Column 3 divides column 2 by column 1, where the ratio is the forecast bias from selection on the excluded characteristics.

Panel A of Table 10 shows misdemeanor forecast bias for case dismissal and incarceration as 1.5 and -0.8 percent. For comparison, Chetty, Friedman and Rockoff (2014*a*) and Petek and Pope (2023) find magnitudes for teacher value-added between -1.6 and 2.2 percent. The confidence interval for case dismissal rules out forecast bias above 3.3 percent, suggesting that sorting on these predetermined characteristics is small on the misdemeanor lists.

Panel B of Table 10 shows more sorting on the felony lists. Forecast bias is 6.8 percent for case dismissal and 9.3 percent for incarceration. The strongest predictor of sorting among felony cases is the number of days from offense to attorney assignment. These results suggest that the felony effects are likely overstated. Adjusting the effects in Table 8 by accounting for the forecast bias in column 3 suggests that the effects in column 1 of Panel B decrease from 3.2 to 3.0 percentage points and from 2.7 to 2.4 in column 2. Importantly, this bound covers only the sorting these characteristics can detect, but the sign and magnitude of the effects remain robust.

Appendix Table B3 tests for sorting on a defendant's history in court. The table reports the results from regressing the outcome of a defendant's previous case, handled by a different attorney, on the quality of the current attorney, who cannot have affected the case outcome. I find that none of the coefficients are distinguishable from zero, suggesting that defendants with favorable earlier outcomes are not systematically assigned to higher-quality attorneys.

Overall, these tests show little evidence of sorting among misdemeanor defendants, with potential sorting for felony cases. The felony effects keep roughly the same magnitude and sign, but may be overstated by roughly a tenth.

[18] Appendix Table B6 reports conventional balance tests of the quality measures on defendant characteristics. However, these are controls in the quality model, and the measure is residualized on them within the same assignment cells. Consequently, the balance of those characteristics cannot speak to bias. None of the predictors used here enters the control set.

VI. The Fiscal and Social Arithmetic

A primary motivation for reducing the hourly pay for court-appointed attorneys was to decrease overall state spending. In Table 11, I calculate the savings to taxpayers from the hourly pay decrease relative to taxpayers' spending on increased incarceration. IDS fee records for 2015 and 2016 show that the average appointed misdemeanor case billed 3.7 hours at $55 an hour. Consequently, the $20 reduction in the hourly rate saved the state at least $74 per case, or $3.7 million a year across its 50,600 affected cases.[19]

On the cost side, the state's savings on lower payments to court-appointed attorneys came with additional active sentences for misdemeanor defendants. Using estimates of the effect of the pay cut on incarceration, I find that the pay reduction increased jail costs for counties by $40 per case, or $2 million a year.[20] Comparing the savings with the costs shows that taxpayers saved roughly $34 per case on net. However, counties spent roughly 54 cents on jail for every dollar the state saved from the pay reduction, suggesting a decrease in overall costs but additional costs shifted to counties.

The hourly pay reduction also affected social costs and benefits. Specifically, recidivism increased, which created negative externalities on the victims of crime, and defendants were more likely to be convicted. I estimate a lower bound of new potential victim costs of $1,290 per case and an upper bound of $4,647 using the most serious offense of new charges, pricing these charges following Rose (2021).

Defendants' losses are the most difficult to price, but may be substantial. The adverse case effects suggest that dismissals were replaced with convictions for 5.5 of every 100 misdemeanor defendants assigned an appointed attorney. A criminal record lowers employer callbacks (Pager, 2003) and increases future criminal justice contact (Agan, Doleac and Harvey,

[19] I compute hours as the amount paid divided by the post-cut rate, using the 2015 and 2016 fee records, when the schedule stood unchanged at $55, $60, and $70 an hour. Roughly 80 percent of district-court payments are exact multiples of $5.50, a tenth of an hour at $55. The saving is the rate reduction times post-cut hours, so it is a lower bound if the cut also lowered the hours attorneys billed.

[20] The effect in Table 2 is concentrated among sentences that are shorter than 91 days. North Carolina reimburses counties $40 per day for housing defendants sentenced between 91 and 180 days. However, terms shorter than 90 days, which are the vast majority of misdemeanor sentences, receive no state reimbursement.

2023). Incarceration and pretrial detention also reduce formal employment (Mueller-Smith, 2015; Leslie and Pope, 2017; Dobbie, Goldin and Yang, 2018). Using the estimates of victim savings, I find that social welfare is lower after the pay reduction if a lost dismissal costs a defendant more than $22,862 at the lower bound or $84,269 at the upper bound.

As a potential benchmark, Hjalmarsson et al. (2025) find that annual earnings five years after a first criminal charge in Sweden are 4.6 percent lower for minor charges and 9.4 percent for major charges.[21] Using the average W-2 earnings of $14,594 for US defendants two years before a first misdemeanor charge reported by Agan et al. (2024), earnings penalties are worth $13,158 for minor and $26,888 for major charges discounted at 3 percent over a 30-year working life, or roughly $725 to $1,470 per appointed case. Consequently, my estimates suggest that the earnings cost of a minor record[22] is below the $22,862 lower bound and that the pay reduction may have increased social welfare.

Even if the pay reduction raised social welfare, the additional costs of losing a dismissal fall on low-income defendants who are already disadvantaged. The court-appointed system was established to provide public defense to low-income individuals who were unable to afford a private attorney. Additionally, the heterogeneous effects of the pay cut by race suggest the adverse effects were larger for Black and Hispanic defendants. Further, the pay reduction widened the gap in misdemeanor dismissal rates between indigent defendants represented by a court-appointed attorney and those represented by public defenders, suggesting a defendant's likelihood of dismissal depended on how the state provides public counsel in the jurisdiction where a defendant is charged. These results shed light on the broader issue of public-work pay disparities and their impact on the quality of government services.

[21] Using charges rather than convictions might overstate the earning effects. Recent research finds no earnings penalty for misdemeanor conviction (Rao and Reeves, 2026). Though, the individuals affected by the reform they study are older than the average defendant.

[22] Minor charges in Sweden are closer equivalents of US misdemeanor charges than major charges in Sweden are.

VII. Conclusion

When defendants are unable to afford legal representation, states are required to provide defense counsel. In this paper, I use detailed administrative court records from North Carolina to examine how public defense attorney compensation and quality affect case outcomes for low-income defendants. My results reveal that decreasing the economic incentives for contracted private attorneys leads to a decline in the quality of public defense provided to low-income defendants. The effects are primarily driven by decreases in attorney effort at the new hourly wage and are comparable to a 1.23 standard deviation reduction in attorney case dismissal quality.

At the same time, the pay reduction for court-appointed cases produced net savings for taxpayers, and it lowered social welfare only if a lost dismissal costs a defendant more than $22,862. However, the cost-benefit analysis obscures who bears the cost. The adverse effects of the pay cut fell on low-income defendants, with larger effects for Black and Hispanic defendants than for white defendants. These results suggest that the lower compensation and quality of publicly contracted attorneys may substantially exacerbate social and economic inequalities for the individuals and communities that depend on public defense.

More generally, as governments increasingly outsource public services to private actors, the gap in compensation between public-sector and private-sector workers poses challenges for providing quality public services. Addressing compensation disparities in public services may enhance the production of effective government and positively impact the individuals and communities most dependent on public services.

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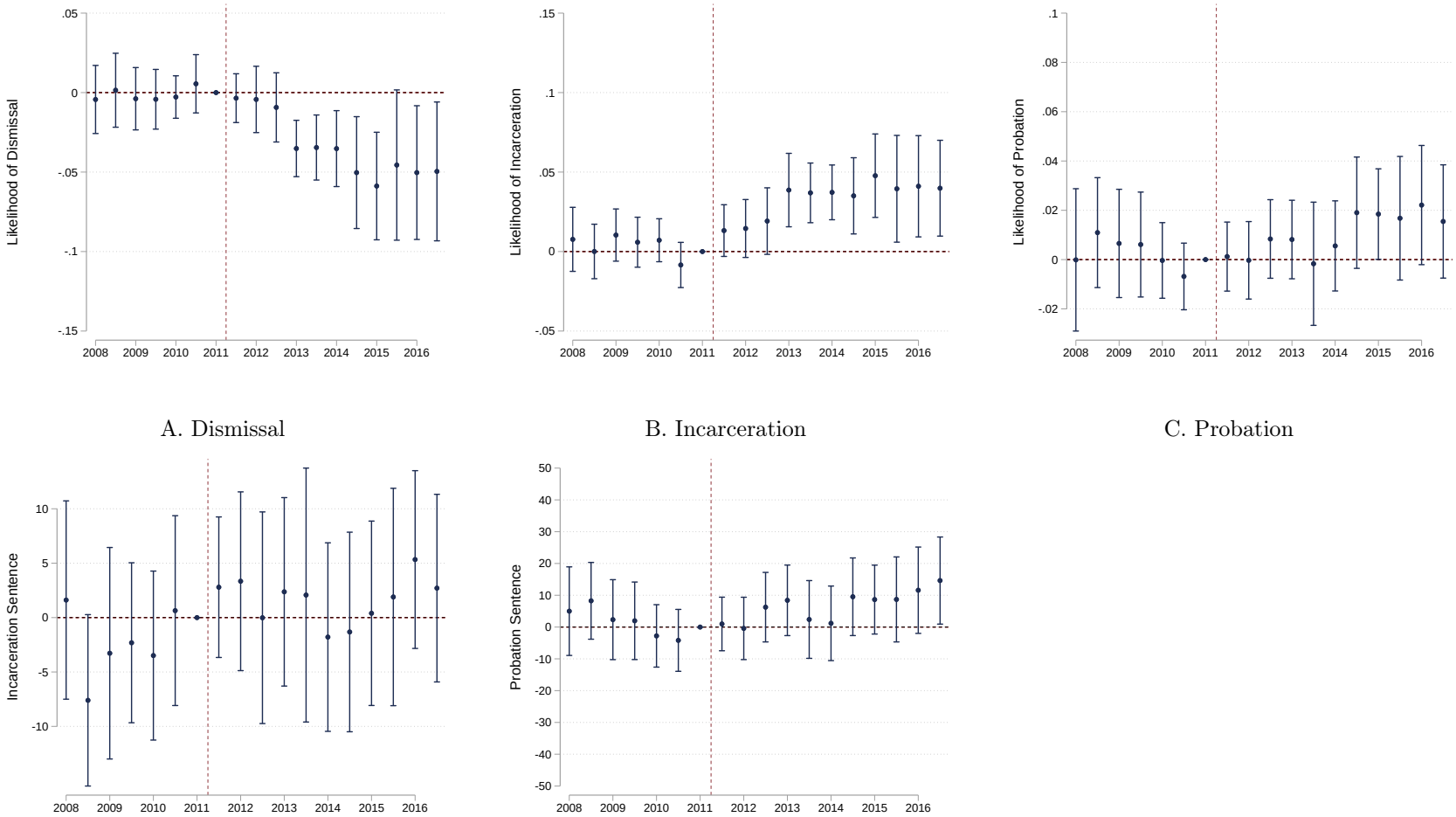
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Figures



A. Dismissal

B. Incarceration

C. Probation

D. Incarceration Sentence

E. Probation Sentence

Figure 1 The figure plots the effects of the 2011 court-appointed attorney pay reduction on the likelihood of case dismissal, incarceration, probation, incarceration sentence, and probation sentence. The vertical red line represents the second half of 2011, when the pay reduction occurred. The event study compares court-appointed attorneys in counties without public defenders with public defenders. Public defenders did not receive a pay reduction. Each specification controls for defendant demographic and case characteristics such as age, sex, race and ethnicity, prior convictions, an indicator for whether the case is resolved in district court, and the charge class of the most serious charge. Bands are 95% confidence intervals. Standard errors are clustered at the county level.

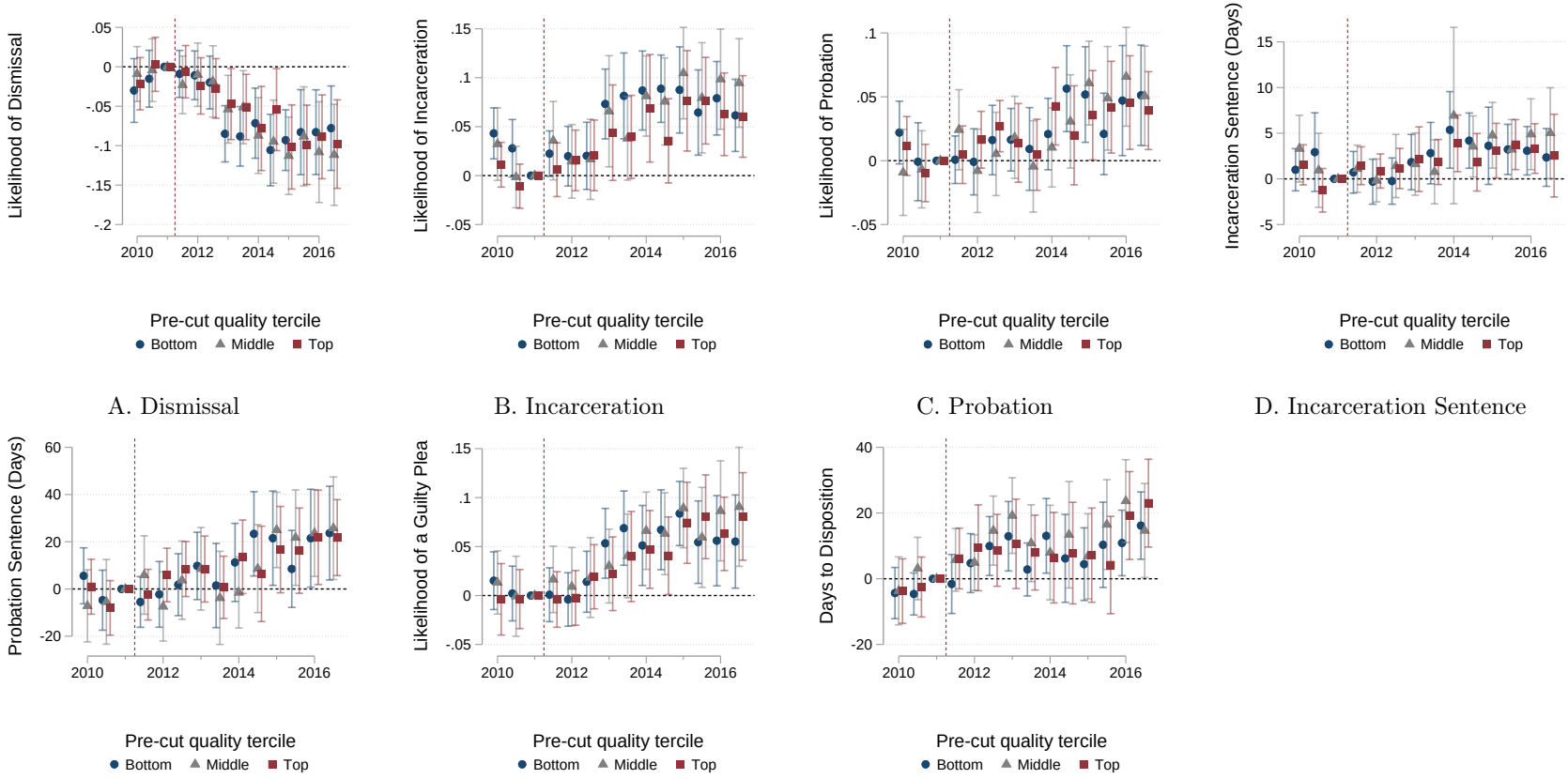


Figure 2 The figure plots the effects of the 2011 pay reduction on misdemeanor case outcomes separately for each tercile of the court-appointed attorney's pre-cut dismissal quality. Quality is the empirical Bayes dismissal quality estimated from 2008–09 assignments, so the event study begins in 2010, and terciles are attorney-weighted among court-appointed attorneys with a pre-cut estimate. Each series compares the cases of one tercile with public-defender cases, relative to the first half of 2011, with attorney, county, half-year, and charge-class fixed effects and the controls in Figure 1. The vertical red line represents the second half of 2011, when the pay reduction occurred. Sentence lengths are in days and include zeros, and time to disposition is the number of days from attorney assignment to disposition. Bands are 95% confidence intervals. Standard errors are clustered at the county level.

Tables

Table 1 Descriptive Statistics for Defendants with Public Attorneys, by Case Severity

	Misdemeanor Cases		Felony Cases	
	Appt Counsel	Pub Defender	Appt Counsel	Pub Defender
A. Outcomes				
Dismissal	0.550	0.658	0.392	0.434
Conviction	0.411	0.298	0.604	0.558
Incarceration	0.233	0.172	0.526	0.468
Probation	0.199	0.145	0.327	0.307
Fines Only	0.054	0.031	0.006	0.003
Guilty Plea	0.329	0.284	0.408	0.387
Incarceration Sentence (Days)	14.14	8.81	208.96	155.98
Probation Sentence (Days)	86.75	61.98	237.65	203.10
1-Year Recidivism	0.367	0.362	0.411	0.423
3-Year Recidivism	0.559	0.542	0.583	0.587
B. Defendant and Case Characteristics				
Male	0.649	0.705	0.760	0.795
Age at charge	32.17	32.38	31.43	31.60
White	0.578	0.364	0.560	0.366
Black	0.358	0.551	0.390	0.569
Hispanic	0.027	0.046	0.021	0.032
Race other	0.037	0.039	0.029	0.033
First time offender	0.544	0.592	0.414	0.461
Num. prior convictions	1.32	1.18	1.79	1.59
Num. charges	1.91	1.75	3.27	2.83
Violent crime	0.275	0.265	0.170	0.176
Property crime	0.173	0.156	0.313	0.283
Financial crime	0.021	0.021	0.148	0.153
Public order crime	0.394	0.407	0.086	0.093
Drug crime	0.131	0.144	0.277	0.291
Miscellaneous crime	0.005	0.007	0.005	0.004
Num. Attorneys	3,321	1,102	2,508	793
Num. Defendants	188,281	151,975	94,675	62,163
Observations	286,580	238,073	130,118	80,725

Note: Summary statistics are for defendants charged with a misdemeanor or felony between 2008 and 2016. All of the defendant case outcomes are indicators equal to one if the outcome occurs, except incarceration and probation sentences, which are measured in days. A case is considered dismissed if all charges are dismissed. Conviction is defined as any guilty disposition, and incarceration, probation, and fines are subsets of conviction. One- and three-year recidivism equal one if the defendant has a new court appearance within one or three years of release from any active sentence.

Table 2 Effect of 2011 Pay Reduction by Case Type

	Main	Misdemeanor	Lower-Level Felony	Higher-Level Felony
	(1)	(2)	(3)	(4)
A. Dismissal Likelihood				
Appointed Counsel × Pay Decrease	-0.042***	-0.055***	-0.014	0.040
	(0.011)	(0.016)	(0.025)	(0.035)
Dep Mean	0.565	0.646	0.417	0.416
B. Incarceration Likelihood				
Appointed Counsel × Pay Decrease	0.037***	0.047***	0.014	-0.046
	(0.011)	(0.015)	(0.017)	(0.032)
Dep Mean	0.320	0.221	0.496	0.557
C. Probation Likelihood				
Appointed Counsel × Pay Decrease	0.012	0.004	0.023	-0.027*
	(0.009)	(0.009)	(0.015)	(0.016)
Dep Mean	0.213	0.158	0.326	0.183
D. Fines only likelihood				
Appointed Counsel × Pay Decrease	0.002*	0.004**	0.001	0.002
	(0.001)	(0.002)	(0.001)	(0.002)
Dep Mean	0.011	0.015	0.004	0.002
E. Probation Sentence				
Appointed Counsel × Pay Decrease	7.661	0.254	17.245*	-15.780
	(5.141)	(4.387)	(9.889)	(18.355)
Dep Mean	117.968	66.756	215.977	173.397
F. Incarceration Sentence				
Appointed Counsel × Pay Decrease	7.053**	0.908*	5.467	18.601
	(2.831)	(0.532)	(7.234)	(42.322)
Dep Mean	64.016	10.313	115.353	677.860
Observations	549,197	356,045	176,787	16,362
County FE	X	X	X	X
Half-year FE	X	X	X	X

Note: Difference-in-differences estimates of the 2011 hourly-rate cut using public defenders as the never-treated control group. Column (1) pools all case types, and columns (2)–(4) split by attorney list type. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3 Effect of 2011 Pay Reduction on Recidivism by Case Type

	Main	Misdemeanor	Lower-Level Felony	Higher-Level Felony
	(1)	(2)	(3)	(4)
A. One-Year Recidivism				
Any	0.023**	0.026**	0.019	0.033**
	(0.011)	(0.011)	(0.014)	(0.015)
Dep. mean	0.391	0.380	0.415	0.379
Violent	-0.004	-0.005	-0.003	0.017*
	(0.003)	(0.003)	(0.003)	(0.010)
Dep. mean	0.077	0.082	0.063	0.127
Property	0.001	0.002	-0.002	0.008
	(0.002)	(0.003)	(0.004)	(0.006)
Dep. mean	0.094	0.087	0.110	0.069
Drug	0.017***	0.018***	0.019**	-0.008
	(0.006)	(0.006)	(0.009)	(0.007)
Dep. mean	0.070	0.064	0.083	0.056
B. Three-Year Recidivism				
Any	0.021*	0.026**	0.018	0.017
	(0.011)	(0.012)	(0.013)	(0.016)
Dep. mean	0.576	0.571	0.590	0.530
Violent	-0.007**	-0.008**	-0.005	0.016
	(0.003)	(0.003)	(0.003)	(0.012)
Dep. mean	0.117	0.126	0.093	0.174
Property	-0.001	-0.001	-0.003	0.007
	(0.003)	(0.003)	(0.004)	(0.008)
Dep. mean	0.130	0.123	0.147	0.091
Drug	0.022***	0.023***	0.024**	-0.014
	(0.007)	(0.007)	(0.009)	(0.009)
Dep. mean	0.104	0.097	0.119	0.078
County FE	X	X	X	X
Half-year FE	X	X	X	X
Controls	X	X	X	X

Note: Difference-in-differences estimates of the 2011 hourly-rate cut using public defenders as the never-treated control group. Column (1) pools all case types, and columns (2)–(4) split by attorney list type. Within each panel, rows represent the new charge to the defendant at reoffense. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4 Decomposition of the 2011 Court-Appointed-Attorney Pay Cut

Outcome	Total (DiD)	Effort (Within-att.)	Total − Effort (All)	Total − Effort (Stayers)	% Effort
A. Overall					
Dismissal	-0.0421***	-0.0303***	-0.0118	-0.0032	72%
	(0.0110)	(0.0110)	—		
Incarceration	0.0375***	0.0293***	0.0081	0.0028	78%
	(0.0105)	(0.0105)	—		
B. Misdemeanor					
Dismissal	-0.0547***	-0.0456***	-0.0090	0.0031	83%
	(0.0162)	(0.0126)	—		
Incarceration	0.0467***	0.0372***	0.0096	-0.0019	80%
	(0.0150)	(0.0133)	—		
C. Felony					
Dismissal	-0.0092	-0.0005	-0.0087	-0.0028	5%
	(0.0259)	(0.0264)	—		
Incarceration	0.0093	0.0056	0.0037	0.0021	60%
	(0.0183)	(0.0186)	—		

Note: Decomposition of the 2011 pay-cut difference-in-differences effect (Table 2) into an effort channel and a composition channel. Column 1 reports the total effect (the DiD), which is the same estimate as Table 2. Columns 2–5 report the effort channel (within-attorney change among attorneys who continue across the cut), the difference between columns 1 and 2, which is the change in the estimate when attorney fixed effects are added for both court-appointed attorneys and public defenders, the same difference for stayers only (near zero by construction), and % Effort, the effort share of the total. Panels A, B, and C report the decomposition for all cases, misdemeanors, and felonies, respectively. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5 Effect of 2011 Pay Reduction Using Retained Cases as Control Group

