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Beyond the law's borders: Evidence on what shapes judicial decisions from U.S. immigration cases

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Abstract

Evidence that publicly-elected judges rule differently to please voters has challenged the belief that judges are independent of political incentives. However, it is less established whether *non-elected* judges also play politics despite their lack of explicit electoral incentives. This dissertation fills this gap, by showing that non-elected U.S. immigration judges are more inclined to deport immigrants following a Republican-winning presidential election and less inclined following a Democratic-winning one. Furthermore, nuanced predictions about the heterogeneity of incentives make it difficult to attribute the empirical results to explanations not rooted in politically-motivated, strategic judicial behaviour.

(7486 words)

1 Introduction

Judicial independence is of great importance, not only because a fair judiciary is a focal aspiration of liberal societies, but also because legal outcomes can greatly impact social welfare. Both laymen and economists perceive judges to be more insulated from politics than others in public service: for instance, Maskin and Tirole's (2004) paper *The Politician and the Judge* deliberately contrasts "pandering" politicians and independent judges. At the same time, studies have challenged the view that judges are independent of economic and political incentives: in the title of his influential 1993 article, former-judge and academic Richard Posner remarks "What Do Judges and Justices Maximize? (The Same Thing Everybody Else Does)."

Indeed, when it comes to judges who are directly elected by voters, there is now clear evidence that they do alter their behaviour to please the electorate. However, there is virtually no robust evidence on whether appointed judges, who do not face explicit electoral incentives, also respond to political pressure. This dissertation fills this gap. Using data from U.S. immigration courts, I find clear evidence that judges who do not face popular elections nevertheless respond to political incentives. I use a regression discontinuity design to demonstrate that judges become more inclined to deport immigrants following a Republican victory in a U.S. presidential election, and less inclined following a Democrat victory. This is consistent with the signalling model proposed in this dissertation in which judges signal their congruence with the government for perks and promotions. The model also predicts nuanced heterogeneity in incentive effects which is borne out in the data: specifically, judges that can easily disguise a decision against the government's ideology as one where they had no discretion will be less compelled to signal. Together with my main finding that a judge's willingness to deport changes in opposite but expected directions depending on the winning party, this presents strong evidence on U.S. immigration judges responding to political incentives.

The importance of the context for this study — asylum cases — should not be understated. Immigration is currently a highly politicized topic with important consequences for labour markets, business creation and cultural capital to name a few. Furthermore, the decisions made in immigration courts can be matters of life-and-death, especially for asylum-seekers fleeing violence. Former judge Dana Marks has compared immigration cases to death penalty decisions.[1] It poses a serious moral concern if judicial decisions in such cases are subject to political manipulation. Indeed, in the U.S., there is a strong suspicion that they are: as Peck (2021) writes, ''[a]s long as the [U.S.] immigration courts remain under the authority of the attorney general, the administration of immigration justice will remain a game of political football."

Existing research on immigration courts have found large variation in decisions both across and within

[1] Marks, (2014). "Death penalty cases in a traffic court setting", *CNN*, 26 June.

judges for immigration cases with similar characteristics (Ramji-Nogales et al., 2007), which is sometimes correlated with irrelevant factors (Chen, 2016; Heyes and Saberian, 2019). The identification methods in these studies have their weaknesses. Nevertheless, by establishing that there are differences in immigration decisions that cannot be easily explained by the features of a case, they raise an important question of what is responsible for this variation. These findings provide a starting point for my investigation into the motives behind immigration decisions.

This dissertation contributes to the broader literature challenging the conventional view of judicial independence (Helland and Tabarrok, 2002; Gordon and Huber, 2007; Berdejó and Yuchtman, 2013; Lim, 2013; Cohen et al., 2015). These studies, which focus on the behaviour of *elected* judges, offer two main insights. Firstly, justices with electoral incentives behave differently: for instance, Lim (2013) finds that Kansas judges in districts with judicial elections pander to their electorate's political ideology, while those in districts with gubernatorial appointment do not. Secondly, the degree of pandering increases as elections loom: Berdejó and Yuchtman (2013) find that Washington State judges impose longer sentences at the end of their election cycle compared to the start. This evidence demonstrates, perhaps unsurprisingly, that judicial elections can make judges behave like politicians.

Across the world, however, elected judges are a tiny minority. As to whether non-elected judges, who ought to be more independent, also pander to politics, the existing literature does not offer a robust answer. Studies suggest they often rule in favour of their appointing body: Sunstein et al. (2006) find a significant difference in voting patterns between Republican-appointed federal justices and Democratic-appointed ones. Yet, such "appointment effects", although important, are not indicative of strategic behaviour, and may be due to selection alone. The study that comes closest to mine is probably Cohen (1991), who detects promotion-seeking behaviour in appointed district judges by using the age of same-state judges in higher circuit courts to proxy for "promotion potential." However, since he only observes cases in 1988, his variation is entirely interstate and cannot account for unobserved state heterogeneity.

Furthermore, existing studies generate variation in political incentives through temporal proximity to judicial elections or promotions. However, exactly when we consider retention/promotion to be salient for a judge is ambiguous, and if a judge's history of decisions can be observed, they may already be signalling before a vacancy or election period. Instead of using ambiguous variation in the magnitude of incentives, this dissertation uses clear variation in their direction. Specifically, I apply a regression discontinuity design in time to examine whether changes in government, and the "target" ideology to pander to, affect judges' deportation decisions. Although this approach has its own potential caveats, I combine it with difference-in-difference methodology to boost its robustness and use a number of stress checks to further establish the credibility of the results.

To sum up, by demonstrating that appointed U.S. immigration justices respond to the outcome of presidential elections, this dissertation contributes new robust evidence on the strategic behaviour of non-elected judges. Section 2 provides background on U.S. immigration adjudication and courts. Section 3 outlines my conceptual framework and 4 the empirical strategy. Section 5 describes the data and 6 presents the results. Section 8 concludes.

