Legal Representation in Immigration Courts: The Impact of Randomly Assigned Observers

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Abstract

In immigration courts, the federal government does not provide guaranteed legal representation even though having legal representation is key to getting relief. This paper reports results of a randomized experiment that allocated law school students to observe immigration judges conducting removal hearings. We investigate whether and to what extent the presence of an observer affected the likelihood that a respondent secures legal representation. We find that an observer's presence increases the likelihood that a judge adjourns the first hearing to allow the respondent time to hire an attorney and also increases the likelihood that the respondent actually secures an attorney at a later date. Investigation of judge heterogeneity shows greater responses among judges who, prior to the experiment, were more inclined to adjourn so that respondents could seek representation.

Keywords: immigration courts, access to justice, legal help, randomized experiment, observers. **JEL Classification**: K00, K37, K40, J15, D63.

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1 Introduction

Every day, thousands of individuals arrive at one of the 72 US immigration courts, often with spouses and small children.¹ The vast majority of these noncitizens lack English proficiency and appear at their initial hearings, called master calendar hearings, with little or no knowledge of immigration laws and without legal counsel. Having legal representation is key to securing permission to remain in the US; a respondent who has secured counsel is 300% to 800% more likely to remain compared to one who has not.² Yet, in immigration courts, the federal government does not provide guaranteed legal representation. Individuals who cannot afford an attorney, or are not aware of the benefits of having one, navigate the system without legal assistance.³ In times when the government rapidly arrests and places people into removal, there are also shortages of trained, affordable counsel, which greatly impacts respondents' ability to secure legal representation. In the interests of promoting a fair hearing, it is critical that immigration judges give sufficient time to find counsel.

This paper reports on a new and potentially scalable program in immigration court proceedings that has the potential to increase access to legal representation. We set up a randomized experiment that allocated non-expert observers (lawyers and students) to judges' immigration hearings in the three New York immigration courts. Judges were informed that these individuals were observing for the purpose of learning about the immigration hearing process. Observes were mainly volunteers from New York Law School and Barnard College and these students were given brief training about the vocabulary and procedure used in immigration courts. The observers were instructed to introduce themselves to the judge and to sit through the hearing, doing nothing except possibly taking notes. The experiment took place over ten months, from July 2023 to May 2024. A weekly randomized protocol assigned the observers to specific judges' master calendar hearings. The hearing schedule was obtained from the calendar circulated to the New York area attorneys by the Immigration and Customs Enforcement prosecutors who are part of the Office of the Principal Legal Advisor (OPLA).

In this paper, we investigate whether and to what extent the presence of these observers affected the likelihood that a respondent secures legal representation. We find that an observer's presence in the first

¹See U.S. Government Accountability Office Report (GAO).

²People who have no counsel rarely apply for any statutory relief. Immigration Courts have no general equitable power. Their authority is limited to the forms of relief specified in the immigration statutes. The government attorneys can grant forms of prosecutorial discretion but not the judge. All of these permissions to remain require some affirmative steps by the respondent. A judge acting alone cannot protect a respondent from removal. These statistics are computed using the EOIR public data set from 2000 to April 2025. The data reflects a range of differential success from a low of 300% to a high of 800% looking at data from 2018 to 2022. We present this data to emphasize that it takes time to adjudicate applications before the court. The process is complex. Table A.1 reports statistics for the years between 2017 and 2022.

³See INA § 292, 8 U.S.C. § 1362.

hearing of a case without an attorney causes an increase in the likelihood that a judge will adjourn the first hearing to allow the respondent time to hire an attorney. Significantly, most of the treatment effect is seen for judges who, prior to our experiment, were relatively more inclined to adjourn "for reason of seeking representation." The observer's presence also causes an increase in the likelihood that the respondent actually secures an attorney later in the proceeding; the presence of a randomly assigned observer increases the likelihood of a case having an attorney by 13.7% within one year of the date of random assignment.

The experiment included two randomly assigned treatment arms: one in which the student-observers merely introduced themselves to the judge, and another in which the student also mentioned that the Court Administration was informed of the observation event. We find that only the second treatment arm has a statistically significant effect, suggesting that the judges' response is stronger in the presence of hierarchical motives/considerations. Because of its potential scalability, our research offers valuable insights into a relatively unexplored yet potentially impactful approach to improving the fairness and transparency of immigration court proceedings.

2 Related Literature

There are several papers arguing that outcomes from the asylum adjudication process depends heavily on the assigned immigration judge. For example, a provocatively-titled study ("Refugee Roulette," Ramji-Nogales et al. (2007)) analyzes a database that includes 133,000 decisions rendered by 884 immigration judges. They document substantial heterogeneity in judge's decisions, even for judges within the same office who receive a similar caseload, depending on which judge adjudicates the case and on the immigrant's nationality. They find a tendency for male judges and more experienced judges to grant asylum less often. Lastly, they show that proceeding durations are longer when the respondent has legal representation but that the effect of having representation on the final outcome is ambiguous. A recent statistical study by Chen and Eagel (2017) also shows that judge identity is a key determinant of immigration court outcomes. They use machine learning methods (random forests) applied to data from 492,903 asylum hearings rendered by 441 judges over the years 1981-2013 to study what variables best predict court decisions. The results show that granting asylum is mainly driven by trends and judicial characteristics with about one third driven by case information, news events and court information. They document substantial autocorrelation in judges' rulings.⁴

Hausman (2016) also observes wide disparities across judges and that, even within the same immigration court, some judges are up to three times more likely than their colleagues to order a respondent deported. Analyzing a database of almost five million immigration cases, Hausman (2016) examines whether the appeals process promotes uniformity across inferior court judges. He concludes that it generally does not, because harsher judges often have immigrants deported early in the process, before they are able to obtain legal representation or to file for asylum (or any other kind of relief). Also, immigrants without lawyers are less likely to appeal the decision.

A study by Eagly and Shafer (2015) empirically examines how having a lawyer affects outcomes of immigration court decisions. Analyzing data on over 1.2 million deportation cases (from 2007-2012), they find that only 37% of all immigrants and 14% of detained immigrants secured representation.⁵ Immigrants with representation were much more likely to seek relief and to obtain relief from removal. Barriers to legal representation were more severe in rural areas and small cities, where almost one-third of cases were adjudicated. Another study by Ryo and Peacock (2021) also explores how legal representation affects the outcomes of immigration cases, analyzing data on 1.9 million removal cases adjudicated between 1998 and 2020. They find that the effect of representation is greater when there is a female judge and/or a more experienced judge. They also observe that the representation effect is larger in times of increasing judge caseloads.

