Partisan Consumers

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Abstract I document a significant fluctuation in US household consumption expenditures associated with changes in federal and state political leadership. Spending on non-durables by households whose preferred political party is out of power is more than 1 percentage point greater than that of comparable households whose preferred political party is in power. I believe this is the result of short-run fluctuations in the household time-preference rate caused by feelings of powerlessness, which reduce self-control and connection to future self. The effect I document is concentrated among upper income percentiles, who are less likely to be credit constrained, and thus more responsive to shifts in the relative value of interest rate and time-preference rate. I calibrate these fluctuations, showing that they are consistent with time-preference rate fluctuations on the order of 2 percentage points. I recruit online panels whose time preferences are estimated using the convex time budget method of [Andreoni and Sprenger, 2012]. These panels are recruited before and after the US elections of 2022 and 2024 and their political knowledge and affinities elicited. Consistent with my hypothesis, respondents whose political affiliation is at odds with whom they believe to be in power exhibit higher present bias when their answers are fit to the quasi-hyperbolic β-δ utility function of [Laibson, 1997].

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1 Introduction

Americans are strongly connected to their preferred political parties, following their battles in the media and rooting for their electoral success in a manner akin to sports teams. When their preferred party is in power, they rate both the future and current states of the economy more highly [Mian et al., 2021]. Yet it is unclear whether such partisan economic evaluations actually affect spending behavior. Finding no spending differences between regions of different partisan affiliation, [Mian et al., 2021] conclude that the partisan component of the survey responses does not reflect differences in the rational evaluation of personal or national economic prospects. Citing others who find that incentives can reduce such partisan gaps in economic evaluation ([Bullock et al., 2015], [Prior et al., 2015]), they contend it is not a serious evaluation of economic prospects, but mere partisan cheerleading as respondents bask in the warm glow of tribal ascendancy.

Yet it would be curious if good feelings did not spill over into consumption. As [Loewenstein, 2000] put it, "[u]nderstanding the emotions people experience at the time of consuming, or deferring consumption, is critical for understanding and predicting the inter-temporal trade-offs they make." Individuals identify with parties and their moods track with how that party fares. [Yun et al., 2024] find that the outcome of the 2020 Presidential election affected incidence of domestic violence. Nor is it confined to election season. [Ford and Feinberg, 2020] and [Ford et al., 2020] offer evidence that politics affects daily mood in the middle of a political term, to the point where participants take actions to respond to and regulate the negative moods induced by political news. [Huddy et al., 2015] find that strong partisans display anger in response to political threats and enthusiasm in response to political success. One of the most common feelings reported by those whose party is out of power, or those listening to negative political news, is powerlessness ([Woodstock, 2014], [Boukes and Vliegenthart, 2017]).

How might these politically induced negative feelings affect consumption choices? One important channel may be through the connection to the future self and thus the utility weight placed on future consumption. Psychologists emphasize that discounting the future derives from an attenuated connection between present and future self ([Pronin et al., 2008], [Bartels and Rips, 2010]). Interventions designed to make the present-future-self connection stronger lead to reduced discounting ([Bartels and Rips, 2010], [Bartels and Urminsky, 2011], [Ersner-Hershfield et al., 2009]). Investigating expressive partisanship and political social identity, [Joshi and Fast, 2013] find direct evidence that "the experience of power enhances one's connection with the future self, which in turn results in reduced temporal discounting." I postulate that being in the political minority leads to greater daily stress and feelings of powerlessness from political news, including simple reminders of the electoral loss when political leaders appear in various media channels. I further postulate that such daily stress leads to a greater discounting of the future as those in the political minority, feeling less connected to their future selves, focus more on the present. By contrast, the daily reminders of the victory of their partisan social identity ought to leave victors feeling relatively empowered, looking to the future. It is unclear whether, in the widely used quasi-hyperbolic β-δ utility function ([Laibson, 1997]), this effect would operate through the present bias parameter (β), or the discount parameter (δ). As I demonstrate via a simple model, these countervailing shocks to the discount rate should lead to a rise in present consumption among households whose preferred party is out of power as compared to that of those whose preferred party is in power.

To test my hypotheses that being in the political minority raises consumption, I employ a large, national, rolling panel of household consumption collected by Nielsen. It covers approximately 60,000 households each year from 2006 – 2023 with specific households remaining in the panel for an average of 61 months. Households record purchases of goods intended for the home from a wide variety of outlets.

I demographically project the partisan identity of these households by first employing a random forest algorithm on Cooperative Congressional Election Survey (CCES) data from 2006-2021 to elucidate the connection between demographics and partisan identity. This establishes a mapping from demographics to political ideology. I then use this mapping to predict the partisan identities of the Nielsen panelists from their demographics. These predicted partisan ideologies can be used to determine whether a given household, in a given year, is supportive of the party in control of the legislative and executive branches of the federal and state governments. Households with split ideology and split political control of a legislative branch each result in intermediate values in the independent variable quantifying a household's ideological agreement with the relevant branch of government. I regress a household's spending on these measures of ideological agreement plus month and household fixed effects.

I find support for my hypothesis. Households that disagree with the government tend to spend more than households that agree with the government. The presence of household fixed effects means these fluctuations are relative to the household's in-sample average spending. Households' projected political ideology is fixed within sample. Thus, identification comes solely from changes in control of various branches of government. Not surprisingly, I find that the effect of federal government is far stronger than that of state government. A complete change in control of the federal government– unified control of the Presidency and Congress switching from one party to the other– would result in a shift in spending of approximately 1 percentage point for households with a unified political ideology.

As expected, the effect of ideological disagreement with Congress is significantly smaller than that of ideological disagreements with the President. This is because the President receives more attention than Congress. As [Dunaway and Graber, 2022] note, "[n]ational stories about Congress are generally fewer, shorter, and less prominently placed than news linked to the presidency." However, I do still find a significant effect of Congress. [Dunaway and Graber, 2022] note that members of Congress have become more adept at attracting attention and that "most stories about Congress deal with individual members or legislative activity on specific issues rather than the body as a whole." Coverage focuses on the game, "where policy efforts are discussed in terms of who will 'win' rather than the implications of the policy proposals themselves." The decline in local media coverage and the nationalization of voting has led to less exposure of individual legislators and a greater focus on the chamber as a whole [Trussler, 2022]. [Noble, 2024] finds that legislators deliberately invoke the President, leveraging the symbolic power of the President to nationalize legislative debate. This focus on the chamber, the team, and the game fits with my overall picture of a public that can see Congress as a body dominated by one side or the other acting either for or against their personal interests. [Dunaway and Graber, 2022] conclude that, "[w]hen coverage of legislative concerns is added to coverage that mentions Congress explicitly, Congress and the presidency receive roughly the same amount of national news attention."

My results seemingly contradict those of [Mian et al., 2021] who find that counties which voted in favor of the victor (Trump) in the 2016 Presidential election do *not* differ in their aggregate credit card spending over the next year as compared to counties which voted in favor of the loser (Clinton). My results highlight the role of Congress, which [Mian et al., 2021] do not consider, but Republicans also retained control of both chambers of Congress in the year of their analysis (2016-7) so counterveiling effects are not a potential explanation for differences between their results and mine. One possibility is that aggregate measures at the county level, such as those used by [Mian et al., 2021], do not have sufficient power to detect an effect of the size I document. I estimate that roughly 27% of my households have a united ideology and would thus exhibit the full effect of the Presidency switching from Obama to Trump. The 2016 election included a switch of President but not Congressional control thus reducing the total potential effect I uncover to 1.1 percentage points of household spending. The inter-quartile range of two party democratic voteshare for the President across counties in 2016 was 21.2%. Thus, a back-of-the-envelope calculation of the aggregate inter-quartile difference in consumer spending would be 1.1pp * 0.27 * 0.212 = 0.063 percentage points. This is likely not detectable in a national sample of 3000 counties.

