

The Conditioning Role of Judicial Independence in the Exercise of Judicial Review

Abstract: Scholars recognize that the exercise of judicial review depends upon judicial independence: an independent court is more likely to invalidate a statute it opposes. But scholars have lost that the previous statement is a conditional hypothesis, in which judicial independence moderates the relationship between a court's ideological preferences and its decision to strike statutes. By analyzing U.S. Supreme Court decisions, I demonstrate that a conditional relationship better models judicial review than currently used additive relationships. I conclude with an analysis of different measures of judicial independence, finding both Type 2 error and measures that fail to meet theoretical expectations

Scholars of constitutional courts have long linked the concept of judicial independence and the action of judicial review. Based on the assumption that judicial review is a countermajoritarian action, constitutional courts only strike laws when they are independent out of fear of political reprisals from elected officials. Some studies of judicial independence operationalize the concept explicitly as the exercise of judicial review (Rios-Figueroa 2007, Clark 2009). Many more studies, while keeping the notions analytically distinct, model a direct, additive, positive relationship between judicial independence and judicial review (Vanberg 2005, Segal, Westerland, and Lindquist 2011). There is strong empirical evidence for this positive relationship between judicial independence and judicial review, though there are notable null findings within the literature.

Yet a subtle but important caveat about the relationship between judicial independence and judicial review has evaded many empirical studies. Consider the case of *Gonzales v. Carhart* (2007), the U.S. Supreme Court decision that upheld a national ban on partial birth abortions. At the time of the ruling, the Court was considered a highly independent institution; historically popular, it issued a highly publicized decision at a time when the U.S. government was divided between a Republican, pro-life President Bush and a Democratic, pro-choice Congress, both of which were controlled by the other party in recent history. All of these factors buttress the independence of the Court which, according to prior empirical efforts, should encourage the Court to strike down the statute. Yet instead of invalidating the statute, the Court upheld it because it was

controlled by a conservative majority of justices who generally find abortion restrictions permissible.

When viewed from a theoretical perspective, this result is not surprising: the Court acted on its ideological preferences at a time when it was independent. But an empirical strategy that models a direct relationship between judicial independence and judicial review could not account for this case or others like it because the independent Court did not strike down a statute. This is due to the underappreciated notion that judicial independence is a conditional predictor of judicial review that moderates the influence of the preferences of judges, ideological or otherwise, on judicial review. This disconnect between a conditional theory and additive empirical strategy has far-reaching consequences. Modelling judicial independence as a direct predictor can undermine empirical tests of the influence of both ideological preferences and judicial independence in a court's probability of striking down a statute, leading to Type 2 error. This is especially true for courts that often review statutes they are likely to uphold, such as the U.S. Supreme Court.

This paper refines current practices of modelling judicial review by matching the nuanced theories advanced by scholars with equally nuanced empirical models. I begin by examining the current literature on judicial independence, focusing particularly on how distinct mechanisms buttress judicial independence. I next highlight that the common yet underappreciated prediction in these theories that judicial independence does not simply encourage constitutional courts to strike down laws, but rather allows their own ideological predispositions to guide their decisions. I then support my claims by

analyzing U.S. Supreme Court constitutional decisions on important federal statutes from 1949-2011 using a variety of determinants of judicial independence as predictors. The analysis generally reveals that modelling judicial independence as a conditional predictor, rather than an additive one, is a superior modelling strategy and demonstrates that the U.S. Supreme Court is much more sensitive to fluctuations in judicial independence than previously appreciated. On a finer level, the analysis tests the performance of various measures of judicial independence, finding that measures have apparently been subject to Type 2 error while other measures may not adequately reflect the concepts they are supposed to measure.

Judicial Independence and its Role in Judicial Review

Judicial independence, broadly defined, is a political construction that allows judges to make decisions free from outside influence. As a latent concept, scholars have taken a wide variety of approaches to measurement in empirical research. Many operationalizations focus either on expert descriptions of courts (Stephenson 2003, Linzer and Staton 2015) or indicators that would allow judges to make decisions free from influence, such as the real salary of judges or the budgets provided to courts (Hayo and Voight 2007, Ura and Wohlfarth 2010). But others focus on judicial review, arguing that a constitutional court's invalidation of a government action is indication of its independence from the other branches of government (Rios-Figueroa 2007, Clark 2009).

While many find judicial independence normatively appealing, independence is far from guaranteed even within a democracy. The U.S. Congress and president, for example, can influence the U.S. Supreme Court by ignoring or circumventing previous

decisions (Epstein and Knight 1998), starving the Court of resources like support staff and salary increases (Ura and Wohlfarth 2010), limiting the amount of discretion the Court has in determining its docket (Harvey 2013), pursuing constitutional changes that would damage it, or even impeach the justices (Whittington 2009). Given the many threats to judicial independence, scholars have identified a number of its determinants.

Perhaps the most important driver of judicial independence is the popularity of a court. Courts in modern democracies often have broad support among their publics (Gibson, Caldeira, and Baird 1998, Gibson, Caldiera, and Spence 2003). This support may cause voters to abandon officials that engage in court-curbing, which in turn insulates courts from elected officials who are fearful of losing their jobs. In turn, constitutional courts are free to strike down laws (Carruba 2009). Comparative evidence demonstrates that constitutional courts are vulnerable to court-curbing when support is low (Helmke 2010). In the U.S. context, supporters of the attitudinal model of judicial decision-making argue the Supreme Court's enduring popularity allows it to ignore strategic considerations and make decisions based on its own preferences (Segal and Spaeth 2002). More recent research shows, however, that variations in legitimacy weigh heavily on even the U.S. Supreme Court, influencing its decision to strike down statutes (Clark 2009, Merrill, Conway, and Ura 2017).

Vanberg (2005) argues that transparency in the political environment moderates the relationship between popularity and judicial review. Public attention to a particular case makes it more difficult for elected officials to circumvent rulings. Similarly, a public that does not observe court-curbing efforts cannot punish them. Vanberg shows that the

German constitutional court is more likely to strike laws in salient cases, a finding supported by additional analysis in Mexico (Staton 2010). Cross-national analysis also finds that countries with higher degrees of press freedom also have more independent judiciaries (Hayo and Voight 2007, Melton and Ginsburg 2014).

