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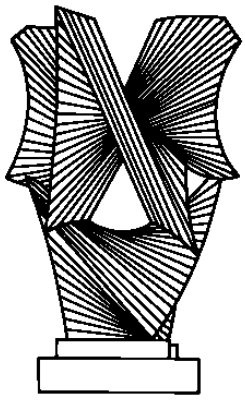

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ARE JUDGES OVERPAID?

A SKEPTICAL RESPONSE TO THE JUDICIAL SALARY DEBATE

Stephen J. Choi, G. Mitu Gulati & Eric A. Posner[1]

ABSTRACT

The public debate over the need to raise judicial salaries has been one-sided. Sentiment appears to be that judges are underpaid. But neither theory nor evidence provides much support for this view. The primary argument being made in favor of a pay increase is that it will raise the quality of judging. Theory suggests that increasing judicial salaries will improve judicial performance only if judges can be sanctioned for performing inadequately or if the appointments process reliably screens out low-ability candidates. However, federal judges and many state judges cannot be sanctioned, and the reliability of screening processes is open to question. An empirical study of the high court judges of the fifty states provides little evidence that raising salaries would improve judicial performance.

1. THE SALARY DEBATE

Chief Justice John Roberts says that the pay increases that his colleagues have received over the past two decades are so inadequate as to create a "constitutional crisis" (Year-End Report 2007). Justice Kennedy says that "[because of inadequate judicial pay] the nation is in danger of having a judiciary that is no longer considered one of the leading judiciaries of the 1

1 Faculty at NYU, Duke, and Chicago law schools respectively. Thanks to Jonathan Baker, Scott Baker, Prea Gulati, Kimberly Krawiec, Jayanth Krishnan, David Levi, Raleigh Levine, Richard McAdams, Ann McGinley, Richard Posner, Jeff Rachlinski, Greg Shafer, David Snyder, Jeff Stempel, Joshua Teitelbaum, Un Kyung Park, and participants at seminars and workshops at the University of Chicago, Loyola University of Chicago, Cornell University, Duke University, Ohio State University, the University of Nevada-Las Vegas, the University of North Carolina, American University, and the William Mitchell Law School for comments. Thanks also to the judges who gave us good-natured albeit highly critical feedback. For research assistance, thanks to Jai Damle, John Niles, Craig Porges, Nathan Richardson, Lauren Tribble, Reed Watson, Jennifer Wimsatt, and Matthew Wolfe. Finally, thanks to Mark Ramseyer and an anonymous referee for their review and comments.

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world" (Hearing 2007a). Justice Alito warns that "eroding judicial salaries will lead, sooner or later, to less capable judges and ultimately to inferior adjudication" (Hearing 2007b). Justice Breyer "believe[s] that something has seriously gone wrong with the judicial compensation system" (Hearing 2007a). Lower court judges, lawyers, public intellectuals, and law professors agree (Parker 2007). One hundred and thirty law school deans signed a letter urging judicial pay increases (Letter 2007). The American College of Trial Lawyers endorses a salary increase (American College of Trial Lawyers 2007). So does Paul Volker, the former Federal Reserve Board Chairman (Volker 2007). The complaints are not new—Chief Justice Rehnquist made them as did his predecessor, Chief Justice Burger (Frank 2003; Holt 2006). And they are not limited to federal judges. State judges and their supporters have been complaining equally loudly about inadequate pay (Schotland 2007; Sherwin 2007; Wise 2008). Judith Kaye, the Chief Judge of the highest court in New York, went so far as to file suit against the legislature and the governor, asserting that the legislature's and governor's refusal to provide adequate salary increases for judges was undermining judicial independence and effectiveness (Stashenko & Wise 2008).

2 Are these complaints plausible? How does one tell whether someone—a judge, or someone else—is underpaid? The judges cite data showing that they are paid less than (some) foreign judges and (some) practicing lawyers in the United States and some law professors, but why are these people the relevant comparison? What if these people are overpaid? To evaluate the argument that judges are underpaid, one needs a theory of wage compensation and empirical evidence. However, so far neither theory nor evidence has played a large role in the public debates. The purpose of this article is to generate a debate over the proper theoretical and empirical foundations for determining the optimal level of judicial compensation. Setting out a simple theoretical model of judicial behavior, we demonstrate that the case for increasing the salaries of federal judges is weaker than recognized. However, the case may be stronger for state judges subject to a meaningful risk of job loss.

3 We make several points. First, judicial pay cannot be evaluated in isolation. Pay is but one aspect of judicial compensation—which includes status, tenure, pensions, and the satisfaction derived from doing justice, affecting policy, and wielding power—and working conditions in general, which

for judges, are usually pleasant. Judges receive assistance from secretaries and clerks, and have control over their schedules and other aspects of their working conditions. These elements must be considered together when determining optimal judicial compensation.

Second, judicial pay should advance the interests of the public. Whereas the existing debate focuses on comparisons between the salaries of judges and other legal professionals such as lawyers and law professors, the relevant question is not whether these salary differences are unfair. Compensation should be designed so as to give judges incentives to perform their office diligently in the public interest and to attract qualified people to judgeships. When raising salary does not change, or worsens, incentives, it is inadvisable; when it attracts people to judgeships who are more productive in the private sector or improves the patronage opportunities of elected officials, it is also inadvisable. 4

Third, whether salary increases improve the performance of judges, and the quality of the people who become judges, is an empirical question. Few empirical studies shed light on these questions. The relevant studies look exclusively at federal judges who are paid the same, and operate under nearly identical conditions, making it difficult to evaluate the impact of differing conditions on performance.[2] A literature on public sector employees exists, but provides few clues (Burgess & Ratto 2003). The literature on the effects of pay increases on public sector performance focuses on the effects of performance incentives (for example, basing teacher salaries on student exam scores) (Burgess & Ratto 2003). No one, however, advocates making a judge's wages turn on the quality or quantity of her judicial output, which 5

2 On point is Baker (2008). Using three different data sets of federal circuit court judges, Baker estimates whether effort, quality, and independence are a function of salary (he finds that they are not). Because the salaries of federal judges at the same level are virtually identical and because Baker had only federal judges in his data set, he had to innovate to create a salary measure with variation. Opportunity costs in the model are the average partner salary within the state in which the court was located. A problem is that federal circuit court judges are an elite group often with a national stature—suggesting that the loss of salary is from a local law firm might not be a good measure of opportunity costs. Baker, however, looks at whether judicial candidates tend to have lived in that particular state for at least ten years prior to becoming judges and finds that most (255 out of 260) have, suggesting that his opportunity cost measure might be valid. It is possible, however, that problems with his opportunity cost measure might be driving the non-results.

in any event would be constitutionally dubious (at least for federal judges) (Pfander 2008).

6 The key question is whether in settings where there is little room for incentive compensation based on judicial outputs, pay can nonetheless motivate better performance. The theoretical case that increased pay will improve judicial performance is uncertain. As we explain in Part 2, because of selection and incentive effects, increasing pay might even reduce the quality of performance.

7 To address the deficiencies in the empirical literature on judicial incentives, we conduct an empirical test of the relationship between judicial performance and pay. For our study, we construct a unique dataset of the decisions of judges of the high courts for every state for the time period from 1998 to 2000. We examine how higher judicial salary and other variables affecting the work environment of judges are related to various measures of judicial performance including productivity, opinion quality, and independence.

8 Part 2 sets out a model of judicial behavior that relates the effects of a salary increase to the behavior of sitting judges and the decisions of prospective judges to seek a judgeship or remain in their current non-judicial position. The model casts doubt on the claim that raising the salaries of judges will improve judicial behavior and the quality of judges when judges are protected by life tenure, suggesting instead that salary increases can improve judicial performance only when judges face a serious risk of termination or other punishment if they perform inadequately. Part 3 describes our empirical inquiry; Part 4 describes the data; Part 5 reports results.

9 The empirical results tell a complicated story. Judicial productivity—opinion-writing—is consistent with our model. Salary does not increase productivity on average. And while judges who face a higher risk of termination (failure to be reelected or reappointed) are more productive than those who do not, we find judges with more secure positions write higher-quality opinions, as measured by out-of-state citations. Finally, we find no relationship between salary and judicial independence, whether or not judges face a risk of termination. We conclude with a discussion of the implications of our findings for the debate about salary increases for federal judges.

10 An earlier draft of this paper prompted a negative response from judges and others in the public debate (*e.g.*, Marek 2008). Although we acknowl-

edge limitations with our methodology, we hope that, at a minimum, we provoke pay raise advocates to explain their theoretical assumptions and provide better empirical support for their arguments that the current levels of judicial pay are inadequate.

2. A MODEL OF JUDICIAL BEHAVIOR

An employer, faced with a request for salary increases, will ask whether the salary increase will result in improved performance by the employee, such that the productivity gains exceed the extra cost. If the answer is yes, the employer will ask whether a salary increase is the best method among available options of improving performance. The performance improvement could occur in one of two ways. The increased salary could give existing workers stronger incentives to exert higher levels of effort (the "incentive effect"). And it could improve the quality of the pool of workers who apply for the job (the "selection effect"). In the context of the judiciary, both the incentive and selection effects depend on how judges, current and prospective, react to the higher salary. 11

The standard economic model of worker behavior has workers maximizing utility subject to constraints. Since individual utility functions vary widely, an assumption often made is that workers maximize wealth (or, simplifying within a job setting, wage income). The judicial utility function, however, is different (Posner 2003; Smyth 2004). Judges are not maximizing wage income because most, if not all, judges could earn significantly higher wages in the private sector. A feature of the judge utility function, therefore, is that this person cares about achieving other goals as well as making money. 12

What might those other goals be? The job of a judge provides a range of nonpecuniary benefits that few other jobs do. Judges have power — the ability to decide the fate of others and to affect public policy. Judges also enjoy a high status in society, interesting work, a large degree of autonomy, and control over their schedules. Various strands of the literature on judicial behavior posit judges as being more or less motivated by combinations of the above factors. For our purposes, it suffices to assume that there is a variety of types of individuals who might find the job of judging attrac- 13

tive, including ideologues with political agendas, leisure or status seekers, workaholics, and "good judges"—people who derive utility from serving justice. In all cases, nonpecuniary benefits, as well as monetary reward, play a role in their decision to become judges (Posner 2008; Stras 2006).[3]

2.1. Incentive Effects

14 A pay increase will improve a worker's incentive to exert effort only if the worker can be punished for failing to exert effort. In the context of judging, the only realistic type of punishment is termination, but not all judges can be terminated. Hence we divide our discussion into two parts: the case of judges with life employment and the case of judges who have fixed terms at the end of which they must undergo reelection or reappointment if they wish to retain their position.

2.1.1. Life Employment

15 Life employment means a near zero risk of termination. Termination is theoretically possible, but will not occur unless there is extreme misbehavior in the form of corruption or an utter failure to perform the job. Because the job of judging provides many nonpecuniary benefits, the types of individuals who will be attracted to the job will be a combination of those who love the job, want to serve the public, want to impose their policy preferences on others, want status, or seek leisure. For our purposes the question is whether any of these types of individuals might be induced to work harder by a salary increase. The answer is probably not.

16 Consider four types of judges for purposes of illustration: the workaholic, the good judge, the status seeker, and the leisure seeker. For all four types, higher salary is unlikely to improve incentives to work hard. The workaholic works because she finds the job intellectually interesting. Maybe she could work more efficiently with a bigger computer screen or better library facilities. But, standing alone, a higher salary is not going to make her find the job more interesting. Next, take the good judge. She works because she wants to

3 In informal conversations with judges, an additional nonpecuniary benefit from judging that came up was client avoidance. Apparently, some judicial candidates see the judiciary as a route to escape having to listen to instructions from clients, especially the demanding clients one finds in private practice.

serve the public and gains utility from doing a good job. She is already doing what she thinks serves the public best — so, paying her more is not going to make her work harder or better.[4] A similar story applies to the status-seeking judge. She desires the job because judges have high status in society. A higher salary does not necessarily produce more status.[5] But even if it does, unless the high salary can be taken away, the status seeking judge enjoys the higher status that comes with the greater salary even without changing her performance level. (To be sure, if a status seeker obtains status from writing good opinions, then her incentives will be largely the same as those of the good judge.) Finally, take the leisure seeker. He has taken the job because it gives him time for leisure. Give the leisure seeker more money and his ability to consume leisure increases. That vacation in Paris becomes a possibility.[6] Essentially, with life employment, the effect of a salary increase is the same as a gift. Because the salary increase is not conditional on exerting greater effort, the rational judge—regardless of what she enjoys about the job—has no incentive to exert greater effort.[7]

2.1.2. Risk of Termination for Inadequate Performance

As noted above, increasing the salary of a judge can provide an incentive to increase effort as long as an important condition is met: the judge can lose 17

4 In theory, the use of higher salaries to induce effort might even worsen matters, by producing a "crowding out" effect, where the use of monetary rewards works at cross purposes with non-monetary or intrinsic motivations (Grepperud & Pedersen 2001; Frey 1997; but *cf.* Eisenberger & Cameron 1996).

5 Part of the status inherent in a judgeship could be the priesthood characteristic of the job (low pay, public service, restrictions on contact with the public, etc.). If so, raising the judicial salaries to a level where candidates are making only a minimal financial sacrifice in taking the job might reduce the job's status value.

6 In economics, this is described as the backward bending portion of the labor supply curve (Nicholson 1998, 676-677).

7 It is possible to construct a story of how an increase in salary could motivate judges to exert more effort. But the story does not stand on a strong foundation. For example, judges might view themselves in a reciprocal gift-giving relationship with the state, where the state (or society) provides the employee with gifts an in exchange the employee gives back the gift of high effort (Akerlof 1983). As best we know, no one has suggested that judges see themselves in this kind of reciprocal gift giving relationship with the state. At bottom, in the life employment context, there seems little reason to expect that higher salaries will result in increased production.

her job. The more the judge values the compensation package—salary as well as the various nonpecuniary benefits—the harder the judge will work in order to avoid losing this scarce job. This is the theory of "efficiency wages" (Akerlof & Yellen 1986; Shapiro & Stiglitz 1984).

18 The fly in the ointment is that there has to be an effective monitoring mechanism for the "efficiency wage" dynamic to work. If an employer increases the salary of an assembly line worker in order to induce greater effort, the employer must also be able to fire the worker if she does not increase her effort. Otherwise, the worker will pocket the money and continue at the same level of effort. Typically, because effort cannot be directly measured, employers use proxies—such as number of pieces assembled. If the worker falls below her quota, she is fired or penalized in some other way. If, however, the proxy is not sufficiently correlated with effort, this strategy will not work. Suppose that the worker is supposed to train new workers, but is paid entirely on the basis of number of pieces assembled. She will shirk on training in order to maximize pay, possibly making the employer worse off. This is the "multitasking" problem (Holmstrom & Milgrom 1991).

19 Federal judges cannot be fired for failing to undertake sufficient effort, but many state judges can, at least in principle. Whether they can in practice depends on whether the state can solve the multitasking problem. In the case of elected judges, the "employer" is the electorate. The electorate is a dispersed body with little ability or incentive to engage in detailed monitoring of its agents. So, the increased effort will likely be exerted in the directions that the electorate is able to observe and not as much in directions that are less observable. Judges will perform on aspects of the job that the electorate cares about and can observe and shirk on other aspects of the job.

