# Judicial Tenure and the Slowing of Legal Development in England, 1600-1800*

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**Abstract**

Conventional wisdom on English development confers iconic status on the Act of Settlement, emphasizing the clause mandating secure tenure for judges. But the Act's effect on tenure was partial, affording the opportunity to empirically identify the effect of tenure on judicial decisions. The empirics uses two new databases, one on the biographies of judges, the other recording all citations to earlier cases made in the *English Reports*. The paper estimates the effect of tenure on citations, a measure of judicial quality. Several strategies aid identification. Explanatory variables capture both judges' human capital and the amount of litigation reaching specific courts at specific times. The court-year panel makes difference-in-differences possible. Two different sets of instrumental-variable estimates are generated. Tenure has a strong, significant, and deleterious effect on the quality of the decisions of associate judges. Tenure has no effect on the quality of the decisions of chief judges. Perhaps England would have developed earlier had it not been for one of the monuments of English constitutional law.

Keywords: judicial quality, lifetime tenure, Act of Settlement, legal citations, institutional development

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> According to the plan of the convention, all judges who may be appointed by the United States are to hold their offices *during good behavior*…The standard of good behavior for the continuance in office of the judicial magistracy, is certainly one of the most valuable of the modern improvements in the practice of government.
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> Alexander Hamilton, Federalist Papers 78.

*1. Introduction.*

When referring to one of the most valuable modern improvements in government, no doubt Hamilton had in mind the English enactment of the Act of Settlement of 1701, which stated that "...judges commissions be made *quamdiu se bene gesserint* [during good behavior]". Up to that point, English judges had frequently been appointed *durante bene placito* (during the King's pleasure, or at will). However, it is not clear where Hamilton could have derived the evidence for his conclusion that this "improvement" was "certainly one of the most valuable…in the practice of government". Relevant evidence would have evaluated the performance of judges under varying degrees of the security of tenure. This issue has been extensively studied in the literature on contemporary courts (e.g., Choi et al. 2010, Ash and MacLeod 2015, 2016). But even today there is no concrete evidence on the effect of judicial tenure in the era of the passage of the Act of Settlement.[1]

In the standard telling, this Act made judicial appointments newly secure and therefore judges more independent of the monarch.[2] Judicial performance improved, thereby establishing a core element of the legal basis for a modern economy.[3] There are three links within this story: the direct effect of the Act on the formal judicial institutions themselves; the subsequent effect of the institutions on behavior; and the resultant effect of that behavior on the development of English law. This paper analyzes the first two links, producing implications for the third.

The standard story concerning that first link requires a major qualification. A systematization of the relevant legal and institutional evidence shows that *quamdiu se bene gesserint* existed before the Act of Settlement and that *durante bene placito* continued after, facts

[1] Klerman and Mahoney (2005) show that financial returns varied with subjective probabilities of the passage of the Act, but this is not a test of the effect of the Act, but rather about expectations of its effect, that is the conventional wisdom in that era.

[2] It is usually assumed that lifetime tenure is a core ingredient of formal and actual independence (Feld and Voigt 2003).

[3] On the period in question see for example North and Weingast (1989: 816-820) and Olson (1993: 574). Despite some cautionary lessons from the recent literature (e.g. Hanssen 2004; Choi et al. 2010), it is an almost universal assumption in development economics and economic history that judicial independence uniformly improves judicial performance (e.g. Klerman and Mahoney 2005: 25).

largely ignored in the literature. Both forms of judicial tenure coexisted for the whole of the seventeenth and eighteenth centuries, sometimes even simultaneously for different judges on the same courts. Section 2 summarizes the facts on the conditions of from 1600-1800, presenting evidence which has hitherto remained scattered in the literature.

The more nuanced picture of the arrangements surrounding judicial employment makes it clear that there is an opportunity for empirical work on the second link, the one from legal institutions to judicial behavior. Because the two systems of tenure existed side-by-side and varied considerably both within time periods and within courts, it is possible to identify the effect of judicial tenure on judicial performance. This is the major contribution of this paper. The measure of performance used is the number of citations to the reports of cases in which a judge participated, a standard measure of the quality of judicial decisions (Landes et al. 1998, Posner 2000, Choi et al. 2010, Ash and MacLeod 2015, 2016).

In a legal system where precedent is important, citations measure how much the decisions of a court have contributed to the development of law, the third element of the standard story. Therefore estimates of the effect of tenure on the quality of judicial decisions immediately address the question of how much "one of the most valuable of the modern improvements in the practice of government" actually advanced the development of law. The answer is not at all; judicial tenure retarded the development of law.

The empirical investigation uses two new databases. Each contains data on English history that has previously remained scattered across the literature in non-digital form, not readily applicable to empirical work. The first provides biographical information on all judges serving on England's highest courts in the seventeenth and eighteenth centuries. The crucial variable is the terms of their appointments. The second dataset documents all citations appearing in the *English Reports* (Renton 1900) to reports of previous cases. The *English Reports* are the definitive set of case reports for the English courts up to 1866.

The empirical analysis estimates the effect of the terms of judicial tenure (*quamdiu se bene gesserint* or *durante bene placito*) on the total number of citations to a given court in a given year. Several empirical strategies help in the identification of this effect. First, the data set is a panel, facilitating the use of year and court fixed-effects. Second, because the length of a judge's service—a measure of human capital—could be related to the terms of the appointment, the

regressions include variables summarizing the previous experience of judges. Third, in this volatile time in English history, political conflict would have simultaneously affected the emergence of significant legal struggles and the monarch's views on judges' terms of appointment. Therefore the analyses include a variable that is proxy for such legal struggles, citations by later courts to relevant statutes. Fourth, the paper presents estimates relying on two different sets of instrumental variables.

The results are remarkably at odds with common assumptions about English history. Tenure has a large, statistically significant, deleterious effect on the decisions of associate, or puisne (think 'puny'), judges. This result is consistent across different estimates. The estimated effect of chief-judge tenure on the quality of decisions is sometimes positive, sometimes negative, and usually insignificant, consistent with a hypothesis of no effect.

These results hold across a variety of specifications of the dependent variable, in particular when one uses citation counts only from citing courts other than the cited court. This is often considered the most rigorous measure of judicial quality because the pressure on outside courts to cite precedents automatically is weak, meaning that the persuasive content of decisions must be higher (Choi et al. 2010). The results hold when one considers only citations made after 1830, ensuring that considerable time has passed beyond all original cited decisions. They hold also when one considers only citations made within 20 years of the original decision, ensuring that the same scope for citation is applied to all cited years.

The instrumental-variable fixed-effect GMM (IV) results are broadly consistent with the OLS fixed-effects results. The IV coefficients are estimated with less precision. For one set of instruments the IV estimates of the puisne-judge tenure effect are larger in absolute size than the OLS estimates. For the other set of instruments, the IV estimates are smaller in absolute size than the OLS estimates. The difference between the three sets of estimates is interpreted below in terms of heterogenous effects and local-average treatment effects.

