The Policy Basis of Measured Partisan Animosity in the United States

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Abstract: Understanding and addressing the consequences of partisan animosity requires knowledge of its foundations. To what extent is animosity between partisan groups motivated by dislike for partisan outgroups per se, policy disagreement, or other social group conflicts? In many circumstances, including extant experimental research, these patterns are observationally equivalent. In a series of vignette evaluation experiments, we estimate effects of shared partisanship when additional information is or is not present, and we benchmark these effects against shared policy preference effects. Partisanship effects are about 71% as large as shared policy preference effects when each is presented in isolation. When an independently randomized party and policy position are presented together, partisanship effects decrease substantially, by about 52%, whereas policy effects remain large, decreasing by about 10%. These results suggest that common measures of partisan animosity may capture programmatic conflict more so than social identity–based partisan hostility.

Verification Materials: The materials required to verify the computational reproducibility of the procedures and analyses in this article are available on the American Journal of Political Science Dataverse within the Harvard Dataverse Network. The data required to verify the computational reproducibility of the results are available from Dataverse, the National Election Studies website (https://electionstudies.org), and NORC at the University of Chicago, under protected access, as described on the American Journal of Political Science Dataverse, at: (https://doi.org/10.7910/DVN/RFECVH).

Partisanship in the contemporary United States is widely understood as a social identity (e.g., Green, Palmquist, and Schickler 2002), which for many individuals defines a partisan ingroup in conflict with an opposing outgroup (Tajfel 1974). Recent scholarship shows that partisan animosity expressed on surveys has grown substantially in recent decades (Iyengar, Sood, and Lelkes 2012; Mason 2015). Such animosity appears to be spilling out of the purely political realm into private interpersonal relationships (e.g., Iyengar and Westwood 2015), much as ethnic conflict has widespread political and social consequences. Growing partisan animosity is feared to reinforce political segregation, exacerbate polarization, and hinder bipartisan cooperation.

While partisanship is correlated with policy positions (e.g., views on social issues) and many other identities (e.g., race), recent scholarship has focused on the direct role of partisan orientations in shaping evaluations of other private citizens. For example, Iyengar, Sood, and Lelkes (2012) show that Americans are less excited about their child marrying a member of the opposition party than their own party. The magnitude of this difference has grown since 1960, and discrimination on the basis of partisanship may have surpassed racial discrimination in some contexts (Iyengar, Sood, and Lelkes 2012; Iyengar and Westwood 2015).

This (survey) experimental evidence provides an unbiased estimate of the total effect of partisanship on these evaluations, but what explains this effect? Some work interprets these effects through the lens of partisanship itself (e.g., Sunstein 2015), whereas other work has suggested either that partisanship proxies other social identities (e.g., Abramowitz and Webster 2018) or conveys information about issue positions that underlie increasingly
polarized elite conflict (e.g., Webster and Abramowitz 2017). Debates about the mechanisms underlying partisan animosity are difficult to diagnose from prior research because these mechanisms have not been independently manipulated. Survey items asking about a child’s marital partner make this difficulty clear. When one learns only the partisanship of a child’s potential spouse, we do not know what other inferences, such as propensity to discuss politics, survey respondents are making on the basis of that characteristic (Klar, Krupnikov, and Ryan 2018). Although one can correlate evaluations of in- and out-partisans with other features of individuals, these observational approaches will be vulnerable to a variety of threats to inference that we discuss in greater detail below.

In light of this ambiguity, this article presents a series of novel experimental analyses examining how partisanship, issue positions, and other social identities affect interpersonal evaluations. We estimate the effects of these manipulations in isolation (when no other information about a person is provided) and also when information is provided about multiple features of an individual. Our argument is straightforward: If partisanship (or issue positions or other social identities) directly cause social evaluations, then such effects should persist even in the presence of information about other features of an individual that might otherwise be inferred.

In addition to allowing us to understand the robustness and underlying nature of partisan divisions, this approach enables us to measure the depth and nature of divisions on the basis of issue positions. More generally, it allows us to test a crucial assumption about the interpretation of survey experiments, which is whether the manipulations operate through the hypothesized mechanism or other potential causal pathways (a violation of the exclusion restriction; see Dafoe, Zhang, and Caughey 2018). Finally, in terms of external validity, we argue that survey items that provide a greater breadth of information more accurately capture private social environments in which individuals evaluate others. The first and only thing one learns about a child’s potential spouse is never his or her partisanship.

In a series of survey experiments asking participants to rate the warmth they feel toward people described in vignettes, we find that issue positions influence interpersonal evaluations more than partisanship. Issue effects remain strong even when partisanship is also presented (they decrease by about 12%), but the effect of partisanship decreases substantially in this setting (by about 60%). Overall, respondents more favorably evaluate out-partisans who hold a shared policy position than copartisans who disagree on policy. The relative importance of issue positions is present across various issues, and it is found for weak and strong partisans. Finally, respondents explicitly opt to learn about issue positions more often than partisanship when asked to arrange a social interaction. Overall, these findings reveal that measured interpersonal partisan animosity in the mass public likely proxies issue conflict more than partisan teamism. When coupled with the observation that there is substantial overlap in issue positions across the mass parties, it implies that fears about partisan animosity undercutting social cohesion are likely overstated. Finally, efforts to ameliorate measured animosity will likely require finding common issue agreement.

**Partisan Identity and Animosity**

The study of partisan animosity draws heavily on established frameworks for understanding partisan groups and social groups more broadly. Foundational work on partisanship in the United States conceived of it as a psychological attachment that exists in relation to other group-based social identities (Campbell et al. 1960). Patterns of identification with parties in the United States appear to fit theories of social identity well (Huddy 2001). Individuals conceive of partisan groups as having distinct characteristics (Green, Palmquist, and Schickler 2002) and associate more positive traits with copartisans than with out-partisans (Iyengar, Sood, and Lelkes 2012). For many individuals, partisanship is a psychologically relevant group identity (Greene 1999).

Although grouping need not imply animosity, researchers have measured partisan discrimination, a preference for copartisan over out-partisan members of the general public, using standard survey questions, implicit association tests, résumé experiments, and behavioral economics experiments, all revealing a substantial preference for one’s own party (Iyengar and Westwood 2015). Partisanship shapes comfort with social interactions, such as preferences for a child’s marriage partner (Iyengar, Sood, and Lelkes 2012) or living near someone who has a Clinton 2016 yard sign (Ahler and Sood 2018). Whereas less than 10% of survey respondents in 1960 reported displeasure about their child marrying an out-partisan, that number grew to 30–50% of respondents in 2010 (Iyengar, Sood, and Lelkes 2012).

