

Hope v. Deportation in Immigration Detention

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Abstract

People in immigration detention confront a choice: they can give up and be deported or fight on and, if they win, be released within the United States. I study a judicial decision that made bond hearings available after six months of detention in some places but not others. I find that the hope of release on bond made detainees more likely to fight their cases for at least six months. Once released, those who endured were likely to appear in immigration court and win their cases. This is the first causal evidence that immigration detention leads people to give up on cases that they might win outside detention. That matters given recent policy changes that increase immigration detention capacity and end access to bond hearings.

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1 Introduction

Congress recently funded the doubling of immigration detention capacity to 116,000 beds per day, with a budget that is 62% larger than the budget for the federal prison system [American Immigration Council, 2025]. Does immigration detention, and the barring of opportunities for release, lead people to give up on their cases? I find that the promise of a bond hearing after six months of detention—and therefore the possibility of release—convinces people to fight their cases until that hearing.¹ Among those who do persist and are released, success rates are high: only 9% fail to appear in immigration court and only one third of those who do appear are ordered deported within ten years.

These findings confirm the accounts of journalists and immigration lawyers, who often report that immigration detention causes people to give up on winnable cases.² Recent work suggests that this attrition has become an important part of the second Trump administration’s mass deportation program [Blair and Hausman, 2026]. But no quantitative study has tested whether detention has this effect. I find that it does, and that people endure additional detention not in order to abscond but instead to contest their cases, which they win most of the time.

The absence of studies on the way detention causes attrition reflects two barriers to inference.

First, before 2023, Immigration and Customs Enforcement (ICE) did not release the necessary data: ICE detention data did not include information about whether a person was eventually deported after release from detention. I collaborated on litigation with the American Civil Liberties Union to obtain this information under the Freedom of Information Act (FOIA), requiring ICE to connect records with anonymized individual identifiers.³ This

¹I am grateful to three anonymous reviewers (and an editor) for pointing me in this direction.

²A few recent headlines convey this: “The Cruel Conditions of ICE’s Mojave Desert Detention Center: How immigration authorities have weaponized medical neglect to encourage self-deportations” [Peleg, 2026]; “ICE is pressuring people in custody to self-deport. Many are giving up.” [Morrissey, 2025]; “As Trump misses deportation goals, ICE pushes migrants to give up their cases” [Hesson and Cooke, 2025].

³ACLU v. ICE, 58 F.4th 643 (2d Cir. 2023).

ICE dataset is now the foundation for reporting on immigration enforcement trends under the current administration, but it has not yet been used in academic research.⁴ Second, even with the necessary data, selection bias hampers any study of detention’s effect: people who are eligible for release are systematically different from those who are not, and those differences might themselves make people more or less likely to continue fighting their cases. For example, people who have criminal convictions are both less likely to be eligible for bond and more likely to lose their cases.

Using this new dataset, I address selection bias by studying the effect of a 2013 judicial decision, in a class action lawsuit called *Jennings v. Rodriguez*, which newly required immigration courts to conduct bond hearings for certain categories of people who had spent at least six months in detention. This decision applied in the Ninth Circuit (California, Arizona, Oregon, Nevada, Washington, Idaho, Montana, Alaska, and Hawai‘i) but not elsewhere. I use a difference-in-differences approach to study the decision’s effect. I compare the chance of remaining detained longer than six months in the Ninth Circuit (where those who remained detained six months had a chance of release on bond) to the chance of remaining detained so long in other parts of the United States (where those who remained detained six months had no access to bond hearings).

Using this research design, I find that the hope of release from detention causes people to hold on and fight their cases. After the 2013 *Rodriguez* decision, the chance of remaining detained over six months went up by more than four percentage points, which meant that it almost doubled. The effect was larger (in absolute terms) for people who had already spent several months in detention. Among people already detained at least three months, the chance of remaining detained over six months went up by over twenty percentage points. And the effect was driven by people without criminal convictions, suggesting that those with the best chance of being released and winning their cases were most likely to hold on through more detention.

⁴I now pursue frequent updates to these datasets through the Deportation Data Project. This and other related datasets are available at deportationdata.org.

I also investigate the mechanisms driving the effect. People did not endure more detention in order to be released and then abscond. Among those released after six months of detention in the Ninth Circuit, the failure-to-appear rate in immigration court was 9%, and among those who did appear, a third were ordered deported within the next ten years. By making bond hearings more available, *Rodriguez* increased the release rate—and the loss rate fell at the same time. I cannot, however, conclude that releases prevented deportations because the increasing release and win rates likely also reflect the changing composition of the population detained at least six months in the Ninth Circuit. Because *Rodriguez* caused people—especially those who were more likely to win their cases—to hold on longer, the group of people detained six months in the Ninth Circuit also included more people likely to win their cases.

Understanding the effect of bond hearings matters for policy in 2026 because the U.S. government recently eliminated access to bond hearings for many detainees.⁵ As this new no-release policy took effect, the deportation rate increased as well. The results of this study suggest a likely reason: the no-release policy caused people to give up their cases and accept deportation.

Beyond the policy implications, these findings advance two academic literatures. First, there is, to my knowledge, no previous causal research on the effect of immigration detention or the prospect of release from it, despite a large literature on other aspects of immigration detention. Detention grew 500% between 1994 and 2017 [Ryo and Peacock, 2018]; detention capacity is now poised to double again [American Immigration Council, 2025]. Scholars have studied the harms associated with immigration detention, including mental health [von Werthern et al., 2018], physical health [Tovino, 2016, Saadi et al., 2022, Patler et al., 2021], and financial [Patler, 2015] harms, as well as harms to family members [Patler and Gonzalez, 2023, Gonzalez and Patler, 2021]. Legal scholars have questioned the normative foundations

⁵Specifically, the Board of Immigration Appeals held that noncitizens who entered the country without inspection are not eligible for bond hearings before an immigration judge. See *Matter of Hurtado*, 29 I&N Dec. 216 (BIA 2025). Although the policy has been challenged and many noncitizens have obtained bond hearings via habeas petitions, it has nonetheless had widespread effect.

of immigration detention [García Hernández, 2017, Kalhan, 2010, Marouf, 2016]. And scholars have studied the decisionmaking processes that determine who is detained, including detention determinations by ICE officers [Nofferi and Koulish, 2014] and immigration judges [Ryo, 2016, 2018, 2019a]. Finally, several scholars have directly discussed the possibility that detention causes people to give up on their cases [Novy, 2019, Eagly and Shafer, 2024], including through mechanisms like stipulated removal [Koh, 2017, Stanford Law School]. I build on this body of work with a first estimate of the causal effect of the prospect of release from immigration detention.

Second, estimating detention’s effect on immigrants’ decisions to keep fighting their cases adds to a large literature on the determinants of immigration court outcomes. Most of this literature is outside of economics, with the notable exception of Ash and Nix [2023], who, like this study, consider noncitizens’ strategic responses to immigration court conditions—but in their case, on the nondetained immigration court docket. Overall, the literature on immigration court outcomes suggests that those outcomes frequently depend on legally irrelevant factors, such as the identity of the immigration judge [Ramji-Nogales et al., 2007, Hausman, 2016]. Descriptive work suggests that one such legally irrelevant factor is detention. Immigration court proceedings are fundamentally different for people who are detained and for those who are not [Eagly and Shafer, 2024, Ryo, 2019b]. Noncitizens fighting deportation from behind bars are less likely to have lawyers, to apply for relief from removal, and to win their cases [Eagly and Shafer, 2024, 2015]. I find a large effect of another factor that is legally irrelevant to the chance of winning a case: the hope of release.

2 Background

In *Rodriguez v. Jennings*, a class of immigration detainees sued the government in federal court in the Central District of California, seeking a right to a bond hearing (and thereby

possible release while fighting their deportation) after six months of detention.⁶ (Appendix Section A contains additional legal background.) In April 2013, they won their case in the Ninth Circuit Court of Appeals, which affirmed the district court’s injunction.⁷

That Ninth Circuit decision required a bond hearing after six months of detention (for people who were otherwise subject to mandatory detention and lacked a final order of removal) in California, Arizona, Nevada, Oregon, Washington, and Hawai‘i. Although similar litigation occurred elsewhere, no similarly broad court order took effect before the end of October 2015.⁸ The results here therefore rely on the comparison between the Ninth Circuit and other circuits between 2012 (the start of the data) and October 2015.⁹ Conversations with attorneys involved in the case suggest that many immigration courts only began complying with the decision after several months, so I expect the decision to take effect throughout the second half of 2013.

The hypothesis that detention causes people to give up and accept deportation is not new. Most obviously, being imprisoned causes a wide range of economic, physical, and mental harms. The unpleasantness of detention helps explain why the prospect of release might motivate a person to hold on. But the harms of detention tell only one part of the story; if holding on merely delays deportation, then we might not expect a large effect. Beyond ending the harms of detention, the prospect of future release might cause people to endure additional detention because release makes winning more likely.

⁶As disclosed elsewhere, I worked on this case, and on related detention litigation, as an attorney and a consulting expert.

⁷See *Rodriguez v. Robbins*, 715 F.3d 1127 (2013). Note that this first Ninth Circuit decision did not encompass people who had received an initial bond hearing but nonetheless remained detained six months; in October 2015, after the study period, the Ninth Circuit affirmed a permanent injunction including these individuals. *Rodriguez v. Robbins*, 804 F.3d 1060 (9th Cir. 2015). Because there is no way to identify individuals who had initial unsuccessful bond hearings in the ICE data, this study includes some people detained at least six months who did not in fact become eligible for bond hearings and therefore underestimates the effect on the release rate for the eligible population. Because the September 2012 district court decision was issued soon after the beginning of the dataset, I do not study the Central District of California.

⁸See *Lora v. Shanahan*, 804 F.3d 601 (2015).

⁹After October 2015, a brief period followed in which other circuits adopted similar decisions, but ultimately the Supreme Court reversed the *Rodriguez* decision in February 2018. *Jennings v. Rodriguez*, 583 U.S. 281 (2018).

Both of these possibilities suggest that release itself may prevent deportation—something that this study’s results imply but do not demonstrate. Release might prevent deportation in one of two ways. First, release could increase failures to appear. When noncitizens are detained, they have no choice but to appear in immigration court, and if they receive a deportation order, ICE typically deports them. In nondetained proceedings, by contrast, noncitizens may fail to appear in immigration court, and if they are ordered deported, a deportation order may or may not result in the mailing of a notice, known as a bag and baggage letter, instructing the noncitizen to appear at a particular place and time for deportation. If the noncitizen does not appear, an arrest may or may not follow. I ultimately find only limited evidence for this mechanism.

Second, release could increase the chance of winning in immigration court. Being released from immigration detention means that a case moves from the detained to the nondetained docket. Procedure differs between the two dockets in ways that make detained cases harder to win. Detained cases are expedited and therefore completed far faster than detained cases.¹⁰ Once on the nondetained docket, noncitizens can change the venue of their case to the immigration court closest to their home, which may make litigation easier and facilitate access to a friendlier immigration judge. Litigation is more difficult within detention than outside. Meetings with lawyers and family members are limited behind bars. Raising funds to hire a lawyer is more difficult because detainees cannot work. And ICE sometimes transfers people to detention centers far from their homes [Pon, 2019].

The first of these possibilities—that release might increase failures to appear—is consistent with detention’s formal purpose of reducing flight risk.¹¹ The second possibility—that release makes winning in immigration court more likely—implies that detention drives immigration court outcomes at the expense of the merits. Either way, I expect noncitizens’ anticipation of the effect of detention on their likelihood of removal to influence their decisions

¹⁰In 2022, the median length of a detained case was 31 days, whereas the median length of a nondetained case was 876 days. [Eagly and Shafer, 2024, 757].

¹¹See *Zadvydas v. Davis*, 533 U.S. 678 (2001). The other lawful purpose of detention is to prevent danger to the community.

about how much detention to endure.

3 Data

The ICE dataset comes from a Freedom of Information Act (FOIA) request (2023-ICFO-42034). This dataset was obtained following litigation (in which I played a role) in *ACLU v. ICE*, 58 F.4th 643 (2d Cir. 2023), in which the Second Circuit required ICE to substitute anonymized unique identifiers for private alien file numbers when producing electronic data under the FOIA. These unique IDs are critical to the analysis here because they allow the tracking of individuals across detention stays and the linking of that information with records of deportations.

In the ICE detention dataset, a row reflects an individual’s stint in a given detention center, defined by book-in date and book-out date. The dataset contains stints that begin or end between January 1, 2012 and November 15, 2023 ($N = 8,415,246^{12}$). Because transfers between detention centers are common, an individual’s *stay* in detention—a continuous period in which the person is not released—may encompass multiple stints. The dataset contains 3,960,463 stays. Many individuals have more than one stay in detention during this period; they may be released and redetained, or they may be deported, reenter the country, and be redetained. The dataset contains 3,193,375 individuals.

In the individual-level analysis in this study, I consider only the first detention stay for each individual, because subsequent detention stays may reflect the effect of being released (or not) from the first stay, and I only consider stays that started between January 2012 and April 2015, yielding a dataset with 1,079,091 rows. When I further drop any detention stay shorter than 180 days or with a final order of removal issued before the month of the end of the stay, as well as any stay in Massachusetts or the Central District of California, I am left with a dataset with 504,671 observations. For the analysis of cases where people

¹²This is the total after dropping a small number of observations duplicated across files produced by ICE, as well as a few missing unique identifiers and information about the location of a detention center. The Appendix provides more details.

spent at least six months in detention, I am left with 14,260 cases; I call this the Prolonged Detention Subset. The Appendix includes more details on sample restrictions.

In order to study immigration court outcomes for the Prolonged Detention Subset, I use publicly available data from those courts. The so-called CASE dataset is posted monthly by the Executive Office for Immigration review at <https://www.justice.gov/eoir/foia-library-0>; I use the version posted in November 2025. This dataset contains detailed information on immigration court outcomes. Because both the ICE and the immigration court datasets are anonymized, I am unable to match them at the individual level. The same sample restrictions yield a dataset of 16,708 immigration court cases.

Both datasets have limitations. The ICE data reliably tracks detention book-ins and book-outs but has some measurement error in its recorded reasons for release; the immigration court data tracks court outcomes reliably but has some measurement error in its information on detention. See Appendix for more details. Because of this measurement error, the immigration court dataset is most useful for the Prolonged Detention Subset. But even for that subset, the estimates from the two datasets draw on slightly different populations: the Prolonged Detention Subset from the immigration court dataset contains more observations than the equivalent sample in the ICE data, likely because of imprecise detention tracking, and therefore overinclusiveness, in the immigration court dataset. Reassuringly, however, overall results in the Prolonged Detention Subset are consistent across the two datasets.

3.1 Descriptive Statistics

Basic descriptive statistics help show how detention unfolds over time. Most stays in detention are brief;¹³ half are over in less than 21 days (the median length), and about a quarter don't last a full day (i.e., the person is booked in and booked out the same day). The 95th percentile is 155 days; about 4% of individuals are detained more than 180 days. Appendix

¹³These descriptive statistics describe the population at issue here, which excludes people with final orders of removal, whose cases are completed faster, on average.

Figure 5 shows this distribution. The skew follows from the fact that most detainees are either promptly released (especially those who are booked out the same day they are booked in) or deported within a few weeks.

Detention and deportation are highly correlated. Table 1 shows that, in the detention population as a whole (see top panel of Table 1), not being released from detention makes deportation highly likely: 91% of people who remain detained until a final decision are deported. By contrast, people who are released from detention have only a 14% chance of deportation within the next eight years.¹⁴ Because demographic information is sparser still in the immigration court data, I do not show descriptive statistics from immigration court here, but the overall pattern for shorter and longer detention stays is similar (see Appendix Figure 13.)

Detainees who hold on until six months in detention make up a small and unrepresentative sample of the full set of detainees. The bottom panel of Table 1 shows this. Being detained until a final decision is still a strong predictor of deportation (or voluntary departure¹⁵) for people detained over six months, but it is noticeably less strong than in the full population. People detained over six months are more likely to avoid deportation if not released before a final decision (about a 74% chance of deportation, compared to the 91% chance for people detained for shorter periods). Those who are willing to endure such a long period in detention may have more expectation of eventually winning their case.

These descriptive statistics show that endurance in detention is correlated with better case outcomes, but they do not tell us whether the hope of release increases detainees' endurance and leads to better outcomes. *Rodriguez*, by introducing a new hope of release, offers a way to study that effect.

¹⁴Note that this relationship might be slightly overstated by a limitation of the ICE detention data: it is possible that ICE miscategorizes a small number of wins as pre-decision releases from detention. For more discussion of this limitation in the ICE data, see the Appendix.

¹⁵Voluntary departure is formally a form of relief from removal, but it does mean being forced to leave the United States.

Table 1: Descriptive Statistics (ICE Data)

	Length (Days)	Deported	Male	Border	Hispanic	Convicted	<i>N</i>
<i>All Detainees Without a Final Order of Removal</i>							
Not Released	52.55	0.91	0.93	0.37	0.64	0.66	241,275
Released	31.91	0.14	0.69	0.29	0.37	0.35	263,396
Total	41.78	0.51	0.81	0.33	0.50	0.50	504,671
<i>Prolonged Detention Subset</i>							
Not Released	365.41	0.74	0.93	0.15	0.57	0.82	8,920
Released	316.82	0.15	0.87	0.39	0.29	0.50	5,340
Total	347.22	0.52	0.91	0.24	0.47	0.70	14,260

3.2 Empirical Strategy

I use a simple difference-in-differences approach to measure the effect of the *Rodriguez* decision on the likelihood of remaining detained over six months. There are two periods (before and after the *Rodriguez* decision) and two groups (those in the Ninth Circuit and those elsewhere).¹⁶ Beginning in April 2013, when the *Rodriguez* decision was released, those in the Ninth Circuit are treated; those elsewhere remain untreated.¹⁷

The final dataset includes one observation for each person. I index each observation by the first month of that person’s detention. I consider a person treated—that is, whose chance of reaching six months in detention was possibly affected by *Rodriguez*—if that person’s first month in detention occurred six months before the *Rodriguez* decision or later.

¹⁶More precisely, I categorize people as affected by the *Rodriguez* decision if (1) they finished their detention in a detention center located in the Ninth Circuit and (2) had not received a final order of removal as of that month. I omit people with preexisting removal orders (both in the Ninth Circuit and elsewhere) because *Rodriguez* affected arriving noncitizens (detained under 8 U.S.C. §1225(b)) and noncitizens convicted of certain crimes (detained under 8 U.S.C. §1226(c)), but not noncitizens detained with a final order of removal, under 8 U.S.C. §1231(a).

