I have been retained by Defendant-Intervenors NRSC and Republican Governor's Association counsel in the case of NANCY CAROLA JACOBSON et al., Plaintiffs, v. KEN- NETH DETZNER, in his official capacity as the Florida Secretary of State, Defendant. I have been asked by the Defendant-Intervenors to review the second report submitted by Dr. John Krosnick for the Plaintiffs. In this report I will refer to the variety of different reports that have been submitted thus far in this case. For brevity and consistency I will refer to the first Krosnick Report as "KR1" and the second Krosnick Report as "KR2". I will also refer to the first report submitted by myself, Dr. Michael Barber, as "BR1".

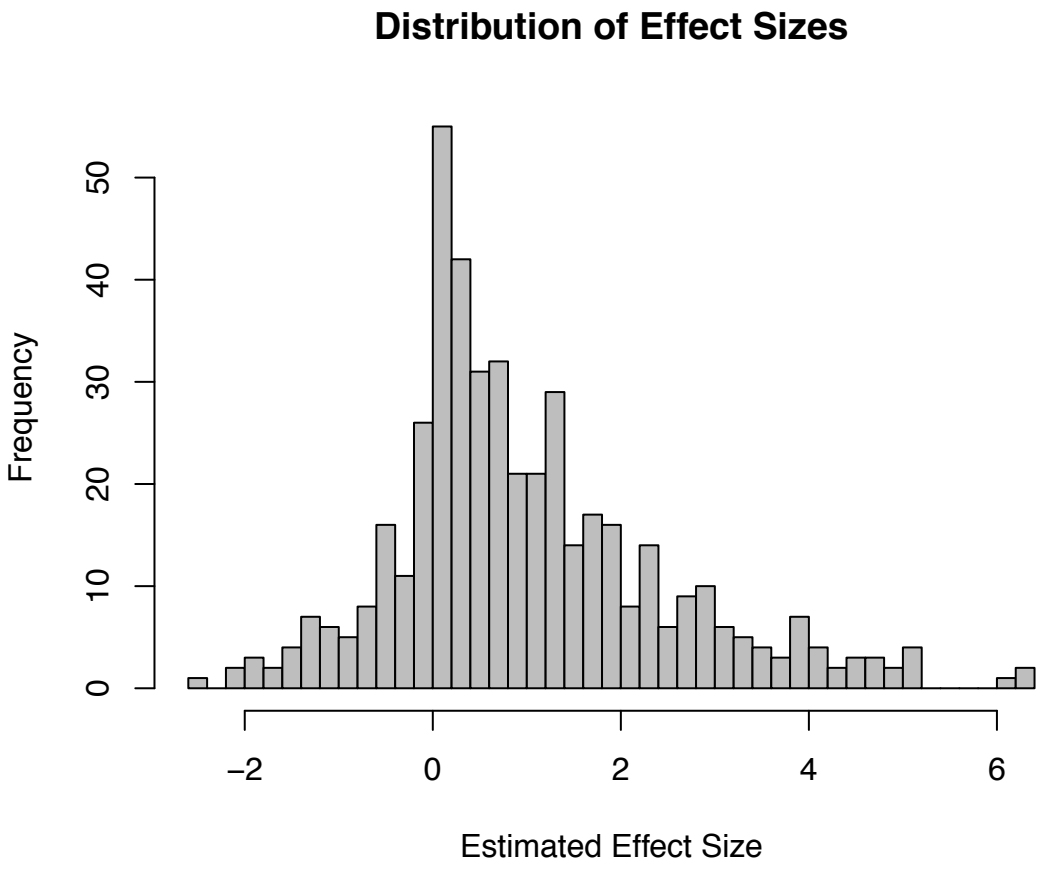
Primacy Effects Estimated in Ohio

In KR2, Dr. Krosnick points to data entry errors in BR1 summarizing primacy effects that were originally reported in Table 8 or KR1. Dr. Krosnick is correct that a copy/paste error in the "ohio.csv" spreadsheet duplicated some of the entries from Table 8 in KR1 and I thank him for pointing this out. Correcting those errors, however, does nothing to change the results that are reported in Figure 1 of BR1. The average primacy effect is reported in BR1 as 1.01 and median effect is reported in BR1 as 0.68. The corrected data show that the average primacy effect is 1.00 and the median effect is 0.67. Thus, my statement in BR1 remains largely the same:

> "Table 8 [of KR1] actually shows that on average the estimated effect of ballot ordering in Ohio is between .68 and 1 percentage point. This is much smaller than the 6 percentage points reported in the Krosnick memo (pg. 5, BR1)"

In neither KR1 nor KR2 does Dr. Krosnick report the summary statistics of Table 8. Rather than report summary statistics, he continues to point out that "there were many primacy effects in these races (Pg. 4, KR2)". While there may be many positive values, I suggest that summary

statistics such as the mean and median present a much better description of the data rather than simply pointing to whether some of the data are positive or negative. And in these data the mean and median estimated effects are quite small.



