Quarterly Journal of Political Science, 2016, 11: 363–383

Economic Behavior and the Partisan Perceptual Screen

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ABSTRACT

Partisans report different perceptions from the same set of facts. According to the "perceptual screen" hypothesis, this difference arises because partisans perceive different realities. An alternative hypothesis is that partisans take even fact-based questions as an opportunity to voice support for their team. In 2009, Gerber and Huber conducted the first behavioral test of the perceptual screen hypothesis outside of the lab. I re-analyze Gerber and Huber's original data and collect new data from two additional U.S. elections. Gerber and Huber's finding of a relationship between partisanship and economic behavior does not hold when observations from a single state-year (Texas in 1996) are excluded from their analysis. Out-of-sample replication based on the two U.S. presidential elections since the original study similarly shows no evidence of an effect. Given these results, the balance of evidence tips toward the conclusion that economic perceptions are not filtered through partisanship.

Keywords: Political economy; political psychology; political parties; political science

Students of political behavior have long asserted that identification with a party alters a voter's perception of the world. In 1954, Berelson *et al.* wrote, "The mirror that the mind holds up to nature is often distorted in accordance with

*I would like to thank Alan Gerber and Greg Huber for making their data publicly available for replication and for their support of this work. Don Green provided guidance throughout the research process. Peter Aronow, John Bullock, David Mayhew, and Pia Raffler gave valuable feedback.

Supplementary Material available from:
http://dx.doi.org/10.1561/100.00015100_supp
MS submitted on 17 July 2015; final version received 28 September 2016
ISSN 1554-0626; DOI 10.1561/100.00015100

the subject's predispositions." Campbell *et al.* (1960) argue that partisanship is stable not because it acts as a reliable representation of political belief, but because it shapes belief. Partisanship generates a "perceptual screen" through which people filter information, causing adherents of opposing parties to perceive different information from the same set of facts. Voter belief and behavior vary by party and hold steady through life because partisans of different stripes move through different versions of reality.

Until recently, the primary evidence in support of a perceptual screen came from what voters said in interviews and surveys. Bartels (2002) demonstrated that Democratic and Republican respondents to the 1988 American National Election Studies (ANES) survey reported different answers to questions of fact related to the state of the economy. Subsequent survey-based and lab studies have found partisan differences in evaluation and interpretation of facts (e.g., Bolsen *et al.*, 2014; Evans and Pickup, 2010; Gaines *et al.*, 2007; Lavine *et al.*, 2012; Taber and Lodge, 2006) and reported spending (Enns and Anderson, 2009). Stanig (2013) documents a relationship between party identification and assessments of the economy outside of the United States, using survey data from 33 countries. And Gerber and Huber (2010) used a panel survey, interviewing respondents immediately before and immediately after the 2006 US midterm elections to show that divergent partisan reports are not the result of changes in respondent partisanship, different criteria by which partisans evaluate the economy, or selective perception or exposure.

But distinguishing true belief from mere talk is difficult in the self-reported data of surveys. Divergent partisan survey responses regarding questions of fact could simply be a form of political cheerleading. Did the Democratic respondents to the 1988 ANES truly look back over the eight years of the Reagan administration and perceive a crumbling economic landscape, while Republican respondents saw a flourishing economy? Partisan respondents may give different reports not because they perceive different realities, but because people responding to surveys take even fact-based questions as an opportunity to express an opinion.[1] Experiments encouraging accurate responses to fact-based questions suggest the presence of cheerleading in survey responses:

[1]Gerber and Huber (2010) make an effort to distinguish partisan cheerleading from a perceptual screen by looking for partisan divergence on measures of self-reported happiness and projected vacation/holiday spending in the 2006 Cooperative Congressional Election Survey. The authors argue that these "alternative measures appear unrelated to politics and therefore unlikely to impel the respondents to tailor their response to their general partisan disposition." This argument rests on a narrow construal of partisan cheering, and of which elements in a political survey respondents take as related to politics. Shortly after an election in which a respondent's favored party suffers an unexpected blow, the partisan may indicate displeasure with the outcome of the election in her responses to a political survey by expressing a general "things are bad" sentiment, reporting low mood and belt-tightening intentions along with a shabby national economy. More to the point, given that the outcome of an election can be expected to affect the mood of self-reported partisans for reasons unrelated to a perceptual screen, and considering the complicated relationship between mood and spending intentions (see, e.g., Cai *et al.*, 2009; Lerner *et al.*, 2004; Mano,

partisan differences can be reduced either by offering monetary incentives for correct answers or by asking respondents to answer as accurately as possible for the sake of scientific validity (Bullock *et al.*, 2015; Prior *et al.*, 2015).

Economic behavior is a more candid expression of belief than survey response. In 2009, Gerber and Huber drew upon economic data to conduct the first large-scale study examining partisan behavior for evidence of a perceptual screen.[2] Using data on taxable sales from roughly 1,450 US counties, the authors looked at post-presidential-election change in local consumption as a function of local partisanship. If partisans truly perceive different economic realities, the economic activity in areas with different partisan leanings should reflect this difference in belief. For example, say that a Democratic candidate wins an election. If partisans adjust their economic expectations and behavior according to whether their party won, then post-election sales should increase relative to pre-election sales in more Democratic counties, and decrease relative to pre-election sales in more Republican counties.

