# Public Partisan Reactions to Judicial Checks: Evidence from Argentina

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Can court decisions limiting executive power influence how citizens evaluate judicial institutions? I study the effects of two court rulings in Argentina, which invalidated a highly salient judicial reform promoted by the President. I hypothesize that the court decisions will undermine public support for judicial institutions among presidential co-partisans, but increase support among out-partisans. Using data from a survey fielded before, during, and after the judicial decisions, I find that the rulings significantly decreased Argentines' overall public trust in the judiciary and that government co-partisans react negatively to the rulings. However, out-partisans are not more supportive of the judiciary following the court decisions. These results both advance our knowledge of the determinants of public evaluation of judicial institutions involved in inter-branch conflicts and have important implications for our understanding about the role of public support for courts.

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# 1 Introduction

To endure and ensure their efficacy, all democratic institutions require some degree of support from the public. If disapproved of by a sufficient number of citizens, institutions are feeble and rendered impotent to achieve their goals. Public support is particularly important for judicial institutions as they lack both the 'purse and the sword' (Federalist No. 78). Acknowledging this institutional weakness, an influential body of research has argued that high levels of public support may shield courts from potential backlash by elected officials (Vanberg 2001, 2005, 2015; Staton 2006, 2010; Helmke 2010*b*; Carrubba 2009; Gibson, Caldeira, and Baird 1998; Staton, Reenock, and Holsinger 2022). But, what makes citizens more prone to support judicial institutions in the first place?

While extant research has examined how characteristics of both individuals and contexts explain public support for judicial institutions,[1] a fundamental aspect of our knowledge of citizens' evaluation of courts arises from understanding "how specific judicial decisions map onto public opinion" (Helmke 2010*b*, 397).[2] Recognizing that court rulings are a critical source of variation in public support for judicial institutions, a now-consolidated body of work has explored how citizens' regard for courts varies following salient judicial decisions (Mondak 1990, 1991; Grosskopf and Mondak 1998; Bartels and Johnston 2013; Christenson and Glick 2019, 2015*b*,*a*; Nicholson and Hansford 2014; Bartels, Horowitz, and Kramon 2021). Importantly, a subset of this literature underscores the central role of instrumental considerations in public reactions to court decisions: that is, individuals evaluate judicial institutions more positively when court rulings advance ideological or partisan interests (Bartels and Johnston 2013; Christenson and Glick 2015*a*; Bartels, Horowitz, and Kramon 2021),

[1] See, among others, the work of Gibson, Caldeira, and Baird (1998), Gibson and Nelson (2014), Benesh (2006), Bartels and Kramon (2020), Walker (2016), Fix, Randazzo, and Martin (2021), Salzman and Ramsey (2013), Aydın-Çakır and Şekercioğlu (2016), and Garoupa and Magalhães (2021).

[2] Similarly, in their pioneering work, Grosskopf and Mondak (1998) conclude that the United States Supreme Court "treads on dangerous ground in releasing controversial edicts, because to do so may cause erosion of public support" (651).

or when judicial outputs are attributed to co-partisan judges (Nicholson and Hansford 2014).

However, this literature suffers from important limitations. First, these studies examine respondents' reactions to either (1) hypothetical court decisions upholding or invalidating a partisan or ideologically salient policy (Nicholson and Hansford 2014; Bartels and Johnston 2013), or (2) real-world judicial rulings that uphold incumbents' vested interest (Christenson and Glick 2015*a*; Bartels, Horowitz, and Kramon 2021). Second, work exploring the effects court decisions on public opinion focuses on attitudes about specific (usually pinnacle) courts, ignoring any impact on citizens' evaluation of the judiciary as the broader, encompassing political institution at stake. As a consequence, this literature has limited potential to account for real-world judicial checks on incumbents' interests and their effects on public support for the judicial branch at large.

Addressing this question is far from trivial for several reasons. First, across both space and time, courts provide meaningful and consequential checks on government power, even in contexts where their influence is thought to be marginal. For instance, in 2010, the Colombian Constitutional Court invalidated a popular referendum called by President Álvaro Uribe that could have allowed him to run for a third term (Bernal 2013; Kraul and Gonzalez 2010). In Kenya, the Supreme Court annulled the incumbent president's victory in the 2017 elections (Bartels, Horowitz, and Kramon 2021). These anecdotal precedents especially suggest that judicial checks on governments' power are now far from rare even in the developing world (Botero, Brinks, and Gonzalez-Ocantos 2022), where levels of judicial independence are argued to be lower and judicial institutions to be weaker (Levitsky and Murillo 2009). Second, courts challenging incumbents bring about instances of inter-branch conflicts (Helmke 2010*a*), which are consequential for the impact they have on public opinion. This impact can be direct—with individuals reacting to judicial checks along partisan lines (e.g., Nicholson and Hansford 2014; Bartels and Kramon 2020; Bartels, Horowitz, and Kramon 2021). But judicial challenges can be meaningful for public opinion also through indirect means: when

courts strike down salient executive policies, we can expect that government and opposition elites will make efforts to mobilize their constituencies—with government officials criticizing judicial intervention and opposition actors praising such checks.[3] In sum, that these institutional checks might influence public opinion, and that they occur in environments where courts are not necessarily broadly respected, makes understanding these dynamics all the more important.

In this paper, I build upon recent work and test a partisan account of public support for the judiciary. If public support for courts is deeply rooted in individuals' instrumental considerations, as suggested by "outcome-based" approaches (Bartels and Kramon 2020), then courts invalidating salient policies advanced by incumbent government will trigger primarily partisan reactions: we should expect incumbents' co-partisans to withdraw support from the judicial institutions, while out-partisans to be more supportive of courts following judicial reversals of incumbents' policies.

I test these theoretical expectations by studying two politically salient judicial rulings released in Argentina in 2013. These court decisions invalidated a controversial judicial reform enacted by the then Argentinean president, Cristina Fernández. Taking advantage of the fact that the 2013 *Latinobarómetro* survey was being fielded as the courts enacted their rulings, I estimate the effect of these judicial decisions on respondents' trust in the judiciary. The results show that, overall, Argentines interviewed after the rulings express less trust in the judiciary than those interviewed before the court decisions. Most importantly, these

[3]This type of elite behavior is not uncommon or unreasonable. Frequently, incumbents react to politically unpleasant judicial rulings by promoting court-curbing proposals. For example, upon a series of Supreme Court rulings invalidating pieces of the 'New Deal,' US President Franklin D. Roosevelt responded with his infamous court-packing plan (Caldeira 1987). Similarly, Vanberg (2000) documents how West Germany's Chancellor Adenauer publicly threatened disobeying a Constitutional Court's decision against the government and even considered institutional changes to the judicial institution. Even if these historic attempts to curb unsubmissive courts were unsuccessful, more recent research suggests that the electoral connection is a useful tool for incumbents aspiring to leverage support for court-curbing proposals (Driscoll and Nelson 2023; Armaly 2018). This evidence suggests that even in the developed world—where judicial independence is argued to be higher—incumbents attack rebellious courts and resort to the electoral connection to gain leverage on proposals to curb judicial institutions.

negatives effects are consistently significant among respondents who identified as co-partisans with President Fernández. Even though out-partisans also show lower levels of trust after the rulings, those effects are not statistically distinguishable from zero at conventional levels in most specifications. Along with the evidence that government co-partisans are more trustful of courts than out-partisans,[4] these results are noteworthy. As discussed in **Section 7**, these findings suggest that there are costs to independent courts, since checking incumbents might not only diminish public trust among co-partisans—which tend to be *more* supportive group of citizens—but it may also fail to increase support from those publics politically aligned with the opposition.[5]

This paper makes several contributions. First, it provides further empirical support for the recent, but influential, work emphasizing the instrumental foundations of citizens' support for courts (see Bartels and Johnston 2020). While this literature has mostly studied the United States (i.e., Clark and Kastellec 2015; Driscoll and Nelson 2023), my empirical focus on Argentina takes the theoretical expectations of the outcome-based theories to a context that is understudied, but typical of the developing world.[6] Specifically, this paper demonstrates that partisanship is a relevant factor for individuals' evaluation of judicial institutions.

Second, this paper speaks to the larger literature on comparative judicial politics and the separation of powers. While a bulk of past research on Latin America has advanced our knowledge on the conditions under which we expect courts to engage in inter-branch conflicts (Helmke 2002; Iaryczower, Spiller, and Tommasi 2002; Chávez 2004; Couso 2003; Hilbink 2007), fewer studies have asked what is the role of public support in such instances

[4]See Bartels and Kramon (2020) and **Section 7** below.
[5]I thank Sivaram Cheruvu for raising this important point.
[6]The work in Africa by Bartels and colleagues (Bartels and Kramon 2020; Bartels, Horowitz, and Kramon 2021) is a notable exception. Although this paper studies a similar phenomenon to that of Bartels, Horowitz, and Kramon (2021), it differs in that I study judicial decisions that *invalidate* a salient policy advanced by the incumbent in a fully *democratic* context.

(Helmke 2010*a*; Staton 2006; Helmke and Staton 2011; Pereira 2022). Yet, to my knowledge, no prior work on the region has empirically examined whether and how specific instances of inter-power disputes—particularly, judicial checks on the government—can influence public attitudes about judicial institutions. That courts may undermine public trust in the judiciary when challenging the government has crucial implications for our understanding of the role of public support amid inter-branch conflicts as well as for our expectations about strategic behaviors of both courts and elected officials (Krehbiel 2016; Vanberg 2001; Staton 2006).

Finally, I also contribute to the growing literature that studies public reactions to judicial decisions using methods for causal identification (i.e., Grosskopf and Mondak 1998; Christenson and Glick 2015*a*; Bartels and Johnston 2013; Bartels, Horowitz, and Kramon 2021).

This paper proceeds as follows. In the next section, I discuss the related literature and make my theoretical expectations explicit. Section 3 describes the context this paper studies. Section 4 discusses the research design, the identification strategy, and its assumptions. Sections 5 presents the main results, while Section 6 addresses alternative explanations and provides robustness checks. Section 7 discusses the findings' broader implications for existing knowledge on public support for courts. Section 8 concludes by describing the limitations of this work and suggesting avenues for future research.

