Judicial Compensation and Performance[*]

Gregory DeAngelo[†]
West Virginia University

Bryan C. McCannon[‡]
West Virginia University

September 1, 2017

Abstract

Judges have considerable influence over legal proceedings and outcomes. Intrinsic motivations are an important component to the explanation of why individuals seek out the bench. Posner (1993) argues, though, that judges maximize the same things that everybody else do, including income. In this research we utilize a unique data set from New York state on appealed felony convictions to identify judicial salary's impact on performance. We exploit a large increase in judicial salaries, which followed a twelve-year period of unchanged salaries, to identify the impact. We differentiate two alternative explanations for salary's effect on outcomes. Increased salaries can act as efficiency wages to induce currently sitting judges to work harder to continue their employment. Also, higher salaries can affect occupational choice encouraging high-quality attorneys to instead seek the bench. In both instances we anticipate an increase in the number of cases that are upheld upon appeal. By exploiting judges who served both before the wage increase and after, we can test for the presence of selection and effort separately. First, we document that the rate at which appealed convictions are reversed or modified decreases by 4.2% after the salary change went into affect. Second, while we do record a slight selection effect, we find that this change comes primarily from the higher salary encouraging higher quality decision making. Specifically, judges on the bench prior to the wage increase are significantly more likely to have their appealed cases upheld after the salary increase went into effect than before. Thus, efficiency wages are an important driver of the legal system's quality.

JEL Codes: K40, K14, H11
Keywords: appeal, career, criminal conviction, election, judge, prosecutor

1 Introduction

Judges are the linchpins of the legal system. They exercise a substantial amount of discretion with little external oversight. Appreciating what motivates them is needed to evaluate the legal system's efficacy. In a seminal contribution to our understanding of judicial decision making, Posner (1993) asks a straightforward question: "What do judges maximize?" His parenthetical response provides his answer: "The same thing everybody else does." Judges prefer to do their job well, want to avoid mistakes (exposed in part by appealed cases), enjoy leisure, and derive utility from consumption (and hence desire to receive satisfactory salaries), among other things. Contributing to the dilemma of judicial motivation, as advanced in one of Tullock's (1971) seminal contributions, judging itself is a public good. As he points out, there is little chance a judge will directly benefit from his own ruling and, therefore, the time and effort invested in reaching a good decision suffers from free riding concerns. This has lead legal scholars to point out that the free riding, principal-agent concerns combined with the substantial discretion and minimal checks on the exercise of that discretion means that a judge's intrinsic motivation is of primary importance (Ash and McLeod, 2015).

One potentially important dimension of the job of being a judge is the salary provided. While non-monetary, intrinsic benefits may be important, there are readily-available substitute occupations for individuals who possess the formal human capital and experience generally demanded to be an effective judge. Therefore a concern has been raised that this important public service is unable to attract the best individuals. While an issue across the country, a prominent example is New York state. Trial court justices did not receive any increase in their nominal wage for twelve consecutive years. Concerns arose that the state was losing viable individuals to the private sector as the opportunity cost of being a justice rose. Beginning in April 2012, a series of aggressive pay increases was initiated with the goal of bringing lower court judge salaries up to those of Federal judges. This event provides a unique opportunity to evaluate how judicial compensation affects the legal system's quality.

We explore a unique data set of every appealed felony conviction in New York state since 2007. Over 20,000 appeals were ruled upon. While errors are, by their very nature, difficult to assess directly, we argue that the appellate court's evaluation of the lower court's decisions is a reasonable metric of mistakes.[1] This is especially relevant given the argument by Judge Posner (1993; 2008)

---

[1] A similar strategy is employed by McCannon (2013) and DeAngelo and McCannon (2017).

that appeal avoidance is an important component to the judicial utility function. Using case-level data, we examine the determinants of upheld appeals of lower court convictions.

Our empirical strategy is to differentiate judges who only serve before the pay increase, those who serve only after the pay increase, and those whose careers straddle the salary change. With the first as the reference group, those who join the bench after the pay adjustments went into effect can be affected both by selection effects (they ran for office in part because of the high wage) and efficiency wages (their effort is enhanced because of the risk of losing the high wage). If the quality of their rulings are higher, we would not be able to differentiate the source of that improvement. Those who straddle the pay increase, though, allow us to differentiate the two effects. Relative to the pre-period, if ruling quality improves within this cohort, then efficiency wages matter. If their decision making does not improve, then a measurable bump in affirmation rates after the pay increase would be driven by selection effects.

We find that after the pay increase, the proportion of appealed trial court convictions that are not upheld decreases by 4.2%. Differentiating justices, we find that those who join the bench after the wage improvement experience an increase in the probability an appealed conviction is upheld by approximately 16 percentage points. For cases overseen by a justice that straddles the pay change, in the period prior to the wage increase, the probability the conviction is upheld increases by 1.4 percentage points, but after the wage increase the affirmation likelihood increases by 14.0 percentage points. These results strongly suggest that efficiency wages are a driving factor in judicial effort provision, and selection effects, while they exist, are a more minor contributor to legal system quality. Given our results, we find some empirical support for New York state's stated policy goal of bringing higher quality justices to the bench. Moreover, the unstated outcome - that higher wages incentivize judicial effort - is shown not only to exist, but to have a meaningful impact on case outcomes.

Our work contributes to the robust literature on judges' behavioral responses to retention concerns. This research focuses primarily on judicial election's impact on sentencing decisions (Berdejo and Yuchtman, 2013; Lim, 2013; Cohen *et al.*, 2015). Alternatively, the comparison between election and appointment mechanisms has received attention (Shephard, 2009; Choi *et al.*, 2010; Iaryczower *et al.*, 2013). Choi *et al.* (2009) addresses the issue of whether judges are underpaid. They conduct a cross-sectional analysis of state supreme court opinion writing and do

not find a relationship between wages and output. However, their analysis is unable to differentiate salary's causal impact on effort from selection effects.[2] Additionally, the existing literature does not examine lower court judges where job retention, promotion possibilities, and compensation are all relevant. To the best of our knowledge we are the first to evaluate the direct impact of judicial pay on the quality of legal system outcomes.

Our work complements research on prosecutor salaries as well. Boylan (2004) shows that Assistant U.S. Attorneys experience a higher turnover when their salaries are lower, which, consequently, lowers output. Furthermore, Boylan and Long (2005) provide evidence that they take more cases to trial when private labor-market opportunities are better. Relatedly, both the intensive and extensive margins of prosecution improve when the funding of the prosecutor's office improves (Rasmusen *et al.*, 2009). Economies of scale can have important effects on prosecutorial production (Detotto and McCannon, 2016; 2017).

The paper proceeds as follows. In Section 2 we briefly outline the history of judicial wages in New York state and develop our working hypotheses. Section 3 presents the data and data collection techniques. The empirical analysis is presented in Section 4 and in Section 5 we conclude.

2 Judge's Salary in New York and Testable Hypotheses

Felony criminal cases in New York state are initially heard in County Court or Supreme Court, which is the state's (rather than the county's) trial court. Each of the sixty-two counties in New York state has one County Court. Also, the state is divided into thirteen Supreme Court districts. Each county in New York is served by one County Court and one Supreme Court, but with potentially many justices within each system.

