

Citizens' Commitment to Judicial Independence

A Discrete Choice Experiment in Nine European Countries

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Abstract. In contrast to the more common focus on judicial independence in relation to government institutions, we study under what conditions citizens are willing to trade democratic principles in favor of expected partisan gains? To disentangle this trade-off, we administered discrete choice experiments in surveys across nine European countries to elicit citizens' reactions to nondemocratic reform proposals of the judiciary. The findings suggest that respondents in all countries show some credible commitment to judicial independence. They support this democratic principle first and are partisans only second. The cross-national comparison widens the scope beyond the typically studied US Supreme Court and shows that in polarized societies – which more often suffer from democratic backsliding – reforms to limit judicial independence are less sharply rejected. This has major implications not only for the literature on comparative judicial politics and democratic stability but also for our understanding of citizens' reactions to democratic backsliding.

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1. Introduction

To what extent are citizens guardians of judicial independence? The democratic principle of an independent judiciary is essential within the system of checks and balances. Accordingly, independent judges offer long-term strategic advantages to a political majority which accepts legal constraints today knowing that an adversary will face the same constraints once in power tomorrow (Vanberg, 2015; Ramseyer, 1994; Stephenson, 2003; Ginsburg, 2003; Ginsburg and Versteeg, 2014; Landes and Posner, 1975). It is commonly assumed that citizens support the democratic principle of an independent judiciary – at least to the extent that they regard highest courts as profoundly legitimate institutions (see Gibson, Caldeira and Baird, 1998; Gibson and Nelson, 2014). By withdrawing support from governments that plan to attack democratic principles citizens can constrain elite behavior to serve as guardians of judicial independence (e.g., Staton, 2010; Vanberg, 2005). But are citizens actually willing to do this?

The playbook of democratic backsliding frequently involves that democratically elected governments introduce reforms to limit judicial independence; for instance, in Hungary and Poland. The respective governments still receive considerable public support. Nevertheless, this does not imply that there is no resistance against nondemocratic reforms. The European Union criticized the judicial reforms in Hungary and Poland (Grabenwarter et al., 2013; Barrett et al., 2017) and citizens took their protest to the streets.¹

This illustrates that citizens face a fundamental dilemma: They can either show some credible commitment to democratic principles – such as an independent judiciary – or play partisan games. In fact, scholars acknowledge that citizens may trade support for

¹ Der Spiegel International. 2012. “Destroying Democracy: Hungarians Protest Controversial New Constitution.” <https://www.spiegel.de/international/europe/destroying-democracy-hungarians-protest-controversial-new-constitution-a-806933.html>; last access 06/04/2020. BBC World News. 2018. “Poland protests: Thousands rally against court changes.” <https://www.bbc.com/news/world-europe-44979353>; last access 06/04/2020.

democratic principles in favor of short-term partisan gains (e.g. Bartels and Johnston, 2020; Bartels and Kramon, 2020; Nicholson and Hansford, 2014; Graham and Svobik, 2020).²

To which extent are citizens willing to reject political attempts to undermine an independent judiciary? In order to allow for causal attributions the experimental literature studies individual attitudes towards judicial independence either within the context of citizens general perceptions of the court's ideology or actual court decisions (Bartels and Johnston, 2020; Clark and Kesteliec, 2015) focusing on one particular case; namely, the US Supreme Court.

Our study extends upon this important research in three ways. First, through our experimental design, we move beyond the common evaluation of citizens' attitudes towards specific political decisions. Respondents explicitly evaluate potential institutional reform of the judiciary that would affect judicial independence. Second, we present a more rigorous experimental design to disentangle the important citizens' trade-off on a micro-level between supporting democratic principles of an independent judiciary or accepting limits to judicial independence. Third, instead of focusing on the well-assessed US Supreme Court, we employ the same experimental design across nine less-well studied countries (eight EU member states and the United Kingdom) using a most-different system design (Przeworski and Teune, 1970). The courts we study are situated in different legal and political systems while the overarching European culture is comparable. In sum, our case selection allows us to expect that the citizens' choice patterns we identify are more likely to be generalizable independent of a particular institutional context.

In order to disentangle the trade-off between the support for an independent judiciary or the support for a preferred political actor who gains from limits to judicial independence, we administer the same discrete choice experiments (Louviere, Hensher

² Online appendix A illustrates the empirical nature of the trade-off further using Poland as an example.

and Swait, 2000; Louviere, Flynn and Carson, 2010) across nine countries. A total of 11,352 respondents report 68,112 choices between two hypothetical reform packages of their respective highest national courts exercising constitutional review. The reform packages are described based on three components: (1) The content of the reform, (2) the actors proposing the reform, and (3) the court's reaction to the reform. All values of each component are completely independent randomized. We derive the content of the reforms from the judicial independence literature. While we design one neutral reform as a baseline, all other reform proposals limit judicial independence.

Our experiments provide consistent evidence across all nine countries that citizens show some commitment to judicial independence. They are more likely to reject proposals that the court criticizes as threatening independence. Further evidence of citizens' commitment is that they prefer the neutral reform over any reform limiting the democratic principle of an independent judiciary. Even supporters of the current governments tend to reject court-curbing reforms that would empower their own team, the government, in favor of a neutral reform. Finally, using robust regression analyses our cross-country results show, first, that the effect for government supporters to reject attempts to limit judicial independence depends on the degree of polarization, and, second, that the extent to which government supporters are influenced by the court's reaction to a reform depends on the level of trust in the court. These findings have several important implications – e.g. for the literature on democratic stability, the civic culture, and the transformation of the system of governance – which we summarize in the conclusion.

2. Does the public demand an independent judiciary?

Judicial independence is a decisive feature of judicial power (Cameron 2002; Staton 2010, 8-9) and while the precise definition of judicial independence is contested (Linzer

and Staton, 2015, 225) a common separation is drawn between *de jure* and *de facto* independence (Feld and Voigt, 2003; Melton and Ginsburg, 2014; Rios-Figueroa and Staton, 2012; Linzer and Staton, 2015; Voigt, Gutmann and Feld, 2015).³ *De jure* judicial independence refers to the formalized rules that insulate the judiciary from political pressure and *de facto* judicial independence refers to the actual compliance with judicial decisions (Rios-Figueroa and Staton 2012, 106-107; Voigt, Gutmann and Feld 2015). This way, judicial independence becomes a core principle of a democratic system with sound checks and balances.

From the general definition, scholars developed functional approaches to describe judicial independence. They defined “components of judicial independents” (Melton and Ginsburg, 2014, 195) – such as judicial tenure (e.g. Melton and Ginsburg 2014, 195-196; Hayo and Voigt 2014, 164; Domingo 2000, 712-715) – and identified characteristics that describe a component – such as term limits or lifetime appointments. Different characteristics are frequently combined in indices to measure judicial independence on a country level (see e.g. Feld and Voigt, 2003; Hayo and Voigt, 2014; Linzer and Staton, 2015).

Research has shown that an independent judiciary contributes to regime stability in democracies (Gibler and Randazzo, 2011; Epperly, 2013). Nevertheless, we know little about how judicial independence develops in the first place (Randazzo, Gibler and Reid, 2016, 583). The most common theory used to explain the origins of an independent judiciary is the insurance theory. Accordingly, the political majority will accept an independent judiciary which may limit political action knowing that such a judiciary will also constrain a political adversary once political turnover occurred (Vanberg, 2015; Ramseyer, 1994; Stephenson, 2003; Ginsburg, 2003; Ginsburg and Versteeg, 2014; Landes and Posner, 1975). Thus, judicial independence is the result of a strategic choice taken by political actors. The theory appears tailor-made towards established

³ This discussion is based upon Engst 2021, 5

democracies (see Epperly, 2018) albeit judicial independence is discussed also with regard to nondemocratic regimes (Popova, 2010; Epperly, 2017). Randazzo, Gibler and Reid (2016), for example, reconsider the insurance theory and show that the strength of judicial independence is conditional on the regime type, political competition, and trust within society (see also Aydin, 2013). Finally, Gutmann and Voigt (2020) recently found that de jure and de facto judicial independence are negatively correlated among European countries. They show that societies with high levels of generalized trust and individualism maintain de facto judicial independence while not requiring high levels of de jure independence.

All these studies have in common that measures and effects of judicial independence are discussed on a macro-level and often from an institutional perspective accounting overwhelmingly for the judicial-political dimension in judicial independence. Moreover, scholarship often embodies the idea that – once activated – public support is always in favor of the judiciary (e.g. Vanberg, 2005, 2001; Staton, 2010). This perspective suggests an implicit assumption that an independent judiciary is always in the public interest. The established findings that citizens support judicial legitimacy and place high trust in the judiciary reinforces this perspective (Gibson, Caldeira and Baird, 1998; Caldeira and Gibson, 1995; Gibson and Nelson, 2014).

We seek to extend the literature focusing on the interaction between political actors and citizens that can moderate support for an independent judiciary. In particular, we evaluate to which extent political reform which undermines the institutional foundation of an independent judiciary affects public opinion. When push comes to shove people may choose to trade judicial independence in favor of support of a preferred government (e.g. Stephenson, 2004) to generate expected political gains. Citizens may follow their partisan alignment rather than blindly supporting the independence of courts (e.g. Clark and Kestelleg, 2015; Bartels and Johnston, 2020; Bartels and Kramon, 2020; Nicholson and Hansford, 2014).

In the next section, we outline specific political reform packages by deriving relevant proposals from the judicial independence literature. The reform packages are designed to be realistic to analyze citizens' trade-off between showing some commitment to judicial independence and playing political games for partisan gains.

3. Political reforms of the judiciary

Citizens do not learn about specific reform proposals of the judiciary in a political void. In reality, specific proposals come pre-packaged. Citizens might evaluate the same content of a reform proposal differently depending on who proposed it or how the court reacts to it. In order to assess how citizens think about specific reform proposals we embed them into reform packages that always consist of three components: (1) the substantive content of the reform, (2) the actor proposing the reform and, (3) the court's reaction to the proposed reform.

3.1. Which reform is proposed?

Scholars describing judicial independence refer to different attributes (e.g. Melton and Ginsburg, 2014; Hayo and Voigt, 2014; Feld and Voigt, 2003; Linzer and Staton, 2015). Therefore, we use commonly referenced attributes to describe the substantial content as first component of the reform packages. In the following, we construct three different reform proposals that limit judicial independence and a fourth proposal that does not limit judicial independence.

First, appointment procedures determine the political influence through the composition of the bench (Domingo, 2000, 708). To ensure fair treatment of citizens in court the influence of an individual political actor on judicial appointments should be as low as possible (Feld and Voigt 2003, 501-502; Melton and Ginsburg 2014, 196). Thus, the first reform allows for a judicial selection process dominated by the government.

1. *The reform involves that ... the [Government] can name half of the judges that serve on the [Court].*⁴

This reform proposal limits judicial independence. If the government is empowered to name half of the bench then the remaining judges will always need the support of at least one judge selected by the government to cast a majority vote. Thus, single-handedly packing the court with loyal judges the government has a better chance to protect its policies from unfavorable judicial votes. Citizens asked to vote on the reform face the trade-off to either support the government or ensure judicial independence.

Second, judges on the Supreme Courts of the United States and the United Kingdom serve life tenure during good behavior which isolates them from political pressure (Ferejohn, 1998, 356-357). In addition, judges who face term limits but serve non-renewable terms are also isolated from pressure. They do not need to account for external actors to shape their careers (Melton and Ginsburg, 2014, 195-196). Finally, judges who might require reelection but serve longer terms than the actors selecting them possess some form of independence (Rios-Figueroa, 2011). Thus, the second reform introduces a requirement for frequent legislative re-confirmation of judges.

2. *The reform involves that ... every third year all judges that serve on the [Court] have to be confirmed by the [Parliament]. Judges not confirmed have to leave the court within a month.*

This item limits judicial independence in two ways. First, judges become dependent on legislative faith and are constrained to the extent that they need to be responsive to those who confirm them. Second, considering that proceedings at highest courts take a long time, being exposed to a legislative vote every third year can hinder judges in completing their work. Moreover, it is plausible that over the course of three years

⁴ Items were translated in the respondent's language and the parts in squared brackets were modified to match country-specifics; e.g. the government in Germany is the *Bundesregierung* and the court is the *Bundesverfassungsgericht*.

the same legislators repeatedly assess the same judges. Nevertheless, it is possible that citizens regard a re-confirmation of judges by an elected legislature as democratic. Still, citizens asked to vote on the reform face the trade-off to either support frequent political interference with the judiciary or ensure judicial independence. This item may make the trade-off less obvious but this is in line with the judicial independence literature. Scholarship has highlighted less obvious measures that limit *de facto* judicial independence connected to judicial income or budgetary constraints (Linzer and Staton 2015, 225; Voigt, Gutmann and Feld 2015, 201; Domingo 2000, 715-716) such as, reducing judicial salaries (Hayo and Voigt, 2014, 164), limiting the number of clerks, the court's access to modern computers or the size of the library (Voigt, Gutmann and Feld, 2015, 201).