Formal protections for constitutional courts, such as salary protections or life terms, also help secure judicial independence by removing tools that can be used to punish a court for unpopular decisions. This in turn insulates a court from political pressure and secures judicial independence. Comparative evidence supports that at least some formal protections for judges do bolster judicial independence (Hayo and Voight 2007, Melton and Ginsburg 2014). Supporters of the attitudinal model also argue that these formal protections enable the U.S. Supreme Court to ignore strategic considerations (Segal 1997). But analysis over a long time horizon indicate that the Court's institutional support was not always so high and, as a result, it invalidated fewer laws (McGuire 2004).

Political fragmentation can also provide the independence courts need to exercise judicial review (Rios-Figueroa 2007). Many modern democracies have a separation-of-powers system in which the ability to govern is divided between multiple political entities. When these entities are not controlled by the same governing coalition, they may disagree on the desirability of court rulings and be unable to retaliate against a court that invalidates its policies. This, in turn, empowers courts to strike laws as they see fit. Similarly, insurance theory argues that judicial independence will be greater when political competition is high (Stephenson 2003). Competition creates uncertainty for

government officials evaluating whether they will be able to keep power. Fearing extreme policies by the opposition, governments support judicial review of its own actions as an insurance mechanism were they to lose power, which subsequently empowers courts to strike laws. While insurance theory has considerable empirical support in a variety of political contexts, evidence supporting the political fragmentation hypothesis is mixed (Rios-Figueroa 2007, Helmke 2010, Vanberg 2015).

In some ways, political fragmentation can be seen as a more basic formulation of Marks' separation of powers model (2015). Marks explained why Congress would tolerate a statutory Supreme Court decision inconsistent with its preferences; this logic was later extended to constitutional decisions (Bergara, Richman, and Spiller 2003; Spiller and Gely 1992).¹ If the pivotal actors in the policy-making process, such as the median member of the House, the median member of the Senate, and the president, all support a law under review, the Court will not try to invalidate it for fear of backlash. But if a single pivotal member opposes the law, the court is free to strike it down so long as doing so would not result in a policy environment more extreme than the ideal policy of the dissenting pivotal member(s). There is considerable debate as to whether it has empirical support; proponents of the attitudinal model in particular argue for a negligible relationship (Segal 1997, Segal and Spaeth 2002, Segal, Westerland, and Lindquist 2011, Spiller and Gely 1992, Bergara, Richman, and Spiller 2003, Hall and Ura 2015).

Independence as a Moderator of Preferences

As mentioned earlier, judicial review has been used as an indicator of judicial independence. To be sure, a truly independent court must not be afraid to strike down the

decisions of other actors. But despite its correlation with independence, it is by no means a valid indicator of the concept. Scholars have long noted that governments may desire for courts to strike down laws under certain conditions (Rogers 2001, Whittington 2009). Striking down a law, therefore, is not a perfect indicator of an independent court, as it could just as easily be a court bowing to the pressure of another branch of government (Carrubba, Gabel, and Hankla 2008). In a similar vein, failing to strike a law is not necessarily an indication of a weak judiciary; sometimes, judges uphold laws because they are consistent with their preferences. Indeed, these competing pressures obfuscate any potential for a direct relationship between judicial independence and judicial review and might actually lead to scholars observing a null or even negative relationship between the two at rates greater than one would expect by chance.

That judicial independence does not have an additive relationship with judicial review is an underappreciated prediction made by a number of formal models of judicial review. Consider Mark's separation of powers model (2015). Many scholars interpret this model to mean that when the elected branches are supportive of a particular law, the Court should be less likely to strike it down (Segal, Westerland, and Lindquist 2011). Conversely, the Court should be more likely to strike down a law when at least one pivotal elected official opposes the law. This characterization of the model omits an important relationship. When a pivotal actor opposes a law under review, the court is free to strike down the law as unconstitutional and move the status quo towards its ideological preferences. But if the law reflects the preferences of the median member of the Court, then the Court will simply uphold the law to avoid costly decisionmaking. The Court is

not forced to strike laws, but rather can “vote its own preferences” (Bergara, Richman, and Spiller 2003).

A similar account can be given for Vanberg’s model of political transparency (2005). A typical description of the model’s equilibria states that when a court is sufficiently popular and operates in an environment of political transparency, it is more likely to invalidate legislation (Staton 2010). But this characterization misses an important part of the equilibria. Assuming that the government supports a statute under review, a court with divergent preferences to the legislature will strike down a statute when both popularity and transparency are high; otherwise, that court will uphold a statute out of fear of court-curbing. A court with convergent preferences to legislature, however, will always uphold a statute under review regardless of its popularity and transparency. Phrased differently, a court’s preferences are only relevant to its decision-making when the court is sufficiently popular and transparent.

I argue that none of the mechanisms of judicial independence discussed above imply simple direct effects on a constitutional court’s decision to strike down a statute, as previous studies have assumed in their empirical models. Rather, these mechanisms condition the effect that a court’s preferences have on striking a statute: the effect of preferences should be strongest when a court is protected from court curbing and that effect should decline as the court becomes more vulnerable. This leads to a general hypothesis:

Conditional Preference Hypothesis: The positive relationship between the ideological preferences of a constitutional court towards striking a statute and the

probability that the court will strike down a statute is conditioned by the degree of independence that court has from other branches of government, with greater levels of independence leading to a more positive relationship.

This account is not to suggest that all empirical studies of judicial independence and judicial review miss this distinction, of course. Rios-Figueroa (2007) carefully details that over the timeframe of his analysis that the judges of the Mexican Supreme Court would likely oppose all statutes being passed by the Institutional Revolutionary Party, thus obviating the need to model a conditional relationship. Yet such careful justification of research design is rare; most scholars model an additive relationship between the two variables with little justification for doing so (Vanberg 2005, Clark 2009, Segal, Westerland, and Lindquist 2011). The implied theoretical approach of these research designs can be summarized as:

Additive Independence Hypothesis: There is a positive relationship between the degree of independence of a constitutional court and the probability that the court will strike down a statute.

Additionally, the attitudinal model of judicial decision-making, as applied to judicial review, makes a distinct additive prediction. Proponents of the attitudinal model argue that the positive relationship between the Court's ideological predispositions and the ultimate decision in a case are constant. Because the Court is sufficiently protected, variance in the level of independence is inconsequential to judicial decision-making.²

This leads to an additional hypothesis:

Additive Preference Hypothesis: There is a positive relationship between the preferences of a constitutional court towards striking a statute and the probability that the court will strike down a statute.