20 In states where the employer's power has been in effect delegated to agents — for example, the governor, the legislature, or the local bar association — the dynamic will play out differently. The employee/judge still has an incentive to exert high effort because she has a job that she does not want to lose, especially if she receives a salary increase. But in this version of the model, the judge's incentives to perform will focus on pleasing the agent, the immediately relevant decision-maker. The agent likely will have a greater ability to monitor the judge than the dispersed populace. But the agent might have a personal agenda. A higher salary will induce more effort, but

in the direction of the agent's preferences. And if the populace is unable to monitor the agent well, judgeships can end up becoming favors that politicians or bar associations grant in exchange for favors done for them by the judges. Whether higher salaries enhance judicial performance thus depends on the quality of the judicial retention system. Rather than improving the quality of the justice system, increasing salaries might encourage judges to decide cases in a manner that advances the interests of elected officials or leaders of the bar.

2.2. Selection Effects

The selection argument is the primary one that the judges themselves make in favor of salary increases. Here, the relevant issue is not life employment versus termination but the process for selecting or appointing judges, and we organize our discussion accordingly. 21

2.2.1. When the Appointments Process Is Crude

If salaries are increased, more people will be attracted to the job. The question here is whether the salary increase is attracting the right type of candidate. The employer wants primarily the good judges and maybe also the workaholics — those who work hard because they enjoy the job or are motivated by public service. It would like to avoid the leisure seekers, the status seekers, and those who seek to bring policy into line with their political preferences. 22

Higher salaries will attract workaholics and good judges. They like more money and will be more likely to give up lucrative jobs elsewhere if the wages of being a judge are higher. The problem is that the leisure and status seekers like higher salaries, too. Increasing salary, then, increases the attractiveness of judgeships both to people who would be good judges and to people who would be bad judges. Indeed, the argument might be made that a low salary will attract more workaholics and fewer leisure and status seekers than a higher salary because leisure seekers need money to be able to purchase leisure, and status seekers will get less status from a low-paid job. Those who are committed to advancing the public good, or who enjoy working for its own sake, by contrast, may still become judges despite a low salary (Greenberg & Haley 1986). If the appointments process is crude, salaries should be low rather than high. 23

2.2.2. When the Appointments Process Is Sophisticated

24 Suppose that those who appoint judges—the electorate, elected officials, or appointed officials, depending on the system—can easily screen prospective judges, and reject the various bad types—leisure seekers, status seekers, and ideologues with policy goals. If so, increasing the salary will straightforwardly improve the pool of those willing to serve as judges, and the quality of those who are selected. In addition, existing judges will have stronger incentives to work hard, as they know that they can be easily replaced with possibly superior judges if they shirk.

25 There are two difficulties with this argument. First, it is unclear whether those who select judges have the proper incentives to screen out the bad judges. Electorates might have trouble evaluating prospective judges; elected officials and others with the power of appointment might prefer appointing cronies. If so, raising salaries will not necessarily improve the quality of the people who are appointed to judgeships.

26 Second, increasing the pool is costly and creates diminishing returns. Suppose that a $10,000 raise will draw one hundred more people into the pool. As a result, the relevant agents might (or might not) select a better judicial candidate. The price of this additional person is $10,000 times the number of existing judges—since everyone receives the raise. The person might be worth it, but the larger point is that increasing the pool is not costless, and one needs to take into account the costs as well as the benefits of producing a larger pool.

27 In any event, it is unclear whether existing screening technologies are sophisticated. Leisure and status seekers have an incentive to mimic workaholics and people who obtain utility by judging well. If they successfully mimic the good types, then they will obtain desirable judgeships. Of course, it may be difficult to engage in such mimicking, especially over the long term. And so good-faith screeners will take a careful look at the prospective judge's record as a practicing lawyer or academic. It seems likely that more successful lawyers will also be more successful judges. But the process will be imperfect.

28 In sum, salary increases will improve the quality of judges only if those who select judges themselves have good incentives to screen out bad judges, and if they have an effective mechanism for distinguishing good applicants and bad applicants. Because states and the federal government have diverse

systems for judicial selection, one might predict that salary increases improve performance in some jurisdictions—those with effective selection systems—and not others.

2.3. Summary

Judges should be paid enough that their total compensation—nonpecuniary as well as pecuniary—equals their social value. Otherwise, lawyers who would benefit society by being judges will prefer to work in the private sector. Because we do not know how much judges value their nonpecuniary compensation, and because we do not know their social value, it is hard to say whether judges are currently correctly paid, overpaid, or underpaid. 29

Beyond this difficulty, we have observed that raising the salaries of sitting judges provides them with no incentive to increase effort or otherwise improve their performance unless judges can be fired or otherwise sanctioned for inadequate performance. In the federal system, judges cannot be sanctioned for inadequate performance. In many state judiciaries, judges who perform inadequately may not be reelected or reappointed. In these systems, increasing salaries therefore might improve performance, but only if those who reelect and reappoint can properly evaluate judicial outputs. 30

In addition, raising the salaries of judges might improve the pool of people willing to be judges. But whether that translates into better judicial performance depends on the quality of the appointments process. With a broken appointments process used by politicians to further their private agendas, the benefits of a salary increase will primarily accrue to the politicians. 31

2.4. Are Judges Overpaid?

Why pay judges more? We should pay judges more only if the incremental increase in pay will improve the social value of judicial performance more than the social cost of the higher pay. Given our model of judicial incentives and pay, it is questionable that more pay, by itself, will improve judicial output, though it is possible. 32

Consider the existing evidence. First, many federal judges stay on the bench rather than retire on a full salary. A recent study reports that since 1984, 80 percent of the federal judges eligible for retirement at full pay 33

chose instead to take senior status and keep working (Yoon 2005).[8] If judges were underpaid, they would retire and become highly paid practicing lawyers, while collecting a pension, often equal to their salary. Instead, they, in effect, work for free (Posner 2008; Yoon 2005). Some judges even decline senior status and, as a result, give up the ability to do less work for identical pay.[9] Further, many people who become judges give up millions of dollars of compensation as a law firm partner. If people give up huge sums to become judges, and judges continue to work when they could stop working without a pay cut, the inescapable inference is that the nonpecuniary benefits of being a judge—including status and the ability to exercise power over the lives of others—substitute for cash compensation, and lots of it.[10]

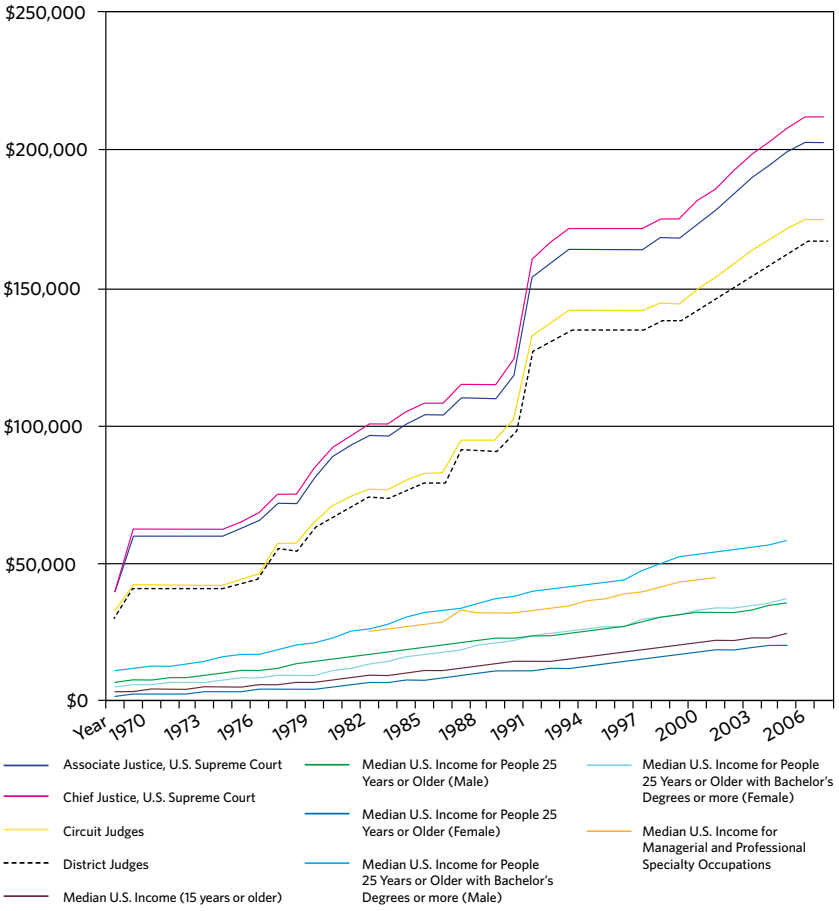
34 The Administrative Office of the U.S. Courts has pointed out that real pay of federal judges had declined since 1969 because raises since then have not kept up with inflation, while the real pay of the average worker has increased during the same time period (Administrative Office Report 2008). But 1969 was the year of an approximately 30 percent raise for federal

8 In two related articles, Yoon looks at whether the gradual reduction in judicial salaries since 1945 has corresponded to higher turnover among judges and whether reducing the age at which judges could take senior status increased the rate at which judges did take this status (Yoon 2003; 2006). The answer to the first question was no — turnover did not increase as a function of the gradually decreasing judicial salaries. And to the second was a yes — judges did take senior status earlier when the age limit was dropped. Yoon's second article suggests the judges respond to monetary incentives. But the first article also tells us that those monetary incentives are not enough to induce them to give up their jobs; only to reduce workload. Yoon's studies, while telling us that judges value the non-pecuniary aspects of the jobs enough that lower salaries don't induce them to quit, are not able to tell us about productivity. Other articles examining the factors that influence judicial retirement include (Hall 2001; Zorn & Van Winkle 2000; Spriggs & Wahlbeck 1995; Barrow & Zuk 1990). A recent study, that looks at the small number of judges who resigned prior to eligibility for retirement, suggests that salary levels impacted those resignation choices. The study does not, however, analyze whether the judges who resign are more or less productive than those who do not (Kominers 2008).

9 Yoon finds that even when federal judges are eligible for retirement or taking senior status, only 35 percent of those eligible take that option immediately; it takes up to five years for the vast majority of these judges to take even the reduced work option (Yoon 2006).

10 An objection we have heard in response to the observation that the supposed low salaries do not appear to be causing judges to flee the judiciary in droves (especially in terms of taking the retirement option) is that the way the pension benefits work for retirees is that retirees get their full salary after retirement, but not any of the subsequent salary increases. And many current judges, expecting a major salary increase to arrive any day now, are delaying their retirement until then.

Figure 1: Comparison of Judicial Salaries and Other Median Incomes 1968–2007



judges, and it is not clear why that year should be chosen as the baseline. The Administrative Office also notes that judicial pay has increased less quickly than that of some senior government officials, law school deans, and senior law professors. But it does not explain why these positions are the relevant comparisons. Figure 1 provides another perspective. It compares nominal judicial salaries for federal judges with the median income of various categories of workers. Federal judicial salaries have increased less quickly than these median incomes, but overall the changes have not been great. In 1968, a circuit judge was paid 8.8 times the median income; in 1969, as a result of

the judicial pay raise, that ratio increased to 12.3. Over the next thirty-five years, that ratio gradually declined, with some fluctuations, but as of 2005, the ratio was still 7.1. Compared to managerial and professional workers, for whom we have less data, judges have done better. From 1982 to 2001, the ratio increased from 3.0 to 3.4, with a high of 4.1 in 1993.

35 Second, anecdotal evidence for state judgeships teaches a similar lesson. On the retirement front, many state judges have lobbied—some have sued—to eliminate mandatory retirement ages.[11] Candidates for elected state judge positions expend effort to obtain these judgeships, taking time away from their practices. News accounts suggest that candidates for elected judicial positions have been spending millions of dollars of campaign funding to win these seats (*e.g.*, Cann 2007a; Michels 2007). Why would people devote energy to become (and stay) state judges if they are going to be underpaid? Either they think the pay is generous or they seek the nonpecuniary benefits of the positions.

36 Third, the evidence of judicial compensation in foreign countries is more ambiguous than has been recognized (Table 1). While Justice Breyer correctly notes that British and Australian judges earn more than American judges, in most other developed countries judges are not paid nearly as much.[12] A comparison of salaries for judges from twenty-eight member countries of the Organisation for Economic Co-operation and Development shows that the United States is fifth highest in terms of unadjusted

11 In *Equal Employment Opportunity Comm'n v. State of Vermont,* 904 F.2d 794 (2nd Cir. 1990), Justice Louis Peck of the Vermont Supreme Court successfully challenged Vermont's constitutional requirement that judges retire at age 70. The next year, however, in *Gregory v. Ashcroft,* 501 U.S. 452, 111 S. Ct. 2395 (1991), the Supreme Court rejected a similar challenge to the mandatory retirement provision of the Missouri Constitution, overruling Peck's victory. Following *Gregory v. Ashcroft,* such challenges have been legislative. In 2006, Hawaii voters rejected a bid to repeal that state's mandatory retirement age of seventy (Reyes 2008). And in November 2007 Texas voters decided on Proposition 14 which repealed the Texas Constitution's mandatory retirement for mid-term state judges over the age of seventy-five (Robbins 2007).

12 Justice Breyer invoked the comparative by observing that American federal judges "receive only 2/3 of the salaries of their judicial counterparts in Australia and 1/2 of their judicial counterparts in England" (Hearing 2007a).

Table 1: Judicial Salaries of National High Courts, 2004/2005[13]

Rank	Country	Judicial salary (in U.S. dollars)	COLA Adjusted salary
1	United Kingdom	$331,738	$299,943
2	Ireland	$248,678	$270,891
3	Australia	$241,498	$264,510
4	New Zealand	$211,900	NA
5	**United States**	**$203,000**	**$203,000**
6	France	$198,201	$212,891
7	Japan	$172,346	$144,707
8	Canada	$166,800	$166,800
9	Iceland	$156,250	NA
10	Luxembourg	$141,606	$174,392
11	Netherlands	$137,500	$164,868
12	Spain	$135,686	$166,282
13	Finland	$131,250	$149,487
14	Austria	$125,225	$139,449
15	Belgium	$117,073	$147,261
16	Denmark	$115,568	$114,311
17	Sweden	$110,520	$130,330
18	Germany	$108,098	$136,487
19	Portugal	$96,979	$122,448
20	Norway	$94,000	$94,000
21	Greece	$70,500	$86,930
22	Italy	$58,245	$64,861
23	Poland	$46,521	NA
24	Hungary	$43,033	NA
25	Czech Republic	$37,464	$46,632
26	Turkey	$33,948	$36,463
27	Korea	$33,600	$27,609
28	Slovak Republic	$11,856	$14,388

13 Obtained from Watson & Wolfe (2008) (providing cross-country data on judicial salaries for OECD nations; albeit missing figures for Mexico and Switzerland that the authors were unable to obtain). On the U.S. state courts, the salaries for judges on the high courts are, for the most part, lower than those on the U.S. Supreme Court. Salaries for Associate Justices on the California Supreme Court, ranked as the top performing state high court in a sister article, were $182,071 in 2006. And the Associate Justice salaries for the lowest ranking state high court, Michigan, were $164,610 in 2005 (National Center for State Courts *Salary Data* 2005). For the "equal weight composite rankings" of the various U.S. state courts, *see* Choi, Gulati & Posner (2008).

salaries (adjusting for cost of living ("COLA") does not change the ranking of the United States significantly)[14]. Below the United States are a number of countries including Germany and Japan that have judiciaries that function well—well enough to support developed market economies and to keep crime low. Finland and Iceland, which pay their high court judges considerably less than the United States, have the lowest scores on the corruption perception index as measured by Transparency International (Lambsdorff 2007).[15] And there is no evidence that the countries above the United States on the salary scale have better justice systems. Of course, judges have different roles and functions in different countries. Comparing the salaries of judges in one country and judges in another may therefore tell us little about the present level of U.S. judge salaries. One would also need to take account of the salaries of competing positions, pension benefits, and the presence or absence of mandatory retirement[16]—which is not our purpose here. Our point is that foreign judicial compensation statistics shed little light on the debate about American judicial compensation, and have so far been used selectively and crudely to support the case for increasing salaries.

