
SHOULD WE PAY FEDERAL CIRCUIT JUDGES MORE?

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According to Chief Justice John Roberts, the most difficult issue facing the federal judiciary is low judicial salaries. His view, shared by other Justices, many federal judges, the American Bar Association, and prominent law school deans, is that low salaries deter many of the most qualified candidates from considering the bench. This Article examines the impact of judicial pay on the

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performance of the federal circuit courts. I exploit variation in the next best financial opportunity for most circuit judges – partnership in a regional law firm – to determine the impact of low judicial salaries. With high judicial salaries, judges give up little money as against their next best opportunity to take the bench. With low judicial salaries, judges give up a lot of money to take the bench. Comparison of the performance of judges with varying “spreads” allows for a prediction about the likely impact of higher judicial salaries. This Article finds that low judicial salaries do not affect the nature of votes in controversial cases, the speed of controversial case disposition, the frequency of citation to outside circuit authority, or the strength of opinions as measured by citation counts. This Article does find, however, that low salaries lead to slightly fewer dissents. This effect, while statistically significant, is nonetheless practically trivial. In short, this Article finds that judicial pay is largely irrelevant to the performance of the circuit courts.

INTRODUCTION

On January 1, 2007, Chief Justice John Roberts released his 2006 annual report on the state of the federal judiciary. In the report, he claimed that inadequate judicial salaries were precipitating a “constitutional crisis.”¹ According to the Chief Justice, the pay gap between federal judges and their counterparts in the private sector was becoming so large that serving on the judiciary was no longer a reasonable option for many highly qualified lawyers. In his 2005 report, the Chief Justice warned that if the pay gap remained too large,

the judiciary will over time cease to be made up of a diverse group of the Nation’s very best lawyers. Instead, it will come to be staffed by a combination of the independently wealthy and those following a career path before becoming a judge different from the practicing bar at large. Such a development would dramatically alter the nature of the federal judiciary.²

¹Chief Justice John G. Roberts, *2006 Year-End Report on the Federal Judiciary*, 39 THE THIRD BRANCH: NEWSLETTER OF THE FEDERAL COURTS (Admin. Office of the U.S. Courts, Wash. D.C.), Jan. 2007, at 1, available at <http://www.uscourts.gov/ttb/jan06ttb/yearend/index.html> [hereinafter *2006 Report*]. The Chief Justice’s remarks are particularly salient because he is the federal judiciary’s spokesman before Congress. On the expanding lobbying role of the Chief Justice, see Judith Resnik & Lane Dilg, *Responding to a Democratic Deficit: Limiting the Powers and the Term of the Chief Justice of the United States*, 154 U. PA. L. REV. 1575, 1611-1613 (2006) (discussing Chief Justice Rehnquist’s role in lobbying against conferring Article III status on bankruptcy judges and against enacting a federal civil rights remedy under the Violence Against Women Act).

²Chief Justice John G. Roberts, *2005 Year-End Report on the Federal Judiciary*, 38 THE THIRD BRANCH: NEWSLETTER OF THE FEDERAL COURTS (Admin. Office of the U.S. Courts, Wash. D.C.), Jan. 2006, at 2-3, available at <http://www.uscourts.gov/ttb/jan06ttb/yearend/index.html> [hereinafter *2005 Report*]. Other justices have also expressed concern about low judicial salaries. See *Fed. Judicial Compensation: Oversight Hearing Before the Subcomm. on the Courts, the Internet, and Intellectual Property of the H. Comm. on the Judiciary*, 110th Cong. 4 (2007) (statement of Justice Samuel Alito) [hereinafter *Fed. Judicial Compensation*, Justice Alito’s testimony] (“Without serious salary reform, the country faces a very real threat to its judiciary.”); *Fed. Judicial Compensation: Oversight Hearing Before the Subcomm. on the Courts, the Internet, and Intellectual Property of the H. Comm. on the Judiciary*, 110th Cong. 1 (2007) (statement of Justice Stephen Breyer).

The Chief Justice's statements – endorsed by prominent law school deans,³ the American Bar Association,⁴ and leading members of the corporate bar⁵ – were correct, at least insofar as they accurately described the large (and growing) pay differential between federal judges and private sector lawyers. In 2005, for example, the average partner in a prominent Chicago-based law firm earned \$2.12 million.⁶ By comparison, the judges of the Seventh Circuit, also based in Chicago, earned \$171,800.⁷

What is less clear, however, is whether the Chief Justice is correct in concluding that this pay gap will “alter the nature of the federal judiciary.” Certainly, Chief Justice Roberts's instinct could very well be right: salary differences might influence who will be willing to join the federal judiciary. Perhaps if judicial pay is relatively low, fewer people will accept the job without accumulating a substantial nest egg beforehand, and some people with college-age children might decline the judgeship altogether. But the fact that some persons may no longer want to serve as federal judges because of pay concerns does not mean that the nature of the federal judiciary will thereby be fundamentally altered. The critical question is not whether judicial salaries affect composition – they might – but whether any resulting change in composition affects the “nature” of the federal judiciary, that is to say, whether relatively low judicial salaries affect the “product” the circuit courts produce.

[hereinafter *Fed. Judicial Compensation*, Justice Breyer's testimony] (“I believe that something has gone seriously wrong with the judicial compensation system.”); *Judicial Security and Independence: Hearing Before the S. Comm. on the Judiciary*, 110th Cong. 7 (2007) (statement of Justice Anthony M. Kennedy) [hereinafter *Judicial Security and Independence*, Justice Kennedy's testimony] (“The current [judicial salary] situation . . . is a matter of grave systemic concern.”); Chief Justice William H. Rehnquist, *2002 Year-End Report on the Federal Judiciary*, 35 THE THIRD BRANCH: NEWSLETTER OF THE FEDERAL COURTS (Admin. Office of the U.S. Courts, Wash. D.C.), Jan. 2003, at 2 (“[T]he need to increase judicial salaries . . . remains the most pressing issue [facing the judiciary].”). The justices' sentiments reflect those of the Volcker Commission – a commission set up by Congress to study compensation for government employees. See NAT'L COMM'N ON THE PUB. SERV., URGENT BUSINESS FOR AMERICA: REVITALIZING THE FEDERAL GOVERNMENT FOR THE 21ST CENTURY 23 (2003).

³Letter from Law School Deans to Senator Patrick J. Leahy, Chair, S. Comm. on the Judiciary (February 14, 2007), available at http://www.abanet.org/poladv/priorities/judicial_pay/deansletter.pdf (supporting Chief Justice Roberts's call for an increase in federal judicial compensation).

⁴*Judicial and Exec. Compensation: Hearing Before the Subcomm. on the Fed. Workforce and Agency Org. of the H. Comm. on Gov't Reform*, 109th Cong. 7 (2006) (statement of the American Bar Association) [hereinafter ABA testimony].

⁵Letter from Corporate Counsels to Congressional Leaders Supporting Judicial Pay Increase (February 15, 2007), available at http://www.abanet.org/poladv/priorities/judicial_pay/ltrcorpleaders022007.pdf.

⁶*The AmLaw 100, 2006*, AM. LAW., May 2006, at 165 (reporting 2005 profits per partner at Kirkland & Ellis).

⁷The office of the U.S. Courts provided data on the salaries for federal circuit judges. SALARIES OF FEDERAL JUDGES, ASSOCIATE JUSTICES, AND CHIEF JUSTICE SINCE 1968 1 (2007), <http://www.uscourts.gov/salarychart.pdf> [hereinafter SALARY DATA].

This Article is the first to test the impact of judicial pay on performance of federal circuit judges. By comparing judicial salaries to salaries of the next best financial opportunity for most circuit judges – partnership in regional law firms – this Article finds that judicial compensation is irrelevant to most quantifiable measures of judicial performance. Regardless of the difference between their salary and their next best opportunity, judges of both political parties vote the same in controversial cases; they are equally likely to cite as persuasive authority opinions by judges from the other political party; they decide controversial cases in the same amount of time; and they write equally strong opinions.⁸ Indeed, the only statistically significant effect of low judicial salaries is that judges paid poorly as against their next best opportunity dissent less often in controversial cases. But the magnitude of this effect is tiny. In short, pretty much nothing would happen if Congress decided to raise judicial salaries.

These empirical results make sense. There are very few federal circuit judgeships, and many people want them. Salary, a generous pension, and a number of non-pecuniary perks make the federal circuit judgeship attractive. The president picks his nominee based on his preferences in combination with the views of the senators. The composition and depth of the candidate pool makes little difference. True, someone might turn down the job for financial reasons, but the next person picked will be indistinguishable in his or her eventual judicial performance.

Part I.A sets forth the constitutional structure, statutory scheme, and history of the law governing judicial salaries. Part I.B summarizes the debate about judicial salaries, considering the arguments made for higher salaries. Based on these arguments, Part I.B articulates competing theories about the likely impact of judicial pay on judicial performance. Part II details the statistical methodology used to test the theories. It develops two approaches – judge-to-judge direct comparisons and pool-to-pool comparisons – that can be used to determine whether higher salaries would alter judicial performance. Part III performs the statistical analysis, reporting that judicial pay does not affect the nature of judicial votes in controversial cases, the speed of case disposition in controversial cases, the character of judicial citations in written opinions, or the strength of judicial opinions. Part III does show that judges who give up a lot of money to take the bench dissent less frequently. By inference, then, low judicial pay (i.e., big spreads between judicial pay and private sector pay) yields marginally less dissent. Part IV deals with some potential objections to the analysis, and, finally, there is a brief conclusion.

I. JUDICIAL SALARIES: BACKGROUND AND THEORIES

A. *Constitutional Requirements and Statutory Background*

Article III, Section 1 of the Constitution provides: “The Judges, both of the supreme and inferior Courts, shall hold their Offices during good Behaviour, and shall, at stated Times, receive for their Services, a Compensation, which shall not be diminished during their Continuance in Office.”⁹ The framers

⁸The opinion results border on statistically significant, but the magnitude of the effect is small.

⁹U.S. CONST. art. III, § 1.

wanted to insulate judges from the whims of the legislative branch and, thus, ensure a more independent judiciary.¹⁰ Yet, the framers did not account for inflation. The text of the Constitution prevents Congress from reducing judicial salaries, but it does not require cost of living increases. Without such increases, inflation diminishes the purchasing power of the judicial salary. As many have noted, that is exactly what has happened over the last thirty years – the real salary for federal judges has declined.¹¹

Congress has tackled the problem of judicial salaries a number of times. In 1967, Congress enacted the Postal Revenue and Federal Salary Act.¹² This Act established a commission to review the salary structure of high-level members of the executive, legislative, and judicial branches.¹³ The commission recommended a salary package to the president and the president then decided on salaries, which took effect unless Congress expressly rejected the proposed salary structure.¹⁴ This Act resulted in a large judicial pay increase in its first year, but had little effect on salaries thereafter.¹⁵

In 1975, Congress made its next foray into judicial salaries. The Executive Salary Cost of Living Adjustment Act provided for automatic cost of living adjustments (COLAs) for members of Congress, the executive, and the judiciary.¹⁶ Despite efforts under this Act to make wage adjustments predictable and consistent, Congress often rejected the automatic COLA

¹⁰THE FEDERALIST No. 78 (Alexander Hamilton) (Henry Cabot Lodge ed., 1900). Hamilton writes:

In a monarchy [fixed judicial salaries] is an excellent barrier to the despotism of the prince; in a republic it is a no less excellent barrier to the encroachments and oppressions of the representative body. And it is the best expedient which can be devised in any government, to secure a steady, upright, and impartial administration of the laws.

Id. at 483; *see also* THE FEDERALIST No. 79, at 491 (Alexander Hamilton) (Henry Cabot Lodge ed., 1900) (reflecting on the judicial compensation clause and stating “[i]n the general course of human nature, a power over a man’s subsistence amounts to a power over his will”).

¹¹*See* RICHARD A. POSNER, THE FEDERAL COURTS 21-34 (2d ed. 1996) (illustrating the decline in the real value of judicial salaries); Kristen A. Holt, *Justice for Judges: The Roadblocks on the Path to Judicial Compensation Reform*, 55 CATH. U. L. REV. 513, 515 (2006) (“Inflation has decreased judges’ purchasing power and ability to maintain a constant standard of living.”); Albert Yoon, *Love’s Labor’s Lost? Judicial Tenure Among Federal Court Judges: 1945-2000*, 91 CAL. L. REV. 1029, 1033 fig.1 (2003).

¹²Pub. L. No. 90-206, 81 Stat. 613, 642-45 (1967) (codified in scattered sections of 28 U.S.C.) (outlining provisions for the salaries of federal employees).

¹³*Id.* § 225, 81 Stat. at 642-43.

¹⁴*Id.* § 225, 81 Stat. at 644.

¹⁵*See* AM. BAR ASS’N & FEDERAL BAR ASS’N, FEDERAL JUDICIAL PAY EROSION: A REPORT ON THE NEED FOR REFORM 5 (2001), *available at* <http://www.abanet.org/poladv/fedcomp2003.pdf> (finding that “[t]he [Federal Salary Act] worked as intended in 1969 Unfortunately, that advance was quickly followed by a retreat; judges and other high-level officials were denied salary adjustments for the next six years.”); Yoon, *supra* note 11, at 1036 (speculating that Congress did not raise judicial pay after the first year because “other policy issues gained greater salience”).

increases for itself and the other branches.¹⁷ This rejection – coupled with the rampant inflation of the late seventies – meant that inflation-adjusted judicial salaries fell almost thirty percent during this period.¹⁸

In 1980, a group of federal district court judges, frustrated with the decline in real salaries, filed a lawsuit claiming that Congress violated the constitutional guarantee of undiminished judicial salaries by postponing or repealing previously-enacted automatic COLA adjustments. In *United States v. Will*,¹⁹ the Supreme Court responded by reinstating the COLA increases for two of the four years the judges requested.²⁰ In picking among the COLA increases, the Court distinguished between COLAs that had vested and those that had not. The Court held that “a salary increase ‘vests’ for purposes of the Compensation Clause only when it takes effect as part of the compensation due and payable to Article III judges.”²¹ The upshot of *Will* is that Congress cannot repeal COLA increases after the judges have received them. Congress, however, can repeal a COLA increase that is simply promised, if money has yet to be distributed under that adjustment.

The Ethics Reform Act of 1989 marks the most recent Congressional activity on judicial salaries.²² The Act accomplished three things. First, it standardized the COLA adjustment, tying the inflation adjustment in judicial salary to the adjustment regularly given other federal government employees.²³ Second, the Act fused any Congressional decision about COLA increases for judges with the decision about COLA increases for members of Congress and high-level executive branch officials.²⁴ If Congress approved a COLA increase for the judiciary, it would necessarily approve a COLA increase for itself and executive officials. This tying froze judicial salaries because members of Congress feared voter backlash if they gave themselves a raise.²⁵

Third, and unrelated to the issue of COLAs, the Act gave an immediate forty percent judicial pay bump.²⁶ At the same time, the Act restricted how much judges could earn from non-judicial activities.²⁷ The Act capped the payment for teaching-style services at fifteen percent of the judicial salary.²⁸ Coupled with the ethical restriction on extra-judicial activities, like serving on corporate boards, the cap effectively ensures that federal judges’ income will be limited to their official salary plus some income from teaching.

¹⁶Pub. L. No. 94-82, 89 Stat. 419 (1975) (codified as amended in scattered sections of 2, 5, 28, 31 & 39 U.S.C.) (amending title 39 “to provide for cost-of-living adjustments of Federal executive salaries, and for other purposes”).

¹⁷AM. BAR ASS’N & FEDERAL BAR ASS’N, *supra* note 15, at 5.

¹⁸POSNER, *supra* note 11, at 389-90 tbl.A.1 (listing judicial salaries in current dollars and 1994 dollars).

¹⁹449 U.S. 200 (1980).

²⁰*Id.* at 230.

²¹*Id.* at 229.

²²Pub. L. No. 101-194 §§ 702-705, 103 Stat. 1717, 1767-71 (1989) (codified in scattered sections of 5 & 28 U.S.C.).

²³28 U.S.C. § 461(a)(2) (2000).

²⁴*Id.*

²⁵*See* AM. BAR ASS’N & FEDERAL BAR ASS’N, *supra* note 15, at 3.

²⁶In nominal terms, salaries for federal circuit court judges rose from \$102,500 to \$132,700.

²⁷5 U.S.C. app. 4 §§ 501-502 (2000).

²⁸*Id.*

B. *The Salary Debate*

Most sitting federal judges find the current salary system deplorable.²⁹ Like every other worker, judges want higher wages, at least enough additional cash to cover inflation. There are three arguments conventionally given for raising judicial salaries.

The first argument involves retention. Declining real salaries will result in judges leaving the bench.³⁰ Turnover might affect judicial performance because the exit of a sitting judge creates transition costs. The vacancy has to be filled and the new judge brought up to speed. Until that happens, the other judges carry a heavier workload, straining the circuit court's capacity.³¹ In addition, high turnover is thought to hamper judicial independence.³² Knowing

²⁹*See supra* note 2 and accompanying text; *see also* Frank M. Coffin & Robert A. Katzmann, *Steps Towards Optimal Judicial Workways: Perspectives from the Federal Bench*, 59 N.Y.U. ANN. SURV. AM. L. 377, 384-85 (2003) (opining that "when salary and benefits do not keep pace with inflation, they can deprive judges of stability"); Harlington Wood, Jr., *Judges Forum No.2: "Real Judges,"* 58 N.Y.U. ANN. SURV. AM. L. 259, 264 (2001) (articulating possible benefits of paying judges more). Federal judges have expressed concern about their salary throughout our country's history. Michael J. Frank, *Judge Not, Lest Yee Be Judged Unworthy of a Pay Raise: An Examination of the Federal Judicial Salary "Crisis,"* 87 MARQ. L. REV. 55, 58-69 (2003). Judge Richard Posner is a prominent exception to the chorus of judicial voices calling for higher judicial salaries. According to Judge Posner, "[r]aising salaries would not do a great deal to attract commercial lawyers to judgeships." Posting of Richard Posner to the Becker-Posner Blog, http://www.becker-posner-blog.com/archives/2007/03/judicial_salari.html (March 18, 2007, 08:42 EST). He also suggests a negative effect of higher salaries, stating that "one effect of raising judicial salaries would be to make the job a bigger patronage plum for ex-Congressmen, friends of Senators, and others with political connections, so that the average quality of the applicant pool might actually fall." *Id.*

³⁰*2006 Report, supra* note 1, at 3 ("[M]any judges who must attend to their families and futures have no realistic choice except to retire from judicial service and return to private practice."); *Fed. Judicial Compensation*, Justice Alito's testimony, *supra* note 2, at 21-22 ("[Eighty] percent of judges who left the federal bench did so for other employment and, in most cases, for significantly higher compensation."); *Judicial Security and Independence*, Justice Kennedy's testimony, *supra* note 2, at 9 (remarking that a "present danger" facing the judiciary branch is that "some of our most talented and experienced judges are electing to leave it").

³¹*Panel Warned About Inadequate Pay for Federal Judges*, THE THIRD BRANCH: NEWSLETTER OF THE FEDERAL COURTS (Admin. Office of U.S. Courts, Wash., D.C.), July 2002, at 1, available at <http://www.uscourts.gov/ttb/july02ttb/july02.html> (quoting Justice Breyer).

³²*Fed. Judicial Compensation*, Justice Alito's testimony, *supra* note 2, at 3; *Fed. Judicial Compensation*, Justice Breyer's testimony, *supra* note 2, at 6 ("[A]ny perception that a judicial appointment is a 'stepping stone' . . . would seriously harm the judicial system, for it is at war with judicial independence."); *Judicial Security and Independence*, Justice Kennedy's testimony, *supra* note 2, at 6 ("A judiciary with permanent tenure, with a sufficient degree of separation from other branches of government, and with the undoubted obligation to resist improper influence is essential to the Rule of Law as we have come to understand that term."); *see also* ABA testimony, *supra* note 4, at 2; Letter from Law School Deans, *supra* note 3, at 1.

that they will eventually be leaving the bench, judges might be reluctant to rule against the interests of potential future employers.³³

This argument assumes that declining inflation-adjusted judicial salaries leads to higher turnover. Yet that does not appear to be the case. Albert Yoon examined the retirement decisions of all district court and federal circuit judges between 1945 and 2000 and found that “tenure trends among the federal judiciary have held fairly constant over the past half century, notwithstanding the cyclical decline in inflation-adjusted salaries.”³⁴

The second argument for higher salaries rests on attracting lawyers from the private bar and maintaining a diversity of backgrounds on the federal bench.³⁵ Private-sector lawyers give up a lot to join the bench. Few talented lawyers in private practice, the argument goes, will make the leap if judicial salaries remain far below those in the private sector. This argument assumes that attracting private-sector lawyers will make the judiciary better in some meaningful sense.³⁶ These lawyers might decide cases with a greater understanding and appreciation of the real world consequences of their decisions or have greater expertise in certain technical subjects like, say,

³³*Fed. Judicial Compensation*, Justice Alito’s testimony, *supra* note 2, at 3; Letter from Corporate Counsels, *supra* note 5, at 2; Letter from Law School Deans, *supra* note 3, at 1.

