Burden and Standard Shifting in Immigration Bond Decisions

Anthony J. DeMattee[*], Hallie Ludsin[†], Grace Shrestha[‡],
Grace Gerenday[§], Devon Thurman[¶], Jeffrey K. Staton[‖]

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Abstract

Non-citizens subject to deportation are often detained in prisons by the United States government. Like the criminally accused, respondents in immigration proceedings may be released on bond if they are judged not to be a danger to the community or a flight risk. Unlike the criminally accused, however, the burden of proof lies with the respondent, not the government. In addition, the standard of proof is subjective. In this paper, we identify the effect of changing these features of immigration court decision-making. To do so, we leverage a decision of the United States District Court for the District of Massachusetts in 2019, which shifted the burden of proof to the government and set an objective standard of proof. Using data on bond hearings from the Executive Office for Immigration Review (EOIR) at the Department of Justice between 2018 and 2020, we conduct a synthetic control analysis of the decision on immigration judge decisions to grant bond as well as the decision to grant bond initially. We find no effect, suggesting that both immigration court decisions and decisions of the government to set an initial bond are unrelated to the orders of the U.S. federal courts.

[*]Post-Doctoral Fellow, Department of Political Science, Emory University

[†]Senior Fellow, Emory University School of Law

[‡]Department of Political Science, Emory University

[§]Department of Political Science, Emory University

[¶]Ph.D. Candidate, Department of Political Science, Emory University

[‖]Professor, Department of Political Science, Emory University

1 Introduction

Between October 2018 and December 2019, Immigration and Customs Enforcement ("ICE") detained an average of 49,000 individuals per day who were subject to removal from the country for alleged violations of U.S. immigration law.[1] In what amounts to pretrial detention in the criminal law setting, individuals are held in prison-like conditions, often literally in prisons. The formerly detained, lawyers, and members of non-governmental organizations have reported squalid conditions, grossly insufficient medical standards, and abuse at the hands of government officials, all of which raise questions about violations of international and U.S. standards of human rights.[2] Although the detained may be released on bond, the process of seeking release is riddled with daunting challenges. Immigration law is notoriously complicated, the detained do not enjoy the right to a court-appointed lawyer free of charge, and they are often tasked with pursuing their own case in a foreign language while appearing in official proceedings via a video feed, even prior to the COVID-19 pandemic (Ryo, 2016; Eagly, 2014; Kim and Semet, 2019). Unlike pre-trial detention in criminal law, the detained carry the burden of proving that they are not a risk to flee the jurisdiction of the immigration court that manages their case or a danger to the surrounding community. They are asked to prove their case to the "satisfaction of an immigration court judge," a standard that allows for considerable discretion on the part of judges directly accountable to the Attorney General of the United States. For all of these reasons, it is not surprising that there are some jurisdictions in which bonded release from detention is rare.[3]

[1]See "Immigrant Detention Numbers Fall Under Biden, But Border Book-Ins Rise," TRAC Immigration Report `https://trac.syr.edu/immigration/reports/640/`.

[2]For example, see "Overcrowded Border Jails Give Way to Packed Migrant Child Shelters," by Eileen Sullivan, Zolan Kanno-Youngs and Luke Broadwater, NY Times Online `https://www.nytimes.com/2021/05/07/us/politics/migrant-children-shelters.html`. Also see multiple discussions here: `https://www.aclu.org/news/by-issue/immigration-detention-conditions/`, `https://www.splcenter.org/attention-on-detention`.

[3]For example, in the first eight months of FY 2018, the Charlotte Immigration Court granted bond in only 17% of cases, "Three-fold Difference in Immigration Bond Amounts by Court Location," TRAC Immigration, `https://trac.syr.edu/immigration/reports/519/`.

In November 2019, the United States District Court for the District of Massachusetts found that the framework for evaluating a detained individual's "flight risk" and "dangerousness" violated the 5th Amendment's Due Process clause, and by implication, the Administrative Procedure Act. In Brito v. Barr, the court ordered that the burden of proof be assigned to the government; and, that the government must prove that the immigrant respondent is a flight risk by the preponderance of the evidence and dangerous by clear and convincing evidence.[4] We leverage the natural experiment created by the Brito decision to consider the causal effect of reallocating the burden of proof and establishing more familiar standards of proof on immigration court outcomes.

The District Court's order made three changes to the existing legal framework, which itself involved the application of an understudied standard of proof. As we will discuss, these features of the Brito case require care in both theory and empirical research design. Drawing on existing models of legal decision-making, we develop two models of immigration court decision-making, which highlight whether and precisely how the decision could have affected immigration detention outcomes. We use these models to structure our empirical analysis. We ask whether Brito caused a change in bond outcomes in immigration court proceedings subject to the jurisdiction of the District Court. Specifically, we consider whether the decision caused an increase in the rate at which the Boston Immigration Court granted bond. We also consider whether the decision caused the court's docket to decrease.

Obtaining a valid estimate of the causal effect of this decision is important for several reasons. The District Court's interpretation of the Constitution and its corresponding remedy may be important purely on normative and legal grounds, but if they materially affect bond outcomes, the stakes of getting the law right takes on a heightened significance. At the same time, finding that the decision had no effect, either on immigration judge decision-making or the initial bond decisions made by ICE would itself raise questions about the ability of the federal judiciary to influence Executive Branch implementation of immigration law. The Brito case also offers a unique opportunity to evaluate the effect of legal standards in a real world case.[5] Our study contributes to both theoretical and empirical scholarship on the burden and standards of proof (Sherwyn and Heise, 2010; Finley and Karnes, 2008; Cheng, 2012; Wexler, 1999), decision-making in immigration court (Kim and Semet, 2019; Ryo, 2016;

[4]Brito v. Barr, 415 F. Supp. 3d 358, 271.

[5]See Finley and Karnes (2008) for a related study on the burden of proof in the U.S. Tax Court

Eagly and Shafer, 2015), as well as general scholarship on the separation of powers system and judicial independence (e.g. Durham, 2005; Spriggs, 1996, 1997; Ferejohn and Shipan, 1990; Staton and Vanberg, 2008).

We divide the remainder of our paper as follows. First, we provide a summary of the law governing immigration detention in the United States. Second, we summarize perspectives over the conceptualization of the burden of proof and standards of proof. We conclude this section with a discussion of existing perspectives over whether changes like those required in Brito should have influenced outcomes in immigration group. Third, we develop two theoretical arguments of bond decisions. In the first, we assume that immigration judges ("IJs") make decisions without considering the possibility that ICE could have been affected by the Brito decision as well. In the second, we consider these decisions in a model that allows IJs to consider the possibilty that Brito had an effect on ICE as well. Fourth, we introduce a synthetic control study of the Brito decision. Fifth, we offer a few concluding remarks. Our empirical analysis is preliminary. We have much more work to complete. For this reason, our concluding section is relatively short.

2 Background

In the United States, immigrant detention is part of the larger legal immigration system.[6] When a noncitizen is charged with violating the country's immigration laws, she enters removal proceedings in immigration court. These proceedings generally include a hearing in which an immigration judge ("IJ") determines whether the individual can be removed from the country,[7] and if so, whether she is eligible for some form of relief from removal.[8] Due to a growing backlog of immigration cases, this process often

[6]For a thorough review of the legal and policy context for immigrant detention decisions, see Kim and Semet (2019).

[7]Some noncitizens are subject to "expedited removal" without formal removal proceedings. *See* 8 U.S.C. § 1229(a).

[8]Congress has authorized a number of forms of discretionary relief from removal, including "asylum," wherein an individual must establish a "well-founded fear of persecution." *See* 8 U.S.C. § 1101(a)(42)(A); *id.* § 1158.

takes years, raising the question of whether noncitizens will be detained pending the completion of their removal proceedings.

The Immigration and Nationality Act ("INA") authorizes the Department of Homeland Security ("DHS") to detain noncitizens arrested for immigration violations and awaiting determinations as to whether they will be ordered removed.[9] This detention framework is designed to serve two aims. First, detention may help to ensure a noncitizen's presence in immigration court for her removal hearing and, if she is ordered removed, may facilitate her removal from the country.[10] Second, detention may eliminate safety and security concerns in cases involving noncitizens who pose a threat to the safety of the community during the removal process.[11]

The modern statutory framework for immigrant detention applies different rules depending on certain characteristics of an individual's immigration and criminal history. INA Section 236(a) governs discretionary detention and is regarded as the "default rule" for noncitizens placed in removal proceedings.[12]. The statute, which is primarily carried out by ICE (itself an agency of DHS), authorizes the

[9] *See* 8 U.S.C. § 1103(a)(1) ("The Secretary of Homeland Security shall be charged with the administration and enforcement of this chapter and all other laws relating to the immigration and naturalization of aliens, except insofar as this chapter or such laws relate to the powers, functions, and duties conferred upon the President, Attorney General, the Secretary of State, the officers of the Department of State, or diplomatic or consular officers . . .").

[10] *See* Demore v. Kim, 538 U.S. 510, 528 (2003) ("Such detention necessarily serves the purpose of preventing deportable criminal aliens from fleeing prior to or during their removal proceedings, thus increasing the chance that, if ordered removed, the aliens will be successfully removed."); Zadvydas v. Davis, 533 U.S. 678, 699 (2001) (noting that detention may serve the additional purpose of "assuring the alien's presence at the moment of removal.").

[11] *See* Matter of Valdez-Valdez, 21 I. & N. Dec. 703, 709 (BIA 1997) (noting that the statutory provisions for immigration detention "were geared toward ensuring community safety and the criminal alien's appearance at all deportation hearings."); Matter of Drysdale, 20 I. & N. Dec. 815, 817 (BIA 1994) ("[I]f the alien cannot demonstrate that he is not a danger to the community upon consideration of the relevant factors, he should be detained in the custody of the Service.").

[12] *See* Jennings v. Rodriguez, 138 S. Ct. 830, 837 (2018).

arrest and detention of a noncitizen pending her formal removal proceedings.[13] However, because immigrant detention under INA Section 236(a) is discretionary, ICE is not required to detain noncitizens faced with removal unless they fall within the class of individuals subject to mandatory detention.[14] Otherwise, ICE may choose either to continue to detain the noncitizen pending her removal proceedings, or to release her on a bond of at least $1500 or on "conditional parole."[15]

2.1 Bond Process

After a noncitizen subject to discretionary detention is arrested, an ICE officer may determine whether she should remain in custody or be released at any time during her removal proceedings.[16] Following this initial custody determination by ICE, a noncitizen may request review of that decision by an IJ at a bond hearing.[17] While INA Section 236(a) permits a detained noncitizen to request a bond hearing, the statute does not specify a requirement for a hearing to be provided at any particular time.[18] If a

[13] 8 U.S.C. § 1226(a).

[14] For an overview of the statutory framework for both discretionary and mandatory immigrant detention, see Smith (2019).

[15] *See* U.S.C. § 1226(a)(1),(2). Conditional parole refers to the release of a noncitizen on her own recognizance, rather than on bond. *See* Ortega-Cervantes v. Gonzales, 501 F.3d 1111, 1115 (9th Cir. 2007).

[16] *See* 8 C.F.R. § 236.1(g)(1). ("At the time of issuance of the notice to appear, or at any time thereafter and up to the time removal proceedings are completed, an immigration official may issue a Form I-286, Notice of Custody Determination.").

[17] *See* 8 C.F.R. §§ 1003.19(a). ("Custody and bond determinations made by [ICE] pursuant to 8 CFR part 1236 may be reviewed by an Immigration Judge pursuant to 8 CFR part 1236."). However, it is worth noting than an IJ may not review an ICE custody determination upon her own motion. *See* Matter of P-C-M-, 20 I. & N. Dec. 432, 434 (BIA 1991) (noting that the regulations "only provide authority for the immigration judge to redetermine custody status upon application by the [noncitizen] or his representative").

[18] *See* Jennings v. Rodriguez, 138 S. Ct. 830, 847 (2018).

noncitizen's request for a bond hearing is granted, that hearing is conducted separately from the rest of the individual's removal proceedings.[19]

During the bond hearing, an IJ may determine whether to require the noncitizen to remain in custody or to release her on bond. The statute also authorizes the IJ to set the bond amount.[20] INA Section 236(a) requires the noncitizen to demonstrate both that she is not a danger to the community and that she is likely to appear for future removal proceedings "to the satisfaction of the officer."[21] Based on this statute, the Board of Immigration Appeals ("BIA") has held that noncitizens bear the burden of proving they should be released from custody. Further, the BIA has held that an IJ should only continue to a determination regarding the extent of flight risk posed by a noncitizen if the noncitizen has demonstrated she does not pose a danger to the community.[22]

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2.2 Immigration Judges

Immigration courts are administrative courts operating under the Executive's Department of Justice (DOJ) and, more specifically, the Executive Office of Immigration Review (EOIR). IJs are lawyers appointed and removed by the Attorney General. They need not have prior immigration law experience to be hired for their post and many come from the ranks of Immigration and Customs Enforcement and other law enforcement bodies.

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[19] *See* 8 C.F.R. § 1003.19(d).

[20] See 8 C.F.R. § 1236.1(d)(1).

[21] *See* 8 C.F.R. §§ 236.1(c)(8), 1236.1(c)(8); *see also* Matter of Adeniji, 22 I. & N. Dec. 1102, 1112 (BIA 1999).

[22] *See* Matter of Urena, 25 I. & N. Dec. 140, 141 (BIA 2009); *see also* Matter of Fatahi, 26 I. & N. Dec. 791, 793 (BIA 2016).

[23] *See* Matter of Urena, 25 I. & N. Dec. 140, 141 (BIA 2009); *see also* Matter of Fatahi, 26 I. & N. Dec. 791, 793 (BIA 2016).

IJs are responsible for deciding whether to keep immigrants in detention once detained by ICE and for deciding the merits of asylum case or cases for removal of a noncitizen immigrant from the United States. The regulations that govern IJs require them to "exercise their independent judgment and discretion" and to make their decisions "impartial[ly]." Their decisions may be appealed to the Board of Immigration Appeals, which also forms part of the EOIR, and, in limited cases, to the federal courts. The Attorney General has the power to undo or modify BIA decisions.[24]

As administrative judges, IJs are not part of the Judiciary or subject to the Code of Conduct for United States Judges. Rather, they are subject to the policies and regulations set out for them by the DOJ and EOIR. The IJs receive directions from the EOIR on which cases they should prioritize and may be dispatched by the EOIR to the border or other immigration courts as needed.[25] The immigration regulations require IJs to "act as the Attorney General's delegates in the cases that come before them." [26] The DOJ's mission is: "To enforce the law and defend the interests of the United States according to the law; to ensure public safety against threats foreign and domestic; to provide federal leadership in preventing and controlling crime; to seek just punishment for those guilty of unlawful behavior; and to ensure fair and impartial administration of justice for all Americans." [27] In contrast, the EOIR describes its mission as "adjudicat[ing] immigration cases by fairly, expeditiously, and uniformly interpreting and administering the Nation's immigration laws." The difference in mandates has led the

[24] 8 C.F.R. § 1003.10 ("(b)...In deciding the individual cases before them, and subject to the applicable governing standards set forth in paragraph (d) of this section, IJs shall exercise their independent judgment and discretion and may take any action consistent with their authorities under the Act and regulations that is appropriate and necessary for the disposition of such cases...." (d)...governed by the provisions and limitations prescribed by the Act and this chapter, by the decisions of the Board, and by the Attorney General (through review of a decision of the Board, by written order, or by determination and ruling pursuant to section 103 of the Act). 8 C.F.R. §1003.1(7)("The decision of the Board shall be final except in those cases reviewed by the Attorney General in accordance with paragraph (h) of this section."

[25] American Bar Association, Reforming the Immigration System Volume 2 (2019) 2-8 - 2-9

[26] 8 C.F.R. §1003.10(a)

[27] The United States Department Of Justice, Organization, Mission and Functions Manual: Overview, available at `https://www.justice.gov/jmd/organization-mission-and-functions-manual-overview`

National Association of Immigration Judges (NAIJ) to identify an "inherent conflict present in pairing the law enforcement mission of the DOJ with the mission of a court of law that mandates independence from all other external pressures, including those of law enforcement priorities."[28] This conflict has led some IJs, as well as the NAIJ and American Bar Association to raise concerns about the lack of independence of the immigration courts.