There is some evidence that partisan conflict affects behaviors in nonpolitical social environments. For

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1 We distinguish these patterns from data about elite evaluations in part because inferences may reflect changing (and accurate) views about polarization of elites. We focus instead on evaluations of members of the mass public.
example, shared partisanship is associated with interaction in online dating (Huber and Malhotra 2017), and absolute rates of cross-partisan marriages appear low (Iyengar and Westwood 2015). But the magnitudes of these effects are modest. Online dating is not primarily driven by partisanship. Rather, geography, reasons for dating, age, shared race, and religion dwarf the effect of partisanship (Huber and Malhotra 2017). Similarly, copartisanship in marriage may be a consequence of the correlation between partisanship and other characteristics, such as religion. Apparent partisan animosity may therefore arise because partisanship predicts other characteristics.

Cumulatively, this work shows that the effect of partisanship on social evaluations is large and has increased over time. But it is not informative about the mechanism explaining this effect. The finding that individuals report liking a hypothetical member of their own political party more than an out-party member could originate in several sources. Moreover, over-time changes in measured partisan animosity could originate in multiple sources beyond partisanship per se, such as changes in the nonpartisan inferences one makes on the basis of partisanship (e.g., increasingly believing Republicans are conservative) or changes in the importance of these inferences (e.g., increasingly disliking conservatives).

Importantly, which factors are at work leads to starkly different interpretations of the high levels of partisan animosity observed in recent survey data. Partisan animosity based on teamsmanship is widely believed to undermine democracy, whereas animosity based on policy convictions is simply programmatic conflict. Not only does this difference in interpretation inform our theoretical understanding of mass partisanship, but also each interpretation implies vastly different responses to curb partisan animosity. For example, if issue conflict is at the heart of animosity, emphasizing the substantial ideological diversity of both parties or areas of common policy agreement may reduce animosity, whereas if partisanship itself is the source of such animosity, other solutions are necessary.

Identity or Inferences?

One possibility is that partisanship per se explains apparent discrimination. In this perspective, individuals react to another person because of his or her partisanship, a view emphasized in accounts that focus on the core role of partisanship as a psychological team orientation (Iyengar et al. 2018). Another possibility, however, is that it is not partisanship itself, but instead features of individuals that are correlated with their partisanship, that explain how partisanship affects evaluations. Partisanship may be a cue about both policy positions and social group orientations, each of which may affect a person’s feelings about another person. In this case, the effect of partisanship on evaluations is not direct, but is instead statistical discrimination. Such discrimination may be enhanced if individuals assume another partisan is an exemplar, adopting the policy positions of party elites rather than reflecting the true heterogeneity of attitudes present in the mass public.

Prior work supports the idea that individuals make inferences about other characteristics on the basis of someone’s partisan orientation and also that such inferences may have changed over time. Party elites and platforms are now more ideologically polarized than they were in the mid-twentieth century. Partisan sorting at the mass level has followed the same pattern, so Democrats (Republicans) are now more likely to hold liberal (conservative) policy positions (Levendusky 2009). Members of the public are aware of this sorting and perceive even larger policy differences between typical partisans than are actually observed (Levendusky and Malhotra 2016). Bougher (2017) and Webster and Abramowitz (2017) argue that increasing partisan animosity over time can be explained by these factors—that out-partisans are (now) policy foes makes them loathsome.

Similarly, individuals describe the parties as demographically distinct and appear to make inferences about nonpolicy characteristics, like racial and class identification, on the basis of party affiliations (Ahler and Sood 2018). This work also suggests that these inferences affect animosity; correcting over-association of partisanship with other groups, including racial and religious groups, reduces partisan animosity (Ahler and Sood 2018). This implies that partisan animosity may partially be a consequence of perceived correlated traits.

Distinguishing among these competing perspectives is an area of ongoing work. Most often, this work examines differences in which individuals engage in discrimination to make arguments about which theoretical perspective explains observed effects. (A related approach is to compare average differences over time, but the same concerns noted here apply to that work.) For example, Rothschild et al. (2018) document heterogeneity in the inferences (policy, group, and trait-based) that individuals make on the basis of partisanship, and they show that trait-based inferences are more strongly correlated with measured partisan animosity. They argue that this pattern is supportive of the view that partisan social identity explains

2 Time-series data support the plausibility of an increased association between inferences about partisan and racial group membership (Abramowitz and Webster 2018).
measured partisan animosity, but they acknowledge “we cannot speak to the causal ordering of these relationships” (16). There is no persuasive strategy to disentangle greater trait stereotypes as a cause or an effect of underlying partisan animosity.

A similar indeterminacy plagues work that seeks to adjudicate between the role of partisan policy conflict and social identity in explaining animosity. Both Mason (2015) and Bougher (2017) examine differences across individuals in animosity using American National Election Studies (ANES) survey data. Bougher (2017) concludes that the alignment of issue positions with partisanship explains growing partisan animosity. This is a similar conclusion to that reached by Abramowitz and colleagues in a series of articles using ANES and related data to highlight the growing programmatic divide between the parties as an explanation for increased animosity (cf. Abramowitz and Saunders 2006; Webster and Abramowitz 2017). Mason (2015), by contrast, concludes, “The strength of a person’s identification with his or her party affects how biased, active, and angry that person is, even if that person’s issue positions are moderate” (141). Notably, however, Mason (2018) finds that ideological attachment also explains partisan discrimination.

Stepping back, our view is that existing data are unlikely to resolve disagreements among these different perspectives. At its core, part of the problem is that it is infeasible to manipulate individual-level issue positions or partisanship (for a notable small-scale exception, see Gerber, Huber, and Washington 2010). Scholars are left in the unenviable position of engaging in ex post mediation and moderation analysis, trying to discern causal pathways from either cross-sectional or occasionally panel data after making (different) assumptions about which variables (e.g., ideology, partisanship, stereotypes, psychological moderators) are properly viewed as exogenous/causally prior to other endogenous measures. Thus, the same data can lead to different conclusions depending on the researcher’s assumptions.

Setting aside questions of analysis approach, measurement error also raises threats to inferences in analysis that uses regression to assess the predictive power of different variables. For example, if a variable (e.g., issue attitudes) is measured with random error and is correlated with another variable (e.g., partisanship), then a regression will place weight on both variables in predicting an outcome (e.g., partisan animosity) even if the latter has no effect. This general problem also plagues panel analysis. The apparent effect of a variable measured at time \( t \) (e.g., partisanship) on an outcome measured at time \( t + 1 \) (e.g., animosity) may arise due to measurement error in another correlated predictor measured at time \( t \) (e.g., issue positions) even when only the latter is actually causal.3

To paraphrase, persuasive causal inference is difficult in analysis of survey data without strong assumptions and is perhaps impossible in the presence of measurement error. It is precisely in these settings that experimentation offers the most potential benefit. But the survey items most often used in the existing survey experimental research on animosity sometimes present partisanship alone, and when partisanship is presented alongside other information, that information does not address most other politically relevant things people may infer on the basis of party. This treatment is ambiguous because we do not know whether it is only partisanship itself or other beliefs that are perturbed by this treatment. After the fact, the analyst is left to examine differences across individuals to make arguments about why the treatment works.

