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# Political Competition and Judicial Independence: How Courts Fill the Void When Legislatures Are Ineffective

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## Abstract

What effect does political competition have in generating *de facto* judicial independence? We argue that competition in a legislature can drive increases in *de facto* judicial independence. Our game-theoretic model reveals that increased competition for seats impedes legislators' ability to enact their platforms, regardless of government turnover probability, and increased legislative fractionalization also makes court intervention more likely. Utilizing a sample of democratic states, empirical evidence suggests when a country's legislature is increasingly fractionalized among parties or has increasing seat turnover, we observe increases in *de facto* independence. This research provides new perspectives on the link between independence and competition.

**Keywords:** Judicial Independence; Political Competition; Comparative Judicial Politics

## Introduction

Ahead of the 2008 U.S. elections, the Democratic National Committee adopted its platform, which included a position on marriage equality: “We oppose the Defense of Marriage Act and all attempts to use this issue to divide us.”<sup>1</sup> DOMA – passed by bipartisan House and Senate votes, and signed by Pres. Bill Clinton in 1996 – prohibited recognizing same-sex marriages under federal law. In September 2009, Rep. Jerry Nadler (D-N.Y.) proposed DOMA's full repeal, supported by 120 co-sponsors. Despite near super-majorities in both chambers,<sup>2</sup> Nadler's bill died

<sup>1</sup>See: <https://www.presidency.ucsb.edu/documents/2008-democratic-party-platform>.

<sup>2</sup>The Democratic party controlled 59 of 100 U.S. Senate seats and 256 of 435 U.S. House seats as of the introduction of H.R. 3567 (111th): Respect for Marriage Act of 2009 on September 15, 2009.

in a House Judiciary subcommittee. Liberal elites publicly decried legislative inaction on DOMA, suggesting that courts might be the better avenue to achieve policy change.<sup>3</sup> Indeed, interest groups turned to litigation; in 2010, the American Civil Liberties Union supported a same-sex couple suing the federal government to recognize their marriage under estate tax law. Ultimately, the U.S. Supreme Court struck down components of DOMA – supplanting legislative initiative in *United States v. Windsor* (2013).

Judicial intervention in a separation of powers system is not unique to the American context. Another example lies in France's *Conseil constitutionnel*, which frequently plays a significant role in the policy-making process. The Council utilized its *ex ante* judicial review powers in a particularly salient 2012 case when the court struck down a controversial law intended to tax the wealthy at a 75 percent rate.<sup>4</sup> The challenge came to the Council by way of referral by at least 60 of the 348 members of the French *Sénat*. That minority challenge to majority policies reveals an opening for courts in the separation of powers game: the French Constitutional Council – indeed, courts more generally – can intervene when the parliament cannot generate sufficient agreement to achieve particular policy goals.

These examples present an important separation of powers puzzle: what effect does legislative political competition – as we observe in both the French and U.S. examples – have in generating *de facto* judicial independence? In political terms, judicial independence necessitates a genuine capacity for judges – in terms of institutional rules and observed behavior – to challenge an incumbent regime without fear of retribution (Iaryczower, Spiller, and Tommasi 2002; Linzer and Staton 2015). We argue that *de facto* judicial independence should be particularly ripe when legislatures are ineffective, transferring policy demands onto courts.

In the American example, despite there being one-party control of government between 2009 and 2011,<sup>5</sup> the Democrats were not sufficiently cohesive to fulfill an element of their platform – one demanded by activists. In the French example, party control of both the presidency and *l'Assemblée nationale* changed in the 2012 elections; incumbent center-right Pres. Nicolas Sarkozy was defeated by center-left candidate François Hollande, whose party – in coalition with left-leaning allies – also took legislative control. That degree of partisan volatility in France at that moment did not provide sufficient footing for the new government to promptly enact its tax policy. Indeed, a sizeable minority in the French *Sénat* invited judicial intervention. The nature of separation of powers in both contexts gave an opening to courts: if elected policymakers fail to address certain activists' preferences, then elites have judicial mechanisms for achieving their policy goals.

This research – in line with our motivating examples – presents a new mechanism for understanding how legislative political competition impacts *de facto* judicial independence. We argue that political competition impacts legislators' ability to make policy and therefore drives changes in *de facto* judicial independence even without a turnover in government. Systems with greater legislative competition cannot assure legislators will be in power to reap the political benefits of costly

<sup>3</sup>Rep. Barney Frank noted in an interview that he and other Democrats prioritized other equal rights issues over repealing DOMA because of their assessment that the repeal could not be achieved. <http://boston.edgemedianetwork.com/index.php?ch=news&sc=&sc2=news&sc3=&id=96326>.

<sup>4</sup>See: <http://www.bbc.com/news/world-europe-20864114>. The tax, revised in 2013, was later upheld.

<sup>5</sup>Democrats controlled the House and Senate from the 2006 midterm elections until the 2010 midterms.

policymaking. In such a system, a legislature's relative lack of policy outputs opens the door to judicial policymaking.<sup>6</sup> Furthermore, such legislatures are ill-equipped to check independent judges, who are then free to exercise *de facto* autonomy and power. Under these conditions, political leaders are not deliberately creating independent judicial institutions. Instead, independent courts are a byproduct of the policy demand created by an ineffective legislature.

We present a game-theoretic model showing that increased political competition impedes legislators' ability to enact their platforms. Because voting is costly and support for the policy is uncertain, legislators are only willing to pay this cost if they are likely to be in power to reap the political benefits and if they are relatively certain the legislation will pass. If these conditions are not satisfied, then they are forced to turn to courts to achieve their policy goals. Using indicators for the conditions leading to legislative ineffectiveness, we specify two empirical expectations; *de facto* judicial independence will increase when legislatures exhibit higher levels of (1) partisan fractionalization and (2) contestation. We also extend the model to incorporate insurance theory (i.e., judicial independence increases in anticipation of turnovers in government control). To empirically assess insurance theory, we see whether electoral volatility corresponds to higher levels of *de facto* independence. Our theoretical model's results for fractionalization and contestation hold regardless of electoral volatility, which shows that the empirical regularities we identify are separate from standard insurance theory.

