JUDICIAL IDEOLOGY EMERGES, AT LAST, IN SECOND AMENDMENT CASES

Adam M. Samaha* & Roy Germano**

ABSTRACT

Americans seem increasingly divided along party lines when asked general questions of value regarding gun policy. Partisan divides are sometimes smaller for relatively specific policy questions, interestingly, but gaps can be found either way. One might also expect ideological divides when judges decide constitutional claims to gun rights. Those claims are not governed by any rigid and comprehensive doctrine. And yet, in an earlier study of federal circuit court cases decided from 2008 through early 2016, no ideological divide between judges was apparent.

With more recent data, however, we cannot as easily suggest that judges are special in the struggle over gun policy. For 2016–2018, three different proxies for judge ideology are significant predictors of judge votes in civil gun rights cases. We estimate a 21-point gap between Republican and Democratic appointees in the probability of supporting a claim during that timeframe. Moreover, the gap appears to result primarily from Democratic appointees becoming less likely to support gun rights claims over time. The explanation for this shift does not appear to be new judges, however. On average, President Obama’s appointees do not seem to vote differently from other Democratic appointees.

The best explanation for the change is not yet known. Possibilities include new judicial attitudes, new issues arising from post-Newtown firearms regulation, and a new political

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environment initiated by the 2016 presidential campaign. In any event, judicial review in gun rights cases is beginning to look more like larger disagreements in this country over concrete gun policy proposals—disagreements with which we are quite familiar.

INTRODUCTION

The thought of gun policy divides us. When the options are formulated in highly abstract terms, public opinion on gun policy seems riven along party lines. And more so today.

Consider a basic question about priorities: In 2000, when the Pew Research Center asked adults in the United States whether they thought it was more important to protect gun rights or control gun ownership, the first option was chosen by 38 percent of self-identified Republicans and 20 percent of self-identified Democrats.\(^1\) By 2018, that 18-point gap exploded into a 57-point divide, as more than 75 percent of Republican respondents prioritized gun rights.\(^2\) Furthermore, under 30 percent of

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2. See id.; see also Morris Fiorina, Unstable Majorities: Polarization,
Republicans indicated that gun laws should be stricter, while 80 percent of Democrats said so. For all the contemporary challenges that opinion polling faces, these splits are remarkable.

In contrast, public responses to some particular gun policy ideas are somewhat more unified and arguably more stable. In Gallup polls, support for laws that would generally ban handgun possession has not exceeded 30 percent for more than a decade, while support for mandatory background checks for all gun purchases hovers around 90 percent. Other policies are subject to long-term disagreement between more-similarly sized groups. Prohibitions against semi-automatic assault rifles are arguable examples.

PARTY SORTING, AND POLITICAL STALEMATE 56–57 (2017) (noting that the apparent partisan sorting on this question “seems to be entirely a matter of Republicans becoming more supportive of gun rights,” while emphasizing that such sorting remains “far from perfect”). Over this 18-year period, Democrats topped out at 30 percent prioritizing gun rights in 2008.


4. We want to be cautious about recent public opinion polling based on telephone surveys, for which response rates have fallen. Perhaps that trend helps explain the seemingly increasing D-R gap on abstract values questions observed in the text—whatever might be the impact on results for more specific policy proposals. On the prospects for telephone-based survey research in general, see Amer. Ass'n for Public Opinion Res. Task Force, The Future of U.S. General Population Telephone Survey Research (Apr. 25, 2017), available at https://www.aapor.org/Education-Resources/Reports.aspx.


7. See Megan Brenan, Snapshot: Majority in U.S. Now Oppose Ban on
Perhaps party identification is becoming more tightly correlated with these policy responses as well. Thus in Gallup polling on support for assault weapon bans, what was a D-R gap of approximately 15 percent in earlier years is about 25–30 percent in more recent years. \textsuperscript{8} Still, these party divides on policy options look smaller than what we see in generic values responses. \textsuperscript{9}

In countless respects, judges are nothing like poll respondents. Their job is not, or not only, to answer abstract questions of value. Or to answer hard questions of policy spontaneously and without real-world impact. Judges decide real and concrete cases, whatever else they do. They are legal professionals who confront a special range of issues, only some of which overlap with policy debates that are familiar to the country at large. \textsuperscript{10} We should not be surprised that empiricists do not always find patterns in judicial behavior that track conventional D-R or left-right divisions. \textsuperscript{11}

One might nonetheless expect that constitutional litigation over gun policy would divide judges along ideological lines, just as

\textit{Assault Rifles} (Oct. 19, 2018) (showing support and opposition generally falling within the 40–60 percent band in episodic polling from 1996–2018).

\textsuperscript{8} See \textit{id.}. People on either side of this policy question might be more perfectly sorting themselves into different parties over time and (or instead) party affiliation might be influencing policy responses more strongly today. See \textit{Fiorina}, \textit{supra} note 2, at 45–48 (depicting the difference between people with stable ideological positions sorting into different parties, and people’s ideological positions changing such that extreme views become more common). In any event, many poll respondents continue to identify as moderates and independents, and to choose middling policy positions. \textit{See id.} at 23–27.

\textsuperscript{9} Possibly the specific-policy kind of question elicits more thoughtful and reliable responses, at least from attentive survey participants. One might want to consider whether the falling response rate for telephone survey research is yielding a larger fraction of attentive respondents, who are less likely to divide into partisan camps on specific policy questions. In this essay, we make tentative suggestions about trends in public opinion on gun policy, along with intellectually provocative comparisons with judge voting behavior over time. We thank Stephen Ansolabehere and Nate Persily for helping us think about the possibilities.

\textsuperscript{10} See Frederick Schauer, \textit{Foreword: The Court’s Agenda—And the Nation’s}, 120 Harv. L. Rev. 4, 8–9 (2006) (investigating gaps between the judiciary’s litigation agenda and the nation’s policy agenda).

\textsuperscript{11} In \textit{Lee Epstein et al.}, \textit{The Behavior of Federal Judges: A Theoretical and Empirical Study of Rational Choice} 159, 168 (2015), the reported voting difference between Democratic and Republican appointees to the U.S. Courts of Appeals, across all case categories, is around 6 percent.
we can perceive judicial divides over, say, abortion rights and affirmative action. And yet, in our original study of gun rights cases decided in the federal courts of appeals from 2008 through early 2016, no ideological divide between judges was apparent.\footnote{12} Lower court judges seemed to vote more or less independently of our proxies for party affiliation and policy preferences in the early years after \textit{District of Columbia v. Heller}\footnote{13} and \textit{McDonald v. City of Chicago}\footnote{14}—despite having little guidance on how to evaluate these claims. Those judges were not responding to gun rights claims like Americans were responding to either abstract or specific poll questions about gun policy.