A major challenge to the literature that studies the impact of legal representation is endogeneity of the decision to acquire legal representation with respect to anticipated immigration court outcomes. Some studies have analyzed the effects of randomly provided legal council, but none that we know of in the context of immigration courts.(See, e.g., Grenier and Pattanayak (2011)) Using nonexperimental data, Ryo and Humphrey (2022) analyze the demand and supply-side factors that affect whether an immigrant respondent obtains legal representation. Controlling for the availability of practicing immigration lawyers in close proximity to a respondent's place of residence, other significant predictors of representation include their geographic location, primary language, and the size of their conational social networks. Ryo and Humphrey (2022) argue that addressing the problem of low rates of legal representation requires consideration of linguistic and social isolation factors.

⁴There are a few studies that examine how immigration court decisions respond to current events. For example, Brodeur and Wright (2019) find that the Sept 11, 2001 terrorist attacks led to a 3.2 percentage point decrease in the likelihood that applicants from Muslim-majority countries are granted asylum. Peacock and Ryo (2022) show that Chinese respondents as well as East and Southeast (E/SE) Asian respondents experienced a significantly higher removal rate during the early pandemic period.

⁵Only 2% obtained pro bono representation.

Another aspect of the immigration crisis in recent years has been an increase in the fraction of immigration removals taking place outside the purview of the immigration court system.⁶ As discussed in Koh (2016), the merits of many cases are never discussed in court. Such cases would include expedited removals at the border, removals based on prior removal orders, removal orders for non-lawful residents with aggravated felony convictions, and stipulated removal orders following waivers of the right to a court hearing. Orders are typically signed by immigration judges, with a large fraction issued *in absentia*. Koh (2016) also presents evidence that the judges removal decisions vary depending on when the judge was appointed and depending on the President in office (Presidents Bush, Jr., Obama or Trump).⁷

3 Institutional background, data sources, and descriptive statistics

Next, we provide an overview of the ecosystem around immigration courts in the US. Then, we describe our data source with special attention to New York City immigration courts, which is where our experiment took place.

3.1 Background on immigration court proceedings and importance of representation

Immigration court proceedings are administrative hearings; as such, they are not governed by the Federal Rules of Civil Procedure, nor are they subject to standardized Rules of Evidence. Instead, the Executive Office for Immigration Review (EOIR), a division of the Department of Justice (DOJ), regularly revises the immigration court practice manual that includes all the guidelines for immigration judges.⁸ Immigration judges are government attorneys employed by the Department of Justice's Executive Office for Immigration Review (EOIR). They are on probation for a minimum of the first three years of service and do not have life tenure. There are roughly 700 judges spread across 75 immigration courts throughout the US. The Department of Homeland Security (DHS) is represented in court by an attorney of the Office of the Principal Legal Advisor (OPLA).

An immigration case officially begins when a branch of DHS files a Notice to Appear (NTA) (See Figure A.1). Once an NTA is issued, a new case is opened whose first proceeding is generated by the NTA, and a spot is automatically reserved on a judge's *master calendar hearing*. A master calendar hearing is a hearing that is intended to lay out the timing for the particular proceeding and to allow the

⁶See Benson (2017) about the growth of using removal procedures outside the immigration court.

 $^{^{7}}$ Kim and Semet (2019) also find that the president in office appears to exert an influence over the outcomes of immigration procedures. Analyzing 780,000 custody decisions by immigration judges, they find that nonimmigrants were less likely to obtain bond during the Trump administration than during the two prior administrations.

⁸See See Immigration Court Practice Manual

parties to review the charges or pleadings. If an individual is pursuing relief from removal, the applicant must have time to present the evidence and legal arguments; the hearings that follow the master calendar hearing are called "Merits" or "Individual" hearings. Our observers were randomly assigned to attend master calendar hearings.

Cases are assigned to each immigration judge's *master calendar* on a random rotational basis, using an automatic calendaring process.⁹ Occasionally, a case may have more than one proceeding attached to it, but the first one to be heard is the one related to the NTA: our empirical analysis analysis focuses on that first "master calendar" hearing and the proceeding generated from it.

During the master calendar hearing, immigration judges evaluate the evidence presented by the respondent and decide what we call a proceeding *outcome*. The outcome could be a determination that the respondent has the right to stay in the U.S. (relief granted), or that s/he should be removed. The most common outcome, however, is that the judge adjourns the proceeding to a reset master calendar hearing or to an individual calendar hearing. EOIR requires the judges to code the reason for adjournment. Occasionally, judges administratively close proceedings, temporarily suspending removal without terminating the case. This effectively removes the case from the active docket of a judge with no additional hearings.¹⁰ If a respondent fails to appear for a scheduled hearing, judges may issue an in absentia order of removal. The relative frequency of different outcomes will be described later and shown in Table 1.

Immigration cases sometimes take a long time to decide, almost three years on average, but with a large variance.¹¹ It is generally thought that respondents benefit from the delaying the decision because, in the interim, they are allowed to stay in the country. Starting in 2021, a subset of judges in New York City were required to use a new dedicated dockets process.¹² Dedicated dockets are specialized dockets designed to expedite the proceedings of selected recently arrived families, and are designed so that cases in these dockets can be decided within 300 days of the initial hearing.¹³

A respondent may be represented by a lawyer but only at their own expenses. The government provides attorneys only in very limited cases for some people with lack of mental capacity and for children held in government detention. In New York, there is some funding for nonprofit attorneys to represent

⁹See Chapter 3, Section 1 of the Uniform Docketing System Manual

¹⁰In 2018, the Trump administration restricted the use of administrative closure, and in 2021, the Biden administration restored the authority of immigration judges to use this type of closure.

¹¹In New York we compute that, conditional on being decided during our sample period, a case took 3.6 years to decide and the variance of the durations is 4 years.

¹²The EOIR administrators initiated dedicated dockets in ten cities: Denver, Detroit, El Paso, Los Angeles, Miami, Newark, New York City, San Diego, San Francisco, and Seattle. Source: Dedicated Docket

 $^{^{13}}$ As previously noted, these specialized dockets have been criticized for potentially rushing cases and limiting the fairness of the immigration process. After the completion of this study, the EOIR announced yet another specialized docket for "recent arrivals: see New Dedicated Docket

people in the removal process. Most of this funding comes from the City of New York, or from the New York State Office of New Americans. Some people in removal are able to connect with private paid or *pro* bono attorneys. As previously noted, the presence of legal counsel significantly influences the outcome of immigration proceedings (see Ramji-Nogales et al. (2007), Miller et al. (2015), Eagly and Shafer (2016), Musalo et al. (2024) and Ryo and Peacock (2021)). Our data reveals a stark disparity: a respondent who has secured counsel is 300% to 800% more likely to remain in the United States compared to one who has not.