Gerber and Huber's analysis of taxable sales data over four U.S. presidential elections (1992–2004) showed just such a pattern — a positive relationship between an area's partisanship and consumption after a party win. Partisans appeared to increase their spending when their candidate won. This pattern looked like a behavioral manifestation of the partisan perceptual screen: partisans acting on different beliefs about the economy under either party's administration. Gerber and Huber's analysis thus seemed to provide powerful new evidence that partisans not only report different versions of reality, but make economic decisions as if living in different versions of reality.

This paper reassesses the evidence for a perceptual screen in partisan economic behavior. I re-analyze the original dataset and collect new data from the 2008 and 2012 US presidential elections. This new data expands the original sample from 19 states to 23, drawing an additional 356 counties into the analysis. My analysis shows that there is no systematic evidence of a perceptual screen in partisans' economic behavior. Examining each of the elections in the original dataset separately reveals a positive relationship in only one year, 1996. A positive result only in this year is difficult to reconcile with the theoretical model, which indicates that, all else equal, 1996 should be the US election *least* likely to exhibit an effect. Disaggregating the data reveals that the 1996 result is driven by a strong relationship in a single state, Texas. When observations from this state-year — Texas 1996 — are excluded from analysis of the original sample, the estimated effect shrinks to one-third of the result reported in Gerber and Huber (2009). Incorporating the new

1999; Saliagas and Kellaris, 1986), these measures seem unsuited to isolate the effect of a perceptual screen.

[2] Trigger (2006) conducted a similar test on a smaller scale. Gerber and Huber (2009, p. 408, fn. 1) note that the relationship Trigger identifies seems overly sensitive to influential data points: "[Trigger's] results appear to be driven by a small number of outlier counties . . .".

data collected from the 2008 and 2012 US presidential elections, the overall effect is a precisely estimated zero.

Overall, the data analyzed here show scant evidence that partisans make economic decisions from behind a perceptual screen. Though partisan reports about the state of the economy depend on which party is in power, partisan economic behavior does not reflect these reports. My findings at the population level are consistent with the individual-level experimental work mentioned earlier (Bullock *et al.*, 2015; Prior *et al.*, 2015). At both the aggregate level and the individual level, the evidence now suggests that it is what partisans say, not what they see, that is distorted in accordance with predispositions.

Electoral Uncertainty and Implications for 1996

In Gerber and Huber's model, the perceptual screen takes the form of a differential effect of partisan control of the White House on partisans' expectations of lifetime income. Before an election, uncertainty about the winning party causes uncertainty about lifetime income. Upon learning the outcome of the election, uncertainty is resolved. In light of a Democratic win, Democratic partisans — who before the election had accounted for the eventuality of the lower lifetime income they foresaw should a Republican win and moderated their consumption accordingly — correct for their uncertainty-driven downweighting by adjusting consumption upward after the election. Republican partisans — who had been accounting for the possibility of the higher lifetime income they associated with a Republican win — respond to the resolution of uncertainty by adjusting their consumption downward after the Democratic win.

The magnitude of the change in consumption is a function of the common prior probability of a win — that is, the magnitude of the electoral uncertainty. If the probability of a Democratic win is 90%, a Democrat downwardly adjusts consumption by 10% of the income she expects under a Republican win, spread over each period of the life-cycle, in order to smooth income given the possibility of a negative shock. If the Democratic candidate then wins the election, the Democratic partisan increases her spending by 10% of the difference in income she expected under a Republican win, in order to make up for her pre-election downward adjustment. For any given level of perceptual screen, as uncertainty over the outcome increases — that is, as the 10% probability moves towards 50% — the difference between pre-election and post-election spending becomes more pronounced. All else equal, an effect of partisanship on economic behavior should be most apparent in elections characterized by the greatest uncertainty, least apparent in elections characterized by the least uncertainty.

With a given level (or strength) of perceptual screen, partisans who expect a better economy when their party wins will expect it to be better by roughly the same amount from one election to the next. But the strength of partisans' perceptual screen might vary across elections. To have a sense of how much the strength of the perceptual screen would need to differ from one election to the next in order to offset the effect of electoral uncertainty, consider a fairly certain election — 90% probability of a Democratic win — and a completely uncertain election — 50% probability of a Democratic win. Assuming that the perceptual screen affects Democrats and Republicans to the same extent, but varies from election to election, the perceptual screen in the fairly certain election would have to be more than five times stronger than the perceptual screen in the uncertain election in order to produce a larger effect.