In the last several years, IJs have also come under immense pressure from the DOJ to resolve a massive backlog in immigration cases. As of early 2021, there was a backlog of 1,322,938 immigration cases across all of the immigration courts.[29] In response to a growing backlog, in 2018, the DOJ established a quota for IJs to clear at least 700 cases per year, with less than a 15% overturned on appeal, which if unmet would result in an unsatisfactory performance evaluation.[30] Some IJs and critics perceive the quota system as "a political tool to advance the current law enforcement policies."[31]

Additionally, given what is ultimately a law enforcement mission, it is unsurprising that the last three Presidential administrations hired IJs who had experience working for the Immigration and Naturalization Service, the Department of Homeland Security and the Department of Justice (e.g., Kim and Semet, 2019, p. 622). Further, Republican presidential administrations stand accused of appointing IJs "based on political and ideological considerations."[32] The Obama Administration sought to insulate the hiring process from politicization after revelations that political appointees in the Bush Administration took into account candidates political and ideological views.[33] In 2017, the DOJ approved changes

[28]American Bar Association, Reforming the Immigration System Volume 2 (2019) 2-10 (quoting Ashley Tabaddor, President National Association of Immigration Judges, Before the Senate Judiciary Committee, Border Security and Immigration Subcommittee Hearing on "Strengthening and Reforming America's Immigration Court System" 2

[29]TRAC Immigration, Immigration court Backlog Tool available at `https://trac.syr.edu/phptools/immigration/court_backlog/`

[30]Joel Rose, Justice Department rolls Out Quotas for IJs, NPR April 3, 2018.

[31]Colleen Long, Immigration Judges Say New Quotas Undermine Independence, AP Sept. 21, 2018.

[32]Immigration Court Hiring Politicization, Human Rights First (2018) 1

[33]Immigration Court Hiring Politicization, Human Rights First (2018) 2

to hiring that gave the politically-appointed assistant attorney general greater say in the appointment process. [34]

As of November 2020, the Trump Administration was responsible for appointing 280 of the 520 IJs, with a concern that it "stacked the court with appointees who are biased toward enforcement, have histories of poor judicial conduct, hold anti-immigrant views. or are affiliated with organizations espousing such views." [35] For example, it appointed a former chief prosecutor for ICE as the Chief Immigration Judge, although he had no prior judicial experience. Its appointment for Chief Appellate Judge advised President Trump on immigration, was a former prosecutor and worked for an immigration reform organization that some label an anti-immigrant hate group.[36] Enhancing the appearance of politicization in hiring and firing, the National Association of IJs filed a grievance against the Trump Administration for firing an immigration judge after he delayed a deportation of an immigrant.[37] This is unsurprising after then Attorney General Jeff Sessions reminded attendees at an EOIR Legal Training Program that immigration proceedings are "subject to such supervision . . . as the Attorney General shall prescribe" ane explained the DOJ's mission to end the "lawlessness" of the immigration system.[38]

2.3 Brito v. Barr

Gilbreto Pereira Brito is a Brazilian citizen and sole provider for his disabled US citizen wife and their three US citizen children. He has been living in the United States for more than ten years and has no criminal record after May 2009. ICE arrested and detained Brito in 2019 after he applied

[34]Immigration Court Hiring Politicization, Human Rights First (2018) 1

[35]Gregory Chen, The Urgent Need to Restore Independence to America's Politicized Immigration Courts, Just Security November 12, 2020

[36]Gregory Chen, The Urgent Need to Restore Independence to America's Politicized Immigration Courts, Just Security November 12, 2020

[37]Jeff Gammage, IJs File Grievance over Justice Dept.'s Removal of Philly Jurist Who Delayed Man's Deportation, The Philadelphia Inquirer, Aug. 8, 2008

[38]Attorney General Sessions Delivers Remarks to the Executive Office for Immigration Review Legal Training Program, Justice News June 11,2018 available at `https://www.justice.gov/opa/speech/attorney-general-sessions-delivers-remarks-executive-office-immigration-review-legal`

for permission to remain in the country to avoid the "exceptional and extremely unusual hardship" to his family that would result if he was removed from the country.[39] Brito was denied release from detention by the Boston Immigration Court because he could not prove that he was not a flight risk or dangerous. Brito v. Barr was a class action lawsuit brought before the United States District Court of Massachusetts on behalf of two classes of noncitizen immigrants, each of whom is held under 8 U.S.C §1226(a) discretionary detention regime pending the final outcome of their removal cases.[40] The two classes are: (1) all immigrants detained or who will be detained in Massachusetts or subject to the jurisdiction of the Boston Immigration Court who received or will receive a bond hearing; and (2) those detained or who will be detained in Massachusetts or who are or will be subject to the jurisdiction of the Boston Immigration Court who have not received or will not receive a bond hearing. The two classes challenged immigration regulation 8 C.F.R. §236.1(c)(8) as a violation of the 5th Amendment Due Process Clause, the Administrative Procedure Act (APA), and the Immigration and Nationality Act. This regulation requires immigrant detainees seeking release from detention to "demonstrate to the satisfaction of the officer that such release would not pose a danger to property or persons, and that the alien is likely to appear for any future proceeding."

The District Court found the evidence that the regulation violated the 5th Amendment and the APA so compelling that it granted summary judgment in favor of the two classes. It agreed with other federal court decisions, including from the Massachusetts District Court, that due process "requires placing the burden of proof on the government."[41] It then criticized that the regulation contained "effectively no standard [of proof] at all," but rather allowed the IJs to arbitrarily pick one.[42] The District Court ordered the Boston Immigration Court and any other immigration court holding custody hearings for detainees held in Massachusetts to place the burden of proof on the government to prove by clear and convincing evidence that the detained immigrants are dangerous or by the preponderance of the evidence that the immigrant is a flight risk and "that no condition or combination of conditions

[39]Brito v. Barr, 415 F. Supp. 3d 258, 264 (2019).

[40]415 F. Supp. 3d 258 (Dist. Mass. 2019)

[41]Brito v. Barr, 415 F. Supp. 3d 258, 266 (Dist. Mass. 2019)

[42]Brito v. Barr, 415 F. Supp. 3d 258, 267 (Dist. Mass. 2019)(citing United States v. Salerno, 481 U.S. 739, 751 (1987)

will reasonably assure the alien's future appearance and the safety of the community." [43] In doing so, the District Court brought the requirements for immigration detention in Massachusetts in line with the constitutional due process requirements for pretrial detention and other forms of civil detention.[44] The order went into effect on December 13, 2019.[45]

3 The burden of proof and standards of proof

The concepts of the burden and the standard of proof derive from the idea that at the start of every legal dispute before a court, it is appropriate to believe that each litigant is equally likely to be correct. In the event that the trier of fact remains equally convinced by the arguments of both parties, the court or jury nevertheless must choose one litigant's evidence over the other; otherwise, the dispute resolution system is inconclusive.

The burden of proof is designed to solve this problem by choosing which litigant should be given the benefit of the doubt that her position is correct. The litigant that does not receive the benefit of the doubt carries the burden of proof, which effectively puts a thumb on the scale of evidence, weighting

[43]Brito v. Barr, 415 F. Supp. 3d 258, 271 (Dist. Mass. 2019)

[44]United States v. Salerno, 481 U.S. 739, 751 (1987); Foucha v. Louisiana, 504 U.S. 71 (U.S. Sup. Ct. 1992)

[45]Federal courts have held that there are limited conditions under which the burden shifts to the government, represented by an ICE attorney, to prove that prolonged detention of certain classes of noncitizens under INA Section 236(a) is warranted. An example is a decision by the U.S. Court of Appeals for the Ninth Circuit, which holds that the government must prove by clear and convincing evidence that denial of bond is justified in a *Casas* bond hearing, referring to a bond hearing involving an individual in prolonged detention following the completion of her administrative proceedings. *See* Singh v. Holder, 638 F.3d 1196, 1203 (9th Cir. 2011). The Supreme Court has not explicitly addressed the constitutionality of placing the burden of proof for custody determinations on the immigrant respondent. The Court has held that INA Section 236(a) does not require the government to grant a bond hearing to a noncitizen, or to prove that prolonged detention under the statute is warranted. *See* Jennings v. Rodriguez, 138 S. Ct. 830, 847–48 (2018). However, while the Court has decided the statute does not require the burden of proof for custody determination be placed on the government, it has left open the question of whether the Due Process Clause does. *See* Darko v. Sessions, 342 F. Supp. 3d 429, 434–35 (S.D.N.Y. 2018).

it against her (Wexler, 1999, p. 75). The litigant with the benefit of the doubt, in contrast, is not required to prove anything to win the case. Perceptions of the negative consequences of an incorrect decision are important in deciding which party should carry the burden. The Supreme Court describes the allocation of the burden of proof as a decision over which litigant should carry the greater risk of a wrongful decision against her.[46] The litigant with the most at stake will suffer the most if the process reaches a wrongful decision; and for this reason, the litigant with less at stake receives the burden of proof. Ultimately societal attitudes determine the weight of each litigant's stakes (Wexler, 1999, pp.75. 77-78).

The standard of proof then determines how much benefit of the doubt to give the litigant entitled to it.[47] This assessment again requires a balancing of each litigant's interests. The weightier the interests of the litigant with the benefit of the doubt relative to the litigant with the burden of proof, the greater the benefit of the doubt offered. The Supreme Court recognizes three different standards of proof – "preponderance of the evidence," "clear and convincing evidence" and "beyond a reasonable doubt." Preponderance of the evidence, which requires the litigant with the burden of proof to prove her facts are more likely than not, tolerates the greatest amount of risk of a wrongful decision and provides the least benefit of the doubt. The Supreme Court describes this standard as dividing "the risk of error in roughly equal fashion" between litigants and particularly appropriate where "mere loss of money" is at stake.[48] The clear and convincing evidence standard tolerates a lower risk of error and provides a greater benefit of the doubt, requiring the litigant with the burden of proof to prove her position is "highly likely."[49] Accordingly, the Supreme Court considers this standard is appropriate where the litigant with the benefit of the doubt has a "particularly important interest" at stake.[50] The standard that tolerates almost no risk of a wrongful decision and therefore provides the greatest benefit of the doubt,

[46]Addington v. Texas, 441 U.S. 418, 423 (U.S. Sup. Ct. 1979)

[47]Ibid, 423.

[48]Ibid, 424.

[49]Federal Civil Jury Instructions Committee, Burden of Proof, - Clear and Convincing Evidence at https://www.vtd.uscourts.gov/sites/vtd/files/BURDEN\%20OF\%20PROOF\%20-\%20CLEAR\%20AND\%20CONVINCING\%20EVIDENCE.pdf.

[50]Ibid, 424.

is beyond a reasonable doubt. It is reserved for criminal cases where "the interests of the defendant are of such magnitude that historically and without any explicit constitutional requirement they have been protected by standards of proof designed to exclude as nearly as possible the likelihood of an erroneous judgment."[51]

3.1 Statistical Interpretations

Standards of proof are often given a statistical interpretation (e.g. Cheng, 2012; Hay and Spier, 1997). Consider that the trier of fact approaches a record with a prior belief about the truth of a factual assertion, which can be interpreted as a probability. After observing evidence, the fact-finder's belief about the truth of this assertion is represented by Bayesian posterior. Specifically, the fact-finder's belief is the probability that an assertion is true given the evidence that she has observed: $Pr(\text{Assertion true}|\text{evidence})$. A simple approach, useful for fixing ideas about key concepts, understands legal standards as thresholds indicating the value of the posterior belief above which the fact-finder should conclude that the assertion is true. Figure 1 illustrates preponderance of the evidence, clear and convincing evidence, and beyond a reasonable doubt as they are commonly understood. Thresholds associated with each standard increase in the posterior probability space, as each standard requires a higher degree of certainty about the truth of an assertion in order to accept it as true in fact.[52]

The burden of proof can also be understood in this framework. Consider a legal conflict between parties A and B. One interpretation is that the burden tells you which party wins if the judge finds herself precisely at a threshold. Although this is natural interpretation, we believe that shifts in the burden of proof can be understood in terms of the thresholds' locations, as well. Each threshold in Figure 1 can be understood to communicate how the standard should be used with the burden of proof placed on the party who typically bears it, e.g., the prosecution in the criminal case or the plaintiff in a civil case. If placing the burden of proof on one party can be understood as akin to "placing a

[51] Ibid, 423.

[52] While the ordering of the thresholds are correct, the precise values are clearly approximations, meant to reflect plausible numbers that reflect the degree of certainty that the standards are meant to communicate.

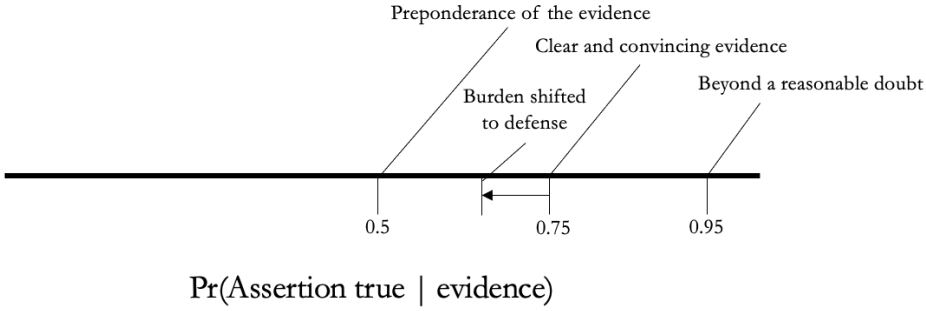

Figure 1: *Illustrates three standards of proof: preponderance of the evidence, clear and convincing evidence, and beyond a reasonable doubt. Standards of proof are understood as thresholds defined with respect a fact-finder's belief about the truth of factual assertion in light of the evidence before her. This belief can be expressed as a probability. The thresholds then indicate the probability above which the fact-finder should accept the assertion as true in light of the evidence, and with the burden of proof placed on the typical party (i.e., the default rule). Also shown is the consequence of shifting the burden of proof in the case of the clear and convincing evidence standard from the plaintiff to the defense.*

thumb on the scale of justice" against the interests of that party, then a change in the typical burden of proof implies that the thumb has been lifted and the standard of proof should change. For example, imagine a factual claim in a civil case, where the relevant standard is "clear and convincing evidence," and the burden is on the plaintiff. Now consider a legal rule that shifts the burden to the defendant, as say in discrimination suits (e.g., Sherwyn and Heise, 2010). The consequence of this shift is that the threshold for accepting the asserted fact as true has moved to the left, because the thumb has been removed from the scale, or equivalently, it now has been placed against the interests of the defendant. Figure 1 illustrates this kind of shift for the "clear and convincing evidence" standard, though the idea is general.

There are two ways of interpreting substantively what has happened as the burden shifted. First, the shift implies that the trief of fact's posterior belief may be less strong in order to find that the assertion is true than it was when the burden was on the plaintiff. Second, the shift places more importance on the quality of evidence brought to bear by the party asserting a fact. This way of thinking about the burden reflects the common idea that the plaintiff has much more work to do when she bears the burden than she does when the burden shifts to the defense.[53]

[53]We might put further structure on this here, by saying that shifts in the burden of proof may be understood to shift decision thresholds, they may not shift them so far that they become another standard. For example, placing the burden of proof on Party A in the example shifts the threshold to the right, but it should shift it so

3.2 Should we expect the burden of proof and standards of proof to matter?

Suppose that immigration judge decisions are reasonably approximated by a model in which they hold beliefs about the truth of factual assertions in light of the evidence they have observed. If the allocation of the burden of proof as well as the standard of proof that controls a legal decision influence the thresholds that IJs use, then surely they should influence decision-making. As we discuss below, whether we can observe evidence consistent with the change will turn on whether the flow of cases IJs resolve remain constant when the rules change; however, in principle, if changes in the law change the way that IJs evaluate factual assertions, then legal changes will cause a change in decision-making. A key question then is whether it is reasonable to believe that changes in the law will change the way that IJs evaluate assertions.

They should matter IJs are not Article III judges; they are not even administrative law judges. Yet as lawyers, they are members of a career that is characterized by a socialization process that promotes a commitment to legal norms (Knight and Epstein, 1996; Baum, 2009). A aspect of this socialization is the belief that legal decision-making is different that decision-making in other contexts: it is rule-bound. In Shapiro's (1981) classic formulation, legal disputes take the form of a triad: two opposing interests seek a resoluton from a neutral third party. The fundamental problem that triadic dispute resolution systems confront is that once the third party has made a decision, she has taken a side. The problem is how to ensure that losers will continue to believe that the process is legitimate, that it is not hopelessly biased against one party or the other. In order to ensure that future parties continue to hold such beliefs, judges have strong incentives to develop and use bodies of law, which at least minimally appear to structure decision-making. We need not believe that legal decision-making is capable of fully neutral application of standards to be believe that legal standards ought to matter. In so far as they matter more than they would outside of a legal context, we can say that legal rules, standards of proof and the burden of proof, are likely to influence decision-making.

Although modern social science rejects the notion that any judge could be a mechanical applier of law, bodies of scholarship suggest that legal decision-making is different. There are strong pressures in

far to the right that "clear and convincing evidence" with the burden on Party A is akin to "beyond a reasonable doubt" with the burden on Party B.

the common law for judges to operate at least with respect to rules developed in past opinions (Friedman, 2006; Gillman, 2001). Scholars have found empirical evidence linking judicial decision-making to legally relevant information (Richards and Kritzer, 2002; Bailey and Maltzman, 2008; George and Epstein, 1992). In a study of the application of legal rules governing the level of scrutiny which should be applied to claims challenging the constitutionality of speech restrictions, Bartels (2009) finds that as the level of strutiny is increased, ideological differences between U.S. Supreme Court justices were decreasingly influential on their decisions. In a study of discrimination cases resolved by state courts of last resort, Baldez, Epstein and Martin (2006) find that the presence of an equal rights amendment in the state constitution is associated with the court using a higher standard of review, which itself increased the chance of the court finding a violation of equal protection.