Research Design

In order to test the above hypotheses, I need to analyze a set of constitutional court decisions with measures of both the court's preferences towards invalidating a particular statute and different mechanisms that secure judicial independence. The U.S. Supreme Court provides an excellent test case for two reasons. First, the U.S. Supreme Court provides a difficult test case for my theory. Considered one of the most independent courts in the world, there is a strong expectation for finding evidence to support the attitudinal model of judicial review and a failure to find evidence for the influence of judicial independence (Segal and Spaeth 2002, Gibson, Caldeira and Spence 2003). Indeed, Harvey (2013) argues that the strong evidence supporting the attitudinal model in the U.S. Supreme Court can be explained, at least in part, by confirmation bias in the coding of certain variables from the Supreme Court Database that are featured prominently in this analysis (Spaeth et al 2016).² Finding evidence that supports the Conditional Preference Hypothesis in the face of such a conservative test, then, would provide compelling support for my theory. Second, the Court is the subject of a wealth of research on the ideological preferences of its members over a long period of time, with a particular emphasis on the measures of justice ideal points (Segal and Cover 1989, Martin and Quinn 2002, Bailey 2013). While comparative research efforts are growing in their ability to collect case data across countries, comparable ideal point estimates for courts or

their judges are still unavailable. For these reason, I analyze a subset of U.S. Supreme Court decisions.

Rather than solely focusing on U.S. Supreme Court decisions, however, this analysis draws on a statute-centered approach of previous studies (Hall and Ura 2015, Harvey and Friedman 2009, 2006). The study of judicial review inevitably leads to studying court decisions. Solely studying them in presence of a discretionary docket, however, can lead to a selection bias as strategic interactions may happen at the certiorari stage. This can negatively impact our ability to make inferences, meaning we must go beyond simply looking at decisions and look at the statutes which the decisions are about. Thus, the unit of observation in this analysis are federal statutes. Of course, there are difficulties with looking at all federal statutes. Collection of the data would be a monumental task and would thus limit analysis to a small time period. As a middle ground, I analyze a subset of statutes enacted between 1949 and 2011. The subset is whether a law is landmark legislation, as defined by Mayhew (2005), for a total of 368 statutes over the time period.

To identify whether the Court decided on the constitutionality of a statute in this dataset, I used the Supreme Court Database (Spaeth et al 2016), the Judicial Review of Congress Database (Whittington 2019), as well as a dataset created by previous scholars (Hall and Ura 2015). I searched each database for cases where the Court explicitly decided on the constitutionality of one of the important statutes. Some of these cases were exclusively about the constitutionality of a statute. Others included smaller, less consequential constitutional decisions about these statutes en-route to decisions on other

topics, like the proper statutory construction of a federal statute or the constitutionality of a state law. All cases were hand-coded as to which statute a case focused on, even those cases that had specific references to statutes in their data.³ This search resulted in 95 statutes being challenged in 213 cases. Parts of 43 statutes were invalidated in 65 of those cases.

In order to account for potential selection effects in the merits stage, I employ a Heckman probit model. The first stage is a model of the Court's decision to hear a challenge of an important statute in a given year, an unbalanced time-series cross-sectional dataset.⁴ The second stage is a model of the Court's decision to invalidate, in part or in whole, the statute on constitutional grounds. Once a statute enters the dataset, it remains in there permanently because the Court did not invalidate any of these statutes in its entirety. This model allows us to control for potential sample selection bias at the merits stage, though it does not allow for us to entangle what social processes are governing whether a statute is granted a constitutional challenge.⁵ All variables to be described in this analysis are included in both the first and second stage, except those variables that are specific to Court decisions rather than statutes or the extant political environment. In the first stage, I also include cubic polynomials to control for duration dependence (Carter and Signorino 2010) and to provide the necessary instrument to estimate the Heckman model. In addition, standard errors are clustered by statute for multiple reasons. Because the Court often decides multiple cases on a given statute, observations are necessarily correlated and thus clustering is appropriate (Box-

Steffensmeier and Zorn 2002). Such a modelling strategy is also consistent with prior approaches (Hall and Ura 2015).

In 17 instances, there were two cases decided on a single statute in a single year. Given this unusual data structure for a duration model, I devise a method for including all observations that purposefully breaks the statute-year observation set-up in the data. For every statute-year that saw the Court decide on the constitutionality of a statute, a duplicate observation was created that was coded as not being subject to a challenge in which the years since the statute was enacted or previously decided upon was set to zero. For a statute-year with multiple cases, only one duplicated observation was created. This results in a statute having $n+1$ observations in a given year, where n is the number of cases about the constitutionality of that statute decided in a given year. These additional observations represent the time after the Court has decided on a case but before the end of the year.⁶ Because they represent a subset of time, they can accommodate more than one case being decided in a given year by simply inserting a later case in between earlier case and this duplicate observation.

The crucial independent variable in this analysis is the court's preferences towards a law. To measure the court's ideological predisposition to striking a statute, I use a combination of Bailey's (2013) ideal point estimates of justices' ideology, the direction of the decision classification from the Supreme Court Database.⁷ First, I identify the Court's median in a given natural court and year. Next, I determine whether striking a statute is consistent with the Court's ideological tilt. If striking a statute is consistent with the median member of the court's ideological predisposition, then the observation is

assigned the absolute value of the median member's ideal point. If not, then the observation is assigned the negative of the absolute value of the median member's ideal point. All cases where the ideological implications of a decision were unclear were coded as zero. This results in a measure of the court's attitudes towards the case where positive values indicate the court is ideologically inclined to striking and negative values indicate the court is ideologically opposed to striking.

Aside from the court's ideological predispositions, I also need measures of the various mechanisms that secure judicial independence.⁸ I include two measures of the Court's popularity in the analysis. First, I include a measure of the Court's popularity relative to Congress (Ura and Wohlfarth 2010, Merrill, Conway, and Ura 2017). The General Social Survey asks respondents to rate the people running different government institutions on a three-point scale, first measured in 1973.⁹ My measure is the average approval for the Court minus the average support for Congress in the previous year. Second, I adopt Clark's (2009) measure of the number of court-curbing bills presented in Congress, which he argues is a signal of the Court's popularity; the data begins in 1973. I both take a lag of this variable and also subject it to a linear transformation by subtracting the number of court-curbing bills from the maximum number of court-curbing bills of any year in the dataset, so that higher numbers indicate fewer bills and reflect greater public support.

