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Comment on “Judicial Compensation and Performance”

*J.J. Prescott**

The most significant challenges to better understanding judicial behavior are lack of data and the absence of plausible exogenous variation in judicial environments. The random assignment of judges to cases has admittedly been helpful in gaining traction on the effects of judicial decisions (e.g., Dobbie, Goldin, and Yang 2018). Yet developing a full empirical account of “what judges maximize” (Posner 1993) would require a setting in which judges are randomly subjected to a wide variety of (real-world) environments with different costs, constraints, and rewards. This prospect remains pie in the sky, but that does not mean that we have not made some headway on the ground. For instance, researchers have deployed the random assignment of cases to judges to back out how judges respond to differences in case attributes when the characteristics of cases (e.g., severity) can be assessed *ex ante* (Leibovitch 2016) and to attempt to gauge how judicial decision making evolves over the course of the day or in response to an empty stomach (Danziger, Levav, and Avnaim-Pesso 2011; Weinshall-Margel and Shapard 2011). These lines of research, however, have more to say about when judges depart from the merits of cases than about which traditional institutional features (e.g., compensation, selection) enhance judicial effort and improve accuracy.

In “Judicial Compensation and Performance,” DeAngelo and McCannon (2017) seek to make progress on the question of what judges maximize by, first, collecting detailed appellate data from New York slip opinions and, second, exploiting plausibly exogenous variation in

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judicial compensation between 2007 and 2017 to better understand judicial behavior. The data assembled for the article are valuable in and of themselves, but the idea of using a sharp discontinuity in state-level pay to explore the role that financial compensation plays in judicial effort and performance is also a meaningful methodological contribution. By comparing patterns in outcomes—specifically, accuracy, in the form of decisions being “upheld”—of three sets of judges, with a focus on those judges serving before and after the shift in compensation, the authors aim to distinguish between two theories for why the accuracy of judicial decisions might improve after a pay increase: (1) better jurists choose to serve as judges when compensation is more generous and/or (2) jurists simply work harder when they are better compensated. With respect to the latter possibility, the authors briefly postulate an efficiency-wage hypothesis in which better compensated judges choose to work harder for fear of losing their position during the next election, but the article is largely agnostic about precisely why higher wages might lead to better outcomes.¹

DeAngelo and McCannon’s (2017) decision to approach their work atheoretically has costs, however. Although little of their article appears to turn on the precise relationship between compensation and effort, there are at least two ways in which their not being more explicit about the possible characteristics and fundamental nature of this relationship has resulted in missed opportunities.

First, more theoretical precision would have allowed the authors to distinguish between different competing theories of judicial effort (i.e., efficiency wages versus reciprocity). The “shirking model” version of the efficiency-wage hypothesis they cite (Shapiro and Stiglitz 1984) depends on employees fearing the consequences of shirking. In this context, low effort equates to a higher probability of being made to leave the bench and find employment elsewhere. The authors’ data are from New York, a state that is geographically diverse, and therefore one that presumably presents very different employment opportunities for former judges in New York City than may be available in Ithaca or Phoenicia. If judicial salaries are constant statewide, then the potential loss from shirking (exogenously) varies from jurisdiction to jurisdiction with this variation in outside options. If the authors were to detect differences in outcomes that align with these different environments, they would have a much stronger case for an efficiency-wage hypothesis relative to other hypotheses (e.g., some versions of reciprocity theory). One can also imagine similar empirical strategies

¹ In a footnote, the authors also allow for a reciprocity theory in which judges exert more effort following a salary increase simply to reciprocate the state’s decision to compensate them more generously.

taking advantage of political polarization and geographic concentration. It must be the case that many trial judges realistically face no possibility of electoral challenge, despite needing the endorsement of a partisan nominating convention.²

Second, more theoretical precision would have nudged the authors to scrutinize their otherwise intuitive measure of performance—the likelihood that a judge’s decision is upheld on appeal. An efficiency-wage hypothesis implicitly incorporates a theory of termination and thus an understanding of the terms and conditions of judicial positions generally. The authors recognize this, and we learn that judges have rather long terms in New York—10 or 14 years, depending on the court—and are generally subject to partisan elections. Assuming that most judges expect to seek reelection, the critical question becomes, What sort of performance is likely to generate electoral victory?³

It is not obvious—and may even run against expectations—that avoiding reversals in criminal cases is how a judge best pursues reelection.⁴ For one, if avoiding reversals is truly the goal, studying criminal appeals may be inappropriate, given the asymmetry in criminal appeal rights. A judge can avoid reversal entirely by granting motions of acquittal for insufficient evidence (which prosecutors generally cannot appeal), and so might do this in cases with well-represented defendants to avoid reversal. This would produce a correlation between electoral motivation and the rate at which decisions are upheld, but that correlation would be the result of judges manipulating the composition of the appellate docket and would actually be a sign that greater incentives lead to additional distortions rather than additional effort. The politics of criminal justice, however, seem more likely to

² One response to this point is that it also implies that the article’s empirical results would be even stronger if the authors categorized only judges facing heightened incentives as receiving the treatment. The authors’ case would benefit from exploring this idea explicitly. One concern with this possibility is that the estimated effect sizes are already quite large, which points to either extreme shirking in the preperiod or a spurious relationship in the data.