	Main	Misdemeanor	Lower-Level Felony	Higher-Level Felony
	(1)	(2)	(3)	(4)
A. Dismissal Likelihood				
Appointed Case × Pay Decrease	-0.030***	-0.024***	-0.042***	-0.033
	(0.007)	(0.008)	(0.009)	(0.031)
Dep Mean	0.561	0.627	0.431	0.377
B. Incarceration Likelihood				
Appointed Case × Pay Decrease	0.027***	0.024***	0.039***	0.057*
	(0.006)	(0.007)	(0.007)	(0.030)
Dep Mean	0.293	0.196	0.483	0.590
C. Probation Likelihood				
Appointed Case × Pay Decrease	0.032***	0.021***	0.051***	0.028
	(0.005)	(0.005)	(0.010)	(0.041)
Dep Mean	0.228	0.174	0.349	0.233
D. Fines only likelihood				
Appointed Case × Pay Decrease	-0.006**	-0.006*	-0.003*	-0.004
	(0.003)	(0.004)	(0.002)	(0.004)
Dep Mean	0.028	0.038	0.007	0.004
E. Probation Sentence				
Appointed Case × Pay Decrease	14.171***	6.993**	31.628***	11.797
	(3.510)	(3.095)	(6.996)	(31.880)
Dep Mean	126.497	73.100	237.010	220.941
F. Incarceration Sentence				
Appointed Case × Pay Decrease	4.504*	0.428	2.419	-16.436
	(2.590)	(0.570)	(4.742)	(75.221)
Dep Mean	60.828	10.143	114.817	716.218
Observations	329,377	221,200	99,246	8,931
Attorney FE	X	X	X	X
County FE	X	X	X	X
Half-year FE	X	X	X	X

Note: Difference-in-differences estimates of the 2011 hourly-rate cut from equation (4). Retained cases are used as the control group, and the sample is attorneys with both appointed and retained cases. Column (1) pools all case types, and columns (2)–(4) split by attorney list type. Standard errors two-way clustered at the attorney and county levels. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6 Case-Handling (Process) Outcomes: 2011 Court-Appointed-Attorney Pay Cut

	Overall (1)	Misd. (2)	Lower Fel. (3)	Higher Fel. (4)
A. Time to disposition				
Assignment to disposition (days)	7.618*	1.588	15.650	12.792
	(4.288)	(3.476)	(9.715)	(11.847)
Dep. mean	130.019	105.824	171.561	224.936
B. Plea negotiation				
Any plea entered	0.039***	0.043***	0.028	0.001
	(0.010)	(0.012)	(0.017)	(0.017)
Dep. mean	0.254	0.253	0.261	0.174
Guilty plea	0.029***	0.050***	-0.015	-0.043
	(0.010)	(0.012)	(0.027)	(0.034)
Dep. mean	0.329	0.295	0.395	0.337
Plea to lesser charge	0.005	-0.002	0.019	0.009
	(0.006)	(0.004)	(0.014)	(0.021)
Dep. mean	0.056	0.016	0.134	0.098
Not-guilty plea (contested)	-0.001	-0.002	-0.000	-0.001
	(0.002)	(0.004)	(0.001)	(0.004)
Dep. mean	0.027	0.040	0.004	0.016
C. Other case handling				
Motion filed	0.003	-0.001	0.005	0.017
	(0.005)	(0.001)	(0.013)	(0.046)
Dep. mean	0.013	0.001	0.032	0.068
Charge consolidated	0.002	0.002	0.003	0.003
	(0.002)	(0.003)	(0.004)	(0.006)
Dep. mean	0.028	0.020	0.044	0.025
County FE	X	X	X	X
Half-year FE	X	X	X	X
Controls	X	X	X	X

Note: Difference-in-differences estimates of the 2011 pay cut on process outcomes, with public defenders as the never-treated control. Each coefficient is the CAA×Post interaction with county and half-year fixed effects and demographic controls. Panel A covers days from attorney assignment to disposition; Panel B covers plea-negotiation outcomes including guilty plea; Panel C covers other case-handling metrics. Standard errors clustered on county. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7 The Scale of Attorney Quality

	Dispersion	By pay regime	
Outcome	σ_θ	$\sigma_\theta(w_0)$	$\sigma_\theta(w_1)$
A. Misdemeanor			
Dismissal	0.041	0.044	0.043
	(0.001)	(0.003)	(0.002)
Incarceration avoidance	0.024	0.027	0.026
	(0.001)	(0.003)	(0.002)
B. Felony			
Dismissal	0.030	0.031	0.032
	(0.002)	(0.006)	(0.003)
Incarceration avoidance	0.029	0.032	0.028
	(0.002)	(0.005)	(0.004)

Note: Each row reports the standard deviation of true attorney quality, $\hat{\sigma}_\theta$, measuring quality by the likelihood of case dismissal or the likelihood of incarceration avoidance. Incarceration avoidance is the negative of incarceration. Dismissal and incarceration avoidance are residualized on demographic and criminal history controls and county $\times$ list $\times$ month fixed effects. The variance $\hat{\sigma}^2_\theta$ is the covariance, across defendants, between a defendant's residual and the mean residual of the attorney's other defendants (equation (9)). σ_θ uses cases assigned 2008–2016. $\sigma_\theta(w_0)$ and $\sigma_\theta(w_1)$ use cases assigned 2008–2010 and 2012–2016, restricted to the 901 misdemeanor and 692 felony attorneys with at least two defendants in each period. Attorney-bootstrap standard errors (200 replications) in parentheses.

Table 8 Effect of Attorney Quality on Case Outcomes, Court-Appointed Attorneys

	(1) Dismissal (p.p.)	(2) Incarceration (p.p.)	(3) Incarceration Days	(4) Probation (p.p.)	(5) Probation Days	(6) 1-Year Recidivism (p.p.)	(7) 3-Year Recidivism (p.p.)
Panel A: Misdemeanors							
Dismissal quality ($\sigma_\theta = 4.09$ p.p.)	4.32***	-2.20***	-0.85***	-2.32***	-10.35***	1.26***	1.12***
	(0.10)	(0.11)	(0.16)	(0.12)	(0.57)	(0.17)	(0.16)
Incarceration avoidance quality ($\sigma_\theta = 2.42$ p.p.)	3.90***	-2.46***	-0.82***	-2.02***	-8.94***	1.01***	0.92***
	(0.18)	(0.12)	(0.23)	(0.16)	(0.74)	(0.19)	(0.17)
Mean dep. var.	0.535	0.237	14.42	0.202	88.22	0.374	0.569
Observations	288,927	288,927	288,927	288,927	288,927	288,927	288,927
Attorneys	2,655	2,655	2,655	2,655	2,655	2,655	2,655
Panel B: Felonies							
Dismissal quality ($\sigma_\theta = 3.01$ p.p.)	3.19***	-2.82***	-4.84**	-2.54***	-16.29***	0.28	0.21
	(0.27)	(0.24)	(2.33)	(0.25)	(2.02)	(0.27)	(0.26)
Incarceration avoidance quality ($\sigma_\theta = 2.89$ p.p.)	3.17***	-2.69***	-4.13	-2.56***	-16.33***	0.13	0.08
	(0.28)	(0.29)	(2.52)	(0.28)	(2.10)	(0.28)	(0.27)
Mean dep. var.	0.404	0.518	187.10	0.321	228.52	0.416	0.590
Observations	118,420	118,420	118,420	118,420	118,420	118,420	118,420
Attorneys	1,964	1,964	1,964	1,964	1,964	1,964	1,964
Demographic controls	Y	Y	Y	Y	Y	Y	Y
Crime controls	Y	Y	Y	Y	Y	Y	Y
List-time FE	Y	Y	Y	Y	Y	Y	Y

Note: Each cell is a separate regression of the column outcome on one measure of attorney quality. Entries multiply the coefficient on the empirical Bayes quality measure, $\tilde{\mu}_{j,-i}$, by $\hat{\sigma}_\theta$ from Table 7, giving the effect of a one-standard-deviation increase in true attorney quality. Both measures are signed so that higher values indicate a better attorney. All regressions include demographic and criminal history controls and county × list × month fixed effects. Outcomes are defined as in Table 1, and sentence lengths include zeros. Standard errors are clustered by attorney and also scaled by $\hat{\sigma}_\theta$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 9 The 2011 Pay Cut in Units of Attorney Quality

	Pay-cut effect (p.p.) (1)	Effect in σ_θ units (2)
A. Dismissal		
All lists (σ_θ = 3.99 p.p.)	-4.21***	-1.05***
	(1.10)	(0.28)
Misdemeanor (σ_θ = 4.44 p.p.)	-5.47***	-1.23***
	(1.62)	(0.37)
Lower felony (σ_θ = 3.15 p.p.)	-1.36	-0.43
	(2.50)	(0.80)
Upper felony (σ_θ = 3.15 p.p.)	4.04	1.28
	(3.53)	(1.15)
B. Incarceration avoidance		
All lists (σ_θ = 2.88 p.p.)	-3.75***	-1.30***
	(1.05)	(0.39)
Misdemeanor (σ_θ = 2.69 p.p.)	-4.67***	-1.74***
	(1.50)	(0.59)
Lower felony (σ_θ = 3.17 p.p.)	-1.43	-0.45
	(1.72)	(0.55)
Upper felony (σ_θ = 3.17 p.p.)	4.62	1.46
	(3.16)	(1.03)

Note: Column (1) reports the difference-in-differences estimates of the pay cut from Table 2; incarceration avoidance is the negative of the incarceration effect. Column (2) divides column (1) by the pre-cut standard deviation of court-appointed attorney quality, $\sigma_\theta(w_0)$ from Table 7, shown in each row label. Lower and upper felonies share the felony σ_θ, and the all-lists σ_θ weights the misdemeanor and felony variances by court-appointed case shares. Standard errors in parentheses are clustered on county in column (1) and use the delta method in column (2). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 10 Forecast Bias in the Attorney Quality Measure

	Actual outcome y (1)	Predicted outcome $\hat{y}$ (2)	Forecast bias (%) (3)
Panel A. Misdemeanor			
Dismissal	1.066	0.0156	1.46
	(0.024)	(0.0099)	(0.93)
Incarceration	1.035	-0.0080	-0.77
	(0.050)	(0.0169)	(1.64)
Panel B. Felony			
Dismissal	1.079	0.0729	6.76
	(0.090)	(0.0201)	(1.86)
Incarceration	0.948	0.0884	9.32
	(0.100)	(0.0238)	(2.51)

Note: Court-appointed cases matched to American Community Survey neighborhood data (273,706 misdemeanor and 108,972 felony dispositions). Each column regresses its dependent variable on the empirical Bayes quality measure, with defendant controls and county × list × month and charge-class fixed effects. Column (1) uses the actual outcome. Column (2) uses the outcome predicted from predetermined characteristics excluded from the quality model: neighborhood socioeconomic status, the average outcome of other court-appointed defendants with the same charge, how the case was initiated, days from offense to attorney assignment, out-of-state residence, and charge type. Column (3) is column (2) divided by column (1), in percent, and is zero in expectation if defendants are randomly assigned to attorneys within cells. Standard errors clustered on attorney in parentheses.

Table 11 Cost-Benefit Analysis of the 2011 Pay Reduction, per Appointed Misdemeanor Case

	Amount	(S.E.)	Notes
Savings per case to taxpayers			
Attorney fee savings (state)	\$74		IDS fee records, 2015–2016
Hours billed per case	3.70		Fee paid ÷ \$55 post-cut rate
Reduction in the hourly rate	\$20		District court, \$75 to \$55
Costs per case to taxpayers			
County jails, terms of 90 days or less	\$40	(13)	DiD estimate; WCB $p = 0.02$
Additional jail days	0.99	(0.34)	DiD estimate
Cost per jail day	\$40		SMCP rate; not reimbursed by the state
Net savings to taxpayers	**\$34**	(13)	Savings minus costs; WCB 95% set [3, 69]
Costs per case to attorneys and defendants			
Attorney fees forgone	\$74		Transfer to taxpayers
Dismissals lost per case	0.055	(0.016)	DiD estimate (Table 2); WCB $p = 0.007$; not priced
Savings per case to potential victims			
At victim-cost prices	\$1,290	(441)	Rose (2021) lower bound; WCB $p = 0.017$
At willingness-to-pay prices	\$4,647	(1,128)	Rose (2021) upper bound; WCB $p = 0.001$
Change in new violent cases	−0.036	(0.011)	DiD estimate (Table A8); WCB $p = 0.007$
Break-even values: the cut lowers social welfare if			
A lost dismissal costs its defendant more than			(Victim savings − jail cost) ÷ 0.055
At victim-cost prices	\$22,862		
At willingness-to-pay prices	\$84,269		
An averted violent case is worth less than	\$1,106		Jail cost ÷ 0.036, if defendants lose nothing
Plus, per \$1 a lost dismissal costs its defendant	\$1.52		0.055 ÷ 0.036

Note: Amounts are 2012 dollars per appointed misdemeanor case, summed over the three years after disposition without discounting. "DiD estimate" is the coefficient on Appointed Counsel × Pay Decrease in the specification of Table 2, column (2); each dollar row reruns that regression on the dollar value of the outcome. Standard errors are clustered on county, and WCB p is the wild cluster bootstrap p-value (Rademacher weights, 9,999 replications). Jail days are the minimum active term imposed, priced at the daily rate the state pays counties under the Statewide Misdemeanant Confinement Program (SMCP) for terms over 90 days. Victim rows price each new case counted in Appendix Table A8 at its most serious offense, using the victim-cost prices of Chalfin and McCrary (2018) and the willingness-to-pay prices of Cohen et al. (2004) as compiled in Rose (2021, Online Appendix Table A26); homicide is excluded, and victimless offenses carry no cost. Changes in state custody spending and in the cost of processing new cases are not distinguishable from zero and are omitted. Break-even values weight every party's dollars equally, treat the fee cut as a transfer, and use unrounded estimates.

Appendix A. The 2011 Court-Appointed Pay Reduction

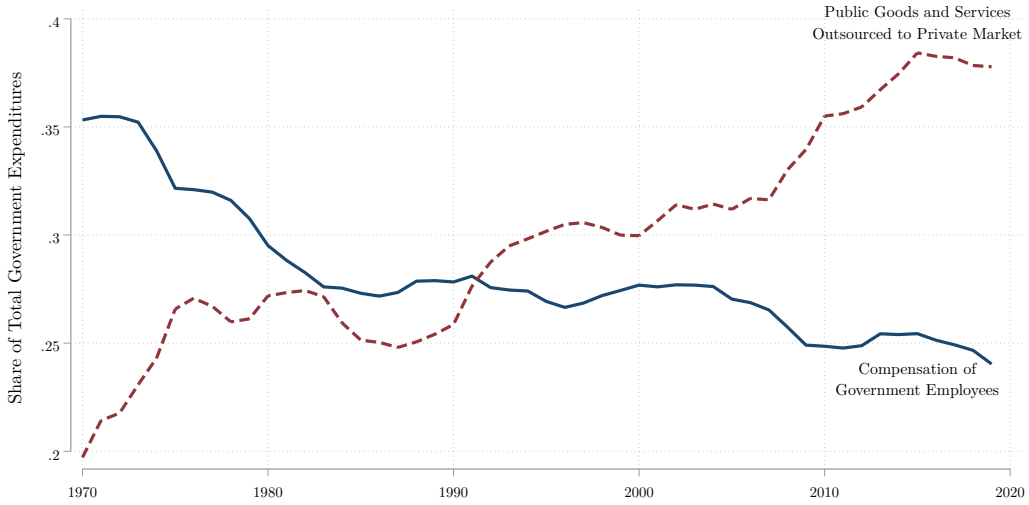
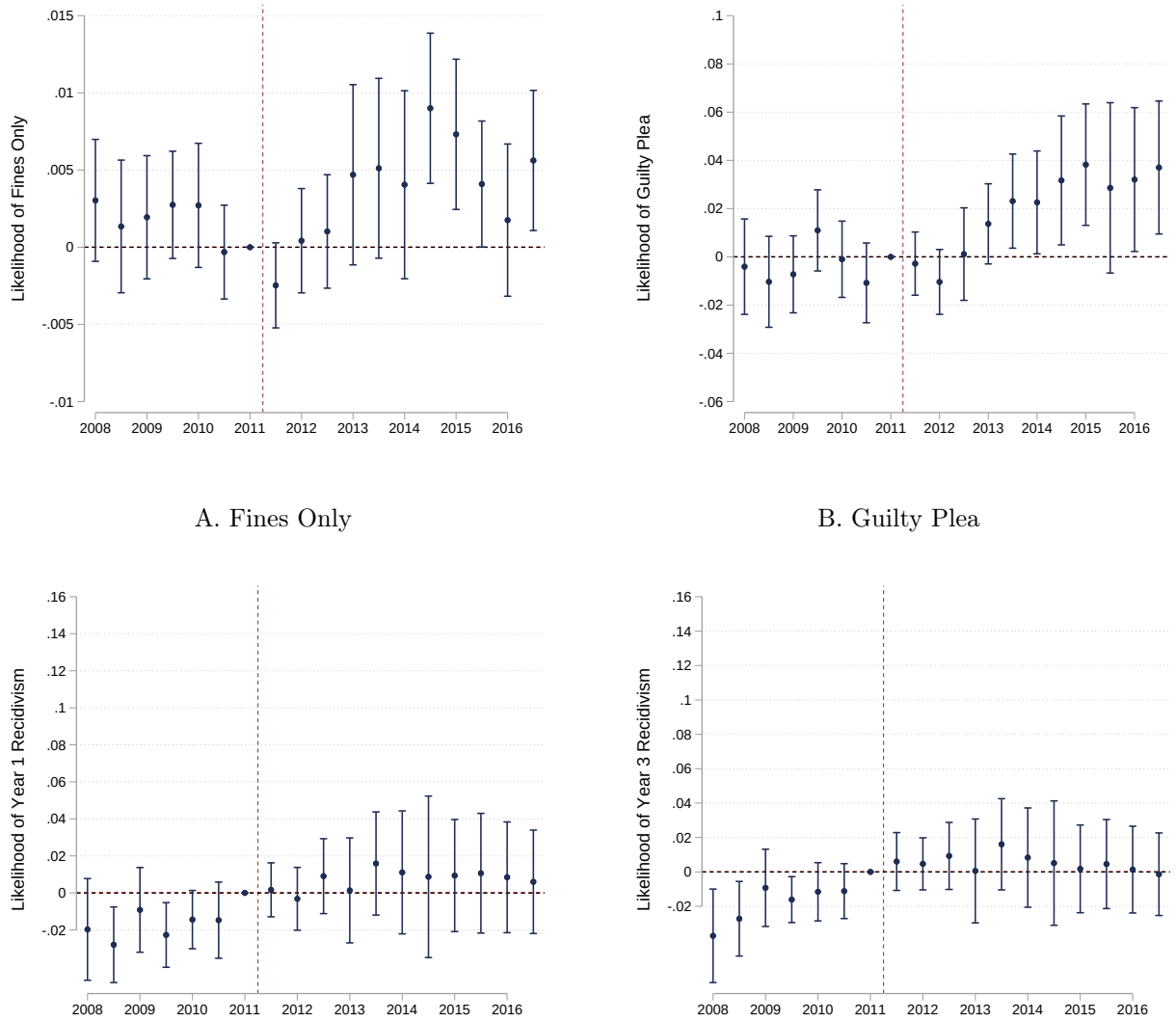


Figure A1 The figure plots two components of US general government expenditure as shares of total general government expenditure, 1970 to 2019. It shows how much government spending has shifted toward services bought from private producers, which is why the price paid to contractors matters. The dashed line is the OECD series "social benefits and social transfers in kind for products supplied to households via market producers." The solid line is compensation of general government employees. Data are from the OECD National Accounts.



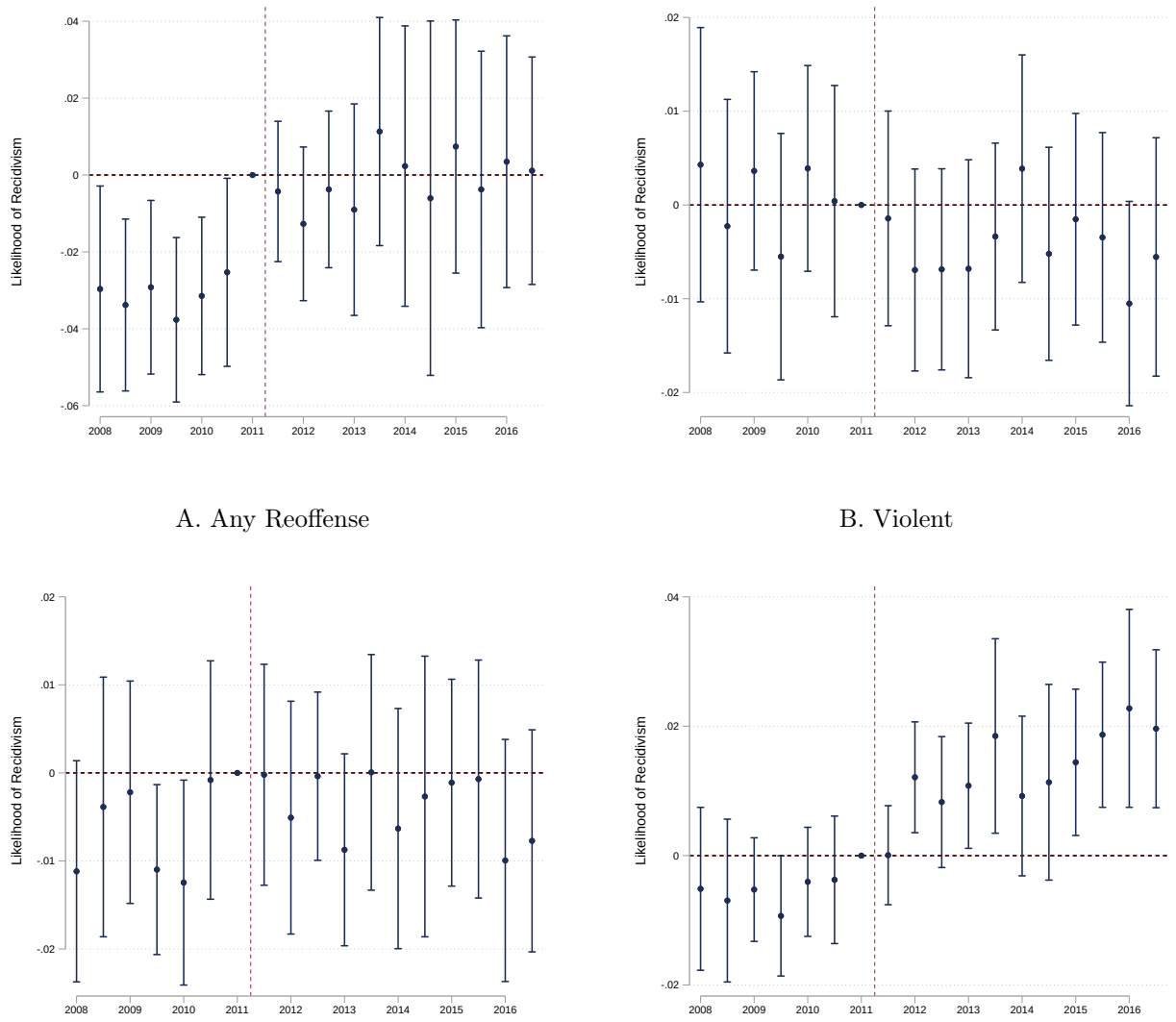
A. Fines Only

B. Guilty Plea

C. 1-Year Recidivism

D. 3-Year Recidivism

Figure A2 The figure plots the effects of the 2011 court-appointed attorney pay reduction on the likelihood of a fines-only sentence and a guilty plea, and on incapacitation-adjusted one- and three-year recidivism. It shows whether each effect begins at the cut or reflects differences already present before it. The dashed vertical line separates the omitted first half of 2011 from the second half. The comparison group and specification are the same as in Figure 1. Capped spikes are 95% confidence intervals. Standard errors are clustered at the county level.