¹⁷The *Rodriguez* litigation began in the Central District of California (one district in the Ninth Circuit), and people detained over six months in that district began receiving bond hearings several months before people in the same position in other parts of the Ninth Circuit. I therefore omit the detention centers and immigration courts in the Central District of California from the analysis. I do not analyze the effect of the Central District decision separately because that decision followed an intensive effort by class counsel that may have resulted in individual releases before the Central District ruled and because there is very little data preceding that decision. I also omit detainees in Massachusetts detention centers, who might have benefited from the ruling in *Reid v. Donelan*, 22 F. Supp. 3d 84 (D. Mass. 2014), limiting prolonged detention.

The familiar key assumption underlying the difference-in-differences approach is that the treatment and control groups would have continued to experience the same trend absent the treatment. That assumption is, of course, untestable, but we can gauge its plausibility by examining whether trends in the two groups were parallel in the pre-period. Figures 1 and 2 offer a gauge of the parallel trends assumption’s plausibility. Pretreatment trends are relatively parallel; although there are some statistically significant deviations from zero in the event studies, they do not suggest a persistent pretreatment trend (more discussion below).

The empirical strategy also requires an assumption that detainees did not anticipate the Ninth Circuit decision; given that its timing and content were uncertain, this assumption is plausible.

One might also be concerned that the results reflect changes in composition across circuits rather than the effect of *Rodriguez*; in Appendix Figure 12, I plot demographic covariates across this period. These do not suggest a systematic change in composition around the time of *Rodriguez*. The plots do, however, help show descriptive changes across the study period; most notable, there was a compositional shift about a year after the *Rodriguez* decision, after a large increase in arrivals of families and children seeking asylum at the border. Robustness checks, including the replication of results in an exact-matched sample, further address these possible concerns.

4 Results: Enduring Six Months’ Detention

Rodriguez caused more detainees to fight their cases through six months of detention. The effect, a four percentage point increase in the percentage of people who reach six months in detention, suggests that *Rodriguez* roughly doubled the chance of enduring detention of that length. The size of the effect varies conditional on the number of months already spent in detention. During the first two months in detention, the hope of release at six months has

a small (though still proportionally large) effect on remaining detained that long. But once a detainee has spent several months in detention, six months no longer seems remote—and release in the interim may seem less likely—so hope of release has a much larger effect. And people without criminal convictions, who are more likely both to be granted bond and to win their cases, respond more to the hope of release from *Rodriguez*. Together, these results suggest that the hope of release prevents detainees from giving up their cases.

The main result is visible in a simple time series. The top left panel of Figure 1 shows the proportion, over time, of detainees who spent over six months in detention. The gray line shows the proportion in the Ninth Circuit; the black line shows the proportion in all other circuits. Before *Rodriguez*, the lines are parallel; after *Rodriguez*, they diverge. The divergence occurs after a delay, since only a small proportion of those whose cases started five months before *Rodriguez*—the date marked by the vertical dashed line—reached five months in detention and were therefore potentially affected by the decision. Although the gap is small, it is proportionally large. The percentage of people remaining detained six months roughly doubles in the Ninth Circuit.

The top right panel of Figure 1 shows the same set of trends, but only including people who already endured three months of detention. The effect of the hope of release is much more obvious, increasing by close to twenty percentage points. At the same time, that effect is proportionally smaller because it starts from a lower base. That makes sense, since part of the effect may already have occurred: *Rodriguez* might have affected people’s choices about whether to endure the first three months of detention, just as it clearly affects choices about enduring another three months of detention at that point. (In the Appendix, I reproduce these figures conditional on one, two, three, four, and five months of detention.)

Another way to show the same pattern is to plot a Kaplan-Meier survival curve; I show that plot in Appendix Figure 6. As expected, the survival curves for the treatment and control groups begin to diverge after a month of detention and converge again after seven or eight months of detention.

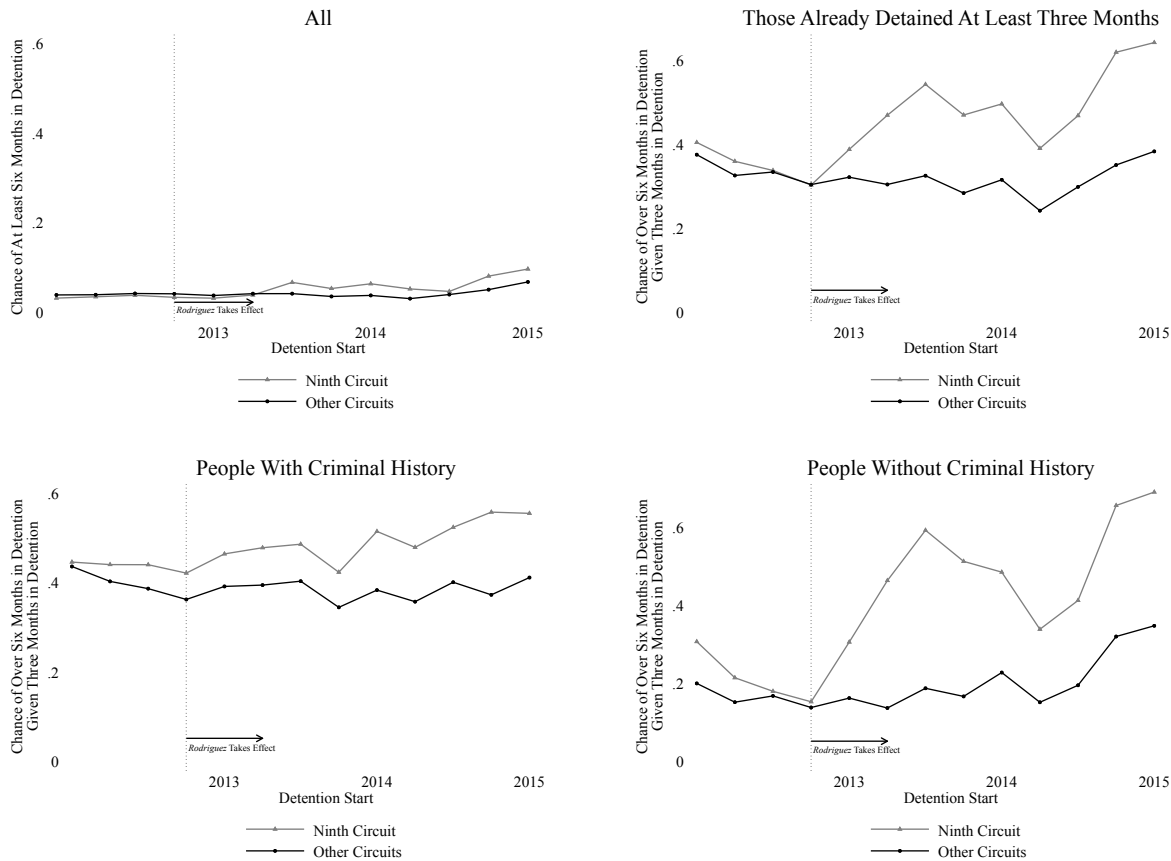


Figure 1: Effect of *Rodriguez* on Enduring Six Months of Detention. As the *Rodriguez* decision took effect in the Ninth Circuit, the chance of remaining detained at least six months increased. The top left panel shows the effect in the full population of detainees (excluding detainees with a final order of removal when booked in to detention). The top right panel shows the same pattern, but only includes people who were detained at least three months. The bottom left panel shows only slight evidence of an effect of *Rodriguez* for people with criminal convictions; the bottom right panel shows a large effect for people without criminal convictions. Both panels exclude detainees with a final order of removal when booked in to detention. Because criminal history makes release on bond and eventual victory less likely, *Rodriguez* likely produced a more realistic hope of release and victory for people without convictions and therefore had a larger effect on their behavior. Note that because the horizontal axis (in both panels) shows each individual’s first month of detention, the decision has a delayed effect by construction: many of the people who might have been affected by the decision in the initial months were released or deported before it was announced.

I expect that people with a stronger hope of release—and of eventual victory after release—will be more affected by the *Rodriguez* decision. The bottom row of Figure 1 compares the effect of *Rodriguez* for people with criminal convictions (left panel) with its

effect for people without such convictions (right panel). The existence of criminal convictions is a crude proxy for the strength of the hope of release and eventual victory: people without criminal convictions are more likely to be released on bond (because an immigration judge is less likely to conclude that they pose a danger to the community) and more likely to win their cases (because criminal convictions make certain forms of relief less likely).

People without criminal convictions were much more likely than those with such convictions to be affected by *Rodriguez*. The bottom row of Figure 1 shows this comparison. Among those with convictions, *Rodriguez* only slightly changes the proportion who endured six months of detention, whereas among those without convictions, the chance of enduring six months' detention jumps dramatically, more than doubling. (The difference in starting levels likely reflects the fact that people without convictions were less likely to make it to three months of detention, most likely because many were eligible for bond early in their proceedings.)

These conclusions from simple time-series figures also hold in difference-in-differences and event study regressions that include month and circuit fixed effects. (In the Appendix, I show results with and without control variables, which have little effect on estimated effects but do increase precision.) I estimate linear probability models of the form:

$$(1) \quad D_i = \beta Z_i + \Delta X_i + \Gamma_c + \Lambda_m + \epsilon_i$$

In equation (1), D_i is an indicator variable for whether individual i was detained longer than six months, Z_i is an indicator variable for presence in the Ninth Circuit and potential exposure to *Rodriguez*, X_i is a set of individual-level control variables (including sex, marital status, age, lawful permanent residence, Hispanic ethnicity, and whether the case began in expedited removal¹⁸), Γ represents circuit (c) fixed effects, and Λ represents month (m) fixed

¹⁸Expedited removal is a fasttrack removal procedure that, at the time, applied only to people apprehended at or near the border.

effects, where the month is the calendar month in which the individual entered detention. Standard errors are clustered at the circuit level, but because there are only eleven clusters, I also show wild-bootstrapped standard errors [Cameron et al., 2008, Roodman et al., 2019]. The event study regressions use the same specification, except that they add an interaction between the calendar quarter and Z_i .

I estimate the same models across six overlapping populations: first all individuals and then those detained at least one month, two months, three months, four months, or five months. Table 2 shows regression results.

Figure 2 shows event study plots for the same groups shown in Figure 1 above. The plots help evaluate the plausibility of the parallel trends assumption. Pre-treatment trends are quite parallel for the full population, both unconditionally and conditional on three months of detention. For people with and without criminal history, there are moderate pre-treatment trends in opposite directions. Given the scale of the pre- and post-treatment trends for people without criminal convictions, that effect seems credible, although the pre-treatment trend might introduce uncertainty about the estimate of the magnitude. For people with criminal convictions, the evidence of an effect conditional on three months of detention is much more tenuous. Appendix Figures 14-19 show all event studies across months and criminal conviction status. Overall, the effect of *Rodriguez* on detention endurance is robust.

Figure 3 offers a summary of these results across months and criminal history. Figure 3 shows that *Rodriguez* had the largest effect in the population of people already detained at least three months, with smaller effects in the full population and in the populations of people detained at least four or five months. This inverted-U shape makes sense. For people detained only a short period, the possibility of eventual release after six months of detention may be less salient, and for people detained four or five months, the chance of remaining detained six months is already high, regardless of *Rodriguez*.

In addition to the larger effect for people already several months into a period in detention, Figure 3 shows that the effect of *Rodriguez* on detention endurance is especially an effect

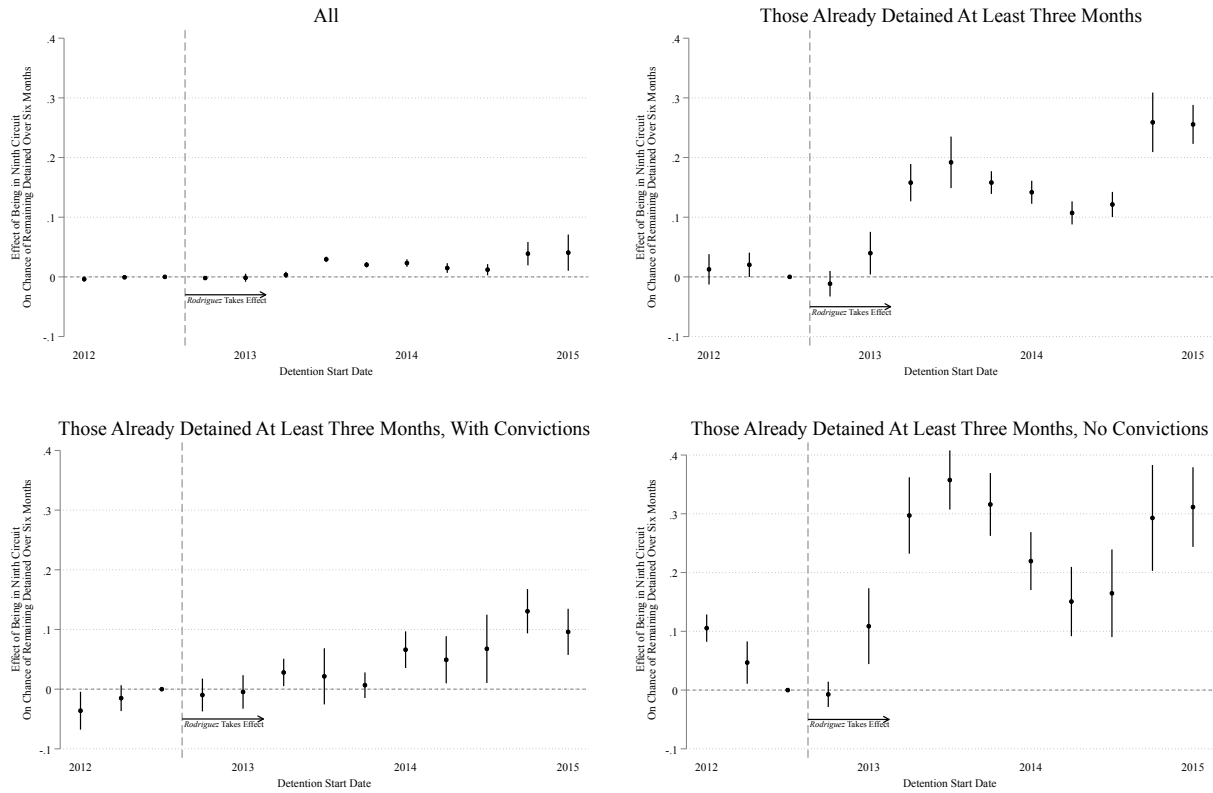


Figure 2: Event Studies: Effect of *Rodriguez* on Enduring Six Months of Detention. As the *Rodriguez* decision took effect in the Ninth Circuit, the chance of remaining detained at least six months increased. The top left panel shows the effect in the full population of detainees (excluding detainees with a final order of removal when booked in to detention). The top right panel shows the same pattern, but only includes people who were detained at least three months. The bottom left panel shows only slight evidence of an effect of *Rodriguez* for people with criminal convictions; the bottom right panel shows a large effect for people without criminal convictions. Both panels exclude detainees with a final order of removal when booked in to detention. Because criminal history makes release on bond and eventual victory less likely, *Rodriguez* likely produced a more realistic hope of release and victory for people without convictions and therefore had a larger effect on their behavior. Note that because the horizontal axis (in both panels) shows each individual’s first month of detention, the decision has a delayed effect by construction: many of the people who might have been affected by the decision in the initial months were released or deported before it was announced.

on people without criminal convictions. Again, that makes sense: people without criminal convictions were more likely to be released at a bond hearing, meaning that *Rodriguez* created a stronger hope of release for them. And people without convictions were also more likely to succeed in their immigration cases on the merits, meaning that *Rodriguez* also implied

a stronger hope of *victory* after enduring six months of detention. (This large difference in effects for people with and without criminal history is not present in the first months of detention, most likely because people without convictions were especially likely to be released in their first month or two of detention, when many likely had bond hearings.)

Of course, criminal history is very far from exogenous, and it might be correlated with the effect of hope of release because it is a proxy for something else. Most plausibly, criminal history might be a crude proxy for the anticipated chance of success in immigration court, but that possibility is not testable. In the Appendix, Figure 11 shows the effect of *Rodriguez* for several subgroups. Notably, lawful permanent residents experienced a smaller effect of *Rodriguez* on detention endurance, maybe because lawful permanent residents were more likely to fight their cases longer in detention even without the hope of release—and maybe because lawful permanent residents were likely to have more serious convictions than average (since absent such convictions, they might not have been placed in deportation proceedings).

In sum, Figure 1 shows how *Rodriguez* shaped people’s decisions about whether to continue to fight their cases from detention. After *Rodriguez*, people became less likely to give up—especially if they had already spent a few months in detention, and especially if they lacked criminal convictions and therefore were particularly likely to be released and to win their cases.

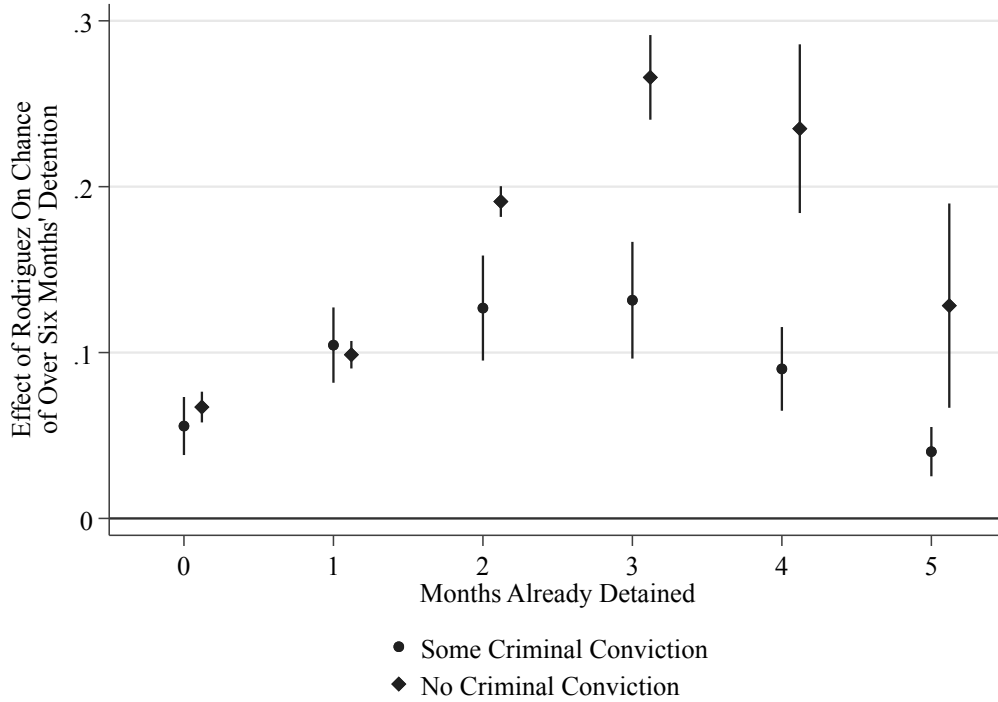


Figure 3: Effect of *Rodriguez*, by Criminal History and Number of Months Already Detained. As the *Rodriguez* decision took effect in the Ninth Circuit, the chance of remaining detained at least six months increased. That chance increased most for the subset of people without criminal convictions already detained 2-4 months. This plot shows the coefficient (on Z_i in equation (1) above) estimating the effect of *Rodriguez* in the overlapping populations of people detained at least one, two, three, four, and five months. As the effect becomes larger, the difference in effects for people with and without criminal convictions also increases. Table 2 shows the same results in regression table form.