This paper's results are relevant to two literatures. Most importantly it reverses an important element of the conventional wisdom in the economics literature on 17$^{th}$ century English history, an era that provides a central paradigm for many works discussing modernization and development. There is no evidence that the Act of Settlement improved the quality of judicial decision-making. Indeed, this Act slowed English institutional development

given that case law provided such a large part of the substance of English law in the eighteenth century, and beyond. Perhaps England would have developed earlier had it not been for this Act, one that is considered a monument of English constitutional law.

Additionally, the results contrast with those in the empirical literature on the determinants of the quality of judicial decision-making in modern courts. This literature has usually concluded that stronger forms of judicial tenure are positively related to higher quality judicial decisions. Evidently, the effect of judicial tenure is contingent on the broader institutional and political context. There arises the question of what particular features of legal organization in seventeenth and eighteenth century England led to judicial behavior being so divergent from that found in modern times. The paper concludes with some conjectures that address this question.

*2. The Legal Framework*

This paper examines the decisions of judges from 1600 to 1800 in the four principal high courts of justice: King's Bench (KB), Court of Common Pleas (CP), Court of Exchequer (EX), and Chancery (CH). All four were partially courts of first instance and partially appeal courts. The first three were common-law courts, the last an equity court. However, all were under the sway of officials and judges who had been trained in the common-law tradition. Indeed the judges in all courts were selected from the same pool of candidates, with judges often moving between courts. Each court had a chief judge and a small number of puisne judges.[4] The number of puisne judges varied both over time within courts and across courts within time periods.

The institutional and organizational arrangements that surrounded the courts were complex and certainly not laid down in clear legislation or regulations. For this reason, there seems to be no convenient summary of those arrangements that could provide a suitable reference for this paper. To fill this lacuna, Table 1 compiles facts on those parts of the courts' arrangements that are valuable in interpreting the empirical design and results that follow. The remainder of this section simply emphasizes the main points.

[4] The official names of the judicial positions varied between courts; for simplicity, this paper only uses the designations chief and puisne.

Tenure arrangements are summarized in Part (a) of Table 1. Until the Act of Settlement was in force, the monarch could choose to appoint judges either *quamdiu se bene gesserint* or *durante bene placito*. The choice of the terms of appointments varied over time and across courts until 1714, when Queen Anne died and the judicial clause of the Act of Settlement became effective. Appointments were not uniformly *durante bene placito* before 1714. Different monarchs, sometimes under the pressure of Parliament, made different choices on the terms of judicial tenure, and these choices varied across the four courts at a given time.

Strictly speaking, lifetime tenure did not arrive until 1761. All appointments expired on the death of a monarch until the passing of an act in that year. And lifetime tenure never applied to the Lord Chancellor, the political head of the legal system and the chief judge of the Chancery. The Act of Settlement did not apply to the Lord Chancellor, who always served *durante bene placito*.

Administrative arrangements are summarized in Part (b) of Table 1. The legal system was under the sway of the legal profession, which at several points in the relevant time period showed that it could resist the demands of the monarchy. The monarch was not a free agent in choosing judges. They were selected from a small pool of lawyers who had risen to the top of the profession. Sitting judges and other influential members of the legal profession advised the monarch on both law and the quality of judicial candidates. Destroying this structure would have sent the legal system crashing, and by 1600 the kingdom was highly dependent on the functioning of that system, in both criminal and civil matters.

Tenure did not mean that judges were free from the effects of kingly caprice. The payment of judges depended on the royal household until the latter part of the 18$^{th}$ century. The Lord Chancellor, a political appointee as well as a lawyer, controlled administrative arrangements, which had a large effect on judges' well-being.

In sum, the Act of Settlement was certainly a legislative watershed. But it is a mistake to assume that English judges were completely dependent on the monarch before that Act. And it is a mistake to assume that the status of an English judge matched the modern ideal of an impartial independent decision-maker after the Act. Hence, the hypothesis that the Act of Settlement improved the functioning of the judiciary is just a theory, and like any theory it should be subjected to empirical analysis.

*3. The Empirical Framework, the Data, and the Variables*

The empirics uses a database of all citations made in the *English Reports* to cases decided between 1600 and 1800 in one of England's four major courts, King's Bench (KB), Court of Common Pleas (CP), Court of Exchequer (EX), and Chancery (CH). The citations database contains nearly 400,000 records, each naming the court and year of the citing case and the court and year of the cited case. An appendix provides the details of the construction of both this database and the one on the biographies of judges.

The dependent variable in all regressions is $CITE_{kt}^{m}$, the number of citations to court *k*'s decisions in year *t* that were made by a set of courts denoted by *m*. *k* takes on the values KB, CP, EX, or CH. *t* varies from 1600 to 1800. The elements of *m*'s domain are combinations of sets of courts and sets of years defining those case reports whose citation records are used to construct the aggregate value of the number of citations to court *k* in year *t*. In the most comprehensive version of $CITE_{kt}^{m}$, *m* refers to all years for all courts. Then, different versions of the core empirical analysis vary *m*. This affords the opportunity to examine how results change when examining citations from only specific courts or from specific time windows. The different specifications of *m* are introduced as they become relevant in the empirical analysis.

The focus is primarily on the estimates of parameters $\beta_c$ and $\beta_p$ in the following equation:

$$CITE_{kt}^{m} = \beta_c CJ_{kt} + \beta_p PJ_{kt} + X_{kt}\omega + \lambda statcite_{kt} + \varphi_k + \gamma_t + \varepsilon_{kt}. \qquad (1)$$

with *k* = KB, CP, EX, CH, and *t* = 1600,…,1800. $\varphi_k$ is a full set of court fixed effects and $\gamma_t$ is a full set of year fixed effects. $\beta_c$, $\beta_p$, and $\lambda$ are scalar parameters, $\omega$ is a parameter vector, and $\varepsilon_{kt}$ is a disturbance term. The immediately ensuing paragraphs define the explanatory variables. Table 2 provides summary statistics.

The explanatory variables of central interest are the tenure measures $CJ_{kt}$ and $PJ_{kt}$, the former for chief judges and the latter for the set of puisne judges. $CJ_{kt}$ reflects the tenure of the chief judge in court *k* at time *t*, a dummy variable equal to 1 if the appointment is *quamdiu se bene gesserint* and 0 otherwise Because the number of puisne judges varies over time and over courts and because citations are attributable only to whole courts, not individual judges, the construction of $PJ_{kt}$ differs from that of $CJ_{kt}$. For each puisne judge, a tenure dummy variable

equals 1 if the appointment is *quamdiu se bene gesserint* and 0 otherwise. Then, $PJ_{kt}$ is the mean of the tenure dummy variables for all puisne judges on court $k$ at time $t$.

