

The Economic Consequences of Partisanship in a Polarized Era

Christopher McConnell Stanford Graduate School of Business
Yotam Margalit Tel Aviv University
Neil Malhotra Stanford Graduate School of Business
Matthew Levendusky University of Pennsylvania

Abstract: *With growing affective polarization in the United States, partisanship is increasingly an impediment to cooperation in political settings. But does partisanship also affect behavior in nonpolitical settings? We show evidence that it does, demonstrating its effect on economic outcomes across a range of experiments in real-world environments. A field experiment in an online labor market indicates that workers request systematically lower reservation wages when the employer shares their political stance, reflecting a preference to work for co-partisans. We conduct two field experiments with consumers and find a preference for dealing with co-partisans, especially among those with strong partisan attachments. Finally, via a population-based, incentivized survey experiment, we find that the influence of political considerations on economic choices extends also to weaker partisans. Whereas earlier studies show the political consequences of polarization in American politics, our findings suggest that partisanship spills over beyond the political, shaping cooperation in everyday economic behavior.*

Replication Materials: The data, code, and any additional materials required to replicate all analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: <https://doi.org/10.7910/DVN/R3GZZW>.

Politics as a domain is typically thought about in the context of its key components: parties and candidates, preferences and ideologies, the government and the governed. However, observers from Aristotle onward have contended that politics spills over into other aspects of people's lives. Indeed, scholars have often described partisan affiliations as a key component of social identity, affecting individuals' preferences and actions in significant ways (e.g., Green, Palmquist, and Schickler 2002). We investigate whether partisanship also shapes behavior in *apolitical* realms. Specifically, we explore whether—and in what ways—partisan affiliations spill over into economic interactions. Despite the centrality of economic decision making to everyday life, there has been little attention paid to how parti-

sanship shapes economic behavior. We do so experimentally, studying whether partisan considerations affect people's economic behavior in a range of contexts, all where clear pecuniary or professional gains are at stake.

Such questions are especially timely in the contemporary American case, given both the large literature on polarization and the divisive 2016 presidential election. Much of this research focuses on whether the ideological distance between the parties, both at the mass and elite levels, has increased over time (Fiorina and Abrams 2008). Yet less research has examined the implications of partisanship in a polarized era. Perhaps the most relevant and novel set of findings in this respect are those documenting a rise in affective polarization, the notion

Christopher McConnell is PhD student, Graduate School of Business, Stanford University, 655 Knight Way, Stanford, CA 94305 (cmcconne@stanford.edu). Yotam Margalit is Associate Professor, Department of Political Science, Tel Aviv University, Naftali Building of the Social Sciences, Tel Aviv, 6997801 (ymargalit@tau.ac.il). Neil Malhotra is Edith M. Cornell Professor of Political Economy, Graduate School of Business, Stanford University, 655 Knight Way, Stanford, CA 94305 (neilm@stanford.edu). Matthew Levendusky is Associate Professor, Department of Political Science, University of Pennsylvania, 208 S. 37th Street, Room 217, Philadelphia, PA 19104 (mleven@sas.upenn.edu). Author names are in reverse alphabetical order.

This research was partially funded by Time-sharing Experiments for the Social Sciences, which itself is funded by the National Science Foundation (SES-1628057). Margalit also thanks the Israel Science Foundation, grant no. 3131433 for support for this research.

American Journal of Political Science, Vol. 00, No. 0, xxxx 2017, Pp. 1–14

that partisans increasingly dislike and distrust supporters of the other party (e.g., Iyengar, Sood, and Lelkes 2012; Iyengar and Westwood 2015; Lelkes and Westwood 2017). This body of research shows, for example, that people increasingly report being upset by the possibility of their children marrying someone of the other party and say that they are less likely to make friends with opposing partisans. It will come as no surprise to anyone following the 2016 election that negative evaluations of the other party reached an all-time high during the campaign (Pew Research Center 2016).

Are there behavioral manifestations to partisan attachments? Do these sentiments spill over and affect economic exchanges between individuals from opposing parties? There is a literature documenting the effect of partisanship on economic *perceptions* as measured in surveys (e.g., Gerber and Huber 2010). Yet the question is whether partisanship also shapes behavior when there are real costs. Because previous efforts to explore this issue rely overwhelmingly on survey responses (or take place within the survey context), significant doubts remain. These survey responses may simply be cheap talk, a way for people to signal their political identities and cheerlead for their team (Bullock et al. 2015; Prior, Sood, and Khanna 2015). Further, because subjects know they are taking a survey, they may also be subject to Hawthorne effects. These same concerns also apply to previous studies of affective polarization.

Non-survey (behavioral) measures are therefore needed. Although one previous study employs non-survey-based measures to investigate how partisanship affects economic expectations (Gerber and Huber 2009), its design allows only for ecological inferences about the link between partisanship and economic behavior. Furthermore, the strength of these ecological findings themselves has been called into question (McGrath 2017). Prior research therefore offers limited insight about whether and how partisanship shapes real-world economic decision making.

This article reports results from field experiments that explore this issue from multiple angles, using designs that go beyond reliance on ecological inference, and can directly tie partisan inclinations to individual-level economic behavior. We study the impact of partisanship on behavior in two of the most basic settings of everyday economic exchange: the workplace and the marketplace. We then complement these studies with an incentivized, population-based survey experiment that employs a large, high-quality, nationally representative sample. This allows us to better understand the characteristics of those exhibiting partisan bias in their economic choices. Our

main finding is that partisanship exerts a systematic influence on individuals' economic behavior. In contemporary American society, people's partisan affiliations influence their economic interactions in a range of contexts and settings, whether they are operating as consumers, workers, or financial contributors.

In the first experiment, carried out in a nationwide online labor market, we assess whether partisan congruence between employer and employee influences the willingness of the latter to work, as well as the quality of work they perform. We do so by tracking the wage proposals and task performance of freelance editors when the document they edit indicates whether their employers are co-partisans or supporters of the out-party. Study 2 examines whether partisan considerations also affect consumer behavior. Specifically, we explore whether people are less likely to pursue an attractive purchasing opportunity if the seller is affiliated with the out-party, and more likely to do so if the seller is a co-partisan. We conducted another field experiment that uses an online marketplace to study this question in a more naturalistic setting, albeit one that relies on ecological inferences. Finally, we replicate these patterns in the context of an incentivized, population-based survey experiment, where we find that fully three-quarters of respondents are willing to forego higher personal remuneration to avoid benefiting the opposing party.