Most critically, the U.S. law requires that the applicant for asylum file within one year of entry of the United States in almost all situations. People frequently do not have a first hearing before the immigration court for six month or longer. Some individuals will try to meet the one year deadline by finding a way to prepare and file the basic asylum application before that deadline. Other individuals only learn of the deadline at their first Master Calendar hearing in the general advisals the judges read to the people in the court. It is in these cases that having legal representation makes a big difference. Attorneys are familiar with the technical jurisdictional rules and exceptions to the one-year deadlines. A person trying to navigate this process without counsel is very unlikely to meet all of the burdens created by the procedures themselves. Attorneys serve other important functions, perhaps most obvious in the data, is that represented people return to court to appear at later hearings. The court has the power to issue in absentia orders.¹⁴ Attorneys explain the negative consequences and encourage people to continue with their applications before the court.

The national rate of legal representation for all proceedings, whether still pending or closed, has dropped dramatically from 61% in 2017 to 18% in 2025. In the New York courts during this period, the rates of representation declined from a high of 84% to 29%. The diminishing representation rate may be partly reflect that respondents do not have sufficient time to secure legal counsel.¹⁵

3.2 Data sources, sample selection, and descriptive statistics

Our data comes from the EOIR case database, which contains anonymized proceeding data from U.S. immigration courts. These data are released monthly as part of the Electronic Reading Room and pursuant to Freedom of Information Act requests.¹⁶ This dataset contains information on all proceedings

¹⁴Table A.2 reports in absentia rates for respondents with an attorney and those without, showing clear disparities between these two groups.

¹⁵As our study only examined the responses of observed judges in New York, it may be that the higher rates of representation in New York also influenced judges to be patient as it is much more likely to find counsel in New York that in areas with fewer resources.

¹⁶The data is available here: EOIR case database

to US immigration courts between January 2000 and April 2025. The data include detailed information on each proceeding, such as the nationality of the respondent, the nature of the charges, and judge's decisions. The EOIR data are organized at the proceeding-hearing level and include 34.6M observations/hearings for the entire court system.

To construct the *baseline sample* for our hearing-level analysis, we start from the EOIR case database and restrict attention to hearings over which a New York City immigration court has jurisdiction.^{17, 18} Essentially all of these hearings (97.6%) pertain to removal cases; we drop all other hearings to ensure homogeneity. We then drop hearings with adjournment reasons "Data Entry Error"¹⁹. We then focus on first hearings and drop those with adjournment reason "Completion prior to hearing".²⁰ We further restrict attention to first hearings held during our sample period (July 10, 2023 to May 10, 2024) of which we can match the identity of the judges to our experimental data.²¹ At this point, the dataset includes 96 judges and 129,664 hearings. We restrict attention to the first hearing of these cases (as opposed to subsequent hearings) to ensure that the hearings we analyze are homogenous and, also, to avoid hearing and proceeding selection issues (this first hearing is necessarily a master calendar hearing). We call this our baseline sample.

Table 1 presents descriptive statistics for hearing-level variables in the baseline sample. Approximately 14% of hearings were assigned an observer, and 6% were effectively observed (compliance with the experimental protocol will be discussed later). Attorneys entered an appearance in 8% of hearings, and around 6% of hearings were dedicated dockets. The data indicates that asylum applications were filed in 0.2% of the master calendar hearings. The most frequent nationalities among respondents were Ecuadorian (19%), Venezuela (11%), Chinese (8.74%), and Colombian (6%). In total, 205 nationalities were represented in the data.

¹⁷Specifically, we keep the approximately 3.6M observations/hearings with *base_city_code* equal NYC, NYB, NYV.

¹⁸There are three immigration courts in New York City. These courts are housed within multipurpose federal buildings and the court system is one tenant in the building. Entry to these buildings requires people to pass through a metal detector and their possessions and bodies are screened by security personnel. The largest court is labeled in the data NYC and is located on the 12^{th} and 14^{th} floors of 26 Federal Plaza. This court is commonly referred to as 26 Federal Plaza. This court covers 73% of the hearings in our data and it currently staffs 36 immigration judges. NYB, commonly called 290 Broadway, is the second largest court (14% of the hearings) and is located on multiple floors of the federal building at 290 Broadway. As of October of 2024 the court has a staff of 27 judges. The court labelled NYV, commonly called Varick, is the third court (12% of the hearings) and is located at the 5^{th} floor of 201 Varick street. It currently staffs 15 judges. Source: Immigration Courts. Over the years these courts represent 3.4M hearings and 299 judges in the EOIR dataset.

¹⁹This step drops 98,630 hearings.

 $^{^{20}}$ Thus dropping a further 56,727 hearings.

²¹For the 10,000 cases with Initial Appearance Docket (IAD) judge we replaced the IAD code with the code of the judge in the next hearing. We drop 1,402 cases assigned to visiting judges (i.e. $ij_code="V11"$) as they are not part of the randomization protocol. We finally drop 500 observations, of which we cannot match the identity of the judges, and 46 observations where the NTA date post-dated the first master calendar hearing

	Ν	Mean	SD
Observer(Assigned)	$129,\!664$	0.1367	0.3435
Observer	$129,\!664$	0.0587	0.2351
Attorney in hearing	$129,\!664$	0.0815	0.2736
Dedicated docket	$129,\!664$	0.0558	0.2296
Asylum application	$129,\!664$	0.0023	0.0482
Respondent from Ecuador	$129,\!664$	0.1902	0.3924
Respondent from Venezuela	$129,\!664$	0.1117	0.3150
Respondent from China	$129,\!664$	0.0874	0.2825
Respondent from Colombia	$129,\!664$	0.0616	0.2404
Last hearing	$129,\!664$	0.1033	0.3044
Absentia	$129,\!664$	0.1551	0.3620
Administrative Closure	$129,\!664$	0.0042	0.0645

Table 1: Descriptive Statistics

Notes: Baseline sample: all master calendar hearings between 10^{th} July 2023 - 10^{th} May 2024.

4 The court observation experiment

Prior to our experiment, Professor Lenni Benson, working with law students from New York Law School and a group from NYU Law School, began attending immigration court hearings to gather qualitative information on how unrepresented people were being treated as they navigated the unprecedented lengthy lines to enter the federal buildings. They also sought information on how judges were handling cases where Customs and Border Patrol had listed random New York city area nonprofit addresses as the respondent's address rather than a home address. Students selected the judge and date and the hearing time and filled out reports of their observations. Information gathered during this process was used by Professor Benson and New York community advocates to alert the community to systemic access problems as well as problems with government documents being served and to note how the judges were providing advisals about the right to counsel. Based on this preliminary project, Professor Benson and the co-authors designed the current project and, in July of 2023, began to randomly assign observers to court hearings and gather the quantitative data used in our study.