**Motivational Analysis: How Partisanship Affects Inferences**

Is the concern that partisanship affects assessments of nonpartisan characteristics reasonable? If individuals do not form inferences about other characteristics on the basis of partisanship, then treating partisanship as a treatment that manipulates only partisanship (i.e., validating the exclusion restriction) is justified. If individuals infer other characteristics, however, then the partisanship treatment is “bundled” and extends beyond partisanship per se. Ahler and Sood (2018) measure the perceived composition of parties, primarily focusing on social groups stereotypically linked to each party, such as southerners and union members. Here, we extend this work by measuring the likelihood that a person randomly described as being a Democrat or a Republican has various traits and preferences. Besides partisanship, all descriptions included two additional traits, such as a job or hobby, and each participant evaluated only one vignette. (Details are presented in Appendix 4 of the supporting information [SI]).

Figure 1 plots results from this study. On the horizontal axis, we plot the average likelihood of having a characteristic that survey respondents estimate for a person who is described as a Democrat. On the vertical axis, we plot the average likelihood for a person described as a Republican. Points above (below) the dashed 45-degree line

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3For a more complete discussion of this general problem in the panel context, see Gerber, Huber, and Washington (2010, 723).
Figure 1 Effects of Partisanship on Evaluations of Nonpartisan Characteristics

Note: May 2018 Lucid survey. N = 634.

Line indicate people believe a Republican (Democrat) is more likely to have that characteristic. For a nonpolitical baseline, exercise, we see that the experimentally manipulated partisanship of the person in the vignette has almost no effect on assessments: the point is close to the 45-degree line, and Democrats are perceived to be about 3 points more likely to exercise than Republicans (56% vs. 53%).

For policy positions, differences are much larger. A Democrat is perceived as being 21 points more likely to support same-sex adoption, 25 points more likely to support limiting gun access, 24 points more likely to support allowing undocumented immigrant children citizenship, and 23 points more likely to support increasing welfare spending.

Finally, we consider racial and religious differences. Most of these points are clustered in the lower-left quadrant near the 45-degree line, showing that individuals neither perceive these groups to be large nor perceive large differences on the basis of partisanship. Nonetheless, Democrats are more likely to be Black (by 10 points), Hispanic (by 5 points), and atheists (by 3 points), whereas Republicans are more likely to be Protestant (by 6 points) and White (by 16 points).

A summary of actual differences between the parties, estimated from the 2016 ANES, appears in SI Appendix 6. These rates show that the average estimated gap between Democrats and Republicans is highly accurate (and in some cases, even overestimates heterogeneity within the parties). Moreover, these data also show that despite growing polarization, respondents perceive a high degree of overlap between the parties in the mass public even on these salient issues for which the elite parties are clearly divided. Overall, individuals do make inferences about the likelihood that a person holds other traits (especially issue positions) on the basis of his or her partisanship. Thus, we cannot rule out the possibility that the partisan effects estimated in existing studies arise because respondents are reacting to differences in inferred beliefs, rather than to partisanship itself.

An Experimental Approach to Disentangling the Effect of Partisanship from Other Inferred Characteristics

In light of the fact that manipulating partisanship appears to alter individuals’ assessments of nonpartisan characteristics, here we propose a different research approach. Building on existing survey experimental work, we examine how the randomly assigned partisanship of an object affects evaluations of it. However, we depart from existing work by considering both the effect of partisanship in isolation and the effect of partisanship when other factors that one might otherwise reasonably infer on the basis of partisanship are also present. If directly manipulating those other factors reduces the importance of partisanship on evaluations, it implies partisanship also operates by influencing our judgments about other characteristics individuals might infer on the basis of their partisanship. If it does not, however, it supports the view that partisanship per se drives evaluations. Additionally, we repeat this analysis in parallel fashion for policy positions, comparing the effect of policy positions on evaluations in the absence and presence of other factors.

Our analysis relies on several assumptions, which we state formally here. In these experiments, we have two treatments: $T_1$, which is the partisanship of an object, and $T_2$, which is whether additional information is provided ($T_2 = 1$). (This formalization ignores the fact that conditional on being present, different treatments can take on different values, which we discuss below.) First, we assume that only $T_1$ affects inferences about the partisanship of the object, but that both $T_1$ and $T_2$ can affect inferences about other features of the object. (Prior work, by contrast, has assumed that $T_1$ affects only inferences about partisanship, which as we show above is likely inaccurate.) Simply stated, this means that if we tell someone that a person is a Democrat, they do not think they are no longer a Democrat if we tell them something else about the person (e.g., that he or she is against gun control). One might object by noting that “typical” Democrats hold liberal gun control positions. However, we are interested in assessing whether the reason people (dis)like Democrats is because they are Democrats or because people assume they hold liberal policy positions. It is precisely for this reason that our second dimension of treatment ($T_2$) attempts to disentangle partisanship from inferences about related characteristics, like policy positions.

Second, we assume that the effect of partisanship on evaluations is altered when additional information is provided because of the content of that information, rather than simply because additional information is provided. That is, it is not just that an additional piece of information reduces the attention given to any individual piece of information, but that $T_2$ communicates relevant considerations. In studies reported below, we find that additional information does not always reduce initial effects.

Our analysis strategy is specified below. We follow prior work by focusing on two core survey measures of average partisan animosity: (1) the difference in warmth of feelings (as measured using feeling thermometers) toward copartisans and out-partisans and (2) the difference in excitement about the prospect of one’s daughter marrying a copartisan rather than an out-partisan. Instead of asking about partisan groups generically, we measure feelings toward individuals described in vignettes. This allows us to randomly assign additional traits and estimate the causal effects of the presence of policy preferences and other group memberships in a vignette on levels of partisan animosity. Additionally, we include a parallel set of manipulations in which policy positions appear with or without partisanship, which allows us to examine whether partisanship and policy preferences have different effects on evaluations, either alone or in the presence of the other characteristic.