## 2 Checks, Partisanship, and Instrumental Support for Judicial Institutions

Judicial review of executive action is risky business for many courts around the world. When judges are called to place a check on other branches and expand their province of jurisdiction (Vallinder 1995), they venture into deciding on issues politically sensitive to sitting, powerful officials. Such involvement not only has the potential to expose courts to

inter-branch disputes (Vanberg 2000), but it also makes judicial institutions more noticeable to the public eye (Grosskopf and Mondak 1998; Gibson, Caldeira, and Spence 2003*b*).

If courts invalidate governments' key policies and these instances become public, we can expect public reactions to judicial institutions to vary according to citizens' co-partisanship with the political actors involved. Theoretically, influential formal work has argued that citizens do condition their support for judicial institutions on instrumental considerations (Carrubba 2009; Stephenson 2004; see also Vanberg 2015). Similarly, a growing body of "outcome-" or "policy-based" approaches (Bartels and Johnston 2020) suggests that public support for judicial institutions is deeply influenced by partisan preferences. For instance, Bartels and Kramon's (2020) "partisan alignment theory" argues that citizens want courts to attain partisan political advantages, and that presidential co-partisans are less likely to support courts' power to constrain the executive power. Moreover, research suggests that citizens take into consideration the political preferences of their leaders when evaluating judicial institutions (i.e., Lenz 2012; Armaly 2018).

Empirically, the literature has provided ample evidence for instrumentalist theories of public support for courts.[7] More relevant to this paper, related research has found partisan reactions to specific court decisions. Using a survey experiment, Nicholson and Hansford (2014) show that acceptance of a US Supreme Court decision increases when the ruling is attributed to Justices of respondents' party. Moreover, Bartels, Horowitz, and Kramon's (2021) research shows that support for the Kenyan Supreme Court decreased among supporters of the opposition candidate who was disfavored by the Court's 2017 ruling.

[7]In particular, scholars have shown that partisan cues influence citizens' opinion about courts. Using a survey experiment, Armaly (2018) exposes respondents to presidential candidates' negative statements about the US Supreme Court, and finds that individuals who liked (disliked) a candidate decreased (increased) their perception of the Supreme Court's legitimacy after being exposed to that candidate's criticism of the Court. Similarly, Clark and Kastellec (2015) show that respondents were more likely to support proposals to curb the US Supreme Court when such proposals had been introduced by co-partisans. Sen's (2017) conjoint experiment demonstrates that, when that information is available, respondents rely heavily on partisanship when evaluating and supporting potential Supreme Court nominees.

Given the ample recent evidence supporting the instrumental approach to citizens' support for courts, we have reasons to expect individuals will react along partisan lines when courts present public challenges to executives' salient policies. Whereas government co-partisans will display decreases in support, government out-partisans will be more supportive of the judicial institution that placed a check on the incumbent government.

**Hypothesis 1 (H1):** *Following court decisions invalidating an incumbent executive's policy, governments' co-partisans will decrease their support for the judiciary.*

**Hypothesis 2 (H2):** *Following court decisions invalidating an incumbent executive's policy, governments' out-partisans will increase their support for the judiciary.*

## 3 Context: The 2013 Reform to the Argentinean Judicial Council

During the opening of the legislative year in March of 2013, Argentina's President Cristina Fernández announced a series of judicial reform bills to be sent to the Congress—reforms whose overall goal was to "democratize the judiciary," but that most opposition groups saw as an attempt to undermine judicial independence and pack the federal judiciary (Llanos 2014; Elias 2015). The most controversial reform involved substantial modifications to the Argentinean (national) Judicial Council, an inter-branch constitutional body that has played a crucial role in the selection and removal of federal lower court judges since 1998.[8] The Judicial Council is composed of members that represent the Congress and the Executive, as well as federal judges, attorneys, and the academic community throughout the country. Although the Argentinean 1994 Constitution established *which* groups are to be represented in the Judicial Council, a law by Congress determines the *exact* number of members and *how* those members are chosen to serve in the Council.[9] Until the reform passed in 2013, the

[8] For a more detailed account of the origins and functions of Argentina's Judicial Council, see Chávez (2007), Elias (2015), and Walsh (2020).

[9] Argentina's Constitution explicitly delegated this task to Congress. Specifically, Article 114 of the National Constitution states: "The Judicial Council, *regulated by a special law passed by an absolute majority of the*

Judicial Council had thirteen members.[10] While the Executive and Congress selected their members to the Council, the representatives of judges, attorneys, and academics were chosen through elections in which only their peers (that is, other judges, attorneys, and academics, respectively) were allowed to vote.

On May 8th, 2013, the national Congress passed Fernández's bill to reform the Judicial Council, increasing the number of representatives of attorneys (from two to three) and academics (from one to six). Moreover, the law determined that the representatives of judges, attorneys, and academics would be selected through popular elections—and not chosen by their peers exclusively, as in the previous legal framework. Also, the reform required that candidates for these seats be affiliated to a political party in order to run for election. After the Judicial Council reform was passed by the Congress, the President issued an executive order which called for elections of representatives of judges, attorneys, and academics to be carried out during the mid-term primary legislative elections in August, 2013. **Table 1** summarizes the relevant changes to the Judicial Council introduced by the 2013 reform.

*totality of the members of each Chamber*, shall be charged with selecting judges and with the administration of the Judiciary. The Council shall be reconstituted periodically so that an equilibrium is achieved among the representation of popularly elected political organs, judges of all instances, and federally licensed attorneys. It shall also include persons from the academic and scientific fields, *of a number and manner of appointment the law shall indicate*."

[10]Specifically, there were six legislators (three Deputies and three Senators), one representative of the Executive, three federal judges, two attorneys representing licensed law practitioners throughout the country, and one academic representing the scientific community.

Table 1: Government's Proposed Reform to the Judicial Council (2013)

| Representative | Seats | | Appointed by | |
|---|---|---|---|---|
| | Pre-reform | Reform | Pre-reform | Reform |
| **Judges** | 3 | 3 | Federal Judges | |
| **Attorneys** | 2 | **3** | Licensed Attorneys | **Popular Election** |
| **Academics** | 1 | **6** | Law Professors | |
| **Deputies** | 3 | 3 | House of Deputies | |
| **Senators** | 3 | 3 | Senate | |
| **Executive** | 1 | 1 | President | |
| Total | 13 | **19** | | |

*Note:* [Caption. Explain cells in **bold**]

The reform to the Judicial Council was extremely salient[11] and triggered criticism from political actors opposed to the government. Moreover, the reform also brought about legal challenges. In particular, a 'list' (e.g., party) of candidates running for attorney seats in the Judicial Council filed a lawsuit against the national government. The lawsuit questioned the constitutional validity of the judicial reform and requested the suspension of the elections for representatives of judges, attorneys, and academics.[12] On June 11$^{th}$, a lower district court judge ruled that the reform to the Judicial Council was in conflict with Argentina's Constitution. The judge also invalidated the call for elections concerning the representatives of judges, attorneys, and academics that the President had issued via executive order. In response, the government filed a direct appeal to the Argentinian Supreme Court, who released its decision on June 18$^{th}$.[13] The Supreme Court's ruling upheld the lower judge's opinion, therefore striking down the Judicial Council reform and suspending the call for

[11] See, for example, **??** (discussed below).

[12] The legal file of the case is "Rizzo, Jorge Gabriel (apoderado Lista 3 Gente de Derecho) s/ acción de amparo c/ Poder Ejecutivo Nacional, ley 26.855, medida cautelar (Expte. N° 3034/13)."

[13] Formally, this Supreme Court decision is cited as "Corte Suprema de Justicia de la Nación, *Fallos* 336:760." The full text of the ruling is available at www.sjconsulta.csjn.gov.ar/sjconsulta/documentos/verDocumentoByIdLinksJSP.html?idDocumento=7026851&cache=1672847525778. See also the report from the Supreme Court's Center of Judicial Information (CIJ) at www.cij.gov.ar/nota-11694-La-Corte-declar--inconstitucional-cambios-en-el-Consejo-de-la-Magistratura.html.

elections for Judicial Council representatives. These series of court rulings became known as the *Rizzo* decisions, given the plaintiff's last name.

These two judicial decisions had widespread media coverage. The three main newspapers featured both the lower court and Supreme Court decisions on their front pages (see **Appendix XX**). Also, the rulings were largely criticized by government officials at the same time that opposition leaders praised the rulings. For instance, the then Chief of Staff characterized the Supreme Court ruling as an "affront to the Argentinean people," considering "inconceivable that, after 30 years of our democracy, some institutions still maintain a retrograde vision, typical of other centuries" (Télam 2013*a*; Rebossio 2013).[14] In contrast, the House leader of the main opposition party asserted that "the Supreme Court acted with honesty and judgmental independence, and it invalidated the [government's] attempt to subjugate the Judiciary" (Clarín 2013).

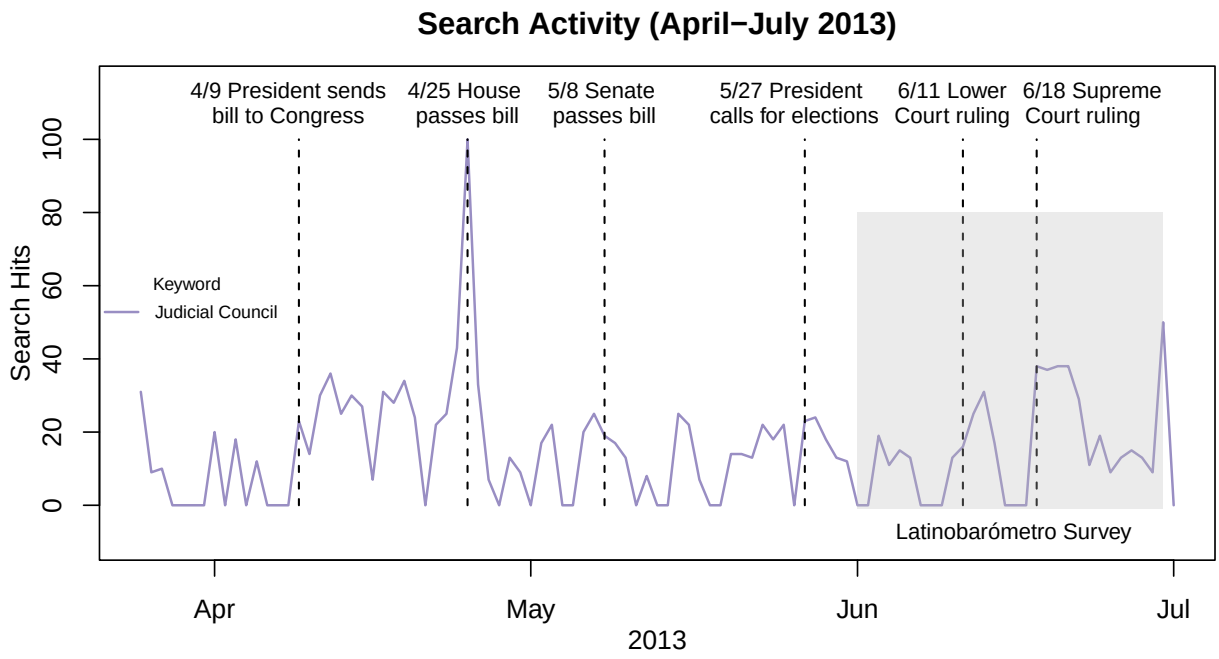
Furthermore, both the lower court and Supreme Court decisions were highly salient to the larger citizenry. **Figure 1** below plots Google Trends data on search activity in Argentina for the term "Judicial Council" (*Consejo de la Magistratura*) from April to July, 2013, period that includes the Latinobarómetro survey (shaded area) discussed below.[15] The figure shows, first, that interest over the Judicial Council increased as it became politically salient. Importantly, the data also show that, in June, search activity on the Judicial Council was largest right after the lower and Supreme Court rulings were released (June 11$^{\text{th}}$ and June 18$^{\text{th}}$, respectively). In sum, **Figure 1** demonstrates that Argentines were responsive