Table 2: Regression Results

Effect of *Rodriguez* For all Detainees

	(All)	(> 1 month)	(> 2 months)	(> 3 months)	(> 4 months)	(> 5 months)
<i>Rodriguez</i>	0.043***	0.106***	0.169***	0.216***	0.184***	0.104***
Clustering	[0.032, 0.055]	[0.090, 0.122]	[0.150, 0.189]	[0.192, 0.240]	[0.165, 0.203]	[0.086, 0.121]
Wild Bootstrap	[-0.025, 0.049]	[-0.020, 0.114]	[0.029, 0.179]	[-0.000, 0.229]	[0.033, 0.231]	[0.013, 0.181]
N	504665	206348	104018	58655	37061	26351

Effect of *Rodriguez* For Detainees with Criminal History

	(All)	(> 1 month)	(> 2 months)	(> 3 months)	(> 4 months)	(> 5 months)
<i>Rodriguez</i>	0.042***	0.104***	0.127***	0.132***	0.090***	0.040***
Clustering	[0.022, 0.062]	[0.082, 0.127]	[0.095, 0.158]	[0.096, 0.167]	[0.065, 0.115]	[0.025, 0.055]
Wild Bootstrap	[-0.046, 0.052]	[-0.104, 0.115]	[-0.157, 0.142]	[-0.189, 0.147]	[-0.108, 0.113]	[-0.076, 0.099]
N	250693	112153	58312	36060	24493	18146

Effect of *Rodriguez* For Detainees with No Criminal History

	(All)	(> 1 month)	(> 2 months)	(> 3 months)	(> 4 months)	(> 5 months)
<i>Rodriguez</i>	0.037***	0.099***	0.191***	0.266***	0.235***	0.128**
Clustering	[0.030, 0.045]	[0.090, 0.107]	[0.182, 0.200]	[0.240, 0.291]	[0.184, 0.286]	[0.067, 0.190]
Wild Bootstrap	[-0.025, 0.126]	[0.010, 0.152]	[0.093, 0.240]	[0.064, 0.468]	[-0.004, 0.548]	[-0.061, 0.425]
N	253972	94195	45706	22595	12568	8205

95% CIs are in brackets.

95% Includes control variables for sex, marital status, age, lawful permanent residence, Hispanic ethnicity, and expedited removal.

5 Mechanisms: Release and Deportation

So far, I have found that *Rodriguez* led more people to endure six months of detention and also changed *who* remained detained so long. What happened to those who did endure? Of 2,146 people in the immigration court sample released on bond in the Ninth Circuit after *Rodriguez*, only 198, or 9%, were ordered deported because they failed to appear at a subsequent hearing. Among the remaining 1,948 people who appeared for their proceedings, only one third (635) were ordered deported within ten years.¹⁹ By contrast, 85% of those who were not granted bond lost their cases in detention.²⁰ These basic descriptive statistics suggest *Rodriguez* caused people to fight their cases for longer not in order to abscond after release but instead to fight (and often win) their cases on the merits in immigration court.

Unfortunately, because remaining in detention for six months is so unusual, there is no good way to measure the effect of *Rodriguez* on release and deportation for all detainees—any effect is swamped by small changes in the much higher release rate for the large group of detainees who spent short periods in detention.²¹

I can, however, analyze the effect of *Rodriguez* on release and deportation rates, bearing in mind that that effect is partly driven by selection (i.e. the effect of *Rodriguez* on persistence shown above). For this analysis, I index each observation by the month in which that person's detention reached 180 days. This means that the datasets include only the individuals who spent at least 180 days in detention and therefore were subject to the *Rodriguez* decision if they were in the Ninth Circuit.

For brevity, I show only event study results in the main text; descriptive plots and

¹⁹Of the remaining two thirds, 935 won their cases and another 378 cases remained pending.

²⁰Of 3,210 people not released on bond in the Ninth Circuit after *Rodriguez*, 2,714 lost their cases within ten years. The loss rate for the control group—other circuits and the Ninth Circuit before *Rodriguez*—the loss rate was a similar 81%.

²¹The reason that the effect on endurance to six months in detention is visible in the whole population is that the proportion of detainees who spent six months in detention is consistently low, so the large proportional change (of around four percentage points) is clear even in a simple time-series figure. The effect of *Rodriguez* on the overall release rate is almost certainly smaller still, since it reflects a 25 percentage point effect *among those who endured six months of detention*. Because the overall release rate is over 50%, a one or two percentage point effect is small relative to fluctuations in the release rate.

regression tables are in the Appendix. Results are visible in Figure 4, which shows event study plots of release and deportation rates over time for people detained six months or longer, comparing the Ninth Circuit to all other circuits. In this figure, each coefficient represents the interaction between a dummy variable for presence in the Ninth Circuit and the effect of *Rodriguez* on release or deportation, including only people detained at least six months.

The top panel of Figure 4 shows the effect of the *Rodriguez* decision on the release rate. The left panel shows effects on release rates in the ICE data, including all types of release (e.g., not only releases on bond but also on recognizance or an ankle bracelet). The right panel shows effects on bond release rates in immigration court. The horizontal axis shows the calendar quarter of each individual’s seventh month in detention (or eighth, ninth or later month for people detained on the date of the decision).

During the period before the *Rodriguez* decision, ICE release rates (top left panel) in the Ninth Circuit and elsewhere were similar, though changes in release rates were not quite parallel in the two circuits, most likely because of releases made for budgetary reasons.²² Bond grants in immigration court (top right panel), by contrast, moved in parallel in this pre-treatment period, maybe because immigration judges did not need to consider the detention budget. After the *Rodriguez* decision, both release and bond rates began to rise in the second half of 2013 in the Ninth Circuit, when attorneys reported that *Rodriguez* bond hearings began to occur in earnest—and when the effect of *Rodriguez* on detention endurance becomes obvious in Figure 1 above.

The bottom row of Figure 4 shows the effect of *Rodriguez* on the ICE deportation rate

²²The early-2013 spike in releases likely reflects sudden budget-driven releases required by so-called sequestration, imposed by the 2011 Budget Control Act, which affected budgeting in early 2013 [Carcamo, 2013]. The largest part of that spike occurred in February 2013, but the specter of a budget shortfall was clear well before that February and might have affected ICE behavior earlier as well. Unfortunately, I am unable to identify which releases were made as a result of the budget shortfall and am therefore unable to verify whether those releases were concentrated in the Ninth Circuit and therefore gave rise to the difference in trends. That explanation is plausible, however, in light of the more parallel pre-treatment trends in the immigration court data, where immigration judge decisions about whether to grant bond were unaffected by the budget shortfall.

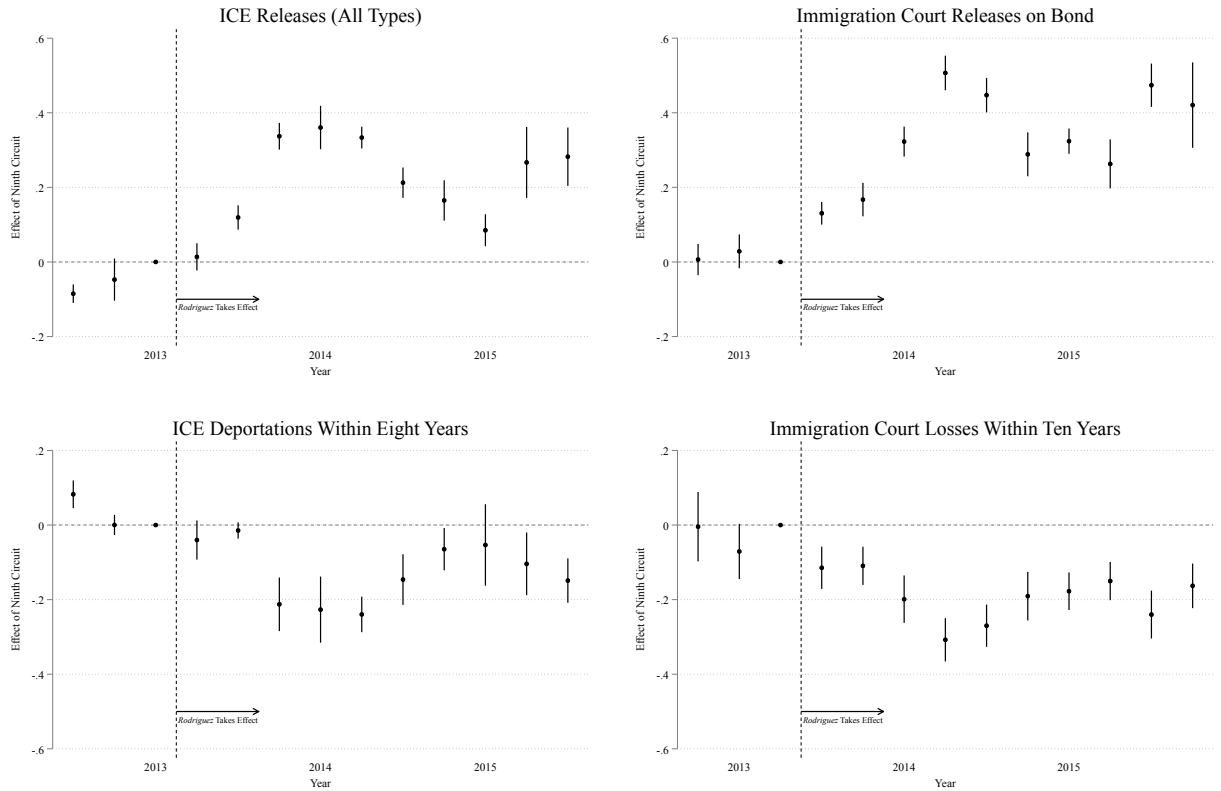


Figure 4: Event Study Plots Showing the Effect of *Rodriguez* on release and deportation. The left column shows effects on release and deportation rates in the ICE data; the right column shows effects on bond rates and eventual loss rates in immigration court. In every plot, the horizontal axis marks the quarter in which each person either did become or would have become eligible for a *Rodriguez* bond hearing, which was usually their seventh month in detention (but the eighth, ninth or a later month for people detained on the date of the decision). The vertical line marks the date of the *Rodriguez* decision, when bond hearings began to be available in the Ninth Circuit, but not elsewhere, for people detained at least six months. *Rodriguez* made release (top left) more likely as immigration judges began to grant bond more frequently (top right); release then made deportation less likely (bottom left), mostly because immigration court losses became less likely (bottom right).

(bottom left panel) and on the loss rate in immigration court (bottom right panel), with a similar post-*Rodriguez* pattern. There is every reason to guess—but no way to demonstrate—that release itself prevented deportation, connecting these two patterns. As explained above, release gives people more time to prepare their cases and find counsel, and release also provides access to the nondetained immigration court docket, where grant rates are much higher. There is, however, no way to distinguish between the effect of release and the effect

of fewer people giving up. The fact that fewer people gave up on their cases before the six month mark likely meant that more people with easier cases got access to bond hearings in the Ninth Circuit. In other words, compositional change, not release itself, might have driven some or most of the effect of *Rodriguez* on deportations. One empirical reason to guess that release played a primary role, though, is that *Rodriguez* had no obvious effect on deportation or case success among those not released on bond. See Figure 21.

In sum, the high success rate and low absentia rate after release suggest that the hope of release caused people to persevere in detention not in order to abscond once released but instead in order to fight their cases more effectively.²³

6 Discussion

Immigration detention, although formally intended only to prevent crime and nonappearance, in fact drives immigrants' decisions about whether to fight their cases. The threat of continued detention causes people to give up on cases that they might otherwise have won. By studying the effect of a judicial decision that made release newly possible, but only for people who had endured at least six months of detention, I show that the hope of release causes people not to give up on their cases, and that those who do persevere and obtain release are unlikely to lose their cases in immigration court.

These results have limitations. Both the ICE and immigration court datasets include some measurement error, but similar patterns across the two datasets increase confidence that this measurement error has only minor implications. There is also no way to test for external validity. These results rely on inference from the approximately four percent of detainees locked up at least six months. Most important, the results do not demonstrate that release prevents deportation. Both the decision to persevere and actual release are more likely for people who have a better chance of winning their case. There is no way here to disentangle

²³People who appear at their hearings and are ordered deported may still fail to appear for deportation. But the combined ICE and EOIR results imply that such nonappearances were relatively rare, since the effect of release on immigration court losses is nearly as large as the effect on deportations.

the likely effect of release on deportation from the (demonstrated) effect of perseverance. But these results are consistent with qualitative and descriptive work suggesting that release makes victory in immigration court more likely: people released after *Rodriguez* had only a one-third chance of losing their case in immigration court, and their failure-to-appear rate was just 9%.

If immigration court wins were determined by the merits, rather than by detention, the hope of release would not cause such a large change in immigrants' decisions to continue fighting their cases. This result matters for policy: the assumption that detention decisions are separate from deportation decisions is false.

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Appendices

A Additional Background

A.1 Background on Mandatory Immigration Detention

Immigration bond procedure is complex, with different paths for different categories of noncitizens. First, release on bond has generally been available (until policy changes in the last year, after the end of this study) for noncitizens arrested within the United States and not convicted of certain crimes. ICE may decide, in its discretion, whether to detain such noncitizens or to release them on bond. When ICE does decide to detain, that decision is subject to relatively prompt review by an immigration judge, who decides whether the noncitizen poses a danger (i.e. might commit crimes) or flight risk (i.e. might not appear at hearings) if released. If the immigration judge concludes that the noncitizen poses no danger or flight risk (or that flight risk can be mitigated by a cash bond or other alternative to detention), the noncitizen is released from detention, and from then on is responsible for returning to immigration court for scheduled hearings. Although the danger and flight risk standards are formally separate from the deportation decision, it is no secret that bond and merits decisions are related: noncitizens who are more likely to win their cases may have a stronger incentive to appear for hearings, and the same criminal convictions that might make an immigration judge find a noncitizen dangerous could make deportation more likely. Still, the decision to detain or release should not, as a formal matter, affect merits outcomes or noncitizens' decisions about whether to fight their cases.

Mandatory immigration detention—detention without a bond hearing—was introduced with the 1996 Illegal Immigration Reform and Immigration Responsibility Act, and it has been subject to litigation ever since. In 2003, the Supreme Court approved the use of mandatory detention for people convicted of certain crimes (under 8 U.S.C. §1226(c)), relying heavily on (partly mistaken) statistics suggesting that most mandatory detainees spent

little time behind bars because they were swiftly deported.²⁴ Over the next decade, immigrants rights’ litigators worked to establish court-ordered limits on hearingless detention that stretched over six months. *Rodriguez v. Jennings*, the case whose effects I study here, was the first of these cases to provide bond hearings for a large group of detainees without final orders of removal. (As disclosed elsewhere, I worked on this case, and on related detention litigation, as an attorney and a consulting expert, but after the years I study here.)

Mandatory detention applies—and prevents access to a bond hearing before the final deportation decision—in three categories of cases. First, noncitizens who are apprehended at or near the border do not receive bond hearings, although they may be released by the Department of Homeland Security on immigration parole.²⁵ Second, noncitizens do not receive bond hearings if they are in deportation proceedings on the basis of certain criminal convictions.²⁶ Finally, noncitizens are generally not entitled to bond hearings if they are detained by ICE after they already have a final order of removal.²⁷ The initial *Rodriguez* decision that I study here applied to the first two of these three categories: arriving noncitizens and those with certain criminal convictions. I exclude noncitizens with final orders of removal from the analysis because the law regarding their access to bond hearings was in flux during this period. I am unable, however to exclude people who were detained six months but not subject to mandatory detention; this means that the sample is somewhat overinclusive.²⁸

A.2 Conversations with Attorneys

Conversations with attorneys who were practicing at the time of the *Rodriguez* were consistent with the quantitative findings here.

²⁴See *Demore v. Kim*, 538 U.S. 510, 529 (2003).

²⁵See 8 U.S.C. §1225(b); until recently, noncitizens apprehended near the border received bond hearings, but they no longer do. See *Padilla v. ICE*, 41 F.4th 1194 (9th Cir. 2022).

²⁶See 8 U.S.C. §1226(c).

²⁷8 U.S.C. §1231(a)(6).

²⁸Eventually, the *Rodriguez* decision was extended to apply to a third category of noncitizens: those who had an initial bond hearing but were not released and who eventually spent six months in detention. *Rodriguez v. Robbins*, 804 F.3d 1060 (9th Cir. 2015). This group was not affected during the study period.

Attorneys described the *Rodriguez* decision as consequential, leading many people to be released who would otherwise have remained detained. Before *Rodriguez*, there was often no way to obtain release for these detainees, and attorneys stressed the effects of detention on immigration cases, explaining that many detainees give up on their immigration cases in order to end detention.²⁹ Because my interviews took place years after the *Rodriguez* decision was overturned by the Supreme Court, attorneys also often bemoaned its loss.³⁰ Some attorneys noted that, although *Rodriguez* allowed some people to obtain release, the bond decision remained in the discretion of each IJ, and some IJs frequently refused bond.³¹

Attorneys thought that release under *Rodriguez* was likely to affect people's chance of avoiding deportation. They named many factors, including the fact that release encourages people not to give up on their cases and allows attorneys more time to build a case and a rapport with their clients. In addition, attorneys explained that deportations might not occur even if clients lost their cases once released. In some cases, attorneys described ICE not sending bag and baggage letters after a final order of removal; attorneys typically did not know whether clients actually appeared if those letters were sent.