We note that our approach assumes that (some) individuals have meaningful and stable policy positions that are independent of their partisanship. This is the same assumption made in prior observational work that seeks to disentangle partisan from issue-based conflict. The issues we select are not novel, so individuals likely have had time to reflect on them independently. Additionally, as we note above, empirically the mass parties are divided internally even on these salient issues. Nonetheless, if individuals’ issue positions are truly unstable, epiphenomenal manifestations of partisanship, we would expect that partisanship would dominate issue positions in shaping interpersonal animosity either because those positions would be given little weight or because they were secondary to the underlying partisan orientations that led to expressed policy positions. Overall, if issue positions are unstable and partisan induced, this would bias against finding that issue positions affected measured animosity in the presence of direct partisan cues.

To provide greater intuition for this design, Figure 2 displays examples of three treatment conditions from one experiment. The first condition provides partisanship alongside minimal social cues ($T_1 \in \{Dem, Rep\}$, $T_2 = 0$). The second condition adds policy positions ($T_1 \in \{Dem, Rep\}$, $T_2 = 1$). Finally, the third condition includes policy positions, but not partisanship, alongside minimal social cues ($T_1 = \emptyset$, $T_2 = 1$). In all cases, the specific value of a manipulated trait was independently
Partisanship + Social Cues: Suppose you meet a person who works as a waiter, is from Colorado, and is a Republican. How warm are your feelings towards this person?

Partisanship + Policy Positions + Social Cues: Suppose you meet a person who is from South Carolina, enjoys watching sports, and is a Democrat. They believe that the federal government should make it easier for people to buy a gun. How warm are your feelings towards this person?

Policy Positions + Social Cues: Suppose you meet a person who listens to rock music, works as a personal injury lawyer, and believes that same sex couples should not be allowed to adopt children. How warm are your feelings towards this person?

Note: Randomly manipulated features are listed in boldface.

randomly assigned so that it is orthogonal to all other traits.

We repeated this core design in three separate studies. In our first and third studies, each vignette provided two pieces of background information (e.g., working as waiter) and respondents were assigned to see additional information about partisanship, a policy preference, or both. In our second study, we included additional treatment arms in which participants observed the race or religion of the person, using this information as examples of nonpartisan social groups. The effect of partisanship can thus be observed (1) in isolation, (2) in the presence of a policy preference, (3) when a person’s race and religion are presented, and (4) when both (2) and (3) are present. SI Appendices 1–4 list all treatment arms and vignette structures, including specific traits, for each study. An overview of treatment designs appears in Table 1.

Data Collection

We summarize the data collection for our three studies in Table 1, with details including demographics and balance tests available in SI Appendices 1–4. We recruited subjects in an online marketplace (Amazon.com’s Mechanical Turk, Studies 1 and 2) or using a third-party sample provider (Lucid, Study 3). Our primary analysis is experimental in nature. As with all opt-in samples, generalization to more representative populations would require certain assumptions, although these platforms are regularly used for experimental studies (Berinsky, Huber, and Lenz 2012; Coppock 2018).

In the panel surveys (Study 1 and part of Study 2), demographics, partisanship, and policy preferences were collected in the first wave, separate from the experimental partisan animosity vignettes that were fielded in the second wave. This design reduces concerns that answering partisan identity or policy items immediately before the vignettes would cue partisan or policy conflict. Study 2 revealed no systematic differences in treatment responses between the two-wave and single-wave design, however, and so for our analysis of Study 2, we merged the data across formats. Study 3 is a single-wave design.

Survey measures including partisanship and policy preferences were modeled after 2016 ANES items. Policy areas were chosen to cover controversial, high-profile issues that participants were likely to have considered and formed preferences about prior to taking the survey. To assess policy preferences in Study 1, we asked respondents three policy questions. Participants chose between policy options for the treatment of young immigrants brought to the United States illegally, levels of federal welfare spending, and the legality of abortion. Studies 2 and 3 added views about same-sex-parent adoption and gun control. All participants who did not answer all relevant questions were dropped from the analysis so that participants are directly comparable across treatment arms.

5 These selection criteria reflect an important scope condition on our argument. We do not believe that highly technical or obscure issues are a likely basis for measured partisan animosity.

6 Across studies, 19% of respondents were excluded from our main analysis because they were Independents or third-party affiliates, and 4% were excluded due to missing or “other” policy preferences.
### Table 1 Summary of Design for Three Studies

<table>
<thead>
<tr>
<th></th>
<th>Study 1</th>
<th>Study 2</th>
<th>Study 3</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Platform</strong></td>
<td>Amazon Mechanical Turk</td>
<td>Amazon Mechanical Turk</td>
<td>Lucid</td>
</tr>
<tr>
<td><strong>Format</strong></td>
<td>two-wave panel</td>
<td>two-wave panel + single survey, pooled</td>
<td>single survey</td>
</tr>
<tr>
<td><strong>Dates</strong></td>
<td>October, November 2016</td>
<td>June, July 2017</td>
<td>December 2017</td>
</tr>
<tr>
<td><strong>Size</strong></td>
<td>N = 1,717</td>
<td>Pooled N = 1,736</td>
<td>N = 4,991</td>
</tr>
<tr>
<td></td>
<td>D = 908, R = 538</td>
<td>D = 963, R = 477</td>
<td>D = 2,206, R = 1,772</td>
</tr>
<tr>
<td>Panel retention</td>
<td>= 86%</td>
<td>= 59%</td>
<td></td>
</tr>
<tr>
<td><strong>Treatments</strong></td>
<td>Pid, policy, pid + policy (all with social cues)</td>
<td>FT: Pid, policy, groups, pid + policy, pid + groups, policy + groups, pid + policy + groups (all with social cues). Marriage: Pid, policy, groups, pid + policy, pid + groups, pid + policy + groups (all with social cues), plus pid with no social cues</td>
<td>Pid, policy, pid + policy (all with social cues)</td>
</tr>
<tr>
<td><strong>PID</strong></td>
<td>R, D, I, not interested</td>
<td>R, D, I, not interested</td>
<td>R, D, I</td>
</tr>
<tr>
<td><strong>Policy</strong></td>
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<td>Immigration, welfare, guns, abortion, same-sex adoption</td>
<td>Immigration, welfare, guns, abortion, same-sex adoption</td>
</tr>
<tr>
<td><strong>Groups</strong></td>
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<td>Race: White, Black, Hispanic, Asian Religion: Protestant, Catholic, Jewish, atheist, not religious</td>
<td>None</td>
</tr>
<tr>
<td><strong>Social Cues</strong></td>
<td>State and job</td>
<td>FT: Age, gender, hobby, music preference</td>
<td>2 of: State, job, hobby, music preference</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Marriage: Age, child-rearing opinion, valence (smoke, divorce, or exercise)</td>
<td></td>
</tr>
</tbody>
</table>

*Note: Sample sizes are participants who completed both panel waves. Study text and exclusion rules appear in SI Appendices 1–4.*

In Study 1, subjects evaluated three vignettes using feeling thermometers. The information in each vignette was independently and fully randomized, but all three vignettes were generated within one treatment condition. In Study 2, subjects completed nine feeling thermometer items and four marriage evaluations. All vignette traits were fully and independently randomized. Feeling thermometer conditions were presented in a random order, and then a random subset of the marriage evaluation conditions were presented in a random order. Finally, in Study 3, subjects completed only a single feeling thermometer question. We analyze the data by comparing responses between subjects. For experiments with repeated observations, we report standard errors clustered by respondent and assess the robustness of our results by examining only the first response individuals provided to each item type.