4.1 The experimental design

We recruited 83 observers from the New York Law School and Barnard College. Students volunteered to be part of the experiment and received an initial online training to understand how immigration hearings typically work and the general project goals.²² During the ten-month experimental period spanning from

 $^{^{22}{\}rm Slides}$ are available at this link: Observer training

July 10th, 2023, to May 10th, 2024, participating students received a weekly email containing a link to a Qualtrics survey. This survey asked them to indicate their available days for the following week. This information was then used to randomly assign students to judges during that weeks' master calendar hearings. The assignment process occurred weekly, matching available students with the judges' calendars provided by the Office of the Principal Legal Advisor (OPLA). Students were randomly allocated to all three New York immigration courts. Because each master calendar hearing day typically involves 30 proceedings, observers were able to observe multiple cases per judge-day of assignment. Throughout the sample period, students were assigned to a total of 462 hearings, for a total of 18,117 individual proceedings.

Observers received detailed instructions for the observation via email, including information about the assigned judge, the court's address, and the specific message they were to deliver. The experiment involved two randomly assigned treatment arms, each with a distinct message:

- Message 1: "Thank you for having me in your court, your honor. My name is (...), I am participating in the immigration court observation project coordinated by Professor Benson at New York Law School. I appreciate this opportunity to learn about the immigration courts.".
- Message 2: "Thank you for having me in your court, your honor. My name is (...). Professor Benson at New York Law School asked me to thank you for this opportunity to learn. We have also let the Court administration know I am observing."

Although the court was informed of the list of observers for the week, the specific message-observerjudge combination remained unknown before the hearings to the judges and the head of the court. After each master calendar hearing, observers were asked to complete a post-observation survey on Qualtrics. The surveys were not always completed: refer to Section 4.3. If completed, the surveys were uploaded to our server.²³

4.2 Check that observers were randomly assigned

The observers were randomly assigned to master calendar hearings in each week. Our baseline sample (see Section 3.2) restricts attention to a subset of these hearings, i.e., to those cases for which the master calendar hearing was also the case's first hearing.²⁴

²³The questionnaire is available at this link: Observer survey

 $^{^{24}}$ We also drop cases from judges who received no cases in the 12 months preceding our experiment. The final sample is of 118,302 hearings. This step ensures the comparability of our final estimation sample across different specifications.



Notes: Dots are the p-values from logistic regressions, where the dependent variable is whether an observer was randomly assigned in a given week and the independent variables include respondent, case, judge, attorney characteristics. Dashed line indicates a 5% significance level. Red crosses highlight instances where the null hypothesis of random assignment was rejected. Sample: all hearings between July 10th 2023, and May 10st 2024.

We can check that the assignment of observers is random within our baseline sample by examining whether any judge/court/respondent characteristics predict the treatment status (i.e. whether a hearing is observed). We consider as potential predictors the fraction of judge's hearings held in the main court (NYC), the gender of the judge, and whether the judge was confirmed (more than 3 years of service) and the gender of the respondent. At the hearing level, the predictor variables include whether an attorney is present, whether the hearing was conducted in a foreign language, the nationality of the defendant. Under random assignment, all of these variables should not predict treatment status. The randomization was performed weekly, so we examine whether these covariates predict treatment status week-by-week.

Figure 1 presents the p-values from logistic regressions where the dependent variable is whether an observer was assigned in a given week and the independent variables includes the following different sets

of predictor variables:

- Whether the hearing was at the main court (NYC)
- Indicator variables for tenured judges and gender (judge and respondent)
- Indicator variable for attorney presence at the hearing
- Indicator variable for hearings not conducted in English
- Indicator variables for four nationalities with the greatest number of immigrants: Mexican, Ecuadorian, Colombian, and Chinese.

In the figure, the dashed line indicates a 5% significance level. Over the 35 weeks of random assignment, two weeks (Nov 20 and Dec 11) had only one observer, and one week (Jan 8) lacked schedule reports. Red crosses highlight 9 instances where the null hypothesis of random assignment was rejected, suggesting an approximate 3% rejection rate. The results are consistent with random assignment of the observer status within each week.²⁵

4.3 Check of implementation accuracy

Although our experiment assigned students to observe court proceedings, the assigned students were not always able to attend the proceedings – mostly because of judge schedule changes, cancellation of hearings, or difficulties accessing the building. We refer to all these problems collectively as "implementation accuracy."

We check the extent of implementation accuracy by estimating the following OLS regression model:

$$Observer_{pjt} = \alpha + \beta Observer(Assigned)_{pjt} + \mu_j + \gamma_t + \varepsilon_{pjt}$$
(1)

where, $Observer_{pjt}$ is an indicator variables that takes value one if a post-observation survey was uploaded for proceeding p of judge j in week t. $Observer(Assigned)_{pjt}$ is an indicator variables that takes value one if an observer was randomly assigned to proceeding p of judge j in week t. The model includes week of hearing fixed effects (γ_t), because our randomization was done weekly. In addition, our specification includes judge fixed effects (μ_j) and week of notice-to-appear (NTA) fixed effects, where the latter are intended to control for any macroeconomic events affecting immigration patterns. With a randomized

 $^{^{25}}$ We also performed balancing tests using a weekly t-test on the same covariates, which gave a similar rejection rate.

experiment, it is not strictly necessarily to control for additional covariates, as they are exogenous with respect to the random assignment. However, the inclusion of covariates that explain some of the variation in the outcomes often leads to greater precision in the estimated treatment effects, as discussed in Hahn (1998). Standard errors are clustered at judge-week of the hearing level.

As seen in Table 2 compliance with our random assignment protocol was imperfect. Although the first-stage F-statistic is strong, randomly assigning an observer to a master calendar hearing increases the probability of that hearing being observed by only 34 percentage points.²⁶ Therefore, when analyzing the impact of an observer on court outcomes we will present both OLS-ITT (Intention-to-Treat) and IV-TT (Treatment on Treated) estimates. The IV-TT estimates use the random assignment observer status as an instrument for the actual observer status. That is, the randomization can be viewed as making a hearing randomly eligible to have an observer. Note that an IV estimator with a discrete instrument usually identifies a LATE (local average treatment effect) estimate, which is the treatment effect for the subgroup of compliers-those who receive an observer only because of our random assignment. In our context, there are no hearings that would receive an observer in the absence of our random assignment (i.e., no "always-takers"), so the group of compliers corresponds to the treated group.(see the definitions of these subgroups in Imbens and Angrist (1994))

As previously mentioned, the noncompliance in our experiment is attributable to a few different factors, including last minute changes in the judge's schedule and cancellations of some originally scheduled hearings. Some noncompliance may also be due to the fact that we relied on volunteer (unpaid) law school students as observers, who may not have been highly motivated. We view some noncompliance as unavoidable and not necessarily a drawback of our experiment. For example, our observation experiment could be scaled up using lowly compensated observers who also may not comply 100% with the experimental protocol.