Evidence on immigration judge decision-making does suggest that the law matters. Although scholars have found extra-legal influences on immigration judge decisions, studies repeatedly find the legally-relevant factors matter. Most relevant for our empirical purposes are Ryo's (2016) findings, which suggest that a respondent's prior criminal record is strongly negatively associated with the probability of being granted bond in an immigration custody hearing, just as it is strongly positively associated with the bond amount, conditional on bond being granted (See also Eagly, 2014; Eagly and Shafer, 2015).

They should not matter There are several reasons to question whether a change in either the burden of proof or the standard of proof would meaningfully affect immigration judge decision-making. The most prominent reason is that generations of a scholarship on judges of multiple types and locations around the world has provided compelling evidence that judicial decision-making can be understood in ideological and partisan terms (Segal and Spaeth, 2002; Lauderdale and Clark, 2012; Ríos-Figueroa, 2007; Desposato, Ingram and Lannes Jr, 2015). Lax and Rader (2010) have also raised serious methodological questions about Richards and Kritzer's (2002) findings purporting to show strong effects of precedent in the form of legal regimes; they find no such evidence. From this alternative perspective, legal standards and the burden of proof are flexible enough and hard enough to audit to allow a judge wide discretion in their application. This is especially relevant in the context of custody decisions in immigration court which are not subject to the most exacting standard of proof, where we might imagine that the constraint on individual discretion might be the strongest. That is, if IJs were explicitly asked

to apply beyond a reasonable doubt to the factual assertions made by government lawyers, perhaps we should expect greater constraint; however, this has never been true and it is not true now.

Although outside of the immigration context, Finley and Karnes's (2008) study of the burden of proof in the U.S. Tax Court is instructive. They considered the possible effect of a reform in the Internal Revenue Service Restructuring and Reform Act of 1998. In order to make it easier for civilian plaintiffs to pursue claims against the IRS, the Act removed the burden of proof from the plaintiff and placed it on the government. Finley and Karens study whether the reduction in tax burden following a Tax Court decision increased after placing the burden on the government. Simply put, there is no evidence of an effect. If anything, the change is associated with an decrease in the reduction (85).

Rules governing immigration judge tenure also raise questions about whether legal rules changed by the U.S. federal courts would influence judicial decisions. IJs are supposed to exercise independence, but they are not independent of the executive branch; instead, they depend on the support of the Attorney General. The Board of Immigration Appeals grants IJs enormous discretion in bond determinations and those judges are not required to provide written decisions in their custody hearings, which means they do not have to explain their reasoning. These two factors make it less likely an immigrant detainee will seek an appeal or that it will be successful. Auditing immigration judge decision-making is hard enough under these circumstances, and a sitting Attorney General who does not wish IJs to have less discretion has no reason to make it easier. Consistent with these hierarchical pressures, Kim and Semet (2019) find that respondents succeeded less in bond hearings during the Trump Administration, even among IJs appointed by other presidents.

Summary The are sensible reasons to expect some kind of effect of a change in the burden and standard of proof following the Brito decision. Yet there are equally compelling reasons for skepticism. Two theoretical matters remain. The first concerns precisely how should the changes in Brito have influenced immigration judge decisions if they did. Even if we believe that changes in the law should matter, knowing how they would have mattered requires a careful understanding of the way that the law manifested prior to the change. As we will develop, there are good reasons to believe that the change in Brito might have not affected all judges in the same way. Second, it is possible that changes in Brito could have influenced the outcomes of detention in the immigration space without influencing immigration judge decision-making itself. In short, Brito could have influenced the initial bond decisions of ICE. To

address these matters, we will require a more careful theoretical structure than we have developed in this section. To that end, we present two simple models of immigration judge decision-making.

4 Theory

Should the shift in the burden and standard of proof in Brito have influenced IJ decision-making? Should it affect the bond process even if IJs should have been no more or less willing to grant bond? As we have just reviewed, it is certainly possible that Brito had no effect on bond outcomes. It is also possible that Brito had an effect, but that the effect was not on IJ decision-making. In this section, we develop two theoretical models designed to address these matters. We first offer a decision-theoretic model, which provides a microfoundation for all of our theoretical work. It illustrates the core connection between legal standards and the costs of decision errors.

The model identifies the conditions under which Brito could have made IJs more likely to grant bonded release. We then consider a game theoretic model in which ICE makes an initial decision on bond grounded in the information it has about the individual they are considering and their perceptions of IJ decision-making. Similarly, IJs are aware that ICE is making intital bond decisions strategically and can use this information in their decision-making process. This model suggests that while Brito should not have influenced IJ decision-making, it should have had a significant effect on the detention process by influencing the initial bond decisions of ICE. Critically, each of these predictions about the possible effect of Brito depends on the belief that the decision would have meaningfully influenced the way that IJs evaluate the costs of making errors. That is to say, shifts in legal standards must affect IJ preferences in order to affect IJ decision-making or the kinds of cases that come to immigration court.

Our models draw on substantial prior theoretical research. We adopt two key features from this tradition. First we will conceive of the judicial task as an evaluation of a factual assertion as presenting decision problem. One important implication of this approach is that judges are not merely asking about the probability that the government's assertion is correct; they have to consider the consequences of getting the answer to this question wrong (e.g. Yntiso, N.d.; Cheng, 2012; Hay and Spier, 1997; Grossman and Katz, 1983; Shotts and Wiseman, 2010). Consistent with standard approaches, we will assume that IJs care about avoiding both Type I errors (failing to grant bond to a respondent who merits it) and Type II errors (granting bond to a respondent who does not). We will then consider a

game theoretic version of this problem, in which judges can learn from expected patterns of behavior from ICE prosecutors. This approach draws on related models from political economy, in which voters draw inferences about prosecutors and judges grounded in their beliefs about expected patterns of prosecution (Huber and Gordon, 2004; Yntiso, N.d.).

4.1 Preliminaries

Generally, an IJ is tasked with answering two questions in a custody hearing. Is the respondent a danger to the community and is the respondent a risk to flee her jurisdiction? For ease of exposition we will collapse these two questions into one: is the respondent a danger to the community?[54] The true status of the respondent is reflected by one of two states $\omega \in \{0, 1\}$, were $\omega = 1$ denotes a respondent who is dangerous and $\omega = 0$ denotes a respondent who is not. An IJ makes a decisions $b \in \{0, 1\}$, in which $b = 1$ denotes that bond is granted and $b = 0$ that it is not.

Importantly, IJs make this decision after they observe written filings, including evidentiary exhibits, as well as oral testimony given in custody hearings. We refer to all of this as the "evidence." We conceptualize the the evidence as a noisy signal that the IJ receives about ω, denoted e. IJs observe one of two signals $e \in \{e_s, e_w\}$, where e_s indicates that the government's evidence is a strong and e_w indicates that is weak. We assume further that the $Pr(e_s|\omega = 1) = 1$ but that $Pr(e_s|\omega = 0) = q$, where we assume $q \in (0, 1)$. This is to say that if a respondent is truly dangerous, the IJ will observe strong evidence of dangerousness; however, if the respondent is not dangerous, the IJ may yet observe strong evidence. For this reason q measures the extent to which the strength of the process by evidence is produced discriminates between those individuals who are and are not dangerous. As q approaches 1, e_s offers less and less useful information to the IJ, since the government will appear to have strong evidence in all cases. Likewise, as q approaches 0, e_s is more and more telling, as very few cases in which the respondent is not truly dangerous nevertheless produce evidence suggesting that they are.

[54] We will want to develop the rationale of this assumption further. One thing to note is that Brito did not change the flight risk standard nearly as much as it did the dangerousness standard. Second, in so far as the standards/burdens shifted, they shifted in the same direction. Third, if you believe that there is a positive correlation between dangerousness and flight risk, the empirical implications are the same. We add a lot of needless complication by looking at both issues in this model.

4.1.1 Model 1

We first consider a model in which IJs simply respond to the cases before them. We assume that IJs wish to make correct legal decisions, so that their goal is to issue a decision such that $b \neq \omega$. We set the value of a correct decision to 0. We will then say that the cost of issuing bond to a person who is truly dangerous is $\beta > 0$, and that the cost of not issuing bond to a person who is not truly dangerous is $\alpha > 0$.[55] Thus, the payoff function for the IJ is given by

$$u(b;\omega) = \begin{cases} 0 & \text{if } \omega \neq b \\ -\alpha & \text{if } \omega = 0 = b \\ -\beta & \text{if } \omega = 1 = b \end{cases}$$

We denote the IJ's prior belief about the respondent's true status $Pr(\omega = 1) = \pi$, and assume that $\pi \in (0, 1)$. In light of the way that evidentiary signals emerge, the IJ should issue bond ($b = 1$) if she observes the weak evidence (e_w), because she will correctly infer that the respondent is not dangerous; however, if she observes the strong evidence (e_s), she will remain uncertain. Her posterior belief, conditional on observing the strong evidence is

$$Pr(\omega = 1|e_s) \quad = \quad \frac{\pi}{\pi + (1-\pi)q} \tag{1}$$

As the evidence becomes more discriminating (q decreases), this probability approaches 1. In contrast, as the evidence becomes less discriminating (q increases), the strong evidence is of decreasing value and the updated belief of the judge converges on her prior belief.

4.1.2 Results and Discussion

We first define a threshold probability above which the IJ should deny bond:

$$\bar{\pi} \quad = \quad \frac{\alpha}{\alpha + \beta} \tag{2}$$

[55] We can restrict $\alpha > \beta$, which is how we typically think about these errors in the law; however, for now it is useful to leave the order unrestricted.

This threshold reflects consequences of Type I and Type II errors. As it becomes more costly to deny bond to a person who deserves it (a Type I error, the cost of which is α), the threshold increases. This implies that IJs will require more discriminating evidence in order to deny bond. Similarly, the threshold decreases as the cost of granting bond to a dangerous person (a Type II error) increases. If we denote the IJ's posterior belief about dangerousness as $\hat{\pi}$, then the IJ's decision rule is given by

$$d(s_i) = \begin{cases} b = 1 & \text{if } (e_w) \text{ or } (e_s \text{ and } \hat{\pi} < \bar{\pi}) \\ b = 0 & \text{if } e_s \text{ and } \hat{\pi} \geq \bar{\pi} \end{cases} \tag{3}$$

This decision rule reflects very closely the discussion of standards of proof in Section 3. It also illustrates several important ideas about legal standards. The first is the direct connection between the costs associated with making mistakes and the standards themselves. When uncertain, the IJ operates under a threshold decision rule. If her posterior belief is at or above $\bar{\pi}$, she will deny bond; if her posterior belief is below this threshold, she grants. In this way, the values associated with making incorrect decisions fully characterize any legal standard. Setting $\alpha = \beta$ yields the "preponderance of the evidence" standard, i.e., $\bar{\pi} = \frac{1}{2}$ when $\alpha = \beta$. Similarly, setting $\alpha = 19 \cdot \beta$ yields "beyond a reasonable doubt." The model handles shifts in the burden of proof in an analogous fashion. Placing the burden on the respondent in immigration court can be understood to shift the clear and convincing evidence standard to the left, as in Figure 1. What this means is that we can consider the implications of a shift in the legal standard or the burden of proof by asking how legal decisions change as an IJs perceived costs of making errors change.

Second, it is also helpful to express the decision rule defined in 3 in terms of q, the parameter which describes how discriminating IJs believe evidence to be. Doing so highlights the importance of processes through which evidence is produced. Given Expressions 1 and 2 we can also express the decision rule as follows:

$$d(s_i) = \begin{cases} b = 1 & \text{if } (e_w) \text{ or } (e_s \text{ and } q \geq \frac{\pi\beta}{\alpha(1-\pi)}) \\ b = 0 & \text{if } e_s \text{ and } q < \frac{\pi\beta}{\alpha(1-\pi)}) \end{cases} \tag{4}$$

When q is relatively high, e_s is not sufficiently discriminating to result in the denial of bond; it is too likely that the evidence appears strong when the respondent is in fact not dangerous. Notice that this

threshold decreases as the costs of denying bond to a person who is not dangerous increase (α), making it more likely to grant bond even as the signal the IJ observes discriminates less and less. The threshold increases as the costs of granting bond to a dangerous person increase and as the prior belief that the respondent is dangerous increases, suggesting that IJs will be more likely to deny bond when they hold very strong prior beliefs in dangerousness and when they believe that granting bond to a dangerous respondent is highly costly.

The third point is that, conceptualized in this way, legal standards are also inherently bound together with the ideological preferences of judges. We might say that conservative judges perceive the costs of denying bond to a person who is not dangerous to be lower than liberal judges; and that the they perceive the costs of granting bond to a dangerous person to be higher that do liberal judges. If the law could completely influence a judge's error costs, it would be possible to set legal standards precisely and identically for all judges. Yet, a century of scholarship on judging suggests that judicial behavior is about more than the law (Generally, see Epstein and Lindquist, 2017). Still, if the law is to meaningfully influence decisions via the selection of standards or the setting of burdens, it will do so only in so far as it impacts the preferences of decision-makers. This is all to say that for standards to matter, judges must believe that they have normative value. Their personal views of costs of making the wrong decision must respond to the standards they are being asked to use. If they do, then this model offers a way of evaluating how changes in the the burden of proof and the standard of proof are likely to influence decision-making.

4.2 The Observable Effects of Brito

We are now in a position to evaluate the Brito decision. Shifting the burden of proof onto the government and making the standard of proof more stringent is equivalent to an increase in the cost of denying bond to a person who deserves it or a decrease in the cost of granting bond to a person who does not. In principle, in both cases, these changes might be expected to make IJs more likely to grant bond by increasing the threshold probability necessary for a finding of dangerousness. Yet to pin down the effects, we need to consider the status quo and ask how Brito would have changed things for different IJs.

Prior to the Brito decision, the burden of proof was on the respondent and dangerousness needed to be proven "to the satisfaction" of the IJ. After Brito, the burden is on the government and the standard

is clear and convincing evidence.[56] From the model's perspective, the shift in the burden should have unambiguously led to an increase in the threshold $\bar{\pi}$; however, the effect of the shift in the standard of proof is less clear.

Given the flexibility allowed under the pre-Brito standard, it is possible that all IJs were using a less stringent standard than what is required by Brito. Yet is is plausible that at least some IJs were using standards that were at least as stringent as what is required by Brito. It is even possible that some IJs were using more exacting standards that Brito requires. Figure 2 illustrates how Brito might have affected these three types of IJs. Consider a conservative IJ (denoted C_{pre}) for whom "to the satisfaction of the IJ" meant something like "preponderance of the evidence;" a liberal judge (denoted L_{1pre}) who interpreted the prior standard to mean something like "clear and convincing evidence," but with the burden placed on the respondent; and, second liberal judge (denoted L_{2pre}) who interpreted the standard to mean something like "beyond a reasonable doubt," but again with the burden on the respondent.

If we believe that Brito shifted their preferences, then we should expect the conservative judge's threshold to shift to the right, perhaps to C_{post}. The burden of proof and the standard of proof both changed, pushing this judge in the same direction. The conservative judge should be more likely to grant bond post-Brito. The effect on the first liberal judge is different, reflected by the smaller shift to the right, i.e., to L_{1post}. The effect is weaker but it is in the same direction as the effect on the conservative judge. This is because although the burden of proof shifted, the standard of proof did not. Finally, and critically, it is possible that Brito could have caused the third IJ, a very liberal judge, to decrease his threshold. Although the burden shift would have pushed his threshold to the right, the requirement to use "clear and convincing evidence" would have created a force in the opposite direction. The net effect of these two forces could have lowered the threshold to something like L_{2post}.

There are two implications of this argument. First, given the shift in the burden of proof, we might expect that all judges were more likely to grant bond after the Brito decision. Yet, we can also see that the direction of the force associated with the change in the standard of proof depends on how

[56] Again, we are referring to the standard with respect to the issue of dangerousness. The standard with respect to flight risk is preponderance of the evidence.

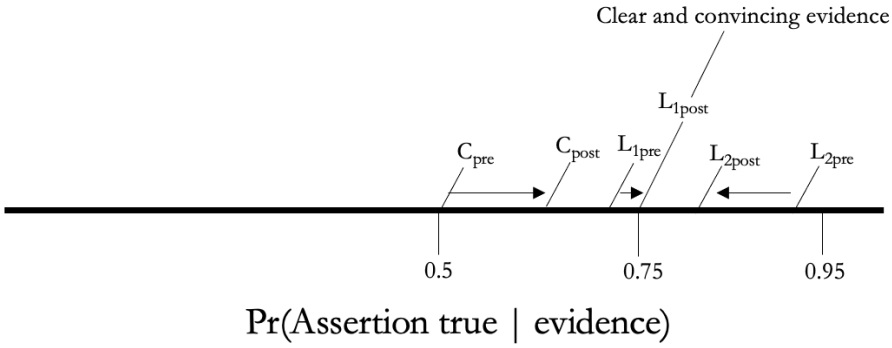

Figure 2: *Possible IJ decision thresholds before and after Brito.*

IJs interpreted "to the satisfaction of the IJ." Since it is likely that conservative IJs were more likely to have interpreted the prior standard as falling below "clear and convincing evidence," we ought to be particularly likely to observe the positive effect on the decision to grant bond among conservative IJs. Empirical evidence of a Brito effect among liberal judges may be more difficult to observe since it is possible that for the at least some liberal judges, Brito created opposing forces. This suggest two empirical implications.