2 Context

2.1 Legal process

Immigration courts chiefly hold proceedings to determine whether to deport an individual or allow them to stay – these are known as "removal proceedings." Individuals enter removal proceedings after being charged by a U.S. immigration official or by filing an application for asylum relief. They firstly attend an "initial hearing" with other immigrants present, after which the judge schedules an individual "merits hearing" which could be months or years later. In the merits hearing, the individual offers evidence, testifies and is cross-examined by immigration attorneys. The judge then renders a decision, typically to conclude the hearing. Excluding interim decisions, the judge can choose to deport the individual, grant them asylum or cancel the charges brought against them by immigration officials.

2.2 Incentive structure of the courts

Immigration judges operate under the U.S. attorney general (AG). According to the Immigration and Nationality Act, immigration judges "shall perform such duties as the Attorney General shall proscribe." Not only can the AG, a political appointee of the president, steer an immigration judge's career progression, they can also influence their discretionary power and reputation. Perhaps unsurprisingly then, immigration practitioners have complained that immigration judges are "faithful to the government, but not faithful to the law."[2] However, there has been no study to robustly test this conjecture.

The AG decides who to promote to the higher Board of Immigration Appeals; since the Board reviews and overturns decisions, the AG promotes like-minded judges in order to shape policy. For instance, in 2019, former Trump AG Jeff Sessions promoted six judges, each of whom had asylum denial rates over 80 percent compared to the national average of 57 percent.[3] In another example, former AG John Ashcroft reformed the Board in 2002 to "purge those members whose substantive views don't conform to [his]."[4] By screening judges based on their political views, the AG creates incentives for judges to pander to the government. Furthermore, the AG can fire justices. Although this is not a common occurrence, Ashley Tabbador, president of the National Association of Immigration Judges, disclosed that judges have issued decisions based on whether the decision would get them fired.[5]

Studies have shown that judges fear being overturned and derive utility from possessing discretionary power (Segal and Spaeth, 2002; Smith, 2006). The AG has considerable influence over both of these concerns. Firstly, they can reverse immigration decisions through their "self-referral" power, which all previous attorneys general have exercised since its inception.[6] Secondly, the AG can restrict the discretion

[2] Innovation Law Lab and SPLC Southern Poverty Law Center (2019)
[3] Transactional Records Access Clearinghouse (2019)
[4] Aleinikoff and Martin, (2002). "Ashcroft's Immigration Threat", *The Washington Post*, 26 February.
[5] Pattinson, (2019). "Some immigration judges say they rule a certain way to avoid firing", *Crux*, 1 October.
[6] Migration Policy Institute (2021)

of judges by managing their case dockets. For example, in *Matter of Castro-Tum*, Judge Steven Morley granted a continuance instead of ordering deportation when the individual failed to appear in court. After this ruling, which might have signalled that Judge Morley had a forgiving attitude towards immigrants, the AG reassigned the case to another judge. During the hearing, Judge Morley asked "am I in trouble?"[7] The notion that immigration justices rule with eyes over their shoulder is reinforced by the existence of regular performance reviews: the AG has the authority to "[e]valuate the performance of the Immigration Courts... and take corrective action where needed."[8]

Through the AG, the incumbent government can influence the career progression, discretionary power, reputation and mental health of immigration judges. In response to this political pressure, it is plausible that judges would alter their behaviour. The next section offers a conceptual framework for how.

[7]Archambeault, (2018). "The Repercussions of How the Administration Has Handled Matter of Castro-Tum", *Think Immigration*, 14 August.

[8]Code of Federal Regulations, Title 8, § 1003.9(b)

3 Conceptual Framework

This section outlines a simple asymmetric information model of immigration judges responding to political pressure, with the purpose of deriving predictions we can take to the data. I show that, in a pooling Bayesian Nash equilibrium, judges who have a different immigration stance than the incumbent party will occasionally betray their personal ideology to signal congruence with the government. This motivates my empirical analysis (Sections 4-6) by predicting that immigration judges alter their propensity to deport depending on who is in power.

In this framework, judges have privately-known "attitudes" towards immigration and use decisions to convey information to a government that wishes to promote like-minded judges and punish different-minded ones (by e.g. restricting discretionary power). For simplicity, I analyse a model where judges make decisions only once.

3.1 Setup

An immigration judge could be one of two privately known types, Harsh or Lenient, with the former preferring to deport immigrants and the latter preferring to grant them stay. Judges incur an ideological cost from ruling against their preferred decision. Without loss of generality, assume the government has an anti-immigration stance and wants to reward (or not punish) Harsh types. For a given judge, the government has belief μ that they are Harsh. Let $R(\mu)$ be the reward from the government to the judge.

A continuum of cases is described by objective grounds for deportation, z. For instance, an immigrant with a violent felony charge would have higher grounds for deportation z than one without, ceteris paribus. Assume z is only observed by the presiding judge; the government cannot due to time/effort constraints. Judges only have discretion over cases with $z \in [\underline{z}, \bar{z}]$. Outside of this interval, the choice is "obvious" as the grounds for deportation are either too high or too low. Judges have no discretion in obvious cases, as any contradicting decision would be overturned in an appeals court.

The timing of the model is as follows: (1) A judge is either Harsh or Lenient; (2) Nature assigns a case of type z to the judge, drawn from cumulative distribution function F; (3) The judge decides to deport or grant stay; (4) The government updates its prior belief about the judge's type and chooses how much to reward (or punish).

The strategy set of a judge is the set of functions which output a decision for every possible z. Given strategy s and CDF $F(z)$, we can derive a judge's probability of deporting, $\rho(s|F(z))$. From this, we can express their expected payoff as:

$$U(\rho(s|F(z))) = \rho R(\mu_1^d) + (1-\rho)R(\mu_1^s) - \phi(s) \tag{1}$$

where μ_1^d and μ_1^s are the government's posterior beliefs observing a deportation and grant of stay respectively, and $\phi(s)$ is the judge's expected ideological cost from choosing strategy s.