With more recent data, however, we cannot as easily suggest that judges are special in gun rights struggles. In our updated dataset, the party of the appointing president is now predictive of judge votes in civil gun rights cases. So are two other variables that attempt to measure judge ideology. Our results hold with the inclusion of several control variables. We are fairly confident, moreover, that this development is not simply the consequence of an increased number of observations.\footnote{15} In addition, the gap might well result from Democratic appointees becoming less likely to support gun rights claims over time. On average, however, President Obama’s appointees do not seem to vote differently from other Democratic appointees. Replacement of older with newer judges is not plainly the cause of the new divide.

In this essay, we will not present confident explanations. There is not enough evidence, for example, to know whether judicial attitudes have changed, whether the character of challenged regulation has changed, or whether litigation behavior has changed. Perhaps the civil claims that judges face have become more divisive in some respect. Perhaps Republican

\footnote{12} See infra Part I. We use the phrase “gun rights” claims loosely to include federal constitutional claims of right to keep and/or bear arms under the Second Amendment and the Fourteenth Amendment. Firearms are not necessarily involved.

\footnote{13} 554 U.S. 570, 635–36 (2008) (invalidating a ban on handgun possession in the home).

\footnote{14} 561 U.S. 742, 791 (2010) (plurality opinion) (addressing handgun restrictions in Chicago similar to those declared invalid in \textit{Heller}).

\footnote{15} See infra Part III.B.1.
appointees are less likely to update their constitutional positions based on mass violence or recent events more generally. Perhaps the gap will narrow in the future. Those are possibilities that we cannot rule out. Furthermore, there is no apparent difference in voting behavior across judges in criminal cases. On that side of the docket, judges are essentially unified and almost always against the asserted claims. Even on the civil side of the docket the D-R differences between judges are not as large as the abstract-question poll divides mentioned above. There is plenty of agreement to see.

But we can suggest, nevertheless, that circuit judges are beginning to look more like the rest of us when they pass judgment on the merits of gun regulations. As the Supreme Court prepares, at last, to resolve another gun rights challenge, we can wonder whether the Justices’ behavior will unsettle or extend these trends. If judicial review in this territory can be more than a law-styled replication of disagreements elsewhere in society, judges might have to work much harder to build shared commitments, at least among themselves. Or else they might consider leaving even more of the contested field of gun policy to other processes. The rest of us, no doubt, will continue to discuss, debate, and disagree either way.

I. ORIGINAL STUDY

Our original dataset included cases decided in the federal circuit courts after Heller and through February 2016. The cases were drawn from a database assembled by the Law Center to Prevent Gun Violence. After adding a number of data fields regarding each case and judge, we modeled the probability of a judge voting to support at least part of a federal constitutional

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claim on the merits.\textsuperscript{18}

Our “barebones models” included several judge characteristics plus circuit fixed effects. Our “kitchen-sink models” added a range of independent variables, some related to formal law and others not. We also separated the civil cases from the criminal and habeas cases. And we rotated in three different measures of judge ideology: (1) party of the appointing president, either Democrat or Republican; (2) Judicial Common Space (JCS) scores, which are continuous;\textsuperscript{19} and (3) Database on Ideology, Money in Politics, and Elections (DIME) scores, which are continuous and based on campaign contributions by judges before they were appointed to the bench.\textsuperscript{20} Finally, we ran the kitchen-sink model using party of the appointing president both with and without en banc cases.

Judge ideology was, as far as the data could show, a nonfactor. In none of the above models was the ideology variable statistically significant at even $p < 0.10$,\textsuperscript{21} even when restricted to civil cases where constitutional claimants are choosing their targets. We clustered standard errors at the individual judge level in those models, so switching to the case level would not have made a difference on this score.\textsuperscript{22} In contrast, all three of the judge ideology proxies were significant at $p < 0.01$ in the abortion rights,

\textsuperscript{18} See id. at 847–48 (discussing the coding of judge votes).
\textsuperscript{20} See Adam Bonica & Maya Sen, A Common-Space Scaling of the American Judiciary and Legal Profession, 25 POLITICAL ANALYSIS 114 (2017); Adam Bonica, Mapping the Ideological Marketplace, 58 AM. J. POL. SCI. 367, 379 (2014).
\textsuperscript{21} A $p$-value represents the probability that a regression coefficient’s value would appear as a matter of chance. For example, $p < 0.10$ indicates that the result is less than 10 percent likely to have appeared by chance. See ROBERT M. LAWLESS ET AL., EMPIRICAL METHODS IN LAW 233–34 (2010) (noting the $p < 0.05$ convention for statistical significance); cf. Philip J. Cook & Jens Ludwig, Aiming for Evidence-Based Gun Policy, 25 J. POLY ANALYSIS & MGMT. 691, 694 (2006) (contending that the standard of statistical significance for policy-relevant evidence should account for information availability and expected costs and benefits).
\textsuperscript{22} Clustering at the case level would simply increase the standard errors and make each variable even less likely to reach conventional levels of statistical significance.
affirmative action, and establishment of religion case sets.\footnote{Judge ideology was not significant in the full set of commercial speech cases, either. For nuanced findings with respect to subsets of those cases, see Samaha & Germano, supra note 17, at 882–91.} Gun rights cases were different.\footnote{See id. at 865 (“Perhaps a partisan gap in judge votes will grow and become meaningful if and when gun rights claims get further off the floor.”).}

In a subsequent study, we added cases through the end of 2016 and compared civil gun rights claims to other types of constitutional claims.\footnote{See Adam M. Samaha & Roy Germano, Is the Second Amendment a Second-Class Right?, 68 DUKE L.J. ONLINE 57 (2018).} That study helped vet the assertion that judges are turning the Second Amendment into a “second-class right,” as Justice Thomas and others put it.\footnote{See, e.g., Silvester v. Becerra, 138 S. Ct. 945, 945 (2018) (Thomas, J., dissenting from denial of certiorari) (charging lower courts and the Supreme Court with treating the Second Amendment as a disfavored right); Peruta v. Cty. of San Diego, 742 F.3d 1144, 1179 (9th Cir. 2014) (O'Scannlain, J.), on reh'g en banc, 824 F.3d 919 (9th Cir. 2016), cert. denied, 137 S. Ct. 1995 (2017); see also David B. Kopel, Data Indicate Second Amendment Underenforcement, 68 DUKE L.J. ONLINE 79, 79 (2018) (“[I]n some circuits, the right to bear arms is not merely underenforced; the right is nullified.”); Linda Greenhouse, Op-Ed, A Call to Arms at the Supreme Court, N.Y. TIMES, Jan. 3, 2019 (“[I]t is Justice Thomas who has taken up the phrase as a weapon, using it in a series of opinions over the past four years to accuse his colleagues of failing in their duty . . . .”).} We did find statistically significant differences between the success rates of civil gun rights claims and the other claim types in our data. But we foregrounded several plausible explanations aside from judicial preferences.\footnote{See Samaha & Germano, supra note 25, at 65–69.} These explanations include variations in the assertiveness of regulators and their opponents.\footnote{See id. at 59–60.} Nonetheless, we explored differences in voting patterns between Republican and Democratic appointees. And we suggested that the underperformance of gun rights claims might have been more attributable to Democratic appointees than to their Republican colleagues.\footnote{See id. at 66–67 (observing that both parties’ appointees were more likely to support challenges to affirmative action than to gun regulation). Our research focus there, however, was not the influence of judge ideology within each case set.}
II. NEW OBSERVATIONS

