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# Did the Independence of Judges Reduce Legal Development in England, 1600-1800?*

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**Abstract**

Conventional wisdom confers iconic status on the clause of England's Act of Settlement (1701) mandating secure tenure for judges. This paper estimates how the move to secure tenure affected the number of citations to judges' decisions, a measure of the quality of decisions. The empirics uses two new databases, on judges' biographies and on citations. Several strategies facilitate identification of the effect of secure tenure. A court-year panel permits difference-in-differences. Controls capture judges' human capital and amount of litigation. Historical evidence, tests of sensitivity to omitted-variable bias, and instrumental-variable estimates support the findings on the effects of tenure arrangements from fixed-effects OLS estimates. Secure tenure had a strong deleterious effect on associate-judge decisions and a comparatively smaller positive effect on chief-judge decisions. The effect of all judges having secure tenure was negative, large, and statistically significant. The iconic Act had an effect opposite to that universally assumed.

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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.
>
> Alexander Hamilton (1788)

> In one sense you could say that Judge [Roy] Bean was independent. He did whatever he liked…
>
> Sandra Day O'Connor (2009)

## 1. Introduction.

When referring to one of the most valuable modern improvements in government, no doubt Hamilton had in mind the clause of the English Act of Settlement of 1701 that stated "...judges commissions be made *quamdiu se bene gesserint*", that is, during good behavior (Berger 1970). This mandated secure tenure for judges. In the 17th century, English judges could be appointed *durante bene placito [regis],* that is, during the King's pleasure, which meant that often they could be removed at the whim of the monarch: they had insecure tenure.[1] The change in tenure arrangements mandated by the Act is universally regarded as marking a watershed in legal history, a defining event in the establishment of judicial independence.

To date, Hamilton's hypothesis on the beneficial effect of the secure-tenure clause of the Act of Settlement has never been subjected to empirical tests.[2] This paper does so, using the number of citations to court decisions as a measure of the quality of those decisions. The results are remarkably at odds with almost-universal assumptions about English history. Secure tenure has a substantively large, statistically significant, deleterious effect on the citations to decisions of associate judges. The estimated effect of chief-judge secure tenure on later citations is positive but always substantively small, and usually statistically insignificant. The associate-judge effect

[1] *Quamdiu se bene gesserint* is literally translated as 'as long as he should conduct himself well', but is usually rendered as "during good behavior'. *Durante bene placito* is literally translated as "during the pleasure [of the grantor of the office]", but is usually rendered in this context as 'during the King's pleasure'. The official name of the act was "An Act for the further Limitation of the Crown and better securing the Rights and Liberties of the Subject." For the wording of the Act, see British History Online (n.d.a).

[2] Klerman and Mahoney (2005) show that financial returns varied with subjective probabilities of the passage of the relevant clause of the Act. This is not a test of the effect of the independence clause, but rather about the prevailing expectations of its effect, that is, the conventional wisdom of that era.

dwarfs that for the chief judges meaning that the effect of all judges having secure tenure on citations is negative, large, and statistically significant.

Even if one viewed the results of this paper in the most conservative fashion, they present a picture that is starkly at odds with assumptions that today go unquestioned. These results reject Hamilton's hypothesis, which is a fundamental element of the conventional wisdom on 17th and 18th century English history, an era that provides a central paradigm for celebrated works discussing modernization and development (for example., North and Weingast (1989), Olson (1993), and Acemoglu and Robinson (2012)). One might even conclude that secure tenure slowed the development of law. Citations are a measure of how much the individual decisions of a court have contributed to increases in the stock of precedents that constitute a significant part of the common law. The secure tenure clause of the Act of Settlement thus reduced legal development in England, by as much as 20% according to some rough calculations presented in the paper's conclusions. The remainder of this introduction summarizes the steps by which these conclusions are reached.

In the standard telling, the Act gave judges secure tenure, making them more independent of the monarch.[3] Then judicial performance improved, thereby establishing a core element of the legal basis for a modern economy.[4] This story has three links: the direct effect of the Act on formal judicial institutions; the subsequent effect of the institutions on judicial behavior; and the resultant effect of that behavior on English legal development. This paper examines the first two links, providing evidence pertinent to the third.

The standard story concerning that first link omits important nuances. The use of secure tenure was frequent before the Act of Settlement. Because the Act did not apply to all judges, insecure tenure continued after.[5] Both forms of judicial tenure coexisted for the whole of the

[3] Secure tenure conditioned only on good behavior is a core ingredient of formal independence and an important determinant of actual independence (Feld and Voigt 2003).

[4] 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 (for example, Hanssen 2004, Choi, Gulati, and Posner 2010), it is an almost universal assumption in the social sciences and law that judicial independence uniformly improves judicial performance (for example, Klerman and Mahoney 2005, 25, Dam 2006).

[5] These facts are not only largely ignored in the literature, but often obscured: And in the following instance, obscured by what should be the font of information on such facts, the Judiciary Office of the UK government (Courts and Tribunals Judiciary n.d.): "The fundamental concept of judicial independence came into being in England and Wales in 1701 with the enactment of the Act of Settlement. This statute formally recognised the principles of security of judicial tenure by establishing that…[judges]…hold office during good behaviour…Before 1701 senior judges held office at the sovereign's pleasure." For just one of many examples

seventeenth and eighteenth centuries, sometimes even simultaneously for different judges on the same courts. Section 2 provides an overview of the facts on the institutions relevant to judicial tenure over the period 1600-1800, integrating evidence that has hitherto remained scattered and often inaccurately portrayed in the literature.

With the more nuanced picture revealing that there was significant variation in the terms of the appointments of judges, it becomes clear that there is an opportunity for empirical work on the second link, the one from legal institutions to judicial behavior. Because the two types of judicial appointments existed side-by-side and varied in relative importance both within time-periods and within courts, it is possible to identify the effect of judicial tenure arrangements on judicial performance. The measure of judicial performance used here is the number of citations to the reports of cases in which judges participated, a standard measure of the quality of judicial decisions.

The empirical investigation uses two new databases, described in Section 3. Each contains data on English history that has previously remained dispersed in several sources in non-digital form, not readily available for empirical work. The first provides biographical information on all judges serving on England's highest courts in the seventeenth and eighteenth centuries. The crucial explanatory variables are the provisions of their appointments. But this database also provides useful controls such as proxies for human capital. The second dataset documents all citations to reports of previous cases that appear in the *English Reports* (Renton 1900). The *English Reports* are the definitive set of reports on cases heard in England's high courts before 1867.

Section 4 presents fixed-effects ordinary least-squares (OLS) estimates of the effect of judicial tenure arrangements (secure tenure versus insecure tenure) on the total number of citations to a given court in a given year. Several empirical strategies aid identification of the causal effect. First, the data set is a panel, making possible the use of year and court fixed-effects. Second, in this volatile time in English history, political conflict could have simultaneously heightened the significance of legal struggles and changed the monarch's policies on the terms of judges' appointments. Therefore, the analyses include a variable that is a proxy for such legal struggles—

in the academic literature, see Rakove (2007) "The Act of Settlement of 1701 marked a true milestone in the annals of Anglo-American jurisprudence. Prior to its adoption, royal judges held their places at the pleasure of the crown…"

citations by later courts to statutes passed immediately prior to the year of court decisions. Third, the regressions use proxies for the human capital of judges, reducing omitted-variable problems.

The core results hold in robustness tests that use variations in the citing court-year combinations used to construct the dependent variable: the results hold for citations made by the same court, made by other courts, made after 1830, and made within 20 years of the original decision. Moreover, the results are virtually identical in substance when using a tenure variable that indicates whether all judges on a court have secure tenure, capturing the presence or absence of the set of arrangements that most closely captures the intentions in the Act of Settlement.