Variation in Primacy Effects

In KR2, Dr. Krosnick states, "In the Miller and Krosnick (1998) study, for example, primacy effects were found in virtually all races examined, even though the magnitude of the primacy effects varied from race to race to some degree, depending partly on the characteristics of the race." I will simply quote from the article noted above to suggest that this isn't actually the case. The first paragraph of their summary of the study, Miller and Krosnick

(1998) state: “Significant name-order effects occurred in 43 percent of the tests we conducted and in 48 percent of the races we examined… And 89 percent of the significant name-order effects we observed were primacy effects, whereby candidates listed early were advantaged. (pg. 315).” By this accounting, 89% of 43% is 38% (.89*.43 = .38) meaning that only 38% of races in this study observed positive, statistically significant ballot order effects. I do not consider 38% to be “virtually all”.

In KR2, Dr. Krosnick also states that I am incorrect when I stated that name order effects were smaller in high profile races with partisan cues. Again, I will quote from Miller and Krosnick (1998) where they state: “Name-order were stronger in nonpartisan races than in partisan races. This finding reinforces the notions that partisan affiliations help voters choose among candidates and that parties as institutions facilitate democratic processes (pg. 315-316).” The authors go on to state: “…name-order effects were stronger for races that had received less coverage by the news media. Second, name-order effects were stronger in races that had the highest rates of rolloff, suggesting that voters knew relatively little about the candidates (pg. 316).” By Miller and Krosnick’s own accounting, name order effects were stronger in partisan races and higher profile races where candidates are better-known and voters are more informed.

Study of Primacy Effects in Florida

Dr. Krosnick agrees with me that studies of ballot order effects in which candidate names are placed on the ballot in a random ordering have been “very informative regarding primacy effects.” I also agree with Dr. Krosnick that it is possible to study this question outside of a randomized trial. However, Florida presents a particularly difficult case. The reason is that there

is a direct correlation with a candidate's placement on the ballot and their party's performance in previous elections. Given that the party that performed better in the prior gubernatorial race is given first placement on the ballot, it is difficult to determine if candidates who are placed first on the ballot are performing so well because of their placement on the ballot or simply because the voters in the state prefer candidates from that party. Consider the following hypothetical: suppose there are no ballot ordering effects and voters in Florida suddenly prefer Democratic candidates over Republican candidates. Democratic candidates, including the gubernatorial candidates, will start to perform better than Republican candidates in elections. As a result of Florida's ballot ordering law, Democratic candidates will also appear first on the ballot. However, the increased performance by Democratic candidates is simply due to a shift in voter preferences and not their placement first on the ballot. However, a statistical analysis would suggest that being placed first on the ballot is related to higher vote shares. This is because the two variables (voters' preferences for a particular party and ballot placement) are highly correlated.

Dr. Krosnick points to observational studies of smoking as an example of how conclusions can be drawn from non-experimental data. I strongly agree that it is highly likely that smoking is bad for one's health and causes cancer. However, smoking provides an excellent example of the difficulty of measuring outcomes from observational data. If those with other poor health habits also smoke, it is difficult to know how much of one's poor health outcomes are attributable to smoking and how much should be attributed to other poor habits that are strongly correlated with smoking. The same logic applies to Florida elections. It is extremely difficult to determine with observational data how much of a candidates' higher vote

share is due to ballot placement and how much is due to voters in the state having a preference for candidates of that party since ballot placement and prior vote performance are not only correlated but directly related to one another by Florida statute. Furthermore, in the regression analysis conducted by Dr. Krosnick, it is *impossible* to disentangle the different effects of being placed first on the ballot and having the same partisanship as the governor. These coattails effects are well documented in prior scholarly research for both sharing the partisan affiliation of the president and governor of the state.[1]

Furthermore, I retain the belief that Dr. Krosnick's regression analysis of Florida election results has significant problems. KR2 responds to my criticism of the data used in the regression analysis by weighting the regression by county population and running a different regression by removing duplicate counties. However, I remain concerned that analyzing the data (even with these weights) at the county-election level severely biases the results of the regression and over-weights rural counties and candidates whose districts overlap multiple counties. These problems make the regression analysis unrepresentative of the state and make it impossible to know what to take from the results. It is helpful to step back and consider what objective we are trying to accomplish – we want to know if appearing first on the ballot makes an individual voter more likely to vote for that candidate. Individual level data, i.e. who each voter voted for,