37 Does it matter if judges are overpaid? There are two reasons why it does.

38 First, if the government overpays public employees, then people will be drawn from the private sector to government work. This might seem attractive, but there is a problem, namely, that government work is not infinitely valuable and private sector work is not valueless. If the person in question is paid her marginal productivity in the private sector, and if the government overpays, then some people who are more productive in the private sector will become less productive government employees. Overpayment is not as much a concern in the private sector. If employers overpay employees, then the employers will lose profits, fail to attract capital, and go out of business.

14 Most salient, the salaries for Japanese high court judges drop from seventh highest on the list to fourteenth. Data were taken from the March 2006 Mercer Worldwide Cost of Living Surveys, where data for four sovereigns, New Zealand, Iceland, Poland, and Hungary were unavailable.

15 Finland and Iceland top the charts with scores of 9.6 out of 10, whereas the United States earns a 7.3, putting it in twentieth place out of 163 countries.

16 For example, the U.S. federal system is one of the only ones in the world that provides life appointment (Watson & Wolfe 2008). Japan is one of the few other countries that provides its judges with life appointments. We also suspect that the pension benefits in the federal system in the United States are more generous than those in most other judiciaries (Watson & Wolfe 2008).

Because governments do not have shareholders, but instead raise money from taxpayers, and because taxpayers cannot determine whether civil servants are overpaid, the danger of overpayment by the government is more severe.

39 Second, if the government overpays public employees, then rent-seeking will occur, as people compete for these positions. In the case of elected offices, candidates will overinvest in campaigns—as appears to be happening in some states with elected judiciaries (Goldberg, Holman & Sanchez 2002). In the case of appointments, prospective judges will overinvest in supporting elected officials who have the power to fill judgeships—through campaign contributions, campaign assistance, and the like. The more lucrative the job, the more that people will compete to be loyal supporters, resulting in both diversion from productive activity and (potentially) excessive loyalty to elected officials—the problem of patronage.

40 Making the argument that judges are overpaid is not the purpose of this article. But the current debate does not even recognize the possibility of overcompensation, let alone its dangers. Our bottom line is that (1) on the evidence that has been advanced in public debates, it is as likely that judges are overpaid as that they are underpaid, and (2) overcompensation is socially harmful, just as undercompensation is. Ultimately, the theory does not provide clear answers; the question boils down to empirics.

3. AN EMPIRICAL TEST

3.1. The Data Set: State High Courts

41 What set of conditions—including salary and other aspects of the job—helps judges work best? Answering this question requires data on how similar sets of employees perform under different conditions. Using data on the performance of a set of federal circuit court judges, who work under similar conditions of pay, status, number of law clerks, salary, etc., would enable us to rank the judges according to relative performance. But it would not allow us to determine what working conditions support higher productivity. For that second inquiry, we need employees laboring under different conditions. For example, if judges with three law clerks produce more than judges with two law clerks and a secretary (assuming that the secretary costs the same as an additional law clerk), and we think the additional productivity

is caused by the difference between a clerk and a secretary, the state might want to encourage judges to use a third clerk rather than a secretary.

42 The state high courts present a useful data set because, in each of the fifty states, we have a set of judges doing similar tasks, but laboring under different conditions (Brace, Hall & Langer 2001). The states vary in how much they pay judges, what the mandatory retirement age is, how many law clerks the judges have, the salaries of the law clerks, and so on. Among the most important differences are the different selection and retention processes. Some states use elections (both partisan and blind), and others use appointment or merit processes and it is likely that the different systems produce judging of a different quality. It is easy to overstate the amount of variation in the state courts, however; it is considerable as compared to the federal system but limited when looked at through an international lens (Epstein, Knight & Shvetsova 2001). None of the states, for example, has minimal educational qualifications for its judges or uses a civil service model for its judiciary.

3.2. Measuring Performance

43 The threshold question for any employer attempting to determine which factors make its employees more productive is how to measure performance. This is a difficult task with judges because the job involves the exercise of qualities such as judgment and fairness that are hard to observe, let alone measure in a quantifiable and objective fashion. It is tempting then to say—as some do—that the task of measuring judicial performance is too difficult (Goldberg 2004; Marshall 2004). But the same measurement difficulty exists in many employment settings—doctors, nurses, lawyers, bankers, architects, policemen, baseball umpires, etc.—and employees in these professions are regularly evaluated by their employers. If employers can measure the performance of those professionals, why should the public not be able to measure the performance of judges?

44 The reason there has not been significant work on measuring judicial performance, we suspect, is not so much a measurement problem but rather a principal problem. That is, the principals (the public) are too dispersed and have inadequate incentives to measure the agents in this case — the judges. The subset of principals who have control of the selection mechanism, the politicians, do evaluate judges, but we have little information about how they do so.

In putting forward a set of measures, we seek to use objective and reproducible measures. Subjective measures may be better at getting at nuanced aspects of an employee's performance. But subjective measures are also vulnerable to bias and inconsistency. Objective and reproducible measures are important where the risk that the evaluator will be biased and attempt to manipulate the measures is high. Given that evaluating judges is an area with political stakes and, therefore, with a danger of evaluators bringing their biases to subjective evaluations, the use of objective and reproducible measures is important. 45

There exist few systematic attempts to measure the performance of state court judges, and almost no academic studies.[17] A number of studies compare the court systems in the different states, examining the question of whether elected judiciaries perform better than appointed ones. In devising measures of court performance, though, these studies focus primarily on measures of independence and ignore other aspects of the job, such as productivity and quality (Cann 2007b). From an employer's perspective, this is inadequate. Independence is a key element of the job of the judge, but there is no value in an independent judge who does not work, decides cases wrongly, or explains her decisions inadequately. The closest the literature comes to measuring other aspects of judicial performance are studies that use subjective rankings by narrow subsets of lawyers or rely on the prior qualifications of the judges (Institute for Legal Reform 2007; Cann 2007b). However, the rankings by a subset of lawyers (*e.g.*, senior counsel of big companies) capture but an aspect of court performance relevant to those lawyers. As for the qualifications of the judges, it is a leap from looking at what a judge's prior profession was, how long she practiced, or where she went to law school thirty years ago, to the inference that he or she must be a better judge than, for example, someone of a similar age who went to a lower-ranked law school, practiced for a shorter period, or had a different prior profession. 46

We propose measures for three aspects of employee production that we assume the hypothetical employers would wish to measure: effort, skill, and independence. 47

17 For an overview of the localized survey-based evaluations, *see* Kourlis & Singer (2007) (providing a general account of the Judicial Performance Evaluation (JPE) programs implemented at the state and local levels).

3.3. Measures of Productivity

3.3.1. Effort

48 Employers prefer employees who work harder, other things equal. An employee who puts in high effort is likely to be someone who cares about his job, who thinks what she is doing is important and relevant. While it is difficult to measure whether a judge is being fair, just, or empathetic, it is likely that someone who exerts high effort—works longer hours than her colleagues who prefer to be at the beach or golf course—cares about the job more and is likely to try to be as fair and just as possible, since those are crucial characteristics of the job.

49 What measures of judicial output indicate that a judge is exerting effort? We use opinion publication rates. Publication rates indicate effort because producing publication-worthy written explanations for decisions is difficult and time consuming. To save time, some judges may instead issue summary dispositions (Pether 2004; Songer 1990). Assuming they have not exerted much effort in writing the opinion, they will prefer to avoid publication of their summary explanations, for fear of public scrutiny and criticism. Alternatively, judges may drag out the time to write an opinion, leading to fewer published opinions in any given year (and a corresponding increase in the backlog of opinions for the judge). Looking at the number of published opinions a judge issues relative to other judges, therefore, provides a measure of the relative effort exerted by her as compared to the others. It is true that the number of opinions that a judge writes will in part be determined by the kinds of cases that her court receives (and this may vary by state depending on whether the state has mandatory jurisdiction over certain types of cases). Even where a court has mandatory jurisdiction over some appeals, judges have discretion in choosing what opinions to publish and how long they take in writing their opinions. Courts may also develop norms; if a majority of judges on a court does not like exerting effort, it may be able to pressure newcomers to behave like it does — so court norms may be driving some of the publication numbers. The fact remains, however, that the newcomer is giving in to that pressure and producing less.

50 States also may vary in their norms about what types of opinions are publication-worthy. To control for that possibility, we use an alternate measure of effort: the number of pages published. Judges who write more publishable pages, other things equal, are doing more work in providing litigants with

explanations and in helping the development of precedent. Again, this is an imperfect measure — it may be that Judge A is writing more pages than Judge B because he is delegating more of the work to his law clerks and law clerks just take longer to explain even basic concepts. That said, almost no judges do all of their own writing; most delegate extensively (*cf.* Choi & Gulati 2005). As a rough relative measure, if Judge A consistently writes 500 pages a year more than Judge B, Judge A is probably exerting more effort.

Neither of these measures is perfect. Judges might be productive by writing a large number of unpublished opinions, resolving disputes even if not making precedent. In theory, a judge who writes a large number of unpublished opinions expends more effort than a judge who writes a few published opinions. We try to address this problem by including controls for a state's publication rules. In addition, judges might publish more opinions just because they have more cases, and they might have more cases because there is more litigation (or fewer courts) in their state, than in other states. Perhaps they have more cases because they write bad opinions that generate uncertainty that leads to disputes. We try to address these problems by controlling for court characteristics (such as size) and state characteristics (proxies for litigiousness), but we acknowledge that our measure of effort is imperfect. 51

3.3.2. Measures of Skill

Employers also typically care about employee skill. The standard measure used to estimate the quality of judicial production is citation rates. Opinions are products — judges create them and then a variety of customers use them. One can estimate the value of the product to the various customer groups by examining how much each group uses the product. 52

The three customer groups we examine are out-of-state state judges, out-of-state federal judges, and authors in the law reviews. We do not use inside-state citations or citations from the federal circuit covering that state because these citations will often be a function of the fact that the opinion in question constituted precedent and, therefore, had to be cited.[18] All of the other citations, by contrast, are voluntary — citations are made because the cited opinion adds value in terms of the arguments it makes. 53

18 Out-of-state citations are also less likely than in-state citations to be part of standard string citations used for propositions such as the standard of review because these tend to be inherently local matters.

54 The disadvantages of using citations as a measure of quality have been discussed elsewhere (Posner 2000; Farber 2005). For all their faults, citation counts are the most popular and useful measure of quality for judicial opinions.

55 As with the measurement of effort, the goal is to use a multiplicity of measures for skill, each of which will capture a different aspect of the value that the opinion in question adds. An opinion that legal academics find most interesting because of its nuance and complexity may be the same opinion that judges find least useful precisely because of its nuance and complexity.[19]

3.3.3. Measures of Independence

56 Judicial independence means judgment on the merits, uninfluenced by the political interests or ideological commitments of political branch officials or by the demands of party. A judge's decision should not be related to her political affiliation. It follows that, if judges are independent, Republican judges should agree (or disagree) with each other as often as they agree (or disagree) with Democratic judges, and vice versa. On the basis of this observation, we construct our measure of Independence.

57 "Independence" can also refer to the ability to resist bribery and other forms of improper pressure from the parties, and unwillingness to submit to pressure from political officials who seek particular outcomes. In many parts of the world, bribery and improper political pressure are significant problems, but the United States is relatively fortunate in this respect. The aspect of independence that remains, the one that is most relevant in the U.S. context, is independence from the ideology of the party that appointed the judge, or non-partisanship.

58 Our Independence measure uses opposing opinions to measure the degree of disagreement among those of similar political orientations. Opposing opinions are defined as either a majority opinion when a dissent exists, or a dissent when a majority exists. Empirical literature has established that judges often vote consistently with policy preferences (typically proxied by the political party of the appointing actor) (George 1998; Shepherd 2007).

19 Again, prior research suggests otherwise. Opinions with lots of citations from one set of customers tend to be the same opinions with high citation numbers from other customers (Choi & Gulati 2004).

A judge who disagrees with those with a similar party background as his is demonstrating independence.[20]

59 To calculate this measure, we start with the number of dissents a judge issues against other judges on his court with the same political affiliation and the number of majority opinions that the judge writes that are opposed by a dissent from someone of the same political affiliation. We divide this number by the total number of dissents and majority opinions against a dissent that a judge authors (the resulting fraction is termed "Opposite_Party"). If Judge X is a Republican and writes 7 out of his 10 dissents against Democratic judges as well as 8 of his 10 majority opinions where there is a dissent by a Democratic judge, then Opposite_Party for Judge X would equal 15 divided by 20 or 0.75.

60 The Opposite_Party number, to provide a comparative measure, has to be normalized to take into account the fact the different courts have different ratios of judges from the opposing parties. A Republican judge who is on a court with one other Republican and eight Democrats will have relatively few chances to oppose someone of the same party, whereas if that same Republican judge were on a court with nine Democrats, he would only be opposing Democrats because there would be no opportunity to disagree with Republicans. To correct for the imbalance problem, we calculate each judge's independence score as a function of what fraction of his court's opinions

20 Party affiliation information is obtained by using three sources. First, we searched NEXIS and the Internet (using Google) for any news reports on the political affiliation of each judge. Second, we also searched for information on political contributions at the opensecrets.org website. In the opensecrets database, we searched for political contributions for each judge by first and last name in the state in which the judge sits on the high court. We also looked at the profession of each donor as provided by opensecrets—counting only donations by persons with the same first and last name and who either listed their profession as on the state high court or who listed a law firm affiliation (where we were able to match the judge to the law firm through other sources). We used the political party of the donee candidate as a proxy for the political party of judges who contributed. Third, we used the party of the governor (if any) who appointed the judge as a proxy for the judge's political party. In most of the cases where we had multiple sources of information on political party, the party was consistent across these sources. When our three sources reported different parties, we gave first priority to the party identified through our NEXIS and Internet searches and second to the party identified in the opensecrets.org database. In our sample, 220 judges were classified as a Democrat and 170 as a Republican (with 16 no data or Independent party judges). Of the 390 judges classified as either a Democrat or Republican, 35 (or 8.97 percent) had a conflict in our three methods of determining political affiliation (Choi, Gulati & Posner 2007).

are generated by those from the opposite party (Opposing_Pool). If a court has seven out of ten judges who are Democrats and they all write the same number of opinions, the three Republican judges will have 70 percent of the court's opinions to oppose. We expect that a Republican judge dissenting at random would dissent 70 percent of the time against a Democrat. We defined Independence as Opposing_Pool minus Opposite_Party. If one of the Republicans disagrees with his Democratic colleagues 72 percent of the time, we estimate an Independence score of –0.02 (that is, the baseline Opposite_Pool number 0.70 minus the individual Opposite_Party number 0.72); the negative score suggests that he is less likely to disagree with those from his own party than the baseline. If the fraction of his disagreements that are with his Republican colleagues is 68 percent of the time, we count that as a +0.02; because he is more willing to disagree with those from his party than with those from the other party and thus more independent.