The most plausible reason for questioning the validity of the average treatment effect (ATE) estimates from OLS would arise from the assumption that the monarch gave secure tenure only to those judges who were less adventurous in producing new law. Section 4 presents qualitative historical evidence indicating that this assumption should be rejected. Section 5 presents instrumental-variables estimates, which aim to remove any bias that might arise should the troubling assumption about monarchical appointment strategy be true. Instruments proxy the amount of conflict between King and Parliament at the time that judges were appointed. In normal times, the monarch's preferences on terms of judicial appointments would have been most important, and therefore the ATE estimates likely reflect the decisions of those judges whose selection for secure tenure was relatively more influenced by the monarch. In contrast, the local-average-treatment effect (LATE) estimates from IV reflect the effect of secure tenure on the decisions of those judges whose selection for secure tenure was relatively more influenced by Parliament. If there is bias in these LATE estimates, it would have the opposite sign of the most plausible bias in the ATE estimates. Given that these two sets of estimates are broadly consistent, the results from the instrumental-variable estimates bolster the conclusions from the fixed-effects OLS estimates.

Section 5 also examines the sensitivity of the fixed-effect-OLS estimates to potential omitted-variable bias by using the method of Oster (2019). These sensitivity tests show that it is highly unlikely that any omitted-variable bias could be large enough to vitiate the general conclusion derived from the average treatment effect (ATE) estimates of Section 4.

Concluding, Section 6 considers more speculative implications of the paper's results. It presents rough calculations on how much the change in tenure arrangements slowed the

accumulation of the stock of precedents, and therefore the development of English case law. The estimate is that the stock grew 20% less in the eighteenth century because of the mandated secure tenure of the Act of Settlement. These findings are not only the opposite to some hallowed wisdom on English legal history but also somewhat of a surprise because the literature on modern courts has usually concluded that stronger forms of judicial tenure are positively related to higher-quality judicial decisions (Choi, Gulati, and Posner 2010, Ash and MacLeod 2015, 2018). Therefore, Section 6 concludes with a conjecture, arguing that the early power and influence of the English legal profession could have led to the different findings for 17th and 18th century English judges and for those in modern times.

## 2. The Historical Context and Legal Framework

The ensuing empirics are set against the background of a turbulent, but decisive, period in English history. During the Tudor years (1485-1603), Parliament had cemented itself at the center of English political life. Over an even longer period, common-law judges and lawyers had developed a sophisticated body of law and a powerful set of legal institutions. In 1603, the last Tudor died without issue, and a foreign-born monarch, James I, came to the throne. For the next 85 years, the monarchs were members of the House of Stuart who had no appreciation of the peculiarly English position of Parliament, the importance of the common-law, and the country's dominant religion. Constant political struggle ensued, comprising, inter alia, civil wars, the beheading of a monarch, the fleeing of another, an interregnum, and the imprisonment of judges, a king, and members of Parliament.

The intensity of the political struggle abated after 1688, when Parliament reached an uneasy compromise with another foreign-born monarch, William III (Prince of Orange, but married to the daughter of the Stuart King who had just fled). One element of this enduring compromise was the Act of Settlement of 1701, which focused on who was eligible to be monarch, but, almost as an afterthought, included the clause on judicial tenure. Importantly, this clause was not a reaction to any threats to judges at the time the Act was passed: in 1701, all sitting judges already had appointment arrangements consistent with those stipulated by the Act.

This paper examines the decisions of judges from 1600 to 1800 in the four principal high courts of justice: King's Bench, Court of Common Pleas, Court of Exchequer, 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. Each court specialized in a different area of law, for example, the King's Bench in criminal law. But the boundaries were fluid, with each court competing for the business of the others.

All four of these courts were under the sway of officials and judges who were steeped in the common-law tradition. Indeed the judges in all courts were selected from a common pool of candidates, with judges often moving between courts. Each court had a chief judge and a small number of associate judges (in the terminology of the times, puisne judges).[6] The number of associate 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. Even the relevant clause of the Act of Settlement was vague as to its domain of applicability. Working arrangements depended as much on informal norms as specific legislation. Perhaps for this reason, there seems to be no suitable reference that could provide a convenient summary of the institutional arrangements relevant to this paper. To fill this lacuna, Online Appendix OA compiles facts on those parts of the courts' arrangements that are useful in interpreting both the empirical design and the results that follow. The remainder of this section provides the essential points.

Before the Act of Settlement was in force, the monarch could choose to appoint judges either to secure tenure or not. The choice of the terms of appointments varied over time and across courts until 1714, when the judicial clause of the Act of Settlement became effective.[7] Appointments did not uniformly carry insecure tenure before 1714. Different monarchs, sometimes in reaction to political pressure, made different choices on the terms of judicial tenure. These choices also varied across the four courts at a given time.

After the Act was in force, the monarch had to use secure tenure for judges on the country's highest-level courts. In a number of ways, the Act strengthened the security of tenure for judges, which is universally assumed to be a precondition of judicial independence.[8] First, appointments could no longer be insecure. Even before 1714 the conditions of secure tenure had been nearly

[6] The official names varied between courts. For simplicity, this paper only uses the designations chief and associate.

[7] The driving force behind the passage of the Act of Settlement was concern over who would become monarch when Queen Anne died. Thus, most clauses of the Act were only to become effective when that happened, which turned out to be 1714.

[8] Before 1761, secure tenure meant holding office until either retirement or death or the monarch's death, or violation of good behavior. Lifetime tenure did not arrive until 1761. All appointments expired on the death of a monarch until an act in that year.

universally respected by the monarchs if secure tenure had originally been bestowed. No doubt, this was because legally removing a judge with secure tenure involved filing suit in court, and hence leaving the court to decide on the merits.[9] Second, the effect of the Act was not simply to transfer removal powers from monarch to Parliament. Rather, it increased veto powers over removal. The new procedures for removal of a judge were that there had to be an 'address' from both Houses requesting removal. Then the monarch had the option of whether to remove the judge or not. Thus, the King and both Houses of Parliament all had to agree on removal. Third, the form of request by Parliament, the address, was regarded as a very solemn, high-stakes, political move, unlikely to be used except in dire circumstances. In the time period covered by the data, there was no such request by Parliament that was relevant to judges. Not surprisingly then, whereas the removal of a judge from office had regularly occurred in the 17th century, it was extremely rare in the 18th century. (See Online Appendix OA, Part (b) for further clarification of the details of the information given in this paragraph.)

There was an important exception to the Act's tenure clause: it was never interpreted as applying to the Lord Chancellor, the political head of the legal system and Chancery's chief judge. The Lord Chancellor always served at the King's pleasure.[10]

The legal system was under the sway of the legal profession, which on a number of occasions showed that it could successfully resist the demands of the monarch.[11] 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 the quality of judicial candidates. Destroying this structure would have sent the legal system crashing, and by 1600 the country was highly dependent on the functioning of that system.

Secure tenure did not mean that judges were free from acts of kingly caprice. Until the latter part of the 18th century, judges were paid from the revenues of the royal household, not the state

[9] There were two cases, in 1628 and 1672, where kings tried to remove judges appointed under secure tenure and, in both cases, the pertinent judges refused to leave office because no suit had been filed in court. Both cases led to stalemate, with the relevant judges continuing in office for a short time but unable to perform their duties in court. Both kings, having tried this stratagem once, learned their lesson and did not try it again. Instead they moved to appointing new judges on the king's pleasure.

[10] Note also that the Act's vague wording was interpreted as not applying to courts below the country's highest level.

[11] And also resist the demands of the head of state during the interregnum (1649-1660).

budget. The Lord Chancellor, a political appointee as well as a lawyer, controlled administrative arrangements, which could have a large effect on judges' well-being.

In sum, the Act of Settlement was 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 after the Act the status of an English judge matched the modern ideal of judicial independence. Hence, the hypothesis that the Act of Settlement improved the functioning of the judiciary is just a theory. Like any theory it should be subjected to empirical testing.

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

The *English Reports* provide the most comprehensive record available of the decisions made by England's high courts before 1860. The empirics uses a database of all citations made in the *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).[12,13] The citation 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. Online Appendix OB provides the details of the construction of both this database and the one on the biographies of judges.