> *The Brito decision should have increased the rate at which IJ's grant bond. This effect should be particularly strong among conservative judges.*

4.2.1 Model 2

Model 1 provides a microfoundation for IJ decision-making, but it envisions a process in which all individuals in detention are reviewed by IJs or one in which cases arrive in immigration court via a process that is of no interest to immigration court judges. Similiarly, it does not consider how ICE prosecutors could have responded to Brito in anticipation of a change in IJ decision-making. We now turn to this possibility.

4.3 Timing and Preferences

Consider a game theoretic model of custody decisions in the context of a removal process. It reflects the basic features of Model 1, but adds an ICE prosecutor who is tasked with an initial bond decision. A game diagram is found in the Appendix, though we fully describe the game's components here.

The true dangerousness of the individual is again characterized by ω. We assume that ICE is better informed about ω than the IJ. For simplicity, we assume that ICE observes ω.[57] Thus the ICE prosecutor has two types, the prosecutor who observes $\omega = 1$ (ICE_1) and the prosecutor who observes $\omega = 0$ (ICE_0). After observing ω both types make a choice to release the detainee ($r = 1$) (either on bond we assume will be paid or on the person's own recognizance) or to deny bond ($r = 0$).

If ICE_i (for $i = 1, 2$) refuses to release a detained individual, we assume that this person will seek bonded release in immigration court. The decision to deny bond implies that ICE_i will bring a case against the respondent. We will assume that this is costly but that the costs depend on ω: ICE_i pays ϵ_i to bring the case, where $\epsilon_0 > \epsilon_1$. Notice that when ICE_0 denies bond she does so with respect to a detainee whom she knows is not dangerous. ICE_0's costs of bringing the case are higher than ICE_1's costs to reflect the idea that it should be easier to build a persuasive case against a truly dangerous respondent than a respondent who is not dangerous. Essentially, ICE_0 puts in effort in order to take advantage of the possibility that, at the hearing, the evidence will point toward dangerousness.

If bond is denied initially, the IJ will again observe observe evidence, update her prior beliefs, and make a decision to grant bond or not by again choosing b. As before, we assume that the IJ observes either the signal e_s or e_w at the custody hearing. And again, we assume that $Pr(e_s|\omega = 1) = 1$ but that $Pr(e_s|\omega = 0) = q$. Thus as before the IJ will always grant bond if she observes e_w. The question is what she will do when she observes e_s.

We will say that ICE_i pays a cost v_i if the detained individual is released; and, we assume that $v_1 > v_0$, reflecting the fact that ICE prefers to detain the dangerous. We will also assume that $v_0 > \epsilon_0$, so that no prosecutor would fail to bring a case if she believed that that the IJ would certainly deny bond.[58]

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[57]This assumption is not necessary, but it simplifies the analysis without losing the key ideas. An alternative assumption, no doubt more realistic, is that ICE observes an additional signal related to ω, so that it is better informed than the IJ but not perfectly informed about the state. This would involve a slight change in the conditions identified by the model but it would not materially affect the key claims, which address how Brito could have affected ICE even if it did not affect IJ decision-making.

[58]This assumption can be relaxed without doing any harm to the analysis. The consequence is that some of the equilibria we identify would require additional conditions to be identified in the results section. Substantively,

Finally, we will assume that the IJ wishes detention outcomes to reflect the true dangerousness of the individuals in detention, so that IJs incur costs if the dangerous are either released by ICE or granted bond and if those who are not dangerous are detained. The IJ's payoff function is as follows.

$$u_{IJ}(b,r;\omega) = \begin{cases} 0 & \text{if } (\omega = 0 \neq r) \text{ or } (\omega = 0 = r \neq b) \text{ or } (\omega = 1 \neq r = b) \\ -\alpha & \text{if } (\omega = 0 = r = b) \\ -\beta & \text{if } (\omega = 1 = r) \text{ or } (\omega = 1 = b \neq r) \end{cases}$$

Given our description above, $\text{ICE}_i's$ payoff function is as follows.

$$u_{ICE_i}(b,r) = \begin{cases} -v_i & \text{if } r = 1 \\ -v_i - \epsilon_i & \text{if } r = 0 \neq b \\ -\epsilon_i & \text{if } r = 0 = b \end{cases}$$

4.4 Results and Discussion

Our solution concept is Perfect Bayesian equilibrium (PBE), which is a pair of sequentially rational strategy profiles and belief profiles (σ, μ). Where players are uncertain, beliefs are determined consistently with the strategies. We will assume that beliefs are formed via passive conjectures at histories that are not reached in equilibrium.[59]

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it is also defensible. It means that ICE_1, who knows that the detainee is dangerous, would not release him simply to avoid paying the costs of litigation. And once we have assumed that ICE_0 is willing to go to court in order to keep a peaceful person in detention, we have already implicitly assumed that v_0 must be relatively large.

[59]This assumes that players (in this case the IJ) do not update when she finds herself at a history that should not be reached if the players adopt their equilibrium strategies. There is one example in our model when this happens, specifically in the case "No Docket," where no prosecutor brings a case to immigration court. This is admittedly an odd equilibrium though if an IJ in such an equilibrium was in fact asked to run a custody hearing, we are assuming that she would not draw an inference about the type of prosecutor before her.

A complete description of the analysis is found in the Appendix. Here we will summarize the key results and then discuss how the Brito decision would have influenced outcomes in this model. Figure 3 offers a visual summary of the four PBE in this game. They are displayed across the range of q.

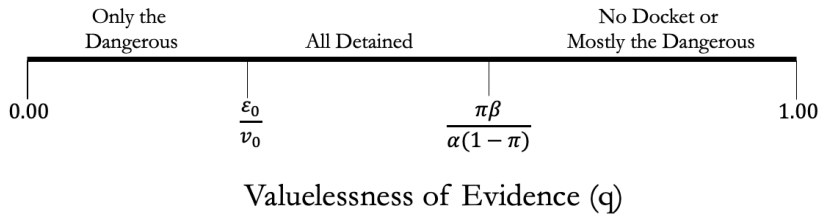


Figure 3: *Shows four equilibria as the value of strong evidence declines (i.e. as q increases).*

Case 1: Only the Dangerous For very low values of q, ICE prosecutors deny bond only to the truly dangerous and IJs make decisions that are consistent with the evidence they observe. With this type of behavioral norm in place, IJs infer that when they observe e_s it can only be ICE_1 making a case against a truly dangerous respondent. In this case, ICE_0 will know that the IJ will deny bond if she observes strong evidence, so this type of prosecutor must not have an incentive to come to court. In order to deter this kind of behavior it must be that $q < \frac{\epsilon_0}{v_0}$, the first threshold in Figure 3. As the costs of bringing a case against a peaceful person rise (ϵ_0), this threshold shifts to the right, and the PBE is more easily sustained. On the other hand, if the cost of releasing this type of detainee increases (v_0), the threshold shifts to the left reflecting the fact that the temptation to go to court has increased for ICE_0.

Case 2: All Detained In this case, as q rises, all prosecutors deny initial bond. When cases arrive in immigration court, the IJ's posterior belief is identical to her prior belief, and the decision setting looks identical to that analyzed in Model 1. As long as $q < \frac{\pi\beta}{\alpha(1-\pi)}$, the IJ will deny bond; otherwise she will grant bond. Given that ICE_1 knows that his stong evidence will be observed at the hearing, he knows that bond will be denied and his incentive to come to court is transparent. In order for ICE_0 to bring a case, it must be sufficiently likely that the evidence will appear strong at the hearing. This requires $q \geq \frac{\epsilon_0}{v_0}$. Thus, for this case, we require q to fall between the two thresholds in Figure 3.

Case 3: No Docket The third case is one in which prosecutors grant bond and all detained individuals are released. In this equilibrium, no cases are expected to come to court. If a case did, passive conjectures implies that the IJ will not update, and the same decision rule in Case 2 (and Model 1) will be applied. This is to say that even if ICE_1 arrived with extremely strong evidence, the IJ would not be able to distinguish him from a lucky ICE_0. To be sure, if the IJ observed weak evidence, she would conclude that the respondent was peaceful, but then this would only result in a grant of bond.

Case 4: Mostly the Dangerous When $q > \max\{\frac{\pi\beta}{\alpha(1-\pi)}, \frac{\epsilon_0}{v_0}\}$ a case exists in which prosecutors fail to discriminate fully, but they do discriminate and they do so in a sensible way. Here, ICE_1 always brings a case and ICE_0 does so with positive probability, which we will denote by λ. The IJ in this case will always grant bond when observing e_w and will grant bond with probability p when observing e_s. As in Case 2, strong evidence is consistent with both types of respondents; however, unlike Case 2, the IJ will be able to update her beliefs. The IJ's posterior belief upon observing e_s is

$$Pr(\omega = 1|e_s) \quad = \quad \frac{\pi}{\pi + (1-\pi)\lambda q}. \tag{5}$$

How strongly the IJ updates in the direction of $\omega = 1$ now depends not only on the value of evidence (q), but on the rate at which ICE_0 attempts to take advantage of the noisiness of the process of producing evidence at custody hearings. If this is very unlikely, then the IJ will update very strongly in the direction of believing the respondent to be dangerous and vice versa. The equilibrium probabilities for ICE_0 and the IJ are as follows.

$$\lambda^* \quad = \quad \frac{\pi\beta}{q\alpha(1-\pi)}, \text{ and} \tag{6}$$

$$p^* \quad = \quad 1 - \frac{\epsilon_0}{qv_0} \tag{7}$$

These probabilities make the IJ indifferent between granting bond and not when she observes strong evidence and ICE_0 indifferent between denying and granting initial bond. Equation 6 shows that ICE_0 will be increasingly likely to bring a case as the IJ's prior and the IJ's cost of releasing a dangerous

person increase. This is because those changes raise the incentive for the IJ to deny bond. ICE_0 can take advantage of this change by increasing his rate of initial bond denials. In contrast, ICE_0 is increasingly likely to grant initial bond as the value of strong evidence decreases and the IJ's cost of detaining a peaceful person increase. This is because these changes increase the IJ's incentive to deny bond, and ICE_0 must reduce his initial denial rate for equilibrium to be sustained.

Equation 7 gives the probability that the IJ will grant bond having observed strong evidence pointing to dangerousness (e_s). As ICE's cost of releasing a peaceful detainee increases, this probability increases, because as this cost increases, ICE_0 has a stronger incentive deny initial bond. The IJ responds by granting bond at a higher rate, again conditional on observing the strong evidence. Likewise, the IJ's rate of granting bond condition on observing strong evidence will increase when the value of evidence decreases (q increases). This is because an increase in q means that ICE_0 has a stronger chance of surprisingly producing strong evidence at the hearing, which again increases his incentive to deny initial bond. This will be offset by a higher conditional bond grant rate. And finally, the IJ's conditional bond grant rate will decrease in the costs of litigation, because as these costs rise, the prosecutor will have a stronger incentive to grant initial bail. For equilibrium, the IJ's grant rate will need to fall.

4.5 The Observable Effects of Brito

How would the Brito decision have influenced this kind of interaction? We continue to conceptualize the change in Brito as operating via an increase in the cost of incorrectly denying bail (α). We first consider the fourth case, which is an equilibrium that best approximates some simple facts about immigration detention. The most obvious observable implications of Cases 1 and 2 is that immigration court judges should deny bond in all cases before them; and the most obvious implication of Case 3 is that immigration judges should have an empty docket of custody cases. Case 4, in contrast, envisions a scenario in which custody hearings are held, IJs both deny and grant bond, and prosecutors discriminate when making initial bond decisions, so that some individuals are released without requesting bond from an immigration court. Thus is the equilibrium in the fourth case captures the empirical reality that we readily observe each year in immigration court.

An increase in α has two kinds of effects on equilibrium behavior in Case 4. The first, direct effect is to lower the rate at which prosecutors deny initial bond to individuals whom they do not believe to be dangerous. This effect would be observed as a reduction in the caseload of the immigration

court. The second effect is indirect. It lowers the probability of observing an IJ decision granting bond. Importantly, this is not because immigration judges become less likely to grant bond when they observe strong evidence – p^* is independent of α. What is happening is that prosecutors are less likely to push weak cases (λ^* decreases as α increases), and so the IJ is less likely to observe weak evidence (e_w). Precisely because the IJ's probablity of granting bail when observing strong evidence is independent of α, the change in the prosecutor's behavior leads to a decrease in the probability of observing a decision granting bail. The Brito decision would have reduced incorrect denials of bond, but in the context of immigration court, you would observe this effect as a decrease in the bond grant rate by IJs.

There are other ways in which increases in α drives these results. First, an increase in α also makes Case 4 an easier equilibrium to sustain as the second threshold would slide to the left. For a sufficiently large α, Case 2 is not possible. What is left are equilibria in which either prosecutors discriminate in their initial bond decisions (Case 1) or in which all individuals are released (Case 3). We do not find Case 3 to be a particularly plausible equilibrium, but if the change in α caused a switch in equilibria, the observable implication in every scenario is a reduction in the caseload. For the cases in which the immigration court has a docket, the observable implication would be a decrease in the IJ's bond grant rate. This suggests the following empirical implications

> *The Brito decision should have decreased the caseload of the MA immigration court relative to immigration courts not subject to Brito. It would have increased the rate at which MA IJs deny bond.*

5 An Empirical Study of Brito v. Barr

The Brito vs.Barr decision offers a compelling natural experiment that provides an opportunity to evaluate the impact of changing the burden and standard of proof in immigration custody decisions. Our empirical expectations suggest that the Brito decision could have affected the immigration detention process in one of two ways: it could have increased the rate at which Massachusetts IJs grant bonded release or it could have decreased the caseload for Massachusetts IJs.

One plausible strategy for evaluating the causal effect of Brito is to employ a difference-in-differences design, though it is essential to recognize that the Brito decision only affected the judges making decisions

in a single state.[60] Also, as will become apparent, although we have access to a large number of bond hearing outcomes, these decisions are tightly clustered within courts and particular judges. Errors are thus highly unlikely to be independent and highly likely to be serially correlated. There are several options for addressing these concerns using a difference-in-differences approach (Gelman and Hill, 2006; Bertrand, Duflo and Mullainathan, 2004; Green and Vavreck, 2008), but we instead appeal to a research strategy equally capable of analyzing clustered data and specially tailored to the natural experiment before us: one in which the Brito decision rearranged the institutional rules for US IJs in Massachusetts while leaving unaffected immigration courts in all other jurisdictions.

5.1 Design

Our primary method of analysis is the synthetic control method for causal inference in comparative case studies (Abadie and Gardeazabal, 2003; Abadie, Diamond and Hainmueller, 2010, 2015, 2020). The method estimates the treatment effect of an intervention by comparing the progression of an aggregate outcome variable for a treated jurisdiction to the progression of the outcome variable for a synthetic control group. The synthetic control group is a weighted combination of control units from a "donor" pool of non-treated jurisdictions. The synthetic control represents a counterfactual for the treated jurisdiction had it not been exposed to the intervention. Suppose that there is a sample of $J + 1$ jurisdictions (states) indexed by j, among which unit $j = 1$ is the jurisdiction of interest and jurisdictions $j = 2$ to $j = J + 1$ are potential comparisons. We say that $j = 1$ is the "treated unit," that is, the jurisdiction exposed to the intervention. The remaining jurisdictions, $j = 2$ to $j = J + 1$ comprise the "donor pool" of potential comparison units unexposed to the intervention under study. We use the synthetic control method in this research to investigate the Brito decision's effect on bond hearing cases in Massachusetts.

5.2 Data and Sample

We use monthly state-level panel data for the period January 2018 thru December 2020. Our sample provides 24 months of pre-treatment data. Our sample period begins in January 2018 because the

[60] There are a very small number of cases subject to the Brito order that are resolved by IJs not in Massachusetts. We exclude all of these cases in our analysis.

synthetic control method requires a perfectly balanced panel, and starting any earlier forces us to drop cases. We truncate the sample in December 2020, the last month of President Donald J. Trump's administration.

We conduct our analysis with data from the EOIR.[61] The data does not contain all necessary information for IJs. To collect this missing data, we scrape data from investiture announcements and code relevant information on each IJ's professional background. The result is a large rectangular dataset where each row represents a unique bond hearing. These observational data nest as follows: states contain multiple IJs, IJs make multiple bond hearing decisions, and detainees may make several appearances in immigration court concerning their bonded release. We collapse these 234,060 observations into a state-month panel. This process transforms our variables from observed values into rates or averages. Bond hearing decisions and IJs' ideology provide examples: individual bond hearings within a particular state in a given month become a state-month measure representing the percentage of bond hearings granting bond; each IJ's ideology aggregates to a sate-month measure that represents the percentage of decisions made by liberal and conservative IJs.