#### Data Coding and Analysis Strategy

For both measures, we model the outcome for respondent $i$ as a function of various indicator variables for aligned partisanship and treatment conditions. For our base specification, we estimate the following:

$$
Outcome_i = B_0 + B_1 \text{MatchPid}_i + B_2 \text{MatchPid}_i \times \text{PolicyPresent}_i + B_3 \text{PolicyPresent}_i + \epsilon_i,
$$

(1)
using ordinary least squares (OLS) regression with robust standard errors, where MatchPid is 1 when a respondent and the person described in the vignette share the same partisanship (versus opposing), PolicyPresent is 1 for the treatment condition in which a policy position is presented in the vignette (versus the baseline condition), and $\epsilon$ is an idiosyncratic error term. Our primary analysis is restricted to self-identified partisans, including so-called partisan “leaners,” and vignette profiles that are either Democrats or Republicans, so all individuals are either aligned or in opposition with the person in the vignette.\textsuperscript{7}

Note that although our analysis conditions on whether the respondent matches the partisanship of the person described (MatchPid), we do not code whether the respondent shared the described person’s policy position. Instead, we record only whether that policy information was present (PolicyPresent = 1). This is because we want to know the average effect of matched partisanship on evaluations in the presence of absence of other information. We later examine the relative importance of matched policy positions.

Interpreting this regression model is straightforward. The coefficient $B_1$ is how much greater the outcome is when partisanship is aligned rather than opposed in the absence of policy information, whereas $B_1 + B_2$ is the effect of aligned partisanship when policy information is also provided. In light of prior research, we expect $B_1$ to be positive and large. If partisanship signals policy positions that affect evaluations, then $B_2$ will be negative, indicating that the effect of partisanship is reduced when policy information is provided. $B_1 + B_2$ is therefore an upper bound on the effect of partisanship per se because partisanship can still affect other inferences beyond the one policy position held constant. If partisan animosity is largely independent of policy conflicts, it should remain high in the presence of a single policy preference, as it would in the presence of any other irrelevant information.

Although our method of measurement is experimental, we do not randomly manipulate survey respondents’ partisanship, issue positions, or other characteristics. This is the same approach taken in all prior experimental research on evaluations of partisans and means that we cannot exclude the possibility that there are unmeasured factors correlated with features of individuals apart from their partisanship that explain their reactions to features of the profiles they evaluate. It is precisely for this reason, however, that we argue prior research is not dispositive about why individuals react to partisan cues in these sorts of profiles. It could be that individuals react because, as partisans, they hold policy (or other) views that are correlated with their partisan identities and infer those about individuals in profiles in an analogous manner. Our analysis therefore does not circumvent the existing inferential limitations that arise because a respondent’s partisanship and other features are not manipulated, but instead tries to assess whether those omitted (but potentially correlated) factors explain the effects that arise by separately and independently manipulating them in the evaluated profile. We therefore make fewer and weaker assumptions than existing work.

We extend this basic framework in Study 2, in which we also include conditions with additional pieces of information. For our analysis of these data, we add additional indicators for those treatment arms and those terms interacted with shared partisanship. Additionally, we present parallel analysis in which we estimate the effect of matched issue positions in the presence or absence of additional information, including partisanship. For the coding of matched issue positions, where variables can take on more than two values (e.g., expand, maintain, or contract welfare spending), we code respondents as matched if the profile has the exact same preferred issue position that the respondent preferred. We view this as a conservative test for the importance of policy views because not all unaligned individuals hold policy views diametrically opposed to the respondent’s.

**Results**

**Warmth of Feelings**

We first examine the effect of a person’s partisanship on warmth of feelings toward that person and how those evaluations are affected by the availability of other information about that person. For ease of presentation, we present our results across all three studies graphically in Figure 3, Panels A, C, and E, with underlying regression specifications reported in SI Table A8. Each row of the figure reports results from a separate study, and each vertical bar plots how much warmer respondents are, on average, toward members of their own party rather than the opposition party, across treatment conditions, with uncertainty indicated by 95% confidence intervals.

Beginning with Panel A (Study 1), the leftmost bar is for the treatment condition in which only partisanship and social cues (state and job) are provided in the profile. On average, respondents are 23 degrees warmer toward copartisans than opposing partisans ($p < .01$), coefficient $B_1$ in Equation (1). This effect is similar in magnitude across all three studies (see Panels C and E, where effects are 22 and 18 degrees, respectively, all significant at

\textsuperscript{7}In SI Appendix 8, we compare in- and out-party effects relative to neutral (Independent) vignettes.
When information about a single policy position is also provided, however, the effect of aligned partisanship on evaluations drops substantially. In each panel, the second bar is the effect of partisan alignment when policy is present ($B_1 + B_2$ in the regression model), regardless of the particular policy position listed in the profile. In Study 1 (Panel A), the effect of shared partisanship drops by 13 degrees to only 10 degrees (difference significant at $p < .01$), a reduction of 55% in the effect of partisan alignment. Results are similar in Studies 2 and 3 (Panels C and E show the treatment effect drops by 13 degrees, 60%, in Study 2, and 11 degrees, 62%, in Study 3). 8

Beyond the use of clustered standard errors, our analysis of these data does not account for the fact that individuals were exposed to multiple profiles in Studies 1 and

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8We provide analysis on an issue-by-issue basis in the supporting information, where we also present results by respondent partisanship.
2. The effects we report could be affected by repeated measurement, so we have also examined only the first profile a respondent evaluated. In Study 1, the effect of partisanship drops from 22 to 7 degrees when policy information is present, and in Study 2, it drops from 28 to 0 degrees. The 7-degree figure is the same result we find in our analysis of Study 3 (Panel E), which is a single vignette design. Overall, a 7-degree effect of partisanship when a single policy position is provided is modest, implying that at least some of the effect of partisanship when policy is not present is due to inferences individuals otherwise make about members of parties.

When racial and religion group memberships are provided alongside party (Study 2 only), the effect of aligned partisanship also decreases relative to the condition without group cues. In Study 2 (Panel C), the effect of partisanship drops by 6 degrees, 26%, to 16 points. This reduction is only half of the reduction observed when a policy position was presented along with partisanship. This result again implies that at least some of the partisanship effect observed when no race and religion are observed is due to inferences made on the basis of party. When group memberships are provided alongside a policy position, the effect of aligned partisanship is nearly identical to the effect of aligned partisanship when just a policy position is provided.

