Are Voters More Likely to Contribute to Other Public Goods? Evidence from a Large-Scale Randomized Policy Experiment

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Voting has been described as a contribution to a public good. Are people who vote frequently therefore more likely to contribute to other public goods? Does partisanship affect how likely a person is to engage in these cooperative behaviors? Although surveys suggest that the answer to these questions is "Yes," few empirical studies examine these questions using observed behaviors. We examine them in the context of a large-scale, randomized controlled trial to induce voluntary action in a common-pool resource dilemma. During a drought in the southeastern United States, pro-social messages that encouraged water conservation were randomly assigned to 35,000 out of 106,000 households. Frequent voters in primary and general elections (1990–2008) were substantially more responsive to the messages, but there was no detectable difference in the responses of Republican and Democrat households. Our results suggest that internalized pro-social preferences promote action for the public good across behavioral contexts.

For decades, scholars have argued that voting is a contribution to a public good and that many voters view it as a civic duty (Campbell et al. 1960; Gerber and Green 2004; Knack 1992; Mueller 1989). In fact, voters often receive an "I voted" sticker after casting their ballots in order to publicize their participation in this socially valued activity (Riker and Ordeshook 1968). Recent experiments have strengthened this view of voting as a pro-social action by demonstrating the power of nonpecuniary incentives to affect voter turnout (Arceneaux and Nickerson 2009; Davenport et al. 2010; Gerber, Green, and Larimer 2008, 2010; Gerber and Rogers 2009). These pioneering experiments have made important contributions to our understanding of the *paradox of voting*—i.e., why individuals vote despite incentives to free ride (Downs 1957; Fowler 2006; Knack and Kropf 1998; Olson 1965).

We move this literature in a different direction by examining the extent to which voters are more likely to contribute to *other* public goods. Recent studies show that some individuals possess pro-social preferences, which affect behavior in laboratory experiments and stated intentions to engage in collective actions like voting (Dawes, Loewen, and Fowler 2011; de Oliveira, Croson, and Eckel 2008; Edlin, Gelman, and Kaplan 2007; Fowler 2006; Fowler and Kam 2007; Jankowski 2002, 2007; Sandroni and Fedderson 2006). Results from these studies suggest that some individuals behave as if they have internalized pro-social preferences that guide whether or not they contribute to public goods (e.g., cooperators versus egoists).

In democratic systems, the degree to which, and circumstances under which, individuals act pro-socially affects how governments solve social dilemmas. Individuals' decisions determine collective outcomes and thus affect the types of policies needed to promote the collective good. Given the provision of public goods is a primary purpose of government, understanding pro-social behavior is thus critical for political science. In particular,

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elucidating the stability of pro-social preferences across decision domains is an important task for political scientists: is the propensity to take pro-social action a stable predisposition across domains, or is it domain-specific?[1]

To answer this question, we examine the behavior of voters and nonvoters and ask, "Are people who vote frequently also more likely to contribute to other public goods?" Additionally, we explore the role of partisanship: does partisanship affect how likely a voter is to engage in pro-social behavior? Although surveys of self-reported behaviors suggest that the answers to these questions may be "Yes," no empirical study has examined these questions using revealed (observable) behaviors. We examine these questions with data from a randomized controlled trial that aimed to induce voluntary action in a common-pool resource dilemma (water conservation during a drought).

Although field experiments in political science are increasingly common, the vast majority focus on voter turnout (Arceneaux and Nickerson 2009; Gerber and Green 2000a, 2000b; Green 2011; Green and Nickerson 2003; Michelson, Bedolla, and McConnell 2009) and none explore voter willingness to contribute to the provision of other public goods. There is clear evidence from the political science literature that pro-social stimuli can affect turnout decisions (Davenport et al. 2010; Gerber and Green 2010; Gerber, Green, and Larimer 2008, 2010; Gerber and Rogers 2009; Green 2011). But political scientists know less about the degree to which pro-social action in these contexts is driven by internalized pro-social preferences to contribute to the public good as opposed to external threats of making noncompliance public. In the latter case, pro-social action results from the desire to avoid public sanction or shame rather than an internalized predisposition to cooperate. The design we describe below allows us to examine whether individuals display stable pro-social preferences across domains in the absence of a threat to make one's behavior public.

Our field experiment builds on a vast literature in the social sciences that uses laboratory experiments to elucidate the voluntary provision of public goods, including the exploitation of common-pool resources (e.g., Andreoni and Croson 2008; Dawes and Thaler 1988; Fehr and Gachter 2000; Fishbacher, Gachter, and Fehr 2001; Hamman, Weber, and Woon 2011; Ostrom 2006). Few such experiments, however, examine the stability of pro-social behavior across domains, and the few that do rely almost exclusively on comparisons between laboratory studies and field experiments—with conflicting results (de Oliveira, Croson, and Eckel 2011; Grossman 2011; Levitt and List 2007; Voors et al. 2012). In contrast, our study uses two naturally occurring decision domains (i.e., a "field-field" comparison).