Taken together, our studies offer substantial evidence that partisanship shapes real-world economic decisions. All four experiments offer evidence that partisanship influences economic behavior even when there are real pecuniary or professional costs. Although the effect sizes vary somewhat across contexts, in some situations, they are quite large. For example, the effect of partisanship on reservation wages in the labor market experiment is comparable to the effect of task-relevant skills such as education and experience. In the marketplace, consumers are much more likely—almost two times as likely—to engage in a transaction when their partisanship matches that of the seller. In our survey experiment, three-quarters of all subjects forego a higher monetary payment to avoid helping the other party. We show that these effects of partisanship are at least as large as the effects of religion, another well-known and salient social cleavage. Even among weak or leaning partisans, fully two-thirds of them reject the partisan offer. In sum, partisanship's effect on economic decisions is not only real but often also sizable, extending throughout the electorate.

To be clear, our experiments cannot measure the causal effect of individual-level partisanship since there may be some source of unobserved heterogeneity between

partisan subgroups that explains the observed behavior.¹ This is an observational study in that people's partisan attachments are not exogenously changed. However, given that we do manipulate the partisan stimuli to which people are exposed, our experiments represent highly controlled devices for measuring partisan behavior in the economic domain.

Our findings have important implications for the study of political polarization and partisanship. Our analysis suggests that the overwhelming scholarly focus on the political outcomes of hyper-partisanship—legislative gridlock, extremely high levels of party-line voting, declining trust in political institutions—is missing an important aspect of what the phenomenon entails and means for contemporary society. The results underscore the power of partisanship as a social identity in an era of polarized parties—partisanship can shape apolitical behavior, including economic transactions. The results also call for paying greater attention to potential discrimination based on partisan affiliation. To date, few social norms are in place to constrain it, as they are with respect to unequal treatment along other social divides (e.g., race and gender). Our analysis suggests that partisan-based discrimination may occur even in the most basic economic settings, and as such should be the subject of more systematic scrutiny.

Finally, we also contribute to the methodological study of polarization. As noted, most of the evidence about the consequences of partisan animus comes from survey settings where subjects' behavior is potentially cheap talk, a costless way of signaling one's group affiliation. By designing large-scale experimental interventions that are carried out in real settings of economic exchange, this study helps advance this line of research and provides much richer evidence about the role of partisanship in shaping the behavior of consumers and workers.

Why Would Partisanship Shape Economic Behavior?

The chief contention underlying all four experiments we discuss below is that partisanship has become a powerful

social identity that shapes behavior (Green, Palmquist, and Schickler 2002). This is particularly the case in the contemporary political environment, where intense partisan competition for control of government strengthens and reifies the power of party identification to shape behavior. These strong divisions between the parties are reinforced by the media, which rarely reports about moderate voters looking for compromise. Instead, most articles center on passionate extremists (Levendusky and Malhotra 2016a). Given partisan social network homophily (Mutz 2006), most people only encounter extreme out-party partisans via these polarized media reports, which consequently exacerbate ingroup/outgroup thinking. As a result, most partisans systematically overstate the degree of polarization in the mass public (Levendusky and Malhotra 2016b). Unsurprisingly, many think that they have little in common with members of the other party and feel very negatively toward them (Pew Research Center 2016). Partisanship is no longer simply a description of one's issue positions; it has become an important and meaningful identity in contemporary American society that signals one's values and worldview.

Once such group-centric thinking is in place, it has the potential to drive behavior with significant consequences. Looking at other identities such as race, ethnicity, and religion, there is a host of evidence that individuals systematically favor those from their own group (see, among many others, Bertrand and Mullainathan 2004). Such findings have been replicated in a number of real-world settings, providing evidence with regard to outcomes such as the likelihood of being called for a job interview, being hired for a position, and one's starting wage. If we are correct that partisanship has become a strong social identity that cleaves society in meaningful ways, then partisanship should exert behavioral consequences similar to those of other prominent social identities, including influencing choices and decisions in the economic realm.

Although the economic consequences of partisanship may parallel those associated with other major social cleavages, there are both theoretical and substantive reasons for our focus on partisanship. First, as we discussed above, Americans are increasingly affectively polarized, making studying partisanship's effect on economic behavior timely and important in its own right (Iyengar, Sood, and Lelkes 2012). Second, changes in the media environment—particularly the growing options available to citizens allowing them to self-select into news coverage—will likely heighten polarization in the years to come (Holbert, Garrett, and Gleason 2010), making the understanding of polarization's apolitical effects even more pertinent. Finally, unlike other cleavages such as

¹This limitation is also shared by previous studies in this literature (e.g., Iyengar and Westwood 2015; Lelkes and Westwood 2017). A notable exception is a previous study that attempted to manipulate partisanship by persuading people to change their voter registration status (Gerber, Huber, and Washington 2010). Of course, even with this sort of design, the exclusion restriction may be violated, making it difficult to measure downstream effects. As explained below, when possible we conduct robustness checks to see whether people are discriminating on a variable other than partisanship.

race and religion, few Americans think about partisanship as a potential source of bias in their decision making. Indeed, to have even asked this question a few decades ago would have seemed silly: Of course partisanship would *not* shape people's apolitical behaviors. Because this traditionally was not an area of concern, there are no long-standing, established norms against partisan discrimination as there are against racial or religious discrimination (Iyengar and Westwood 2015). Consequently, the potential for partisan attachment to influence people's behavior may be large. Investigating whether the traditional, more benign view of partisanship is justified is therefore an important motivation for our study.

Study 1: Field Experiment in an Online Labor Market

Design and Procedures

Our first experimental test of the behavioral consequences of partisanship takes place within a nationwide online labor market: Amazon's Mechanical Turk (MTurk). Although MTurk is popular for conducting public opinion surveys (Berinsky, Lenz, and Huber 2012), its main purpose is as a platform for contract work, which is how we leverage it in this analysis.² We acted as the employers and offered workers a contracting job in which they were asked to copyedit website content for grammatical and typographical mistakes, a typical job on MTurk. After they clicked on the link to accept the job, subjects completed a brief questionnaire to measure their education and editing experience in order to increase the mundane realism of the task. We also collected other demographic information about the workers (age, gender). Subjects were told that this information was important to learn about the diversity and background of the employees. Most importantly, a question asking workers about their partisan affiliations was unobtrusively included within this questionnaire, allowing us to condition on partisanship at the individual level.