Assumption 1: $R'(\mu) > 0$; the reward from the anti-immigration government increases with the belief that the judge is Harsh.

Assumption 2: F is a continuous function.

Assumption 3: For Harsh types, the cost of granting stay is continuously increasing as z increases, starting from zero at $\underline{z}$. Conversely, for Lenient types, the cost of deporting is continuously increasing as z decreases, starting from zero at $\bar{z}$. Intuitively, the less "deportable" a case, the more painful it is for a Lenient judge to rule against their ideology.

3.2 Lenient-type strategies

Observation 1: It is unoptimal for a Lenient judge to play any strategy that deports a case with grounds for deportation z_a in interval A, but does not deport one with z_b in interval B, where $\sup(A) < \inf(B)$. This is strictly dominated by a strategy which achieves the same ρ, and expected reward, while minimising ideological cost, by always deporting the cases with highest z first.

Corollary 1: By elimination of strictly dominated strategies, we can express any Lenient judge strategy as an ex ante probability of deportation e, where e is achieved by choosing a threshold grounds for deportation x. The judge deports any immigrant with $z > x$, and allows others to remain. Therefore, $e = 1 - F(x)$ and:

$$U(e) = eR(\mu_1^d) + (1-e)R(\mu_1^s) - C(e) \tag{2}$$

where $C(e)$ is the expected ideological cost from choosing e.

Assumption 2 ensures e is a continuous variable between zero and one that weakly increases as the threshold for deportation x becomes easier to satisfy.

Assumption 3 ensures that $C' \geq 0$, $C'' > 0$ and $C'(0) = 0$. Intuitively, when lowering the threshold x to increase e, the judge initially deports immigrants with substantial grounds for deportation, but gradually runs out of "low-hanging fruit" to deport. $C'(0) = 0$ because the judge feels no "guilt" from deporting an immigrant on the margin of being obviously deportable.

3.3 Harsh-type strategies

Observation 2: In an equilibrium with consistent beliefs, a Lenient judge will deport with a strictly lower probability than a Harsh judge. Suppose this were not the case: belief consistency implies that the government would not punish a judge for granting stay rather than deporting. Then, Lenient judges would deport with lowest possible probability, contradicting the initial supposition. Anticipating this in equilibrium, the government will never punish a judge after they deport.

Corollary 2: It is a dominant strategy for Harsh judges to deport in all cases where they have discretion. Deporting improves reputation while incurring no ideological cost. Therefore, $e_H^* = 1 - F(\underset{\sim}{z})$.

3.4 Belief revision

The government updates its beliefs about judge types according to Bayes' rule. In equilibrium, the government believes that $e_H = 1 - F(\underset{\sim}{z})$ and $e_L = \hat{e}$, where $\hat{e}$ is the equilibrium e_L we will solve for. Letting μ_0 be the government's prior belief:

$$\mu_1^d = \frac{\mu_0(1 - F(\underset{\sim}{z}))}{\mu_0(1 - F(\underset{\sim}{z})) + (1 - \mu_0)\hat{e}} \tag{3}$$

$$\mu_1^s = \frac{\mu_0 F(\underset{\sim}{z})}{\mu_0 F(\underset{\sim}{z}) + (1 - \mu_0)(1 - \hat{e})} \tag{4}$$

Since $C'' > 0$, differentiating $U(e)$ with respect to e yields a unique best response e^* to the government's beliefs.

$$R(\mu_1^d(\hat{e})) - R(\mu_1^s(\hat{e})) = C'(e^*) \tag{5}$$

Beliefs must be consistent in any Weak Perfect Bayesian Equilibrium: $e^* = \hat{e}$. Observe that the difference between μ_1^d and μ_1^s becomes smaller as $\hat{e}$ increases; intuitively, the more a Lenient judge acts like a Harsh judge, the less information the government gleans from case outcomes. This implies μ_1^d and μ_1^s are decreasing and increasing in $\hat{e}$ respectively. Using chain rule differentiation in combination with Assumption 1, the left-hand side of (5) must be decreasing in $\hat{e}$. Since $C'' > 0$, this implies there is a unique equilibrium where $e^* = \hat{e}$.

Observation 3: The LHS of (5) is strictly positive due to Assumption 1; since $C'(0) = 0$ and $C'' > 0$, this implies $e^* > 0$. In equilibrium, incongruent judges will sometimes rule against their ideology to pander to the government.

3.5 Comparative statics

3.5.1 Obvious cases

Let $a = F(\underline{z})$ be the probability a judge receives a case whose outcome is to obviously grant stay. Differentiating (3) and (4) with respect to a gives:

$$\frac{\partial \mu^d}{\partial a} = -\frac{\mu_0(1-\mu_0)(1-\hat{e})}{(\mu_0 a + (1-\mu_0)(1-\hat{e}))^2} < 0 \tag{6}$$

$$\frac{\partial \mu^s}{\partial a} = \frac{\mu_0(1-\mu_0)(\hat{e})}{(\mu_0(1-a) + (1-\mu_0)\hat{e})^2} > 0 \tag{7}$$

Substituting the above derivatives and Assumption 1 into (5), a Lenient judge's optimal e^* is decreasing in the likelihood they receive a case whose outcome is to obviously grant stay. Intuitively, when a is larger it is easier to disguise a decision against the government as one where the judge had no choice. Resultingly, incongruent judges have a lower incentive to signal as they incur less reputational damage from following their own ideology.

3.5.2 Prior reputation

The effect of prior reputation μ_0 is ambiguous and depends on R and the level of μ_0. Bar-Isaac (2003) provides some intuition for its effects. On one hand, at higher μ_0, the marginal returns to "investing" in reputation through pandering are lower. On the other, having a better initial reputation means that the judge has more to lose.