Vanberg (2005) has a number of measures of political transparency that can be adapted to a study of the U.S. Supreme Court. Vanberg argues that easily understood policy areas should be more transparent than more complex ones. To measure the

complexity of a statute, I use a count of the number of pages in a statute as paginated by Lexis Nexis (Maltzman et al 2014). I take a linear transformation of this count by subtracting it from the maximum number of pages of any statute in the dataset, so that higher numbers reflect shorter statutes that are easier to understand and thus bolster transparency of the political environment. Additionally, Vanberg argues that whether a case has oral arguments is a good indicator of transparency. While this measure works well in the German context, it is less helpful for the U.S. Supreme Court where most cases have oral arguments. Instead, I adopt Clark, Lax, and Rice's (2015) measure of the latent salience of a Supreme Court case prior to a decision being made. A continuous measure in which higher numbers reflect greater salience, it ranges from roughly -1 to 2 in the sample. The measure is only available from 1953-2008.

To test Marks' separation of powers model, I adopt a measure in the literature that estimates whether the current pivotal policymakers support or oppose a given statute under review (Segal, Westerland, and Lindquist 2011, Hall and Ura 2015). I collect the original roll call votes for each public law from VoteView (Lewis et al 2017). Using logit, I then regress these roll call votes on the Common Space Score of Members of Congress and the president (Poole 1998).¹⁰ Using the resulting model coefficients, I can then predict the probability that a future elected official supports a law using their Common Space Score. Note that for those laws passed unanimously or via voice votes in both chambers, there is no variation to run regression models. In these instances, the predicted support for all future officials is 1.

I then identify pivotal actors in the policymaking process, relying on the insights of Krehbiel (1998), and record the minimum level of predicted support to a statute from any of the pivotal actors.¹¹ I test three different pivot models: the floor median model, the Senate filibuster model, and the party gatekeeping model. Each of these models are outlined in more detail in Hall and Ura (2015). The resulting measure gives the probability that the most hostile pivotal actor supports the law based on their ideology, as measured by Common Space scores. While the paper's hypotheses only makes predictions for the decision stage, Hall and Ura (2015) have shown how this variable also effects the certiorari stage.

Operationalizing political fragmentation is relatively easy in the U.S., a system marked by separation of powers. I create a simple dummy measure in which a 1 indicates divided government and a 0 indicates unified government. In contrast, insurance theory is difficult to operationalize when only examining a single political system like the U.S. as there is little variation in the competitiveness of elections over time. An indirect test of this theory, however, can be derived. Insurance theory assumes that the political parties disagree on policy and, due to competitive elections, respect judicial review. But when the government party and opposition party agree on policy, however, there is no reason to respect judicial review because a change in government will not lead to a change in policy preferences. Thus, governments should be constrained to respect judicial review in partisan matters but not bipartisan ones. I measure the partisanship of a statute as the absolute value of the proportion of House Republicans that voted for a statute minus the proportion of House Democrats that voted for the statute. This results in a continuous

measure that assigns a 0 when both parties equally support a statute and a 1 when a statute passes on a strict party-line vote.

In addition to these variables of interest, I include a number of controls. A growing body of literature shows that both the ideology of justices and the outcomes of particular cases are influenced by ideological tilt of public opinion (McGuire and Stimson 2004, Casillas, Enns, and Wohlfarth 2011). Individual perceptions of the legitimacy of the Court are also influenced by their approval of particular decisions (Gibson, Caldeira, and Spence 2003). To control for these relationships in my analysis, I use a combination of Stimson's (1999) public mood and the direction of the decision from the Supreme Court Database.¹² First, I mean-center a year lag of public mood for the time period of my analysis so that positive values indicate a liberal public in that time-period and negative values indicate a conservative public. Then, as with the measure of the court's attitude, I assign an observation the absolute value of the transformed public mood if striking is aligned with the public's ideological predisposition. I assign the negative of the absolute value of the transformed public mood if striking is against the public's ideological interests. Positive values mean the public wants a strike and negative values mean the public does not.

In addition, the sample includes cases in which the constitutionality of a federal statute is the main controversy of a case and cases in which it is a relatively minor, ancillary question. One would expect that cases that feature controversies over the constitutionality of a statute are more likely to result in the invalidation of that statute. Prior research also shows that cases which feature controversies over the constitutionality

of a statute are more salient (Clark, Lax, and Rice 2015). To account for whether a case focused on the constitutionality of a federal statute, I create a binary measure from the Supreme Court Database on what basis of authority the Court made a decision using `authoritydecision1` and `authoritydecision2`. If a case focused on the constitutionality of a federal statute according to these variables, it was coded as a one. If not, it was coded as a zero. Of the 213 cases in the sample, 148 focus on constitutionality of a federal statute. These variables represent an informed scholars' view of what issues a Supreme Court case focused on, rather than a definitive judgement about whether a case involved judicial review. That variation exists in this measure proves this interpretation.

I also control model unit effects and the temporal structure of the data. I include two-way fixed effects in both stages of the analysis. The first set controls for the Chief Justice of the Supreme Court who 1) managed the Court for the majority of a given year in the first stage of the analysis, and 2) managed the Court when it decided a particular case the second stage. The second set controls of the policy area of a given statute; besides a miscellaneous category, these policy areas are agriculture, the budget, civil rights, energy and the environment, good governance, immigration, international affairs, law and crime, minimum wage, regulation, Social Security, social welfare, taxes, technology, and transportation.

I include a number of additional technical considerations in the appendix. Descriptive statistics for all variables are located there, broken up by whether the variable varies based on year, statute, year and statute, or case. More technical formulae of some of these variables are also given. In addition, there is concern that ideal-point estimates of

judicial ideology are inappropriate predictors of Court decisions due to the “votes predicting votes” criticism (Martin and Quinn 2005). As a robustness check, I substitute Segal-Cover scores instead of Bailey’s estimates to create a new measure of the Court’s ideological predisposition to strike a statute (Segal and Cover 1989). I use this new measure to replicate the analysis.