Our data include a large sample of democratic states – ensuring that we observe legitimate political competition across time and space. We use the Linzer and Staton (2015) *de facto* judicial independence measure predicted by legislative competition indicators. As a country's legislature (1) is increasingly fractionalized among political parties, or (2) has increasing seat turnover (not necessarily changes in majority party), we find *de facto* judicial independence increases. Electoral volatility, however, is not a statistically reliable predictor of independence. Although we do not regard this as evidence against insurance theory, our main findings regarding legislative effectiveness provide a new perspective on the link between judicial independence and political competition. Juxtaposed to existing theories, we believe that our research elucidates how policy demand can impact *de facto* independence.

### Judicial–legislative dynamics in separation of powers

Arising out of a separation of powers framework, judicial independence is a fundamental consideration in determining, first, whether judges are free to decide cases according to their sincere beliefs or evaluations of the law – without fear of punishment or retribution. And second, judges must also have their decisions implemented by the governing regime. A wide array of scholarship gives focus to how prospective political competition during constitution-making might yield formalized protections and insulation for judicial institutions – that is, *de jure* independence (e.g., Ferejohn 1998; Ginsburg 2003; Melton and Ginsburg 2014).<sup>7</sup> But political competition can

<sup>6</sup>The French example reveals a similar dynamic: if a legislature lacks sufficient expertise or cohesiveness (e.g., due to new majority status or intra-party preference heterogeneity) to enact its preferred policy, then courts offer an outlet.

<sup>7</sup>When elites anticipate competition at the time of constitutional rule-making, they are likely to endow courts with greater *de jure* independence (e.g., Hayo and Voigt 2014). Constitutionally insulated courts

likewise engender *de facto* judicial independence, which allows judges to accumulate power informally to achieve compliance with their rulings, especially among the other branches of government (Linzer and Staton 2015).

Examining *de facto* judicial independence within a separation of powers context necessarily considers the intersectionality of judicial policymaking, legislative preferences, and public support. The alignment of preferences between a court and a legislature is a strong consideration in the level of constraint imposed on judicial actors. In the U.S. context, when the Supreme Court is ideologically disproximate from Congress, the justices are hesitant to engage in judicial review (e.g., Segal, Westerland, and Lindquist 2011), which has a direct impact on independence (i.e., a “court’s ability to make decisions unaffected by political pressure from outside the judiciary” (Clark 2010, 5)). While pressure on courts can emanate from a variety of external actors who can affect the implementation of judicial policy (Hall 2014), members of Congress have a broad capacity to threaten federal judicial institutions with court-curbing measures – even if such measures are never enacted. Indeed, court-curbing threats can impact the Supreme Court’s institutional support and therefore diminish its capacity to induce compliance among the American public. As Clark (2009) documents, a strategic court will exercise judicial review less frequently when a legislature engages in position-taking on court-curbing measures. Recent research bolsters this line of inquiry; Bartels and Johnston (2020) show that the American public is more willing to curb the Court’s independence when policy emanating from the bench does not align with an individual’s preferences.

Much of the comparative literature on *de facto* independence has centered on “insurance” theories; anticipating power transitions can give rise to greater power and autonomy for courts. Under those circumstances, legislators – hedging their bets on retaining office – will allow courts to increase independence, which then provides policy insurance in the face of the opposition gaining power. The empirical findings with regard to insurance hypotheses vary, with studies disparately suggesting that independent judges strategically defect when incumbent regimes are failing (Helmke 2002, 2004), impose higher costs on policy changes after party transition (Vanberg 2008), and allow competing parties to impose some mutual restraint (Stephenson 2003).

Competition can also arise to a significant degree outside of insurance considerations. In the separation of powers game, judges face constraints in exercising *de facto* independence, which primarily hinges on a judge’s evaluation of the level of political competition within the system. For instance, Randazzo, Gibler, and Reid (2016) utilize various electoral indicators of competition to better understand the impact on courts. Among democratic regimes, a party’s electoral performance is a strong signal

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prospectively bind the hands of future office-holders. The benefits of such a constraint far outweigh the disadvantage of preventing the current government from enacting all of its preferred policies (Hirschl 2009). Constitutional framers tend to be willing to empower courts under conditions of electoral uncertainty – that is, parties will formulate judicial institutions that “protect political minorities and do not clearly favor future election winners” (Magalhaes 1999, 47). This is especially true in emerging democracies, where political actors often face a trade-off in sufficiently insulating judicial institutions; at lower levels of insulation for courts, politically motivated legislators or executives may achieve their policy goals more easily. Prioritizing immediate policy goals during a transition comes at the cost of (1) institutionalizing the rule of law in neutral courts and (2) prospectively allowing similar behavior by other actors and factions in future governments (Larkins 1996).

for when legislators in government need “to develop judicial institutions as protection following a change in leadership” (586). As a more latent process, high levels of ethnic fractionalization lead to an increased likelihood of out-party challenges to the sitting government, which then suggests that courts would have more *de facto* independence. Courts – when exercising discretion in statutory interpretation – also consider the likelihood of legislative response (Cooter and Ginsburg 1996). Strategic judges seek to avoid spurring new legislation that would counter the court’s interpretations. Therefore, “prudent judges pursue their own preferences to a point that stops short of provoking legislative repeal” (296). The risk of legislative backlash is increasing as there are fewer veto points to enact new policy. Additionally, legislators – in considering statutory revisions or court curbing measures – must consider public attention to court-curbing attempts (Vanberg 2001). Judicial actors likewise consider the possibility of backlash based on (1) the expected duration of the current government and (2) the concentration of power (Leiras, Tuñón, and Giraudy 2015).