We then exposed subjects to the editing task, in which the treatment was subtly embedded. Subjects were shown approximately one page of text and told that the text was from the website of a new software company that we (the employers) hoped to launch soon. They were asked to read the text carefully and to mark all errors

that they found in the text in a comment box provided to them. This text also contained the randomly assigned treatment, which signals the partisanship of the company's founders. In the control condition, the text stated that the founders met while working for an unspecified nonprofit organization. In the two treatment conditions, subjects read that the founders met while working with either the Democratic Party or the Republican Party on their fundraising efforts. This design therefore allows us to unobtrusively signal the partisanship of an economic agent (in this case, the employer) to measure subjects' reactions.

The materials for Study 1 can be found in Online Appendix 2 in the SI. Descriptive statistics of the study participants and balance tests can be found in Online Appendix 3. Subject recruitment occurred in two waves. The first wave recruited 301 respondents in February 2015, and the second wave brought in an additional 935 participants in August 2016, for a total of 1,236 workers. Missing responses for four workers bring the final sample to 1,232. We report results using the combined sample in order to maximize statistical power. Results separated by wave (and including controls for wave) are presented in Online Appendix 4.

We measure three relevant dependent variables. First, after subjects completed the task, we asked them to state their reservation wage—how much they would require to do another similar job for us in the future. On average, subjects requested \$3.34 (s.d. = \$1.39), which, based on the average completion time of 15.1 minutes, equates to an hourly wage of \$13.30/hour (or \$23.60/hour, as implied by the median completion time).³ Second, we record the number of errors the subjects caught out of a possible total of 11 errors we purposely embedded in the text. On average, subjects properly corrected 5.60 errors (s.d. = 2.88). We aimed for the correction rate to be in the middle of the possible range to avoid floor and ceiling effects. Third, we count the total number of corrections made by the subjects to measure their general level of effort. On average, subjects made 6.85 corrections (s.d. = 4.13).

Theoretical Predictions

As discussed above, we expect partisanship to shape individuals' economic behavior with respect to the three

²We preregistered this study with EGAP as Study ID #20150206AA. The preanalysis plan, and deviations from it, are discussed in Online Appendix 1 in the supporting information (SI). This and all other studies were approved by a university institutional review board.

³In our analysis, the wage outcome variable is truncated to exclude requests above \$20. This removes a few outlying observations that likely result from typos or confusion about the question. The results are robust to other truncation thresholds (see Online Appendix 6 in the SI).

TABLE 1 The Effect of Employer Partisanship on Employee Behavior (Study 1)

	Dependent Variable					
	Wage (1)	Errors Caught (2)	Total Edits (3)	Wage (4)	Errors Caught (5)	Total Edits (6)
Co-partisan	-0.22*	-0.29	-0.58*	-0.21*	-0.22	-0.48
	(0.10)	(0.20)	(0.29)	(0.10)	(0.20)	(0.28)
Counter-partisan	0.01	0.05	-0.12	0.01	0.05	-0.11
	(0.10)	(0.20)	(0.29)	(0.10)	(0.19)	(0.28)
Education	—	—	—	0.06	0.48**	0.63**
				(0.03)	(0.07)	(0.10)
Experience	—	—	—	0.16**	0.22*	0.36**
				(0.04)	(0.09)	(0.13)
Constant	3.41**	5.68**	7.09**	2.80**	3.18**	3.66**
	(0.07)	(0.14)	(0.20)	(0.17)	(0.34)	(0.49)
Co-partisan minus Counter-partisan	-0.23*	-0.34	-0.45	-0.22*	-0.28	-0.37
	(0.10)	(0.20)	(0.29)	(0.10)	(0.20)	(0.28)
Observations	1,232	1,232	1,232	1,232	1,232	1,232
R ²	0.006	0.003	0.004	0.022	0.055	0.050

Note: Cell entries are OLS regression coefficients with standard errors in parentheses. Education is a 6-point scale (less than a high school degree to graduate degree). Experience is a 4-point scale (“no experience” to “substantial experience”).

* $p < .05$, ** $p < .01$ (two-tailed).

variables above: the reservation wage, the number of errors caught, and the number of corrections made.⁴ With respect to their reservation wage, we hypothesize that subjects will demand a lower wage from a co-partisan employer (relative to the control group) and a higher wage from a counter-partisan employer. Affective polarization implies that people expect a psychic cost from working for an opposite-party boss, and therefore they will seek additional compensation for performing the task. Analogously, they will demand a lower wage from a co-partisan employer.

The expected directional effects of partisanship on the other two outcome variables are less clear. Because partisanship is a powerful social identity, subjects could expect that an opposite-party employer will be more suspicious of them and therefore monitor them more carefully. If so, subjects might do a more rigorous job—in our case, catch more editing errors—when working for an opposite-party partisan, and catch fewer errors working for a co-partisan. This pattern of results may also arise if people believe that opposite-party employers are of lower quality and therefore expect more mistakes in the text *ex ante*. As an alternative theoretical expectation, performance may be a function of feelings toward

⁴We also collected several survey-based perception measures in our study that were not part of the preanalysis plan. We present those results in Online Appendix 5 in the SI.

the employer rather than expectations about the employer’s level of oversight or quality. If people harbor negative feelings toward members of the opposite party, then they may perform low-quality work due to lower motivation. This need not be due to a deliberate attempt at sabotage; rather, it may be an unconscious process of shirking in response to an undesired boss (Gift and Gift 2015).⁵

Results

We find that respondents demand a lower reservation wage from co-partisan employers (suggesting ingroup favoritism), but find no evidence that people demand compensating differentials in the form of higher wages to work for opposing partisans (outgroup aversion). Table 1 presents the results of ordinary least squares (OLS) models predicting the outcome variables, with dummy variables representing whether workers were assigned to the condition in which the employers were of the same party (*Co-partisan*) or a condition in which the employers were of the opposite party (*Counter-partisan*).⁶ The

⁵In our preanalysis plan, we focused only on this latter theoretical expectation.