This discussion of the theoretical model highlights two main points. First, at any given level of the perceptual screen, electoral uncertainty determines a partisan's change in spending. When a partisan is less certain about the outcome of an election, her spending will change more upon learning the election result. If that partisan were very certain about the outcome of an election, her spending would change very little upon learning the result. In other words, an effect of partisanship on economic behavior should be most pronounced in elections characterized by the greatest uncertainty. A second point to note is that even if the strength of the perceptual screen varies from election to election, in order to observe an aggregate effect of the perceptual screen that is larger in a fairly certain election than in an uncertain election, something must cause the perceptual screen to increase by around a factor of five in the predictable election relative to the toss-up.

Given the importance of electoral uncertainty for the manifestation of a perceptual screen, a positive result only in 1996 is difficult to reconcile with the theoretical model. Hibbs' "Bread and Peace" model predicted a Democratic win in the 1996 US presidential election with a probability between 94% and 98% (Hibbs, 1996). In the leadup to the election, Bill Clinton consistently led by a wide margin in public opinion polls (see, e.g., Gallup, 1996). This contrasts with the other elections covered in the sample, which all featured at least one reversal of the lead and were characterized by much closer margins in general (Gallup, 2008, 2012). Of the six US elections in the sample, partisans would be expected to have the least uncertainty about the outcome of the election in 1996. The theoretical model would indicate that, barring dramatic growth of the perceptual screen during 1996 as compared to the other election years, 1996 should exhibit the least evidence of a partisan perceptual screen.

Estimation

Gerber and Huber apply their model to data from the four US presidential elections between 1992 and 2004 to test whether partisan economic behavior after an election suggests evidence of a partisan perceptual screen. This section describes Gerber and Huber's primary specification and the measurement of each variable. The same variable definitions apply to the new data I collect, and I take Gerber and Huber's primary specification as the starting point for my analysis in the next section.

The model indicates an area's change in consumption as the dependent variable, and the area's proportion of partisans as the independent variable. The dataset consists of quarterly records of county-level taxable sales as a measure of consumption and county-level Democratic vote share as a measure of partisanship. Gerber and Huber's primary specification is:

$$\begin{aligned}
&\ln\left(\frac{\text{sales}_{i,t+1,\text{1st Qtr.}}}{\text{sales}_{i,t,\text{3rd Qtr.}}}\right)\\
&\quad = B_0 + B_1 * \text{avg.Dem.voteshare}_i {*} \text{winningparty}_t\\
&\qquad + B_2 * \frac{D.V._{\cdot i,t-1} + D.V._{\cdot i,t-2} + D.V._{\cdot i,t-3}}{3}\\
&\qquad + B'\text{State-year} + B'\text{County} + e,
\end{aligned}$$

for area i in election year t.

The dependent variable, change in partisans' consumption upon learning which party won the election, is represented by each county's log change in taxable sales from Quarter 3 of the election year (the quarter immediately preceding that in which the election takes place) to Quarter 1 of the following year (the quarter immediately following that in which the election takes place). Gerber and Huber collected taxable sales data from 26 states, a census of states with recoverable sales tax data.[3] They report that no geographic or attrition bias is apparent in which states make data available. The primary specification includes two sample restrictions that exclude some states and counties: (1) analysis is limited to states for which data are available during at least one Democratic win (1992 or 1996) and one Republican win (2000 or 2004); and (2) any county with a recognized Native American reservation is excluded from analysis due to concerns about casino sites and on-reservation sales reporting. These restrictions reduce the functional sample in the original analysis to the 19 states shown in Table 2.

To account for a county's recent consumption history, Gerber and Huber include a three-year lagged average of the dependent variable on the right-hand side. Quarter-3-to-next-year-Quarter-1 log change in sales is calculated for each of the three years since the previous election, and the average of those three inter-election values is included as a covariate for each election-year observation.

County-level partisanship score, the independent variable, is obtained by averaging the county's Democratic share of the two-party presidential vote

[3] Data are obtained from the following states: AL, AR, AZ, CA, CO, FL, GA, IA, ID, IL, KS, ME, MN, MO, NC, NE, OK, SC, SD, TN, TX, UT, VA, VT, WA, and WI.

over the four elections from 1992 through 2004, and multiplying this county-level average by -1 in the two Republican-won years (2000 and 2004). This partisanship score thus takes on just two values for each county: for the years in which the Democratic presidential candidate wins, the partisanship score is the county's average Democratic proportion of the two-party presidential vote over the elections analyzed; for the years in which the Democratic presidential candidate loses, partisanship score is the negative of that value. In the first two periods studied, every county's partisanship is characterized by some positive fraction Democratic; in the next two periods studied, every county is characterized by the negative of that fraction.

Because each county's average Democratic voteshare is multiplied by -1 in Republican-won years, the more Republican a county is, the more positive its 2000/2004 partisanship score relative to less Republican counties. For example, compare a county with an average Democratic voteshare of 0.7 (largely Democratic) to a county with an average Democratic voteshare of 0.2 (largely Republican). In 2000/2004, the more-Republican county's partisanship score would be -0.2, while the more-Democratic county's score would be -0.7. This means that in each election, an increase in spending by partisan winners after the election would be marked by a positive coefficient.