The Supreme Court has unlimited jurisdiction in civil and felony criminal cases. Each County Court handles felony cases, misdemeanors, and lawsuits less than $25,000. Supreme Court justice pay is set by the state legislature. By law the County Court justices are paid 95% of the salary of Supreme Court justices. Judges in New York state are selected in popular, partisan elections serving 10-year (county) and 14-year (supreme) terms, respectively. Additionally, the Governor is able to make appointments to fill vacancies.

---

[2]Legal scholars have debated the issue, but the discussion has focused on selection effects, and relatively ignored efficiency wage arguments, and focused on higher courts (Frank, 2003; Baker, 2008; Anderson and Helland, 2012).

3

On January 1, 1999 judge's salaries were increased. The lower court judges in New York state, then, went twelve years without an increase in their nominal salary. In 2011, amid growing concerns that judicial quality was being hampered as skilled individuals chose higher paying, private sector jobs, judicial pay was increased (Commission, 2015). The first salary increase took effect April 1, 2012 and increased annually thereafter. Again, in 2015 a state bill was passed to continue the wage improvements with the objective to have the pay of state Supreme Court justices equal Federal judge salaries by 2018.[3] Quoting the Commission's findings, "to sustain and enhance that stature, New York must maintain and strengthen its ability to attract the best and brightest legal minds to its Judiciary and retain them" (Commission, 2015, p.1). The adjustment represents a 41% increase in pay between April 2012 and April 2016 with additional increases planned for 2017 and 2018.

New York state does not conform to either the Federal judiciary or other states. To illustrate this, we collect data from the National Center for State Courts, which provides annual surveys of judicial compensation across the country. Figure 1 tracks trial court justice's salaries in New York state and compares them to Federal judges and neighboring states New Jersey, Pennsylvania, and Connecticut. Salaries are normalized to their 1999 levels within each state.

New York state sits well below the comparison states across the entire time period. Interestingly, growth in salary in New Jersey, Pennsylvania, and Connecticut outpace the Federal judge salaries, which is New York's benchmark. Prior to 1999 justice salaries in New York were updated regularly. For example, for the decade of the 1990s, New York justices received nominal wage increases in 7 of the 10 years. Overall, justice's nominal salary increased by over 28% in the 1990s, compared to 0% in the 2000s. Thus, the wage disparity was different not only from other states, but from New York's own history.[4]

How might judge's salary affect the legal system's outcomes? One clear channel is through selection. The Commission's report provides information on the salaries of private attorneys in the major cities across the state. If an individual is making a career decision, higher private market wages can shift the occupational choice decision for many individuals. Under a reasonable assumption that the wage an individual can receive in the private sector is correlated with the

---

[3]Part E, chapter 60 of the Laws of 2015 was enacted to create a quadrennial commission to "examine, evaluate and make recommendations with respect to adequate levels of compensation and non-salary benefits" for judges.

[4]Appellate justices in New York state earn a slight premium over the trial court justices. This is because they are selected by the Governor from the set of elected lower court justices.Therefore, they consistently earn a modest premium over the trial court justices.

individual's professional quality, then the wage disparity creates a lower average quality of those who choose to serve as a judge. Therefore, an occupational selection framework would hypothesize that judicial quality should improve with salary.

Additionally, the theory of efficiency wages (Shapiro and Stiglitz, 1984) suggests that the threat of losing one's job, and the high wage it provides, has the benefit of discouraging shirking by employees. Applied to judges, there is the threat that they will either not be re-elected by the voters or not be re-appointed by the Governor. If wages are high, then this becomes a threat that incentives the effort required to do a good job.[5] Thus, the efficiency wage theory would hypothesize that there is a change in the judge's incentives that should improve the quality of their decisions when compensation improves. Therefore, our identification strategy is to use this dramatic pay increase in New York state to differentiate the effect of the salary's selection effect from its efficiency wage effect.

3 Data Description

Our primary data source comes from New York state's appellate court. To adequately describe the data used in the project, we first provide a brief description of New York state's appellate court and the published opinions we use in our analysis. Second, we detail the information on judge biographies and backgrounds.

3.1 Appellate Court

Whether a case originates in the County Court or Supreme Court, appeals of those convictions are handled by the Appellate Division. New York state is partitioned into four appellate departments. Within each department there are 12-15 appellate justices who serve in, typically, five-person panels. Throughout the year, the panels rotate in composition. The appellate justices are selected by the Governor from the pool of elected Supreme Court justices to serve five-year terms. The appellate court hears appeals of all criminal and civil cases across the department.[6]

---

[5]Alternatively, high wages can trigger a preference for reciprocity, expressed as more effort being expended as a reciprocal exchange for the greater salary (Akerlof, 2982). Our analysis is unable to differentiate the two sources of effort provision.

[6]While not included in the data studied here, appeals of misdemeanor convictions are handled by the County Court in the Third and Fourth Departments (upstate New York), while the Appellate Term reviews such convictions in the First and Second Departments. We restrict attention to felony crimes. Additionally, while the selection and

For each appeal decided, a *slip opinion* is published. The length and content of each slip opinion varies. Each, though, provides basic information on the dispute: decision, lower court involved, dates, appellate justices, lower court judge and defensive representation, etc. A short narrative is included providing a justification for the decision reached. Frequently, a brief summary of the appellant's claims and the court's response is provided. Relevant case law is cited.

Each slip opinion is maintained in portable document format on each of the four appellate department's web sites, which we scraped, then cleaned for further analysis. Our data contains slip opinions that start in 2007 and extend through April 2017. A total of 117,398 slip opinions are collected. Of these, 20,644 involved appeals of criminal convictions. The remainder are appealed civil cases. These slip opinions correspond to original court cases that extend back as early as 1981.

The corpus of slip opinions was converted into a flat file database. Classifiers were then developed to conduct a series of regular expression and *bag of word* extractions of specific information from the slip opinions. The date of the original case and the appeal, the outcome of the appeal, and which court the case was heard were collected. The outcome can either affirm, dismiss, modify, or reverse the trial court's conviction. Also, indicator variables for whether the original case was decided in a bench trial, jury trial, or whether a guilty plea was entered were created. The document length (in words) was also gathered. Finally, we recorded the name of the original justice that presided over the case[7], the county where the case was heard, and the identity of the appellate justices on the panel that ruled on the appeal.

Two additional, important pieces of information were also collected from the slip opinions. First, the crime being appealed was recorded. Overall, thirty-one separate crime categories were identified with a unique indicator variable created for each. Second, the grounds of the appeal were identified from the text.[8] A total of twelve separate grounds for appeal were recorded. Table 7 in the Appendix contains a full list of the crimes and grounds for appeal that were extracted from the slip opinions. Both of these extractions were completed utilizing both standard regular expression

---

effort of appellate justices are likely to be an important issue, it is unclear how and whether it would affect conviction affirmations.

[7]In some cases multiple justices were involved. For example, one justice might have handled the preliminary hearing, while another justice handled the trial. In these instances, we only coded the justice that handled the trial portion of the court proceedings.