⁶This approach conserves statistical power by pooling all respondents. Results conditional on party identification are reported in

omitted condition is the control condition with no information about employer partisanship. The first three columns do not include covariates, and the next three columns do.

As shown in column 1, compared to the control group, people demand 6.5% lower wages from same-party employers ($p = .02$).⁷ In terms of raw dollar amounts, reservation wages were \$0.22 lower in the co-partisan condition compared to the control group.⁸ On the other hand, we do not detect a difference between the average wage demanded in the control and counter-partisan conditions (difference = \$0.01, $p = .94$). In this study, it appears that employees are willing to give co-partisan employers a discount but do not charge a premium to a boss from the opposite party. The difference between the co-partisan and counter-partisan conditions is also statistically significant (difference = \$0.23, $p = .02$). Results are substantively similar when adjusting for covariates (see column 4).

The effects of the covariates are also in the expected direction, increasing our confidence that the editing task worked as expected. Workers with more education demand higher wages; going from no high school degree to a graduate degree is associated with a 9.0% increase relative to the average wage (about \$0.30 in raw terms). The effect of partisanship was therefore over 74% of the effect of moving from the bottom to the top of the education scale. As expected, workers with more experience also exhibit higher reservation wages; moving from the bottom to the top of the experience scale is associated with a \$0.48 increase in the requested wage, or a 14% increase relative to the average wage. Hence, the effect of partisanship was almost half the total effect of experience.

Interestingly, we find that workers perform slightly worse when working for co-partisan employers, though these effects do not quite reach conventional levels of statistical significance. As shown in column 2 of Table 1, workers caught about 0.29 fewer errors on average compared to the control group ($p = .15$); relative to those in the counter-partisan condition, the difference is 0.34 ($p = .09$). Again, as we would hope if the editing task were working properly, those with more education and editing experience catch more errors. Moving from the lowest to highest category of education is associated with

catching 2.4 more errors, and moving from the lowest to highest category of experience is associated with catching 0.66 more errors. The effect of having a neutral (rather than a co-partisan) boss on error correction is about 12% and 44% of the effect of education and experience, respectively. As with the wage demanded, we find little difference between the performance of individuals in the counter-partisan condition and the control group. Participants in the counter-partisan condition catch 0.05 more errors on average than those in the control group, a substantively and statistically insignificant difference ($p = .81$). Taken as a whole, the impact of the partisan treatment on the actual task performance was weaker than the effects on the reservation wage.

We might be worried that this result reflects lower attention on the part of co-partisans but not reduced effort. For instance, perhaps workers respond to a politically aligned employer by working with more enthusiasm, even if it is accompanied by a greater number of errors. To test this possibility, we run the same model as above, but instead of grading workers on the number of errors they successfully catch, we measure how many total edits they made to the document. As shown in column 3 of Table 1, workers in the co-partisan condition provided about 0.58 fewer edits on average than those in the control group ($p = .05$) and about 0.45 fewer than those in the counter-partisan treatment ($p = .12$). To put this into perspective, the effect of having a neutral (rather than co-partisan) boss on the worker's performance is approximately 18% of the effect of moving from the top to the bottom education category. Once more, we observe no statistically significant difference between the control and counter-partisan conditions. These effort effects are substantively larger than the effects on task performance.⁹

Looking across these different outcome variables, we find a consistent pattern of results: Although workers behave differently when they believe their employer shares their partisan affiliation—either by requesting a lower wage or by performing (slightly) worse on the editing task—they do not distinguish between a boss who is from the other party and one who does not announce her partisanship. Our results, then, appear to be driven by affinity toward in-party members rather than aversion

Online Appendix 4 in the SI. We cannot reject the null that partisan subgroups reacted similarly to the treatments. However, the results appear to be larger for Democratic workers.

⁷All reported statistical tests in the article are two-tailed tests.

⁸These effects are larger for strong partisans (\$0.45, $p = .006$) compared to weak/leaning partisans (\$0.15, $p = .23$).

⁹As mentioned above, the experiment is designed to measure how individuals react to political stimuli. It is not designed to assess the causal effect of individual-level partisanship. However, as a robustness check, we estimate models including demographics as well as those demographics interacted with the treatment variables (see Online Appendix 6 in the SI). These analyses suggest that it is unlikely that workers are discriminating on a variable other than partisanship.

toward the out-party. While our findings are consistent with a large literature in social psychology (Brewer 1999), they differ from much of the literature on affective polarization, which centers on out-party dislike as the key phenomenon (Iyengar and Westwood 2015). We further discuss this difference in the conclusion.

Study 2: Partisan Congruity and Consumer Choice

Design and Procedures

Our second study is a field experiment that examines how partisanship shapes the economic behavior of buyers and sellers in the marketplace.¹⁰ For this study, we contacted 1,787 individuals—primarily Democrats—who had previously completed a survey for another project. These individuals originally signed a petition on the website Care2 on climate change in July 2013. The original survey invitation was sent in February 2014. For this study, on October 4, 2016, each of the participants received an email with an offer to register their interest for purchasing a steeply discounted Amazon gift card. The card was worth \$50; participants would be asked to pay only \$25 if they were selected to buy the card. All subjects were told in the email that the cards were leftover thank-you gifts for volunteers at a fundraiser, thereby providing a justification for the discount.

While the offer for each individual was the same, the text of the email was slightly different depending on the experimental condition. Subjects were randomly assigned to one of three groups. In roughly a third of the emails, participants were told that the gift cards were left over from “our collaboration with volunteers on Democratic campaigns.” In another third of the emails, the text instead reported that the collaboration was with volunteers on Republican campaigns. Finally, in a neutral baseline condition, recipients were told that the cards were left over from work with volunteers from a nonprofit organization. Participants indicated their interest by clicking on a link and completing a survey that asked them to affirm their desire to purchase the card. At the conclusion of the study, we randomly selected five respondents to receive a gift card and notified all others who had completed the survey of the outcome.¹¹ Study materials can be found in Online Appendix 8 in the SI.

¹⁰The preanalysis plan for the study (and deviations from that plan) can be found in Online Appendix 7 in the SI.

¹¹Given the limited number of cards we had at our disposal, some of those who made the request to purchase had to be notified that the seller had run out of stock.