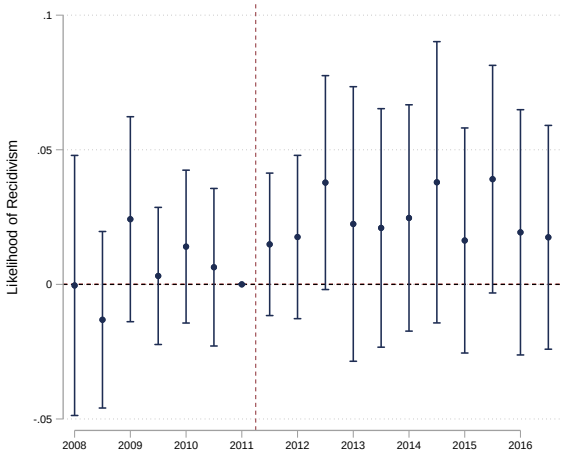
A. Any Reoffense

B. Violent

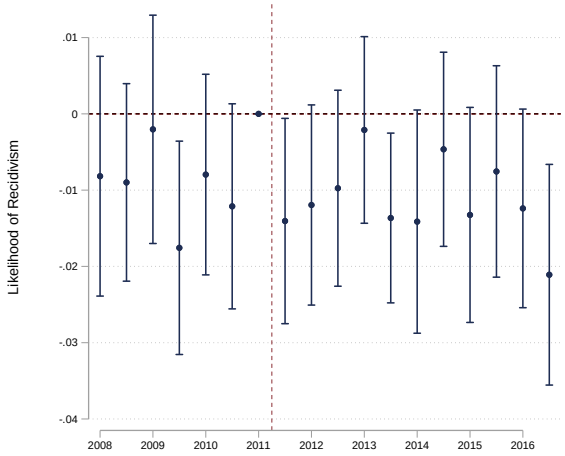
C. Property

D. Drug or Alcohol

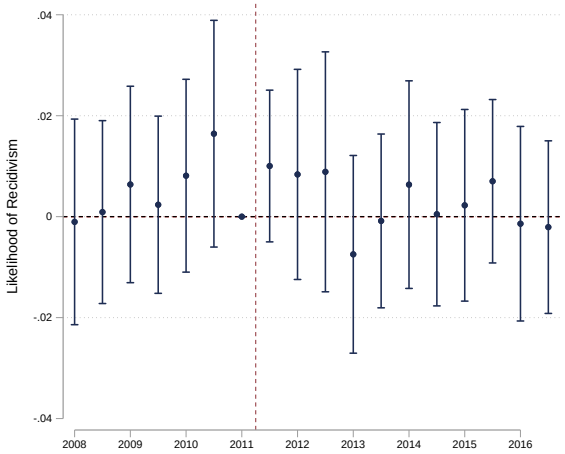
Figure A3 The figure plots the effects of the 2011 court-appointed attorney pay reduction on incapacitation-adjusted one-year recidivism in misdemeanor cases, overall and by the offense type of the defendant's next case. It traces the estimates of Table 3, Panel A, column (2), by half-year, to show whether they begin at the cut. The dashed vertical line separates the omitted first half of 2011 from the second half. The comparison group and specification are the same as in Figure 1. Capped spikes are 95% confidence intervals. Standard errors are clustered at the county level. The p-values of joint tests that the six 2008–2010 coefficients equal zero are 0.012 in panel (a), 0.36 in (b), 0.064 in (c), and 0.46 in (d).



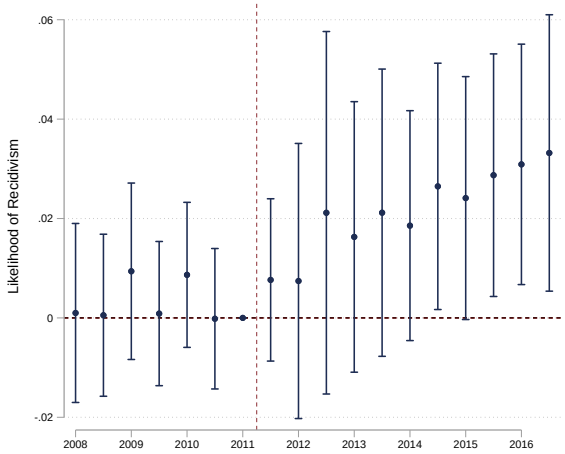
A. Any Reoffense



B. Violent

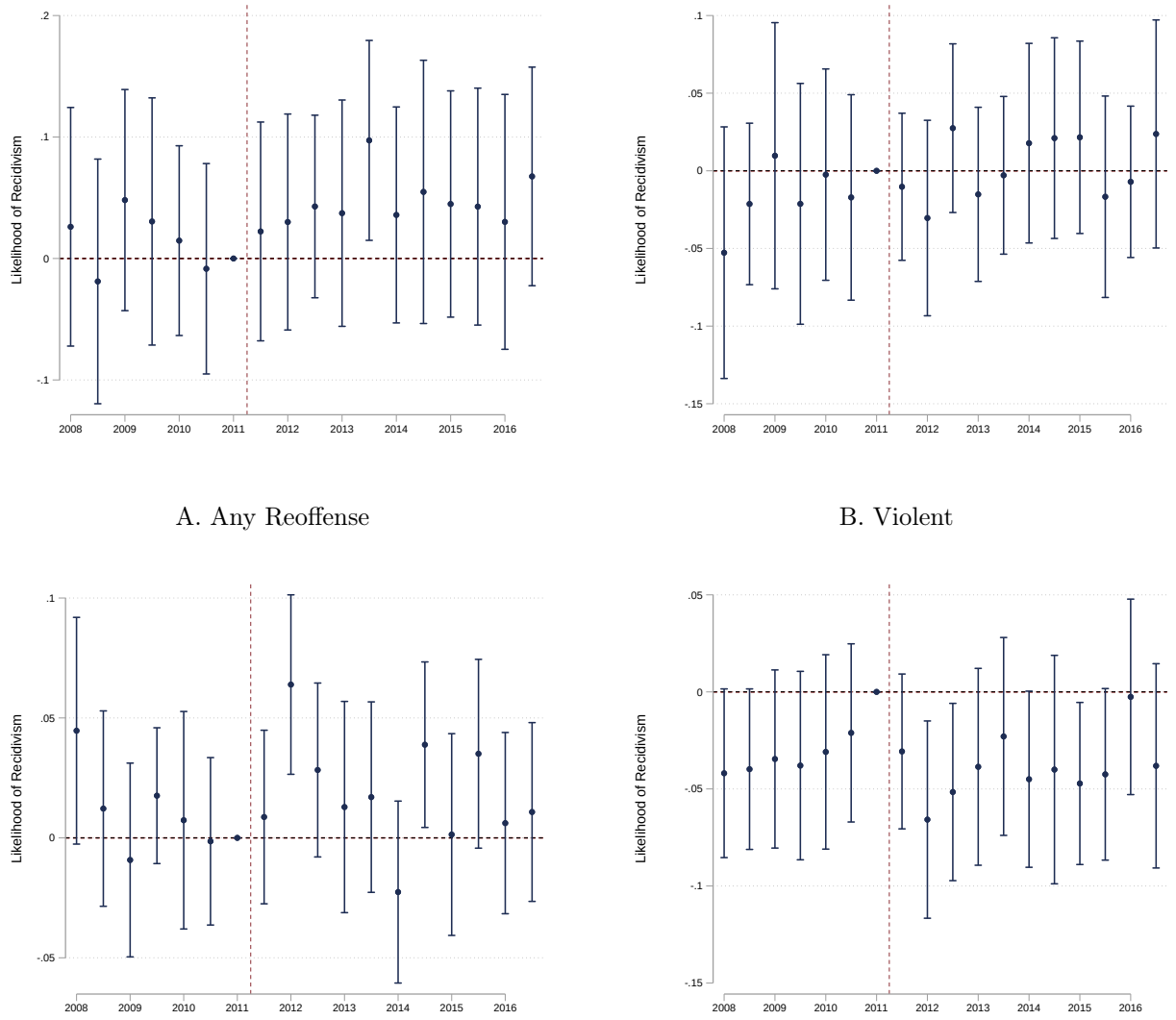


C. Property



D. Drug or Alcohol

Figure A4 The figure plots the effects of the 2011 court-appointed attorney pay reduction on incapacitation-adjusted one-year recidivism in lower-level felony cases (Class E–I), overall and by the offense type of the defendant's next case. It traces the estimates of Table 3, Panel A, column (3), by half-year, to show whether they begin at the cut. The dashed vertical line separates the omitted first half of 2011 from the second half. The comparison group and specification are the same as in Figure 1. Capped spikes are 95% confidence intervals. Standard errors are clustered at the county level. The p-values of joint tests that the six 2008–2010 coefficients equal zero are 0.23 in panel (a), 0.11 in (b), 0.35 in (c), and 0.56 in (d).



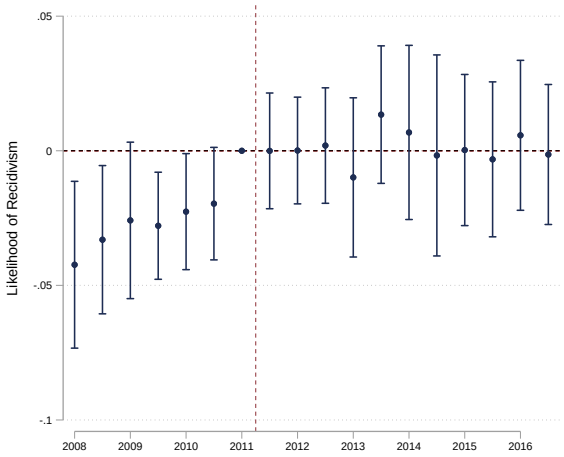
A. Any Reoffense

B. Violent

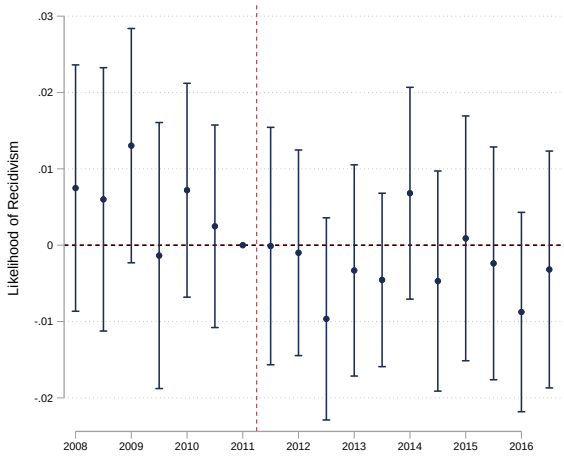
C. Property

D. Drug or Alcohol

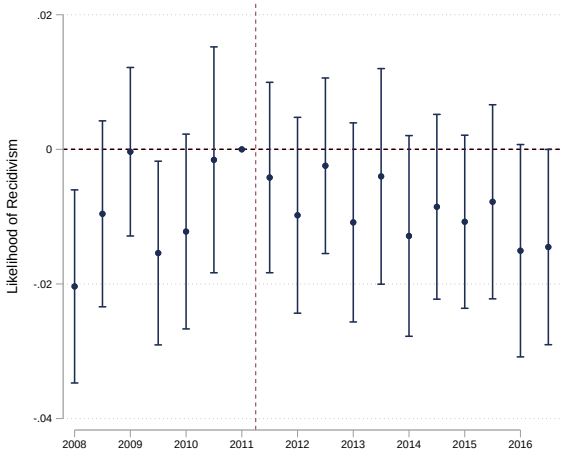
Figure A5 The figure plots the effects of the 2011 court-appointed attorney pay reduction on incapacitation-adjusted one-year recidivism in higher-level felony cases (Class A–D), overall and by the offense type of the defendant's next case. It traces the estimates of Table 3, Panel A, column (4), by half-year, to show whether they begin at the cut. The dashed vertical line separates the omitted first half of 2011 from the second half. The comparison group and specification are the same as in Figure 1. Capped spikes are 95% confidence intervals. Standard errors are clustered at the county level. The p-values of joint tests that the six 2008–2010 coefficients equal zero are 0.78 in panel (a), 0.50 in (b), 0.21 in (c), and 0.42 in (d).



A. Any Reoffense



B. Violent

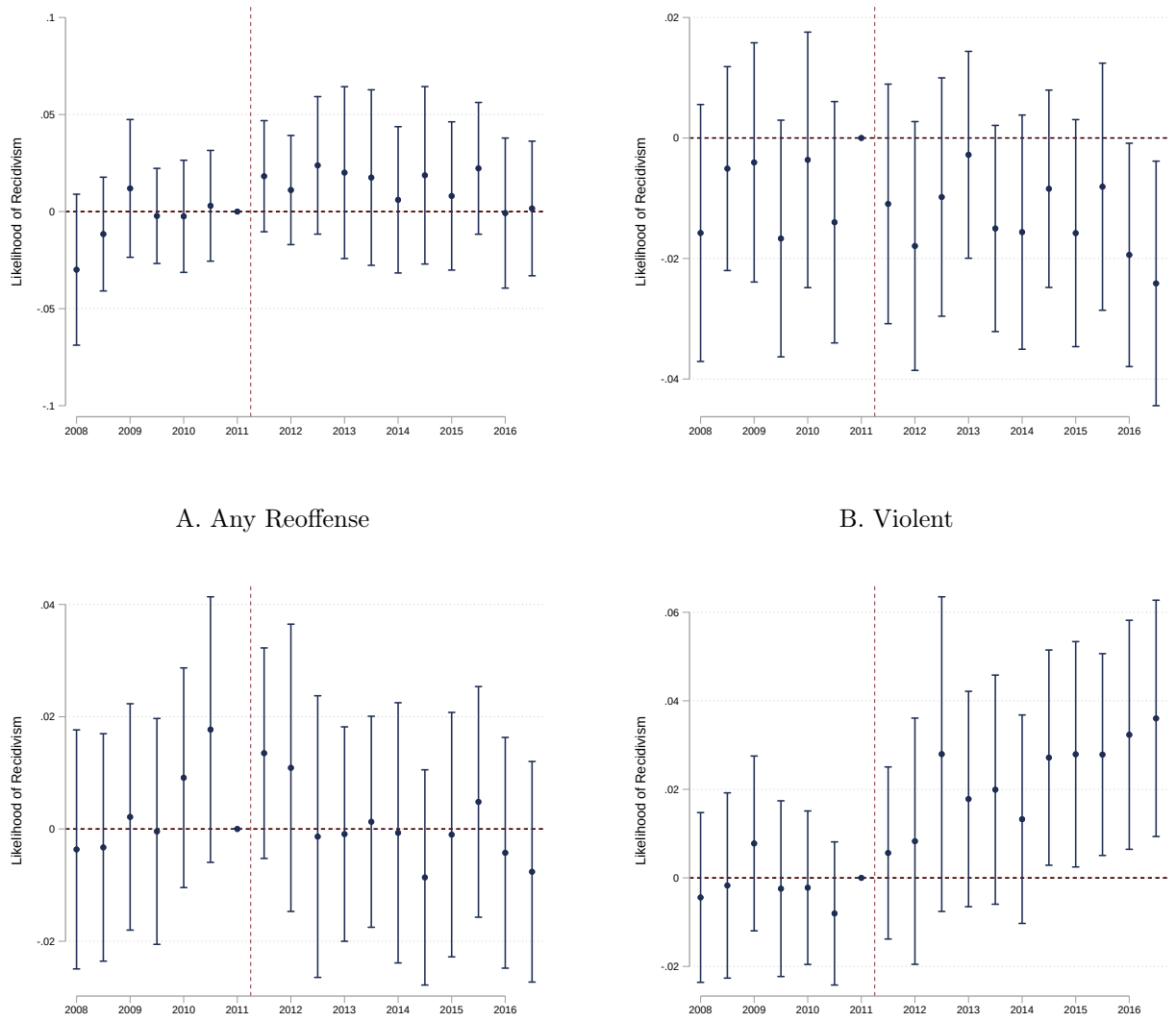


C. Property



D. Drug or Alcohol

Figure A6 The figure plots the effects of the 2011 court-appointed attorney pay reduction on incapacitation-adjusted three-year recidivism in misdemeanor cases, overall and by the offense type of the defendant's next case. It traces the estimates of Table 3, Panel B, column (2), by half-year, to show whether they begin at the cut. The dashed vertical line separates the omitted first half of 2011 from the second half. The comparison group and specification are the same as in Figure 1. Capped spikes are 95% confidence intervals. Standard errors are clustered at the county level. The p-values of joint tests that the six 2008–2010 coefficients equal zero are 0.038 in panel (a), 0.10 in (b), 0.019 in (c), and 0.45 in (d).



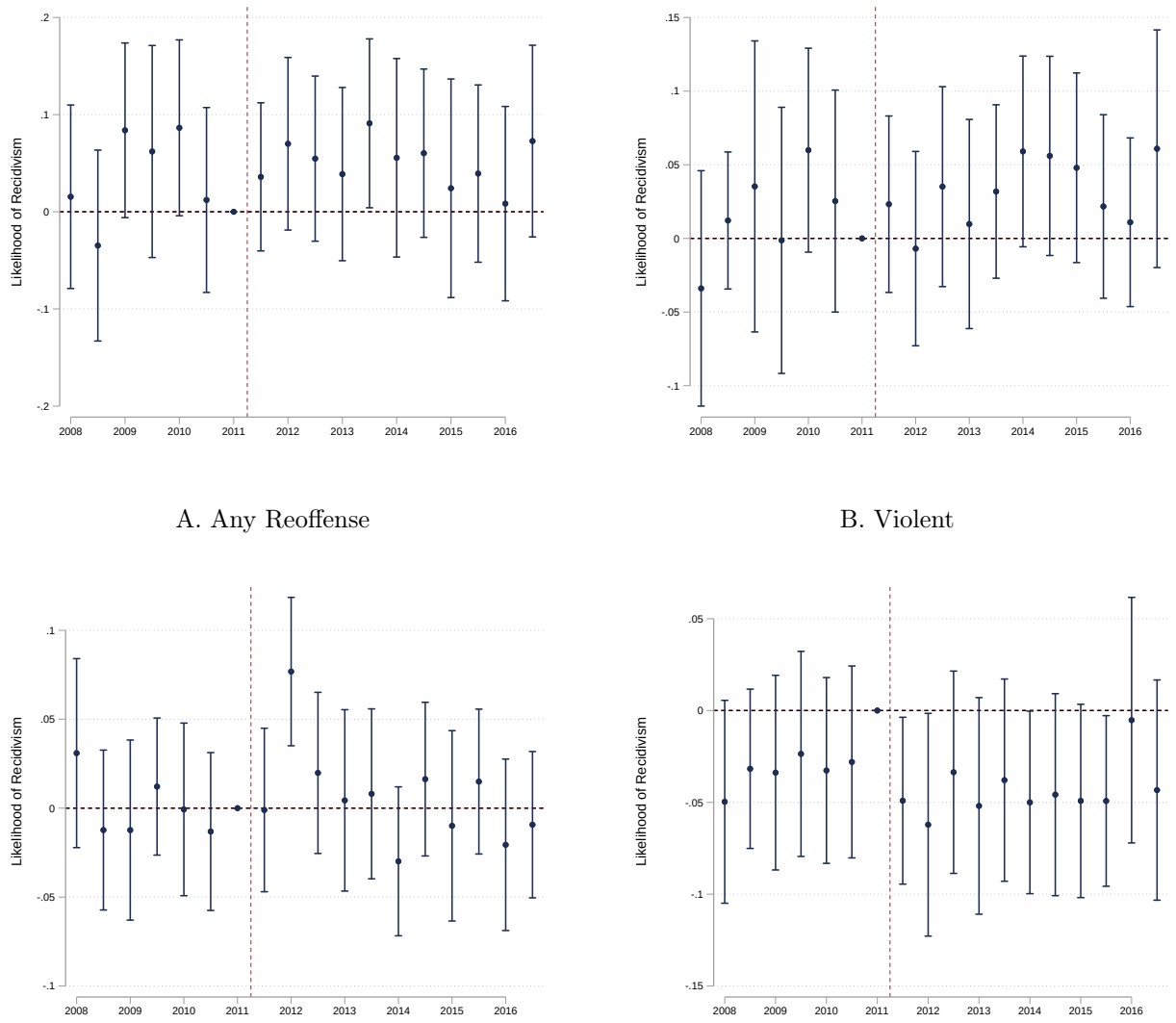
A. Any Reoffense

B. Violent

C. Property

D. Drug or Alcohol

Figure A7 The figure plots the effects of the 2011 court-appointed attorney pay reduction on incapacitation-adjusted three-year recidivism in lower-level felony cases (Class E–I), overall and by the offense type of the defendant's next case. It traces the estimates of Table 3, Panel B, column (3), by half-year, to show whether they begin at the cut. The dashed vertical line separates the omitted first half of 2011 from the second half. The comparison group and specification are the same as in Figure 1. Capped spikes are 95% confidence intervals. Standard errors are clustered at the county level. The p-values of joint tests that the six 2008–2010 coefficients equal zero are 0.30 in panel (a), 0.23 in (b), 0.38 in (c), and 0.38 in (d).



A. Any Reoffense

B. Violent

C. Property

D. Drug or Alcohol

Figure A8 The figure plots the effects of the 2011 court-appointed attorney pay reduction on incapacitation-adjusted three-year recidivism in higher-level felony cases (Class A–D), overall and by the offense type of the defendant's next case. It traces the estimates of Table 3, Panel B, column (4), by half-year, to show whether they begin at the cut. The dashed vertical line separates the omitted first half of 2011 from the second half. The comparison group and specification are the same as in Figure 1. Capped spikes are 95% confidence intervals. Standard errors are clustered at the county level. The p-values of joint tests that the six 2008–2010 coefficients equal zero are 0.045 in panel (a), 0.28 in (b), 0.53 in (c), and 0.58 in (d).

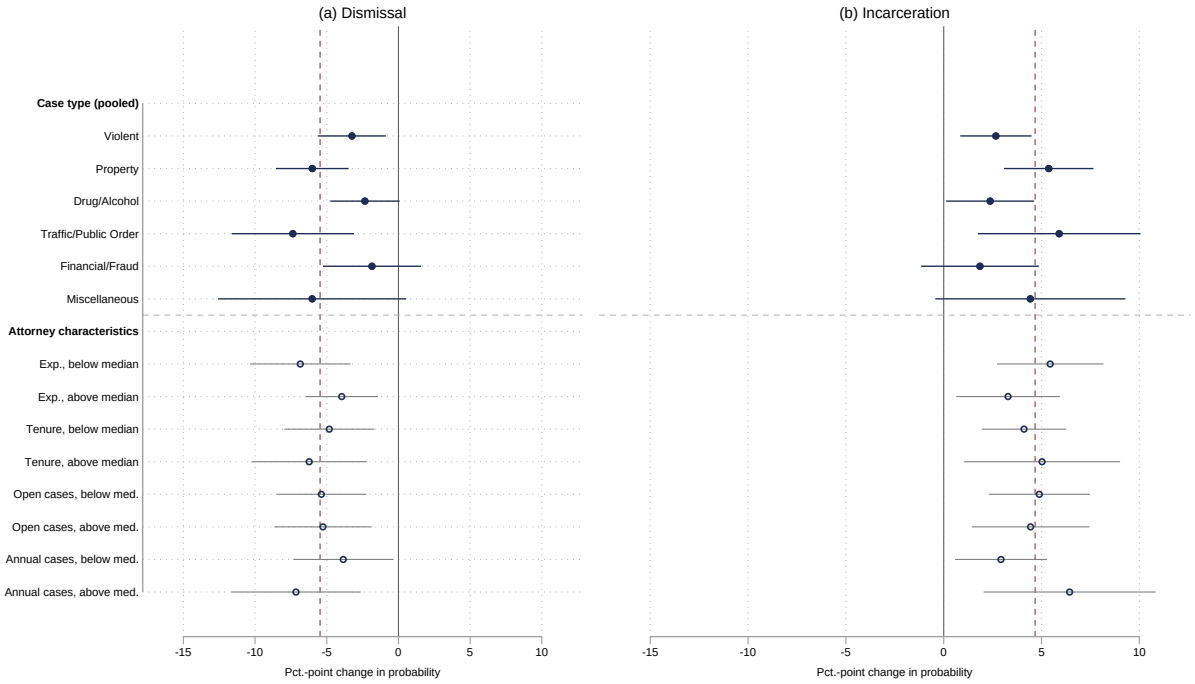
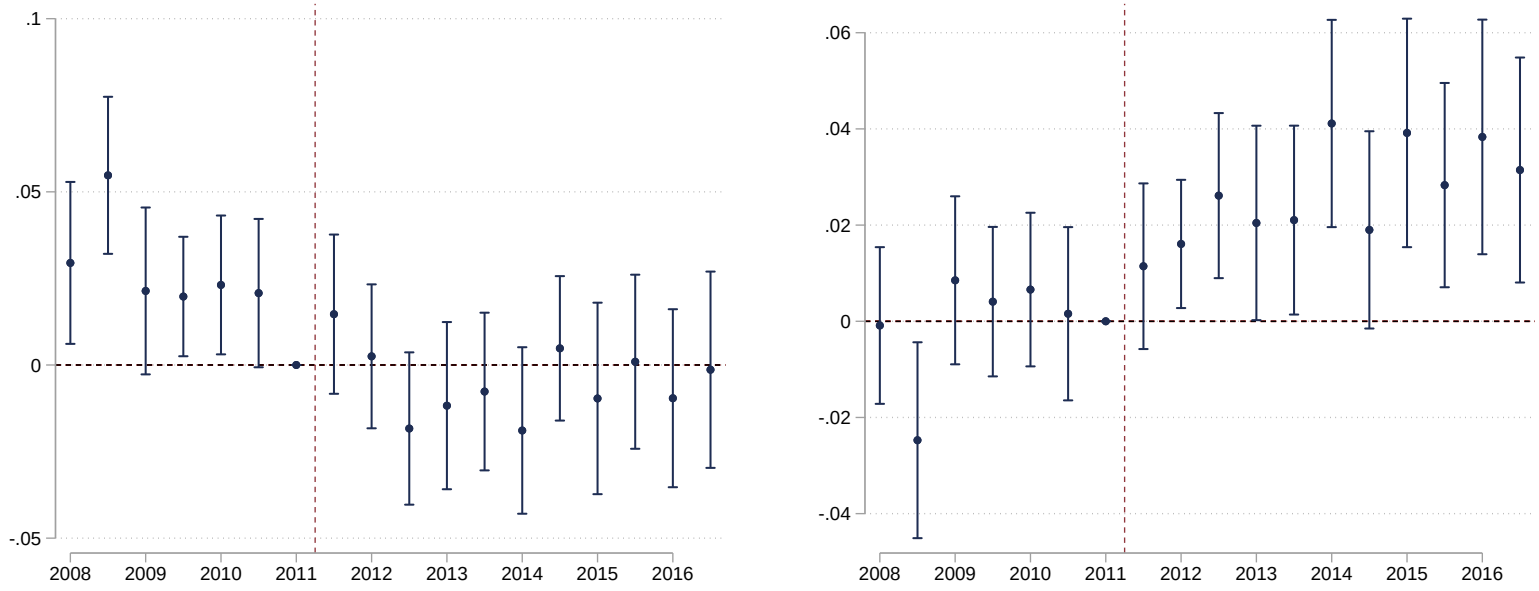


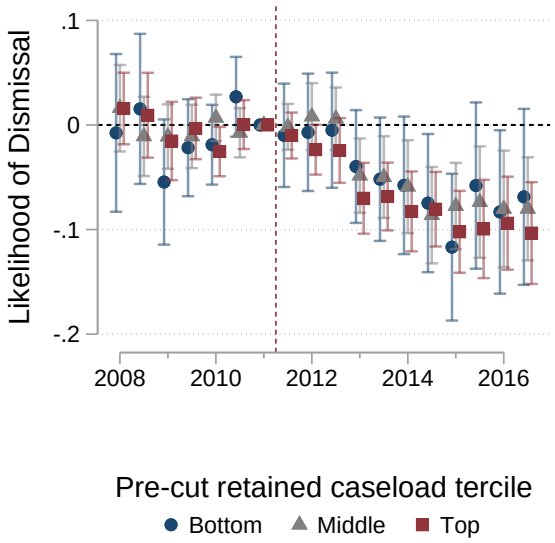
Figure A9 The figure plots the effects of the 2011 court-appointed attorney pay reduction on the likelihood of dismissal and incarceration in subsamples, using the specification of Table 2. Filled circles are crime-type subsamples, pooled across appointment lists with list-type fixed effects. Open circles split misdemeanor cases at the median of each attorney characteristic, computed within counsel type. The dashed vertical line is the misdemeanor estimate in Table 2, column (2). Horizontal lines are 95% confidence intervals. Standard errors are clustered at the county level.