[8]Crimes were identified by conducting a regular expression extraction from the plea or conviction statement in the slip opinion. So, the crimes were extracted from either the statement "upon his/her plea of guilty of [CRIME HERE]" or "convicting him/her of [CRIME HERE], upon". Grounds for appeal were extracted using a regular expression extraction of the grounds for appeal from McCannon (2013).

and bag of words extractions. The extractions were manually checked to confirm their accuracy.

Regarding the decision reached on a slip opinion, we consider a distinct measure of the appellate court's ruling on the trial court's conviction. Specifically, our dependent variable is binary taking a value of one if the lower court's decision stands upon appeal. It is common for a particular individual's conviction to involve multiple crimes. Each conviction can be appealed and, therefore, multiple decisions are made by the appellate court and published on one slip opinion. As such, we consider only cases where the appellate justices believe no errors were made in the original judgment (i.e., no conviction was reversed or modified). We denote this as the indicator variable *Upheld*, which is the same dependent variable used in McCannon (2013) and DeAngelo and McCannon (2017).

3.2 Data on Judges

To supplement the case-level data, biographies were created on each justice in New York state. Of the 20,644 slip opinions in our data set, a total of 518 justices appear in the data. Biographic information is primarily identified from the New York State Unified Court System.[9]

From these two sources we record basic information, such as the justice's gender, law school attended, and the year s/he graduated from law school. Graduation year allows us to calculate an experience measurement.[10] We differentiate justices who attended a law school within the state from out-of-state justices recognizing that selection is driven in part by local political connections. Information on the justice's career prior to becoming a judge is also collected. Specifically, we identify whether the individual worked as a prosecutor or a public defender. Additionally, we record which courts s/he has served in over his/her career.[11]

---

[9]See nycourts.gov, which provides biographies of all active justices. Missing information is gathered from the Ballotpedia project (www.ballotpedia.org), which provides biographic information on elected politicians across the country. In a small number of situations, online web searches are used to fill in missing information. For example, local media coverage of a justice's retirement provides missing career information. Additional information from media sources is collected for only 44 justices (6.7% of all justices in the sample), which represents 7.8% of the cases in our data set. Our data collection efforts result in the coverage of 88.5% of the justices and 98.82% of all slip opinions. Therefore, the justices with unrecoverable biographic information are those who are involved in very few cases. Indicator variables are created for the missing information.

[10]One justice in New York state did not attend law school. Thus, the year used is one year less than the year he was admitted to the bar. This justice presided over 126 appealed felony convictions in our data set.

[11]Supreme Court justices are distinguished by whether they hold an elected position, or are appointed by the Governor. Justices in the Supreme Court are separated into three categories: those who only have an appointed position (labeled $SCAJ$), those who only have an elected position (SC), and those who were first appointed but then elected to the position ($SCAJ$-SC). It is not uncommon for an elected County Court justice to also receive an associate justice appointment in the Supreme Court. Also, it is not rare for an individual to first be elected to serve as a County Court justice (labeled as the indicator variable CC) and, later in his/her career, run for a Supreme Court position. Such an individual would be recorded as having served in both.

Finally, information on the election cycles are used. For each justice we identify the year in which s/he became a judge and which years s/he ran for re-election. This allows us to identify the pay regime in which a justice served under.

Table 1 is divided into two sections. The first panel provides judge-level summary statistics, while the second panel provides the case-level summary statistics used in the empirical analysis. In Panel A we examine the average total number of appeals per justice and the outcome of appealed cases, as well as the justice's experience at the time of the ruling and which court that they served. On average, justices have approximately thirty-six judgments appealed. There is a substantial amount of variation in judicial experience. Cases are distributed rather equally across the two court systems. On average, a justice runs for just over one re-election.

A conviction is upheld approximately 80% of the time and affirmed 90% of the time. Dismissals, modifications and reversals of cases each occur approximately 10% of the time. As stated previously, since one slip opinion can provide the appellate ruling for an individual with more than one conviction, these rulings sum to a number greater than one. Similar outcome breakdowns arise in Panel B. It is noteworthy that the standard deviations are greater. This suggests that there is more consistency across justices than within a justice over time. It is also consistent with the hypothesis that variation in incentives over time are important.

In Panel B we examine case-level data from the 20,644 criminal appeals in our data set. On average, we find that each appealed case takes almost 2.5 years to reach the appellate court. There is a wide variation in opinion length. Jury trials are almost as prevalent as guilty pleas. Since plea bargaining occurs in 95% of felony cases typically, this suggests there is an important selection effect in which convictions are appealed.[12]

We divide our justices into three groups based on the April 2012 wage increase. First, we identify justices that exist in our data only prior to the wage change (*Pre-Wage*). Second, we identify those who were in office both before and after the wage change (*Both*). Finally, we identify justices who were only in office after the wage change (*Post-Wage*). In Table 2 we provide summary statistics for these three groups of justices. The most notable difference is that justices in the *Pre-Wage* group have more experience and more appealed cases compared to either the *Post-Wage* or *Both* groups. If the ratio of appeals per year is calculated, the pre- and post-wage cohorts are similar.

---

[12]The omitted category is opinions that do not reveal the mode of conviction. Even if all of these were in fact plea bargained cases, jury trials would still represent almost one-quarter of all cases.

Otherwise, we do not observe obvious differences in appeal outcomes in our summarization of the data. To further examine the relationship between the likelihood that a case is upheld and wage increases for judges, we now turn to a more robust empirical analysis.

4 Empirical Analysis

To engage in the empirical analysis, we first present the main results highlighting the change in upheld, appealed convictions. Second, we decompose the intertemporal effects, both the gradual change over time after adoption, and the possible anticipation beforehand. Finally, we evaluate the robustness of the main results.

4.1 Main Results

To examine the effect of a justice's wage on performance, Equation 1 employs a standard difference-in-differences approach to determine the effect of the wage increase.

$$
\begin{aligned}
Upheld_{ijmy} = \beta_1 PostWage_{ijmy} + \beta_2 Both_{ijmy} + \beta_3(Both_{ijmy} * PostPeriod) + \\
X_j + X_{jmy} + m * y + \delta_i + \varepsilon_{ijmy}
\end{aligned}
\tag{1}
$$

As noted above, we divide our sample into three groups of judges: (1) justices who are only in our data prior to the wage increase, (2) justices who are only in our data after the wage increase, and (3) justices who are in our data both before and after the wage increase. Our omitted group is the justices that were active only prior to the wage increase.[13] The group of justices that only appear after the wage increase are captured in the $Post\text{-}Wage_{ijmy}$ variable. These denote justice j in county i of month m and year y, who only experience the high wage after April 2012. We separate the remaining justices into the variables $Both_{ijmy}$ and $Both_{ijmy}*Post\text{-}Period$. The $Both_{ijmy}$ variable captures the justice's behavior in the period prior to the wage increase, while the $Both_{ijmy}*Post\text{-}Period$ variable measures the the April 2012 wage improvement's effect on justices

---

[13]Another way of thinking about these groups is that there are four types of opinions in our data. Opinions that are generated (1) pre-increase by a justice who was only on the bench prior to the wage increase, (2) pre-increase by a justice who was on the bench prior to and after the wage increase, (3) post-increase by a justice who was only on the bench after the wage increase, and (4) post-increase by a justice who was on the bench both prior to and after the wage increase.

who were in their position prior to the salary change.