Our updated data reach into mid-December 2018 \((n = 897\) judge votes). We collected post-February 2016 cases by searching for published and unpublished circuit court opinions that reached the merits of federal constitutional claims to keep and bear arms.\(^{30}\) We retained our concept of cases decided “on the merits.”\(^{31}\) Thus we excluded judge votes that were based on justiciability or procedure, as well as votes to deny or grant rehearing or rehearing en banc. In contrast, we included votes on applications for stays pending appeal. This approach likewise includes a number of other situations in which claims are not resolved with finality on appeal, such as opinions that rule on motions for summary judgment or preliminary injunctions where additional proceedings are possible in the district courts. Note as well that we include the votes of district judges who were sitting by designation on circuit court panels \((n = 32\) judge votes).\(^{32}\)

As before, we coded each judge vote as for the claim(s) in full, for the claim(s) in part, or not for the claim(s) even in part.\(^{33}\) For our analysis here, we collapse judge votes for claims in full and in part. That is, a judge is counted as voting “for” if he or she supported at least part of at least one relevant claim on the merits.

30. A codebook is available from the authors upon request. Our intentionally overinclusive Westlaw search, within the U.S. Courts of Appeals Cases database, was adv: “second amendment” “right #to bear arms” “right #to keep #and bear arms”. For the time period after February 2016 until mid-December 2018, that search returns 164 cases from which we excluded 97 cases as not reaching gun rights claims on the merits.

Our working conception of a Second Amendment and gun rights claim was broad enough to include a case in which the plaintiff invoked the Amendment in seeking tighter regulation of firearms on campus. See Glass v. Paxton, 900 F.3d 233, 244 (5th Cir. 2018). On the other hand, we have thus far excluded claims that law enforcement’s stop-and-frisk authority is limited by Second Amendment related concerns. See United States v. Robinson, 846 F.3d 694, 707 (4th Cir.) (en banc) (Harris, J., dissenting), cert. denied, 138 S. Ct. 379 (2017).

31. See Samaha & Germano, supra note 17, at 847 & n.121.

32. We likewise left in one vote cast by a judge of the Court of International Trade who was sitting by designation, as well as three votes cast by retired Justices O’Connor and Souter while riding circuit. The latter three votes will drop out of the analysis where certain judge-characteristic variables are included in the regression model.

33. See Samaha & Germano, supra note 17, at 847.
in the case at hand. Many cases are understood by judges to present only one constitutional claim, some cases multiple claims. We have simplified those differences by leaving claims within single-case packages that are up for one vote by each judge.

Furthermore, we have added cases decided during the timeframe of our original study but that were later identified and generously shared by Eric Ruben and Joseph Blocher. These cases were not in the Law Center’s database when we conducted our original analysis. The bulk of them are criminal cases (n = 111 judge votes)—most of them unpublished and none attracted judge votes for the gun rights claims at issue. But a few were civil cases (n = 15 judge votes) and gun rights claimants had some success there. Those observations would not have changed our original conclusions about judicial ideology in gun rights cases, as we will show below. As well, those observations were included in our subsequent study that compared the success of gun rights claims with other claims. In any event, we are fairly confident that our updated dataset is at least close to comprehensive within our chosen timeframe, court type, and subject matter.

The most recent case in the dataset is now Association of New Jersey Rifle and Pistol Clubs, Inc. v. Attorney General, which is a divided panel decision from December 2018. There the Third Circuit upheld New Jersey’s recently enacted prohibition against large capacity magazines. An interest group and two of its


35. The most important omission from the original study was the initial panel decision in Peruta v. County of San Diego, 742 F.3d 1144 (9th Cir. 2014), which would have invalidated the County’s good cause policy for concealed carry permits in light of other state law restrictions on public carry. See id. at 1179. Judge Thomas dissented. See id. at 1199; cf. Richards v. Prieto, 560 F. App’x 681, 682 (9th Cir. 2014) (No. 11-16255) (following Peruta in Yolo County); id. (Thomas, J., concurring) (same). Peruta was reheard en banc, after the timeframe of our original study. See Peruta v. Cty. of San Diego, 824 F.3d 919 (9th Cir. 2016) (en banc).

36. See Samaha & Germano, supra note 25, at 62.

37. 910 F.3d 106 (3d Cir. 2018).

38. See id. at 110 (affirming the denial of a preliminary injunction against a statute that outlaws firearm magazines that hold more than ten rounds of ammunition).
members filed suit to challenge the new state statute. After the district court denied the claimants’ request for a preliminary injunction, the claimants appealed and the majority affirmed that order.\textsuperscript{39} Judge Shwartz, joined by Judge Greenaway, pointed to evidence that mass and active shootings had increased in recent years,\textsuperscript{40} and concluded that the statute satisfied intermediate scrutiny on the claimants’ Second Amendment challenge.\textsuperscript{41} Hence these two judges are coded as having not voted for the claim(s) even in part. In dissent, Judge Bibas voted to apply strict scrutiny, enjoin the statute preliminarily, and remand for additional evidence.\textsuperscript{42} He is coded as having voted for the claim(s) either in full or in part.

III. NEW RESULTS

A. Criminal Cases

On the criminal side of the docket ($n = 554$), there is no apparent relationship between ideology and judge votes. Democratic and Republican appointees almost always voted against all aspects of the gun rights claims that they faced on the merits in criminal and habeas cases. The fraction of judge votes in favor of even part of a gun rights claim is under 4 percent for both of the parties’ appointees, and the difference between them is less than a percentage point.

The vast majority of these criminal and habeas cases involved challenges to federal government action. One might then wonder whether the federal character of these cases is what dissuades judges from supporting gun rights claims. But it seems that the criminal rather than the federal character of these cases is associated with poor success rates. We combined all civil and criminal observations and ran our original barebones model, plus

\textsuperscript{39} See id. at 110, 126.

\textsuperscript{40} See id. at 110. An introduction to data on mass shootings, including different measures and disagreement over whether they are increasing, is Rosanna Smart, Mass Shootings: Definitions and Trends, RAND CORP., https://www.rand.org/research/gun-policy/analysis/supplementary/mass-shootings.html (last visited Jan. 14, 2019).