A vector of five variables, $X_{kt}$, captures judge experience that has arisen both inside and outside court $k$. These variables are all proxies for human capital. Chief-judge variables reflect the data for a single individual, whereas all the puisne-judge variables reflect the mean values for all the puisne judges on court $k$ at time $t$. Chief-judge previous service measures the number of years that the chief judge served as a judge elsewhere before taking up the current position. Puisne-judges' previous service is defined analogously. Years in position, chief judge measures the number of years that the chief judge has served in the current position at time $t$. Years in position, puisne judges is defined analogously. Chief judge served on same court is a dummy variable indicating whether the chief judge previously served as a puisne judge on the same court.

Same-court citations to statutes ($statcite_{kt}$) measures the total number of citations made after $t$ by court $k$ to statutes passed in the years $t$-*10* to $t$-*1*. It is included in the regressions as a proxy for expectations at time $t$ about the likely importance of statute-related litigation that will come before court $k$ in year $t$. The number of citations made to a statute by court $k$ in a later period reflects the relative importance of a statute to the business of court $k$ in an earlier period. It is an aid in the identification of the tenure effect, as detailed below. Note that $statcite_{kt}$ varies across courts because it reflects only those citations made by citing court $k$.

*4. Results*

Section 4.1 provides a descriptive picture of how citing rates changed in the years after the Act of Settlement. That analysis also serves to introduce the use of fixed effects and the use of different measures of the dependent variable (that is, variations in $m$). Then, Section 4.2 directly examines the effect of judicial tenure within a fixed-effects OLS framework, introducing the explanatory variables whose inclusion directly strengthens identification of the causal effect. Section 4.3 presents instrumental-variable fixed-effect estimates of the effect of tenure.

*4.1 Very reduced forms*

The defining event that drives the investigation in this paper is the coming into force of the judicial clause of the Act of Settlement in 1714. The descriptive analysis of this subsection

examines citations before and after that year. Using court fixed-effects adds rigor by removing any variation that is simply due to varying levels of citations across cited courts. Moreover, because the Act's judicial clause did not apply to all judges, the court-year combinations when the Act was in force are not perfectly collinear with time. Thus, one can also add year fixed-effects, which remove any variation that results purely from changes in how well cases are reported over time and from secular changes in the importance of issues brought before the courts.

This structure provides a difference-in-differences analysis. Consider the following:

$$CITE_{kt}^{m} = \theta_0 D_{kt}^{0} + \varphi_k + \gamma_t + \varepsilon_{kt} \tag{2}$$

where $D_{kt}^{0}$ is a dummy variable with a value of 1 if both $t > 1714$ and $k$ is a common law court and 0 otherwise (since the Act of Settlement did not apply to the Lord Chancellor, the head of Chancery, an equity court). All other variables are as defined as in (1).

Column (1) of Table 3 contains the results obtained when $m$ is all courts and all years. After the legislation comes into force there is almost a one-standard deviation decline in the rate that courts are cited. Although descriptive and not causal, these results alone indicate that it is necessary to investigate systematically the oft-claimed salutary effects of the Act on judicial behavior.

One can go further in examining the association of citation rates and legislation. As documented in Section 2, until 1761 lifetime tenure meant the lifetime of the monarch, not the judge. After 1761, tenure was for the life of the judge. Estimates of the following equation are therefore instructive:

$$CITE_{kt}^{m} = \theta_1 D_{kt}^{1} + \theta_2 D_{kt}^{2} + \varphi_k + \gamma_t + \varepsilon_{kt} \tag{3}$$

where $D_{kt}^{1}$ is a dummy variable equal to 1 if $1714 < t < 1762$ and $k$ is a common law court, while $D_{kt}^{2}$ is a dummy variable equal to 1 if $1761 < t$ and $k$ is a common law court. The results are reported in column (2) of Table 3. They are broadly consistent with the first set of results since the passage of the Act of Settlement is associated with a large fall in citation rates. The passage of the 1761 judicial reform is associated with a 40% of a standard deviation increase in citation rates relative to the immediately preceding time period but a 66% decrease in citation rates

compared to the earliest part of the sample.[5] As higher salaries could cause judges to focus more on quality and less on outside activities (Ash and MacLeod 2015), it is possible that the increase in citation rates after 1761 was not due to changes in tenure arrangements but due to the large increase in judicial salaries that had occurred two years earlier (Klerman and Mahoney 2005).

Variations in *m* provide robustness exercises. Columns (3) and (4) present estimates that examine whether the results are sensitive to the courts included in *m*. In the literature on modern courts, citations by courts other than the one being cited are usually regarded as the best measure of the quality of judicial decisions because within-court citations are often made routinely whereas citations by different courts reflect the extra degree of persuasiveness needed to cross jurisdictional and subject-matter boundaries (Landes et al. 1998; Posner 2000; Choi et al. 2010; Ash and MacLeod 2015, 2016). In column (3) *m* indicates citation counts produced in all years by courts other than the one being cited: that is, *m* does not include court *k*. The citation counts used for the dependent variable in column (4) are same-court citations for all citing years. The results in columns (3) and (4) are completely consistent with those in column (1), the smaller magnitude of the coefficients simply reflecting the fewer citations included in the dependent variables.

Columns (5) and (6) examine whether the results are sensitive to the citing time-period included in *m*. In column (5), *m* includes only citations made within 20 years of year *t*, a check on whether the results simply reflect the fact that more citing years are possible for earlier time periods.[6] In column (6), *m* includes only citations made after 1830, a check on whether the results are due to the fact that citing practices changed over time and this has an asymmetric effect across courts.[7] The results are consistent across all columns of Table 3.

*4.2 Directly examining the effect of tenure*

Having established that there is much reason to doubt the conventional wisdom on the Act of Settlement and having set up the basic empirical framework to be used, the paper now focuses

[5] The estimated coefficients for the immediately pre- and post-1761 periods are statistically significantly different from each other at the 1% level.

[6] Although, countering this, there is much more detailed reporting in the latter half of the eighteenth century than in the first half of the seventeenth century.

[7] The asymmetric effect is emphasized because any effect that is related simply to time and to courts, independently, will be removed by the fixed effects. It is the interaction of time and courts that would lead to results that are misleading, even descriptively so.

on the effect of judicial tenure. The equation to be estimated is (1); the variations in *m* are given by the specifications in the columns of Table 3. This subsection begins with OLS, but several elements of (1) already serve to bolster identification of the causal effects of judicial tenure.

The panel structure admits a standard difference-in-differences framework. The court fixed effects remove any bias that might arise from a covariation across courts between citation rates and the popularity of different types of judge tenure. The year fixed effects remove any bias that might occur because of covariation over time between the level of court activity and the popularity of different types of judge tenure. This is especially likely in this era of great political and institutional change.