During a drought in the southeastern United States, a water utility randomly assigned pro-social, norm-based messages that encouraged households to conserve water (Ferraro and Price 2013).[2] These messages caused households to use less water, on average. We merge data from this field experiment with individual-level data on voting histories in primary, general, and special elections (1990–2008) to explore whether a household's voting frequency is correlated with its responsiveness to a randomized pro-social message promoting water conservation.

We find that frequent voters are more responsive to the randomized pro-social messages, a result that suggests internalized pro-social preferences are stable across contexts.[3] Our experimental design reduces the likelihood of spurious inferences because past voting decisions are uncorrelated with whether or not a household receives a conservation message. Correlation is absent because the sample is large (>100,000 households), and treatments were randomized within small neighborhood units (see Data and Methods).

Our analysis also sheds light on the poorly understood relationship between partisanship and willingness to contribute to environmental public goods. We

[1]Data and replication files for this study are available at AJPS Data Archive on Dataverse: hdl:1902.1/21394. A preference comprises "comparative evaluations of (i.e., a ranking over) a set of objects" (Druckman and Lupia 2000, 2). The stability of private (selfish) preferences is a long-standing subject of debate among scholars (Converse 1964; Tversky and Kahneman 1981; Zaller 1992). Preference stability is central to normative debates regarding the extent to which citizens are capable of playing a meaningful role in democratic governance (Bartels 2003; Druckman 2004). Moreover, economic models of decision making typically assume stable preferences.

[2]Although conservation responses to the messages can also yield private benefits, subsequent analyses of the experimental data support the interpretation that the treatment effects were driven by pro-social preferences rather than rival, private-preference-based mechanisms (e.g., signals of privately optimal behavior to agents with incomplete information) (Ferraro and Miranda 2013).

[3]Identifying the specific form or forms of these stable internalized preferences is beyond the scope of this study. The economic and political science literatures postulate a range of plausible forms of pro-social preferences in collective action settings, including adherence to a civic duty or social norm (Bolsen 2013; Chong 2000; Cialdini 2003; Davenport et al. 2010; Gerber, Green, and Larimer 2008, 2010; Gerber and Rogers 2009; Knack and Kropf 1998; Kropf 2009; Schultz 1999; Thogersen 2008), conditional cooperation (Allcott 2011; Alpízar, Carlsson, and Johansson-Stenman 2008; Frey and Meier 2004; Shang and Croson 2009), altruistic preferences (Fowler 2006; Fowler and Kam 2007), and civic norms promoting cooperation (Beck et al. 2002; Huckfeldt and Sprague 1992; Ikeda and Richey 2005; Nickerson 2008; Putnam 1966). See Gächter, Nosenzo, and Sefton (2012) for one recent study that tries to differentiate rival pro-social preference forms (norms vs. altruism) in a laboratory gift-exchange experiment.

investigate whether responses to the experimental treatments are conditional on partisanship, as captured by voting patterns in primary elections. Survey results suggest that Republicans are less willing than Democrats to contribute to environmental public goods, but we know of only one study that measures observed behavior, rather than self-reported attitudes. We find that Republican and Democratic primary voters respond similarly to pro-social conservation requests. Overall, the results from our study have implications for understanding *when* and *why* individuals take actions that promote the public good, as well as for crafting policy interventions targeting behavior change.

Private Actions for the Public Good

Previous research that explores the stability of pro-social preferences across decision contexts makes lab-to-field comparisons of behavior (Benz and Meier 2006; Carpenter and Seki 2011; de Oliveira, Croson, and Eckel 2008; Karlan 2005; Voors et al. 2012) or uses self-reported measures of field behavior (Knack 1992; Knack and Kropf 1998; Kropf 2009). The external validity of laboratory experiments that measure pro-social behavior, however, is controversial (Barmettler, Fehr, and Zehnder 2012; Levitt and List 2007; also see Jerit, Barabas, and Clifford 2013), and self-reported measures of intentions are subject to social desirability biases (and the studies that use them fail to control for confounding variables).

In contrast, our study uses observed behaviors in two naturally occurring decision domains and an experimental design to aid with inferences. Our design allows us to observe whether a tendency to express pro-social behavior in one context (voting) is correlated with responsiveness to a randomized request to contribute to the public good in an unrelated common-pool resource dilemma. Voting is a contribution to a pure public good because the benefits are nonrival and nonexcludable. The likelihood that one's vote will be pivotal in any election is close to zero, resulting in incentives to free ride and abstain from contributing to the public good. Conversely, an individual's water use is not a pro-social behavior per se; it is a private consumption decision. However, an explicit request to reduce water consumption during a local drought is an appeal to cooperate by refraining from using a rival but nonexcludable public good. As in voting, water conservation is costly and free riding on the nonexclusive benefits is possible. As Kropf noted, a positive correlation between pro-social behavior in one domain and pro-social behavior in a different domain suggests "the same underlying latent variable—that is a sense of cooperation—affects incidences of these behaviors" (2009, 544; see also Brehm and Rahn 1997; Fisher and Ackerman 1998; Knack 1992; Knack and Kropf 1998).