The synthetic control method uses these state-month measures to construct a counterfactual Massachusetts, which is a weighted average of control states in the donor pool. The method specifies weights so that the outcome variable in the control closely matches the outcome variable in the true Massachusetts before the Brito decision. The weights are also chosen so that the distribution of predictor variables (e.g., percentage of hearings held via VTC or percentage of hearings in which the respondent had a criminal record) are balanced across Massachusetts and its synthetic control.

Because the control attempts to reproduce the observed data for a counterfactual Massachusetts absent the Brito decision, we need to remove from the donor pool cases facing similar constraints as those requirements set forth by the Brito decision. To evaluate whether a similar decision affected other jurisdictions, our research team contacted immigration lawyers within each of the US immigration courts to determine whether the burden or standard of proof had changed for immigrants held in detention

[61] Retrieved January 4th, 2021 from `https://www.justice.gov/eoir/foia-library-0`. The agency posts this relational database as a large ZIP file containing 98 tables that users need to reassemble themselves. We use the EOIR Case Data Code Key and unique identifiers to reassemble information on bond hearings, case information, criminal charges, and immigration judge identifiers.

pending immigration removal proceedings.[62] We learned from these informed sources that no other jurisdiction experienced a change similar to Massachusetts and that legal rules in all other jurisdictions require detainees to prove to the IJ's satisfaction that they are neither dangerous nor a flight risk.[63] Thus, the only states we remove from the donor pool are those where no bond hearings decisions occured in at least one during our study period.

5.3 Outcome variables

Our first outcome variable is the rate of bond hearings in which IJs granted detainees bonded release (or simply, "granted bond"). We use EOIR data for individual bond hearings[64] to calculate this state-month measure. The data required extensive cleaning and we removed observations using a field that records each IJ's bond decisions.[65] The initial sample includes 234,060 bond hearing decisions made between January 2018 and December 2020. We removed observations in which the decision code indicates the decision is outside our scope, undefined, or illogical. Decisions in which IJs declared they had no

[62]Human subject research conducted under Emory IRB protocols. We only asked lawyers factual information about the applicable law in the jurisdictions in which they practice, and thus this did not amount to human subjects research.

[63]After a comprehensive legal review, we find that two other federal courts that have shifted the burden and standard of proof identically to the Massachusetts District Court: the Western District of New York, which governs the Batavia and Buffalo immigration courts, in a case decided in September 2020; and the Maryland District Court, which governs all of Maryland's immigration courts, in a case decided May 29, 2020. All other immigration courts continue to be governed by the existing immigration regulation.

[64]The specific table we use is named `TblAssociatedBond.csv` in the publicly available file. In the January 2021 release, this file contained 51 variables and 1,356,809 observations.

[65]The coded values are **C** ('New Amount') the IJ set a different bond from the bond ICE set, or set a bond where ICE denied bond; **R** ('Recognizance') the IJ released the individual on recognizance; **S** ('No Change') the IJ did not change ICE's bond decision; **A** ('No Action') the IJ did not take any action; **N** ('No Bond') the IJ denied bond; **J** ('No Jurisdiction') the IJ determined that he or she lacked jurisdiction to make a bond decision because a statute made the individual ineligible for bond; **D**, **G**, **O**, **F**, and **L** are codes that exist in the database no longer used.

jurisdiction are outside our scope (7,967 observations). Undefined decisions are those with missing (12,208 observations) or uninterpretable codes (2,188 observations). The data contain several illogical coding combinations that we remove. First, we omit observations in which the decision code indicates the IJ decided on a new bond amount, but the data is missing information on this new amount (109 observations). Second, we also strike observations where the decision code indicates the IJ made 'no change' to ICE's earlier decision, but data on the initial bond is missing while data on the new bond is non-missing (198 observations). We take similar action when the decision code indicates the IJ took 'no action' to ICE's earlier decision, but data on the initial bond is missing while data on the new bond is non-missing (418 observations). Fourth, we eliminate observations in which the initial and new bond amounts are zero, but the decision codes include 'recognizance,' 'no change,' and 'no bond' (16 observations). We then create a binary variable identifying whether an IJ decides to grant a detainee bonded release. Table 1 shows our coding protocol for this outcome:

Table 1: Coding Outcome Variable Granted Bond

Coded as 0 under any of the following:	Coded as 1 under any of the following:
• IJ denied bond; • IJ did not change the prior bond decision *and* the bond set by ICE in its initial custody determination is zero or is missing; or • IJ did not take any action *and* the bond set by ICE in its initial custody determination is zero or is missing.	• IJ updates the preexisting bond amount; • IJ releases the individual on recognizance; • IJ did not change the prior bond decision *and* the bond set by ICE in its initial custody determination is non-missing and greater than zero; or • IJ did not take any action *and* the bond set by ICE in its initial custody determination is non-missing and greater than zero.

We use a battery of predictors to estimate our outcome variables. Five variables are measured at the same level our outcome variable: whether the hearing occurs in-person or through video teleconferencing (VTC), whether the detainee has legal representation, the number of times the detainee has

appeared for a bond hearing, and whether DHS alleges the detainee violated a criminal charge.[66] Judicial ideology accounts for differences between judges. Our proxy for this concept is a binary variable representing the party of the president whose Attorney General appointed them an immigration judge (0 = conservative/Republican, 1 = liberal/Democrat). We use investiture announcements that the EOIR makes public to code this variable. Six remaining variables account for political and economic differences between jurisdictions: percentage of seats filled by democrats in the state's Senate,[67] labor force participation,[68] state GDP (logged),[69] state population (logged),[70] and government employees per capita.[71] The synthetic control uses these predicted estimates averaged over the 24-month pre-treatment period to construct a synthetic control group from the donor pool. We add three periods of the lagged outcome variable (months -18, -12, and -6 months) to improve the estimates.

The synthetic control method allows us to construct a counterfactual Massachusetts that mirrors the values of the aggregate outcome variable in the actual Massachusetts before the Brito decision. We then estimate the effect of the decision on future bond hearing decisions as the difference in the rate of bond hearings granted bonded release in Massachusetts compared to its counterfactual synthetic. If the decision affects judicial decision-making, then we expect an increases in the percentage of bond hearings in which IJs grants bonded release. By "increase" we mean that the rate granted bonded release in the observed Massachusetts will be measurably higher than the synthetic Massachusetts. If, however, the Brito decision leads prosecutors to anticipate IJ decisions, the effect will be lower caseloads for IJs in Massachusetts. We can infer that the Brito decision does not affect judicial decision-making

[66]EOIR makes available data that includes measures for all these variables

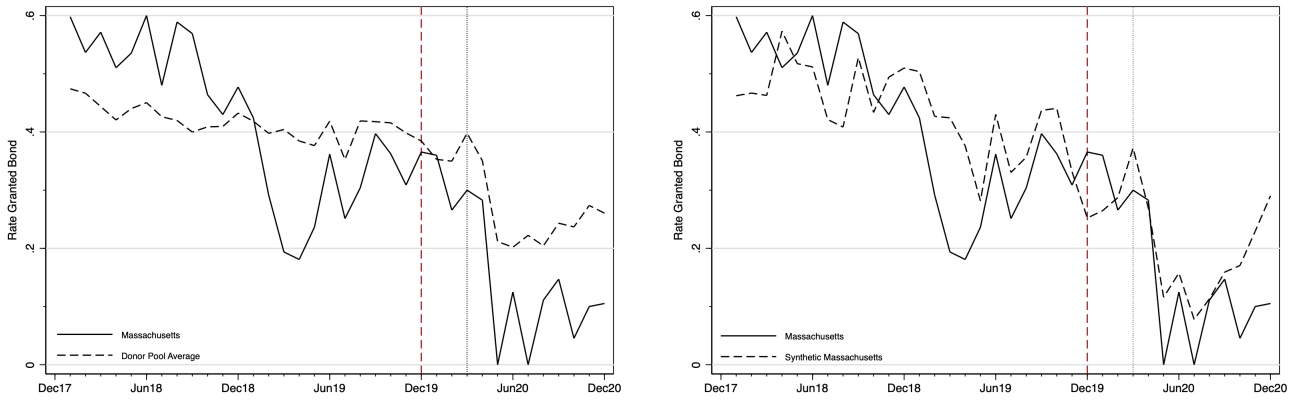
[67]National Conference of State Legislatures

[68]Labor-force participation rate is the number of all employed and unemployed workers divided against the state's civilian population. Source: Federal Reserve Bank of St. Louis

[69]Total GDP in millions of dollars (annually). Transformed into equally-weighted monthly values. Source: Federal Reserve Bank of St. Louis

[70]Residents' measured in thousands of persons (annually). Source: Federal Reserve Bank of St. Louis

[71]Number of people in government occupations. Source: Federal Reserve Bank of St. Louis



(a) Actual MA versus Donor Pool Average

(b) Actual MA versus Synthetic MA

Figure 4: Comparing the Progression of Rate Granted Bond in Massachusetts, the Donor Pool, and Synthetic Massachusetts

or prosecutorial discretion if there is no measurable distinction between the real and counterfactual Massachusetts.

5.4 Results

5.4.1 Rate Granted Bond

Figure 4a plots the progression of the rate at which IJs granted bonded release in Massachusetts compared to the broader donor pool. The vertical reference line marks the month in which the Brito decision went into effect (December 2019). The figure suggests that the donor pool is an unsuitable, real-world comparison to estimate the effect of the decision on bond hearings in Massachusetts. Even before the Brito decision, the difference between the rate IJs granted bonded release in Massachusetts differed notably. In 2018, the rate in Massachusetts ranged between 40% to 60%, but in the ensuing 18 months, the rate averaged only 30%. In contrast, the granted bond rate in the rest of the United States was regularly about 40% from January 2018 through March 2020. To evaluate the effect of the Brito decision on the rate of bonded release in Massachusetts, the key question is how the rate would have progressed in Massachusetts after December 2019 in the absence of the Brito decision. The synthetic control method provides a systematic way to estimate this counterfactual.

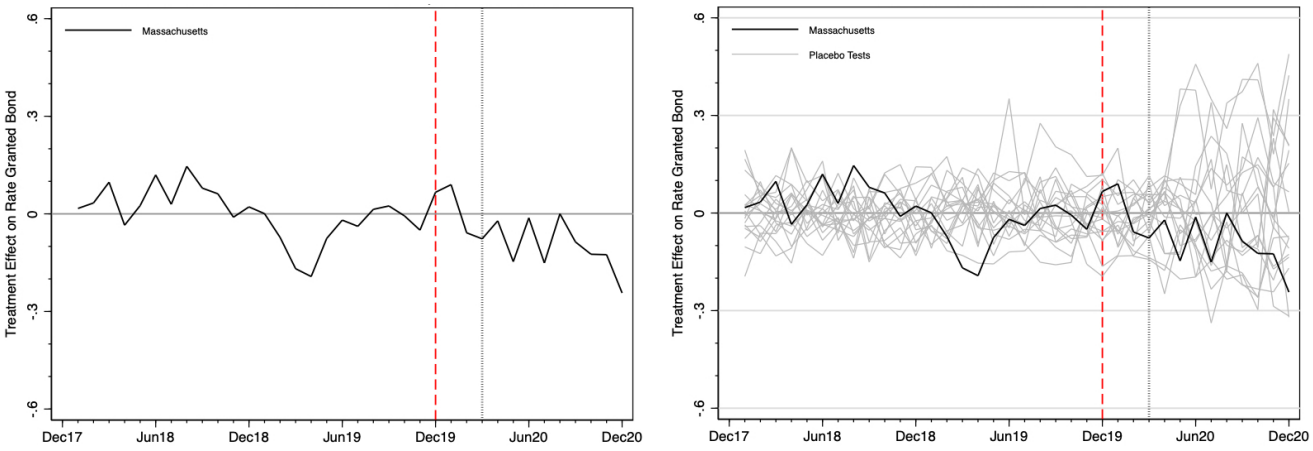
A combination of New Mexico, Washington, California, and Minnesota reproduce the observed values with the most accuracy (see weights in Appendix Table 4). Table 2 compares pre-treatment characteristics in Massachusetts, the synthetic counterfactual, and the broader donor pool to assess the

relative accuracy of the synthetic control. Similar to Figure 4a, we see that the average of states unaffected by the Brito decision does not provide a suitable control group for Massachusetts. In particular, before the decision, the rate of hearings occurring in-person and the percentage of state Senate seats filled by democrats are higher in Massachusetts than the donor pool's average. By contrast, the rate of hearing conducted though VTC and the proportion of IJs appointed under Democratic presidents are substantially lower in Massachusetts than the donor pool. The synthetic Massachusetts accurately reproduces the values predictor variables had in Massachusetts prior to the Brito decision. What is more, notice that in Figure 4b the synthetic control closely follows the same progression as the observed granted bond rate. The synthetic may not replicate the observed values perfectly, but we see that it offers us a sensible counterfactual compared to the granted bond rates in the donor pool.

Table 2: Granted Bond Rates Predictor Means

	Massachusetts		
Predictor	Actual	Synthetic	Donor Pool
Granted Bond Rate ($t = -18$ months)	0.60	0.49	0.45
Granted Bond Rate ($t = -12$ months)	0.48	0.45	0.43
Granted Bond Rate ($t = -6$ months)	0.36	0.36	0.42
Judicial Ideology (R=0, D=1)	0.30	0.41	0.47
Hearing VTC	0.03	0.14	0.49
Hearing In-Person	0.85	0.85	0.44
Respondent Represented	0.52	0.43	0.54
Number of Appearances	1.28	1.25	1.37
Alleged Criminal Charges	0.07	0.05	0.10
Senate Seats Filled by Democrats	0.83	0.61	0.47
Labor Force Participation	67.58	61.44	63.41
State GDP (logged)	13.23	12.84	13.13
State Population (logged)	15.75	15.64	15.93
Government Employees per capita	0.07	0.08	0.07

The estimated treatment effect of the Brito decision on the rate IJs grant bonded release in Massachusetts is the difference between bonded release rate in observed Massachusetts and synthetic Massachusetts. If our first prediction that the Brito decision increases the rate IJs grant bonded release is correct then the solid line representing the true Massachusetts will diverge in a positive direction from the synthetic control. The discrepancy will be stark if the decision has a full and immediate effect. As Figure 4b shows, the rate IJs grant bond in both the actual and counterfactual Massachusetts continue



(a) Difference Between Actual MA and Synthetic MA (b) Difference Between Actual and Synthetic Placebos

Figure 5: Treatment Effects on Rate Granted Bond

to decline after the Brito decision. What is more, the proximity between the two lines in the 12 months after the decision suggests the ruling did not affect judicial decision-making in Massachusetts. Plotting the difference in rate granted bond in Massachusetts minus the rate granted bond in the synthetic counterfactual more clearly shows the Brito decision's estimated treatment effect. Figure 5a shows the decision has not increased the rate IJs grant bonded release in Massachusetts. Rather, our results suggest that in the twelve months after the ruling came into effect, the rate IJs grant bonded release decreased by an average 7.9% per month, a decline of nearly 25%.[72]

We follow Abadie, Diamond and Hainmueller's (2010) suggestion for inference in the context of a synthetic control study, adopting a randomization inference approach. Under the sharp null hypothesis (i.e., that the treatment effect is 0 for all units), we can construct a reference distribution of test statistics (for us, the difference between treatment and control) by permuting the treatment assignment for each of our $J + 1$ units and then conducting the same synthetic control analysis. This is to say that we conduct a series of placebo tests. In the first, we assume that a donor state had actually received the

[72]We took several steps to test these findings' robustness. One was to extend the pre-treatment period to January 2017. This did not change our findings but did make the synthetic control slightly less precise. In another step, we added predictors to assess whether they changed our results. They did not. Our results persisted regardless of which predictors we added to the synthetic control. The predictors used for robustness checks include the detainee's gender, language, and nationality; IJ's years of experience in the position; and state-level measures for governorships and COVID cases and fatalities.

Brito treatment and that Massachusetts did not. We then run the synthetic control analysis. In the second test, we assume that a different donor state received the Brito treatment and reassigned the placebo donor state from the first test to the donor pool, along with Massachusetts. We then re-ran the synthetic control analysis. We did this for all permutations of a single treatment unit. The result is a distribution of treatment effects that would be observed if the sharp null hypothesis is true. We can then compare our actual analysis with the correct treatment assignments to this distribution and obtain an exact p-value for the effect.

Figure 5b shows the results for these iterative placebo tests. The thin lines represent the treatment effect for each state in the original donor pool. The thicker line denotes the treatment effect for Massachusetts. The figure makes apparent that the Massachusetts treatment effect is middling and unremarkable relative to the distribution of the placebos' treatment effects. Figure 5 suggests the Brito decision did not affect judicial decision-making in Massachusetts.