Third, the way a country designs judicial review influences judicial independence (Hayo and Voigt, 2014, 164-165). In particular, courts cannot enforce decisions themselves. Instead, courts traditionally require political action to implement decisions (Hamilton 1788, Federalist No. 78). Thus, the *de facto* judicial independence is conditional on the recognition of judicial power and the acceptance of judicial decision-making (Rios-Figueroa and Staton 2012, 107; Linzer and Staton 2015, 225). This is why the third reform formally empowers the legislature to evade judicial decisions.

3. *The reform involves that ... the [Parliament] can override a court decision if 75 percent of its members agree.*

The requirement of a qualified majority may once again make this reform seem democratic. However, in any democratic system, the legislature has the power to pass legislation. In this regard, the legislative majority can always pass legislation to evade a judicial decision (Engst, 2021, Ch.2). Thus, the item makes an implicit limitation to judicial independence visible. Citizens asked to vote on the reform face the trade-off to either support the explicit empowerment of political actors to evade judicial decisions or

ensure judicial independence. With a public vote on this reform, the political branches could seek to legitimize legislative action threatening judicial independence.

Finally, it is necessary to develop a neutral item to compare a respondent's willingness to accept reforms that limit judicial independence over a neutral proposal. The neutral item should not limit judicial independence. This is why we make use of the fact that highest courts are commonly overwhelmed by their case-load. The fourth reform grants the court more administrative staff.

4. *The reform involves that ...* the [Court] receives more administrative staff to better handle the increased caseload.

This reform speaks to less obvious measures of judicial independence. It has been mentioned that the manipulation of judicial salaries and budgetary constraints can limit independence (Linzer and Staton, 2015; Hayo and Voigt, 2014; Voigt, Gutmann and Feld, 2015). However, the reform is phrased explicitly positive saying more staff is better for the court. This item implies that courts receive a higher budget instead of budget being withdrawn. Thus, the reform allows courts to work more efficiently strengthening their independence. Citizens asked to vote on this reform face no trade-off that would negatively affect an existing court.

Considering the content of four substantive reform proposals, we have the following expectation with regard to a respondent's choice:

Hypothesis on reform proposal: Citizens show some commitment to judicial independence if they prefer the neutral reform proposal over those proposals limiting independence.

3.2. Who is proposing the reform?

We argue that a respondents' preferences for a reform proposal are endogenous and may change once a preferred political actor proposes a reform – even if such reform

would limit judicial independence. There is evidence at least for the case of the US Supreme Court that citizens' opposition to court-curbing proposals depends on the political actor proposing it (e.g., Bartels and Johnston, 2020; Clark and Kastellec, 2015; Nicholson and Hansford, 2014). To disentangle the trade-off between support for judicial independence and partisan preferences we make the proposing actor a second component of our hypothetical reform packages.

In each country, we randomly allow five actors – four of which are partisan – to propose political reforms of the judiciary: First, members of the party in the lower quartile of the country's Comparative Manifesto Project Scores (CMP, Volkens et al. 2019) with the most parliamentary seats. This is an extreme party to the left. Second, members of the party in the upper quartile of the country's CMP Scores with the most parliamentary seats. This is an extreme party to the right. Third, members of the party between the lower and upper quartile of the country's CMP Scores with the most parliamentary seats. This is the largest moderate party. Fourth, the country's national government.

Our fifth actor is decidedly non-political. Previous research (e.g., Bertson, 2021) uncovered that citizens often prefer to involve non-partisan experts in governmental decision-making processes, supposedly to ensure efficient and effective governance, despite the difficulty of holding such experts accountable. We operationalize such a non-political actor as “a non-partisan committee of legal experts.”

3.3. How does the court react?

Although courts are not empowered to implement decisions themselves, previous research (e.g., Clark and Tom S. Clark, 2010; Meyer, 2020; Staton, 2010; Sternberg, 2019; Vanberg, 2001, 2015) has shown that courts can use strategies to activate public opinion to strengthen their position vis-à-vis the political branches. From this point of view,

respondents may care how the court reacts when evaluating various reform packages. We, therefore, use the court's reaction, to a proposed reform as a third component.

In order to account for the court's reaction we randomly add one of the following three reactions to the hypothetical reform packages.

1. *The judges on the [Court] say that the reform ... potentially threatens the court's ability to work independently.*
2. *The judges on the [Court] say that the reform ... threatens the court's ability to work independently.*
3. *The judges on the [Court] say that the reform ... definitely threatens the court's ability to work independently and to decide cases in a fair and just manner.*

Considering the three reactions by the court we have the following expectation with regard to a respondent's choice:

Hypothesis on judicial reaction: Citizens show some commitment to judicial independence if they prefer reforms considered less harmful by the court over reforms considered more harmful.

Having introduced all components and respective characteristics of the hypothetical reform packages we outline our experimental design in the next section.

4. A discrete choice experiment across nine countries

How do citizens think about judicial independence? One strategy to study judicial independence at the individual-level is to ask citizens directly whether they would support or oppose it. However, most citizens have never thought about such issues before. To state their preference, respondents are likely to look for any information or heuristic to draw on in order to construct an answer. Moreover, social desirability might

creep into the answers. Respondents might dwell on widely shared democratic norms in order to come up and justify what they are supposed to think about the judiciary.

Rather than prompting those processes, we propose to study how citizens think about judicial independence using an experimental design in which we randomly vary the substantive content, who proposed a particular reform, and also how the court reacts to a particular reform. Specifically, we employ a discrete choice experiment (Louviere, Hensher and Swait, 2000) that allows us to identify and estimate the causal effects of many treatment components simultaneously. In fact, discrete choice experiments are the favorite methodology in marketing or applied economics to study how buyers make trade-offs between attributes of different products (Green, Krieger and Wind, 2001). In contrast to conjoint analysis, discrete choice experiments are based on random utility theory rather than conjoint measurement theory (Louviere, Flynn and Carson, 2010).

We let respondents explicitly choose among pairs of randomly generated reform packages to infer citizens' preferences about judicial independence from the actual choices respondents make. Because we randomly generate the relevant components of each reform package, we can, by design, identify and estimate each component's causal effect. For instance, does a respondent still oppose a certain court-curbing measure even if her preferred party proposes it, and does this hold independent of the court's reaction to it? To sum up, rather than asking directly about judicial independence we use a design that allows us a more controlled and rigorous way to figure out to what degree individuals care about judicial independence and what the inherent trade-offs are when choosing between different reform packages.

Case selection Our study design incorporates a comparative aspect. We implemented our online survey experiment simultaneously across nine countries and their particular highest courts with review powers: France, Germany, Hungary, Italy, Netherlands, Poland, Spain, Sweden and the United Kingdom. Respondents were asked to actively

Table 1: Components, items and country-specifics of the randomly generated reform packages

Component 1	Proposal (P)	The reform involves that ...
Item P1	... the [Government] can name half of the judges that serve on the [Court].	
Item P2	... every third year all judges that serve on the [Court] have to be confirmed by the [Parliament].	Judges not confirmed
Item P3	... the [Parliament] can override a court decision if 75 percent of its members agree.	
Item P4	... the [Court] receives more administrative staff to better handle the increased caseload.	
Component 2	Actor (A)	Reform proposed by ...
Item A1	... members of the [Party 1].	
Item A2	... members of the [Party 2].	
Item A3	... members of the [Party 2].	
Item A4	... the [Government].	
Item A5	... a non-partisan committee of legal experts.	
Component 3	Court's Reaction (R)	The judges on the [Court] say that the reform ...
Item R1	... potentially threatens the court's ability to work independently.	
Item R2	... threatens the court's ability to work independently.	
Item R3	... definitely threatens the court's ability to work independently and to decide cases in a fair and just manner.	
<hr/>		
	France	Hungary
Government	Gouvernement de la République française	Magyarország Kormánya
Court	Conseil Constitutionnel	Magyarország Alkotmánybírósága
Parliament	Assemblée Nationale	Országgyűlés
Party 1*	PS	MSZP
Party 2*	REM	Jobbik Magyarországért Mozgalom
Party 3*	LR	Fidesz – Magyar Polgári Szövetség
<hr/>		
	Italy	Poland
Government	Governo della Repubblica Italiana	Rada Ministrów Rzeczypospolitej Polskiej
Court	Corte Costituzionale	Trybunał Konstytucyjny
Parliament	Camera dei Deputati	Sejm
Party 1*	PD	PO
Party 2*	M5S	PIS
Party 3*	FI	PSL
<hr/>		
	Spain	United Kingdom
Government	Gobierno de España	British Government
Court	Tribunal Constitucional	Supreme Court
Parliament	Congreso de los Diputados	House of the Commons
Party 1*	En Comú Podem	Labour Party
Party 2*	PSOE	SNP
Party 3*	PP	Conservative Party

* Party abbreviations were also spelt out in brackets to ease the cognitive burden for respondents.

Figure 1: Setup of the discrete choice experiment as seen by respondents

The [Court] is the only court in [Country], which can evaluate and reject laws. Rejected laws cannot be applied anymore.

Let's assume for the moment that the following two reforms to change the [Court] are proposed. The table below outlines what a reform involves, who proposed a reform and how the [Court] responds to each reform. Please read the descriptions of the two reforms carefully and choose the reform you personally prefer more. There are no wrong choices but only your sole personal choice matters.

	Reform 1	Reform 2
The reform involves that the [Parliament] can override a court decision if 75 percent of its members agree.	... the [National Government] can name half of the judges that serve on the [Court].
Reform proposed by members of [Party 1]	... the [National Government].
The judges on the [Court] say that the reform threatens the court's ability to work independently.	... definitely threatens the court's ability to work independently and to decide cases in a fair and just manner.

If you had to choose between Reform 1 and Reform 2, which one of the two would you prefer?

Reform 1	Reform 2
<input type="radio"/>	<input type="radio"/>

choose six times between two reforms packages randomly composed of the items and country-specific insertions summarized in table 1.

The case-selection strategy covers a variety of different institutional characteristics in a most-different system design (Przeworski and Teune, 1970) and, thus, allows to uncover evidence to what degree the results hold cross-nationally. One may consider the possibility that a respondent's institutional frame of reference and knowledge can potentially affect the choices made. In table 2 we summarize differences in the frame of reference across countries; e.g. in some countries political actors dominate judicial selection (see France, Germany, Poland etc.) while in others non-political actors shape the judicial selection (see Netherlands, Sweden, and the United Kingdom). We further discuss the cross-country difference in online appendix B and account for those throughout the comparative analyses.

Implementation In our implementation respondents saw a screen similar to the one in figure 1. They were asked to choose among two fully randomized reforms packages (proposals \times actors \times reactions) consisting of three components. This allows

Table 2: Institutional context in the nine European countries under consideration (October 2020)

Country	France (Const. Court)	Germany (Const. Court)	Hungary (Const. Court)
Judicial appointment	<ul style="list-style-type: none"> · 3 judges, president of republic, with leg. committees approval, > $\frac{2}{3}$ majority · 3 judges, president of national assembly · 3 judges, president of senate · <i>ex officio</i>, former presidents of republic 	<ul style="list-style-type: none"> · 8 judges, first leg. chamber, $\frac{2}{3}$ majority · 8 judges, second leg. chamber, $\frac{2}{3}$ majority 	<ul style="list-style-type: none"> · 15 judges, nominated by leg. committee · Elected by leg., $\frac{2}{3}$ majority
Source	Art. 56 I - II, Art. 13 V Const.	Art. 94 Const.; §6 Act on Court	State - Art. 24 VIII Const.; §7 Act on Court
Tenure	<ul style="list-style-type: none"> · 9 years, non-renewable · life tenure, former presidents of republic 	<ul style="list-style-type: none"> · 12 years, non-renewable, age limit 68 years 	<ul style="list-style-type: none"> · 12 years, non-renewable, age limit 70 years
Source	Art. 56 I - II Const.	§4 I - III Act on Court	§6 Ib & III Act on Court
Knowledge	48% of 1,118 respondents	48% of 1,074 respondents	53% of 1,122 respondents
Country	Italy (Const. Court)	Netherlands (Supreme Court)	Poland (Const. Court)
Judicial appointment	<ul style="list-style-type: none"> · 5 judges, president of republic · 5 judges, joint leg. seating, $\frac{2}{3}$ majority · 5 judges, supreme courts 	<ul style="list-style-type: none"> · Max. 53 judges on the supreme court · Court and prosecutor general suggest 6 candidates to first leg. chamber who selects 3 nominees · Government selects judge, named on royal decree 	<ul style="list-style-type: none"> · 15 judges, first leg. chamber, absolute majority
Source	Art. 135 I Const.	Art. 117 I, Art. 118 I Const.; Art 72 I Judicial Organization Act; Art. 2 I, Art. 5c VI Judicial Officers Act	Art. 17 I-II Act on Court.
Tenure	<ul style="list-style-type: none"> · 9 years, non-renewable 	<ul style="list-style-type: none"> · life tenure, age limit 70 years 	<ul style="list-style-type: none"> · 9 years, non-renewable
Source	Art. 135 II Const.	Art. 117 II Const.; Art. 46h III Judicial Officers Act	Art. 17 II Act on Court.
Knowledge	26% of 1,170 respondents	21% of 1,190 respondents	70% of 1,127 respondents
Country	Spain (Const. Court)	Sweden (Supreme Court)	United Kingdom (Supreme Court)
Judicial appointment	<ul style="list-style-type: none"> · 4 judges, first leg. chamber, $\frac{3}{5}$ majority · 4 judges, second leg. chamber, $\frac{3}{5}$ majority · 2 judges, government · 2 judges, General Council of Judiciary, $\frac{3}{5}$ majority 	<ul style="list-style-type: none"> · 16 judges, legally qualified candidates apply · Non-political board vets and proposes nominees · Government selects judge 	<ul style="list-style-type: none"> · 12 judges, Lord Chancellor with Selection Committee suggests candidates · Prime minister recommends judge to Her Majesty
Source	Section 159 I Const.; Art. 16 I-II Act on Court	Ch. 11 Art. 1, Art. 6 Const.; Ch. 3 Sect. 4 I, Ch. 4 Sect. 1A Code of Judicial Procedures; Sect. 3-11 Law on Appnt. of Ordinary Judges; Zamboni 2019, Fn. 58	Art. 26 Const. Reform Act 2005
Tenure	<ul style="list-style-type: none"> · 9 years, non-renewable (once >3 years on bench) 	<ul style="list-style-type: none"> · life tenure, age limit 67 years 	<ul style="list-style-type: none"> · life tenure, age limit 70 years
Source	Art. 16 III-IV Act on Court	Ch. 11 Art. 7 Const.; Zamboni 2019, 680	Art. 33, Art. 35 III Const. Reform Act 2005; Art. 26 IV Judicial Pension and Retirement Act 1993
Knowledge	37% of 1,147 respondents	24% of 1,318 respondents	32% of 2,186 respondents