In addition to informing our understanding of the stability of pro-social preferences, our study also contributes to related literature that explores how drawing attention to social norms can increase voter turnout (Davenport et al. 2010; Gerber, Green, and Larimer 2008, 2010; Gerber and Rogers 2009). These studies provide clear evidence that turnout is shaped by nonpecuniary incentives, but the experimental pro-social messages often include external pressure to comply with prevailing norms—e.g., fear that one's neighbors will be informed that he or she did not vote. Thus, it can be difficult to disentangle whether pro-social action results from (1) the manifestation of an internalized pro-social disposition or (2) compliance with prevailing norms so as to avoid isolation or social sanction. In our study, the threat of public knowledge of one's actions is absent.

Inspired by these disparate literatures, our first research question asks: *Are individuals who contribute to the public good in one policy-relevant context also more responsive when asked to contribute in a different context?* Our study provides the first answer to this question using revealed behavior measures and tests the following hypotheses:

Null Hypothesis: There is no relationship between the frequency with which people vote and their response to a pro-social appeal to conserve water during a drought.

Alternative Hypothesis: The more frequently one votes, the more responsive he or she will be to a pro-social appeal to conserve water during a drought.

A second distinct research question we explore in this article relates to whether or not partisanship affects responsiveness to a randomized intervention requesting pro-social action in a common-pool resource dilemma. Recently, scholars have begun to explore the impact of partisan identification on behavior in laboratory settings that require trust and cooperation (Carlin and Love 2013; Fowler 2006; Fowler and Kam 2007). For example, Carlin and Love (2013) find that copartisans in the United States exhibit greater trust compared to rival partisans in a dictator game. Rand et al. (2009) demonstrate that in-group biases shaped the behavior of partisans in a dictator game during the 2008 presidential election season. For instance, in experiments conducted during the campaign, supporters of Clinton and Obama gave systematically more money to supporters of the same candidate than to supporters of the other candidate. In another recent lab experiment, Gromet, Kunreuther, and Larrick (forthcoming) find that conservative students were less likely to purchase a more expensive energy-efficient light bulb when it was labeled with a proenvironmental message. These studies suggest that looking at the relationship between partisanship and willingness to contribute to a public good would be a fruitful path of inquiry.

It is no secret that concern for the environment has become a liberal identifier. In 2008, a survey revealed a 34% gap between Democrats and Republicans in their agreement that global warming exists (Dunlap and McCright 2008; also see McCright and Dunlap 2011). In 2009, a survey revealed a 23% gap between the Democrat and Republican agreement "that people ought to pay higher prices to protect the environment" (Pew Research Center 2009). These gaps are even greater among elected officials. The 2010 National Environmental Scorecard of the League of Conservation Voters gives the House Democratic leadership a score of 100 (out of 100) and the Republican leadership a score of 0. The Senate Democratic leadership also received a score of 100, while Republicans received a 5. Although environmental concern has not always correlated strongly with partisan identification in the United States, the last decade has seen increasing polarization along party lines. Guber explains, "Today, political ideology and partisan identification are important determinants of *general* environmental concern, and are not exclusive to global warming" (2013, 94). Thus, partisans may differ in the way in which they respond to requests for pro-social action in a common-pool resource dilemma.

We know of only one study to examine the relationship between citizen partisanship and environmental action (rather than stated preferences or intentions). In the context of household electricity consumption, Costa and Kahn (2013) found that a subgroup of liberals in liberal neighborhoods responds more to the receipt of a Home Energy Report and reduces its energy consumption more than a subgroup of conservatives in conservative neighborhoods.[4]

These studies motivate our second research question: *Does partisanship affect responsiveness to pro-social appeals to contribute to environmental public goods?* Our study tests the following hypotheses:

Null Hypothesis: Democratic and Republican primary voters, on average, respond equally to a pro-social appeal to conserve water during a drought.

Alternative Hypothesis: Relative to Republican primary voters, Democratic primary voters are more responsive, on average, to a pro-social appeal to conserve water during a drought.

Data and Methods

Study Site

Ferraro and Price (2013) report that in 2006, Cobb County, Georgia, contained an estimated 679,325 people—71% of whom identified as white/Caucasian and 23% as black. The 2000 census reported 227,487 households, approximately two-thirds of which were owner occupied. In addition, 89% of adults over the age of 25 had a high school degree, and 40% had a bachelor's degree. In 2004, median household income was estimated at $52,936. Cobb County, the home district of the former Republican House Speaker Newt Gingrich, is not known for environmentalism. Its current congressmen have some of the lowest League of Conservation Voter scores (LCV 2008) recorded in 2007 and 2008 (Price 9% and Gingrey 0%).