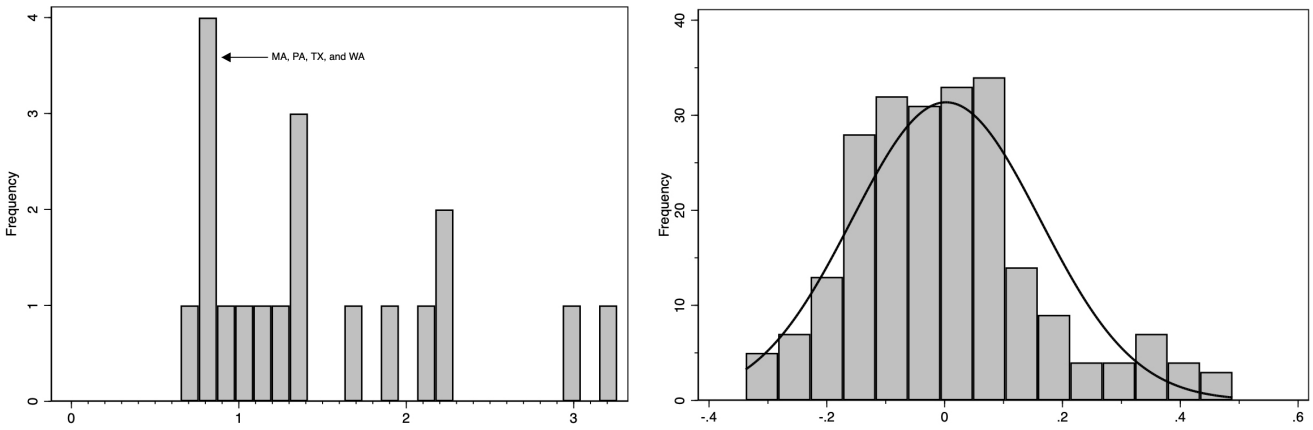
Confidence in these placebo tests depends on the synthetic control's ability to estimate the donor pool's pre-treatment values as well as it did the Massachusetts values. The root mean squared prediction error (RMSPE) assesses a synthetic's accuracy. The RMSPE is the average of the squared discrepancies between granted bond rates in the actual and counterfactual, with lower values indicating higher precision. During the pre-treatment period, the Massachusetts RMPSE is about 0.075. The pre-treatment mean and median RMSPE of the 18 states in the donor pool is 0.068 and 0.078, respectively. These statistics indicate that the synthetic control method provides a reasonable estimate for granted bond rates for most states in our sample, including Massachusetts.

If the synthetic control method produces unreasonable estimates for granted bond rates for Massachusetts in the 24 months before the Brito decision, we would expect the post-treatment estimates to share those failings. Likewise, placebo tests with poor fits (i.e., high RMSPEs) in the pre-treatment period offer less value when evaluating the relative rarity of estimating treatment effects. Eliminating placebo tests with high RMSPEs provides a more accurate distribution–and harder test–against which to compare the Massachusetts results. With this in mind, we include two versions of Figure 5b that cull states with pre-treatment RMSPEs that are at least 15% (Appendix Figure 12a) and 10% (Appendix Figure 12b) greater than Massachusetts' RMSPE. Removing these less precise synthetics does not alter our finding: the Brito decision does not show a measurable effect on judicial decision-making in Massachusetts.

Inspecting the distribution of post-treatment/pre-treatment RMPSEs ratios is another means to infer whether the Brito decision affects IJs in Massachusetts. The ratio quantifies how precise a synthetic control is in the post-treatment period by comparing its accuracy measured in the pre-treatment period. Large ratios identify units with synthetic control that are far less accurate in the post-treatment period than they were in the pre-treatment period. With respect to our analysis, the expectation is that the Brito decision affected Massachusetts IJs if the state has a RMPSE ratio that is uncharacteristically large compared to the donor pool.

Random noise and confounding factors can also decrease a synthetic control's accuracy. We therefore compare the Massachusetts ratio to the distribution of ratios generated by the donor pool, which allows us to probabilistically evaluate the ratio. Where a state's ratio falls in the distribution of all ratios in the sample can be interpreted as the probability of obtaining a similar RMSPE ratio if the treatment were randomly assigned in the data. We can infer that a state's post-treatment period is significantly different from its pre-treatment period if its RMSPE ratio is an outlier compared to the distribution. Figure 6a shows the distribution of RMPSEs ratios of Massachusetts and the donor pool's 18 states. California and Nevada have ratios greater than three, which indicates the post-treatment RMPSEs in those states are three-times larger than in the pre-treatment period. In contrast, the Massachusetts ratio is an unremarkable 0.83. The Massachusetts post-treatment RMSPE is about 17% smaller than the RMSPE for the pre-treatment period, which is similar to Pennsylvania, Texas, and Washington. The Massachusetts ratio's location in the distribution means that if one were to assign the intervention at random, the probability of obtaining a post-treatment/pre-treatment ratio as large as Massachusetts' is $14/19 = 0.74$.

A final assessment of the Brito decision on judicial decision-making in Massachusetts is to compare the distribution of monthly treatment effects in the post-treatment period. Massachusetts, which is the treated unit, provides 12 datapoints. The donor pool provide 216 state-month treatment effects that comprise a control sample that we know the Brito decision did not affect. The distribution of monthly treatment effects (Figure 6b) has a standard deviation of 0.16 and its 5^{th} and 95^{th} percentiles are -0.23 and +0.34, respectively. We can say that observations outside these values are statistically significant at the 0.10 level. The monthly treatment effects in Massachusetts only ranges between -0.24 (December 2020) to +0.09 (January 2020), placing them in the distribution's 4^{th} and 77^{th} percentiles, respectively.



(a) RMSPE Ratios in MA and Donor Pool (b) Monthly Treatment Effect Distribution (n=228)

Figure 6: Distributions of Treatment Effects on Rate Granted Bond

This information leads us to conclude that the Brito decision did not affect judicial decision-making in Massachusetts in a sustained or meaningful way.

5.5 COVID-19

The effect of COVID-19 on the immigration processes within the United States is beyond the scope of this paper. Still, some readers may appreciate an explanation concerning the degree to which COVID jeopardizes our results.

We do not expect COVID threatens inference for several reasons. First and foremost, the virus affected the whole country and we study a federal agency that implemented a consistent outcome strategy in all its locations. Thus, any effect COVID or COVID-response had on bond hearings in Massachusetts should be the same as Illinois or Washington. This rationale would be weaker if our study analyzed local governments implementing different strategies at different times. A foreseeable adaptation to bond hearing operations in the wake of the pandemic is video teleconferencing (VTC) replacing in-person hearings. Our synthetic control addresses this by using both mediums as predictors of the outcome. The sequencing of events also works to our advantage. The Brito decision affected Massachusetts in mid-December 2019 (dashed reference line graphs), and the COVID response in the United States began in earnest in mid-March 2020 (dotted reference line in graphs). The implication of this is that the synthetic control method uses 24 months of COVID-free donor pool data to create the synthetic control group. Fourthly, if the COVID effect overpowered the Brito effect, we would expect to see an inflection point in the treatment effect at or shortly after America's COVID lockdown. Instead,

the first major adjustment we see to the treatment effect occurs before COVID: the estimated treatment effect on granted bond rates is about +10% in January 2020 and -5% in February (Figure 5a). Statistics from the Centers for Disease Control and Prevention,[73] indicate that as of February 29th, 2020, the United States had recorded a total of 150 COVID cases and only two COVID deaths, with one case and zero deaths in Massachusetts. These numbers tell us that COVID had not yet affected American life to the degree necessary to explain the January-February adjustment. Taken together, we do not consider COVID a threat to inference because EOIR did not institute a response strategy that varied between states nor did COVID affect society before the Brito decision.

5.5.1 Average Caseload

Rather than increasing the rate Massachusetts IJs granted bonded release, it is possible that the Brito decision decreased the average caseload for IJs in Massachusetts. We next apply the synthetic control analysis to the average number of bond hearing decisions IJs in a particular state made in a given month (or simply, "average caseload"). Using the same dataset as our initial analysis, we measure this variable at the state level by dividing the number of bond hearing decisions made in a given month by the number of IJs making decisions in that month.

Applying the synthetic control method to the average caseload shows the synthetic control group that best approximates the observed values is a composite of Washington, New Mexico, California, and Pennsylvania. Three of these states contribute to the granted bond synthetic, but do so with weightings that are different than this synthetic control group (see Appendix Table 4). Because this analysis uses the same sample and predictors, the predictor means in Table 3 are identical to the prior analysis (Table 2), with the synthetic control and the lagged outcome variables being the exceptions. As before, we see that the synthetic reproduces the predictors' mean variables with greater accuracy than the donor pool. Notice also that in Figure 7b, the average caseload in the synthetic closely tracks the observed values in the actual Massachusetts before the Brito decision. The donor pool, which has an average monthly

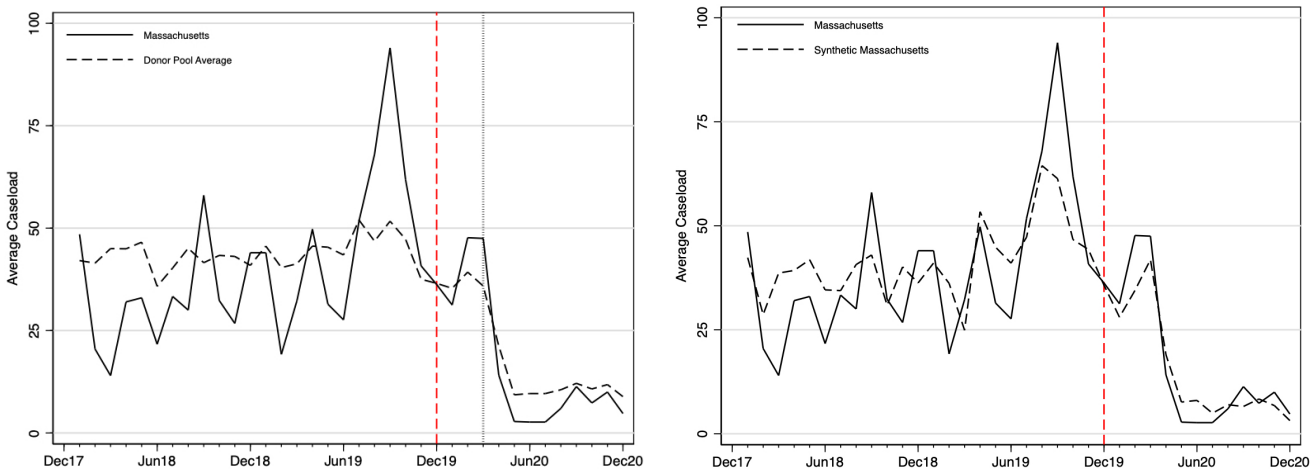
[73]United States COVID-19 Cases and Deaths by State over Time (Retrieved May 16th, 2021, from `www.data.cdc.gov`). The earliest numbers from the U.S. Immigration and Customs Enforcement are from March 26th, 2020 and report only 21 COVID cases among detaineees and ICE employees (`www.ice.gov/coronavirus` via the Internet Archive).

Table 3: Average Caseload Predictor Means

	Massachusetts		
Predictor	Actual	Synthetic	Donor Pool
Average Caseload ($t = -18$ months)	21.67	29.35	0.45
Average Caseload ($t = -12$ months)	44.00	38.55	0.43
Average Caseload ($t = -6$ months)	27.63	36.13	0.42
Judicial Ideology (R=0, D=1)	0.30	0.11	0.47
Hearing VTC	0.03	0.14	0.49
Hearing In-Person	0.85	0.88	0.44
Respondent Represented	0.52	0.41	0.54
Number of Appearances	1.28	1.25	1.37
Alleged Criminal Charges	0.07	0.05	0.10
Senate Seats Filled by Democrats	0.83	0.60	0.47
Labor Force Participation	67.58	61.82	63.41
State GDP (logged)	13.23	12.89	13.13
State Population (logged)	15.75	15.67	15.93
Government Employees per capita	0.07	0.08	0.07

caseload of almost 50 decisions per IJ during the pre-treatment period, fails to track with any degree of accuracy the pattern seen in Massachusetts (Figure 7a).

Our theoretical expectation is that the Brito decision decreased the average caseload for Massachusetts IJs. For our analysis to support this prediction, the solid line representing the true Massachusetts in Figure 7b must meaningfully diverge in a negative direction from the synthetic control. As was the case with our prior analysis, if the Brito decision has a full and immediate effect, then the difference between the actual and counterfactual Massachusetts will be noticeable. Figure 7b shows the average caseload for actual Massachusetts IJs decreased after the Brito decision. The synthetic control unit, which represents the counterfactual Massachusetts absent the Brito decision, is in lockstep with the true Massachusetts throughout the post-decision period. Figure 8 reframes the treatment effect as the true average caseload minus the synthetic caseload. To support our theoretical prediction that the Brito decision deceased the average caseload, the solid line measuring the treatment effect in Massachusetts should be negative and starkly different than the placebo tests. The estimated treatment effect in Massachusetts ranges between increasing the average monthly caseload by roughly 13 decisions (February 2020) and decreasing the average caseload by more than five decisions (June 2020). Our



(a) Actual MA versus Donor Pool Average

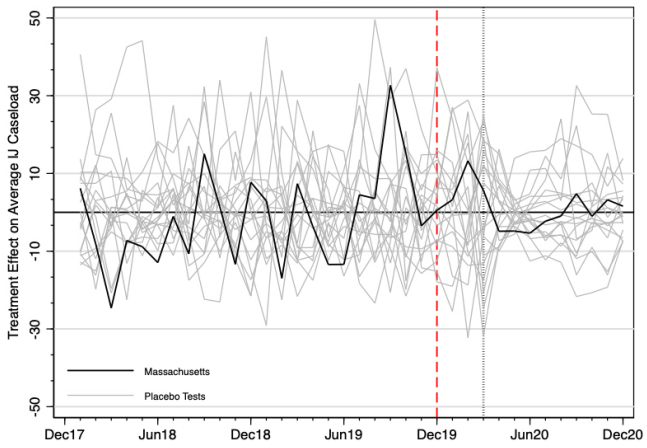
(b) Actual MA versus Synthetic MA

Figure 7: Comparing the Progression of Average Caseload in Massachusetts, the Donor Pool, and Synthetic Massachusetts

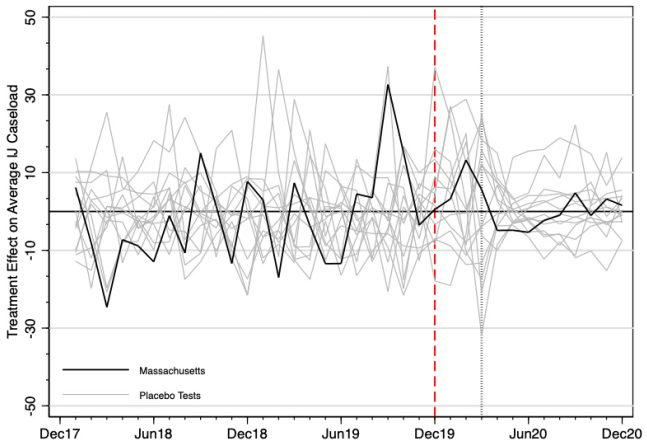
initial results suggest that the average caseload for Massachusetts IJs increased by one decision per month in the twelve months after the Brito decision came into effect.

Our placebo tests affirm that the Brito decision did not affect Massachusetts IJs to the degree expected. Figure 9a shows the distribution of RMPSE ratios of the states studied here. Flordia, Illinois, and Missouri have ratios that are near one. Massachusetts and New Mexico have ratios of 0.40. With only six states possessing lower RMPSE ratios, the placebo tests indicate there is a two-thirds probability that randomly assigning the Brito decision to another state would yield a similar ratio ($13/19 = 0.684$). This high probability tells us there scant evidence supporting our prediction that the Brito decision affected average caseloads in Massachusetts.

Our final assessment of the Brito decision's affect on prosecutorial discretion compares the distribution of monthly treatment effects in the 2020 post-treatment period. Massachusetts provides 12 datapoints while the donor pool's 18 control states contribute 216 monthly treatment effects. For our analysis, the monthly treatment effect distribution (Figure 9b) has a mean of 0.34, standard deviation of 10.7, and its 5^{th} and 95^{th} percentiles are -16.3 and +22.3, respectively. The monthly treatment effects in Massachusetts was its lowest in June 2020 (-5.4, 26^{th} percentile) and highest in February 2020 (+13.2, 88^{th} percentile). Relative to the distribution of monthly treatment effects generated by the placebo tests, the largest Massachusetts treatment effects are too small to support our argument that the Brito decision affected prosecutorial discretion and decreased average caseloads Massachusetts IJs.