Finally, interviewees recalled *Rodriguez* as having a delayed effect in practice. The Ninth Circuit's decision affirmed an injunction that applied in the Central District of California. Eventually, immigration judges throughout the Ninth Circuit accepted the requirement that they schedule bond hearings for people detained for at least six months under 8 U.S.C. §§1225(b) or 1226(c), but many immigration judges did not accept the ruling immediately. For example, attorneys who practiced in Arizona said that some immigration judges first began holding *Rodriguez* bond hearings in September 2013, five months after the April 2013 Ninth Circuit decision.³² These attorneys said that in the initial months after the decision, there was disagreement about whether it applied in Arizona, perhaps because some hoped that the government would seek a swift stay of the decision from the Supreme Court (it did

²⁹Personal communication, attorney, September 13, 2024.

³⁰Personal communication, attorney, September 13, 2024.

³¹Personal communication, attorney, September 24, 2024.

³²Personal communication, attorney, September 13, 2024.

not). Even where IJs agreed to hold hearings, these hearings did not occur immediately. Detainees had to learn that they could request one, often through materials that attorneys first had to develop and distribute.³³ And then detainees had to actually make the request and wait for the hearing to be scheduled.

In sum, attorneys reported that *Rodriguez* increased the chances of release for their clients, and that release made deportation far less likely, partly because clients who remained detained were more likely to give up on their cases. They also reported that the effect of *Rodriguez* occurred with a delay.

B Data

B.1 ICE Detention and Deportation Data

The dataset includes all detention stints that began on or after January 1, 2012. The sample here therefore includes everyone who entered detention on or after that date or who was transferred to a new detention center after that date. (The dataset includes the date not only of the individual’s book-in to that particular detention stint, but also the individual’s detention *stay* book-in date. The stay book-in date is the date of the first book-in of a set of contiguous detention stints.) The data including stay book-in dates before 2012 is incomplete, since it only includes people who also had a subsequent book-in in 2012 or later. As a result, my analysis includes only people whose stays began in January 2012 or later.

A key limitation of the ICE dataset is that it lacks a reliable indicator of whether the noncitizen won his/her case after being released. This study is possible because the dataset does include records of actual deportation; it does not distinguish between noncitizens who avoided deportation by winning their cases and those whose cases are ongoing or those who received a final order of removal but were not actually deported. I therefore use a separate

³³For example, attorneys in Washington state described working on materials to distribute to unrepresented noncitizens in September 2013. Personal communication, attorney, September 25, 2024.

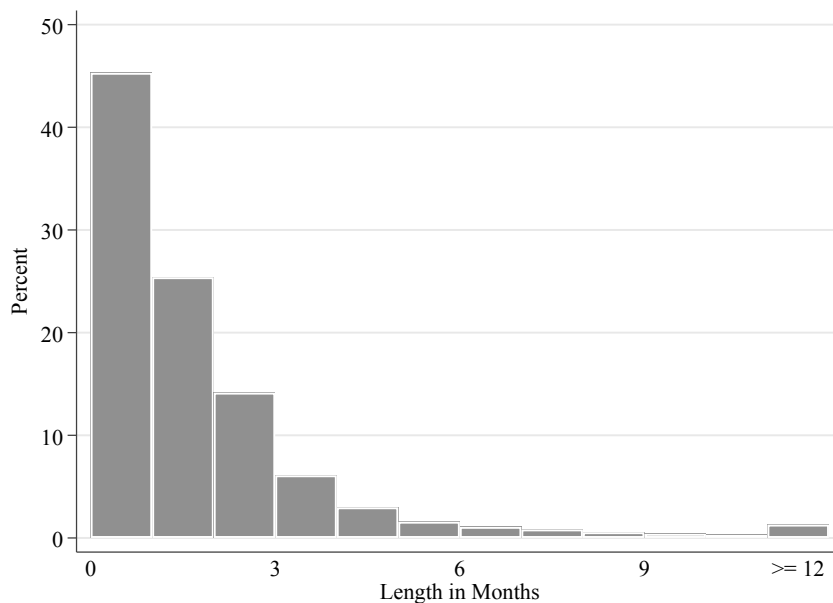


Figure 5: Histogram of detention lengths for noncitizens (excluding those with a final order of removal when first booked into detention).

dataset to study the effect of release on outcomes in immigration court.

The ICE dataset does include a reason for release, and that reason is sometimes marked “proceedings terminated.” (In immigration court, termination of proceedings means that the immigration judge concluded that the government lacked grounds to place the noncitizen in removal proceedings in the first place; the result is the restoration of the noncitizen’s former immigration status.) It is clear that ICE does not always give that phrase its technical meaning; the dataset often indicates that a person was released on the basis of termination at the same time that it describes the case status as “relief granted.” (In immigration court, a grant of relief allows a noncitizen to stay in the country despite being removable.) I treat terminations as wins on the merits, regardless of whether they technically followed from termination or a grant of relief. This indicator of merits wins is entirely absent from the ICE data for people who were released from detention.

To prepare the detention data for the analysis here, I drop a small number of observations that are duplicates and/or missing key information. Then, as explained in the main text, I simplify the data by keeping only the last stint in each individual’s first stay in detention.

Table 3 shows these steps.

Table 3: Sample Restrictions for Panel Dataset

Total Observations	8,458,563
Drop duplicates	8,448,726
Drop observations with missing IDs	8,415,246
Keep the last detention stint in each detention stay	3,960,463
Drop observations with mismatched book-in or book-out dates	3,847,698
Drop observations missing detention center locations	3,838,190
Keep first stay for each person	3,147,416
Drop if deportation recorded more than 7 days before book-out	3,144,829
Drop observations where first book-in is in or after April 2015	1,079,791

For the analysis, I also remove cases with final orders of removal (those either marked as reinstatement of removal cases or including a final order of removal before book-in) and drop cases from Massachusetts and the Central District of California, where separate court orders at different times might affect the analysis. This produces a dataset with 504,671 observations. Finally, in order to study the downstream effects of the decision on people who reach six months in detention, I construct a smaller dataset that only includes one observation for each person who reaches their seventh month (or a later month if detained more than seven months on the date of the *Rodriguez* decision) in detention. Because the vast majority of people are detained for under six months, this produces a much smaller dataset (14,620 observations). Table 4 shows these sample restrictions in more detail.

Table 4: Sample Restrictions for Individual-Level Dataset

Total Observations	1,079,791
Drop reinstatement of removal cases	850,274
Drop central district of California	820,473
Drop Massachusetts	815,632
Drop detainees with final orders of removal	504,671
Prolonged Detention Subset (six months' detention before a final order)	14,620

B.2 Immigration Court Data

Immigration court data come from the November 2025 release of the CASE dataset (covering the period through October 2025), which the Executive Office for Immigration Review posts monthly in its online FOIA library.

The immigration court dataset has several key limitations that pose challenges for identifying people affected by the *Rodriguez* decision. (Because of these limitations, I use the immigration court data only to study the downstream consequences of *Rodriguez* for people who remained detained six months; the ICE data is the better source of information on detention endurance.) First, although EOIR does record detention dates, those dates are, in my understanding, filled in by hand and often inaccurate, since detention is the responsibility of ICE. (ICE records book-in and book-out dates to keep track of its detention population, but again, those dates are not linkable to the EOIR data at the individual level.)

In order to identify people detained at least six months, I develop a rough proxy for detention length. If EOIR has no record of a detention date, I use the date of the Notice to Appear in immigration court to mark the beginning of an individual’s case. If EOIR does have a detention date recorded, I use the later of (1) the earliest date of detention in the custody history table or (2) the detention date recorded in the proceeding table. I then drop individuals whom EOIR marks as “never detained” and who never had a bond hearing. I consider individuals potentially affected by the *Rodriguez* decision if (1) they are marked as “detained throughout” and their cases lasted over 180 days or (2) they had a bond hearing that occurred more than 180 days after the start of their case. I then mark individuals as released on bond if the new bond field is filled in. Note that this method has a key shortcoming: I eliminate cases in which individuals were released for reasons other than a grant of bond. Since the detention information in the dataset is flawed, I would be able to obtain only an unrepresentative sample of this group and therefore omit it entirely. Because a small number of people have multiple bond hearings and multiple stays in detention, this account slightly oversimplifies the steps taken to find individuals detained at least 180 days;

see the replication archive for more details.

I next adopt the sample restrictions in Table 5 below, largely mirroring those for the ICE data.

Table 5: Sample Restrictions for EOIR Dataset

Total Cases June 2012 - September 2015	722,315
Drop Massachusetts and Central District of CA	635,427
Keep only Removal/Exclusion/Deportation Cases	610,587
Keep only Rodriguez-eligible cases	16,708

C Additional Descriptive Figures

One flexible way to visualize these results is with a Kaplan-Meier survival figure, displayed below.

Figure 1 above shows a selection of descriptive figures that captures the overall pattern. Here, I plot the time series for each relevant subgroup, conditional on the number of months already detained, both with and without controls: the full population (Figure 7), those with criminal history ((Figure 8), and those with no criminal history ((Figure 9).

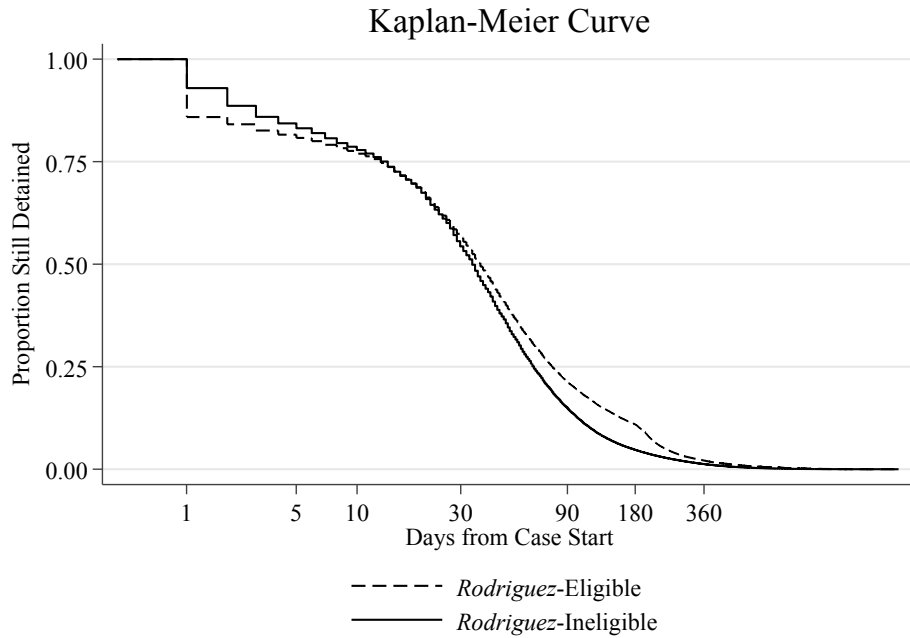


Figure 6: Kaplan-Meier Survival Curve. The dashed line shows the survival curve for the period after *Rodriguez* in the Ninth Circuit; the solid line shows the same curve for the other circuits and the pre-*Rodriguez* Ninth Circuit. The horizontal axis shows length of detention on a logarithmic scale (but with labels indicating length of detention in days).

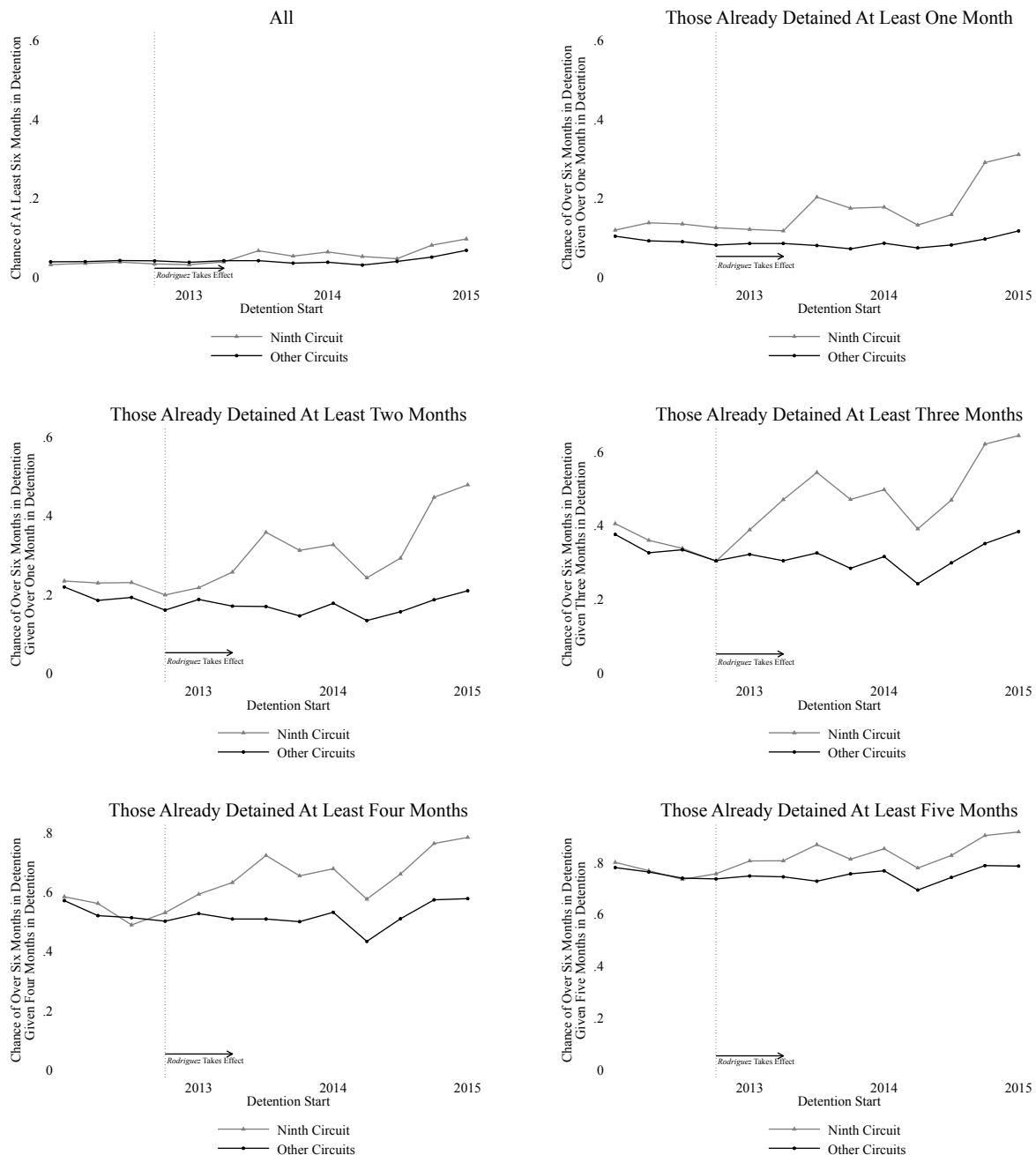


Figure 7: Descriptive Plots: Effect of *Rodriguez* on Detention Endurance (full population). As the *Rodriguez* decision took effect in the Ninth Circuit, the chance of remaining detained at least six months increased. The panels all show the same time series, conditional on different numbers of months already spent in detention. Note that because the horizontal axis (in both panels) shows each individual’s first month of detention, the decision has a delayed effect by construction: many of the people who might have been affected by the decision in the initial months were released or deported before it was announced.

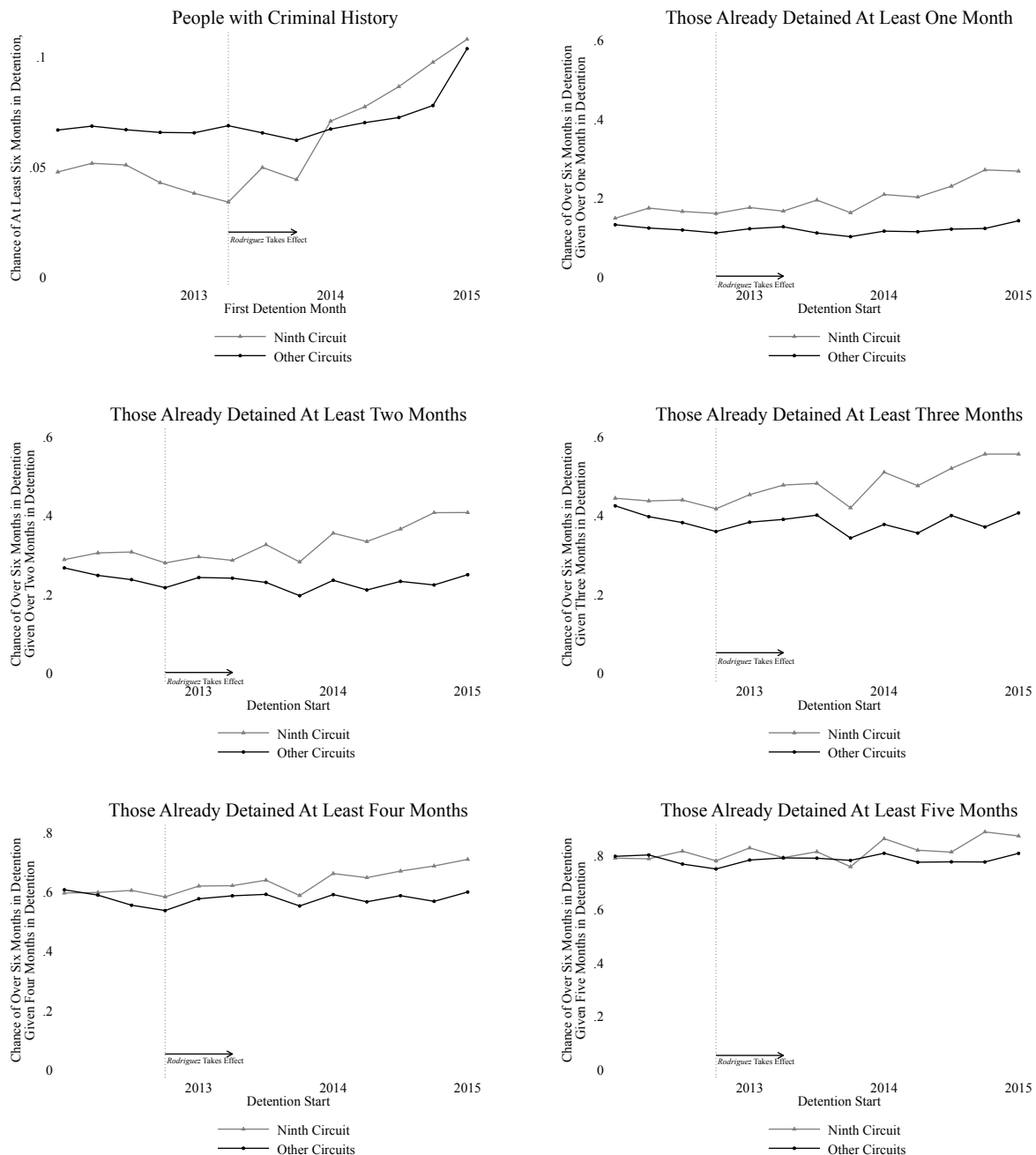


Figure 8: Descriptive Plots: Effect of *Rodriguez* on Detention Endurance (Given Some Criminal History). As the *Rodriguez* decision took effect in the Ninth Circuit, the chance of remaining detained at least six months increased. The panels all show the same time series, conditional on different numbers of months already spent in detention. Note that because the horizontal axis (in both panels) shows each individual's first month of detention, the decision has a delayed effect by construction: many of the people who might have been affected by the decision in the initial months were released or deported before it was announced.