Experimental Data

The field experiment tested whether conservation messages affected monthly household water consumption between June and September 2007 (Ferraro and Price 2013). Ferraro partnered with the Cobb County Water System (CCWS)—an agency of the Cobb County Government in the metropolitan area of Atlanta, Georgia—to administer three versions of a water conservation message to 35,093 households, with another 71,779 households assigned to a control group (106,872 in total, all single-family dwellings).[5] In 2000, Cobb County was Georgia's second

[4]Specifically, Costa and Kahn stated, "We find that among political liberals who purchase electricity from renewable resources, who donate to environmental causes, and who live in a census block group where the share of liberals is in the top 75th percentile, receiving a HER led to reductions in electricity usage of 3.6%. In contrast, among political conservatives who do not pay for renewable electricity, who do not donate to environmental groups, and who live in a census block group where the share of liberals is in the bottom 25th percentile, receiving a HER led to reductions in electricity usage of 1.1%" (2013, 682).

[5]CCWS distributes treated surface water to about 170,000 Cobb County customers. About 150,000 of these customers reside in single-family dwellings. Monthly pre- and postexperiment water data were provided by the CCWS billing department. Customers whose billing address had changed between May 2006 and April 2007 were excluded from the experimental sampling because one of the treatments required water-use history. Customers with consumption lower than 4,000 gallons per month were excluded by the utility. Water meters are read and bills are sent daily based on a household's assignment to one of 390 "meter routes." Ferraro and Price explain, "To ensure that we have no systematic differences across treatments in the day of the month an outcome is measured, we randomized treatment assignment within meter routes

largest user of the public water supply, using almost 8% of Georgia's public water supply (Ferraro and Price 2013). The experiment was designed to provide feedback to CCWS on the effectiveness of mail-based conservation programs.

The experiment comprised a control group and three treatment groups:

(1) **Treatment 1**: An "information only" message that comprised a two-sided tip sheet about ways the household could conserve water.
(2) **Treatment 2**: The tip sheet (Treatment 1) plus a pro-social appeal encouraging customers to "do their part" and "work together to use water wisely."
(3) **Treatment 3**: The tip sheet (Treatment 1), the pro-social appeal (Treatment 2), and a social comparison. The social comparison contrasted each household's water use from June to October 2006 to the median county household use for the same period and indicated the percentile in which the household fell during this period (see supporting information, Appendix A1, for a copy of each message).

The first treatment, a technical advice letter, works through a single channel—scrutiny—and has the smallest effect on household water consumption. The second treatment augmented the technical advice letter by including an appeal to pro-social preferences highlighting the importance of conserving water. This appeal highlights a social norm—conservation and concern for the environment—and led to additional reductions in average household water use. Treatment 3 makes the social norm more salient by including a social comparison and may heighten the extent to which the household believes its actions are scrutinized. Ferraro and Price (2013) predict and find evidence that this higher level of scrutiny generated the largest reductions in overall water consumption. The messages were all mailed during the week of May 21, 2007, via first-class mail in official CCWS envelopes. Four weeks later, all treatment households received a second copy of the tip sheet (and no other component of the treatments). Households were unaware that the messages were part of an experimental design (a so-called "natural field experiment"). Our sample includes all households in Ferraro and Price's field experiment.

which correspond to neighborhood sections. . . . [This] increases the precision of the estimates of treatment effects provided that unobservables affecting treatment response are more similar within rather than between meter routes" (2013, 69).

Voting Data

We merged data from the field experiment with individual records of voter turnout (purchased from www.aristotle.com) and county tax-assessor data on the home.[6] We linked households from the experiment with the voter database by matching addresses of individuals within households. Using addresses and address-matching software (reclink command in Stata; Blasnik 2010), we were able to match over 85% of the households in the water-conservation experiment with the registered-voter database. We assume that members of unmatched households are not registered in the electoral system and include a dummy variable for these households in the statistical analyses (see last subsection below). The results we report below are robust to excluding these households: the estimated treatment effects differ by less than 8% when these households are excluded (results are available upon request).

Measures

The dependent variable in our analyses is a measure of each household's *water use* for June through September 2007 (monthly, in thousands of gallons). Given the treatment assignment is at the household level, we must aggregate voting data from the individual to the household level. We measure voting frequency and partisanship as follows:

(1) *Vote frequency* = The number of times every registered voter in the house voted in a primary, general, or special election (1990–2008) divided by the number of times every registered voter in the household could have voted, which depends on the birth year of each registered voter. This measure treats differently aged individuals who vote in every election equally. In contrast, a simple count of vote frequency would also be picking up the effects of age (older people, by definition, have more opportunities to vote in our panel). As robustness checks (see supporting information, Tables A4–A6), we also run our analysis using a simple count of voting frequency as well as three other methods

[6]We verified the accuracy of the vote history data from Aristotle with data on voting history purchased from the Cobb County Board of Elections. Although the experiment took place in the summer of 2007, we include voting data from the general election in 2008 due to the high turnout in that election. In separate analyses, we confirmed that receiving a treatment administered in the summer of 2007 promoting water conservation did not affect the likelihood of voting in the 2008 general election.

of aggregation: (a) considering only the oldest registered voter in the household, (b) considering only the most frequent voter in the household, and (c) selecting at random one member of the household. All results are similar and do not change the inferences drawn from the data.