to generate $(4 \times 5 \times 3 =)$ 60 different reform packages. The order of the components on the screen was randomized across respondents to rule out order effects but then fixed across all six screens for each respondent to ease the cognitive burden. In total across our nine different country studies, 11,352 respondents saw 136,224 different reform packages and made 68,112 choices. Respondents' observed choice outcomes, whether they choose 'Reform 1' or 'Reform 2', is the dependent variable we analyze estimating conditional (fixed-effects) logit models. The set of covariates consists of $([4 - 1] + [5 - 1] + [3 - 1] =)$ 9 indicator variables for every item of each of the three components while one item per component has to be set as the reference category.⁵ The estimated raw coefficients for each country as well as the total observations are presented in table C in the appendix.

To enhance substantive interpretation of our results we follow Hainmueller, Hopkins and Yamamoto (2013) and estimate *average margin component effects* (AMCE). The estimate represents the average difference in the probability of choosing the preferred reform package when comparing two different component values; e.g. having a reform proposed by the government or a non-partisan committee of legal experts.⁶

In the next section, we present the choices respondents make also accounting for their ideological position in comparison to the government's position. This will allow to empirically answer the question of whether respondents are willing to sacrifice a court's long-term position as a referee in favor of expected partisan gains.

5. Results

In this section we analyze the data of our discrete-choice experiments by estimating conditional (fixed-effects) logit models in order to predict the probability that a respon-

⁵ We choose the following reference categories: item A5 'Expert Committee' (actor), item P4 'More administrative staff' (proposal), and item R1 'Potentially threatens Court' (reaction).

⁶ Online appendix D outlines a detailed example explaining the estimation.

dent chooses a certain reform package based on the items that describe its components. In the first subsection, we estimate baseline models for each country separately across all respondent choices comparing the AMCEs of all items from table 1 to provide an overview of citizens' opinions on judicial independence. Due to randomization and the resulting orthogonality of each item with respect to every other, the AMCEs, i.e.– the average difference in the probability of choosing a reform package when comparing an item to its baseline category – can be interpreted causally.

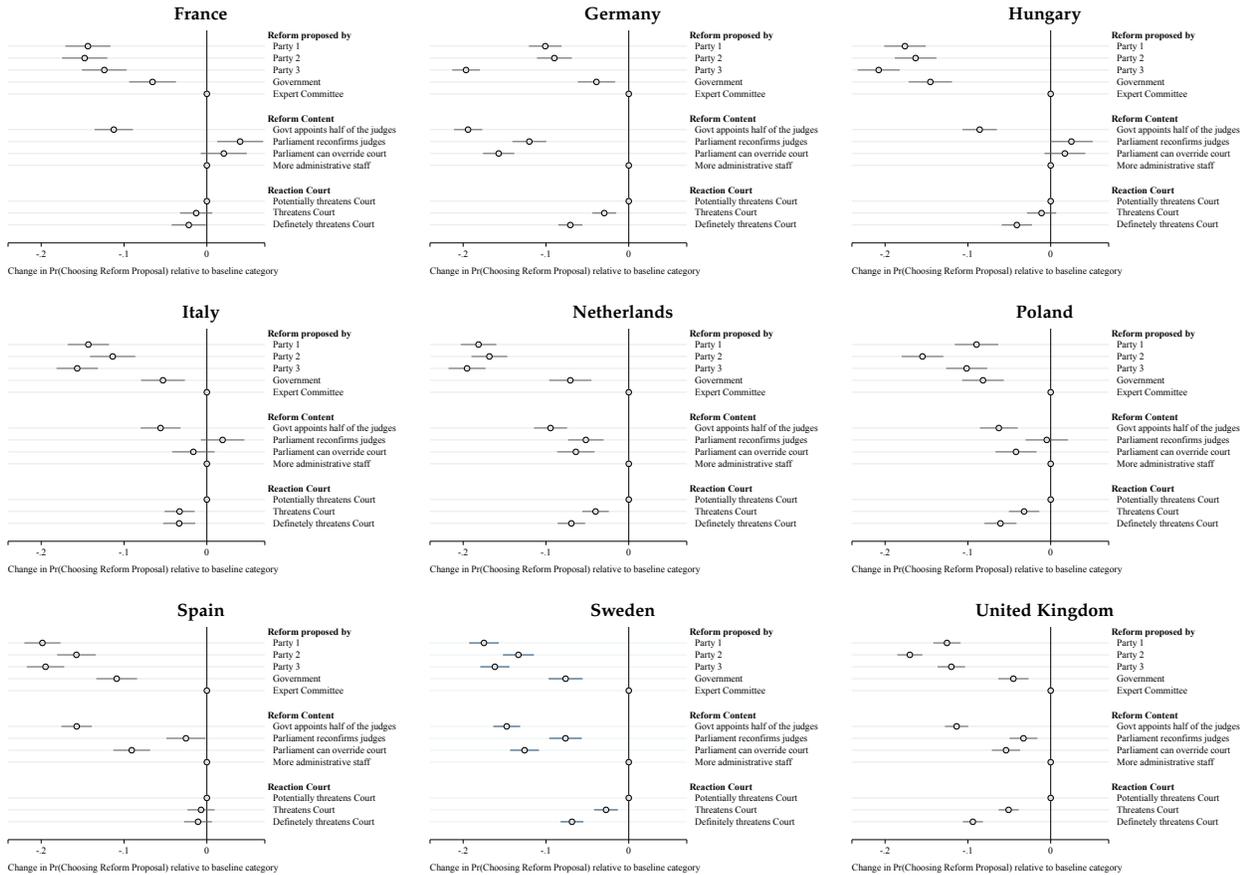
In the second subsection, we estimate separate models for government supporters. We argue that this provides a harder test whether public support for an independent judiciary in Europe is robust enough to deter nondemocratic behavior to limit independence by elected governments and parliamentary majorities.

In the third subsection, we leverage our comparative design and change the analytical focus to the macro level. We show using robust regressions that important macro-level characteristics, such as the degree of polarization and the level of trust in the court within a society are associated with the size of the AMCEs we estimated previously for the subset of government supporters across the countries.

5.1. Judicial independence not at stake?

In this section, we present the analysis of our baseline model across all respondents in each of the nine countries. Figure 2 presents the estimated size of the AMCEs and their respective 95% confidence intervals per country while the three baseline categories are also included as a reference. Across all countries figure 2 shows remarkably similar patterns within all three components of the reform packages. First, let us take a look at the role of the proposer of a reform. Across this diverse set of countries, we find that citizens are most likely to support reforms of the judiciary if proposed by a non-partisan expert committee. The next preferred proposer is always the government. Thus, the common pattern to have reforms proposed by non-partisan actors might suggest a

Figure 2: Average Marginal Component Effects across 9 Countries



preference for a judiciary independent of partisan views who can fulfill a role as referee. At least, political actors are seen as less preferred proposers of judicial reforms. The seemingly strongest effect we uncover in Hungary, where citizens are on average about 15 percentage points more likely to favor the same reform if only proposed by an expert committee (the baseline category) compared to the government.

Second, when looking at the actual reform content we find with the exception of France and Hungary that the neutral reform, to get more administrative staff, is most preferred (or as in Italy and Poland at least not systematically less preferred). Two of the reforms we designed that involve the parliament – either to grant them the right to reconfirm judges every third year or to override a court decision with a 75 percent majority – seem to be most popular among the court-curbing measures for respondents

in all countries. In our samples in France and Hungary the idea that parliament is granted the power to regularly reconfirm judges is even systematically more popular than the neutral reform item. It seems that involving the parliament rather than the government (although the government should easily secure a majority in parliament) when granting power to curb the court seems to be slightly more acceptable. Again, across all countries, the proposal that in each country the government can appoint every other judge that serves on the court is seen as the least preferred among all proposed court-curbing measures. Moreover, across all countries, we also find that the legislature should be granted the power to regularly reconfirm judges rather than to allow legislative overrides of a court's decision with a 75 percent majority.

These findings suggest that on average citizens show some commitment to judicial independence given that they prefer almost everywhere the neutral reform not systematically less than any of the proposals limiting judicial independence. Interestingly, some citizens either do not understand that reforms involving the legislature limit judicial independence or citizens accept limits to judicial independence once an elected parliament is accountable. Finally, a government may use its own legislative majority to hide nondemocratic action. While the opposition can express concerns on the floor the legislative majority may eventually pass proposals that are designed to limit judicial independence. Passing proposals whose content is not easy to understand with the help of democratically elected representatives can make it hard for citizens to uncover nondemocratic action. If one considers democratic backsliding a gradual, multi-stage game then our results are consistent with the argument that citizens may not uncover a government's true intentions in the early stages (Nalepa, Vanberg and Chiopris, 2019).

Third, we also find common patterns when looking at the effect of a court's reaction to the proposed judicial reform measures. The reaction of the court matters in all countries. All countries show the same ranking albeit some items are not systematically different from one another. The negative effect for choosing a reform proposal is highest

if the court responds that this proposal *definitely* threatens the independence of the court and smallest when the court merely responds that the reform *potentially* threatens the independence of the court. This would be in line with a preference for judicial independence as long as citizens trust the court and regard the judiciary as independent; for example, respondents do not significantly account for different statements by the French *Conseil Constitutionnel* and the Spanish *Tribunal Constitucional*. In our surveys, we also find comparatively low levels of public trust in those courts. This evidence is consistent with previous research on those courts in France (Hönnige, 2007; Sternberg, 2019) and Spain (Garoupa and Magalhães, 2020, p.274). Thus, citizens who prefer judicial independence may not consider deeply the signals of a court complaining about limits to judicial independence once they already consider the court as less trustworthy.

In the next section, we evaluate to which extent respondents close to the government permit nondemocratic reforms proposed by the government and to which extent respondents not close to the government reject such reforms.

5.2. Judicial independence (not) at stake!

The previous section shows similar response patterns across all countries in our sample: Reform proposals of the judiciary are most popular when they do not undermine the court's independence and when proposed by non-partisan experts. Political actors proposing reforms are widely depreciated – no matter what the reform actually entails.

How do we know that support for judicial independence is sufficiently strong so that citizens would not be willing to trade-off judicial independence for expected policy benefits or partisan concerns? Well, it needs to hurt to be credible. We argue that a commitment to judicial independence is credible if, first, rejecting court-curbing proposals is costly and, second, if the signal a court sends matters.