Analysis

The analysis proceeds in two parts. The analysis is first conducted with a simple additive model in which mechanisms that secure judicial independence are included but not interacted with court ideology. The results of this analysis are contained in Tables 1 and 2.¹³ For each model, a positive, statistically significant coefficient for the court ideology variable indicates support for the Additive Preference Hypothesis. A positive, statistically significant coefficient for the independence variables indicates support for the Additive Independence Hypothesis.

Court ideology is a robust and relatively stable predictor of whether the court will strike down a statute in the models. In most models, the average marginal effect for court ideology in the sample is roughly 25%, meaning a one-unit increase in the court’s ideological predisposition to striking down a statute results in a 25% increase in the probability of striking (Hanmer and Kalkan 2013). To help illustrate, Justice O’Connor retired in 2005 as the median justice on the Court. Her replacement, Justice Alito, was decidedly more conservative: on the Bailey ideal point scale, his first ideal point measure in 2006 was roughly one unit larger than O’Connor’s in 2005. Thus if Alito had become the median justice after replacing O’Connor, the Court would be 25% more likely to

strike down liberal statutes and uphold conservative statutes.¹⁴ This result supports the Additive Preference Hypothesis.

<Table 1 and 2 About Here>

In contrast with court ideology, however, the various mechanisms of judicial independence are largely not statistically significant even though most of them are signed in the correct direction. There are a couple of exceptions to these findings, however: the models of case salience and political fragmentation have positive, statistically significant coefficient in line with theoretical expectations. In the case salience model, the average marginal effect in the sample is roughly 15% increase in the probability of striking a statute. In the political fragmentation model, the average discrete effect in the sample of moving from a unified government to a divided government is also roughly 15%.

The lack of statistical significance for the other seven mechanisms of judicial independence tested, however, casts doubt on the Additive Independence Hypothesis. While a lack of statistical significance does not necessarily indicate a negligible effect, at minimum it does indicate that the data does not support the Additive Independence Hypothesis (Rainey 2014). The results for court popularity and transparency, in particular, are either partial or total failures to replicate some of the findings in previous research. In addition, the analysis would also fail to find evidence for an implication of insurance theory. Were the analysis to end here, one would question whether the mechanisms of popularity and transparency, for whatever reason, do not hold up as well in American context or if there is a problem with previous or current analysis.

In addition to the analysis relevant to the articulated hypotheses, there a number of other relevant pieces of information to be gleaned from the tables. The public's ideology has a relatively stable, modest, and positive coefficient in the models. The p-value for this coefficient, while not significant at the 0.05 level in most models as shown in the tables, is significant at the 0.10 level in most models. In contrast, the variable measuring the focus of the case before the Court is always positive and statistically significant. The average discrete effect in the sample of moving from a case focused on other issues to a case focused on the constitutionality of the federal statute under review is a roughly 20% increase in the probability of striking. Additionally, some measures of independence are statistically significant in the first stage of the equation, indicating that they have a net-effect on the certiorari process. Specifically, there is a robust replication that the level of opposition to a statute among pivotal political actors is positively related to probability that the Court will grant certiorari to a constitutional challenge to that statute (Hall and Ura 2015).

While individual coefficients of the two-way fixed effects and the cubic polynomials are not reported, Wald tests of their joint significance by group are reported. Both sets of fixed effects are statistically significant in the first stage, but not the second. The cubic polynomials are also statistically significant. This is likely driven by the discrepancy in degrees of freedom in the first and second stages. Even so, the p-values for the policy area fixed effects and the cubic polynomials are statistically significant at the 0.001 level, indicating there is more than just sample size considerations driving these results. There are a few substantive observations we can glean from these control

variables. First, the Court is much more likely to grant certiorari in some policy areas than others: the Court is more likely to hear cases on fiscal issues, crime and civil rights, good governance, and Social Security relative to the miscellaneous category. Second, the Court is much more likely to hear a challenge to a law right after it passes rather than waiting long periods of time, indicating that statutes with dubious constitutionality are considered by the Court swiftly.

Wald tests of selection effects are also reported in the analysis. There is no evidence of selection effects in any of these models. None of them were statistically significant at the 0.05 level, with the smallest p-value in any model being 0.57. While null results do not disprove the existence of selection effects in U.S. Supreme Court decision-making, it does cast some doubt on how they might affect inferences. Unfortunately, it is difficult to compare this result to the rest of the literature as prior studies employing similar strategies do not report results concerning selection effects (Hall and Ura 2015, Harvey and Friedman 2009). In future research, scholars should be more explicit about their findings.

The results in Tables 3 and 4 are similar to the models as the Tables 1 and 2, but this time including an interaction term between court ideology and the independence variables. There is little change in the values of the control variables, including fixed effects and cubic polynomials. There is also no evidence of selection effects, with similarly large p-values for the Wald test on all models. A positive interaction term indicates support for the Conditional Preference Hypothesis, while a negligible

interaction term, in addition with positive, statistically significant coefficients on the constitutive terms, indicate support for the other hypotheses.

Like the previous models, the coefficient for the various mechanisms of judicial independence are rarely statistically significant. Unlike in the previous models, the coefficient for court ideology has wide variation: the coefficient almost doubles in the statute length model, is statistically insignificant and centered on zero in the pivotal support models, and large and negative in the Court popularity model. These results are inconsistent with the Additive Preference and Additive Independence Hypotheses, which would predict positive, statistically significant coefficients for each set of constitutive variables and negligible interaction term. In contrast, six of the nine interaction terms are positive and four of those are statistically significant. This is again consistent with the Conditional Preference Hypothesis: as the level of independence increases, the Court's ideological preferences play a stronger role in their decisions.

<Table 3 and 4 About Here>

Yet scholars are not directly concerned with the coefficients of interaction terms, especially not those in probit models. Rather, scholars are concerned with marginal effect estimates at substantively interesting levels of conditioning variables (Brambor, Clark, and Golder 2006). Figures 1 and 2 present the average marginal effect of court ideology at the empirical minimum and maximum levels of independence in the data.¹³ The results in Figure 1 provide some support the Conditional Preference Hypothesis. When the Court is popular relative to Congress, a one-unit increase in the court's ideological predisposition to strike results in a 55% increase in the probability of striking in the

sample, a relationship which flips in direction when the Court and Congress have similar levels of popularity. Similarly, when the Court considers a case that is salient to the public, a one-unit increase in court's ideological predisposition to strike translates into roughly a 40% increase in the probability of striking; this effect become statistically indistinguishable from zero when a case is not salient.