Have I just made the case that my results, being too small to generate observable aggregate effects, are irrelevant? I think not. I believe that the demonstration of a psychological effect of elections on consumption at the level of 1% of monthly spending is an important insight into consumer behavior, even if a divided electorate means the effect is largely offset in the aggregate. Moreover, the possibility of a discount rate shock is quite novel.

To test whether election results and ideological disagreement can be plausibly linked with a discount rate shock, and thus lend credence to this particular interpretation of the estimated effect of elections on household expenditures, I conduct an online experiment to simultaneously measure the time rate of preference and the political affiliations of respondents. I implement the convex time budget design of [Andreoni and Sprenger, 2012] to separately estimate parameters for the CRRA curvature, the exponential discount factor, and the present bias by eliciting responses to a series of convex inter-temporal budget decisions. Using [Andreoni and Sprenger, 2012]'s nonlinear least squares method to recover estimates of these three parameters from respondents' answers, I then connect these individual parameter values to the respondent's education, income, and political ideology. I find that these demographic variables have little relationship to curvature and exponential discount factors but are strongly correlated with present bias. Those with higher income and higher education are modestly less likely to display present bias. Those who share a political ideology with the president are far less likely to display present bias, in keeping with my main hypothesis.

In the following sections, I first lay out the model, highlighting the effects of a discount rate shock on spending. I then develop the analysis of the consumer spending panels. Finally, I explain the experimental setup and present the analysis of the parameters.

2 Discount Rate Shocks and Steady State Consumption

I posit a time-separable CRRA utility function with quasi-hyperbolic discounting ([Laibson, 1997]).

$$U(c_0, c_1, ..., c_T) = u(c_0) + \beta \sum_{t=1}^{T} \delta^t u(c_t) \tag{1}$$

where δ is the one-period discount factor, β is the present bias parameter, and $u(c)$ is a CRRA utility function with coefficient of relative risk aversion, $\alpha \in (0, \infty)$.

$$u(c) = \frac{c^{1-\alpha} - 1}{1 - \alpha} \tag{2}$$

Abstracting from income risk and interest rate volatility, I presume a NPV of lifetime wealth of W such that the household budget constraint is a standard, non-stochastic

$$\sum_{t=0}^{T} \left(\frac{1}{1+r}\right)^t c_t = W \tag{3}$$

from which the path of consumption optimized at time 0 is

$$\begin{aligned} c_0 &= W\left[1 - (\beta\delta)^{\alpha-1}(1+r)^{\alpha-2}\right] \\ c_t &= c_0\left[\beta\delta^t(1+r)^t\right]^{\alpha-1}, \quad t > 0 \end{aligned} \tag{4}$$

I use this baseline to consider how an agent would respond to an unexpected, permanent shock to either of their discount parameters, β or δ. In my empirical work with observational household consumption data, I observe a political shock which appears to change average monthly consumption by about 1 percentage point. Working backwards, we can infer the magnitude of the shock to either β or δ that would be required to produce a shift of this magnitude on impact. Table 1 displays the sensitivity of present consumption as a fraction of lifetime wealth, c_0/W to changes in parameters under the quasi-hyperbolic function. I take as my baseline the median values for each parameter as estimated from the sample of respondents to my online panel, discussed in section 5. In particular, $\beta = 0.9111$, $\delta = 0.9983$, $\alpha = 0.9732$. I then calculate the percent change in present consumption, that would occur with a ± 25 percentile shift in each of the discount parameters. Finally, I show the magnitude of a shift in β required to produce a 1pp shift in present consumption. It is well within the sample inter-quartile range of β.

Table 1: Effects of parameters on period zero consumption

Parameter	Sample Median	Sample δ pctile 25th	75th	Sample β pctile 25th	75th	Changing β $\lvert\Delta c_0\rvert = 1\%$	
δ	0.9983	0.9974	0.9995	0.9983	0.9983	0.9983	0.9983
β	0.9111	0.9111	0.9111	0.8324	0.9743	0.8947	0.9278
α	0.9732	0.9732	0.9732	0.9732	0.9732	0.9732	0.9732
r	0.05	0.05	0.05	0.05	0.05	0.05	0.05
c_0/W	0.0464	0.0464	0.0465	0.0441	0.0482	0.0460	0.0469
Δc_0	n/a	-0.05%	0.07%	-4.98%	3.69%	-1.00%	1.00%
$\Delta\delta$ or $\Delta\beta$	n/a	-0.09%	0.12%	-8.64%	6.94%	-1.80%	1.83%

3 Consumer Panel Design

3.1 Partisan Classification

In this section, I document the effect of political (dis)agreement on household consumption. The main challenge to my measurement is the lack of a panel of individuals wherein both partisan political affiliation and consumption behavior are recorded. I address this problem in two steps. The first step is to use the cooperative congressional election study [Schaffner et al., 2021] to deduce and elucidate the mapping from demographics to partisan political affiliation. The second step is to apply this mapping to the Nielsen consumer panel, thereby uniting projected partisan affiliation with a detailed consumer history.

Deducing the mapping between demographics and political affiliation is a classification problem which I solve using a random forest algorithm. The demographic variables which are common to both CCES and Nielsen are: age, gender, ethnicity, household income, zip code, educational attainment, marital status, children at home, and the year. One omission that is relevant for political affiliation is religious affiliation and attendance, which is collected by CCES

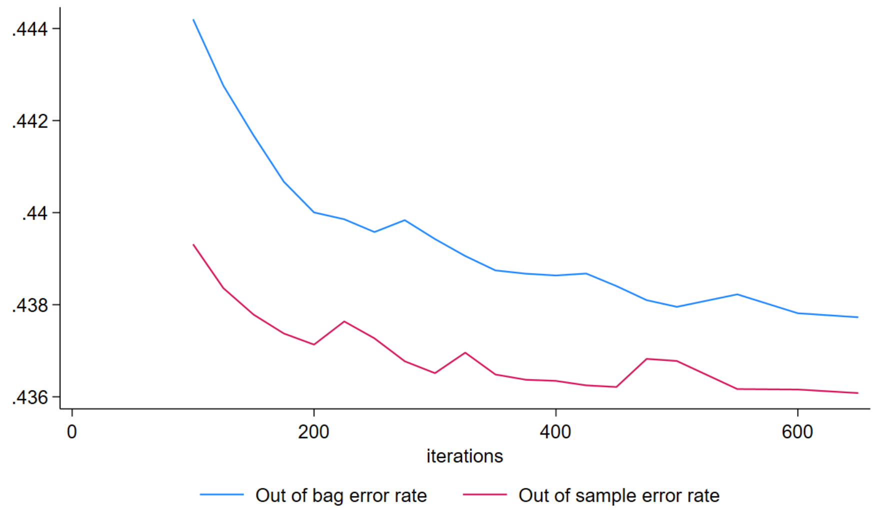

Figure 1: Out-of-bag and out-of-sample error rates by number of iterations.

but not by Nielsen. In cases where the categories are not congruent between the two datasets, I harmonize so as to preserve as much variation as possible. While geography is a critical determinant of political identity, zip codes are too numerous for a classification tree. I use state and a measure of rural-urban commuting areas (RUCA) defined by the US department of agriculture (USDA). This measure classifies zip codes into 20 different categories based on metro/micro/rural location and the commuting relationship between that location and other nearby towns.