Additional controls are included in the analysis. With regards to each justice we include controls for the gender, previous experience (previously an assistant district attorney or assistant public defender), and whether the justice attended a New York state law school. Our case-level controls include whether the case was handled in the Supreme Court or County Court, whether the case was a jury or nonjury trial, and the number of days between the original judgment and initial appeal date. Lastly, thirty-one controls for the crime committed in the original case and twelve grounds for appeal controls are also included in our analysis.

Utilizing this specification allows us to separately identify two effects of the wage increase. First, β_1 identifies whether trial court outcomes are improving. It cannot, though, tell us the source of improvement. In other words, if $\beta_1 > 0$, then either higher wages are inducing the selection of more-capable individuals or encouraging higher effort from those holding the judicial seat. Second, β_3 measures the effect of the wage increase on the performance of those currently holding the judicial office. If $\beta_3 > 0$, then this implies that justices are responding to higher wages by performing their job with fewer errors in the period following the wage increase. The coefficient β_2 on the other hand, considers this cohort's decisions prior to the wage increase. It, therefore, can only be affected by selection. If only β_3 is nonzero, then the efficiency wage argument dominates. Alternatively, if only β_2 is nonzero, then selection alone is driving the improvements. If the interaction term is dropped from (1), then β_2 would capture both the selection and effort effects. By including the interaction term, the two effects are disentangled. Comparing the two coefficients allows us to gauge the relative importance of the two factors we hypothesize affects outcomes.

Table 3 examines the likelihood that an appealed case is upheld. In the first column we run an OLS model that includes county, month and year fixed-effects while controlling for the convicted crimes in the original case, grounds for appeal, and a trend for the number of appealed cases in a given month.[14] The specification in the second column is identical to the first column, expect that we estimate a Probit model. Columns 3 and 4 are similar to the first two columns, except that we now include month by year fixed effects rather than separate month and year fixed effects.

We find that justices who joined the bench after April 2012 are approximately 6 to 16 percentage points more likely to have their cases upheld upon appeal, relative to justices who are on the bench

---

[14]Less saturated models with very similar results generate the same qualitative results.

only in the period prior to the wage change. From Table 1, this represents as much as a 19.5% increase in the affirmation rate. Thus, the effect is non-negligible. Of those justices who were on the bench both before and after the April 2012 wage change, we observe a 1.4 percentage point higher likelihood that their appealed case is upheld relative to justices on the bench only in the preperiod. Additionally, for those judges in both the pre- and postperiod, we observe a 3 to 14 percentage point higher likelihood that their case is upheld in the postperiod. The Probit analysis finds similar results. Moreover, our AIC is much smaller when we include month-by-year fixed effects, so we will focus on this model as we proceed forward in our analysis.

These findings strongly suggest that the salary increase achieved its goal of improving the quality of the legal institutions in New York state. Our novel finding is that the selection effect, which is the dominant argument for proponents of the wage increase, is minimal. The efficiency wage effect is substantial.

We conduct an F-test to determine whether the *Post-Wage*, *Both* and *Both × Post-Period* coefficients are statistically different from each other using the specification in the first column. The β_2/β_3 and β_1/β_2 pairs are significantly different from one another. As such, we find support for both an entry and efficiency wage effect. Since β_1 and β_3 are not statistically different from one another, this implies that entering judges are no better than those currently sitting on the bench. However, since β_2 is statistically different from β_3 (and β_1), this implies that the efficiency wage argument induces "better" decision making for justices on the bench. Moreover, this means that those entering and those staying on the bench are both higher quality justices than those who have historically sat on the bench.

4.2 Event-Study Analysis

To more directly examine the effect of the wage change on the quality of the judgments, we conduct an event study analysis by dividing our appeals data into six month intervals before and after the wage change. We then re-run the above specification, but include separate indicators for each six-month period, as seen in Greenstone and Hanna (2014), with separate month and year fixed effects. The control group is comprised of those individuals that either have not received the wage increase yet or that will not receive the wage increase because they leave the data set prior to the wage increase. The treatment group is comprised of the justices that have received a wage increase

in the postperiod. This approach allows us to identify whether heterogeneous effects of the wage change exists over time.

Figure 2 plots the difference-in-difference estimates in the six-month intervals leading up to and after the wage change, including all previous controls. Importantly, the estimates satisfy the parallel trends assumption, as the differences in the likelihood that a case is upheld in the preperiod between the treatment and control groups are not statistically different from one another. This can be seen by all difference-in-difference estimates and confidence intervals hovering around zero. However, in the postperiod we see our estimates pulling apart. In all but the first two six-month intervals in the postperiod, we find significant differences in the likelihood that a case is upheld. In fact, by 3 to 6 years after the wage change the likelihood that a case is upheld is 20-40% higher than the control group. Thus, when we break our difference-in-difference estimates apart we find even stronger evidence that the wage change dramatically affected the quality of the judicial performance.

4.3 Testing for Anticipation Effects

While Table 3 identifies significant positive effects of justices who were in the position of a justice before and after the wage change on the likelihood a case is upheld, this could be an anticipation effect (Ashenfelter, 1978). In other words, individuals could be anticipating the wage increase and running for the elected judicial positions prior to the higher wages being put in place. Thus, we have argued that the positive value of β_2 is coming from a selection effect. Alternatively, justices in the *Both* group could have been in the position before any knowledge that a wage increase was announced. They choose to run for the seat when the real wages are low. Therefore we further divide the *Both* group into those judges that were in the position prior to 2009 (*Entry Pre-2009*) and those entering the position after 2009, but before April 2012 (*Entry Post-2009*). This enables us to examine whether the wage increase improved legal outcomes by inducing more effort from those already in a judicial position, or induced better quality candidates to seek the position.

In Table 4 we separately identify whether entry or effort appear to be driving the increase in judicial quality that occurs when wages increase. As noted above, we have divided the justices in the *Both* group so the *Entry Pre-2009* subgroup will be justices that are increasing their effort in response to the wage increase while the *Entry Post-2009* are justices who entered the judge

position and were likely knowledgeable about the forthcoming wage increase. We examine the separate effects of these groups on the likelihood that a case was upheld both in the pre- and postperiods.

Examining the same specifications from Table 3, we find evidence that supports both the effort and entry stories. Again, the effort story appears to be much more pronounced and rather uniform across the two groups. Examining the first column where we run an OLS regression, we note that the *Both × Entry Pre-2009* and *Both × Entry Post-2009* variables capture any differences in the likelihood that a case would be upheld in the pre-period relative to those justices who are only in our data for the pre-period. Interestingly, we find weak evidence that the group of justices who are likely unaware of the forthcoming wage change (*Both × Entry Pre-2009*) are the ones with an effect statistically distinguishable from zero. However, the estimated effects are nearly identical fo the two. Therefore, the argument for selection effects is weak.