In particular, legislative effectiveness – as an indicator of competition in separation of powers systems – can have an impact on *de facto* independence. In Colombia, Rodriguez-Raga (2011) provides evidence that courts are increasingly willing to overturn government action as the costs of legislative court-curbing and other anti-court actions increase. Furthermore, political fragmentation or divided government may produce an environment where courts are less likely to face backlash from other branches of government (Ríos-Figueroa 2007), and they would therefore be willing to diverge from the governing majority’s preferences. Research on the Russian constitutional court suggests that judges have incentives to avoid retaliatory attacks from other governing branches (Epstein, Knight, and Shvetsova 2001).

These dynamics suggest that courts could find themselves in positions of constraint due to an oppositional legislature and dissatisfied public. Still, other circumstances offer courts opportunities for policymaking. For instance, Whittington (2005) points to a variety of political factors that would allow or encourage judicial review, as it can be used to strike down statutes enacted by prior governing coalitions. Legislators who are unable – perhaps due to coalitional fragmentation – to repeal existing policies might instead rely on courts to achieve those goals. Among Whittington’s considerations are the division of political power within and across branches of government and preference cross-pressures among constituents or governing coalitions, as majority parties often suffer from preference heterogeneity. Under these circumstances, controversial policies can be shifted outside the legislative arena.

Our theory and empirical tests build on these overarching questions stemming from the literature we discuss above: when will judges have/take the opportunity to assert their independence? What sort of inter-institutional power dynamics would facilitate *de facto* court power independent of government transitions? The story we tell below offers a possible answer. If a dominant national party or coalition becomes ineffective or reaches political deadlock, then it will be unable to settle a public dispute. In this way, judicial independence may be necessary for effective governance.

### A formal model of *de facto* judicial independence

Existing scholarly explanations of how political competition affects judicial independence focus on two important considerations: first, political agents’ strategic

decisions to impair courts' formal ability to produce policy impact; and second, how electoral vulnerability may affect that decision. We provide a theoretical perspective that examines both of these while also accounting for the impact of political competition within a legislature on demand for judicial policymaking.

We argue that *de facto* judicial independence is not only determined by legislators' conscious decisions to create independent judicial institutions. Rather, legislative ineffectiveness develops an environment ripe for judicial action. In other words, as political competition increases, legislative productivity decreases, which leaves more space for judicial independence. We can imagine political competition taking several forms: (1) partisan fractionalization within a legislature and (2) high rates of turnover within a legislature. A third option – electoral volatility changing the composition of a legislature across elections – is more akin to traditional insurance theory. Comparisons of political competition and legislative productivity can occur across time and space.

To make this argument, we study a model of legislative policymaking with costly voting. The model has four players: three legislators (indexed by  $i \in \{1,2,3\}$ ) and a court. Each legislator can be from faction *A* or faction *B*.<sup>8</sup> First, each legislator decides whether to vote for or against a given policy, which we call a reform to the status quo. Each legislator from faction *A* supports reform with probability  $\pi$ ; legislators from faction *B* always favor the status quo. Voting for the reform costs  $\kappa_i$ . If a majority votes for the reform, it passes and is implemented. If the reform does not pass, the court can decide to implement the reform on its own. After the vote, there is an election, and a member from faction *A* wins each seat  $i$  with some probability  $q_i$ .

We analyze two scenarios. First, passage of reform by faction *A* is permanent with no chance of repeal. Second, passage of reform by faction *A* is subject to possible repeal from faction *B* if faction *B* wins a legislative majority. In both scenarios, there is no possibility of reversing a court-created policy. We use these two scenarios to show conditions under which we expect the legislature to defer to the court instead of passing the reform itself.

### Information and Preferences

Each member of faction *A* wants to pass the reform with some positive probability,  $\pi$ . Each legislator knows whether they favor reform but only knows the probability that each other legislator favors reform. If a legislator favors reform, their policy utility is as follows:<sup>9</sup>

$$PU_{ir} = \begin{cases} 1 & \text{If the reform passes and } i \text{ wins reelection} \\ u_r & \text{If the court implements reform or reform passes and } i \text{ loses reelection} \\ 0 & \text{If there is no reform} \end{cases}$$

<sup>8</sup>We use the term “faction” instead of “party” intentionally. While factions and parties will be closely tied in the United States, that need not be the case in parliamentary systems. Instead, coalitions of parties can support certain reforms. Our terminology is intended to capture the general nature of our setting.

<sup>9</sup>The subscript  $R$  stands for a legislator that supports reform and  $S$  for a legislator that supports the status quo.

Voting for reform costs  $\kappa$  regardless of whether it passes. Total utility for reform-favoring legislator is

$$U_{i_R} = PU_{i_R} - \mathbb{1}\kappa_i$$

where  $\mathbb{1} = 1$  is an indicator function that equals 1 if the legislator voted for reform, and 0 otherwise.

A legislator who favors the status quo has a similar utility function:

$$PU_{i_S} = \begin{cases} 0 & \text{If the reform passes} \\ u_s & \text{If the court implements reform or reform passes and loses reelection} \\ 1 & \text{If there is no reform} \end{cases}$$

and

$$U_{i_S} = PU_{i_S} - \mathbb{1}\kappa_i$$

Finally, the court favors reform:  $U_C = \begin{cases} 1 & \text{If there is reform} \\ 0 & \text{If there is no reform} \end{cases}$

### Assumptions and Discussion

Before proceeding to the analysis, we highlight some of our assumptions and discuss some of the features of the model. First, we assume that legislators cannot alter the court's policy. This is a simplification to focus our analysis on the legislature's incentives to pass or not pass legislation. As long as government turnover does not always result in reversing the court's policy, all of our results hold.

We also assume legislators receive higher payoffs from their preferred policies and lower payoffs from their disfavored policies when in office. This is meant to emphasize that politicians get value from passing legislation. These legislative activities allow incumbents to (1) engage in fundraising activities, (2) credit-claim with constituents, and (3) receive some personal utility from accomplishing implementation of a desired reform. This higher payoff requires the legislator both to vote for reform and to win reelection.