My measure of partisan affiliation is the CCES respondent's response to the question, "generally speaking, do you think of yourself as a... republican/democrat/independent/other/not sure". I group this into a trinary republican/unaffiliated/democrat variable which I call *partisan affiliation*.

A random forest algorithm with 300 iterations (trees) and no restrictions on depth or number of features produces an in-sample accuracy of 97.5%. Figure 1 plots the out of bag and classification error rates for increasing numbers of iterations, showing that after about 300 iterations, there is minimal gain to out-of-sample classification error rates. Out of sample error rates rates stabilize just below 44%. Because this is trinary rather than binary, this is not as poor as it might seem, though it is admittedly noisier than one would prefer. To give a sense of the errors, I randomly split the sample in half, train on one half, and tabulate the errors when projecting into the other half. The results (2) show that, because of dirty classification, we have a muted signal.[1] The group that we classify as Democrats is actually 58.8% Democrats, 16.5% Independents, and

[1]For comparison, Table 9 notes the distribution of in-sample errors.

24.7% Republicans rather than 100% Democrats. If our hypothesis is correct that Republicans and Democrats experience opposing effects from whomever is in office, then the strength of this group is actually (53.4-17.2)=36.2% of the pure signal. Similarly, the strength of the Republican group is only (42.9-25.3)=17.6% of the pure signal. As we are identifying off the difference, we have a difference of 36.2%+17.6%=53.8% rather than the ideal 200%, thus we have just over 1/4th the full signal strength. So, the true effects should be 2/.538 = 3.72 times the magnitude of our parameter estimates. I will come back to this when interpreting the magnitudes of the empirical work in section X and relating to the experimental measurements in section Y.

Table 2: Out of sample prediction of political affiliation

		Model prediction		
		Dem	Ind	Rep
Actual affiliation	Dem	53.4%	28.8%	25.3%
	Ind	29.4%	46.0%	31.8%
	Rep	17.2%	25.2%	42.9%

Finally, as consumption is recorded at the household level, I need a household partisan score. Households consist of either one or two heads for which I have full demographic information plus a non-negative number of dependents. I define household, h's partisan affiliation, $Ideology_h$, to be the average of the partisan affiliation of the head or heads of household. The combinations are enumerated in Table 10.

3.2 Partisan Control

For every year between 2006 and 2023, I recorded the control of the executive and legislative branches of the federal and fifty state governments. For the executive, that means the party of the President or Governor (Republican, Democrat, occasionally Independent). For the legislature, this means either unified Republican control, unified Democrat control, or split control. I encode these in $Ideology_l$ for levels of government $l \in p, c, g, sl$ as listed in Table 11.

The consumption panel I constructed is monthly. Because I am considering indicators of political news and ascendancy, I have coded political control to switch at the election date in November rather than from the assumption of institutional control in January. Thus the political control variables change in November of election years.

From the (estimated) household partisan affiliation and this political control, I generate measures of congruence between a person and the state and federal governments where they live in a given month. These measures are defined as the product of a household's partisan affiliation and the control of government and are then normalized by dividing by 2 to ensure that the distance between complete agreement and complete disagreement is one unit In this way, when a household's preferred party is in charge, the measure takes the value +0.5. When their preferred party is out of power, the measure takes the value -0.5. Nielsen panel households can have either one or two heads of household and these might have different estimated political ideologies based on gender, age, and education. Thus households are not always given a pure ideology. Household ideology is the average of that of the heads of household.

$$Ideology_l = \begin{cases} +1 & \text{Republican} \\ 0 & \text{Independent (voter) or Split Control (legislature)} \\ -1 & \text{Democrat} \end{cases} \tag{5}$$

$$Ideology_h = \sum_{i \in h} Ideology_i \tag{6}$$

$$\begin{aligned} SharedIdeoGov_h &= Ideology_h * Ideology_{gov}/2 \\ gov &\in \{President, Congress, Governor, StateLegislature\} \end{aligned} \tag{7}$$

3.3 Consumption

Nielsen spending data follows roughly 60,000 households per year. These households record all purchases of products intended to be used in their home. In practice, this includes a wide variety from food and clothing to electronics and sports equipment. But it does not include services, dining out, or ticketed entertainment. Nielsen estimates that 30% of household spending is covered by their survey. For each distinct shopping trip, respondents record individual purchases and prices, which are classified by both brand and type of good, as well as the retailer where they were purchased. Online purchases are included.

The panel is unbalanced as households are replaced as they drop out. I have a total of 207,715 households appearing in at least one month. Most households stay a precise number of years from 2 to 18. For these households, the distribution of years is that of a hazard rate with a declining hazard function. A smaller number of households drop out a month or two before the end of the full year and thus end up in the sample for 34 or 47 months. The median household lasts 36 months. The mean duration is 61 months. The inter-quartile range is 12 to 84 months. Nielsen provides sample weights which I use in my estimates.

My dataset is a household-month panel spanning January 2006 through December 2023. For each household-month, I aggregate total spending, number of shopping trips, number of days on which purchases were made, and a breakdown of spending into certain categories. My main dependent variable is a household's monthly spending. Summary statistics for monthly household expenditures are reported with summary statistics of the constructed shared ideology variables in table 3. Table 4 shows the cross tabulations for shared ideology of households with President and Congress.

Table 3: Summary Statistics of Spending and Shared Ideology

Variable	Obs	Mean	Std.	Min	Max
Total spending this month	12,657,799	681.82	551.11	0	$ 86,999
Spend as a fraction of HH avg monthly spend	12,657,799	1.00	0.517	0	18.18
Trips this month	12,657,799	14.72	11.18	1	369
Retailers this month	12,657,799	6.99	4.57	1	114
Shopping days this month	12,657,799	8.74	5.20	1	31
HH shares ideology with President	12,657,799	0.02	0.34	-0.5	0.5
HH shares ideology with Congress	12,657,799	-0.00	0.26	-0.5	0.5
HH shares ideology with Governor	12,641,711	0.02	0.34	-0.5	0.5
HH shares ideology with State Legislature	12,657,799	0.020	0.32	-0.5	0.5
Moskowitz (frac of HH's DMA in HH's state)	12,657,799	0.852	0.24	0.002	1

Household by month observations spanning Jan 2006 - Dec 2023. Spending data from Nielsen consumer panel. Ideological congruence variables normalized such that total disagreement is -0.5 and total agreement is +0.5 so that the distance between them is one unit. Households may have one or two heads and thus may be split in opinion.

Table 4: Cross Tabs: HH Sharing Ideology with Branches of Federal Government

		Household Shares Ideology w/President					
		Both Opposed	1 Opp, 1 Neutral	Household Neutral	1 Support, 1 Neutral	Both Support	Total
Shares Ideo w/ Congress		-0.5	-0.25	0	0.25	0.5	
Both Opposed	-0.5	672,527				599,731	1,272,258
1 Opp,1 Neutral	-0.25		510,149		455,824		965,973
Household Neutral or Congress Split	0	748,042	652,347	3,485,480	581,879	931,559	6,399,307
1 Supp,1 Neutral	0.25		401,557		482,260		883,817
Both Support	0.5	490,413				615,069	1,105,482
Total		1,910,982	1,564,053	3,485,480	1,519,963	2,146,359	10,626,837