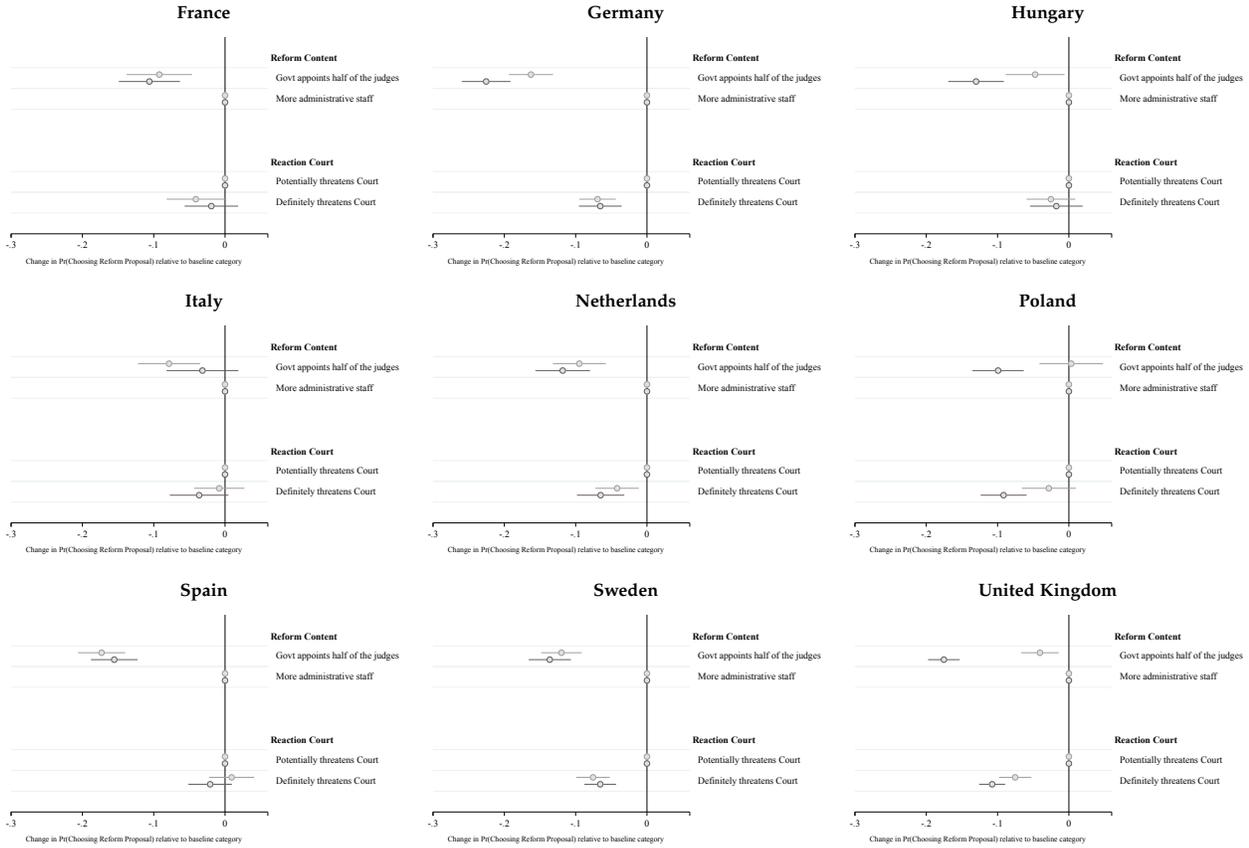
First, citizens not supporting the government are likely to reject court-curbing measures that provide the government with additional power. They might do so because

they value judicial independence or just to avoid giving political adversaries extra power. Thus, simply rejecting a proposal that empowers the government is not necessarily diagnostic of a commitment to the democratic principle of judicial independence. For government supporters, however, rejecting a court-curbing proposal empowering the (preferred partisan) government likely hurts and is, therefore, diagnostic of a commitment to the democratic principle of judicial independence. We test this among the subset of government supporters: If the estimated AMCE of the court-curbing reform (“Government appoints half of the judges”) is significant and negative relative to the neutral reform (“More administrative staff”) then even government supporters reject on average judicial reforms that would otherwise empower their own team.

Second, given that highest courts might constrain the government’s agenda in a system of checks-and-balances, government supporters that show some commitment to judicial independence should also be less likely to choose a reform heavily criticized by the court. In contrast, if government supporters behave as partisans-only then they would not be concerned with the court’s reaction that a reform threatens judicial independence. We test this by looking at the estimated AMCEs with regard to the court’s reaction, again, among government supporters: If the estimate of the verbally most drastic reaction by the court (“definitely threatens”) is significant and negative relative to the most moderate response (“potentially threatens”) then this is evidence that citizens as government supporters possess some non-trivial commitment to judicial independence; otherwise, they would act simply as partisans of the government.

Figure 3 summarizes the relevant AMCEs to test both implications based on the same model specification as before. For each country, we separate *government supporters*, i.e., respondents who position themselves *close to the government* (gray estimates and 95% confidence intervals) from those who do not support the government and, hence, distance themselves *from the government* (black estimates and 95% confidence intervals) on the left-right scale as a reference. We take the upper (‘far from government’)

Figure 3: Effects of Reform Content and Court’s reaction to different reforms by Government support



and lower tertile (‘close to government’) of the distribution of perceived absolute distances between the respondents own location and where they place their respective government to define these subsets in each country.

Consider the estimated AMCEs for the content of reforms with regard to the first implication. The reform to empower the government at the cost of limiting judicial independence is on average less strongly rejected by government supporters than by those not supporting their respective government.⁷ This is true in all countries except for Italy and Spain (although the estimated difference of the AMCEs is not statistically significant there). Nevertheless, in all countries but Poland we find that government

⁷ We formally test this by estimating conditional logit models for both subsets simultaneously. Such a model has only one likelihood function. Thus, we are able to test the difference between functions of coefficients (such as the relevant AMCEs) across both subsets. We report the estimated coefficients in the appendix, tables D and E.

supporters are on average more likely to reject a proposal that strengthens their own team over the neutral reform. In Poland, we find no systematic difference between whether government supporters prefer a reform that empowers their own team or a neutral reform. In sum, supporters of the government in all countries except for Poland systematically reject judicial reforms that would empower their own team. Thus, there seems to be at least a minimum level of support for judicial independence to deter nondemocratic behavior by elected governments and parliamentary majorities to limit independence.

To assess the second implication we focus on the estimated AMCEs about the courts' reactions with regard to reforms among government supporters in figure 3. In Hungary, Italy, Poland, and Spain we find that government supporters do not systematically change their behavior as the court uses more drastic language to signal a threat to judicial independence because the respective gray confidence intervals include the zero reference line. For the remaining five countries we find results consistent with the behavior expected by citizens who value judicial independence. In particular, if courts point out that a particular reform "definitely threatens" the independence of the court then government supporters in France, Germany, the Netherlands, Sweden, and the United Kingdom are statistically less likely to support such a reform.

In sum, we find that citizens in all countries except Poland show at least a minimum level of support for judicial independence although to varying degrees. Furthermore, we also find considerable variance in the respective estimated AMCEs that represent the strength of the court's signal that a reform threatens judicial independence. Even government supporters are systematically less likely to support such a reform in most countries.⁸ In the next section we will test whether the estimated AMCEs vary meaningfully with characteristics that describe a society's political culture, its degree of polarization, and its trust in the institution of the respective highest court.

⁸ In online appendix J we show that we do not find substantive differences between respondent's who are knowledgeable about the court and those who are not knowledgeable.

5.3. Macro Analysis: Modelling Variation in estimated AMCEs

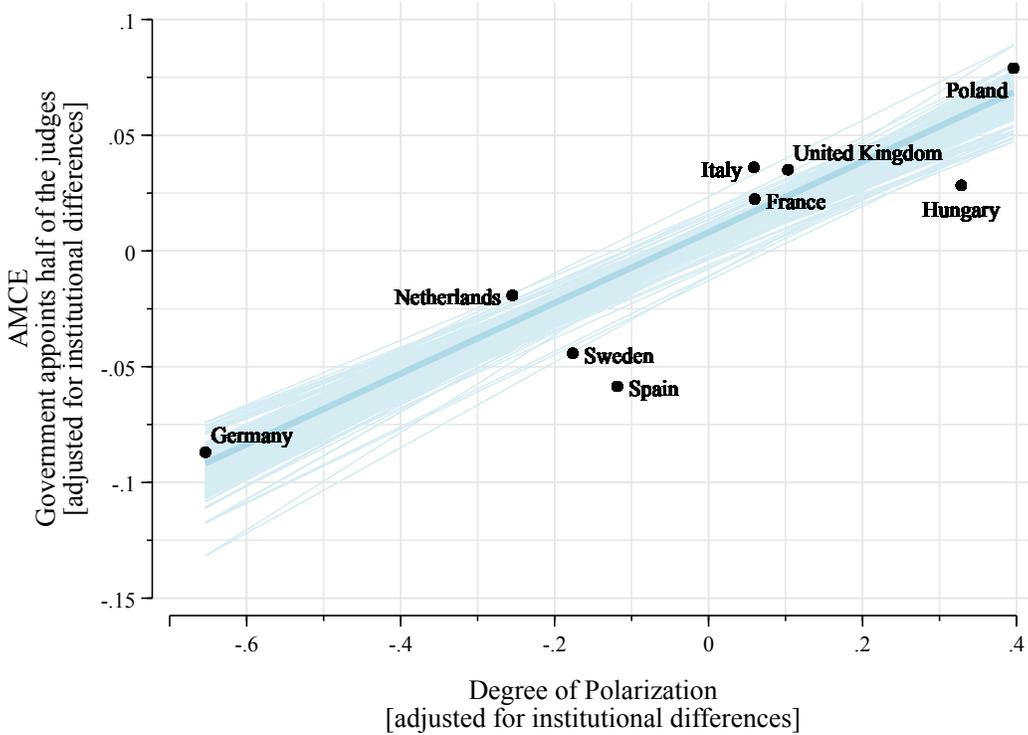
Given the comparable experimental design and the very same baseline reform package across all nine countries we can systematically relate the variance of the estimated AMCEs across countries to interesting country characteristics. Thus, we change the analytical focus in this section from an individual-level to a macro-level analysis.

The good news of our findings from the previous section is that even government supporters seem to show some commitment to judicial independence. They are more likely to choose a neutral reform over a proposal that would strengthen their own team by limiting the court. We have also seen that the degree to which they reject particular court-curbing proposals varies across countries. Previous studies (e.g., Bartels and Johnston, 2020; Levitsky and Ziblatt, 2018; Svobik, 2019) suggest that varying degrees of resilience are associated with different levels of polarization across countries. We, therefore, test whether we can recover such a pattern across countries.

Polarization is a concept that does not directly travel to parliamentary democracies such as most countries in our sample.⁹ However, in parliamentary democracies the respective opposition offers an ideological alternative to the government. If this alternative is seen as very distant from the government, such a country is more polarized than, say, countries where respondents say that government and opposition cannot be distinguished from one another. This has implications for how much ideological disagreement respondents see between themselves and the government. We expect that there is more variation in perceived ideological distances to the government when the political culture is more polarized as in the former case. There is less variation when the opposition is not perceived to offer a distinctly different ideological platform as in the latter case. Thus, the *Degree of Polarization* of a country is at least correlated with the

⁹ Strictly speaking France is a semi-presidential republic but by definition we find a strong parliament in a semi-presidential system.

Figure 4: Preference for Court-Curbing Measure across countries depends on Degree of Polarization



Note: Figure 4 plots the estimated AMCE for the court-curbing proposal that the ‘Government appoints half of the judges’ for the subsample of government supporters against the *Degree of Polarization* adjusted for institutional differences. The plot includes partial regression lines based on a linear fit from robust regressions (using a Huber-M-estimator) to account for outliers and possibly high leverage points. The robust regressions are estimated on 100 replicated data-sets that vary because of estimation uncertainty in the adjusted AMCEs. The thick line represents the expected values based on a robust regression using the point estimates of the adjusted AMCEs per country as dependent variable for the subsample of government supporters.

variance or, as we measure it, the standard deviation of the distribution of perceived absolute ideological distance of respondents to the government.

Our comparative design provides an additional challenge. While the probability to which respondents reject court-curbing might be associated with the degree of polarization, it is likely that part of this variance is simply due to a different frame of reference respondents employ to evaluate the court-curbing proposal that the ‘Government appoints half of the judges’. Whether or not the government already has an active role in selecting judges to the highest court seems to be the most relevant institutional characteristic that might generate a different frame of reference. Table 2 shows, that the government in three countries (France, Italy, and Spain) explicitly

selects a number of judges to the respective highest court while this is not so in the remaining six countries. Government supporters in those countries should be less likely to reject the court-curbing proposal that the ‘Government appoints half of the judges’ because of their different frame of reference. Thus, in order to adjust for institutional differences that might lead government supporters to employ a different frame of reference and evaluate the very same court-curbing proposal differently, we partial out the institutional effects using a dummy variable scoring ‘1’ for France, Italy, and Spain and ‘0’ otherwise from both, the estimated AMCEs for the subsample of government supporters per country as well as the *Degree of Polarization* per country.¹⁰

Is the support of the court-curbing proposal we studied in the previous section associated with the degree of polarization across countries even if we adjust for institutional differences? Figure 4 summarizes our analyses and illustrates that aggregated individual behavior corresponds to central characteristics of how the political culture varies across countries. We find that the more the perceived distance to the government among citizens varies, i.e., the higher the degree of polarization, the more likely are government supporters to choose the nondemocratic proposal that would strengthen their own team over the neutral reform as indicated by the respective AMCE. The estimated slope of the thick robust regression line in figure 4 is 0.15 (with standard error of 0.02) and suggests that, on average, government supporters in countries with less polarized political cultures, like Germany, Netherlands, or Sweden, reject court-curbing proposals at higher levels than in countries with more polarized societies, such as Hungary and Poland accounting for outliers and down-weighting high leverage points.¹¹ The thinner robust regressions lines stem from the analysis of 100 replicated datasets that vary

¹⁰ Partialling-out institutional effects is equivalent to running a linear regression of the estimated country-specific AMCEs on the degree of polarization “controlling for” our dummy variable that characterizes institutional differences across 9 cases.

¹¹ The robust regression results in table F in the appendix are very similar if we do not control for institutional differences in that way. We also show there that simply distinguishing between countries with or without a supreme court to account for different legal traditions and adjusting for it does not do the trick.