\textsuperscript{41} See N.J. Rifle & Pistol Clubs, 910 F.3d at 122.

\textsuperscript{42} See id. at 126–27 (Bibas, J., dissenting).
dummy variables for criminal cases and for challenges to federal action. The coefficient for criminal case is negative and statistically significant ($p = 0.000$), while the federal action variable is not statistically significant ($p = 0.339$).

B. Civil Cases

Accordingly, the civil side of the docket ($n = 344$) is a different story. The overall fraction of judge votes in favor of all or part of gun rights claims is 22 percent, including en banc cases. Democratic appointees voted for at least part of a gun rights claim just 10 percent of the time, while the comparable figure for Republican appointees is 35 percent. That 25 percent D-R voting gap in civil gun rights cases is not as large as what we see in our abortion rights and affirmative action case sets during the same timeframe (2008–2018), but it is larger than the D-R gap in our establishment clause case set.

A first step in investigating time trends is simply to graph the votes of Democratic and Republican appointees by year. Figures 1 and 2 display fractions of judge votes for gun rights claims in civil cases, with and without the en banc cases. The dotted line in the middle represents the overall fraction of judges who voted in favor of gun rights claims. For the most part, Democratic and Republican appointees voted similarly between 2009 and 2013. A D-R gap then emerges in 2014, narrows in 2015, and widens thereafter. The D-R gap is more pronounced in 2016 and 2017 when en banc cases are included, but the somewhat jagged trend lines are broadly similar in both figures.

43. That number is 18 percent for three-judge panels.
Figure 1: Judge votes for civil gun rights claims (3-judge panels).

Figure 2: Judge votes for civil gun rights claims (with en banc).
1. Barebones models revisited

We ran a series of logit and ordinary least squares (OLS) regressions to explore whether ideology has a systematic effect on how judges vote in civil gun rights cases. Our original study was limited to logistic regressions. There is reason to reconsider this approach, however, due to concerns that logit is not a consistent estimator in models with fixed effects.\(^4\) Because our models include fixed effects for each circuit court, we believe that OLS is the better estimator. It is conceivable that the two estimators would produce drastically different results, but in our case this did not happen. With few exceptions, levels of statistical significance and the signs and relative magnitude of coefficients are similar whether we run logistic or OLS regressions. For the sake of clarity and brevity, our regression tables only report the OLS results.

Table 1 compares results for our barebones specification. As in our original study,\(^4\) we ran a model with a proxy for judge ideology and variables to control for various judge characteristics. While the judge ideology variable was not statistically significant in our original study (Model 1), it is significant \((p = 0.000)\) in our expanded dataset of civil cases spanning 2008–2018 (Model 3). The coefficient on the judge party variable is much larger in the expanded sample \((0.248)\) compared to the original sample \((0.0832)\)—nearly triple the size.

These new results are not fully explained by the newly added cases that were decided before February 2016. When we add only those observations to the original dataset and not any later observations (Model 2), the coefficient on the judge party variable does increase \((0.125)\) but it is not significant at \(p < 0.05\) \((p = 0.075)\). Contrast the result when the data is restricted to the newly added civil cases, whether decided before or after our original dataset ended: The barebones model yields a large coefficient for judge party \((0.376)\) that is statistically significant \((p = 0.000)\).\(^4\)


45. See Samaha & Germano, *supra* note 17, at 867 tbl. A.1).

46. We have not run the barebones model restricted to the post-February 2016 observations.
In each of the foregoing models, we followed our original study by clustering standard errors on individual judges and by including en banc cases in the sample. But we find that the large and significant effect of the judge party variable in Model 3 holds even when we make more statistically conservative choices. The judge party variable remains significant at $p < 0.01$ for the full updated dataset of civil cases even when we cluster standard errors on cases and exclude en banc observations.

Table 1: Barebones models of circuit judge votes for gun rights claims, in full or in part—civil cases.

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<td>0.125*</td>
<td>0.248***</td>
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*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors, clustered on judges, are in parentheses. The First Circuit is the baseline for circuit fixed effects.
2. New models

To learn more, we constructed new models. These models examine all observations on the civil side of the dataset and then split the sample into two time periods. The most conservative models cluster errors at the case level and exclude en banc observations. En banc cases are selected by the circuit judges for rehearing as a group, they tend to be more important or controversial, and they permit the circuit judges to overrule in-circuit precedent. All of that suggests additional room for extralegal preferences to influence votes.

In the new models, we reduce the number of judge-characteristic variables and introduce a limited set of non-judge variables. Beyond judge party, we add age and gender. Perhaps an older generation of judges is less open to gun rights litigation, and perhaps women are less persuaded by these claims than men. A recent Gallup poll indicates that female respondents are more likely than male respondents to support stricter regulation of gun sales. We include a measure for one type of panel effect as well: the fraction of the other judges on the panel who are Republican appointees.

Furthermore, we introduce a continuous variable related to each circuit’s propensity to affirm on the merits. Relying on data from the Federal Judicial Center, we associated each case with the fraction of appeals affirmed on the merits in the relevant circuit during the relevant calendar year—using 1 minus that fraction when the gun rights claimant was the appellant. This variable accounts for the fact that the circuit courts tend to affirm, which indicates that the appellant is likely to lose all else equal, while allowing for gradations in the likelihood of affirmance across circuits and across time.


48. Possible explanations include formal standards of review, deference to trial judge work, trial judge votes being predictive of appellate judge votes, and appellants facing low incremental costs for appeals. See Kevin M. Clermont & Theodore Eisenberg, Litigation Realities, 88 CORNELL L. REV. 119, 150–52 (2002). Claimants are the appellants in a remarkably large fraction of gun rights cases. See Samaha & Germano, supra note 25, at 65.
As for the claimants, we include dummy variables for whether one or more claimants is a *business* (such as a licensed firearms seller) or an *interest group* (such as the National Rifle Association). Businesses and organized interests might be relatively sophisticated or well-resourced claimants.

As for their targets, we use a dummy variable for whether *federal government action* of some kind is challenged, as opposed to state or local action. The federal government might well perform better in litigation than state and local governments.\(^{49}\) One reason is disparity in litigation resources when the federal government is a party to the suit, which we are not coding directly. Another reason might be that national politics tend to yield policies and practices that are less extreme, more carefully advised, and easier to defend in court. However, it could be that claimants and lawyers select their targets such that any differences across levels of government will not reappear in cases that are filed and appealed.\(^{50}\)

We also include the variable for recent *Supreme Court decisions* that we built for our original study.\(^{51}\) It is a continuous variable that represents positive or negative messages from the Supreme Court regarding the relevant constitutional claim type within two-year time frames. Since *Heller*, the Court has issued only two opinions on the merits of the right to keep and bear arms, one of them a short per curiam.\(^{52}\) But perhaps those decisions influenced subsequent judge votes in lower courts, at least for a period of time. Over time, regulators and potential constitutional claimants may adjust their behavior.