(2) *Democrat* = "1" if the number of times every registered voter in the house voted in a Democratic primary election is greater than the number of times every registered voter in the house voted in a Republican primary election (1990–2008); "0" otherwise.

(3) *Republican* = "1" if the number of times every registered voter in the house voted in a Republican primary election is greater than the number of times every registered voter in the house voted in a Democratic primary election (1990–2008); "0" otherwise.

Among the households we label as Democrat, 76% only vote in Democratic primaries, and 65% of households we label as Republican only vote in Republican primaries. As robustness checks, we also run our analysis removing the households that have members who vote in both primaries, as well as using the three aggregation methods described for the *Vote Frequency* measure. We use voting frequency in primary elections to measure partisanship rather than party registration for two reasons. First, voters in Georgia do not register with a party affiliation and are allowed to vote in any primary they wish (one primary per year). Second, even if they were to register with a party, we believe that the act of voting in a primary election is an equally valid measure of partisanship compared to a party affiliation someone declares when first registering to vote. We assume that individuals who incur the costs associated with voting in primary elections are more partisan, on average, than individuals who do not. Thus, our measure picks up the two tails of partisanship much more clearly at the cost of lumping together independents and voters who would register with a party but never vote in the primaries.[7]

Covariate Balance and Statistical Controls

In experimental studies in which treatments were not randomized within subgroups (e.g., partisan groups) or in any observational study, one must be cautious when estimating heterogeneous treatment effects. Randomization does not guarantee covariate balance within subgroups, and thus one might mistake spurious correlations for heterogeneous causal effects (Imai and Strauss 2011). However, our sample size is large, and our randomization was done within small neighborhood groups (almost 400 meter-route groups). Thus, one would expect balance among observable and unobservable characteristics across the experimental treatment arms within groups defined by our voting frequency and partisanship measures. To provide evidence of this balance, we examine pretreatment water use across the treatment and control groups within each voting frequency decile (supporting information, Table A2) and within our households labeled Democrat, Republican, and "Neither" (supporting information, Table A3). For example, we test (F-test) whether pretreatment mean water uses across treatment and control groups are statistically indistinguishable from each other within the first decile of voter frequency, within the second decile, etc. With 10 sequential tests and Type I error rate set to 0.05, we would expect approximately one of them to reject the null hypothesis of no difference through chance alone. In no test is the null hypothesis rejected. Alternatively, one could test whether the mean voting-frequency measure is equal across treatment arms. In all treatment arms, the mean is identical at 0.10, and we fail to reject the null of equality. Looking at the three sequential tests for partisanship, we also fail to reject the null hypothesis of no differences across treatment arms in all cases. Thus, our data appear balanced with respect to the pretreatment outcome variables within subgroups across treatment arms.

Based on recommendations from Bruhn and McKenzie (2009) and to increase the statistical precision of our estimates, we include dummy variables for the water-meter routes in which randomization was conducted. These are excluded from the tables for presentational simplicity. To further increase statistical precision, we also include in our regression models pretreatment household water use, fair market value of the house (dollars), age of the house (years), and a dummy variable indicating if the home is owner occupied. We also create another dummy variable for households with no registered voters—i.e., the 15% of households we could not match to the registered voter list. Our results are robust to excluding these covariates. Indeed, the estimated effects barely change (at second decimal place), further strengthening our assertion that the control subgroups are valid counterfactuals for the treated subgroups even though randomization was not conducted within subgroups.

[7]The omitted registered voter group thus comprises households with registered voters who never vote in a primary (25,703) and households with registered voters whose primary election counts cancel each other out (2,568).

TABLE 1 Linear Regression: Water Use from June through September 2007

	Model A	Model B
Vote Frequency (%)	4.303***	4.316***
	(0.651)	(0.651)
Any Treatment (= 1)	−0.691***	—
	(0.162)	
Any Treatment * Vote Frequency	−2.507**	—
	(1.032)	
Treatment 1	—	0.005
		(0.279)
Treatment 2	—	−0.695***
		(0.240)
Treatment 3	—	−1.380***
		(0.227)
Treatment 1 * Vote Frequency	—	−1.625
		(1.691)
Treatment 2 * Vote Frequency	—	−2.685*
		(1.579)
Treatment 3 * Vote Frequency	—	−3.237**
		(1.475)
Unregistered (= 1)	−0.091	−0.087
	(0.173)	(0.173)
Water Use from June to November 2006	0.333***	0.333***
	(0.013)	(0.013)
Water Use in April and May 2007	0.812***	0.811***
	(0.046)	(0.046)
Fair Market Value	1.80e-05***	1.80e-05***
	(2.56e-06)	(2.56e-06)
Age of Home	0.028**	0.028**
	(0.011)	(0.011)
Ownership Status (= 1 if owner)	0.491***	0.485***
	(0.182)	(0.182)
Age of Homeowner (= 1 if >65 years old)	0.012	0.003
	(0.192)	(0.192)
Constant	−2.420	−2.425
	(1.731)	(1.732)
Observations	103,340	103,340
R^2	0.64	0.64

Note: Cell entries are parameter estimates (robust standard deviations in parentheses) for a linear regression estimation of water use (in thousands of gallons) in summer 2007 on the covariates. Not listed are 390 dummy variables for water meter routes that control for neighborhood effects. We use one-tailed significance tests because our alternative hypothesis is directional.
$^{*}p < 0.05$, $^{**}p < 0.025$, $^{***}p < 0.005$.