³⁴Yoon, *supra* note 11, at 1032. Between 2000 and 2004, one active federal circuit judge left the bench and one other federal circuit judge retired. See Fed. Judicial Ctr., The Federal Judges Biographical Database, <http://www.fjc.gov/public/home.nsf/hisj> (last visited Jan. 5, 2008) [hereinafter Biographical Database]. This is so despite inflationary erosion of the judicial salary. Given these small numbers, Yoon’s conclusion undoubtedly extends to this period. For a recent study of the relationship between judicial pay and the turnover of district court judges see KEVIN SCOTT, CONG. RESEARCH SERV., JUDICIAL SALARY: CURRENT ISSUES AND OPTIONS FOR CONGRESS 16 (2007) (finding that “[t]he correlations between judicial salary and the number of judges who resign or retire (rather than taking senior status) . . . appear to be limited”).

³⁵See *Fed. Judicial Compensation*, Justice Breyer’s testimony, *supra* note 2, at 7; 2005 Report, *supra* note 2, at 2-3; Lee Epstein et al., *The Norm of Prior Judicial Experience and Its Consequences for Career Diversity on the United States Supreme Court*, 91 CAL. L. REV. 903, 908 (2003). There is a vast literature assessing the impact of prior work experience on judicial performance. See, e.g., Orley Ashenfelter et al., *Politics and the Judiciary: The Influence of Judicial Background on Case Outcomes*, 24 J. LEGAL STUD. 257, 275-77 (1995) (finding that prior experience as a judge or prosecutor does not explain much of the variation in outcomes in the federal district courts); James J. Brudney et al., *Judicial Hostility Toward Labor Unions? Applying the Social Background Model to a Celebrated Concern*, 60 OHIO ST. L.J. 1675, 1741-1743 (1999) (finding that federal circuit judges with experience as management-side NLRA lawyers were more supportive of unions); Gregory C. Sisk et al., *Charting the Influences on the Judicial Mind: An Empirical Study of Judicial Reasoning*, 73 N.Y.U. L. REV. 1377, 1470-80 (1998) (finding that prior experience variables were significant in predicting a federal district judge’s stance on the constitutionality of the federal sentencing guidelines); Ahmed E. Taha, *Publish or Paris? Evidence of How Judges Allocate Their Time*, 6 AM. L. ECON. REV. 1, 19-20 (2004) (finding that district court judges with prior political experience were more likely to publish decisions); Kevin Scott & Corey Ditslear, *Does the Résumé Matter? The Effect of Career Experience on the Behavior of the Supreme Court* 14-18 (Aug. 15, 2006) (unpublished manuscript on file with author) (finding that prior experience in the legislative or executive branches explained whether a justice used ideology in deciding Fourth Amendment cases).

³⁶See Letter from Corporate Counsels, *supra* note 5, at 2.

securities law.³⁷ Empirically testing this particular argument is hard, and this Article does not aim to do so. This Article does find, however, that holding constant the net cost of taking a judgeship, lawyers who come directly from private practice perform similarly to those coming from government jobs, other judgeships, or academia across a range of judicial performance measures.³⁸

The third argument for higher salaries is that higher judicial salaries lead to higher quality judges.³⁹ A circuit judgeship brings with it substantial non-pecuniary benefits and a generous pension.⁴⁰ The job offers prestige, power, influence, control of one's schedule, and interesting work. It is not hard to find lawyers willing to take circuit judgeships because the actual wage is only one – arguably small – component of the total compensation package. The intuition is that lower pay might lead to “worse” judges, not zero judges.⁴¹ The next subsection develops this intuition in detail, before Section III takes the intuition to the data.

1. The Salary Matters Theory

The familiar economic argument is that higher wages attract better workers. In other words, workers with the greatest skill or human capital command the highest wages.⁴² This argument does not readily transfer to the pool of federal

³⁷Prior experience diversity is also a concern among senators. Harry Reid, for example, has called for more Supreme Court nominees with experience as practicing lawyers. Press Release, Senator Harry Reid, Statement of Senator Harry Reid on the Nomination of Harriet Miers to the U.S. Supreme Court (Oct. 3, 2005), available at <http://reid.senate.gov/newsroom/record.cfm?id=246777>.

³⁸This finding differs from the standard one in the literature. Epstein et al., *supra* note 35, at app. The studies Epstein reviews consider a variety of judicial output measures. However, none of these studies considers the net cost of taking the judgeship, the variable of interest here.

³⁹See *Fed. Judicial Compensation*, Justice Breyer's testimony, *supra* note 2, at 9; *Judicial Security and Independence*, Justice Kennedy's testimony, *supra* note 2, at 9; ABA testimony, *supra* note 4, at 2; *2006 report*, *supra* note 1, at 2; Letter from Corporate Counsels, *supra* note 5, at 2.

⁴⁰As a pension benefit, federal judges draw their existing salary and health benefits until they die. 28 U.S.C. § 371 (2000). The so-called “rule of 80” determines eligibility. The pension vests if the judge is at least sixty-five years old and has at least ten years of service, so long as the judge's age and service sum to eighty. *Id.* § 371(c). For a detailed discussion of the history of federal judicial pensions, see Albert Yoon, *Pensions, Politics, and Judicial Tenure: An Empirical Study of Federal Judges, 1869-2002*, 8 AM. L. & ECON. REV. 143 146-48 (2006).

⁴¹As Ann Althouse wrote for the *New York Times*:

If the pay is low, the judges will be the kind of people who don't care that much about money. They might be monkish scholars, or they might be ideologues who see in the law whatever it is they think is good for us. . . . Low judicial pay should trouble us not because the judges will somehow lack 'excellence.' It should trouble us because the law will be articulated by ideologues and recluses.

Ann Althouse, *An Awkward Plea*, N.Y. TIMES, February 17, 2007, at A1.

⁴²This idea dates back to Adam Smith. See ADAM SMITH, THE WEALTH OF NATIONS 90 (Everyman's Library ed., Knopf Books 1991) (1776). Jacob Mincer developed these

circuit judicial nominees. Almost every nominee for a judgeship takes a pay cut for the bench. Even nominees that come from the public sector could, if they wanted to, work in law firms, which would pay more than a circuit judgeship. The real impact of higher judicial salaries is a reduction of the pay cut nominees have to take. As we shall see, reducing the size of the pay cut could theoretically affect the judiciary's performance.

People care about both non-pecuniary and pecuniary aspects of a job.⁴³ For any person, a preference profile can be constructed indicating how much he or she subjectively values each non-pecuniary aspect and each pecuniary aspect of a given job. This profile will differ for each person depending on the individual's wealth, how much he or she values consumption versus leisure, and many other personal factors.

Now take judges. Judges care about a number of things besides money: status, prestige, leisure, power to affect policy, and public service.⁴⁴ Different people attach different weights to these non-pecuniary aspects of the job. The spread between the judicial salary and the wage in a candidate's next best opportunity reveals the strength of the candidate's attachment to the non-pecuniary aspects of judging. In other words, the spread reflects the person's taste for becoming a judge; a candidate willing to accept a large spread has a strong preference for judging.⁴⁵ Furthermore, an individual's preferences over the various non-pecuniary aspects of a judgeship might then influence eventual judicial performance. A strong desire for the circuit judgeship could, for example, correlate with a strong preference for leisure, which might manifest itself by that judge taking a long time to write her opinions.

By raising salaries, Congress reduces the spread between judicial salaries and the candidate's next best opportunity. As a result, higher salaries might weed out some of the people with the strongest desires for the judicial role. Sure, the true ideologue, the leisure maximizer, the prestige-obsessed, and the committed public servant will still be interested in the judgeship, but now so

ideas in the modern era, articulating what has become known as human capital theory. See JACOB MINCER, SCHOOLING, EXPERIENCE, AND EARNINGS (1974); Jacob Mincer, *Investment in Human Capital and Personal Income Distribution*, 66 J. POL. ECON. 281 (1958).

⁴³For survey results reporting the relationship between job satisfaction and the pecuniary and non-pecuniary aspects of a job, see Daniel S. Hamermesh, *The Changing Distribution of Job Satisfaction*, 36 J. HUM. RESOURCES 1, 26 (2001) (examining the effect of earnings inequality on job satisfaction and concluding that because the "nonpecuniary and nonwage pecuniary returns to work is income-elastic . . . it would be very worthwhile to examine a broader set of economic determinants of satisfaction"), and David S. Hamermesh, *Changing Inequality in the Markets for Workplace Amenities*, 114 Q.J. ECON. 1085, 1085-86 (1999) (investigating the possibility that "rising wage inequality has been partly offset by a negatively correlated reduction of the nonpecuniary returns to work").

⁴⁴See Richard A. Posner, *What Do Judges and Justices Maximize? (The Same Thing Everyone Else Does)*, 3 SUP. CT. ECON. REV. 1, 31-39 (1993). For a book-length treatment on what motivates judges, see generally LAWRENCE BAUM, *JUDGES AND THEIR AUDIENCES: A PERSPECTIVE ON JUDICIAL BEHAVIOR* (2006).

⁴⁵Cf. Mary Ellen Benedict et al., *The Price of Morals: An Empirical Investigation of Industry Sectors and Perceptions of Moral Satisfaction – Do Business Economists Pay for Morally Satisfying Employment*, 50 AM. ECON. 21, 27-29 (2006) (finding that economists working for non-profits make thirty-eight percent less than their counterparts in for-profit firms and attributing this compensation differential to the non-pecuniary benefit of working in a morally satisfying industry).

will a lot of other people. Under the “salary matters” theory, increased competition affects the kind of person eventually selected for the bench.

To see why this might be so, suppose that the pay for circuit judges is zero. In this case, individuals willing to take the job must really want to be judges. These individuals value non-pecuniary aspects of the job a lot – leisure, power, prestige, public service, etc. – and money less so (perhaps because they are wealthy already). Suppose the pay is increased to \$150,000 a year. In that case, people who would take the judgeship for nothing would still compete for the judicial slot, but now people who place a lower value on non-pecuniary perks and a higher value on wages would enter the pool. Increasing pay to \$2 million a year expands the pool even further; it now includes some lawyers who do not care much about the non-pecuniary aspects of the judgeship and care a lot about money. In this way, raising judicial pay (1) expands the candidate pool and (2) alters the profile of “tastes” for the judicial role among pool members.

From this theory, one testable implication is that changes in judicial pay affect judicial performance. Holding all else equal, with a high spread between judicial pay and the next best opportunity, the judiciary will be composed of people who are more partisan, lazier, more driven by prestige, and/or place a higher value on public service. These judges will act like it by, for instance, voting more consistently along party lines (the partisan judge), only citing judges from the same political party (the partisan judge), writing opinions more slowly (the lazy judge), or investing more time writing decisions other judges will cite (the prestige-motivated judge).

Upon closer inspection, then, there is some substance to the proponent’s claim that higher judicial salaries will attract better-quality judges.⁴⁶ Once unpacked, some possible effects of higher judicial salaries do, in fact, point in the direction of a higher-quality judiciary: higher salaries might lead to a harder working judiciary. Counter-intuitively, other possible effects of higher judicial salary point in the direction of a lower-quality judiciary: higher salaries might lead to the appointment of judges less committed to public service or less concerned with their own judicial influence. Still other effects of higher judicial salaries are ambiguous. For example, it depends on one’s normative view whether a more partisan judiciary is good or bad.⁴⁷ But all this is just theory. Section III tests whether any of these effects are present in the data.

⁴⁶For Supreme Court Justices making this claim, see sources cited *supra* note 2; for commentary, see sources cited *supra* notes 3-5.

⁴⁷With regard to voting behavior, there is another possible effect of higher salaries, an idea unrelated to the preferences of the candidate pool. By expanding the pool, higher salaries might allow the president to get a nominee who reflects his political values the most – his first choice who otherwise wouldn’t be available. Under this theory, higher salaries should lead to a more, rather than less, partisan judiciary. As we shall see, the evidence on voting patterns does not support this theory either; instead it is consistent with the idea that size of the judicial salary is unrelated to judicial voting patterns.

2. The Substitutes Theory

There is an alternative theory about the impact of raising judicial salaries. Suppose political tides select the same kind of people for judgeships regardless how the candidate pool is composed. In this case, deepening the pool to include people who care more about salary does not make sense. The judiciary will have the same number of leisure maximizers, ideologues, influence-peddlers, and committed public-servants, independent of the wage. The spread between judicial pay and a candidate's next best opportunity does not make a difference. For reasons that will become clear, I denote this alternative theory the substitutes theory.

For the substitutes theory to be true, two conditions must hold: (1) politics alone must drive judicial selections; and, (2) the pool, at present and historic salary levels, must be saturated with candidates who are near-perfect substitutes for those people unwilling to take the job because of salary concerns. By near-perfect substitutes, I mean the candidates in the pool are the same in terms of their ability to be confirmed, their appeal to the president, and their anticipated judicial performance. Under these conditions, expanding the pool does not change the type of person who reaches the bench. The president has his man or woman picked out already. If that person declines because of salary concerns to join the bench, the next person selected will be indistinguishable in her judicial performance. Because the number of interchangeable candidates is so large, odds are one of them will take the job at the prevailing wage.

In other words, even if low salaries reduce the number of candidates willing to take the circuit judgeship, that reduction might be inconsequential. It depends on the relationship between the number of comparable remaining candidates and the number of appointment slots. Reducing the pool, for instance, from 500 identical candidates to 250 identical candidates is immaterial if the president only appoints ten judges. This insight is the thrust of the substitutes theory.

II. TWO STATISTICAL APPROACHES TO ASSESSING THE IMPACT OF HIGHER JUDICIAL SALARIES

To unravel which of the two theories is true requires an inquiry into whether judicial pay affects judicial performance. If judicial pay does not impact performance, the data support the substitutes theory. If judicial pay does impact performance, the data support the salary matters theory. But such an analysis presupposes that it is possible to determine the relationship between

judicial pay and judicial performance.⁴⁸ On this score, the standard economic methodology is not much help.

Labor economists, for example, interested in measuring the impact of higher salaries typically compare two sets of workers. The first set of workers is paid more than the second set of roughly similar workers. Higher pay is said to have an effect if the high-paid workers produce more or quit less often than the low-paid workers.⁴⁹

For federal circuit judges, such an approach is not feasible. All federal circuit judges make roughly the same judicial salary. As a result, one cannot just compare judges with high salaries to judges with low salaries. To get around this problem, notice that judges are not equally well-paid as against their next best opportunity. The spread between private sector salaries and judicial salaries differs dramatically across time and across regions. I exploit this variation to conduct the statistical analysis. To detail this methodology further, consider two approaches to assessing the impact of judicial salaries: direct comparison and pool comparison.

A. *Direct Comparison Approach*

The direct comparison approach asks whether people who give up more money to become judges simply want the job more than people who give up less money. The strong preference for the judgeship translates into: (1) a stronger desire to impose policy preferences (revealed by, for example, more

⁴⁸The literature studying the link between judicial pay and the performance of the federal circuit courts is scant. To my knowledge, there are no other articles on the topic. The closest related literature involves state court judges. The question addressed there is whether appointed state court judges behave differently from elected state court judges. See, e.g., DANIEL R. PINELLO, *THE IMPACT OF JUDICIAL-SELECTION METHOD ON STATE-SUPREME-COURT POLICY* 130 (1995) (“A self-consciously rigorous and comparative methodology demonstrates selection method does significantly affect judicial policy in several important areas of law.”); John Blume & Theodore Eisenberg, *Judicial Politics, Death Penalty Appeals, and Case Selection: An Empirical Study*, 72 S. CAL. L. REV. 465, 488 (1999) (suggesting there is little correlation between partisan election of judges and death penalty reversals); F. Andrew Hanssen, *The Effect of Judicial Institutions on Uncertainty and the Rate of Litigation: The Election Versus Appointment of State Judges*, 28 J. LEGAL STUD. 205, 232 (1999) (concluding that “appointment better protects judges from political influence than does election”); Judith L. Maute, *Selecting Justice in State Courts: The Ballot Box or the Backroom?*, 41 S. TEX. L. REV. 1197, 1240-44 (2000). Elected state court judges must either self-finance election and re-election campaigns or spend time fundraising. Both activities, in effect, reduce the take-home pay of the judicial salary.

⁴⁹See, e.g., Peter Cappelli & Keith Chauvin, *An Interplant Test of the Efficiency Wage Hypothesis*, 106 Q.J. ECON. 769, 769 (1991) (“[T]he results suggest that greater wage premiums are associated with lower levels of shirking [or, unproductive behavior] as measured by disciplinary dismissals.”); Alan B. Krueger & Lawrence H. Summers, *Efficiency Wages and Inter-Industry Wage Structure*, 56 ECONOMETRICA 259, 280 (1988) (finding that reduced turnover “appears to accompany higher wages”); Sushil B. Wadhvani & Martin Wall, *A Direct Test of the Efficiency Wage Model Using UK Micro-Data*, 43 OXFORD ECON. PAPERS 529, 530 (1991).

partisan voting and citation practices);⁵⁰ (2) a stronger desire for leisure (revealed by, for example, taking longer to file published decisions and by dissenting less frequently);⁵¹ and/or, (3) a stronger desire to exert judicial influence (revealed by drafting opinions that garner more citations).⁵² All this, of course, must also control for the initial amount of wealth a candidate possesses. No matter the strength of their “taste” for the judgeship, wealthy candidates can more easily afford a pay cut than non-wealthy candidates. For precisely this reason, the empirical analysis controls for wealth of the candidate at the time of appointment.

Comparing the spread between judicial salary and a judge’s next best opportunity is the foundation of the statistical analysis. A judicial pay raise reduces the financial sacrifice every judge must make to take the bench. One way to assess the effect of a reduced sacrifice is to compare behavior of judges who actually made big financial sacrifices with behavior of judges who made small financial sacrifices. If, on the one hand, the two sets of judges behave similarly, judicial performance is independent of the financial sacrifice made. Congress, then, might as well leave judicial salaries where they are; the resulting increase in every future nominee’s financial sacrifice should not affect judicial performance. If, on the other hand, the two sets of judges behave differently, judicial performance does depend on the level of financial sacrifice and, accordingly, reducing the required financial sacrifice should alter the circuit courts’ functioning.

One limitation of this analysis is that I don’t (and can’t!) observe the judicial behavior of people who actually turned down the judgeship for financial reasons. The great, productive New York City lawyer who would have taken the judgeship if it paid \$1,000,000 is not in the sample. I do, however, have a clue as to how that lawyer would have acted on the bench. Suppose that Congress decided to “match” judicial salaries with private sector salaries, to pay the New York City lawyer one million dollars a year. Now that lawyer would sacrifice nothing for the judgeship. Judges in my sample who actually did give up close to nothing for the bench may be good proxies for candidates like this one who, with a substantial judicial pay raise, would enter the pool. If so, examination of the former’s behavior can be used to predict the likely performance of the latter.

The opportunity cost for a federal judicial nominee is her forgone wages from her next best employment opportunity. I construct this measure for 259 federal circuit judges appointed between 1974 and 2004. For a lawyer of the candidate’s age, law firm salaries in the region at the date of confirmation serve as the relevant benchmark.⁵³ Of course, many judges come from

⁵⁰See discussion *infra* Part III.A.1.

⁵¹See discussion *infra* Part III.B.2.

⁵²See discussion *infra* Part III.C.3. I do not test for the strong preference for public service because I am unsure what judicial performance measure would correlate with such a preference.

⁵³For this project, the best available law firm salary data comes from publications by Altman Weil, a law firm consulting firm. See generally ALTMAN WEIL PUBLICATIONS, INC., THE SURVEY OF LAW FIRM ECONOMICS (2005) [hereinafter ALTMAN WEIL SURVEY]. Data from previous years comes from prior editions of the survey. For reasons described *infra* notes 142-143 and accompanying text, the other leading sources of law firm salary information, the AmLaw100 and AmLaw200 lists of profits per partner, do not provide a good measure of

academia, government positions, and other judgeships. For these judges, any lost salary at the time of appointment is small;⁵⁴ their current salaries and federal circuit judges' salaries do not differ that much. I nevertheless use lost law firm wages as the relevant opportunity cost.⁵⁵

I then control for prior experience to account for systematic differences in lawyers coming from government service, prior judgeships, or academia because the very fact that these judges come from places other than private practice might reveal something about their eventual judicial behavior. Government lawyers, lower-court judges, and academics might, for instance, prefer leisure more than private sector lawyers. And so, holding opportunity cost constant, a judge coming from one of these positions might write opinions

the salary judges forgo by taking the judgeship.