Legislators bear a cost of voting for the reform. Note that this cost is born at the vote – not at the implementation. This reflects the fact that legislators may not want to have a vote on record for a controversial reform – especially one that does not pass. Rather, having a controversial reform enacted by a court would be preferable for a faction A legislator, as they do not have to pay the cost  $\kappa_i$  of voting for the reform, but they gain at least  $u_r$ .

In the main body of the paper, we focus on pro-reform courts for ease of exposition. We analyze a model with anti-reform courts in the appendix and show that our results qualitatively hold. We focus on cases where faction A has a majority. That is, we compare outcomes when two legislators are members of faction A as opposed to outcomes when all three legislators are members of faction A. We stick to

this case because we are specifically interested in the cases where legislators want to change policy but do not undertake reform themselves.

Finally, we assume there is no cost for faction *B* when they attempt to overturn reform after the election. Again, this is for algebraic simplicity. As long as there is some probability faction *B* overturns a reform, our qualitative results hold.

### Analysis

We use Subgame Perfect Nash Equilibrium in Pure Strategies as our solution concept (hereafter, just *equilibrium*).

#### Baseline

We begin with the baseline model where faction *B* cannot overturn a reform after the election. If the legislature does not pass the reform, the court will implement the reform, and all pro-reform politicians will receive a payoff of  $u_R$ . Therefore, a pro-reform politician can guarantee herself at least  $u_R$  by voting against reform. To vote for reform, she must receive an expected payoff at least as high as  $u_R$ .

However, voting for reform is risky; if at least one other legislator does not also vote for reform, it will fail and she will have paid the voting cost  $\kappa$  without the legislator achieving their sincere policy goals. Further, even if the reform passes, the legislator does not get the full benefit if she loses reelection, which in reality could be more likely with a recorded vote on a controversial issue.

If there are two faction *A* legislators, a pro-reform legislator will vote for reform if

$$\pi(q_i \cdot 1 + (1 - q_i)u_R) - \kappa_i \geq u_R.$$

If there are three faction *A* legislators, a pro-reform legislator will vote for reform if

$$(2\pi - \pi^2)(q_i \cdot 1 + (1 - q_i)u_R) - \kappa_i \geq u_R.$$

Both inequalities have the same general form. First, the probability that at least one other legislator favors reform is  $\pi$  in the two-legislator majority case and  $2\pi - \pi^2$  in the three-legislator majority case. Second, the probability of legislator *i*'s reelection is  $q_i$ , multiplied by the payoff of passing reform and winning reelection, 1. Third, the probability of not winning reelection, multiplied by the payoff of passing reform but not winning reelection is  $u_R$ . Fourth and finally, there is the cost of voting,  $\kappa_i$  and the payoff from court-implemented reform,  $u_R$ . The first proposition characterizes the comparative statics of passing reform:<sup>10</sup>

**Proposition 1.** *For a pro-reform legislator, the expected utility of voting for reform is*

1. *Increasing in the number of party A legislators.*
2. *Increasing in the legislator's reelection probability.*

The intuition for both comparative statics is simple. First, when faction *A* holds three seats instead of two, it is more likely that there will be at least two legislators who favor

<sup>10</sup>All proofs and full characterization of the equilibrium are in the appendix.



reform. This makes voting for reform less risky for a pro-reform legislator. Second, as the probability of winning reelection increases, the benefit of passing reform increases. This makes a pro-reform legislator more willing to bear the voting cost.

We refer to the level of partisan division of a legislature as *fractionalization*. A legislature with two faction *A* legislators and one faction *B* legislator has greater fractionalization than a legislature with three faction *A* legislators. The probability that a single legislator is reelected to a given seat is *contestation*: the lower the probability a legislator will achieve reelection, the greater the contestation. **Proposition 1** states that greater fractionalization and greater contestation lead to lower expected utility from voting for the reform. As we detail in our empirical expectations section, a lower expected utility of voting for reform means that the legislature will be less likely to enact reform on its own and will instead be more likely to turn to the courts.

### Repeal

One question that naturally arises relates to faction *B* ability to repeal the reform after the election. Would our comparative statics remain as in the baseline model? As the following extension makes clear, even allowing for faction *B* to return to the original status quo after an election does not alter the implications of the model.

We now allow the possibility of faction *B* – after taking a majority in an intervening election – to repeal a reform passed by faction *A* before the election. If anti-reform faction *B* legislators repeal a reform, pro-reform legislators receive a payoff of 0. The expected utility of voting for a reform now takes into account the possibility of receiving 0 even if the reform passes before the election:

If there are two faction *A* legislators, a pro-reform legislator will vote for reform if

$$\pi \left[ q_i (q_j + q_k - q_j q_k) \cdot 1 + (1 - q_i) (q_j q_k) u_R \right] - \kappa_i \geq u_R.$$

If there are three faction *A* legislators, a pro-reform legislator will vote for reform if

$$(2\pi - \pi^2) \left[ q_i (q_j + q_k - q_j q_k) \cdot 1 + (1 - q_i) (q_j q_k) u_R \right] - \kappa_i \geq u_R$$

where  $q_j$  and  $q_k$  are the probabilities faction *A* wins seats  $j$  and  $k$  in the election.

We refer to the probability of a change in which faction is in the majority as *volatility*. As volatility increases, reform is more likely to be repealed in the future. This lowers the expected utility of passing the reform today. We would then expect that as volatility increases, legislators would be more likely to let the court implement reform instead of the legislators bearing the cost of voting for reform themselves. This intuition aligns with existing insurance theory; when majorities anticipate transitions in government, those legislators may formally empower or informally tolerate greater judicial autonomy.

A comparison of the model with turnover to the baseline model highlights two important facts. First, fractionalization and contestation are not dependent on volatility – that is, government turnover. They exist both in the first model without the possibility of repeal and in the second model when reform can be repealed. Second, the possibility of repealing the reform does not change the direction of the

fractionalization and contestation comparative statics. They point in the same direction regardless of whether policy can be reversed. Therefore, whether we find evidence for insurance theory (by way of volatility) in our empirical exercise should have no bearing on whether we find evidence for fractionalization and contestation on the likelihood of judicial independence.