When we examine the post-period, we observe significant differences in behavior. First, the *Entry Pre-2009* group is approximately 14 percentage points more likely than the *Pre-Wage* justices (omitted group) to have their case upheld in the post-period, whereas the *Entry Post-2009* group is only 11 percentage points more likely. While distinct, the two cannot be statistically differentiated from each other. Moreover, both of these coefficients are not significantly different from the *Post-Wage* coefficient in the post-period. By breaking up the justices who remain on the bench after the wage change into those who could have anticipated the effect from justices who likely could not have anticipated the wage change, we find that these justices, in addition to the justices that join the bench after the wage increase, converge toward a higher quality performance, as measured by a higher likelihood of having their cases upheld on appeal.

4.4   Robustness Checks

To examine the effect of the wage change on judge behavior, we conduct a permutation test where we randomly assign a wage change date and examined the effect of this random assignment on the behavior of justices who were in the position both before and after the actual wage change. This creates a 'placebo' salary adjustment. The regression coefficients are calculated (using the specification in Table 1, first column) and recorded. This process is repeated 1000 times creating 1000 estimates from the placebo wage changes. If our results arise from a spurious correlation,

then the randomly-created placebo wage changes will frequently have a statistically significant correlation with appeal outcomes. If, alternatively, the wage change has a direct effect, then the placebo policy changes will rarely correlate with upheld conviction likelihoods.

Table 5 presents the results of this permutation test. The coefficients on the *Both* and *Both* × *Postperiod* variables are presented. In the pre-period our results are not statistically different from the results that we would have obtained by randomly assigning a wage change date. However, when we examine the interaction between the *Both* group and the post-period, we find that our estimates are significantly different. Examining the estimated coefficient of the 1st and 99th percentile, our estimate falls outside this interval. It is very unlikely that the upheld rate increase observed is due to random chance. Therefore, we conclude that the increase in effort exerted by the group of justices who straddle the change in the post-period is not due to spurious correlation, but rather is a unique effect that is driven by the introduction of the wage increase in April 2012.

Additionally, we ran a falsification test of the wage change by dropping all observations after 2009 from our sample and then imposing a false wage change in April 2005. We chose this date as it is in the middle of a time period when justices received no change in their compensation. We then re-estimated the specification in Table 3, which we present in Table 6.

Importantly, we do not find any effect of the false wage change on the behavior of justices who joined the bench after 2005 or for those who were on the bench prior to 2005 and remained on the bench after 2005.[15] These falsification results further reinforce the impact of the true wage changes, as they appear to be unique to the environment where an actual wage change occurred, and not due to spurious correlation.

5 Conclusion

In 2012 New York state began a series of aggressive pay increases for its lower court justices raising their wage over 40%. This rectified a twelve year freeze in their nominal wage. Under concerns that low relative pay was driving highly qualified individuals to pursue alternative legal careers, New York acted with the intention to improve the quality of its legal system. Using this event, we evaluate the impact of judicial salary. To measure trial court's quality, we utilize a data set of over

---

[15]Our results are not sensitive to the use of 2005 as a false wage change date. In fact, we ran 48 falsification tests for each month of the years 2004-2007 and obtained nearly identical estimates for all of these results.

20,000 felony conviction appeals. First, we document that the probability an appealed conviction is upheld increases after the pay increase. Second, we explore the driver of this change. While the policymakers' stated objective was to select high-skilled individuals, we provide evidence that higher pay acts as an efficiency wage that can induce more effort, which leads to better outcomes. We disentangle the two effects and find that while selection effects are prevalent, most of the increase in the quality of the lower court's rulings in criminal cases comes from greater effort.

Thus, not only do we provide direct empirical evidence that improved judicial pay leads to better outcomes of the legal system, but that it is an unstated determinant that leads to the improvement. As famously argued by Judge Posner (2003), judges maximize the same things as everyone else. Thus, incentivizing judicial effort is an important and relatively unexplored area of empirical legal research.

Our analysis focuses on the criminal justice system. Presumably, judicial compensation improves the decision making in civil cases as well. Documenting this effect is left for future research. Additionally, within criminal law, we use appeals as our metric of mistakes. Successful appeals will primarily be driven by procedural problems. Thus, it is a test of judge's officiating function. Sentencing is a different, important action that justices perform. We also leave for future research an evaluation of whether sentencing differences occur with pay changes.

Our analysis evaluates the effect that wages have on incentivizing judges to perform their job well. There are numerous other external motivations that matter as well. For example, a lower court justice may be interested in advancing politically to higher positions. While for the most the position is the pinnacle of previous advancements, for a few promotions to a higher court, for example, may be possible. One would expect motivations such as this are orthogonal to wages and, therefore, unrelated to the dramatic wage shift that occurred.

There are some important empirical limitations to consider. For example, the decision to file an appeal is endogenous. While the factors that influence the decision to appeal are likely unrelated to the judge's salary directly, it is an open question whether our results are causal. Relatedly, the legislative decision to raise salary opens up potential endogeneity concerns. Falsification tests and permutation tests strongly suggest there is a direct effect. Additionally, we use as our data set a comprehensive collection of appealed convictions. We do not have information on the universe of all criminal cases ruled upon during the period. Thus, our results are best interpreted as conditional

on the case being appealed.

6   References

Acquaviva, Gregory L. and John D. Castigilione (2009), Judicial Diversity on State Supreme Courts, *Seton Hall Law Review* 39: 1203-1261.

Akerlof, George A. (1982), Labor Contracts as Partial Gift Exchange, *Quarterly Journal of Economics* 97: 543-569.

Anderson, James M. and Eric Helland (2012),How Much Should Judges Be Paid? An Empirical Study on the Effect of Judicial Pay on the State Bench, *Stanford Law Review* 64:1277-1342.

Ash, Elliott and W. Bentley MacLeod (2015), Intrinsic Motivation in Public Service: Theory and Evidence from State Supreme Courts, *Journal of Law and Economics* 58(4): 863-914.

Ashenfelter, Orley, Theodore Eisenberg, and Stewart J. Schwab (1995), Politics and the Judiciary: The Influence of Judicial Background on Case Outcomes, *Journal of Legal Studies* 24(2): 257-281.

Baker, Scott (2008), Should We Pay Federal Judges More?, *Boston University Law Review* 88: 63-112.

Bandyopadhyay, Siddhartha and Bryan C. McCannon (2013), Re-election Concerns and the Failure of Plea Bargaining, *Theoretical Economics Letters* 3(1): 40-44.

Bandyopadhyay, Siddhartha and Bryan C. McCannon (2014), The Effect of the Election of Prosecutors on Criminal Trials, *Public Choice* 161(1-2): 141-156.

Bandyopadhyay, Siddhartha and Bryan C. McCannon (2015), Prosecutorial Retention: Signaling by Trial, *Journal of Public Economic Theory* 17(2): 219-256.

Baye, Michael and Joshua Wright (2011), Is Antitrust too Complicated for Generalist Judges? The Impact of Economic Complexity and Judicial Training on Appeals, *Journal of Law and Economics* 54(1): 1-24.

Besley, Timothy and Stephen Coate (2003), Elected Versus Appointed Regulators: Theory and Evidence, *Journal of the European Economic Association* 1(5): 1176-1206.

Bibas, Stephanos (2004), Plea Bargaining Outside the Shadow of Trial, *Harvard Law Review* 117(8): 2463-2547.