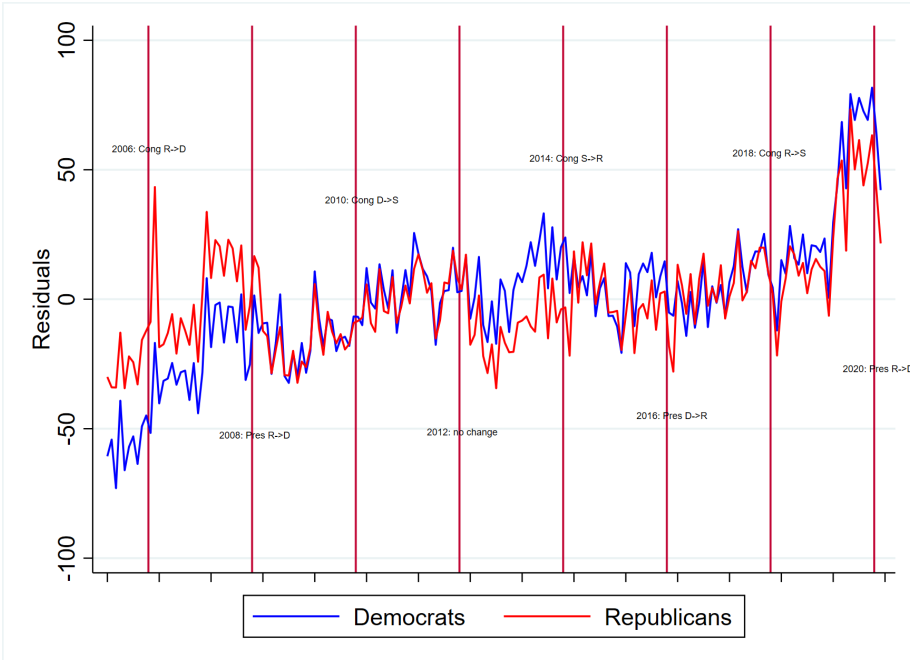

Figure 2: Median Republican and Democratic households' spending across elections.

4 Consumer Panel Results

Before reporting regression results, I first show a simple graph to illustrate the core argument. Figure 1 displays the ratio of actual monthly expenditures to sample average monthly expenditures for the median Republican and median Democratic households after removing year and month-of-year fixed effects. I have overlaid the election dates and a summary of the results. To emphasize, the y-axis shows the median household's deviation from their sample average spending. Looking at the period following 2006, when Congress shifted from Republican to Democratic, we see a post-election spike that is noticeably larger among Republican households. When the Republicans captured the White House in 2016, Republican spending dropped more than Democrat spending. During the period of Democratic control from 2006 - 2010, Republicans spent more than Democrats. During the period of Republican control from 2016-2020, Democrats spent more than Republicans. In each of these cases, the households spending less were those whose party had been victorious. There are episodes of short-term election response and episodes of longer trends. There are examples of responses to executive and legislative control. Next, I will look for systematic evidence of these responses using simple panel regressions.

Baseline results are reported in Table 5. These results confirm the hypothesis: households whose preferred party is out of power spend more relative to their sample average than do those households whose preferred party is in power. The dependent variable is household monthly spending in dollars. Shortly, I will translate this into a percentage. The independent variables of interest take the value +0.5 (-0.5) if the household's projected ideology aligns (opposes) with that of the officeholder in question. Thus, I can interpret column A to show that households whose projected preferred political party control both the White House and Federal Legislature spend roughly \$3.41 less than those whose projected party is out of power in both branches. The use of the word *projected* is a deliberate reminder that our projections are dirty, which attenuates our estimated effect by a factor of 3.72 (see the discussion around table 2). Multiplying by this factor, we estimate that a household whose preferred political party controls both Congress and the White House spends 3.71*\$3.41=\$12.67 per month less than a household opposed to both federal branches. The sample mean is \$681.82 so we document an effect of \$12.67/\$681.82 = 1.9 percentage points.

The state legislature is also significant and negative but, as I show in column 2, this is driven by availability of media coverage in a way that is oddly unstable. Following **??**, I define a variable, *Moskowitz*, which measures the fraction of a household's DMA that is in the household's state. As **??** shows, living in a DMA which lies mostly in another state reduces a household's access to media coverage about their own state's representatives, with predictable consequences for knowledge of officeholders. Interactions between the Moskowitz variable and ideological agreement with the state legislature produce the expected pattern of signs, but the magnitudes are implausibly large, representing a puzzle which I set aside for the moment to focus on federal effects.

In column 3, after limiting focus to federal government, I separately estimate interaction effects for a two-month window (October and November) around presidential election years and off-election years. In Presidential election years, the effect of ideological agreement with the President is stronger in the pre- and post-election months, consistent with the idea that salience is relevant to the effect in question. In a non-Presidential election year (e.g. 2002, 2006, ...), ideological agreement with the President is irrelevant. In column 4, I include ideological agreement with the household's own representative. This is not significant; it is control of Congress that is relevant rather than agreement with ones' local representative.

Notice if there were a reporting issue, I would expect it to go the other way. I would expect that those demoralized by politics would be less conscientious about reporting to Nielsen and thus would show less spending than their politically satisfied counterparts.

In table 6, I report the manner in which the effect varies with household education and income. The effect of the Presidency does not vary significantly with education (columns 1,3). On the other hand, the effect of Congress is driven entirely by those with 4-year college degrees. This would suggest that the college-educated pay more attention and place more importance on Congress than their less-educated peers. Effects for both branches of federal government

Table 5: Baseline Results

Depvar: monthly spending ($)	[1]	[2]	[3]	[4]
Share Ideo w/President (b1)	-2.095**	-2.145**	-2.041**	-3.298**
	(0.363)	(0.364)	(0.376)	(0.474)
Share Ideo w/Congress (b2)	-1.312**	-1.225*	-1.556**	-3.534**
	(0.482)	(0.483)	(0.492)	(0.637)
Share Ideo w/State Governor (b3)	0.611	-1.544		
	(0.450)	(1.569)		
Share ideo w/State Assembly (b4)	-2.242**	6.629**		
	(0.519)	(1.849)		
Shared Ideo w/own Congressional Rep				0.531
				(0.598)
Share ideo w/G * Moskowitz (b5)		2.812		
		(1.803)		
Share ideo w/SL * Moskowitz (b6)		-10.51**		
		(2.105)		
Share ideo w/P * Proximity to Pres Elec (b7)			-2.454	
			(1.663)	
Share ideo w/C * Proximity to Pres Elec (b8)			-0.331	
			(2.098)	
Share ideo w/P * Proximity to Off Elec (b9)			3.909*	
			(1.649)	
Share ideo w/C * Proximity to Off Elec (b10)			0.998	
			(2.181)	
Constant	716.6**	704.1**	716.5**	704.3**
	(2.010)	(2.831)	(2.008)	(2.407)
F-stat: b3+b5=0		5.46		
p-value		0.020		
F-stat: b4+b6=0		39.39		
p-value		0.000		
F-stat: b1+b7=0			7.63	
p-value			0.006	
F-stat: b2+b8=0			0.83	
p-value			0.361	
F-stat: b1+b9=0			1.32	
p-value			0.250	
F-stat: b2+b10=0			0.07	
p-value			0.794	
Observations	12,641,711	12,577,216	12,657,799	6,990,601
Number of households	207,503	206,685	207,715	136,589
R-squared	0.02	0.02	0.02	0.022

Standard errors in parentheses: ** p<0.01, * p<0.05
Includes month and household FE

are strongly associated with income (columns 2,3). The effect is essentially null for those with incomes of less than $40,000 (the omitted category). This is consistent with the self-care literature, which notes that those with financial leeway are more likely to indulge in self-care (**??**). It would at first seem to be inconsistent with the notion that being in political opposition results in a greater discounting of the future. But, as has been noted, lower income Americans already live paycheck-to-paycheck because their desire to spend exceeds their credit limit. Further declines in the discount rate have little effect on the credit constrained.