### Expectations for empirical models of judicial independence

Our models above account for political competition generally by allowing for variations in the majority party's ability to implement its policy. Although research addresses the multitude of ways in which competition can influence a legislature's policy-making effectiveness, we address three. First, fractionalization is the partisan division within the legislature. In our model, we operationalize fractionalization by changing the size of faction *A*'s legislative majority.

As fractionalization increases, factions need a greater percentage of their members to vote for legislation. Because not every member of a party or faction may support given legislation, needing a greater percentage of a faction makes passing legislation more difficult. In our model, that results in fewer pro-reform legislators actually voting for reform. Therefore, we expect that as legislative fractionalization increases, *de facto* judicial independence will likewise increase.

**Fractionalization Hypothesis:** As a country's legislature is increasingly divided among political parties, we expect an increase in *de facto* judicial independence.

Second, legislative contestation conceptualizes a different aspect of political competition – that is, competition occurring through elections, similar to the approach taken by Randazzo, Gibler, and Reid (2016). More specifically, contestation conceptualizes seat turnover through elections. This manifests in two ways. First, losing a seat makes the odds of a party maintaining its majority smaller. In turn, the odds of a party maintaining a governing majority decrease, which makes it harder for it to pass its preferred policies. Second, it means the losing candidate cannot take advantage of having passed legislation through fundraising, affecting its implementation, or showing constituents how much she has accomplished.<sup>11</sup> Both of these effects are captured in our parameter  $q_i$ , the reelection probability of a given legislator. A higher probability of reelection translates to a higher probability of voting for legislation and therefore a lower probability of judicial independence:

**Contestation Hypothesis:** As a country's legislature has increasing levels of seat turnover, we expect an increase in *de facto* judicial independence.

Finally, we turn to our third measure of political competition – legislative volatility – which captures the degree to which a particular election changes the partisan control of a legislative chamber. Volatility is high when a large majority party loses its control of a chamber, and the incoming majority is likewise large. France's 2017 elections are a prime example of volatility, as the post-election majority party – the brand-new *En Marche!* party, which had recently won the French presidency – won 350 seats anew. The pre-election majority party – the French Socialists – lost 286 seats.

<sup>11</sup>The value of the seat itself to the politician is why contestation is not synonymous with turnover in government.

In the American context, the Tea Party takeover of the U.S. House in 2010 is similar evidence of volatility, as the Republicans took over 63 seats – easily becoming the majority party.

On its face, volatility speaks directly to prior findings regarding insurance theory; a party anticipating losing control of the government would allocate additional power and autonomy to the courts in order to produce policy stability. Indeed, this is what the extension to our baseline model predicts. Still, it is unclear whether volatility should correspond to increased reliance on the courts. A new, incoming majority party is likely to rely on its electorally derived mandate to implement its platform. Our model ends after the election and does not take future policymaking into account. However, if voters have switched their support from the prior governing party to the new majority, that would suggest dissatisfaction with past policies and the prior government (e.g., Helmke 2004) and predict policy overturns.

However, a party that had previously been out of government might need to expand its coalition by moderating its platform. That, in turn, would lead to more heterogeneous policy goals within the new majority party. Given these countervailing considerations, our initial expectations are in line with the existing literature regarding insurance theory, which suggests that the outgoing party anticipated its ouster and therefore allocated greater autonomy and power to courts – whether by informal or formal mechanisms. Although the incoming majority party will rely on its new-found governing status to pass its preferred policies, we would expect the courts to lag in terms of their ideological alignment with the new government. Therefore, we expect that as legislative volatility increases, *de facto* judicial independence will likewise increase.

**Volatility Hypothesis:** As an election changes the party composition of a country's legislature by increasingly large margins, we expect an increase in *de facto* judicial independence.

### Measuring independence and competition

In order to measure political competition and judicial independence, we rely on several different data sources. That being said, it is imperative to make note of how our data are constrained to democratic countries with legislative institutions that have real competition and turnover. We identify democratic states using Polity IV data. We include any state achieving a score of 6 or higher in the polity rating. Additionally, to ensure the effects of regime transitions are not captured by the analysis, we include only democracies that have survived for at least five years.

Measures of judicial independence range widely in how they are constructed. This is of particular importance given the inherent intangibility and difficulty of measuring an idea – such as *de facto* judicial independence – without a clear and universal definition. The data created by Linzer and Staton (2015) combine multiple measures into one continuous proportion measuring *de facto* independence, with 0 being not independent and 1 being fully independent.<sup>12</sup>

They model the measurement of independence for 200 countries from 1960 to 2010 and allow for estimation and a measure of uncertainty.<sup>13</sup> Because the model

<sup>12</sup>For a more detailed account of the data generating process, see Linzer and Staton (2011).

<sup>13</sup>Because this variable is the outcome variable, its uncertainty does not need to be accounted for because the model error term captures this.

estimation includes many of the different measurement techniques discussed above, the final product is relatively flexible and includes a great deal of information. As such, this is the most appropriate data to test the theory of this paper.<sup>14</sup>

We use three different measures to assess the degree of political competition. This helps alleviate some of the potential measurement errors by testing the theory using data generated through different processes. It also allows us to test our theory on a wider range of countries and over a longer period of time because data for each measure is not available for every country-year provided in the Linzer-Staton database.

The first measure of political competition, which we label *Fraction* (Beck et al. 2001),<sup>15</sup> captures fractionalization within a legislature. More specifically, it is the probability that two deputies picked at random from the legislature will be of different parties. Legislators who are of different parties could (1) both be in the governing coalition, (2) both be in opposition parties, or (3) have one in government with the other in the opposition. In this way, fractionalization is meant to capture how many different parties exist within a given legislature, along with the relative size of the parties. A fractionalized governing coalition may have difficulty enacting its platform given variations in the likelihood that some parties fulfill their promises (e.g., Martin and Vanberg 2011). This measure speaks directly to the theory of this paper; when there is greater fractionalization, there is greater competition in the sense that one party will have greater difficulty controlling the legislative agenda. These data have 872 observations over 44 countries.