One reaction to this pattern of results might be to assume that any additional information about a person reduces the effect of existing pieces of information. By this logic, perhaps it is not policy per se that matters, but merely the presence of an additional piece of information. Our design allows us to investigate this possibility by conducting parallel analysis of the effect of policy alignment in the presence and absence of partisan signals. Once again, we present our results graphically in Figure 3, Panels B, D, and F, with underlying regression specifications reported SI Table A9. These results show that the effect of policy positions persists despite the presence of information about a person's partisan orientation, in sharp contrast to partisanship effects.

Beginning with the upper-right pair of bars in Study 1 (Panel B), we show that the effect of shared policy positions is to increase warmth by 28 points when partisanship information is not present (p < .01), an effect that drops by only 2 points to 26 points in the presence of partisanship (differences not significant). Not only is the effect of policy larger than the effect of partisanship when it is the only politically relevant characteristic in a vignette (28 degrees for policy vs. 23 for partisanship), but also the effect of policy on warmth is almost entirely unaffected by partisan information. Panels D and F show similar results in Studies 2 and 3. In both studies, shared policy increases warmth by 26 degrees (both p < .01), and providing partisanship reduces the effect of shared policy by only 2 to 4 degrees. This reduction is statistically significant in the much larger Study 3 sample (p < .05), but the effect of shared policy remains to increase feelings of warmth by 22 degrees (p < .01). Likewise, the policy effect drops by only 2 degrees when race and religion are present (differences not significant). The smallest policy effect observed across studies and treatment arms is a 19-degree aligned policy effect when profiles included partisanship, race, and religion (Panel D). This effect is still substantially larger than the effect of partisanship (11 degrees) from the same arm (Panel C).

Overall, these results reveal three things. First, when partisanship is the only politically relevant piece of information people learn about a person, it has meaningful effects on feelings of warmth. This is the result reported in prior research. Second, however, providing policy information substantially reduces the effect of shared partisan orientations on warmth, by at least 55%. This supports our hypothesis that when partisanship is the only politically relevant piece of information we learn about a person, individuals infer other characteristics on the basis of party. Third, the effect of one shared policy position is larger than the effect of shared partisan orientation. Unlike partisanship, the magnitude of the effect of a shared policy position is only modestly diminished by the presence of partisanship. In a horse race between partisanship and policy, therefore, policy appears to dominate, calling into question the argument that partisanship as a group orientation dominates policy positions as an explanation for partisan animosity.

**Fiancé Evaluation**

We next examine the effect of partisanship and other characteristics on evaluations of potential sons-in-law. In this case, the outcome is an evaluations scale that ranges from −3 to 3, with 3 indicating *extremely pleased* and −3 indicating *extremely displeased* by the prospect of marriage. We again present our results graphically in Figure 4, with underlying regression specifications reported in SI Tables A6 and A7.

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8See the supporting information for complete results from Studies 1 and 2 using only the first profile.

10We chose to present a daughter- and son-in-law, rather than leaving gender unspecified, as part of a broader effort to humanize the vignettes. Given that we reproduce prior baseline partisan animosity estimates in the absence of additional information, this choice does not appear consequential.
Figure 4 Treatment Effects of Partisanship and Policy Preference on Son-in-Law Evaluations Given Varied Levels of Background Information

Panel A shows the effect of the fiancé’s shared partisan orientations on evaluations across multiple conditions. In the first condition, the fiancé is described only by his partisanship, and in the second, partisanship is included alongside three social cues, including a valence characteristic and child-rearing opinion. (As before, we examine how the presence, rather than value, of these additional cues affects evaluations.) The left bar shows that shared partisanship is associated with a 1.70 unit increase in evaluations (p < .01), which is equivalent to moving almost 2 scale points on the evaluations item. The second bar shows that when social cues are provided, the effect of shared partisanship drops by .94 units (p < .01), or about 55%, to .77 units.

Building on the design from our feeling thermometer experiment, in subsequent conditions we added different information to the partisanship and social cues condition. In particular, we added one of three types of information: (1) a policy position, (2) group characteristics (race and religion), or (3) a policy position and group characteristics. The remaining bars in Panel A plot the effect of partisanship when additional information is provided, the effect of shared partisanship drops by .94 units (p < .01), or about 55%, to .77 units.

To summarize the results shown in Panel A, partisanship alone appears to substantially shape fiancé evaluations, but including social cues alongside partisanship reduces the effect of partisanship by about half. Additional information, in particular policy positions, further reduces that effect by between 20 and 40%, but those reductions are imprecisely estimated and statistically indistinguishable from one another (although all are statistically distinct from the effect of partisanship by itself).

As with the feeling thermometers evaluation, we can compare the effect of providing additional information on the importance of shared partisanship to the effect of providing additional information on the importance of shared policy views. This analysis therefore compares the effect of shared policy positions from a condition with policy and social cues to conditions that also add (1) partisanship or (2) partisanship and group characteristics. Results from this analysis appear in Figure 4, Panel B, with underlying regression analysis in SI Table A7.

In a pooled model, adding policy information reduces the effect of shared partisanship by .23 units (p = 0.11). See SI Appendix 8.
leftmost bar shows that shared policy positions are associated with a 1.36 unit (p < .01) increase in evaluations when only policy and social cues are provided, which is notably larger than the .77 unit effect of shared partisanship when only partisanship and social cues are provided (the second from the left bar in Panel A). The second bar shows that adding partisanship does not reduce this effect, which remains at 1.35 units, meaning that when partisanship is added, the effect of shared policy is much larger than the effect of shared partisanship when policy is present (1.35 vs. .61 units). Adding both partisanship and group orientations in the third bar reduces the effect of shared policy slightly, by .25 units (difference not significant), but once again, the effect of shared policy remains a substantial 1.11 units and is larger than the .46 unit effect of shared partisanship when both policy and group characteristics are provided.

Cumulatively, the results of the fiancé vignette are roughly similar to the results of the feeling thermometer experiment, with the added observation that in evaluating potential fiancés, characteristics such as age, relationship history, smoking and exercise habits, and attitudes about child-rearing appear very important and substantially reduce the effect of shared partisanship on evaluations compared to the effects that we estimate when only partisanship is provided. Although partisanship affects evaluations, those effects are not as large as the effect of shared policy positions. The effect of providing partisanship in isolation when describing a potential fiancé yields an estimated effect that is substantially larger than it is when partisanship is provided alongside other information individuals are likely to learn before learning a fiancé’s partisanship.