Altman Weil's survey reflects self-reports by law firms throughout the country. In 2005, for example, the survey includes 7,516 associates and 9,704 partners, working in 340 U.S. law firms. ALTMAN WEIL SURVEY, *supra*, at 5. Altman Weil sends the survey to law firms that have contact with the company, specifically firms that have purchased their consulting services, subscribe to their newsletter, or participated in the survey's prior editions. *Id.* at 11.

I measure the judge's next best financial option as working for a law firm in their region. The assumption rules out the possibility that a judge's next best financial option is a higher paying law firm in a totally different regional market. The regional restriction makes sense for most judges in the sample. Of the 259 judges, 240 judges remained in the same region for the ten years before taking the bench. *See infra* text accompanying notes 56-62 (describing the methodology used to construct each judge's opportunity cost).

⁵⁴*Compare* SALARY DATA, *supra* note 7, at 1-2 (providing salary information on federal district court judges), *and* NAT'L CTR. FOR STATE COURTS, SURVEY OF JUDICIAL SALARIES 4-10 (Apr. 1, 2005), *available at* http://www.ncsconline.org/WC/Publications/KIS_JudComJudSal040105Pub.pdf (providing salary information on state court judges), *with* Richard T. Boylan, *What Do Prosecutors Maximize? Evidence from the Careers of U.S. Attorneys*, 7 AM. L. & ECON. REV. 379, 400 (2005) (proxying assistant U.S. attorney pay as level 11 from the U.S. government schedule) *and* Howard A. Glickstein, *2003-2004 SALT Salary Survey*, THE SALT EQUALIZER (Soc'y of Am. L. Teachers, St. Paul, Minn.), Feb. 2004, at 1-3 (providing salary information for law professors at 98 law schools).

⁵⁵This assumes that any government lawyer, judge, or academic considered for a circuit court judgeship is talented enough to be a law firm partner – if they so choose – at an average firm in their region. The evidence supports this assumption. Prosecutors move into law firms. *See* Boylan, *supra* note 54, at 383 (“Of the 570 [assistant] U.S. attorneys in the study . . . 19.65% took a position in a large private practice, and 39.12% took a position in a small private practice.”). State court judges rely on contacts to secure positions in local firms. *See* Jonathan P. Nase, *Why Judges Leave the Bench: Pennsylvania 1978-1993*, 68 TEMP. L. REV. 739, 752 (1995). Federal district court judges become partners in law firms. *See* EMILY FIELD VAN TASSEL ET AL., WHY JUDGES RESIGN: INFLUENCES ON FEDERAL JUDICIAL SERVICE, 1789 TO 1992 App. Index 3 (1993) (finding that many federal district court judges left for private practice between 1789 and 1992). Talented academics become of counsel at firms in their area. *See* Rory K. Little, *Law Professors as Lawyers: Consultants, Of Counsel, and the Ethics of Self-Flagellation*, 42 S. TEX. L. REV. 345, 366 (2001) (reporting that, of the sixty-six law schools who responded to a survey, twenty-seven had faculty with formal of counsel law firm relationships).

less swiftly than a judge coming straight from the private bar. The dummy variables for prior experience capture these potential differences.

The lost wages calculation for a person considering the bench consists of eight steps. First, calculate, at the time of the appointment, the number of years the candidate would likely remain at the law firm if they did not take the judgeship. Second, determine the likely law firm compensation for each of those years, considering increasing compensation due to increased seniority in the firm. Third, estimate how much law firm compensation in general is likely to increase during that time. Fourth, discount the total amount back to present value using the real discount rate.⁵⁶ Fifth, estimate the anticipated judicial wage for the number of years of expected service on the bench and discount this amount back to present value. Sixth, to get the net cost of taking the judgeship – the financial sacrifice made – subtract the present value of the anticipated judicial salary from the present value of the lost law firm wages. Seventh, adjust this net sacrifice for geographic cost of living differences, revealing, in effect, the purchasing power of the wages forgone. Finally, place that lost purchasing power into constant dollars, enabling the comparison of the financial sacrifices made by judges appointed at different times.

To illustrate more explicitly, consider a specific example. Judge James Sprouse was appointed and confirmed to the U.S. Court of Appeals for the Fourth Circuit in 1979. Judge Sprouse was 56 at the time of his confirmation, had graduated law school in 1949, and was likely admitted to the bar in 1950.⁵⁷ According to the 1979 edition of the Altman Weil survey, a lawyer who graduated law school the same year as Judge Sprouse and who worked at a firm in the South Atlantic region – encompassing West Virginia, where Judge Sprouse located his chambers – earned \$97,578 that year.⁵⁸ That amount gives one year of lost wages; to calculate Judge Sprouse’s aggregate forgone wages requires adding to \$97,578 the amount a lawyer with one more year of seniority at a firm in the same region made that same year (\$113,557), and adding the amount a lawyer with two more years seniority would have made in the same year, and so on, until the salary of the lawyer with eight more years of seniority is included. The result is a stream of nine years worth of lost salary, based on the assumptions that: (1) had he not become a judge, Judge Sprouse would have retired from the practice of law at the age of sixty-five; and, (2) Judge Sprouse’s law firm salary would have increased in accordance with the general increase in law firm salary as the lawyer ages in that region. Discounting this sum back to present value using a real interest rate of three percent arrives at total forgone wages of \$868,319.⁵⁹

⁵⁶These first four steps replicate the computation of lost earnings in a run-of-the-mill tort case. See Gary A. Anderson & David L. Roberts, *Stability in the Present Value Determination of Future Lost Earnings: An Historical Perspective with Implications for Predictability*, 39 U. MIAMI L. REV. 847, 852 (1985) (“The goal of personal injury litigation is to award plaintiffs . . . their lost earnings. The court calculates the present value of future lost earnings by forecasting future lost earnings and then discounting the present value.”).

⁵⁷Biographical Database, *supra* note 34.

⁵⁸ALTMAN WEIL PUBLICATIONS, INC., THE SURVEY OF LAW FIRM ECONOMICS (1979).

⁵⁹Picking the appropriate rate to discount future earnings is tricky. See generally Michael T. Brody, Comment, *Inflation, Productivity, and the Total Offset Method of Calculating Damages for Lost Future Earnings*, 49 U. CHI. L. REV. 1003 (1982). The analysis uses three percent as the appropriate real rate. See *Jones & Laughlin Steel Corp. v. Pfeifer*, 462 U.S.

Next, consider Judge Sprouse's judicial salary. In 1979, a circuit judge made \$65,000 a year. To get the present value of the estimated income stream from the judicial salary, this figure should be multiplied by the nine years until expected retirement and then discounted to present value.⁶⁰ Subtracting the aggregate judicial salary from the aggregate law firm wages forgone results in a total opportunity cost of \$272,221.92. Accounting for geographic cost of living differences⁶¹ and inflation,⁶² Judge Sprouse gave up \$949,120.79 in 2004 dollars to take the bench.

Table 1 provides summary statistics of the net cost measure:

Table 1
Summary Statistics NETCOST ("NC")

Circuits	No obs	Avg NC	Var NC	Min NC	Max NC
1	10	1,033,113	332,983.90	379,974	1,466,571
2	26	782,442	394,566.70	209,344	1,708,354
3	21	1,188,235	557,249.40	0	2,474,461
4	16	1,253,176	479,565.30	593,846	2,152,587
5	34	1,372,013	843,779.40	57,476	3,112,091

523, 548 (1983) (holding that discounting with a real rate of interest of between one and three percent is appropriate for computing lost earnings). I did the same analysis with discount rates ranging from 1 to 6 percent. The statistical results all still hold. Note that inflation is not included in the growth rate of the law firm wages. As such, the real rate of interest is used to discount back to present value. This approach thus treats inflation the same in the numerator and denominator of the lost earnings equation. See O'Shea v. Riverway Towing Co., 677 F.2d 1194, 1199-1201 (7th Cir. 1982) (Posner, J.) (holding the calculation of a plaintiff's lost earnings was not unreasonable after computations using this method).

⁶⁰Nominal judicial wages have, of course, increased over time, from \$42,500 in 1974 to \$175,100 in 2006. Inflationary pressures drove much of this judicial wage growth, albeit not enough to make the judicial wage constant in real terms. As with lost law firm salaries, in computing the present value of the judicial wage, I did not bump the wage up to account for inflationary increases. At the same time, the real, not nominal, discount rate is used. The treatment of inflation is thus the same in the numerator and the denominator of the judicial salary computation.

⁶¹The ACCRA index is used to account for geographic cost of living differences. This index is commonly used for comparing relative cost of living across the country. See, e.g., Michael S. Knoll & Thomas D. Griffith, *Taxing Sunny Days: Adjusting Taxes for Regional Living Costs and Amenities*, 116 HARV. L. REV. 987, 990 n.18 (2003). It measures the differential costs of a bundle of goods typically purchased by consumers in the top income quintile. The index surveys prices in over 400 urban areas. For details on this index, see the website of the Council for Community and Economic Research, <http://www.coli.org/Method.asp> (last visited Jan. 11, 2008). For a precise description of the ACCRA data used in the statistical analysis see the data collection memo, available at <http://www.law.unc.edu/faculty/directory/default.aspx> (follow "Baker, Scott A." hyperlink).

⁶²Inflation adjustments use the annual consumer price index (CPI); the data comes from the Bureau of Labor Statistics, <http://data.bls.gov/cgi-bin/surveymost> (last visited July 21, 2007).

6	26	1,117,551	511,608.30	246,836	2,104,809
7	15	1,277,400	560,018	337,301	2,202,034
8	18	1,037,208	690,709.10	32,570	3,113,461
9	47	943,997	656,503.50	0	2,715,934
10	17	1,188,050	595,922.50	350,948	3,001,509
11	10	1,548,358	443,424.60	816,768	2,048,498
D.C.	18	1,395,165	730,446.70	136,421	3,048,630
Full Sample	259	1,141,561	635,367.50	0	3,113,461

The descriptive statistics reveal a few under-appreciated points in the judicial salary debate. First, the debate focuses on a comparison of annual judicial salary versus annual salary in private firms or academia, with a focus on the large and ever-increasing first year associate salaries in major markets.⁶³ There is a shock value to this focus. In 2006, including year end bonus, first year associates at major New York City law firms made as much or more than circuit court judges.⁶⁴ How could a judge be valued the same as a first year associate?⁶⁵ But, for a person considering the bench, this annual comparison is immaterial because it ignores differences in cost of living. Judicial salaries do not vary by location; law firm salaries generally do.⁶⁶ Comparing judicial pay for a judge sitting in, say, Omaha, Nebraska with law firm salaries in Washington, D.C. or New York City misses the point that a dollar buys a lot more in Omaha.

Second, because few circuit judges ever leave the bench, use of an annual comparison also hides differences in lost lifetime earnings – the true wages forgone.⁶⁷ Judges appointed early in life had the highest net cost of taking a judgeship. The four judges who made the biggest sacrifice – Judges William Pryor, Jerry Smith, Lavenski Smith, and Karen Henderson – were all appointed in their early or mid-forties. The extra years of earnings they lost swamp differences in geographic cost of living and differences in law firm salaries.

Third, the net cost of taking the bench has not increased substantially since 1974. There is a lot of variation across judges, but only a small upward trend over time.⁶⁸ Although law firm salaries have increased in real terms, the age of appointment has bounced around. President Ronald Reagan appointed relatively young federal judges (average age 49).⁶⁹ President George W. Bush

⁶³*Fed. Judicial Compensation*, Justice Breyer's testimony, *supra* note 2, at 3-4 ; *Judicial Security and Independence*, Justice Kennedy's testimony, *supra* note 2, at 10-11; Letter from Corporate Counsels, *supra* note 5, at 2.

⁶⁴*Compare* NAT'L ASS'N FOR LAW PLACEMENT, 2006-2007 NALP DIRECTORY OF LEGAL EMPLOYERS 1072 (2006) (stating that total compensation of first year associates at Cravath, Swaine & Moore was \$180,000), *with* SALARY DATA, *supra* note 7, at 1 (showing that federal circuit judges made \$ 175,100 in 2006).

⁶⁵*See Fed. Judicial Compensation*, Justice Breyer's testimony, *supra* note 2, at 4; *Judicial Security and Independence*, Justice Kennedy's testimony, *supra* note 2, at 10.

⁶⁶*See* Bureau of Labor Statistics, U.S. Dep't of Labor, Occupational Outlook Handbook: Lawyers 4 (2006), *available at* <http://stats.bls.gov/oco/ocos053.pdf>.

⁶⁷*See* RICHARD A. POSNER, *HOW JUDGES THINK* (forthcoming 2008) (noting that only eight circuit judges have actually quit the bench since 1981).

⁶⁸The correlation between year of appointment and net cost is 0.12.

appointed some older judges and some younger judges (average age 52).⁷⁰ Comparing the two sets on the Third Circuit Court of Appeals, for example, shows that some Ronald Reagan appointees sacrificed more purchasing power than some George W. Bush appointees.⁷¹

The data appear to undermine the notion – implicit in the arguments by proponents of higher salaries – that appointees from ten or twenty years ago paid a small price to take the bench, whereas appointees today pay a hefty price.⁷² The truth is that lost purchasing power depends on the judge’s age and her geographic cost of living, not just the absolute salary in the private sector. Every judge appointed before the age of forty-five took a serious financial hit in taking the bench. Again, annual comparisons to the salaries of lawyers in large market mega-firms, law school professors or law school deans are not revealing. If low judicial salaries are a problem now, they probably always were a problem.

B. *Pool Comparison Approach*

The direct comparison approach only looks at those candidates nominated and confirmed to the bench and thus does not capture the strength of the candidate pool from which the president selects. A common argument for higher judicial salaries is that an increase would deepen the candidate pool.⁷³ With higher judicial salaries, financial considerations would no longer deter some candidates from considering the judgeship. The deeper pool would provide more people from which the president could choose. Indeed, under the salary matters theory, higher judicial salaries can make the pool better as well as larger, by luring people with tempered preferences for the judicial role into the candidate pool. Any analysis of the impact of judicial salaries must therefore compare the strength of the pools the nominees come from as well as the strength of individual nominees. If nominees from small candidate pools are “worse” judges than nominees from large candidate pools, then Congress buys something with higher judicial salaries.

Pool comparisons require a measure of pool strength. For each judge, the net cost for the typical 49-year-old lawyer in the judge’s region at the time of the judge’s appointment proxies the strength of the pool from which that judge came.⁷⁴ To wade into the candidate pool, this typical lawyer would have to give up sixteen years of law firm income, adjusted for increased seniority in

⁶⁹The average age figures can be easily derived from the dataset for this project, which is available at <http://www.law.unc.edu/faculty/directory/default.aspx> (follow “Baker, Scott A.” hyperlink).

⁷⁰*Id.*

⁷¹Compare, for example, the Reagan-appointee Judge Greenberg with the George W. Bush appointee Judge M. Fisher. The data memo contains a complete listing of the net cost data and is available at <http://www.law.unc.edu/faculty/directory/default.aspx> (follow “Baker, Scott A.” hyperlink).

⁷²*Fed. Judicial Compensation*, Justice Breyer’s testimony, *supra* note 2, at 4; *2006 report*, *supra* note 1, at 2.

⁷³*Fed. Judicial Compensation*, Justice Breyer’s testimony, *supra* note 2, at 6.

the firm. As in the direct comparison, the discounted value of the judicial wage is deducted from the present value of the lost law firm wages. The net cost figure is then adjusted for geographic cost of living differences and inflation. The result is a measure of the “typical” loss in purchasing power for a lawyer who decided to take a judicial appointment at that time in that region.⁷⁵ If the typical lawyer would have had to give up little purchasing power, forgone income should be a relatively small barrier to entry into the judicial nomination process and, as a result, the candidate pool should be quite deep.

Table 2 provides descriptive statistics on the net costs for the various circuit pools from which the presidents selected.

Table 2
Summary Statistics NETCOSTPOOL (“NCPOOL”)

Circuits	No obs	Avg NCPool	Var NCPool	Min NCPool	Max NCPool
1	10	1,435,677	317,256.80	955,168	1,955,180
2	26	1,428,937	278,546.30	985,690	2,196,387
3	21	1,543,771	280,206.30	1,107,419	2,225,188
4	16	1,603,556	335,393.20	1,160,698	2,209,480
5	34	1,752,028	557,878.60	437,773	2,715,140
6	26	1,450,678	225,646.60	1,079,356	1,877,353
7	15	1,484,654	268,212.70	958,831	1,969,727
8	18	1,346,870	498,334.30	649,935	2,807,475
9	47	1,287,493	630,860.10	306,423	2,932,259
10	17	1,243,428	512,065.40	526,321	2,630,343
11	10	1,498,876	241,705.40	1,204,928	1,851,947
D.C.	N/A				
Full Sample	241	1,457,772	464,530.80	306,423	2,932,259

⁷⁴Age 49 is arbitrary. The same results hold, however, assuming the “typical” lawyer is 45 or 55.

⁷⁵The D.C. Circuit judges are not included in the pool comparisons. Since the president selects these judges from the national market, there is not a natural regional pool. As such, it was hard to decide the relevant region that a “typical” D.C. circuit judge might come from. In addition, the president looks to specific states for the regional circuit appointments. See Carl Tobias, *The Federal Appellate Court Appointments Conundrum*, 2005 UTAH L. REV. 743, 768 (stating that senators “must cooperate with the presidents . . . on important matters, such as whether the senate will continue to honor traditions that hold that appeals court judges should be residents of the states in which positions open, and should have chambers in those states”). To capture this fact, the pool strength is measured by state. Moreover, the direct comparison approach accounted for geographic cost of living differences by assessing the relative costliness of the city where a specific judge lived. The pool comparisons are adjusted for geographic cost of living differences by averaging the geographic cost of living index statewide.

III. WOULD THE CIRCUIT COURTS PERFORM ANY DIFFERENTLY WITH HIGHER JUDICIAL SALARIES?

This section tests three hypotheses concerning the relationship between higher judicial salaries and judicial performance. Drawn from the salary matters theory, the three hypotheses are: (1) paying circuit judges more creates a less ideological judiciary; (2) paying circuit judges more creates a harder working judiciary; and (3) paying circuit judges more creates a judiciary that is less concerned with its own influence. To test the three hypotheses, I used an econometric model to look for a statistical relationship between the amount of money a judge gave up to take the bench and the available measures of judicial performance.

A. *Hypothesis One: Paying Circuit Judges More Creates a Less Ideological Judiciary*

Measuring judicial ideology is a tricky business. The common perception is that some judges are conservative like, say, Judge Edith Jones of the Fifth Circuit,⁷⁶ while other judges are liberal like, say, Judge Stephen Reinhardt of the Ninth Circuit.⁷⁷ But what traits make Judge Jones conservative and Judge Reinhardt liberal? And, more to the point, can those traits be quantified? In short, testing whether judicial pay impacts judicial ideology requires some measure of ideology.

This Article's analysis tackles ideology two different ways. The first subsection considers whether judicial pay impacts judicial voting in controversial cases. The operative assumption is that a more ideological judiciary will engage in more partisan voting patterns in these cases. A true conservative ideologue will always cast a conservative vote; the opposite holds for the liberal ideologue. By this measure, a more ideological judiciary consists of republican appointees who more routinely cast conservative votes and democratic appointees who more routinely cast liberal votes.

The second subsection examines the relationship between judicial pay and citation practices. Judges write opinions in addition to voting. These opinions often cite outside circuit judicial opinions to support their analysis. Because judges exercise substantial discretion as to when and what extra-circuit precedent they will cite, these citations can then be investigated for evidence of judicial ideology.⁷⁸ Under this measure, a more ideological judiciary consists

⁷⁶See, e.g., Anita Bernstein, *Treating Sexual Harassment with Respect*, 111 HARV. L. REV. 445, 475 n.173 (1997) (referring to Judge Jones as a well-respected conservative judge).

⁷⁷See, e.g., Ward Farnsworth, *The Role of Law in Close Cases: Some Evidence from the Federal Courts of Appeals*, 86 B.U. L. REV. 1083, 1090 (2006) (stating that Judge Reinhardt enjoys a reputation as being very liberal).

⁷⁸See Stephen J. Choi & G. Mitu Gulati, *Ranking Judges According to Citation Bias (as a Means To Reduce Bias)*, 82 NOTRE DAME L. REV. 1279, 1302 (2007) [hereinafter Choi & Gulati, *Rankings*] (using citation practices as a measure of judicial bias, "particularly out-of-jurisdiction opinions that are not cited for precedential value"); Stephen J. Choi & G. Mitu Gulati, *Bias in Judicial Citations: A Window into the Behavior of Judges?* 1 (NYU Law and Economics, Working Paper No. 06-21, 2007) [hereinafter Choi & Gulati, *Bias*] (interpreting

of judges who seldom, if ever, recognize the opinions of judges from the other political party as persuasive authority.