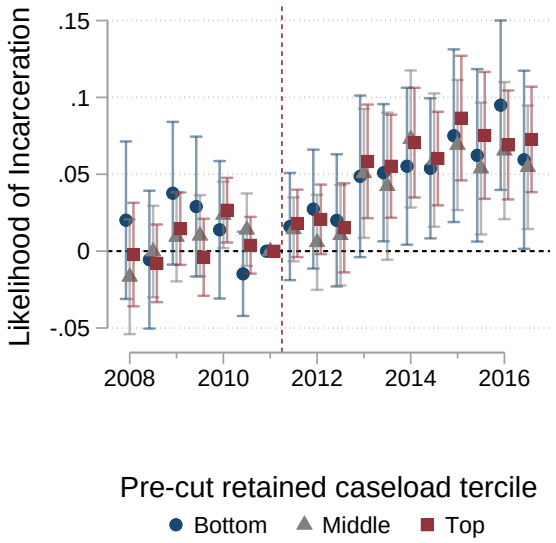
A. Dismissal

B. Incarceration

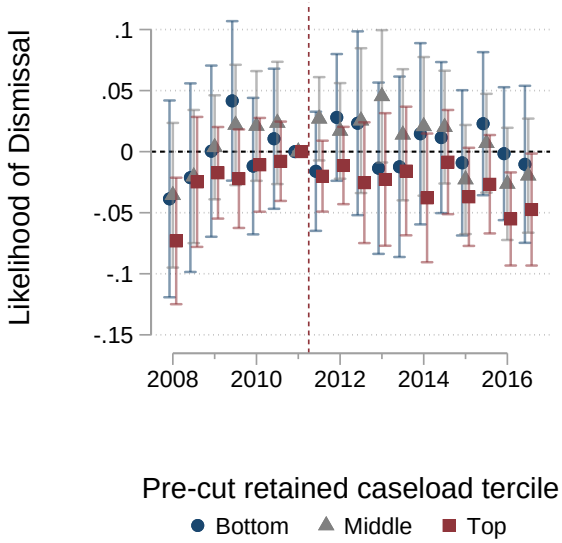
Figure A10 The figure plots the effects of the 2011 court-appointed attorney pay reduction on the likelihood of case dismissal and incarceration, comparing each attorney's appointed cases with the same attorney's retained cases. The sample and specification are those of Table 5, column (1), with the appointed-case indicator interacted with half-year of assignment. The sample consists of attorneys with both appointed and retained cases, and each regression includes attorney, appointed-case, county, half-year, and charge-class fixed effects and the defendant controls. The dashed vertical line separates the omitted first half of 2011 from the second half. Capped spikes are 95% confidence intervals. Standard errors are two-way clustered at the attorney and county levels. The six pre-period coefficients are jointly different from zero ($F(6, 97) = 4.20$, $p < 0.001$, for dismissal; $F(6, 97) = 2.81$, $p = 0.014$, for incarceration). The four coefficients for 2009 and 2010 do not differ from one another ($p = 0.99$ and 0.92), and for dismissal they lie about 2 percentage points above the omitted half-year.



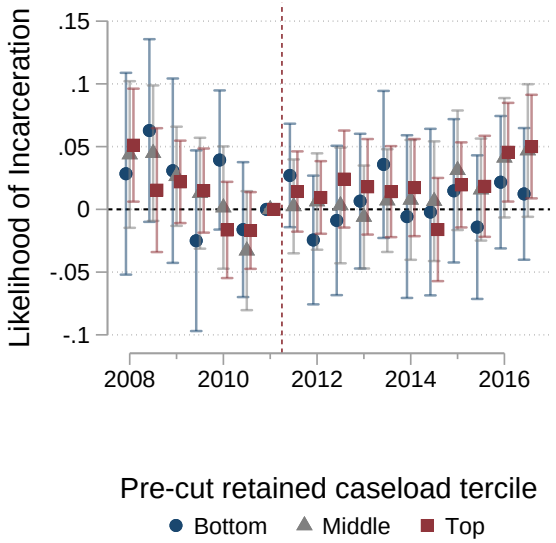
A. Misdemeanor Dismissal



B. Misdemeanor Incarceration



C. Felony Dismissal



D. Felony Incarceration

Figure A11 The figure plots the effects of the 2011 court-appointed attorney pay reduction on the likelihood of dismissal and incarceration separately for each tercile of attorneys' retained cases per year before the cut. Each series compares the appointed cases of attorneys in one tercile with public-defender cases, relative to the first half of 2011. The sample, tercile cutoffs, and specification, including attorney fixed effects, are those of Table A7. Misdemeanor panels use the misdemeanor appointment list and felony panels the two felony lists pooled. The dashed vertical line separates the omitted first half of 2011 from the second half. Capped spikes are 95% confidence intervals. Standard errors are clustered at the county level. The six pre-cut coefficients are jointly different from zero at the 5 percent level in 5 of the 12 series, and equal pre-cut coefficients across terciles are rejected in all four panels.

Table A1 Pay-Cut Effect by Defendant Race, Sex, and Prior Record

	Defendant race			Defendant sex		Defendant prior record	
	White	Black	Hispanic	Male	Female	First-time	Prior record
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Misdemeanor							
Dismissal	-0.044***	-0.063***	-0.064**	-0.055***	-0.051***	-0.032**	-0.082***
	(0.016)	(0.017)	(0.025)	(0.019)	(0.013)	(0.014)	(0.023)
Incarceration	0.038***	0.046***	0.039*	0.047**	0.042***	0.025***	0.072**
	(0.013)	(0.017)	(0.020)	(0.018)	(0.009)	(0.008)	(0.027)
Observations	164,821	172,288	9,676	243,442	112,602	199,368	156,676
Panel B. Lower-Level Felony							
Dismissal	-0.019	-0.011	0.040	-0.013	-0.018	-0.014	-0.012
	(0.015)	(0.031)	(0.058)	(0.026)	(0.022)	(0.030)	(0.022)
Incarceration	0.009	0.012	-0.013	0.016	0.009	0.007	0.021
	(0.013)	(0.022)	(0.051)	(0.018)	(0.020)	(0.019)	(0.017)
Observations	82,235	85,438	4,766	135,808	40,974	78,076	98,706
Panel C. Higher-Level Felony							
Dismissal	0.005	0.063*	0.071	0.037	0.098*	0.038	0.039
	(0.037)	(0.034)	(0.072)	(0.035)	(0.053)	(0.036)	(0.041)
Incarceration	-0.023	-0.065**	-0.070	-0.047	-0.065	-0.043	-0.046
	(0.035)	(0.031)	(0.065)	(0.031)	(0.060)	(0.033)	(0.037)
Observations	5,815	9,351	740	14,467	1,894	7,731	8,629

Note: This table shows which defendants bore the cost of the cut. Specification as in Table 2, estimated separately for each attorney list type and defendant group. The race groups are mutually exclusive, and defendants recorded as another race fall in none of columns (1)–(3). Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A2 Pay-Cut Effect by Crime Type, by Appointment List

	Violent	Property	Drug/ Alcohol	Traffic/ Public Order	Financial/ Fraud	Miscel- laneous	p(joint equality)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Misdemeanor							
Dismissal	-0.036**	-0.076***	-0.052**	-0.078***	-0.011	-0.064	0.006
	(0.015)	(0.016)	(0.022)	(0.025)	(0.031)	(0.041)	
Incarceration	0.030**	0.061***	0.047**	0.062**	0.031	0.051	0.039
	(0.012)	(0.016)	(0.019)	(0.025)	(0.021)	(0.031)	
Observations	128,481	81,728	71,834	60,731	9,909	3,352	
Panel B. Lower-Level Felony							
Dismissal	-0.025	-0.013	0.021	-0.037	-0.048**	-0.003	0.226
	(0.025)	(0.025)	(0.035)	(0.024)	(0.019)	(0.082)	
Incarceration	0.020	0.024	-0.013	0.034	0.027	-0.007	0.291
	(0.021)	(0.017)	(0.028)	(0.021)	(0.019)	(0.063)	
Observations	19,413	55,599	54,615	15,978	30,230	935	
Panel C. Higher-Level Felony							
Dismissal	0.050	-0.012	-0.086	0.045	0.183	–	0.195
	(0.036)	(0.040)	(0.116)	(0.132)	(0.571)		
Incarceration	-0.057*	0.001	0.061	-0.074	-0.275	–	0.308
	(0.031)	(0.042)	(0.117)	(0.121)	(0.511)		
Observations	12,675	2,151	948	470	68	–	

Note: This table shows whether the pay-cut effect is concentrated in particular crime types. Specification as in Table 2, estimated separately for each attorney list type and crime type. Column (7) reports the p-value from a joint test that the crime-type coefficients in the row are equal. A dash marks a cell with too few cases to estimate. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A3 Pay-Cut Effect by Attorney Experience, Tenure, and Caseload

	Experience		Tenure		Open caseload		Annual caseload	
	Below (1)	Above (2)	Below (3)	Above (4)	Below (5)	Above (6)	Below (7)	Above (8)
Panel A. Misdemeanor								
Dismissal	-0.068***	-0.040***	-0.048***	-0.062***	-0.054***	-0.053***	-0.038**	-0.071***
	(0.018)	(0.013)	(0.016)	(0.020)	(0.016)	(0.017)	(0.018)	(0.023)
Incarceration	0.054***	0.033**	0.041***	0.050**	0.049***	0.044***	0.029**	0.064***
	(0.014)	(0.013)	(0.011)	(0.020)	(0.013)	(0.015)	(0.012)	(0.022)
Observations	171,301	170,577	184,326	170,998	177,755	178,289	179,261	176,783
Panel B. Lower-Level Felony								
Dismissal	-0.019	-0.009	-0.022	-0.006	-0.016	-0.018	-0.007	-0.021
	(0.030)	(0.022)	(0.024)	(0.029)	(0.030)	(0.022)	(0.039)	(0.015)
Incarceration	0.023	0.010	0.022	0.010	0.016	0.020	0.008	0.019
	(0.020)	(0.017)	(0.016)	(0.021)	(0.021)	(0.016)	(0.028)	(0.015)
Observations	86,540	86,194	91,759	84,211	88,683	88,101	90,391	86,392
Panel C. Higher-Level Felony								
Dismissal	0.027	0.059	0.035	0.047	0.072**	-0.023	0.052	0.036
	(0.031)	(0.046)	(0.039)	(0.031)	(0.031)	(0.035)	(0.034)	(0.043)
Incarceration	-0.029	-0.064	-0.046	-0.043	-0.075***	0.008	-0.056*	-0.046
	(0.027)	(0.043)	(0.036)	(0.027)	(0.028)	(0.031)	(0.031)	(0.038)
Observations	8,057	7,955	8,823	7,448	8,242	8,119	8,658	7,704

Note: Specification as in Table 2, estimated separately for each attorney list type and for cases below and above each characteristic's median, taken within list type and counsel type. Experience is years since bar admission, and its columns exclude attorneys without an admission date. Tenure is years since the attorney's first observed case. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A4 Attorney Turnover and the Composition Channel of the 2011 Pay Cut

	Entrants − stayers (1)	Exiters − stayers (2)	Entrants − exiters (3)	Turnover effect (4)	Share of total (5)
A. Dismissal					
All lists (0.22 / 0.10)	-0.79*	-0.24	-0.55	-0.15	4%
	(0.43)	(0.66)	(0.74)	(0.11)	
Misdemeanor (0.23 / 0.11)	-0.18	0.22	-0.40	-0.07	1%
	(0.51)	(0.82)	(0.91)	(0.14)	
Felony (0.21 / 0.09)	-2.04***	-1.05	-0.99	-0.34**	37%
	(0.59)	(1.12)	(1.25)	(0.16)	
B. Incarceration					
All lists (0.22 / 0.10)	1.00***	0.32	0.68	0.19**	5%
	(0.35)	(0.60)	(0.67)	(0.10)	
Misdemeanor (0.23 / 0.11)	0.24	0.60	-0.36	-0.01	0%
	(0.41)	(0.65)	(0.75)	(0.12)	
Felony (0.21 / 0.09)	2.50***	-0.35	2.85**	0.57***	61%
	(0.55)	(1.05)	(1.17)	(0.15)	
Entrants / exiters / stayers	1,025 / 587 / 1,255				
Cases	213,436				

Note: Court-appointed cases from the Table 2 sample, restricted to attorneys matched to a bar number. Attorneys are divided into three groups, separately on each appointment list (the misdemeanor list and the two felony lists pooled), as in Table 4. Entrants take appointed cases only after the cut, exiters take cases only before the cut, and stayers are in both periods. Columns (1) and (2) are coefficients on entrant and exiter indicators from a case-level regression with county × list × month and charge-class fixed effects and defendant controls, so each group is compared with stayers in the same cells. Column (3) is their difference. Column (4) is $s_E \times (1) - s_X \times (2)$, where the row labels give entrants' share of post-cut cases (s_E) and exiters' share of pre-cut cases (s_X). Column (5) divides column (4) by the pay-cut effect in column (1) of Table 4. Because the felony effects in that denominator are not statistically different from zero, the felony shares are not informative. The last row counts attorney-by-list units. Entries are in percentage points. Standard errors clustered at the attorney level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5 Pay-Cut Effect by Attorney Entry/Exit Status

	All (1)	All, bar-matched (2)	Stayers only (3)	Stayers to entrants (4)	Exiters to stayers (5)	Exiters to entrants (6)
A. All lists						
Dismissal	-0.0421***	-0.0471***	-0.0448***	-0.0599***	-0.0414***	-0.0609***
	(0.0110)	(0.0105)	(0.0102)	(0.0140)	(0.0127)	(0.0161)
Incarceration	0.0375***	0.0409***	0.0384***	0.0500***	0.0404***	0.0554***
	(0.0105)	(0.0106)	(0.0104)	(0.0136)	(0.0124)	(0.0165)
Guilty plea	0.0290***	0.0341***	0.0310***	0.0557***	0.0117	0.0449***
	(0.0102)	(0.0097)	(0.0096)	(0.0118)	(0.0139)	(0.0140)
N (cases)	549,197	536,048	495,755	413,473	443,461	361,179
CAA cases, pre-cut	74,768	66,144	59,219	59,219	6,925	6,925
CAA cases, post-cut	153,543	149,018	115,650	33,368	115,650	33,368
Entrants / exiters / stayers				994 / 556 / 1,250		
B. Misdemeanor						
Dismissal	-0.0547***	-0.0619***	-0.0592***	-0.0775***	-0.0598***	-0.0866***
	(0.0162)	(0.0159)	(0.0151)	(0.0216)	(0.0174)	(0.0239)
Incarceration	0.0467***	0.0518***	0.0493***	0.0638***	0.0485***	0.0697***
	(0.0150)	(0.0155)	(0.0149)	(0.0190)	(0.0172)	(0.0226)
Guilty plea	0.0503***	0.0581***	0.0535***	0.0865***	0.0389***	0.0791***
	(0.0122)	(0.0116)	(0.0114)	(0.0156)	(0.0141)	(0.0179)
N (cases)	356,045	347,277	321,512	272,116	289,435	240,039
CAA cases, pre-cut	47,083	41,227	36,652	36,652	4,575	4,575
CAA cases, post-cut	94,688	91,776	70,586	21,190	70,586	21,190
Entrants / exiters / stayers				536 / 311 / 652		
C. Felony						
Dismissal	-0.0092	-0.0103	-0.0077	-0.0210	0.0006	-0.0227
	(0.0259)	(0.0258)	(0.0256)	(0.0263)	(0.0302)	(0.0295)
Incarceration	0.0093	0.0101	0.0064	0.0189	0.0096	0.0307
	(0.0183)	(0.0182)	(0.0181)	(0.0202)	(0.0221)	(0.0232)
Guilty plea	-0.0184	-0.0178	-0.0191	-0.0061	-0.0460	-0.0175
	(0.0278)	(0.0279)	(0.0277)	(0.0297)	(0.0329)	(0.0328)
N (cases)	193,151	188,770	174,242	141,356	154,025	121,139
CAA cases, pre-cut	27,685	24,917	22,567	22,567	2,350	2,350
CAA cases, post-cut	58,855	57,242	45,064	12,178	45,064	12,178
Entrants / exiters / stayers				458 / 245 / 598		

Note: This table tests whether the difference-in-differences estimate depends on which court-appointed attorneys handle the cases before and after the cut. Specification as in Table 2. Every column keeps all public-defender cases and varies only which court-appointed cases enter before and after the cut. Each heading names the attorneys whose cases enter before and after the cut. For example, "Stayers to entrants" keeps stayers' pre-cut cases and entrants' post-cut cases. Columns (2)–(6) use bar-matched attorneys only. Stayers, entrants, and exiters are classified on each appointment list as in Table A4. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A6 Pay-Cut Effect by Entry/Exit Status: Marginal (First/Last) Practice Year

		Bar-matched attorneys				
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-cut CAA cases from	All	All	Stayers	Stayers	Exiters, last year	Exiters, last year
Post-cut CAA cases from	All	All	Stayers	Entrants, first year	Stayers	Entrants, first year
A. All lists						
Dismissal	-0.0421***	-0.0471***	-0.0448***	-0.0603***	-0.0389**	-0.0638***
	(0.0110)	(0.0105)	(0.0102)	(0.0114)	(0.0151)	(0.0146)
Incarceration	0.0375***	0.0409***	0.0384***	0.0563***	0.0326**	0.0577***
	(0.0105)	(0.0106)	(0.0104)	(0.0101)	(0.0128)	(0.0129)
Guilty plea	0.0290***	0.0341***	0.0310***	0.0534***	0.0141	0.0468***
	(0.0102)	(0.0097)	(0.0096)	(0.0107)	(0.0148)	(0.0138)
N (cases)	549,197	536,048	495,755	392,211	440,240	336,696
CAA cases, pre-cut	74,768	66,144	59,219	59,219	3,704	3,704
CAA cases, post-cut	153,543	149,018	115,650	12,106	115,650	12,106
Entrants / exiters / stayers				991 / 553 / 1,250		
B. Misdemeanor						
Dismissal	-0.0547***	-0.0619***	-0.0592***	-0.0699***	-0.0530***	-0.0772***
	(0.0162)	(0.0159)	(0.0151)	(0.0192)	(0.0183)	(0.0215)
Incarceration	0.0467***	0.0518***	0.0493***	0.0624***	0.0330**	0.0554***
	(0.0150)	(0.0155)	(0.0149)	(0.0169)	(0.0167)	(0.0195)
Guilty plea	0.0503***	0.0581***	0.0535***	0.0787***	0.0341**	0.0672***
	(0.0122)	(0.0116)	(0.0114)	(0.0139)	(0.0167)	(0.0162)
N (cases)	356,045	347,277	321,512	259,465	287,310	225,263
CAA cases, pre-cut	47,083	41,227	36,652	36,652	2,450	2,450
CAA cases, post-cut	94,688	91,776	70,586	8,539	70,586	8,539
Entrants / exiters / stayers				536 / 309 / 652		
C. Felony						
Dismissal	-0.0092	-0.0103	-0.0077	-0.0213	-0.0034	-0.0264
	(0.0259)	(0.0258)	(0.0256)	(0.0279)	(0.0300)	(0.0307)
Incarceration	0.0093	0.0101	0.0064	0.0279	0.0181	0.0503**
	(0.0183)	(0.0182)	(0.0181)	(0.0208)	(0.0232)	(0.0251)
Guilty plea	-0.0184	-0.0178	-0.0191	-0.0088	-0.0298	0.0012
	(0.0278)	(0.0279)	(0.0277)	(0.0301)	(0.0304)	(0.0316)
N (cases)	193,151	188,770	174,242	132,745	152,929	111,432
CAA cases, pre-cut	27,685	24,917	22,567	22,567	1,254	1,254
CAA cases, post-cut	58,855	57,242	45,064	3,567	45,064	3,567
Entrants / exiters / stayers				455 / 244 / 598		

Note: This table checks that the contrasts in Table A5 do not reflect when entrants and exiters practiced. Specification, sample, and attorney groups as in Table A5, except that entrants' cases are limited to the 365 days beginning with their first appointed case and exiters' cases to the 365 days ending with their last. Columns (1)–(3) are therefore identical to Table A5. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7 The Pay-Cut Effect by Attorneys' Pre-Cut Retained Caseload

	Dismissal			Incarceration		
	Bottom (1)	Middle (2)	Top (3)	Bottom (4)	Middle (5)	Top (6)
Panel A. Misdemeanor						
Appointed Counsel × Pay Decrease	-3.33	-3.31**	-5.25***	3.37*	2.97**	4.09***
	(2.29)	(1.33)	(1.31)	(1.93)	(1.48)	(1.33)
Top − Bottom		-1.92			0.72	
		(1.96)			(1.54)	
Attorneys	311	273	289	311	273	289
Panel B. Felony						
Appointed Counsel × Pay Decrease	0.43	0.91	-0.44	-0.73	0.10	0.90
	(3.07)	(2.66)	(2.69)	(2.39)	(1.89)	(1.93)
Top − Bottom		-0.87			1.63	
		(1.76)			(1.68)	
Attorneys	260	220	244	260	220	244