[14]Even President Fernández implicitly criticized the Supreme Court while giving a speech in a public event that took place two days after the ruling: she claimed that "rather sooner than later, Argentines will be able to vote democratically for *all* the political bodies of the Argentine Constitution" (Notarfrancesco 2013).

[15]To estimate search interest, Google Trends employs a random (unbiased) sample of the full data on Google searches, which consist of billions of searches per day. Essentially, Google Trends includes a largely unfiltered sample of actual search requests made to Google. The search data are normalized to the time and location of a query, so that such query returns the "search interest for that topic in a given region as a proportion of all searches on all topics on Google in that same place and time" (Rogers 2016). The data are scaled to 0–100, being 100 the maximum search interest for the time and location selected. Besides the Google News Lab post on Google Trends (Rogers 2016), see also Google Trends FAQs at `https://support.google.com/trends/answer/4365533?hl=en&dark=1`.

to the political events surrounding the reform to the Judicial Council—including the court rulings in June—and provides evidence that the judicial decisions were salient to the general public.

Figure 1: Google Trends Search Activity




*Note:* The panels use Google Trends data to plot search interest over the term "Judicial Council" (*Consejo de la Magistratura*). See **fn.15** (page **10**) for more information on how Google Trends collects and measures search activity.

## 4 Research Design

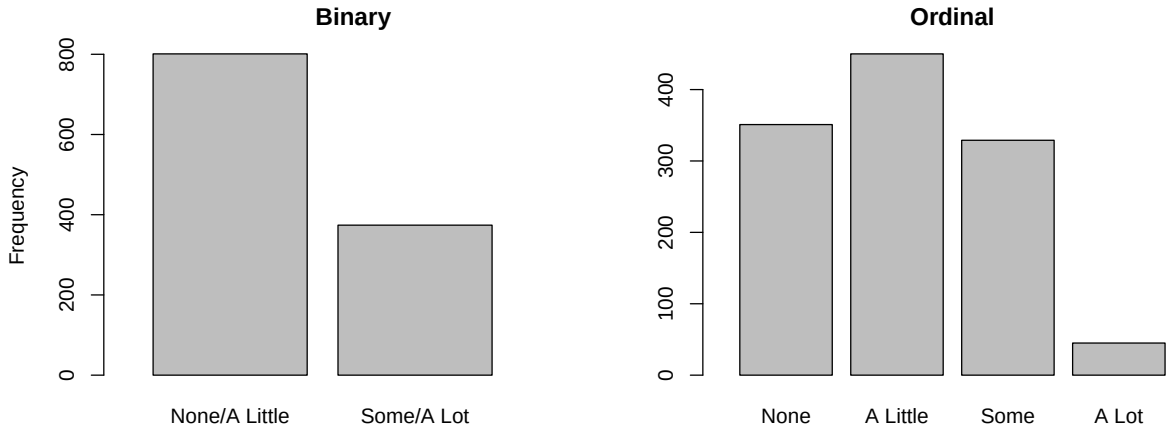
### The 2013 *Latinobarómetro* Survey in Argentina

While these political events were taking place in Argentina, the *Latinobarómetro Corporation* was interviewing individuals for their 2013 nationally representative survey.[16] The survey includes a four-point scale that asks respondents how much trust they have in the

[16]Corporación Latinobarómetro, Santiago, Chile, www.latinobarometro.org.

Judiciary:[17] "a lot," "some," "a little," or "no" trust. **Figure 2** shows the distribution of the *Trust in the Judiciary* outcome variable. For simplicity, in the results presented below I employ the binary version, where 0 means a a respondent had a little or no trust in the judiciary, and 1 reflects a lot or some trust (mean $\approx 0.32$, sd $\approx 0.47$).[18]

Figure 2: Trust in the Judiciary

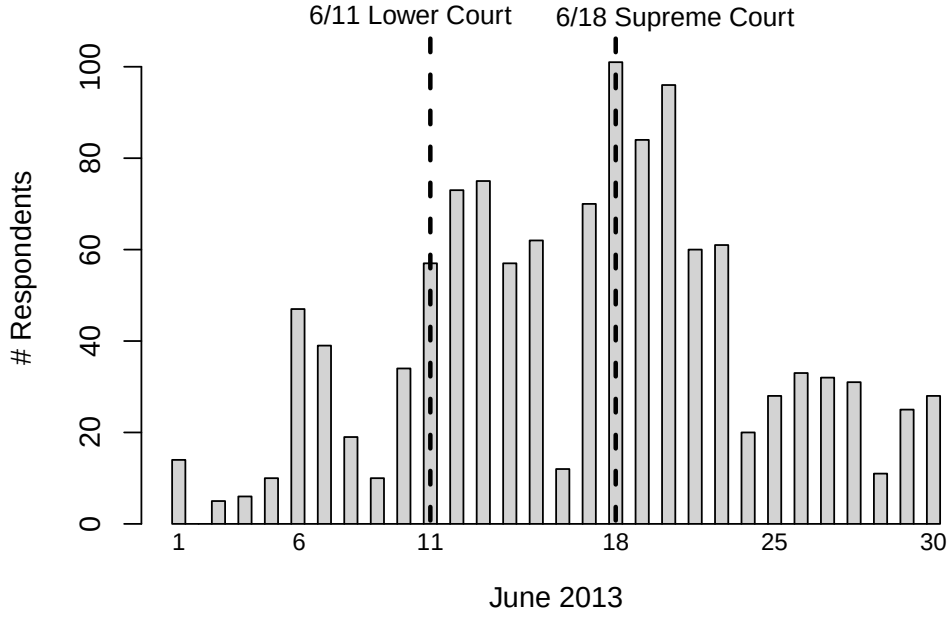



The 2013 Latinobarómetro survey in Argentina was fielded from June 1st to June 30th, and it interviewed a total of 1,200 respondents. **Figure 3** displays the distribution of Argentinean respondents throughout June 2013 as well as the dates the lower and supreme court rulings were released.

[17]The Latinobarómetro survey item I use as the dependent variable is "trust" in the judiciary. A rich literature in judicial politics has examined the extent to which confidence or trust items are a valid measure of public support. In particular, scholars showed that trust survey items tap into Easton's (1965; 1975) classic concept of 'specific' (rather than 'diffuse') support for judicial institutions. In fact, research demonstrated that the trust item "reflects a blend of short-term and long-term judgments of the institution," rather than a more enduring loyalty toward the institution itself" (Gibson, Caldeira, and Spence 2003*a*, 364; see also Driscoll and Nelson 2018 and Gibson and Caldeira 2009). In this sense, it has been argued that the trust item is a function of perceptions and evaluations of specific court opinions (Gibson, Caldeira, and Spence 2003*b*). While I acknowledge the relevance of this debate and the limitations of the trust outcome as a measure of specific support for the judiciary, it is still worthwhile to investigate whether court rulings can influence citizens' reported trust in the judiciary. As Benesh (2006, 701fn.11) suggests, even using the trust measure we can answer interesting questions. Moreover, the Latinobarómetro item on trust or confidence in the Judiciary has been widely used in the literature (see, for example, Walker 2009 and Helmke 2010*b*).

[18]The results hold when employing the original ordinal measure or a (scaled) continuous version of this outcome. See **Appendix XX**.

Figure 3: Distribution of Respondents during the 2013 Latinobarómetro Survey




Taking advantage of the fact that the rulings were released while Latinobarómetro was in the field, I can divide the sample of respondents into three 'quasi-experimental' groups: a 'control' condition and two 'treatment' conditions, as shown in **Table 2**.[19] Under certain assumptions, this context offers an unusual opportunity to estimate the effects of these court decisions on Latinobarómetro respondents' trust in the judiciary.

Table 2: Quasi-Experimental Conditions

| Condition | Survey Interval | Ruling Date | N |
|---|---|---|---|
| **Control** | 6/1 to 6/11 | - | 241 |
| **Lower Court** | 6/12 to 6/18 | 6/11 (night) | 450 |
| **Supreme Court** | 6/19 to 6/30 | 6/18 (evening) | 509 |

This research design faces two main concerns that threat causal identification. First, the Latinobarómetro survey could have been administered in a way that citizens more or

[19]Importantly, the results are robust to removing the individuals interviewed on June 11$^{th}$ and/or June 18$^{th}$. See **Appendix XX**.

less prone to support the judiciary were systematically interviewed before or after the court decisions, making treated and control units fundamentally different.[20] For instance, since the survey is nationally representative and it was not conducted simultaneously around the country, it is plausible that demographically dissimilar provinces, cities, or neighborhoods were surveyed earlier or later in the process. Indeed, although most provinces and cities have respondents in either the control or treatment groups, there are still a number of regions that were interviewed only after the lower or supreme court rulings.[21] To make credible claims, however, this research design requires that Latinobarómetro respondents interviewed before the rulings are comparable to those interviewed following the rulings—otherwise, changes in trust in the judiciary could be an artifact of differences in relevant covariates, rather than the court rulings' effects.