In SI Appendix 10, we present the relative magnitudes of effects on evaluations of all the manipulations included in the most detailed vignette. Having a shared policy opinion yields greater approval than having shared partisanship (although shared party still matters), and some attitudes and characteristics (i.e., child-rearing views and fitness) are more important than either policy or partisanship in garnering approval.

**Strong and Weak Partisans Give Policy More Weight**

The average effects of policy and party on evaluations we estimate may mask evidence that some individuals engage in greater identity-based partisan discrimination. To assess this possibility, we present a series of subgroup analyses. This analysis relies on Study 3 due to its large size, although we also report results from Study 2 in SI Table A14 because it includes additional measures of partisan identity (social identification with a party). Using Study 3, we separately examine effects for strong partisans, weak partisans, and partisan leaners using regression models identical to those specified in Equation (1). These results, along with analogous policy results, appear in Table 2.

The highest baseline partisan animosity is unsurprisingly observed among strong partisan respondents (column 5), who rate copartisans 23 points warmer than out-partisans. However, this effect drops to 6 points (23.3–16.9) when a policy position is present, which is nearly identical to the effect among weak partisans when a policy position is provided (column 3). In short, strong partisans do not appear to evaluate other partisans primarily on the basis of party group affiliation independently of policy positions.

By contrast, columns 2, 4, and 6 show that all three groups feel substantially warmer toward individuals with a shared policy preference than those taking other positions, in the range of 24 to 28 points. These effects remain strong in the presence of a partisan cue (the coefficients on Match Policy × Pid Present are modest in size). Overall, weak partisans and leaners show substantially more sensitivity to a profile’s policy preference than their partisanship, both when these cues are presented in isolation and when they are presented simultaneously. Strong partisans appear only slightly more sensitive to policies than party on their own, but when they are both available, policy drives evaluations far more than party.

**Policy Is More Influential When Incongruent with Party**

Our preceding analysis examines how the effect of shared partisanship alters affective judgments in the presence or absence of policy information. We speculate that such effects occur because individuals otherwise infer policy from party. If this account is correct, the effect of providing policy on evaluations should be larger when the policy position presented departs from predictions individuals likely otherwise make on the basis of party. So, for example, learning a Republican is pro-choice should substantively alter the effect of being a Republican, whereas learning a Republican is pro-life should not because individuals likely already make such an assumption. Figure 5 displays average warmth toward vignettes with no policy, party-congruent policy, and party-incongruent policy positions, where the most liberal (conservative) policies are coded as congruent with Democratic (Republican) partisanship.
**Table 2: Partisan and Policy Effects by Respondent Partisan Strength**

<table>
<thead>
<tr>
<th></th>
<th>Warmth of Feelings (Study 3)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Leaners (1)</td>
</tr>
<tr>
<td>MatchPid</td>
<td>12.785***</td>
</tr>
<tr>
<td></td>
<td>(2.883)</td>
</tr>
<tr>
<td>PolicyPresent</td>
<td>−3.497</td>
</tr>
<tr>
<td></td>
<td>(3.408)</td>
</tr>
<tr>
<td>MatchPid × PolPresent</td>
<td>−3.473</td>
</tr>
<tr>
<td></td>
<td>(4.506)</td>
</tr>
<tr>
<td>MatchPolicy</td>
<td>27.728***</td>
</tr>
<tr>
<td></td>
<td>(2.474)</td>
</tr>
<tr>
<td>PidPresent</td>
<td>5.047*</td>
</tr>
<tr>
<td></td>
<td>(2.519)</td>
</tr>
<tr>
<td>MatchPol × PidPresent</td>
<td>−5.509</td>
</tr>
<tr>
<td></td>
<td>(3.762)</td>
</tr>
<tr>
<td>Constant</td>
<td>56.840</td>
</tr>
<tr>
<td></td>
<td>(2.275)</td>
</tr>
<tr>
<td></td>
<td>429</td>
</tr>
<tr>
<td>N</td>
<td></td>
</tr>
<tr>
<td>Adjusted R²</td>
<td>0.054</td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td>Pid effect</td>
<td>12.8</td>
</tr>
<tr>
<td>Policy effect</td>
<td>27.7</td>
</tr>
<tr>
<td>Pid effect</td>
<td>Policy</td>
</tr>
<tr>
<td>Policy effect</td>
<td>Pid</td>
</tr>
</tbody>
</table>

*Note: Linear regression estimates of partisan and policy effects for leaners, weak, and strong partisans are shown.*

*p < .05, **p < .01, ***p < .001.*

For copartisans, incongruent policy positions are punished, although there is no discernible difference between warmth for copartisans without a policy position and those with a congruent position. For out-partisans, incongruent policies appear to alleviate hostility, whereas congruent policies confirm fears and are punished above and beyond the out-partisanship effect. Conditional on shared partisanship, congruence also proxies respondent policy preferences. Respondents appear to be indifferent between out-partisans and copartisans with incongruent positions, but conditioning directly on shared policy preferences, we see greater warmth for out-partisans who take a shared policy position than copartisans who take an opposing position (68 vs. 53 degrees in Study 1, 63 vs. 49 in Study 2, and 70 vs. 55 in Study 3). This corresponds to a largely unchanged partisanship effect when congruent policies are presented, and a sign flip for the partisanship effect when incongruent policies are presented. Overall, this supports the view that positions are informative precisely because they fill in details that individuals otherwise infer.

### Demand for Policy Information and Partisanship

The proceeding evidence about the relative importance of partisanship and policy in social situations is indirect. To assess the absolute and relative demand for partisanship and policy information in a setting in which social animosity might have undesirable consequences, we put respondents from Study 2 in the situation of assigning seats for a friend’s wedding. We asked respondents to try to make sure everyone has “a good time,” and directed them to choose three pieces of information from a list for their friend to share about each guest. In one condition, partisanship was included alongside five other pieces of potential information (hobby, personality, religion, gender/sexuality, how they know the couple), and in the second condition, we also offered policy information. In the condition without policy, we can therefore assess the absolute importance of partisanship to individuals in creating positive social interactions vis-à-vis the other characteristics listed. In the condition where policy
positions were also available, we can assess the relative demand for policy, as well as whether policy substitutes for or complements the demand for partisanship.\footnote{We do not know what assumptions individuals make about the distribution of party or policy views among the guests.}

We present our results graphically in Figure 6, which shows the proportion of respondents requesting each type of information when policy is not an option (dark bars) and when it is available (light bars). In the first condition, respondents are choosing three of six options, so completely equal demand would yield figures near 50%. In the second condition, where we add policy (a total of seven options), complete equal demand would yield figures near 43% (3/7).