The second variable, *Contest*, is a principal component factor of many political contestation measures. Teorell et al. (2013) develop this measure as an indicator of real political competition within a country. Higher values indicate more frequent turnovers through elections and greater power-sharing between parties.<sup>16</sup> These data include 574 observations over 37 countries.

The third variable is *Volatility*, which we utilize to flesh out the causal mechanisms at play (Powell and Tucker 2014).<sup>17</sup> Volatility captures the degree to which an election changed the party composition of a legislature. It is highest when a drastic power shift occurs – that is, when one party loses a very large majority at an election and is replaced by another party gaining a very large majority. It is smallest when no changes occur in party composition within the legislature. The data have 55 observations over 14 countries, typically with between 4 and 6 observations per country.

Unfortunately, the volatility data are only available for European countries in election years, so our empirical models are limited. If competitive parties create independent judiciaries to maintain policy preferences or to create stability in a power-sharing arrangement, then high levels of volatility will be associated with increases in judicial independence. If, however, changes in party control allow a governing majority to consolidate its power and enact its preferred policies, then we would find a null or negative effect on judicial independence. This would further imply that a party would have sufficient power to curb an interventionist judiciary.

<sup>14</sup>Arguments more in line with Hanssen's reasoning could be focused on measuring the degree of *de jure* independence, which might be of interest to understanding legislative behavior, but it is outside the scope of this paper.

<sup>15</sup>Available at: <https://datacatalog.worldbank.org/search/dataset/0039819>.

<sup>16</sup>Data obtained from Quality of Government Institute Standard Data: <http://www.qog.pol.gu.se>.

<sup>17</sup>Data available at: <http://www.eleanorneffpowell.com/data.html>.

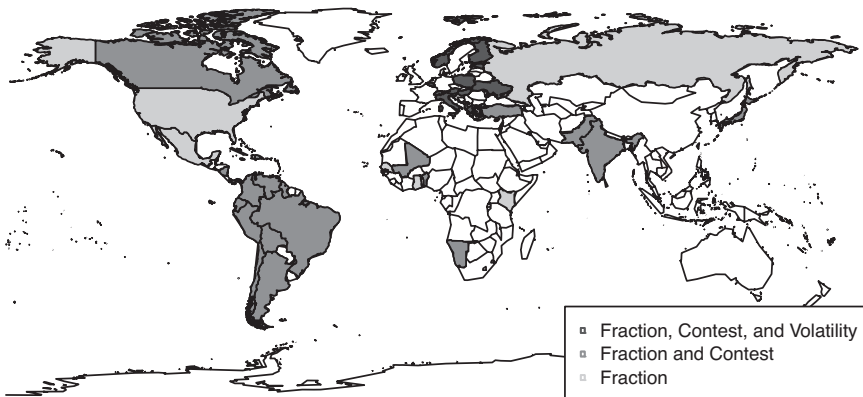
**Table 1.** Missingness

Fraction	Contest	Volatility	<i>n</i>
62	334	877	936

Because not all of these measures are available for each country-year in the subsetted Linzer and Staton data, we use multiple imputation to alleviate some of the bias introduced by missingness. As the volatility index is missing a very high proportion of data, we do not impute this variable. However, we repeated the results shown below with case-wise deletion for the volatility variable, and the results are almost identical. Table 1 shows the number of missing observations, and Figure 1 depicts the data availability. We use the mice package (van Buuren and Groothuis-Oudshoorn 2011) in the R software environment (R Core Team 2017) to create 10 data sets.

The first control we use is the Polity IV rating of democratic governance. It is conceivable that high levels of democracy cause both increased measures of judicial independence and an increased degree of political competition. Though all states we consider are democracies, the level of democracy – vis-à-vis democratic consolidation and democratic values – varies across observations. The second control is the length of democratic governance.<sup>18</sup> Again, it is conceivable that the length of democratic governance directly affects the level of judicial independence and the degree of political competition.

We also utilize a measurement for presidential systems versus parliamentary systems. The variable takes a two if the system is parliamentary, one if there is an assembly-elected president, and zero if it is a presidential system.<sup>19</sup> A presidential system may impact the amount of political competition by unifying strong parties.

**Figure 1.** A world map showing where the data is available and which countries we analyze.

<sup>18</sup>Both measures are available at <http://www.systemicpeace.org/inscrdata.html>.

<sup>19</sup>We treat this variable as continuous as there seems to be a logical ordering of the values. Regressions run treating this as a categorical variable yield no substantive changes.

Furthermore, it may impact the amount of judicial independence by creating an additional check on courts.<sup>20</sup>

Finally, we use a dummy indicating the type of colonial history of a given country. Colonial rule undoubtedly has an impact on the nature of political institutions, and it is possible that a particular colonial history could impact both political competition and judicial independence.<sup>21</sup>

### Empirically modeling *de facto* judicial independence

To assess our hypotheses, we utilize time-series data across countries, as political competition and judicial independence can vary across both space and time. Therefore, we employ a multilevel model with varying intercepts for each country. We do not vary the effects of the controls and the independent variables of interest. Each model uses a different independent variable to capture political competition: fractionalization, contestation, and volatility. We logistically transform the outcome variable – the Linzer-Staton measure of *de facto* judicial independence – as it is a proportional measure.<sup>22</sup> The first model has *Fraction* as the variable of interest, the second has *Contest*, and the third has *Volatility*. Formally, the three models are specified:

$$\begin{aligned} \text{logit}(\text{LJI}_i) &\sim \mathcal{N}\left(\alpha_{j[i]} + \beta_0 + \beta_1 \times \text{interest}_i + \beta_2 \times \text{system}_i + \beta_3 \times \text{polity}_i + \beta_4 \times \text{years}_i, \sigma_y^2\right), \\ &\text{for } i = 1, \dots, n, \\ \alpha_j &\sim \mathcal{N}\left(\gamma_0 + \gamma_1 \times \text{colonial}_j, \sigma_\alpha^2\right), \text{for } j = 1, \dots, J, \end{aligned}$$

where our primary concern lies in the independent variable of interest for the given model,  $n$  is the number of observations across both time and space (936), and  $J$  is the number of countries (44).<sup>23</sup> Because the data are not fine-grained with respect to time (i.e., we only have yearly data), we report the instantaneous estimated effects (i.e., no lags are in the explanatory or control variables). This allows us to maintain as much data as possible. However, with the awareness that the effects of these *may* be lagged, we include in the Online Appendix the results from models, identically specified, with a lag of one year, two years, and three years across all variables but the outcome. The results are very insensitive, and the reliability of the effects is maintained, across lagging schemes.