The results in Figure 2 more strongly support the Conditional Preference Hypothesis. When all pivotal policymaking members support a statute, for any of the pivotal policymaker models, court ideology does not have a statistically significant influence on the court's decision to strike a statute. But when a single pivotal member is opposed to a statute, a one-unit increase in the court's ideological predisposition to strike translates into roughly a 60% increase in the probability of striking in the sample. The partisan vote model results are even more extreme. For a bipartisan or nonpartisan statute, court ideology is statistically insignificant. For a strictly partisan statute, however, a one-unit increase in court ideology results in a 70% increase in the Court's probability to strike. These results indicate that even small changes in the ideological disposition of the Court can have huge consequences on the Court's propensity to strike a law when the Court is independent. These results disappear, however, when the Court is not independent.

<Figures 1 and 2 About Here>

The court-curbing model, statute length, and political fragmentation models do not support the Conditional Preference Hypothesis: the models' relevant coefficients are not statistically significant, and the direction of the relationship (as well as the interaction

term) is in the opposite direction as would be expected by theory. While the results do not necessarily show that court-curbing or statutory complexity do not play roles in the Court's decision to strike a statute, it does cast some doubt on those findings especially given the null results in the additive models. The political fragmentation model just barely misses out on statistical significance for its constitutive term and is near identical to its value in the additive model, which was statistically significant. This provides support to the Additive Independence Hypothesis for this model. But caution should be taken with this finding for a number of reasons. The Conditional Independence Hypothesis has more support overall. Political fragmentation also only matters if political parties have different policy preferences, a phenomenon which is better measured by other models in Tables 2 and 4.

Finally, a word about robustness checks. The research design discusses a robustness check that measures the ideological tilt of the Court in a case using Segal-Cover scores rather than Bailey ideal point estimates. The results are presented in the appendix. But to summarize, the substantive conclusions of the paper remain unchanged. These results demonstrate that my theoretical argument is robust to different estimation strategies.

Discussion

This paper provides strong evidence that judicial independence is a conditional predictor of judicial review, rather than additive predictors as tested in previous empirical models. Rather than encouraging the Court to strike down a statute or other government policy, as implied by previous empirical tests of theory, higher degrees of independence

for the Court enables it to make decisions based on its own ideological predispositions, whether those predispositions support striking a policy or upholding it. As shown in the tests, the Court is remarkably sensitive to shifts in its independence; the finding is notable given the evidence showing that the Court enjoys high levels of legitimacy. This evidence is consistent with prior informal discussion of these theories but represents an improvement in both the clarity of the presentation and attempts in empirically modelling them. Indeed, many scholars previous work would either partially or fully fail to replicate absent this advance in empirical modelling.

In the future, scholars should either model judicial independence as a conditional predictor of judicial review or carefully craft a research design that obviates the need to do so. The incorporation of the preferences of judges is easy in almost all areas of American politics. It is notably more difficult for comparative studies, where relatively fewer attempts have been made to measure the preferences of judges. Yet there is a clear need for them and an existing measurement strategy for doing so (Segal and Cover 1989, Martin and Quinn 2002, Bailey 2013).

Independent of the general, methodological contribution of the paper, the substantive results from individual models also make a couple of contributions. The separation of powers models reveal a potential instance of Type 2 error in the literature. Segal and his coauthors have consistently found that the separation of powers model has no explanatory power on Supreme Court constitutional decisions (Segal and Spaeth 2002, Segal, Westerland, and Lindquist 2011, see also Hall and Ura 2015). But this seems to be due to empirical tests that restrict the government's preferences over statutes under

review to having a direct effect on the decision to strike a law. When the government's preferences are allowed to condition the effects of the Court's ideological preferences, as the theory implies, then there are strong results consistent with both the theory and other research (Bergara, Richman, and Spiller 2003).

The null results presented in the paper are also interesting in that they do not comport with theoretical expectations. The court-curbing, statute complexity, and political fragmentation models all had incorrectly signed interaction terms. I had no prior expectation for the failure of these particular measures. In order to avoid post-hoc rationalization, I conclude that such results are simply due to random chance.

But I will also briefly suggest that future research into the matter might be usefully guided by the possibility of measurement error in each of these three variables. Court-curbing is supposed to capture the popularity of the Court, but also captures strategic dynamics in Congress. While such dynamics have been previously modelled (Clark 2009), it is entirely possible that there are additional dynamics that make the measure too noisy to be useful. Statutory complexity may not measure salience at all. While some easy statutory issues, like abortion and civil rights, are highly salient, complex issues like economics also tend to be salient. The controversy surrounding *National Federation of Independent Businesses v. Sebelius* (2012), which focused on the 906 page long Affordable Care Act, seems provides a prominent example. And political fragmentation assumes that opposing political parties cannot agree to curb courts, even though bipartisan and nonpartisan issues exist. The support for the separation of powers models and the insurance theory model prove a compelling contrast. Scholars should

carefully consider what concepts these measures truly capture in relation to judicial independence, in particular when the use of such measures result in findings inconsistent with theory.

This study is not without limitations. As mentioned in a footnote, not all of the mechanisms for judicial independence are tested. The *de jure* protection afforded to members of the Court has remained relatively stable over the Post-War era. This lack of variation prevents analysis of these theories in light of the arguments in this paper. Comparative analysis must be conducted in order to fully evaluate these theories.

There is also a concern about the generalizability of the findings. The research design focuses on statutes that are regarded as important at the time of passage. For the most part, many are also considered landmark statutes in retrospective review. But the focus on important statutes excludes statutes with moderate to minor importance. In these cases, it's entirely possible that the Additive Preference Hypothesis would hold because a government simply would not care about whether a minor statute was struck down. This would be consistent with some models of judicial independence, in which the cost of retaliating against a constitutional court is greater than the benefit received from reenacting a statute (Vanberg 2005). Still, the comparison of statutes with varying degrees of importance would be an interesting avenue for future research.

Additional research should also be conducted on political competition and judicial review. This paper suggests a more nuanced understanding of insurance theory: political competition empowers judicial review of partisan statutes but not bipartisan ones. A crucial assumption made in insurance theory is that political parties have opposing policy

desires. While true in many policy areas, it is not difficult to imagine values opposing political parties in democracies might share: democracy, capitalism, a strong national defense, etc. In these areas, governments may be less inclined to tolerate judicial review of its actions. Analysis of the American context supports this claim. However, additional comparative analysis over a wider range of political contexts would provide more robust support for this argument.