3.6 Empirical predictions

First, we have shown that judges with a different ideology to the government will try to signal their congruence by sometimes ruling in line with the government's ideology. Hence, in equilibrium, the frequency of deportations will depend on which government is in power. This provides the basis for our empirical investigation: we expect judges to change how frequently they deport immigrants when the government changes.

Second, we show that signalling will be weaker when the judge receives a large proportion of obvious cases, providing a testable empirical implication.

4 Empirical Strategy

4.1 Regression discontinuity in time using presidential elections

Having hypothesised that U.S. immigration judges respond to political pressure, I now seek to find evidence if this is actually the case. Section 3 demonstrates that strategically behaving immigration judges rule more frequently in favour of the government's ideology. Therefore, if judges do react to politics, we would expect changes in the government's immigration stance to affect immigration decisions. Presidential elections can achieve this variation by nominating new governments with different immigration stances.

However, estimating the effect of elections on immigration decisions can be problematic. A naive treatment regression with a postelection dummy is flawed due to unobserved variables correlated with time. For instance, the success of an anti-immigration party and an increase in deportation rates might both be due to an underlying rise in xenophobic sentiment. Because there is no cross-sectional variation in nationwide election treatment, it is difficult to control for time-confounding variables.

To address this, I use a regression discontinuity design in time (RDiT). This assumes that, for small windows (or "bandwidths") around election day, confounding variables are essentially unchanged. For example, immigration policy or xenophobic sentiment are unlikely to notably change over a month (especially the former, since the new president would not have been inaugurated). However, it is untenable to assume this resolves all time-confounding variables. Seasonal or day-of-week effects still exist, since election day is always on the Tuesday after the first Monday of November. Assuming seasonal and day-of-week trends are common between years, I include non-election years as a control group. Therefore, the identifying assumption is that, in a small window around election day, the parallel trends assumption is valid between election (treatment group) and non-election years (control group).

A basic regression, for case i occurring in year y on day t, would be:

$$D_{iyt} = f(t) + \zeta_1 \text{electionyear}_y + \zeta_2 \mathrm{T}_t + \zeta_3 \text{electionyear}_y \mathrm{T}_t + \varepsilon_{iyt} \tag{8}$$

where:

- $D = \mathbf{1}(\text{Deportation ordered})$
- $f(t)$: Functional form of the running variable, time. This is assumed to be equal between years for a small window, a.k.a parallel trends.
- $\mathrm{T} = \mathbf{1}(t > t_0)$ where t_0 is the Tuesday after the first Monday of November.

4.2 Developing the model

4.2.1 Linear probability model

I use a linear probability model to avoid issues associated with applying difference-in-difference frameworks to probit or logit models. In non-linear regressions, Ai and Norton (2003) show that the coefficient on the treatment regressor ($\text{electionyear}_y \text{T}_t$) is not necessarily representative of the actual treatment effect. Furthermore, coefficients in linear models are easier to interpret as they signify percentage point changes in deportation rates.

For transparency, I include the results of a probit regression in Table 3 of Section 6. Reassuringly, the coefficients of the variables of interest have the same sign as the linear model.

4.2.2 Year effects

I include year effects as we are interested in within-year variation. This also removes unobserved heterogeneity which can cause omitted variable bias; for instance, immigration decisions are probably fundamentally different in 2000 compared to 2020, despite both being election years.

4.2.3 Case covariates

Wooldridge (2016) shows that including covariates in the regression increases our confidence in the parallel trends assumption, by reducing the possibility of samples differing systematically before and after elections. In our instance, this can also control for sorting effects across the election threshold (see Section 4.3.5). More practically, Angrist and Pischke (2009) suggest including case-level covariates to increase the precision of estimates. I account for the following:

1. *Detention and criminal status*: Immigrants in detention or with a criminal charge have higher legal grounds for deportation, ceteris paribus.

2. *Legal representation*: Immigrants with lawyers may have better chances in court.

3. *Latin American*: Ryo (2019) finds that judges are likelier to rule against immigrants of Hispanic ethnicity.

4. *Chinese*: Chinese women are likelier to qualify for asylum due to Beijing's birth-control policies and a 1996 U.S. law.[9]

5. *Day-of-week effects*: These are important to control for as elections are always on a Tuesday – results may be biased if e.g. judges are "grumpier" on Mondays

6. *Judge fixed effects*: I am interested in within-judge variation and do not want estimates to be

[9] Rosenberg et al., (2017). "For U.S. asylum seekers, some judges are a better bet than others," *Reuters*, 17 October.

detecting one type of judge happening to rule more frequently postelection, particularly when evidence suggests different types of judges can rule disparately on similar cases (Ramji-Nogales et al., 2007).

4.2.4 Functional form of *f(t)*

Although non-election years control for the average time trend between pre- and postelection periods, a daily time trend is still required to identify a discontinuous change at election day. Correctly specifying $f(t)$ is important to avoid mistaking a polynomial function for a discontinuity (Angrist and Pischke, 2009).

At the same time, Gelman and Imbens (2014) argue that higher-order polynomials should not be used when specifying functional form. Firstly, results based on high order polynomials are sensitive to their order; yet, “we do not have good methods for choosing that order in a way that is optimal for a good estimator.” Secondly, they show that inference with higher-order polynomials is poor and can lead to over-optimistic results. To address this, they recommend “local linear regressions;” as the window used to select the sample narrows, the number of polynomial terms needed to model $f(t)$ should decrease. Eventually, $f(t) = \beta_0 + \beta_1 t$.

4.2.5 Separating Republican and Democratic victories

The model rests on the notion that different administrations have different immigration stances. At the very least, the regression should separate the effects of different party victories. Let DEM $= \mathbf{1}(t > t_0$ in a Democratic-winning year) and REP $= \mathbf{1}(t > t_0$ in a Republican-winning year).