61 The measure has problems. One problem is that the Opposite_Party ratio is determined from a small number of opinions in question for almost all judges. Judges do not dissent often — it is hard work. Assuming that judges would prefer to avoid this additional work, they will seek to compromise where possible. A judge's independence score under our measure might therefore be the product of only a handful of dissents. A judge who does not often dissent, might have had a couple of cases involving issues that he felt strongly about during the time period that our data spans. Given that our data spans only a handful of years (a short period as compared to the judicial life span for most judges), it is possible that some judges have high scores on our independence measures due to the random arrival of cases they feel strong enough about to dissent against even fellow political travelers on the bench (a situation that may not have occurred for most other types of cases).

62 Another issue is that the measure only looks at extreme behavior—situations where compromise has broken down to the point that the judges are engaging in public disagreement. Judges, however, might also be displaying independence in their private negotiations with colleagues over case outcomes and the language of opinions in cases where there end up being no dissents; that is, the majority of cases (Revesz 1997). Our measure undercounts these moderate displays of independence.[21]

21 This undercounting problem is not as severe if a judge's ability to threaten dissent is a function of her actual willingness to dissent. Those with high dissent numbers against those from

Our measure differs from the standard measures of independence (or bias) used in the empirical literature on judges. Those measures tend to start with a series of assumptions about the policy preferences of Democratic judges versus Republican judges. For example, Democratic judges are typically assumed to dislike big corporations, oppose the death penalty, and support criminal defendant rights; they favor the little guys (*e.g.*, George & Epstein 1992; Brace & Hall 1997; Miles & Sunstein 2008). The cases are then coded as a function of the direction of the vote. If it turns out that the Democrat appointed judge votes in favor of the individual litigant more often than in favor of the big corporation, then the conclusion is that the judge is voting consistent with his policy preferences. An example of the use of this methodology is Joanna Shepherd's (2007) recent article where she focuses on whether sitting state judges bias their votes in the direction of the policy preferences of the group that has the power to decide whether to retain these judges or not (they do, even when the judge's politics differ from those of the group voting on retention). We do not use this methodology for a couple of reasons. 63

First, the methodology focuses exclusively on votes. Votes capture the 64

the same party will probably also be those who are actively negotiating with their colleagues. It is possible, however, that high dissent numbers instead simply represent cantankerousness and an unwillingness to compromise.

There is an additional set of problems that we do address. First, the problem of states that are dominated by a single party. Consider the case where all judges on a particular state high court are all of the same political party (say all Republican). Our Independence measure will equal zero since Opposite_Party will equal Opposite_Pool (and both will equal zero since there are no Democrat-authored opinions). Table 4 excludes judges who come from states with no variation in political party among judges for this reason. But, by the same token, we lose data.

Second, even where all judges are not of the same political party in a state, if an imbalance exists, the range of the Independence variable will vary. Consider two Republican judges. One is in a state with 90 percent of the majority opinions written by Democrats and the other is in a state with 10 percent of the majority opinions written by Democrats. For the first, Independence can range from –0.1 to +0.9. For the second, Independence can range from –0.9 to +0.1. So the second judge could have a much more negative Independence score than the first judge simply because the range is shifted over. To address this, we created a version of the Independence variable that is less dependent on the background political makeup of a particular state court. Independence_Indicator is defined as 1 if independence is greater or equal to zero and zero otherwise. The indicator variable addresses the range problem but also throws out information: it suggests judges subject to non-partisan elections are less independent than the other types, who are about the same. None of the differences in mean Independence_Indicator levels among the varying selection systems are statistically significant. There is also the possibility that dissenting is a greater display of independence than writing a majority opinion. However, we found no significant differences exist in the Independence scores of active dissenters compared with occasional dissenters for judges of any of the four selection systems (Choi, Gulati & Posner 2007).

underlying political dynamics of an opinion only imperfectly. Two opinions with votes in favor of a corporate litigant, for example, may generate different precedent for future opinions depending on how the opinion is written. Even if three Democrats on a panel compromise with the two Republicans to write the opinion narrowly, in exchange for voting in favor of the corporation (that is, allowing the corporation to win in this case, but making it more difficult for corporations to win in the future), the case gets coded as having a Republican outcome. Given that political parties likely care more about policies rather than individual case outcomes, looking only at voting outcomes may miss the mark. Put another way, reasoning and precedent are more important than the vote in any particular decision. It is only in the subset of cases involving dissents that we can be confident that the issues are important enough to the judges that they are not willing to trade votes for reasoning—and therefore examining votes (and against whom such votes are cast in a decision) in dissent situations provides a more accurate measure of the political dynamics among judges. Over the same time period then, our measure yields a smaller number of data points, but we can be more confident in the information that they yield.

65 Second, standard measures of independence also rely on subjective approximations to determine whether a judge's votes are classified as more aligned with Democratic preferences or Republican ones (*cf.* Epstein, Staudt & Wiedenbeck 2006).[22] For example, voting against an individual bringing a tax challenge against the government might be coded as a Democratic vote because of an assumption that Democrats are pro-big government. But what if it is the case that most individuals who bring tax challenges are poor? Weren't Democrats supposed to favor the little guys, the individual litigants, against the government? The same kind of argument can be made with respect to coding assumptions in a variety of areas such as securities fraud and medical malpractice. The standard measures likely work well in the subset of cases that empiricists studying the courts typically focus on — that is, civil rights type cases. But they get problematic when one moves away from the hot button areas into areas such as business law. Given that our data set covers all state high court cases decided in a defined period of time (from 1998 to 2000), using the

22 For an examination (and critique) of some of these coding assumptions in the tax context, *see* Epstein, Staudt & Wiedenbeck (2006).

standard subjective coding method would result in a potentially high error rate. By contrast, our measure codes only those cases where the Democratic judges vote (and write) against their Republican counterparts as politically divided. Instead of making assumptions about what constitutes Democratic or Republican preferences, we in effect allow the judges to tell us.

4. DETERMINING THE EFFECTS OF SALARY AND TENURE

4.1. The Salary and Tenure Mechanisms

The fifty states use a range of tenure and salary combinations. Salaries range 66
from $83,550 in Montana to over $150,000 in New York as of 1999. For tenure, at the high end, a few states such as New Hampshire mimic the federal system with life tenure. Most states have judicial terms, ranging from just a few years, to 14 (New York), and always with the possibility of reappointment or reelection. A judge's actual longevity depends on her ability to be reelected or reappointed, plus the incentives to retire prior to the end of the term. In 1997, median longevity by state was 8.6 years, ranging from a low of 3.5 in West Virginia, to a high of 19.1 in Oklahoma.

The tenure variable presents a potential complication. Because most 67
states have mechanisms such as impeachment by which to remove judges, there is the possibility that judges in a particular state court might have a low tenure because they are no good. That is, the tenure variable may not be exogenous. Prior research, however, indicates that the tenure variable is stable over time, suggesting only a minimal endogeneity problem (Hanssen 1999). The tenure variable is highly correlated with the type of selection system that a state has (*e.g.*, election versus appointment) and these selection systems have remained mostly stable over the last few decades, again suggesting that the endogeneity problem is not large (Hanssen 1999).

4.2. Hypothesis

Conventional wisdom in the salary debate holds that higher wages translate 68
into higher judicial productivity. That means that the high-wage judges, vis-à-vis their lower wage counterparts, should be exerting more effort, be demonstrating higher skill, and exercising greater independence. Theory, however, suggests that the operation of wages on production quality will

not just be a function of wages, but also of tenure and the quality of the selection system. We therefore look to see how judges perform not just under different wage regimes, but different wage, tenure, and selection system combinations. Further, participants in the salary debate ignore the fact that basic employment conditions such as the number of assistants and their wages and tenure likely affect productivity. We examine the impact of various employment conditions.

69 To test the hypothesis that higher salary causes higher judicial productivity, we use models in which various productivity measures are used as dependent variables, and salary and various controls are used as independent variables. At the outset, we should address an issue common to all the models. Our model is essentially cross-sectional because of data limitations, so we are constrained in our ability to get a good handle on causation. If our results show higher salaries as correlated with higher productivity, these results could be interpreted as showing that greater workloads cause legislatures to raise salaries, rather than showing that higher salaries generate greater output. However, salaries are more likely to be exogenous than output. Legislatures set salaries in response to political pressures, and while those political pressures could reflect concerns about judicial quality, much else affects these decisions as well. Because courts control their own workload, output is endogenous. In any event, because we find either no relationship or a weak relationship between the two variables, these concerns are largely idle. With our weak results, it is likely that neither causal story is correct.

4.3. Data Description

4.3.1. The Dataset

70 The decisions of the high courts of every state for three years (1998-2000) constitute the dataset. Texas and Oklahoma have two high courts, for civil and criminal appeals, and are counted as two states each, making a total of 52 states. The District of Columbia is excluded because of its unusual character. The dataset contains 408 judges, approximately 8 per court. The average judge spent 2.65 of the 3 years in our sample period on the court. Each judge wrote on average about 24.9 opinions per year.

71 The data is cut three ways, as a function of the type of data. For effort, we examine opinions at the judge level for each year. Each observation is a judge for a particular year, giving us 1082 observations (408 times 2.65).

For citations, the number of data points is larger because those numbers were measurable at the opinion level. There are 27,596 majority opinions. Finally, because the data pool is smaller for independence (small number of dissents relative to number of opinions or citations), we calculate judge-level effects for the aggregate of the three years. For independence, there are 408 observations. The number of observations in the regressions is sometimes lower as a result of incomplete data for certain variables. The states vary in terms of the judicial selection mechanisms they use. Table 2 separates the staes as a function of the mechanisms they use.

Table 2

Appointed	Merit Selection	Non-Partisan Election	Partisan Election
Connecticut	Alaska	Georgia	Alabama
Delaware	Arizona	Idaho	Arkansas
Hawaii	California	Kentucky	Illinois
Maine	Colorado	Louisiana	North Carolina
Massachusetts	Indiana	Michigan	New Mexico
New Hampshire	Iowa	Minnesota	Pennsylvania
New Jersey	Florida	Mississippi	Texas
New York	Kansas	Montana	West Virginia
Rhode Island	Maryland	North Dakota	
Vermont	Missouri	Nevada	
Virginia	Nebraska	Ohio	
South Carolina	Oklahoma	Oregon	
	South Dakota	Washington	
	Tennessee	Wisconsin	
	Utah		
	Wyoming		

Because there is little temporal variation in salaries, our analysis is essentially a cross-sectional analysis. We control for the year, but we do not use state fixed effects because of the lack of temporal variation. Ideally, we would have data over a longer period of time and use state fixed effects. The absence of state fixed effects means that our model, if it does not control for all other state-level

72

variation, may not accurately reveal the relationship between salary and productivity. To address this problem we include a number of state level controls.

4.3.2. Variables for the Multivariate Model

73 We estimate a series of multivariate regression models using our proxies for productivity, quality, and independence. Our key independent variable of interest is the associate justice salary level for a particular court. We adjust the salary data for the cost of living for the city in which the court is located (Adjusted Associate Justice Salary).[23] In the multivariate models, we relate our proxies for productivity, quality, and independence with Adjusted Associate Justice Salary to test the importance of salary for judicial performance.

74 Because our focus is on the differential cost of being a judge, we include an opportunity cost measure into our models. To calculate opportunity cost, we use the average wage paid to equity partners at large law firms in that state (Adjusted Partner Salary). We assume that the type of lawyer who might be interested in a state judgeship likely has a local orientation and that her best alternative position would be at a local law firm.[24]

75 The method of judicial selection is also an important determinant of productivity, quality, and independence. We include independent variables for whether the state high court judges in our sample are selected through Partisan Election, Non-Partisan Election, or Merit Selection (as compared to the base category of Appointed judges).

76 In addition to our judicial salary and selection variables, we include a set of variables relating to the characteristics of each state high court. These variables, described below, may also determine judicial performance and provide alternate levers for policymakers seeking to affect performance.

4.3.2.1. Court Characteristic Variables

77 The court characteristic variables seek to capture differences among states with respect to the structure of judicial chambers; they provide a sense of

23 If not available, a nearby city with a similar profile in terms of income, crime rates, and population is used.

24 A more fine-tuned measure of opportunity cost might calculate not just the difference between judicial salary and average partner salary for that year, but also calculate in expected future streams of earning for the two jobs. Such a calculation would require both estimations as a function of age-wage profiles for the individual judges, but also detailed information regarding the pension plans of each state.

whether certain court characteristics, such as the wages paid to law clerks, have an impact on a judge's productivity.

The first variable concerns whether the mix of judges on the high court remained the same throughout our sample time period from 1998 to 2000 (Stable Court) and the size of the bench during the 1998 to 2000 period (Number of Active Judges on Bench). Teams that have high turnover likely operate differently than those that don't, and the size of the team may impact dynamics. We include an indicator variable for whether the judges in a specific court do not face mandatory retirement (No Mandatory Retirement). Next, to measure the effects of law clerks, three variables are used. The average number of clerks per judge for the 1998 to 2000 period is one variable (Number of Clerks Per Judge).[25] Because long-term clerks—who will be more experienced in the later years of their tenure as clerks—may be more effective at assisting the judge, a variable for whether the clerkships are longer is used. The typical clerkship at the federal level is one year and is considered standard, therefore clerkships lasting for two or more years are defined as long-term (Long-Term Clerk).[26] 78

There is a potential causation issue with some of the clerkship variables. On the one hand, it is likely that clerks who are more experienced can help their judges more — and the prediction is that more experienced clerks will result in higher productivity. On the other hand, low quality judges may find a job that provides experienced assistants attractive. So, a state that provides its judges with a number of skilled assistants might attract more low quality judges, thereby producing the prediction that higher quality assistants will correlate with lower quality judging. 79

To capture the opportunity cost of being a law clerk, the difference between the average salary of an entering associate at a large law firm in that state and the law clerk salary is used (Law Clerk Opportunity Cost). Given the amount of status, training, and networking benefits clerkships provide, 80

25 Although clerks are generally assumed to increase judicial productivity there has been little research quantifying the effects of law clerks. An example of the assumption about value added by clerks is the literature on the caseload explosion in the federal courts in recent years. Judges are thought to have used clerks to help deal with the caseload increase (Richman & Reynolds 1996).

26 A problem here is that we treat a wide range of long-term clerkships — ranging from two-year clerkships to twenty-year clerkships — as the same. We were not able to obtain the more finely calibrated data. However, as explained later (*see* notes 36, 42, and 47 infra), because California is reputed to be unusual in terms of the number of career clerks, we can run a robustness check separating out California.

we expect that clerkship salaries will have zero impact on judicial productivity. Indeed, the scarcity of clerkships and long queues of applicants suggests that clerkship salaries could even be reduced.