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Footnotes

1. This paper went unpublished for many years, leading to an inconsistent timing of publications.
2. In particular, Harvey argues that confirmation bias in the direction of the decision variable bias findings in favor of the attitudinal model and away from strategic models of judicial decisionmaking. This variable is used in the construction of my measure of judicial preferences for invalidating statutes, detailed later.
3. In both databases, there were errors when associating cases with statutes that were revealed during initial data examination that prompted the hand-coding.
4. A statute enters the dataset the year it passes.
5. The certiorari process is influenced by a number of actors, including litigants, lower court judges, political elites, and the justices themselves.
5. Such an approach is not without precedent. For a given statute, the first year it enters the data is the year it was passed and signed into law. But statutes are not passed only at the beginning of the year; rather, they are created at many different points of time. These initial observations, then, do not represent a full year but randomly represent shorter periods of time, much like these duplicate observations represent shorter periods of time.
6. A few cases in the dataset were summary judgements and, therefore, are not present in the Supreme Court Database. This results in missing data for variables involving the Database, which I then hand code using the documentation provided with the Database. This process was followed for all missing data that resulted from summary judgments not being in the Supreme Court Database.

7. One mechanism of judicial independence could not be operationalized in this dataset. The de jure protections afforded to members of the Court has remained stable over the Post-War era and is thus not included in this analysis.
8. The measure has not been asked annually. In years after 1973 where the data is missing, it is imputed using the average of the two most proximate years.
9. One statute, public law number 107-40, was predicted perfectly. Coefficients could not be generated and it was subsequently excluded from analysis using these variables.
10. Because Common Space scores are not available for the president prior to the 83rd Congress, data are missing for these variables prior to 1953.
11. Because public mood is only available starting in 1952 and, therefore, a lag of mood could only be created starting in 1953, this creates problems for a single case decided in 1952. To avoid dropping an observation, I assigned public mood at 1951 to be equal to mood at 1952.
12. Due to missing data for some of the measures of judicial independence, the N for these models vary. Because all instances of missing data are due to limited observation windows in time series data, I assume such data is missing at random. There does not seem to be any indication that the observation windows are correlated with any of the other variables in the model. I refer the reader back to the research design section and its footnotes to indicate limitations for particular time series.
13. While Alito did not become the new Court median when O'Connor retired, the median of the Court has been known to drastically shift with a single retirement. The retirement of Justice Warren and his replacement with Justice Burger created a large

shift; the retirement of Justice Kennedy and his replacement with Justice Kavanaugh will likely see a large shift.

14. Because the hypotheses in this paper are directional, 90% confidence intervals are reported to comport with the one-tailed tests used for p-value calculations.

Tables

Table 1: Heckman Models of Decision to Invalidate an Important Federal Statute Passed Between 1949-2011 that Test Additive Relationships

Stage 2: Invalidations of important federal statutes that are challenged	Relative Court Popularity	Court-Curbing Bills ⁺	Statute Length ⁺	Case Salience
Court Ideology	0.10 (0.46)	0.13 (0.47)	0.86** (0.30)	0.98** (0.29)
Independence	2.46 (1.85)	0.01 (0.02)	-0.00 (0.00)	0.53** (0.18)
Public Ideology	0.02 (0.02)	0.02 (0.02)	0.04* (0.02)	0.03 (0.02)
Focus on Constitutionality of Federal Statute	0.78** (0.30)	0.76** (0.30)	0.91** (0.24)	0.75** (0.28)
Policy Area Fixed Effects	15.25	14.64	12.36	15.23
Chief Justice Fixed Effects	0.39	1.51	1.16	1.00
Stage 1: Challenges to important federal statutes				
Independence	0.33 (0.45)	0.00 (0.09)	-0.00 (0.00)	-
Policy Area Fixed Effects	81.12**	81.73**	106.29**	101.27**
Chief Justice Fixed Effects	8.67*	9.41**	12.65**	8.10*
Cubic Polynomials of Duration Dependence	153.58**	154.19**	120.48**	118.64**
Wald Test of Independent Equations	0.32	0.20	0.07	0.21
N Stage 1	10207	10207	11897	10709
N Stage 2	169	169	213	199

*p<0.05, ** p<0.01, one-tailed tests used for individual coefficients

Grouped Coefficients report Wald Test of Joint Significance

⁺Independence Variable Subject to a Linear Transformation

Standard Errors Clustered on Statute are in Parentheses

Table 2: Heckman Models of Decision to Invalidate an Important Federal Statute Passed Between 1949-2011 that Test Additive Relationships, Continued

Stage 2: Invalidations of important federal statutes that are challenged	Floor Median Model	Senate Filibuster Model	Party Gatekeeping Model	Partisan Vote	Political Fragmentation
Court Ideology	0.89** (0.30)	0.89** (0.30)	0.86** (0.30)	0.91** (0.30)	1.00** (0.32)
Independence	0.13 (0.47)	0.11 (0.47)	0.28 (0.45)	0.30 (0.49)	0.52* (0.31)
Public Ideology	0.03 (0.02)	0.03 (0.02)	0.03 (0.02)	0.04* (0.02)	0.03 (0.02)
Focus on Constitutionality of Federal Statute	0.88** (0.26)	0.87** (0.26)	0.87** (0.26)	0.89** (0.26)	0.93** (0.26)
Policy Area Fixed Effects	12.32	12.31	11.96	12.92	15.27
Chief Justice Fixed Effects	1.45	1.44	1.21	1.58	2.12
Stage 1: Challenges to important federal statutes					
Independence	0.35** (0.13)	0.31** (0.13)	0.34** (0.13)	0.23 (0.15)	0.04 (0.08)
Policy Area Fixed Effects	101.10**	101.35**	102.77**	102.07**	102.03**
Chief Justice Fixed Effects	11.91**	12.29**	13.36**	12.51**	9.55*
Cubic Polynomials of Duration Dependence	144.75**	143.56**	144.97**	145.18**	143.07**
Wald Test of Independent Equations	0.11	0.11	0.09	0.06	0.04
N Stage 1	11838	11838	11838	11897	11897
N Stage 2	210	210	210	213	213

*p<0.05, **p<0.01, one-tailed tests used for individual coefficients
 Grouped Coefficients report Wald Test of Joint Significance
 Standard Errors Clustered on Statute are in Parentheses