Incorporating the above considerations, the **main specification** of the model can be expressed as:

$$D_{ijyt} = \beta_0 + \beta_1 t + \beta_2 \mathrm{DEM}_{yt} + \beta_3 \mathrm{REP}_{yt} + \alpha_y + \lambda_j + \gamma \mathrm{X}_{iyt} + \varepsilon_{ijyt} \tag{9}$$

where j is an index for judges, X is a vector of covariates, and α_y and λ_j represent year and judge effects.

4.3 Assumptions and concerns

4.3.1 Time-confounding variables

The identifying assumption is that there are parallel trends in immigration decisions between election and non-election years for a bandwidth around election day. As Imbens and Lemieux (2008) highlight, this assumption is more defensible for smaller bandwidths. Intuitively, suppose the bandwidth included observations after inauguration, when the new president enters office; such a design is likelier to be confounded by changing immigration policy, compared to one with a narrower bandwidth excluding post-inauguration observations.

However, as Geneletti et al. (2015) point out, there is a trade-off between precision and bias because smaller bandwidths reduce sample size. For robustness, the regression is repeated for bandwidths of 29, 15 and 7 days. In comparison, in a survey of RDiT studies, Hausman and Rapson (2017) find the typical researcher uses a bandwidth of two years. I can afford to be more conservative as my data are high-frequency.

For robustness, I run regressions with randomly-drawn placebo election dates (Table 2). I also check if observed covariates vary around election day, which can indicate time-confounding variables (Table 3). Reassuringly, placebo elections do not have a significant effect, and observed case characteristics do not significantly change after real elections.

4.3.2 Independence of guidelines

The threshold must be exogenous to immigration decisions, which is true for presidential elections as neither their date or outcome can be feasibly manipulated by an immigration judge.

4.3.3 Continuity of expected outcome variable

The predicted probability of deportation must be continuous in time. This holds if we fit a linear functional form $f(t)$ and derive the binary outcome from a latent, continuous "propensity to deport," which is naturally the case for both linear probability and probit models (Wooldridge, 2016). Corroborating this, van Leeuwen et al. (2018) show that regression discontinuity designs remain valid with dichotomous dependent variables.

4.3.4 Association of treatment with threshold

Treatment is a change in a judge's knowledge of the next president. This is clearly correlated with whether we are before or after an election. Although election results may not be immediately clear (e.g. in 2000) they still provide a discontinuous change in judges' beliefs regarding the future administration.

4.3.5 Sorting and anticipation effects

So far, we have ignored the notion that judges may expect and plan around election results. This gives rise to two pressing considerations.

Firstly, there may be endogenous sorting across the election threshold if judges schedule cases around elections. McCrary (2008) shows we can check the existence of this behaviour by running a density test (reported in Section 6.1.3) to see if there is bunching around election day. The regressions on observed covariates, in Table 3, are a further check for this behaviour.

Secondly, forward-thinking judges may already be altering their behaviour in the run-up to an election. For instance, if judges correctly anticipate a Harsh candidate to win, they may already be deporting at a higher rate before election day. However, such "pricing in" would imply my estimates indicate a lower bound of the actual effect.

On the other hand, it is plausible to assume that judges can never fully anticipate the winner of an election. This allows us to include election years where the same party is reelected. Before an election, suppose that immigration judges believe both candidates could win with strictly positive probability. This prevents them from fully pandering to one party or the other. Postelection, judges are now certain who the next president will be – this generates variation in incentives, even for elections resulting in second terms.

To remove anticipation effects, one could rerun the analysis with only close elections, where winners are ex ante similar to losers (Lee, 2001). However, this is unfeasible due to the small number of presidential elections observed in my data.

4.3.6 Autoregressive properties

It is important to consider the time series properties of a RDiT as misspecification can lead to bias. However, it is unlikely that immigration judge decisions have autoregressive properties. This is because (i) cases are randomly assigned to judges once they are received by an immigration court; and (ii) hearings are scheduled months to years in advance, which insulates them from influxes of immigrants with common characteristics. Unless a judge purposely schedules cases in patterns based on deportability, which is unlikely, these points imply case types are serially independent.

5 Data

5.1 Description

I use administrative data from the Executive Office of Immigration Review.[10] After cleaning, this accounts for 2.8 million immigration court cases from 1998 to 2021. The data include information on hearing dates, outcomes, judge, court and various case characteristics (including nationality, legal representation, criminal charges and detention status). I also rely on data assembled by Kristina Cooke, Reade Levinson and Mica Rosenberg on judges' appointing presidents and start dates.[11]

I focus on removal proceedings (see Section 2.1 for a description). There are other types of proceedings, but they are typically ancillary to removal proceedings, and concern specific matters (e.g. asylum relief or bond) for the same individual. Therefore, including these would imply data points are not independent.

There are three possible ways to conclude an immigration case: (1) the individual is deported; (2) the individual is allowed to remain; (3) the individual voluntarily departs the country. I exclude cases that result in voluntary departure since the judge's decision (which is what I am concerned with) is unobserved. However, this can result in endogenous sample selection if individuals that voluntarily left were likelier to have been deported. Respecting my empirical strategy, this would only threaten internal validity if elections affected voluntary departures. To check if this is the case, I regress voluntary departures on elections following the main specification in Section 4.2. The results are in Table 3: for a bandwidth of 29 days, I fail to find evidence that elections affect the frequency of voluntary departures.

[10] Available at: https://www.justice.gov/eoir/foia-library-0 (Accessed on 10/10/2021).

[11] Levinson et al., (2021). "How Trump administration left indelible mark on U.S. immigration courts", *Reuters*, 8 March.