81 Judges may also act differently if facing a high workload, particularly if an intermediate appellate level court does not exist to help with the workload. The log of the number of trial cases in the state measured in 1998 (ln(Number of Trial Cases in the State)) and an indicator variable for the presence of an intermediate appellate court (Intermediate Appellate Court) are included.[27] The number of clerks and clerk tenure, the size of the bench, and the number of trial cases may also influence a judge's choice to devote time to any specific case and those variables are included as well. Courts differ in terms of their court rules for publication rules, with some allowing the judges discretion and others mandating publication. We use an indicator variable to distinguish states that mandate the publication of all opinions (mandatory publication). These rules could distort the judges' allocation of their scarce resources; for example, if they have to publish a lot of opinions, the quality of individual opinions might drop.

82 In addition to judicial salary and selection variables, we include a set of control variables in our models of productivity, quality, and independence. The control variables divide into two types: Judge Controls and State Controls.

4.3.2.2. Judge Controls

83 Our models include an indicator variable for whether the judge was the chief judge of the high court (Chief Judge). A chief judge may have less time to author opinions. The chief judge may also command greater respect and receive greater numbers of citations as a result. Additionally, the chief may be able to assign herself the more important opinions and garner more citations that way (*cf.* Langer 2003). Also included is the number of years between 1998 and

27 To control for the effects of court control of its docket on quality, we run the regressions with a control variable for the ratio of cases resulting from mandatory appeal to the total number of cases. We also try to capture this same effect by using a variable that measures whether the state's internal publication rules mandate publication of opinions or allow the issuance of unpublished opinions (the former being more likely in states where the court has discretion in the cases it chooses to hear). In neither case do these additional controls make a difference to the coefficients for our main results (*cf.* Brace & Hall 1990) (finding that institutional variables, such as whether the state has an intermediate appellate court, explain a significant portion of the variation in dissent numbers).

the year in which the judge received her law degree (Post Law-School Experience) and the number of years the judge has been on the high court (Court Experience). More experienced judges may decide cases with greater skill and speed, leading to more opinions and more citations.

84 The problem of judges behaving differently as elections approach is often mentioned in criticisms of electoral systems of selecting judges (Bright & Keenan 1995; *cf.* Berkowitz, Bonneau & Clay 2005). To measure the effects of elections, we examine whether the judges in our data set raised campaign funds during any of the years for which we collected data (Election Spending). A variable for whether a judge retired in 2001 or earlier captures the possibility of end game problems of a different sort (Retirement Close) — judges who are about to retire have little to lose from sloughing off on the job. Additionally, they may be retiring because they no longer find the job interesting.[28] Given that Chief Justice Roberts expressed concerns about the reduced presence on the bench of lawyers from private practice, we calculate a variable for whether the judge's primary prior employment prior to joining the bench was in the private or public sector (Private Practice).[29] Lastly, we include the PAJID score for each judge as developed by Brace, Hall & Langer (2001). These scores locate judges on a political continuum from highly conservative (0) to highly liberal (100).

85 To capture demographic effects—standard in testing the factors that influence worker productivity—we use variables for age (Age) and gender (Female). Prior research has examined judicial behavior as a function of gender, age, and educational background.[30] For example, women judges have been

28 If judges are motivated primarily by the desire to serve the public, rather than interest in the job, the fact that they no longer find the job interesting should have but a minimal effect on their performance.

29 Although a number of prior studies have looked into the impact of prior experience on aspects of judicial performance, almost none have used a broad measure of productivity, focusing instead on narrower measures, tending to relate to independence or bias. *E.g.,* Scott & Ditslear (2007); Taha (2004); Ashenfelter, Eisenberg & Schwab (1995); Brudney, Schiavoni & Merritt (1999); Sisk, Heise & Morriss (1998). Among the exceptions that do look to the impact of background on broader measures of judicial productivity are Baker (2008); Landes, Lessig & Sollimine (1998). Going further back, Caldeira looked to whether occupational background was a predictor of judicial "greatness" (Caldeira 1988).

30 Among the studies examining the effects of gender, age, and educational background on aspects of judicial behavior, *see, e.g.,* Songer, Davis & Haire (1994); Smyth & Bhattacharya (2003); Teitelbaum (2006).

hypothesized to behave differently in discrimination cases than their male counterparts (Peresie 2005). With age, it has been suggested that younger judges are more politically biased and more productive than their older counterparts (Teitelbaum 2006; Baker 2008). Further, the problem of older judges overstaying their welcome, particularly on the U.S. Supreme Court, has led to proposals for mandatory retirement (Crampton & Carrington 2005). To test this, we include variables for age and for whether the state has mandatory retirement. The latter, mandatory retirement, might capture a selection effect —in that the type of individual attracted to a job that mandates retirement might be different from one who wants to hold on to power and status until he is physically unable to do so any longer.[31]

4.3.2.3. State Controls

86 State controls include variables relating to the general characteristics of each state. These controls help correct for the possibility that differentials in state characteristics, rather than the institutional characteristics we are interested in, might be driving the results. Differences in state population (ln(Population)), gross state product (ln(Gross State Product)), and crime rates (Crime Index) may lead to different mixes of cases and judicial responses to these cases. Likewise, the median age of the population (Median Age of Population) and state median per capita income (State Median Income) as measured in the 2000 U.S. Census may affect the mix of cases and judicial response. Prior research suggests that judges are influenced by their colleagues in neighboring states—plus, states may be more likely to borrow from the law of their neighbors than from distant states because of famil-

31 Given that we look at multiple occupational background variables (experience on the bench, years after graduation, and whether one's prior primary occupation was in the private or public sector) and given that judges are not typically appointed until middle age, we did not expect eliteness of law school attended to be a useful predictor of judicial performance. However, increasing the quality of credentials has long been a goal of judicial reform movements. Our cynical reading of this goal and the history of the move toward merit selection is that this goal appears to have been driven more by the desire to enhance the status of the profession than a belief that these credentials translate into better judging. *See* Carrington (1988). Assuming that the goal is to raise credentials then, independent of productivity, we find it the case that a higher law school rank under the U.S. News correlates with a higher salary. See infra (discussion of results). But the effect is not significant. Cutting the data more finely, we see that raising salaries increase credentials only in merit states; perhaps that is so because lawyers pick judges in the merit systems and they are more impressed with credentials.

iarity, if those neighbors have more developed laws (Harris 1986). Further, larger neighboring states might produce more or different types of cases. For these reasons, we calculate a variable for the aggregate population of border states (ln(Border Population)).

We also include a measure of the age of the state (State Age). Older states likely 87
have longer judicial traditions, more precedent, and perhaps a more sophisticated jurisprudence on which contemporary jurists can draw. State Age controls for the possibility that judges from older states are cited more often outside of the state just because they can draw on the more sophisticated jurisprudence of their state. We include the fraction of the population comprised of blacks as obtained from the 2000 Census (Black Population Fraction). Greater racial heterogeneity may produce greater complexity in the mix of cases that go to the state high court and affect a judge's attitudes toward such cases. We include a variable for citizen ideology based on election results in each district (Citizen Ideology Score).[32] The background ideology of the citizens of a state may affect the behavior of judges

Other variables such as the number and educational backgrounds of 88
staff attorneys, the differentials in judicial pension systems across states, or the educational backgrounds of the law clerks would have also been useful to include. Adequate data, however, was not available.

4.4. Effort

Our first multivariate model focuses on the total number of opinions au- 89
thored yearly by a judge as our measure of effort. Figure 2 displays a mean comparison of the total number of opinions for judges paid less than or equal to the median adjusted associate justice salary and judges paid greater than the median. For purposes of reporting summary statistics, we divide the states into states where the salaries are above the median and those where it is below. The median salary of an associate justice in 1999 was $111,758. Judges paid above the median are more productive than those below. The mean difference is significant at the <1 percent confidence level.

32 This variable is taken from Berry, Ringquist & Hanson (1998) (updated data available at www.uky.edu).

Figure 2: Productivity by Salary

90 Other factors may affect the effort exerted by a judge on a particular court. We estimate a multivariate model with number of opinions authored in a year as the dependent variable. The equation estimated, using an ordinary least squares regression with standard errors clustered by state:

$$\begin{aligned}\ln(\text{Total_Opinions}_i) = {} & \alpha + ß_{1i}\text{Adj. Assoc. Justice Salary} + ß_{2i}\text{Adj. Partner Salary} \\ & + ß_{3i}\text{Election_Partisan} + ß_{4i}\text{Election_Non-Partisan} \\ & + ß_{5i}\text{Merit_Plan} + \sum ß_{ji}\text{Court Characteristics}_{ji} \\ & + \sum ß_{ki}\text{Judge Controls}_{ki} + \sum ß_{li}\text{State Controls}_{li} \\ & + \text{Year Effects} + \varepsilon_i\end{aligned}$$

91 The model relates the log of the total number of opinions per active judge per year, with the Adjusted Associate Justice Salary for the judge. The Adjusted Partner Salary for the state is included as a control for the opportunity cost of becoming a judge. The regression model includes year-level Court Characteristic variables and Judge and State Controls.

92 The selection method used by a state will affect both the type of person who becomes a judge and the incentives facing judges to produce published opinions. Accordingly, indicator variables for Non-Partisan Election, Partisan Election, and Merit Plan states are included. The three variables use Appointed states as the baseline. The model includes year fixed effects. Model 1 of Table 3 reports results. A variation on Model 1 is also reported, replacing the selection method for judges with the average tenure of judges in a particular state (Tenure). Reported as Model 2, the variation allows examination of the importance of the retention mechanism on judicial effort. Judges with

longer tenures (more job security) may face fewer pressures to exert effort.

For Models 1 and 2, we see that the coefficient on the associate justice variable is positive and significant only in Model 2 (at the 10 percent level) and the coefficient on the partner salary variable is not significant.[33] Higher judicial salaries only weakly correlate with greater productivity (opinions written). Moreover, the magnitude of the effect is relatively small. For a $10,000 increase in salary, under Model 1, effort increases by 6.6 percent (and 9.4 percent under Model 2). The mean level of opinion production for judges in our sample was 24.9 opinions per year. For the mean judge, $10,000 more in compensation corresponds to an increase of 1.6 and 2.3 more opinions per year for Models 1 and 2. We see, however, that greater job security does not correlate with higher productivity; the Tenure measure (Model 2) correlates with lower productivity at the 5 percent significance level.[34]

33 Unreported, when we use an opportunity variable calculated as the difference between Partner Salary minus Judge Salary, the coefficient is not significant.

34 The regression models in Table 3 utilize an indicator variable for whether the state has a mandatory publication rule for opinions to control for workload differences among the states. We lack data for these variables for all our states. As a robustness test, we omit the Mandatory Publication indicator variable from the Model 1 in Table 3 and re-estimate the model instead with the addition of indicator variables for whether the state high court has mandatory jurisdiction over civil (Mandatory Civil Jurisdiction) or criminal (Mandatory Criminal Jurisdiction) cases. Unfortunately, we lack information on these variables for all of our states and only use them for robustness tests. Unreported, the coefficient on associate justice salary is positive but not significantly different from zero.

As additional robustness tests, we re-estimate Model 1 with the replacement of ln(Total Opinions) with the log of the total number of pages written yearly for our sample judges (ln(Total Pages)) as the dependent variable. Under this alternative specification, higher associated justice salary correlates with more pages written but the relationship is not statistically significant. We also re-estimate Model 1 with the replacement of ln(Total Opinions) with the log of the total number of majority opinions authored yearly for our sample judges (ln(Majority Opinions)) as the dependent variable. Unreported, the coefficient on associate justice salary is positive and insignificant.

We also re-estimate Model 1 with the addition of an indicator variable for whether the judge also teaches as an adjunct professor (Professor). Unreported, the model produces the same qualitative results as Model 1. The coefficient on judicial salary is positive and significant. The coefficient on Professor is positive but not significantly different from zero.

We re-estimate Model 1 with the addition of an indicator variable for whether the judge comes from a state that gives its high court judges lifetime employment (Lifetime). Unreported, the model produces the same qualitative results as Model 1. The coefficient on judicial salary is positive but insignificant. The coefficient on Lifetime is negative but also insignificant.

Lastly, we re-estimate Model 1 with the addition of indicator control variables for whether the state is a member of U.S. Census region Mid-West, Northeast, or South (using West as the

94 There are other variables with larger coefficients — important information for those who seek to increase judicial effort. The type of selection system matters. Election Partisan, Election Non-Partisan, and Merit selection states have more productive judges compared with states that use Appointment systems (Choi, Gulati & Posner 2007). The presence of a stable court also correlates with greater productivity, whereas closeness to retirement correlates with lower productivity.

Table 3: Effort

	Model 1	**Model 2**
Dependent Variable	ln(Total Opinions)	ln(Total Opinions)
Independent Variables		
Adjusted Associate Justice Salary	0.007 (1.510)	0.009+ (1.830)
Adjusted Partner Salary	-0.001 (-1.030)	-0.001 (-1.050)
Election Partisan	0.518* (2.620)	
Election Non-Partisan	0.299+ (1.730)	
Merit Plan	0.262 (1.490)	
Tenure		-0.039* (-2.270)
Stable Court	0.359* (2.250)	0.322* (2.130)
Number of Active Judges	-0.009 (-0.230)	0.032 (1.280)
No Mandatory Retirement	-0.289+ (-1.880)	-0.303* (-2.170)
Long-Term Clerk	-0.131 (-1.060)	-0.033 (-0.300)
Number of Clerks Per Judge	0.036 (0.390)	0.053 (0.580)

base case). Unreported, the coefficient on associate justice salary is positive and significant at the 10 percent level. As with Model 1, however, the coefficient is not large in magnitude.

Overall, our robustness tests reveal that the relationship between higher judicial salary and productivity is weak. In many of our tests, the correlation is not statistically significant. Even where significant, the relationship is small in magnitude relative to other aspects of the high court system.

	Model 1	**Model 2**
Dependent Variable	ln(Total Opinions)	ln(Total Opinions)
Independent Variables		
Law Clerk Opportunity Cost	-0.009* (-2.200)	-0.008* (-2.300)
ln(Trial Cases in the State)	-0.091 (-1.110)	-0.061 (-0.770)
Intermediate Appellate Court	-0.232 (-0.820)	-0.231 (-0.870)
Mandatory Publication	0.232+ (1.780)	0.132 (0.900)
Chief Judge	-0.147* (-2.520)	-0.161* (-2.660)
Court Experience	0.009+ (1.860)	0.011* (2.230)
Post-Law School Experience	0.007 (1.100)	0.004 (0.660)
Retirement Close	-0.208** (-3.990)	-0.215** (-3.950)
Age	0.001 (0.130)	0.004 (0.760)
Female	-0.043 (-0.660)	-0.048 (-0.720)
Private Practice	-0.028 (-0.290)	-0.029 (-0.290)
Election Spending	-0.035 (-0.570)	-0.023 (-0.360)
PAJID Score	0.003+ (1.840)	0.003 (1.550)
Constant	31.037** (3.830)	29.551** (4.060)
Year Fixed Effects State Controls	Yes Yes	Yes Yes
N Adj. R2	998 0.2146	998 0.2155

The t-statistics (in parentheses) are calculated using standard errors clustered by state. Variable definitions are in the Appendix.