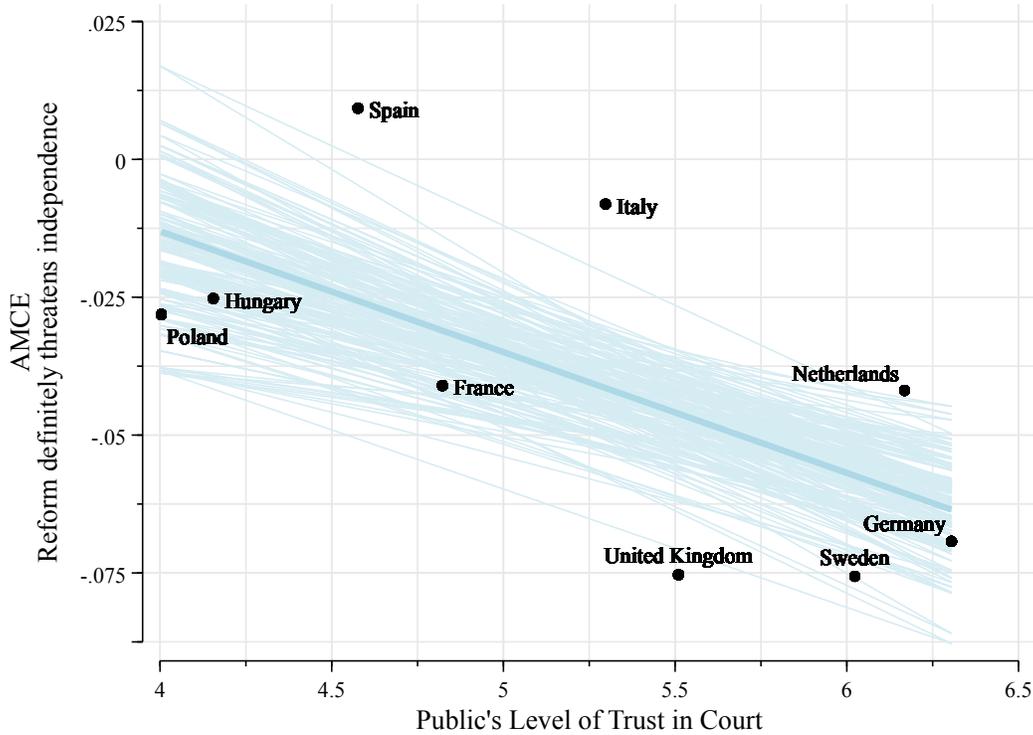
because of estimation uncertainty in the AMCEs.¹² Overall, the results seem to be consistent with expectations in the literature that point to the degree of polarization in society as a mediating factor why countries are more or less backsliding.

Our second observation from the previous section points to the role of trust in the court as an institution for its strategy to go public and the expected effect of the signal the court sends when complaining about particular judicial reform measures. Court signals matter in all countries but with varying degree. Figure 5 shows that the variation of the degree to which court signals matter – represented by the estimated size of the respective AMCEs across our nine countries – is associated with the public’s level of trust in the respective court as an institution. We use the mean level of trust in the respective highest court per country, the standard trust-in-institution item measured on an 11-point scale.

Figure 5 illustrates how strong the effects of a court’s signal that a proposed reform “definitely threatens” judicial independence is associated with the average trust level in our sample of countries. We can see that, as expected, the higher the public’s level of trust in its highest court the more effective is a signal of the court. The estimated slope of the thick robust regression line in figure 5 is -0.02 (with standard error of 0.007). It suggests that, on average, countries with more trust in their highest courts, like Germany, Netherlands or Sweden, reject proposals at higher rates when the court uses the most drastic language to portrait proposed reforms as threats to judicial independence than in backsliding countries, such as Hungary and Poland. We find low rejection rates also in established European democracies, such as Spain and France, that report a comparatively low level of trust in their highest court. An increase of one unit in a country’s mean level of trust on the 11-point scale is on average associated

¹² We generated those replicated data sets by drawing randomly 100 replicates for each country’s estimated AMCEs from a normal distribution with a mean that represents the point estimate of the respective AMCE and a standard deviation that represents the estimated standard error.

Figure 5: Strength of a courts signal across countries depends on public’s level of trust in the court



Note: Figure 5 plots the estimated AMCE for the court’s signal that a reform proposal ‘definitely threatens independence’ for the subsample of government supporters against the reported mean level of trust in the respective highest court as institution per country. The plot includes a linear fit from a robust regression (using a Huber-M-estimator) to account for outliers and possibly high leverage points. The robust regressions are estimated on 100 replicated data-sets that vary because of estimation uncertainty in the AMCEs. The thick line represents the expected values based on a robust regression using the point estimates of the AMCEs per country as dependent variable for the subsample of government supporters.

with an increase of the estimated rejection rate of 2 percentage points accounting for outliers and down-weighting high leverage points.

Overall, the results seem to be consistent with our expectation that points to the public’s level of trust in the court as an important factor that can predict how strong on average the crucial subgroup of government supporters responds to a reform proposal that the court portrays as limiting judicial independence. More comparative research is necessary to see whether this can also explain a court’s general strategy to go public when such judicial reforms are perceived as threats to their independence. The associations in figures 4 and 5 represent neither causal statements nor does the limited number of countries allow us to formally adjust for all possible confounders. Nevertheless,

the patterns we uncovered are relevant because they point to important macro-level characteristics of societies, the degree of polarization and the level of trust in the court, that might inform the literature to improve the comparative theory of democratic backsliding and to point out the conditions under which certain nondemocratic reforms in the playbook of democratic backsliding are more or less likely to be successful.

6. Conclusion

We study whether citizens are willing to trade democratic principles in favor of expected partisan gains. In order to disentangle this trade-off, we administered a discrete choice experiment in nine countries. Respondents had to choose between hypothetical reform proposals regarding the highest national court exercising constitutional review.

Our results indicate that respondents in all countries show some commitment to the democratic principle of an independent judiciary. Citizens consistently prefer neutral over nondemocratic reform proposals of the judiciary. The findings suggest that it seems harder for respondents to uncover court-curbing measures (Nalepa, Vanberg and Chiopris, 2019) once a reform proposal empowers an elected legislature. Nevertheless, even then (with the exception of France and Hungary) a neutral reform is not systematically less preferred than one empowering the legislature. The strongest test is based on the subset of government supporters. Even they tend to reject court-curbing reforms that would empower their own team, the government but limit judicial independence. Polish government supporters are the only exception to the extent that they are indifferent between the nondemocratic and neutral reforms.

While the country-specific findings are promising with regard to public support for judicial independence, our cross-country findings suggest to be cautious: In highly polarized societies – which more often suffer from democratic backsliding – reforms to limit judicial independence are less sharply rejected than in less polarized societies.

Moreover, in societies where trust in the highest court is low citizens account for the court's reactions to a lesser extent when assessing threats to judicial independence compared to societies where trust in courts is high.

Our findings have several important implications not only for the literature on comparative judicial politics but also for the literature on democratic stability and civic culture. Overall citizens seem to safeguard judicial independence which implies strong diffuse support for the judiciary. Diffuse support is the support "independent of the effects of daily outputs" (Easton, 1965, 273) which suggests that highest courts established a solid position in the system of checks and balances. Moreover, comparing government supporters to non-supporters within countries and comparing general patterns of behavior across countries we find stable "patterns of orientation" (Almond and Verba, 1963, 15) towards the democratic principle of an independent judiciary. Thus, while our findings may not imply that a government will immediately become punished as a reaction to court-curbing attempts, they do suggest that governments may not easily get away with this in the long run.

Moreover, our findings have implications for understanding the transformation process of the current democratic system of governance. We can observe a tendency that elected officials delegate more and more power to non-elected actors of so-called *non-majoritarian institutions* (Bovens and Schillemans, 2020) in order to make public policy. Regulatory bodies, central banks, or as the institution we focus on here, highest courts, are prime examples. Our results show that the public may even defend the independence of institutions that are not directly accountable to them. The conditions under which this happens despite the lack of accountability deserve further research.

Finally, our results imply that we need to more strongly account for the context citizens are exposed to when studying the adherence to democratic principles. In highly polarized societies and those where courts are trusted less partisan support may out-weight support for democratic principles. In this regard, the variety of countries

studied here allows putting established findings on the increasingly polarized American society in perspective. At a first glance democratic principles are rather stable in the democracies assessed here but at a second glance increasing polarization and possible miss-trust in courts will put democratic principles to a stress test.

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Online Appendix

Citizens' Commitment to Judicial Independence

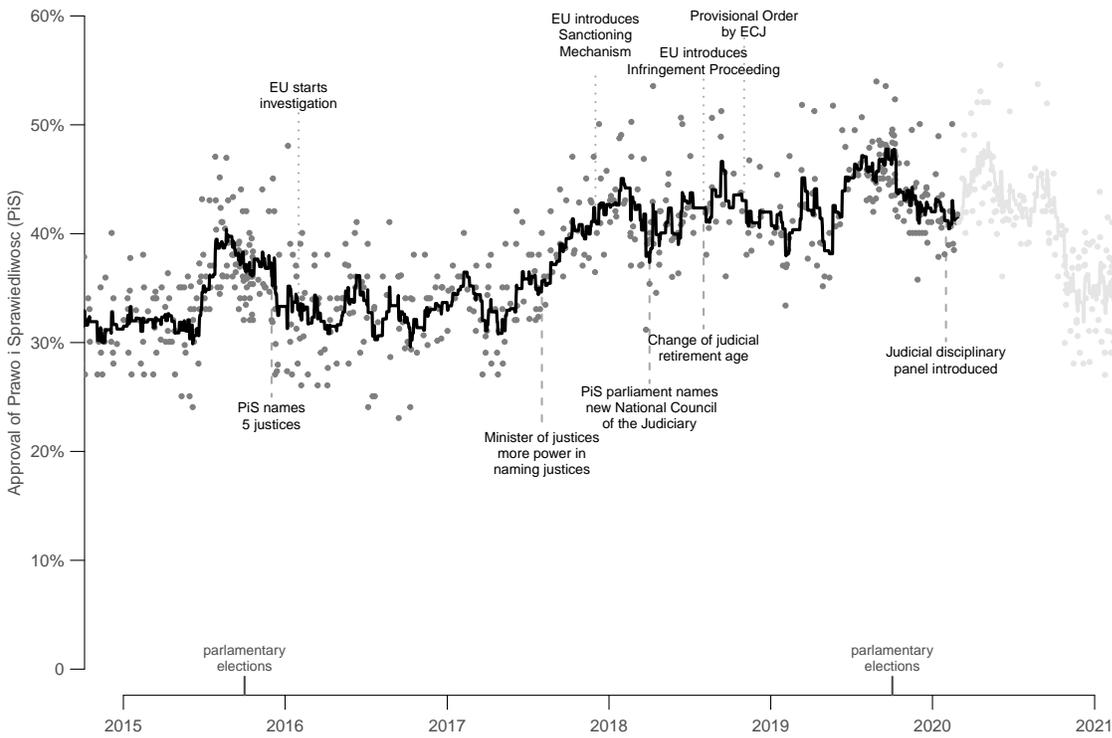
A Discrete Choice Experiment in Nine European Countries

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A. Electoral support and judicial reform in Poland

Figure A: Approval of the PiS who introduced judicial reform in Poland and the European response to such reform over time



Own illustration with data from <https://www.politico.eu/europe-poll-of-polls/poland/> and information by Reuters. 2018. "Chronology: Poland clashes with EU over judicial reforms, rule of law." Kellermann. 2020. "Justizreform in Polen." Deutschlandfunk.

Poland is a suitable empirical example of the trade-off addressed throughout this study. The solid line in figure A shows the approval of the Polish governing Party PiS over time with an emphasis on the electoral period between 2015 to 2019.¹ The approval shows some variation but overall an increasing trend. This is the case despite political efforts by the PiS to limit judicial independence. The examples mentioned in the figure (connected to the dashed lines) illustrate some of these efforts. That citizens were aware of the severity of the politically introduced reform is highlighted by the fact that the European Union had introduced various legal measures to counteract the undemocratic reform (connected to the dotted lines in figure A). Moreover, as mentioned, Polish citizens took to the streets against the reform. In other words, there were Polish citizens

¹ Please note that the gray part of the solid line highlights the approval since the first Covid-19 case in Poland, which is why we ignore this period for now albeit showing the approval rate for completion.

who opposed political reforms limiting judicial independence while at the same time citizens kept supporting the governing majority which had introduced such reform.

B. Respondents' Frame of Reference

Respondents from different countries are potentially affected by a different frame of reference when evaluating reform packages. The first proposal (P1, in table 1) grants the government power to explicitly select judges. In France, Italy, and Spain the government can explicitly select up to a third of the judges (see table 2). However, in Germany, Hungary, and Poland the legislature selects all judges. The government is involved only implicitly through its legislative majority. Finally, in the Netherlands, Sweden, and the United Kingdom non-partisans dominate the selection process. In these countries the government may select judges only from prepared lists; for example, in Sweden where qualified candidates apply to a non-political board which proposes nominees to the government (see table 2). Thus, across countries respondents are exposed to a different frame of reference when evaluating the proposal that the government can name half of the judges.