Lastly, I check the role of liquidity constraints. I first apply the code of [Kaplan et al., 2014] to SCF data to achieve a classification of consumers according to their three categories: wealthy hand-to-mouth (wHtM), poor hand-to-mouth (pHtM), and not hand-to-mouth (nHtM). I am then able to use this dataset to make the connection between demographics and HtM status. As before, I apply a classification tree to estimate the likelihood that a household with specific demographics are in each of these three categories. I then apply the classification tree results to the Nielsen dataset to classify each household in my sample as one of the three categories, based solely on their demographics. I emphasize, this is based solely on demographics including the household income and ethnicity and the age and education of the male and female heads of household and *not* an analysis of their finances as per the original classification of the SCF data. That is to say, if I classify one of the Nielsen households as wHtM, that is because those of similar education, ethnicity, age, and income are frequently wHtM in the SCF data. I group the two HtM categories together so as to compare HtM and non-HtM households. I then interact projected HtM status with ideological agreement with the two elected branches of federal government. Given that I have an interaction between two variables resulting from classification trees based on demographics, this is now a complex and speculative combination of demographics. I cannot reject the null that the coefficients are the same for HtM and non-HtM households (Table 6, column 4). That is, my demographically based estimate of hand-to-mouth status does not relate to the magnitude of this effect.

5 Online Experiment Design

While the empirical results using observational data are consistent with elections delivering a discount rate shock, they are also consistent with other channels. To test the specific channel proposed in my theory, I recruited an online panel to take a measurement of time rate of preferences using the Convex Time Budget (CTB) method pioneered by [Andreoni and Sprenger, 2012]. I conducted this experiment around both the 2022 and 2024 US Federal elections. For each of these two years, I worked with an online sample provider, VuPoint Research, to recruit a nationally representative sample of adult heads of household. For each year I recruited three waves at different dates relative to election day. The first wave was conducted 2 weeks before election day, the

Table 6: Education and Income

Depvar: monthly spending ($)	[1]	[2]	[3]	[4]
Share Ideo w/President	-2.459**	-0.0331	-0.185	-2.471**
	(0.457)	(0.608)	(0.638)	(0.643)
Share Ideo w/P * 4-year degree	0.422		0.913	0.441
	(0.753)		(0.773)	(0.764)
Share Ideo w/Congress	1.058	1.398	3.084**	-0.0394
	(0.595)	(0.790)	(0.827)	(0.833)
Share Ideo w/C * 4-year degree	-7.210**		-6.847**	-6.887**
	(0.988)		(1.015)	(1.002)
Share Ideo w/P * Income $40k-$70k		-2.799**	-2.931**	
		(0.887)	(0.892)	
Share Ideo w/P * Income $70k+		-3.784**	-4.047**	
		(0.856)	(0.878)	
Share Ideo w/C * Income $40k-$70k		-3.703**	-2.849*	
		(1.157)	(1.164)	
Share Ideo w/C * Income $70k+		-5.053**	-3.281**	
		(1.123)	(1.154)	
Share Ideo w/P * not HtM				0.005
				(1.004)
Share Ideo w/C * not HtM				2.459
				(1.307)
Constant	722.2**	692.8**	695.7**	721.9**
	(2.042)	(2.056)	(2.069)	-2.048
Observation	13,030,784	13,030,784	13,030,784	13,030,784
R-squared	0.026	0.027	0.027	0.026
Number of households	207,715	207,715	207,715	207,715

Standard errors in parentheses: ** $p<0.01$, * $p<0.05$
Includes month and household FE and intercepts for the demographic variables being tested via interaction effects: college in col 2, income brackets in col 3, college and HtM in col 4.

second wave 2 weeks after election day, and the third wave 12 weeks after election day, shortly after inauguration. In 2022, I targeted 100 valid participants in each wave while in 2024, I targeted 80 participants in each wave. The change in sample size between 2022 and 2024 was due to available funding. In each year, the actual number reached varied from wave to wave on account of the selection procedure described below.

In each year, I asked the panel provider to target a panel that was representative of the 9 US census divisions. This meant setting population-based quotas for each division and refusing to admit participants from a division once that quota was met. In practice, the stops were not perfect as there was some delay in realizing which panelists had started the survey and thus a division could easily go over quota if, before the quota was applied by the panel provider, more panelists from that division entered than remaining spots existed. In practice, this meant these quotas tended to be lower limits rather than precisely hit targets and I often, though not always, overshot my targeted sample size.

I chose VuPoint rather than MTurk, PrimePanels, Prolific, or another popular online sample provider because they had several mechanisms for helping me ensure high quality respondents who would give serious thought to the somewhat onerous calendar questions that are the heart of the CTB method. First, the underlying panel of participants solicited by VuPoint is a repeated panel that is continually groomed and maintained based on the quality of participants. Second, they allowed me to pay only those participants who completed the task to my satisfaction and to do so immediately. This allowed me to preset decision parameters for rejecting participants based on the length of time it took to complete the calendar questions or those whose answers were simple repetitions of interior solutions that did not respond to changes in treatment.[2] Those who completed too swiftly were deemed not to have given much thought. *In the robustness section, I show that these answers are of much lower quality on several dimensions.*

The day-of process was as follows. First, I set targets for each division. Then, the VuPoint representative would open the survey to those in the pool provided by the sample provider. Entering subjects would be sent to the website I designed (more below). As a census division filled its quota, the VuPoint representative would block further members of that census division from entering, albeit with a lag that permitted overruns as mentioned above. As respondents completed, they would be evaluated as to whether to accept or reject according to rules based on time and completeness of answers mentioned above and then, if accepted, would be paid as described below. That would reject several applicants and then determine which divisions needed additional applicants. That category would be re-opened to fill the remaining quota. The lag between participants entering the queue and the accept or reject decision at the end would typically deliver a modest overrun of my targeted sample size. Table 7 shows the summary statistics for the 2022 sample while Figure 3 presents histograms with the distributions. The sample skews male and Democrat, is somewhat

[2]Repetition of a corner solution is entirely consistent with considered choice.

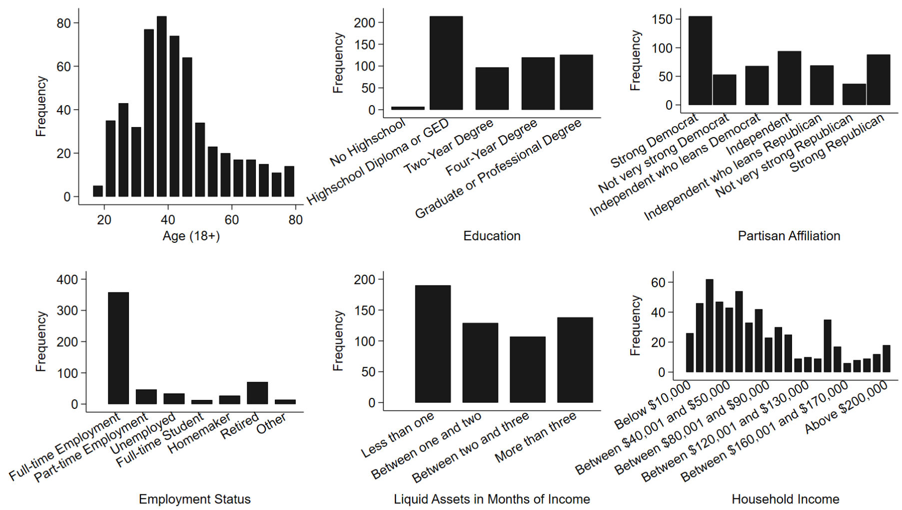

Figure 3: Histograms of the demographic responses from the 564 respondents.

more educated than the US average, but is otherwise reasonably aligned.