Results

Table 1 reports the results from ordinary least squares (OLS) linear regression models of household water use in the four months following the randomized intervention. Each regression model includes meter-route dummy variables and household characteristics described in the previous section.[8] In Model A, we collapse the treatments to a single dummy variable to increase statistical precision and because we are interested in whether frequent voters are more responsive to any appeal for voluntary conservation action. The key variable of interest for our first research question is the interaction between receiving any treatment and our household measure of vote frequency (*Any treatment* * *Vote frequency*).

Registered voters who have no voting history and are exposed to treatment reduce their water consumption by 691 gallons on average (second row, Table 1). The interaction term of voting frequency and treatment (−2.507) indicates that households with a voting history of 100% reduce water consumption by an estimated additional 2,507 gallons, on average, as a result of receiving a conservation request by mail (over 3,000 gallons in total). The size of the additional estimated average treatment effect for the most frequent voting household represents a 6.2% reduction in water consumption in summer 2007 compared to the estimated mean counterfactual use (dividing 2,507 by the average consumption of the control group in 2007). To better illustrate the magnitude of this difference, consider that a five-minute shower uses anywhere from 10 to 25 gallons of water, and the average top-load washing machine between 40 and 45 gallons of water per load.

Drawing from the literature in behavioral psychology and economics, Ferraro and Price present a theory that posits that as one moves from treatment 1 to treatment 3, the strength of pro-social preferences increases (see the fourth section). Model B in the right-hand column of Table 1 reports the results after each treatment is individually interacted with our measure of vote frequency. If one accepts Ferraro and Price's theory and if pro-social preferences were stable across our two contexts, the estimated mean effects of each treatment interacted with our vote frequency measure would also be ordered similarly: *treatment 3* + *treatment 3* * *vote frequency* > *treatment 2* + *treatment 2* * *vote frequency* > *treatment 1* + *treatment 1* * *vote frequency*. We indeed see this predicted ordering. Among nonvoters, treatment 1 has no detectable effect on water use, whereas treatment 2 reduces water use by 695 gallons, and treatment 3 reduces water use by 1,380 gallons, on average. For households with the highest voting frequency, treatment 1 reduces water use by an additional 1,625 gallons, treatment 2 reduces water use by an

[8] We lose about 3,000 observations in Tables 1 and 2 because of missing covariate values in the tax-assessor data.

additional 2,685 gallons, and treatment 3 reduces water use by an additional 3,237 gallons, on average.

The tests in Table 1 lead us to reject our null hypothesis that there is no relationship between the frequency with which people vote and their response to a pro-social appeal to conserve water during a drought: frequent voters are more responsive to pro-social requests to reduce water consumption. The results in Table 1 thus provide evidence of stable social preferences across behavioral domains.

Partisanship and Cooperative Preferences

Our second research question asks whether there is heterogeneity in responses to receiving an experimental treatment based on partisanship. To explore this question, we estimate four models to infer the effect of a household's partisanship, as defined by voting history in primary elections between 1990 and 2008, on responsiveness to pro-social appeals to conserve water (Table 2 and Table 3). Table 2 reports the results from two OLS linear regressions that test whether Republicans or Democrats are more responsive to the experimental treatments. Model A collapses the three different versions of the treatment into a single binary measure (*Any treatment*).

The estimates in the third row imply that unregistered and registered, nonprimary voting households receiving a treatment message reduced summer 2007 water consumption by an estimated 656 gallons. The fourth and fifth row estimates imply that Democrat and Republican households respond more strongly to a treatment message than the nonprimary voting households. However, the responses of Democrat and Republican households are similar in magnitude and not statistically different from each other (F-statistic = 0.29). Thus, in contrast to our first hypothesis test, we are unable to reject the second null hypothesis that, among Democratic and Republican primary voters, there is no difference in the average response to a pro-social appeal to conserve water during a drought.

Because *both* Democratic and Republican primary voters significantly reduce water consumption in response to receiving an experimental treatment, the results from Model A in Table 2 also bolster the results in Table 1, which we argued are consistent with the hypothesis that frequent voters display stable cooperative preferences across contexts. In other words, our results in Table 1 are not simply driven by Democratic voters responding to the experimental treatments, but rather they are consistent with the hypothesis that a common factor—e.g., adherence to norms of cooperation—drives behavior across both contexts. Framing this hypothesis in another way, the data are consistent with the existence of an internalized sense of civic-mindedness that drives some individuals to contribute to public goods across contexts.