The measure of judicial performance used here is the number of citations to the reports of cases in which judges participated, a standard measure of the quality of judicial decisions in economics, law, and related fields (see Landes and Posner 1976, Landes, Lessig, and Solimine 1998, Posner 2000, Choi, Gulati, and Posner 2010, Cross and Spriggs 2010, Black and Spriggs 2013, Ash and MacLeod 2015, 2018, Fix and Fairbanks 2020).[14] The reason for using citations as a measure of judge quality here is the same as in those papers. Judges cite past cases in order to pin down the authority underlying a decision and therefore almost intrinsically point to a case with

[12] This study examines time periods both before and after the Act of Settlement in order to produce precise estimates. Secure tenure is highly correlated with court and year. Citations are highly correlated with court and year for many reasons other than the ones in which the paper is interested. Therefore, any causal estimate of tenure effects must, at a minimum, include court and year fixed effects. In the post-Act period, after these fixed-effects are removed from the tenure variable, there is little variation left over in the key explanatory variables.

[13] There were a number of other courts functioning at times during these two centuries, usually specializing in very specific areas, such as the law of the sea or of religion. The four courts studied here were the dominant superior courts that functioned throughout 1600-1800. The highest-level court was actually Parliament, but its involvement in the legal system as a court of appeal was not routine in this period.

[14] Over a decade ago, Choi and Gulati (2007) provided an extensive list of the uses of citations as a measure of quality, in the law, in the social sciences, and in the wider media. Such use has become more extensive since then. Cross and Spriggs (2010) provide a comprehensive overview of the advantages, and problems, of using citations to measure the best judges and opinions, concluding that "Citations are the central metric for assessing the significance of opinions, at least from a legal perspective."

authority. If a case report is not cited, then it has not provided useful input into subsequent legal reasoning. A case report of an incoherent or bad decision is not likely to be cited for long as it will not provide courts with compelling information to justify a decision. Good opinions are therefore cited, and bad ones are usually not. Good opinions are an important measure of judge quality.

Of course, the studies cited above are on the workings of the modern legal system not on judicial behavior centuries ago. However, the core features of the modern legal system on which these studies depend is a direct product of the logic developed in English courts in much earlier times. The practice of invoking past cases was a feature of dispute adjudication in English courts as early as the 13th century and continually increased over time. Plowden and Coke, the producers of early *English Reports*, are acknowledged as providing influential examples of precedent-based logic. This approach to justifying legal decisions increased in importance monotonically thereafter, in the end solidifying in the modern notion of *stare decisis*.

Perhaps the most persistent criticism of the use of citations as a measure of judge quality in the literature on modern courts is that a citation can be a negative one, critical of a previous case. In response, many scholars (for example, Cross and Spriggs 2010) have pointed out that negative citations are actually quite rare, and can even indicate influence—bad reasoning in past cases can be simply ignored, while a negative cite is only needed against a strong past argument. The same is true of the *English Reports*. Generally, it was simply not the style of the judges to explicitly mention cases with which they disagreed, rather than distinguishing which past cases applied to the issues under consideration. To investigate the prevalence of negative cites in the database derived from the *English Reports*, I examined a 0.1% sample of the 122,122 reports of cases used to derive the data for this study.[15] In the 122 sampled cases, 84 had one or more citations, with 14 of those 84 containing negative citations. There were 698 citations in total in the sample of cases, of which only 20 citations could be interpreted as negative. Indeed, even in the 20 negative cases identified, the adjective 'negative' is perhaps too strong to capture the tenor of the report.[16] Based

[15] The details of the sample and the data obtained therefrom are available on request to the author.

[16] For example, see the Chancery case of Mackenzie v. Robinson in 1747 (3 Atkyns 559-560, ER 1122), in which there are four positive cites to English Report cases and one that is ambiguously negative: "A petition was presented on behalf of a mortgagor, that the mortgagee of a naked advowson may accept of his nominee, and present him upon an avoidance…Mr. Clarke, of counsel for the mortgagee, insisted, as there is a large arrear of interest, he ought to present…and cited the case of Gardiner versus Griffith, 2 Wms. 404 [where] the court gave no opinion, but seemed to incline that the defendant Griffith, the mortgagee, had a right to present…. Lord Chancellor. I am of opinion that…(Vide Amhurst v Dawling, 2 Vern. 401. Attorney General v Scarisbrick, ibid. 550. Jory v Cox, Prec. Cha. 71. Galley v Serjeant Selby, 1 Stra. 403. Com. 343, S. C.) Lord Chancellor mentioned the next day, that he was not quite clear as to this point, and that he had looked into the case of Gardiner versus Griffith since yesterday…he

on this sample, one can be confident that the deletion of negative cites would be very unlikely to have any effect on the overall set of findings of this paper.

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 in cases decided in the court-year combinations included in the set denoted by *m*. *k* takes on the values KB, CP, EX, or CH. *t* varies from 1600 to 1800. In the most comprehensive version of $CITE_{kt}^{m}$, *m* contains all court-year combinations. Then, robustness exercises use variations in *m*, affording the opportunity to examine how results change when examining citations from only specific courts or from only specific time-windows. Variations in *m* are introduced as they become relevant.

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

$$CITE_{kt}^{m} = \beta_c C_{kt} + \beta_a A_{kt} + X_{kt}\omega + \lambda statcite_{kt} + \phi_k + \gamma_t + \varepsilon_{kt}. \quad (1)$$

*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. $\beta_c$, $\beta_a$, and $\lambda$ are scalar parameters, $\omega$ is a parameter vector, and $\varepsilon_{kt}$ is a disturbance term. The immediately ensuing paragraphs define the variables. Table OB.1 in the Online Appendixes provides summary statistics.

The explanatory variables of interest are the judge-tenure measures $C_{kt}$ and $A_{kt}$, the former for chief judges and the latter for associate judges. $C_{kt}$ reflects the tenure of the chief judge in court *k* at time *t*, a dummy variable equal to 1 if the appointment is with secure tenure and 0 with insecure tenure. Because the number of associate judges varies over time and over courts and because citations are attributable only to courts, not to individual judges, the construction of $A_{kt}$ differs slightly from that of $C_{kt}$. For each associate judge, a tenure dummy variable equals 1 if the appointment has secure tenure and 0 otherwise. Then, $A_{kt}$ is the mean of the tenure dummy variables for all associate judges on court *k* at time *t*.

The panel structure admits a difference-in-differences framework. The court fixed-effects ($\varphi_k$) absorb all time-invariant omitted variables that might induce covariation across courts between the amount of court activity and judge tenure. The year fixed-effects ($\gamma_t$) prevent bias arising from

said, that was a mixed case, and that he doubted himself whether a covenant that the mortgagee should present (as was the case there ) was not void…"

time-invariant omitted variables that might induce covariation over time between court activity and judge tenure.[17]

A vector of seven variables, $X_{kt}$, proxies human capital, addressing the concern that the type of tenure would likely be correlated with the human capital of judges. $X_{kt}$ includes measures capturing judge experience and judge age (Choi, Gulati, and Posner 2010, Ash and MacLeod 2018).[18] As in the case of the tenure variables, the chief-judge variables in $X_{kt}$ reflect the data for a single individual, whereas the associate-judge variables in $X_{kt}$ reflect mean values for all associate judges on court $k$ at time $t$. 'Chief-judge previous service' measures the number of years that the chief judge served as a judge prior to taking the current position. 'Associate-judge previous service' is defined analogously. 'Chief judge served on same court' is a dummy variable indicating whether the chief judge previously served as an associate judge on the same court. The age variables are measured in years. '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, associate judges' is defined analogously.

One threat to identification of the causal effect of tenure arrangements might be that the monarch determined tenure status on the basis of expectations about the importance of litigation that was likely to come before a specific court.[19] Possibly, when that litigation was expected to raise politically controversial issues, judges in those specific courts would have been less likely to have appointments with secure tenure. At the same time, decisions on politically controversial cases would possibly garner more citations than other cases.

To counter this potential problem, equation (1) includes 'same-court citations to statutes' ($statcite_{kt}$)—the total number of citations made after $t$ by court $k$ to statutes passed in the years $t$-10 to $t$-1.[20] In this era, the most important political struggles were between the King and Parliament and they often resulted in the passage of legislation that was a compromise, providing fodder for

[17] Because very few judges served under different types of tenures at different times, there is no possibility of using judge fixed-effects as in Ash and MacLeod (2018).