1. Voting Patterns in Controversial Cases

The Chicago Judge's Project provides data on judicial voting patterns in the circuit courts.⁷⁹ The project tracks circuit courts' recently published judicial decisions in controversial cases. The cases involve:

[A]bortion, capital punishment, the Americans with Disabilities Act, criminal appeals, takings, the Contracts Clause, affirmative action, Title VII race discrimination cases brought by African-American plaintiffs, sex discrimination, campaign finance, cases in which plaintiffs sought to pierce the corporate veil, industry challenges to environmental regulations, and federalism challenges to congressional enactments under the Commerce Clause.⁸⁰

The dataset includes 4958 decisions and 14,874 individual judicial votes.⁸¹ Each judge's vote is coded "liberal" or "conservative." Although the labels are imprecise, they do track common notions of liberal and conservative jurisprudence. For example, a liberal vote in a sex discrimination case is a vote for the employee; a conservative vote is a vote for the employer.⁸²

To determine whether judicial pay impacts voting patterns, the analysis controls for other factors that might influence a judge's vote. One of the most important factors is the politics behind the judicial nomination process.⁸³ No matter the level of judicial pay, a republican president facing a republican-controlled Senate will probably appoint a more conservative judge than will a democratic president facing a democratic-controlled Senate.⁸⁴ Just using an appointing president's political party as a proxy for an appointed judge's ideology, though, misses much of the nuance. Not all Republicans are equally conservative and not all Democrats equally liberal.⁸⁵ Furthermore, because of

the finding that judges "cite judges of opposite political party less compared with the fraction of the total pool of opinions attributable to the opposite political party judges" to suggest that "judges base outside circuit citation decisions in part on the political party of the cited judge").

⁷⁹University of Chicago Law School: Chicago Judges Project, <http://www.law.uchicago.edu/academics/judges/index.html> (last visited Jan. 5, 2008).

⁸⁰Cass R. Sunstein et al., *Ideological Voting on Federal Courts of Appeals: A Preliminary Investigation*, 90 VA. L. REV. 301, 311-13 (2004) [hereinafter Sunstein et al., *Voting*]. For a more complete discussion of the dataset, see CASS R. SUNSTEIN ET AL., *ARE JUDGES POLITICAL? AN EMPIRICAL ANALYSIS OF THE FEDERAL JUDICIARY* 147 (2006) [hereinafter SUNSTEIN ET AL., *JUDGES*] (finding "striking evidence of a relationship between the political party of the appointing president and judicial voting patterns").

⁸¹As is, the database is too broad for my inquiry. It includes votes by district court judges sitting by designation and circuit judges appointed before 1974 for whom opportunity cost data is unavailable. Truncating the dataset left 8661 judicial votes.

⁸²See SUNSTEIN ET AL., *JUDGES*, *supra* note 80, at 19 ("[A] vote counts as stereotypically liberal if it favors a plaintiff who is complaining of discrimination based on sex.").

⁸³*E.g.*, Sunstein et al., *Voting*, *supra* note 80, at 307 (finding that democrat appointees cast more liberal votes than republican appointees).

⁸⁴See Barry Friedman, *The Politics of Judicial Review*, 84 TEX. L. REV. 257, 278 n.104.

⁸⁵*E.g.*, Workshop on Empirical Research in the Law, *On Tournaments for Appointing Great Justices to the U.S. Supreme Court*, 78 S. CAL. L. REV. 157, 176 (2004).

senatorial courtesy a republican president facing democratic senators from the nominee's home state might be able to push through a different judge than a republican president facing republican home-state senators. Fortunately, Micheal Giles, Virginia Hettinger, and Todd Peppers have constructed a measure of the appointing president's and confirming senate's ideologies, controlling for the possibility of senatorial courtesy and the so-called "blue slip process."⁸⁶

Giles et al. measure the appointing president's ideology based on his votes on various pieces of legislation. Political scientists call this the common space score.⁸⁷ The same type of score measures the ideology of relevant senators. The index combines and weights each of these factors, creating a measure of the judicial nominee's likely ideology. The index runs from -1 to 1, with 1 being the most conservative score possible. Absent senatorial courtesy, the nominee's ideological score equals the common space score of the appointing President. If there was senatorial courtesy for the nomination, the ideological score weights the common space scores of the President and the home state Senators.

⁸⁶Micheal W. Giles et al., *Picking Federal Judges: A Note on Policy and Partisan Selection Agendas*, 54 POL. RES. Q. 623, 627 (2001) (using a complex model of selection that focuses "on determining if the behavior of the judges once appointed is consistent with the operation in the selection process of" a partisan agenda reflecting the preference of state party elites, or a policy agenda reflecting the preference of the president, "and the influence of senatorial courtesy on either of these agendas"); Michael W. Giles et al., *Measuring the Preferences of Federal Judges: Alternatives to Party of the Appointing President* (July 11, 2002) (unpublished manuscript); see also Lee Epstein et al., *The Judicial Common Space*, 23 J.L. ECON. & ORG. 303, 306 (2007) (lauding the Giles et al. measure as "the state-of-the-art measure for the preferences of U.S. Court of Appeals judges").

⁸⁷Gregory C. Sisk and Michael Heise recount the development of the common space score as follows:

Professors Keith Poole and Howard Rosenthal developed measures of ideological preferences for members of Congress, conceptualizing all aspects of legislative voting in terms of a single ideological dimension (with a second dimension, such as civil rights, rising to greater importance during certain historical periods). Poole extended this approach to derive "common space" scores for members of Congress on a metric that is common across time, that is, a Senator's policy preference "common space" score is held constant across time and is the same for all periods. Subsequently, Poole extended this approach to derive common space scores for the policy preferences of Presidents since Eisenhower.

Gregory C. Sisk & Michael Heise, *Judges and Ideology: Public and Academic Debates About Statistical Measures*, 99 NW. U. L. REV. 743, 786-87 (2005); accord KEITH T. POOLE & HOWARD ROSENTHAL, *CONGRESS: A POLITICAL-ECONOMIC HISTORY OF ROLL CALL VOTING* 227 (1997) (finding that "except for two periods of American history, when race was prominent on the agenda, [roll call] voting can be captured" by a one dimensional special model, such that "political parties appear to be the critical element in promoting stable voting alignments"); Keith T. Poole, *Recovering a Basic Space from a Set of Issue Scales*, 42 AM. J. POL. SCI. 954, 987 (1998) (using scale procedure and finding that "members of congress are very stable in their location on the liberal/conservative dimension over time").

Combining the data from the Chicago Judges Project with the Giles et al. measure reveals a consistency between the two datasets, demonstrated in Table 3:

Table 3
 Relationship Between Giles et al. Measure of the Confirmation Process and
 Judicial Voting Patterns
 Probit Model
 Dependent Variable: Probability Judge Casts a Liberal Vote

Regressors	
selpref	-0.156 (10.24)**
circdum1	-0.031 (0.97)
circdum2	-0.009 (0.31)
circdum3	0.052 (1.49)
circdum4	-0.099 (3.10)**
circdum5	-0.145 (5.23)**
circdum6	-0.07 (2.42)*
circdum7	-0.157 (6.14)**
circdum8	-0.163 (6.19)**
circdum9	0.06 (2.08)*
circdum10	-0.039 (1.29)
circdum11	-0.034 (1.15)
Observations	8661

Robust z statistics in parentheses

* significant at 5%; ** significant at 1%

Estimated coefficients reflect marginal effects when all the other independent variables are measured at their mean.

The dependent variable is the probability that the judge casts a liberal vote in a controversial case. The independent variables include the Giles et al. measure of the confirmation process (“selpref”) and circuit dummy variables to control for differences across circuits. The Giles et al. measure is negative and highly statistically significant indicating, as predicted, that judges scoring higher (closer to 1), by the Giles measure, are less likely to cast a liberal vote. The more conservative the players in the nomination and confirmation process, the more likely the judge will be to cast a conservative vote in a controversial case.

I now turn to the hypothesis that higher judicial pay will lead to a less ideological judiciary. Tables 4 and 5 present the result of the direct comparison approach. I first divided the sample into votes by democratic appointees and votes by republican appointees. The dependent variable is the probability the judge casts a liberal vote in a controversial case. If the hypothesis is correct, the sign of the coefficient for the net cost variable (“NETCOST”) should be positive and statistically significant for democratic appointees and negative and significant for republican appointees. As described in Section II, NETCOST measures the lump sum value of the lost

lifetime earnings – that is, the financial sacrifice made. I measure NETCOST in \$400,000 increments; that is to say, an increase in one unit of NETCOST represents an increase of \$400,000 in spendable dollars.⁸⁸ For the lawyer living in the average city, \$400,000 is, roughly, \$50,000 additional dollars a year over 11 years, discounted at three percent.

Besides the Giles et al. measure, other controls included in the regression model are: (1) if available, the judge's net worth at the time of appointment, adjusted for inflation and geographic cost of living;⁸⁹ (2) circuit court dummy variables; (3) prior experience dummy variables, controlling for whether the judge came from private practice, academia, another judgeship, or other government service;⁹⁰ (4) the nominee's age at the time of appointment; (5) the nominee's gender; (6) whether the nominee came from a top-five legal market (New York,⁹¹ Chicago, Los Angeles,⁹² San Francisco,⁹³ or Washington D.C.); and (7) an interaction term between the top-five legal market and NETCOST variables.

Because this is the first of many regressions, a brief discussion of these control variables is in order. The net worth variable captures differences in wealth at the time of appointment. Because of the diminishing marginal utility of money, a salary hit of \$1.5 million will cost a judge with accumulated earnings of \$5 million much less than it would cost a judge with accumulated earnings of \$100,000. The net worth variable accounts for this fact. Unfortunately, net worth data are only available for 121 of the 259 judges in the sample.

The circuit court dummy variables control for unobserved differences in voting patterns across circuits due to, for example, the culture of the circuit. For example, no matter the value of NETCOST, judges from the Fifth Circuit might be more apt to cast a conservative vote than judges from the Ninth Circuit.⁹⁴

⁸⁸Spendable dollars is defined as extra dollars adjusted for geographic cost of living. For example, to give \$400,000 spendable dollars to a judge from New York City, Congress would have to authorize a salary increase for that judge of more than \$800,000 (i.e., \$100,000 a year for eleven years, discounted at three percent). The reason is that New York City is more than twice as expensive as the average city in the United States. See *supra* note 75 and accompanying text.

⁸⁹Gary Zuk et al., S. Sidney Ulmer Project: Attributes of Federal Court Judges, <http://www.as.uky.edu/polisci/ulmerproject/auburndata.htm> (last visited Jan. 11, 2008) (providing judges' net worths).

⁹⁰Biographical Database, *supra* note 34.

⁹¹Judges from Newark, N.J. are coded as part of the New York City legal market.

⁹²Judges from Pasadena, Cal. are coded as from the Los Angeles legal market.

⁹³Judges from Berkeley, Cal. and Oakland, Cal. are coded as part of the San Francisco legal market.

⁹⁴Among legal commentators, the Fifth Circuit is thought to be a relatively conservative circuit. See Sheldon Goldman, *Unpicking Pickering in 2002: Some Thoughts on the Politics of Lower Federal Court Selection and Confirmation*, 36 U.C. DAVIS L. REV. 695, 704-05 (2003) ("Pickering's opponents argued that his record as a federal district judge suggested that he would . . . help push an already conservative Fifth Circuit even further

The prior experience dummy variables (“Judge,” “Professor,” and “Private Practice”) capture differences in preferences associated with the candidate’s prior work experience. If, say, a circuit court judge who comes directly from a job as a government lawyer is more partisan than one who comes from private practice, the coefficient on “Private Practice” should be statistically significant.

“Age” is included because judges appointed late in life might be less partisan than judges appointed early in life. Someone willing to take a judgeship at, say, age 35 might care more about policy outcomes than someone willing to take the job at, say, age 55. The 35 year-old will, after all, have a longer judicial career over which she can influence outcomes.⁹⁵ “Sex” controls for differences between the judicial performance of men and women.⁹⁶

The variable “Top Five Legal Market” controls for a potential error in the measurement of NETCOST. NETCOST assumes that candidates forgo the average salary of a comparable law firm partner in their region at the date of appointment.⁹⁷ Yet some appointees might give up more money than the

right.”); E. Farish Percy, *Making a Federal Case of It: Removing Civil Cases to Federal Court Based on Fraudulent Joinder*, 91 IOWA L. REV. 189, 192 n.9 (2005) (“[T]he Fifth Circuit and many of the district courts within the Fifth Circuit are generally perceived as conservative.”); Garrick B. Pursley, *Thinking Diversity, Rethinking Race: Toward a Transformative Concept of Diversity in Higher Education*, 82 TEX. L. REV. 153, 173 (2003) (referring to the Fifth Circuit as conservative). The Ninth Circuit is thought to be a relatively liberal circuit. See Michael Abramowicz, *En Banc Revisited*, 100 COLUM. L. REV. 1600, 1606 (2000) (“[T]he circuits seem to have ideological casts, with the liberal Ninth Circuit . . . perceived as being [at one side] of the spectrum.”); Jerome Farris, *Judges on Judging: The Ninth Circuit – Most Maligned Circuit in the Country – Fact or Fiction?*, 58 OHIO ST. L.J. 1465, 1471 (1997) (“Some observers contend that the Ninth Circuit is reversed so often because it is the most liberal circuit in the country and because the Supreme Court is currently conservative.”); Stephen J. Wermiel, *Exploring the Myths About the Ninth Circuit*, 48 ARIZ. L. REV. 355, 355 (2006) (commenting that the Ninth Circuit is considered quite liberal).

⁹⁵See SHELDON GOLDMAN, PICKING FEDERAL JUDGES: LOWER COURT SELECTION FROM ROOSEVELT THROUGH REAGAN 346 (1997) (indicating President Reagan’s preference for younger judges who would be able to advance his agenda over a longer period of time); James R. Acker & Elizabeth R. Walsh, *Challenging the Death Penalty Under State Constitutions*, 42 VAND. L. REV. 1299, 1314 n.82 (1989) (noting that young judges “are expected to have a long-term impact on federal court decision making”).

⁹⁶On the much-studied relationship between gender and judicial performance, see Theresa M. Beiner, *The Elusive (but Worthwhile) Quest for a Diverse Bench in the New Millennium*, 36 U.C. DAVIS L. REV. 597, 601-03 (2003) (suggesting life experiences shape female judges’ policy, especially regarding decisions in “women’s cases” such as abortion rights or sex employment discrimination); Donald R. Songer et al., *A Reappraisal of Diversification in the Federal Courts: Gender Effects in the Courts of Appeals*, 56 J. POL. 425, 432-36 (1994) (finding female judges voted in favor of victims in employment discrimination cases more often than males, but gender did not affect votes in search and seizure and obscenity cases); Jennifer L. Peresie, Note, *Female Judges Matter: Gender and Collegial Decisionmaking in the Federal Appellate Courts*, 114 YALE L.J. 1759, 1776-79 (2005) (finding a higher probability of favorable judgments for plaintiffs in sexual discrimination cases when a female judge was involved in the case).

⁹⁷Of course, circuit judges might be above-average lawyers, not average lawyers. The average partner salary, then, might underestimate the true opportunity cost. If, as is plausible, the average salary for a law firm partner in a region highly correlates with the law firm salary for the above-average lawyer, the analysis still works. Because the variance in

average partner in the region, while other appointees might give up less. “Top Five Legal Market” captures this effect because law firm partners in the five major markets make significantly more money than law firm partners elsewhere.⁹⁸ The interaction term TOPFIVENETCOST allows for the increase in one unit of net cost to have a different effect on a judge from a major market than an increase in one unit of net cost on other judges in the region. For example, the judge from Chicago, coded as sacrificing \$400,000, might really be giving up \$800,000. Her taste for being a judge would therefore be larger than the NETCOST measure reflects. The implication is that this stronger preference should correlate with more partisan judicial voting patterns. The interaction term estimates these differential effects.

Tables Four and Five report the probit regression results.

Table 4
 Relationship Between Democratic Financial Sacrifice and Voting Patterns
 Probit Model
 Dependent Variable: Probability Democratic-Appointee Casts a Liberal Vote

	Model (1) (Full Sample)	Model (2) (subsample w/ Networth)
Regressors		
NETCOST	0.001 (0.15)	0.005 (0.34)
selpref	0.042 (0.53)	0.159 (1.27)
Age	0.001 (0.52)	0 (0.09)
Sex	-0.012 (0.59)	0.01 (0.31)
Top Five Legal Market	-0.026 (0.50)	-0.241 (2.31)*
PrivatePractice	-0.056 (1.28)	-0.158 (2.09)*
Professor	-0.018 (0.36)	-0.116 (1.30)
Judge	-0.063 (1.43)	-0.151 (2.14)*
TOPFIVENETCOST	0.008 (0.35)	0.12 (1.70)
circdum1	-0.012 (0.20)	-0.019 (0.07)
circdum2	-0.019 (0.37)	0.086 (0.73)
circdum3	0.081 (1.29)	0.133 (1.05)
circdum4	-0.128 (2.08)*	-0.04 (0.31)
circdum5	-0.175 (3.02)**	-0.115 (0.90)

the average partnership salary tracks the variance in the salary for the above-average lawyer, the results remain the same.

⁹⁸See, e.g., William J. Wernz, *The Ethics of Large Law Firms – Responses and Reflections*, 16 GEO. J. LEGAL ETHICS 175, 178 (2002) (highlighting a substantial difference in profits per partner between major city firms and smaller city firms). Data buttressing this point is available in the annual American Lawyer magazine issues about the Am Law 100 and Am Law 200 firms. See, e.g., *The AmLaw 100, 2006*, AM. LAW., May 2006, at 173-76 (reporting 2005 profits per partners by location).

circdum6	-0.08 (1.53)	-0.085 (0.70)
circdum7	-0.178 (3.90)**	-0.014 (0.14)
circdum8	-0.109 (2.00)*	0.004 (0.03)
circdum9	0.104 (2.18)*	0.253 (2.30)*
circdum10	-0.022 (0.41)	0.009 (0.07)
circdum11	-0.027 (0.44)	0.053 (0.42)
NETWORTH	N/A	0 (0.26)
Observations	3312	1701

Robust z statistics in parentheses

* significant at 5%; ** significant at 1%

Estimated coefficients reflect marginal effects when all the other independent variables are measured at their mean.

Table 5
Relationship Between Republican Financial Sacrifice and Voting Patterns
Probit Model
Dependent Variable: Probability Republican-Appointee Casts a Liberal Vote

	Model(1) (Full Sample)	Model (2) (subsample w/ Networth)
Regressors		
NETCOST	0.004 (0.47)	0.011 (0.98)
selpref	-0.036 (0.86)	-0.11 (1.46)
Age	0.002 (0.97)	0.005 (1.62)
Sex	0.02 (0.73)	0.067 (1.87)
Top Five Legal Market	0.09 (1.58)	0.085 (1.01)
PrivatePractice	-0.024 (0.77)	-0.057 (1.34)
Professor	-0.021 (0.61)	0.035 (0.47)
Judge	0.014 (0.45)	-0.09 (1.91)
TOPFIVENETCOST	-0.032 (2.08)*	-0.049 (2.17)*
circdum1	-0.043 (0.99)	-0.021 (0.35)
circdum2	0.021 (0.43)	-0.063 (0.94)
circdum3	0.049 (1.03)	0.033 (0.54)
circdum4	-0.087 (1.93)	-0.175 (2.67)**
circdum5	-0.147 (4.05)**	-0.235 (4.82)**
circdum6	-0.088 (2.18)*	-0.093 (1.60)
circdum7	-0.12 (3.39)**	-0.127 (2.55)*
circdum8	-0.163 (4.34)**	-0.188 (3.70)**
circdum9	0.015 (0.36)	-0.012 (0.21)
circdum10	-0.042 (0.95)	-0.125 (2.15)*
circdum11	-0.069 (1.71)	-0.103 (1.97)*
NETWORTH	N/A	-0.004 (1.02)
Observations	5349	2713

Robust z statistics in parentheses

* significant at 5%; ** significant at 1%

Estimated coefficients reflect marginal effects when all the other independent variables are measured at their mean.

NETCOST is not statistically significant for either party in the entire sample, or the subsample for which net worth data are available. Table 6 presents the results of the pool comparison. The net cost variable (“NETCOSTPOOL”), again measured in \$400,000 units, is not statistically significant for either democratic or republican appointees.