Fixed effects are included for state-year and for county. State-year indicators differentiate each state in each election year, so that, for example, Texas-1992 observations are distinguished from Texas-1996 observations. Inclusion of state-year indicators accounts for the effect of partisanship on consumption change that is particular, within a single election, to the collection of counties that make up a state. Inclusion of county indicators accounts for cross-election differences in effect particular to each county. The relationship between partisanship and consumption change is then estimated on the basis of variation within a county over different elections. Standard errors are clustered at the county level.

Note that the combination of a lagged dependent variable and fixed effects creates biased coefficients and underestimated standard errors (see Hurwicz, 1950; Nickell, 1981; Orcutt, 1948). These problems arise from modeling influence along a short dimension (time) and heterogeneity in a longer dimension (county). The coefficient bias is of order $1/T$, so that the bias diminishes as the number of time periods observed increases. If the shorter dimension becomes sufficiently long, the coefficient bias can be ignored. But if the dimension along which heterogeneity is modeled is longer, the bias in standard error estimates remains no matter how long the shorter dimension becomes. This downward bias to the standard errors increases with the ratio of N/T, and as long as N is larger than T, the downward bias does not diminish even as both dimensions approach infinity (Alvarez and Arellano, 2003; Arellano and Hahn, 2006; Lee and Yu, 2010). The result is over-rejection of the null hypothesis of no effect, with a false rejection rate that rises with N/T (Gaibulloev *et al.*, 2014). This means that adding election years to the dataset decreases false rejection of the null hypothesis, but standard errors are still underestimated with this specification as long as the data include more counties than years.

Analysis

In this section, I begin by replicating Gerber and Huber's original analysis. I also present results from each election year of the original sample separately. I then disaggregate the data to show an anomalous result in Texas, 1996, and present results from the original data when the TX-1996 observations are excluded from analysis. Following my re-analysis of the original data, I present results from the updated dataset, incorporating data from the 2008 and 2012 US presidential elections. Finally, I show variations on the original specification that represent different strategies of identifying an effect while avoiding (or reducing) the biases described earlier. An appendix presents the results graphically, mapping the cross-election estimates for each county.

Replication and Re-analysis of Original Data

Table 1 replicates the original analysis in the first column, and presents estimates from each election of the original sample separately in the next four columns. In the original analysis, fixed effects are included for county and state-year, so that the pooled estimate is based on variation within each county across Democratic wins (1992 and 1996) versus Democratic losses (2000 and 2004). In the columns for an individual election year, fixed effects are included for each state, but not for county or state-year because only one observation is present for each county in a given year. This means that in the individual-election specifications, estimates are based on variation across counties within each state-year, pooling states for that election year. Because the coefficient bias mentioned earlier diminishes as the number of periods observed increases, this bias is considerably larger for the individual-election specifications. However, standard errors in the individual-election specifications have less downward bias because fixed effects are included for state rather than for county.

When each election is estimated separately, only the 1996 election shows a positive relationship between county partisanship and post-election change in spending. As noted in the section describing the theoretical model, 1996 would seem the least likely of the US elections to show evidence of a partisan perceptual screen, as it was characterized by the least uncertainty of the six U.S. elections in the sample.

Table 2 shows each state in the sample considered separately. Due to the sample restrictions described earlier, the functional sample for the original analysis consists of 19 states comprising roughly 1,450 counties. The number

Table 1: Regression estimate of the effect of partisanship and election outcomes on local consumption.

	Pooled	1992	1996	2000	2004
Partisanship score	0.030*	−0.002	0.200*	−0.033	−0.033
	[0.015]	[0.027]	[0.078]	[0.025]	[0.047]
Three-year avg. LDV	0.144	0.877	0.594	0.764	0.813
	[0.042]	[0.040]	[0.182]	[0.050]	[0.048]
Constant	0.035	−0.177	−0.190	0.001	0.093
	[0.023]	[0.011]	[0.076]	[0.014]	[0.024]
Observations	5426	1087	1453	1422	1464
R-squared	0.811	0.733	0.549	0.644	0.655
County FE	Yes	No	No	No	No
State FE	No	Yes	Yes	Yes	Yes
State-year FE	Yes	No	No	No	No

Robust standard errors in brackets.
Significance marked only for Partisanship score: $^{*}p < 0.05$.

of observations (counties) on which each estimate is based is shown in the column marked “N”. In the left panel, fixed effects are included for county and for election year, so state-level estimates are based on variation within each county across Democratic wins versus losses. This specification follows that used by Gerber and Huber, except pooled only to the state level rather than to the national level. The right panel isolates the 1996 election. Here, estimates are based on variation across counties within each state at the 1996 election, and no pooling takes place.

The left panel shows that, estimating across all four elections in the original sample, a roughly equal number of states show positive coefficients (9 states) and negative coefficients (10 states). In the right panel, which shows only the data from 1996, the coefficient for Texas stands out dramatically, 10 percentage points larger than the next largest estimate. Without Texas, the distribution of the other states’ coefficients has a mean of 0.031 and a standard deviation of 0.106, indicating that the Texas estimate is more than three standard deviations higher than the mean.