One concern about bias in OLS estimates would be that tenure is correlated with the human capital of judges. For example, tenure might mean that a judge serves longer, leading to a correlation between tenure and the time on the court that a judge accumulates, the latter being a proxy for the human capital of the judge. Such causes of possible bias are lessened by including $X_{kt}$, which includes measures capturing judge experience that has arisen both inside and outside court *k* (Choi et al. 2010: 305; Ash and MacLeod 2016: 24).

Same-court citations to statutes ($statcite_{kt}$) is included because there is the possibility that the form of the judicial appointment could be correlated with the appointing authority's expectations about which types of cases would be likely to come before a specific court. This is a time- and court-varying omitted variable that is not captured by the fixed effects. In particular, given the vicissitudes of politics in this era, a time of critical legislation relevant to court *k* might well coincide with a time when there was a specific disposition on how judges should be appointed to court *k*. Identification of the tenure effect is therefore improved if the regression includes a variable that captures the importance of the cases likely to come before a court. Thus, $statcite_{kt}$ is included on the premise that it is a proxy for expectations at time *t* about the likely importance of statute-related litigation to come before court *k* in future years. Note that, by construction, $statcite_{kt}$ varies across both courts and time and therefore captures court-varying time effects in the intensity of court activity. This variable could even be a proxy for more general (that is, not purely statute related) variations in the intensity of court activity, since social and political conflict would have spread contemporaneously across broad areas of legal activity, not simply channeled into litigation over legislation.

Table 4 presents estimates of equation (1) that result from fixed-effects OLS. Standard techniques are used for estimation, that is, treating the $\gamma_t$ as nuisance parameters that are solved out and not estimated. Consistency of the techniques relies on a strict exogeneity assumption on the $\varepsilon_{kt}$ (Wooldridge 2010: Ch. 10). All t-statistics are calculated using standard errors that are robust to heteroscedasticity and clustered at the year level.

The most remarkable result in Table 4—and this will be repeated in all the estimates that follow—is the one on the tenure of puisne judges: a highly significant, very large, negative effect on citations. Using column (1) as an example, a court that has all puisne judges appointed *quamdiu se bene gesserint* will garner 288 fewer yearly citations than a court that has all puisne judges appointed *durante bene placito*. This is 89% of a sample standard deviation of the number of citations. The estimated effect of the tenure of chief judges is much smaller, varies in sign, and statistically significant in only one case. Given that the standard error of the estimate of the coefficient of the chief-judge tenure variable is less than the analogous statistic for the puisne variable, the differences between the statistical significances of the coefficient estimates for the two types of judges are not simply due to differential precision in estimation. In all cases, the puisne-judge tenure effect dwarfs that of the chief judge, so that a court in which all judges have tenure receives many fewer citations than a court in which all judges can be fired at the will of the sovereign.

Turning to the results for variables that are not of primary interest for this paper, the number of years of service has a weak effect on judicial quality, some estimates being significant and positive, others being non-significant and positive. Years of service outside the current position is more important than years of service in the current position. But these effects are dwarfed by the effect of puisne-judge tenure. For example, for the estimates in column (1) of Table 4, a one-standard-deviation increase in the years of previous service of the chief judge increases citations by 28, whereas a one-standard-deviation increase in the puisne-tenure variable reduces citations by 112. Note also how remarkable are the results in column (5) given that they reflect the decisions of English courts two centuries after the time period on which there has been most modern commentary about the insecure status of English judges.

One ancillary result worth noting is that on the effect of the dummy variable capturing whether the chief judge served previously on the same court. This is negative, statistically

significant, and economically important. A one-standard-deviation change in this variable reduces citations by 20% of a standard deviation. This could be simply due to the fact that judges produce their most citable innovations when they are new to the type of litigation passing through a specific court. But it also could indicate selection effects: puisne judges who are promoted to chief judge on the same court could be the ones that have shown themselves to be reliable to the monarch and therefore less likely to introduce innovation. It is the possibility of such selection effects, but those on the crucial tenure variables, that suggests the use of instrumental variable methods.

*4.3 Instrumental variable estimates of the effect of tenure*

The process of selection of judges in 17th and 18th century England was shrouded in mystery and the information that did exist is largely lost to history. There is little in the historical record to guide the formulation of a model explaining which judges received tenure and which did not. The search for instruments must be conjectural. For this reason, two different instrument sets are used to produce two different sets of estimates. Perhaps robustness in estimates can compensate for absence of detailed knowledge of the selection process.

Common to both sets of instruments are the birth and death dates of the chief judge and the mean birth and death rates of the puisne judges. These birth and death dates are proxies for the expectations of those making appointment decisions about how long a judge might serve.[8] Such expectations could affect the type of appointment given to a judge because the costs and benefits of judge tenure for an appointing authority would depend upon the judge's expected longevity. The one concern about excludability—that such dates could proxy human capital—is blunted by noting that $X_{kt}$ includes five different measures of human capital.

The variables that distinguish the first set of instruments are the ten-year lags of $CJ_{kt}$ and $PJ_{kt}$. These lagged tenure variables are included because each court in each era had different customs on the nature of appointments.[9] The long lag—10 years—minimizes the possibility that lagged tenure is mechanically correlated with current tenure solely because of the

[8] Of course the death date is unknown at the time of appointment, but it is the best available proxy for contemporaneous expectations concerning the health of a potential appointee. Note also that since year fixed effects are implicitly included, it is not necessary to adjust raw birth and death dates for *t*.

[9] This strategy reflects some aspects of the more general Arellano-Bond GMM methodology.

long service of some judges. (In fact, in the dataset 85% of the observations on years of service for judges in the critical seventeenth century are less than 10 years.)

Table 5 presents the results using the first set of instruments. The regressions in that table match those of Table 4, except for the use of IV rather than OLS. Table 5's results are obtained using a GMM that treats error terms as clustered at the year level and that allows for arbitrary heteroscedasticity. The diagnostic tests included in Table 5 show that the instruments are strong, suggesting that more than 80% of the bias from OLS has been removed conditional on the assumptions of the model. The hypothesis that the model fits the data is accepted at the 10% level (using the Hansen J-stat) for four of the five dependent variables.[10] In sum, the diagnostics suggest that the results in Table 5 provide a reliable guide to causal estimates.

The results are broadly consistent between Tables 4 and 5 across all specifications of the dependent variable. Not surprisingly, the precision of all estimates declines when using instrumental variables. The estimates of the coefficients of the chief-judge tenure variable do not evidence any consistent change between the two tables, suggesting, as does each of the tables considered alone, that the mix of estimates is consistent with variations around zero: there is no effect of chief-judge tenure. All the estimates of the coefficients of the puisne-judge tenure variable are larger in absolute value in Table 5 than in Table 4. Given the lower precision of the estimates, two of the puisne-judge-tenure coefficients are now not significant at the 10% level, but the weight of evidence in Table 5 bolsters the conclusion that tenured puisne judges produced lower quality decisions than non-tenured puisne judges.