+ Coefficient significant at the 10 percent level or less.

* Coefficient significant at the 5 percent level or less.

** Coefficient significant at less than the 1 percent level.

95 Interestingly, given the push in many states to remove or modify the current mandatory retirement ages for judges, the lack of a mandatory retirement age correlates with lower effort. This may be because courts without a mandatory retirement age have older judges who are unable to exert as much effort. The coefficient on the Age variable, however, is not significantly different from zero.[35] Alternatively, perhaps there is a screening effect resulting from mandatory retirement. The types of individuals attracted to a job that requires mandatory retirement may be different from (and more inclined to exert effort than) those attracted to a job without mandatory retirement. Also interesting, given the asserted need to use higher salaries to attract more candidates from the private sector, is that the coefficient on private practice is not significant; lawyers whose primary prior profession was in private practice do not turn out to be more productive than their colleagues from the public sector.

96 The results for the clerkship variables are surprising. Having long-term clerks does not correlate significantly with judicial output; if anything, the correlation is negative. Surely, long-term clerks are better at their jobs than short-term ones who usually have only just graduated from law school. If so, it is likely that there is a selection effect at play here as well.[36] Perhaps judges who use long-term clerks are inherently less productive (that is why they are attracted to the job that provides better assistance) and that this lack of increase in judge productivity is driving the non-effect. Surprising also is that the number of clerks does not correlate with productivity. Again, it seems uncontroversial that more clerks can do more work than fewer clerks. So, the lack of correlation here might again be telling us something about selection effects.

97 Finally, law clerk opportunity costs show up in both models as negatively correlated with effort (significant at the 5 percent level). The higher the cost of becoming a law clerk, perhaps the lower the skill level of the clerks who apply for the job. In the context of the salary debate, the point here is that raising clerk

35 Nor is the variable for gender.

36 Among the comments we received to a prior draft were some from career clerks on the California high court who asserted that lumping in the career clerks in California with the two-year and three-year clerks that other states might be using was likely distortionary. The claim being that the effect of long-term clerks in California was likely positive. As a robustness test, we added an additional indicator variable for California clerk separate from the long-term clerk variable (with non-long-term clerks as the base category). The coefficients on long-term clerk and California clerk were both negative and not significantly different from zero. The coefficient on the judicial salary variable was not significantly different from zero.

salaries looks to be more likely to produce the same increase in the number of published opinions as raising judge salaries by the same amount.

Recall that our model of judicial compensation implies that salary should matter most when judges can lose their jobs. To investigate this possibility, we see whether the effect of judicial salary on productivity varies according to the type of selection system, length of tenure,[37] and the presence of lifetime employment. Table 4 reports the data using a division of our data based on whether the judge is from a state with greater than (or less than or equal to) the median adjusted associate justice salary for the fifty-two states.[38] 98

Table 4: Total Number of Opinions (Yearly) and Judge Selection and Retention

	Adj. Assoc. Justice Salary Less than or Equal to Median	Adj. Assoc. Justice Salary Greater than Median	p-value
Election Partisan	21.8	30.2	0.021
Election Non-Partisan	26.2	32.4	0.007
Merit	25.0	22.8	0.103
Appointed	23.3	17.5	0.000
Tenure Less than or Equal to Median	23.2	28.7	0.000
Tenure Greater than Median	26.2	24.4	0.204
Not Lifetime Employment	24.9	26.7	0.099
Lifetime Employment	22.6	15.7	0.002

Elected Partisan judges, judges with shorter tenure, and judges without lifetime employment work harder when salary is higher—what our model suggests (although the productivity differential for judges without lifetime employment is significant at only the 9.9 percent level). Judges behave like the rest of us — they 99

37 We use the median tenure of justices for our sample of fifty-two states measured as of 1997 (equal to 7.75 years).

38 We use the median adjusted associate salary for our sample of fifty-two states measured as of 1997 (equal to $103,410).

exert more effort when they have more to lose and when their bosses (as opposed to their agents who may care about other things) are watching.

100 To provide a multivariate test of the mean comparison tests of Table 4, we replace the Adjusted Associate Justice Salary variable with an indicator variable for salaries greater than the median Adjusted Associate Justice Salary for our sample states (Big Salary). We also add interaction terms between the method of judge selection (Election Partisan, Election Non-Partisan, and Merit) and Big Salary to Model 1 of Table 3. Unreported, Big Salary does not correlate with increased productivity in general or when interacted with the method of judge selection. We similarly add Big Salary and interaction terms between Big Salary and the Tenure variable to Model 2 of Table 3. Unreported, Big Salary now does correlate with increased productivity at the <1 percent significance level. In contrast, the interaction term between Big Salary and Tenure is negative and significant at the <1 percent level). Evidence exists therefore that where judges face competitive pressure (shorter tenure), greater salary levels correlate with increased effort compared with where judges face less competitive pressure (higher tenure).

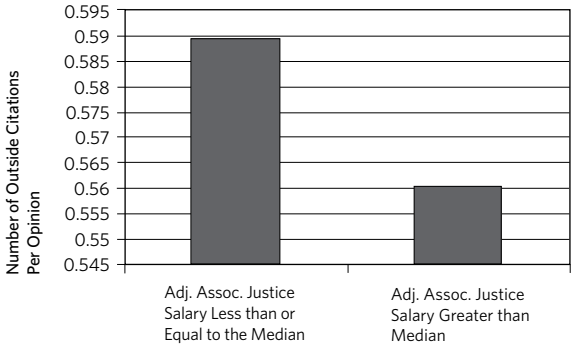
4.5. Quality

101 The number of outside state citations per opinion is our measure of opinion quality. Outside state citations include outside federal court citations (including U.S. Supreme Court citations) and outside state court citations. We measure the number of outside state citations using the LEXIS Shepard's service up until January 1, 2007. Figure 3 compares mean outside citations for judges paid more and less than the median salary. Judges paid less generate more citations than those paid more. The t-test of the difference is not significant, but at this stage it does not appear that higher salaries are inducing higher quality opinions.

102 What about other factors? The following model using ordinary least squares and standard errors clustered by judge is estimated.

$$\begin{aligned}\ln(1+\text{Outside State Citations}_i) = {} & \alpha + ß_{1i}\text{Adj. Assoc. Justice Salary} \\ & + ß_{2i}\text{Adj. Partner Salary} + ß_{3i}\text{Election_Partisan} \\ & + ß_{4i}\text{Election_Non-Partisan} + ß_{5i}\text{Merit_Plan} \\ & + ß_{6i}\text{Number Dissents} + ß_{7i}\text{West Key Pages} \\ & + ß_{8i}\text{Opinion Length} + \sum ß_{ji}\text{Subject Matter}_{ji} \\ & + \sum ß_{ki}\text{Court Characteristics}_{ki} \\ & + \sum ß_{li}\text{Judge Controls}_{li} + \sum ß_{mi}\text{State Controls}_{mi} \\ & + \text{Year Effects} + \varepsilon_i\end{aligned}$$

Figure 3: Outside Citations



The model relates the number of outside state citations (Outside State Citations) for any specific majority opinion with measures for salary and opportunity costs.

Because this model is estimated at the opinion level, it includes Opinion Characteristic controls. These include the number of dissents written against the majority opinion in question (Number of Dissents). A majority opinion with one or more dissents may deal with more novel issues of law and generate more citations as a result. The model includes the number of west key pages (West Key Pages) as a rough measure of the topical importance of the opinion.[39] Similarly the model includes the length of the opinion (Opinion Length); longer opinions are more likely to contain analysis that other judges may cite compared with shorter opinions, all other things being equal. To control for the disproportionate effect that cases in certain subjects might have (and some states may receive more of these), we include subject matter fixed effects for twelve different subject matter categories, including Administrative, Attorney and Client, Capital Punishment, Church and State, Commercial, Criminal, Family, First Amendment, Labor, Property, Rights, and Torts.[40] The category of Other opinions is the baseline.

39 An issue with using this variable is that it may dampen the effect that we are trying to measure, since both the left and right hand side now have variables that measure opinion quality. What we are trying to separate out, though, is the effect of judge quality from the effect of having hit a number of important topics (or having had the luck to get a case that raises numerous important issues first). And the West Key Note variable is aimed at doing that.

40 The subject matter controls are chosen as a function of the subjects that are often suggested in the literature as being most salient to judges, both on the state and federal courts. *See* Choi

Table 5: Quality

	Model 1	Model 2
Dependent Variable	ln(Total Opinions)	ln(Total Opinions)
Independent Variables		
Adjusted Associate Justice Salary	0.001 (1.430)	0.000 (0.630)
Adjusted Partner Salary	0.000 (0.180)	0.000 (-0.610)
Election Partisan	-0.036 (-1.410)	
Election Non-Partisan	0.008 (0.350)	
Merit Plan	-0.058* (-2.330)	
Tenure		0.000 (-0.160)
Stable Court	-0.050** (-2.890)	-0.039* (-2.200)
Number of Active Judges	-0.021** (-4.210)	-0.017** (-3.080)
No Mandatory Retirement	-0.002 (-0.120)	0.001 (0.040)
Long-Term Clerk	-0.086** (-5.460)	-0.081** (-5.140)
Number of Clerks Per Judge	-0.011 (-0.910)	-0.010 (-0.790)

104 Court Characteristic variables and Judge and State Controls are used in the model. Because we count citations up until January 1, 2007, precisely when a judge retires is important. A judge who retires in 2000 may find his influence wane immediately after retirement, leading to fewer citations of his opinions by 2002. In contrast, a judge who retires in 2004 may not see as great a reduction in citations in our count until January 1, 2007. To control for this possibility, instead of a single Retirement Close variable, we use a set of more specific indicators for whether retirement occurred in 2001 or earlier, 2002, 2003, 2004, or 2005. Year fixed effects are also estimated.

& Gulati (2008); McLeod (2007); Champagne (2005).

	Model 1	Model 2
Dependent Variable	ln(Total Opinions)	ln(Total Opinions)
Independent Variables		
Law Clerk Opportunity Cost	0.003** (5.320)	0.002** (4.710)
ln(Trial Cases in the State)	-0.000 (-0.000)	0.000 (0.040)
Intermediate Appellate Court	0.020 (0.720)	-0.017 (-0.620)
Mandatory Publication	-0.014 (-0.880)	-0.015 (-0.840)
Constant	-6.469** (-6.670)	-5.787** (-6.430)
Year Fixed Effects	Yes	Yes
Opinion Characteristic Controls	Yes	Yes
Judge Controls	Yes	Yes
State Controls	Yes	Yes
Subject Matter Controls	Yes	Yes
N	18321	18321
Adj. R2	0.1341	0.1327

The t-statistics (in parentheses) are calculated using standard errors clustered by judge. Variable definitions are in the Appendix.
+ Coefficient significant at the 10 percent level or less.
* Coefficient significant at the 5 percent level or less.
** Coefficient significant at less than the 1 percent level.

Model 1 of Table 5 reports results. As before, we re-estimate Model 1, replacing the selection method for judges with the average tenure of judges in a particular state (Tenure). The results of the model with the Tenure variable are reported as Model 2. 105

The coefficients on both the salary and opportunity cost measures are small and statistically insignificant. For Model 1, a $10,000 increase in salary correlates with only a 1 percent increase in the number of outside state citations per opinion. Salary does not appear to make a difference in determining quality. 106

As with effort, other factors correspond to more significant changes in the number of citations. Judges in Merit Plan states have lower levels of 107

outside state citations (corresponding to 5.8 percent fewer citations respectively compared with Appointed judge states respectively in Model 1). Judges from stable courts correlate with fewer outside state citations and the number of active judges correlates with reduced outside state citations in Models 1 and 2. The presence or absence of mandatory retirement appears to make no significant difference to the quality.[41]

108 On law clerks, shorter-term clerks correlate with 8.6 percent and 8.1 percent greater outside state citations per opinion compared with long-term clerks in Models 1 and 2. Judges with shorter-term clerks not only produce more opinions (reported in Table 3), they produce opinions of higher quality.[42] Surprisingly, the opportunity cost of being a clerk shows up with a positive (albeit small) coefficient in both models. Perhaps there is an inverse correlation between the quality of life at a job at a law firm and the salary that the law firm pays (with the biggest cities obviously paying more and having jobs that are more unpleasant). But it is puzzling that the sign on the coefficient is positive for the quality analysis and was negative for the effort analysis.[43]

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41 Unreported in Table 5, it does not make a significant difference to quality as to whether a judge's primary prior profession was in the private sector.

42 As a robustness test, we added an additional indicator variable for California clerk separate from the long-term clerk variable (with non-long-term clerks as the base category). The coefficients on long-term clerk remained negative and significantly different from zero; the coefficient on California clerk was positive but not significantly different from zero (indicating a similar effect for California clerks as short-term clerks on outside citations). The coefficient on the judicial salary variable was not significantly different from zero.

43 The regression models in Table 5 utilize an indicator variable for whether the state has a mandatory publication rule for opinions to control for workload differences among the states. As a robustness test, we omit the Mandatory Publication indicator variable from the Model 1 in Table 5 and re-estimate the model instead with the addition of indicator variables for whether the state high court has mandatory jurisdiction over civil (Mandatory Civil Jurisdiction) or criminal (Mandatory Criminal Jurisdiction) cases. Unfortunately, we lack information on these variables for all of our states and only use them for robustness tests. Unreported, the coefficient on associate justice salary is positive but not significantly different from zero.

As a robustness test, we replace ln(Outside Citations) with the log of one plus the number of law review citations to an opinion (ln(1+Law Review Citations)) in Model 1 of Table 5. Using this alternative specification, we obtain a stronger relationship between salary and law review citations compared with Table 5. Higher associat justice salary correlates with significantly more law review citations. The coefficient on associate judge salary, nonetheless, is small in magnitude (0.003). A $10,000 pay increase correlates with only a 3 percent increase in the number of law review citations.

We re-estimate Model 1 with the addition of an indicator variable for whether the judge also

Table 6: Outside Citation/Quality Comparison

	Adj. Assoc. Justice Salary Less than or Equal to Median	Adj. Assoc. Justice Salary Greater than Median	p-value
Election Partisan	0.848	0.591	0.000
Election Non-Partisan	0.636	0.667	0.471
Merit	0.709	0.820	0.021
Appointed	0.720	1.153	0.000
Tenure Less than or Equal to Median	0.750	0.730	0.617
Tenure Greater than Median	0.650	0.782	0.000
Not Lifetime Employment	0.695	0.730	0.203
Lifetime Employment	0.724	1.983	0.000

Table 6 compares the mean number of outside state citations per majority opinion for judges who make less than or equal to the median salary

teaches as an adjunct professor (Professor). Unreported, the model produces the same qualitative results as Model 1. The coefficient on judicial salary is positive but not significantly different from zero. The coefficient on Professor is negative and significant at the 10 percent level. Apparently those judges who spend time as adjunct professors produce lower quality opinions.

We next re-estimate Model 1 with the addition of an indicator variable for whether the judge comes from a state that gives its high court judges lifetime employment (Lifetime). Unreported, the model produces the same qualitative results as Model 1. The coefficient on judicial salary is positive but not significantly different from zero. The coefficient on Lifetime is positive and insignificant.