[18] Age has been chosen to proxy human capital in many areas of the social science literature. However, it does not have much explanatory power for judges in modern courts (Teitelbaum 2006). The results below confirm this for 17th and 18th century England.

[19] Given the inclusion of fixed-effects, there is no threat to identification arising from expectations about the importance of future litigation cosming to courts in general.

[20] Note that $statcite_{kt}$ varies across courts because it reflects only those citations to statutes made by citing court k.

litigation in subsequent years and possibly a changed attitude to judicial appointment.[21] Therefore, it is prudent to control for expectations at time $t$ about the likely importance of politically controversial litigation to come before court $k$ in future years. $statcite_{kt}$ is a proxy for such expectations. The inclusion of this variable should diminish the potential omitted-variable problem noted in the immediately preceding paragraph. As it happens, the first-stage instrumental-variable results presented in Section 5 show no evidence that this problem is actually present. In that case, the value of the inclusion of $statcite_{kt}$ is simply to add precision to the estimates.

## 4. Estimates of the effect of secure tenure

This section presents fixed-effects OLS estimates of equation (1). The discussion in Subsection 4.1 centers on estimates of the parameters $\beta_c$ and $\beta_a$, the coefficients capturing the effect of secure tenure for each type of judge. Subsection 4.2 presents estimates of an alternative version of equation (1), using a tenure variable that captures whether all judges on the court have secure tenure. Subsection 4.3 presents both historical and empirical evidence arguing that the results of Subsections 4.1 and 4.2 should be viewed as capturing causal estimates of the effect of secure tenure. Section 5 provides additional evidence in support of that argument, offering instrumental variable estimates that match the results of Subsections 4.1 and 4.2 and using the methodology of Oster (2019) to probe the sensitivity of the OLS estimates to omitted variable bias.

### *4.1 Estimating $\beta_c$ and $\beta_a$.*

Table 1 presents fixed-effects-OLS estimates of equation (1).[22] The estimation is standard, that is, treating the $\gamma_t$ as nuisance parameters that are solved out and not estimated. Consistency relies on a strict exogeneity assumption on the explanatory variables (Wooldridge 2010: chap. 10). All estimates of standard errors are robust to heteroscedasticity and clustered at the year level.[23]

[21] Consider the Test Act of 1672, formally entitled "An Act for preventing Dangers which may happen from Popish Recusants" (British History Online n.d.b). Since the King's brother was one of those recusants, the political controversy is evident. This measure inspired much important litigation, mostly confined to the King's Bench. Around this time, Charles II also began appointing judges on insecure tenure after previously always giving secure tenure. But this new policy on appointments was applied across all courts, which, after the application of fixed-effects, would actually imply that there would be no correlation between the tenure variables and the expectation of the types of litigation coming in front of specific courts.

[22] For the sake of brevity, the results of regressions without fixed-effects are not presented. As it happens, these results provide a picture that is consistent with what follows, and therefore could be regarded as providing a further robustness test.

[23] This operationalizes the assumption that disturbance terms might be correlated across courts in a given year, reflecting the temporal vicissitudes of a politically volatile era. The number of year clusters (201) is sufficiently large for reliable estimates of clustered standard errors using standard, that is non-bootstrap, procedures. Using standard criteria, the number of court clusters (4)

Focus first on column (2) of Table 1, where (defining $m$) the citations included in the dependent variable are from all courts in all years. The key results are the estimates of the coefficients of the tenure variables, $\beta_c$ and $\beta_a$. For associate judges, secure tenure has a statistically significant, very large, negative effect on citations. A court that has all associate judges appointed with secure tenure will garner 287 fewer yearly citations (0.89 standard deviations) than a court with all associate judges having insecure tenure. The regression in column (1), without any of the control variables apart from fixed effects, has an estimate of $\beta_a$ that indicates a slightly stronger negative effect.

The estimated effects of secure tenure for chief judges are small, positive, and statistically insignificant in both columns (1) and (2) of Table 1.[24] Moreover, the associate-judge secure-tenure-effect dwarfs that of the chief judge: in the estimates in column (1), a court in which all judges have secure tenure receives 267 fewer citations than a court in which all judges could be fired at the will of the sovereign; for column (2), it is 248 fewer. These results for chief judges are substantively important because it is standard in the English history literature to assume that mandatory secure tenure had a positive effect on the legal system. In that sense, the results for chief-judges and associate-judges are consistent: they both do not provide support for the standard assumption in the literature.

Variations in $m$ provide robustness exercises. Columns (3)-(6) of Table 1 examine whether the results are sensitive to the citing time-period included in $m$. In columns (3) and (4), $m$ includes only citations made after 1830, a check on whether the substantive conclusions are affected by any change in citing practices over time. In columns (5) and (6), $m$ includes only citations made within 20 years of year $t$, a check on whether the results are influenced by more citing years being available for earlier time periods.

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is too small for reliable estimates of clustered standard errors using standard procedures. Section 5 presents estimates of the statistical significance of the key parameters when clustering on both court and year, using a wild bootstrap (Roodman et al. 2019).

[24] Given that the standard errors of the estimates of $\beta_c$ and $\beta_a$ are of similar magnitude, the differences between the statistical significances of the two estimates are not due to differential precision in estimation. A test of equality of the coefficients is rejected at the 99% level. To consider whether multicollinearity could be responsible for the differences in the results for the two levels of judges, the regressions in Table 1 were re-run twice, each run containing only one of the Ckt and Akt variables. The estimates for the Akt coefficients closely matched those in Table 1. The estimates for the Ckt coefficients were negative, economically important and statistically significant, although still much smaller in magnitude than the corresponding coefficients for Akt. This suggests some role for multicollinearity in the differences between the results for chief- and associate-judges. However, none of the conclusions drawn in this paper depend on those differences.

The estimated $\beta_a$ in the regressions with all the controls appearing in columns (4) and (6) of Table 1 indicate larger effect sizes than in column (2) once the scale of $CITE_{kt}^{m}$ is taken into account: the effect sizes are 1.05 and 1.41 standard deviations, compared to 0.89 for column (2). The effect sizes for $\beta_c$ are 0.12, 0.03, and 0.29 standard deviations in columns (2), (4), and (6) respectively. In all the regressions in this paper that include the controls, the estimated coefficient on $\beta_c$ in column (6) is the only estimate of either $\beta_c$ or $\beta_a$ that is positive and significant (albeit at only the 90% level). In column (6), the associate-judge coefficient is twice the size of the chief-judge coefficient.

Columns (7)-(10) examine whether the results are sensitive to the courts used in $m$. The results in column (8)—for same-court citations—provide strong support for the conclusion that secure tenure had a negative effect on judge quality. (Effect sizes are 2.0 and 0.22 standard deviations, on $\beta_a$ and $\beta_c$ respectively.) However, 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: 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. Lessig, and Solimine 1998, Posner 2000, Choi, Gulati, and Posner 2010, Ash and MacLeod 2015, 2018). The relevant results are reported in columns (9) and (10). In the regression in column (10) with all controls, the highly statistically significant estimated $\beta_a$ indicates an effect size of 0.96 standard deviations and there is a small (0.1 standard deviations), statistically insignificant estimated negative effect for chief judges.

A summary of all the results on the tenure variables would place them in the context of the existing consensus in the historical literature, that is, the theory that the Act of Settlement had a positive effect on the quality of judge decisions. A cautious summary would be that chief-judge secure tenure has a small, statistically insignificant positive effect and associate-judge secure tenure has a substantively large, statistically significant negative effect. There is no support for the conventional wisdom on the effect of the Act of Settlement.