5.2 Summary statistics

Table 1: Sample averages of key variables

Variable	Definition (all variables binary)	Full sample	Election years	Non-election years	1999-2010	2010-2021
dep	= 1 if case resulted in deportation	0.663	0.655	0.665	0.730	0.584
criminal	= 1 if case has criminal charge	0.210	0.210	0.210	0.258	0.153
detained	= 1 if case is detained	0.560	0.572	0.556	0.644	0.467
represented	= 1 if case has a lawyer	0.479	0.475	0.480	0.382	0.587
latinx	= 1 if case nationality is Latin American	0.644	0.641	0.645	0.632	0.662
chinese	= 1 if case nationality is Chinese	0.048	0.046	0.048	0.045	0.050
Y	Number of years in group	23	6	17	12	12
N/Y	Average observations per year	122182	108519	127005	134384	113043

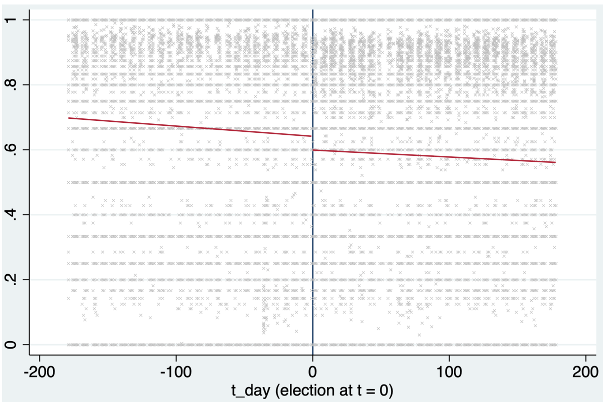


Figure 1: Democrat-winning years

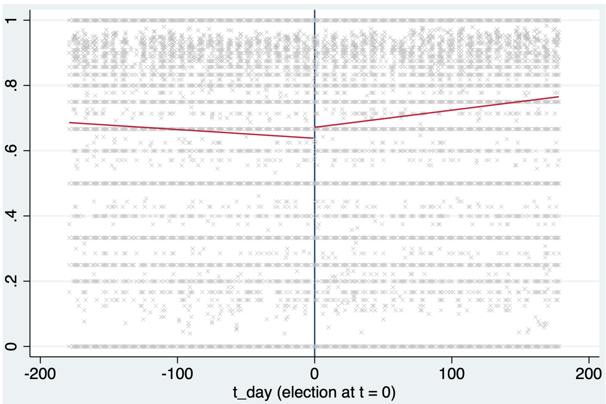


Figure 2: Republican-winning years

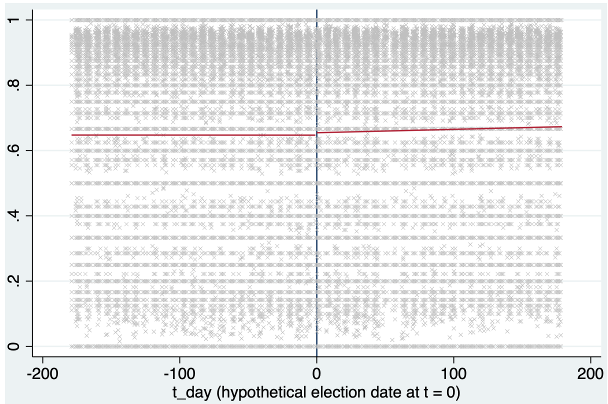


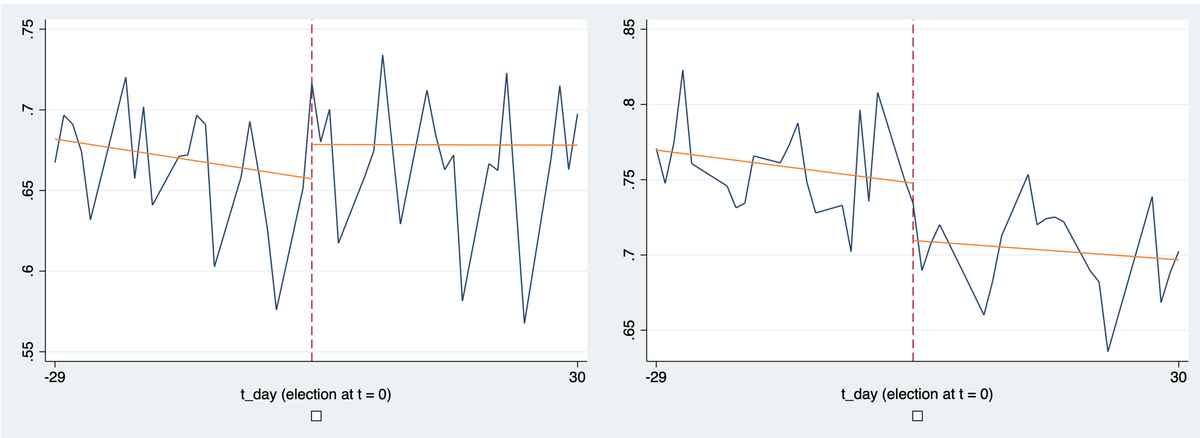
Figure 3: Non-election years

Table 1 shows the sample proportions of key variables. Election and non-election years are not noticeably different, providing evidence that there are no between-year sorting effects. Case outcomes and characteristics have evolved in the long run, justifying the inclusion of year effects.

Figures 1-3 fit linear trends to daily judge deportation rates (deportations over cases per judge per day) on either side of election day, for different groups of years. Firstly, the discontinuous gap between trends is much larger for election years than non-election years. Secondly, the gap is positive for Republican-winning years and negative for Democrat-winning ones. This anticipates our result that presidential elections affect deportation rates, and in opposite directions depending on winning party.

This suspicion is reinforced by Figure 4, which also shows discontinuous gaps in nationwide deportation rates for the 2004 and 2008 elections. These years are chosen as they display relatively less day-to-day volatility, and hence present the effect more clearly.