To check for balance across groups, I use pre-treatment survey items asked in the 2013 Latinobarómetro poll.[22] **Table 3** displays mean values, standard deviations, and exact $p$-values (see Young 2019) using randomization inference tests. The table shows that that there is imbalance between control and treatment groups with respect to individuals' education (more educated in treatment groups), income (wealthier in the treatment groups), class (higher in the *Lower Court* group), presidential co-partisanship (fewer co-partisans in the *Supreme Court* group), and political knowledge (more knowledgeable in the treatment groups). Such imbalance is potentially problematic because research has shown that political knowledge and sophistication, education, awareness, and partisanship are associated with support for courts.[23] To deal with this concern, the analyses presented below employ balance

[20] Alternatively, the timing of the decisions could have been driven by judges' attempt to maximize its support among those interviewed by the survey. If that were the case, treatment assignment would not be random, but driven by the strategic calculation of the courts. However, first, it is unlikely that the (lower and Supreme Court) judges had known about the 2013 Latinobarómetro survey at all, let alone the schedule of the survey administration. Second, the timing of the *Rizzo* decisions was not in full control of the judges, as the legal processes also depended on the actions of plaintiffs and defendants (i.e., filing of the lawsuit, appeals, etc.)

[21] See **Appendix Tables XX**.

[22] See Appendix A2 for more information on these survey items. [**note XX *co-partisanship* survey item and discussion about concerns over post-treatment bias.**]

[23] See, for example, Gibson, Caldeira, and Baird (1998), Gibson and Nelson (2015), Fix, Randazzo, and

weights (Hainmueller 2012).[24] This strategy not only helps mitigate model dependency but it also ensures that the results are not driven by differences in *observable* covariates.[25] By employing entropy balancing, I am able to create exactly balanced samples (on observable covariates and on average) between the control and treatment conditions.

Table 3: Covariate Balance Across Conditions

| | Control | | Lower C. | | Supreme C. | | (exact) $p$-value | |
|---|---|---|---|---|---|---|---|---|
| | Mean | sd | Mean | sd | Mean | sd | Lower vs Control | Supreme vs Control |
| Female | 0.548 | 0.499 | 0.524 | 0.5 | 0.495 | 0.5 | 0.561 | 0.177 |
| Age | 42.751 | 16.842 | 43.56 | 18.111 | 43.432 | 17.153 | 0.571 | 0.608 |
| Education | 10.631 | 3.101 | 11.476 | 2.94 | 11.293 | 3.072 | 0.001 | 0.005 |
| Income | 0.512 | 0.242 | 0.578 | 0.221 | 0.588 | 0.229 | 0.000 | 0.000 |
| Class | 0.353 | 0.197 | 0.386 | 0.168 | 0.359 | 0.186 | 0.016 | 0.665 |
| Pres. Co-Partisan | 0.336 | 0.473 | 0.302 | 0.46 | 0.261 | 0.44 | 0.356 | 0.034 |
| Internet Use | 0.537 | 0.5 | 0.589 | 0.493 | 0.579 | 0.494 | 0.202 | 0.285 |
| Pol. Knowledge | 0.277 | 0.295 | 0.4 | 0.302 | 0.342 | 0.297 | 0.000 | 0.006 |
| N | 241 | | 450 | | 509 | | | |

*Note:* The table shows means and exact $p$-values (Young 2019) using randomization inference tests with 10,000 random samples.

Second, besides the *Rizzo* decisions, there could have existed concurrent events during the administration of the Latinobarómetro survey that influenced respondents' attitudes towards the judiciary. One specific event is the Judicial council reform itself, which was remarkably salient. However, the introduction and approval of the reform is controlled by design: the reform was passed before any Latinobarómetro respondent was interviewed—that is, all individuals were exposure to the judicial reform itself. Another concern is that, before

Martin (2021), Benesh (2006), Salzman and Ramsey (2013), Aydın-Çakır and Şekercioğlu (2016), Garoupa and Magalhães (2021), Driscoll and Nelson (2023), and Bartels and Johnston (2013).

[24]Entropy balancing is a data preprocessing method that involves a "reweighting scheme that directly incorporates covariate balance into the weight function that is applied to the sample units" (Hainmueller 2012, 26).

[25]Below in **section XX** I conduct placebo tests that address the possibility that *unobservables* are confounding my findings.

the June 11th decision, other judges around the country had issued rulings either striking down or upholding the judicial reform.[26] Nonetheless, those specific decisions did not have widespread media coverage (especially relative to the June 11th and June 18th rulings),[27] making it thus very unlikely that respondents were strongly exposed to judicial decisions other than the *Rizzo* rulings.[28]

In sum, if my strategy to address these identification concerns is sound, this research design has the potential to make a significant contribution to our knowledge on public support for judicial institutions and inter-power conflicts. While in studying the effects of salient judicial rulings I follow previous work,[29] my research design has the advantage of picking up "true" attitudes about the judiciary without the need to prime respondents about the court decisions—potentially introducing bias in respondents' evaluation of the judiciary (Hitt, Saunders, and Scott 2019, 37, Grosskopf and Mondak 1998, 650).[30] Moreover, by exploiting politically relevant, real-world judicial rulings, not only this design leverages greater external validity compared to laboratory experiments that expose individuals to hypothetical court decisions (i.e., Mondak 1991, 1990; Bartels and Johnston 2013), but—critically—this methodology is well-suited to address theoretical expectations related to instrumental sup-

[26]Some of these rulings were enacted before the Latinobarómetro survey (Télam 2013*b*), while other five lower-court rulings were issued between June 4th and June 7th (Hauser 2013).

[27]For instance, those June rulings were mentioned only in a single article of newspaper with national coverage (Hauser 2013).

[28]Even if respondents were indeed exposed to those other rulings, this would bias against the effect of the decisions under study—in other words, we would not see significant differences in public trust in the judiciary following the *Rizzo* rulings.

[29]For instance, past research has studied the impact of actual, salient US Supreme Court rulings on public attitudes about both judicial institutions (Christenson and Glick 2019; Hitt, Saunders, and Scott 2019; Grosskopf and Mondak 1998; Gibson, Caldeira, and Spence 2003*b*) and policy issues (Christenson and Glick 2015*b*; Hoekstra 1995; Hoekstra and Segal 1996). The work of Christenson and Glick (2015*a*) also centers around a politically salient US Supreme Court decision (e.g., the constitutionality of the Affordable Care Act [ACA]). However, it studies the effects of a treatment vignette experimentally assigned by the researchers, rather than the direct impact of the court decision itself.

[30]Even though I am not able to determine whether the Latinobarómetro respondents had the rulings in mind when being asked about their trust in the judiciary, **Figure 1** above provides evidence that the Judicial Council reform was salient during the survey and especially so after the court decisions. It is very plausible, then, that 'treated' respondents were aware of the judicial rulings at the time of the Latinobarómetro interview.

port for judicial institutions engaged in publicly salient political contentions.[31]

## 5 Results

In this section, I present the main findings. I provide evidence that, across the two conditions and a wide range of model specifications, the rulings have a negative effect on public trust in the judiciary. Nevertheless, these effects are only consistently significant for co-partisans, and the differences in marginal effects between out- and co-partisans are only consistently significant between the control and the *Supreme Court* condition.

Together, these results indicate that the rulings decreased public support for the judiciary among co-partisans, especially following the Supreme Court decision. However, contrary to my expectations, the judicial decisions did not make out-partisans significantly more supportive of the judiciary. Overall, these findings provide support for **H1**, but run against **H2**. Later on, **Section 7** discusses what these results imply for public support for courts and inter-branch conflicts in the developing world.

### Partisanship and Trust in the Judiciary

**Table 4** presents linear models of the effects of the court rulings on trust in the judiciary.[32] All models include province fixed effects and all specifications compute bootstrapped standard errors clustered by province to account for potential correlation within the small number of clusters (see Cameron, Gelbach, and Miller 2008; Cameron and Miller 2015).[33]

[31] This paper's research design is most similar to Bartels, Horowitz, and Kramon (2021), who examine the effect of Kenya's Supreme Court rulings that upheld the results of the 2017 presidential elections allowing the incumbent's reelection. Yet Bartels, Horowitz, and Kramon (2021) employ panel data, whereas my design is limited by the cross-sectional nature of the Latinobarómetro data and therefore estimates between-subject change.

[32] Results do not change when employing a probit model for this binary outcome, the original ordinal outcome (probit), or a continuous version of the outcome (OLS). See **Appendix XX**.

[33] That is, due to how the survey was implemented, some provinces have respondents in each treatment group (control, lower court, and supreme court), other provinces have respondents in two treatment groups, and some provinces only have respondents in one treatment group. A total of 14 (out of 25) Argentinean

The first four columns use the unbalanced sample. Model 1 displays the bivariate relationship between the rulings and the outcome. Models 2 and 3 include the co-partisanship indicator (1 for presidential co-partisans and 0 otherwise) as well as the remaining controls. Model 4 includes interactions of the treatment indicators with all the covariates, relaxing the assumption of constant treatment effects and thus correcting for model misspecification (Schafer and Kang 2008).[34] The last six columns show the effects of the lower court (models 5–7) and supreme court decisions (models 8–10) using the entropy-balanced samples.[35] Models 5 and 8 estimate the bivariate association, models 6 and 9 include the co-partisanship indicator interaction, and models 7 and 10 include full covariate adjustment.[36]

provinces (including the Autonomous City of Buenos Aires) were surveyed in the 2013 Latinobarómetro. See **Appendix A**.

[34]Full regression results are displayed in Appendix **Table B1**.

[35]Specifically, the entropy balance weights for these models are calculated by balancing on the first moment of the following variables: *Presidential Co-partisan*, *Education*, *Female*, *Age*, *Income*, *Class*, *Internet Use*, *Political Knowledge*.

[36]Full regression results are displayed in Appendix **Table B2**.