The results in Table 1 on $statcite_{kt}$ and $X_{kt}$ are of less direct substantive interest here. $statcite_{kt}$ is consistently positive and statistically significant in three of the five regressions. This justifies its inclusion as a proxy for expectations of the likely importance of politically controversial litigation to come before a court. The proxies for human capital in $X_{kt}$ have estimated effects that are most

often of weak significance, statistically and substantively, perhaps due to multicollinearity. The estimated coefficients on previous-years-service are sometimes statistically significant, and all positive as one would expect of proxies for human capital. The variation in the estimated coefficients on the age and years-in-position variables is sufficiently large that the estimates do not offer no insights. The estimated effect of the chief judge having served previously on the same court is consistently negative and statistically significant. This could be simply due to the fact that judges produce their most citable innovations when they are new to the type of cases passing through a specific court.

*4.2 Estimating the Effects of Secure Tenure for the Whole Court*

One objection to the previous framework as giving insight into the effects of the Act of Settlement is that the Act dictated that all judges on specific courts should have secure tenure, whereas the estimates of $\beta_c$ and $\beta_a$ capture the effects of marginal increases in secure tenure. It might be that the institution of secure tenure only has a powerful effect when all judges have secure tenure. This objection can be examined by estimating the following equation:

$$CITE_{kt}^{m} = \beta_w W_{kt} + X_{kt}\omega + \lambda statcite_{kt} + \phi_k + \gamma_t + \varepsilon_{kt} \qquad (2)$$

where the only change from (1) is that $C_{kt}$ and $A_{kt}$ have been replaced by $W_{kt}$, a dummy variable equal to 1 iff all judges on the court have secure tenure.

The results appear in Table 2. All estimated $\beta_w$ are statistically and substantively significant and show a larger effect-size than the corresponding estimates of $\beta_c + \beta_a$ derived from Table 1. This suggests that there is an extra effect due to all judges having secure tenure. However, the extra effect has the same sign as in previous estimates, underscoring previous conclusions.

*4.3 Are the Estimated Effects of Secure-Tenure Causal?*

Acceptance of the above results as providing evidence on the causal effect of secure tenure on citations depends critically on the credibility of one assumption. This is that selection for secure tenure is conditionally independent of predictions at the time of appointment of future judge quality as measured by later citations.

The most obvious theory questioning this assumption is that the monarch might be more likely to give secure tenure to judges who would favor the King, and that these types of judges would be of a conservative disposition, less likely to produce the type of new law that would get

highly cited in later years. This has surface plausibility, but it is much less than a forgone conclusion. Six reasons argue against this conclusion. First, it confounds political conservatism with legal conservatism. On a number of occasions, it was the monarch who favored positions that were legally more radical than those held by lawyers who sided with Parliament (Edie 1985). Second, judges were chosen from a narrow pool of candidates that had undergone the arduous and intellectually demanding process of rising through the legal profession. Everybody in that pool had potential, at least, to generate high quality legal decisions.

Third, attachment to the King did not imply a lack of legal or intellectual prowess, as evidenced, for example, by the case of Francis Bacon (Coquillette 1992). Even the notorious Judge Jeffreys, whose behavior during trials was characterized by Roscoe Pound (1928, 11) as leaving "an abiding impression of unnecessary brutality of manner" and an "obvious and vehement partisanship", left a legacy of contributions to the law:

> In that brilliant Whig apologia, Macaulay's *History of England*, Jeffreys is portrayed as a drunken brute, quite ignorant of law, who…reached high judicial position as a ready instrument of the tyranny and rapacity of James II. I well remember the shock when as a first-year student of law I came upon a decision of Jeffreys as Chancellor in a collection of authorities on the law of Property. With my mind full of Macaulay's invective, it seemed incredible that what such a man may have decided could possibly deserve or have any authority. Later when I had to teach the law of Trusts, and hence was led to study the old equity decisions, I was astonished to find how well Jeffrey's decisions as Chancellor had maintained themselves. I was amazed to find how much more he counted in the reports which have made our law than Somers, who is next to William III, the hero of Macaulay's history…Whatever else he may have been, the law books show clearly enough that Jeffreys was a lawyer—indeed was no ordinary lawyer. Pound (1928, 7-8)

Pound's amazement surely reflected his surprise at realizing that he had to reject that enticing theory that those judges who favored the King were the ones who were less likely to produce innovative law.

Fourth, the results in column (4) of Table 1 and column (2) of Table 2 reflect citations made after 1830, long after the passage of time had made settled law of the issues that animated 17$^{th}$-century monarchs and Parliaments. Fifth, given the lackluster performance in forecasting judicial

decisions by modern experts with a wealth of information (Ruger et al. 2004),[25] one might be attributing a little too much perspicacity to the monarchs in assuming that they could look into the future and make a prediction of the judge's performance in matters of law. Many of the monarchs were educated in environments that placed no emphasis on the workings of the common law, which would have diminished their forecasting capacity.[26] Certainly, James I interacted closely with the greatest lawyer of them all, Edward Coke, before appointing him Chief Justice of Common Pleas, but later was more than a little surprised by his performance (Holdsworth 1935).

Sixth, some quantitative information can be brought to bear on implications of the theory that judges favorable to the monarch would be simultaneously more likely to gain secure tenure and less likely to be of high quality. If the monarch were using predictions of the future performance of judges when selecting for secure tenure, then such selection would have been most important when the courts were likely to face particularly controversial issues. But results reported in Online Appendix OC show that there is no significant difference between the mean values of predictors of the importance of impending litigation when new judges were appointed with secure tenure and the analogous mean values for appointments with insecure tenure.[27] The same result is shown for judge firings and non-renewals, which actually occurred more often when the predicted importance of impending litigation was less. Thus, in examining the circumstances surrounding the monarch's hiring and firing decisions, Online Appendix OC provides no evidence for direct implications of the theory that monarchs gave secure tenure to trusted judges who, in turn, would not produce

[25] In the 2002 Supreme Court term, a group of experts in the pertinent areas of the law was able to forecast 59% of binary decisions correctly (Ruger et al. 2004). The hypothesis that the forecasts were no better than random was not rejected at the 1% significance level.

[26] James I was the monarch who was most knowledgeable about law, but to the frustration of his critics, he viewed the law through a European prism. Charles I's education was most influenced by his father and a Scottish tutor. Charles II spent the years when he might have learned about the law under the influence of his mother, a member of the French royal family. Both demand and supply assured that the education of James II was limited. William III was 39 years old before he took the English throne. His wife, Mary, and her sister, Queen Anne, received the standard amount of political and legal education for women at that time, virtually nothing. George I only came to England when he was 54, and only had a smattering of English. There is no evidence that George II or George III possessed either the intellects or the preferences necessary to gain a deep understanding of British legal and constitutional matters.

[27] These mean values are calculated using residuals from regressions on court and year fixed-effects, since there would be no omitted-variable bias arising from monarch preferences that did not vary across courts in specific years or across years in specific courts.

citable decisions.[28] This evidence can be viewed as a substitute for the standard pre-treatment-trends analysis that is not feasible given the structure of the current data set.

All six of these arguments serve to bolster the credibility of the crucial assumption that selection for secure tenure is conditionally independent of predictions at the time of appointment of future judge quality and therefore that the unobservables omitted from equation (1) are not likely to be important in explaining selection for treatment. Therefore, the results presented in Tables 1 and 2 arguably reflect the causal effect of secure tenure on citations.

## 5. Robustness Exercises

This section presents further evidence bolstering the conclusion summarized in the immediately preceding paragraph. The evidence is provided in a series of robustness exercises. None of the many details of the exercises reported in this section alter in any important way the conclusions reached in the previous section. Therefore, many of those details, including the tables of results, are relegated to Online Appendixes. Instead, the focus here is on providing an overview of procedures and results. Subsection 5.1 describes instrumental variable estimates. Subsection 5.2 summarizes an implementation of the Oster (2019) procedure that examines the sensitivity of OLS estimates to omitted variable bias. Subsection 5.3 reports on bootstrap estimates of standard errors for the OLS and instrumental-variable estimates, particularly by implementing clustering on courts. Section 5.4 examines sensitivity to the presence of outliers by modifying the measurement of the dependent variable.