4.4 No anticipation effects

To check against the possibility (unlikely, in our view) that judges had advance knowledge of which hearings were going to be observed, we check whether judges were more likely to decide certain proceedings in advance of the observed hearing, as opposed to during it. If, hypothetically, the judge had advance knowledge of which hearings were going to be observed and wanted to decide a particular proceeding

²⁶In all specifications, the first-stage F-statistic of the strength of the assignment variable as an instrument (equivalent to the square of the t-test since we have one instrument) is well above 10. The table also shows that the implementation accuracy is not related to the treatment type, because the coefficients $Observer_{pjt}(Assigned) : Message1$ and $Observer_{pjt}(Assigned) : Message2$ are the same.

Dep.Var.	Observer	Observer	Observer	Observer
	(1)	(2)	(3)	(4)
Observer(Assigned)	$\begin{array}{c} 0.343^{***} \\ (0.025) \end{array}$		$\begin{array}{c} 0.337^{***} \\ (0.025) \end{array}$	
Observer(Assigned): Message 1		0.327^{***}		0.329^{***}
Observer(Assigned): Message 2		(0.033) 0.357^{***} (0.033)		(0.030) 0.345^{***} (0.033)
Observations	118,302	118,302	108,094	108,094
Judge FE	Yes	Yes	Yes	Yes
Week Ass. FE	Yes	Yes	Yes	Yes
Week NTA FE	Yes	Yes	Yes	Yes

Table 2: First stage estimates.

Notes: Coefficient (standard error in parentheses) of the effect on having an Observers on Observer (Assigned) the day of the master calendar hearing to a judge. Columns 3 and 4 report estimates in the estimating sample sub-sample of cases with no attorneys in the first hearing, which represents our main estimation sample in Sections 5.1 and 5.2. Regressions include Judge, Week of the hearing and Week of the NTA FEs. SEs are clustered at the judge-week level. Significance at 10% (*), 5% (**), and 1% (***). Sample: all hearings between July 10^{th} 2023, and May 10^{st} 2024.

without being observed, s/he could make a decision in advance. For this analysis, we add to the baseline sample described in Section 3.2 the adjournment reason "Completion prior to hearing", and we estimate eq. 2 considering this outcome. Table A.3 shows that, while some proceedings (3%) are decided "ahead of schedule," the likelihood is not higher for observed hearings suggesting, as expected, that judges did not know in advance which of their hearings would be observed. We provide details of our estimated model in next section.

5 Empirical results

In this section, we first study how the presence of an observer at a case's first hearing affects that hearing's outcome in a key dimension: whether the judge adjourns the hearing to give the respondent time to secure an attorney (Section 5.1). Then, in Section 5.2, we show that the observer's presence improves the likelihood that a respondent actually secures an attorney in the case's future hearings.

5.1 Hearing-level impact: adjournment to "seek attorney representation"

Because we are focusing on the respondent's ability to secure representation having started without one, we drop from our sample proceedings in which the respondent already has an attorney at the start. This brings the sample size down to 108,094. We refer to this sample as to our "estimation sample."

We estimate the following linear probability model:

$$y_{pjt} = \alpha + \beta \text{Observer}_{pjt} + \mu j + \gamma_t + \varepsilon_{pjt}$$
(2)

where: y_{pjt} is an indicator for whether the first master calendar hearing in proceeding p for judge j in week t is adjourned to see attorney representation; Observer_{pjt} is an indicator variable that takes value one if the first hearing of proceeding p was observed (and not merely if an observer was scheduled to attend); μj and γ_t are judge- and week-of-the-hearing fixed effects. By including judge fixed effects, we control for the known variability across judges (see Figure 2) and rely in estimation on within-judge variation in observation status. As previously noted, we also include week of master calendar hearing and week of the NTA fixed effects. Standard errors are clustered at the judge-week level.²⁷

Table 3 shows the estimates when $y_{pjt} = 1$ denotes that the reason for adjournment is "respondent to seek representation," else $y_{pjt} = 0$. This outcome means that, at the end of the hearing, the judge asks the respondent to hire an attorney and schedules a second hearing some months later. Column 1 presents the OLS estimates of β from eq: (2). Without accounting for observer compliance issues, the effect of having an observer in the courtroom increases the likelihood that a hearing outcome is "adjourned for respondent to seek representation" by 14%, compared to non-treated. Column 2 is the reduced form (ITT): the effect of randomly assigning an observer on outcome y_i . The effect, 6%, is somewhat smaller than in col. 1 due to partial compliance (some observers, for the reasons discussed in Section 4.3, fail to show up at the assigned courtroom). The best estimate, in our view, is column This estimate uses the observer's experimental assignment $Observer_{pit}(Assigned)$ to a hearing to 3. instrument for $Observer_{pjt}$, i.e., for whether the hearing was actually observed (TT). This estimate (0.061 pp/.320 = 19%) is somewhat higher than the OLS estimate due, perhaps, to the fact that some observers ended up observing judges other than the one they were assigned to observe, a shift that may be correlated with judge characteristics:²⁸ the TT estimate corrects for this endogeneity. In any case, all the estimates are statistically and economically significant and, roughly, in the same ballpark quantitatively.

²⁷Recall that our procedure assigns observers to judges randomly weekly.

²⁸Certain judges may be more inclined to deny access and, also, be differentially responsive to the treatment. In some cases, the judge was ill and court personnel moved the observer to a new courtroom.

Dep.Var.	SeekRep	SeekRep	SeekRep
Model	OLS	OLS-ITT	IV-TT
	(1)	(2)	(3)
Observer	0.046***		0.061**
	(0.013)		(0.031)
Observer(Assigned)		0.020^{**}	
		(0.010)	
Observations	108,094	108,094	108,094
Judge FE	Yes	Yes	Yes
Week Ass. FE	Yes	Yes	Yes
Week NTA FE	Yes	Yes	Yes
Mean Y	0.320	0.320	0.320
F-first			183.9

Table 3: Impact of having an observer on seeking representation.