We also re-estimate Model 1 without the Opinion Length variable. Longer opinions may also indicate higher quality opinions. By controlling for opinion length, we may artificially reduce correlation between citations and overall opinion quality in our model. In the model without the Opinion Length variable, the coefficient on associate justice salary is positive but not significantly different from zero.

Lastly, we re-estimate Model 1 with the addition of indicator control variables for whether the state is a member of U.S. Census region Mid-West, Northeast, or South (using West as the base case). Unreported, the coefficient on associate justice salary is positive and insignificant.

Overall, our robustness tests reveal that the relationship between higher judicial salaries and opinion quality is non-existent. In our various tests, the correlation is not statistically significant.

and for judges who make greater than the median salary.[44] This comparison is made for (a) the different selection mechanisms for judges, (b) states where judges have less than or equal to the median tenure versus states with judges with greater than the median tenure,[45] and (c) states with lifetime employment for judges versus states without lifetime employment.

110 Greater compensation correlates with fewer citations for Election Partisan states. In contrast, greater compensation correlates with *more* citations for Election Non-Partisan, Merit, and Appointed judge states (although the difference for Election Non-Partisan judges is not significant). Similarly, greater compensation does not correlate with a change in citations for judges with relatively shorter tenures; for judges with longer tenure, more pay correlates with more citations. Higher salary also correlates with increased citations for lifetime employment states (although not for non-lifetime employment states). In sum, while greater salary correlates with higher quality opinions where judges face a lower retention risk, greater salary does not correlate with higher quality opinions where judges are at greater retention risk (and indeed, correlates in some instances with lower quality opinions). Maybe judges who face the risk of job loss focus their energies on the metrics of performance (number of opinions authored) that are easily observed by those deciding whether to hire or fire them (the populace, in an election state). Less observable measures, such as the quality of opinions are ignored — except by those who do not have to please the voters. This pattern is consistent with multitasking theory, which predicts that agents will exert effort with respect to measurable types of performance and shirk with respect to other types of performance.

111 To provide a multivariate test of the comparison tests of Table 6, we replace the Adjusted Associate Justice Salary variable with an indicator variable for salaries greater than the median Adjusted Associate Justice Salary for our sample of fifty-two states (Big Salary). We also add interaction terms between the method of judge selection (Election Partisan, Election Non-Partisan, and Merit) and Big Salary to Model 1 of Table 5. We similarly add

44 We use the median adjusted associate salary for our sample of fifty-two states measured as of 1997 (equal to $103,410).

45 We use the median tenure of justices for our sample of fifty-two states measured as of 1997 (equal to 7.75 years).

Big Salary and interaction term between Big Salary and the Tenure variable to Model 2 of Table 5. Unreported, these additional multivariate tests provide similar qualitative results to those of our mean comparisons in Table 6. Higher salary correlates with more outside state citations in Appointed, Election Non-Partisan, and Merit states (although the increase is significant only for Appointed and Election Non-Partisan states). In Partisan Election states, however, the effect of higher judicial salaries on the number of outside state citations is negative but not significantly different from zero. The relationship of salary with outside state citations per opinion varies with the particular judicial selection system. Where judges face more competition for their jobs (proxied by partisan elections), increased salary does not correlate with more citations per opinion. If anything, higher salary results in a low quality product.

We find in our multivariate test with the Tenure variable that high salary 112
correlates with greater outside state citations. In contrast with our summary statistic comparison above, the coefficients on the Tenure variable and the interaction between Tenure and Big Salary are not significantly different from zero. In sum, employment conditions appear to matter in determining whether salary makes a difference in judicial quality—although the precise relationship between higher salaries and increase opinion quality is not straightforward (and may turn negative for certain regimes including in particular states where judges are elected and face retention pressure).

4.6. Independence

We use a regression model to examine the relationship between our Inde- 113
pendence measure and the judicial salary and opportunity cost measures. The following equation on pooled data from 1998 to 2000 is estimated using an ordinary least squares model with standard errors clustered by state:

$$\begin{aligned}\text{Independence}_i = {} & \alpha + ß_{1i}\text{Adj. Assoc. Justice Salary} + ß_{2i}\text{Adj. Partner Salary} \\ & + ß_{3i}\text{Election_Partisan} + ß_{4i}\text{Election_Non-Partisan} \\ & + ß_{5i}\text{Merit_Plan} + ß_{6i}\text{Opensecrets} + ß_{7i}\text{Number of Dissents} \\ & + \sum ß_{ji}\text{Subject Matter}_{ji} \\ & + \sum ß_{ki}\text{Court Characteristics}_{ki} \\ & + \sum ß_{li}\text{Judge Controls}_{li} + \sum ß_{mi}\text{State Controls}_{mi} \\ & + \text{Year Effects} + \varepsilon_i\end{aligned}$$

Pooled versions of the Court Characteristic variables and Judge and State Controls are used in the model. As a control for subject matter composition of the pool, the number of majority opinions that deal with the subject matter divided by the number of majority opinions for the state in the 1998 to 2000 time period is used. Two new variables are added. The first, Opensecrets, is an indicator variable defined as 1 if the judge in question contributed money to a political candidate as tracked by the opensecrets.org website. Those who contribute their personal resources to the political campaigns of others are likely more partisan in their political beliefs. The second, number of dissents, is defined as the total number of dissents the judge in question authored during the 1998 to 2000 time period. Number of Dissents controls for the possibility that high levels of dissenting behavior might correlate with independence. We exclude judges from states where all judges in our sample were of the same political party from the analysis (Georgia, Maryland, New Mexico, South Carolina, South Dakota). The model is reported in Table 7 as Model 1; Model 2 reports the model with the substitution of average judge tenure (Tenure) instead of the judicial selection system variables (Election Partisan, Election Non-Partisan, and Merit Plan).

114 Table 7 does not support the view that higher salaries leads to more independent judges. In Models 1 and 2, the coefficient on judicial salary is insignificant. The opportunity cost measure in Models 1 and 2 (Adjusted Partner Salary) is significant and positive, suggesting that judges who give up more might be more independent. But the small coefficient suggests that the magnitude of the effect is not large.

115 In terms of institutional design and other factors that might affect independence levels, we see that having a stable court has little impact on independence and that the presence of mandatory retirement does not reduce independence (if anything, increases it).[46] Long-term clerks also again appear to produce no positive effects (if anything, negative effects).[47] The number of clerks per judge does positively correlate with independence.

46 Unreported in Table 7, having one's primary prior profession in the private sector does not correlate with higher independence either.

47 As a robustness test, we added an additional indicator variable for California clerk separate from the long-term clerk variable (with non-long-term clerks as the base category). The coefficients on long-term clerk and California clerk were both negative and not significantly different from zero. The coefficient on the judicial salary variable was not significantly different from zero.

We re-estimate Model 1 using a logit model with the use of Indep01 as 116
the dependent variable, defined to equal one if Independence is greater or equal to zero and zero otherwise. Indep01 is less vulnerable to the critique that the range of our Independence measure may vary based on the underlying political composition of a high court. In contrast, the Indep01 provides less information than our Independence measure, treating all judges with a positive or zero Independence score as the same (and likewise for all judges with a negative Independence score). Model 3 of Table 7 reports that with Indep01 as the dependent variable, judicial salary is not correlated with independence. We therefore find no evidence that higher salaries helps improve independence.[48]

Table 8 compares the Independence score for judges who make less than or 117
equal to the median salary and for judges who make greater than the median

48 The regression models in Table 7 utilize an indicator variable for whether the state has a mandatory publication rule for opinions to control for workload differences among the states. We lack data for these variables for all our states. As a robustness test, we omit the Mandatory Publication indicator variable from the Model 1 in Table 7 and re-estimate the model instead with the addition of indicator variables for whether the state high court has mandatory jurisdiction over civil (Mandatory Civil Jurisdiction) or criminal (Mandatory Criminal Jurisdiction) cases. Unfortunately, we lack information on these variables for all of our states and only use them for robustness tests. Unreported, the coefficient on associate justice salary is positive but not significantly different from zero.

We also re-estimate Model 1 with the addition of an indicator variable for whether the judge also teaches as an adjunct professor (Professor). Unreported, the model produces the same qualitative results as Model 1. The coefficient on judicial salary is positive but not significantly different from zero. The coefficient on Professor is negative and not significant.

We re-estimate Model 1 with the addition of an indicator variable for whether the judge comes from a state that gives its high court judges lifetime employment (Lifetime). Unreported, the model produces the same qualitative results as Model 1. The coefficient on judicial salary is positive but not significantly different from zero. The coefficient on Lifetime is positive and significant at the 10 percent level. Some evidence exists therefore that judges with lifetime employment may act more independently of party affiliation.

We re-estimate Model 1 with the omission of the number of dissents variable. While our independence measure is not necessary correlated with the number of dissents, the construction of the independence variable does, in part, focus on the number of opposite party dissents (in the numerator) and the total number of dissents (in the denominator). Unreported, the coefficient on judicial salary is again negative and not significantly different from zero.

Lastly, we re-estimate Model 1 with the addition of indicator control variables for whether the state is a member of U.S. Census region Mid-West, Northeast, or South (using West as the base case). Unreported, the coefficient on associate justice salary is positive and insignificant.

Overall, our robustness tests reveal that the higher judicial salaries do not correlate with increased judicial independence.

Table 7: Independence

	Model 1	Model 2	Model 3
Dependent Variable	Independence	Independence	Indep01
Independent Variables			
Adjusted Associate Justice Salary	0.000 (-0.290)	-0.001 (-0.560)	-0.010 (-0.490)
Adjusted Partner Salary	0.002** (3.580)	0.002** (3.270)	0.020** (3.340)
Election Partisan	-0.060 (-0.900)		-1.173 (-1.150)
Election Non-Partisan	-0.165+ (-1.910)		-3.215** (-2.910)
Merit Plan	-0.173* (-2.050)		-3.558** (-2.900)
Tenure		-0.004 (-0.580)	
Number of Dissents	0.002 (1.300)	0.002 (1.400)	0.002 (0.080)
Opensecrets	-0.046 (-1.370)	-0.041 (-1.230)	-0.596 (-1.220)
Stable Court	0.083 (1.010)	-0.009 (-0.100)	2.414** (2.880)
Number of Active Judges	0.006 (0.380)	-0.010 (-0.480)	0.420* (2.150)
No Mandatory Retirement	-0.080+ (-1.760)	-0.036 (-0.770)	-0.659 (-1.100)
Long-Term Clerk	-0.043 (-1.340)	-0.059* (-2.110)	0.774** (2.680)

salary.[49] This comparison is made for (a) the different selection mechanisms for judges, (b) states where judges have less than or equal to the median tenure versus states with judges with greater than the median tenure,[50] and (c) states with lifetime employment for judges versus states without lifetime em-

49 We use the median adjusted associate salary for our sample of fifty-two states measured as of 1997 (equal to $103,410).

50 We use the median tenure of justices for our sample of fifty-two states measured as of 1997 (equal to 7.75 years).

	Model 1	Model 2	Model 3
Dependent Variable	Independence	Independence	Indep01
Independent Variables			
Number of Clerks Per Judge	0.080** (3.140)	0.075** (3.170)	0.251 (0.730)
Law Clerk Opportunity Cost	-0.002+ (-1.730)	0.000 (-0.060)	-0.071** (-5.810)
ln(Trial Cases in the State)	-0.072* (-2.640)	-0.014 (-0.680)	-0.835* (-2.030)
Intermediate Appellate Court	0.049 (0.810)	-0.010 (-0.180)	0.517 (0.510)
Mandatory Publication	0.014 (0.240)	-0.066 (-1.190)	2.493** (3.290)
Constant	3.035 (1.200)	2.352 (0.870)	93.040** (3.000)
Year Fixed Effects Judge Controls	Yes Yes	Yes Yes	Yes Yes
State Controls Subject Matter Controls	Yes Yes	Yes Yes	Yes Yes
N Adj R2	324 0.1295	324 0.1220	309 0.1999

The t-statistics (in parentheses) are calculated using standard errors clustered by state. Variable definitions are in the Appendix. The logit model for the INDEP01 binary dependent variable reports the Pseudo R2 instead of the Adjusted R2.

+ Coefficient significant at the 10 percent level or less.

* Coefficient significant at the 5 percent level or less.

** Coefficient significant at less than the 1 percent level.

ployment. As in Table 7, we exclude judges from states where all judges in our sample were of the same political party from the analysis.

Greater compensation does not generally correlate with a difference in independence levels for the different systems of selecting judges. Similarly, greater compensation does not correlate with independence regardless of a judge's tenure. Higher salary does not correlate with greater independence for judges from non-lifetime or lifetime employment states. As a robustness check, we redo the mean comparisons in Table 8 using the INDEP01 binary

form of our independence measure. Unreported, we again find no differences in independence level.

119 To provide a multivariate test of the mean comparison tests of Table 8, we replace the Adjusted Associate Justice Salary variable with an indicator variable for salaries greater than the median Adjusted Associate Justice Salary (Big Salary). We also add interaction terms between the method of judge selection (Election Partisan, Election Non-Partisan, and Merit) and Big Salary to Model 1 of Table 7. Unreported, Big Salary does not correlate with increased independence in general or when interacted with the method of judge selection. We similarly add Big Salary and interaction terms between Big Salary and the Tenure variable to Model 2 of Table 7. Unreported, Big Salary does not correlate with increased independence in general or when interacted with Tenure.

Table 8: Independence Summary Statistics

	Adj. Assoc. Justice Salary Less than or Equal to Median	Adj. Assoc. Justice Salary Greater than Median	p-value
Election Partisan	-0.002	-0.030	0.434
Election Non-Partisan	-0.029	-0.091	0.171
Merit	-0.023	-0.034	0.723
Appointed	-0.035	-0.018	0.784
Tenure Less than or Equal to Median	-0.034	0.027	0.816
Tenure Greater than Median	-0.019	-0.055	0.190
Not Lifetime Employment	-0.036	-0.043	0.742
Lifetime Employment	0.050	-0.038	0.234

120 In sum, the relationship of salary with our Independence measure does not vary with the particular judicial selection system. We also find no evidence that independence and salary are related depending on the amount of risk a judge faces in retaining her position (as reflected in the selection system and Tenure variables).

4.7. Other Measures of Judicial Quality

121 We focus on productivity, opinion quality, and independence as measures of

judicial quality. Other ways to measure judicial quality are possible. One that has been suggested to us on multiple occasions at workshops is that the ranking of the law school which a judge attended may act as a proxy for the quality of the judge. Such a measure treats judges who graduated from a top law school as more capable than judges who graduate from lesser law schools; a plausible but questionable proposition. Nevertheless, we question whether higher salaries result in judges who graduated from higher ranked law schools.

122 To test the relationship between higher ranked law schools and judicial salaries, we estimated a logit model using judge level data and an indicator dependent variable for whether the judge graduated from a top ten law school as measured in the U.S. News Rankings for 2005 (Law School Ranking). For independent variables, we use the pooled versions of the state, court, and judge controls used in the Independence model (see Table 7). Unreported, the coefficient on associate justice salary is negative but not significantly different from zero.

123 To assess whether higher salaries may have a differential effect depending on the method of judge selection, we re-estimate our logit Law School Ranking model, replacing the Adjusted Associate Justice Salary independent variable with an indicator variable for salaries greater than the median Adjusted Associate Justice Salary (Big Salary). We also add interaction terms between the method of judge selection (Election Partisan, Election Non-Partisan, and Merit) and Big Salary. Unreported, we find that none of the Big Salary, the method of judge selection, and the interaction variables are significantly different from zero.