The original finding is not reproduced when observations from this single state-year — Texas 1996 — are excluded from analysis. Table 3 shows estimates based on the original data, pooling both counties and state-years as in the original analysis. The left column shows the original estimate. The middle column shows the coefficient estimate when the Texas counties’ 1996 observations are removed from the analysis, and the right column shows the estimate when all 1996 observations are excluded. County and state-year fixed effects are included in all three columns.

Table 2: Estimated effect of partisanship and election outcomes in sampled states.

Across elections: 1992–2004					1996 Election			
State	Coef.	Std. err.	N	Elections	State	Coef.	Std. err.	N
AZ	−0.001	0.082	12	4	AZ	n/a	—	3
CA	0.061	0.061	152	4	CA	−0.059	0.077	38
CO	0.071	0.061	240	4	CO	0.031	0.312	60
FL	0.101	0.051	247	4	FL	0.087	0.104	61
IA	−0.050	0.059	392	4	IA	0.062	0.121	98
ID	0.079	0.084	117	3	ID	0.053	0.155	39
IL	−0.006	0.060	306	4	IL	−0.028	0.073	102
KS	−0.019	0.051	412	4	KS	−0.076	0.082	103
MO	−0.004	0.042	345	3	MO	−0.024	0.066	115
NC	−0.008	0.056	294	3	NC	0.014	0.064	98
NE	−0.026	0.079	338	4	NE	0.265	0.186	84
OK	−0.075*	0.037	292	4	OK	−0.054	0.057	73
SC	0.034	0.041	180	4	SC	−0.004	0.064	45
TN	0.049	0.057	301	4	TN	−0.033	0.199	69
TX	0.083*	0.034	1003	4	TX	0.366**	0.086	251
UT	−0.150	0.145	80	4	UT	−0.123	0.192	20
VA	0.002	0.047	535	4	VA	0.014	0.092	134
WA	−0.073	0.124	69	3	WA	0.220	0.224	23
WI	0.078	0.232	111	3	WI	0.184	0.337	37

$^{*}p < 0.05$, $^{**}p < 0.01$.
Robust standard errors in both panels.
The left panel includes fixed effects for county and year, std. err. clustered on county.

The middle column shows that the coefficient estimate based on all of the original data except Texas 1996 is one-third the size of the original estimate, and smaller than its standard error. Disaggregating the original data reveals that the original finding appears to be driven by a strong positive relationship between partisanship and post-election spending in a single state during a single election year, 1996. The estimate based solely on the data from this year (0.200 [0.078], shown in Table 1) falls well outside the 95% confidence interval estimated using data from the other three elections (95% CI: −0.031, 0.046), as can be seen from the right column of Table 3.

Analysis of Updated Dataset

The results presented earlier highlight the strong influence of data from a single state-year in Gerber and Huber's original estimate. But influential points do

Table 3: Pooled estimate with and without Texas 1996.

	Original Sample	Excluding TX-1996	Excluding 1996
Partisanship score	0.030*	0.010	0.007
	[0.015]	[0.015]	[0.020]
Three-year avg. LDV	0.144	0.128	0.252
	[0.042]	[0.043]	[0.091]
Constant	0.035	0.043	−0.113
	[0.023]	[0.022]	[0.047]
Observations	5426	5175	3973
R-squared	0.811	0.826	0.833
County FE	Yes	Yes	Yes
State FE	No	No	No
State-year FE	Yes	Yes	Yes

Robust standard errors in brackets.
Significance marked only for Partisanship score: $^{*}p < 0.05$.

not invalidate an estimate: just because a model is sensitive does not mean it is wrong.[4] New data must be collected to learn whether out-of-sample tests support the weight given to influential points, or reinforce the interpretation of these points as outliers.

I updated Gerber and Huber's original dataset by collecting county-level taxable sales records and partisanship data through the 2012 US presidential election, thus expanding the dataset from four elections to six. Adding data from the 2008 and 2012 elections also serves to draw four new states into the analysis — Alabama, Arkansas, Georgia, and South Dakota — which together contribute 356 counties, 25% of the original sample N. These states were excluded from the original analysis due to the restriction that a state's data must include at least one Republican and one Democratic win. The original data for these states covered a range of years with only Republican-won presidential elections. Adding the Democratic-won elections of 2008 and 2012 allows the inclusion of these states in the updated analysis. The new data comprises the 19 states listed in Table 2 as well as AL, AR, GA, and SD.

For the analyses based on this updated data, I recalculate each county's partisanship score to cover the elections included in the updated sample. Instead of averaging the county's Democratic share of the two-party presidential vote over the four elections between 1992 and 2004, the updated partisanship score averages over the six elections between 1992 and 2012. I also corrected minor data errors present in the original dataset. These changes do not alter

[4]See for example, Stigler's critique of Kramer (1971).

Table 4: Updated sample.