Boylan, Richard T. (2004), Salaries, Turnover, and Performance in the Federal Criminal Justice System, *Journal of Law and Economics* 47(1): 75-92.

Boylan, Richard T. and Cheryl X. Long (2005), Salaries, Plea Rates, and the Career Objectives of Federal Prosecutors, *Journal of Law and Economics* 48(2): 627-651.

Choi, Stephen J., G. Mitu Gulati, and Eric A. Posner (2009), Are Judges Overpaid? A Skeptical Response to the Judicial Salary Debate, *Journal of Legal Analysis* 1(1): 47-117.

Choi, Stephen J., G. Mitu Gulati, and Eric A. Posner (2010), Professionals or Politicians: The Uncertain Empirical Case for an Elected rather than Appointed Judiciary, *Journal of Law, Economics, and Organization* 26: 290-336.

Cohen, Alma, Alon Klement, and Zvika Neeman (2015), Judicial Decision Making: A Dynamic Reputation Approach, *Journal of Legal Studies* 44(S1): S133-S159.

Commission on Legislative, Judicial and Executive Compensation (2015), *Final Report Judicial Compensation*, December 24, http://www.nyscommissiononcompensation.org/pdf/compensation-report-dec24.pdf.

Craig, Steven G., Yi-Cheng Ho, and Alan J. Satterlee (2008), The Demand for Judicial Sanctions: Voter Information and the Election of Judges, *Economics of Governance* 9(3): 265-285.

Cranes-Wrong, Brandice, Tom Clark, and Jee-Kwang Park (2012), Judicial Independence and Retention Elections, *Journal of Law, Economics, and Organization* 28(2): 211-234.

Damle, Sarang Vijay (2005), Specialize the Judge Not the Court: A Lesson from the German Constitutional Court, *Virgina Law Review* 91: 1267-1311.

DeAngelo, Gregory and Bryan C. McCannon (2017), Using Appellate Decision to Evaluate the Effect of Judicial Elections, Working Paper.

DeFigueiredo, John M. and Emerson H. Tiller (1996), Congressional Control of the Courts: A Theoretical and Empirical Analysis of Expansion of the Federal Judiciary, *Journal of Law and Economics* 39(2): 435-462.

Detotto, Claudio and Bryan C. McCannon (2017a), Economic Freedom and Public, Non-Market Institutions: Evidence from Criminal Prosecution, *Economics of Governance* 18(2): 107-128.

Detotto, Claudio and Bryan C. McCannon (2017b), Consolidation of Prosecutor Offices, Working Paper.

Dyke, Andrew (2007), Electoral Cycles in the Administration of Criminal Justice, *Public Choice* 133(3/4): 417-437.

Francois, Patrick (2000), Public Service Motivation as an Argument for Government Provision, *Journal of Public Economics* 78(3): 275-299.

Frank, Michael J. (2003), Judge Not, Yet Yee Be Judged Unworthy of a Pay Raise: An Examination of the Federal Judicial Sarly 'Crisis', *Marquette Law Review* 87: 55-122.

Greenstone, Michael and Rema Hanna (2014), Environmental Regulations, Air and Water Pollution, and Infant Mortality in India, *American Economic Review* 104(10): 3038-3072.

Guthrie, Chris, Jeffrey J. Rachlinski, and Andrew J. Wistrich (2009), The "Hidden" Judiciary: An Empirical Examination of Executive Branch Justice, *Duke Law Journal* 58: 1477-1530.

Hanssen, F. Andrew (1999), The Effect of Judicial Institutions on Uncertainty and the Rate of Litigation: The Election Versus Appointment of State Judges, *Journal of Legal Studies* 28(1): 205-232.

Huber, Gregory A. and Sanford C. Gordon (2007), Directing Redistribution: On the Political Control of Lower Court Judges, *Journal of Law, Economics, and Organization* 23(2): 386-420.

Kang, Michael S. and Joanna M. Shepherd (2011), The Partisan Price of Justice: An Empirical Analysis of Campaign Contributions and Judicial Decisions, *New York University Law Review* 86: 69-130.

Iaryczower, Matias, Garrett Lewis, and Matthew Shrum (2013), To Elect or to Appoint? Bias, Information, and Responsiveness of Bureaucrats and Politicians, *Journal of Public Economics* 97: 203-244.

Kang, Michael S. and Joanna M. Shepherd (2015), Partisanship in State Supreme Courts: The Empirical Relationship between Party Campaign Contributions and Judicial Decision Making, *Journal of Legal Studies* 44(S1): S161-S185.

Katz, Leo (2011), *Why is the Law So Perverse?*, University of Cambridge Press.

LaFountaine, Robert C., Richard Y. Schauffler, Shauna M. Strickland, and Kathryn A. Holt (2012), *Examining the Work of State Courts: An Analysis of the 2010 State Court Caseloads*, Court Statistics Project. National Center for State Courts.

Landes, William M. and Richard A. Posner (1975), Independent Judiciary in an Interest-Group Perspective, *Journal of Law and Economics* 18(3): 875-901.

Lim, Claire S. H. (2013), Preferences and Incentives of Appointed Versus Elected Public Officials: Evidence from State Trial Court Judges, *American Economic Review* 103(4): 1360-1397.

Lim, Claire S. H. and James M. Snyder Jr. (2015), Is More Information Always Better? Party Cues and Candidate Quality in U.S. Judicial Elections, *Journal of Public Economics* 128: 107-123.

Lim, Claire S. H., James M. Snyder Jr., and David Strömberg (2015), The Judge, The Politician, and the Press: Newspaper Coverage and Criminal Sentencing across Electoral Systems, *American Economic Journal: Applied Economics* 7(4): 103-135.

Macey, Jonathan R. (1994), Judicial Preferences, Public Choice, and the Rules of Procedure, *Journal of Legal Studies* 23(1): 627-646.

McCannon, Bryan C. (2013), Prosecutor Elections, Mistakes, and Appeals, *Journal of Empirical Legal Studies* 10(4): 697-715.

Miceli, Thomas J. and Metin M. Cosgel (1994), Reputation and Judicial Decision Making, *Journal of Economic Behavior and Organization* 23(1): 31-51.

Nordhaus, William D. (1975), The Political Business Cycle, *Review of Economic Studies* 42(2): 169-190.

Posner, Richard A. (1993), Whe Do Judges and Justices Maximize? (The Same Thing Everybody Else Does), *Supreme Court Economic Review* 3(1): 1-41.

Posner, Richard A. (2008), *How Judges Think*, Harvard University Press.

Rasmusen, Eric, Manu Raghav, and Mark Ramseyer (2009), Convictions versus Conviction Rates: The Prosecutor's Choice, *American Law and Economics Review* 11(1): 47-78.

Salzberger, Eli and Paul Fenn (1999), Judicial Independence: Some Evidence from the English Court of Appeal, *Journal of Law and Economics* 42(2): 831-847.

Sample, James, Lauren Jones, and Rachel Weiss (2006), *Politics of Judicial Elections 2006*, Justice at Stake Campaign, Washington D.C.

Shapiro, Carl and Joseph E. Stiglitz (1984), Equilibrium Unemployment as a Worker Discipline Device, *American Economic Review* 74(3): 433-444.