### *5.1 Instrumental-Variable Estimates of the Effect of Secure Tenure*

As already noted, the most obvious theory predicting a bias in OLS estimates is that there is a link between the types of judges selected by the monarch and the subsequent citability of those judges' decisions. One way to diminish, or even reverse, this bias is to obtain estimates of $\beta_c$, $\beta_a$, and $\beta_w$ that more closely reflect the effect of secure tenure for those judges appointed at times when the monarch was relatively constrained in choosing the terms of judicial appointments.

[28] Given the available data, it is not possible to directly test the hypothesis that monarchs were able to pick for secure tenure those types of judges who would be reliable, and therefore not be highly cited. Instead, it is necessary to examine implications one-step removed from the hypothesis. Details are provided in Online Appendix OC.

Implementation of this approach uses an instrumental variable reflecting the tightness of those constraints on the monarch, producing local-average-treatment-effect (LATE) estimates.

The defining struggle in the era surrounding the Act of Settlement was between monarch and Parliament. One element of this struggle was Parliament's preference to make the judges more independent of the monarch.[29] The supporters of Parliament would have had relatively little influence on judge appointments when the monarch was either strong enough or determined enough not to call Parliament. In contrast, when Parliament was meeting regularly, either the differences in opinion between monarch and Parliament were muted or the monarch needed Parliament's cooperation because the government was in need of Parliamentary approval of increased financing. The required instrumental variable captures Parliamentary activity: the number of days that Parliament was in session over the ten years previous to $t$. But, given the inclusion of the fixed effects, an instrument must vary across $k$ for given $t$. Therefore, days-Parliament-in-session is interacted with each of the court dummy variables. This reflects the fact that King and Parliament would have been more focused on some courts than others. The two courts whose litigation was mostly likely to reflect political conflict or cooperation were the King's Bench and the Common Pleas. The excludability condition for instrument validity is satisfied because the inclusion of $statcite_{kt}$ in the second-stage regression captures any direct link from political conditions to citation rates. $statcite_{kt}$ is formulated exactly to operationalize this link.

Even though 'Days Parliament in session × court dummies' gives four variables, only three are independent and they only reflect two pieces of information, political conflict and relative preference over courts. Given two endogenous variables, another instrument would be useful. The process of selection of judges in 17th and 18th century England was shrouded in mystery and there is little in the historical record to explain which judges received secure tenure and which did not. It is necessary to conjecture. Perhaps, the more years the chief judge was expected to serve the less likely that new judges on the court would be given secure tenure because that would have locked-in the make-up of the court for many years, an outcome that would have reduced future options for monarch and profession. The additional instrument is the expected life expectancy of the chief

[29] Parliament let its preferences be known formally in 1640, 1645, 1674 1680, 1689, 1691, and 1696. See Online Appendix OA, part (a) for details.

judge of the court, proxied by the number of years that the judge did live.[30] The excludability condition for instrument validity is satisfied because this is purely a measure of the expected longevity of a chief judge's position on the court rather than the chief-judge's qualities as a judge. To the extent that there are concerns that life expectancy might proxy human capital these would be blunted by the inclusion of the seven variables in $X_{kt}$.

It is worthwhile emphasizing what type of selection for treatment the instrumental-variable estimates will reflect. Let us focus on the 'Days Parliament in session × court dummies' variable. The estimates are local-average-treatment effects reflecting the effect of secure tenure on the decisions of judges who have two characteristics: they would receive secure tenure when Parliament had relatively more influence over judge-appointment decisions and they would not receive secure tenure when the monarch could safely ignore Parliament. Their secure tenure reflects Parliament's preferences and not the monarch's. Therefore these instrumental-variable estimates are not subject to the type of bias that would arise from a theorized link between the types of judges selected for secure tenure by the monarch and the subsequent (non) citability of those judges' decisions. Those same concerns would not arise for judges favored by Parliament.

Table OD.1 in the Online Appendixes presents the first-stage regressions, together with standard diagnostics, which suggest strong instruments. The estimated coefficients of the political conflict variables in those regressions indicate that in periods of greater conflict there would be fewer appointments with secure tenure in the King's Bench and the Common Pleas. Chief-judge life expectancy has a negative effect.

Table OD.2 in the Online Appendixes presents the instrumental-variable estimates of equation (1). Crudely summarized, the $\beta_a$ from IV are an average of 10% smaller in absolute magnitude than the corresponding coefficients from OLS; the estimated sizes of the $\beta_c$, on average, differ little from OLS. Naturally, the standard errors are considerably higher in IV estimation than in OLS, but even then a majority of the estimated $\beta_a$ are significant at the 5% level. The $\beta_c$ are all non-significant. Tables OD.3 and OD.4 in the Online Appendixes provide the results for a similar exercise for whole-court secure tenure. The $\beta_w$ from IV are an average of 17% smaller in absolute

[30] Of course the death date is unknown at the time of appointment, but it is the best available proxy for contemporaneous expectations concerning the future health of a potential appointee.

magnitude than the corresponding coefficients from OLS. Three of the five coefficients are statistically significant at the 5% level.

The results of Sections 4 and 5 complement each other when drawing overall conclusions. The ATE estimates of Section 4 reflect the decisions of those judges whose terms of appointment were relatively more influenced by the monarch. The LATE estimates of this section reflect the effect of secure tenure on the decisions of those judges whose terms of appointment were relatively more influenced by Parliament. If there were bias resulting from the characteristics of the types of judges that were selected for secure tenure, then one would expect to see stark differences between these two sets of estimates. In fact they give broadly consistent results, thereby bolstering each other in providing evidence for this paper's overall conclusions.

*5.2 The Sensitivity of the OLS Estimates to Omitted-Variable Bias*

An alternative approach to examining the validity of the evidence from OLS is to probe the sensitivity of those estimates to possible omitted-variable bias. One immediate perspective can be gained from a comparison of the results in columns (1) and (2) of Table 1, the first omitting the eight controls and the second including them.[31] Addition of the eight controls reduces the absolute size of the estimated $\beta_a$ by only 9% on average, slightly weakening the negative effect of associate-judge secure-tenure. In reducing the estimated $\beta_c$ by 40%, the addition of the controls slightly weakens the already-weak positive effect of chief-judge secure-tenure. The insight of Altonji, Elder, and Tabler (2005) is that the size of these changes provides information on the likely effect of unobservables. Oster (2019) builds on this insight and develops a method of estimating bounds on the causal value of the coefficient of interest.

The details of the application of the Oster (2019) method are relegated to the Online Appendixes, as are the results from that application. The briefest summary suffices here. The Altonji, Elder, and Tabler (2005)-Oster (2019) procedure is generally viewed as providing estimates of bounds on the likely true coefficient, producing an estimated interval in which an estimated coefficient would almost certainly lie if all omitted-variable problems were solved. Thus, for example, while the estimate of $\beta_a$ originally reported in column (2) of Table 1 is -287, the estimated interval for $\beta_a$ is [-287, -169]. Using three alternative assumptions to implement the

[31] In the following discussion, the fixed-effects are always included in the pertinent regressions, and thus are not included in the set of variables referred to as controls.

Oster (2019) procedure, fifteen such intervals are presented in Table OE.1, three for each of the five relevant $\beta_a$ estimates of Table 1 (those in even-numbered columns). Only three of the fifteen intervals contain zero, and the major part of each of these three intervals lies in negative territory. It is unnecessary to carry out the same exercise for the estimates of $\beta_c$ in Table 1 because the estimates of $\beta_c$ with controls are all smaller than the estimates without controls. Application of the Oster (2019) procedure to these $\beta_c$ would therefore lead to an interval whose maximum is the estimated $\beta_c$ and whose minimum is closer to zero or, more likely, negative, thereby providing evidence consistent with previous conclusions.

When applying the Oster (2019) procedure to the $\beta_w$ estimates reported in Table 2, the overall conclusion is nearly identical to that reached for the analogous exercise on $\beta_a$ (see Table OE.2). Recalling that the almost universal hypothesis in the historical literature is that $\beta_c$, $\beta_a$, and $\beta_w$ should all be positive, the application of the Altonji, Elder, and Tabler (2005)-Oster (2019) procedure provides further evidence that this hypothesis should be rejected.