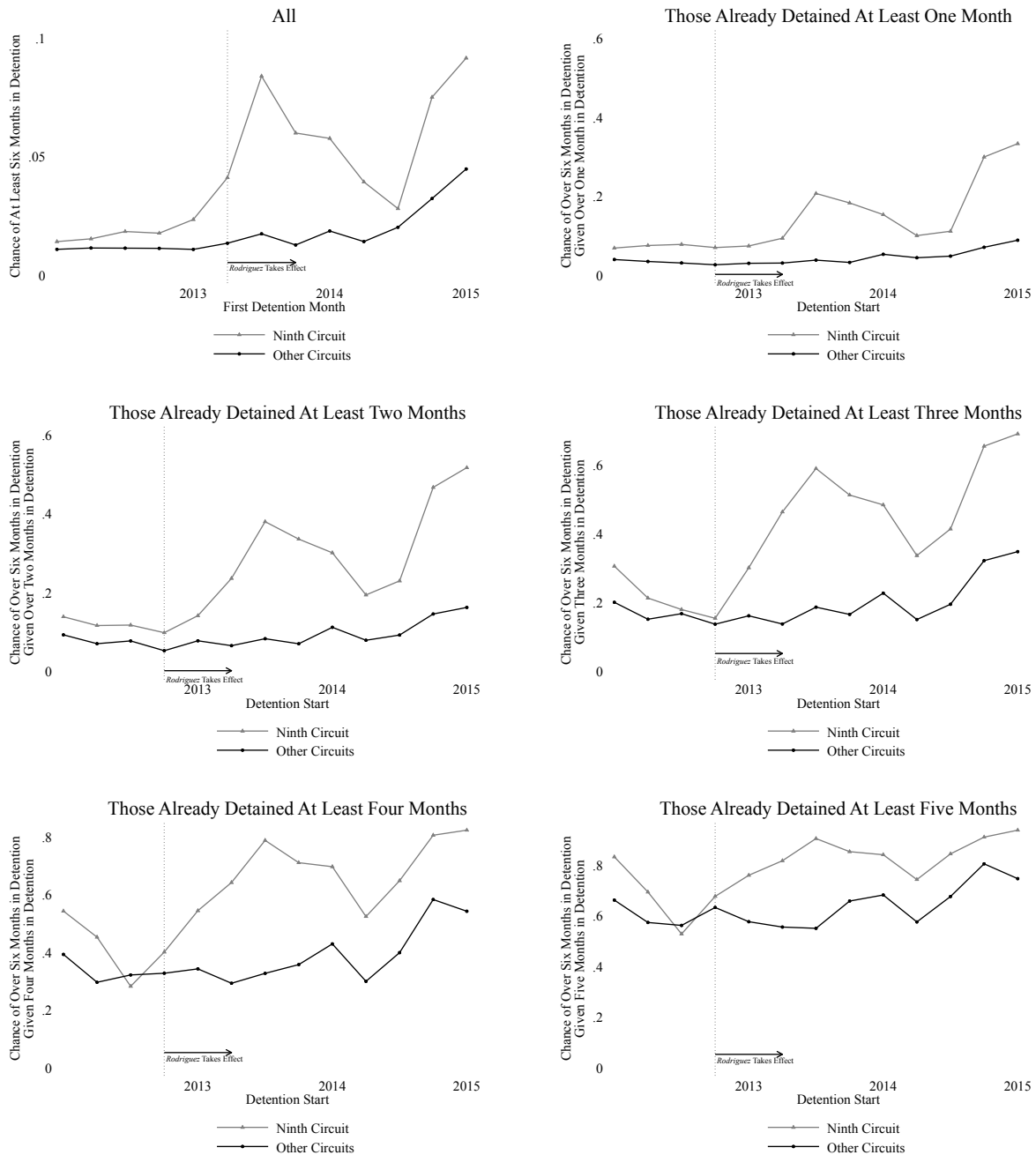


Figure 9: Descriptive Plots: Effect of *Rodriguez* on Detention Endurance (Given No Criminal History). As the *Rodriguez* decision took effect in the Ninth Circuit, the chance of remaining detained at least six months increased. The panels all show the same time series, conditional on different numbers of months already spent in detention. Note that because the horizontal axis (in both panels) shows each individual's first month of detention, the decision has a delayed effect by construction: many of the people who might have been affected by the decision in the initial months were released or deported before it was announced.

D Downstream Results

This section shows descriptive figures and regression table results for people who remained detained at least six months. Descriptive results are visible in Figure 10, which plots release and deportation rates over time for people detained six months or longer, comparing the Ninth Circuit to all other circuits (see Figure 4 below for corresponding event study results). In this figure, each dot represents a quarterly average, within the Ninth Circuit or elsewhere, across people detained at least six months.

The top panel of Figure 10 shows the effect of the *Rodriguez* decision on the release rate. The left panel shows release rates in the ICE data, which include all types of release, both on bond and otherwise (e.g., on recognizance or an ankle bracelet). The right panel shows bond release rates in immigration court; these rates are lower because they include only immigration judge bond decisions and not other types of releases. The horizontal axis shows the quarter when each person either became eligible for bond under *Rodriguez* or would have become eligible had *Rodriguez* existed before April 2013.³⁴

I also reproduce the pattern in Figure 10 in difference-in-differences (Tables 6) and event study (Figure 4 in the main text) regressions. I estimate linear probability models of the form:

$$(2) \quad D_i = \beta_1 Z_i + \Gamma_c + \Lambda_m + \Omega_l + \epsilon_i$$

In equation (2), D_i is the dependent variable (release/bond or deportation/loss) and is 1 or 0 for individual i , Z_i is an indicator variable for the availability of a bond hearing (i.e.

³⁴For most people, that means that the plotted quarter is the quarter of their 180th day in detention. But anyone who was in their eighth, ninth, or higher month of detention when *Rodriguez* was decided also became eligible for release, so in these figures the second quarter of 2013 includes everyone detained seven months *or longer* who remained detained in that quarter. This is a flaw in the visualization, but it does not affect the regression (or event study) results. In those models, because I include length-to-treatment fixed effects for the relative detention month in which each individual was treated (i.e. 7, 8, 9, etc.), I do not change the calendar quarter for this group of people.

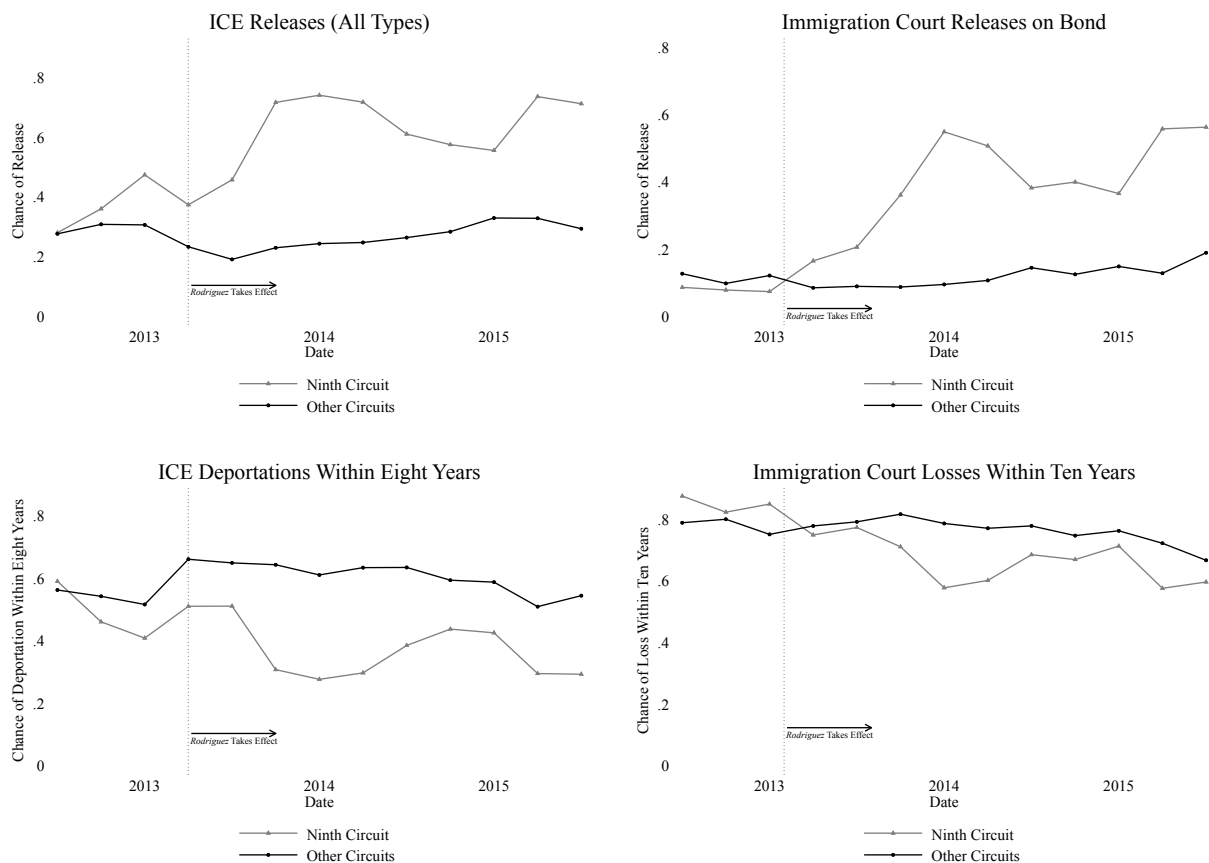


Figure 10: Outcomes Over Time for People Detained Over Six Months in the Ninth Circuit vs. Elsewhere. The left column shows ICE enforcement outcomes; the right column shows immigration court outcomes. In all four plots, the horizontal axis marks the quarter in which that each person either did become or would have become eligible for a *Rodriguez* bond hearing, which was usually their seventh month in detention (but the eighth, ninth or a later month for people detained on the date of the decision). The vertical line marks the quarter of the *Rodriguez* decision, when bond hearings began to be available in the Ninth Circuit, but not elsewhere, for people detained at least six months. The top panel shows releases over time—all types of releases in the ICE data and bond releases only in the immigration court data. The bottom panel shows eventual outcomes—deportations within eight years in the ICE data and losses within ten years in the immigration court data (including cases that remained pending after ten years). Although the data sources are separate and not matchable at the individual level, they show notably similar patterns.

presence in the Ninth Circuit after *Rodriguez*) for an individual, Γ represents circuit (c) fixed effects, and Λ represents month (m) fixed effects, where the month is the calendar month in which the individual entered their seventh month in detention. The event study plots in Figure 4 use the same setup and show the coefficients on the interaction between the

calendar quarter and Z_i .) Ω represents length-to-treatment (l) fixed effects to account for the fact that a first cohort of individuals was subject to *Rodriguez* after being detained more than six months: these are dummy variables that equal, for example, one if an individual was treated in their seventh month in detention and zero otherwise, with additional variables eight months to treatment, nine months to treatment, and so on. Again, the only people treated in a month later than their seventh month are people who were in detention in April 2013 and had already been detained at least seven months. Standard errors are clustered at the circuit level, but because there are only eleven clusters, I also show wild-bootstrapped standard errors [Cameron et al., 2008, Roodman et al., 2019].

Table 6 shows results. The coefficients match the visual results in Figure 10, and all are significantly different from zero at conventional levels, both with standard clustering and wild-bootstrapped standard errors. In the ICE data, *Rodriguez* made the release rate 27 percentage points higher and the deportation rate 16 percentage points lower, relative to the rates in other circuits. Immigration court results are similar: *Rodriguez* made the bond rate 30 percentage points higher and the loss rate 16 percentage points lower, again relative to other circuits. These results are consistent with event study regressions (see Figure 4).

Table 6: Regression Results: Effect of Rodriguez on Bond/Release and Loss/Deportation

<i>ICE Results</i>		
	(1) Release	(2) Deportation
Ninth Circuit * Post- <i>Rodriguez</i>	0.270***	-0.157***
95% CI with standard clustering	[0.225,0.316]	[-0.192,-0.122]
95% CI with wild bootstrap	[.1794, .5165]	[-.3499, -.05943]
<i>N</i>	14260	14260

<i>Immigration Court Results</i>		
	(1) Bond	(2) Loss
Ninth Circuit * Post- <i>Rodriguez</i>	0.305***	-0.160***
95% CI with standard clustering	[0.276, 0.334]	[-0.202, -0.118]
95% CI with wild bootstrap	[.2573, .5403]	[-.3259, -.03189]
<i>N</i>	16708	16708

95% confidence intervals in brackets

Standard Errors Clustered at the Circuit Level

Includes Circuit and Month Fixed Effects

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The immigration court results increase confidence in the ICE results and offer evidence of the mechanism driving those results. In both datasets, *Rodriguez* had about half as large an effect on the deportation rate as it did on the release rate. Those released were about fifty percentage points more likely to win their cases. That difference is consistent with qualitative evidence that release prevents deportation, but I cannot draw that inference reliably here, since the changing composition of detainees might also have driven the change.

E Heterogeneity

Figure 11 shows varying effects of *Rodriguez* on detention endurance by subgroup. This is broadly consistent with the heterogeneity, in terms of criminal convictions; lawful permanent residents are more likely to have convictions because, without convictions, they would be unlikely to be in removal proceedings.

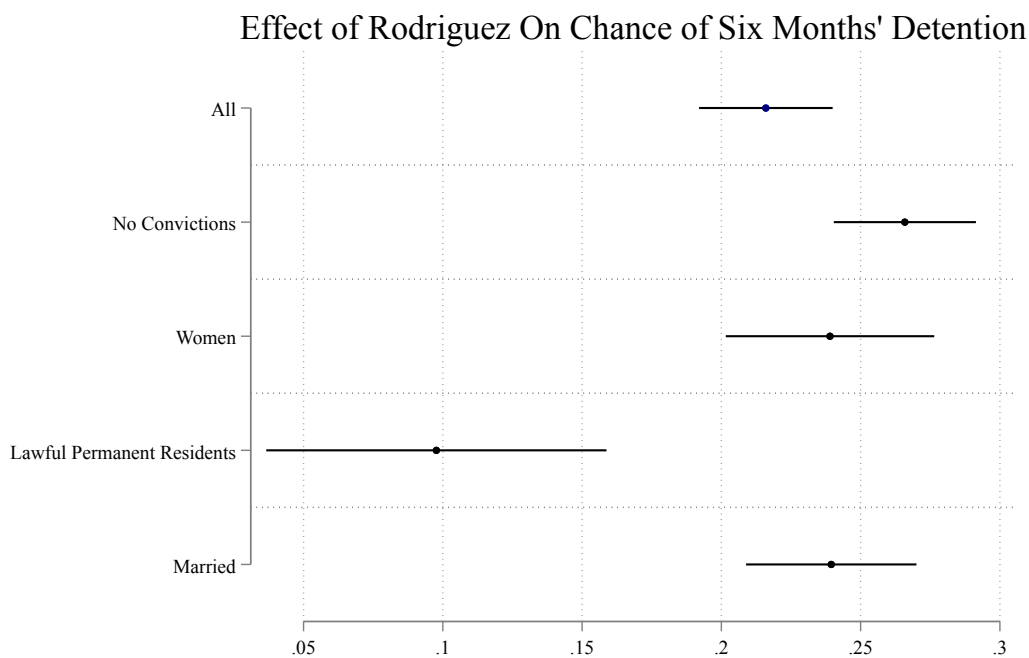


Figure 11: Case Characteristics and the Effect of *Rodriguez*. The figure shows the effect of *Rodriguez* on remaining detained six months for several subgroups. The largest effect is for people without criminal convictions; the smallest effect is for lawful permanent residents, perhaps because they are most likely to be in removal proceedings if convicted of crime.

F Assessing Compositional Change

If there were differential compositional changes in the ninth circuit vs. the other circuits, that might undermine causal inference here. Figure 12 shows trends over time in the relatively few case characteristics available in the ICE data. There are not obvious differential trends around the time of the *Rodriguez* decision. The figures do, however, help explain the presence of breaks in the trends in mid-2014. Notably, the total number of detainees in the other circuits spiked in mid-2014, most likely because of the sudden arrival of families and children seeking asylum at the southern border, mostly in Texas (in the fifth rather than the ninth circuit).