The second proposal (P2, table 1) does not imply a different frame of reference. While the term length of judges may vary from nine years in the constitutional courts of France, Italy, Poland, and Spain to life-tenure with age limits in the supreme courts of the Netherlands, Sweden, and the United Kingdom none of the countries allow for re-election (see table 2). Thus, nowhere are procedures in place that come close to our hypothetical reform that the legislature reconfirms judges.

The reform proposal P3 (table 1) grants the legislature formal rights to evade judicial decisions. To our knowledge such formal rights do not exist in any of the countries under consideration. It has been outlined that implicitly the legislature can always pass bills to circumvent the judiciary (Engst, 2021, Ch.2) but this is a defining feature of legislative power. Thus, by definition there is no cross-country variation on this dimension and respondents face a similar frame of reference.

It is worth noting that the highest courts in the Netherlands, Sweden, and the United Kingdom follow in the tradition of the Anglo-American supreme court model while the remaining countries follow the Austrian-German tradition of constitutional review (see e.g. Epstein, Knight and Shvetsova 2001, 120-123, Kelsen 1942). The outlined features (see also table 2) highlight differences and similarities among the courts across their legal tradition. We account for the different frames of reference respondents might employ when comparing the results across countries.

Finally, the row on “knowledge” in table 2 indicates the share of respondents correctly identifying the chief justice of a court. It is plausible to assume that a court’s societal salience – which we cannot measure – influences the public’s knowledge regarding that court – which we can measure in our samples. Indeed we find that 53% and 70% of the respondents correctly identify the chief justices in Hungary and Poland – both countries with courts being widely publicly discussed. In contrast, only about 21% are able to identify the Dutch chief justice, which is less surprising considering that the court’s power to settle constitutional matters is limited (Art. 120 Dutch Constitution). One may argue that a court’s salience, measured by the public’s knowledge about the court as a proxy, influences how people evaluate reform packages of the judiciary. However, we show in online appendix J that knowledge about the court does not substantively influence our findings.

C. Comparative Survey Design

The discrete choice experiment was administered as part of an online survey on political institutions and parties in nine European countries from February, 2nd to March 4th, 2020 (with some variation among countries). Respondents were informed about who designed the survey, the content and the aim of the survey on the introductory screen. Moreover, they were informed that they could refuse to answer individual questions or fully abort the survey at any time. Finally, it was made clear that responses are stored anonymously and kept completely confidential. Personal identifiable information was never exchanged between the survey company and the researchers. Each survey was about 20 minutes long and to rule out effects of the survey’s length the discrete choice experiment was randomly positioned either at the first or second part of the survey. About 2000 respondents per country were recruited by the hired survey company based on predefined quotas composed of a country’s gender and age distribution adopted from the 2019 European Election Study (Schmitt et al., 2020) and Gallup International.

In Tables A and B we examine the extent to which the realized samples in each country accurately represent the respective population. Specifically, we compare those quotas to the realized proportions in the country-specific samples illustrating that the deviations from the quotas in the realized samples are neglectable.

Moreover, measures were implemented to ensure data quality; such as, the identification of speeders who rushed through the survey, straight-liners who constantly choose the same answer category, and an open question was included to evaluate mindless responses.

Table A: Country-specific Quotas and Deviations (I)

Category	Germany			Hungary			United Kingdom			France			Italy		
	Proportion	Quota	Difference	Proportion	Quota	Difference	Proportion	Quota	Difference	Proportion	Quota	Difference	Proportion	Quota	Difference
Female	51.21	51.09	0.12	51.96	52.99	-1.03	50.27	51.15	-0.88	53.80	52.39	1.40	53.58	51.88	1.69
Male	48.78	48.91	-0.14	48.03	47.02	1.01	49.72	48.84	0.87	46.19	47.59	-1.41	46.41	48.09	-1.69
18-24	9.77	9.12	0.65	9.18	9.60	-0.43	10.70	10.96	-0.26	11.10	10.20	0.89	7.94	8.19	-0.26
25-39	23.37	22.75	0.62	25.13	24.18	0.95	25.06	25.49	-0.43	23.45	23.25	0.20	19.74	20.63	-0.90
40-54	26.90	25.56	1.34	28.34	26.91	1.43	25.38	25.33	0.04	24.17	25.37	-1.20	29.82	28.29	1.53
55-64	14.80	17.00	-2.20	16.48	16.33	0.14	14.95	15.07	-0.12	15.21	16.00	-0.80	16.75	15.94	0.81
65+	25.13	25.58	-0.45	20.85	22.96	-2.12	23.87	23.13	0.73	26.05	25.15	0.89	25.72	26.91	-1.20

Country specific quotas are based on information from European Elections Study 2019 and Gallup International. *Proportion* represents the realized quotas per category in our samples.

Table B: Country-specific Quotas and Deviations (II)

Category	Spain			Netherlands			Poland			Sweden		
	Proportion	Quota	Difference	Proportion	Quota	Difference	Proportion	Quota	Difference	Proportion	Quota	Difference
Female	52.13	51.49	0.63	50.55	50.76	-0.21	53.41	52.24	1.16	49.84	50.13	-0.30
Male	47.86	48.50	-0.64	49.44	49.24	0.20	46.58	47.74	-1.17	50.15	49.88	0.27
18-24	8.10	8.22	-0.13	11.65	10.81	0.83	10.73	9.51	1.22	8.58	10.43	-1.86
25-39	22.84	23.35	-0.51	22.47	23.09	-0.62	29.10	28.24	0.85	25.30	25.31	-0.02
40-54	30.16	29.58	0.58	24.86	25.99	-1.14	23.24	24.16	-0.93	26.06	24.77	1.29
55-64	15.51	15.44	0.06	18.34	16.61	1.73	16.06	17.18	-1.12	16.48	14.39	2.08
65+	23.36	23.38	-0.03	22.66	23.47	-0.82	20.85	20.88	-0.04	23.55	25.09	-1.54

Country specific quotas are based on information from European Elections Study 2019 and Gallup International. *Proportion* represents the realized quotas per category in our samples.

D. The Average Marginal Component Effect

The *average margin component effects* (Hainmueller, Hopkins and Yamamoto, 2013) represent the average difference in the probability of choosing the preferred reform package when comparing two different component values; e.g. having a reform proposed by the government or a non-partisan committee of legal experts. This calculation requires the following five steps:

First, take a specific reform package (A) that consists of one of the four different substantive reforms, is proposed by the government and, also assume one of the three specific reactions by the court. Then estimate the predicted probability that this specific package A is chosen over another reform package (B) with another specific (but arbitrary) set of component values of the substantive content of the reform, which actor proposed it, and a response by the court. Second, take the exact values of our chosen reform package A but change the proposing actor to a non-partisan committee. We thus obtain reform package C that differs from A only with respect to the proposing actor. Third, we estimate the probability that C is chosen over B and calculate the difference between the probabilities of choosing A over B and C over B . This difference in probability measures the effect of changing the proposing actor from the government to a non-partisan committee for a particular reform packages defined by the previously chosen set of values for the substantive content of the reform and a response by the court. Then, fourth, we successively compute the same difference in predicted probabilities between all other possible reform packages for all other different substantive reforms and responses of the court, each time taking the difference in

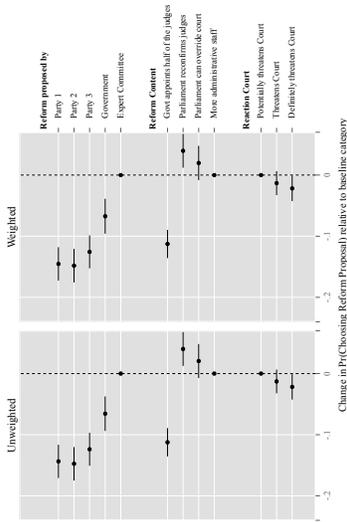
probability of changing the proposing actor from the government to a non-partisan committee. Finally, we take the weighted average of these differences between those two actors over all possible combinations of the component values according to their joint distribution. The resulting AMCEs of the national government (as opposed to the chosen baseline of a non-partisan committee) provide the overall measure of how much, on average, reforms proposed by the government are preferred to reforms proposed by non-partisan experts, i.e., the marginal effect of changing the actor who proposes the reform from non-partisan experts to the government.

E. Comparison of Unweighted and Weighted Data

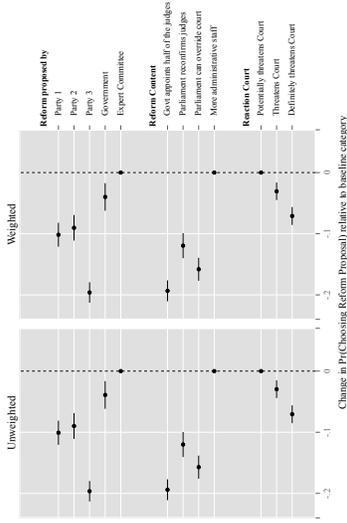
In figure B we replicated our analysis of the unweighted data in the main body of the paper with weighted data. We construct the weights based on state-of-the-art methodology, the so-called raking procedure (Deming and Stephan, 1940). The raking procedure iteratively matches weighted marginal distributions of a sample to known population margins, i.e., a country's gender and age distribution. High-quality comparative survey instruments such as the European Social Survey (ESS) (European Social Survey, 2014) and even the American National Election Studies (ANES) (Debell and Krosnick, 2009) are using the same procedure. As shown in figure B, the results are essentially the same no matter whether we use the weighted or the unweighted data. We, therefore, opt for the parsimonious approach and use the unweighted data throughout our analysis.

Figure B: Comparison of Unweighted and Weighted Data

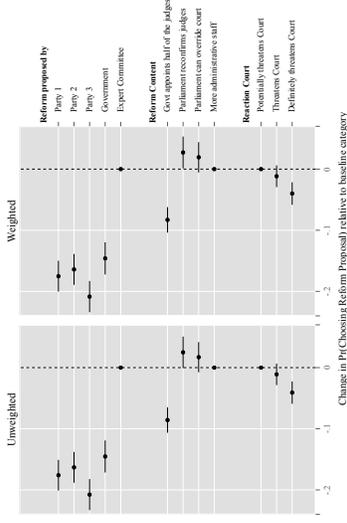
France



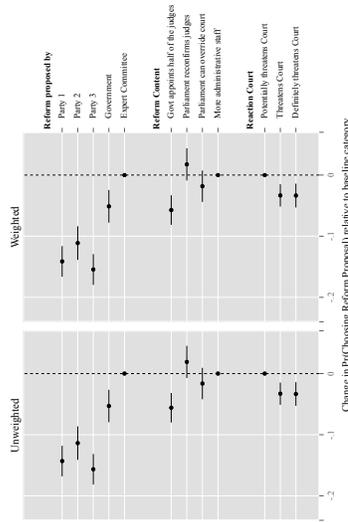
Germany



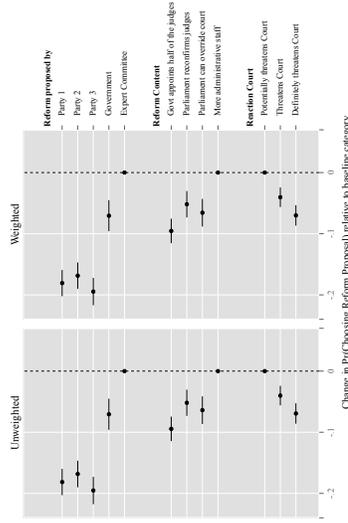
Hungary



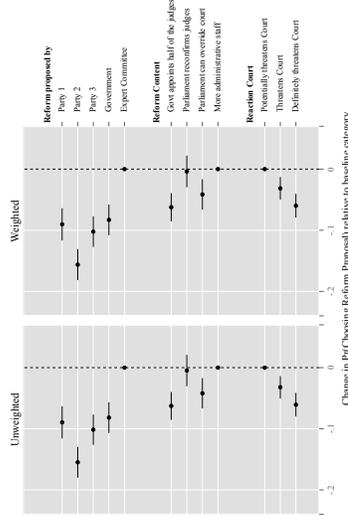
Italy



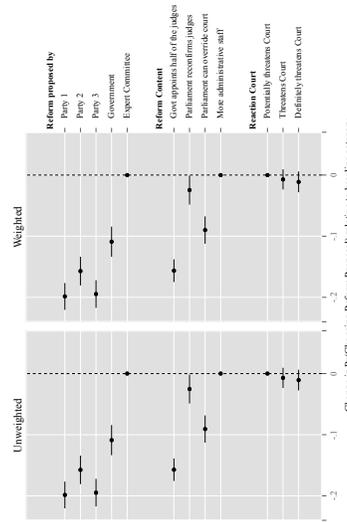
Netherlands



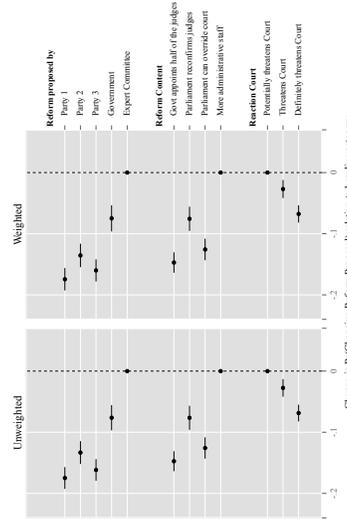
Poland



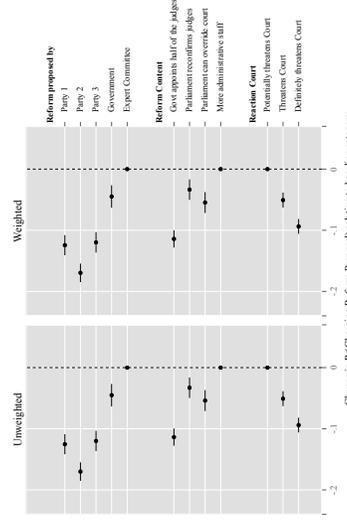
Spain



Sweden



United Kingdom



F. Baseline Estimation Results

Below we document the estimated raw coefficients of the same conditional logit models estimated within each of our nine countries based on unweighted data. These are the coefficients from which we calculate the AMCEs which we present as a quantities-of-interest in the main body in figure 2. Moreover, this table provides an overview over the number of choices given the number of different respondents one can observe in each of the nine realized samples.