*5.3 Standard Errors When Clustering on Courts and Years*

The results reported so far use estimates of standard errors assuming clustering on years. The number of year clusters (201) is sufficient for large-sample theory to provide reasonable criteria for interpretation of the estimates, facilitating the use of standard techniques. However, the number of court clusters (4) is sufficiently small as to necessitate the use of alternative techniques. The wild bootstrap has been found to perform well in such circumstances (Roodman et al. 2019). Online Appendix OF presents *p*-values and confidence sets for all of the estimates of $\beta_c$, $\beta_a$, and $\beta_w$ reported above, but now using wild bootstrap with clustering on both court and year.

The standard errors of coefficients do become larger, as is usual. However, the pattern of previous results on statistical significance is preserved. All of the $\beta_a$ reported in Table 1 are statistically significant at the 10% level when there is clustering on both court and year. None of the $\beta_c$ of Table 1 are statistically significant at the 10% level. Four of the five $\beta_w$ reported in Table 2 are statistically significant at the 1% level, while the other is significant at the 5% level. There is very little change in the significance levels of the estimated $\beta_c$ and $\beta_a$ produced by IV and reported in Table OD.2 and in the estimated $\beta_w$ reported in Table OD.4.

*5.4 Using the Logarithm of Citations as the Dependent Variable*

Online Appendix OG provides a further set of robustness exercises, containing a set of tables that match those discussed above, but using $\ln(1 + CITE_{kt}^{m})$ as the dependent variable instead of $CITE_{kt}^{m}$. This transformation provides a check on whether estimates are unduly affected by large positive outliers in the dependent variable. As it happens, the results in Online Appendix OG provide even stronger support for the main conclusions of this paper than is provided by the results included in the body of the paper.[32]

## 6. Conclusions and Conjectures

This paper has examined the effect of judicial tenure arrangements, and implicitly the Act of Settlement, on the quality of judicial decision-making during England's formative years of modern development. Conferring secure tenure resulted in a large decline in the quality of the decisions of associate judges, while perhaps improving the quality of the decisions for chief judges, but the net effect for the court as a whole was negative. In a common law system, judges' decisions establish precedents, the reasons for applying legal rules that are implicitly used in decisions.[33] Then the body of precedents forms a legal capital stock that later supplies information for judges or private agents. One measure of the amount of legal development is the growth of this precedential capital stock, with the importance of the information flows from specific precedents measured by the number of citations to them. From this perspective, the judicial clause of the Act of Settlement delayed the development of English law.

By how much was development slowed? Using the estimates in column (2) of Table 1 and assuming that tenuring rates from 1715-1800 were at the mean rate for the period 1600-1714, the counterfactual prediction is that decisions made from 1715 to 1800 were cited at a 19.5% lower rate than they would have been had the Act of Settlement not been in force. This is obviously a very crude measure of the amount of precedential reasoning lost, but it is one indication of the substantive significance of the estimates reported above.

[32] In a further robustness test addressing the issue of the effect of outliers, all observations from the data set that contained more than 50 citations for the dependent variable were deleted. Then all the regressions in the paper were re-run. When comparing the results for the restricted dataset and those in Tables 1, 2, OD.2, and OD.4, one can conclude that none of the paper's core conclusions would be changed by relying on the restricted dataset. These results are available from the author on request.

[33] This formulation follows closely that of Landes and Posner (1976).

The overall conclusion of this paper is at odds with the conventional wisdom that has held sway for more than two centuries. To quote Justice Harlan from 1891, expressing a sentiment that reaches back to Hamilton and forward to today:[34] "No one, in my judgment, under our system of law, can be appointed a judge…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…". (*McAllister v. United States*, 141 U.S. 174, 187 [1891])

Are this paper's results externally generalizable beyond this historical episode? Certainly, there are several studies on modern courts that reach the opposite conclusion on the effect of different forms of tenure on the quality of judges' decisions (Choi, Gulati, and Posner 2010, Ash and MacLeod 2015, 2018). Perhaps the paper's most important lessons are confined to a particular period of institutional and political development. However, this period saw institutional changes of profound importance, which Hamilton, Harlan, and many of today's scholars regard as providing paradigmatic lessons about institutions, growth, and development (for example, North and Weingast 1989, Olson 1993, Acemoglu and Robinson 2012, and Besley and Ghatak 2009) It is in this sense that the lessons of this paper apply to a much broader canvas than that of English history. Not least among these lessons is that the effect of institutional changes can be highly context contingent. And given that 17th century England was a developing economy, perhaps this paper's results are germane when deliberating on today's developing countries.

What was the context of 17th and 18th century England that could have led to the negative effect where a positive one has been the universal prior? This question is beyond the scope of this paper's empirical analysis. But one conjecture reflects the sentiment in the quote from Justice O'Connor in this paper's epigraph—incentives. The legal profession was very powerful.[35] Untenured judges had two principals, officially the monarch and unofficially their profession. The

[34] Moving forward through time, McIlwain (1913, 217) states that "In the history of the tenure of English judges the…Act of Settlement, is the greatest landmark"; Tarkow (1943) emphasizes the tenure clause while comparing the Act of Settlement to Magna Carta; Shetreet and Turenne (2013) view the Act of Settlement as fundamental in securing judicial independence, which they regard as essential to the rule of law; and Baker (2019, 178-9) comments that contemporaries believed that the best solution to the problem of Crown intervention into the courts was the awarding of life-tenure for judges.

[35] On the rise and the strength of the English legal professions, see, for example, Baker (1986), Cromartie (2006), Plucknett (1983), and Halliday and Karpik (1997). One measure of that strength is that in the first half of the 17th century over one half of the members of parliament had been educated in the Inns of Court (Cromartie 2006,180-1).

monarch would have been reviewing for political reliability, but the profession for quality. The admiration of the legal profession was the judge's best protection when facing an aggrieved monarch in a very dangerous political environment. The protection would have been greater for those judges perceived by their peers as producing higher quality decisions.

These speculations gain force when comparing the results for chief judges and associate judges, the effect of secure tenure being clearly negative for the latter and positive, but much closer to zero, for the former. Such was the prestige of the law in England that the chief judges were politically powerful simply by dint of their positions. They had already risen to such a high level of prestige that the protection of the profession probably did not mean much for them: they were the ones who provided the political protection for lesser members of the profession. This could explain why the effect of secure tenure was insignificant for chief judges.

Things were certainly different for the associate judges, who might be especially vulnerable. Under secure tenure, associate judges could treat their position as a sinecure if they did not have either intrinsic motivation or ambition to rise in the legal hierarchy. Arguably, they worked less hard or their decisions became tainted by the many opportunities available at that time to make money out of a judicial position. There were very lax rules on what constituted corruption and what was a conflict of interest, making outside sources of income extremely lucrative for the judges. Indeed, in the time period covered by the data, the amount of money to be earned from judgeships and practice of the law in general was as high, relatively speaking, as at any time in English history (Rubinstein 1983, Wasson 1998). Notably, when the chief judges advised the King in 1691 on a bill that contained features that were similar to those that were eventually embodied in the judicial clause of the Act of Settlement, they encouraged him not to sign because they thought it was not appropriate for a judge to be independent of the Crown.[36] A chief judge would find it much harder to control an independent associate judge.

[36] Burnet (1734, 86). The bill to which Burnet was referring was entitled 'An Act for ascertaining the commissions and salaries of judges.' Burnet 's words were "Among the Bills that were offered to the King, at the end of the Session, one was to secure the Judges Salaries; and to put it out of the King's power to stop them. The Judges had their Commision, during their good behaviour; Yet their Salaries were not so secured to them, but that these were at the King's pleasure. But the King put a stop to this, and refused to pass the Bill: for it was represented to him, by some of the Judges themselves, that it was not fit they should be out of all dependence on the Court." I have interpreted "some of the judges" to mean the chief judges, since they would certainly have been those consulted by the King.