Note: This table tests the conceptual framework's private-practice margin: whether attorneys with larger retained caseloads before the cut responded differently to it. Specification as in Table 2, with attorney fixed effects added. The sample includes all court-appointed attorneys with appointed cases on the panel's appointment list before the cut (Panel A, the misdemeanor list; Panel B, the two felony lists pooled, as in Table 4). Columns are terciles of their retained cases per year before the cut, with cutoffs set among attorneys with appointed cases on both sides of the cut. Those attorneys identify the effects, and the Attorneys row counts them. Top − Bottom is the difference between the top- and bottom-tercile coefficients from the same regression. Entries are in percentage points. Standard errors clustered at the county level (81 counties in Panel A, 82 in Panel B). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8 Effect of 2011 Pay Reduction on the Number of New Cases by Case Type

	Main (1)	Misdemeanor (2)	Lower-Level Felony (3)	Higher-Level Felony (4)
A. One-Year Recidivism				
All	0.030	0.026	0.037	0.079***
	(0.026)	(0.027)	(0.029)	(0.029)
Dep. mean	0.654	0.638	0.690	0.612
Violent	-0.011**	-0.014***	-0.005	0.034**
	(0.004)	(0.005)	(0.004)	(0.016)
Dep. mean	0.125	0.133	0.103	0.195
Property	-0.002	-0.001	-0.008	0.016
	(0.006)	(0.006)	(0.006)	(0.012)
Dep. mean	0.165	0.154	0.191	0.115
Drug	0.032**	0.031***	0.037**	0.002
	(0.012)	(0.012)	(0.016)	(0.010)
Dep. mean	0.112	0.105	0.130	0.090
B. Three-Year Recidivism				
All	0.055	0.057	0.057	0.069
	(0.054)	(0.061)	(0.047)	(0.050)
Dep. mean	1.425	1.442	1.409	1.231
Violent	-0.028***	-0.036***	-0.015**	0.047
	(0.008)	(0.011)	(0.007)	(0.029)
Dep. mean	0.282	0.305	0.226	0.368
Property	-0.008	-0.004	-0.017*	-0.015
	(0.009)	(0.010)	(0.010)	(0.023)
Dep. mean	0.346	0.340	0.368	0.233
Drug	0.072***	0.076***	0.070***	-0.007
	(0.025)	(0.026)	(0.025)	(0.021)
Dep. mean	0.245	0.238	0.266	0.190
County FE	X	X	X	X
Half-year FE	X	X	X	X
Controls	X	X	X	X

Note: This table reports the counts that the victim-cost rows of Table 11 price. Specification and sample as in Table 3. Each outcome is the number of new cases within one or three years of release (Table 1), in total and by type of most serious charge, whereas Table 3 records only the type of the next case. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix B. Attorney Value-Added

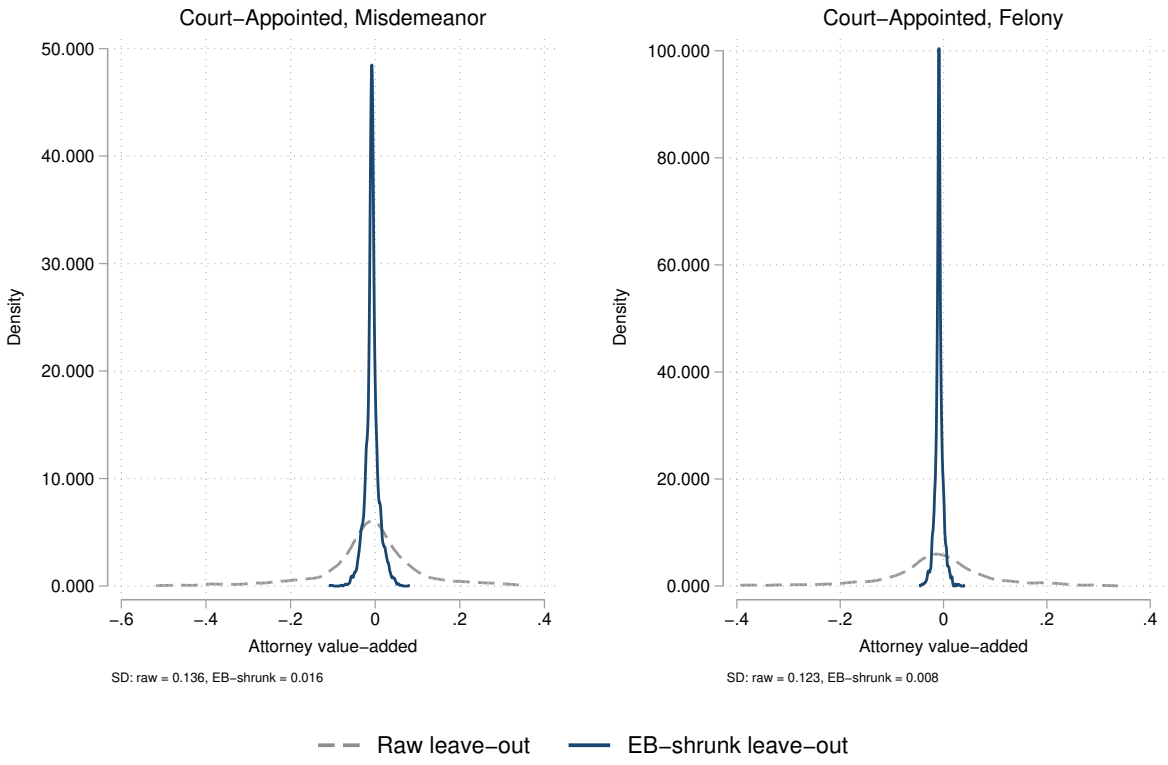


Figure B1 The figure plots kernel densities of court-appointed attorneys' dismissal value-added before and after empirical Bayes shrinkage, separately for misdemeanors and felonies. It shows how much shrinkage compresses the unshrunk measure. Each attorney contributes the mean of the leave-one-defendant-out measure over that attorney's cases. Densities are drawn over the 1st to 99th percentiles of the unshrunk measure, and each panel reports the standard deviation of each measure.

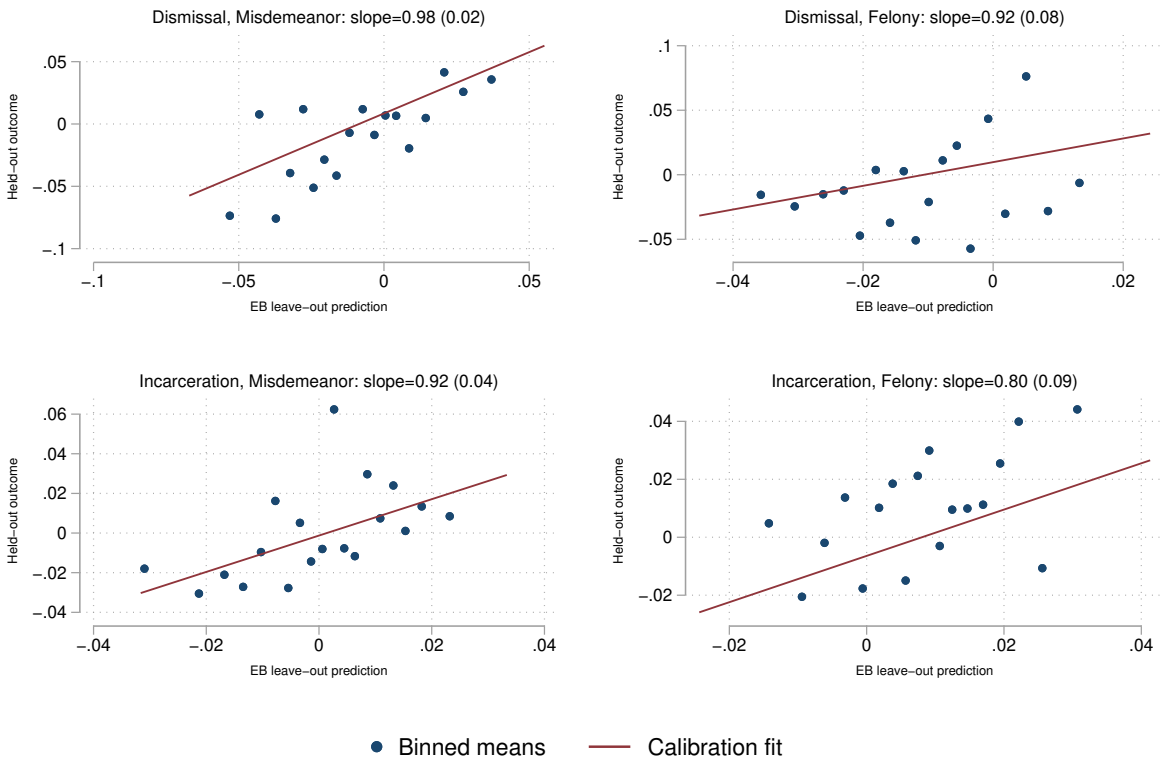


Figure B2 The figure plots the short form of the calibration test in Table B5, Panel A, on the quality measure of Table 8. It shows whether the slope holds across the range of the measure. Each point is the mean residual outcome of court-appointed defendants in one of twenty equal-sized bins of attorney–defendant pairs ordered by the empirical Bayes quality measure, with residuals net of the controls and fixed effects of Table 10, column (1). Lines are linear fits weighted by each pair's cases. Each panel reports the slope and its standard error, clustered at the attorney level, which are the EB entries of Panel A. Bins outside the central 95% of the measure are not shown.

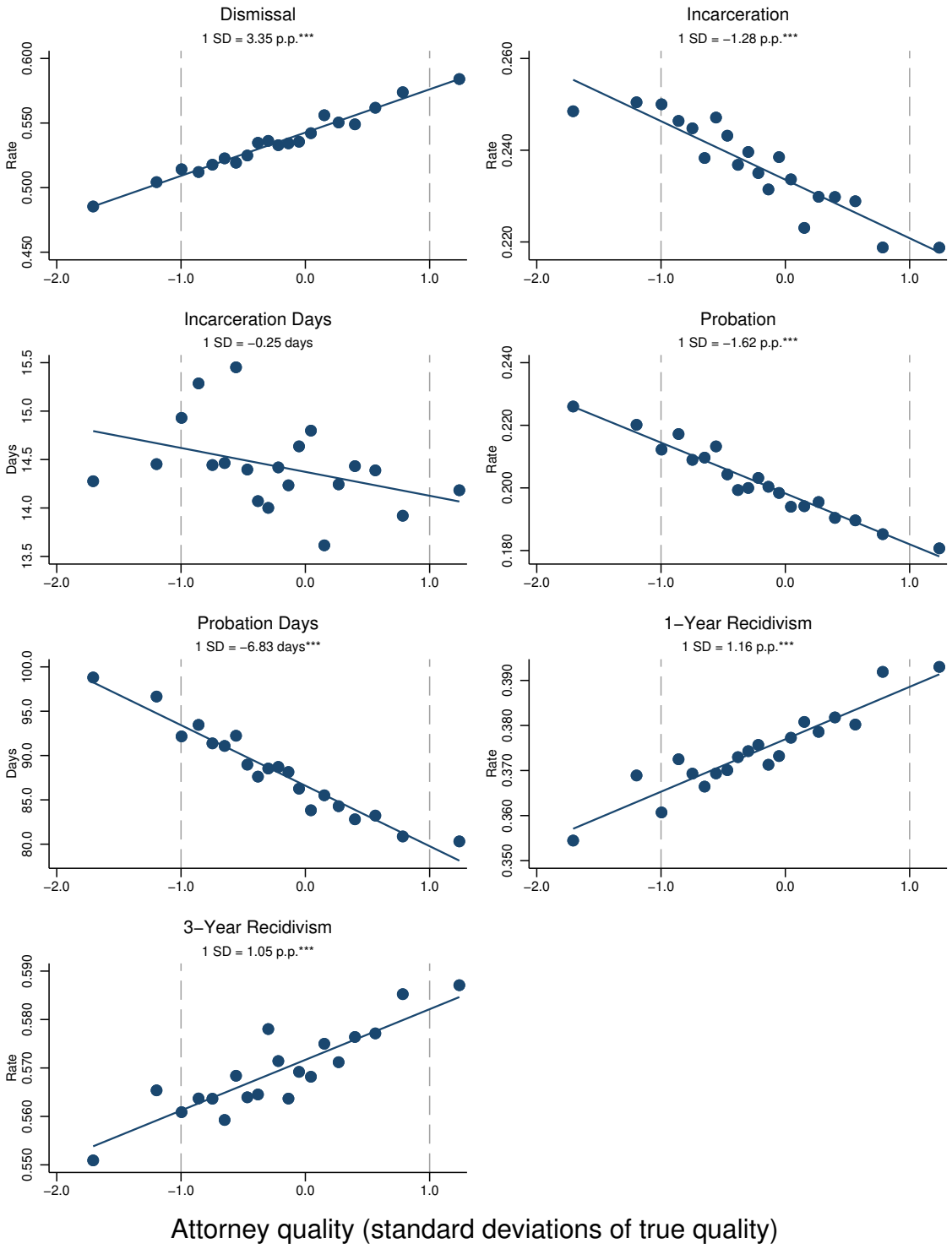


Figure B3 The figure plots binned means of each case outcome against dismissal quality for misdemeanor cases handled by court-appointed attorneys, using twenty equal-sized bins. It shows whether each relationship in Table 8 is linear across the quality distribution. Quality is the empirical Bayes quality measure divided by σ_θ from Table 7, so the horizontal axis is in standard deviations of true quality, and dashed lines mark ± 1. Both axes are residualized on demographic and criminal history controls and county $\times$ list $\times$ month fixed effects. Outcomes are defined as in Table 1, and sentence lengths include zeros. Each panel reports the effect of a one-standard-deviation increase in quality from the dismissal-quality row of Table 8, Panel A, with stars based on standard errors clustered at the attorney level. Figures B4–B6 show the other case types and quality measures. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

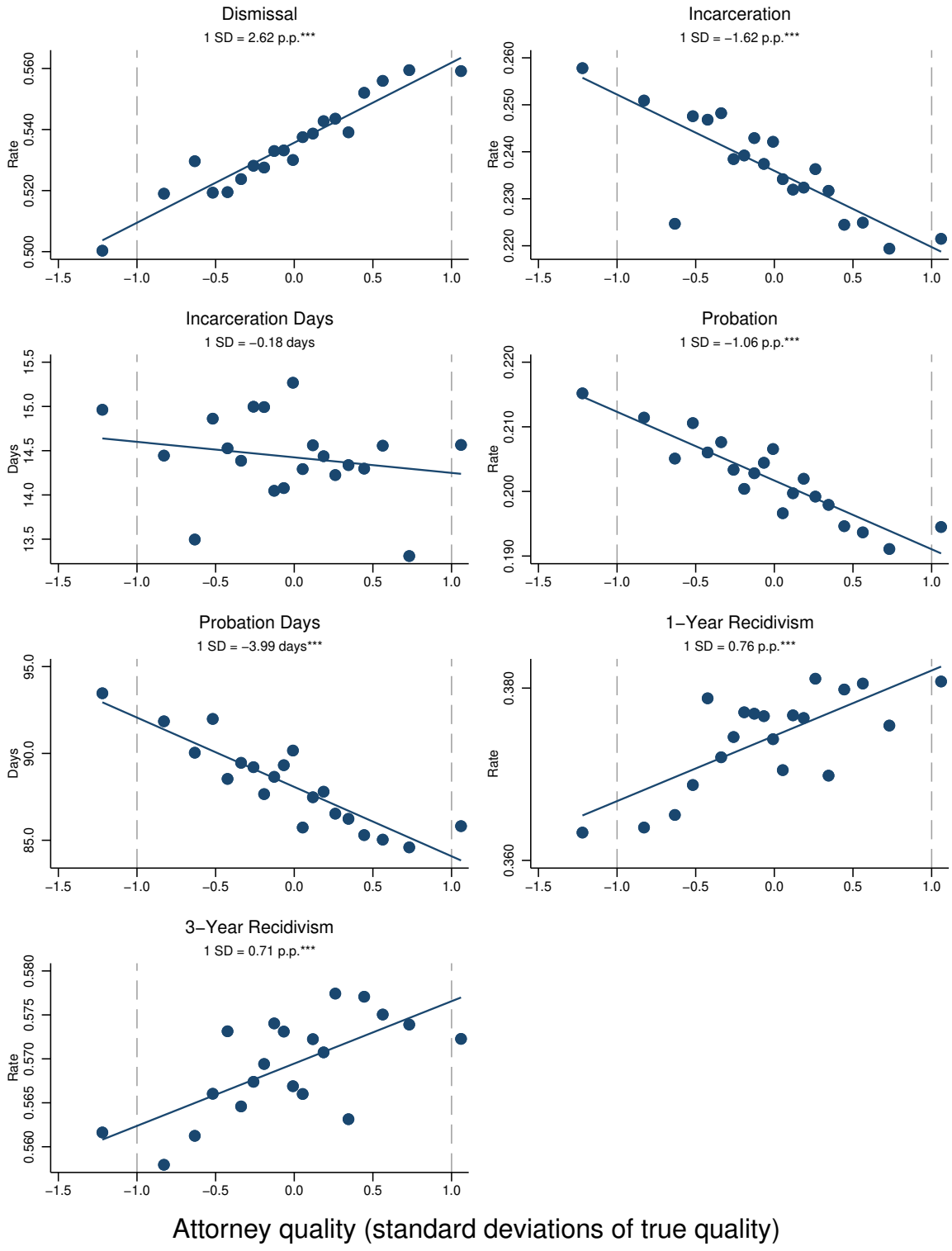


Figure B4 The figure plots binned means of each case outcome against incarceration avoidance quality, the incarceration value-added multiplied by −1, for misdemeanor cases handled by court-appointed attorneys, as in Figure B3. The slope printed on each panel is the corresponding cell of Table 8, Panel A, second row. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

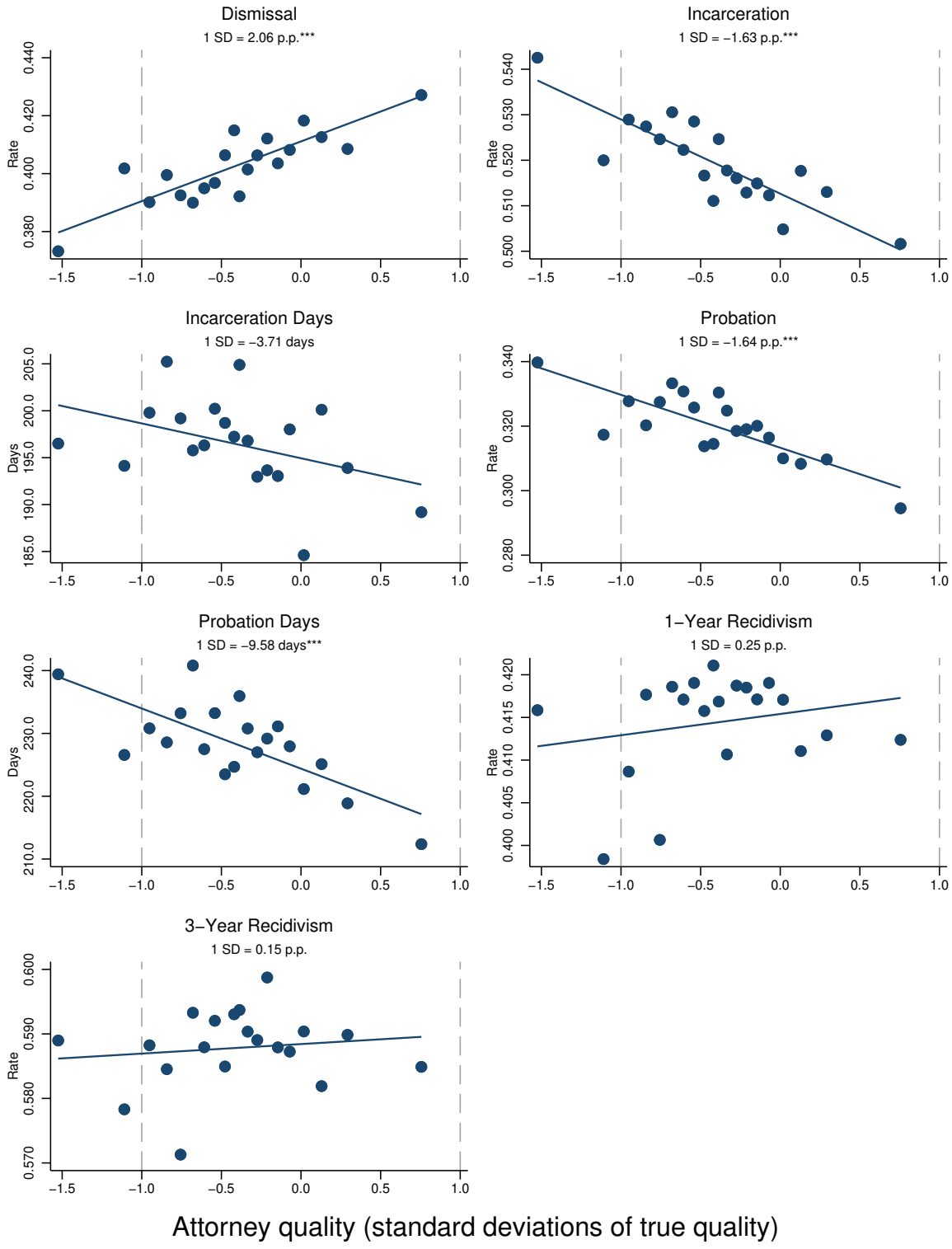


Figure B5 The figure plots binned means of each case outcome against dismissal quality for felony cases handled by court-appointed attorneys, as in Figure B3. The slope printed on each panel is the corresponding cell of Table 8, Panel B, first row. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

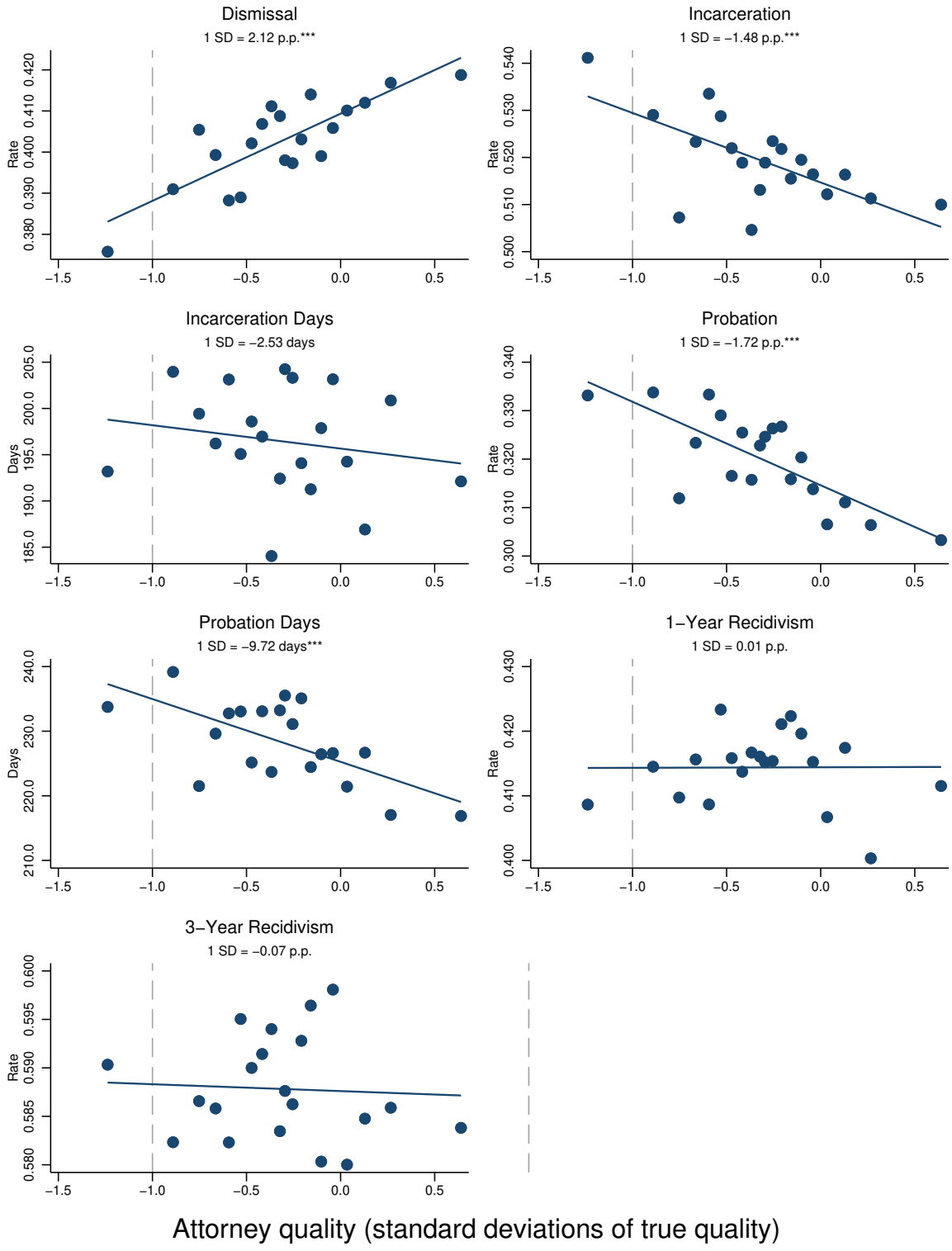


Figure B6 The figure plots binned means of each case outcome against incarceration avoidance quality for felony cases handled by court-appointed attorneys, as in Figure B4. The slope printed on each panel is the corresponding cell of Table 8, Panel B, second row. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