Notes: Coefficient (standard error in parentheses) of the effect of having an Observers or an Observer (Assigned) the day of the master calendar hearing to a judge on adjournment is "respondent to seek representation". Regressions include Judge, Week of the hearing and Week of the NTA FEs. F-first is the first-stage Fstatistic of the significance of the instrument. SEs are clustered at the judge-week level. Significance at 10% (*), 5% (**), and 1% (***). Sample: all hearings with no attorneys in the first hearing between July 10th 2023, and May 10st 2024.

Mechanism The bulk of the effects on "seeking representation" seem to arise in our treatment 2 (refer back to page 10), i.e., when the judge is told that the court administration has been made aware that the judge is being observed on that day. Absent this communication (i.e., in treatment 1), the effects are similar in magnitude but are not statistically significant in our preferred specification (IV-TT): see Table 4. This observation suggests that part of the judge's response to the observer is due to the judge's perception of being monitored.

Heterogeneous effects by judge Next, we explore the heterogeneous effects of our treatment by judge type. We classify judges according to their 12-month-pre-treatment probability of adjourning unrepresented first hearings for reason of "seeking representation." We interpret this probability as the judge's individual propensity to facilitate representation. We are interested in whether our treatment has a greater effect on judges who were more (or less) inclined to facilitate representation.

The distribution of judge propensities is shown in Figure 2. The median judge has a pre-treatment probability of adjourning for reason of "seeking representation" equal to 0.31. However, the standard deviation of the judge distribution is relatively large: 0.13. This large standard deviation means that being randomly assigned to one judge or another causes a predictable disparate advantage or disadvantage for

Dep.Var.	SeekRep	SeekRep	SeekRep
Method	OLS	OLS-ITT	IV-TT
	(1)	(2)	(3)
ObserverXMess.1	0.036^{**}		0.053
	(0.018)		(0.040)
ObserverXMess.2	0.064^{***}		0.077^{**}
	(0.018)		(0.036)
Observer(Assigned)XMess.1		0.018	
		(0.014)	
Observer(Assigned)XMess.2		0.028**	
		(0.013)	
Observations	108,094	108,094	108,094
Judge FE	Yes	Yes	Yes
Week Ass. FE	Yes	Yes	Yes
Week NTA FE	Yes	Yes	Yes
Mean Y	0.320	0.320	0.320
Diff_Eff	0.0283	0.00971	0.0236
pval	0.237	0.578	0.628
F-first			105.6

Table 4: Impact of different messages on seeking representation.

Notes: Coefficient (standard error in parentheses) of the effect of having an Observers or an Observer (Assigned) the day of the master calendar hearing to a judge on adjournment is "respondent to seek representation". Messages 1 and 2 are described in Section 4.1. Diff_Eff (pval) is the difference of the estimated coefficients (the p-value of the test for the difference). F-first is the first-stage F-statistic of the significance of the instrument. Regressions include Judge, Week of the hearing and Week of the NTA FEs. SEs are clustered at the judge-week level. Significance at 10% (*), 5% (**), and 1% (***). Sample: all hearings with no attorneys in the first hearing between July 10^{th} 2023, and May 10^{st} 2024.

the respondent in his/her ability to secure representation. We refer to this disparity as a "representation roulette."²⁹

As shown earlier, for the average judge, the presence of an observer increases the baseline probability of adjournment for seeking representation by about 20 percent (col. 3 of Table 3). Here, we estimate the heterogeneous effect of this treatment by whether the treated judge lies above or below the median in Figure 2. In col. 3 of Table 5 we see that the entirety of the observer effect arises from judges above the median distribution (40 percent of the control mean); the treatment has no impact on judges with below-median pre-experimental propensity to adjourn for "seeking representation." This suggests that the the observer's presence actually increases, to some degree, the variance implied by random assignment to judges. We will return to this point in our concluding remarks.

 $^{^{29}\}mathrm{Refer}$ back to the discussion of "refugee roulette" at page 3.

Figure 2: Representation roulette



Notes: This figure shows the distribution of judges distinguished by their pre-experiment propensity to adjourn for "Respondent to seek representation". The dashed line represents the median distribution. For graphical presentation, extreme values of the distribution were excluded.

Heterogeneous effects by case type Finally, for descriptive interest, in Appendix B we explore the heterogeneous effects of our treatment on different proceeding types, including "dedicated dockets" (proceedings of recently arrived families that are slated for a relatively fast decision, refer to Section 3.1), asylum applications, and most-common respondent nationalities.

5.2 Proceeding-level impact: securing attorney representation

In Section 5.1, we have shown that experimentally assigning an observer to a first hearing causes a substantial increase in the probability that the judge adjourns the case specifically to give the respondent more time to "seek representation." Here, we examine whether our experimental intervention caused an *actual increase* in attorney representation, as seen 12 months after the end of our experimental observation period (April, 2025). To this end, we re-estimate eq. (2) with the dependent variable y_{pjt} now being equal to 1 if the respondent *eventually* secured an attorney, i.e., if the respondent initially appeared without an attorney at the master calendar hearing and then later is recorded in the EOIR database as having attorney representation any time after.

For this proceeding-level analysis, we follow the 108,094 proceedings in the original estimation sample until they are adjudicated or, if not adjudicated, up to twelve months after the conclusion of the experiment (until April, 2025). At that time, only 45% have been adjudicated. Thus, there is right censoring in

Dep.Var.	SeekRep	SeekRep	SeekRep
Method	OLS	OLS-ITT	IV-TT
	(1)	(2)	(3)
Observer	0.016		-0.029
	(0.020)		(0.051)
ObserverXJudgeAboveMed	0.045^{*}		0.132^{**}
	(0.025)		(0.060)
Observer(Assigned)		-0.011	
		(0.017)	
Observer(Assigned)XJudgeAboveMed		0.047**	
		(0.021)	
Observations	108.094	108.094	108.094
Judge FE	Yes	Yes	Yes
Week Ass. FE	Yes	Yes	Yes
Week NTA FE	Yes	Yes	Yes
Mean Y	0.320	0.320	0.320
Sum_Eff	0.0607	0.0354	0.103
pval	0.001	0.004	0.005
F-first			63.26

Table 5: Impact of having an observer on seeking representation.

Notes: Coefficient (standard error in parentheses) of the effect of having an Observers or an Observer (Assigned) the day of the master calendar hearing to a judge on adjournment is "respondent to seek representation". JudgeAboveMed are judges with preexperiment propensity to adjourn "Respondent to seek representation" above the meadian. Sum_Eff (pval) is the sum of the estimated coefficients (the p-value of the test for the sum). F-first is the first-stage F-statistic of the significance of the instrument. Regressions include Judge, Week of the hearing and Week of the NTA FEs. SEs are clustered at the judge-week level. Significance at 10% (*), 5% (**), and 1% (***). Sample: all hearings with no attorneys in the first hearing between July 10th 2023, and May 10st 2024.

that respondents in unresolved cases may still eventually obtain counsel, which could lead to downward bias in our estimates. Also, we note that the judge hearing a case is not necessarily the same judge that heard that case's first master calendar hearing. To avoid potential concerns about endogeneity of the judge fixed effects, in our estimation, the judge fixed effects pertain to the judge who heard the case at its first master calendar hearing (i.e., the time when our randomization took place).