For completeness, we also estimate a second model in Table 2 (Model B) in which we interact each treatment individually with partisanship variables. Because we break up our sample into many subgroups, we do not have enough statistical power to discriminate among the different treatment messages. A couple of coefficients are significantly different from zero in a potentially intriguing pattern, but the only definitive conclusion that can be drawn from the model is the same conclusion we draw from Model A: Democratic and Republican primary voters respond similarly when receiving a request to take pro-social collective action for the environment.

The analysis in Table 2 attempts to estimate the effect of partisanship on responsiveness to a pro-social appeal to contribute to an environmental public good. In contrast to the analysis in Table 1, a potential concern with the analysis in Table 2 is that there may be time-invariant unobservable factors associated with being a Republican or Democrat that influence one's willingness to reduce water use in response to receiving a randomly assigned treatment message. To address this concern, we take advantage of monthly pre- and postexperimental measures of each household's water use to estimate two fixed-effects panel-data regression models. These models give up statistical power in exchange for greater control over time-invariant unobservable characteristics at the household level. Table 3 reports these results.

Receiving any treatment significantly reduces water use an estimated 295 gallons (monthly; $p < 0.001$). In contrast to the coefficient estimates in Table 2 that imply Democratic and Republican households reduce water use more than nonpartisans after receiving a treatment message, the coefficients in Table 3 imply no difference among these households: the coefficients for *Any Treatment * Democrat * Postexperiment period* and for *Any Treatment * Republican * Postexperiment period* are not statistically different from zero. The estimated coefficients, however, are consistent with the main result in Table 2: Democratic and Republican households respond similarly after receiving a request to take pro-social collective action for the environment.[9]

[9]Our results seem to conflict with Costa and Kahn (2013), but a closer look suggests they are more similar than different. The coefficient on their "Liberal" coefficient (which combines Greens, Peace and Freedom, and Democrat party members) is tiny and not statistically significantly different except in the one regression (out of five) that has the fewest controls. Only when they create subgroups by combining the weak effect of the political party variable

TABLE 2 Linear Regression: Partisanship and Water Use from June through September 2007

	Model A	Model B
Democrat (= 1)	0.318*	0.319*
	(0.186)	(0.186)
Republican (= 1)	1.051***	1.052***
	(0.168)	(0.168)
Any Treatment (= 1)	−0.656***	—
	(0.186)	
Any Treatment * Democrat	−0.619**	—
	(0.294)	
Any Treatment * Republican	−0.459*	—
	(0.267)	
Treatment 1	—	0.079
		(0.334)
Treatment 2	—	−0.669**
		(0.273)
Treatment 3	—	−1.354***
		(0.253)
Treatment 1 * Democrat	—	−1.008**
		(0.474)
Treatment 2 * Democrat	—	−0.522
		(0.438)
Treatment 3 * Democrat	—	−0.364
		(0.431)
Treatment 1 * Republican	—	−0.100
		(0.446)
Treatment 2 * Republican	—	−0.559
		(0.397)
Treatment 3 * Republican	—	−0.762**
		(0.387)
Unregistered (= 1)	−0.115	−0.115
	(0.182)	(0.182)
Water Use from June to November 2006	0.333***	0.332***
	(0.013)	(0.013)
Water Use in April and May 2007	0.811***	0.811***
	(0.046)	(0.046)
Fair Market Value	1.78e-05***	1.79e-05***
	(2.56e-06)	(2.56e-06)
Age of Home	0.028***	0.028***
	(0.011)	(0.011)
Ownership Status (= 1 if owner)	0.520***	0.515***
	(0.180)	(0.180)
Age of Homeowner (= 1 if >65 years old)	0.146	0.140
	(0.192)	(0.192)
Constant	−2.538	−2.549
	(1.728)	(1.728)
Observations	103,448	103,448
R^2	0.64	0.64

Note: Cell entries are parameter estimates (robust standard deviations in parentheses) for a linear regression estimation of water use (in thousands of gallons) in summer 2007 on the covariates. Not listed are 390 dummy variables for water meter routes that control for neighborhood effects. We use one-tailed tests because our alternative hypothesis is directional.
$^{*}p < 0.05$, $^{**}p < 0.025$, $^{***}p < 0.005$.