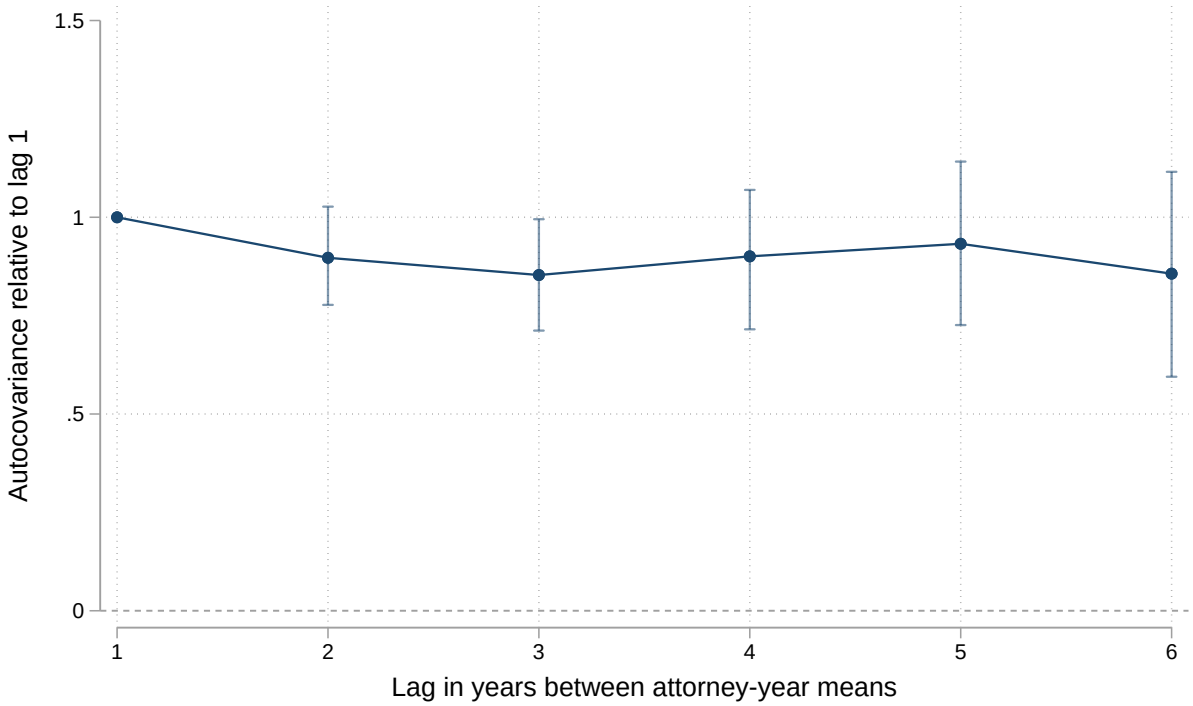


Figure B7 The figure plots the covariance between a court-appointed attorney's mean misdemeanor dismissal residuals in years t and $t+s$, divided by its value at $s=1$. It shows whether quality persists enough over time to justify pooling the measure across years. Residuals net out demographic and criminal history controls and county $\times$ list $\times$ month fixed effects. Each covariance pools attorney-year pairs with at least two defendants in both years (6,737 pairs at $s=1$, 1,232 at $s=6$), weighting each pair by $n_t n_{t+s}/(n_t+n_{t+s})$. The points at $s=2,3,5,6$ are the estimates in Table B10, Panel A. Capped spikes are 95 percent intervals from 300 bootstrap replications that resample attorneys.

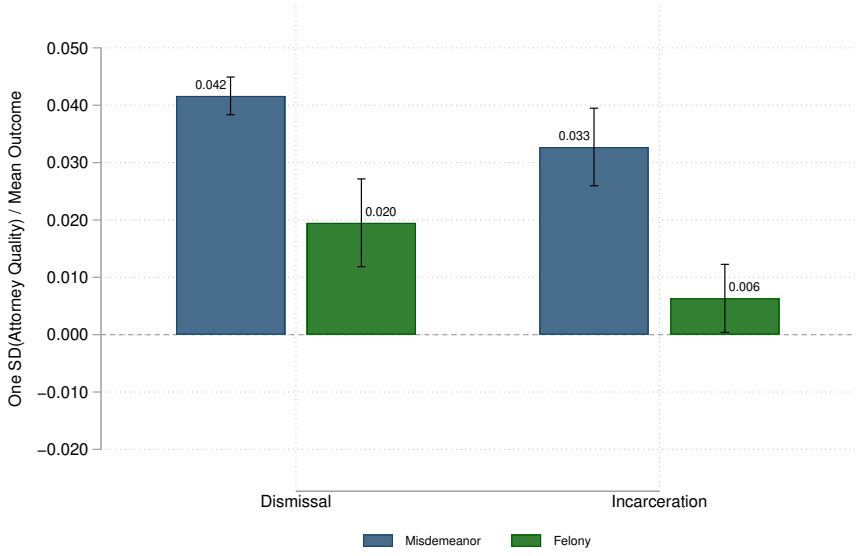


Figure B8 The figure plots the coefficient from regressing dismissal or incarceration on the court-appointed attorney's value-added for that outcome, standardized within county and case type, divided by the mean outcome. Regressions include demographic and criminal history controls and county × list × month and charge-class fixed effects. Incarceration value-added is not multiplied by −1. Error bars are 95% confidence intervals. Standard errors are clustered at the attorney level.

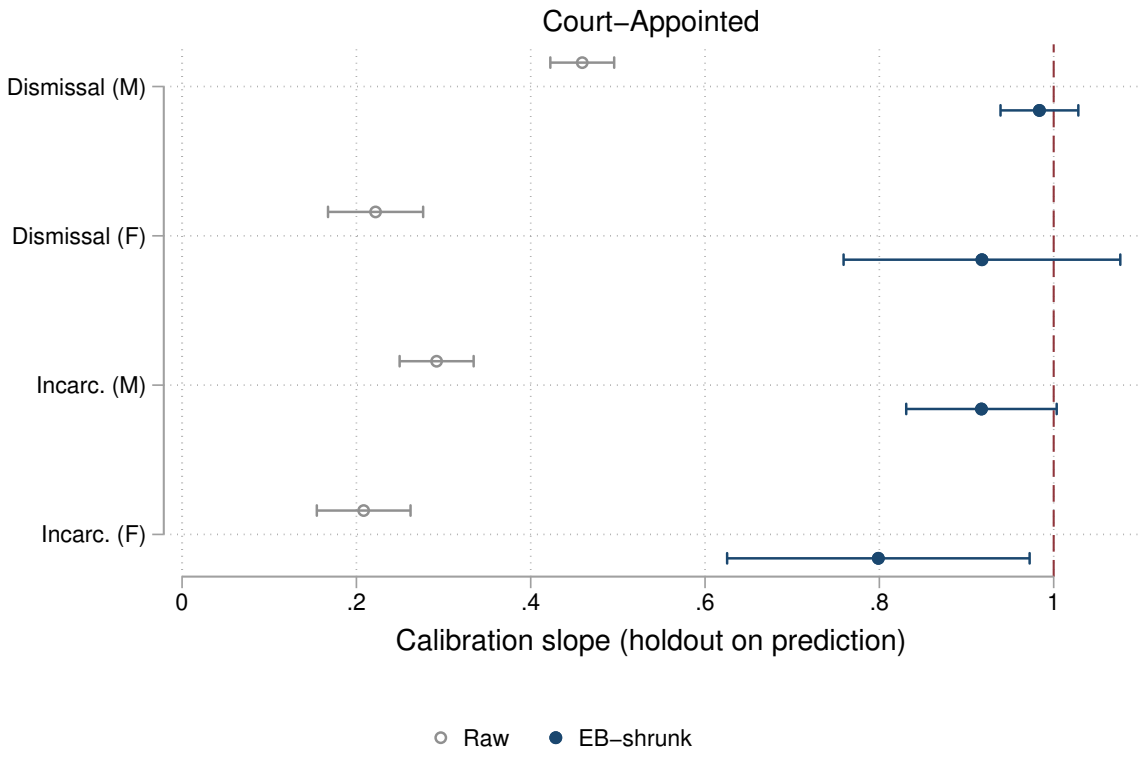


Figure B9 The figure plots the short-form calibration slopes of Table B5, Panel A, on the quality measure of Table 8. It shows how far each slope sits from one with and without shrinkage. Each slope comes from regressing a court-appointed defendant's residual outcome on the attorney's leave-out quality measure, unshrunk (Raw) or empirical Bayes shrunk (EB). Residuals net out the controls and fixed effects of Table 10, column (1). Rows show dismissal and incarceration for misdemeanor (M) and felony (F) cases. The dashed line marks a slope of one. Bars are 95% confidence intervals. Standard errors are clustered at the attorney level.

Table B1 Does the Return to Attorney Quality Differ Across Wage Regimes?

	(1) Dismissal	(2) Incarceration	(3) Incarceration Days	(4) Probation	(5) Probation Days	(6) 1-Year Recidivism	(7) 3-Year Recidivism
Panel A: Misdemeanors							
Dismissal quality							
Pre-cut VA (2008–2010)	1.0309***	-0.5362***	-25.12***	-0.6268***	-292.32***	0.2963***	0.2259***
	(0.0487)	(0.0541)	(7.52)	(0.0528)	(26.05)	(0.0726)	(0.0675)
Post-cut VA (2012–2016)	1.0350***	-0.5399***	-20.94***	-0.5143***	-229.71***	0.3406***	0.2910***
	(0.0354)	(0.0343)	(5.24)	(0.0357)	(17.61)	(0.0421)	(0.0448)
Difference (post − pre)	0.0041	-0.0037	4.18	0.1124*	62.61**	0.0443	0.0651
	(0.0572)	(0.0645)	(8.80)	(0.0607)	(30.16)	(0.0776)	(0.0757)
Incarceration avoidance quality							
Pre-cut VA (2008–2010)	1.4057***	-0.8771***	-33.11*	-0.8759***	-431.79***	0.3834***	0.3246**
	(0.1222)	(0.1115)	(18.51)	(0.1229)	(58.58)	(0.1364)	(0.1320)
Post-cut VA (2012–2016)	1.5502***	-0.9508***	-37.61***	-0.7264***	-318.38***	0.4909***	0.4060***
	(0.0923)	(0.0697)	(13.20)	(0.0779)	(37.32)	(0.0814)	(0.0794)
Difference (post − pre)	0.1444	-0.0738	-4.50	0.1495	113.41*	0.1075	0.0814
	(0.1484)	(0.1314)	(23.18)	(0.1418)	(68.10)	(0.1489)	(0.1477)
Observations	259,259	259,259	259,259	259,259	259,259	259,259	259,259
Attorneys	2,533	2,533	2,533	2,533	2,533	2,533	2,533
Panel B: Felonies							
Dismissal quality							
Pre-cut VA (2008–2010)	1.1402***	-1.1591***	-290.87	-1.1668***	-843.80***	0.1827	0.2097
	(0.2400)	(0.2219)	(227.80)	(0.2321)	(178.76)	(0.2025)	(0.2172)
Post-cut VA (2012–2016)	0.8884***	-0.7464***	-143.31	-0.6633***	-420.05***	0.0503	0.1072
	(0.1202)	(0.1073)	(98.37)	(0.1113)	(87.08)	(0.1154)	(0.1106)
Difference (post − pre)	-0.2518	0.4128*	147.56	0.5036*	423.74**	-0.1325	-0.1025
	(0.2563)	(0.2321)	(254.62)	(0.2572)	(194.83)	(0.2327)	(0.2439)
Incarceration avoidance quality							
Pre-cut VA (2008–2010)	1.2030***	-1.1252***	-354.77	-1.2614***	-907.35***	0.2731	0.2238
	(0.2564)	(0.2842)	(268.65)	(0.2652)	(207.30)	(0.2188)	(0.2553)
Post-cut VA (2012–2016)	0.9240***	-0.7586***	-173.82	-0.7911***	-516.34***	0.0195	0.1361
	(0.1381)	(0.1366)	(114.97)	(0.1332)	(101.57)	(0.1299)	(0.1234)
Difference (post − pre)	-0.2790	0.3666	180.95	0.4704	391.01*	-0.2535	-0.0877
	(0.2778)	(0.2977)	(295.35)	(0.2974)	(229.57)	(0.2497)	(0.2821)
Observations	107,448	107,448	107,448	107,448	107,448	107,448	107,448
Attorneys	1,866	1,866	1,866	1,866	1,866	1,866	1,866
Period-specific controls	Y	Y	Y	Y	Y	Y	Y
Period-specific list-time FE	Y	Y	Y	Y	Y	Y	Y

Note: This table tests whether a given amount of attorney quality had the same effect on outcomes before and after the cut, the condition that justifies pooling the quality measure across the two regimes. Specification as in Tables C4 and C5, with quality left unscaled and both periods stacked in one regression that gives quality and every control a period-specific slope. Columns (3) and (5) are in days per unit of value-added. Difference rows report post minus pre, and their stars test that difference. Standard errors clustered at the attorney level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B2 Correlation Across Attorney Quality Measures (Split-Half Corrected)

Measure	Dismissal	Incarceration
Court-Appointed — Misdemeanor		
Dismissal	1.000	-0.653
Incarceration	-0.653	1.000
Attorney cells	3,290	
Court-Appointed — Felony		
Dismissal	1.000	-0.985
Incarceration	-0.985	1.000
Attorney cells	2,415	

Note: Correlations between court-appointed attorneys' dismissal and incarceration value-added. Each attorney's cases are split at random into halves, and the off-diagonal entry divides the cross-half covariance of the two measures by the square root of the product of each measure's own cross-half covariance. Incarceration value-added is not multiplied by -1.

Table B3 Falsification Test: Does the Current Attorney's Quality Predict the Outcome of the Defendant's Earlier Case?

	Dismissal	Incarceration avoidance
Panel A: Court-Appointed Counsel — Misdemeanor		
Placebo slope	-0.0037	0.0038
	(0.0040)	(0.0040)
First-stage slope	0.0722	0.0399
CFR ratio	-0.051	0.095
Observations (pairs)	66,144	
Attorney-year clusters	8,077	
Panel B: Court-Appointed Counsel — Felony		
Placebo slope	-0.0030	0.0022
	(0.0070)	(0.0074)
First-stage slope	0.0459	0.0397
CFR ratio	-0.066	0.056
Observations (pairs)	26,857	
Attorney-year clusters	5,507	

Note: Consecutive cases of one defendant with different attorneys, the current one court-appointed, keeping pairs whose earlier case was disposed before the current attorney was first assigned to the defendant (80 percent of misdemeanor and 77 percent of felony pairs). The placebo slope regresses the earlier case's outcome on the current attorney's empirical Bayes quality measure, both standardized, absorbing the current case's county × list × month cell. The first-stage slope uses the current outcome instead, and the CFR ratio divides the placebo slope by it. Reversing the sign of both the outcome and the measure for incarceration avoidance leaves each entry unchanged. The test follows the spirit of Rothstein (2010). Standard errors two-way clustered at the attorney-year and defendant levels.

Table B4 VA Design Comparison: Attorney-Year, Leave-Year-Out, and Attorney-Pooled Measures

	Attorney-Year				*Leave-Year-Out*				*Attorney-Pooled*			
	unit: attorney-year				unit: attorney-year				unit: attorney			
	Leave-out defs.	VA sig. SD	Shrunk SD	Eff. 1 SD	Leave-out defs.	VA sig. SD	Shrunk SD	Eff. 1 SD	Leave-out defs.	VA sig. SD	Shrunk SD	Eff. 1 SD
Court-Appointed — Misdemeanor												
Dismissal	50.9	4.41	2.39	4.10^{***}	271.1	4.04	2.94	4.27^{***}	317.6	4.09	3.11	4.32^{***}
				(0.22)				(0.12)				(0.10)
Incarceration avoidance	50.9	2.59	1.11	2.07^{***}	271.1	2.37	1.47	2.42^{***}	317.6	2.41	1.58	2.45^{***}
				(0.24)				(0.14)				(0.12)
Court-Appointed — Felony												
Dismissal	25.2	2.75	0.88	2.01^{***}	134.3	3.14	1.81	3.28^{***}	157.5	2.96	1.78	3.10^{***}
				(0.58)				(0.29)				(0.26)
Incarceration avoidance	25.2	2.46	0.70	1.28^{**}	134.3	3.12	1.69	2.80^{***}	157.5	2.86	1.61	2.63^{***}
				(0.65)				(0.31)				(0.29)

Note: Court-appointed attorneys. Each design applies equations (6)–(8) to the same defendant residuals: attorney-year uses the attorney's other defendants that year, leave-year-out uses the attorney's defendants in other years with the no-drift weights of Chetty, Friedman and Rockoff (2014*a*), and attorney-pooled is the measure used in the paper. Leave-out defs. is the number of the attorney's other defendants behind a defendant's measure, averaged over defendants. VA sig. SD is $\hat{\sigma}_\theta$ from equation (9); for leave-year-out it is the square root of the covariance between a defendant's residual and the attorney's mean residual in other years. Shrunk SD is the standard deviation of the shrunk measure across defendants. Eff. 1 SD is $\hat{\sigma}_\theta$ times the coefficient from regressing the outcome on the unstandardized shrunk measure, with the controls and fixed effects of Table 8 plus charge-class fixed effects. Standard deviations and effects are in percentage points. Pooling puts six times as many defendants behind each measure as the attorney-year design. Pooled and leave-year-out effects are close to one-for-one with $\hat{\sigma}_\theta$, while attorney-year effects fall 7 to 48 percent short. For felonies, the attorney-year $\hat{\sigma}_\theta$ lies below the leave-year-out value because an attorney's defendants in the same assignment cell have mechanically negative residual covariance. Standard errors clustered at the attorney level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B5 Forecast-Bias Calibration: Leave-Defendant-Out Prediction

		Court-Appointed					
Outcome	Case Type	Raw	EB	SE	p($\beta = 1$)	R^2	N
Panel A. Short form: held-out residual on the prediction							
Dismissal	Misdemeanor	0.459	0.984	0.023	0.473		288,973
Dismissal	Felony	0.222	0.918	0.081	0.310		118,558
Incarceration	Misdemeanor	0.292	0.917	0.044	0.060		288,973
Incarceration	Felony	0.208	0.799	0.089	0.023		118,558
Panel B. Long form: outcome on the prediction, with controls and fixed effects							
Dismissal	Misdemeanor	0.490	1.056	0.024	0.019	0.068	288,973
Dismissal	Felony	0.252	1.049	0.089	0.583	0.125	118,558
Incarceration	Misdemeanor	0.314	1.017	0.049	0.722	0.099	288,973
Incarceration	Felony	0.236	0.920	0.100	0.425	0.132	118,558

Note: This table tests whether the quality measure predicts case outcomes one for one, as forecast unbiasedness requires, with and without empirical Bayes shrinkage. Court-appointed cases. The prediction is the quality measure of Table 8, unshrunk (Raw) or empirical Bayes shrunk (EB). Panel A regresses the residual case outcome on it with no further controls. Panel B adds demographic and criminal history controls and county × list × month and charge-class fixed effects, the specification of column (1) of Table 10, on all court-appointed cases. Panel A's residuals use Panel B's controls and fixed effects, so each Panel B slope is the Panel A slope divided by $1 - R^2$, where R^2 is the share of the EB measure's variance they explain. N counts cases. SE and $p(\beta = 1)$ refer to the EB slope. Standard errors clustered at the attorney level.

Table B6 Covariate Balance: Actual Outcomes vs. Attorney Value-Added

	Court-Apptd.—Misd.		Court-Apptd.—Fel.	
	Actual	VA	Actual	VA
Panel A: Dismissal				
Age	-0.001***	-0.000***	-0.001***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Male	-0.037***	0.001***	-0.034***	0.000
	(0.002)	(0.000)	(0.004)	(0.000)
Black	-0.011***	0.000	0.033***	0.000
	(0.002)	(0.000)	(0.003)	(0.000)
Hispanic	-0.025***	-0.001	0.023**	-0.000
	(0.006)	(0.001)	(0.010)	(0.000)
Violent	0.341***	0.000	-0.034**	0.000
	(0.006)	(0.000)	(0.016)	(0.001)
Property	0.125***	0.001*	-0.029*	-0.001
	(0.007)	(0.000)	(0.016)	(0.001)
Financial	0.151***	0.000	-0.042***	-0.001*
	(0.010)	(0.001)	(0.016)	(0.001)
Drugs/Alcohol	0.137***	0.001***	0.011	-0.001
	(0.006)	(0.000)	(0.015)	(0.001)
Traffic/Pub. Order	0.080***	-0.000	0.010	-0.000
	(0.005)	(0.000)	(0.016)	(0.001)
Num. Charges	-0.099***	0.000*	-0.064***	0.000
	(0.001)	(0.000)	(0.001)	(0.000)
Joint F	2594.96 [0.000]	5.45 [0.000]	651.79 [0.000]	2.67 [0.003]
Max. 1-SD shift (SD units)	0.298	0.011	0.290	0.021
Panel B: Incarceration				
Age	0.001***	0.000***	0.001***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Male	0.060***	-0.000	0.052***	-0.000
	(0.002)	(0.000)	(0.004)	(0.000)
Black	0.010***	-0.000	-0.025***	-0.000
	(0.002)	(0.000)	(0.003)	(0.000)
Hispanic	-0.002	0.001**	-0.020**	0.000
	(0.005)	(0.000)	(0.010)	(0.000)
Violent	-0.007	-0.000	0.061***	-0.000
	(0.005)	(0.000)	(0.016)	(0.001)
Property	0.119***	-0.000	0.034**	0.000
	(0.005)	(0.000)	(0.016)	(0.001)
Financial	0.075***	0.000	0.024	0.001
	(0.007)	(0.000)	(0.016)	(0.001)
Drugs/Alcohol	0.070***	-0.000*	0.005	0.000
	(0.005)	(0.000)	(0.015)	(0.001)
Traffic/Pub. Order	0.012***	0.000	0.003	0.000
	(0.004)	(0.000)	(0.016)	(0.001)
Num. Charges	0.101***	-0.000	0.071***	-0.000
	(0.001)	(0.000)	(0.001)	(0.000)
Joint F	1287.70 [0.000]	2.85 [0.002]	992.25 [0.000]	1.40 [0.176]
Max. 1-SD shift (SD units)	0.296	0.008	0.315	0.014

Note: This table tests whether defendant characteristics that predict outcomes also predict the quality of the assigned attorney. Each column regresses the outcome (Actual) or the attorney's empirical Bayes quality measure (VA) on ten defendant characteristics, absorbing county × list × month cells. Nine of the ten are controls both in building the quality measure and in the effect regressions. The last row is the largest $|b_k|\,\mathrm{SD}(x_k)$ across characteristics, in standard deviations of the dependent variable. Joint-F p-values in brackets. Standard errors clustered at the attorney level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B7 Oster (2019) Coefficient-Stability Bounds

		Court-Appointed			
Outcome	Case Type	$\hat{\beta}\,\hat{\sigma}_\theta$ (pp)	Oster δ	$\beta^*(\delta{=}1)\,\hat{\sigma}_\theta$ (pp)	ΔR^2
Dismissal	Misd.	4.28	29.7	4.15	0.0434
Dismissal	Felony	3.17	15.5	2.97	0.0155
Incarceration avoidance	Misd.	2.47	26.9	2.38	0.0255
Incarceration avoidance	Felony	2.70	8.8	2.40	0.0088

Note: This table reports how strong selection on unobservables would have to be to explain away the quality effects. Court-appointed cases with both predictors observed (275,211 misdemeanor and 112,357 felony). $\hat{\beta}$ is the coefficient on the empirical Bayes quality measure from a regression of the outcome on it, the quality-model controls, a linear ACS neighborhood socioeconomic index, and statute leniency, absorbing county × list × month cells. Statute leniency is the leave-defendant-out mean outcome residual for the charged statute, pooling court-appointed and public-defender cases. $\hat{\beta}$ and β^* are multiplied by $\hat{\sigma}_\theta$ of Table 7, so they are in percentage points per standard deviation of true quality, as in Table 8. δ is the selection on unobservables, relative to selection on the two predictors, that would set β to zero, with $R_{\max} = 1.3\hat{R}^2$. $\beta^*(\delta{=}1)$ is the bias-adjusted coefficient, and ΔR^2 is the predictors' incremental within-cell R^2. Selection on unobservables would need to be 9 to 30 times as strong as selection on the two predictors to eliminate the effects, and the bias-adjusted coefficients keep 89 to 97 percent of the estimates.