Table 4: OLS Results - Unbalanced and Balanced Samples

| | *Dependent variable:* Confidence in the Judiciary (Binary) | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Unbalanced Sample | | | | Entropy Balancing | | | | | |
| | | | | | Lower Court | | | Supreme Court | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Lower C. Ruling | −0.093* | −0.060 | −0.079+ | −0.174 | −0.157*** | −0.076+ | −0.068 | | | |
| | (0.040) | (0.051) | (0.045) | (0.252) | (0.042) | (0.043) | (0.061) | | | |
| Supreme C. Ruling | −0.129*** | −0.071 | −0.084 | −0.091 | | | | −0.192** | −0.086+ | −0.078+ |
| | (0.034) | (0.072) | (0.065) | (0.241) | | | | (0.060) | (0.044) | (0.044) |
| Co-partisan | | 0.331* | 0.336*** | 0.335** | | 0.333** | 0.333** | | 0.374*** | 0.361*** |
| | | (0.139) | (0.096) | (0.124) | | (0.109) | (0.114) | | (0.056) | (0.056) |
| Lower × Co-partisan | | −0.223 | −0.222* | −0.206+ | | −0.217* | −0.221+ | | | |
| | | (0.137) | (0.098) | (0.124) | | (0.109) | (0.117) | | | |
| Supreme × Co-partisan | | −0.270+ | −0.274** | −0.266+ | | | | | −0.311*** | −0.305*** |
| | | (0.152) | (0.106) | (0.136) | | | | | (0.079) | (0.079) |
| Constant | 0.408*** | 0.253*** | 0.121 | 0.169 | 0.332*** | 0.247*** | 0.033 | 0.327*** | 0.233*** | 0.017 |
| | (0.032) | (0.070) | (0.083) | (0.201) | (0.051) | (0.062) | (0.117) | (0.063) | (0.054) | (0.123) |
| Mean(*Confidence*) | 0.318 | 0.318 | 0.321 | 0.321 | 0.347 | 0.347 | 0.347 | 0.326 | 0.326 | 0.326 |
| SD(*Confidence*) | 0.466 | 0.466 | 0.467 | 0.467 | 0.476 | 0.476 | 0.476 | 0.469 | 0.469 | 0.469 |
| Controls? | | | ✓ | ✓ | | | ✓ | | | ✓ |
| Province FE? | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Full Interactions? | | | | ✓ | | | | | | |
| Observations | 1,175 | 1,175 | 1,147 | 1,147 | 668 | 668 | 668 | 712 | 712 | 712 |
| $R^2$ | 0.011 | 0.067 | 0.077 | 0.087 | 0.058 | 0.110 | 0.121 | 0.054 | 0.108 | 0.125 |
| Adjusted $R^2$ | 0.009 | 0.053 | 0.057 | 0.055 | 0.041 | 0.091 | 0.092 | 0.035 | 0.087 | 0.096 |

*Note:* Bootstrapped standard errors clustered by province. Outcome: Confidence in the Judiciary (1 = "A lot"/"Some"; 0 = "A little"/"None"). Controls: *Education*, *Female*, *Age*, *Income*, *Class*, *Internet Use*, *Political Knowledge*. Models 5–10 employ entropy balance weights (Hainmueller 2012).
+p<0.1; * p<0.05; ** p<0.01; *** p<0.001

**H1** predicted the court rulings to have a negative impact among those respondents who are co-partisans of the president and a positive effect among out-partisans, as the judicial decisions invalidated a key policy enacted by the government. Except for model 2, all the coefficients on the interactions between the court rulings and the co-partisanship indicator are negative and statistically significant at least at the 10% level.

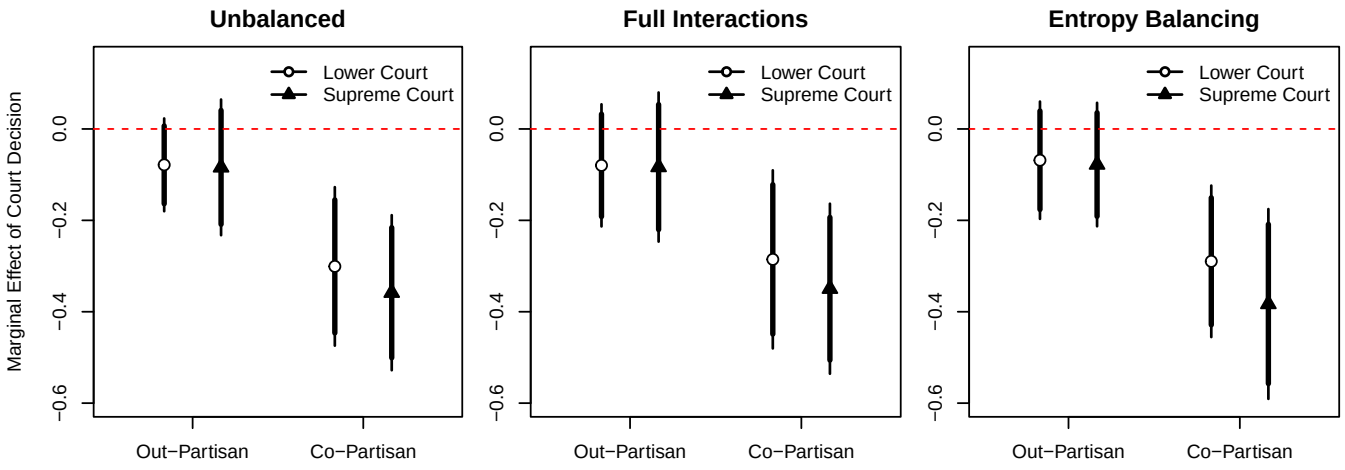
To better understand the coefficients on the interaction terms, **Figure 4** plots the expected changes in trust in the judiciary following the court decisions across presidential co- and out-partisans, along with 90% and 95% confidence intervals. The figure is based on the four interactive models presented in **Table 4**: the left panel plots the unbalanced sample with controls and fixed effects (model 3), the middle panel plots the full interaction model (model 4), and the right panel shows the marginal effects of the (covariate-adjusted) entropy

balancing models for the *Lower Court* (models 7) and *Supreme Court* (model 10) conditions.

Substantively, among *co-partisans*, the *Lower Court* ruling (white circle) had an average marginal effect of about $-0.30$ ($p < 0.01$) across all models. These effects are of considerable magnitude: they are roughly equal to the mean value of the outcome (see **Table 4**) and translate into a decrease of 0.31 standard deviations in the outcome. The marginal effects among out-partisans are consistently negative, however, these estimates are not statistically distinguishable from zero at the 10% level. The differences in these marginal effects between co- and out-partisans are significant at the 5% (left panel) and 10% (middle and right panels) levels (see models 3, 4, and 7 in **Table 4**).

The *Supreme Court* decision (solid triangle) had larger effects. The average marginal effects are between $-0.35$ and $-0.38$ ($p < 0.001$), which in all cases are larger than the mean. These magnitudes translate into decreases of 0.37-0.40 standard deviations in trust in the judiciary. Again, among out-partisans, the effects are imprecisely estimated and indistinct from zero. Importantly, the difference in slopes between co- and out-partisans in the *Supreme Court* condition are significant at conventional levels (left panel: $p < 0.01$, middle: $p < 0.1$, right: $p < 0.001$; see models 3, 4, and 10 in **Table 4**).

Figure 4: Marginal Effects of the Rulings Conditioned by Co-Partisanship



These results provide support for **H1**, showing that the effect of the rulings is negative and significant among those respondents politically aligned with President Fernández. However, I do not find support for **H2**, which expects the rulings to increase public trust in the judiciary among out-partisans. In contrast, **Table 4** shows a negative, although insignificant, effect of the rulings on out-partisans' trust in the judiciary.

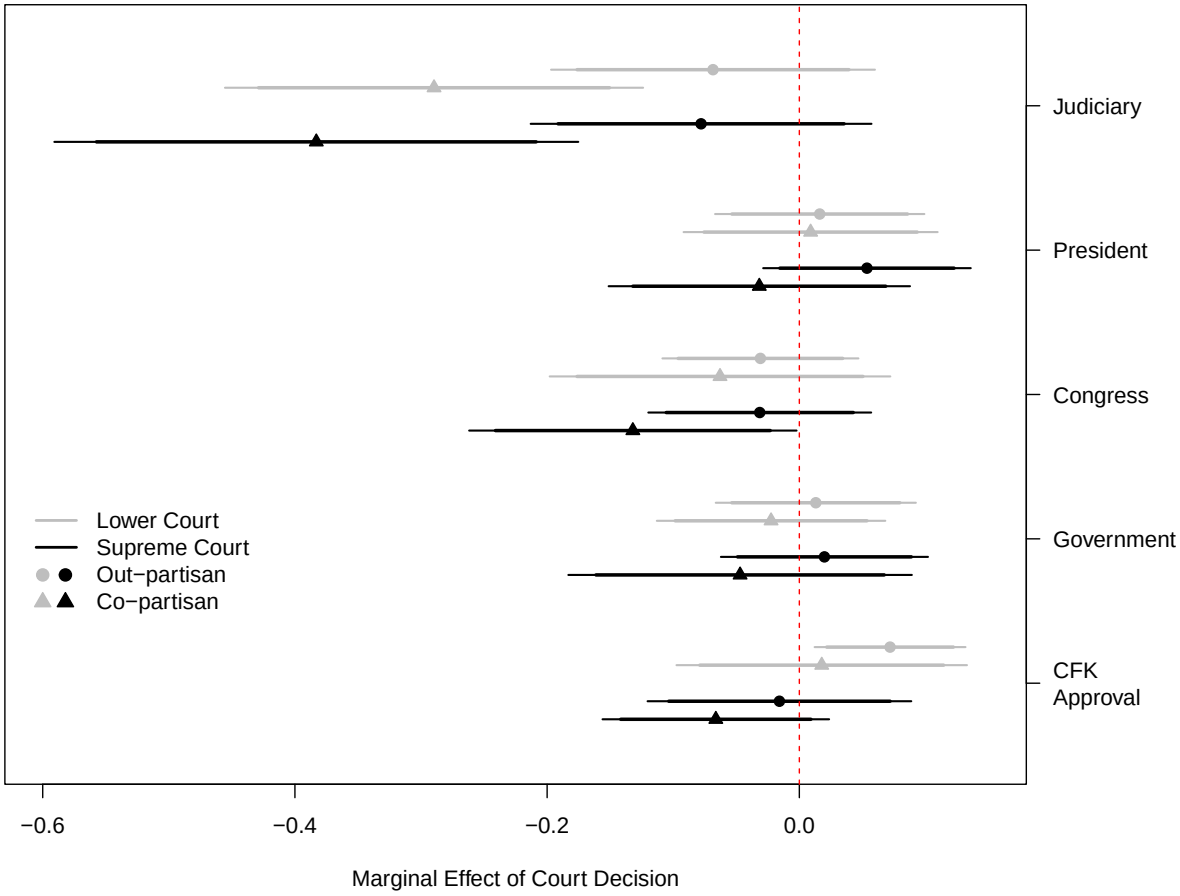
## 6 Robustness Checks

The results shown above support the argument that the court rulings negatively affected public trust in the judiciary among co-partisans. However, existing research shows that individuals may not discriminate among political institutions, so that measures of support for one institution could well reflect attitudes towards other institutions or the whole system (Aydın-Çakır and Şekercioğlu 2016; Grosskopf and Mondak 1998; Salzman and Ramsey 2013). Such a scenario would weaken the credibility of the results described above, as we would not know whether the changes in trust towards the judiciary are instead being driven by attitudes towards other institutions. Put differently, even if the court rulings have a significant effect on trust in the judiciary, it could be possible that the results are picking up respondents' general assessment of political institutions (and, by implication, 'non-attitudes' towards the judiciary), rather than respondents' meaningful evaluations of the judicial branch.