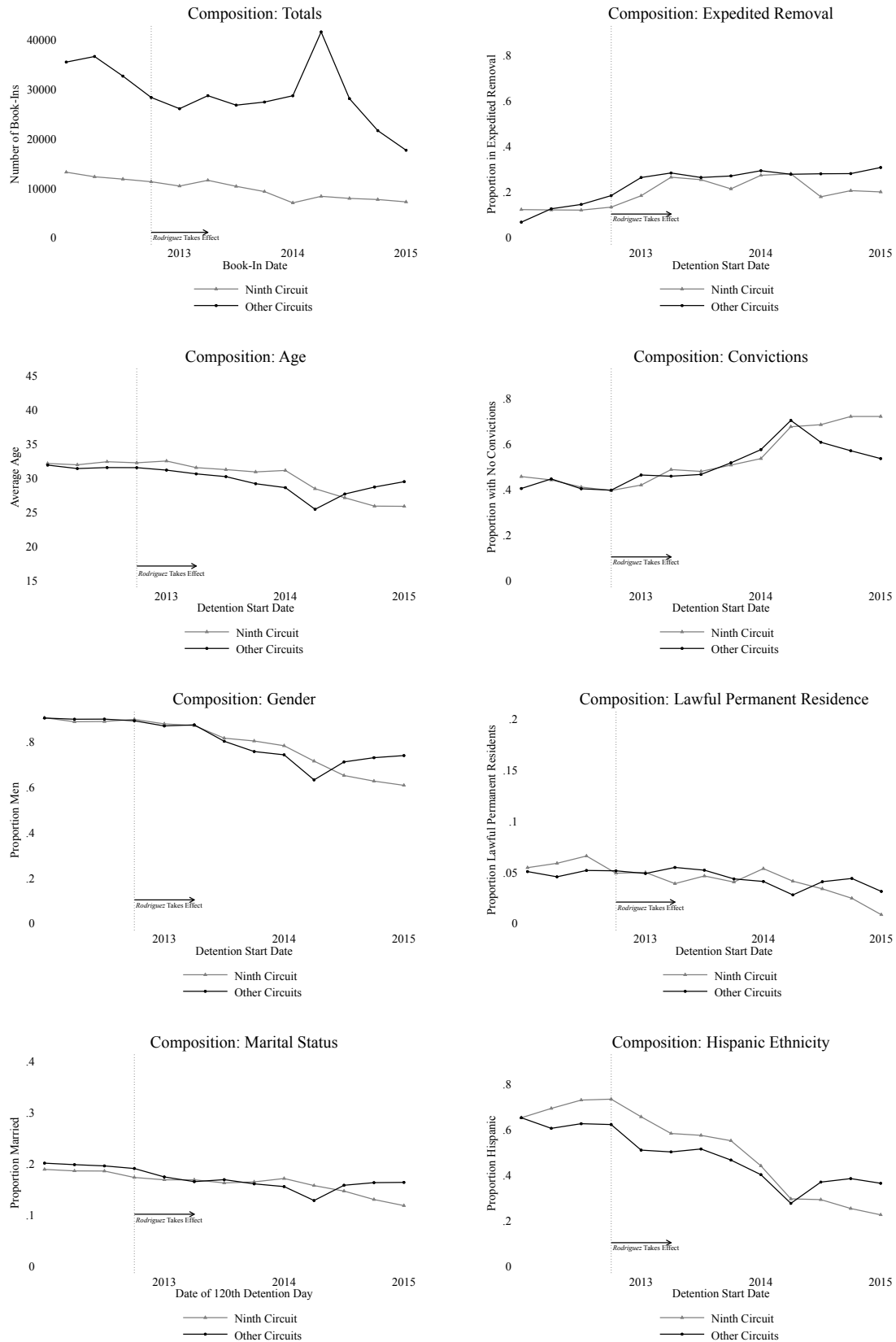


Figure 12: Compositional Trends Over Time in the Ninth Circuit vs. Other Circuits. There are not obvious changes in differential trends around the time that *Rodriguez* took effect.

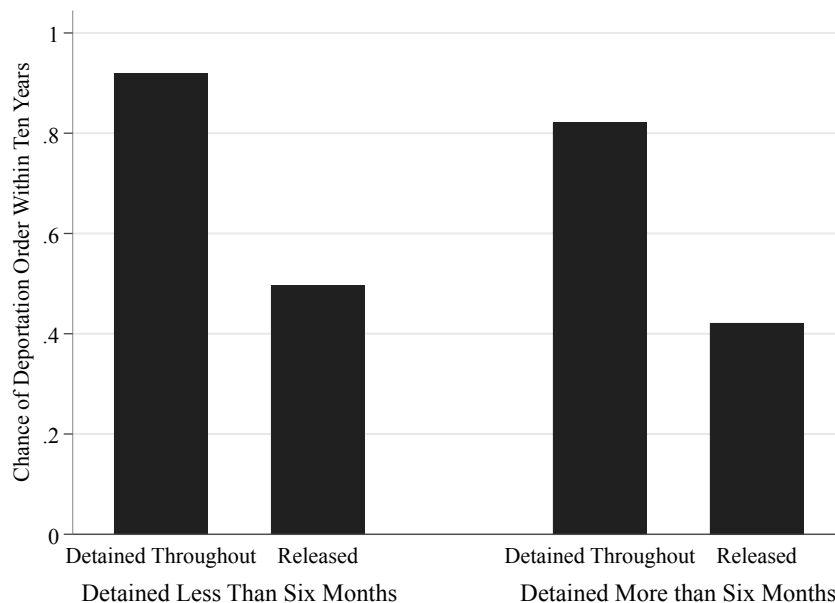


Figure 13: Bar Graph of Deportation Order Rates by Release Status, 2012-2015. Compare to Table 1. This figure uses immigration court data to show the relationship between detention and deportation *order* rates. The left panel shows this comparison for the vast majority of detainees, who spent under six months in detention. The right panel shows the same comparison, but for the small minority of detainees who spent more than six months in detention. The association between detention and deportation is strong, but weaker for people who were detained at least six months.

G Addressing Outcome Measurement Error in ICE Data

In the short presentation of descriptive statistics in the main text, I note ICE’s possible miscategorization of some wins in immigration court as pre-decision releases. In this section, I compare the coding of releases in ICE and immigration court datasets.

First, in order to get a sense of the possible scale of this measurement problem, I turn to the immigration court data. The immigration courts track whether noncitizens fighting deportation are ordered deported. The bottom panel of Figure 13 shows the relationship between release, prolonged detention, and deportation orders, using data from the immigration courts for fiscal years 2012 to 2015 (the same period as the ICE dataset). Because the immigration court data is posted monthly, I am able to look at the deportation rate within

ten years instead of eight.

Notice the key difference, both as a data matter and a substantive matter, between a deportation order (recorded by the immigration courts) and an actual deportation (recorded in the ICE data). There is no requirement that ICE physically deport someone who receives a final order of deportation, and, when a deportation order is issued to someone outside detention, ICE frequently does not actually deport that person. Unsurprisingly, then, deportation orders (Figure 13) are more common than deportations (Table 1) among people who are released. The rates of deportations and deportation orders are similar, however, for people who remained detained throughout their cases. The comparison confirms that the deportation rate among people who remain detained is slightly inflated in the ICE data, likely because ICE miscategorizes some wins as pre-outcome releases. Still, the similarity, for detained people, between the deportation rate in the ICE data and the deportation order rate in the immigration court data increases confidence in both data sources.

H Additional Visual Evidence

I Additional Event Study Evidence

Here, I display event study plots for each relevant subgroup, conditional on the number of months already detained, both with and without controls: the full population (Figures 14 and 15), those with criminal history ((Figures 16 and 17), and those with no criminal history ((Figures 18 and 19).

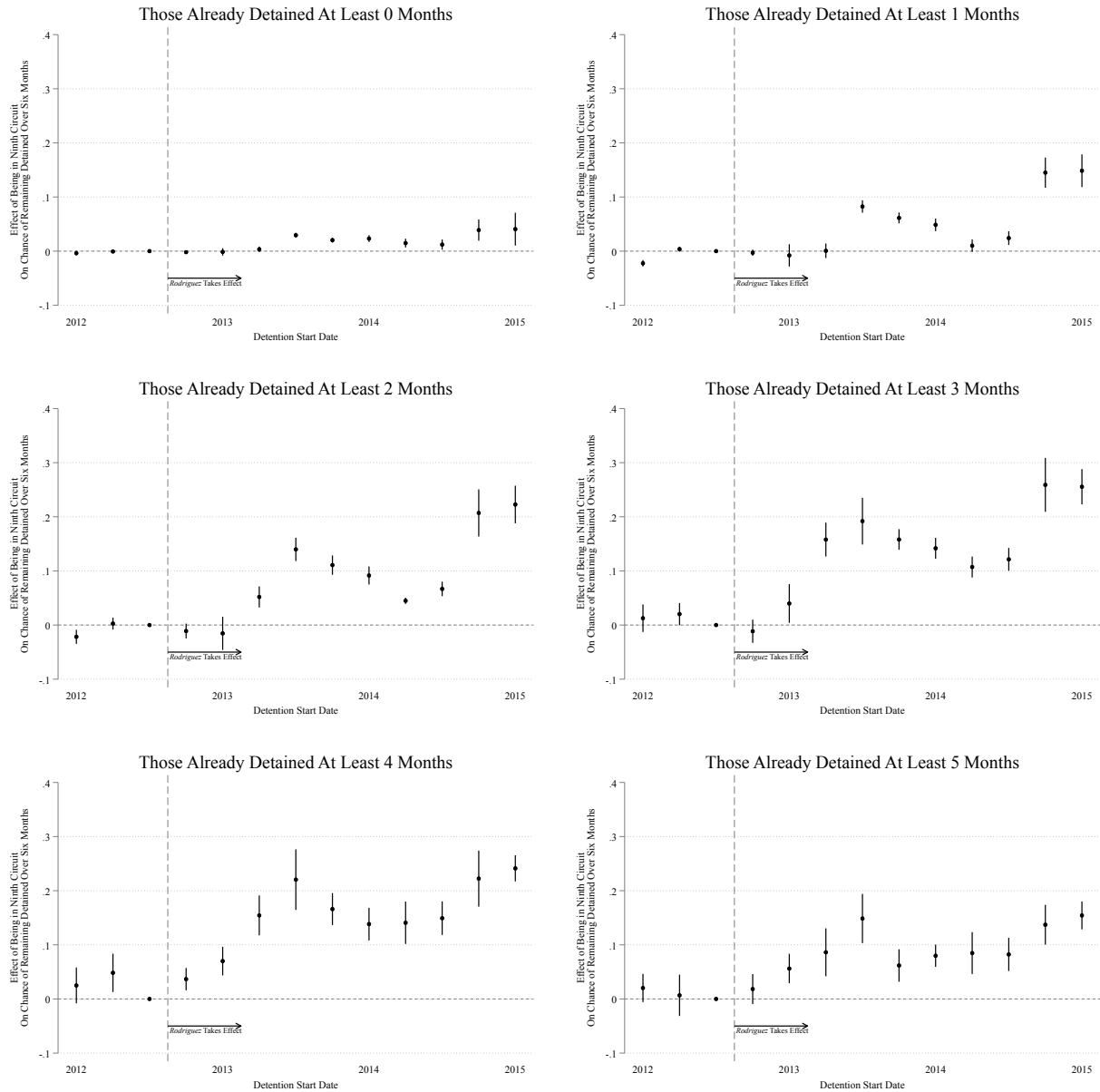


Figure 14: Event Study Evidence: Effect of *Rodriguez* on Detention Endurance (full population, including controls). As the *Rodriguez* decision took effect in the Ninth Circuit, the chance of remaining detained at least six months increased. The panels all show the same effect, conditional on different numbers of months already spent in detention. Note that because the horizontal axis (in both panels) shows each individual’s first month of detention, the decision has a delayed effect by construction: many of the people who might have been affected by the decision in the initial months were released or deported before it was announced.

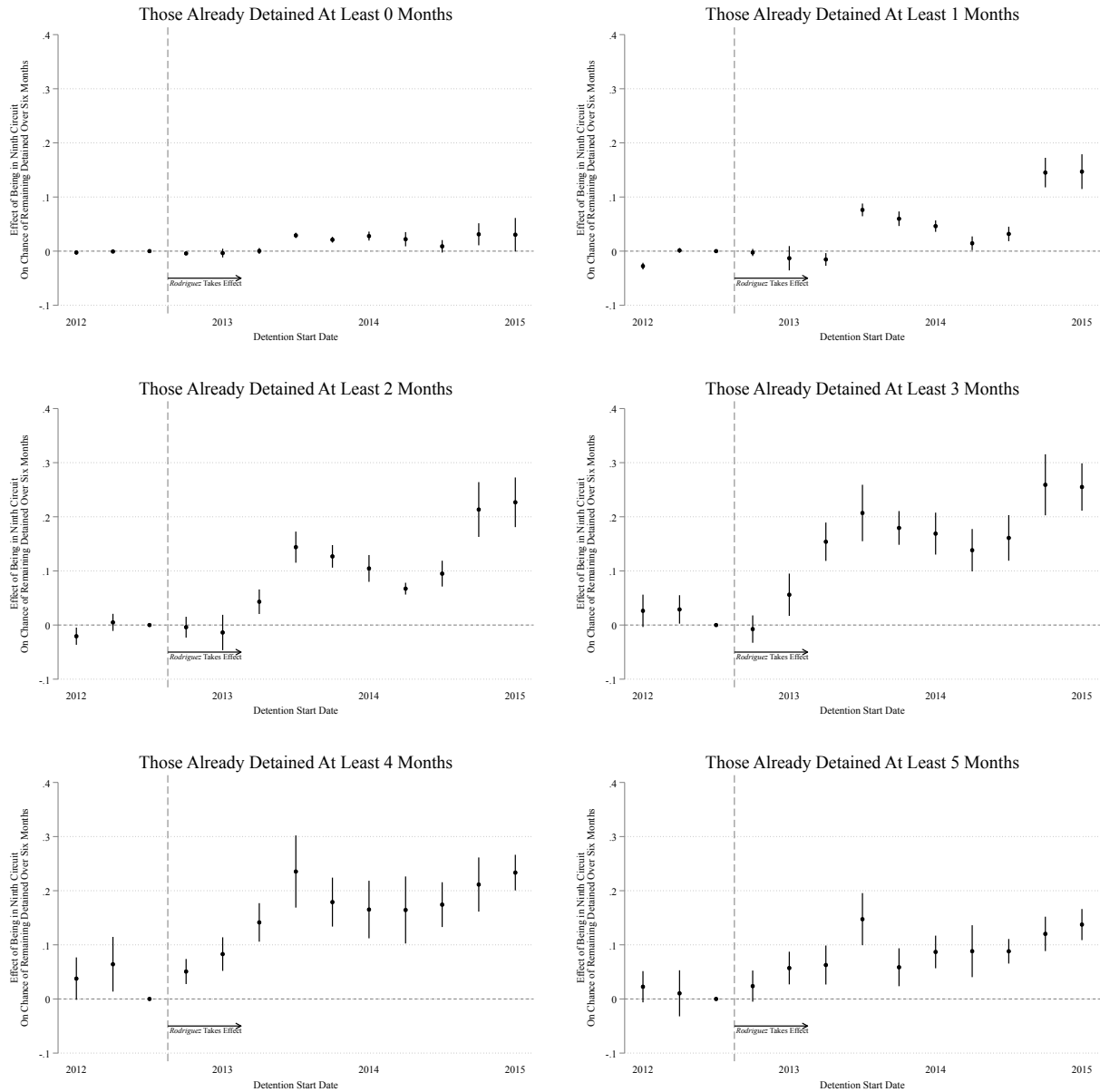


Figure 15: Event Study Evidence: Effect of *Rodriguez* on Detention Endurance (full population, no controls). As the *Rodriguez* decision took effect in the Ninth Circuit, the chance of remaining detained at least six months increased. The panels all show the same effect, conditional on different numbers of months already spent in detention. Note that because the horizontal axis (in both panels) shows each individual's first month of detention, the decision has a delayed effect by construction: many of the people who might have been affected by the decision in the initial months were released or deported before it was announced.

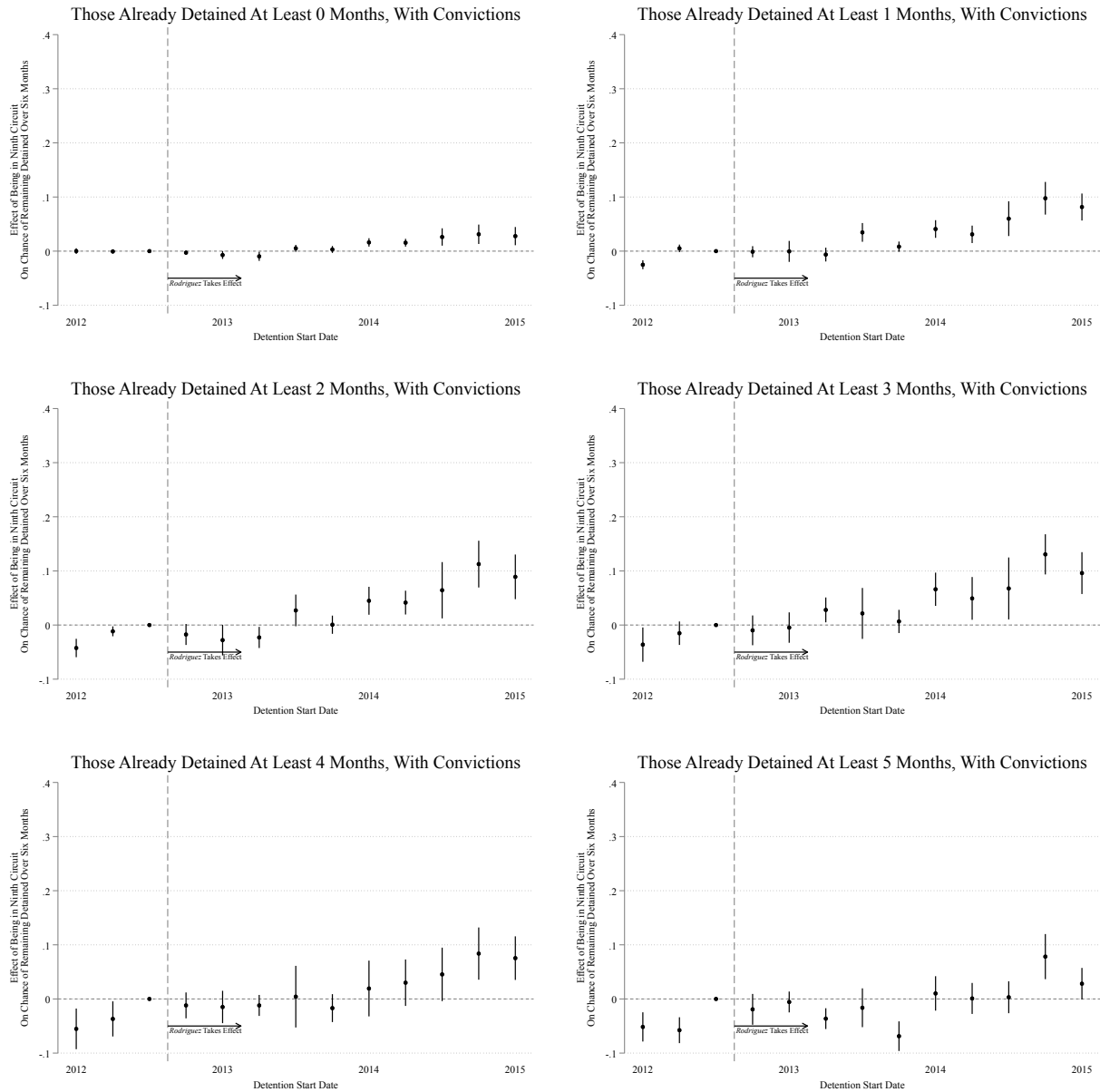


Figure 16: Event Study Evidence: Effect of *Rodriguez* on Detention Endurance (those with criminal history, including controls). As the *Rodriguez* decision took effect in the Ninth Circuit, the chance of remaining detained at least six months increased. The panels all show the same effect for people with criminal history, conditional on different numbers of months already spent in detention. Note that because the horizontal axis (in both panels) shows each individual’s first month of detention, the decision has a delayed effect by construction: many of the people who might have been affected by the decision in the initial months were released or deported before it was announced.