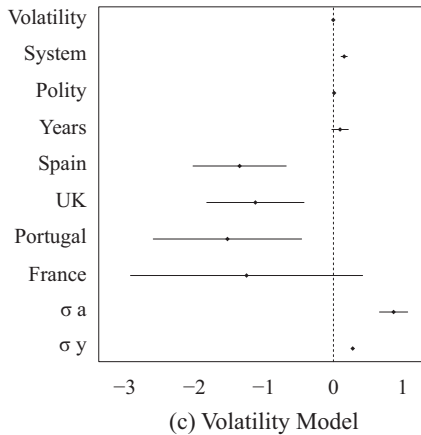
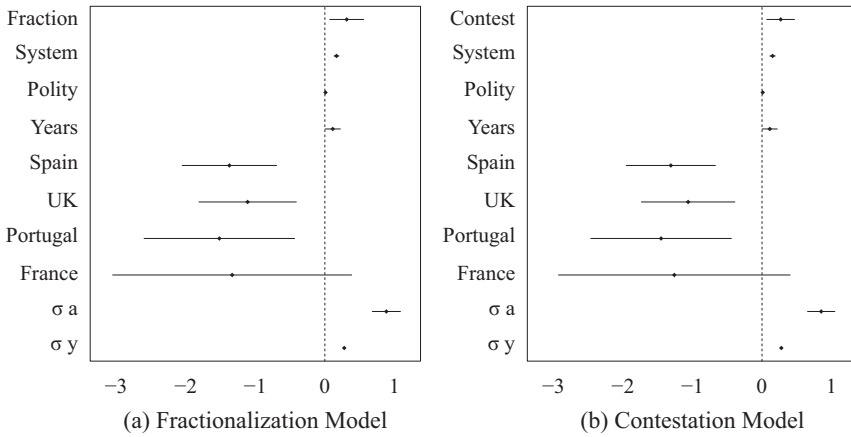
We model each of the ten data sets separately and pool the results. We accomplish this by averaging the point estimates and calculating the standard errors by taking the

<sup>20</sup>We considered utilizing a dummy for proportional representation systems, but the effect of this variable was not statistically reliable, the magnitude of the estimate was very close to zero, and inclusion did not statistically improve model fit.

<sup>21</sup>These data available at <http://siteresources.worldbank.org/INTRES/Resources/469232-1107449512766/DPI2012.dta>.

<sup>22</sup>The results hold without this transformation, but we maintain it to ensure predictions and fits are within the possible bounds.

<sup>23</sup>Colonial history is captured through country-level predictors for France, Portugal, Spain, and the United Kingdom. The controls do not change over the course of the data set. Interestingly, the system does vary within a few countries.



**Figure 2.** Multilevel Models Predicting *de facto* Judicial Independence across Several Explanatory Variables  
 Note: All model estimates include 95% credible intervals.

square root of the sum of the within variance and the between variance, scaled by  $(m + 1)/m$ , with  $m$  equal to 10 (number of data sets).<sup>24</sup> The degrees of freedom are estimated as outlined in Barnard and Rubin (1999). The prior distributions are not theoretically motivated and, as such, are conservative. Specifically, the prior distributions for the  $\beta$ 's and  $\gamma$ 's are normal distributions with mean 0 and standard deviation 3.

The  $\sigma$ 's have inverse-gamma prior distributions with shape 2 and scale 0.5. The results of all models are shown in Figure 2. All three panels in Figure 2 show the point estimates and 95% credible intervals, with intercepts suppressed.

Model (a) in the top-left panel of Figure 2 examines the Fractionalization Hypothesis discussed above. The point estimate for the *Fraction* variable is both positive and reliable, with 95% credible interval [.07, .56] and point estimate 0.31. As

<sup>24</sup>See Rubin (2004) for a more detailed description of this process.

such, we see that as partisan fractionalization within a legislature increases, *de facto* judicial independence also increases, suggesting support for our hypothesis. There is also evidence of more variability between countries than within countries, which validates the use of the multilevel model. Though a discussion of the controls is not necessary, it is interesting to note that a country with a colonial past has a negative impact on the intercept, and all but French colonial history estimates are reliable.

In model (b) in the top-right panel of Figure 2, the variable of interest is *Contest*, which is both positive and reliable at the 95% level [.07, .47]. This is highly suggestive of support for the *Contestation Hypothesis* above, which states that increases in electoral contestation correspond to increased political competition and, therefore, increased *de facto* judicial independence. Again, variability is larger between countries than within countries but is slightly lower than in the previous model. There are no noteworthy changes to the controls.

Model (c) in the bottom panel of Figure 2 includes the volatility measure for our explanatory variable of interest. The estimate for *Volatility* is very close to zero and slightly negative. The 95% credible interval tightly bounds zero. This indicates that we cannot reject the null hypothesis that judicial independence is not related to volatility. In this case, we have no evidence to support insurance theory. As judicial independence is positively related to competition in our other models, it is somewhat surprising that there is no evidence that it is related to volatility. As we discussed above, although volatility is sometimes linked to particular notions of political competition, it is unlikely that a new majority party has a deficit of political capital. As such, a new majority party likely has the capacity to constrain a court through prospective court curbing. This is in line with the premise that competition is related to independence through its effect to constrain a legislature.