Table C: Conditional Logit Results for nine Countries

	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		(9)	
	Germany	Hungary	UK	France	Italy	Spain	Netherlands	Poland	Sweden									
	Coef.	Std.Err.	Coef.	Std.Err.	Coef.	Std.Err.	Coef.	Std.Err.	Coef.	Std.Err.	Coef.	Std.Err.	Coef.	Std.Err.	Coef.	Std.Err.	Coef.	Std.Err.
Reform proposed by																		
Party 1	-0.56***	0.06	-0.77***	0.06	-0.61***	0.06	-0.98***	0.07	-0.90***	0.06	-0.38***	0.06	-0.96***	0.06				
Party 2	-0.49***	0.07	-0.71***	0.06	-0.48***	0.06	-0.74***	0.06	-0.82***	0.06	-0.69***	0.06	-0.69***	0.06				
Party 3	-1.34***	0.08	-0.93***	0.07	-0.56***	0.04	-0.95***	0.07	-0.99***	0.07	-0.43***	0.06	-0.87***	0.06				
Government	-0.20***	0.06	-0.62***	0.06	-0.22***	0.06	-0.49***	0.06	-0.31***	0.06	-0.35***	0.06	-0.37***	0.05				
Reform content																		
Govt appoints half of the judges	-1.16***	0.07	-0.42***	0.06	-0.25***	0.06	-0.85***	0.06	-0.50***	0.06	-0.28***	0.05	-0.82***	0.06				
Parliament reconfirms judges	-0.63***	0.06	0.11*	0.06	0.17***	0.06	-0.12**	0.06	-0.26***	0.06	-0.02	0.06	-0.39***	0.06				
Parliament can override court	-0.88***	0.07	0.08	0.06	-0.26***	0.04	-0.45***	0.06	-0.33***	0.06	-0.19***	0.06	-0.68***	0.06				
Opinion Court																		
Threatens independence	-0.17***	0.05	-0.05	0.04	-0.24***	0.03	-0.06	0.04	-0.14***	0.04	-0.21***	0.04	-0.15***	0.04				
Definitely threatens independence	-0.45***	0.05	-0.19***	0.05	-0.47***	0.03	-0.10**	0.05	-0.15***	0.04	-0.06	0.05	-0.37***	0.05				
Observations	12888		13464		13416		13764		14040		13080		13524		15816			
Log-Likelihood	-3925		-4427		-4446		-4403		-4725		-4237		-4566		-5046			
No. of choices	6444		6732		6708		6882		7020		6540		6762		7908			
No. of Respondents	1074		1122		1118		1147		1170		1090		1127		1318			

Reference categories omitted; * p < 0.10; ** p < 0.05; *** p < 0.01.

G. Individual Heterogeneity - Closeness to the Government

In the tables D and E we replicate the baseline models across all nine countries within two subsets of the data – a subset of government supporters, i.e., respondents who position themselves close to the government, and those who do not support the government and, hence, distance themselves from the government on the left-right scale. We take the upper (‘far from government’) and lower tertile (‘close to government’) in each country of the distribution of perceived absolute distances between the respondents’ own location and where they place their respective government on the same scale to define these subsets. We use these raw estimated coefficients to calculate the AMCEs which we present as a quantities-of-interest in figure 3 as well as for our macro analysis presented in figures 4 and 5.

Table D: Conditional Logit results for Respondents *close to* and *far from* the Government

	Germany		Hungary		UK		France		Italy	
	Close to Govt.	Far from Govt.								
Reform proposed by										
Party 1	-0.64*** (0.10)	-0.57*** (0.14)	-0.77*** (0.10)	-0.50*** (0.15)	-0.83*** (0.07)	-0.15 (0.09)	-0.71*** (0.12)	-0.44*** (0.13)	-0.32** (0.10)	-1.03*** (0.13)
Party 2	-0.28** (0.10)	-0.76*** (0.14)	-0.71*** (0.10)	-0.89*** (0.15)	-1.05*** (0.08)	-0.58*** (0.09)	0.04 (0.12)	-1.26*** (0.14)	-0.32** (0.11)	-0.68*** (0.13)
Party 3	-1.51*** (0.12)	-0.94*** (0.15)	-0.17 (0.11)	-1.91*** (0.18)	-0.19** (0.07)	-1.35*** (0.10)	-0.29** (0.11)	-0.82*** (0.13)	-0.86*** (0.11)	-0.15 (0.13)
The National Government	-0.06 (0.11)	-0.42** (0.13)	-0.18 (0.10)	-1.32*** (0.16)	0.03 (0.07)	-0.55*** (0.09)	-0.05 (0.11)	-0.57*** (0.13)	-0.08 (0.10)	-0.39*** (0.11)
Reform content										
Govt appoints half of the judges	-0.89*** (0.11)	-1.51*** (0.14)	-0.21* (0.09)	-0.80*** (0.14)	-0.19** (0.06)	-1.10*** (0.09)	-0.40*** (0.11)	-0.53*** (0.12)	-0.34*** (0.10)	-0.14 (0.12)
Parliament reconfirms judges	-0.56*** (0.10)	-0.67*** (0.14)	0.11 (0.10)	0.30* (0.15)	0.11 (0.06)	-0.52*** (0.09)	0.08 (0.11)	0.23 (0.12)	-0.14 (0.10)	0.29* (0.12)
Parliament can override court	-0.76*** (0.11)	-1.08*** (0.15)	0.11 (0.09)	-0.04 (0.15)	0.11 (0.07)	-0.75*** (0.09)	0.17 (0.11)	0.04 (0.12)	-0.17 (0.10)	0.20 (0.12)
Reaction Court										
Threatens independence	-0.12 (0.08)	-0.21* (0.09)	-0.08 (0.07)	0.04 (0.10)	-0.17** (0.05)	-0.27*** (0.07)	-0.09 (0.08)	-0.03 (0.09)	-0.14 (0.07)	-0.14 (0.08)
Definitely threatens independence	-0.40*** (0.08)	-0.46*** (0.12)	-0.11 (0.08)	-0.10 (0.11)	-0.34*** (0.05)	-0.70*** (0.07)	-0.17 (0.09)	-0.09 (0.09)	-0.04 (0.08)	-0.16 (0.09)
Observations	4944	2892	4740	2652	10020	7128	4080	3468	4440	3360
Log-Likelihood	-1497	-875	-1577	-770	-3209	-2155	-1349	-1104	-1481	-1099
No. of choices	2472	1446	2370	1326	5010	3564	2040	1734	2220	1680
No. of Respondents	412	241	395	221	835	594	340	289	370	280

Reference categories omitted; * p < 0.10; ** p < 0.05; *** p < 0.01.

Table E: Conditional Logit results for Respondents *close to* and *far from* the Government

	Spain		Netherlands		Poland		Sweden	
	Close to Govt.	Far from Govt.						
Reform proposed by								
Party 1	-0.65*** (0.11)	-1.54*** (0.14)	-1.00*** (0.11)	-0.84*** (0.13)	-0.48*** (0.11)	-0.29* (0.12)	-1.00*** (0.10)	-1.12*** (0.13)
Party 2	-0.40*** (0.11)	-1.15*** (0.12)	-0.57*** (0.11)	-1.23*** (0.14)	-0.09 (0.11)	-1.50*** (0.15)	-0.50*** (0.09)	-1.18*** (0.12)
Party 3	-1.24*** (0.12)	-0.52*** (0.13)	-1.09*** (0.12)	-0.81*** (0.14)	-0.43*** (0.10)	-0.30* (0.12)	-0.81*** (0.09)	-1.06*** (0.12)
The National Government	-0.30** (0.10)	-0.76*** (0.11)	-0.18 (0.10)	-0.57*** (0.12)	0.03 (0.09)	-0.90*** (0.12)	-0.27** (0.09)	-0.53*** (0.11)
Reform content								
Govt appoints half of the judges	-0.88*** (0.10)	-0.93*** (0.12)	-0.48*** (0.10)	-0.68*** (0.13)	0.01 (0.09)	-0.56*** (0.12)	-0.63*** (0.08)	-0.84*** (0.12)
Parliament reconfirms judges	-0.02 (0.09)	-0.16 (0.11)	-0.29** (0.10)	-0.18 (0.12)	0.19 (0.10)	-0.33** (0.12)	-0.30*** (0.09)	-0.36** (0.11)
Parliament can override court	-0.46*** (0.10)	-0.47*** (0.12)	-0.35*** (0.11)	-0.39** (0.13)	-0.15 (0.10)	-0.19 (0.12)	-0.61*** (0.09)	-0.67*** (0.11)
Reaction Court								
Threatens independence	-0.04 (0.08)	0.04 (0.09)	-0.11 (0.08)	-0.21* (0.09)	-0.07 (0.07)	-0.40*** (0.09)	-0.18** (0.07)	-0.04 (0.08)
Definitely threatens independence	0.05 (0.08)	-0.12 (0.09)	-0.21** (0.08)	-0.37*** (0.10)	-0.12 (0.08)	-0.50*** (0.10)	-0.41*** (0.07)	-0.42*** (0.08)
Observations	4800	4140	4488	3000	4512	3312	6180	4248
Log-Likelihood	-1509	-1261	-1443	-957	-1530	-1021	-1988	-1319
No. of choices	2400	2070	2244	1500	2256	1656	3090	2124
No. of Respondents	400	345	374	250	376	276	515	354

Reference categories omitted; * p < 0.10; ** p < 0.05; *** p < 0.01.

H. Robust Regression Results of Macro Analysis

In table F we document the estimated coefficients of four differently specified robust regressions we run to show whether the degree to which supporters of the government actually reject court curbing proposals is associated with the degree of polarization in our sample of nine countries. In Model (1) we simply run a bivariate robust regression to account for outliers and possibly high leverage points given to low number of countries. The results indicate that, as expected, the degree of polarization in those societies is in fact positively associated with strength of the preferences among government supporters to chose a court-curbing proposal that suggests that the ‘Government appoints half of the judges’ over the neutral proposal that would *not* limit judicial independence. Thus, at least in our sample of countries we find that the more polarized a society is the stronger the tendency of a government supporter to prefer a proposal that limits judicial independence if their own team is benefiting from it.

Moreover, we hypothesized that part of this variance is simply due to a different frame of reference respondents employ to evaluate such a proposal. Such different frames of references likely emerge because of institutional differences between countries. We adjust for two institutional differences, first, whether the country uses a supreme court rather than a constitutional court and, second, whether the appointment rules determine that the government actively selects judges to the highest court.

In Model (2) we adjust for the fact that respondents in countries with a supreme court rather than a constitutional court (see table 2) might have developed a different frame of reference. However, this characteristic seems not to be systematically related to the estimated size of the AMCE. The same inference can be drawn from Model (4) as well. In contrast, Model (2) provides evidence that in a country where the appointments rules are such that the government actively selects judges to the highest court, government supporters seem on average to be .05 percentage points more likely to prefer court-curbing measures that limit judicial independence over the baseline category of the neutral reform in our discrete choice experiment.

Table F: Robust Regression Results predicting preference for court-curbing as a function of polarization and institutional characteristics across countries

	(1)	(2)	(3)	(4)
Degree of Polarization	0.14*** (0.02)	0.15*** (0.04)	0.15*** (0.02)	0.15*** (0.02)
Supreme Court (=1)		0.03 (0.02)		0.00 (0.01)
Govt. selects judges (=1)			-0.05*** (0.02)	-0.05** (0.02)
Constant	-0.46*** (0.06)	-0.50*** (0.11)	-0.48*** (0.05)	-0.49*** (0.04)

N = 9; * p < 0.10; ** p < 0.05; *** p < 0.01.

In the main body of the paper we work with Model (3) in order to adjust for institutional settings that potentially make a significant difference to respondents' choices and visualize the results in figure 4.