Estimates using the second set of instruments are presented in Table 6, whose structure matches that of Tables 4 and 5. This set of instruments also uses judge birth and death dates but discards the ten-year lag of $CJ_{kt}$ and $PJ_{kt}$. The concern with the ten-year lag is that there are a few judges who did last more than 10-years in their positions, possibly making the association with current tenure mechanical rather than causal.

The second set of instruments uses the interactions of each of the four court dummy variables with a measure of the number of days that Parliament was in session over the ten years previous to *t*. Given that the defining struggle was between monarch and Parliament and given

[10] This is a test of overall model fit and therefore the one failure is not necessarily an indictment of the instruments.

that this struggle often resulted in the monarch not calling Parliament to meet, parliamentary activity is a proxy for the intensity of that conflict (inversely). When that struggle was at its highest it would have been more important for the monarch to have judges that could be fired at will. Hence, there is a causal link from political conflict (proxied by Parliamentary activity) to tenure. The interaction with court dummy variables is because this link would be different for courts handling different types of litigation.

The diagnostic tests included in Table 6 evidence a strong set of instruments, with those diagnostics suggesting that there is nothing to choose between these instruments and those in Table 5. The central conclusions from the previous analyses hold. Chief-judge tenure has no effect on the quality of judicial decisions and tenured puisne judges produce lower quality decisions than non-tenured puisne judges. The major difference between the estimates in Table 5 and those in Table 6 lies in the comparisons to the OLS estimates. Whereas the first set of instruments resulted in estimates for the coefficient of puisne-judge tenure that are greater in absolute value than the OLS estimates, the second set of instruments produces estimates that are smaller in absolute value than the OLS estimates.

Differences between the estimates in Tables 4, 5, and 6 must be viewed through the lens of heterogenous effects and local-average treatment effects (LATE). In the OLS estimates of Table 4, the estimated coefficient of puisne-judge tenure reflects the decisions of judges selected by the monarch to have tenure. The monarch could be selecting on the basis of judge quality, which is the core reason for worries about a possible bias when treating the OLS estimates as causal effects of tenure. Focusing solely on the instrumental variables that differ between Tables 5 and 6, the estimates in Table 5 can be interpreted as reflecting the effect of tenure for those judges who receive tenure simply because the specific court has that custom at that time. Given the relation between the estimates in Tables 4 and 5, the set of all puisne judges obtaining tenure contains judges of higher quality than the set of puisne judges obtaining tenure simply because tenure has been the custom of their court. This is consistent with the natural assumption that higher quality judges are more likely to be chosen for positions that have tenure.

Now turn to comparisons between Tables 4 and 6. Focusing again solely on the instrumental variables that differ between Tables 5 and 6, the estimates in Table 6 reflect the effect of tenure for those judges whose receipt of tenure is due to the fact that the struggle

between monarch and parliament is either relatively subdued or irrelevant for the specific court.[11] Given the relation between the estimates in Tables 4 and 6, the set of all puisne judges obtaining tenure contains judges of lower quality than the set of puisne judges obtaining tenure simply because political struggle is at a low ebb. Stated in a more natural way, lower quality judges are more likely to obtain tenure at times when political struggle is high than when it is low. When there is political struggle, there is more on the mind of the monarch than judge quality.

*5. Conclusions*

This paper has examined the effect of judicial tenure, and implicitly the Act of Settlement, on the quality of judicial decision-making in 17th and 18th century England. Giving chief judges tenure had no effect on quality. Giving puisne judges tenure resulted in a large decline in quality, as measured by the numbers of citations in later court decisions. The judicial clause of the Act of Settlement delayed the development of English law, given the importance of precedent in a common-law system.

This is a conclusion that is at odds with what has been the conventional wisdom for at least two centuries. To quote Justice Harlan from over a century ago expressing a sentiment that reaches back to Hamilton and forward to today "No one, in my judgment, under our system of law, can be appointed a judge of a court of record, having jurisdiction of civil and criminal cases, to hold the office at the pleasure and will of another. No such doctrine has been maintained in England since the [Act of Settlement]…, one of the great acts which followed the revolution of 1688. Previously to that period most of the judges of the higher courts held their offices during the pleasure of the crown….This power exerted a most baleful influence upon the administration of justice…".[12]

To what extent are this paper's results externally generalizable? Certainly, one must be very cautious in claiming wider implications of the results because there are several studies on modern courts that reach the opposite conclusion concerning the effect of judicial tenure on the quality of judges' decisions (Choi et al. 2010, Ash and MacLeod 2015, 2016). Perhaps the paper's most important lessons are confined to a particular period of English history. However,

[11] The assumed sign of this relationship is verified in the first-stage regressions.

[12] McAllister v. United States, 141 U.S. 174, 187 (1891).

this period saw institutional changes of profound importance that Hamilton, Harlan, and many scholars today regard as providing paradigmatic lessons about growth and development.[13] It is in this sense that the lessons of this paper apply to a much broader canvas than just English history. Not least among these lessons is that the effect of institutional changes can be highly context contingent.

What was the context of 17th and 18th century England that could have led to the negative sign in regressions where a positive one has been the universal prior? This question is beyond the scope of this paper's empirical analysis. But one can conjecture. As Section 2 makes clear, the legal profession was enormously powerful.[14] Judges would have been aware that the profession was as important in assessing their progress as was the monarch (or Parliament). The monarch would have been reviewing for reliability, but the legal profession for quality. The admiration of the legal profession was the best protection that a judge could have against an aggrieved monarch. Producing decisions perceived by peers as of higher quality would have encouraged that admiration and protection. This would have been especially the case for puisne judges who did not have the status and power already held by chief judges.

Acknowledging the power of the legal profession in English history not only provides an explanation for the surprising results of this paper, but also allows one to see the broad consistency between these results and those on modern US courts, despite apparent inconsistencies (e.g., Choi et al. 2010, Ash and MacLeod 2015, 2016). Judges are the agents. The principals differ under different institutional arrangements. In the analyses of modern courts, competitive elections denote insecure tenure, with the public and politicians becoming important principals for the judges. The absence of such elections gives securer tenure, making intrinsic motivation and reputation among the broader legal profession more important in a judge's calculus (Posner 1993).[15] In 17th and 18th century England, a non-tenured judge was implicitly facing a continuous review of performance and would, just like modern judges facing election challenges (Shepherd 2009), be very aware of the principal's preferences. The legal

[13] For example, North and Weingast (1989), Olson (1993), Acemoglu and Robinson (2012), and Besley and Ghatak (2009).

[14] On the rise and the strength of the English legal professions, see, for example, Baker (1986), Plucknett (1983), and Halliday and Karpik (1997).