Table B8 Cinelli and Hazlett (2020) Sensitivity Analysis

		Court-Appointed					
					Benchmark: statute leniency		
Outcome	Case Type	Partial $R^2_{Y\sim D\|X}$	$\text{RV}_{q=1}$	$\text{RV}_{q=1,\alpha=.05}$	$R^2_{D\sim Z\|X}$ (1×)	$R^2_{Y\sim Z\|D,X}$ (1×)	k^*_D
Dismissal	Misd.	0.0057	0.073	0.069	0.00004	0.057	157
Dismissal	Felony	0.0020	0.044	0.038	0.00003	0.020	80
Incarceration avoidance	Misd.	0.0018	0.041	0.038	0.00001	0.033	145
Incarceration avoidance	Felony	0.0013	0.035	0.029	0.00002	0.012	65

Note: This table reports how strong a confounder would have to be, relative to statute leniency, to explain away the quality effects. Sample and regression as in Table B7. Every entry uses that OLS regression and its residual degrees of freedom, and none depends on the scale of the quality measure. Partial $R^2_{Y\sim D|X}$ is the share of the outcome's residual variance explained by quality. RV is the partial R^2 with both quality and the outcome that a confounder needs to reduce the estimate to zero ($q = 1$) or to insignificance at 5 percent ($\alpha = .05$). The next two columns give the partial R^2s of a confounder as strong as statute leniency. k^*_D is the multiple of statute leniency's partial R^2 with quality that a confounder would need to eliminate the estimate even if it explained all residual variation in the outcome. A confounder as strong as statute leniency would explain at most about 6 percent of the residual variation in the outcome and almost none of the variation in quality. To eliminate the effects, a confounder would need more than 60 times statute leniency's association with quality, even if it explained all remaining outcome variation.

Table B9 Dispersion of True Attorney Quality: Moment Versus Leave-Out Estimator

		Moment estimator		Leave-out (KSS)			
Counsel	Outcome	σ_θ	σ_θ^2	σ_θ	σ_θ^2	95% CI	Ratio
Court-Appointed (Misd.)	Dismissal	0.073	0.0054	0.041	0.0017	[0.038, 0.044]	1.80
Court-Appointed (Misd.)	Incarceration	0.050	0.0025	0.024	0.0006	[0.022, 0.027]	2.06
Court-Appointed (Felony)	Dismissal	0.046	0.0021	0.030	0.0009	[0.025, 0.034]	1.51
Court-Appointed (Felony)	Incarceration	0.063	0.0040	0.029	0.0008	[0.024, 0.034]	2.18

Note: This table shows how much every result stated in standard deviations of true quality depends on the choice of dispersion estimator. Both estimators use the residuals behind Table 7. The moment estimator is the variance of attorney mean residuals, unweighted across attorneys with at least two defendants, less the mean sampling variance of those means. The leave-out estimator, as in Kline, Saggio and Sølvsten (2020, Example 1), is the σ_θ of Table 7. It is used throughout because it builds the variance from products of different defendants' residuals for the same attorney, so case-level sampling noise drops out without being modeled. In contrast, the variance of raw attorney means overstates dispersion. Ratio divides the moment σ_θ by the leave-out σ_θ. Confidence intervals from 200 bootstrap resamples of attorneys.

Table B10 Is Attorney Quality Persistent Enough to Pool Across the Pay Change?

		A. Autocovariance γ_s/γ_1				B. Re-ranking placebo		
Counsel	Outcome	s=2	s=3	s=5	s=6	across cut	placebo	diff.
Court-Appointed (Misd.)	Dismissal	0.90	0.85	0.93	0.86	0.167	0.177	-0.009 [-0.126, 0.123]
Court-Appointed (Misd.)	Incarceration avoidance	1.00	0.60	0.76	0.66	0.091	0.167	-0.075 [-0.208, 0.054]
Court-Appointed (Felony)	Dismissal	0.47	0.44	0.43	0.66	0.190	0.007	0.183 [0.051, 0.310]
Court-Appointed (Felony)	Incarceration avoidance	0.51	0.25	0.55	0.93	0.150	0.075	0.074 [-0.059, 0.219]

Note: Court-appointed cases assigned 2008–2016, residualized as in Table 7. Panel A divides the covariance between an attorney's mean residuals in years t and $t+s$ by its value at $s=1$, pooling attorney-years with at least two defendants and weighting each pair by $n_t n_{t+s}/(n_t+n_{t+s})$. Panel B correlates attorney mean residuals in 2008–2010 with those in 2012–2014, across the cut, and those in 2012–2014 with those in 2015–2016, with no rate change, for attorneys with at least two defendants in both windows. The diff. column is the first correlation less the second, with a 95 percent attorney-bootstrap interval (200 replications) in brackets. Misdemeanor dismissal quality is persistent, with the covariance at 0.85 to 0.93 of its one-year value out to six years, while the incarceration avoidance and felony measures are less persistent. On the misdemeanor list, the across-cut and placebo correlations differ little, but the intervals are wide, so the table cannot distinguish stable rankings from substantial re-ranking at the cut.

Table B11 Effect of Attorney Quality on Predicted Case Outcomes and Recidivism

	(1) Dismissal (p.p.)	(2) Incarceration (p.p.)	(3) Incarceration Days	(4) Probation (p.p.)	(5) Probation Days	(6) 1-Year Recidivism (p.p.)	(7) 3-Year Recidivism (p.p.)
Panel A: Misdemeanors							
Dismissal quality (σ_θ = 4.09 p.p.)							
Actual outcome	4.35***	-2.23***	-0.83***	-2.34***	-10.44***	1.25***	1.09***
	(0.10)	(0.11)	(0.17)	(0.12)	(0.58)	(0.17)	(0.16)
Predicted outcome	0.06	0.07*	0.07**	0.01	0.10	0.09***	0.09***
	(0.04)	(0.03)	(0.03)	(0.03)	(0.15)	(0.02)	(0.02)
Incarceration avoidance quality (σ_θ = 2.42 p.p.)							
Actual outcome	3.94***	-2.50***	-0.78***	-2.05***	-9.09***	1.00***	0.88***
	(0.18)	(0.12)	(0.24)	(0.16)	(0.75)	(0.19)	(0.18)
Predicted outcome	0.03	0.02	0.07*	0.01	0.13	0.06***	0.06***
	(0.05)	(0.04)	(0.04)	(0.04)	(0.19)	(0.02)	(0.02)
Predictor R^2	0.150	0.090	0.042	0.095	0.093	0.016	0.019
Observations	273,706	273,706	273,706	273,706	273,706	273,706	273,706
Panel B: Felonies							
Dismissal quality (σ_θ = 3.01 p.p.)							
Actual outcome	3.25***	-2.91***	-6.66***	-2.53***	-16.22***	0.35	0.28
	(0.27)	(0.25)	(2.19)	(0.26)	(2.10)	(0.27)	(0.27)
Predicted outcome	0.22***	-0.17***	-0.34	-0.24***	-1.87***	0.01	0.00
	(0.06)	(0.06)	(0.36)	(0.08)	(0.61)	(0.04)	(0.04)
Incarceration avoidance quality (σ_θ = 2.89 p.p.)							
Actual outcome	3.21***	-2.74***	-5.45**	-2.57***	-16.58***	0.17	0.12
	(0.29)	(0.29)	(2.38)	(0.29)	(2.18)	(0.29)	(0.29)
Predicted outcome	0.30***	-0.26***	-0.43	-0.36***	-2.69***	0.02	0.01
	(0.07)	(0.07)	(0.39)	(0.08)	(0.65)	(0.04)	(0.04)
Predictor R^2	0.050	0.056	0.189	0.080	0.068	0.021	0.028
Observations	108,972	108,972	108,972	108,972	108,972	108,972	108,972

Note: This table extends the predicted-outcome placebo of Table 10 to every outcome in Table 8, to bound how much of each effect sorting on observables could explain. Specification, predictors, and sample as in Table 10, applied to every cell of Table 8 in its units and signs. Predicted-outcome rows replace the outcome with its prediction from those predictors and are zero under conditional random assignment. Predictor R^2 is the fit of that prediction. Standard errors clustered at the attorney level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix C. Estimating Attorney Quality

In this appendix, I describe how I construct the attorney quality measure, $\tilde{\mu}_{j,-i}$, of Section V and the alternative constructions in Tables C1–C6. I run every step separately by track and quality outcome, using only court-appointed cases. The misdemeanor track is the misdemeanor list, and the felony track pools the two felony lists. While the construction follows equations (5)–(9), it departs from them in the normalization of θ_j, the definition of the overall mean $\bar{Y}^*$, the dispersions reported in Table 7, and the measure behind the terciles of Figure 2. I note each departure in the step where it occurs.

C.1 Estimating attorney quality

Step 1: Residualization. I estimate β in equation (5) by ordinary least squares, regressing Y_{ict} on X_{ict} without attorney effects, and denote the estimate $\hat{\beta}$. The fixed effects in X_{ict} are county $\times$ list $\times$ assignment-month cells, where the list is one of the three attorney lists for misdemeanors, lower-level felonies, and higher-level felonies. The two felony lists therefore share $\hat{\beta}$ but keep separate cells. For court-appointed attorneys, the regression uses 289,946 misdemeanor-track cases and 121,486 felony-track cases, the cases with complete controls after missing sex and crime-type indicators are set to zero. When a cell contains a single case, its fixed effect fits that case exactly and leaves a residual of zero. Because the cell effects are estimated without attorney effects, they absorb the average quality of the attorneys assigned within each cell. Consequently, the residuals average zero in every cell, and equation (5) measures θ_j relative to the attorneys assigned in the same cells. This normalization matches the comparison that the rotation randomizes, and $\hat{\sigma}_\theta$ accordingly measures dispersion within cells, not between counties.

The conditional random assignment of Section V leaves θ_j uncorrelated within cells with any control determined before assignment, so estimating β without attorney effects is consistent when every element of X_{ict} is predetermined. In contrast, Chetty, Friedman and Rockoff (2014*a*) estimate their control coefficients from variation within teachers, an ap-

proach that remains consistent when quality is correlated with the controls. Additionally, X_{ict} excludes prosecutor identity, because prosecutors can respond to the assigned attorney through charging decisions, plea offers, and trial preparation. Conditioning on the prosecutor could therefore absorb part of the attorney's effect.

Step 2: Collapse to defendants. I average the case residuals within each attorney over each defendant's dispositions to form $\bar{Y}^*_{ij}$, so that every defendant contributes one observation. The measure pools the attorney's cases across counties and years, giving one θ_j per attorney, track, and outcome. I write the defendant-level residual as

$$\bar{Y}^*_{ij} = \theta_j + \bar{\epsilon}_{ij}, \tag{C1}$$

where $\bar{\epsilon}_{ij}$ is the mean of ϵ_{ict} over defendant i's cases with attorney j. I assume that $\bar{\epsilon}_{ij}$ has mean zero given θ_j and is independent across defendants, with a variance that may differ across defendants and attorneys. Errors may be correlated across a defendant's own cases, since the collapse averages them into a single term. As a check on independence across defendants, a footnote in Section V reports $\hat{\sigma}_\theta$ when each leave-out mean also excludes the attorney's other defendants disposed of on the same day, who may share a court-session shock.

Step 3: Leave-out mean. Equation (6) gives each defendant the mean of $\bar{Y}^*_{i'j}$ over the attorney's other $D_j - 1$ defendants. Because all of a defendant's cases with the attorney are left out together, $\widehat{\mu}_{j,-i}$ shares no error with $\bar{Y}^*_{ij}$ under equation (C1). In contrast, leaving out only the focal case would keep the defendant's other cases in the mean, along with any error they share. The leave-out mean requires $D_j \geq 2$, the only caseload restriction. On the misdemeanor and felony tracks, 3,302 and 2,442 attorneys represent 247,430 and 107,588 attorney–defendant pairs, and 2,656 and 1,968 of these attorneys have at least two defendants. Attorneys with one defendant still enter Step 1 and the overall mean $\bar{Y}^*$, but

they receive no quality measure. Low-caseload attorneys otherwise remain in the sample with small shrinkage factors λ_j.

Step 4: Noise and signal variance. To construct those shrinkage factors, I first estimate the noise variance by pooling the within-attorney spread of the defendant residuals,

$$\hat{\sigma}_\epsilon^2 = \frac{\sum_j \sum_i \big(\bar{Y}_{ij}^* - \bar{Y}_j^*\big)^2}{\sum_j \big(D_j - 1\big)}, \tag{C2}$$

where $\bar{Y}_j^*$ is attorney j's mean of $\bar{Y}_{ij}^*$ over all D_j defendants. I compute the signal variance from equation (9), with N the number of attorney–defendant pairs with $D_j \geq 2$ (246,784 and 107,114) and $\bar{Y}^*$ the overall mean of equation (7). Computed over all attorney–defendant pairs, this overall mean weights attorneys by their defendants. In contrast, the case-level mean of Y_{ict}^* is zero by construction. Although no reported cell is negative, a negative value of equation (9) would be set to zero.

Each term of equation (9) multiplies one defendant's error by the errors of other defendants under equation (C1), and these products have mean zero. Consequently,

$$E\big[\hat{\sigma}_\theta^2\big] = \frac{1}{N} \sum_{j:\, D_j \geq 2} D_j \big(\theta_j - \bar{\theta}\big)^2 + O\big(N^{-1}\big), \tag{C3}$$

where $\bar{\theta}$ is the defendant-weighted mean of θ_j over all attorneys, the population counterpart of $\bar{Y}^*$, and the last term comes from estimating it. In other words, $\hat{\sigma}_\theta^2$ estimates σ_θ^2, the first term on the right of equation (C3) and the variance of quality across the attorneys that defendants draw. Equation (C3) requires neither homoskedastic errors nor equal caseloads across attorneys. Table B9 compares $\hat{\sigma}_\theta$ with the moment estimator, which weights attorneys equally.

Equation (9) is, up to that centering term, the covariance form of the leave-out estimator of Kline, Saggio and Sølvsten (2020) for the analysis-of-covariance model without common regressors (their Example 1), with attorneys as groups and $\bar{Y}_{ij}^*$ as observations.

Their Lemma 1 establishes unbiasedness with independent, heteroskedastic errors when every group has at least two observations. Moreover, their Lemma 3 and Theorem 2 give consistency and asymptotic normality as the number of attorneys grows, under conditions that they verify for this example in their Section 8. At the same time, applying their results to the Step 1 residuals treats those residuals as data. The results therefore do not account for sampling error in $\hat{\beta}$ or in the cell effects.

Step 5: Shrinkage. With $\hat{\sigma}_\epsilon^2$ and $\hat{\sigma}_\theta^2$ in hand, I shrink $\widehat{\mu}_{j,-i}$ toward $\bar{Y}^*$ using equation (7) and the shrinkage factor λ_j in equation (8), evaluated at the leave-out caseload $D_j - 1$. If every defendant's error has the common variance σ_ϵ^2 that $\hat{\sigma}_\epsilon^2$ estimates, the noise in $\widehat{\mu}_{j,-i}$ has variance $\sigma_\epsilon^2/(D_j - 1)$. In that case, $\tilde{\mu}_{j,-i}$ is the best linear predictor of θ_j given $\widehat{\mu}_{j,-i}$, with no assumption on the distribution of θ_j (Chetty, Friedman and Rockoff, 2014*a*). Importantly, a regression of an outcome on $\tilde{\mu}_{j,-i}$ is therefore not attenuated by the noise in $\widehat{\mu}_{j,-i}$, the forecast-unbiasedness property that Tables 10 and B5 test. Because λ_j uses the pooled $\hat{\sigma}_\epsilon^2$, it treats the noise variance as common across attorneys, an assumption that $\hat{\sigma}_\theta^2$ does not require.

Step 6: Scaling. Because the shrinkage in equation (7) compresses the measure, the standard deviation of $\tilde{\mu}_{j,-i}$ is smaller than $\hat{\sigma}_\theta$, and a coefficient on a standardized measure would understate the effect of quality. I therefore do not standardize $\tilde{\mu}_{j,-i}$, and each entry of Table 8 is $\hat{\beta}_{\text{raw}} \times \hat{\sigma}_\theta$, the effect of a one-standard-deviation difference in θ_j. Incarceration avoidance quality, the measure built from incarceration multiplied by -1, has the same $\hat{\sigma}_\theta$. At the same time, the dispersions in Table 7, which scale Tables 8 and 9, come from a second evaluation of equation (9). That pass residualizes both tracks in one regression, drops cases alone in their cell, and assigns tracks by the case's own misdemeanor flag. Its pooled dispersion differs from the $\hat{\sigma}_\theta$ inside λ_j by less than 2 percent in every cell.

Inference. The standard errors on these dispersions in Table 7 are the standard deviation of $\hat{\sigma}_\theta$ across 200 bootstrap samples of attorneys, holding the Step 1 residuals fixed. The standard errors on the effects are clustered by attorney and treat both $\tilde{\mu}_{j,-i}$ and $\hat{\sigma}_\theta$ as fixed. The tests of the measure appear in Section V.D and in Tables 10, B3, B5, B6, and B11.

C.2 Alternative constructions

I build each alternative construction of the measure by changing one element of the steps above and re-estimating everything that depends on it. Tables C1–C5 report the specification of Table 8 for each construction, scaled by the construction's own $\hat{\sigma}_\theta$, shown in each row label.

Leaving out the month or the year. In Tables C1 and C2, I replace the defendant with the attorney's assignment month or year as the unit that is averaged and left out. Step 1 is unchanged. Step 2 averages the attorney's case residuals within each month or year. From there, equations (6)–(9) and (C2) apply with periods in place of defendants. Because no attorney–defendant pair spans two assignment months, leaving out a month always leaves out the defendant, along with every other case the attorney was assigned that month. Consequently, a shock shared across an attorney's cases within the period, such as a court session that disposes of many of them, moves from the signal into the noise. Moreover, the new unit changes the weights, since each period counts once in the leave-out mean regardless of its caseload. Additionally, $\hat{\sigma}^2_\theta$ weights attorneys by their number of periods, and attorneys active in only one period drop out.

Attorney-year quality. Unlike Tables C1 and C2, which keep one θ_j per attorney, Table C3 lets quality change from year to year. I repeat Steps 2 through 5 for each attorney and assignment year, with the number of the attorney's distinct defendants in that year in place of D_j. Each defendant is therefore left out of a mean built from the attorney's other defendants in the same year, and $\hat{\sigma}^2_\theta$ becomes the variance of attorney-year quality, counting year-specific quality as signal. The cost is precision: among units with at least ten

defendants, the mean shrinkage factor from equation (8) for misdemeanor dismissal is 0.27 for attorney-years and 0.42 for attorneys. For felony dismissal, the two values are 0.10 and 0.27, and incarceration avoidance follows the same pattern at lower levels.

Quality before and after the pay cut. I rebuild the measure in Tables C4 and C5 from cases assigned in 2008–2010 and in 2012–2016, excluding 2011 from both. Every step, from $\hat{\beta}$ and the cell effects to λ_j, is re-estimated inside each regime, so an attorney active in both regimes has separate estimates of quality at the two wages, $\theta_j(w_0)$ and $\theta_j(w_1)$. In these tables, the row labels report each regime's $\hat{\sigma}_\theta$ for all attorneys active in it. In contrast, the regime columns of Table 7 hold the attorneys fixed. They residualize once on the cases of both regimes and evaluate equation (9) within each regime for the attorneys with at least two defendants in each. Their dispersions therefore differ from these row labels. Because each table scales by its own regime's $\hat{\sigma}_\theta$, a one-standard-deviation move is a different absolute amount of quality in each regime. For that reason, Table B1 compares the two regimes on the unscaled measure. For the terciles in Figure 2, I repeat Steps 2 through 5 on the Step 1 residuals of cases assigned in 2008 and 2009, which share no case with the event-study window that begins in 2010. Because no case overlaps, that measure averages over all of the attorney's defendants and evaluates λ_j at D_j.

The pooling test. Lastly, Table C6 tests whether attorneys keep their relative quality across the cut. It regresses the outcomes of cases assigned in 2012–2016 on the attorney's pre-cut measure, the mean over the attorney's pre-cut cases of the $\tilde{\mu}_{j,-i}$ behind Table C4, with the specification of Table 8. Because no defendant appears with the same attorney on the same list in both regimes, the pre-cut measure never contains the defendant whose outcome it predicts. Let ρ denote the correlation between $\theta_j(w_0)$ and $\theta_j(w_1)$ among the

attorneys in the test. The slope on the pre-cut measure, $\hat{\beta}_{\text{raw}}$, then satisfies

$$\text{plim } \hat{\beta}_{\text{raw}} = \rho \, \frac{\sigma_\theta(w_1)}{\sigma_\theta(w_0)}, \tag{C4}$$

because shrinkage removes the attenuation that noise in the pre-cut mean would otherwise cause. The column $\hat{\rho}$ divides the slope by the ratio of the regime dispersions in Table 7 to recover ρ. As further evidence on persistence, Table B10 reports the autocovariance of attorney residuals over time and a placebo for re-ranking across the cut.