Shepherd, Joanna M. (2009), The Influence of Retention Politics on Judges Voting, *Journal of Legal Studies* 38(1): 169-206.

Tabarrok, Alexander and Eric Helland (1999), Court Politics: The Political Economy of Tort Awards, *Journal of Law and Economics* 42(1): 157-188.

Tullock, Gordon (1971), Public Decisions as Public Goods, *Journal of Political Economy* 79(4): 913-915.

Vidal, Jordi Blanes i and Clare Leaver (2011), Are Tenured Judges Insulated from Political Pressures, *Journal of Public Economics* 95(7-8): 570-586.

7 Referee Appendix

Table 1: Summary Statistics

	Mean	Std. Dev.	Observations
Panel A: Judges			
Total Appeals	35.593	48.701	518
Upheld	0.800	0.233	518
Affirmed	0.904	0.179	518
Dismissed	0.104	0.208	518
Modified	0.093	0.163	518
Reversed	0.100	0.183	518
Re-elect	1.016	3.172	518
Judge Experience	8.618	7.127	518
County Court	0.407	0.461	518
Supreme Court	0.593	0.461	518
Panel B: Opinions			
Appeal Length (words)	3052.612	3298.833	20644
Days between Appeal and Decision	890.196	520.748	20644
Upheld	0.834	0.372	20644
Affirmed	0.923	0.266	20644
Dismissed	0.071	0.257	20644
Modified	0.080	0.271	20644
Reversed	0.084	0.277	20644
Jury Trial	0.241	0.428	20644
Nonjury Trial	0.030	0.170	20644
Guilty Plea	0.307	0.461	20644

Table 2: Summary Statistics by Judge

	Pre-Wage	Post-Wage	Both
Total Appealed Cases	111.726	22.884	46.689
	(72.665)	(24.605)	(32.725)
Judge Experience	10.576	3.684	12.547
	(6.737)	(4.979)	(9.555)
Upheld	0.831	0.811	0.830
	(0.340)	(0.368)	(0.348)
Affirmed	0.923	0.916	0.924
	(0.266)	(0.278)	(0.265)
Dismissed	0.070	0.065	0.079
	(0.255)	(0.246)	(0.269)
Modified	0.079	0.100	0.082
	(0.270)	(0.300)	(0.275)
Reversed	0.085	0.071	0.079
	(0.278)	(0.257)	(0.270)

Table 3: Effect of Judicial Wage Increases on Judicial Performance

	Upheld (OLS)	Upheld (Probit)	Upheld (OLS)	Upheld (Probit)
Post-Wage	0.058**	0.234**	0.163***	0.826***
	(0.028)	(0.117)	(0.047)	(0.282)
Both	0.014*	0.063*	0.014*	0.070*
	(0.008)	(0.036)	(0.008)	(0.041)
Both x Post-Period	0.028***	0.146**	0.140**	0.720**
	(0.009)	(0.067)	(0.057)	(0.332)
County FEs	✓	✓	✓	✓
Month FEs	✓	✓	-	-
Year FEs	✓	✓	-	-
Month x Year FEs	-	-	✓	✓
Crime Controls	✓	✓	✓	✓
Grounds for Appeal Controls	✓	✓	✓	✓
Other controls	✓	✓	✓	✓
N	19471	19471	19471	19471
R^2	0.165	-	0.211	-
AIC	12839.88	14308.71	4,106.9	4,104.6

Robust standard errors clustered at the county level in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Controls include whether the justice was previously a prosecutor or public defender, SCAJ, SC, SCAJSC, the school that the justice attended, whether the case was heard by a jury, bench trial or plea deal, whether the justice was female, the number of days between original judgment and appeal, as well as fixed effects for the appeals justices hearing the case. In columns 1 and 2 we also include a trend for the number of appealed cases in each month in our sample.

Table 4: Does Entry or Effort in Response to Wage Increases Improve Judicial Performance?

	Upheld (OLS)	Upheld (Probit)
Post-Wage	0.162***	0.816***
	(0.049)	(0.292)
Both × Entry Pre-2009	0.014*	0.070*
	(0.008)	(0.041)
Both × Entry Post-2009	0.019	0.076
	(0.021)	(0.107)
Both× Entry Pre-2009 × Post-Period	0.141**	0.718**
	(0.056)	(0.327)
Both × Entry Post-2009 × Post-Period	0.108*	0.578*
	(0.058)	(0.351)
County FEs	✓	✓
Month x Year FEs	✓	✓
Crime Controls	✓	✓
Grounds for Appeal Controls	✓	✓
Other controls	✓	✓
N	19471	19471
R^2	0.211	-
AIC	4,106.9	4,104.6

Robust standard errors clustered at the county level in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Controls include whether the justice was previously a prosecutor or public defender, SCAJ, SC, SCAJSC, the school that the justice attended, whether the case was heard by a jury, bench trial or plea deal, whether the judge was female, the number of days between original judgment and appeal, as well as fixed effects for the appeals justices hearing the case.

Table 5: Permutation Effects of Re-election on Upheld Appeals

	(1) Coefficient Estimates	(2) 1st Percentile	(3) 99th Percentile
Table 3			
Both	0.014	0.010	0.020
Both x Post-Period	0.140^*	-0.015	0.011

Table 5 reports the permutation results from 1,000 iterations of random assignment of wage change dates. Columns 2 and 3 report the 1% and 99% coefficient estimates from the regressions with the permuted wage changes. Asterisks (*) indicate that the coefficient estimates from Table 3 lie outside the bounds of columns 2 and 3.

Table 6: Effect of Falsified Judicial Wage Increases on Judicial Performance

	Upheld (OLS)	Upheld (Probit)
Post-Wage	0.044	0.322
	(0.104)	(0.393)
Both	0.071	0.442
	(0.104)	(0.383)
Both x Post-Period	0.003	0.037
	(0.097)	(0.471)
County FEs	✓	✓
Month x Year FEs	✓	✓
Crime Controls	✓	✓
Grounds for Appeal Controls	✓	✓
Other controls	✓	✓
N	12434	12434
R^2	0.209	-

Robust standard errors clustered at the county level in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Controls include whether the justice was previously a prosecutor or public defender, SCAJ, SC, SCAJSC, the school that the justice attended, whether the case was heard by a jury, bench trial or plea deal, whether the justice was female, the number of days between original judgment and appeal, as well as fixed effects for the appeals justices hearing the case.