[15] Lim (2013) shows that judges receive large non-pecuniary benefits from their employment, and that following the preferences of their principals when making decisions reduces these benefits a great deal.

profession was an important principal for 17th and 18th century judges, implying that insecurely-tenured judges would emphasize quality, the yardstick of the legal profession. Thus, the natural alignment between the objectives of the legal profession and the intrinsic objectives of judges leads to an overall theory that reconciles the contrasting empirical results from the modern era and those appearing above.[16]

[16] Interestingly, that alignment between the objectives of the legal profession and the intrinsic objectives of judges is at the heart of McKenzie's (2010) theory of the reason why secure tenure is not an issue for the, currently untenured, US federal bankruptcy judges.

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## Appendix: Construction of Data Sets

*1. Construction of data on the judges*

Sainty (1993) is the primary source for the information on judges' years of service in particular positions and their terms of appointment. The information in Sainty (1993) was supplemented and checked using Foss (1848, 1870), Campbell (1846), and Cannadine (2016). Cannadine (2016) was the primary source for the birth and death dates of judges, but was supplemented and checked with information from Foss (1848, 1870) and Sainty (1993). Occasionally when birth year was not available, year of baptism was used as a proxy. When neither birth year nor baptismal year was available, the birth year was calculated as 22 years before the date at which the future judge entered one of the Inns of court.

*2. Construction of dataset on citations*

Schmidt (2015) constructed an electronic database of citations reflecting the information in Renton (1900) as digitized by Juta Law (2010). Full information on the process of construction of the database can be found in Schmidt (2015), whose procedures relied almost wholly on text recognition algorithms programmed in Python. The core of Schmidt's database is a set of records, each linking a citing case in the *English Reports* to a cited case in the *English Reports*, providing the years of both cases and the courts in which each case was decided. Schmidt's database contains 397,164 records with the dates of cited cases available in 391,997 and the dates of citing cases available in 392,054.

Because Schmidt (2015)'s analysis focused on the period after the 17$^{th}$ century, most of the missing information in his database is concentrated in the years before 1700, which are very important to the present study. To complete the database for the current study, the author filled in the missing dates using three procedures. First, visual inspection of the cited pages of the printed volumes of the *English Reports* provided a large number of dates. In this step, a randomization procedure was used to pick a case, and therefore date, from a page where a number of cited cases appeared. Second, using regularities in the correspondence between the pages of volumes and the dates that appeared thereupon, dates could be easily predicted for cases where cited or citing dates were missing. Lastly, for four smaller reports within the *English Reports* no dates were available for cases. A randomization procedure was therefore used to assign dates within the years covered by the volumes. This last step has no relevance for the present study, given that the pertinent volumes covered either before 1600 or a small three-year period in the 19$^{th}$ century. These procedures resulted in a database with 397,164 records each linking the year and court of a citing case to the year and court of a cited case.

*3. Other variables*

The measure of Parliamentary activity is the number of days in which the House of Lords was in session in a year, derived primarily from the Journal of the House of Lords (n.d.). The House of Lords was chosen because the data on the House of Commons are available for fewer relevant years in a consistent fashion. During the interregnum (1649-1659), when the Lords did not exist, Parliamentary activity is measured by the number of days that the Commons was in session, obtained from Journal of the House of Commons (n.d.). In the Journal of the House of Lords (n.d.) there were no data for the House of Lords for 1794-1800. For these years, the number of days in session was derived from information in History of Parliament Trust (n.d.)

**Table 1: 17th and 18th century institutions affecting judicial emplyoment in England. Part (a): Terms of appointment.**

| Relevant institutions, laws, organization | Applicable years | Comments |
|---|---|---|
| Appointment terms decided by monarch. | 1600-1714 | Traditionally, English judges had been appointed *durante bene placito* (during the monarch's pleasure), but with notable exceptions. See below. |
| Variations in the terms of appointment | 1600-1714 | During the late Tudor and early Stuart periods, the judges of the Court of Exchequer were appointed *quamdiu se bene gesserint* (McIlwain 1913: 220). Charles I accepted *quamdiu se bene gesserint* in 1641 (Haynes 1944: 63). Cromwell accepted all sitting judges and appointed judges *quamdiu se bene gesserint* (Black 1976; Firth and Rait 1911). Charles II used *quamdiu se bene gesserint* from 1660-1672 (McIlwain 1913; Haynes 1944). Both William III and Anne used *quamdiu se bene gesserint* 1689-1714. |
| King and Parliament consider legislating *quamdiu se bene gesserint* | 1689-1701 | Mandatory *quamdiu se bene gesserint* was in the Declaration of Rights but omitted from the Bill of Rights. Horwitz (1977: 366-7). William twice refused to sign bills to establish mandatory *quamdiu se bene gesserint*, perhaps because judges themselves did not deem such independence appropriate or perhaps because the bills continued the traditional arrangement of charging the judges' salaries to his hereditary revenues (Horwitz 1977: 75-76; Prest 1991: 82, 85; McIlwain 1913: 224). |
| Appointment on *durante bene placito* forbidden | 1714-1800 | Act of Settlement 1701, in force 1714,: "...judges commissions be made *quamdiu se bene gesserint*…" But see two exceptions immediately below. |
| Lord Chancellor appointed *durante bene placito* | 1600-1800 | The Lord Chancellor's appointment terms were not governed by the Act of Settlement. The Lord Chancellor was head of Chancery and in formal control of all judicial organization. |
| *quamdiu se bene gesserint* only for life of monarch | 1600-1761 | Appointments ended on accession of a new monarch until 1761 (Prest 1991: 82). Reappointment possible, but did not always occur. |
| *quamdiu se bene gesserint* for life of judge | 1761- | An Act promoted by George III. All previous eighteenth century monarchs had refused to renew the terms of some judges on accession (Anne two judges, George I three judges, and George II one judge). See Jay (1997: 20-21); Klerman and Mahoney (2005: 11-12); Haynes (1944: 79). |
| Monarch could no longer unilaterally decide whether "*bene gesserint*" had occurred. | 1714-1800 | Act of Settlement 1701, in force 1714: " …upon the address of both Houses of Parliament it may be lawful to remove [judges]." Largely irrelevant, since even in the seventeenth century when monarchs appointed on *quamdiu se bene gesserint*, they respected its terms. |