	Pooled	1992	1996	2000	2004	2008	2012
Partisanship score	0.001	0.014	0.201*	−0.016	−0.002	−0.008	0.017
	[0.017]	[0.031]	[0.077]	[0.025]	[0.038]	[0.070]	[0.089]
Three-year avg. LDV	0.135	0.904	0.593	0.772	0.831	0.794	0.635
	[0.042]	[0.034]	[0.182]	[0.047]	[0.042]	[0.064]	[0.162]
Constant	0.084	−0.189	−0.188	−0.041	0.109	−0.358	−0.013
	[0.030]	[0.015]	[0.077]	[0.004]	[0.019]	[0.036]	[0.039]
Observations	9390	1080	1446	1555	1773	1730	1806
R-squared	0.702	0.748	0.548	0.640	0.649	446	0.474
County FE	Yes	No	No	No	No	No	No
State FE	No	Yes	Yes	Yes	Yes	Yes	Yes
State-year FE	Yes	No	No	No	No	No	No

Robust standard errors in brackets.
Significance marked only for Partisanship score: $^{*}p < 0.05$.

results from the original sample in any meaningful way. The original finding can be reproduced from the updated dataset by reverting to the 1992–2004 partisanship score and restricting analysis to the observations available in the original dataset.

Table 4 shows an estimate based on all six elections in the updated dataset taken together, along with each of the six US elections estimated separately — an updated version of Table 1. Estimates and sample sizes of the four original elections differ slightly from the original estimates due to the minor data error corrections. The coefficient estimate based on the pooled data from all six of the elections in the updated dataset is almost exactly 0, as shown in the first column of Table 4. The 95% confidence interval for this updated estimate now nearly excludes the original estimate of 0.030 shown in the first column of Table 1 (95% CI: $-0.032, 0.037$). The 1996 estimate remains more than 10 times greater in magnitude than any other election-year coefficient, and as in the original sample (see Table 3) falls well outside the 95% confidence interval estimated using data from the other five elections (95% CI: $-0.049, 0.028$).

As noted in the section on estimation, problems arise when a model includes both fixed effects and a lagged dependent variable. Table 5 shows estimates from five alternative identification strategies, each representing a variation upon the full pooled specification presented in column 1 of Table 4. The first column in Table 5 drops the lagged dependent variable while retaining county and state-year fixed effects. The lagged dependent variable was included in the original model to account for a county's average Quarter-3-to-next-year-Quarter-1 change in consumption over the three years leading up to each election. Removing this variable eradicates the biases that arise from pairing a lagged dependent variable with fixed effects. Given that fixed effects in

Table 5: Alternative identification strategies.

	No LDV	State-year FE	State FE	Year FE	No FE
Partisanship score	−0.002	0.025	−0.050**	0.065**	−0.051**
	[0.017]	[0.017]	[0.004]	[0.016]	[0.004]
Three-year avg. LDV		0.741	0.481	0.555	0.524
		[0.031]	[0.038]	[0.032]	[0.034]
Constant	0.127	0.170	−0.021	−0.002	−0.060
	[0.064]	[0.097]	[0.008]	[0.008]	[0.003]
Observations	9390	9390	9390	9390	9390
R-squared	0.699	0.560	0.332	0.334	0.278
County FE	Yes	No	No	No	No
State FE	No	No	Yes	No	No
Year FE	No	No	No	Yes	No
State-year FE	Yes	Yes	No	No	No

Robust standard errors in brackets.
Significance marked only for Partisanship Score: $^{**}p < 0.00$.

this specification account for unobserved differences between state-years and between counties, forgoing the additional adjustment for a county's recent characteristic Q3:Q1 change in consumption should not introduce any serious threats to identification.

The remaining specifications shown in Table 5 retain the lagged dependent variable and discard county-level fixed effects. Columns 2–4 include, respectively, state-year, state, and year fixed effects along with the lagged dependent variable. Column 5 presents estimates with no fixed effects. Under further assumptions about the nature of selection bias, estimates from a specification including only the lagged dependent variable and from a specification including only fixed effects can provide bounds on the causal effect (for a complete exposition, see Guryan, 2001, Appendix 1 or Angrist and Pischke, 2009, Section 5.4 for an example).

A number of robustness checks are presented in the supplementary materials. For example, the independent variable in the analyses presented here includes future election results in the measure of partisanship. Using an independent variable based only on past values of partisanship does not meaningfully alter the results. Another potential concern is that partisanship in the South was changing during the period studied, complicating detection of a perceptual screen in this region as partisans switched affiliation from Democrat to Republican. As shown in the supplementary materials, no compelling evidence of a partisan perceptual screen appears when southern states are excluded from the analysis. Additional analyses using alternative constructions of the dependent variable, population weighting, and spatially-weighted models are also presented in the supplementary materials.

Discussion

The intersection of politics and economics represented by the perceptual screen hypothesis has profound theoretical and substantive importance. Bartels (2002) states that his analysis of survey evidence "suggests that partisan loyalties have pervasive effects on perceptions of the political world" (p. 138). But the evidence Bartels adduces in support of the perceptual screen hypothesis implies a stronger claim than he articulates: partisan bias in perception of objective economic conditions would indicate that the influence of partisan loyalties stretches beyond the explicitly political realm. Gerber and Huber (2009) presented evidence supporting this strong form of the perceptual screen hypothesis: partisanship altered economic perception, shaping individual consumption decisions. Gerber and Huber's behavioral data seemed to reveal the economic manifestation of a partisan perceptual screen. The magnitude of the estimate from their analysis implied a dramatic effect of partisanship on aggregate consumption.