Table 7: Sample Statistics: Online Experiment

Variable	Obs	Mean	Std. dev.	Min	Max
Age	564	42.04	13.84	18	80
Female	557	0.41	0.49	0	1
Employed Full Time	564	0.63	0.48	0	1
Bachelors Degree	564	0.44	0.50	0	1
Household Income ($10,000)	564	7.11	5.48	0	20
Liquid Assets (months of income)	564	1.34	1.18	0	3
Partisan Affiliation (7-pt scale)	564	-0.41	2.14	-3	3

Subjects were paid with Amazon gift cards sent to the email address they specified in their survey responses. These cards went out on the day in question, as promised, and for an amount specified to the penny. Critically, the day-of cards were delivered within 5 minutes of completion, and advertised as such, to mimic as closely as possible the sense of the present bias parameter.

The survey was administered via a bespoke web interface to which respondents were redirected upon recruitment by the panel provider. The survey began with a battery of demographic questions listed in Table 12. After answering these simple questions, respondents were then given instructions on the CTB method. Respondents were required to click through the instructions and were given access to a video which explained showed examples. After reading the instructions, they proceeded to answer the CTB questions. Each subject was asked to make 45 convex budget decisions in which they allocate a fixed number of tokens between payment now and payment later. I choose my treatments to closely mimic those originally laid out by [Andreoni and Sprenger, 2012]. Like them, I requested 45 decisions involving combinations of early payment date (t), delay between early and late payment date (k), and interest rate (r).

$$(1 + r)c_t + c_{t+k} = m \tag{8}$$

In each decision, the respondent allocated 50 tokens between an early date and a later date. Tokens at the later date were always worth 20 cents. The value of tokens at the earlier date varied by decision, taking values p = 19, 18, 16, 13, 10. The number of days between the date on which the survey was taken and the early payment date varied, taking values t = 0, 7, 28. The number of days between the early payment date and the later payment date varied, taking values k = 28, 70, 98. Each possible combination of t, k, and p was implemented, meaning that respondents made 5x3x3 = 45 decisions. The variation in p and k ensured a variety of rates of return to patience.

As advertised to participants, one row was randomly selected for payment. Payments were made with Amazon gift cards sent to the email address provided by the participant. The amounts were those indicated by the participants' choices plus an extra $5 on each date. The extra money ensured that all participants would receive a non-zero payment on both early and late dates, thus removing the possibility that answers might be skewed to ensuring a single payment. If a participant allocated all tokens to the later date, they would earn ($5,$15). If the participant allocated all money to the earlier date and was unlucky in the choice of treatment, they might earn as little as ($10,$5). Given the average time spent by participants who gave valid answers was 14 minutes, these were very generous pay rates.

As per [Andreoni and Sprenger, 2012], the subjects' decision screen displayed a dynamic calendar and a series of nine decision tabs corresponding to the nine choice sets deriving from the combinations of t and k. Each decision tab presented 5 decision lines corresponding to the different values of p. For each tab, a different calendar was shown with the experiment date circled and the early and late decision dates circled and the delay between them highlighted to enable visualization of the delay. The interface automatically calculated the early and late payments for any allocation. After making their decisions for each line, respondents pressed a button to finalize and submit.

Show the selection criteria based on the speed of response
illustration of the questions
link to the video explanation

6 Online Experiment Results

I follow [Andreoni and Sprenger, 2012] in positing a time-separable CRRA utility function with β-δ discounting ([Laibson, 1997]),

$$U(c_t, c_{t+k}) = \frac{1}{\alpha}(c_t - \omega_1)^\alpha + \beta\delta^k \frac{1}{\alpha}(c_{t+k} - \omega_2)^\alpha \quad (9)$$

then using the responses to the token allocation decisions to estimate each respondent's δ, β, and α parameters where δ is the one-period discount factor, β is the present-bias parameter, and α is the CCRA curvature parameter. This is done by solving the consumer allocation problem to derive consumer demand as a function of the parameters.

$$ln(\frac{c_t - \omega_1}{c_{t+k} - \omega_2}) = \begin{cases} (\frac{ln\beta}{\alpha-1}) + (\frac{ln\delta}{\alpha-1})k + (\frac{1}{\alpha-1})ln(1+r) & \text{if } t = 0 \\ (\frac{ln\delta}{\alpha-1})k + (\frac{1}{\alpha-1})ln(1+r) & \text{if } t > 0 \end{cases} \quad (10)$$

Non-linear least squares estimation of this demand function can then recover estimates of the parameter values. To do so, I use the code posted by [Andreoni and Sprenger, 2012] with minor adaptations.

Figure 4 displays the histograms of the individually estimated utility function parameters for the 564 respondents. The top set are the raw histograms. The bottom set have split the sample into four categories by education and liquidity constraints. Those who are constrained are those who answered they would not have \$400 for an emergency. Those who are high education are those who have completed a 4-year degree.

The unconditional distribution of estimated α_i-s (Figure 4 top left) shows that while the bulk of the distribution is between 0.9 and 1, I do estimate curvature parameters in excess of 1 for 8.0% of the sample. My range is between 0.5 and 1.3 whereas Andreoni and Sprenger estimate very few values above 1 but some values that are negative. As can be seen in the lower right panel of Figure 4 and is confirmed by the regressions in table 8, I do not find a strong relationship between α_i and education, liquidity, income, or politics.

The unconditional distribution of estimated δ_i-s (Figure 4 top right) shows almost the entirety of the distribution clustered between 0.995 and 1. The translates into a range for an annual discount factor between 0.16 and 1. From the conditional histograms (lower right), there is a slight visual suggestion that those exhibiting extreme impatience are more likely to come from the liquidity constrained but this is not confirmed by the regression estimates.

The unconditional distribution of estimate β_i-s (Figure 4 top middle) is less peaked, with a pronounced left tail. This left tail is clearly dominated by the liquidity constrained. While less clear visually, regressions show those with lesser educational attainment are also more likely to exhibit present bias. There is also a nontrivial fraction, 9.4%, of overly patient respondents whose estimated present-bias parameter is greater than 1.

To investigate the relationship between the estimated parameters and the demographics of the respondents, I regress each of the estimated parameters in turn—α_i, β_i, δ_i— on self-reported household income, liquidity constraint, level of education, and ideological agreement with the federal government. Household income is reported in \$10,000 buckets up to "more than \$200,000". To flag the

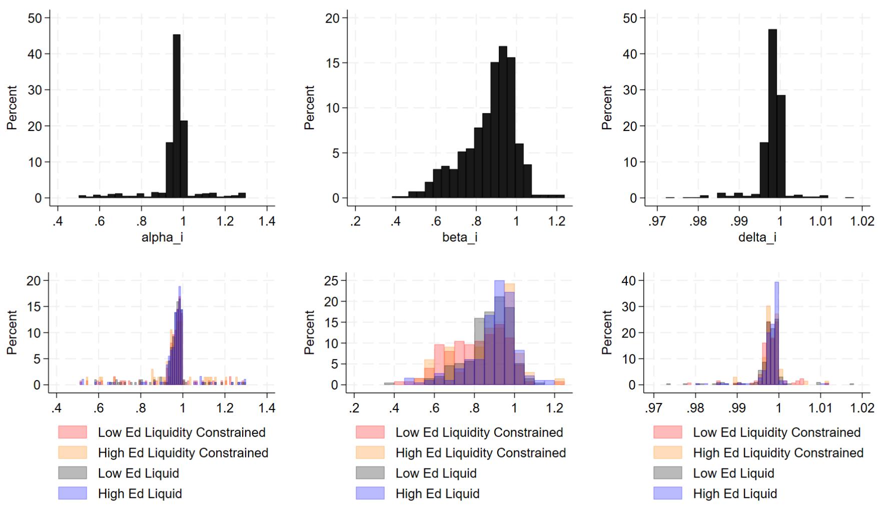

Figure 4: Histograms of the estimated utility function parameters from the 564 respondents. The top set are the raw histograms. The bottom set have split the sample into four categories by education and liquidity constraints. Those who are constrained are those who answered they would not have $400 for an emergency. Those who are high education are those who have completed a 4-year degree.