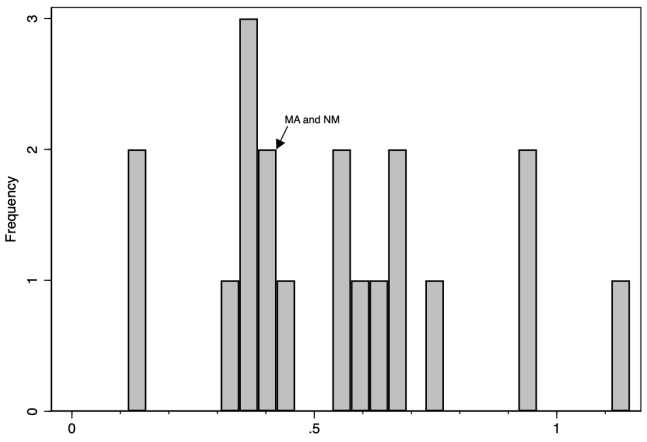


(a) Difference Between Actual and Synthetic Placebos

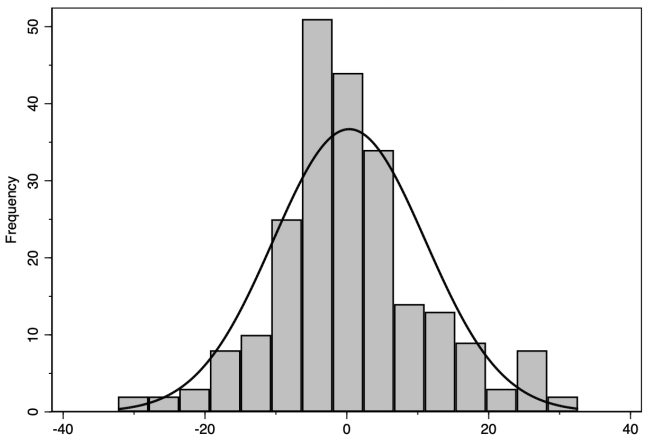


(b) Omits 5 States with RMSPEs 10%+ larger than MA

Figure 8: Differences in Average Caseload Between Actual States and Placebo Tests



(a) RMSPE Ratios in MA and Donor Pool



(b) Monthly Treatment Effect Distribution (n=228)

Figure 9: Distributions of Treatment Effects on Average Caseload

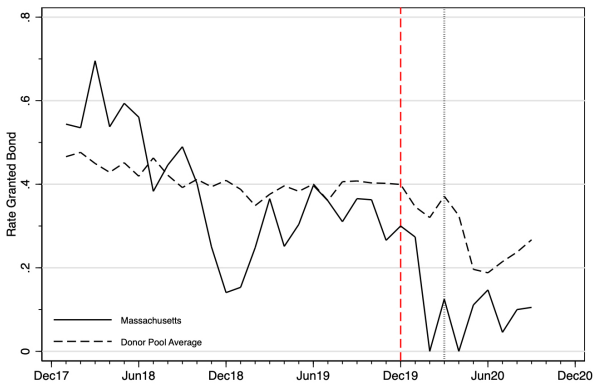
5.6 Effects among GOP Appointees

The previous analyses found no evidence supporting our theoretical expectations that the Brito decision either affected judicial decision-making to increase the rate Massachusetts IJs grant bonded release, or influenced prosecutorial discretion to decrease the average caseload for IJs in Massachusetts. Yet, Model 1 predicts that the effect should be observed among conservative IJs.

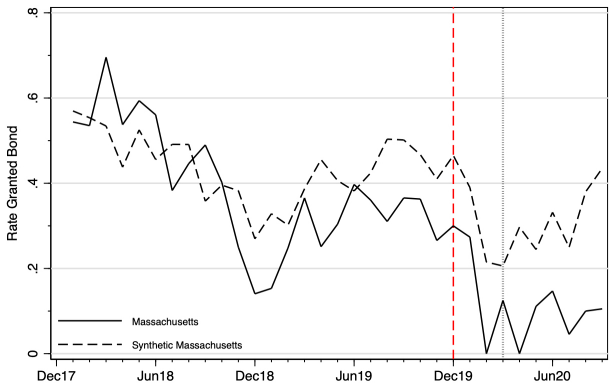
To consider this possibility, we restrict our sample to bond hearing decisions made by IJs most likely affected by the Brito decision. Our working hypothesis is that IJs appointed under liberal Democratic presidents have preexisting preferences that make them more likely to grant bonded release relative to their conservative colleagues. We expect the Brito decision had little effect on these IJs; therefore, we restrict our sample to IJs appointed under conservative Republican presidents. This culls our original sample from 234,060 to 129,879 observations. As before, we collapse the data into a state-month panel. Restricting the sample in this way causes us to end the analysis in September 2020 because only liberal IJs made bond hearing decisions in Massachusetts during the last three months of the post-treatment period. Restricting the sample in this way forced us to discard Illinois, Missouri, and Pennsylvania from the donor pool to maintain the perfectly balanced panel that the synthetic control method requires.

A combination of Minnesota, New York, Colorado, and Nevada reproduce the observed values in this restricted sample with the most accuracy (see weights in Appendix Table 4). Similar to the previous analyses, the synthetic control more closely tracks the observed values than the donor pool (see also predictor means in Appendix Table 5). Notice that in Figure 10b the observed rate conservative IJs granted bond in Massachusetts after the Brito decision is consistently lower than the synthetic control group, which suggests that the treatment effect is either zero or negative. The results of the placebo tests (Appendix Figure 13) show that the treatment effect in Massachusetts is similar to the placebos. Massachusetts' inability to separate its treatment effect from the donor pool is evidence against the Brito decision affecting judicial decision-making in the state.

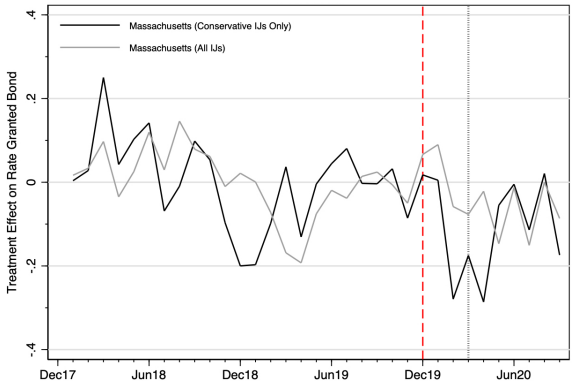
Figure 10c shows the Brito decision's estimated treatment effect on Massachusetts IJs appointed under conservative Republican presidents. The average treatment effect in the nine months following the ruling exceeds -11%. Even excluding the February thru April treatment effects, which ranged between -17% to -29%, the average effect is over -5%. For reference, the lighter line shows the treatment effect from the earlier analysis that did not exclude any observations. That analysis suggested that the Brito



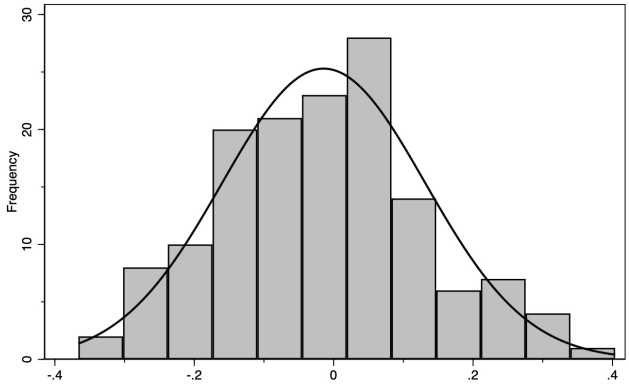
(a) Actual MA versus Donor Pool Average



(b) Actual MA versus Synthetic MA



(c) Estimated Treatment Effect



(d) Monthly Treatment Effect Distribution (n=144)

Figure 10: Robustness Check for Treatment Effect on Rate Granted Bond Using a Sample Restricted to Immigration Judges Appointed Under Republican Presidents

decision may have increased granted bond rates nearly 10% in January 2020. Figure 10c shows that once we omit decision made by IJs appointed under liberal Democratic presidents, the rate conservative IJs in Massachusetts granted bond appears lower in six of the nine months following the Brito decision.

Figure 10d is the distribution of monthly treatment effects in the post-treatment period, which for this robustness check in January thru September 2020. Massachusetts provides nine observations while the donor pool's 15 control states contribute 135 monthly treatment effects. The distribution has a standard deviation of 0.15, and its 5^{th} and 95^{th} percentiles are -0.26 and +0.24. The monthly treatment effects in Massachusetts was its lowest were February (-0.28, 3^{rd} percentile) and April (-0.29, 2^{nd} percentile), and its highest was August 2020 (+0.02, 60^{th} percentile). Relative to the distribution of treatment effects generated by the placebo tests, only the February and April effects are strong enough to be considered significantly different. The concern, of course, is that these are only two of nine months, and the directionality of the effect is opposite of our prediction.

6 Discussion and Conclusion

The analysis that we have conducted to date suggests that Brito did not have an effect either on IJ or ICE decisions. The caseload plots clearly indicate that COVID-19 reduced caseloads. Brito seems neither to have done so nor caused IJs in Massachusetts to grant bond at a higher rate.

Here we offer a few bullet points laying out what analysis we will conduct next. Deriving normative implications of the study is premature given the work we have yet to conduct, but it is worth at least speculating on what is likely to come of this.

Work to do

- We are still confirming that our understanding of the national legal landscape is correct. It is possible that an error in assigning immigration courts to the donor pool could result in poor counterfactual MA.
- We are currently measuring ideology with a crude proxy: the partisanship of the appointing Attorney General's president. We are working on several alternative and will complete that work shortly.
- The synthetic control method requires balance across the distribution of the predictor variables. One source of imbalance is the use of VTC hearings in MA. We are considering whether this source of imbalance is responsible for the observed effects.
- We have conducted a series of difference-in-differences analyses, which find mixed results, and which are themselves not robust to corrections for multiple comparisons. We have yet to complete this work.
- We are planning interviews with stake holders in MA to learn what they believe has happened post-Brito.

Ideas on implications

- If these findings hold up, we believe that they are important. They would suggest that a profound change in the rules IJs use for evaluating assertions about flight risk and dangerousness have no impact on decision-making or prosecutorial discretion. One possibility is that the MA IJs were

already applying the rules laid out in Brito. We find this somewhat implausible, but are open to learning that it was true. The alternative is that neither IJs nor ICE (at least under the Trump administration) simply ignored the consequences of the District Court's decision. Given the career incentives of IJs and ICE prosecutors, perhaps this should not be surprising; however, it raises an important concern about the U.S.'s continued use of immigration courts to monitor ICE.

- Picking up on the first idea, these results would offer significant support to a reform effort aimed at placing the federal courts in a position of conducting more direct monitoring of the U.S. immigration system.

- Our theoretical analysis suggests that the effects of Brito could have been more variable than we have been able to observe in this study. We believe that it is important to learn how IJs interpret and apply the statutory standard. Experimental studies may be instructive, even conducted on law students. We are particularly interested in studies conducted on a sample of former IJs. But even simple interview based research could produce highly useful information about how the burden of proof and standards of proof influence immigration court decisions.

- Finally, we note the similarity between our findings and Finley and Karnes's (2008), who found no effect of the shift in the burden of proof in Tax Court. It is entirely possible that these features of the law have not meaningful effect generally, at least once you control for extra-legal influences.

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7 Appendix

7.1 Model 1

The structure of the decision problem studied in Model 1 is described fully in the text. Given that $Pr(e_s|\omega = 1) = 1$, if ever the IJ observes e_w, she can infer $\omega = 0$; and, she will naturally set $b = 1$. The question is what to do when she observes e_s. In this case, she has either observed ICE_1 bring a case of a truly dangerous respondent or ICE_0 bring a case of a truly peaceful respondent, but where the evidentiary process has resulted in a record that points to dangerousness. The expected utility of granting bond having observed e_s is

$$\text{EU}_{IJ}(b = 1|e_s) \quad = \quad \frac{\pi}{\pi + (1 - \pi)q}(-\beta),$$

and the expected utility of not granting bond having observed e_s is

$$\text{EU}_{IJ}(b = 0|e_s) \quad = \quad \frac{(1 - \pi)q}{\pi + (1 - \pi)q}(-\alpha).$$

To grant bond, it must be that $\text{EU}_{IJ}(b = 1|e_s) \geq \text{EU}_{IJ}(b = 0|e_s)$. Solving this inequality for q yields

$$q \quad \geq \quad \frac{\pi\beta}{\alpha(1 - \pi)},$$

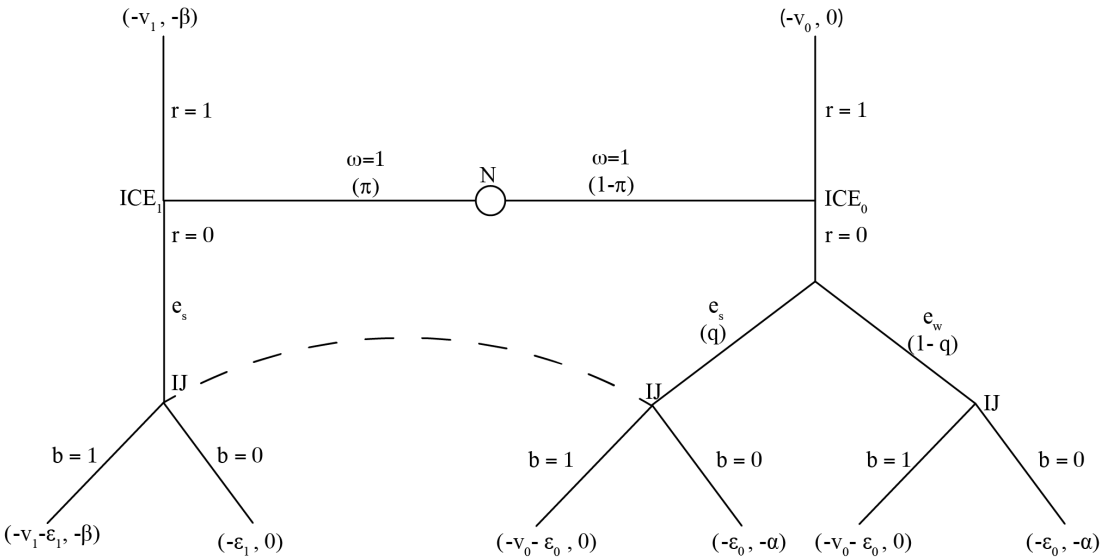
which is the condition described in the text.

7.2 Model 2

Figure 11 shows the structure of the bond game. There are three Perfect Bayesian equilibria in pure strategies, as well as a single semi-separating equilibrium. We consider each in turn.

7.2.1 Case 1

The first case has ICE_1 choose $r = 0$ and ICE_0 choose $r = 1$. The IJ always chooses $b = 1$ if she observes e_w. Given the ICE strategy, when the IJ observes e_s, she believes that $Pr(\omega = 1|r = 0, e_s) = 1$ and



Game Diagram of Model 2. *Illustrates terminal histories in the bond decision game. The game begins with a random draw from the state space, which selects the true level of dangerousness of the detainee (ω). ICE makes an intial bond decision (r). If ω=1, IJ will observe e_s at the hearing; and, if ICE denies bond, IJ will observe e_s with probability q. IJ is then tasked with making a bond decision (b).*

Figure 11

$Pr(\omega = 0|r = 0, e_s) = 0$. Given this belief structure, she selects $b = 0$ if she observes e_s, because $\beta > 0$. ICE_1 clearly has no incentive to set $r = 1$, given the IJ's strategy. Thus, we only need to consider ICE_0's decision. For ICE_0 to choose $r = 1$, it must be that

$$\begin{aligned} q(-v_0 - \epsilon_0) + (1 - q) &< -v_0, \text{which holds when} \\ q &< \frac{e_0}{v_0}. \end{aligned}$$

7.2.2 Case 2

The second case has ICE_i choose $r = 0$. The IJ makes a decision consistent with the signal she observes, setting $b = 0$ if e_s and $b = 1$ otherwise. Given the ICE strategy, the IJ's beliefs when she observes e_s are identical to what they are in Model 1. And thus, she will deny bond if $q < \frac{\pi\beta}{\alpha(1-\pi)}$. In order for

ICE_0 to set $r = 0$, it must be sufficiently likely that the IJ will observe e_s if ICE_0 denies the initial bond. This requires $q \geq \frac{\epsilon_0}{v_0}$.

7.2.3 Case 3

The third case has ICE_i choose $r = 1$, and the IJ sets $b = 1$ no matter what signal she observes. Beliefs in this equilibrium are not defined via Bayes's rule, because the probability of observing any signal of dangerousness is 0 given the ICE strategy. We have assumed that the IJ will not update her beliefs. Given that it is impossible to e_w if $\omega = 1$, the IJ knows that the respondent is not dangerous after observing e_w. Given this IJ strategy, neither prosecutor has an incentive to bring a case, since $\epsilon_0 > 0$.

When observing e_s in this equilibrium, the IJ's beliefs are exactly as they are in Model 1. For this set of strategies and beliefs to be in equilibrium, $Pr(e_s|\omega = 0, r = 0)$ has to be sufficiently large so that the IJ is willing to ignore the signal and simply set $b = 1$. Thus, we require $q \geq \frac{\pi\beta}{\alpha(1-\pi)}$.