## Model implications and limitations

The motivating examples we employ at the start of this paper are worth revisiting in light of our empirical results. The models above reveal relatively broad support for our theory and the corresponding hypotheses across both time and space. As we saw with the controversies surrounding the Defense of Marriage Act (DOMA) in the United States and Pres. Hollande's "supertax" in France, our empirical results show that higher levels of legislative political competition tend to lead to increases in *de facto* judicial independence. In the U.S., the Democratic majority was sufficiently heterogeneous to prevent legislative action on DOMA, which activists had ardently demanded. In France, the *Sénat* was starkly fractionalized between the majority Socialist party, which had won 177 seats in the 2011 elections, and the minority coalition, which had 171 seats. The 60-senator referral of the tax statute to the *Conseil constitutionnel*, along with the broader partisan dynamics, invited judicial intervention. And Pres. Hollande's 75-percent tax would ultimately fail by 2015.

In this way, partisan fractionalization and contestation within a legislature speak directly to a majority coalition's ability to achieve its preferred policies. In our models above, fractionalization and contestation lead to the expected changes in judicial independence. As a governing coalition finds it more difficult to pass legislation itself, the courts are able to step in and make policy independently. As such, we see that courts have greater room to intervene (a) given a more heterogeneous legislature



(fractionalization) or (b) through the possibility of a legislator losing their seat (contestation).

Importantly, we do not find evidence that volatility leads to the same changes in judicial independence, and we discussed our reasoning as to why we were uncertain regarding that relationship. Chiefly, large swings in legislative majorities might imply strong public support for an incoming government, and, in turn, an increased capacity for the new legislature to implement its policy platform. Whether or not our null finding in our volatility model poses a direct challenge to insurance theory is an open question and one that we do not address here. However, as our formal model shows, the effects of fractionalization and contestation can occur with or without the effects of volatility.

In short, the mechanisms of political competition that yield higher levels of fractionalization, contestation, and volatility do not necessarily all lead to the same conclusion with regard to court independence. Volatility, in particular, can still allow for a majority party to engage in court curbing, which would decrease perceptions of *de facto* independence. The difference between volatility – on the one hand – and fractionalization or contestation – on the other hand – shows that *competition* by itself is too broad a concept for predicting judicial independence. Our model and empirical results show how subtle differences in types of competition can differentially affect whether judiciaries act independently.

## Conclusions and future work

Scholars have extensively studied the important relationship between political competition and judicial independence, providing broad empirical support for the proposition that the two are related. While the main thrust of the literature has been to explain the supply-side effect – answering the question of why politicians would provide courts with institutional protections – our theoretical model uncovers and explains important dynamics largely unexplored by the literature so far. Our empirical tests are largely supportive of these conclusions.

In particular, much of the current literature argues that legislators will create an independent judiciary when faced with the probability of losing power as a means to ensure stability in their preferred policies. We, however, argue it may not only be insurance in the face of possible power loss but also a result of a politically constrained legislature. As the legislature becomes less able or willing to deliver policy, the figurative door is left open for the judiciary to exercise independence and provide reform.

Why would the legislature be less willing or less able to deliver policy? Our model gives multiple reasons separate from insurance theory. First, the possibility of losing an individual seat means a legislator will not enjoy the benefits of passing legislation, which will make her less likely to support costly legislation. Second, when a legislator is *uncertain* over the preferences of her co-partisans, she is less likely to support new policy. Co-partisans from districts that do not support reform may not be willing to bear the cost of voting for reform. This means that even if some faction member votes for reform, it still may not pass. As such, voting for reform is a risky endeavor even when a member supports the policy. Further, these two dynamics interact, making a legislator less willing to vote for reform when her party holds fewer seats.

To test our hypotheses, we estimate three multilevel models across time and countries – each model with a different independent variable of interest. The results strongly suggest that legislative political competition is linked to judicial independence. There is also weak evidence that the driving mechanism is not turnover as would be suggested by insurance theory, but instead fractionalization and competition. High levels of volatility were actually (insignificantly) negatively related to judicial independence, and there is suggestive, but very weak, evidence that there might be an inverted-U relationship between volatility and independence. To further test this, future work should gather more data on volatility to cover a wider range of countries over longer periods of time. Another potentially fruitful avenue would be to gather data on legislative productivity. If, within a country, legislative productivity is negatively related to judicial independence, there would be further evidence that the driving mechanism is a constrained legislature. Furthermore, prior literature suggests that indicators of legislative effectiveness may be interrelated – chiefly, that there are interactive effects between fragmentation and turnover (Leiras, Tuñón, and Giraudy 2015). We believe future studies of *de facto* independence should examine the conditional relationships between measures of effectiveness and their impact on court power.

Our model provides more predictions than what we empirically test in the current article. In particular, it predicts that as political parties themselves become more diverse – and therefore have more heterogeneous preferences – passing legislation will be more difficult. Therefore, they should rely more often on the courts. We do not directly test this prediction, and we believe it is a fruitful area for future research. Other related empirical tests in line with our theoretical explanation that we do not undertake include (1) analyzing actual court decisions and their relationship to legislative policies and (2) considering the role of (a) party discipline and (b) intra-party preference heterogeneity as dimensions our concept of fractionalization.

If the link between political competition and judicial independence is a politically constrained legislature, we note potential alternative explanations. Specifically, this paper argues that *de facto* judicial independence may not be a conscious decision made by legislators. Nothing in this paper tests for this, and deriving a test would be challenging. Callander and Krehbiel (2014) argue that a gridlocked legislature will delegate to other actors to adapt to changing environments. In this context, the courts could be viewed as this actor, suggesting the legislature is consciously granting or allowing the courts to be more independent. Perhaps the best way to test this in future work would be to look at *de jure* judicial independence. If the legislature is consciously promoting an independent judiciary, legislation should be passed to this end. Studying the timing of independence-promoting legislation might shed some light on the mindset of the legislators. Although this paper strongly challenges some of the previously argued causal mechanisms linking judicial independence to political competition, there is still much work to be done to better understand the latent processes leading to *de facto* court power.

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