Table 6
 Relationship Between Pool Strength and Voting Patterns
 Probit Model
 Dependent Variable: Probability Judge Casts a Liberal Vote

Regressors	Model(1)		Model (2)	
	Dem. Appointees		Rep. Appointees	
NETCOSTPOOL	0.012	(1.37)	-0.009	(1.13)
selpref	0.02	(0.28)	-0.01	(0.35)
circdum1	0.001	(0.02)	0.04	(1.11)
circdum2	0.003	(0.07)	0.11	(2.71)**
circdum3	0.101	(1.70)	0.12	(3.06)**
circdum4	-0.105	(2.17)*	-0.02	(.61)
circdum5	-0.147	(3.41)**	-0.07	(2.33)*
circdum6	-0.05	(1.16)	-0.01	(0.29)
circdum7	-0.13	(3.03)**	-0.06	(2.27)*
circdum8	-0.07	(1.71)**	-0.09	(3.18)**
circdum9	0.14	(3.43)**	0.06	(1.76)
circdum10	0.01	(0.29)	0.001	(0.04)
Observations	3096		5349	

Robust z statistics in parentheses

* significant at 5%; ** significant at 1%

Votes by DC Circuit judges not included; 11th circuit is the baseline group.

Estimated coefficients reflect marginal effects when all the other independent variables are measured at their mean.

Both the analyses indicate that raising judicial salaries (i.e., lowering the net cost of taking the bench) would not impact judicial voting patterns in controversial cases. This empirical evidence suggests low pay does not lead to the appointment of more partisan judges, a finding consistent with the substitutes theory.

2. Citation Practices in Opinion Writing

Voting patterns are the most studied metric of judicial ideology.⁹⁹ Stephen Choi and Mitu Gulati, however, recently looked at judicial ideology through a different lens – citations to persuasive authority.¹⁰⁰ Choi and Gulati collected data on judicial opinions rendered between January 1, 1998 and December 31, 1999, amassing data on the citation practices of ninety-eight circuit judges.¹⁰¹ In particular, they examined who cites whom as persuasive authority. Choi and Gulati believe that the outside circuit citation practices can reveal a judge’s ideology: a true ideologue would not be inclined to cite an opinion by a judge from a different political party.¹⁰² For an ideologue, the reasoning of judges from the other political party is never persuasive.

Choi and Gulati found evidence of citation bias. Specifically, they found that judges tend to cite opinions from judges of the same political stripe, especially in “hot button” cases, such as civil rights and campaign finance.¹⁰³ They also found that dissent exacerbates bias. Dissenting judges and judges writing majority opinions in the face of dissent engage in more biased citation practices.¹⁰⁴ If presidents of opposing parties appointed the majority judges and the dissenting judge, the bias gets a further boost.¹⁰⁵

Choi and Gulati defined citation bias as follows: They first constructed the mean fraction of cites for a judge’s opinions to outside circuit judges from the opposite political party.¹⁰⁶ If, for example, a judge cited to outside circuit judges of the same political stripe seventy-five percent of the time, the mean fraction of cites to judges of the opposite party would be twenty-five percent. Second, Choi and Gulati controlled for the pool of potentially citable opinions.¹⁰⁷ If most judges are republican-appointees, most outside circuit citations will be to republican-appointed judges.¹⁰⁸ In this case, the failure of a

⁹⁹See, e.g., Frank B. Cross, *Decisionmaking in the U.S. Circuit Courts of Appeals*, 91 CAL. L. REV. 1457, 1497-514 (2003) (testing several theories of judicial decisionmaking by comparing judicial votes to characteristics of the judges); Richard L. Revesz, *Environmental Regulation, Ideology, and the D.C. Circuit*, 83 VA. L. REV. 1717, 1719 (1997) (finding “[political] ideology significantly influences judicial decisionmaking on the D.C. Circuit”); Donald R. Songer, *The Policy Consequences of Senate Involvement in the Selection of Judges in the United States Courts of Appeals*, 35 W. POL. Q. 107, 111 (1982) (finding some support for the hypothesis that home-state senator involvement in judicial appointment will affect the policy positions taken by judges in the United States Court of Appeals); Donald R. Songer & Martha Humphries Ginn, *Assessing the Impact of Presidential and Home State Influences on Judicial Decisionmaking in the United States Courts of Appeals*, 55 POL. RES. Q. 299, 321-22 (2002) (finding that “judicial voting behavior does reflect the political preferences of appointing Presidents”).

¹⁰⁰Choi & Gulati, *Rankings*, *supra* note 78, at 1281; Choi & Gulati, *Bias*, *supra* note 78, at 4.

¹⁰¹Choi & Gulati, *Rankings*, *supra* note 78, at 1294; Choi & Gulati, *Bias*, *supra* note 78, at 15-16.

¹⁰²Choi & Gulati, *Rankings*, *supra* note 78, at 1280; Choi & Gulati, *Bias*, *supra* note 78, at 11.

¹⁰³Choi & Gulati, *Bias*, *supra* note 78, at 19-28.

¹⁰⁴*Id.* at 29-30.

¹⁰⁵*Id.* at 31.

¹⁰⁶*Id.* at 19.

¹⁰⁷Choi & Gulati, *Rankings*, *supra* note 78, at 1294.

¹⁰⁸Choi & Gulati, *Bias*, *supra* note 78, at 16.

republican judge to cite democratic appointees would not indicate bias, but instead would merely reflect the lack of opinions in the citable pool authored by democratic appointees. To control for this, Choi and Gulati constructed a mean fraction of democratic-appointee and republican-appointee opinions in the pool.¹⁰⁹ Citation bias is the distance between the mean fraction of opposite party cites a judge makes and the mean fraction of republican opinions (for democrats) or democrat opinions (for republicans) in the pool.¹¹⁰ The closer the distance is to zero, the less prevalent the citation bias.¹¹¹

If judges who give up lots of purchasing power are more ideological than judges who give up little purchasing power, low judicial salaries should increase citation bias. To test this hypothesis, I regressed the citation bias measure from the Choi and Gulati dataset against the same set of control variables used in the voting pattern regressions. Table 7 reports the results.

Table 7
 Relationship Between Financial Sacrifice and Citation Bias
 OLS Model
 Dependent Variable: Extent of Citation Bias

	Model(1) (Direct)	Model (2) (Pool)
Regressors		

¹⁰⁹*Id.* at 18-19.

¹¹⁰*Id.* at 20; *see also* Choi & Gulati, *Rankings*, *supra* note 78, at 1295.

¹¹¹Choi & Gulati, *Rankings*, *supra* note 78, at 1295.

NETCOST	-0.001	(0.14)	N/A
selpref	-0.003	(0.21)	-0.015 (1.04)
Age	0	(0.02)	N/A
Sex	-0.003	(0.25)	N/A
Top Five Legal Market	0.028	(1.03)	N/A
NETCOSTTOPFIVE	-0.01	(1.39)	N/A
Judge	0.026	(1.54)	N/A
Professor	0.009	(0.46)	N/A
Private Practice	0.026	(1.55)	N/A
circdum1	-0.031	(1.65)	-0.02 (0.94)
circdum2	-0.01	(0.37)	-0.003 (0.10)
circdum3	0.003	(0.17)	0.004 (0.19)
circdum4	-0.013	(0.60)	-0.01 (0.48)
circdum5	-0.018	(0.91)	0.001 (0.03)
circdum6	-0.021	(0.89)	-0.02 (0.86)
circdum7	-0.022	(1.29)	-0.022 (1.07)
circdum8	-0.026	(1.44)	-0.023 (1.12)
circdum9	0.038	(1.37)	0.039 (1.24)
circdum10	-0.024	(1.12)	-0.026 (1.19)
circdum11	-0.01	(0.43)	N/A
NETCOSTPOOL	N/A		-0.007 (1.17)
Constant	0.05	(0.63)	0.089 (3.03)**
Observations	96		88
R-squared	0.24		0.22

Robust t statistics in parentheses
 * significant at 5%; ** significant at 1%

The net cost measure is statistically insignificant in the direct and pool comparisons. The sample size is small here, limiting the power of the statistical test. With that caveat in mind, at least on this crude measure, there is little evidence that low judicial salaries result in a judiciary more prone to ideological thinking.

B. *Hypothesis Two: Paying Circuit Judges More Creates a Harder Working Judiciary*

Testing whether increased judicial pay would result in a harder working judiciary requires measuring the “work effort” of circuit judges. Actual effort is unobservable, however. I do not know how many hours each judge works, the number of weekends she takes off, etc. Instead, proxies are needed – quantifiable measures of judicial output that correlate with judicial work effort. The next two subsections explore the relationship between judicial pay and two such proxies: (1) dissent rates in controversial cases; and (2) how long it takes a judge to file a published opinion after hearing oral argument in a controversial case.

1. Dissents in Controversial Cases

Dissenting takes work. For the dissenting judge, dissent requires separate drafting, finding and articulating the flaws in the majority opinion, and disagreeing publicly with the panel majority. Dissent also imposes more work on the judge writing for the majority, who often alters the majority opinion to address points raised by the dissent.¹¹² Dissent imposes other costs too. A dissenting colleague might be seen as less collegial or as someone unwilling to find common ground.¹¹³ Despite its costs, though, dissent has value. Dissents might sharpen the majority's reasoning.¹¹⁴ Circuit court dissent might convey important information to the Supreme Court about the state of the law, encouraging the grant of certiorari.¹¹⁵ Dissent can also influence the way the majority opinion is viewed by other circuit and district courts.¹¹⁶ Finally, dissent can serve as a form of judicial self-expression.¹¹⁷ Most of the benefits of dissent accrue to other judges in the circuit or people outside the judiciary. One might suspect that a judge inclined toward leisure would write fewer dissents, because the individual judge bears the cost of dissent and much of the benefits flow to others.

Table 8 presents the dissent results. The Chicago Judge's Project provides the dependent variable: the probability a judge writes a dissent in a

¹¹²Indraneel Sur, *How Far Do Voices Carry: Dissents from Denial of Rehearing En Banc*, 2006 WIS. L. REV. 1315, 1360-61; see also William J. Brennan, Jr., *In Defense of Dissents*, 37 HASTINGS L.J. 427, 429 (1986) (describing the historical objection that dissents "cloud" the majority opinion); Robert G. Flanders, Jr., *The Utility of Separate Judicial Opinions in Appellate Courts of Last Resort: Why Dissents Are Valuable*, 4 ROGER WILLIAMS U. L. REV. 401, 402-03 (1999) (stating that when a judge dissents, the writer of the majority opinion can no longer address the losing side's arguments in the way he sees fit and must face greater media scrutiny of his opinion).

¹¹³See Evan A. Evans, *The Dissenting Opinion – Its Use and Abuse*, 3 MO. L. REV. 120, 128 (1938) (mentioning the objection to dissents which says they "weaken the court in esteem and confidence of the public . . . [and] adversely affect the prompt and effective disposition of litigation"); Robert Post, *The Supreme Court Opinion as Institutional Practice: Dissent, Legal Scholarship, and Decisionmaking in the Taft Court*, 85 MINN. L. REV. 1267, 1310-11 (2001) (describing Justice William Howard Taft's dislike of dissents as "a form of egotism"); Randall T. Shepard, *What Can Dissents Teach Us?*, 68 ALB. L. REV. 337, 338 (2005); Meredith Kolsky, Note, *Justice William Johnson and the History of the Supreme Court Dissent*, 83 GEO. L.J. 2069, 2088-93 (1995).

¹¹⁴See Scott C. Idleman, *A Prudential Theory of Judicial Candor*, 73 TEX. L. REV. 1307, 1347 (1995); Lewis A. Kornhauser & Lawrence G. Sager, *The One and the Many: Adjudication in Collegial Courts*, 81 CAL. L. REV. 1, 9 (1993); Shepard, *supra* note 113, at 338.

¹¹⁵See Andrew F. Daughety & Jennifer F. Reinganum, *Speaking Up: A Model of Judicial Dissent and Discretionary Review*, 14 SUP. CT. ECON. REV. 1, 3 (2006).

¹¹⁶Sur, *supra* note 112, at 1346.

¹¹⁷See Flanders, *supra* note 112, at 404 (recounting Justices Scalia's and Cardozo's statements describing freedoms associated with writing a dissent); Idleman, *supra* note 114, at 1367-68; Kolsky, *supra* note 113, at 2086.

controversial case.¹¹⁸ The independent variables are the same as in the previous regressions. In addition, I add a variable to control for the caseload in the circuit. To do this, for any given year, I use the number of cases determined on their merits in the circuit divided by the number of active judges in that circuit.¹¹⁹ The thinking here is that higher caseloads might make dissent less likely to occur because dissent requires extra work, and judges with a high caseload might just not have the time.

Table 8
 Relationship Between Financial Sacrifice and Dissent Rates
 Probit Model
 Dependent Variable: Probability Judge Files a Dissent

	Model(1) (Full Sample)	Model (2) (Sample w/ Networth)	Model (3) (Pool)
Regressors			
NETCOST	-0.007 (3. 29)**	-0.013 (4. 13)**	N/A
selpref	0 (0. 07)	0.019 (2. 18)*	-0.005 (0. 84)
Age	0 (1. 03)	-0.001 (2. 20)*	N/A
Sex	0.007 (1. 30)	0.004 (0. 57)	N/A
Top Five Legal Market	-0.011 (1. 02)	-0.012 (0. 83)	N/A
Private Practice	-0.009 (1. 21)	-0.004 (0. 35)	N/A
Professor	-0.017 (2. 26)*	-0.027 (2. 70)**	N/A
Judge	-0.013 (1. 66)	-0.007 (0. 61)	N/A
TOPFIVENETCOST	0.006 (1. 42)	0.009 (1. 81)	N/A
circdum1	0.013 (0. 82)	-0.013 (0. 89)	0.012 (0. 72)
circdum2	-0.005 (0. 35)	-0.005 (0. 32)	-0.004 (0. 31)

¹¹⁸See SUNSTEIN ET AL., JUDGES, *supra* note 80, at 64-66 (detailing dissent results from the study).

¹¹⁹Merit terminations mean decisions in which the judges decided the case on grounds other than a procedural hurdle, such as subject matter jurisdiction or missed filing deadlines. The variable “merit terminations per judge” comes from Stefanie Lindquist, who derived the measure using data from the administrative office of the courts. For a complete description of what counts as a merit termination, see Stefanie A. Lindquist, *Bureaucratization and Balkanization: The Origins and Effects of Decision-Making Norms in the Federal Courts of Appeals*, 41 U. RICH. L. REV. 659, 668 n.31 (2007) (“[M]erits terminations [differ] from procedural terminations, which involve dispositions based on default, settlement or jurisdictional defect.”).

circdum3	0.002 (0.11)	-0.002 (0.10)	0.003 (0.16)
circdum4	0.029 (2.05)*	0.004 (0.31)	0.033 (2.19)*
circdum5	0.015 (1.18)	0.01 (0.70)	0.01 (0.85)
circdum6	0.056 (3.11)**	0.04 (2.10)*	0.058 (3.13)**
circdum7	0.002 (0.13)	-0.004 (0.27)	0.002 (0.18)
circdum8	-0.002 (0.16)	-0.002 (0.17)	-0.003 (0.28)
circdum9	0.04 (2.48)*	0.016 (1.10)	0.047 (2.83)**
circdum10	0 (0.03)	-0.01 (0.69)	-0.001 (0.04)
merits_per_idg	0 (0.76)	0 (1.17)	0 (0.85)
NETWORTH	N/A	0 (0.03)	N/A
NETCOSTPOOL	N/A	N/A	-0.002 (-0.86)
Observations	8083	4071	8083

Robust z statistics in parentheses

* significant at 5%; ** significant at 1%

Because the number of merit decisions for the D.C. Circuit was not available, votes by D.C. Circuit judges are not included in any model; the 11th Circuit is the baseline group. For a few other judges merit decisions were also not available. Those judges are not included in the regression. Estimated coefficients reflect marginal effects when all the other independent variables are measured at their mean.

For the direct comparison approach, the coefficient on net cost (NETCOST) is statistically significant for the entire sample and for the subsample where net worth data are available. The coefficient on net cost in the pool comparison (NETCOSTPOOL) is not statistically significant. The negative sign of the estimated coefficient on NETCOST suggests poorly paid judges dissent slightly less often. The idea that higher judicial pay results in fewer leisure-seeking judges and a slightly harder working judiciary overall supports the salary matters theory. But one should not overstate this result. Although the coefficient on NETCOST is statistically significant, its magnitude is tiny. Bumping federal judicial salaries up by \$50,000 a year would increase the number of dissents by a little less than one percent in controversial cases.

The results from Table 8 should be interpreted with caution for another reason as well. The results are consistent with a judiciary composed of judges trying to find common ground. It is not just the lazy judge who writes fewer dissents, but also the more considerate judge. The dissent results support either story. Given this ambiguity, the following subsection takes another approach to estimating judicial work effort: considering whether judicial salaries impact the time it takes a judge to render a published decision.

2. Time it Takes To Render a Published Opinion in Controversial Cases

Judges vary as to the speed with which they dispose of cases. Rather than consider all cases, this subsection considers the speed of disposition of those controversial cases contained in the truncated Chicago Judge's Project dataset. This limitation serves three purposes. First, these decisions involve controversial issues. A natural assumption is that judges care more about

controversial cases and, as a result, are more likely to devote their own effort to resolve these cases. In other words, judges are unlikely to simply hand off a controversial case to their clerks without any supervision.¹²⁰ Second, the decisions are all published. Accordingly, judges are less likely to delegate these cases to staff attorneys.¹²¹ Third, most of these decisions involve oral argument. The oral argument date provides an important marker. From the oral argument date forward, judges in all circuits have significant individual responsibility for case disposition.¹²² After oral argument, slow case disposition is hard to pin on the actions of other court officials, such as the clerk of courts.

Immediately after oral argument, the senior active judge on a panel or the chief judge of the circuit makes opinion assignments for all cases argued that day.¹²³ The assigned judge is responsible for drafting and circulating the opinion. After the opinion is circulated, the other judges on the panel agree, draft a separate concurrence, or draft a dissent. Occasionally, judges will informally request changes to the majority opinion.¹²⁴

¹²⁰See Penelope Pether, *Sorcerers, Not Apprentices: How Judicial Clerks and Staff Attorneys Impoverish U.S. Law*, 39 ARIZ. ST. L.J. 1, 27-28 (2007) (“Judges . . . are more likely actually to themselves decide ‘important cases (usually measured by monetary value),’ such as ‘important securities or antitrust,’ or ‘corporate tax’ cases and those brought by ‘powerful litigants.’”); William M. Richman & William L. Reynolds, *Elitism, Expediency, and the New Certiorari: Requiem for the Learned Hand Tradition*, 81 CORNELL L. REV. 273, 289 (1996) (stating that “law clerk influence is likely to be the greatest in less important cases, which are not argued and will not be published”).

¹²¹See ASHLYN K. KUERSTEN & DONALD R. SONGER, DECISIONS ON THE U.S. COURTS OF APPEALS 5 (2001).

¹²²For a discussion of the significant judicial responsibilities for opinion assignment and opinion writing which occur after oral argument, see *id.* at 6-8.

¹²³In the Third, Fifth, Sixth, Seventh, Eighth, Ninth, Tenth, Eleventh, and D.C. circuits, the published internal court rules specify that the senior active judge on the panel makes the opinion assignment. INTERNAL OPERATING PROCEDURES OF THE UNITED STATES COURT OF APPEALS FOR THE THIRD CIRCUIT § 4.2 (2002); RULE AND INTERNAL OPERATING PROCEDURES OF THE UNITED STATES COURT OF APPEALS FOR THE FIFTH CIRCUIT 34 (2006); SIXTH CIRCUIT INTERNAL OPERATING PROCEDURE § 206(a) (2007); UNITED STATES COURT OF APPEALS FOR THE SEVENTH CIRCUIT INTERNAL OPERATING PROCEDURES § 9(h) (2001); INTERNAL OPERATING PROCEDURES, UNITED STATES COURT OF APPEALS FOR THE EIGHTH CIRCUIT § 4A (2007); UNITED STATES COURT OF APPEALS FOR THE NINTH CIRCUIT, FEDERAL RULES OF APPELLATE PROCEDURE, NINTH CIRCUIT RULES & CIRCUIT ADVISORY COMMITTEE NOTES, at xxix (2007); PRACTITIONERS’ GUIDE TO THE UNITED STATES COURT OF APPEALS FOR THE TENTH CIRCUIT § 9A (2006); UNITED STATES COURT OF APPEALS FOR THE ELEVENTH CIRCUIT, FEDERAL RULES OF APPELLATE PROCEDURE WITH ELEVENTH CIRCUIT RULES AND INTERNAL OPERATING PROCEDURES 99 (2006); HANDBOOK OF PRACTICE AND INTERNAL PROCEDURES, UNITED STATES COURT OF APPEALS FOR THE DISTRICT OF COLUMBIA CIRCUIT § 12B (2007). In the Fourth Circuit, the chief judge makes the assignment whether or not he or she served on the panel. UNITED STATES COURT OF APPEALS FOR THE FOURTH CIRCUIT, INTERNAL OPERATING PROCEDURES § 36.1 (2007). In the First and Second Circuits, the internal rules do not specify opinion assignment procedures. Discussions with the clerks from these two circuits revealed that the senior active judge on the panel makes the opinion assignment after discussion with the other panel members. Telephone Interview with Opinion Clerk for the U.S. Court of Appeals for the First Circuit (August 17, 2007); Telephone Interview with Opinion Clerk for the U.S. Court of Appeals for the Second Circuit (August 17, 2007).