TABLE 3 Fixed-Effects Linear Regression: Water Use Partisanship (Panel Data, Monthly)

	Model A	Model B
Postexperiment	0.457***	0.456***
	(0.068)	(0.068)
Democrat * Postexperiment	−0.126	−0.125
	(0.089)	(0.089)
Republican * Postexperiment	0.510***	0.511***
	(0.071)	(0.071)
Unregistered * Postexperiment	−0.295***	−0.294***
	(0.107)	(0.107)
Any Treatment * Postexperiment	−0.297***	—
	(0.114)	
Any Treatment * Democrat * Postexperiment	0.039	—
	(0.126)	
Any Treatment * Republican * Postexperiment	0.156	—
	(0.117)	
Treatment 1 * Postexperiment	—	−0.027
		(0.091)
Treatment 2 * Postexperiment	—	−0.491
		(0.318)
Treatment 3 * Postexperiment	—	−0.369***
		(0.073)
Treatment 1 * Democrat * Postexperiment	—	−0.146
		(0.128)
Treatment 2 * Democrat * Postexperiment	—	0.202
		(0.325)
Treatment 3 * Democrat * Postexperiment	—	0.0560
		(0.117)
Treatment 1 * Republican * Postexperiment	—	0.114
		(0.116)
Treatment 2 * Republican * Postexperiment	—	0.319
		(0.321)
Treatment 3 * Republican * Postexperiment	—	0.027
		(0.099)
Constant	8.552***	8.552***
	(0.006)	(0.006)
Observations (month-household)	1,813,590	1,813,590
Number of households	106,682	106,682

Note: Cell entries are parameter estimates (standard deviations in parentheses) for a fixed-effects panel data estimator of monthly water use (in thousands of gallons). Fixed effects are modeled at the household level. We use one-tailed significance tests because our alternative hypothesis is directional.
$^{*}p < 0.05$, $^{**}p < 0.025$, $^{***}p < 0.005$.

with the effects of other more influential variables (e.g., donating to environmental causes, living in top-quartile liberal neighborhood) do they find significant differences.

Conclusions

Understanding why some citizens, but not others, take action for the public good strikes at the heart of political science. The degree to which individuals are willing to make voluntary contributions to the public good determines the policies that need to be in place to reach collectively desirable outcomes. To contribute to a broader understanding of pro-social behavior, we study the expression of pro-social preferences across domains and shed light on the mechanisms by which pro-social messages impact politically relevant actions.

Our first research question explores the stability of pro-social preferences: is the propensity to take pro-social action a stable predisposition across domains, or is it domain-specific? In contrast to previous studies that make lab-to-field comparisons of behavior or use self-reported measures of behavior, we use a field-to-field comparison of revealed preferences with data from a randomized policy experiment and the voting histories of households. Consistent with the hypothesis of stable pro-social preferences across domains, we find that frequent voters were more responsive to a randomized policy intervention that used pro-social appeals to encourage participation in a collective action: frequent voters reduced water use significantly more than less frequent voters and nonvoters.

Our second research question contributes to the inchoate literature on the impact of partisan identification on cooperative behavior. Counter to expectations that Democrat households would be more responsive to pro-social appeals to contribute to an environmental public good, we find no evidence that Republican and Democrat households respond differently in our sample. This result also implies that the relationship we find between voting and responsiveness to pro-social appeals to conserve water is not being driven by reductions in water use among Democrats only (or Republicans only). Thus, a common factor—e.g., adherence to civic norms, altruistic preferences, or a sense of civic-mindedness—may be driving the increased responsiveness to pro-social requests among partisans of both stripes.

In addition to informing our understanding of pro-social preferences and partisanship, our study also contributes to the growing literature in political science that examines how norm-based interventions affect politically relevant actions (Bolsen 2013; Davenport et al. 2010; Gerber, Green, and Larimer 2008, 2010). This literature highlights how social pressure shapes willingness to engage in collective actions. Less clear, however, is the extent to which pro-social actions are driven by internalized behavioral predispositions as opposed to compliance out of fear of social sanction. Our study demonstrates that, *in the absence of external pressure or fear that one's behavior is going to be revealed publicly*, taking action for the public good in one domain is correlated with responsiveness to a pro-social message encouraging collective action in a different domain. Of course, political scientists have long known that voters possess pro-social preferences that drive them to vote (Fowler 2006). What is novel about our study is that it is the first to examine revealed preferences with respect to whether voters are more likely to participate in an unrelated collective action, *in a different domain*, when presented with an explicit request for cooperation.

Our study also has implications for policy makers and officials who craft messages to influence private actions with public consequences. Messages promoting action for the public good may be more cost-effective if they target individuals who are more predisposed to cooperate (e.g., frequent voters).[10] More research, however, would be needed to confirm these conjectures as well as our findings on the stability of pro-social preferences and the relationship between partisanship and responsiveness to pro-social appeals to contribute to an environmental common-pool resource. Future research on partisanship in other environmental contexts is needed to elucidate in what ways partisanship affects responses to environmental policies and programs. The results in this case suggest that, counter to common intuition, Democrats and Republican voters respond similarly to a pro-social request for conservation; however, these results may be restricted to water use and not apply to global warming–related behaviors or other environmentally relevant actions. Future studies also should, like our study, take advantage of the growing number of randomized controlled social experiments using large sample sizes. By testing our hypotheses in other geographic and behavioral contexts, we can greatly improve our understanding of the way in which pro-social preferences and partisanship shape collective action.

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[10] Of course, the total aggregate effect from targeting, say, frequent voters would depend on how many frequent voters are in the population.

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Supporting Information

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

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Treatment 3: tip-sheet and Treatment 3 letter
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