**Table 1: 17th and 18th century institutions affecting judicial emplyoment in England. Part (b) Administration and salaries.**

| Relevant institutions, laws, organization | Applicable years | Comments |
|---|---|---|
| Strong legal profession with large degree of autonomy and a base of power independent of the Crown and Parliament. | 1600-1800 | The centralized court structure and the Inns of Court provided barristers, and particularly the top judges, with mechanisms to exert informal control over legal-administrative matters. They controlled who became barristers, who were appointed to the upper ranks of barristers (the pool from which judges were selected), who performed daily administrative tasks, and who argued cases in court (Francis (1983: 46, 94). The profession was so opposed to the monarch in the 1620's that Charles I considered excluding them from Parliament, but failed (Medley 1902: 455). Attempts at radical legal reforms during the interregnum foundered on the opposition of the legal profession (Francis 1983: 47-48). |
| Monarch appoints judges | 1600-1800 | (Or by Cromwell during the interregnum). Appointees were always from the upper ranks of the barristers. |
| Lord Chancellor's organizational decisions affect judges | 1600-1800 | The judges' work conditions were largely under the control of the Lord Chancellor, the monarch's direct employee, who was chosen from the upper ranks of the legal profession. For example, assignment of judges to specific circuits was used as a cudgel. (Wales, instead of Norfolk, was dire punishment.) These tools were used well into the eighteenth century (Cockburn 1972). |
| Judges' salaries paid out of the monarch's household funds. | 1600-1786 | At least partially. The measure ending this passed in 1786, as part of a more general statute dividing the monarch's household expenditures from those necessary for government (Rubini 1967: 344). Tarkow (1943: 557) suggests the date was 1761 rather than 1786. |
| Judges' income dependent on monarch and litigants | 1600-?? | Judges salaries were purely at the monarch's discretion at the beginning of the Stuart period and were augmented by litigants' fees. A very gradual changing of culture, and some legislation, made the situation very different by the end of the eighteenth century (Prest 1991). |
| Commons increases judges' salaries | 1645 | An increase of over 500 percent that was not reversed, which in an era of somewhat stable long-term price levels (Allen 2001) meant that judges' salaries were kept at more reasonable levels given alternatives (Prest 1991: 83). |
| Judges salaries to be fixed. | 1714-1800 | Act of Settlement 1701, in force 1714: "...judges…salaries [to be] ascertained and established". Essentially an endorsement of a practice gradually developed over the previous century and that continued to be developed over the next century. |

## Table 2: Summary statistics

| | Obs. | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| Total citations to court *k* in year *t* made by all courts in all years, the most comprehensive version of $CITE_{kt}$ | 804 | 179 | 324 | 0 | 2646 |
| Citations to court *k* in year *t* made by courts other than *k* | 804 | 67 | 110 | 0 | 755 |
| Citations to court *k* in year *t* made by court *k* | 804 | 112 | 231 | 0 | 1978 |
| Citations to court *k* in year *t* fewer than 20 years after *t* | 804 | 48 | 110 | 0 | 939 |
| Citations to court *k* in year *t* after 1830 | 804 | 45 | 86 | 0 | 623 |
| Tenure of chief judge ($CJ_{kt}$) | 804 | 0.56 | 0.49 | 0 | 1 |
| Mean tenure of puisne judges ($PJ_{kt}$) | 804 | 0.79 | 0.39 | 0 | 1 |
| Chief judge previous service, years (an element of $X_{kt}$) | 804 | 3.14 | 4.94 | 0 | 23 |
| Chief judge served on same court (an element of $X_{kt}$) | 804 | 0.12 | 0.31 | 0 | 1 |
| Mean of puisne judges' previous service, years (element of $X_{kt}$) | 804 | 1.12 | 1.99 | 0 | 12.3 |
| Years in position, chief judge (an element of $X_{kt}$) | 804 | 6.92 | 5.68 | 1 | 32 |
| Mean years in position, puisne judges (an element of $X_{kt}$) | 804 | 8.00 | 4.51 | 1 | 25 |
| Same-court citations to statutes, previous 10 years ($statcite_{kt}$) | 804 | 257 | 311 | 0 | 1330 |
| Tenure of chief judge, ten year lag | 764 | 0.55 | 0.49 | 0 | 1 |
| Mean tenure of puisne judges, ten year lag | 764 | 0.77 | 0.40 | 0 | 1 |
| Mean birth date of puisne judges | 804 | 1640 | 61 | 1532 | 1746 |
| Mean death date of puisne judges | 804 | 1712 | 60 | 1607 | 1823 |
| Birth date of chief judge | 804 | 1641 | 59 | 1529 | 1747 |
| Death date of chief judge | 804 | 1712 | 61 | 1604 | 1826 |
| Days parliament in session, total previous ten years | 804 | 813.7 | 473.2 | 0 | 2494 |

## Table 3: The association between citation rates and the coming-into-force of legislation on judicial tenure

| Citations included in dependent variable | (1) OLS-FE All courts, all years | (2) OLS-FE All courts, all years | (3) OLS-FE Other courts, all years | (4) OLS-FE Same court, all years | (5) OLS-FE All courts, within 20 years of decision | (6) OLS-FE All courts, after 1830 |
|---|---|---|---|---|---|---|
| Common law court, after 1714 | -314.8*** | | | | | |
| | (-9.48) | | | | | |
| Common law court, between 1714 and 1762 | | -383.7*** | -141.3*** | -242.4*** | -169.1*** | -102.7*** |
| | | (-10.41) | (-9.52) | (-10.57) | (-9.36) | (-9.54) |
| Common law court, after 1761 | | -231.8*** | -39.06* | -192.8*** | -159.4*** | -47.10** |
| | | (-5.13) | (-2.34) | (-6.47) | (-4.29) | (-2.98) |
| Observations | 804 | 804 | 804 | 804 | 804 | 804 |
| Year fixed effects | YES | YES | YES | YES | YES | YES |
| Court fixed effects | YES | YES | YES | YES | YES | YES |
| R-sq. | 0.313 | 0.326 | 0.238 | 0.335 | 0.223 | 0.170 |

*t* statistics in parentheses, using standard errors clustered at year level;
$^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

## Table 4: The effects of judicial tenure: OLS fixed-effects estimates

| Citations included in dependent variable | (1) OLS-FE All courts, all years | (2) OLS-FE Other courts, all years | (3) OLS-FE Same court, all years | (4) OLS-FE All courts, within 20 years of decision | (5) OLS-FE All courts, after 1830 |
|---|---|---|---|---|---|
| Tenure of chief judge | 26.49 | -2.546 | 29.04 | 30.26* | 8.175 |
| | (0.69) | (-0.24) | (0.98) | (2.01) | (0.86) |
| Tenure of puisne judges | -288.1*** | -66.44*** | -221.6*** | -68.00*** | -49.92*** |
| | (-6.19) | (-5.34) | (-6.17) | (-4.65) | (-3.95) |
| Chief judge previous service, years | 5.622* | 0.924 | 4.698* | 1.325 | 0.722 |
| | (2.26) | (1.18) | (2.56) | (1.33) | (1.03) |
| Chief judge served on same court | -209.7*** | -81.38*** | -128.3** | -36.61* | -58.06*** |
| | (-4.02) | (-5.18) | (-3.33) | (-2.09) | (-3.90) |
| Puisne judges' previous service, years | 14.80* | 2.208 | 12.59* | 6.192* | 1.817 |
| | (1.98) | (0.85) | (2.40) | (2.31) | (0.87) |
| Years in position, chief judge | 1.063 | 1.767* | -0.705 | 1.051* | 1.718** |
| | (0.66) | (2.45) | (-0.61) | (2.00) | (2.98) |
| Years in position, puisne judges | 0.759 | 0.299 | 0.460 | -0.929 | -0.529 |
| | (0.37) | (0.42) | (0.32) | (-1.33) | (-0.97) |
| Citations to statutes of previous 10 years | 0.342** | 0.050+ | 0.293*** | 0.100* | 0.038 |
| | (3.18) | (1.74) | (3.59) | (2.57) | (1.44) |
| Observations | 804 | 804 | 804 | 804 | 804 |
| Year fixed effects | YES | YES | YES | YES | YES |
| Court fixed effects | YES | YES | YES | YES | YES |
| R-sq. | 0.361 | 0.295 | 0.345 | 0.245 | 0.260 |

t statistics in parentheses, using standard errors clustered at year level;
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ ;