¹²⁴See, e.g., FRANK M. COFFIN, ON APPEAL: COURTS, LAWYERING, AND JUDGING 219-20 (1994) (describing the often touchy nature of informal comments on the opinions of other judges).

For each case in the truncated dataset, information on the date argued and date published was culled from Westlaw. Each case involved three-judge panels. The speed of disposition information was matched for a specific judge on a panel if that judge wrote the majority opinion, a separate concurrence, or a dissent. These judges do more than vote, and these “writing” activities might affect the speed of the decision.

The dependent variable is speed of disposition. The independent variables include all the controls used in the prior regressions. In addition, I controlled for whether the judge writing the majority opinion faced either a concurring opinion or a dissent. The thinking is that those opinions might take longer to write as the writing judge responds to points raised in either the dissent or the concurrence. Table 9 reports the results.

Table 9
Relationship Between Financial Sacrifice and Speed of Disposition in
Controversial Cases
OLS Model

Dependent Variable: Days Between Oral Argument Date and Disposition Date

	Model(1) (Full Sample)		Model (2) (Sample w/ Networth)		Model (3) (Pool)	
Regressors						
NETCOST	0.699	(0. 23)	6.671	(1. 61)	N/A	
selpref	8.19	(1. 08)	-2.104	(0. 18)	2.474	(0. 37)
Age	0.504	(0. 88)	0.311	(0. 33)	N/A	
Sex	14.971	(2. 21)*	21.483	(1. 94)	N/A	
Top Five Legal Market	32.102	(1. 89)	75.289	(2. 86)**	N/A	
Private Practice	-45.71	(3. 91)**	-4.433	(0. 27)	N/A	
Professor	-51.186	(4. 20)**	-18.884	(0. 86)	N/A	
Judge	-39.631	(3. 41)**	-0.711	(0. 04)	N/A	
TOPFIVENET COST	-12.789	(2. 42)*	-19.42	(2. 25)*	N/A	
circdum1	-39.355	(1. 81)	1.511	(0. 07)	-42.283	(1. 97)*
circdum2	22.879	(1. 06)	57.717	(2. 46)*	22.54	(1. 07)
circdum3	-2.477	(0. 11)	31.198	(1. 17)	-2.72	(0. 12)
circdum4	-29.076	(1. 55)	-27.638	(1. 35)	-25.034	(1. 37)
circdum5	-10.975	(0. 59)	-1.318	(0. 06)	-15.289	(0. 84)
circdum6	-1.262	(0. 06)	34.772	(1. 42)	-0.358	(0. 02)
circdum7	-1.014	(0. 05)	17.806	(0. 79)	-10.973	(0. 55)
circdum8	-27.64	(1. 52)	-0.629	(0. 03)	-32.472	(1. 83)
circdum9	20.733	(1. 04)	33.785	(1. 57)	17.454	(0. 89)
circdum10	34.548	(1. 44)	52.448	(2. 02)*	27.362	(1. 17)
merits_per_jdg	0.007	(0. 07)	0.105	(0. 86)	0.014	(0. 14)
secondary opinion	65.592	(11. 48)**	67.055	(8. 15)**	65.638	(11. 44)**
NETWORTH	N/A		-1.017 (2. 88)**		N/A	
NETCOST POOL	N/A		N/A		1.693	(0. 62)
constant	151.266	(3. 16)**	67.386	(1. 18)	137.97	(4. 34)**
Observations	2696		1303		2696	
R-squared	0.1		0.11		0.09	

Robust t statistics in parentheses. * significant at 5%; ** significant at 1%. Votes by DC Circuit judges and some other judges are not included in any model because merit decisions were not available; 11th circuit is the baseline group. Estimated coefficients reflect marginal effects when all the other independent variables are measured at their mean.

The net cost measure is not statistically significant in either the direct or pool comparisons. This finding suggests that low judicial pay does not change the speed of case disposition in controversial cases.¹²⁵ It is noteworthy that the dummy variable “Private Practice” is statistically significant, suggesting that those judges coming from private practice write opinions faster than those coming from positions as government lawyers. To the extent that low judicial salaries deter some private sector lawyers from joining the bench, one might expect low salaries to decrease the speed of disposition of cases. But even this effect is not terribly big. Lawyers directly from the private sector decide cases about a month and a half faster than government lawyers.

C. *Hypothesis Three: Paying Circuit Judges More Creates a Judiciary Less Motivated by Its Own Influence*

Outside circuit citations roughly capture judicial influence. Rules of precedent dictate inside circuit citations; that is to say, circuit precedent must be followed and cited.¹²⁶ By contrast, judges cite outside circuit opinions as persuasive authority to bolster arguments in their own opinions.¹²⁷ True, occasionally opinions criticize or distinguish an outside circuit opinion, but the need for such treatment still demonstrates the opinion’s impact. After all, an opinion that is ignored is less influential than an opinion which a judge feels obliged to consider.¹²⁸

¹²⁵The results of a more complicated duration model, not reported here, were substantially similar to the OLS results.

¹²⁶Unsurprisingly, every circuit follows this rule. Arranged by order of circuit number, see *Clockedile v. N.H. Dep’t of Corr.*, 245 F.3d 1, 4-5 (1st Cir. 2001); *Shain v. Ellison*, 273 F.3d 56, 70 (2d Cir. 2001); *Martinez-Sanes v. Turnbull*, 318 F.3d 483, 488 (3d Cir. 2003); *Statewide Reapportionment Advisory Comm. v. Beasley*, 99 F.3d 134, 134 (4th Cir. 1996); *Wicker v. McCotter*, 798 F.2d 155, 157-58 (5th Cir. 1986); *Smith v. U.S. Postal Serv.*, 766 F.2d 205, 207 (6th Cir. 1985); *United States v. Polichemi*, 201 F.3d 858, 863 (7th Cir. 2000); *Emergency Med. Serv., Inc. v. St. Paul Mercury Ins. Co.*, 495 F.3d 999, 1008 (8th Cir. 2007); *United States v. Bolanos-Hernandez*, 492 F.3d 1140, 1146 n.3 (9th Cir. 2007); *United States v. Bush*, 405 F.3d 909, 922 n.7 (10th Cir. 2005); *United States v. Duncan*, 400 F.3d 1297, 1305 (11th Cir. 2005); *United States v. Carson*, 455 F.3d 336, 384 n.43 (D.C. Cir. 2006).

¹²⁷*E.g.*, *United States v. Mosley*, 454 F.3d 249, 266 (3d Cir. 2006); *United States v. Cartwright*, 413 F.3d 1295, 1298 (11th Cir. 2005); *Blue Cross & Blue Shield Mut. v. Blue Cross & Blue Shield Ass’n*, 110 F.3d 318, 328 (6th Cir. 1997); *see also* William M. Landes et al., *Judicial Influence: A Citation Analysis of Federal Court of Appeals Judges*, 27 J. LEGAL STUD. 271, 272-73 (1998) (stating that “citations to an opinion from within a circuit may reflect either the opinion’s precedential or persuasive effect, while citations to an opinion from another circuit will reflect its persuasive effect alone”).

¹²⁸Landes et al., *supra* note 127, at 273. Outside circuit citation counts are, of course, an imprecise and messy measure of judicial influence. *See* Daniel A. Farber, *Supreme Court Selection and Measures of Past Judicial Performance*, 32 FLA. ST. U. L. REV. 1175, 1188-92 (2005); Stephen J. Choi & G. Mitu Gulati, *Choosing the Next Supreme Court Justice: An Empirical Ranking of Judge Performance*, 78 S. CAL. L. REV. 23, 54-58 (2004) (investigating whether the quality of opinions or the “outrageousness” of the judge

A judge who greatly valued her own influence would write more published opinions and try to ensure each opinion attracted more outside citations. The idea is that this judge – the influence maximizer – would write more opinions that “sell” in the opinion-citation market. Perhaps the influence maximizer would write shorter opinions, delegate less opinion writing to clerks, or spend more time ensuring the reasoning of opinions is sound and persuasive. In contrast to the judge who, say, valued leisure, the judge who valued influence would write more opinions and spend a lot of time on each one.

The salary matters theory predicts that low judicial pay leads to the appointment of judges who place a high value on judicial influence, and thus judges who gave up a lot of money to take the bench should be more influential than judges who gave up a little bit of money. As noted earlier, judges who make the biggest financial sacrifice probably have the greatest “taste” for judging.¹²⁹ One manifestation of a strong taste for judging is a need to be influential. To satisfy this need, influence-motivated judges might work hard to ensure they are cited.

To test this claim, I use citation data collected by William Landes, Larry Lessig, and Mike Solimine.¹³⁰ Landes et al. gathered data for 205 federal circuit judges on the bench in 1992 and looked at the number of outside circuit citations to the opinions authored by these judges. To measure impact, they considered two different models of outside circuit citation.¹³¹ First, they constructed a model of total influence.¹³² In this model, Landes et al. measured the raw number of citations to a judge’s opinions and then controlled for, among other things, the length of judicial tenure (obviously a judge who has been around longer will have more citations).¹³³ The second model – average influence – measured the number of citations per opinion, controlling for other factors.¹³⁴ A judge that scores well in average influence but low in total influence writes fewer opinions, but each one is a “gem.”¹³⁵ The opposite is true for a judge that scores well in total influence and low in average influence.¹³⁶ This judge floods the market with opinions, each one garnering relatively modest outside attention.¹³⁷

contributes to a high citation count). Although not all of the problems with using citation counts for measuring academic influence transfer, some do. See Nancy Levit, *Defining Cutting Edge Scholarship: Feminism and Criteria of Rationality*, 71 CHI.-KENT L. REV. 947, 949-52 (1996). For example, there could be outside circuit “citation clubs” – judges only citing other judges that cite them back. Furthermore, judicial citations might be more a matter of luck than judicial influence. Since cases are assigned randomly to panels, a judge’s opinion might be cited frequently because that judge was the first to rule on an issue. With these caveats in mind, citations represent the best available measure of opinion quality and the most used quantitative metric to assess judicial influence.

¹²⁹See *supra* text accompanying notes 41-42.

¹³⁰Landes et al., *supra* note 127, at 276-79.

¹³¹*Id.* at 280.

¹³²*Id.*

¹³³*Id.* The other controls include: (1) whether the judge was on senior status when the opinion issued; and, (2) dummy variables accounting for whether the opinion issued in the judge’s first, second, or third years of the bench. *Id.* at 282-83.

¹³⁴Landes et al., *supra* note 127, at 280.

¹³⁵*Id.* at 280-81.

¹³⁶*Id.* at 281.

¹³⁷*Id.*

Landes et al. then measured judicial influence in terms of citations, above what a judge's tenure, status, and other control variables predict.¹³⁸ For example, in terms of total influence, the estimated coefficient for Judge Posner is 4.41.¹³⁹ This coefficient means that Judge Posner's influence is a little less than four and a half percent higher than predicted by his tenure, status, and other controls.

Tables 10 and 11 report the results of the total influence and average influence regressions respectively.

Table 10
 Relationship Between Financial Sacrifice and Total Number of Outside Circuit Citations
 OLS Model
 Dependent Variable: Total Influence Measure

	Model(1) (Direct)	Model (2) (Pool)
Regressors		
NETCOST	0.044 (1. 11)	N/A
selpref	-0.317 (3. 30)**	-0.025 (2. 52)*

¹³⁸*Id.* at 284-302.

¹³⁹*Id.* at 288, tbl. 2A.

Age	-0.002	(0.21)	N/A
Sex	-0.115	(1.31)	N/A
Top Five Legal Market	-0.193	(1.20)	N/A
Private Practice	0.027	(0.26)	N/A
Professor	0.21	(1.46)	N/A
Judge	0.112	(1.05)	N/A
TOPFIVENETCOST	0.108	(1.59)	N/A
NETCOSTPOOL	N/A		0.056 (1.74)
Constant	2.9	(6.05)**	2.828 (24.70)**
Observations	141		132
R-squared	0.15		0.04

Robust t statistics in parentheses

* significant at 5%; ** significant at 1%

D.C. Circuit judges left out of the pool model.

Table 11
Relationship Between Financial Sacrifice and Average Number of Outside
Circuit Citations
OLS Model
Dependent Variable: Average Influence Measure

	Model(1) (Direct)	Model (2) (Pool)
Regressors		
NETCOST	0.035 (1.58)	N/A
selpref	-0.262 (4.60)**	-0.208 (3.50)**
Age	0 (0.03)	N/A
Sex	-0.004 (0.06)	N/A
Top Five Legal Market	-0.069 (0.79)	N/A
Private Practice	-0.061 (0.82)	N/A
Professor	0.001 (0.01)	N/A

Judge	0.032	(0.42)	N/A
TOPFIVENETCOST	0.024	(0.69)	N/A
NETCOSTPOOL	N/A		0.034 (1.78)
Constant	0.173	(0.61)	0.159 (2.43)*
Observations	140		131
R-squared	0.15		0.07

Robust t statistics in parentheses

* significant at 5%; ** significant at 1%

D.C. Circuit judges left out of the pool model.

If low salaries result in a judiciary composed of more people who highly value their own judicial influence, the coefficient on financial sacrifice should be positive and significant. In both the total influence regression and the average influence regression the coefficients on “NETCOST” and “NETCOSTPOOL” are just barely insignificant. The take away is that the citation data are consistent with the substitutes theory: lowering the financial sacrifice judges must make would not change opinion quality all that much. True, the effects here border on statistically significant, but the estimated coefficients are nonetheless tiny. The best prediction is that increasing judicial pay by \$50,000 a year for eleven years would decrease opinion quality by between three and five percent.

IV. POTENTIAL OBJECTIONS

This last section deals with potential objections to the analysis. The first set of objections has to do with the data. As noted earlier, the opportunity cost measure is imprecise.¹⁴⁰ One weakness is that the measure does not capture the fact that some judges would have made better law firm partners than others. That said, the data source used, the Survey of Law Firm Economics, provides the most comprehensive overview of the national law firm market.¹⁴¹ The survey has been published over a longer period of time than any other law firm salary database.¹⁴² Thus, it provides the best source for comparable law firm partner salaries.¹⁴³

¹⁴⁰See *supra* notes 54-55 and accompanying text.

¹⁴¹See *supra* note 53.

¹⁴²The American Lawyer first published the AmLaw 100 in 1993 and the AmLaw 200 in 1999. The National Association for Law Placement (NALP) is the other common source of law firm salary information. While more geographically comprehensive than the American Lawyer Series, the NALP data suffers a different flaw: NALP reports first year associate salaries only. See, e.g., NAT'L ASS'N FOR LAW PLACEMENT, 2006-2007 NALP DIRECTORY OF LEGAL EMPLOYERS (2006). Obviously, a comparison to first-year associate salaries would understate the opportunity cost for a seasoned lawyer deciding to take the federal bench.

¹⁴³The AmLaw 100 and the Am Law 200 report salaries from the prominent national firms only. For some judges like, say, Judge Frank Easterbrook of the Seventh Circuit, partner salary in a prominent firm is a closer measure of his true opportunity cost. While perhaps getting a clearer picture of Judge Easterbrook's lost earnings, the Am Law 100 and Am Law

The second data objection is that all the analysis really captures are regional differences in law firm salaries and differences across the appointees' age at the time of appointment. After all, older candidates give up less money and candidates across circuits give up different amounts of money. Under this objection, the NETCOST measure is not really judge-specific in any sense other than region and age; the variation in salary that drives the analysis is really just variation across circuits and the appointees' ages at appointment. NETCOST does not provide additional information that is not already available in the circuit dummy variables and the age variable. True, NETCOST, age, and the circuit dummies are highly correlated. This "multicollinearity" increases the standard errors, which might then generate the insignificant results. This is a serious objection, but not decisive.

Age and circuit specific effects explain about sixty percent of the variance in NETCOST, leaving additional explanatory value to the NETCOST measure. Second, multicollinearity leads to large standard errors, which increases the confidence intervals. There is no reason, however, to suspect that the NETCOST coefficient is a biased estimate. More importantly, even if the true effects of higher salaries rest at the extreme ends of the confidence intervals, the effects are nonetheless practically trivial for most measures of judicial performance.

Another related data objection is this: if some people who give up a lot of money are motivated by the power to affect policy, others motivated by influence, others motivated by a desire for leisure, and still others motivated by a call to public service, each of these people will perform differently on the various measures of judicial performance. As a result, the statistical tests will contain a lot of noise. The policy-motivated judge who cares little about her influence will vote her policy preferences, but will not invest energy in writing opinions that other judges will cite. The leisure-maximizing judge will seldom vote her policy preferences, but will always take a long time to write her opinions. The influence-motivated judge will write well-cited opinions, but will not always vote strictly along party lines. Because there are many reasons a person might forgo income to become a judge, the statistical tests cannot tease out any single "true" motivation. This results in a failure to find a statistical relationship between financial sacrifice and judicial performance.

This objection is not serious, given the purpose of the analysis. Basically, the objection says that the findings are consonant with low judicial salaries attracting a hodgepodge of folks with different motivations. These people will perform differently along various metrics of judicial performance and those different performances will largely cancel each other out. That is fair enough. The end result is the same: no link between judicial salaries and judicial performance, and little empirical support for raising judicial salaries.

The next objection involves errors in the measurement of judicial performance. The analysis focuses on the "measurables" – voting patterns, citation counts, dissents, time to decision, etc. It does not immediately follow

200 present significant other problems. Unlike the Law Firm Survey, the Am Law 100 and Am Law 200 do not report anticipated increases in compensation due to increased seniority in the firm, an important part of the net cost calculation. Second, the Am Law 100 and Am Law 200 do not provide information for many of the judges on the federal bench. For example, there are simply no Am Law 100 or Am Law 200 firms operating in Cheyenne, Wyoming (Judge O'Brien) or Columbia, South Carolina (Judge Hamilton).

from the finding that the “measurables” would not change much that the judiciary would not look different with higher salaries. There are not data on everything that goes into judicial performance. And even the output that is measured correlates only imperfectly with the “true” judicial product. Moreover, many non-measured attributes that go into making a good judge might be influenced by higher salaries. Higher salaries might, for example, attract those committed to the judiciary as an institution – people just trying to do a good job without baser motives. The analysis says nothing about possibilities like this.

One final set of objections involves some other potential costs of low judicial salaries. Allowing judicial salaries to lag significantly behind private sector salaries might signal that a circuit judge is less valuable than a run-of-the-mill lawyer. The weak signal could then impact how the public feels about the judiciary. Alternatively, judges might be demoralized because they make less than judicial clerks do in their first year after leaving a judge’s chambers. Under this concern, relative pay is what matters to the judge, not absolute pay. With low relative pay, judges feel undervalued and, as a result, do a worse job.¹⁴⁴ These final two objections are valid. I do not test for them, but that does not mean they are unimportant.

With respect to federal circuit court judges, the analysis is the best that can be done with the available data. The statistical analysis hunts for a “constitutional crisis,” for some impact of judicial salaries on judicial performance. It measures the impact of judicial salaries by two methods – pool comparisons and direct comparisons – taking both methods to a wide variety of judicial output measures. Yet despite this hunt, these data show judicial salaries have a minimal impact on judicial performance. This Article shifts the burden to the advocates for higher judicial pay. The advocates need to show that the impact on softer variables and concerns outweighs the tiny effect of higher judicial salaries on measurable aspects of judicial performance.

CONCLUSION

Chief Justice Roberts, his brethren, and many prominent members of the legal community have issued statements about the corrosive effect of low judicial salaries. The heated rhetoric is itself telling: low judicial salaries are creating a “constitutional crisis”;¹⁴⁵ because of low salaries “the nation is in danger of having a judiciary that is no longer considered one of the leading judiciaries of the world”;¹⁴⁶ and “eroding federal judicial salaries will lead, sooner or later, to less capable judges and ultimately to inferior adjudication.”¹⁴⁷

This Article is the first to test whether judicial salaries really do impact judicial performance. Given the available data, the effect of low judicial pay is non-existent, at least when judicial pay is measured against the next best financial opportunity for most circuit judges. Low pay does not impact voting

¹⁴⁴POSNER, HOW JUDGES THINK, *supra* note 67.

¹⁴⁵2006 Year-End Report, *supra* note 1, at 1.

¹⁴⁶Judicial Security and Independence, Justice Kennedy’s testimony, *supra* note 2, at 6-7.

¹⁴⁷Fed. Judicial Compensation, Justice Alito’s testimony, *supra* note 2, at 2.

patterns, citation practices, the speed of controversial case disposition, or opinion quality. Low pay does lead to slightly fewer dissents. While statistically significant, the magnitude of this effect is slight.