7.2.4 No other pure strategy PBE

The remaining pure strategy profile would involve ICE_0 choosing $r = 0$ while ICE_1 chooses $r = 1$. Under this strategy, the IJ would believe that $Pr(\omega = 1|e_s, r = 0) = 0$ and $Pr(\omega = 0|e_s, r = 0) = 1$, and would accordingly set $b = 1$ since $\alpha > 0$. In so far as the IJ sets $b = 1$, ICE_0 would clearly prefer to set $r = 1$, since $\epsilon_0 > 0$. Thus, this kind of profile can not be part of a PBE.

7.2.5 Semi-Separating equilibrium

Now consider a semi-separating equilibrium in which ICE_1 chooses $r = 0$ and ICE_0 chooses $r = 0$ with positive probability, which we denote λ. The IJ always chooses $b = 1$ if observing e_w. She chooses $b = 1$ with positive probability if she observes e_s, which we denote p. For this profile to be a PBE, it must be that the ICE_1 strictly prefers to deny initial bond (r=0) when the IJ sets $Pr(b = 1) = p$ while ICE_0 is indifferent between denying and granting initial bond. Simultaneously, the IJ must be indifferent between granting bond and denying bond when ICE_0 sets $Pr(r = 0) = \lambda$.

For the IJ expected utility of granting bond having observed e_s is

$$\text{EU}_{IJ}(b = 1|e_s) \quad = \quad \frac{\pi}{\pi + (1 - \pi)\lambda q}(-\beta),$$

and the expected utility of not granting bond having observed e_s is

$$\begin{aligned} \mathrm{EU}_{IJ}(b=0|e_s) &= \frac{(1-\pi)\lambda q}{\pi+(1-\pi)\lambda q}(-\alpha). \end{aligned}$$

In equilibrium, it must be that $\mathrm{EU}_{IJ}(b=1|e_s) = \mathrm{EU}_{IJ}(b=0|e_s)$. Solving this equation for λ yields

$$\begin{aligned} \lambda^* &= \frac{\pi\beta}{\alpha q(1-\pi)}. \end{aligned}$$

Given allowable values of the parameters, $\lambda^* > 0$. For $\lambda^* < 1$, it must be that $q > \frac{\pi\beta}{\alpha(1-\pi)}$

The cost of $r = 1$ is v_0 for ICE_0. If ICE_0 chooses $r = 0$, there are three possible outcomes: the IJ observes e_w, the IJ observes e_s and sets $b = 1$, and the IJ observes e_s and sets $b = 0$. Given the IJ's strategy, equilibrium requires that the expected utility of $r = 0$ for ICE_0 is equal to the cost of $r = 1$, which to say that

$$\begin{aligned} -v_0 &= (1-q)(-v_0-\epsilon_0) + qp(-v_0-\epsilon_0) + q(1-p)(-\epsilon_0). \end{aligned}$$

Solving this equation for p yields

$$\begin{aligned} p^* &= \frac{qv_0-\epsilon_0}{qv_0}, \text{or equivalently} \\ p^* &= 1-\frac{\epsilon_0}{qv_0}. \end{aligned}$$

Given the allowable values of the parameters $p^* < 1$ always, and $p^* > 0$ when $q > \frac{\epsilon_0}{v_0}$.

For equilibrium, ICE_1 must prefer to bring a case in light of the IJ's strategy. This requires

$$\begin{aligned} -v_1 &\leq p^*(-v_1-\epsilon_1) + (1-p^*)(-\epsilon_1) + q(1-p)(-\epsilon_0), \text{so that} \\ p^* &\leq \frac{v_1-\epsilon_1}{v_1}, \quad \text{and plugging in for the equilibrium } p \text{ and solving for q we have,} \\ q &\leq \frac{v_1\epsilon_0}{v_0\epsilon_1}. \end{aligned}$$

This last inequality always holds. To see how, note that $v_1 > v_0 > \epsilon_0 > \epsilon_1 > 0$, and so that the right hand side is always greater than one.

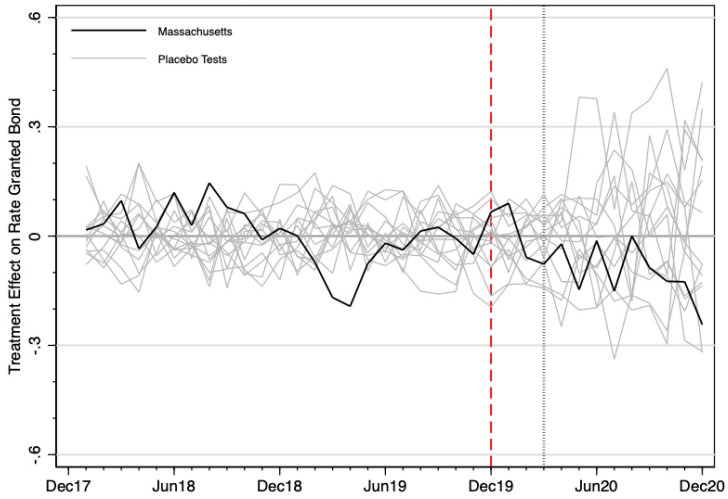
7.3 Synthetic Control Supplemental

Table 4 displays the weights of states for each analysis. Zeros identify states available in the donor pool but that do not contribute to the synthetic control. The synthetic control method requires a perfectly balanced panel; therefore, states omitted from the table either do not contain bond hearing locations or show no record of hearing decisions in at least one month between January 2018 and December 2020. States in which hearings occurred but recorded no bond hearing decisions in a particular month are: CT, GU, HI, ID, KY, MD, MI, NC, OH, OR, PR, TN, VI, and UT.

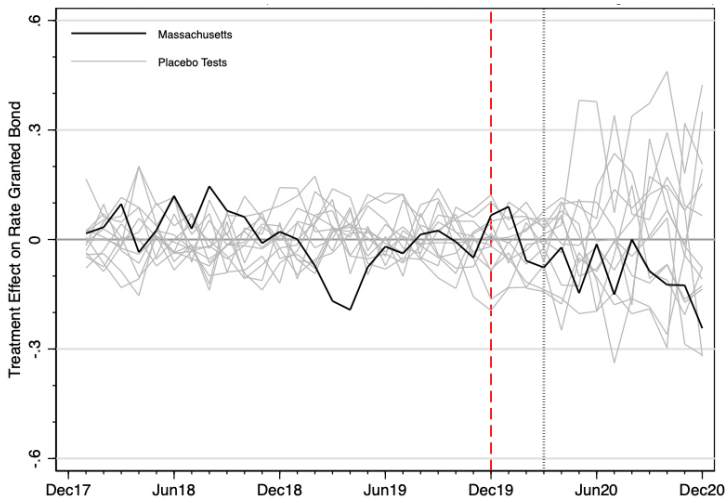
Table 4: State Weights in the Synthetic Massachusetts for all Outcome Variables

	Full Sample		Restricted Sample	
	Granted Bond	Caseload	Granted Bond[a]	Caseload[b]
Arizona	0	0	0	0
California	0.210	0.168	0	0.061
Colorado	0	0	0.205	0.010
Florida	0	0	0	0.007
Georgia	0	0	0	0.338
Illinois	0	0	x[c]	0
Louisiana	0	0	0	0.159
Minnesota	0.021	0	0.495	0
Missouri	0	0	0	0
Nebraska	0	0	0	0
Nevada	0	0	0.014	x[c]
New Jersey	0	0	0	x[c]
New Mexico	0.405	0.362	0	x[c]
New York	0	0	0.285	0
Pennsylvania	0	0.006	x[c]	0
Texas	0	0	0	0.086
Virginia	0	0	0	0
Washington	0.363	0.465	0	0.081

[a] Restricted to decisions by IJs appointed under Republican presidents.
[b] Restricted to decisions where ICE alleges criminal charge.
[c] State dropped to maintain a perfectly balanced panel.



(a) Omits States with RMSPEs 15%+ larger than MA



(b) Omits States with RMSPEs 10%+ larger than MA

Figure 12: Differences in Rate Granted Bond Between Actual States and Placebo Tests

7.3.1 Rate Granted Bond (Full Sample)

Figure 12a discards placebos with pre-treatment RMSPEs at least 15% larger than Massachusetts. After excluding Colorado, Louisiana, Minnesota, and Missouri, we see that Massachusetts in the post-treatment period is not noticeably different from the remaining donor pool. Lowering the threshold to 10% (Figure 12b) discards a fifth state, New Mexico. The synthetic controls for these states are less precise than Massachusetts; still, after removing them, Massachusetts does not present itself as an 'unusual' line and fails to show evidence that the Brito decision had a measurable effect on judicial decision-making in the state.

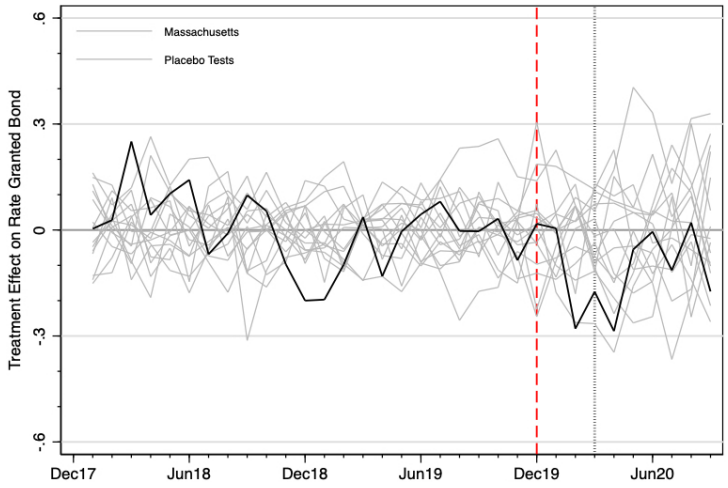
7.3.2 Rate Granted Bond (Conservative IJs only)

Table 5 compares pre-treatment characteristics in Massachusetts, the synthetic counterfactual, and the broader donor pool to assess the relative accuracy of the synthetic control. We see that the average of states unaffected by the Brito decision is a poor control group for Massachusetts. In particular, before the decision, the proportion of hearing occurring in person and the percentage of state senate seats filled by democrats are higher in Massachusetts than the donor pool's average. By contrast, the rate of VTC hearings and the percentage of hearings in which detainees had representation are lower in Massachusetts than the donor pool.

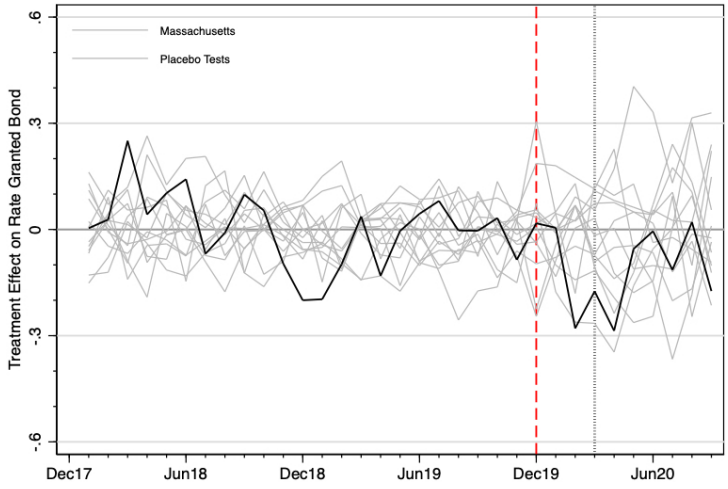
Table 5: Rate Granted Bond Predictor Means (Conservative IJs Only)

	Massachusetts		
Predictor	Actual	Synthetic	Donor Pool
Granted Bond Rate ($t = -18$ months)	0.56	0.43	0.42
Granted Bond Rate ($t = -12$ months)	0.14	0.28	0.41
Granted Bond Rate ($t = -6$ months)	0.40	0.34	0.40
Judicial Ideology (R=0, D=1)	0.00	0.00	0.00
Hearing VTC	0.08	0.25	0.47
Hearing In-Person	0.76	0.64	0.47
Respondent Represented	0.50	0.53	0.55
Number of Appearances	1.30	1.34	1.37
Alleged Criminal Charges	0.06	0.10	0.08
Senate Seats Filled by Democrats	0.83	0.51	0.48
Labor Force Participation	67.66	67.07	63.44
State GDP (logged)	13.24	13.21	13.09
State Population (logged)	15.75	15.88	15.89
Government Employees per capita	0.07	0.08	0.07

Figure 13 shows the results of the placebo tests for all states in the donor pool (Figure 13a) and those with with less precise synthetic controls according to the pre-treatment RMSPEs (Figure 13b). Even after removing placebos with less precise synthetic controls, Massachusetts does not present itself as an 'unusual,' which is evidence against the Brito decision affecting judicial decision-making in the state.



(a) Difference Between Actual and Synthetic Placebos



(b) Omits States with RMSPEs 10%+ larger than MA

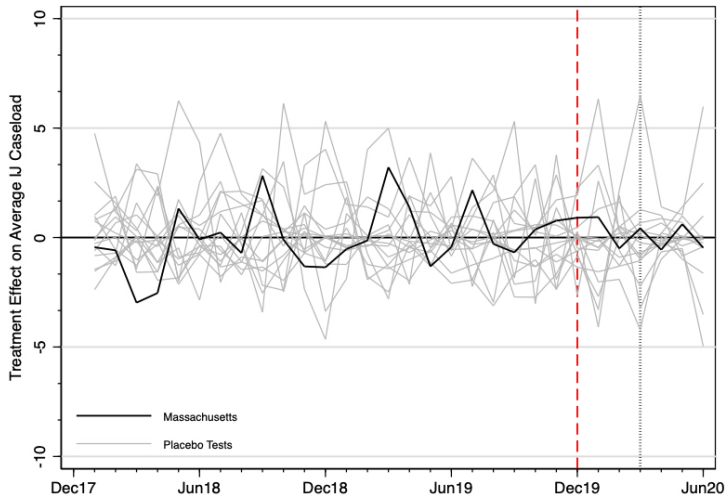
Figure 13: Placebo Tests for Rate Granted Bond among Conservative IJs

7.3.3 Average Monthly Caseload (Alleged Criminal Offenses)

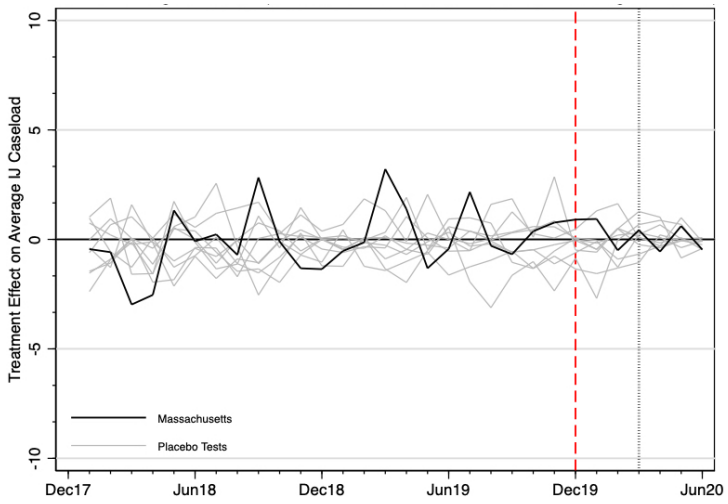
Table 5 compares pre-treatment characteristics in Massachusetts, the synthetic counterfactual, and the broader donor pool to assess the relative accuracy of the synthetic control. We see that the average of states unaffected by the Brito decision is a poor control group for Massachusetts. In particular, before the decision, the proportion of hearing occurring in person and the percentage of state senate seats filled by democrats are higher in Massachusetts than the donor pool's average. By contrast, the rate of VTC hearings and the percentage of hearings in which detainees had representation are lower in Massachusetts than the donor pool.

Table 6: Average Monthly Caseload (Alleged Criminal Offenses)

	Massachusetts		
Predictor	Actual	Synthetic	Donor Pool
Average Monthly Caseload ($t = -18$ months)	0.56	0.43	0.42
Average Monthly Caseload ($t = -12$ months)	0.14	0.28	0.41
Average Monthly Caseload ($t = -6$ months)	0.40	0.34	0.40
Judicial Ideology (R=0, D=1)	0.00	0.00	0.00
Hearing VTC	0.08	0.25	0.47
Hearing In-Person	0.76	0.64	0.47
Respondent Represented	0.50	0.53	0.55
Number of Appearances	1.30	1.34	1.37
Alleged Criminal Charges	0	0	0
Senate Seats Filled by Democrats	0.83	0.51	0.48
Labor Force Participation	67.66	67.07	63.44
State GDP (logged)	13.24	13.21	13.09
State Population (logged)	15.75	15.88	15.89
Government Employees per capita	0.07	0.08	0.07



(a) Difference Between Actual and Synthetic Placebos



(b) Omits States with RMSPEs 10%+ larger than MA

Figure 14: Placebo Tests for RAverage Monthly Caseload (Alleged Criminal Offenses)