Across both conditions, personality traits are the most commonly requested information, followed by a description of how the guest knows the couple and a hobby. Gender and sexuality, religion, and partisanship are requested less often. Each is requested by about 20% of respondents given these six options. Notably, this implies that partisanship does not rank highly (it is requested only more than religion) in trying to create a positive social situation.

But what happens when policy is also available? If partisanship per se is the basis for anticipated social conflict (by some), we would not expect the availability of policy information to depress demand for partisanship. However, if policy conflict is perceived as a greater basis for social animosity than partisanship, we would expect the demand for policy to be larger than partisanship, and potentially substitute for the demand for partisanship. This is in fact what we observe. When policy is present, the demand for partisanship drops by 30% (from 23% to 16%, $p < .01$).\footnote{This drop is driven by respondents who strongly identify with their party (who drop from 34% to 21%, $p < .01$).} Moreover, the demand for policy is 28%, which is greater than the previously observed 23% of respondents requesting partisanship (despite there being one additional option to choose from). Furthermore, while 63% of respondents only request nonpolitical information and 7% request both party and policy preferences, 9% request partisanship only but 21% request only policy preferences.

Overall, these results show that most respondents are not overly concerned about political conflicts being a source of social animosity at a wedding compared to...
other differences. A small proportion of respondents appear to believe that partisanship and policy preferences contain non-redundant information, but more people perceive conflict along the dimension of policy rather than partisanship, and if they choose one over the other, they choose policy at a rate greater than 2 to 1.

**Discussion and Further Robustness**

Taken together, our results show that (some) policy positions are more important to interpersonal evaluations than partisanship, and that the apparent effect of partisanship when presented in isolation is likely the combination of party and a collection of inferences made on the basis of party. Partisanship is still important in shaping evaluations even in the presence of other information, but those effects are smaller than the effects of policy for the issues we study. When each is presented alone, the effect of party is about three-quarters as large as the effect of policy, and when they are presented together, the effect of partisanship is less than half as large as the effect of policy. In sum, the estimated effect of partisanship on measured interpersonal animosity in isolation, with no social cues or characteristics present, appears to vastly overestimate levels of pure partisan animosity in the contemporary public.

Our findings suggest important empirical and theoretical revisions to extant understandings of partisanship in the mass public. Foremost, it shows that a great deal of measured partisan animosity reflects disagreement about contentious issues and not simply teamism. Members of the mass public may dislike one another, but they do so because their partisanship proxies issues positions that are salient and important beyond partisanship per se.
Theoretically, if partisanship were the key driver of both animosity and issue positions, we would not expect the presence of issue positions to substantially reduce the importance of partisanship in shaping animosity. (Nor would we expect the degree of issue position heterogeneity we observe in the mass membership of each party, shown in SI Table A5.) Notably, the parties remain ideologically diverse even on salient issues like abortion, gun control, welfare, immigration, and gay rights, even as party elites have taken long-standing and clearly distinct positions on these issues. This is not to say that partisanship is irrelevant, but instead that it provides a (noisy) cue about issue positions that are a more important source of animosity.

We arrive at these conclusions through a novel experimental approach. Instead of using respondent characteristics to infer the reasons for their affective evaluations, we compare evaluations after constraining specific potential mechanisms. If evaluations depend on assumed policy positions (or other assumptions such as race), providing a policy position (or race) should reduce the effect of partisanship on evaluations. Although this approach has limitations, it is an innovation that requires weaker assumptions than prior work and allows new insight into the mechanisms of partisan social conflict.

Our work does not show why measured partisanship has grown over time. Much of the prior evidence concerns evaluations that are likely elite oriented, like feelings toward “the parties,” but we also observe growing animosity in responses to the marital partner question used in prior work. One possibility is that clearer elite polarization, coupled with somewhat greater partisan sorting in the mass public, has altered the inferences individuals make on the basis of partisanship alone in those settings. Notably, several of the salient political challenges of the mid- to late twentieth century, such as civil rights, the war in Vietnam, anti-communism, and the role of women in society, crossed traditional party lines, whereas the issues we examine here and many other salient contemporary policy conflicts (e.g., global warming, health care provision) are clearly divided on party lines. Unfortunately, given that multiple factors changed during this period, it is difficult to persuasively rule out other explanations for these observed patterns.

Our analysis is limited by our reliance on the five policy positions that we chose for study. These do not include any issues around which wide consensus exists or intraparty conflict dominates. Instead, we used relatively “easy” issues about which we assume many respondents have meaningful positions. Even in this scope, effects vary across issues, as is shown in SI Tables A16 and A17. The basic pattern of issue positions dominating partisanship is present for four of the five issue areas we study. For welfare, we observe relatively weak effects of shared policy. Future work should examine heterogeneity by policy area.

We caution against extrapolating these results to the study of candidate choice. Upon learning a congressional candidate’s partisanship, it is entirely reasonable to infer that he or she supports the vast majority of his or her party’s platform. To the extent that elite parties have achieved ideological uniformity, there may not even be a meaningful difference between animosity on the basis of partisanship and animosity against policy foes. However, there is meaningful divergence between party and policy in the mass public. Most partisans do not come close to full endorsement of their party platforms, and people make inferential errors when they assume that partisans embody partisan stereotypes.

Even after setting aside elites and working with issues people are likely to have considered, we are limited by our inability to constrain certain assumptions about a hypothetical partisan without changing others, although to a lesser extent than prior work. Ultimately, even with our design, we are unable to elicit evaluations of people solely on the basis of their partisan group affiliations. It is hard to identify a way to obtain such an inferential target, and yet a notion of blind animosity is the foundation of many concerns regarding partisan social conflict.

Sunstein (2015) coins the term partyism to highlight the similarities between partisan animosity and racism. With strong partyism, potential opportunities for mass consensus may be forgone because information or a new solution to collective problems will be evaluated based solely on team membership. Our results suggest that such solutions are themselves the primary object of evaluation, with partisan evaluations largely dependent on underlying policy preferences.

Although we have documented the importance of policy preferences to any understanding of partisan animosity, we by no means intend to deny that any raw identity-based animosity exists between partisans. To the extent that such animosity exists, these concerns remain pressing. Much work remains in the quest to isolate this group-based animosity from the noise of programmatic conflict, as well as to address the origins of animosity and its deleterious consequences.

References


Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

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Appendix 2: Study 2 data collection
Appendix 3: Study 3 data collection
Appendix 4: Study 4 data collection
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Appendix 6: US citizens often take moderate and counter-partisan positions
Appendix 7: Regression models underlying figures
Appendix 8: Additional regression models and specifications
Appendix 9: Heterogeneous effects by party and party strength
Appendix 10: Magnitude of all vignette trait effects on fiancé evaluations
Appendix 11: Results by policy area