Figure 4: Deportation rates (nationwide, daily)



(a) Year 2004: Bush victory

(b) Year 2008: Obama victory

6 Results

Table 2: OLS Results

	Main			Placebo		
	(1)	(2)	(3)	(4)	(5)	(6)
REP	**0.034****	**0.029****	**0.020**	−0.003	−0.012	−0.014
	(0.012)	**(0.012)**	**(0.015)**	(0.008)	(0.007)	(0.011)
DEM	**−0.054*****	**−0.040****	**−0.041***	−0.003	0.010*	−0.011
	(0.017)	**(0.015)**	**(0.022)**	(0.004)	(0.005)	(0.013)
t_day	0.000	0.000	0.000	0.000	−0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
criminal	0.030***	0.032***	0.031***	0.033***	0.031***	0.027***
	(0.006)	(0.006)	(0.007)	(0.005)	(0.005)	(0.007)
detained	0.445***	0.452***	0.452***	0.457***	0.445***	0.437***
	(0.030)	(0.028)	(0.028)	(0.022)	(0.023)	(0.025)
represented	−0.133***	−0.132***	−0.128***	−0.141***	−0.145***	−0.146***
	(0.019)	(0.017)	(0.017)	(0.013)	(0.013)	(0.014)
latinx	0.136***	0.135***	0.141***	0.114***	0.120***	0.125***
	(0.027)	(0.027)	(0.029)	(0.019)	(0.021)	(0.022)
chinese	−0.070***	−0.073***	−0.078***	−0.089***	−0.069***	−0.073***
	(0.018)	(0.019)	(0.017)	(0.017)	(0.015)	(0.014)
N	233878	119952	55493	230023	213474	199402
Bandwidth (days)	29	15	7	29	29	29
Placebo month				April	June	September
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Robust LZ standard errors in parentheses (clustered by year). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Controls include judge and day-of-week effects.

Placebo election days are the Tuesday after the first Monday of a randomly-drawn placebo month.

Presidential elections have a significant effect on immigration decisions, corroborating the discontinuities identified in Figures 1-4. Taking an average of coefficients in Columns (1)-(3), Republican and Democratic victories change a judge's propensity to deport by +2.73 and -4.50 percentage points respectively.

The results are consistent with the conceptual framework in Section 3. After election, judges update their knowledge of who will be in power and start signalling their congruence to the president-elect's political ideology. This is in order to receive rewards such as career progression or discretionary power in the future. These findings indicate that even non-elected judges, without explicit electoral incentives, can respond to political pressure.

At all bandwidths, t-tests cannot reject the null that the coefficients on REP and DEM are of equal magnitude (p = 0.168, 0.283 and 0.192 for 29, 15 and 7 day bandwidths respectively), suggesting that both parties engender similar incentives to pander. In December 2021, there were 1.6 million pending immigration cases.[12] My estimates imply that, depending on which political party is in office, average deportation rates could swing by 7.23 pp. This represents 115,000 more or fewer individuals deported due to political pandering.

The results appear initially robust. Firstly, while the effect of Republican victories becomes insignificant at a 7-day bandwidth, the point estimates remain similar. Secondly, the coefficients on covariates make intuitive sense. Thirdly, all but one of the placebo coefficients are insignificant.

[12] Aguilera, (2022). "A Record-Breaking 1.6 Million People Are Now Mired in U.S. Immigration Court Backlogs", *Time*, 20 January.

6.1 Robustness checks

Table 3: Robustness checks

	Alternatives to OLS		Alternative outcome variables					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
REP	0.131**	0.033*	0.009	0.002	−0.006	0.003	−0.007	0.006
	(0.064)	(0.018)	(0.008)	(0.010)	(0.009)	(0.011)	(0.005)	(0.004)
DEM	−0.216**	−0.050**	0.013	0.013	−0.014**	−0.004	−0.001	−0.004
	(0.097)	(0.018)	(0.008)	(0.009)	(0.006)	(0.006)	(0.004)	(0.005)
t_day	0.000		−0.000	0.000	0.000	−0.000	0.000	−0.000*
	(0.001)		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
postelection		0.002						
		(0.007)						
N	233878	44	233878	233878	233878	233878	233878	295690
Regression	Probit	WLS						
Outcome			criminal	detained	represented	latinx	chinese	vol_dep
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	No	No	No	No	No	No	Yes

Standard errors in parentheses. Clustered by year apart from (2). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Bandwidth of 29 days used for all regressions.

6.1.1 Probit model

Column (1) reports the coefficients of a probit regression following the specification in Section 4.2. They corroborate the linear model by indicating that deportation rates increase after a Republican victory and decrease after a Democratic victory.

6.1.2 Standard errors

Since election treatment is assigned on a year level, I cluster by year using Liang and Zeger (1986) standard errors. Additionally, this accounts for heteroskedasticity inherent in linear probability models and within-year serial correlation (Arellano, 1987). However, while LZ standard errors rely on asymptotic theory, the sample only has 22 year clusters. Although Hansen (2007) and Cameron and Miller (2015) suggest that 20 clusters can be sufficient, this issue should not be brushed over. To address too few clusters, Donald and Lang (2007) recommend a WLS regression of group means weighted by group size.

Because the means will approximate a normal distribution, finite-sample properties apply and inference can be based on a t-distribution. The results are reported in Column (2) of Table 3; the effect of elections remains significant, indicating that LZ standard errors are not unacceptably biased.

6.1.3 Sorting effects

Columns (3)-(7) show that, apart from legal representation, case covariates are unaffected by elections. Firstly, this corroborates the absence of time-confounding variation in the sampled bandwidth. Secondly, it suggests judges do not sort across election thresholds. This is further supported by a density test proposed by Calonico et al. (2017), which fails to reject the null that there is no discontinuous bunching of observations around election day ($p = 0.226$).