Because all of the subjects had completed the initial 2014 survey, we knew their previously stated partisan identification.¹² Based on this information, we determined whether subjects thought that we had collaborated with their preferred political party or with their political opponents. As in the employment study, we refer to subjects who received an email indicating we had collaborated with the aligned political party as receiving the co-partisan treatment, whereas those who received emails suggesting we had worked with the opposite party belong to the counter-partisan condition. Finally, subjects who were told that we had worked with an unspecified nonprofit organization constitute the control group.

For each respondent, we record whether a participant responded to the offer. Additionally, we note whether he or she continued to be interested in completing the transaction after the initial inquiry (which we label “request to purchase”). Each outcome captures a unique aspect of economic exchange. Willingness to respond at all captures whether people will even begin the exchange with different types of sellers; request to purchase measures whether they ask to see the transaction through. We analyze both dependent variables below and find substantively similar results, although the results are stronger for the outcome measuring initial responses.

In our analysis, we exclude pure Independents (i.e., those who do not lean toward one party or the other; $N = 51$), as well as those with an invalid email address ($N = 79$). The final sample therefore contained 1,657 respondents: 521 participants were assigned to the co-partisan condition; 555 were assigned to the counter-partisan condition; 581 received the neutral email. The distribution of political preferences in the overall sample skews heavily Democratic, which is unsurprising given that we originally obtained the email addresses by having people sign a petition about combating climate change. In our sample, 95% of respondents were classified as at least leaning toward the Democratic Party, with 46% of contacted participants identifying as strong Democrats.¹³ For descriptive statistics and balance tests, see Online Appendix 9 in the SI.

¹²We are assuming that partisan identification did not change between 2014 and 2016, which is reasonable given its high intertemporal stability (Green, Palmquist, and Schickler 2002). To the extent that partisanship changes, it will add noise to our estimates, making it harder to find effects of our experimental manipulations.

¹³Due to this partisan distribution, we could not conduct analyses similar to those presented in Online Appendix 6 in the SI.

TABLE 2 The Effect of Seller Partisanship on Buyer Behavior (Study 2)

	Dependent Variable					
	Responded to Email Full Sample (1)	Responded to Email Strong Partisans (2)	Responded to Email Weak/Leaning Partisans (3)	Request to Purchase Full Sample (4)	Request to Purchase Strong Partisans (5)	Request to Purchase Weak/Leaning Partisans (6)
Co-partisan	0.018 (0.010)	0.031* (0.015)	0.005 (0.013)	0.013 (0.009)	0.023 (0.014)	0.003 (0.011)
Counter-partisan	0.001 (0.010)	0.002 (0.015)	0.0003 (0.012)	0.003 (0.009)	0.002 (0.014)	0.003 (0.011)
Constant	0.021** (0.007)	0.019 (0.010)	0.022** (0.009)	0.017** (0.006)	0.019 (0.010)	0.016* (0.008)
Co-partisan minus Counter-partisan	0.017 (0.010)	0.029 (0.015)	0.004 (0.013)	0.011 (0.009)	0.022 (0.014)	0.000 (0.011)
Observations	1,657	775	882	1,657	775	882
R ²	0.002	0.007	0.0002	0.002	0.004	0.0001

Note: Cell entries are OLS regression coefficients with standard errors in parentheses.

* $p < .05$, ** $p < .01$ (two-tailed).

Theoretical Predictions

If partisanship shapes economic decision making, then subjects will respond differently to co-partisan versus counter-partisan sellers. As in Study 1, we expect that individuals are more likely to respond to a co-partisan seller, and more likely to say they are interested in buying the gift card (relative to a nonpartisan seller). Likewise, they should be less likely to respond to or pursue a transaction with a counter-partisan seller (relative to a nonpartisan seller). Further, we expect to find larger effects once we focus our attention on strong partisans. This is so for two reasons. First, strong partisans are those most likely to be affected by the partisan treatments. Second, strong partisans are less likely to have switched their partisan identification since February 2014 than those who lean toward one party or another (Green, Palmquist, and Schickler 2002), and therefore we can expect less attenuation bias in the treatment dummies due to mismeasurement. All of these expectations follow from our arguments above about the role that affective polarization plays in generating ingroup/outgroup thinking with respect to party identification.

Results

Of the 1,657 participants, 44 responded to the offer of the gift card, or 2.7%. Of these 44 individuals, 37 informed

us that they were ultimately interested in completing the transaction (2.2% of the sample and 84.1% of those who initially replied to the offer). Although these percentages seem small, they are actually quite high given the 0.1% transaction rates typically found in email solicitations per the digital marketing literature (Sahni, Zou, and Chintagunta 2017).

We find that subjects who received information indicating that the researchers had collaborated with their favored political group were more likely to respond to the offer, but these effects do not quite reach conventional levels of statistical significance. Table 2 reports results from OLS models similar to the ones reported for Study 1, predicting the outcome variables with dummy variables for the co-partisan and counter-partisan conditions.¹⁴ As shown in column 1, the response rate in the co-partisan condition was 1.8 percentage points higher than the response rate in the control group ($p = .07$) and 1.7 percentage points higher than the response rate in the counter-partisan condition ($p = .09$). Thus, the co-partisan treatment nearly doubles the baseline response rate. On the other hand, we estimate a relatively

¹⁴We estimate analogous models using logistic regression and obtain similar results (see Online Appendix 10 in the SI). We also estimate models conditioning on partisanship and cannot reject the null that partisan subgroups reacted similarly to the treatments (see Online Appendix 10). Although there are very few Republican buyers, the effects appear larger for Democrats.

precise zero effect for participants assigned to the counter-partisan condition in comparison to the control group (see column 1). Thus, while a signal that the seller had collaborated with an aligned political group increases the propensity for a subject to respond, learning that the seller had instead worked with the opposite political party does not seem to influence buyers.

These effects are concentrated among strong partisans, where the estimates achieve conventional levels of statistical significance (see column 2). In this subgroup, the response rate in the co-partisan condition is about 2.2 times higher than in the control ($p = .03$) and counter-partisan ($p = .05$) conditions. We do not observe any negative treatment effects of the counter-partisan condition compared to the control group, again estimating a near-zero effect. As shown in column 3, weak partisans did not exhibit any treatment effects; the acceptance rates were statistically and substantively similar across experimental conditions.