Table 3: Heckman Models of Decision to Invalidate an Important Federal Statute Passed Between 1949-2011 that Test Multiplicative Relationships

Stage 2: Invalidations of important federal statutes that are challenged	Court Popularity	Court-Curbing Bills ⁺	Statute Length ⁺	Case Salience
Court Ideology	-3.24 (2.01)	0.95 (1.88)	2.69* (1.56)	0.86** (0.35)
Independence	2.81 (1.96)	0.00 (0.02)	-0.00 (0.00)	0.54** (0.18)
Court Ideology x Independence	8.83* (5.21)	-0.03 (0.07)	-0.00 (0.00)	0.19 (0.33)
Public Ideology	0.00 (0.02)	0.02 (0.02)	0.04* (0.02)	0.03 (0.02)
Focus on Constitutionality of Federal Statute	0.72** (0.32)	0.77** (0.31)	0.88** (0.25)	0.76** (0.28)
Policy Area Fixed Effects	22.12	14.33	12.57	15.22
Chief Justice Fixed Effects	0.53	1.58	1.19	1.65
Stage 1: Challenges to important federal statutes				
Independence	0.34 (0.45)	0.00 (0.00)	-0.00 (0.00)	-
Policy Area Fixed Effects	81.09**	81.73**	106.29**	101.29**
Chief Justice Fixed Effects	8.69*	9.40**	12.70**	8.10*
Cubic Polynomials of Duration Dependence	151.42**	155.52**	119.91**	118.77**
Wald Test of Independent Equations	0.57	0.14	0.10	0.23
N Stage 1	10207	10207	11897	10709
N Stage 2	169	169	213	199

*p<0.05, **p<0.01, one-tailed tests used for individual coefficients
 Grouped Coefficients report Wald Test of Joint Significance
⁺Independence Variable Subject to a Linear Transformation
 Standard Errors Clustered on Statute are in Parentheses

Table 4: Heckman Models of Decision to Invalidate an Important Federal Statute Passed Between 1949-2011 that Test Multiplicative Relationships, Continued

Stage 2: Invalidations of important federal statutes that are challenged	Floor Median Model	Senate Filibuster Model	Party Gatekeeping Model	Partisan Vote	Political Fragmentation
Court Ideology	0.38 (0.51)	0.18 (0.56)	-0.03 (0.60)	0.29 (0.50)	1.37** (0.52)
Independence	0.18 (0.48)	0.21 (0.47)	0.41 (0.46)	0.54 (0.50)	0.51 (0.32)
Court Ideology x Independence	1.58 (1.07)	2.03* (1.08)	2.33* (1.03)	2.25* (1.32)	-0.67 (0.68)
Public Ideology	0.03 (0.02)	0.03 (0.02)	0.03 (0.02)	0.04* (0.02)	0.03 (0.02)
Focus on Constitutionality of Federal Statute	0.92** (0.28)	0.95** (0.29)	0.97** (0.29)	0.94** (0.27)	0.93** (0.27)
Policy Area Fixed Effects	13.52	14.09	13.59	11.80	13.51
Chief Justice Fixed Effects	1.38	1.16	0.70	1.40	2.09
Stage 1: Challenges to important federal statutes					
Independence	0.35** (0.13)	0.31** (0.13)	0.34** (0.13)	0.23 (0.15)	0.04 (0.08)
Policy Area Fixed Effects	101.10**	101.36**	102.77**	102.07**	102.05**
Chief Justice Fixed Effects	11.91**	12.31**	13.38**	12.54**	9.57*
Cubic Polynomials of Duration Dependence	145.82**	144.47**	145.57**	144.29**	145.12**
Wald Test of Independent Equations	0.15	0.16	0.15	0.06	0.08
N Stage 1	11838	11838	11838	11897	11897
N Stage 2	210	210	210	213	213

*p<0.05, **p<0.01, one-tailed tests used for individual coefficients

Grouped Coefficients report Wald Test of Joint Significance

Standard Errors Clustered on Statute are in Parentheses

Figures

Figure 1: Average Marginal Effect of Court Ideology of Table 3, at the Empirical Minimum and Maximum Levels of Independence (90% C.I.)

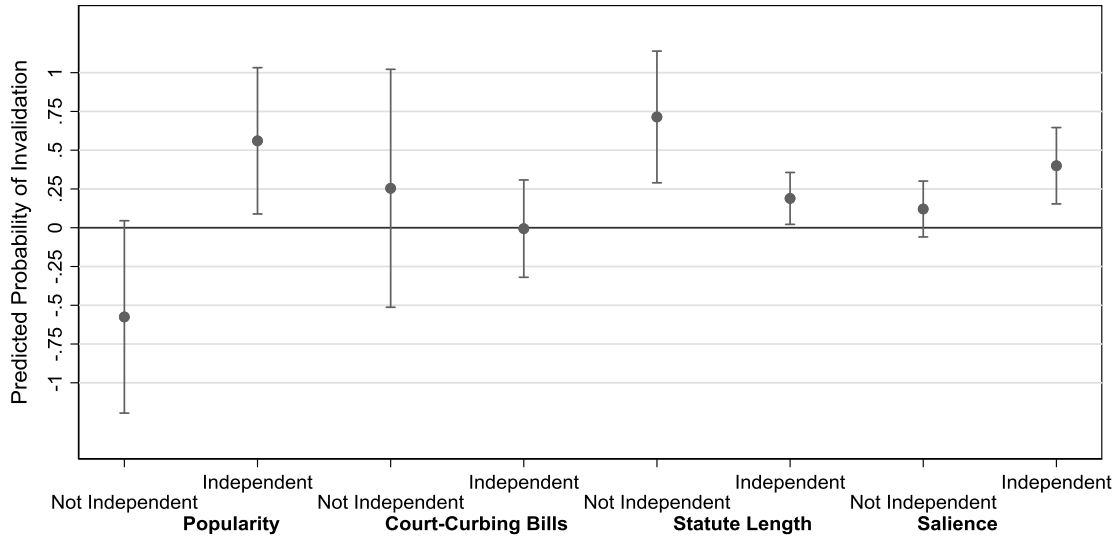


Figure 2: Average Marginal Effect of Court Ideology of Table 4, at the Empirical Minimum and Maximum Levels of Independence (90% C.I.)