Low judicial salaries might have a corrosive character. The source of the corrosion, however, rests outside judicial performance. Chief Justice Roberts is probably half right: low judicial salaries erect a barrier to entry onto the bench for some candidates. But this barrier is inconsequential if those candidates who are willing to take judgeships are indistinguishable from those candidates driven from the applicant pool by low judicial salaries. That is the story these data support.

RESPONSE

REFINING THE JUDICIAL SALARY/JUDICIAL PERFORMANCE DEBATE: A RESPONSE TO PROFESSORS CROSS, CZARNEZKI, HENDERSON, MARKS, AND ZORN

SCOTT BAKER

Three years ago, I began collecting data for the article “Should We Pay Federal Circuit Judges More?”¹⁴⁸ At the outset, I had a hunch: Low judicial pay was affecting judicial performance. Specifically, low pay resulted in federal circuit judges that were more partisan, more prone to leisure, and more motivated by the prospect of their own influence. I suspected to discover a statistically significant and economically meaningful link between judicial pay and judicial performance. Scholars, after all, always treat the converse -- statistically insignificant results -- with skepticism. The failure to reject a null hypothesis of no association does not prove that the variables, in fact, lack association..

After conducting the analysis, the data did not support my hunch. For most of my measures of judicial performance, I did not find a statistically significant effect. These “non-results” were fairly precise, however. The confidence intervals of the estimates were tight around zero, enabling me to reject large effects of salary on the performance measures.¹⁴⁹ Given this, the results stood in stark contrast to Chief Justice John Roberts’s hypothesis that low judicial pay was causing a constitutional crisis. So, I decided to publish the article. At the same time, I placed my data and the statistical programs underlying the analysis in the public domain. That way, other researchers could replicate, critique, and improve on the project. In a welcome development, that is exactly what has happened.

The three replies in this issue represent generous and illuminating responses to the work. They offer valid criticisms and important refinements to the claims made in the paper. Indeed, I agree with most of what these scholars say. But after all this discussion about statistics, economic theory, and data, a question remains unanswered: What, if any, impact of judicial pay on judicial performance justifies a pay raise? Framed this way, notice how the data have shifted the debate from the assertion of a “constitutional crisis” toward a

Professor of Law and Professor of Economics (courtesy), UNC Chapel Hill, School of Law. Thanks to John Conley, Doug Lichtman, Mitu Gulati, Adam Feibelman, Anup Malani, and especially Tom Mroz for helpful suggestions on this response.

¹⁴⁸Scott Baker, *Should We Pay Federal Circuit Judges More?*, 88 B.U. L. REV. 63 (2008).

¹⁴⁹In the original paper, all the significance results are from two-tailed tests. The results are much the same if a one-tailed significance test is employed instead. See Christopher Zorn, William D. Henderson, & Jason J. Czarnezki, *Working Class Judges*, 88 B.U. L. REV. XXX, XXX tbl.1.

deeper investigation about the size and kind of concrete results we hope to achieve with higher pay. My article just starts that discussion. Coupled with these replies, my hope is that this work will spur on further efforts to uncover links between judicial pay and judicial performance.¹⁵⁰

My response to the replies comes in three parts. Part I responds to Frank Cross's concerns about the statistical analysis itself and the inferences drawn from that analysis. Part II considers the effects of salary increases on judges coming from "top-five" markets as identified by Jason Czarnezki, Bill Henderson, and Chris Zorn (collectively "CHZ"). Part III comments on Stephen Marks's two objections to my measure of a judge's opportunity cost.

I. WHAT'S THE NULL?

Professor Cross makes four points in his reply. First, he suggests several reasons why my estimate of opportunity cost (NETCOST) does not capture the real opportunity cost for a specific judge. Most salient is the crudeness of the law firm salary data – it reflects average partnership income by region. There is no reason to suspect that judges from a specific city in a region would make the average partner salary for that region overall. Second, he questions whether any of the judicial performance measures used truly capture "judicial quality." If not, the failure to find a statistical correlation between a judge's financial sacrifice and those measures means little. Third, he argues that my voting pattern results are limited because I fail to control for possible panel effects. The omission of this relevant independent variable will tend to bias the results. Depending on the direction of the bias, the regressions will overestimate or underestimate the true effect of financial sacrifice on judicial performance. Finally, he asserts that I put too much faith in the failure to reject the null hypothesis. In conventional statistical significance testing, the null hypothesis is that no relationship exists between the independent and dependent variables. Strictly speaking, the study cannot confidently rule out that the estimated coefficients are the result of chance, rather than reflecting an underlying association between the variables of interest.¹⁵¹ In short, Professor Cross argues that my data and analysis are just too limited to do the job asked, i.e., assessing whether there is empirical support for the proposition that judicial pay impacts judicial performance. Given the failure of the statistical project, he advocates relying on anecdotal evidence and economic theory, including consideration of behavioral economics.

¹⁵⁰The ball has started rolling on this topic. See Stephen Choi, Mitu Gulati, & Eric Posner, *Are Judges Overpaid?: A Skeptical Response to the Judicial Salary Debate* (Univ. of Chicago Law and Economics, Olin Working Paper 376, available at papers.ssrn.com/sol3/papers.cfm?abstract_id=1077295 (finding that salary does not have much of an impact on the behavior of state court judges); Reed Watson & Matthew Wolfe, *Comparing Judicial Compensation: Apples, Oranges, and Cherry Picking* (unpublished manuscript on file with author) (finding that, when making international comparisons of judicial salaries, the justices "cherry pick the highest paid judiciaries, but not necessarily the best performing ones"). The debate about judicial salaries continues to rage in the public domain. See George F. Will, *Bargain Basement Judiciary*, WASH. POST, Mar. 23, 2008, at B07. A New York State Supreme Court Justice has even filed a lawsuit trying to force the state comptroller to increase pay. See Anemona Hartocollis, *New York's Top Judge Sues Over Judicial Pay*, N.Y. TIMES, Apr. 11, 2008, at A4.

¹⁵¹See DAVID FREEDMAN ET AL., STATISTICS 478 (3rd ed. 1998).

I agree that the opportunity cost measure is imprecise. The article corrects the imprecision a few different ways, but none of the corrections are completely satisfying.¹⁵² The best data would come from the judges themselves – self-reports of the income they gave up for the bench. Absent that, I don't know of a better way to estimate opportunity costs or a better data source to use. For the reasons discussed in the article, the estimate likely correlates with a judge's true opportunity cost.¹⁵³ Even if all the judicial candidates would have been "above average" partners, rather than "average" partners, the analysis still holds if average and above-average partnership incomes move together.¹⁵⁴ True, if some candidates are better private sector lawyers than others, the assumption that all nominees forgo an average or above average partnership salary weakens the analysis. Nonetheless, the "private practice" dummy variable should pick up part of any differential effects. Suppose, as is likely, that nominees coming directly from private practice are the better, more successful private sector lawyers – their relative success made them more likely to remain in private practice before their appointment. The "private practice" dummy variable should then capture differences in opportunity cost attributable to differences in the success at practicing law. In the end, unfortunately, the precise degree of correlation between NETCOST and the judges' true opportunity cost is hard to know. As such, all the results must be taken with this measurement error in mind.¹⁵⁵

Professor Cross is also correct that my judicial performance measures don't perfectly capture judicial quality.¹⁵⁶ But if not these measures, what measures exist to assess the quality of federal circuit court judges? The short answer – none – is unsatisfying. The "I-know-a-good-federal-circuit-judge-when-I-see-one" angle is hard to test. The article employs every metric I could think of, including most of the metrics used by scholars studying the circuit courts.¹⁵⁷ At

¹⁵²Corrections include: (1) adding a dummy variable, TOPFIVE, for whether the judge came from a city in a top five legal market and (2) adding a variable interacting TOPFIVE with the NETCOST measure. For a fuller discussion of this interaction dummy, see Zorn, Henderson & Czarneski, *supra* note 149 and *infra* Part II.

¹⁵³Most of the judges in the sample (239 out of 259) remained in the same region for the ten years prior to taking the bench. Part III.A., *infra*, discusses the consequences of relaxing the assumption that no judges would have left their region for a law firm job in a higher paying region.

¹⁵⁴See JEFFREY M. WOOLDRIDGE, *INTRODUCTORY ECONOMETRICS* 40 (explaining how the variance in – not the absolute value of – independent variables determines the predictive power of a regression analysis).

¹⁵⁵On the significant consequences of mismeasuring independent variables, see PETER KENNEDY, *A GUIDE TO ECONOMETRICS* 137 (3rd ed. 1992).

¹⁵⁶Professor Marks raises this same concern in his reply. See Steven Marks, *A Comment on the Relationship Between Judicial Salary and Judicial Quality*, 88 B.U. L. REV. XXX (2008).

¹⁵⁷For scholars studying voting patterns in the circuit courts, see, for example, VIRGINIA A. HETTINGER, STEFANIE A. LINDQUIST & WENDY L. MARTINEK, *JUDGING ON A COLLEGIATE COURT: INFLUENCES ON FEDERAL APPELLATE DECISION MAKING* (2006); Frank B. Cross, *Decisionmaking in the U.S. Circuit Courts of Appeals*, 91 CAL. L. REV. 1457 (2003); Orin S. Kerr, *Shedding*

the start of the project, my sense was that Congress would care about these measures when considering a judicial pay raise. Suppose that the study had found statistically significant and economically meaningful correlations between financial sacrifice and voting patterns in controversial cases, dissent rates, the time it takes to render decisions, citation practices in opinion writing, and the number of outside circuit citations opinions tend to garner. In that case, I suspect Chief Justice John Roberts himself would have pointed to the study to “prove” to Congress the need for higher salaries.

Professor Cross’s suggestion to control for panel effects also resonated.¹⁵⁸ So, I did just that. Panel effects arise when a circuit judge’s vote is influenced by the political proclivities of the other judges on the panel deciding a particular case. The new voting pattern regressions are reported in Table 1. The panel effects have the expected sign and significance level. Democratic-appointees voting with two other democratic-appointees were eleven percent more likely to cast a liberal vote. Republican-appointees voting with two other republican-appointees were three percent more likely to cast a conservative vote. Inclusion of panel effects did not alter the results on the opportunity cost variable.

Table 1

Light on Chevron: An Empirical Study of the Chevron Doctrine in the U.S. Courts of Appeals, 15 YALE J. ON REG. 35-37 (1998); Richard L. Revesz, *Environmental Regulation, Ideology, and the D.C. Circuit*, 83 VA. L. REV. 1717 (1997); Cass R. Sunstein, David Schkade & Lisa Michelle Ellman, *Ideological Voting on Federal Courts of Appeals: A Preliminary Investigation*, 90 VA. L. REV. 301 (2004); Donald R. Songer & Sue Davis, *The Impact of Party and Region on Voting Decisions in the United States Courts of Appeals, 1955-1986*, 43 W. POL. Q. 317 (1990). For scholars studying dissenting behavior in the circuit courts, see Stephen J. Choi & G. Mitu Gulati, *Mr. Justice Posner? Unpacking the Statistics*, 61 N.Y.U. ANN. SURV. AM. L. 19 (2005); Jeffrey A. Lefstin, *The Measure of the Doubt: Dissent, Indeterminacy, and Interpretation at the Federal Circuit*, 58 HASTINGS L.J. 1025 (2007); Sunstein et al., *supra*. For scholars studying the time it takes for decisions, see Stefanie A. Lindquist, *Bureaucratization and Balkanization: The Origins and Effects of Decision-Making Norms in the Federal Appellate Courts*, 41 U. RICH. L. REV. 659 (2007). For scholars using the impact of outside circuit citations as a measure of opinion quality, see Stephen J. Choi & G. Mitu Gulati, *Choosing the Next Supreme Court Justice: An Empirical Ranking of Judge Performance*, 78 S. CAL. L. REV. 23 (2004); William M. Landes et al., *Judicial Influence: A Citation Analysis of Federal Courts of Appeals Judges*, 27 J. LEGAL STUD. 271 (1998).

¹⁵⁸Professor Cross was the first legal scholar to consider panel effects. See Frank B. Cross & Emerson H. Tiller, *Judicial Partisanship and Obedience to Legal Doctrine: Whistleblowing on the Federal Courts of Appeals*, 107 YALE L. J. 2155 (1998). Political scientists had looked at such effects earlier. See, *example.g.*, Burton M. Atkins, *Judicial Behavior and Tendencies Toward Conformity in a Three-Person Small Group: A Case Study of Dissent Behavior on the U.S. Court of Appeals*, 54 SOCIAL SCI. Q. 41 (1973). The panel effects literature has now blossomed. See generally Sunstein et al., *supra* note 157; Thomas J. Miles & Cass R. Sunstein, *The Real World of Arbitrariness Review*, 75 U. CHI. L. REV. (forthcoming 2008); Sean Farhang & Gregory Wawro, *Institutional Dynamics on the U.S. Court of Appeals: Minority Representation Under Panel Decision Making*, 20 J. L. ECON & ORG. 299 (2004); Pauline T. Kim, *Deliberation and Strategy on the United States Courts of Appeals: An Empirical Exploration of Panel Effects* (unpublished manuscript available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1115357).

Relationship Between Financial Sacrifice and Voting Patterns
Controlling for Panel Effects
Probit Model

Regressors	Model(1) dem. judges	Model (2) dem. judges networth sample	Model (3) rep. judges	Model (4) rep. judges networth sample
NETCOST	0.01 (0.89)	0.01 (0.65)	0.007 (0.77)	0.006 (0.49)
selpref	0.107 (1.10)	0.253 (1.71)	0.014 (0.28)	-0.079 (0.82)
Age	0.002 (0.92)	0 (0.11)	0.003 (1.14)	0.004 (1.05)
Sex	-0.007 (0.27)	0.009 (0.24)	0.025 (0.78)	0.079 (1.91)
Top Five	-0.07 (1.11)	-0.21 (1.74)	0.088 (1.35)	0.072 (0.75)
PrivatePractitioner	-0.04 (0.81)	-0.132 (1.53)	0.004 (0.11)	-0.017 (0.33)
Professor	-0.008 (0.14)	-0.081 (0.75)	0.006 (0.15)	0.052 (0.60)
Judge	-0.044 (0.88)	-0.119 (1.47)	0.024 (0.67)	-0.053 (0.95)
TOPFIVE NETCOST	0.025 (0.97)	0.121 (1.50)	-0.024 (1.37)	-0.04 (1.51)
demjudge/ dempanel	0.117 (3.92)*	0.145 (3.55)*	N/A	N/A
demjudge/ repubpanel	-0.01 (0.41)	-0.009 (0.27)	N/A	N/A
repjudge/ dempanel	N/A	N/A	0.027 (1.09)	0.023 (0.68)
repjudge/ reppanel	N/A	N/A	-0.037 (2.13)*	-0.056 (2.26)*
NETWORTH	N/A	0.001 (0.53)	N/A	-0.002 (0.49)
NETCOST (topfive)	0.03 (1.37)	0.13 (1.62)	-0.16 (0.89)	-0.03 (1.26)
circuit dummies	Yes	Yes	Yes	Yes
Observations	2338	1166	3934	1957
Pseudo R- squared	0.04	0.05	0.02	0.02

Robust z statistics in parentheses
significant at 1%

* significant at 5%; **

Estimated coefficients reflect marginal effects when all independent variables are measured at their mean. The base category for the panel effects is a judge voting with a split panel: one democratic-appointee, one republican-appointee. My dataset did not include judges appointed before 1974, after 2004, and district court judges sitting by designation. Since I constructed panel effects for those cases where three judges in my dataset participated in the decision, the number of observations differs from those reported in the original article. In light of CHZ's reply, I also report NETCOST (Topfive) as the estimate for judges from top-five markets.

Finally, Professor Cross correctly points out that researchers rarely rely on statistically insignificant results. The lack of significance could mean a bunch of things. It could be the result of mis-measured data, not enough data, too

much correlation between the independent variables, or it could mean no association between the variables of interest.¹⁵⁹ A small number of studies do, however, rely on and report statistically insignificant results.¹⁶⁰ And when they do, even with all the limitations noted above, it is because our intuition, economic theory, or the previous literature tells us that there should be a correlation.

The link between judicial salaries and judicial performance fits that bill. The reason is the nature of the claims advanced by the advocates of higher judicial pay, especially the Chief Justice. Conceding all the problems identified by Professor Cross, my data and analysis tell another side to the “constitutional crisis” story bandied about in the public domain and before Congress.¹⁶¹

¹⁵⁹See WOOLDRIDGE, *supra* note 154, at 135 (explaining the consequence of small sample sizes); *see also* KENNEDY, *supra* note 155, at 179-99 (explaining the consequences of multicollinearity); *id.* at 137 (explaining the consequences of mismeasured data).

¹⁶⁰Such studies appear, on rare occasion, in the leading peer-reviewed economics journals. *See, e.g.*, Koleman Strumpf & Felix Oberholzer, *The Effect of File Sharing on Record Sales: An Empirical Analysis*, 115 J. POL. ECON. 1, 1 (2007) (finding that downloads had “an effect on [music] sales which is statistically indistinguishable from zero”). On rare occasions, they appear in the leading peer-reviewed sociology journals. *See, e.g.*, Alexandra Kalev et al., *Best Practices or Best Guesses? Diversity Management and the Remediation of Inequality*, 71 AM. SOC. REV. 589, 610 (based on statistically insignificant results, concluding that some popular diversity programs don’t help women or African-Americans reach management positions). Occasionally, they appear in leading peer-reviewed law and economics journals. *See, e.g.*, Orley Ashenfelter et al., *Politics and the Judiciary: The Influence of Judicial Background on Case Outcomes*, 24 J. LEGAL STUD. 257, 281 (1995) (stating that “we cannot find that Republican judges differ from Democratic judges in their treatment of civil rights cases”). And they sometimes appear in the leading law reviews. *See, e.g.*, Thomas J. Miles & Cass R. Sunstein, *Do Judges Make Regulatory Policy? An Empirical Investigation of Chevron*, 73 U. CHI. L. REV. 823, 858-59 (2006) (finding that “[f]or politically mixed panels, the [agency] validation rates of Democratic and Republican judges are very similar to each other; all but one of the differences are 10 percentage points or less and are statistically insignificant” and, concluding from this, “the influence of panel composition on judicial decisionmaking appears largely cabined to politically unified panels”).

¹⁶¹See Chief Justice John G. Roberts, *2006 Year-End Report on the Federal Judiciary*, 39 THE THIRD BRANCH: NEWSLETTER OF THE FEDERAL COURTS (Admin. Office of the U.S. Courts, Wash. D.C.), Jan. 2007, at 1, available at <http://www.uscourts.gov/ttb/jan06ttb/yearend/index.html>; *see also Fed. Judicial Compensation: Oversight Hearing Before the Subcomm. on the Courts, the Internet, and Intellectual Property of the H. Comm. on the Judiciary*, 110th Cong. 4 (2007) (statement of Justice Samuel Alito) (“Without serious salary reform, the country faces a very real threat to its judiciary.”); *Fed. Judicial Compensation: Oversight Hearing Before the Subcomm. on the Courts, the Internet, and Intellectual Property of the H. Comm. on the Judiciary*, 110th Cong. 1 (2007) (statement of Justice Stephen Breyer) (“I believe that something has gone seriously wrong with the judicial compensation system.”); *Judicial Security and Independence: Hearing Before the S. Comm. on the Judiciary*, 110th Cong. 7 (2007) (statement of Justice Anthony M. Kennedy) (“The current [judicial salary] situation . . . is a matter of grave systemic concern.”); Chief Justice William H. Rehnquist, *2002 Year-End Report on the Federal Judiciary*, 35 THE THIRD BRANCH: NEWSLETTER OF THE FEDERAL COURTS (Admin. Office of the U.S. Courts, Wash. D.C.), Jan. 2003, at 2 (“[T]he need to increase judicial salaries . . . remains the most pressing issue [facing the judiciary].”).

To see this, rather than consider standard statistical significance, slice the data another way. Look at the confidence intervals reported for NETCOST and each judicial performance measure. Table 2 reports these results.¹⁶²

Table 2
Confidence Intervals for Impact of \$400,000 Salary Increase on NETCOST

Performance Models	Confidence Interval
Voting – Democratic Appointees*	
Model 1 (Full Sample)	[-.01, .03]
Model 2 (Subsample w/NETWORTH)	[-.01, .03]
Voting – Republican Appointees*	
Model 1 (Full Sample)	[-.01, .02]
Model 2 (Subsample w/NETWORTH)	[-.01, .03]
Citation Bias Analysis	[-.01, .009]
Dissents Analysis	
Model 1 (Full Sample)	[-.01, -.002]
Model 2 (Sample w/NETWORTH)	[-.01, -.007]
Speed of Disposition	
Model 1 (Full Sample)	[-5.2, 6.6]
Model 2 (Sample w/NETWORTH)	[-1.4, 14.8]
Extra-Circuit Citations: Total Influence	[-.03, .13]
Extra-Circuit Citations: Avg. Influence	[-.004, .08]

* Voting pattern regressions include panel effects.