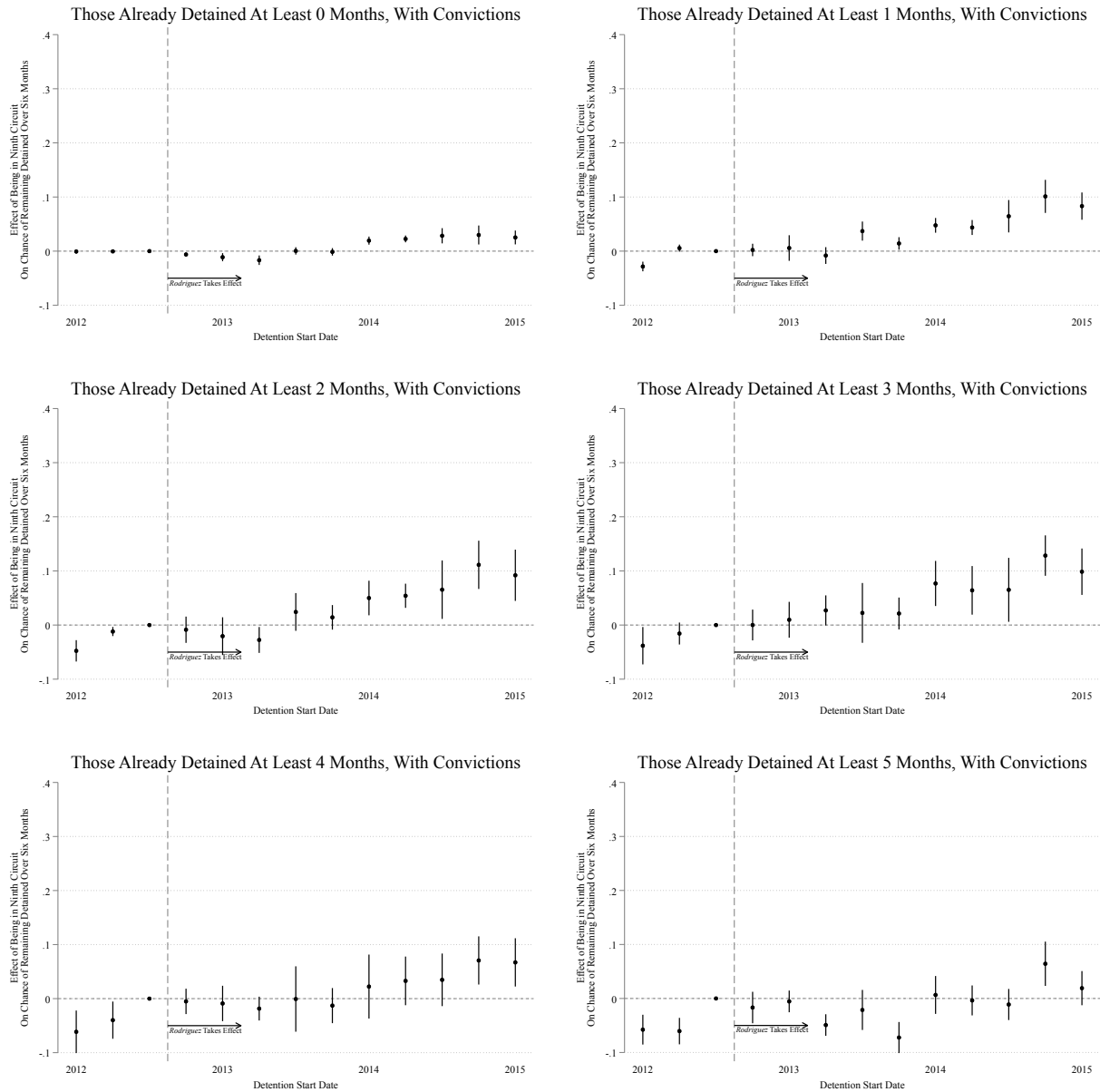


Figure 17: Event Study Evidence: Effect of *Rodriguez* on Detention Endurance (those with criminal history, no controls). As the *Rodriguez* decision took effect in the Ninth Circuit, the chance of remaining detained at least six months increased. The panels all show the same effect for people with criminal history, conditional on different numbers of months already spent in detention. Note that because the horizontal axis (in both panels) shows each individual’s first month of detention, the decision has a delayed effect by construction: many of the people who might have been affected by the decision in the initial months were released or deported before it was announced.

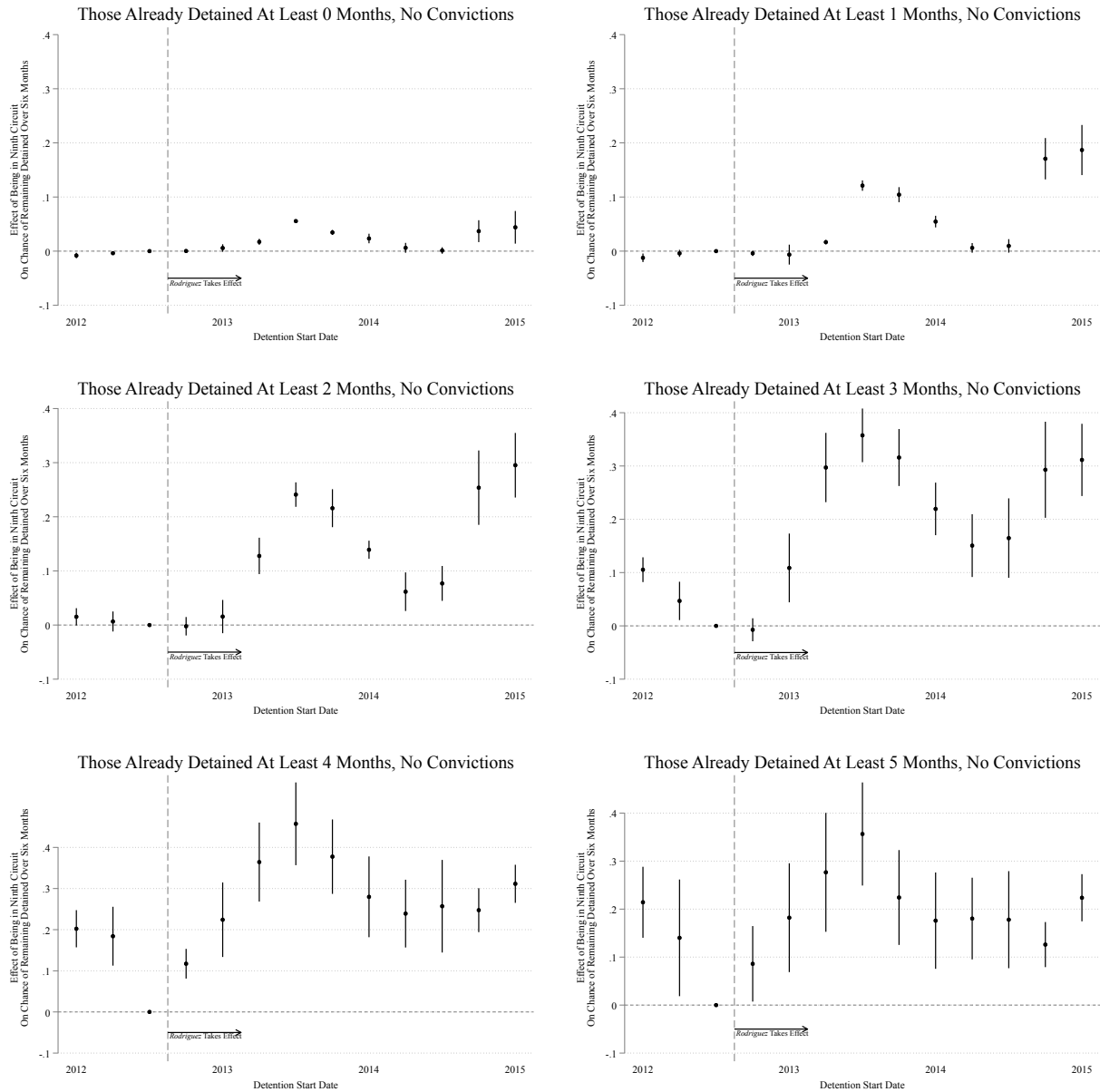


Figure 18: Event Study Evidence: Effect of *Rodriguez* on Detention Endurance (those with no criminal history, no controls). As the *Rodriguez* decision took effect in the Ninth Circuit, the chance of remaining detained at least six months increased. The panels all show the same effect for people without criminal history, conditional on different numbers of months already spent in detention. Note that because the horizontal axis (in both panels) shows each individual’s first month of detention, the decision has a delayed effect by construction: many of the people who might have been affected by the decision in the initial months were released or deported before it was announced.

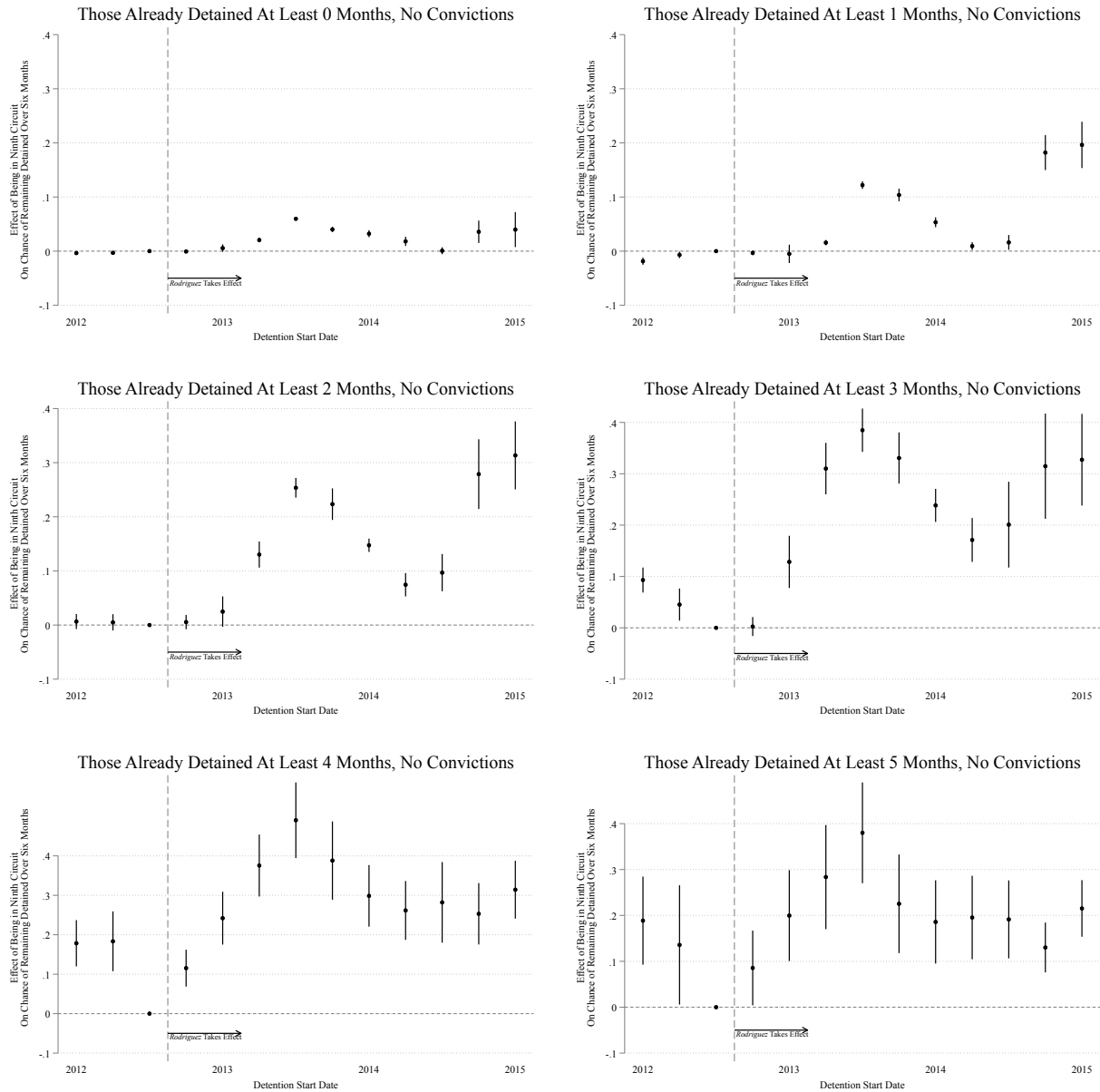


Figure 19: Event Study Evidence: Effect of *Rodriguez* on Detention Endurance (those with no criminal history, no controls). As the *Rodriguez* decision took effect in the Ninth Circuit, the chance of remaining detained at least six months increased. The panels all show the same effect for people without criminal history, conditional on different numbers of months already spent in detention. Note that because the horizontal axis (in both panels) shows each individual’s first month of detention, the decision has a delayed effect by construction: many of the people who might have been affected by the decision in the initial months were released or deported before it was announced.

J Downstream ICE Bond Outcomes

In the main text, I show the downstream effect of *Rodriguez* on all types of releases, rather than on bond releases alone. I make this choice because of the possibility that some people not released on bond were released in another way (e.g. on recognizance), and that those releases may have occurred in anticipation of releases on bond that were required by *Rodriguez*. Unsurprisingly, the effect of *Rodriguez* on bond releases alone is larger than the effect on all releases. Figure 20 shows this effect

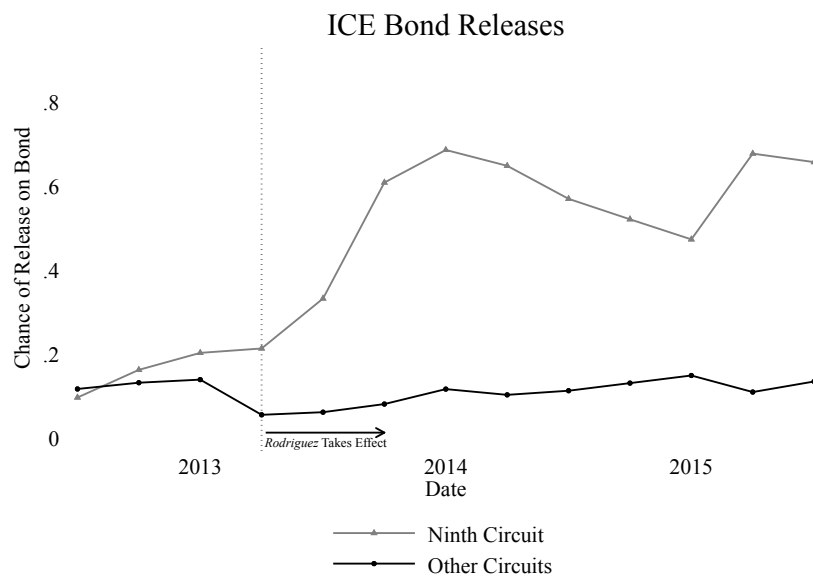


Figure 20: Bond Release Over Time for People Detained Over Six Months in the Ninth Circuit vs. Elsewhere. The horizontal axis marks the date that each person either did become or would have become eligible for a *Rodriguez* bond hearing, which was usually their seventh month in detention (but the eighth, ninth or a later month for people detained on the date of the decision). The vertical line marks the date of the *Rodriguez* decision, when bond hearings began to be available in the Ninth Circuit, but not elsewhere, for people detained at least six months. This figure shows bond releases only. Figure 10 in the main text shows all releases (including bond) and deportations.

Similarly, Table 7 shows regression results; these are identical to the results in Table 6, except that they also include the effect of *Rodriguez* on bond releases. *Rodriguez* made release on bond about 40 percentage points more likely, release of all kinds about a quarter more likely, and deportation about a sixth less likely.

Table 7: ICE Regression Results, Including Bond Releases Separately

<i>Effect of Rodriguez on Bond/Release and Deportation</i>			
	(1)	(2)	(3)
	Bond	Any Release	Deportation
Ninth Circuit * Post- <i>Rodriguez</i>	0.396***	0.270***	-0.157***
95% CI with standard clustering	[0.338,0.453]	[0.225,0.316]	[-0.192,-0.122]
95% CI with wild bootstrap	[.3125, .6836]	[.1794, .5165]	[-.3499, -.05943]
<i>N</i>	14260	14260	14260

95% confidence intervals in brackets

Standard Errors Clustered at the Circuit Level

Includes Circuit and Month Fixed Effects

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

K Outcomes for People Not Released on Bond

I expect *Rodriguez* to affect detention endurance through the hope of release on bond. I also expect *Rodriguez* to reduce deportations among those released, both by changing the composition of the population eligible for release and by making immigration cases easier to win via release itself. It is also possible, however, that *Rodriguez* might cause people to fight their cases, remain detained until the six month threshold, and then have a better chance of winning despite remaining detained.

In Figure 21, I find little evidence that *Rodriguez* increased non-bond releases or that *Rodriguez* reduced the chance of deportation for people not released on bond. Although it is theoretically possible that ICE released people in anticipation of bond releases—and I therefore do not strictly expect a null effect—I find little evidence of it. Notably, the 2013 budget-driven releases (which cause a brief fall in the deportation rate) are clearly visible among non-bond releases, and had a larger impact in the Ninth Circuit than elsewhere, while post-*Rodriguez* trends are largely parallel. Those post-*Rodriguez* parallel trends in non-bond releases (and deportations among people not released on bond) increase my confidence that *Rodriguez* caused the diverging trends in *bond* releases.