As shown in columns 4 and 5, the effects are a bit weaker (both statistically and substantively) when evaluating request to purchase instead of initial ad response. In the full sample, the response rate in the co-partisan condition was 1.3 percentage points higher than in the control condition ($p = .13$). Among strong partisans, this difference was 2.3 percentage points ($p = .10$). Again, we find no difference between the counter-partisan condition and the control group, as well as no effects among weak and leaning partisans.

In summary, although the propensity of engaging the gift card offer increases when we send a potential customer a signal that we are aligned with his or her preferred party—a result consistent with expressions of in-party affinity—we see no detectable decrease in the frequency with which subjects exposed to the counter-partisan treatment reply to the offer. This pattern of in-group affinity is similar to the results we found in Study 1 in the employment market.

To expand the external validity of the study and also capture a more diverse sample in terms of partisanship, we also carried out a similar experiment using an ecological design, where we assess the effects of partisan bias on consumer behavior at the market level (see Online Appendix 11 in the SI for full details and results). Overall, looking across our two studies of consumer behavior, we find that partisanship colors the willingness of buyers to engage with sellers. We have stronger individual-level evidence of this phenomenon in Study 2, and weaker ecological-level evidence in the market-level study, but both support the same substantive conclusion.

Study 3: An Incentivized, Population-Based Survey Experiment

Design and Procedures

We expand on our findings in the field experiments described above by addressing two questions: (1) To what extent does the partisan effect on economic behavior—as observed among participants in multiple marketplaces—extend to the general population? (2) What characterizes the individuals who exhibit the strongest partisan behavior?

Specifically, we investigate whether individuals forego guaranteed monetary gains in order to express their partisan preferences. Although a survey experiment is more artificial than our unobtrusive field experiments, it does offer us a significant advantage in terms of the control we maintained over the experimental context.¹⁵ The survey was conducted on a nationally representative sample collected by GfK (formerly Knowledge Networks), which is a leading source of high-quality survey data from random probability samples. This is important for our purposes since our aim here is to shed light on the behavior of American citizens broadly, and therefore obtaining a representative sample is crucial.

At the beginning of the survey, we measured respondents' partisan identification using the standard question wording employed by the American National Election Study (see Online Appendix 13 in the SI for the full questionnaire). Respondents also answered several additional questions. First, they reported their religious affiliation. Second, they were asked whether they live east or west of the Mississippi River. We use these two questions to develop baseline (apolitical) benchmarks, described below. They also answered two open-ended distracter questions.¹⁶

The final item in the survey constitutes the experimental manipulation. We told respondents: "As an additional thank you for filling out this questionnaire, we would like to give you a bonus cash payment. You can choose one of the two options below." One option is a simple payment of \$3, which we refer to as a nonpartisan offer. The second option, which we call the partisan offer, is a higher payment plus a donation to a group that

¹⁵These data were collected via Time-sharing Experiments for the Social Sciences (TESS). Our TESS proposal serves as our preanalysis plan for this study (see Online Appendix 12 in the SI).

¹⁶The questions were: (1) "Think back to the last time you saw a movie in a theater. What was the name of the movie?" (2) "Think about where you would like to take your next vacation. Where would you like to go?"

is presumably disliked by the respondent. Respondents were randomly assigned to one of four experimental conditions, each of which presents a different offer.

First, respondents could be randomized into the baseline political treatment, which we call the partisanship condition. Here, they are given the choice between the nonpartisan offer of receiving \$3 or the partisan offer of receiving a \$6 payment themselves with an additional \$4 donation to the opposing party's national committee (which we explained works to elect candidates from the other party). The donation here is designed to be small enough such that respondents know it will not affect the outcome of elections but large enough to evoke animus toward a disliked group. In all conditions, respondents saw this trade-off both as text and as figures in a comparison table to ensure that they actually understood the proposed exchange. To express their political preferences and avoid contributing to the other party, respondents have to leave half of the money they have been offered on the table.¹⁷ This design produces a fairly straightforward test of the economic consequences of partisan sentiment: Will respondents forego gains to express their partisan preferences?

Second, respondents could be offered the same setup as the baseline political treatment, except now they are offered \$9 instead of \$6 (all other variables, including the payment to the other party, remain the same); we refer to this condition as the higher payment condition.¹⁸ In this case, instead of simply doubling the payment they receive for helping the other party, we triple it, making it even more difficult to accept the nonpartisan offer. This condition allows us to test the elasticity of these partisan effects: As the cost of expressing one's partisanship increases, how does behavior change?

In the third condition (the religion condition), respondents are presented with an offer to fund a religious outgroup. Christian respondents were told they would be funding the American Atheists, and atheist/agnostic respondents were told they would be funding the Christian Legal Society.¹⁹ All dollar amounts remain the

same: a \$6 payment to the individual and \$4 to the religious outgroup. This provides an apolitical benchmark against which we can compare the effects of partisanship. Religion is a large and socially meaningful cleavage (Pew Research Center 2016). By assessing how the effects of partisanship compare to religion, we can better understand the effect sizes in the partisan conditions and put them into context.

Finally, in the fourth condition (the geography condition), respondents have the option of funding another geographic region. We divided people into those east and west of the Mississippi River using the item above. We told those east (west) of the Mississippi River they would be funding the Association of Western (Eastern) States, which advocates for policies that benefit those living west (east) of the Mississippi River. We use this as a relatively meaningless placebo division: The Mississippi River does not represent any meaningful division in American political, economic, or social life. This scenario directly parallels the minimal group paradigm from social psychology (Tajfel and Turner 1979).²⁰ This condition therefore provides a floor effect: How much will people pay to express their group identity when that group identity is trivial? Contrasting the effects of partisanship to those of religion and geography helps us contextualize our findings.

We conducted this experiment on 3,266 respondents²¹ from the GfK Knowledge Panel between May 26, 2015, and June 15, 2015. GfK recruits subjects via probability sampling techniques such as address-based sampling and random digit dialing, and panelists complete surveys in exchange for cash and other incentives. For this sample, 68.3% of the panelists invited to complete the questionnaire did so.²²

¹⁷To avoid deceiving subjects, we paid all subjects and organizations the amounts they were owed as a result of the experiment.