To examine this alternative, I run placebo regressions in which the outcome is trust in different institutions (the 'President,' 'Congress,' 'government') as well as approval for the President's performance ('CFK Approval'). These placebo outcomes are binary, where 1 denotes a lot or some trust, and 0 means a little or no trust for each institution. For the *CFK Approval* outcome, I employ the original binary measure.[37]

[37]The English version of the survey questionnaire reads, 'Do you approve or not of the performance ofthe government led by President [name]?' 1 = Approve, 0 = Disapprove. Only 65 respondents did not answer.

Figure 5




As with the main results presented above, I fit linear models that regress the binary trust (and approval) variables on each treatment indicator (*Lower Court* vs. *Control*, and *Supreme Court* vs. *Control*) using entropy balance weights.[38] **Figure 5** plots the marginal effects as well as 90% and 95% confidence intervals of the interaction between the co-partisanship and the treatment indicators for each condition and for each outcome.

Overall, **Figure 5** provides assurance that the rulings' effects are not driven by respondents' trust in other institutions. Examining the figure more carefully, most coefficients have the a negative sign, which would suggest that the court rulings could have decreased trust

[38]These models employ controls, province fixed effects, and province-clustered bootstrapped standard errors. The models are thus analogous to those presented in columns 7 and 10 of **Table 4**.

in other institutions as well. However, other than trust in the judiciary, almost all of the trust intervals of the marginal effects include zero. The only exceptions are the Congress (marginal effect of the *Supreme Court* condition among co-partisans, $p = 0.046$) and approval of the president (marginal effect of the *Lower Court* condition among out-partisans, $p < 0.018$). Importantly, trust in the President and Government do not seem to have been significantly affected by the judicial decisions among any respondents. Moreover, relative to other outcomes, the treatment effects for both rulings are the largest when respondent were asked to express their trust in the judiciary.

## 7 Further Implications for Courts Limiting Incumbents' Power

Bartels and Kramon (2020) provide evidence that government co-partisans are more trustful of judicial institutions than out-partisans. Previous Latinobarómetero data in Argentina is consistent with that previous research. **Figure 6** below plots the mean levels of trust in the judiciary (binary) by co-partisanship using surveys from 2008 to 2011, period during which Fernández was President.[39] The figure demonstrates that Fernández's co-partisans were indeed significantly more supportive of the judiciary than out-partisans. Although the same pattern is found in the results discussed in the previous section, **Table 4** also shows that the rulings affected the influence of co-partisanship on trust in the judiciary in meaningful ways. How do judicial checks affect the 'presidential co-partisan' advantage judicial institutions enjoy in developing contexts?

[39]Unfortunately, Latinobarómetero was not fielded in 2012.

Figure 6: Trust in the Judiciary by Co-Partisanship (2008–2011)

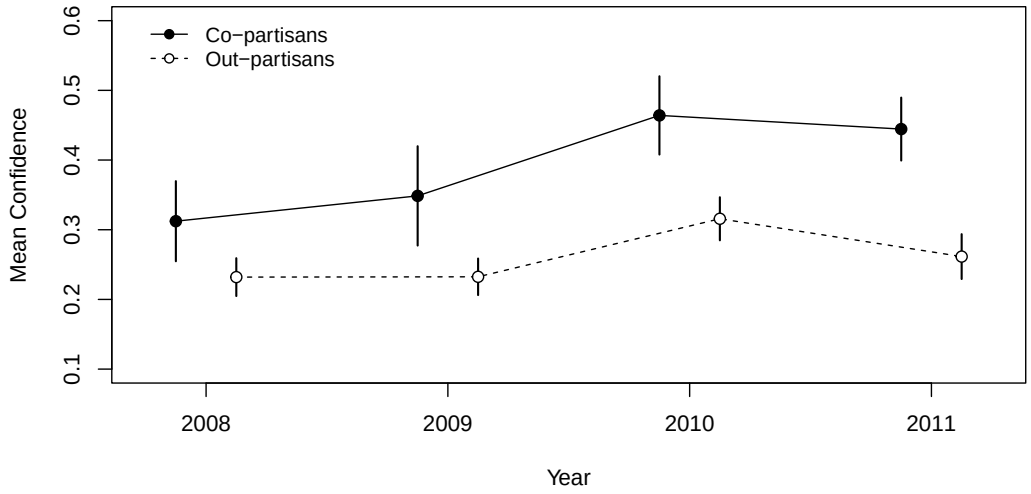
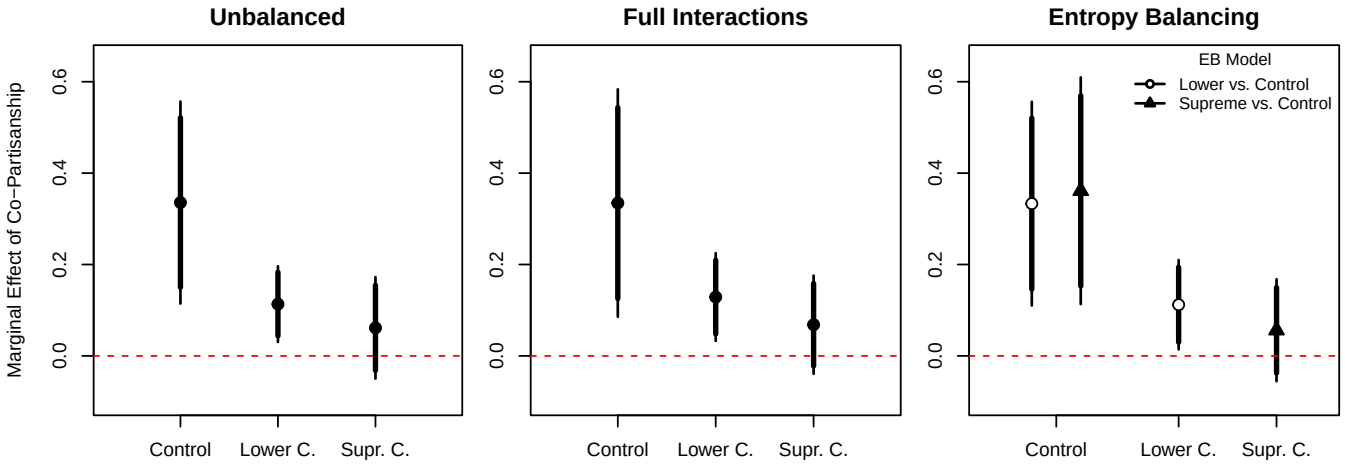



**Figure 7** plots the effects of partisanship on trust in the judiciary conditioned by the court rulings, along with 90% and 95% confidence intervals. That is, the figure shows the effect of moving from an out-partisan to a co-partisan respondent, as the treatments vary from the control (no court decision) to the *Lower Court* and *Supreme Court* decisions. The figure indicates that, as the treatments vary, the marginal effect of co-partisanship decreases or is statistically indistinguishable from zero. For instance, before the rulings, a change from out-partisan to co-partisan leads to an increase of about 0.34 (or a third of a standard deviation) in the outcome, across all models ($p < 0.01$). That is, among respondents interviewed before the rulings were released, co-partisans were significantly more supportive of the judiciary. However, following the lower court ruling, the marginal effect of co-partisanship leads to an average decrease in the outcome of around 0.11 (or 0.11 standard deviations)—this means a drop of a 30% in the difference in support between co- and out-partisans relative to the control group. Furthermore, following the Supreme Court decision, the effects of co-partisanship become statistically zero at beyond the 10% level across all models.[40] This last piece of evidence suggests that co-partisans were no longer

[40]Specifically, $p = 0.28$ (unbalanced model, left panel), $p = 0.21$ (full interactions model, middle panel), and

more supportive of the judiciary than out-partisans once the Supreme Court ultimately invalidated the Judicial Council reform.

Figure 7: Marginal Effects of Co-Partisanship Conditioned by the Rulings




Although not theorized in this paper, these results have important implications for broader discussions of judicial power. Indeed, that (1) those who would sympathize with the courts' decision not only *do not* increase their trust in the judiciary following the ruling (but actually can potentially *decrease* their support) and that (2) the lion's share of public support for judicial institutions comes from government co-partisans, suggests that independent courts willing to keep transgressing incumbents in check may incur in costly actions. This is especially problematic if we expect public support to shield judges from political backlash (Staton 2006; Carrubba 2009; Vanberg 2001; Krehbiel 2016; Vanberg 2015).

## 8 Conclusion

This paper studies an instance of inter-power conflict in which courts provided consequential limits to executive power. I tested an instrumental account of public support for judicial institutions, which expected checks on executives' salient policies to decrease (in-

$p = 0.32$ (entropy balancing models, right panel).

crease) support for the judiciary among government co-partisans (out-partisans). I found that the court rulings brought about decreases in the public's trust in the judiciary and that co-partisans reported decreases in public trust. However, out-partisans do not increase their trust in the judiciary in the wake of seemingly politically favorable rulings.

However, this research has some limitations.

...

Despite these limitations, this paper ultimately helps raise new questions to be addressed by future research. For one, given the findings described here, further work could study whether these types of court rulings can undermine more fundamental measures of public support for judicial institutions (i.e., legitimacy). Moreover, future investigation could examine court decisions' medium- and long-term effects on public evaluations of judicial checks or the implications of such public reactions for both incumbents' and courts' strategic behavior.