The ICE data’s miscategorization of some wins as releases means that some fraction of

the increase in the release rate post-*Rodriguez* could reflect an increase in the win rate. Reassuringly, both the qualitative evidence and Figure 21 suggest that *Rodriguez* did not help noncitizens win their cases if they were not released.

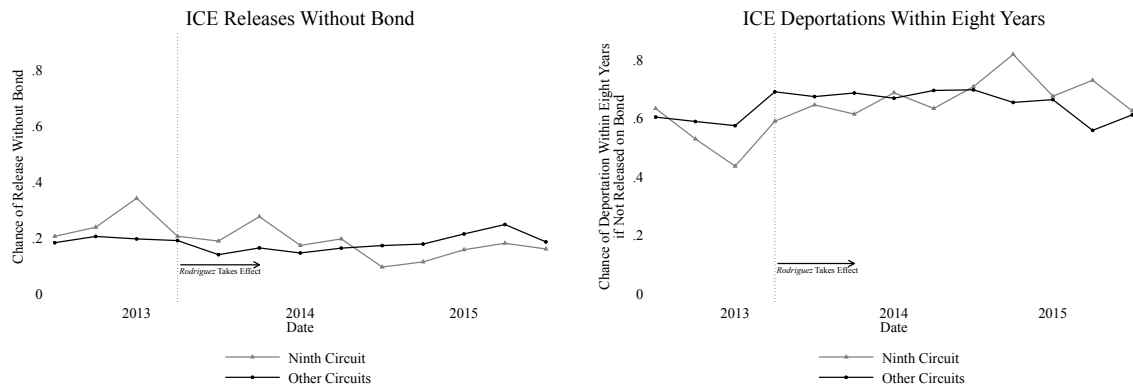


Figure 21: Outcomes Over Time for People Detained Over Six Months and Not Released on Bond. The left panel shows the noneffect of the decision on non-bond releases. The right panel shows the noneffect of the decision on deportations for people who were not released on bond. The horizontal axis marks the date that each person either did become or would have become eligible for a *Rodriguez* bond hearing, which was usually their seventh month in detention (but the eighth, ninth or a later month for people detained on the date of the decision). The vertical line marks the date of the *Rodriguez* decision, when bond hearings began to be available in the Ninth Circuit (but not elsewhere) for people detained at least six months.

K.1 Person-Time Variation

The figures so far summarize the data at the individual level, where the outcomes are *eventual* release or deportation. But another way to look at the effect of the *Rodriguez* decision might be to consider its effect on individual case trajectories over time. (For simplicity, I do not use this source of variation in the main results; that way, I avoid modeling the chance of release over time in each individual case.) I expect the court order to make release in each individual’s seventh or eighth month more likely (bond hearings may take some time to schedule). Before the court order is in effect, by contrast, I expect the chance of release to remain roughly flat in the seventh and eighth months of detention: releases are relatively rare once bond hearings are complete, typically in the first few months in detention. In

Figure 22, I plot the likelihood of release after 1, 2, 3, etc. months in detention, up to a year.³⁵ The two lines in Figure 22 compare detainees in the pre-period—those whose cases start at least a year before the court order, and who therefore remain unaffected throughout the 12 months in the figure—to those in the post-period, who are subject to the court order after six months in detention. As expected, there is a jump in the release rate at seven/eight months in detention for those in the post-period, but not for those in the pre-period. The pre- and post-periods are also different in other respects (see the different release rate in month 1), and these differences might be driven by a range of factors, but it is difficult to come up with another likely reason for the increase in release rates at seven/eight months.

³⁵Whenever an individual leaves detention—whether through a release, a deportation, or (more rarely) a win on the merits—that individual leaves the data for this plot. In other words, there are fewer people in each person-month in Figure 22, regardless of whether the chance of release rises or falls in that month.

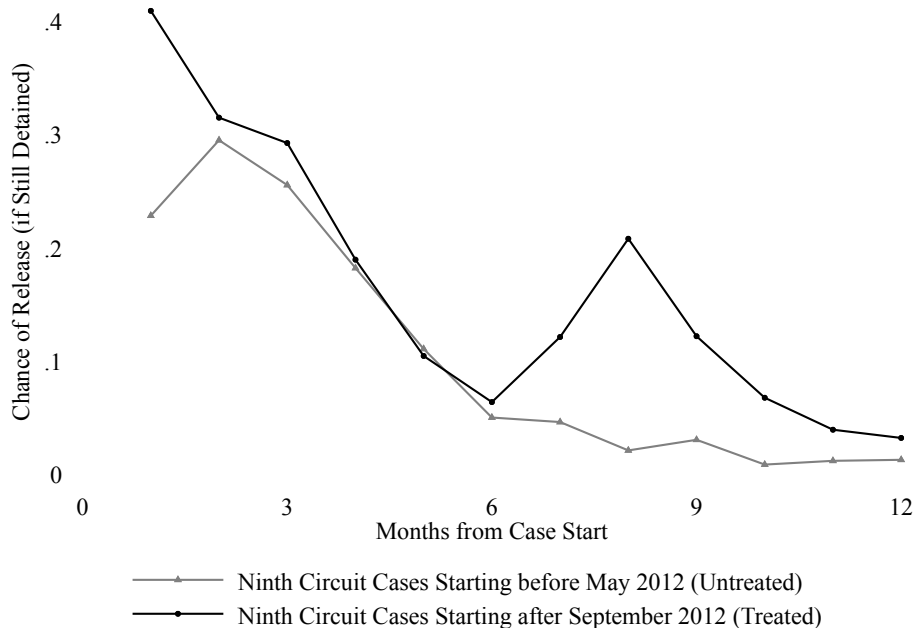


Figure 22: Release Likelihood by Months in Detention, Before and After *Rodriguez*. The solid line shows release likelihood over time for detainees whose cases began at least twelve months before the *Rodriguez* order; the dashed line shows the same likelihood for detainees whose cases began six months or fewer before the order, and who therefore were subject to it. The increase in release rates in months seven, eight, and nine likely reflects the new availability of bond hearings, as a result of the *Rodriguez* decision, for noncitizens detained at least six months. The data includes only individuals who remained detained that many months; when a person leaves detention, that person exits the data for this plot. Data include all detainees in the Ninth Circuit except those in the Central District (who were subject to a similar court order sooner) and those with final orders of removal.

L Robustness: Regressions Without Controls

Table 2 in the main text shows regression results including control variables; Table 8 shows the same results not including controls. Effect sizes remain similar.

Table 8: Regression Results, Not Including Controls

Effect of *Rodriguez* For all Detainees, Matched Sample

	(All)	(> 1 month)	(> 2 months)	(> 3 months)	(> 4 months)	(> 5 months)
<i>Rodriguez</i>	0.044***	0.106***	0.178***	0.226***	0.180***	0.093***
Clustering	[0.030, 0.058]	[0.086, 0.125]	[0.155, 0.202]	[0.195, 0.257]	[0.155, 0.205]	[0.077, 0.109]
Wild Bootstrap	[-0.034, 0.051]	[-0.026, 0.115]	[0.002, 0.190]	[-0.041, 0.245]	[-0.013, 0.214]	[-0.025, 0.153]
N	504671	206349	104018	58655	37061	26351

Effect of *Rodriguez* For Detainees with Criminal History, Matched Sample

	(All)	(> 1 month)	(> 2 months)	(> 3 months)	(> 4 months)	(> 5 months)
<i>Rodriguez</i>	0.045***	0.113***	0.134***	0.139***	0.091***	0.037***
Clustering	[0.023, 0.066]	[0.090, 0.136]	[0.102, 0.166]	[0.103, 0.175]	[0.065, 0.118]	[0.022, 0.052]
Wild Bootstrap	[-0.043, 0.055]	[-0.104, 0.124]	[-0.167, 0.149]	[-0.186, 0.165]	[-0.108, 0.117]	[-0.076, 0.085]
N	250698	112154	58312	36060	24493	18146

Effect of *Rodriguez* For Detainees with No Criminal History, Matched Sample

	(All)	(> 1 month)	(> 2 months)	(> 3 months)	(> 4 months)	(> 5 months)
<i>Rodriguez</i>	0.039***	0.103***	0.204***	0.284***	0.251***	0.137***
Clustering	[0.028, 0.051]	[0.091, 0.116]	[0.192, 0.216]	[0.259, 0.309]	[0.206, 0.297]	[0.078, 0.197]
Wild Bootstrap	[-0.056, 0.164]	[-0.042, 0.158]	[0.058, 0.222]	[0.026, 0.397]	[-0.021, 0.489]	[-0.054, 0.422]
N	253973	94195	45706	22595	12568	8205

95% CIs are in brackets.

M Robustness: Matched Sample

In order to address concerns that compositional changes across circuits might be driving these results, I replicate regression and event study results in a matched sample of observations. I use a $k - to - k$ coarsened exact matching approach [Iacus et al., 2012, Blackwell et al., 2009], matching observations across treatment and control groups on age, lawful permanent residence status, Hispanic ethnicity, gender, placement in expedited removal (which suggests that the case began at the border), and criminal convictions (yes/no). Figure 23 reproduces Figure 3 above, but in the matched sample, and Tables 9 and 10 shows the results with and without controls. Results are similar to results using the full sample, but often with larger confidence intervals, reflecting the smaller sample size after matching.

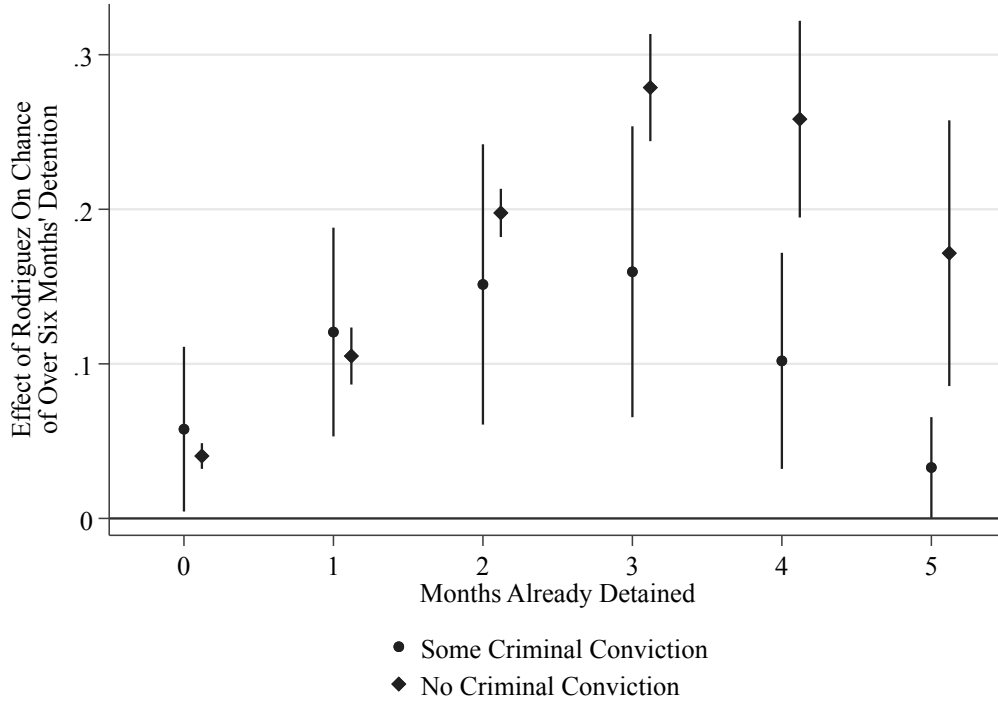


Figure 23: Effect of *Rodriguez*, by Criminal History and Number of Months Already Detained (Matched Sample). This plot reproduces Figure 3 in the main text, now using a smaller exact-matched sample. As the *Rodriguez* decision took effect in the Ninth Circuit, the chance of remaining detained at least six months increased. That chance increased most for the subset of people already detained 2-4 months and for people without criminal convictions. This plot shows the coefficient (on Z_i in equation (1) above) estimating the effect of *Rodriguez* in the overlapping populations of people detained at least one, two, three, four, and five months (including control variables). As the effect becomes larger, the difference in effects for people with and without criminal convictions also increases. Table 9 shows the same results in regression table form, and Table 10 shows the results without control variables.

Table 9: Regression results in exact-matched sample, including Controls

	(All)	(> 1 month)	(> 2 months)	(> 3 months)	(> 4 months)	(> 5 months)
<i>Rodriguez</i>	0.051**	0.118***	0.186***	0.235***	0.193***	0.102***
Clustering	[0.021, 0.081]	[0.072, 0.164]	[0.126, 0.246]	[0.168, 0.302]	[0.149, 0.237]	[0.085, 0.119]
Wild Bootstrap	[0.002, 0.072]	[0.035, 0.146]	[0.105, 0.221]	[0.126, 0.270]	[0.121, 0.215]	[0.055, 0.125]
N	247206	88258	45572	26754	17545	12841

Effect of *Rodriguez* For Detainees with Criminal History, Matched Sample

	(All)	(> 1 month)	(> 2 months)	(> 3 months)	(> 4 months)	(> 5 months)
<i>Rodriguez</i>	0.058*	0.121**	0.151**	0.160**	0.102**	0.033*
Clustering	[0.004, 0.111]	[0.053, 0.188]	[0.061, 0.242]	[0.065, 0.254]	[0.032, 0.172]	[0.000, 0.066]
Wild Bootstrap	[-0.009, 0.098]	[-0.009, 0.158]	[-0.014, 0.202]	[-0.019, 0.214]	[-0.011, 0.146]	[-0.017, 0.105]
N	118650	44695	23736	15066	10485	7851

Effect of *Rodriguez* For Detainees with No Criminal History, Matched Sample

	(All)	(> 1 month)	(> 2 months)	(> 3 months)	(> 4 months)	(> 5 months)
<i>Rodriguez</i>	0.040***	0.105***	0.198***	0.279***	0.258***	0.172**
Clustering	[0.032, 0.049]	[0.087, 0.123]	[0.182, 0.213]	[0.244, 0.313]	[0.195, 0.322]	[0.086, 0.258]
Wild Bootstrap	[0.006, 0.044]	[0.052, 0.116]	[0.130, 0.203]	[0.095, 0.295]	[-0.018, 0.317]	[-0.186, 0.295]
N	128556	43563	21836	11688	7060	4990

95% CIs are in brackets.

95% Includes control variables for sex, marital status, age, lawful permanent residence, Hispanic ethnicity, and expedited removal.

Table 10: Regression results in exact-matched sample, Not Including Controls

Effect of <i>Rodriguez</i> For all Detainees, Matched Sample							
	(All)	(> 1 month)	(> 2 months)	(> 3 months)	(> 4 months)	(> 5 months)	
<i>Rodriguez</i>	0.053**	0.116***	0.194***	0.244***	0.191***	0.096***	
Clustering	[0.018, 0.088]	[0.062, 0.170]	[0.123, 0.264]	[0.167, 0.322]	[0.133, 0.248]	[0.072, 0.120]	
Wild Bootstrap	[-0.003, 0.082]	[-0.034, 0.148]	[-0.076, 0.235]	[0.081, 0.287]	[0.060, 0.217]	[0.022, 0.111]	
N	247208	88258	45572	26754	17545	12841	

Effect of <i>Rodriguez</i> For Detainees with Criminal History, Matched Sample							
	(All)	(> 1 month)	(> 2 months)	(> 3 months)	(> 4 months)	(> 5 months)	
<i>Rodriguez</i>	0.063*	0.131**	0.159**	0.167**	0.103**	0.031*	
Clustering	[0.005, 0.121]	[0.062, 0.201]	[0.069, 0.250]	[0.075, 0.259]	[0.034, 0.173]	[0.000, 0.062]	
Wild Bootstrap	[-0.006, 0.106]	[-0.009, 0.169]	[-0.012, 0.209]	[-0.022, 0.219]	[-0.011, 0.146]	[-0.008, 0.097]	
N	118652	44695	23736	15066	10485	7851	

Effect of <i>Rodriguez</i> For Detainees with No Criminal History, Matched Sample							
	(All)	(> 1 month)	(> 2 months)	(> 3 months)	(> 4 months)	(> 5 months)	
<i>Rodriguez</i>	0.043***	0.108***	0.212***	0.297***	0.274***	0.181**	
Clustering	[0.028, 0.059]	[0.086, 0.131]	[0.189, 0.235]	[0.257, 0.337]	[0.205, 0.342]	[0.090, 0.272]	
Wild Bootstrap	[-0.013, 0.050]	[-0.029, 0.119]	[-0.095, 0.221]	[0.062, 0.316]	[-0.059, 0.326]	[-0.199, 0.306]	
N	128556	43563	21836	11688	7060	4990	

95% CIs are in brackets.

N Effects Without Selection?

The main finding of this study is that *Rodriguez* caused people not to give up on their cases. This might be called a selection effect, since it changed the composition of people who remained detained more than six months. The effects of *Rodriguez* on release and deportation are therefore mediated through this compositional change.

Conversations with attorneys, however, suggest that *Rodriguez* likely had an effect independent of this compositional change as well. As a test of that hypothesis, I separately analyze the effect of the *Rodriguez* decision on people who were already past their seventh month in detention as of the date of the decision. Note that, because some immigration judges did not begin to conduct *Rodriguez* bond hearings until September 2013, we would expect these estimates to be smaller than the main estimates even absent compositional effects. Note also that these regressions are cross-sectional because there is no previous analogous cohort with which to compare this one (although I do include fixed effects for the month in which the person entered detention).

Table 11 shows these estimates. In the cohort of people already detained six months at the time of the *Rodriguez* decision, there is a smaller effect on bond releases and roughly similar effects on release and deportation, but confidence intervals are large and include zero. This is suggestive evidence, consistent with qualitative work, that release leads to a lower chance of deportation independent of the compositional effects of the hope of release.

Table 11: Estimates of the Effect of the Rodriguez Decision on the Already-Eligible Cohort

	(1)	(2)	(3)
	Bond	Release	Deportation
Ninth Circuit	0.135***	0.139	-0.128
95% CI with Standard Clustering	[0.0853, 0.185]	[-0.0657, 0.344]	[-0.393, 0.138]
95% CI with wild bootstrap	[-.1756, .3047]	[-.6001, 1.58]	[-1.899, .9643]
<i>N</i>	1694	1694	1694

95% confidence intervals in brackets

Standard Errors Clustered at the Circuit Level

Includes Month Fixed Effects

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$