¹⁸Throughout the analysis, we treat Independent leaners as partisans (Keith et al. 1992), though omitting them does not change our substantive conclusions. In both the baseline partisanship and higher payment conditions, we omit the small number of respondents (44 subjects) who are pure Independents since they lack a meaningful reference category.

¹⁹We only include Christians (Protestants, Catholics, and those who inputted another Christian denomination when selecting "Other") and atheists/agnostics when analyzing the religion condition (excluding 107 respondents or 11.3% of the respondents assigned to that condition).

²⁰According to the minimal group paradigm, individuals will show favoritism toward their own group even based on trivial distinctions, such as whether they over- or underestimate the number of dots in a painting, have a Social Security number that begins with an odd or even digit, or, in this case, live east or west of the Mississippi River.

²¹To maximize statistical power, we did not randomize an equal number of subjects to each experimental condition, but instead based the size of each condition on the expected acceptance of the nonpartisan offer (determined via a pretest). For example, because we expect very few people to accept the nonpartisan offer in the geography condition, we only randomized a small number of subjects into that condition. The number in each condition was as follows: baseline partisanship, $N = 1,169$; higher payment, $N = 985$; religion, $N = 876$; and geography, $N = 201$.

²²As expected given random assignment, we obtained balance across experimental treatments on various demographic/attitudinal variables; see Online Appendix 14 in the SI. Descriptive statistics of the sample are also presented in the online appendix.

Theoretical Predictions

If partisanship shapes economic decisions, then we would expect respondents to forego material gains when those gains require them to take an action that benefits the other party. As in the other experiments, because of affective polarization, individuals should be hesitant to benefit the other party and should be more likely to accept the nonpartisan offer (Ryan 2017). While this can be because of pure animus (people dislike those from the other party) or from strategic motivations (people do not want to help the opposition, even in a trivial way), the end result will be the same. Comparing across conditions, we expect that subjects will be least likely to accept the nonpartisan offer (lower payment, but no benefit to the other side) in the geography condition compared to the other three conditions. That is, subjects will be less likely to pay to express their minimal-group geographic preference, but they will leave money on the table to express religious or partisan preferences. The higher payment may cause some people to be willing to donate to the other party.

Results

Table 3 reports results from regression models predicting whether the respondent chose the nonpartisan option (foregoing material gains to avoid benefiting the other party), with dummy variables representing the four experimental conditions with the partisanship condition set as the baseline. We find strong evidence that partisanship leads people to forego economic gains and therefore distorts conventional decision making. The acceptance rate for the nonpartisan option in the partisanship condition was 75.4%. This means that three-fourths of respondents were willing to give up a doubling of their bonus payment simply to avoid making a donation to the other party. Partisanship and affective polarization have significant behavioral consequences.²³

²³Readers might worry if two factors influenced our results. First, some subjects might select the nonpartisan offer as a result of pragmatic concerns rather than due to animus (i.e., they fear the \$3 will help the opposing party elect their candidates). We designed an additional experiment to test this possibility by making offsetting donations to both parties. We find that some subjects do act more out of pragmatic concerns rather than animus, but that animus is still an important part of the story. Specifically, about half of the treatment effect can still be attributed to animus even when explicitly accounting for instrumental motivations. Second, readers might wonder whether the study design inflated the effects by asking about partisanship in the same survey prior to the experimental stimulus (i.e., that we primed party identification). We conducted a randomized experiment to show that the acceptance rate of the nonpartisan offer is unaffected by whether party identification is asked at the beginning of the survey. We discuss both studies and their results in Online Appendix 17 in the SI.

Does this high percentage reflect aversion to benefiting the opposing party, or is it instead merely a reflection of people's unwillingness to donate to *any* political cause? To address this issue, we conducted a follow-up experiment on Amazon's Mechanical Turk,²⁴ where we offered people a choice between receiving a payment of \$0.50 and receiving a higher payment of \$1.00 plus a \$0.50 donation to their *own* party. In this case, the expectations are reversed. If our mechanism is correct, then most people should be selecting the latter option since they receive personal benefits *and* get to help out their own party. On the other hand, if people were against donating to anything political, then they would prefer to take the lower payment. However, we do indeed find that the acceptance rate of the higher payment is 85% (with a 95% confidence interval of 81% to 90%), which is substantially different from the acceptance rate of 24.6% in the original experiment. This makes us confident that our results are not simply due to an aversion to politics—even if we subtracted from the main results 15 percentage points (i.e., the percentage of people who seemingly refuse to give to any political cause), we still find that a substantial majority of subjects reject the higher payment.

Returning to our main experiment, the acceptance rate in the partisanship condition was significantly higher than the 33.7% acceptance rate in the minimal-group geography condition ($p < .001$), again suggesting that partisanship produces real behavioral ramifications. The acceptance rate in the partisanship condition was statistically indistinguishable from and substantively similar to the 77.0% acceptance rate in the religion condition ($p = .43$), meaning that partisanship reflects as large a cleavage as religion, another long-standing and deeply rooted division in American society.

Further, we find that partisan preferences were inelastic. Increasing the payment in Option B from \$6 to \$9 (i.e., doubling the price of expressing a partisan opinion from \$3 to \$6) decreased the acceptance rate of the nonpartisan offer by only 4.8% ($p = .01$). Although this difference is statistically significant (given that the study is very well powered), it is not that substantively large, implying an elasticity of only 0.07.²⁵

²⁴This study was conducted between November 16, 2015, and November 29, 2015, on a sample of 272 respondents.

²⁵One might argue that even \$9 is not enough to change respondents' behavior, but if we had offered them (say) \$500, their behavior would change. Although this argument is surely true in the limit, two factors work against it here. First, most GfK panelists complete surveys in exchange for relatively modest payments and rewards (typically around \$1), making a \$9 payment especially salient. Second, a large literature in economics suggests that these sorts of preferences are largely inelastic to payment amounts in

TABLE 3 Foregoing Material Gains to Avoid Helping the Other Party (Study 3)

	Dependent Variable			
	Accept Non- Partisan Offer (1) OLS	Accept Non- Partisan Offer (2) OLS	Accept Non- Partisan Offer (3) Logit	Accept Non- Partisan Offer (4) Logit
Religion Condition	.02 (.02)	.06** (.02)	.09 (.11)	.31* (.13)
Geography Condition	-.42** (.03)	-.35** (.04)	-1.80** (.17)	-1.46** (.20)
Higher Payment Condition	-.05* (.02)	—	-.24* (.10)	—
Strong Partisan	—	.21** (.03)	—	1.33** (.18)
Religion × Strong Partisan	—	-.15** (.04)	—	-1.03** (.25)
Geography × Strong Partisan	—	-.20** (.07)	—	-1.29** (.38)
Constant	.75** (.01)	.68** (.02)	1.12** (.07)	.78** (.08)
Observations	3,159	2,176	3,159	2,176
R ² /Log Likelihood	.05	.10	-1,808.02	-1,181.55

Note: Standard errors are in parentheses.