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# Online Appendix

Public Partisan Reactions to Judiciary Checks: Evidence from Argentina

# A The 2013 Latinobarómetro Survey

## A1 Technical Information and Regional Administration of the Survey

The 2013 Latinobarómetro survey in Argentina, conducted by the firm MBC MORI, is a nationally representative survey of 1,200 Argentinean respondents across 14 provinces and 24 cities. The survey employed a modified probability sampling method in three phases, by quotas in the final phase (Latinobarómetro 2013, 86).

Table A1: XX

| Province | Control | Lower C. | Supreme C. |
|---|---|---|---|
| Capital Federal | 24 | 22 | 60 |
| Mendoza | 6 | 68 | 4 |
| Chaco | 6 | 13 | 33 |
| Corrientes | 0 | 21 | 17 |
| Entre Ríos | 2 | 11 | 30 |
| Jujuy | 0 | 0 | 32 |
| Salta | 0 | 54 | 13 |
| Tucumán | 8 | 13 | 17 |
| Buenos Aires | 79 | 164 | 194 |
| Córdoba | 52 | 3 | 50 |
| La Pampa | 0 | 0 | 30 |
| Santa Fe | 46 | 56 | 12 |
| Neuquén | 6 | 9 | 17 |
| Río Negro | 12 | 16 | 0 |

*Note:* The table shows the number of respondents in each treatment group by province.

Table A2: XX

| Province-City | Control | Lower C. | Supreme C. |
|---|---|---|---|
| Entre Ríos-Gualeguay | 2 | 11 | 30 |
| Jujuy-San Pedro | 0 | 0 | 32 |
| Salta-Gran Salta | 0 | 54 | 13 |
| Tucumán-Gran S. M. de Tucumán | 8 | 13 | 7 |
| Tucumán-Santa Rosa de Leales | 0 | 0 | 10 |
| Buenos Aires-Gran Buenos Aires | 70 | 113 | 148 |
| Buenos Aires-Bahía Blanca | 0 | 27 | 13 |
| Buenos Aires-Chapadmalal | 7 | 5 | 0 |
| Buenos Aires-Mar del Plata | 2 | 19 | 1 |
| Buenos Aires-Lobos | 0 | 0 | 32 |
| Córdoba-Gran Córdoba | 7 | 3 | 42 |
| Córdoba-Río Tercero | 45 | 0 | 8 |
| La Pampa-General Pico | 0 | 0 | 30 |
| Santa Fe-Gran Rosario | 4 | 28 | 12 |
| Santa Fe-Arroyo Seco | 23 | 0 | 0 |
| Santa Fe-Rafaela | 19 | 28 | 0 |
| Neuquén-Neuquén | 6 | 9 | 17 |
| Neuquén-Villa Manzano | 12 | 0 | 0 |
| Río Negro-Río Colorado | 0 | 16 | 0 |

*Note:* The table shows the number of respondents in each treatment group by city.

## A2 Variables

**Outcome: Trust in the Judiciary (Binary).** "Please look at this card and tell me how much trust you have in each of the following groups/institutions. [Judiciary] Would you say you have a lot, some, a little, or no trust?"

Recoded as binary (1 = A Lot/Some; 0 = A Little/None).

**Female (Binary).** "Gender of the interviewee." Female = 1, Male = 0.

**Age (Continuous).** "What is your age?" Min = 18, Max = 90.

**Income (Continuous).** "Does the salary you receive and your total family income allow you to cover your needs in a satisfactory manner? Which of the following statements describes your situation? It's sufficient and we can save (1); It's just sufficient and we don't have major problems (2); It's not sufficient and we have problems (3); It's not sufficient and we have major problems (4)."

Reversed and scaled (0–1).

**Class (Continuous).** "People sometimes describe themselves as belonging to a social class. Which social class would you describe yourself as belonging to...? High (1); Upper-middle (2); Middle (3); Lower-middle (4); Low (5)."

Reversed and scaled (0–1).

**Presidential Co-Partisan (Binary).** "Belong to political party." 1 = Government; 0 = Opposition/Other/Party not Mentioned.

**Internet Use (Binary).** "Have you ever used e-mail or connected to Internet? Yes, every day (1); Yes, occasionally (2); Yes, rarely (3); No, never (4)."

Recoded as binary (1 = Yes, every day/Yes, occasionally; 0 = Yes, rarely/No, never).

**Political Knowledge (Continuous).** "Out of the institutions listed on this card, please tell me which you know: Mercosur (Southern Common Market); BID (Inter-American Development Bank); CAF (Corporación Andina de Fomento); OEA (Organization of American States); UNDP (United Nations Development Program); UNASUR (Union of South American Nations)."

For each institution, 1 = Know; 0 = Do Not Know. Computed the sum of the answers to each institution (min = 0; max = 6) and scaled (0–1).

# B Full Regression Tables

## B1 Binary Outcome - Unbalanced Sample

Table B1: Unbalanced Sample - Binary Confidence Outcome

| | *Dependent variable:* Confidence in the Judiciary (Binary) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Lower C. Ruling | −0.093* | −0.116* | −0.161*** | −0.110* | −0.150*** | −0.060 | −0.079+ | −0.174 |
| | (0.044) | (0.046) | (0.041) | (0.044) | (0.033) | (0.042) | (0.046) | (0.262) |
| Supreme C. Ruling | −0.129*** | −0.145*** | −0.182** | −0.133*** | −0.170** | −0.071 | −0.084 | −0.091 |
| | (0.034) | (0.040) | (0.060) | (0.031) | (0.052) | (0.062) | (0.068) | (0.268) |
| Co-partisan | | | | 0.140*** | 0.142*** | 0.331** | 0.336*** | 0.335** |
| | | | | (0.041) | (0.039) | (0.119) | (0.101) | (0.109) |
| Lower C. × Co-partisan | | | | | | −0.223+ | −0.222* | −0.206+ |
| | | | | | | (0.121) | (0.108) | (0.113) |
| Supreme C. × Co-partisan | | | | | | −0.270* | −0.274* | −0.266* |
| | | | | | | (0.130) | (0.122) | (0.125) |
| Education | | 0.004 | 0.002 | 0.007 | 0.006 | | 0.005 | −0.004 |
| | | (0.006) | (0.006) | (0.005) | (0.005) | | (0.005) | (0.018) |
| Female | | 0.007 | 0.008 | 0.010 | 0.011 | | 0.012 | 0.017 |
| | | (0.032) | (0.034) | (0.028) | (0.029) | | (0.032) | (0.080) |
| Age | | −0.0004 | −0.0003 | −0.0001 | 0.00002 | | −0.00004 | 0.0002 |
| | | (0.001) | (0.001) | (0.001) | (0.001) | | (0.001) | (0.002) |
| Income | | 0.059** | 0.048* | 0.047* | 0.036* | | 0.039* | 0.095* |
| | | (0.019) | (0.019) | (0.020) | (0.017) | | (0.017) | (0.040) |
| Class | | 0.017 | 0.028 | 0.020 | 0.033 | | 0.027 | −0.041 |
| | | (0.029) | (0.023) | (0.023) | (0.020) | | (0.020) | (0.091) |
| Internet Use | | −0.049 | −0.033 | −0.039 | −0.023 | | −0.023 | 0.015 |
| | | (0.042) | (0.035) | (0.042) | (0.036) | | (0.033) | (0.124) |
| Pol. Knowledge | | 0.004 | 0.001 | −0.0004 | −0.003 | | −0.004 | 0.008 |
| | | (0.010) | (0.010) | (0.009) | (0.008) | | (0.008) | (0.030) |
| Constant | 0.408*** | 0.293*** | 0.260*** | 0.210*** | 0.175* | 0.253*** | 0.121 | 0.169 |
| | (0.030) | (0.062) | (0.072) | (0.052) | (0.071) | (0.057) | (0.086) | (0.196) |
| Controls? | | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ |
| Province FE? | | | ✓ | | ✓ | ✓ | ✓ | ✓ |
| Full Interactions? | | | | | | | | ✓ |
| Observations | 1,175 | 1,147 | 1,147 | 1,147 | 1,147 | 1,175 | 1,147 | 1,147 |
| $R^2$ | 0.011 | 0.022 | 0.049 | 0.040 | 0.067 | 0.067 | 0.077 | 0.087 |
| Adjusted $R^2$ | 0.009 | 0.014 | 0.030 | 0.032 | 0.048 | 0.053 | 0.057 | 0.055 |

*Note:* Bootstrapped standard errors clustered by province.
+p<0.1; * p<0.05; ** p<0.01; *** p<0.001

## B2 Binary Outcome - Entropy Balancing

Table B2: Entropy Balancing - Binary Confidence Outcome

| | *Dependent variable:* Confidence in the Judiciary (Binary) | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Lower Court | | | | | Supreme Court | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Court Ruling | −0.157*** | −0.151** | −0.139*** | −0.076+ | −0.068 | −0.192** | −0.180** | −0.169*** | −0.086+ | −0.078+ |
| | (0.042) | (0.046) | (0.041) | (0.043) | (0.061) | (0.060) | (0.063) | (0.047) | (0.044) | (0.044) |
| Co-partisan | | | 0.224*** | 0.333** | 0.333** | | | 0.217*** | 0.374*** | 0.361*** |
| | | | (0.059) | (0.109) | (0.114) | | | (0.057) | (0.056) | (0.056) |
| Ruling × Co-partisan | | | | −0.217* | −0.221+ | | | | −0.311*** | −0.305*** |
| | | | | (0.109) | (0.117) | | | | (0.079) | (0.079) |
| Education | | 0.003 | | | 0.008 | | 0.0005 | | | 0.006 |
| | | (0.006) | | | (0.007) | | (0.010) | | | (0.007) |
| Female | | 0.012 | | | 0.026 | | −0.011 | | | −0.002 |
| | | (0.034) | | | (0.042) | | (0.044) | | | (0.035) |
| Age | | 0.001 | | | 0.001 | | −0.0002 | | | −0.0002 |
| | | (0.001) | | | (0.001) | | (0.001) | | | (0.001) |
| Income | | 0.085** | | | 0.068* | | 0.098*** | | | 0.081** |
| | | (0.028) | | | (0.032) | | (0.021) | | | (0.028) |
| Class | | −0.027 | | | −0.034 | | 0.006 | | | −0.002 |
| | | (0.037) | | | (0.036) | | (0.029) | | | (0.026) |
| Internet Use | | −0.014 | | | −0.003 | | −0.049 | | | −0.033 |
| | | (0.052) | | | (0.061) | | (0.051) | | | (0.045) |
| Pol. Knowledge | | 0.004 | | | −0.003 | | 0.015 | | | 0.009 |
| | | (0.010) | | | (0.013) | | (0.013) | | | (0.011) |
| Constant | 0.332*** | 0.118 | 0.276*** | 0.247*** | 0.033 | 0.327*** | 0.129 | 0.267*** | 0.233*** | 0.017 |
| | (0.051) | (0.098) | (0.055) | (0.062) | (0.117) | (0.063) | (0.152) | (0.066) | (0.054) | (0.123) |
| Controls? | | ✓ | | ✓ | | | ✓ | | ✓ | |
| Province FE? | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 668 | 668 | 668 | 668 | 668 | 712 | 712 | 712 | 712 | 712 |
| $R^2$ | 0.058 | 0.072 | 0.100 | 0.110 | 0.121 | 0.054 | 0.078 | 0.088 | 0.108 | 0.125 |
| Adjusted $R^2$ | 0.041 | 0.045 | 0.082 | 0.091 | 0.092 | 0.035 | 0.050 | 0.068 | 0.087 | 0.096 |