The effect on legal representation is not too concerning because: (i) the effect is not detected for Republican victories; (ii) judges often do not know if an immigrant will secure a lawyer when they first schedule the case, making sorting based on representation unlikely; (iii) the coefficient sign suggests my estimates on the effect of Democratic victories are a lower bound.

6.1.4 Voluntary departures

Column (8) suggests voluntary departures are unaffected by elections. This indicates that their exclusion does not threaten the internal validity of my empirical strategy.

7 Heterogeneity in Incentives

The previous section offers robust evidence that U.S. presidential elections affect immigration decisions. To support the signalling model in Section 3 as a valid explanation for why, I examine whether the model's additional implication regarding when we might observe heterogeneity in incentives (Section 3.5) is also borne out in the data.

Specifically, the model predicts that an incongruent judge panders less if they are more likely to receive cases whose outcomes are "obviously" against the government's ideology. Suppose a Lenient (Democratic) government perceives a certain court to receive more "obvious deportations" which they have no discretion over. A Harsh judge in that court would incur less reputational damage from deporting, even for a case where they *do* have discretion, as it is easier to pass the decision off as one where they had no choice. This reduces their incentive to pander.

We are able to test this because we can roughly identify courts which receive many more obvious deportations than others. They are: (1) "processing centres" which handle a higher proportion of immigrants in detention or with criminal charges; (2) "border courts" within 100 miles of Mexico, which handle more immigrants who have recently crossed the border with higher grounds for deportation.[13] Processing centres and border courts have deportation rates of 92.9% and 80.4% respectively, compared to the sample-wide average of 66.3%.

Let dcourt $= \mathbf{1}$(Presiding court c meets definition (1) or (2)):

$$D_{ijcyt} = \delta_0 + \delta_1 t + \delta_2 \mathrm{DEM}_{yt} + \delta_3 \mathrm{dcourt}_c + \delta_4 \mathrm{DEM}_{yt}\mathrm{dcourt}_c + \alpha_y + \lambda_j + \gamma \mathrm{X} + \varepsilon_{ijcyt} \tag{10}$$

The marginal effect of Democratic victories is given by $\delta_2 + \delta_4 \mathrm{dcourt}_c$. Section 3.5 predicts $\delta_2 < 0$ while $\delta_4 > 0$. Importantly, I **exclude cases in detention or with criminal charges**. Otherwise, we may simply be detecting the mechanical effect that judges in these courts respond less because they have less discretion – we are instead interested in whether judges *choose* to signal less in cases where they do have discretion.

[13] Unfortunately, it is not possible to identify courts which receive many "obvious grants of stay" in the same way. Hence, I cannot conduct the analysis for Republican victories.

Table 4: Heterogeneity in Incentives

	Processing centres			Border courts		
	(1)	(2)	(3)	(4)	(5)	(6)
DEM	-0.043^{*}	-0.028	-0.046^{*}	-0.042^{*}	-0.032	-0.039^{*}
	(0.021)	(0.024)	(0.026)	(0.024)	(0.036)	(0.022)
dcourt	0.135^{***}	0.096^{*}	0.125	-0.028	-0.012	0.075
	(0.039)	(0.050)	(0.092)	(0.046)	(0.057)	(0.066)
DEM_dcourt	0.034^{*}	0.064	0.053	0.055^{**}	0.040	0.051
	(0.017)	(0.041)	(0.081)	(0.022)	(0.046)	(0.044)
N	89079	45657	21228	89079	45657	21228
Bandwidth (days)	29	15	7	29	15	7
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

LZ standard errors in parentheses (clustered by year). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Sample excludes cases in detention or with criminal charges.

For a bandwidth of 29 days, both types of regression yield significant results consistent with the theory. Although we lose significance at shorter bandwidths – possibly due to the smaller samples – the results still support the notion that judges who can easily disguise a decision against the government as one where they had no discretion will have less of an incentive to pander. From Columns (1) and (4), t-tests cannot reject the null that $\delta_2 + \delta_4 = 0$ ($p = 0.370$ and 0.345 respectively), suggesting that judges in these courts do not respond to political incentives at all. That this additional prediction from the signalling model is observed in the data further corroborates our hypothesis that immigration judges respond to presidential elections due to *strategic* considerations, in particular to signal congruence with the government.

8 Conclusion

This dissertation provides novel evidence of U.S. immigration judges pandering to the government. Using a regression discontinuity design, I estimate that the average propensity to deport an immigrant increases by 2.73 pp after a Republican-winning election and decreases by 4.50 pp after a Democratic-winning one. If these effects are persistent, an individual's probability of being deported can swing by 7.23 pp depending on which party is in office. Given there are currently 1.6 million pending immigration cases, this represents 115,000 decisions in the near future, with potential life-or-death consequences, caused by political pandering. An additional insight – that judges are less likely to pander when they can disguise decisions as "obvious" choices – further supports the prediction in Section 3 that immigration judges strategically respond to political pressure.

What I find is valid for other non-elected judges. However, bear in mind that: (i) my econometric model, which samples a window no greater than a month around election day, cannot say much about the persistence of election effects; (ii) the magnitude of political incentives may be particularly amplified in immigration courts due to their institutional structure. Nonetheless, if we are observing such effects in U.S. courts, they are likely to also be prevalent in developing countries with fewer checks and balances. Moreover, the nuanced nature of my findings – that judge propensities to deport change in opposite directions depending on winning party, along with observed heterogeneity in effects – makes it harder to offer alternative mechanisms not rooted in strategic behaviour.

The evidence this dissertation provides suggests that, when considering institutional design, we should view judges (elected or not) through the lens of political economy models. In contrast to Maskin and Tirole's deliberate distinction between *The Politician and the Judge*, perhaps the two are not so different after all.

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