This reasoning helps to explain the differences between this paper's conclusions and those in the studies on modern US courts that focus on the effects of different forms of tenure on the quality of judicial decisions. Judges are the agents. The principals differ under alternative institutional arrangements. In the analyses of modern courts, judges who face competitive elections have insecure tenure, with the public and politicians becoming important principals. The absence of such elections gives securer tenure, strengthening the relative importance of the incentives arising from intrinsic motivation and reputation within the broader legal profession (Posner 1993). This incentivizes higher quality decisions.[37] In the modern era then, secure tenure increases the relative importance of the incentives to produce decisions that are recognized as having high quality.

In 17th and 18th century England, judges with insecure tenure were implicitly facing a continuous review of performance and would, just like modern judges facing election challenges, be aware of their principals' preferences (Shepherd 2009). However, in contrast to modern times, a strengthening of tenure arrangements in 17th and 18th century England would have decreased the relative importance of the incentives induced by the legal profession, the principal that would have focused on the quality of decisions. In early England then, only intrinsic motivation would have increased in relative importance on the awarding of secure tenure, giving broader scope for a 'Judge Roy Bean' effect. Thus, in historical England, the incentives provided by the legal profession became less important after the awarding of secure tenure, in contrast to the effects of secure tenure in modern courts. There is no fundamental inconsistency in the contrasting empirical results from the modern era and those of this paper, once one considers the differing institutional contexts.[38]

[37] Lim (2013) shows that judges receive large non-pecuniary benefits from their employment, and that following the preferences of their principals reduces these benefits significantly.

[38] One example from modern times that is consistent with the results from English history is the alignment between the objectives of the legal profession and the intrinsic objectives of judges in US federal bankruptcy courts, which McKenzie (2010) argues is the reason why secure tenure is not an issue for the, currently untenured, judges.

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## Table 1: The effects of chief- and associate-judge secure tenure on citations: OLS fixed-effects estimates

| Citations included in dependent variable | (1) All courts, all years | (2) All courts, all years | (3) All courts, after 1830 | (4) All courts, after 1830 |
|---|---|---|---|---|
| Secure tenure of chief judge | 66.68 | 39.08 | 14.72 | 2.751 |
| | (1.50) | (0.84) | (1.30) | (0.23) |
| Secure tenure of associate judges | -329.5*** | -287.0*** | -62.37*** | -47.45*** |
| | (-6.86) | (-6.04) | (-5.61) | (-3.50) |
| Chief judge previous service, years | | 4.715+ | | 0.118 |
| | | (1.70) | | (0.16) |
| Associate-judge previous service, years | | 14.43* | | 2.713 |
| | | (1.99) | | (1.37) |
| Chief judge served on same court | | -211.0*** | | -59.47*** |
| | | (-4.08) | | (-4.05) |
| Years in position, chief judge | | -0.334 | | 0.565 |
| | | (-0.15) | | (0.82) |
| Years in position, associate judges | | -0.629 | | 1.773 |
| | | (-0.16) | | (1.57) |
| Age, chief judge | | 1.543 | | 1.058* |
| | | (0.88) | | (2.41) |
| Mean age, associate judges | | 1.209 | | -2.153* |
| | | (0.37) | | (-2.33) |
| Same-court citations to statutes, previous decade | | 0.348** | | 0.036 |
| | | (3.16) | | (1.36) |
| Court fixed-effects | yes | yes | yes | yes |
| Year fixed-effects | yes | yes | yes | yes |
| Number of observations | 804 | 804 | 804 | 804 |
| $R^2$ | 0.56 | 0.62 | 0.58 | 0.63 |

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

## Table 1, continued: The effects of chief- and associate-judge secure tenure on citations: OLS fixed-effects estimates

| Citations included in dependent variable | (5) All courts, within 20 years of decision | (6) All courts, within 20 years of decision | (7) Same court, all years | (8) Same court, all years | (9) Other courts, all years | (10) Other courts, all years |
|---|---|---|---|---|---|---|
| Secure tenure of chief judge | 40.80* | 32.36+ | 58.66+ | 51.34 | 8.015 | -12.26 |
| | (2.52) | (1.84) | (1.68) | (1.48) | (0.74) | (-0.92) |
| Secure tenure of associate judges | -71.89*** | -67.90*** | -244.5*** | -222.9*** | -85.09*** | -64.15*** |
| | (-5.03) | (-4.53) | (-6.46) | (-6.16) | (-7.59) | (-4.80) |
| Chief judge previous service, years | | 1.198 | | 4.142* | | 0.573 |
| | | (1.10) | | (1.99) | | (0.67) |
| Associate-judge previous service, years | | 6.112* | | 11.16* | | 3.274 |
| | | (2.45) | | (2.21) | | (1.30) |
| Chief judge served on same court | | -36.77* | | -128.5*** | | -82.48*** |
| | | (-2.12) | | (-3.36) | | (-5.32) |
| Years in position, chief judge | | 0.861 | | -1.324 | | 0.990 |
| | | (1.05) | | (-0.79) | | (1.14) |
| Years in position, associate judges | | -1.203 | | -3.800 | | 3.171* |
| | | (-0.83) | | (-1.34) | | (2.35) |
| Age, chief judge | | 0.215 | | 0.912 | | 0.630 |
| | | (0.34) | | (0.68) | | (1.03) |
| Mean age, associate judges | | 0.242 | | 3.868+ | | -2.659* |
| | | (0.20) | | (1.67) | | (-2.34) |
| Same-court citations to statutes, previous decade | | 0.101* | | 0.301*** | | 0.0466 |
| | | (2.55) | | (3.63) | | (1.61) |
| Court fixed-effects | yes | yes | yes | yes | yes | yes |
| Year fixed-effects | yes | yes | yes | yes | yes | yes |
| Observations | 804 | 804 | 804 | 804 | 804 | 804 |
| $R^2$ | 0.46 | 0.50 | 0.52 | 0.59 | 0.57 | 0.62 |

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

## .Table 2: The effects of whole-court judge secure tenure on citations: OLS fixed-effects estimates

| Citations included in dependent variable | (1) All courts, all years | (2) All courts, after 1830 | (3) All courts, within 20 years of decision | (4) Same court, all years | (5) Other courts, all years |
|---|---|---|---|---|---|
| Whole-court secure tenure | -250.1*** | -45.91*** | -55.87*** | -179.8*** | -70.30*** |
| | (-6.92) | (-4.46) | (-4.77) | (-6.70) | (-6.48) |
| Chief judge previous service, years | 4.382+ | 0.0786 | 1.111 | 3.836* | 0.546 |
| | (1.70) | (0.11) | (1.09) | (2.00) | (0.66) |
| Associate-judge previous service, years | 13.54+ | 2.547 | 6.609* | 10.84* | 2.698 |
| | (1.82) | (1.27) | (2.53) | (2.07) | (1.06) |
| Chief judge served on same court | -211.1*** | -59.34*** | -33.37+ | -127.5** | -83.62*** |
| | (-3.99) | (-3.92) | (-1.85) | (-3.25) | (-5.36) |
| Years in position, chief judge | -0.303 | 0.563 | 1.187 | -1.134 | 0.831 |
| | (-0.14) | (0.85) | (1.44) | (-0.70) | (0.99) |
| Years in position, associate judges | 0.559 | 1.926+ | 0.0120 | -2.333 | 2.892* |
| | (0.17) | (1.96) | (0.01) | (-1.00) | (2.39) |
| Age, chief judge | 1.317 | 1.028* | -0.108 | 0.593 | 0.724 |
| | (0.76) | (2.40) | (-0.16) | (0.45) | (1.21) |
| Mean age, associate judges | 0.677 | -2.200** | -0.512 | 3.053 | -2.376* |
| | (0.24) | (-2.64) | (-0.52) | (1.60) | (-2.31) |
| Same-court citations to statutes, previous decade | 0.343** | 0.0348 | 0.103** | 0.301*** | 0.0421 |
| | (3.16) | (1.31) | (2.64) | (3.65) | (1.50) |
| Court fixed-effects | yes | yes | yes | yes | yes |
| Year fixed-effects | yes | yes | yes | yes | yes |
| Number of observations | 804 | 804 | 804 | 804 | 804 |
| $R^2$ | 0.62 | 0.63 | 0.50 | 0.59 | 0.62 |

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