My analysis shows that Gerber and Huber's finding of a relationship between partisanship and economic behavior is driven by a single anomalous state-year. But identification of the model's sensitivity to this outlier is post-hoc analysis. More telling is my finding that out-of-sample replications based on the 2008 and 2012 US presidential elections, data unavailable to Gerber and Huber at the time of their analysis, show no evidence of an effect. Considering all six US elections together, the data here show the relationship between partisanship and economic behavior to be a precisely estimated zero.

It could be the case that partisans truly behave as if perceiving two different economic realities, but that county-level taxable sales data are not refined enough for detection of the behavioral distortions. Analysis of more fine-grained consumption data may reveal convincing evidence of a perceptual screen. As elections occur and data accumulate, coefficient bias and over-rejection of the null hypothesis will continue to decrease. Replications in other countries will shed additional light on the question.

Bullock *et al.* (2015) and Prior *et al.* (2015) demonstrated that, despite giving divergent responses to factual survey questions, partisans are aware of the truth (or are aware that they don't know the truth). In their conclusion, Prior *et al.* noted that whether or not partisan behavior reflects this awareness remained an open question: "It is possible that partisans are aware of uncongenial facts, but still ignore them in their judgments." My findings indicate that although partisans report biased perceptions of economy, their economic behavior reflects an unbiased perception of the state of the world.

Partisan divergence over the state of the economy is just one example of survey participants keying on party cues. Experiments on party-cue effects have found that the presence of party labels alone can alter how partisans respond to survey questions. For example, a Democratic respondent might report viewing a policy as more favorable when the policy is described as sponsored by Democrats than when that same policy is described as sponsored by Republicans; a Republican respondent would exhibit the opposite response pattern (see, e.g., Cohen, 2003; Greitemeyer *et al.*, 2009).[5] Similarly, observational studies have found response patterns in survey data suggesting partisan bias in interpretation of facts about political events (Gaines *et al.*, 2007), attribution of positive or negative experiences to policy change (McCabe, 2016), and evaluations of candidates and office-holders (Goren, 2007; Lebo and Cassino, 2007).

A large literature has developed trying to uncover what drives this phenomenon. In this literature, debate has primarily focused on whether respondents are employing an informational shortcut, or heuristic, in order to conserve cognitive effort (e.g., Lau and Redlawsk, 2006; Lupia and McCubbins, 2006), or engaging in motivated reasoning (e.g., Bolsen *et al.*, 2014; Cohen, 2003; Petersen *et al.*, 2013; Slothuus and Vreese, 2010). Since the partisan responses are taken to reflect genuine belief, the stakes of this debate are high. While dependence on heuristics can lead citizens astray (Kuklinski and Quirk, 2000; Lau and Redlawsk, 2006), such informational shortcuts are at least compatible with representative democracy. Motivated information processing, on the other hand, "raises deeply troubling questions about political representation and accountability ..." (Lavine *et al.*, 2012).

The preponderance of studies on partisan bias operate under the assumption that partisan divergence on survey questions reflects deeply rooted differences in partisan perception. Lavine *et al.* (2012) refer to the partisan perceptual screen as, "a principal, if normatively undesirable aspect of mass belief systems: Perceptions of political reality are inextricably intertwined with citizens' political preferences and identities." Recent studies report divergent survey responses as evidence of partisan differences in perception of the economy (e.g., Enns and McAvoy, 2012; Evans and Pickup, 2010; Stanig, 2013), perception of corruption (Anduiza *et al.*, 2013; Wagner *et al.*, 2014), perception of government responsibility (Bisgaard, 2015) and factual beliefs regarding policy issues (e.g., Joslyn and Haider-Markel, 2014).

But the findings here and elsewhere support a third possibility, raised nearly 20 years ago by Gerber and Green (1999): partisan bias in survey

[5] The effect of party cues has been found to vary a great deal by issue (see, e.g., Arceneaux, 2008) and by the extent and content of other information provided (Berinsky, 2009; Bullock, 2011).

responses may often represent a costless expression of partisanship rather than beliefs upon which partisans are willing to act. The evidence presented here, along with Bullock *et al.* 2015, Prior *et al.* 2015, and other recent studies (e.g., Berinsky *et al.*, 2011; Blais *et al.*, 2010; Hill, 2016), casts doubt on the existence of a partisan perceptual screen.