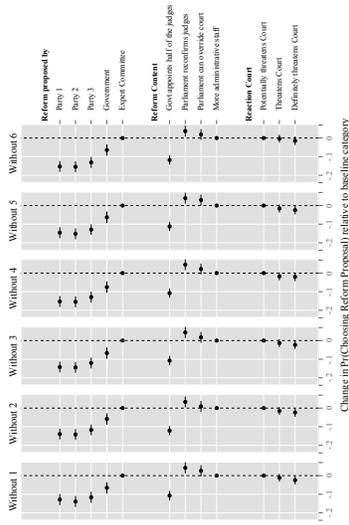
I. Assumption of No Carryover Effects

One diagnostic test involves checking whether there are any carryover effects (Hainmueller, Hopkins and Yamamoto, 2013). The assumption of 'No Carryover Effects' implies that respondents would choose the same reform package as long as the choice among the two reform packages on a particular screen were identical, regardless of which reform packages they had already seen or would see later. We assess the plausibility of this assumption by estimating the AMCEs based on six different subsets of the data we analyze in the main body of the paper. Each subset excludes the data of one particular screen respondents see during our discrete choice experiment, i.e., one pair of reform packages to choose from. If there are carryover effects then dropping

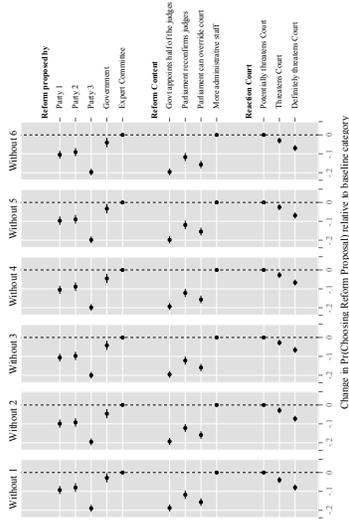
the data of one screen should change the results across subsets. Figure C provides an overview of the resulting AMCEs. The estimated AMCEs are very similar across all the six different subsets within each of the nine countries in our study. This supports the underlying assumption of 'No Carryover Effects'.

Figure C: Analysis of Carryover Effects

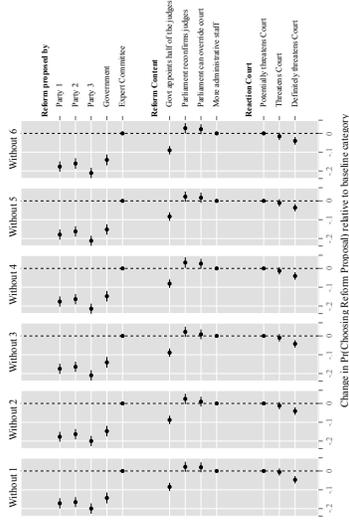
France



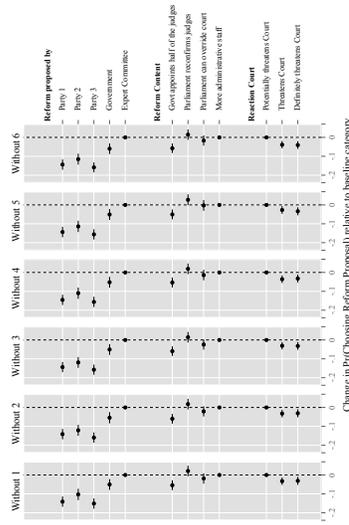
Germany



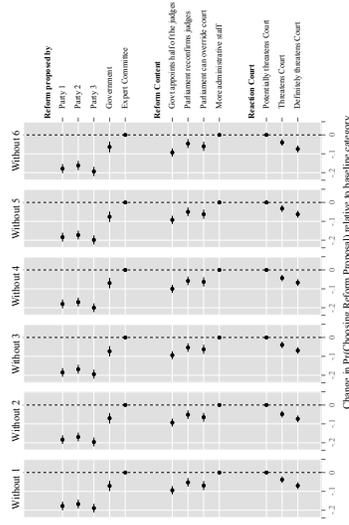
Hungary



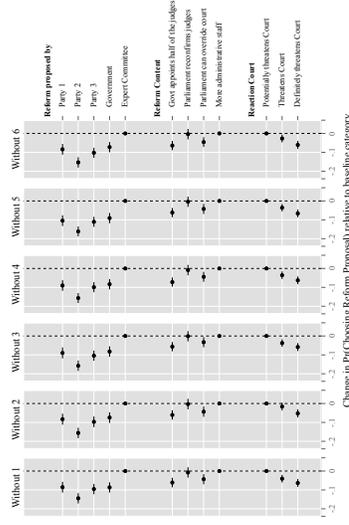
Italy



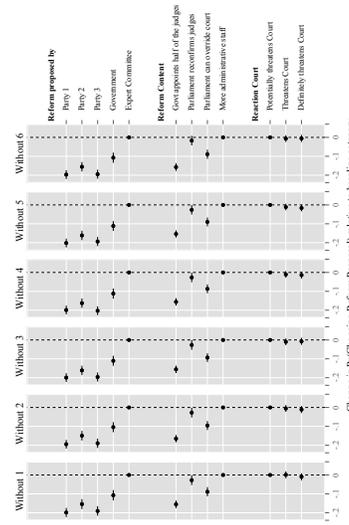
Netherlands



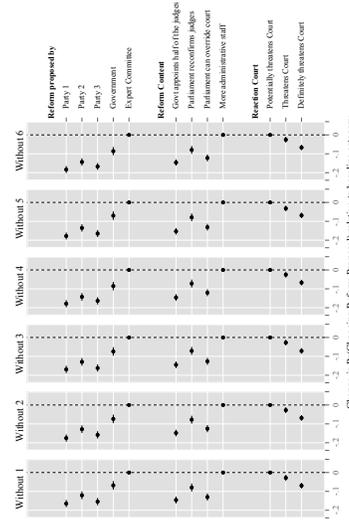
Poland



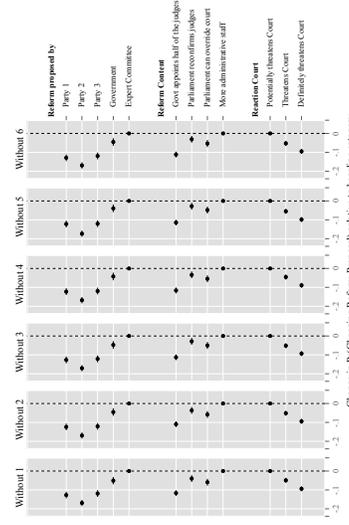
Spain



Sweden



United Kingdom



J. Does Knowledge about the Court matter?

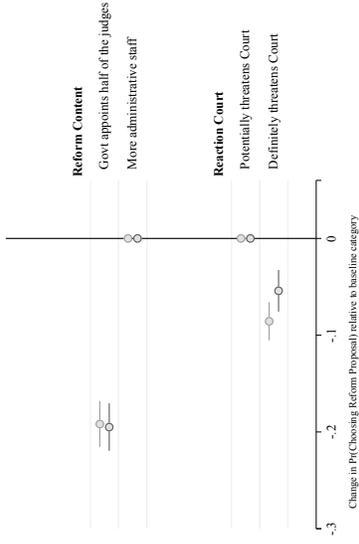
In general, individual-specific items should not matter a great deal because of randomization. Nevertheless, we assess here whether respondents who have some knowledge about their highest court behave differently than other respondents.

We included the following item to assess respondents' knowledge about the court. We asked the following: "*Do you happen to know the name of the current chief justice of the [Court]? If you are not sure, please make a guess*". We presented the respondents with three answers whose order were randomized. One was the (country specific) name of the chief justice while the other two options have been the respective name of each country's member of the *International Olympic Committee* (IOC), and the respective name of the country's ambassador to the US. In a few cases where these members were too prominent – e.g. HRH Princess Anne as member of the IOC in the UK – we selected a country's representative for the European Union. From this item we created a knowledge indicator scoring '1' if respondents identified the respective chief justice correctly and '0' otherwise.

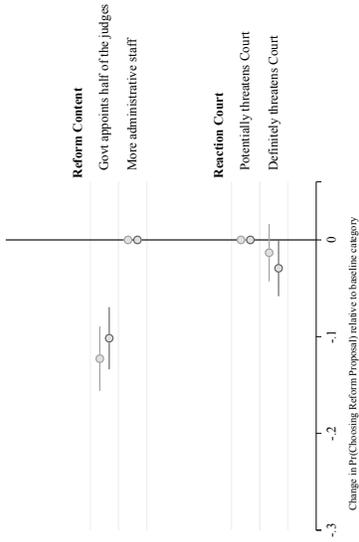
We compared the estimation results among two subgroups, those who correctly identified the chief justice (estimates in light-grey) and those who did not (estimates in dark-grey) in figure D. Specifically, we present the estimated AMCEs for the substantively most interesting reform proposals and the subsequent reactions of the court across both sub-samples. From 18 ($= 2 \times 9$) comparisons across the 9 countries we cannot find statistically different estimates in 14 of them. The four exceptions to this rule, where the estimates across the two sub-samples differ, are the estimated AMCE of the reform content in Hungary and the AMCEs of the reaction of the court in Germany, Italy, and United Kingdom. In all those exceptions we find that knowledgeable respondents evaluate the respective component more negatively than the respective baseline. However, in no single case do those estimates lead to a different substantive conclusion. We, therefore, conclude that substantive knowledge about the court does not lead to substantively different results.

Figure D: Analysis of Knowledge Effects

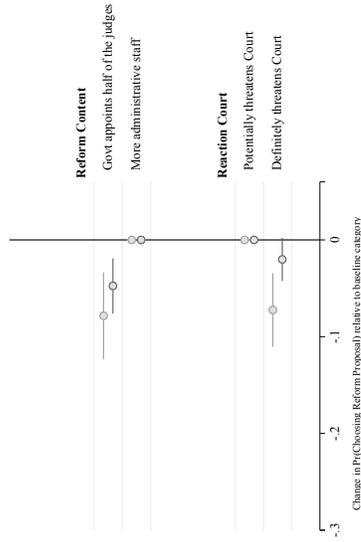
Germany



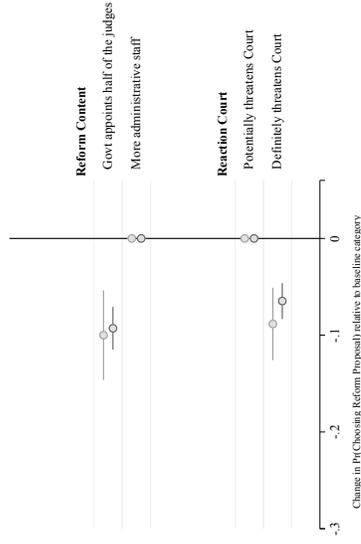
France



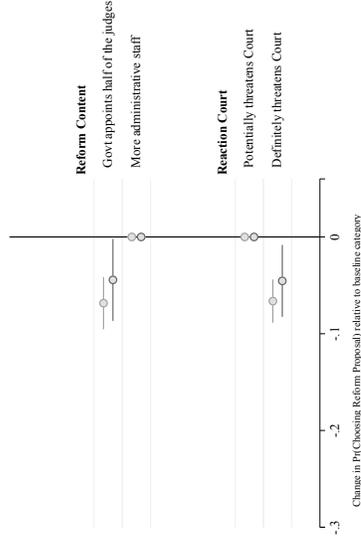
Italy



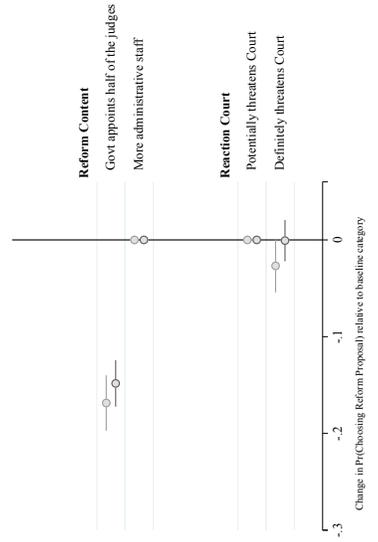
Netherlands



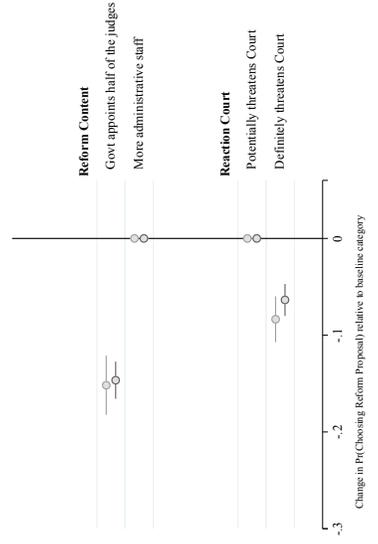
Poland



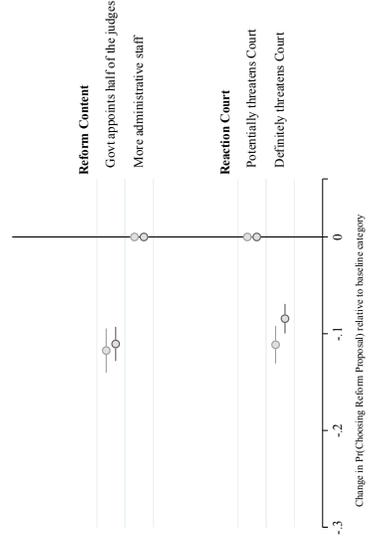
Spain



Sweden



United Kingdom



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