Finally, as we did in our original study, we include circuit fixed effects to account for unmeasured variation in the regulation, litigation, and adjudication practices across circuits.

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\(^{49}\) See Samaha & Germano, *supra* note 25, at 64. We corrected one entry from our original dataset on this variable: The unsuccessful challenge in Schrader v. Holder, 704 F.3d 980 (D.C. Cir. 2013) was to federal action rather than state action.

\(^{50}\) See Samaha & Germano, *supra* note 25, at 64.

\(^{51}\) See Samaha & Germano, *supra* note 17, at 854 (detailing this score).

\(^{52}\) See Caetano v. Massachusetts, 136 S. Ct. 1027, 1028 (2016) (per curiam) (rejecting three reasons for upholding a stun-gun ban and remanding); McDonald v. City of Chicago, 561 U.S. 742, 791 (2010) (plurality opinion) (incorporating gun rights into the Fourteenth Amendment).
a. All civil cases

Table 2 shows OLS coefficients with standard errors clustered at the case level. Models 1.1 and 1.2 cover all years in our data for civil gun rights cases, 2008–2018.\textsuperscript{53} The judge party variable is statistically significant ($p = 0.000$) in these samples. This result holds whether or not en banc cases are included, and whether errors are clustered on judges or cases. In comparable logit models, we found that the log-odds coefficients on judge party in these civil gun rights cases (Model 1.1: $2.087$; Model 1.2: $1.883$) are roughly similar to our earlier findings for judge votes in abortion rights, affirmative action, and establishment clause cases under our kitchen-sink models for the applicable timeframes.\textsuperscript{54}

b. Old versus new cases

Dividing observations into two time periods helps us search for trends. More than one defensible choice can be made on the timeframes of interest, of course. For this initial inquiry, we use the 2016 presidential election year as a dividing line. Although gun policy has been foregrounded in more than one election cycle, the 2016 presidential election is viewed by many as especially divisive on a range of issues. Choosing this dividing line, moreover, yields a roughly similar number of observations in the earlier and later subsets for the three-judge panel decisions. Models 2.1 and 2.2 report results for the older cases, while Models 3.1 and 3.2 cover the newer cases.

For 2008–2015, the results are quite consistent and similar to the results in our original study. The judge party variable is not statistically significant for civil gun rights claims, whether we exclude en banc cases (Model 2.1: $p = 0.113$) or include them (Model 2.2: $p = 0.479$), and whether we cluster standard errors at the judge level or case level. Thus the early gun rights civil cases do not show clear signs of ideological voting.

For 2016–2018, the results tend toward the opposite direction.

\textsuperscript{53} There are no observations for 2008 or 2010. \textit{Heller} was not handed down until June 2008, and the circuit courts probably held gun rights cases while \textit{McDonald} was pending until it was issued in June 2010.

\textsuperscript{54} See Samaha & Germano, supra note 17, at 874, 880 (Tables B.1 and B.4).
Looking at all civil gun rights cases decided in this later time period (Model 3.2), the judge party variable is statistically significant ($p = 0.000$), whether errors are clustered on judges or cases. And the coefficient on judge party—that is, the difference in the probability that a Republican appointee voted for the claim compared to a Democratic appointee—is substantively large (0.382). Results are similar when we analyze a subsample that excludes en banc cases (Model 3.1)\textsuperscript{55}. For three-judge panel decisions from 2016–2018, the coefficient on judge party is once again statistically significant ($p = 0.007$). True, that coefficient is smaller without the en banc cases (0.210). But Republican appointees remain significantly and notably more likely to have voted for civil gun rights claims, even controlling for the other variables in our model.\textsuperscript{56}

Figure 3 helps us visualize these differences over time. It shows the probability that a Republican or Democratic appointee voted in favor of a civil gun rights claim in (a) the full 2008–2018 period, (b) the early 2008–2015 period, and (c) the later 2016–2018 period. The dots represent the predicted probability while the bars are 95 percent confidence intervals. All control variables are held at their means. These probabilities are limited to the three-judge panel estimates from Models 1.1, 2.1, and 3.1.

What we see in this figure is reminiscent of what we saw in Figure 1, which displayed growing raw-percentage differences between Republican and Democratic appointees on three-judge panels during recent years. Over the entire 2008–2018 period, Democratic appointees had a 0.09 probability of voting in favor of the claim compared to 0.27 for Republican appointees. But the gap was not so large over the 2008–2015 period (0.15 compared to 0.29), and the overlapping confidence intervals indicate that the difference may not be statistically different from zero. As our

\textsuperscript{55} In the 2016–2018 period, en banc votes amounted to one-third of all observations in the civil gun rights case set. That figure is only 7 percent for 2009–2015.

\textsuperscript{56} A logit regression for Model 3.1 (2016–2018, no en banc) with circuit fixed effects ends up losing a large number of observations (37 of 133 dropped) and yielding large standard errors. The coefficient on the judge party variable remains large but the $p$-value increases to 0.055. If circuit fixed effects are omitted, judge party is again significant at $p < 0.01$ using logit.
regression results showed, the gap between Democratic and Republican appointees grew after 2015. During 2016–2018, the probability of voting for the claim fell to 0.04 for Democratic appointees compared to 0.25 for Republican appointees—a difference that is statistically significant. The figure shows that much of the gap widening is due to a drop in Democratic appointees’ support for civil gun rights claims.

It is possible that our regression results depended upon our choice of ideology variable. As a robustness check, we ran the same models but switched out judge party for the other two proxies for judge ideology.

The headlines on ideological voting are generally consistent. For instance, in civil cases for all years the coefficients on judge party, JCS scores, and DIME scores have the expected sign and are statistically significant ($p < 0.01$), whether en banc cases are included or excluded.57 In civil cases decided from 2016–2018, again we see a pattern similar to when we ran the models with the judge party variable. In the sample that includes cases decided en banc, the coefficients on JCS and DIME are relatively large, positive, and statistically significant ($p < 0.01$). When we exclude en banc cases, signs and levels of statistical significance remain the same, but the substantive effect of ideology decreases somewhat, just as it did when we ran the models with the judge party variable. For the earlier 2008–2015 timeframe, none of the three judge ideology variables are statistically significant, whether en banc cases are included or excluded.

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57. As of this writing, neither JCS nor DIME scores are available for President Trump’s judicial appointees. Our analysis necessarily drops that handful of observations when we use either of those variables.
### Table 2: OLS models of judge votes for gun rights claims, in full or in part—civil cases.