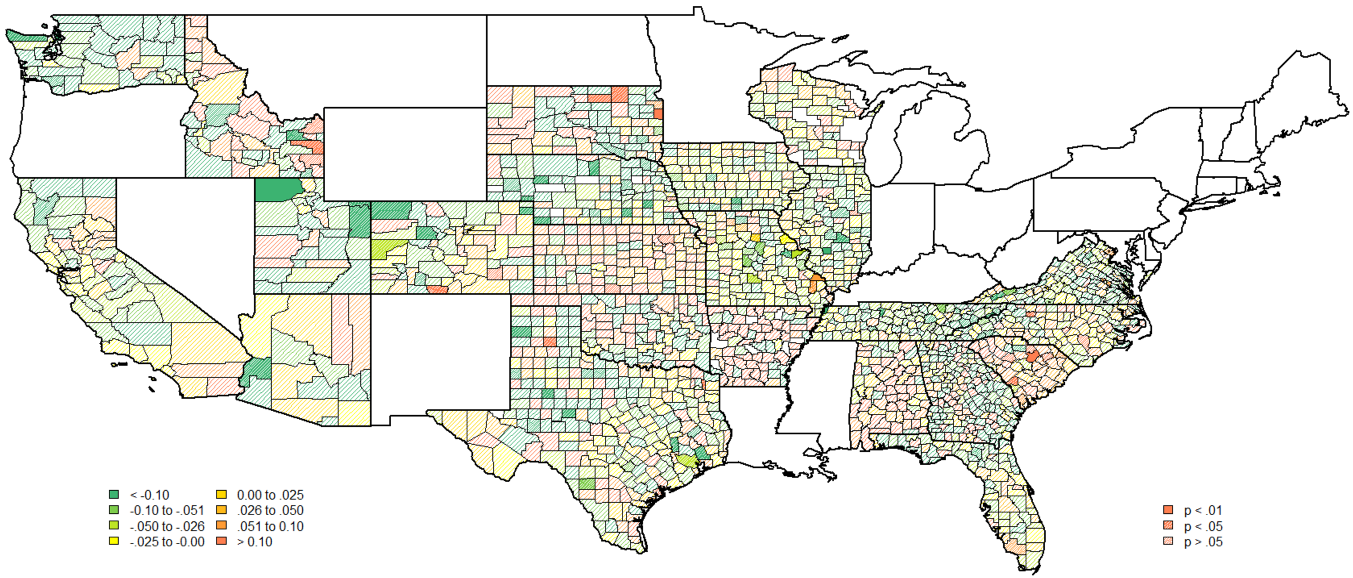
Prior *et al.* (2015) write that the "motivated responding" exhibited by partisans is a type of motivated reasoning, but with the important distinction that it does not involve bias in information processing. Though the results of this study highlight that partisan reports should not be assumed to correspond with partisan behavior in the real world, the bias evident in those reports is not necessarily negligible. If political elites take such reports as representing citizens' true beliefs and respond accordingly, then partisan cheerleading could be politically consequential even when respondents are unwilling to act on their own statements. The motivated reasoning literature remains informative in its efforts to illuminates the processes that lead to these reports.

In sum, data from the past six US presidential elections provide scant evidence that economic behavior reflects the partisan divergence in economic assessments documented in surveys. This finding has two notable implications. First, the results presented here reverse the balance of evidence on the perceptual screen hypothesis. Whether or not partisanship changes the way citizens see the world is central to debate over the nature of partisanship. Gerber and Huber remark that "perceptual bias is a (perhaps the) key channel through which partisanship affects political behavior." (2009, p. 423). The data provide no indication that partisanship causes perceptual bias.

Second, the results speak to the methodological question of whether surveys provide a valid instrument for studying behavior. In their conclusion, Gerber and Huber write: "Had we found no partisan differences in consumption following elections, this would have cast doubt on the importance of survey reports of different partisan expectations regarding the economy, as well as on the whole collection of partisan-tinged responses to economic assessment items." The results presented here suggest conclusions about the link between partisanship and economic behavior that differ diametrically from those of the original analysis.

Appendix — Figure A1

Figure A1 maps the population-weighted cross-election coefficient estimate for each county in which at least four elections are observed. This map provides a geographic rendering of the data along the original dimension of analysis, in which a pooled estimate is based on variation within each county across Democratic wins (1992, 1996, 2008, 2012) versus Democratic losses (2000 and 2004).



Green indicates that across elections, a negative relationship exists between a county's avg. partisanship and the county's post-election change in spending: Partisans in green counties increase spending after the opponent wins.
Orange indicates that across elections, a positive relationship exists between a county's avg. partisanship and the county's post-election change in spending: Partisans in orange counties increase spending after their own candidate wins.
Solid fill indicates the coefficient is estimated with more certainty; lighter shading indicates lesser degrees of estimate certainty.

Figure 1: Partisanship score and post-election spending. Within cross-election estimates change from pre- to post-election spending regressed on Partisanship score Partisanship score is avg. 92–12. Democratic share of two-party Presidential vote, multiplied by -1 in 00 and 04.

Orange indicates a positive relationship between partisanship and pre- to post-election change in sales. A county in which partisans increase spending after a win would be colored orange. Green indicates a negative relationship: a county in which partisans tend to increase spending after a *loss* would be colored green. The certainty of the cross-election estimate is indicated by fill-pattern. In this map, then, behavior indicative of a partisan perceptual screen would appear as a more-orange, and more solidly orange, United States.

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