liquidity constrained, I asked the standard question from the SCF regarding whether the household would be able to absorb an unexpected $400 expense. I grouped educational attainment into a binary variable of whether the respondent has completed a 4-year college degree. Ideological agreement with the Federal government is based on their self-reported ideology and their self-reported belief as to which party is in control of Congress and the Presidency. Answers range from -2 for those who believe both branches are controlled by the opposition to +2 for those who believe both branches are controlled by their preferred party. For each of the three parameters, I include two treatments of the outliers where the estimated parameter lies beyond the theoretically permissible range (0,1]. In the first treatment, I winsorized the dependent variable if it fell outside the theoretically permissible range (0,1]. In the second treatment, I dropped that observation.

The regression results for δ are fairly easy to interpret. The constant is statistically indistinguishable from 1 in both specifications. While none of my covariates are significant when outliers are winsorized, dropping outliers shows that higher incomes is associated with more patience, though, somewhat surprisingly, optimism about the family's financial future is associated with less patience. In each case, the magnitudes of the impact of the covariate is extremely small.

The results for β are more interesting. Depending on which method of treatment for outliers is used, a college education and greater liquidity are associated with less present bias. Those with college education are more likely to give answers leading to an estimated $\beta_i > 1$; the college educated constitute 43.6% of the total sample but 58.5% of the winsorized $\beta - s$. Hence, whether we believe education is associated with present-bias in this sample depends on how to interpret this out-of-bounds behavior. Likewise, Perhaps the most interesting result is that ideological agreement with the federal government reduces present bias by an amount greater than that of a college education or an emergency cushion.

subject confusion and multiple switching responsiveness of mean choices to changing interest rates and delays

7 Discussion

This is a paper of two parts. In the first part, I document a connection between a household's monthly spending and their projected ideological congruence with the party in control of the executive and legislative branches of the federal government. Surprisingly, and in contrast to prior hypotheses, it is those in opposition who spend somewhat more. In the second part, I elicit time preferences from an online panel of subjects and show that ideological agreement with the federal government is strongly associated with their estimated present-bias parameters.

I argue that these results are consistent with households' present-bias varying according to their ideological agreement with their political leadership. I

Table 8: Demographic Influences on Time Preference Parameters

	[1]	[2]	[3]	[4]	[5]	[6]
dependent variable	Estimated α_i		Estimated β_i		estimated $delta_i$	
Liquidity:	0.00592	0.00888	0.0161	0.0294*	0.00869	-0.0000966
>2 months income	(0.009)	(0.009)	(0.016)	(0.012)	(0.006)	(0.000)
HH income	0.00168	0.00115	-0.00121	-0.000189	0.000412	9.61e-05**
($10,000 buckets)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.000)
Family Financial Forecast	-0.00513	-0.000355	-0.0145	0.000194	-0.00279	-0.000260*
	(0.006)	(0.006)	(0.011)	(0.008)	(0.004)	(0.000)
Earned at least	-0.00961	-0.0042	0.0595**	0.0193	-0.000728	8.52E-05
4-yr degree	(0.010)	(0.010)	(0.018)	(0.013)	(0.007)	(0.000)
Ideological agreement	-0.00245	-0.00552	0.100**	0.0638**	-0.00309	0.000134
with Federal govt	(0.010)	(0.010)	(0.017)	(0.012)	(0.007)	(0.000)
Constant	0.935**	0.933**	0.805**	0.821**	0.988**	0.997**
	(0.008)	(0.008)	(0.014)	(0.010)	(0.006)	(0.000)
Outliers $\alpha_i, \beta_i, \delta_i \notin (0,1]$	winsorized	dropped	winsorized	dropped	winsorized	dropped
Observations	564	446	564	446	564	446
R-squared	0.007	0.008	0.088	0.083	0.006	0.104

Std Errors in parentheses, ** p<0.001, * p<0.05

argue that this is consistent with scholarship in political science documenting citizen's emotional connection to politics and feelings of powerlessness when in opposition and with scholarship in psychology noting that powerlessness attenuates the connection to the future self. Given the relatively small size of the present effect– perhaps a single percentage point of monthly spending– and the balance of political ideology, the aggregate effects on spending are likely to be quite modest. However, as this is the first study of which I am aware to document a systematic shock to a discount parameter, the result should nonetheless be of interest to economists.

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Table 9: Partisan Identity Classification

Partisan Identity		Predicted Democrat	Predicted Independent	Predicted Republican	Predicted Total
Actual	Democrat	**188,076**	2,473	1,904	192,453
	Independent	2,356	**194,427**	2,031	198,814
	Republican	1,972	2,389	**135,968**	140,329
	Total	192,404	199,289	139,903	531,596

Correctly classified: 97.5%, erroneously classified as the "other" party: 0.7%; independents erroneously classified as partisans: 0.8%; partisans erroneously classified as independents: 0.9%.

Table 10: Household Partisan Affiliation

Party	**Household partisan affiliation score (A_h)**	**Combinations of Head of Household Affiliations**
Democrat	1	{D}, {D,D}
	0.5	{D,I}
Independent or Split	0	{I}, {I,I}, {D,R}
	-0.5	{R,I}
Republican	-1	{R}, {R,R}

Table 11: Control of Government

Partisan Control (C_b)	Executive	Legislative
-1	Republican	Republican
0	Independent	Split
1	Democrat	Democrat

Table 12: Online questions

Questions (in order asked)	Responses permitted
1. What is your age?	Type integer value between 18 and 99
2. What is your gender	Female/Male/Non-binary/Transgender/Prefer not to say
3. What state do you live in?	pulldown menu
4. What is your zipcode?	entry of 5-digit number
5. What is the highest level of education you have completed?	No HS/HS diploma or GED/2-yr degree/4-yr degree/grad or professional degree
6. Are you a head of household or are you a financial dependent?	head of household/financial dependent*
7. Approximately what is your annual household income?	Pulldown menu: Below \$10,000/\$10,000-20,000/.../\$190,000-\$200,000/Above \$200,000
8. Suppose that you have an emergency expense that costs \$400. Could you pay for this out of your savings?	Yes/No
9. Consider the amount you have in your savings and checking and other bank accounts plus the value of stocks and bonds. (But do not consider retirement accounts, health insurance, or home equity). After paying your credit card balances, what is the largest amount you would have left to meet an emergency expense?	less than one month of my income//between one and two months of my income//between two and three months of my income//more than three months of my income
10. Do you think a year from now that you and your family will be	better off financially/worse off financially/about the same financially
11. Do you think the business conditions in the US during the enxt year will be	improving/declining/staying about the same
12. Do you think the business conditions in your region during the next year will be	improving/declining/staying about the same