<table>
<thead>
<tr>
<th>Independent Variables</th>
<th>(1.1)</th>
<th>(1.2)</th>
<th>(2.1)</th>
<th>(2.2)</th>
<th>(3.1)</th>
<th>(3.2)</th>
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<tr>
<td>Judge party</td>
<td>0.185***</td>
<td>0.250***</td>
<td>0.146</td>
<td>0.0731</td>
<td>0.210***</td>
<td>0.382***</td>
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<tr>
<td></td>
<td>(0.0500)</td>
<td>(0.0581)</td>
<td>(0.0904)</td>
<td>(0.102)</td>
<td>(0.0738)</td>
<td>(0.0763)</td>
</tr>
<tr>
<td>Party panel effect</td>
<td>0.106</td>
<td>0.0631</td>
<td>0.0211</td>
<td>-0.103</td>
<td>0.145</td>
<td>0.128</td>
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<tr>
<td></td>
<td>(0.0778)</td>
<td>(0.0833)</td>
<td>(0.171)</td>
<td>(0.195)</td>
<td>(0.101)</td>
<td>(0.108)</td>
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<tr>
<td>Judge age</td>
<td>-0.000915</td>
<td>2.45e-05</td>
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<td>0.000176</td>
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<tr>
<td></td>
<td>(0.00164)</td>
<td>(0.00154)</td>
<td>(0.00298)</td>
<td>(0.00311)</td>
<td>(0.00203)</td>
<td>(0.00180)</td>
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<tr>
<td>Judge gender</td>
<td>0.0277</td>
<td>0.0208</td>
<td>0.0845</td>
<td>0.0796</td>
<td>-0.00562</td>
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<tr>
<td></td>
<td>(0.0417)</td>
<td>(0.0491)</td>
<td>(0.0696)</td>
<td>(0.0701)</td>
<td>(0.0520)</td>
<td>(0.0648)</td>
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<tr>
<td>Cir. affirmance score</td>
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<td>0.414</td>
<td>0.548</td>
<td>-0.337</td>
<td>-0.192</td>
<td>0.395</td>
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<tr>
<td></td>
<td>(0.261)</td>
<td>(0.303)</td>
<td>(0.590)</td>
<td>(0.658)</td>
<td>(0.239)</td>
<td>(0.377)</td>
</tr>
<tr>
<td>Business</td>
<td>0.398**</td>
<td>0.224*</td>
<td>0.484*</td>
<td>0.165</td>
<td>0.288</td>
<td>0.190</td>
</tr>
<tr>
<td></td>
<td>(0.155)</td>
<td>(0.134)</td>
<td>(0.255)</td>
<td>(0.255)</td>
<td>(0.173)</td>
<td>(0.142)</td>
</tr>
<tr>
<td>Interest group</td>
<td>0.106</td>
<td>0.168***</td>
<td>0.229</td>
<td>0.350**</td>
<td>0.110**</td>
<td>0.154**</td>
</tr>
<tr>
<td></td>
<td>(0.0735)</td>
<td>(0.0623)</td>
<td>(0.164)</td>
<td>(0.174)</td>
<td>(0.0545)</td>
<td>(0.0629)</td>
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<tr>
<td>Fed. gov’t challenged</td>
<td>-0.00827</td>
<td>0.0727</td>
<td>0.0730</td>
<td>0.106</td>
<td>-0.0771</td>
<td>-0.0229</td>
</tr>
<tr>
<td></td>
<td>(0.0816)</td>
<td>(0.0847)</td>
<td>(0.153)</td>
<td>(0.167)</td>
<td>(0.0681)</td>
<td>(0.0923)</td>
</tr>
<tr>
<td>S. Ct. case score</td>
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<td>0.571</td>
<td>0.310</td>
<td>-0.309</td>
<td>0.888</td>
</tr>
<tr>
<td></td>
<td>(0.280)</td>
<td>(0.388)</td>
<td>(0.363)</td>
<td>(0.400)</td>
<td>(0.806)</td>
<td>(0.900)</td>
</tr>
<tr>
<td>Constant</td>
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<td>-0.261*</td>
<td>-0.107</td>
<td>-0.0125</td>
<td>-0.0734</td>
<td>-0.175</td>
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<tr>
<td></td>
<td>(0.161)</td>
<td>(0.135)</td>
<td>(0.246)</td>
<td>(0.249)</td>
<td>(0.169)</td>
<td>(0.151)</td>
</tr>
</tbody>
</table>

Circuit fixed effects: Yes, Yes, Yes, Yes, Yes, Yes
Observations: 265, 343, 132, 143, 133, 200
R-squared: 0.331, 0.259, 0.368, 0.239, 0.379, 0.374

*** p < 0.01, ** p < 0.05, * p < 0.10. Robust standard errors, clustered on cases, are in parentheses.
Figure 3: Estimated probabilities of judge votes for gun rights claims—civil cases, three-judge panels only.
IV. REFLECTIONS

In criminal cases, gun rights claims almost always fail and judge votes cannot be reliably predicted by the party of the appointing president. Those findings are consistent with our original study. In civil cases, however, the situation is different—and changing. Our extended data indicate an emerging and increasing divide between Democratic and Republican appointees in civil gun rights cases. Two other proxies for judge ideology deliver the same message. The divide appears to have opened within the last three to five years of our data. More than one possible explanation is available and the explanations are not all mutually exclusive. At this point, we can only offer suggestions. We can group them into three categories.

▪ (1) More precedent, more discretion. One possibility involves the accumulation of precedent. Some observers have suggested that an increasing stock of judicial precedent offers something for everyone, allowing or even prompting judges to cherry pick the authorities that they prefer.58 This notion that more precedent yields more discretion might be reflected in the increasing influence of judicial ideology on case outcomes over time. The logic behind this hypothesis is contestable, however,59 and the empirical support for the theory has been questioned recently.60 Further testing of the idea is welcome. In any event, it is worth asking whether litigants select different types of disputes over time in ways that reopen judicial discretion—even if or because judges feel more constrained as the stock of precedent accumulates.

▪ (2) Issues changing. Following that thought, a second possibility is that the litigated issues have changed. New firearms
regulations have been adopted in some jurisdictions after high-profile mass shootings in Newtown, Charleston (South Carolina), Orlando, Las Vegas, and elsewhere.61 Perhaps some of the new regulations are more debatable in constitutional law or policy terms than were the regulations litigated in the past. Perhaps some federal judges are responding differently to those debates in accord with their stable constitutional or policy orientations.

Furthermore, perhaps litigation behavior has changed. The more-assertive gun rights claimants and their attorneys might have been deterred by mounting litigation losses, or everyone might have learned better what makes for easy and hard cases in court. Regulators and claimants can thereby generate a more closely matched set of claims for adjudication.