Table 6 reports the estimates of β from eq. (2) and variants of it. Column 1 shows that the presence of an observer increases the likelihood of the respondent eventually securing an attorney by 7% (0.02/0.298=7%), compared to hearings without an observer. Column 2 displays the reduced form, or Intention-to-Treat (ITT) estimate, which assesses the effect of randomly assigning an observer: the estimated ITT effect is 4.7%, somewhat smaller than the OLS estimate in Column 1, a difference attributable to partial compliance with the experimental assignment. Column 3 reports what we view as the best estimate, derived using the experimental assignment of an observer to a hearing as an instrument for actual

Dep.Var	Attorney	Attorney	Attorney	Attorney	Attorney	Attorney
Method	OLS	ITT	IV-TT	OLS	ITT	IV-TT
	(1)	(2)	(3)	(4)	(5)	(6)
Observer	0.020^{**}		0.041^{**}			
	(0.010)		(0.020)			
Observer(Assigned)		0.014^{**}				
		(0.007)				
ObserverXMess.1				0.005		0.022
				(0.015)		(0.027)
ObserverXMess.2				0.034^{**}		0.042^{*}
				(0.015)		(0.024)
Observer(Assigned)XMess.1					0.008	
					(0.009)	
Observer(Assigned)XMess.2					0.015^{*}	
					(0.009)	
Observations	108,094	108,094	108,094	108,094	108,094	$108,\!094$
Judge FE	Yes	Yes	Yes	Yes	Yes	Yes
Week Ass. FE	Yes	Yes	Yes	Yes	Yes	Yes
Week NTA FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y	0.298	0.298	0.298	0.298	0.298	0.298
Diff_Eff				0.0290	0.00781	0.0200
pval				0.147	0.505	0.539
F-first			183.9			105.6

Table 6: Impact of having and observer on having an attorney

Notes: Coefficient (standard error in parentheses) of the effect of assigned observers or effective observers on whether the proceeding has a respondent's attorney within one year after the end of our randomization period (Attorney). Messages 1 and 2 are described in Section 4.1. Diff_Eff (pval) is the difference of the estimated coefficients (the p-value of the test for the difference). F-first is the first-stage F-statistic of the significance of the instrument. Regressions include Judge, Week of the hearing and Week of the NTA FEs. SEs are clustered at the judge-week level. Significance at 10% (*), 5% (**), and 1% (***). Sample: all hearings with no attorneys in the first hearing between July 10th 2023, and May 10st 2024.

observer presence. This TT estimate is 0.041, which represents a 13.7% increase (0.041/0.298=13.7%) in the likelihood that a respondent eventually secures an attorney.

Columns 4-6 of Table 6 investigate heterogeneity in the estimated treatment effects depending on whether the judge is told that the court administration is aware of the observation (Message 1 versus Message 2, as described on page 10). The estimates indicate that the observer effect arises when the court administration is aware of the observation (Message 2). Estimates from treatment 1 are small and are not statistically significant in our preferred specification (IV-TT).

In sum, we find that our experimental intervention increased the likelihood of having attorney representation after the first hearing of an observed proceeding, particularly when judges were informed that the court administration was aware they were being observed. The effect was largest among judges who, prior to our intervention, showed a greater inclination to allow respondents time to seek representation.

6 Conclusions

In immigration courts, the government does not provide guaranteed legal representation, and individuals who cannot afford an attorney must navigate the system without legal assistance. Given the complexities of immigration law, having an attorney is crucial for getting relief. In our data, whether a case is adjourned to allow respondents time to seek legal representation depends heavily on the judge to which the case is assigned, consistent with a phenomenon that the literature has termed "immigration roulette." Our data also show that a respondent with legal counsel is 300% to 800% more likely to remain in the US than one without. Thus, whether immigration judges give respondents sufficient time to secure counsel is a critical determinant of court outcomes.³⁰

To increase the likelihood that respondents have an opportunity to secure counsel, this paper explores a new, simple and scalable intervention: court observation. It reports results of a randomized experiment that allocated observers (mainly law school student volunteers) to judges' immigration hearings. We investigate whether and to what extent the presence of these observers changed judicial behavior and increased the likelihood that respondents secure representation at a later date.

We find that having an observer increased the likelihood that the judge adjourns the first hearing to allow the respondent time to hire an attorney. Most of the impact occurs on days when the judge was told that the court administration was informed about that day's observation, suggesting that part of the judge's behavioral response to being observed may stem from hierarchical motives/considerations. We also investigate whether there are heterogeneous effects across judges and find a greater response to observation for judges who, prior to our experiment, were more inclined to adjourn "for reason of seeking representation." Lastly, we examine whether our experimental intervention caused an actual increase in attorney representation after the first hearing and find that it did. Initially unrepresented respondents were more likely to be represented in later hearings if an observer was allocated to their first hearing.

Given the significant impact that legal representation has on case outcomes, our findings point to simple strategies that can be used to improve fairness in the immigration court system. Even a simple intervention, such as external observation, can have sizable effects in the courtroom. The evidence suggests that the pressure stemming from being observed, particularly when the court administration is made aware of the observation, is a key driver of the effects. This insight suggests that even roughand-ready monitoring mechanisms which, in our case, rely on unpaid students, can potentially improve procedural fairness and transparency within the court system. More generally, our results highlight the

³⁰The problem is further compounded by shortages in qualified, affordable counsel in many regions of the country.

importance of accountability and public scrutiny in such settings.

This study focuses on a timely and pressing social issue: the challenges faced by unrepresented individuals navigating the complex US immigration court system. By shedding light on the impact of observation on court proceedings, the research contributes to the ongoing conversation about improving access to justice, reducing disparities, and ensuring fairness in immigration proceedings.