Table 13: Online questions continued

Questions (in order asked)	Responses permitted
13. In general, how would you describe your own political viewpoint?	Very liberal/liberal/moderate/conservative/very conservative/not sure
14. Would you say you follow what is going on in government and public affairs?	most of the time/some of the time/only now and then/hardly at all/I do not know
15. Which political party is the current US President a member of?	Republican/Democrat/I don't know
16. Which political party currently controls Congress as a result of the most recent election?	Republicans control both chambers/Democrats control both chambers/split with each controling one chamber/I do not know
17. Which political party is your Governor (or Governor elect if you just had an election) a member of?	Republican/Democrat/I'm from DC/I don't know
18. Which political party currently controls your state legislature as a result of the most recent election?	Republicans control both chambers/Democrats control both chambers/split with each controling one chamber/I'm from Nebraska/I'm from DC/I do not know
19. What party do you identify with?	strong Democrat/not very strong Democrat/independent who leans Democrat/independent or no affiliation/independent who leans Republican/not very strong Republican/strong Republican
20. What is your primary source of political news?	television/newspaper or magazine (online or print)/word of mouth/social media/other news source/none
21. What best describes your employment status?	full-time employment/part-time employment/unemployed/homemaker/full-time student/retired/other
22. What is your email (to send you your money upon completion of the calendar section)	
23. Just so we know you're not a robot and are paying attention, please select the one item below that is alive.	desk/airplane/rockpencil/eagle/mountain/football

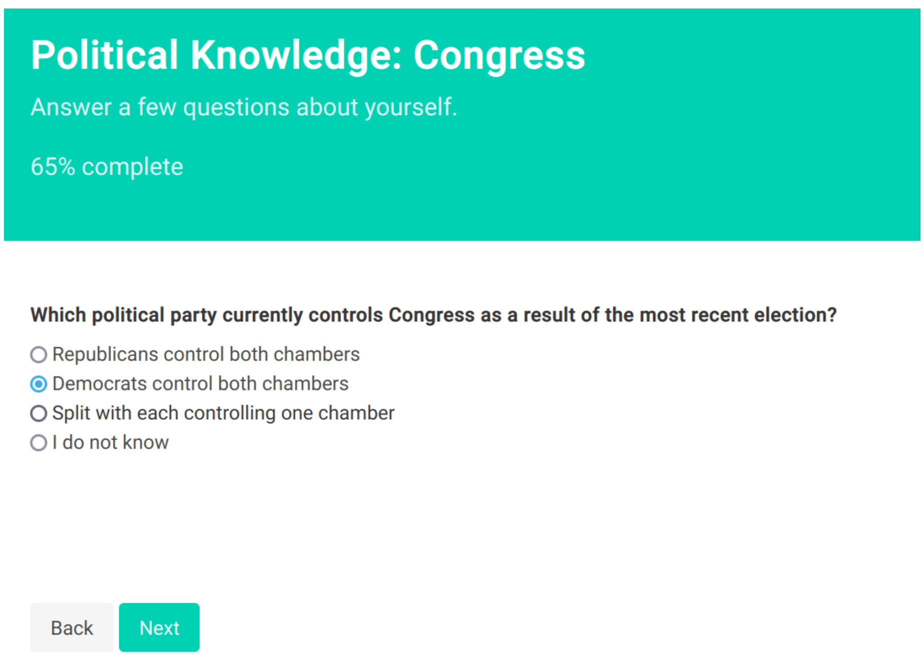

Figure 5: The online interface for the questions in Table 12

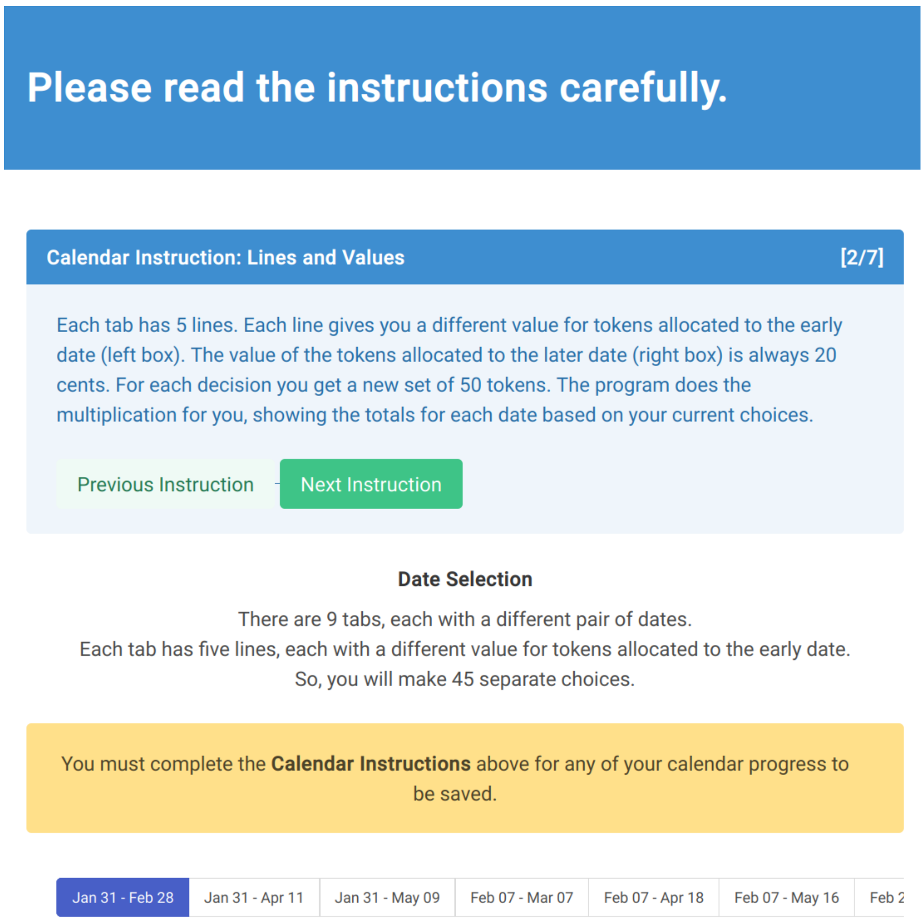

Figure 6: The instructions for the token allocation questions.

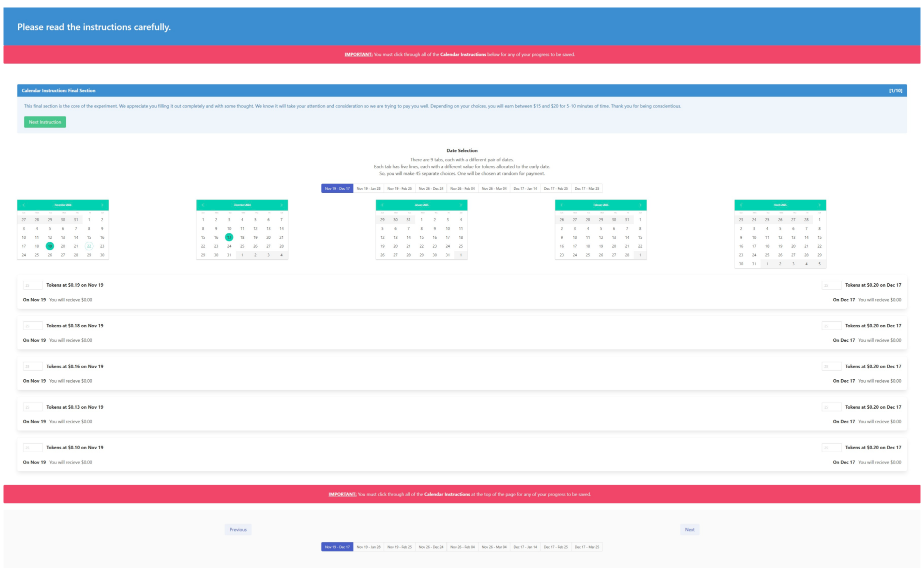

Figure 7: The online interface for the token allocation questions.