However, our data do not clearly show an increasing likelihood of votes for gun rights claims from their arguably low baseline success rate. An upward trajectory could have reinforced the notion of selection toward hard cases, given that claimants were not succeeding very often in the earlier timeframe. That pattern is not apparent. Figure 3 indicates that, if anything, both Democratic and Republican appointees became less likely to support gun rights claims over time. If these estimates are about right, then a prominent challenge is to explain why Democratic appointees were even less likely to vote for gun rights claims during 2016–2018.62

Litigated issues are undoubtedly changing to an extent. The question is whether the issues that judges face are becoming more divisive, whether or not they are becoming more persuasive overall. Maybe so. We observed above that the latest judge votes in our data come from the New Jersey Rifle case, a divided panel decision on large capacity magazines.63 The statute under challenge was enacted in 2018. The two judges in the majority were appointed by President Obama, the dissenter by President Trump. For what it’s worth, the earliest judge votes in the data unanimously reject the jury instruction demands of a felon who

62. None of this rules out selection toward hard cases. Other developments could swamp the selection effect such that we cannot measure it with our data.
63. See supra text accompanying notes 40–44.
allegedly possessed dozens of firearms, including rifles in the AK family and, in the past, explosives. Felon-in-possession restrictions go back decades and are not a subject of much controversy today.

Those are just two cases in a large universe of observations, and available data are not precise enough to track each important issue trend. Our case subset codes do show a few signs of movement that might be worth investigating. For instance, the fraction of judge votes on sales regulations seems to grow after 2015 (from 0.00 to 0.11), while the share of votes on location restrictions seems to shrink (from 0.20 to 0.04). Perhaps these case subsets correlate with more and less ideologically divisive issues for judges.

But, it seems to us, those issue categories are very unlikely to provide a complete explanation for the emerging judicial divide. Including dummy variables to control for all of our case subset codes does not change the basic results for judge ideology in Table 2. We also interacted judge party with a dummy variable identifying sales restrictions claims, and separately with a dummy variable identifying weapons restrictions claims—which includes assault weapons bans and large capacity magazine limits. Neither of those interactions yielded results indicating that Democratic and Republican appointees were especially driven apart when they faced those types of regulation.


65. These observations rely on the principal case subset code for each judge vote in civil gun rights cases, including en banc cases.

66. There are ten subset codes for our gun rights case data. The codebook is available from the authors upon request. In brief, those ten codes are: (201) felon-in-possession restrictions; (202) other persons restricted from firearms possession including domestic violence misdemeanants; (203) weapons restricted including assault weapons and large capacity magazines; (204) locations restricted including government property and parks; (205) carry-related regulations including concealed carry, open carry, and public carry; (206) registration regulations including marked-weapon requirements; (207) licensing regulations including background checks, training, and firing range restrictions; (208) criminal sentencing; (299) sales regulations; (299) other.
That said, this is a complex area of behavior on which we have little direct data to analyze at the moment. Indeed litigation selection effects can influence the divisiveness of the docket even if we observe no difference in the frequency of these somewhat broad case subset codes.

- **(3) Judges changing.** Third and finally, judges could be changing their positions on guns. The composition of judges is always changing, albeit slowly, and even relatively senior judges can respond differently to similar arguments made at different times. Judges can learn on the job. A possibility, though, is that judges are changing their responses in line with whatever forces are affecting the public at large. In some but not all respects, ordinary persons’ reported views on gun policy have become more correlated with their party affiliations. True, many poll respondents could be changing their party affiliations while holding to their views on gun policy. But our data do not permit judges to change party. And judges could be changing their positions on certain firearms issues because of the surrounding policy, political, and communications environment.

Democratic and Republican appointees might be increasingly likely to lean toward the views of the political coalitions that brought them to office—a bit like many other people relying on cues from affinity groups to help them formulate reactions to pressing issues. Those cues might be more powerful in today’s environment. And those issues include what to do about the country’s increasingly transparent failure to achieve basic levels

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67. *Cf.* City of Erie v. Pap’s A.M., 529 U.S. 277, 310 (2000) (Souter, J., concurring in part and dissenting in part) (“I hope it is enlightenment on my part, and acceptable even if a little late.”).
68. See supra notes 1–7 and accompanying text.
69. See supra note 8 (discussing party sorting as distinct from policy preference changes).
70. JCS scores can change over time, but not because of judicial behavior.
71. On a related note, there is now evidence of somewhat more ideologically divided voting by circuit judges in the quarter preceding a presidential election. See Carlos Berdejó & Daniel L. Chen, *Electoral Cycles Among U.S. Courts of Appeals Judges*, 60 J.L. & Econ. 479, 485 (2017) (“Democratic appointees are 3.5 percentage points more likely than Republican appointees to cast a liberal vote relative to a neutral or conservative vote, but this difference increases by 3.9 percentage points before the election.”).
of safety and felt security, along with the longstanding concerns of responsible gun owners that their situations and values are at risk of disregard.\textsuperscript{72} Conceivably, for example, complaints about a “second-class right” could have backfired by motivating recent Democratic appointees to oppose gun rights claims, at least when those judges confronted claims of right to large capacity magazines, assault-style rifles, and more.

Not all of our data fit easily with this last explanation, however, and the explanation itself deserves further specification. First of all, the judge voting patterns in our gun rights cases are not simple functions of presidential or other election years, even if those events do play a role. Consider the absence of a sizeable gap between appointees in 2012 and the apparent gap in 2017 (Figures 1 and 2).\textsuperscript{73}

As well, the D-R gaps and trends in public opinion polls seem different from our judge voting data. The extraordinary D-R polling divisions over abstract priorities for gun policy appear to emerge earlier than the divisions in the judge data. Perhaps the judicial appointments process generates a lag and partial buffer between general public opinion and federal judicial perspectives. That could be part of an adequate explanation on the timing of the divides. But, in addition, some polling indicates that the increased partisan divide largely resulted from Republican respondents becoming more likely to prioritize gun rights over gun control.\textsuperscript{74} In our judge data, by contrast, the emerging divide appears to result from Democratic appointees becoming less likely to support gun rights claims over time.

In any event, we cannot confirm that new personnel is the explanation for the divide. The JCS and DIME scores of the Democratic appointees in our data do not drift leftward over time. More tellingly, President Obama’s appointees do not seem to vote

\textsuperscript{72} We have not interacted interest group participation with judge ideology. Nonetheless note that Models 3.1 and 3.2 in Table 2 show, during 2016–2018, a significant positive relationship between the presence of an interest group as a gun rights claimant and the attraction of judge votes ($p < 0.05$). The interest group variable was not significant during 2008–2015 for three-judge panels.

\textsuperscript{73} Note that our party panel effects variable is not significant in the Table 2 models.