Table 7: Information Extracted from the Slip Opinions

	Mean	Std. Dev.	Min	Max	Observations
Grounds for Appeal					
Coercion	0.022	0.148	0	1	20,644
Severity	0.142	0.349	0	1	20,644
Jury Instruction	0.047	0.212	0	1	20,644
Re-sentencing	0.113	0.316	0	1	20,644
Juror Misconduct	0.058	0.234	0	1	20,644
Mental	0.089	0.284	0	1	20,644
Double Jeopardy	0.018	0.131	0	1	20,644
Speedy Trial	0.014	0.119	0	1	20,644
Evidence Suppression	0.149	0.357	0	1	20,644
Sufficiency of Evidence	0.279	0.449	0	1	20,644
Incapacitated	0.004	0.063	0	1	20,644
Youth	0.022	0.145	0	1	20,644
Crimes					
Arson	0.002	0.047	0	1	20644
Assault	0.058	0.234	0	1	20644
Bail	0.001	0.031	0	1	20644
Burglary	0.050	0.219	0	1	20644
Child	0.014	0.119	0	1	20644
Conspiracy	0.006	0.075	0	1	20644
Contempt	0.009	0.093	0	1	20644
Contraband	0.002	0.045	0	1	20644
Fraud	0.002	0.047	0	1	20644
Forgery	0.006	0.079	0	1	20644
Harassment	0.001	0.038	0	1	20644
Homicide	0.001	0.031	0	1	20644
Indecent	0.001	0.022	0	1	20644
Intoxication	0.009	0.093	0	1	20644
Kidnapping	0.002	0.049	0	1	20644
Larceny	0.025	0.156	0	1	20644
Manslaughter	0.017	0.128	0	1	20644
Marijuana	0.006	0.077	0	1	20644
Menacing	0.004	0.065	0	1	20644
Murder	0.041	0.197	0	1	20644
Property	0.016	0.125	0	1	20644
Rape	0.016	0.127	0	1	20644
Reckless Endangerment	0.009	0.094	0	1	20644
Robbery	0.068	0.251	0	1	20644
Sale or Possession of Illegal Goods/Services	0.025	0.157	0	1	20644
Sex	0.081	0.273	0	1	20644
Sodomy	0.003	0.052	0	1	20644
Stalking	0.001	0.023	0	1	20644
Unlicensed	0.005	0.069	0	1	20644
Vehicle Related	0.013	0.111	0	1	20644
Weapon	0.072	0.258	0	1	20644

Table 8: Effect of Real Wage Increases on Percent of Cases Upheld

	% Upheld (OLS)	% Upheld (Probit)	% Upheld (OLS)	% Upheld (Probit)
Post-Wage	0.055**	0.001	0.065***	0.004
	(0.028)	(0.014)	(0.013)	(0.060)
Both	0.055*	0.010	0.065***	0.016
	(0.029)	(0.014)	(0.013)	(0.058)
Both x Post-Period	0.056**	0.002	0.066***	-0.005
	(0.029)	(0.017)	(0.014)	(0.064)
County FEs	✓	✓	✓	✓
Month FEs	✓	✓	-	-
Year FEs	✓	✓	-	-
Month x Year FEs	-	-	✓	✓
Crime Controls	✓	✓	✓	✓
Grounds for Appeal Controls	✓	✓	✓	✓
Other controls	✓	✓	✓	✓
N	19471	19471	19471	19471
R^2	0.139	-	0.155	-
AIC	6,373.90	9,430.11	6,024.61	9,192.51

Robust standard errors clustered at the county level in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Controls include whether the justice was previously a prosecutor or public defender, SCAJ, SC, SCAJSC, the school that the justice attended, whether the case was heard by a jury, bench trial or plea deal, whether the justice was female, the number of days between original judgment and appeal, as well as fixed effects for the appeals justices hearing the case. In columns 1 and 2 we also include a trend for the number of appealed cases in each month in our sample.
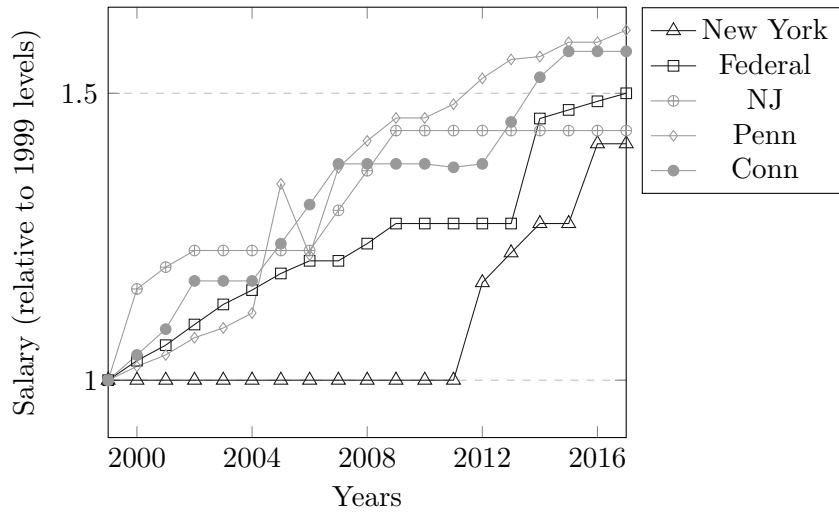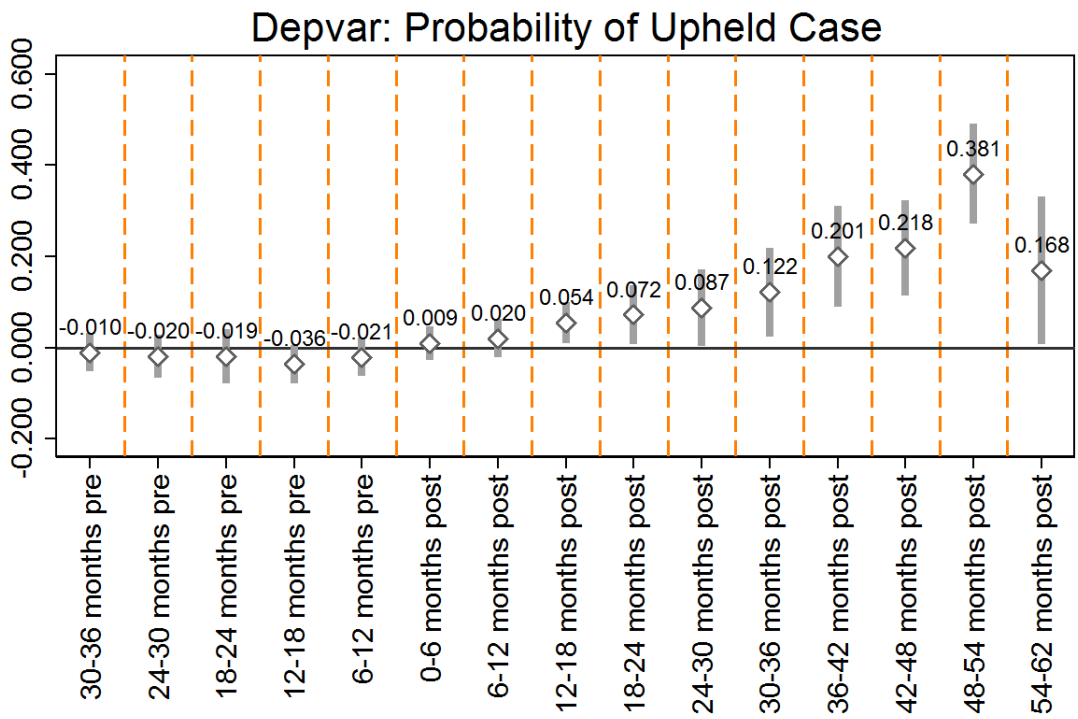
Figure 1: Judge Salaries Across Time

Figure 2: Likelihood of a Case being Upheld

Depvar: Probability of Upheld Case

Model includes month city and year fixed effects