Table C1 Attorney Value-Added and Case Outcomes: Leave-One-Month-Out

	(1) Dismissal (p.p.)	(2) Incarceration (p.p.)	(3) Incarceration Days	(4) Probation (p.p.)	(5) Probation Days	(6) 1-Year Recidivism (p.p.)	(7) 3-Year Recidivism (p.p.)
Panel A: Misdemeanors							
Dismissal quality (σ_θ = 4.86 p.p.)	4.85***	-2.45***	-1.07***	-2.60***	-11.61***	1.50***	1.30***
	(0.13)	(0.12)	(0.17)	(0.13)	(0.64)	(0.19)	(0.18)
Incarceration avoidance quality (σ_θ = 3.12 p.p.)	4.12***	-2.81***	-1.03***	-2.20***	-9.85***	0.97***	0.87***
	(0.21)	(0.14)	(0.23)	(0.17)	(0.83)	(0.21)	(0.19)
Mean dep. var.	0.535	0.237	14.43	0.202	88.23	0.374	0.569
Observations	288,801	288,801	288,801	288,801	288,801	288,801	288,801
Attorneys	2,612	2,612	2,612	2,612	2,612	2,612	2,612
Panel B: Felonies							
Dismissal quality (σ_θ = 3.65 p.p.)	3.60***	-3.12***	-5.84**	-2.82***	-17.54***	0.39	0.21
	(0.27)	(0.26)	(2.43)	(0.27)	(2.08)	(0.27)	(0.25)
Incarceration avoidance quality (σ_θ = 3.64 p.p.)	3.48***	-3.12***	-5.49**	-2.78***	-17.15***	0.12	-0.04
	(0.29)	(0.28)	(2.68)	(0.29)	(2.20)	(0.28)	(0.26)
Mean dep. var.	0.404	0.518	187.02	0.321	228.51	0.416	0.590
Observations	118,371	118,371	118,371	118,371	118,371	118,371	118,371
Attorneys	1,944	1,944	1,944	1,944	1,944	1,944	1,944
Demographic controls	Y	Y	Y	Y	Y	Y	Y
Crime controls	Y	Y	Y	Y	Y	Y	Y
List-time FE	Y	Y	Y	Y	Y	Y	Y

Note: Specification as in Table 8, with quality rebuilt leaving out all of the attorney's cases from the defendant's assignment month rather than the defendant alone. Row labels report σ_θ re-estimated for this measure, and attorneys with cases in only one month drop out. Standard errors clustered at the attorney level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C2 Attorney Value-Added and Case Outcomes: Leave-One-Year-Out

	(1) Dismissal (p.p.)	(2) Incarceration (p.p.)	(3) Incarceration Days	(4) Probation (p.p.)	(5) Probation Days	(6) 1-Year Recidivism (p.p.)	(7) 3-Year Recidivism (p.p.)
Panel A: Misdemeanors							
Dismissal quality (σ_θ = 5.94 p.p.)	6.91***	-3.50***	-1.42***	-3.52***	-15.53***	2.50***	2.32***
	(0.35)	(0.25)	(0.32)	(0.26)	(1.29)	(0.32)	(0.30)
Incarceration avoidance quality (σ_θ = 4.50 p.p.)	5.34***	-3.46***	-0.91**	-2.81***	-12.32***	1.51***	1.59***
	(0.40)	(0.32)	(0.38)	(0.30)	(1.46)	(0.34)	(0.32)
Mean dep. var.	0.535	0.237	14.44	0.202	88.34	0.375	0.570
Observations	284,883	284,883	284,883	284,883	284,883	284,883	284,883
Attorneys	2,275	2,275	2,275	2,275	2,275	2,275	2,275
Panel B: Felonies							
Dismissal quality (σ_θ = 5.30 p.p.)	4.22***	-4.14***	-7.68**	-3.16***	-18.44***	0.13	-0.19
	(0.46)	(0.42)	(3.84)	(0.44)	(3.21)	(0.42)	(0.40)
Incarceration avoidance quality (σ_θ = 6.03 p.p.)	3.76***	-3.87***	-5.40	-2.94***	-16.61***	-0.34	-0.59
	(0.43)	(0.43)	(3.54)	(0.43)	(3.18)	(0.40)	(0.38)
Mean dep. var.	0.404	0.517	187.24	0.321	228.31	0.416	0.590
Observations	117,053	117,053	117,053	117,053	117,053	117,053	117,053
Attorneys	1,730	1,730	1,730	1,730	1,730	1,730	1,730
Demographic controls	Y	Y	Y	Y	Y	Y	Y
Crime controls	Y	Y	Y	Y	Y	Y	Y
List-time FE	Y	Y	Y	Y	Y	Y	Y

Note: Specification as in Table C1, leaving out the attorney's cases from the defendant's assignment year rather than month. Attorneys with cases in only one year drop out. Standard errors clustered at the attorney level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C3 Attorney Value-Added and Case Outcomes: Quality Estimated by Attorney-Year

	(1) Dismissal (p.p.)	(2) Incarceration (p.p.)	(3) Incarceration Days	(4) Probation (p.p.)	(5) Probation Days	(6) 1-Year Recidivism (p.p.)	(7) 3-Year Recidivism (p.p.)
Panel A: Misdemeanors							
Dismissal quality (σ_θ = 4.41 p.p.)	4.10***	-2.26***	-0.74***	-2.30***	-10.44***	1.22***	1.06***
	(0.22)	(0.15)	(0.21)	(0.16)	(0.76)	(0.20)	(0.19)
Incarceration avoidance quality (σ_θ = 2.59 p.p.)	3.76***	-2.07***	-0.33	-2.14***	-9.57***	0.86***	0.95***
	(0.25)	(0.24)	(0.28)	(0.21)	(1.03)	(0.25)	(0.23)
Mean dep. var.	0.535	0.237	14.39	0.202	88.20	0.374	0.569
Observations	287,048	287,048	287,048	287,048	287,048	287,048	287,048
Attorneys	2,479	2,479	2,479	2,479	2,479	2,479	2,479
Panel B: Felonies							
Dismissal quality (σ_θ = 2.75 p.p.)	2.06***	-1.94***	-1.75	-2.24***	-17.05***	-0.32	0.37
	(0.58)	(0.52)	(4.06)	(0.45)	(3.59)	(0.45)	(0.44)
Incarceration avoidance quality (σ_θ = 2.46 p.p.)	2.09***	-1.32**	-0.77	-2.43***	-19.42***	-0.22	0.17
	(0.61)	(0.64)	(4.60)	(0.54)	(4.22)	(0.50)	(0.51)
Mean dep. var.	0.404	0.517	186.43	0.321	228.32	0.416	0.590
Observations	116,873	116,873	116,873	116,873	116,873	116,873	116,873
Attorneys	1,822	1,822	1,822	1,822	1,822	1,822	1,822
Demographic controls	Y	Y	Y	Y	Y	Y	Y
Crime controls	Y	Y	Y	Y	Y	Y	Y
List-time FE	Y	Y	Y	Y	Y	Y	Y

Note: Specification as in Table 8, with quality estimated separately for each attorney-year rather than pooled over the attorney's career. Row labels report σ_θ of attorney-year quality. Standard errors clustered at the attorney level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C4 Attorney Value-Added and Case Outcomes: Quality Measured Before the Pay Cut

	(1) Dismissal (p.p.)	(2) Incarceration (p.p.)	(3) Incarceration Days	(4) Probation (p.p.)	(5) Probation Days	(6) 1-Year Recidivism (p.p.)	(7) 3-Year Recidivism (p.p.)
Panel A: Misdemeanors							
Dismissal quality (σ_θ = 4.49 p.p.)	4.63***	-2.41***	-1.13***	-2.82***	-13.13***	1.33***	1.02***
	(0.22)	(0.24)	(0.34)	(0.24)	(1.17)	(0.33)	(0.30)
Incarceration avoidance quality (σ_θ = 2.77 p.p.)	3.89***	-2.43***	-0.92*	-2.42***	-11.95***	1.06***	0.90**
	(0.34)	(0.31)	(0.51)	(0.34)	(1.63)	(0.38)	(0.37)
Mean dep. var.	0.504	0.249	17.67	0.241	107.97	0.365	0.556
Observations	79,737	79,737	79,737	79,737	79,737	79,737	79,737
Attorneys	1,410	1,410	1,410	1,410	1,410	1,410	1,410
Panel B: Felonies							
Dismissal quality (σ_θ = 2.77 p.p.)	3.16***	-3.21***	-8.05	-3.23***	-23.36***	0.51	0.58
	(0.67)	(0.62)	(6.37)	(0.65)	(5.00)	(0.57)	(0.61)
Incarceration avoidance quality (σ_θ = 2.55 p.p.)	3.07***	-2.87***	-9.05	-3.22***	-23.14***	0.70	0.57
	(0.66)	(0.73)	(6.92)	(0.68)	(5.34)	(0.56)	(0.66)
Mean dep. var.	0.378	0.533	196.30	0.343	251.00	0.397	0.574
Observations	30,863	30,863	30,863	30,863	30,863	30,863	30,863
Attorneys	1,088	1,088	1,088	1,088	1,088	1,088	1,088
Demographic controls	Y	Y	Y	Y	Y	Y	Y
Crime controls	Y	Y	Y	Y	Y	Y	Y
List-time FE	Y	Y	Y	Y	Y	Y	Y

Note: Specification as in Table 8, on cases assigned 2008–2010, with quality rebuilt from those cases alone. Row labels report σ_θ re-estimated on all attorneys in the period, not the attorneys active in both periods behind $\sigma_\theta(w_0)$ in Table 7. Standard errors clustered at the attorney level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C5 Attorney Value-Added and Case Outcomes: Quality Measured After the Pay Cut

	(1) Dismissal (p.p.)	(2) Incarceration (p.p.)	(3) Incarceration Days	(4) Probation (p.p.)	(5) Probation Days	(6) 1-Year Recidivism (p.p.)	(7) 3-Year Recidivism (p.p.)
Panel A: Misdemeanors							
Dismissal quality (σ_θ = 4.14 p.p.)	4.28***	-2.23***	-0.87***	-2.13***	-9.50***	1.41***	1.20***
	(0.15)	(0.14)	(0.22)	(0.15)	(0.73)	(0.17)	(0.19)
Incarceration avoidance quality (σ_θ = 2.50 p.p.)	3.87***	-2.37***	-0.94***	-1.81***	-7.94***	1.23***	1.01***
	(0.23)	(0.17)	(0.33)	(0.19)	(0.93)	(0.20)	(0.20)
Mean dep. var.	0.545	0.234	13.15	0.185	79.78	0.380	0.576
Observations	179,522	179,522	179,522	179,522	179,522	179,522	179,522
Attorneys	2,029	2,029	2,029	2,029	2,029	2,029	2,029
Panel B: Felonies							
Dismissal quality (σ_θ = 3.09 p.p.)	2.74***	-2.30***	-4.43	-2.05***	-12.97***	0.16	0.33
	(0.37)	(0.33)	(3.03)	(0.34)	(2.68)	(0.36)	(0.34)
Incarceration avoidance quality (σ_θ = 2.89 p.p.)	2.67***	-2.19***	-5.02	-2.28***	-14.91***	0.06	0.39
	(0.40)	(0.39)	(3.31)	(0.38)	(2.92)	(0.37)	(0.35)
Mean dep. var.	0.413	0.514	185.47	0.316	222.16	0.427	0.598
Observations	76,585	76,585	76,585	76,585	76,585	76,585	76,585
Attorneys	1,477	1,477	1,477	1,477	1,477	1,477	1,477
Demographic controls	Y	Y	Y	Y	Y	Y	Y
Crime controls	Y	Y	Y	Y	Y	Y	Y
List-time FE	Y	Y	Y	Y	Y	Y	Y

Note: Specification as in Table C4, on cases assigned 2012–2016, with quality rebuilt from those cases alone. Standard errors clustered at the attorney level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C6 Does Pre-Cut Attorney Quality Predict Post-Cut Outcomes?

Quality measure	Slope on pre-cut quality ($\hat{\beta}$)	Test: $\hat{\beta} = 1$ t	p-value	$\hat{\rho}$	N	Attorneys
Panel A: Misdemeanors						
Dismissal quality	0.8272	-2.34	0.019	0.846	115,525	996
	(0.0738)					
Incarceration quality	0.7311	-2.53	0.011	0.759	115,525	996
	(0.1062)					
Panel B: Felonies						
Dismissal quality	1.0018	0.01	0.994	0.970	49,956	773
	(0.2406)					
Incarceration quality	0.8763	-0.51	0.612	1.001	49,956	773
	(0.2439)					

Note: Specification as in Table 8, regressing dismissal or incarceration for court-appointed cases assigned 2012–2016 on the attorney's matching pre-cut quality from Table C4, unstandardized. The column $\hat{\rho}$ divides the slope by $\sigma_\theta(w_1)/\sigma_\theta(w_0)$ in Table 7. No defendant has the same attorney on the same list in both periods, so no defendant's own cases enter both the pre-cut measure and the post-cut outcome. Standard errors clustered at the attorney level.

Appendix D. Data Appendix

D.1 Court Records

The court records come from the North Carolina Administrative Office of the Courts (AOC) and cover charges disposed in District and Superior Court from January 2008 through December 2019. Because the AOC delivered the records in overlapping pulls, I keep one record per case from the highest-priority pull. The two latest pulls, covering 2013–2020 and 2015–2020, take precedence over a legacy pull covering 2006–2013, which in turn outranks supplemental pulls for individual counties. Additionally, two auxiliary AOC files classify each charge by offense class and by crime type, with versions that follow statutory reclassifications over time.

Before any sample restriction, I clean the records in four steps.

1. I merge the attorney-assignment, case, and offense records, standardize the offense, arraignment, disposition, and assignment dates and the defendant's name, date of birth, sex, race, and zip code, and convert sentence lengths to days.
2. I remove charge records for motions, habitual-offender and other sentencing enhancements, show-cause failures, contempt and failure-to-appear entries, and bills of particulars, none of which records the disposition of a substantive charge.
3. I keep cases disposed from 2008 through 2019 with an attorney assigned from 2008 through 2019. Assignments from 2017 through 2019 enter only as later cases for the recidivism outcomes, and cases without a recorded attorney assignment do not enter at all.
4. I drop cases in which no charge reaches a terminal disposition, because their outcome is not in the records. These are 0.19 percent of court-appointed and public-defender cases on the misdemeanor list, 3.7 percent on the lower-level felony list, and 7.4 percent on the higher-level felony list (1.4 percent overall).

The resulting analysis file holds 8.9 million charge records. The replication package lists the disposition and verdict codes behind each step and each outcome.

D.2 Linking Defendants Across Cases

Because state identification numbers appear on only 16.4 percent of cases, I link a defendant's cases with a supervised record-linkage classifier. The classifier compares records on first, middle, and last name, suffix, date of birth, zip code, race, and sex. Importantly, it never sees the state identification number, the counsel type, or any case outcome. Candidate pairs come from five blocking keys that combine names, phonetic names, birth dates, and zip codes, and these keys retain 95.9 percent of true same-person pairs in the test fold.

I train the gradient-boosted classifier on candidate pairs labeled by state identification numbers. I split the 801,517 labeled persons into training and test folds, holding out 30 percent of persons and placing no person in both folds. A pair links when its score is at least 0.93. Pairwise F_1 in the training folds is flat for thresholds between 0.90 and 0.97, and I set the threshold just below its peak to protect recall out of sample. To build the identifier, I join linked records into connected components. I also join records that share a state identification number and drop any classifier link between records with different state identification numbers.

On the held-out test fold, the classifier attains a pairwise F_1 of 0.982. In contrast, a six-pass deterministic rule attains 0.941. That rule requires exact agreement on name and date of birth, with phonetic first names and birth year and month as fallbacks. The deterministic rule rarely links two different people (precision 0.9995). However, it misses 11 percent of true same-person pairs, which are records of one person under different spellings, surnames, birth dates, or zip codes. Across the 17.2 million cases in the extract, the classifier finds 13.5 percent fewer distinct persons than the deterministic rule, raising cases per person from 3.26 to 3.77.

Errors in this linkage enter the prior-conviction count and the recidivism outcomes. A

missed link drops a case from a defendant's history, while a false link adds another person's case. In the training folds, the classifier's precision (0.994) exceeds its recall (0.967). Consequently, missed links likely outnumber false links, and residual error likely understates prior convictions and recidivism.

D.3 Linking Attorneys to the State Bar

I identify attorneys by North Carolina State Bar number, so an attorney keeps one identifier across years and counties. I assign bar numbers through four routes in a fixed order, each filling only cases that still lack one. The shares below refer to the value-added case file in row A15 of Table D1.

1. A match of the cleaned attorney name to the State Bar roster, accepted only when the name is unique in the roster (37.2 percent).
2. The bar number recorded on the case in the AOC data, which becomes nearly universal in 2015 (34.1 percent).
3. A dictionary from AOC name strings to recorded bar numbers, used only when a name maps to a single bar number (5.3 percent).
4. The bar number most often recorded for the same name string elsewhere in the records, carried to that name's remaining cases (23.4 percent).

The roster, retrieved in June 2024, lists 48,377 members. Because 15,644 of them are inactive, deceased, suspended, or disbarred, attorneys who stopped practicing before the AOC recorded bar numbers can still be matched.

When no route yields a bar number, attorneys keep a name-string identifier on all their cases. They stay in the difference-in-differences sample and leave the value-added sample entirely. The bar-number restriction removes 6.8 percent of otherwise eligible cases, at nearly the same rate for court-appointed attorneys and public defenders. At the same time, coverage is lower early in the window. Among cases assigned from 2008 through 2012, 89.0

percent of court-appointed and 90.5 percent of public-defender cases carry a bar number, against 98.0 and 97.0 percent from 2013 on. Column 2 of Appendix Table A5 restricts the court-appointed arm of the difference-in-differences sample to bar-matched attorneys.

D.4 Sample Construction

Table D1 reports the counts that remain after each restriction, in the order I apply them. Both panels start from the analysis file and collapse charges to one record per defendant-disposition. Each record keeps the most serious charge by charged offense class, breaking ties by the earliest assignment, while outcomes use every charge. I build prior convictions and recidivism before the counsel-type and bar-number restrictions, so a defendant's history includes cases with retained counsel or an unmatched attorney.

Panel A builds the value-added sample, which imposes no minimum caseload on attorneys. It keeps counties where court-appointed lists and a public defender office coexist, which supply 58.3 percent of the Table 1 sample. Additionally, it drops DWI charges and infractions but keeps traffic misdemeanors with an offense class, which are 20.1 percent of the Table 1 sample. Its last row drops cases with an unclassified crime type, leaving the Table 1 sample.

Panel B builds the difference-in-differences sample, which is not restricted to bar-matched attorneys. It keeps court-appointed cases from the 50 counties served only by court-appointed lists and public-defender cases from the 32 counties with a public defender office, so each county enters a single arm. In contrast to Panel A, it drops all traffic offenses and the two flat-fee counties. Consequently, the two panels share 77 of their 82 counties. Panel A alone holds the two flat-fee counties and three counties that contribute only court-appointed cases, while Panel B alone holds five counties that pair a public defender office with contract counsel. The last row of Panel B drops 25,729 cases missing age, sex, crime type, or the District Court indicator, leaving the estimation sample of Table 2. For the retained-case comparison in Table 5, I add retained cases that pass rows B1 through B8, B10, and B11.

Table D1 Sample Construction

	Defendant-dispositions	Defendants	Attorneys	Counties
A. Value-added sample				
A1. Analysis file	3,426,593	1,781,745	–	100
A2. Defendant 18 or older at offense and assignment	3,249,913	1,715,422	–	100
A3. Assigned 2008–2016	2,482,962	1,352,857	–	100
A4. Not a capital case (separate appointment process)	2,480,774	1,351,991	–	100
A5. Not a family or child-welfare offense (outside the lists)	2,434,065	1,337,970	–	100
A6. One attorney on the disposition	2,365,938	1,327,484	–	100
A7. Sentence fields valid	2,365,893	1,327,467	–	100
A8. Court-appointed or public defender	1,120,828	551,290	–	100
A9. Not an infraction	1,119,890	550,815	–	100
A10. Not DWI or unclassified traffic	1,076,077	537,306	–	100
A11. Assignment cell observed	1,075,684	537,196	–	100
A12. Probation term at most 3,600 days	1,075,600	537,169	–	100
A13. Attorney has a bar number	1,002,731	511,219	5,421	100
A14. Not a contract-counsel county	775,061	398,876	4,105	82
A15. Same attorney in both courts (value-added case file)	756,318	392,916	4,055	82
A16. Crime type classified (Table 1 sample)	735,496	387,287	4,041	82
B. Difference-in-differences sample				
B1. Rows A1–A2	3,249,913	1,715,422	–	100
B2. Not an infraction or waived counsel	3,248,845	1,714,868	–	100
B3. Rows A4–A7	3,102,162	1,686,599	–	100
B4. Assignment cell observed	3,022,106	1,663,010	–	100
B5. Not DWI or unclassified traffic	3,021,110	1,662,777	–	100
B6. Not a flat-fee county	2,903,997	1,599,849	–	98
B7. Assigned 2008–2017	2,483,376	1,386,512	–	98
B8. Same attorney in both courts	2,436,349	1,375,350	–	98
B9. Comparison arms only	821,604	434,270	–	84
B10. Not a traffic offense	638,832	357,466	–	84
B11. Assigned 2008–2016 (difference-in-differences file)	574,926	328,978	–	82
B12. Controls complete (Table 2 sample)	549,197	315,654	–	82

Note: Each row reports the counts that remain after the restriction, applied in the order shown. The analysis file holds cases disposed from 2008 through 2019 with an attorney assigned from 2008 through 2019 and at least one terminal charge. Attorney counts begin once attorneys are identified by bar number. Before that row, attorneys without one carry name-string identifiers, and before 2015 the AOC name field records about two spellings per appointed attorney. Restrictions A4 through A7 form a single step in the build, so their order within the step is arbitrary.

D.5 Variable Definitions

I define all variables per defendant-disposition, listing outcomes and characteristics in the row order of Table 1.

Outcomes.

Terminal charge. A charge record that carries a dismissal code, a guilty or responsible verdict, or a prayer for judgment continued. Transfer and administrative records cannot carry a verdict and are not terminal. A felony transferred to Superior Court carries its outcome on the refiled record in the same disposition.

Dismissal. An indicator for whether every terminal charge was dismissed, with at least one terminal charge, so one convicted charge rules out dismissal. Dismissals include dismissal by the court or prosecutor, no true bill or probable cause, and acquittal or arrested judgment. Requiring every charge record to be dismissed would recode 2.3, 12.0, and 16.1 percent of court-appointed and public-defender cases on the misdemeanor, lower-level felony, and higher-level felony lists as non-dismissals.

Prayer for judgment. An indicator for whether every terminal charge was dismissed or ended in a prayer for judgment continued, with at least one prayer for judgment. Because a prayer for judgment is a finding of guilt with no judgment entered, I hold it apart from dismissal and conviction, and the three categories partition the sample. It is 3.1 percent of the Table 1 sample (4.1 percent on the misdemeanor list and 0.6 percent on the felony lists).

Conviction. An indicator for whether any charge carries a guilty or responsible verdict, including verdicts on a lesser offense, and excluding prayers for judgment. I count any guilty verdict, because conviction without incarceration falls with dismissals but rises when incarceration is avoided.

Incarceration. An indicator for whether any charge carries an active sentence with a positive minimum term. Every incarcerated defendant is also convicted.

Probation. An indicator for whether a convicted charge carries a community or intermediate punishment or a positive probation term. It is zero for dismissals and prayers for judgment, and one disposition can carry both probation and incarceration.

Fines only. An indicator for whether a convicted charge carries a positive fine and the disposition carries neither incarceration nor probation.

Guilty plea. An indicator for whether any charge is resolved by a guilty plea.

Incarceration sentence. The longest minimum active term across charges, in days, and zero without an active sentence. A term above 40 years marks a miscoded record, and row A7 drops the disposition.

Probation sentence. The longest community, intermediate, or probation term across charges, in days, and zero for dismissals and prayers for judgment. The value-added sample drops terms above 3,600 days (row A12).

Recidivism. An indicator for a later disposition of the same defendant in the analysis file within one year (365 days) or three years (1,095 days) of the current disposition, with each window extended by the days of custody imposed. Custody is the active minimum term, or the maximum or recorded active term where the minimum is missing, and zero otherwise. A defendant with no later disposition is coded 0. Because later cases are dated by disposition, a case pending at the current disposition counts as a return. The records end in December 2019, which truncates the three-year window for the 7.7 percent of Table 1 cases disposed after 2016 and the one-year window for the 1.0 percent disposed after 2018. In the difference-in-differences sample, a later disposition of infractions alone does not count.

Defendant and case characteristics.

Sex and race. Indicators for male, Black, Hispanic, and other race as recorded by the AOC, with white as the omitted race. Sex is missing when unrecorded, and an unrecorded race is coded as other.

Age. Years from the date of birth to the offense date, rounded down, and missing for an invalid birth date or an age above 70.

Prior convictions and first-time offender. The number of the defendant's earlier dispositions with a conviction, counted over the full records before any sample restriction and top-coded at 10. A first-time offender has none. Because convictions before 2006 are not in the records, prior counts are understated, more so early in the window.

Number of charges. The number of distinct charges in the disposition, top-coded at 10. A charge prosecuted in both courts appears as two records and counts once. Such charges appear in 73.8 percent of higher-level felony, 50.5 percent of lower-level felony, and 8.9 percent of misdemeanor cases in the Table 1 sample.

Crime type. Indicators for the AOC crime type of the most serious charge (violent, property, financial and fraud, drugs and alcohol, traffic and public order, and miscellaneous), with violent omitted. A disposition without a classified type is missing on all six (row A16).

Offense class and court. Indicators for the offense class of the most serious charge, and an indicator for a charge disposed in District Court with no Superior Court disposition. The court indicator is missing without a District Court disposition date.

Design variables.

Counsel type. The AOC attorney-type code on the most serious charge, court-appointed or public defender. Retained cases enter only the defendant histories and the comparison in Table 5.

Appointment list. The list from which a case is assigned, set by the most serious arraignment class among the defendant's charges in the disposition (misdemeanor classes A1 and 1 through 3, lower-level felony classes E through I, higher-level felony classes A through D). Misdemeanor and felony results use this split throughout, and the value-added measures pool the two felony lists.

Assignment date and cell. The assignment date is the earliest date on which the attorney was assigned to the defendant over all of the pair's cases. Consequently, a returning defendant keeps the date of the draw that first paired them. In the Table 1 sample, 10.9 percent of cases carry a date from an earlier year than their own assignment. The assignment cell is the county, appointment list, and month and year of the assignment date.

Post-cut period. An indicator for court-appointed cases whose assignment date falls in the second half of 2011 or later, coded by half-year. Public-defender cases are never treated. Because the date belongs to the pair, 10,390 court-appointed cases assigned after the new rates took effect in May 2011 are coded pre-cut. Of these, 3,672 were assigned in May and June 2011, and 6,718 were assigned later to an attorney who had first represented the same defendant before July 2011.

Attorney value-added. Built from the outcomes above within assignment cells and appointment lists, for attorneys with a bar number. Section V and Appendix C define the estimator.