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Online Appendix

A Appendix Figures and Tables



U.S. Department of Homeland Security		Ν	lotice to Appear
In removal Proceedings under section	on 240 of the Immigration a	nd Nationality Act:	
Subject ID :	FIN #:		
	DOB:	File No:	
		Event No:	
In the Matter of			
in the Matter of			
Respondent:		cun	rently residing at:
2	(Number Street site and 7)	(P code)	
	(rounder, Sheer, city and Zi	Ir coue)	
 You are an arriving alien. 			
2. You are an alien present in the United St	ates, who has not been admitted or p	aroled.	
3. You have been admitted to the United St	ates, but are removable for the reaso	ns stated below.	
The Department of Homeland Security alleges t	hat you:		
and my more	man man	man and	man man
non ma	mm/ mm	my my m	
~ ~~~~ ~ ~ ~			
an at to s	ow why you should not be removed	from the United States based on the	
(Date) (Time)		and an older start of the	
harge(s) set forth above.		(T-4 CI · OM)	
	(Sign	anire and 1itle of Esuing Officer)	
Date:			
		(City and State)	
			Form 1965

Year	Relief for	Relief for
	Represented	Un-represented
2019	0.17	0.05
2020	0.14	0.03
2021	0.18	0.02
2022	0.21	0.03

Table A.1: Reliefs by Representation Status

Notes: Statistics from the EOIR case database (cases filed from 2000 through April 2025). Relief represents the fraction of cases in which the immigration court's decision was "Relief Granted" conditional on having an application for asylum of the type: "ASYL", "ASYW", "WCAT", "245", "VD", "42A", "42B". Represented and Un-represented: are respondent with an attorney or not, respectively.

Table A.2: In Absentia by Representation Status

Absentia for	Absentia
Represented	Un-represented
0.02	0.17

Note: Statistics from the EOIR case database (cases filed from 2000 through April 2025). Absentia represents the fraction of cases in which the immigration court's decision was in absentia. Represented and Un-represented: are respondent with an attorney or not, respectively.

Dep.Var.	Comp.Pre.	Comp.Pre.	Comp.Pre.
Method	OLS	OLS-ITT	IV-TT
	(1)	(2)	(3)
Observer	0.001		-0.001
	(0.003)		(0.007)
Observer (Assigned)		-0.001	
		(0.002)	
Observations	$121,\!842$	$121,\!842$	$121,\!842$
Judge FE	Yes	Yes	Yes
Week Ass. FE	Yes	Yes	Yes
Hearing FE	Yes	Yes	Yes
Week NTA FE	Yes	Yes	Yes
Mean Y	0.0285	0.0290	0.0290
F-first			194.2

Table A.3: No anticipation/Placebo test: "IJ Completion prior to hearing".

Notes: Coefficient (standard error in parentheses) of the effect of having an Observers or an Observer (Assigned) the day of the master calendar hearing to a judge on judge completion prior to the master calendar hearing. F-first is the first-stage F-statistic of the significance of the instrument. Regressions include Judge, Week of the hearing and Week of the NTA FEs. SEs are clustered at the judge-week level. Significance at 10% (*), 5% (**), and 1% (***). Sample: all hearings between July 10th 2023, and May 10st 2024

B Heterogeneous effects on different proceeding types.

Table B.1 shows the four respondent nationalities that are most common in our database - Ecuador (19%), Venezuela (11%), China (9%), and Colombia (6%). Other nationalities have a proportion less than 5%. The last column of the table shows the percentage of individuals that have an attorney at the first hearing, which is highest for Chinese respondents (22%). For the other three nationalities shown in the table, 2.5% or less have an attorney at the first hearing.

Table B.2 explores whether our observer treatment had heterogeneous effects for different types of proceedings and for respondents of different nationalities. Col. 1 shows that the treatment does not have a heterogeneous effect on "dedicated dockets," indicated by D.D., which are proceedings of recently arrived families that are slated for a relatively fast decision (refer to Section 3.1). Nor is there a heterogeneous effect for the very few proceedings which involve an asylum application (col 2.)³¹

Finally, we compute separate observer effects for the most common respondent nationalities. The observer effect is greatest for respondents from Colombia, Ecuador and Venezuela and is basically zero for Chinese respondents. Recall that respondents from the Latin American countries much lower representation rates at their first hearing in comparison to the Chinese respondents, (see Table B.1). This suggests that nationalities with low rates of attorney representation at the first hearing experience greater potential benefits from having an observer present.

Nationality	% Sample	% Attorney in hearing
Ecuador	19	2.5
Venezuela	11	1
China	9	22
Colombia	6	2.5

Table B.1: Nationalities and Representation

Notes: The table reports the distribution of the four main nationalities of the respondents in our sample. Other nationalities are presented in a proportion smaller than %5. % Attorney in hearing represents the proportion of master calendar hearings with an attorney present in the hearing.

 $^{^{31}}$ Only 301 of our observations feature an asylum application. This makes sense because most asylum applications are filed after the first hearing.

Dep.Var.	SeekRep	SeekRep	SeekRep
Method	IV-TT	IV-TT	IV-TT
	(1)	(2)	(3)
ObserverXD.D.Case	-0.109		
	(0.129)		
Observer	0.069^{**}	0.061^{**}	
	(0.031)	(0.031)	
D.D.Case	0.279^{***}		
	(0.030)		
ObserverXAsylum Appl.		-0.809	
		(1.511)	
Asylum Appl.		0.256^{**}	
		(0.104)	
ObserverXOther Nat.			0.055^{*}
			(0.030)
ObserverXEcuador			0.140^{**}
			(0.055)
ObserverXVen.			0.065
			(0.070)
ObserverXChina			0.007
			(0.035)
ObserverXColombia			0.080
			(0.075)
Ecuador Nat.			0.159***
			(0.007)
Ven. Nat.			0.144***
			(0.008)
China Nat.			-0.247***
			(0.006)
Colombia Nat.			0.154***
	100.004	100.004	100.004
Observations	108,094	108,094	108,094
Judge FE	Yes	Yes	Yes
Week Ass. FE	Yes	Yes	Yes
Week NTA FE	Yes	Yes	res
Mean Y	0.320	0.320	0.320
Sum_En	-0.0390	-0.749	
pvai E finat	0.750	0.020	95 90
г-ilfSt	80.42	91.93	20.29

Table B.2: Heterogenous effects.

Notes: Coefficient (standard error in parentheses) of the effect of having an Observers or an Observer (Assigned) the day of the master calendar hearing to a judge on adjournment is "respondent to seek representation". In col.3, Other Nat. is an indicator variable for respondents not from Ecuador, Venezuela, China or Colombia. Sum_Eff (pval) is the sum of the estimated coefficients (the p-value of the test for the sum). Ffirst is the first-stage F-statistic of the significance of the instrument. Regressions include Judge, Week of the hearing and Week of the NTA FEs. SEs are clustered at the judge-week level. Significance at 10% (*), 5% (**), and 1% (***). Sample: all hearings with no attorneys in the first hearing between July 10^{th} 2023, and May 10^{st} 2024.