³ Exogenous variation in the number of years until reelection and the demographic characteristics that are themselves exogenously correlated with a judge’s likelihood of being interested in reelection (e.g., age) are also available to probe the robustness of the efficiency-wage hypothesis. Note that judicial terms in New York are long. The authors report that judges are reelected on average approximately only once, hinting that a large percentage of New York judges are indifferent to their reelection chances because they are in their second term, close to retirement, etc. Presenting data on the frequency with which judges seek reelection and/or retire in the middle of a term would have been very useful for better understanding judicial employment dynamics in New York.

⁴ DeAngelo and McCannon (2017) ought to be able to empirically assess, for instance, whether judges in New York, all else equal, are more likely to lose a bid for reelection if they are reversed relatively more often.

be consistent with a different dynamic. Judges are purportedly rewarded in many places for being “tough” on criminal defendants. If true, a judge seeking reelection might favor the prosecution more aggressively to the point of becoming *more* likely to be reversed, on average. Moreover, judges might be just fine with being reversed in particular cases. An appeals court refusing to uphold a trial court’s punitive treatment of a criminal defendant might be political gold when the district court judge below wants to signal to the public that he or she is concerned first and foremost with public safety or that, unlike many “out-of-touch” judges, he or she is more interested in “the truth” than procedural niceties.⁵

If one accepts that there is an empirical relationship between the sharp increase in judicial wages in New York beginning in 2012 and judicial “effort” as measured by an increase in the likelihood a conviction is upheld, one can still profitably ask how we ought to interpret such a finding. Although a sharp discontinuity in judicial wages is more salient to all concerned, and any association with behavioral outcomes is both easier to detect and more likely to be causal, its use also raises a few difficult questions.

First, a sharp, politics-driven, media-reported increase in judicial wages may be endogenous to judicial behavior, perhaps a response to declining judicial performance or quality, followed by subsequent regression to the mean.⁶ Although a sharp discontinuity initially presents as an ideal situation in which to study the effects of compensation on judicial performance, judicial salary changes that occur according to a predetermined formula or a standardized procedure established without reference to recent judicial behavior may allow for findings that are more amenable to a causal interpretation.

Second, exploiting a significant, salient, and apparently long-overdue increase in compensation raises important questions about precisely what the authors are measuring—and, ultimately, whether the article provides valuable lessons on how to reduce judicial error. Put another way, does DeAngelo and McCannon’s (2017) analysis teach us about the effect of wages on judicial behavior or, instead, the effect of a *change* in wages on judicial behavior? An increase in compensation, especially a large one, is infused with many other meanings (e.g., a showing of respect to the employee or a recognition of the employee’s inequality aversion) that would not accompany stable wages at a higher

⁵ The authors could explore this idea empirically in their data by looking to see whether appeal rates (without trial-level data, the authors would need to assume a constant flow of a cases) and reversal rates vary for trial judges over the tenure cycle.

⁶ DeAngelo and McCannon’s (2017) theory also suggests that we should observe an increasing trend in reversals prior to the 2012 raise because judges’ real wages were declining steadily (at least in relative terms) during that period.

level many years later. A simple efficiency-wage hypothesis imagines a constant effect of the higher salary over time, whereas the effect of a change in wages would presumably diminish over time (although one can imagine alternative theories). DeAngelo and McCannon's figure 2 seems at odds with both theories, assuming the figure shows the timing of the trial judge's behavior along the x-axis. The figure reveals a slow increase over time with, at best, relatively small effects at the outset—notwithstanding the large and salient increase in compensation at that time. One explanation might be that it takes judges time to respond or that higher effort levels only make a difference in cases initiated after the rise in compensation. On this latter score, it is worth observing that judicial wages continued to increase after 2012. DeAngelo and McCannon do not use this variation in their work, but a model of effort in which judicial compensation had to cross some threshold, one not crossed in New York until years after 2012, might explain the patterns in their data.⁷

The New York slip opinions that DeAngelo and McCannon (2017) have collected and coded for their analysis will be of considerable value for future research. Nevertheless, the data do have important limitations. First and foremost, the authors' data include solely appellate-level decisions, meaning that we cannot know how the underlying population of cases evolved over time—perhaps in reaction to policy changes, although it seems unlikely that an increase in judicial salaries would alter criminal behavior or prosecutorial charging decisions (unless prosecutors also experienced a contemporaneous change in compensation).⁸ More generally, the composition of appellate cases may have evolved over the sample period in unobservable (or at least unobserved) ways. Future researchers using these data would likely benefit by examining crime, arrest, charging, and trial court data in New York for compositional change. DeAngelo and McCannon do attempt to control for such selection in their work but do so by including likely endogenous re-

⁷ DeAngelo and McCannon do not use the size or timing of the salary increases (other than the timing of the first increase) in their analysis. This makes how best to interpret their results less obvious and seems to leave a significant amount of useful information on the table. Thinking about how to assess a 12-year lull in salary increases and then a subsequent catch-up requires context, including an understanding of how unusual this pattern is for state employees and whether New York judges suffered alone or with other fellow civil servants.

⁸ DeAngelo and McCannon also ought to have clarified whether and how New York's appellate judges were affected by the salary increases evaluated in the article. The outcome of interest is necessarily a function of both the trial judge's performance and the appellate judge's performance. Even if appellate judges are assigned randomly to lower-court decisions, significant effort and selection effects at the appellate level would alter the interpretation of the authors' findings.

gressors—for example, whether the case involved a trial or a guilty plea, the number of days between judgment and appeal, and the grounds for appeal. Given the power of trial court judges to influence these case dimensions, they are best categorized as outcomes themselves. By controlling for them, the authors introduce potential bias into their primary estimates of interest.

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