5. CONCLUSION

124 Our results defy easy description. By the same token they demonstrate that the debate about judicial salaries reflects an excessively simple picture of the judicial market.

125 At the level of theory, the claim that increasing the salaries of judges will improve judicial output, while holding the other aspects of the judges' position constant, has little support. Higher salaries should not improve the incentives of judges unless judges can be fired or otherwise punished for inadequate performance—which they often cannot. Higher salaries might increase the pool of people willing to be judges, but they also might encour-

age people who lack a judicial temperament to enter that pool, and give politicians better incentives to offer patronage rather than to appoint good judges. Further, it is a mistake to look at salaries in isolation from the other elements of judicial compensation—benefits, job security (for federal and some state judges), prestige, and power—and the general legal and political environment (type of selection system, political competition, caseload, assistance from clerks and staff). Comparisons of the salaries of judges and law firm associates are for this reason likely to be misleading. Finally, when high-quality people become judges, they must leave the private sector, vacating jobs where they may have a relatively high value compared with their value as judges. Judges should be paid their social value, and not more, but no one knows whether their social value is higher or lower than their current salary.

126 The empirical results provide some support for salary advocates in those states where judges face a meaningful risk of termination, as theory would predict. But they do not provide much support in the area that has been the epicenter of the debate—the federal judiciary, where judges are appointed and have life tenure. If our empirical results carry over from the state courts to the federal judiciary, we would predict that increasing the salary of federal judges will not increase their productivity or independence. It *might* increase the quality of their opinions. The most plausible mechanism for this effect is that of selection: higher quality people would be willing to serve, and the appointment system, which involves both the president and the senate, screens out low-quality candidates (Lindstadt, Segal & Westerland 2006; but *cf.* Lott 2005). However, given the lack of robustness of our findings, such a conclusion is premature.

127 Thus, the case for increasing the salaries of federal judges is not particularly strong. Even if increasing salary increases the quality of opinions, we would still need to know whether the social value of this extra quality is worth the price. Proponents of salary increases need to show that this is true. In addition, as we have seen, there may be better ways of improving the quality of judges—for example, by restricting judges to the use of short-term clerks. Or it might be better to have more low-paid judges than to have fewer highly paid judges. More work needs to be done before the relationship between salary and judicial quality is understood.

128 We should acknowledge once again that our measures of judicial quality

are imperfect. Many aspects of judicial performance might be unobservable or better captured with different variables. However, salary advocates who do not accept our measures should produce better measures. Otherwise, there is just no evidence that justifies salary increases.

129 In informal discussions, judges have advanced a number of criticisms of our argument. Some judges argue that our theory fails to capture the real reason why the failure to raise salaries will produce a diminishment of judicial productivity. They argue that judges fear that the credibility of the system and its legitimacy will be undermined if the majority of lawyers and even court administrators end up being paid more than the judges, who are supposed to be at the top of the hierarchy. Litigators and litigants will simply not respect judges who are not highly paid. Another argument we have heard is that Congress has breached its implicit contract with the judiciary to give it regular COLA raises, raises the federal judges did not receive for a long period in the 1990s and that afterwards only received intermittently in the form of "diet COLAs." Pay has also not kept up with the increase in the case load, which has resulted in more work and worse working conditions. On the empirics, judges disagree with our measures of judicial production. Alternate measures suggested include reversal rates, estimates of case backlogs, and appointments to high level judicial reform committees. One judge argued that judges who need to earn additional money might have to work as adjunct professors and that would take time away from their work as judges. We tested this proposition and found that there was indeed a negative correlation (albeit significant only at the 10 percent level) between quality and having taught as an adjunct.[51] Testing the other hypotheses would require additional work, which would raise new theoretical and empirical questions. We hope that we and other scholars will be able to address them in future research.

130 A final point is that some scholars use judicial salary as a proxy for aspects of judicial quality in cross-national regressions (Feld & Voigt 2003). Our results show that in the United States the correlation between salary and judicial quality is not strong, and therefore suggest that the use of salary as a proxy for quality may be inadvisable.

51 *See* note 42.

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APPENDIX 1

Key Variable Definitions

Variable	Definition
Total Opinions	Total number of majority, concurring, and dissenting opinions authored by a particular judge in one year (ranging from 1998 to 2000).
Outside State Citations	Total number of citations from (1) federal courts outside the federal circuit that includes the state in question and (2) courts in other states. Citations are measured in opinions authored up until January 1, 2007 (as tracked in the LEXIS Shepard's database).
Opposite_Party	The total number of opposing opinions written against an opposite party judge divided by the total number of opposing opinions written against either a judge of the opposite or same party as the judge in question for the 1998 to 2000 time period. Opposing opinions include dissents written against a majority opinion and majority opinions where a dissenting opinion exists.
Opposite_Pool	Total number of majority opinions written by the high court judges of the opposite political party (from the perspective of the judge in question) divided by the total number of majority opinions written by judges of both the same and opposite parties from 1998 to 2000.
Independence	Defined as Opposite_Pool minus Opposite_Party. A more negative Independence score occurs when Opposite_Pool < Opposite Party, indicating an increased tendency to write an opposing opinion against an opposite party judge. Conversely, a more positive Independence score indicates a decreased tendency to write an opposing opinion against an opposite party judge.
Election Non-Partisan	Indicator variable equal to 1 if the state uses a non-partisan election to select high court justices and 0 otherwise.
Election Partisan	Indicator variable equal to 1 if the state uses a partisan election to select high court justices and 0 otherwise.
Merit Plan	Indicator variable equal to 1 if the state follows the Missouri Merit Plan or a variant (including the Tennessee Plan) to select high court justices and 0 otherwise.
Tenure	The average tenure of high court judges for the state in question, measured as of the spring of 1997 (from Hanssen 1999, Table 1).
Number of Dissents	Indicator Variable equal to 1 if the judge authoring an opinion is Republican and 0 otherwise.
West Key Pages	Number of pages in an opinion associated with the West key pages section (as provided in the West reporter version of the opinion and tabulated on Westlaw).

Opinion Length	Number of pages from the start of the opinion to the end of the opinion as provided in the West reporter version of the opinion and tabulated on Westlaw. For majority opinions, we measured from where the authoring judge's actual opinion starts to the end of the majority opinion.
Open Secrets	Indicator Variable equal to 1 if the judge authoring the opinion in question has donated to a political candidate and 0 otherwise. Political contributions are tracked by www.opensecrets.org and include Federal Election Commission records of receipts from all individuals who contribute at least $200 from 1992 to 2006.

APPENDIX 2

Judge-Level Variable Definitions

Variable	Definition
Chief Judge	For year-level data, indicator variable equal to 1 if the judge in question is the chief judge of the court in the year in question and 0 otherwise. For pooled data, indicator variable equal to 1 if the judge in question is the chief judge of the court for any year from 1998 to 2000 and 0 otherwise.
Court Experience	For year-level data, the difference between the year in question and the year the judge first joined the high court. For pooled data, the difference between 1998 and the year the judge first joined the high court (if the judge started on the court in 1998 or later court experience is set to 0).
Post-Law School Experience	The difference between 1998 and the year the judge graduated law school.
Retirement Close	Indicator variable equal to 1 if the judge in question retired from the bench in 2001 or earlier and 0 otherwise.
Age	Age of the judge in years.
Female	Indicator variable equal to 1 if the judge is female and 0 if male.
Private Practice	Indicator variable equal to 1 if the judge had private practice experience before becoming a judge and 0 otherwise.
Election Spending	Indicator variable equal to 1 if the judge raised funds relating to election campaign expenditures for the current year and 0 otherwise.
PAJID Score	PAJID score for each judge as developed by Brace, Hall & Langer (2001). These scores locate judges on a political continuum from highly conservative (0) to highly liberal (100).

APPENDIX 3

Court-Level Variable Definitions

Variable	Definition
Adjusted Associate Justice Salary	For year-level data, the associate justice salary reported in the prior year for the state (so 1997 for 1998 judge-level data) divided by a cost of living adjustment for the year in question measured for the metro area in which the high court of the state is located. For pooled data, the associate justice salary reported in 1997 divided by the cost of living adjustment for 1998 (in thousands of dollars).
Adjusted Partner Salary	For year-level data, the average partner salary reported for the year in question for the state divided by a cost of living adjustment for the year in question measured for the metro area in which the high court of the state is located. For pooled data, the average partner salary in 1998 divided by the cost of living adjustment for 1998 (in thousands of dollars).
Stable Court	Indicator variable equal to 1 if the state high court justices stayed the same from 1998 to 2000 and 0 otherwise.
Number of Active Judges on Bench	Number of judges who were active at any time from 1998 to 2000 for the state in question.
No Mandatory Retirement	Indicator variable equal to 1 if the judges on the state high court do not face mandatory retirement and 0 otherwise.
Long-Term Clerk	Indicator variable equal to 1 if state clerks are tenured for more than one year and 0 if tenure is 1 year or less.
Number of Clerks Per Judge	Average number of clerks per judge in the 1998 to 2000 time period.
Law Clerk Opportunity Cost	The difference between the average salary of an entering associate at law firm in that state and the law clerk salary (in thousands of dollars).
Number of Trial Cases in the State	Number of trial cases in the entire state in 1998 (in thousands).
Intermediate Appellate Court	Indicator variable equal to 1 if the opinion is in opposition to the opinion of another judge in the same case and 0 otherwise. In the case of a dissenting opinion written by the judge in question, the opinion is treated as in active opposition to the majority opinion. In the case of a majority opinion by the judge in question, active opposition exists if the majority opinion is opposed by a dissenting opinion.
Mandatory Publication	Indicator variable equal to 1 if judges on the state high court face a mandatory publication rule and 0 otherwise.

APPENDIX 4

State-Level Variable Definitions

Variable	Definition
State Age	Age of the state. For year-level data this is defined as the difference between the year in question and the year of admission of the state into the United States. For pooled data, this is defined as the difference between 1998 and the year of admission of the state into the United States.
State Population	For year-level data, the population of the state in millions measured in the year prior to the year in question (so the population in 1997 if the data year is 1998). For pooled data, the population of the state in millions measured for 1997.
Border Population	Total population of all bordering states of the state in question (measured as of 1997 in millions).
Crime Index	For year-level data, overall crime rate for the state (including property and violent crime) per 100,000 people from the FBI Crime Report for the year prior to the year in question. For pooled data, the overall crime rate measured for 1997.
Gross State Product	Gross State Product (measured as of 1998 in billion of dollars).
Median Age of Population	Median age of state population (2000 U.S. Census).
State Median Income	Median per capita income of the state population (2000 U.S. Census in thousands of dollars).
Black Population Fraction	Fraction of the population comprised of blacks as obtained from the 2000 Census.
Citizen Ideology Score	Measure of citizen ideology based on election results in each district, which are then used to compute a statewide average (ultimately based on interest group ratings of a given state's federal congressional delegation) (from Berry, Ringquist & Hanson 1998).

APPENDIX 4 (continued)

Subject Matter Categories

Variable	Definition
Administrative	Review of Agency/Government Decision-making (not in another subject matter category). Also includes Government Actions (*e.g.*, State suit to comply with state statute that does not fit in other categories); private actions suing state actors for negligence, etc. (unless the case involves prisoner rights which is included in the "Criminal" category of cases).
Attorney and Client	Attorney Misconduct; Attorney fees (unless fits in one of above categories); Disbarment; Contempt of court order against attorney.
Capital Punishment	Capital Punishment-related actions.
Church and State	Pledge of Allegiance; Funding for Private Religious Schools; Prayer in School; Ten Commandments.
Commercial	Contracts; Insurance; Private arbitration; Creditor v. Debtor; Lessor-Lessee; Usury Laws; Franchise v. Franchisor; Employment Contractual Disputes; Corporate Law; Piercing the Corporate Veil; Tax; Bankruptcy; Enforcement of mechanics lien; Implied warrant of merchantability.
Criminal	Sentencing Guidelines; Prisoners' Rights; Murder; Rape; Drugs/Controlled Substances; Attorney-Client Privilege in Criminal Context; Grand Jury-related; Juvenile Criminals. Excludes Capital Punishment cases.
Family	Divorce; Adoption; Child Support; Probate/Inheritance.
First Amendment	Employment issues (excluding employment contractual disputes); ERISA; National Labor Relations Board (NLRB); Occupational Safety and Health Act (OSHA); Fair Labor Standards Act (FLSA); Wrongful Discharge; Labor Management Relations Act (LMRA); Family and Medical Leave Act (FMLA); Employee Benefits; Worker's Compensation claims; Retaliatory Discharge claims.
Labor	Employment issues (excluding (1) employment contractual disputes that are not Worker's Comp or state administrative wage rate related—these go to "Commercial" and (2) excluding discrimination-type claims that fit in "Civil Rights"); ERISA; NLRB; Occupational Safety and Health Act (OSHA); Fair Labor Standards Act (FLSA); Wrongful Discharge; Labor Management Relations Act (LMRA); Family and Medical Leave Act (FMLA); Employee Benefits; Worker's Compensation claims; Retaliatory Discharge claims; State Wage Rate Claims.
Property	Takings claims; Zoning issues; Property rights; Property Licensing-Related or Permit-Related; Landlord-Tenant-Related.

Rights	Race Discrimination; Sex Discrimination; Affirmative Action; Civil Rights; Age Discrimination; Privacy; Handicap Discrimination; Abortion (includes discrimination in employment context cases); Voting Rights-Voting Related.
Torts	Federal Tort Related Act; Medical Malpractice; Products Liability; Wrongful Death; Libel; etc.
Other	All other cases.

Table 5: Quality

	Model 1	**Model 2**
Dependent Variable	ln(1+Outside Citations)	ln(1+Outside Citations)
Independent Variables		
Adjusted Associate Justice Salary	0.001 (1.430)	0.000 (0.630)
Adjusted Partner Salary	0.000 (0.180)	0.000 (-0.610)
Election Partisan	-0.036 (-1.410)	
Election Non-Partisan	0.008 (0.350)	
Merit Plan	-0.058* (-2.330)	
Tenure		0.000 (-0.160)
Stable Court	-0.050** (-2.890)	-0.039* (-2.200)
Number of Active Judges	-0.021** (-4.210)	-0.017** (-3.080)
No Mandatory Retirement	-0.002 (-0.120)	0.001 (0.040)
Long-Term Clerk	-0.086** (-5.460)	-0.081** (-5.140)
Number of Clerks Per Judge	-0.011 (-0.910)	-0.010 (-0.790)
Law Clerk Opportunity Cost	0.003** (5.320)	0.002** (4.710)
ln(Trial Cases in the State)	-0.000 (-0.000)	0.000 (0.040)
Intermediate Appellate Court	0.020 (0.720)	-0.017 (-0.620)
Mandatory Publication	-0.014	-0.015

	(-0.880)	(-0.840)
Constant	-6.469**	-5.787**
	(-6.670)	(-6.430)

The t-statistics (in parentheses) are calculated using standard errors clustered by judge. Variable definitions are in the Appendix.

+ Coefficient significant at the 10 percent level or less.

* Coefficient significant at the 5 percent level or less.

** Coefficient significant at less than the 1 percent level.