\textsuperscript{74} See supra notes 1–2 and accompanying text.
differently from other Democratic appointees in our data. We ran our Table 2 models with a dummy variable that identifies Obama appointees. The Obama judge variable is not significant in any of these models, but the judge party variable remains significant during 2016-2018 and in the full set of cases ($p < 0.05$). So knowing that the judge was appointed by President Obama is not adding to our understanding of judge voting behavior, beyond what we can gather from judge party.

Moreover, if we drop the Obama appointees, drop the Trump appointees, or drop both from our Table 2 models, the results do not change substantively: Judge party is not significant during 2008–2015, but judge party remains significant during 2016–2018 and in the full set of cases ($p < 0.05$). Interacting judge party with the year of the judge’s commission does not change the basic picture, either. Judges who were appointed by more recent political coalitions do not seem significantly more or less likely to support gun rights claims in our data, whether those judges were appointed by Democrats or Republicans.\textsuperscript{75}

This still leaves the possibility that Democratic and Republican appointees are not themselves changing much, but that they are responding differently to current events. For example, Democratic appointees might always have been inclined to adjust their support for constitutional claims to weapons in light of apparently increasing frequencies of mass violence, while their Republican-appointee colleagues might always have been predisposed against such updating. We cannot confirm this possibility; the mix of litigated issues could be changing instead or as well, and judicial attitudes in this field might escape adequate measurement by our proxies. At the same time, there is something sensible in the idea that different groups of judges are differentially inclined to revise their legal positions as they gather new information or impressions.

In all events, we can now see a familiar divide. Recall that not every opinion poll on gun policy shows a 50-point partisan split.

\textsuperscript{75} Using the judge party * judge commission year interaction term with our Table 2 models yields modestly sized coefficients for commission year that are negative for Democratic appointees and positive for Republican appointees—but, again, these coefficients are not significant at conventional levels.
Questions about relatively specific policy proposals may elicit disagreement without correlating so strongly with party identification, even putting aside self-described independents who tend toward intermediate positions. Gallup polling on semi-automatic assault rifle bans, for instance, suggests a D-R divide of approximately 25–30 percentage points during 2016–2018.\(^76\) The partisan divide over handgun bans might now reach 30 points as well,\(^77\) while the divide over universal background checks probably is closer to 5–10 points.\(^78\)

Now compare our data on judge voting behavior. The estimates from our regressions for 2016–2018 indicate that the “D”-”R” judge gap in likelihood of support for a gun rights claim in civil litigation was 21 points. The judge votes in these cases are not merely or perhaps not at all representations of abstract value commitments. Whatever else judges accomplish when they adjudicate cases, they try to resolve concrete disputes over particular regulations. When judges do that, their overall differences on the bottom line across ideological groupings no longer look very different from those that ordinary people produce when they are asked to respond to relatively concrete policy ideas.

One might then suggest that federal judges are injecting common sense or median-voter values into our gun laws. The idea warrants attention to the extent that judges are voting like ordinary people on gun rights issues, and to the extent that political elites are more divided than ordinary people on those issues. However elite federal judges might be in some respects, perhaps the appointments process and the discipline of court proceedings and legal reason somehow shape the pattern of their decisions back toward the distribution of policy views in the

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76. See supra note 8 and accompanying text. To be clear, a similarly large gap appears in the 2012 poll. See id. The D-R gap seems to have increased from about 15 percentage points during 1996–2004, with Democratic respondent support remaining fairly stable and Republican and independent support falling off. See id.

77. See Reinhart, supra note 45 (reporting results from an October 2018 Gallup poll that indicated a 32-point D-R divide over a handgun ban with exceptions for police and authorized persons).

general population. This is a possible account of contemporary politics and judicial behavior.

But in constitutional litigation, federal judges are a special kind of voter: *late, superior, and largely negative*. Constitutional judicial review tends to arrive after an episode of ordinary policymaking, it tends to be expressed in definitive terms, at least when declaring laws unconstitutional, and it tends to impose an additional veto gate for regulation rather than require more stringent regulation. These are generalizations and they do not cover the full dynamics of constitutional litigation, but often they hold.

One resulting sketch of gun policy development in the United States is this: Relatively polarized policymakers and influence groups establish gun policy for a less-polarized general population. In only some political communities and timeframes are the regulatory forces strong enough to move formal law toward gun control.\footnote{79. Often though not always, gun policy in the United States is made at the state level and by legislatures, rather than at the federal level or by administrative agencies or local governments. Local regulations may be preempted by state law which, in many states, is protective of gun ownership. But not every state follows this model and so pro-regulatory forces may prevail in some cities and some states within some time frames, even when the opposing forces are well-organized and present a credible threat of litigation and victory in court. See generally Philip J. Cook, Jens Ludwig & Adam M. Samaha, *Gun Control After Heller: Threats and Sideshows from a Social Welfare Perspective*, 56 UCLA L. REV. 1041, 1050–57 (2009).} The gun control policies (not the gun rights policies) that survive the political process are now subject to challenge in court by individuals, businesses, nonprofits, and attorneys who are relatively committed and have relatively firm anti-regulation positions. Some of these claimants are well-resourced and they might pursue a deterrence strategy against future regulatory efforts, which means that constitutional claims with even a low likelihood of attracting judge votes can be valuable enough to litigate.

On the merits, the federal judges who adjudicate gun rights claims might well vote like a random sample of ordinary citizens—dividing on general values yet often agreeing on particular policies, and usually rejecting challenges to laws that already have been filtered through the political process. But just because those
judges would be acting as another filter on surviving policy, they would not be switching out the will of the elite in favor of the will of the people. Instead, in this sketch, it seems that federal judges would be replicating policy divisions in the population as a whole only after political elites had set the agenda and chosen the policies subject to veto in a court of law.

CONCLUSION

Party is not exactly destiny for judges deciding gun rights cases, even ignoring all criminal cases. Both Republican and Democratic appointees are far less than 50 percent likely to vote for gun rights claims in our full set of civil cases—although that number might say more about the assertiveness of constitutional claimants than the shared restraint of judges. Nor can our models explain all of the variation in outcomes, of course. But new divisions are now apparent. The most recent data indicate that, unlike the early years after *Heller*, judge ideology has become a significant predictor of judge votes in civil gun rights cases.

This development should be enough to raise serious questions for all of us. The questions include why judge voting patterns are changing, whether judges evaluate gun rights claims very differently from others who care about these issues, and, if not, the true value of constitutional litigation in this field. Constitutional debate has its place, in court and elsewhere. But its aspiration is greater than a replication of societal divisions in the form of briefs and decrees. At this moment, judicial review in gun rights cases is beginning to look more like larger disagreements in this country over gun policy proposals—disagreements with which we are quite familiar.