*Note:* Bootstrapped standard errors clustered by province.
+p<0.1; * p<0.05; ** p<0.01; *** p<0.001

## B3 Combined Treatments - Binary Outcome - Entropy Balancing

Table B3: Combined Treatment Conditions - Entropy Balancing - Binary Confidence Outcome

| | *Dependent variable:* Confidence in the Judiciary (Binary) | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Court Rulings | $-0.181^{***}$ | $-0.174^{***}$ | $-0.158^{***}$ | $-0.085$ | $-0.079$ |
| | (0.039) | (0.042) | (0.032) | (0.053) | (0.049) |
| Co-partisan | | | $0.221^{***}$ | $0.353^{**}$ | $0.346^{***}$ |
| | | | (0.045) | (0.135) | (0.102) |
| Rulings × Co-partisan | | | | $-0.261^{+}$ | $-0.262^{*}$ |
| | | | | (0.141) | (0.103) |
| Education | | 0.002 | | | 0.006 |
| | | (0.006) | | | (0.005) |
| Female | | 0.002 | | | 0.012 |
| | | (0.031) | | | (0.027) |
| Age | | 0.0003 | | | 0.0003 |
| | | (0.001) | | | (0.001) |
| Income | | $0.091^{***}$ | | | $0.075^{***}$ |
| | | (0.016) | | | (0.017) |
| Class | | $-0.010$ | | | $-0.018$ |
| | | (0.024) | | | (0.022) |
| Internet Use | | $-0.034$ | | | $-0.021$ |
| | | (0.038) | | | (0.036) |
| Pol. Knowledge | | 0.010 | | | 0.003 |
| | | (0.010) | | | (0.009) |
| Constant | $0.330^{***}$ | 0.133 | $0.270^{***}$ | $0.239^{***}$ | 0.035 |
| | (0.045) | (0.083) | (0.048) | (0.060) | (0.083) |
| Controls? | | ✓ | | | ✓ |
| Province FE? | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 1,147 | 1,147 | 1,147 | 1,147 | 1,147 |
| $R^2$ | 0.053 | 0.070 | 0.091 | 0.106 | 0.117 |
| Adjusted $R^2$ | 0.041 | 0.053 | 0.079 | 0.093 | 0.099 |

*Note:* Bootstrapped standard errors clustered by province.
$^{+}$p<0.1; $^{*}$ p<0.05; $^{**}$ p<0.01; $^{***}$ p<0.001

## C Placebo Tests

### Concurrent Events

**[Note: this plot was created in previous iterations of this paper and has become outdated. I need to run new analyses to create an analogous (up-to-date) figure]**

Given the saliency of the reform to the Judicial Council, the politicized environment that preceded the court rulings could have well affected public views on the judiciary. If that is the case, the results described above could have been driven by other relevant events (or citizens' anticipation of the courts' rulings) and not by the judicial decisions themselves and their outcomes.

In fact, besides individuals' expectation of the involvement of the courts and direction of rulings, other factors could have affected public trust in the judiciary as well. For one thing, even before the rulings, political elites' made public statements about the benefits or harms that the judicial reform would bring to the judiciary at large. For instance, on June $10^{\text{th}}$, President Fernández, while giving a speech at a university, claimed that the judicial reform was a tool for the "cleaning of the [judicial] system," and would achieve "what the people demand, which is justice but also security [...] If we do not get a better justice, we will not have more and better security in Argentina" (Página12 2013). Moreover, before the June $11^{\text{th}}$ decision, other judges around the country had issued rulings either striking down or upholding the judicial reform. Even though those decisions did not attract high levels of media attention, respondents could have still been affected by them.

To evaluate this possibility, following Jiang and Yang (2016), I randomly shift the date of the lower court ruling to a date $t \in T = [1, 30]$ (that is, any date within the interval in which the Latinobarómetro survey was fielded in Argentina) and the Supreme Court ruling

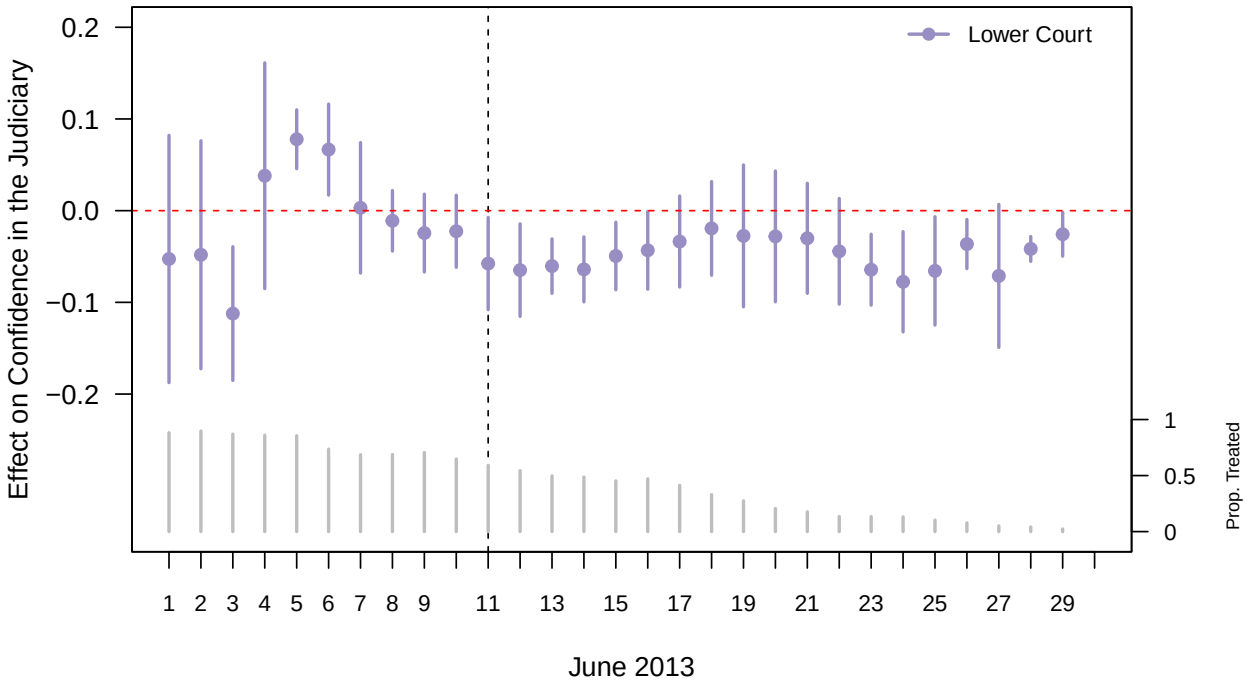
to $t + 7$.[41] I then rerun model **XX** in **Table 4** (main text) and repeat the process for every date from June 1st to 30th.[42] The expectation is that the coefficient for the lower court ruling will be maximized at the actual date in which the decision was released (e.g., June 11th). **Figure C1** plots OLS coefficients and 95% confidence intervals for different cutoff dates—that is, the treatment effects of the lower court ruling on trust in the judiciary. The rug plot (and corresponding y-axis on the right) shows the proportion of 'treated' units, that is, respondents in the *Lower Court* condition relative to the total number of respondents in both the *Lower Court* and *Control* groups.[43] Finally, the dotted vertical line indicates the date of the actual lower court ruling.

[41] The reason for this is that the Supreme Court issued its decision seven days after the lower court ruling.

[42] The model rerun here slightly differs from **Table 4**'s model 5 in that the covariates used to calculate the entropy balance weights are different. Specifically, due to multicollinearity induced by the smaller number of units on either the 'control' and 'treatment' groups at both early and late June, I had to drop a few binary covariates from the analysis. The entropy balance weights for this analysis are calculated from the following covariates: *Female*, *Age*, *Education*, *Internet Use*, class (*Mid. Lower*, *Middle*, and *Mid. Upper*), and *Co-partisanship*.

[43] Note that the denominator employed to calculate the proportion of 'treated' units in **Figure C1** is not the same as the total number of respondents in the survey. Instead, I calculate $\frac{N_{(Tr=1|T=t)}}{N_{(Tr=1|T=t)}+N_{(Co|T=t)}}$, where $N_{(Tr=1|T=t)}$ is the number of *Lower Court* units at time $t$ and $N_{(Co|T=t)}$ is the number of *Control* units at time $t$. Therefore, the number of *Supreme Court* units at time $t$ ($N_{(Tr=1|T=t)} = N_{T=t} - N_{(Tr=2|T=t)} + N_{(Co|T=t)}$) is not taken into account to calculate the proportions shown the the bottom of **Figure C1**.

Figure C1: Shifting Lower Court Ruling Date




To some extent, the figure supports the robustness of my findings. Even though there seems to be some significant negative (June 3$^{rd}$) and positive (June 5$^{th}$ and 6$^{th}$) shifts in support for the judiciary, the small sample of control units (19, 34, and 81, respectively) makes these estimates less reliable. When the number of respondents in the control group increases, it is only on the actual lower court ruling date (June 11$^{th}$) that the negative effect becomes statistically significant. Following the actual date of the lower court decision, we see consistent, significant negative changes in public trust in the judiciary. These effects decrease and become insignificant around one week after the actual decision date.