¹⁶²Confidence intervals for the regressions considering strength of the nominee pool can be found here: <http://www.law.unc.edu/faculty/directory/details.aspx?cid=3>.

All the confidence intervals involve two-tailed tests. Using the expected sign from the theory, I also conducted a one-tailed test to find the threshold value the data rejects. This test yielded similar results and is not reported here.

These intervals mean that I can reject, at a 95-percent confidence level, any null hypothesis outside the interval.¹⁶³ Now let the Chief Justice set the null: Low pay is creating a constitutional crisis. What counts as a crisis is tough to quantify. Any number would be contestable, so I won't even try. Suppose that a constitutional crisis means increasing the chance that democratic appointees cast a liberal vote by more than two percent. I can reject that "crisis null" at 95-percent confidence. Suppose a constitutional crisis means increasing the chance that republican appointees will cast a conservative vote by more than one percent. I can reject that null at 95-percent confidence. Suppose a constitutional crisis means that the expected days between oral argument and a final decision decrease by more than 5 days. I can reject that null at 95 percent. And so on. In short, even with the imprecise judicial performance measures, the limited proxy for financial sacrifice, and the multicollinearity, the confidence intervals for most performance measures are tight around zero.¹⁶⁴ This means that, for almost all my measures, the data rejects a large effect from a salary change.

Yet this analysis leaves an issue open: What is a large effect? Maybe improving the total number of outside circuit citations for each opinion by more than 12 percent or reducing partisan voting by more than two percent are worth the cost of the judicial pay raise. Who knows? This is ultimately a political, not a statistical question, which requires some estimate of the social return from having a "better" judiciary as measured along these lines.

II. ARE JUDGES FROM TOP-FIVE MARKETS DIFFERENT?

In their reply, CHZ point out that a judicial pay raise is likely to impact judges from the top-five markets differently than judges in other markets. All the regressions in my study included a term interacting NETCOST with whether the judge came from a top-five market. This interaction alleviated some of the measurement error created by using regional partnership data as a judge's opportunity cost.

Private practitioners in top-five markets make more than the average partner in their respective region. The use of regional partnership data thus likely underestimated the opportunity cost for judges in the mega-markets. The interaction term mitigated this concern because it allows a one-unit increase in NETCOST to have different and presumably greater effect on judges in top-five markets. My article, however, reports the estimate on NETCOST as the overall effect for all judges, not distinguishing between top-five markets and other markets. CHZ correctly point out that the impact of a change in NETCOST might differ between judges from top five markets and judges from other markets (which is why I used the interaction term in the first place). CHZ show how those differences play out. Further, CHZ use new data on the lateral market for government attorneys moving to law firms in top-five markets to show exactly how much I might have underestimated the opportunity cost for judges in these markets.

¹⁶³See FUMIO HAYSASHI, *ECONOMETRICS* 38 (2000).

¹⁶⁴For judges from top-five markets, the results are different when it comes to voting patterns for republican-appointed judges and the speed of disposition. For all the other regressions, the results reported in Table 2 are a good estimate of the effect of a salary change on the behavior of all judges. For a fuller discussion of why this is so, see *infra* Part II.

For three of the eleven judicial performance models, CHZ find a change in NETCOST has significant effects on the performance of judges from top-five markets.¹⁶⁵ These results stand in contrast to the insignificant effect for judges from other markets. In addition, CHZ do not find a significant effect on dissent patterns for these judges – a result in contrast to the statistically significant and negative dissent results for judges in other markets from my original article. Interestingly, CHZ interpret their findings as evidence that judges in top-five markets are more willing to trade off salary for voting power and influence, whereas judges in other markets are more willing to trade off salary for leisure.

To see more clearly what is going on with the interaction term, Table 4 reports NETCOST, the coefficient estimate for judges in non-top-five markets, and TOPFIVENETCOST, the estimate on the interaction term.

Table 3

Interaction Between NETCOST and Top-Five Markets

Performance Models	NETCOST	TOPFIVENETCOST
Voting - Democratic Appointees (Probit)		
Model 1 (Full Sample)	0.001 (0.15)	0.02 (0.97)
Model 2 (Subsample w/ NETWORTH)	0.004 (0.34)	0.12 (1.70)
Voting - Republican Appointees (Probit)		
Model 1 (Full Sample)	0.003 (0.47)	-0.031 (2.08)**
Model 2 (Subsample w/ NETWORTH)	0.01 (0.98)	-0.04 (2.17)**
Citation Bias Analysis (OLS)		
	-0.001 (0.14)	-0.01 (1.39)
Dissents Analysis (Probit)		
Model 1 (Full Sample)	-0.006 (3.29)**	0.005 (1.42)
Model 2 (Subsample w/ NETWORTH)	-0.01 (4.13)**	0.009 (1.81)

¹⁶⁵Those regressions were: (1) democratic-appointee voting patterns (subsample with networth data); (2) republican-appointee voting pattern (full sample); (3) extra-circuit citations: average influence and (4) extra-circuit citations: total influence. CHZ, *supra* note 149, at XXX.

Speed of Disposition (OLS)			
Model 1 (Full Sample)	0.699	(0.23)	-12.8 (2.42)**
Model 2 (Subsample w/ NETWORTH)	6.67	(1.61)	-19.42 (2.25)**
Extra-Circuit Citations: Total Influence (OLS)			
	0.05	(1.25)	0.1 (1.62)
Extra-Circuit Citations: Avg. Influence (OLS)			
	0.039	(1.77)	0.025 (0.72)

In four of the eleven regressions, the interaction term is statistically significant.¹⁶⁶ For these regressions, CHZ are right. Their results should be taken as an important qualification to the results reported in the original article. For the remaining seven regressions, the interaction term is insignificant. It is these regressions I want to focus on now.

Insignificance of the interaction term means that I can't reject the hypothesis that judges in top-five markets react the same to changes in opportunity cost as judges in other markets. Yet, in these regressions, CHZ find different effects depending on whether the judge comes from a major market. If we can't reject the hypothesis that top-five market judges respond similarly to changes in NETCOST as do judges in other markets, why do CHZ find that the effect depends on the judge's home market in these regressions? More importantly, which effect – the one for judges from a top five market or the one for judges from the other markets – best represents the “true” effect of a change in NETCOST on judicial performance for *all* judges.¹⁶⁷

This puzzle and an ambiguity in interpreting the effect of changes in NETCOST on judicial performance can be seen more clearly with a little math.

Adopting CHZ's notation, my typical regression took the following form:

(1)

¹⁶⁶I use a two-tailed significance test here. CHZ use a one-tailed significance test in replicating the results. Under a one-tailed test, three of the eleven regressions have a significant interaction term. Zorn, Henderson & Czarnecki, *supra* note 149, at XXX. Under a one-tailed test, the interaction term is significant for (1) democratic-appointee voting patterns in the network sub-sample; (2) republican-appointee voting patterns in the full sample and (3) republican-appointee voting patterns in the network subsample. Unlike the two-tailed test, the interaction term is insignificant for both regressions involving speed of disposition. The reason is that the coefficient doesn't have the expected sign in those regressions. The choice between a one-tailed and two-tailed test reflects how confident a researcher is that his theory gets the sign of the effect right. See JEFFREY M. WOOLDRIDGE, *INTRODUCTORY ECONOMETRICS* 121-22 (2d ed. 2003).

¹⁶⁷To avoid this ambiguity, one solution would be to drop the interaction term in all the models where it was insignificant and rerun the regressions. Then, I might have reported the NETCOST coefficient from the new regression as the overall effect. Such a move is undesirable, however, because it leads to pre-test bias of the estimates. See PETER KENNEDY, *A GUIDE TO ECONOMETRICS* 189-91 (3d ed. 1992)

$$\begin{aligned}
f^{-1}(\text{Performance}_i) = & \\
& \beta_0 + \beta_1 \text{NETCOST}_i + \beta_2 \text{TOPPFIVE}_i + \\
& \beta_3 (\text{TOPPFIVE}_i \times \text{NETCOST}_i) + \mathbf{X}_i \gamma
\end{aligned}$$

As CHZ make clear, in this regression β_1 represents the effect of a change in NETCOST for judges outside the top-five markets; $\beta_1 + \beta_3$ represents the effect of a change in NETCOST on judges in top-five markets; X represents the set of controls.

I could have run the following regression instead.

(2)

$$\begin{aligned}
f^{-1}(\text{Performance}_i) = & \\
& \alpha_0 + \alpha_1 \text{NETCOST}_i + \alpha_2 (1 - \text{TOPPFIVE}_i) + \\
& \alpha_3 ((1 - \text{TOPPFIVE}_i) \times \text{NETCOST}_i) + \mathbf{X}_i \gamma
\end{aligned}$$

With (2), α_1 represents the effect of a change in NETCOST for judges in top-five markets; $\alpha_1 + \alpha_3$ represents the effect of a change in NETCOST for judges in non-top-five markets; X, again, is a set of controls.

The difference between (1) and (2) is the group subject to the interaction term. In (1), NETCOST is interacted with judges from the top-five markets. In (2), NETCOST is interacted with judges from non-top five markets. Moving from (1) to (2) flips the assumption. Rather than assume regional partnership salaries under-reports the opportunity cost for judges in top-five markets, equation (2) assumes that regional partnership salaries over-reports the opportunity cost for judges outside the top-five markets. The unmeasured salary difference between the two groups remains the same. So, the assumption change, while unnatural, should be irrelevant.

The coefficient α_1 in equation (2) is the effect reported by CHZ. My article reports, β_1 , the coefficient estimate from equation (1). A little algebra shows that $\alpha_1 + \alpha_3 = \beta_1$ and $\beta_3 = -\alpha_3$ no matter the size of the coefficients. For seven of the regressions, however, I can't reject that the interaction term has no effect (i.e., that $\beta_3 = -\alpha_3 = 0$). As a result, I can't reject that α_1 equals β_1 . But looking at the estimates, it is clear that the coefficients aren't, in fact, equal. CHZ report different estimates than reported in the original article. In seven of those regressions, however, we can't reject that any reported differences are simply noise.

A deeper question lurks behind the results. What is the effect of a one-unit increase in NETCOST for "all" judges where the interaction term is insignificant? The answer is this: Both α_1 and β_1 are plausible candidates. Either one works and it is probably safest to report both estimates. In defense of the estimate provided in the original article as the true overall effect, that estimate has (a) the smaller standard error (it is more "accurate") and (b) the

sample contains many more judges in non-top five markets, making them the more natural baseline group.

Still, CHZ advance the analysis by providing both sets of results side by side. For the regressions where the interaction term is insignificant, what happens if we accept CHZ's bigger estimate as the "true" effect of higher salaries for all judges? Not much. The economic significance of any effect is small. Is it worth, for example, increasing salaries by \$50,000 a year to increase average outside circuit citations by six percent? To increase total outside circuit citations by fourteen percent?

In four of the regressions, the evidence suggests that judges from top-five markets are different; they respond differently to changes in salary. CHZ show how this difference manifests itself. Most dramatically, they identify that higher salaries could diminish partisan voting among judges in top-five markets. This result is a welcome refinement to the article.

Even with this refinement, I submit, the bottom line remains the same. For judges in most places, the data allow me to exclude that a salary increase will have a large impact on the performance measured studied. Interestingly, while they don't support across the board salary increases, CHZ's results might be used to support more aggressive COLA adjustments for judges in major markets – a proposal Judge Richard Posner has been advocating for a number of years.¹⁶⁸

III. HOW DO YOU MEASURE LOST OPPORTUNITY?

In his reply, Professor Marks raises two concerns involving the appropriate measure of a judge's lost opportunity. First, he suggests the NETCOST measure is inadequate because it does not allow for the possibility that a judge in a region with low partner salaries could be giving up a position in a higher paying region when she takes the bench. Second, Marks demonstrates how measuring NETCOST in terms of judges' cumulative lost lifetime earnings may affect the results. I consider each criticism in turn.

A. *Problems with the Mobility Assumption*

Professor Marks questions the assumption that judges won't leave their region for a higher paying law firm job elsewhere. In his well-crafted example, Professor Marks demonstrates how this simple assumption can alter the results. The judge who viewed her next best opportunity as a partnership at a law firm in the highest paid city in the country would have a higher net cost than a judge who viewed her next best financial opportunity as partnership in a law firm in her local city. Of the 259 judges in the sample, 239 hadn't moved in the ten years prior to their appointment to the bench. For these judges, it seems reasonable to suspect a hometown attachment made them unlikely to move outside the region for a law firm job.

But what about the 19 other judges? Professor Marks shows how making the wrong assumption about the mobility of these judges weakens the results. The assumption means that I consistently underestimate the opportunity cost for these judges. On this point, Professor Marks is right. In light of this critique, I investigated whether grouping the mobile judges and immobile judges together changed the analysis. To do this, I analyzed two new

¹⁶⁸See RICHARD A. POSNER, *HOW JUDGES THINK* 172-72 (2008).

variables. The first variable is a dummy variable, MOBILE, for whether the judge moved in the ten previous years before taking the bench. The second variable is an interaction term between MOBILE and NETCOST. Similar to the interaction term between the dummy variable, TOPFIVE, and NETCOST, this term allows for a one-unit increase in NETCOST to have a greater effect on mobile judges.

Table 4 reports the results on the variables of interest. The results remain the same, except for speed of disposition and dissents. For mobile judges in markets outside the top-five, giving up lots of cash does not have a significant effect on dissenting behavior. This is in contrast to immobile judges from these markets, for whom NETCOST has a significant and negative effect. With regard to speed of disposition, the coefficient for mobile judges from non-top five markets is significant and positive. While small in magnitude (15 days), this result suggests Congress could reduce decision time for the mobile judges by increasing their salaries.

Table 4

Performance Models Controlling For Potential Mobility By Judges

Performance Models	NETCOST mobile judge non-top-five market	NETCOST immobile judge non-top- five market	NETCOST mobile judge top-five market	NETCOST immobile judge top-five market
Voting – Democratic Appointees (Probit)				
Model 1 (Full Sample)	-.06 (.78)	.01 (.94)	-.03 (.44)	.03 (1.44)
Model 2 (Subsample w/ NETWORTH)	-.18 (.76)	.007 (.45)	-.06 (.24)	.13 (1.63)
Voting – Republican Appointees (Probit)				
Model 1 (Full Sample)	-.02 (.78)	.005 (.57)	-.03 (.99)	-.002 (.15)
Model 2 (Subsample w/ NETWORTH)	.06 (.45)	.003 (.29)	.04 (.28)	-.02 (.74)
Citation Bias Analysis (OLS)				
	.02 (1.84)	-.001 (.36)	.016 (1.15)	-.01 (1.41)
Dissents Analysis (Probit)				
Model 1 (Full Sample)	-.0009 (.15)	-.007 (3.34)**	.002 (.34)	-.003 (.97)
Model 2 (Sample w/NETWORTH)	-.01 (1.65)	-.01 (3.75)**	-.02 (1.65)	-.009 (1.63)
Speed of Disposition				

(OLS)				
Model 1 (Full Sample)	12.5 (1.32)	.38 (.13)	-3.12 (.33)	-15.3 (2.66)**
Model 2 (Sample w/NETWORTH)	32.42 (2.22)**	6.42 (1.54)	10.96 (.84)	-15 (10.43)
Extra-Circuit Citations: Total Influence (OLS)	-02 (.21)	.05 (1.31)	.08 (.58)	.15 (2.61)**
Extra-Circuit Citations: Avg. Influence (OLS)	.03 (.56)	.04 (1.86)	.05 (.77)	.05 (1.68)

B. *Problems with Cumulating Earnings*

Professor Marks's second concern involves my use of lost lifetime earnings to measure a judge's opportunity cost. Two examples illustrate his point. Professor Marks's first example shows how, by looking at the lifetime stream of lost earnings, two judges that were, in fact, identical might appear different in the data. His second example demonstrates how a stream of earnings calculation might treat a judge with a weak preference for leisure as if she had a strong preference for leisure.

The first example presents a difficulty. The reason: As evidence against the theory that judicial salary matters, I take the failure to reject the hypothesis that two judges – who the data report as different, but Professor Marks shows really aren't – act the same. The second example poses a problem because the analysis relies on NETCOST being a valid proxy for the judge's taste for the judicial role, i.e., her valuation of the non-pecuniary aspects of judging. In short, Professor Marks suggests that cumulating earnings over time creates meaningless variation in the NETCOST variable. As a result, we can't be sure what is explaining the variation in the dependent judicial performance variables: the true variation in the NETCOST or the meaningless variation introduced through cumulating and then discounting net losses back to present value.

Controlling for a judge's age at appointment should mitigate some of the problem Professor Marks identifies. In both examples, meaningless variation arises because one judge serves two terms (forfeiting two years of partner income), while the other judge serves one term (forfeiting one year of partner income). The only difference between the two judges is that one judge serves longer than the other. Under the assumption that both judges serve until age sixty-five, the regression will not treat these two judges the same. The judge who took the bench at age forty-four will not be treated the same as the judge who took the bench at age forty-five. Instead the regressions, in effect, compare two judges appointed at age forty-four with different levels of opportunity cost.¹⁶⁹

Even controlling for age at the time of appointment, a related concern still lingers. Take two judges appointed at age forty-five. Suppose the two judges have different opportunity costs as I measured them. The judge with the greater opportunity cost is assumed to have the more intense preference for the non-money aspects of the judicial role. NETCOST assumes each judge will serve on the bench until age sixty-five – in this example, the model would treat

¹⁶⁹See WOOLDRIDGE, *supra* note 154, at 200 (providing this interpretation of a control).

both judges as if they expected twenty years of judicial service. Yet, the years of expected judicial service might not be the same for the two judges. A judge with an intense preference for, say, imposing policy preferences might intend to serve longer than a judge with a weak preference for dictating policy. Despite the intense preference, this judge might have a lower NETCOST. That is to say, this judge might give up relatively little money over the twenty-year time-span, but anticipates a much longer judicial career. The same problem arises for a judge with, say, health problems. A judge appointed at age forty-five with a history of heart disease might not anticipate serving until age sixty-five. By assuming a twenty-year judicial career, NETCOST over-estimates the intensity of this judge's preference for the judicial role.

These issues seem insurmountable. We don't have data on the likely career path for each individual judge; their health problems, if any, at the time of appointment; the likelihood they will retire at age sixty-five, remain active, or take senior status; or, if they take senior status, how long they will serve in that capacity.

Because of the difficulties in cumulating earnings over time, Professor Marks suggests a more fruitful measure of opportunity cost would examine a judge's lost earning over a single year.¹⁷⁰ While solving some of the problems noted above, the single period approach discards relevant data. Consider two judges, *A* and *B*. Both are appointed at the same age and forgo \$50,000 in their first year on the bench. Judge *A* works in a region where law firm partnership salaries increase, on average, 25 percent a year. Judge *B* works in a region where partnership salaries increase, on average, 10 percent a year. Measuring pay as lost earnings in a single period treats these two judges as making the same financial sacrifice. Yet the truth is Judge *A* gave up more cash for the bench.

To sum up, Professor Marks is correct that cumulated earnings are an imperfect proxy for a judge's opportunity cost; yet single period earnings are also imperfect. What to do? Given these imperfections, I also considered whether the strength of the pool against which a judge competed for the nomination impacted her judicial performance. The thinking here was that higher relative judicial salaries made for a stronger pool. This alternative approach yielded similar results and should mitigate any concern over cumulating earnings for the NETCOST measure.

CONCLUSION

Let me emphasize in concluding that the study – qualified by these replies -- doesn't "prove" that Congress should leave judicial salaries where they stand. It doesn't "prove" the performance measures considered reflect judicial quality. It doesn't even "prove" that higher pay wouldn't affect these

¹⁷⁰Stephen Choi, Mitu Gulati, and Eric Posner take this approach when studying the impact of pay on state court justice behavior. See Choi et al., *supra* note 150, at 45. Unlike the vast majority of federal judges, many state judges leave judgeships before qualifying for retirement. Hence, measuring opportunity cost as a single period loss makes more sense in the context of state court justices.

measures. The basic point is that we shouldn't assume – as Chief Justice Roberts does – that pay will improve judicial performance. The article searches for a statistical significant correlation between some judicial performance measures and a crude proxy for the financial sacrifice of the judges. For most measures and most judges, it finds none. To be precise, the data rejects large effects of judicial pay on performance and fails to reject tiny or negligible effects of pay on performance, meaning that a change in salary is unlikely to have a meaningful (i.e., large) effect on judicial performance for most judges in most places.

As for Professor Cross's suggested study of law professor pay, I won't do that study right now. But who knows – maybe I could be motivated to do it by a little raise.