## Table 5: Instrumental variable estimates of the effects of judicial tenure: instruments including ten-year lag of appointment terms

| Citations included in dependent variable | (1) IV-FE All courts, all years | (2) IV-FE Other courts, all years | (3) IV-FE Same court, all years | (4) IV-FE All courts, within 20 years of decision | (5) IV-FE All courts, after 1830 |
|---|---|---|---|---|---|
| Tenure of chief judge | 26.55 | 50.92 | -24.37 | 51.47 | 30.24 |
| | (0.17) | (1.04) | (-0.20) | (0.72) | (0.79) |
| Tenure of puisne judges | -470.9* | -194.6** | -276.3 | -164.3 | -126.3* |
| | (-2.11) | (-2.77) | (-1.61) | (-1.51) | (-2.48) |
| Chief judge previous service, years | 5.131+ | 1.099 | 4.032+ | 1.304 | 0.690 |
| | (1.67) | (1.13) | (1.79) | (1.08) | (0.77) |
| Chief judge served on same court | -184.1** | -75.23*** | -108.9* | -25.44 | -53.33*** |
| | (-3.19) | (-4.35) | (-2.53) | (-1.31) | (-3.38) |
| Puisne judges' previous service, years | 16.00* | 2.495 | 13.50* | 6.918* | 2.041 |
| | (2.15) | (0.96) | (2.54) | (2.44) | (1.00) |
| Years in position, chief judge | 2.909+ | 2.491** | 0.418 | 1.686** | 2.275*** |
| | (1.68) | (3.10) | (0.34) | (2.82) | (3.58) |
| Years in position, puisne judges | 1.762 | 0.502 | 1.260 | -0.639 | -0.326 |
| | (0.77) | (0.60) | (0.81) | (-0.79) | (-0.54) |
| Citations to statutes of previous 10 years | 0.386** | 0.060+ | 0.326** | 0.119* | 0.050 |
| | (2.94) | (1.79) | (3.23) | (2.49) | (1.53) |
| Observations | 764 | 764 | 764 | 764 | 764 |
| Year fixed effects | YES | YES | YES | YES | YES |
| Court fixed effects | YES | YES | YES | YES | YES |
| Cragg-Donald Wald stat | 8.36 | 8.36 | 8.36 | 8.36 | 8.36 |
| Kleibergen-Paap Wald stat. | 10.35 | 10.35 | 10.35 | 10.35 | 10.35 |
| critical value, 20% relative IV bias | 6.08 | 6.08 | 6.08 | 6.08 | 6.08 |
| critical value, 10% relative IV bias | 9.48 | 9.48 | 9.48 | 9.48 | 9.48 |
| SW p-value, chief tenure | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| SW p-value, puisne tenure | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| p value Hansen J stat. | 0.290 | 0.118 | 0.138 | 0.307 | 0.027 |

t statistics in parentheses, using standard errors clustered at year level;
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ ;
SW = Sanderson-Windmeijer multivariate F test of excluded instruments;

## Table 6. Instrumental variable estimates of the effects of judicial tenure: instruments including measures of parliamentary activity

| Citations included in dependent variable | (1) IV-FE All courts, all years | (2) IV-FE Other courts, all years | (3) IV-FE Same court, all years | (4) IV-FE All courts, within 20 years of decision | (5) IV-FE All courts, after 1830 |
|---|---|---|---|---|---|
| Tenure of chief judge | -36.03 | 6.027 | -42.06 | 54.08 | 15.32 |
| | (-0.46) | (0.23) | (-0.72) | (1.49) | (0.69) |
| Tenure of puisne judges | -207.0** | -37.59 | -169.4** | -110.5** | -29.55 |
| | (-2.73) | (-1.51) | (-3.01) | (-2.74) | (-1.28) |
| Chief judge previous service, years | 5.190+ | 1.061 | 4.130* | 1.468 | 0.829 |
| | (1.87) | (1.20) | (2.05) | (1.29) | (1.00) |
| Chief judge served on same court | -212.7*** | -88.86*** | -123.8** | -33.19+ | -63.57*** |
| | (-3.82) | (-5.22) | (-3.04) | (-1.79) | (-4.00) |
| Puisne judges' previous service, years | 14.50+ | 1.202 | 13.30* | 6.611* | 1.074 |
| | (1.88) | (0.43) | (2.46) | (2.37) | (0.49) |
| Years in position, chief judge | 1.058 | 1.580* | -0.522 | 1.107* | 1.579** |
| | (0.67) | (2.25) | (-0.46) | (2.13) | (2.82) |
| Years in position, puisne judges | 0.865 | -0.110 | 0.975 | -0.856 | -0.834 |
| | (0.42) | (-0.14) | (0.68) | (-1.19) | (-1.42) |
| Citations to statutes of previous 10 years | 0.353** | 0.046 | 0.307*** | 0.097* | 0.033 |
| | (3.03) | (1.48) | (3.50) | (2.33) | (1.11) |
| Observations | 804 | 804 | 804 | 804 | 804 |
| Year fixed effects | YES | YES | YES | YES | YES |
| Court fixed effects | YES | YES | YES | YES | YES |
| Cragg-Donald Wald stat | 24.50 | 24.50 | 24.50 | 24.50 | 24.50 |
| Kleibergen-Paap Wald stat. | 9.70 | 9.70 | 9.70 | 9.70 | 9.70 |
| critical value, 20% relative IV bias | 6.20 | 6.20 | 6.20 | 6.20 | 6.20 |
| critical value, 10% relative IV bias | 10.22 | 10.22 | 10.22 | 10.22 | 10.22 |
| SW p-value, chief tenure | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| SW p-value, puisne tenure | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| p value Hansen J stat. | 0.457 | 0.236 | 0.097 | 0.307 | 0.056 |

t statistics in parentheses, using standard errors clustered at year level;
+ p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001 ;
SW = Sanderson-Windmeijer multivariate F test of excluded instruments;