* $p < .05$, ** $p < .01$ (two-tailed).

In column 2 of Table 3, we present estimates from a model including interactions between the treatment dummies and an indicator representing strong partisans.²⁶ The acceptance rate of the nonpartisan offer among strong partisans in the partisanship condition was an extremely high 89.1%.²⁷ This figure is statistically distinguishable from, and substantially larger than, the 68.5% acceptance rate among weak and leaning partisans ($p < .001$). Further, this 20.7 percentage point difference is significantly larger than the 6 percentage point gap between the two groups in the religion condition, as shown by the estimate of the interaction term “Religion × Strong Partisan” ($p < .001$). Finally, a placebo test

similar experiments until the amounts become very large, equivalent to a sizable fraction of monthly/yearly income (Slonim and Roth 1998).

²⁶We exclude respondents assigned to the higher payment condition in this model because that condition was not intended to be a baseline/placebo group. We did not find any heterogeneity by partisan strength in that condition (see Online Appendix 15 in the SI).

²⁷As shown in the questionnaire, we also asked respondents about their opinions on social and economic issues. Moderation by these variables is discussed in Online Appendix 15.

finds that strength of partisanship does not predict the acceptance rate in the geography condition. The difference between the subgroups—represented by the summation of the coefficients “Strong Partisan” and “Geography × Strong Partisan”—is about 1 percentage point ($p = .89$).²⁸

It is not terribly surprising to find that strong partisans overwhelmingly reject the partisan offer (indeed, if we had not, it would have called our design into question). What is more surprising and unexpected is that fully two-thirds of weak and leaning partisans similarly reject the partisan offer. Even if one adjusts this figure downward by the 15% of people who reject all political donations, it still suggests that a majority of people, including among those with only modest ties to their party, are willing to forego economic gains to express their partisan identities. The economic consequences of partisanship are therefore

²⁸As in Study 1, we might think that individual-level partisanship is confounded by some correlated, omitted variable. As shown in Online Appendix 16 in the SI, we find little heterogeneity in the treatment effects by individual-level demographics, suggesting that it is partisanship per se that is driving how people are responding to the treatment information. We also report results conditional on partisanship and find no significant differences between partisan subgroups.

not confined to a narrow segment of the public but rather extend broadly throughout the electorate.

Conclusion and Implications

These experiments highlight the extent to which people's partisan commitments influence their economic choices and behaviors. Clearly, in the contemporary United States, partisanship's effects extend well beyond the political realm. For many Americans, politics has become an integral feature of their social identities, influencing and shaping their behavior in domains seemingly unrelated to politics. Although our results vary across contexts somewhat, overall we show that partisanship can systematically condition economic behavior.

Our results are notable in a number of respects. First, unlike earlier studies, we are able to present rich evidence from outside the survey context. Previous studies of the effects of partisanship in the contemporary era focus largely on survey responses, which may simply reflect partisan cheap talk or reactions to researcher monitoring. Our results from field experiments on consumers and workers are therefore particularly valuable and can serve as a building block for other studies of partisanship in nonpolitical realms.

Second, in our experiments, we show some support for both ingroup favoritism and outgroup aversion as explanations for our effects. Our results in Study 1 (where we find a co-partisan discount) and Study 2 (where we find especially strong partisans more likely to respond to a co-partisan seller) are more consistent with ingroup favoritism, which concords with a large literature in social psychology (Brewer 1999). Yet our findings in Study 3 are consistent with outgroup animus, keeping with previous studies of polarization (e.g., Iyengar and Westwood 2015). Given that we conduct only a handful of experiments, this study cannot definitively explain this variation. However, we think our results do point to a potentially important dimension of the problem: the saliency of partisanship and politics. In the survey context, the political information is not subtly signaled but instead is an explicit part of participant choice, perhaps making outgroup animus more attractive. Yet in the real world, partisanship is a secondary piece of information people have at their disposal. As a result, even as partisanship shapes economic decision making, the effect is channeled more through ingroup favoritism rather than by punishing members of the out-party (as compared to a nonpartisan baseline). Ultimately, more work is needed on this topic, but our findings here suggest that more field experiments, in

addition to survey experiments, are needed to fully establish these mechanisms.

The findings also underscore a broader and important implication of our study—the power of partisanship as a social identity. Others have shown how such partisan social identities powerfully shape political behaviors and attitudes (e.g., Mason 2015). We show how these consequences spill over into apolitical domains and distort economic transactions, suggesting that the consequences of these findings are even more significant than previously appreciated. In an era of polarization, partisanship's power is profound indeed.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Online Appendix 1: Pre-Analysis Plan and Deviations, Study 1

Online Appendix 2: Materials for Study 1

Online Appendix 3: Descriptive Statistics and Balance Tests, Study 1

Online Appendix 4: Results by Wave of Data Collection and Partisanship, Study 1

Online Appendix 5: Survey-Based Results on Perception of Firm, Study 1

Online Appendix 6: Robustness Checks, Study 1

Online Appendix 7: Pre-Analysis Plan and Deviations, Study 2

Online Appendix 8: Study Materials, Study 2

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Online Appendix 10: Logistic Regressions and Results by Partisanship, Study 2

Online Appendix 11: Market-Level Consumer Study

Online Appendix 12: Pre-Analysis Plan, Study 3

Online Appendix 13: Questionnaire, Study 3

Online Appendix 14: Descriptive Statistics and Balance Tests, Study 3

Online Appendix 15: Additional Results, Study 3

Online Appendix 16: Robustness Checks and Results by Partisanship, Study 3

Online Appendix 17: Additional Studies, Study 3