[1] Meredith, Mark. (2013). Exploiting Friends-and-Neighbors to Estimate Coattail Effects. American Political Science Review, 107(4), 742-765.
Hogan, Robert. (2005). Gubernatorial Coattail Effects in State Legislative Elections. Political Research Quarterly, 58(4), 587-597.
Campbell, James and Joe Sumners. (1990). Presidential Coattails in Senate Elections. American Political Science Review, 84(2), 513-524.
Campbell, James. (1986). Presidential Coattails and Midterm Losses in State Legislative Elections. American Political Science Review, 80(1), 45-63.

would be the ideal for answering this question. Dr Krosnick (KR2, pg. 37) and I both agree that this is the case. However, votes are private and only aggregate election results are reported. Thus, the next best approach would be to aggregate up to the candidate level and look at how much a candidate's vote shares increased as a result of his/her name being placed first on the ballot. This suggests that the best possible way to organize the data is at the candidate level, meaning one observation per candidate. Introducing county boundaries and splitting candidate's election results by the various counties that overlap the candidate's electoral district takes us further away from the ideal study. Given that we cannot analyze individual votes, the next-best alternative is to analyze candidate election outcomes. This suggests using candidates' overall vote shares as the unit of analysis. Using counties double counts candidates whose districts overlap multiple counties. The better approach would be to use electoral districts as the unit of analysis and have one observation per candidate. On page 38 of KR2, Dr. Krosnick presents the results of a new analysis that uses districts rather than counties as the unit of observation. The results appear to change from his original analysis, but no regression table is reported so it is difficult to fully ascertain what the results are. However, the summary he provides shows that the effect appears to attenuate while also failing to achieve traditional standards of statistical significance. We should note that all of the prior studies of ballot ordering effects in which Dr. Krosnick is an author analyze data at the level at which the name

ordering occurs. None of them conduct analyses at the county level, which duplicates observations for many candidates, as Dr. Krosnick does here in Florida.[2]

In KR2, Dr. Krosnick points to several studies that use counties as the unit of analysis. The problem is that while all of these studies use counties as the unit of analysis, Dr. Krosnick uses county-candidates as the unit of analysis. This means that individual candidates and races are included multiple times because candidates' districts overlap multiple counties. For example, Florida's governor's and senators' districts cover all 67 of Florida's counties. If candidates name ordering were changed across counties, it would then make sense to compare election results for Governor in one county to another county. However, this is not the case in Florida. The ordering on the ballot is the same for all voters in a candidate's district. And because Dr. Krosnick is interested in studying *election* outcomes here, the unit of analysis should be the *electoral* district, not the portion of that electoral district that overlaps each county.

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[2]Kim, N., Krosnick, J. A., & Casasanto, D. (2015). Moderators of candidate name order effects in elections: An experiment. *Political Psychology*, *36*, 525-542 analyze the effects at the individual voter level.
Chen, E., Simonovits, G., Krosnick, J. A., & Pasek, J. (2014). The impact of candidate name order on election outcomes in North Dakota. *Electoral Studies*, *35*, 115-122 analyze the effects at the precinct level because candidate names are randomized across precincts, which is not the case in Florida.
Pasek, J., Schneider, D., Krosnick, J. A., Tahk, A., & Ophir, E. (2014). Prevalence and moderators of the candidate name-order effect: Evidence from all statewide general elections in California. *Public Opinion Quarterly*, *78*, 416-439 analyze the effects at the California assembly district level because candidate names for statewide office are randomized across assembly districts, which is not the case in Florida.
Miller, J. M., & Krosnick, J. A. (1998). The impact of candidate name order on election outcomes. *Public Opinion Quarterly*, *62*, 291-330 analyze the effects at the precinct level in Ohio because candidate names are randomized across precincts, which is not the case in Florida.

In KR2 Dr. Krosnick also offers a justification for his inclusion of Ohio election results in his regression analysis of Florida election outcomes, which I suggest is an inappropriate decision. Dr. Krosnick offers the following justification for this variable: "This variable was included to allow for the possibility that from election to election, the success of candidates in Florida elections may be partly a function of general trends in the popularity of the Republican Party and of the Democratic Party nationally (KR2, pg. 24)." I agree with Dr. Krosnick that it would be important to account for national partisan trends in election results, especially since elections have become increasingly correlated nationally, as Dr. Krosnick also notes. However, if controlling for national trends is the objective, then why not include a variable that actually measures this? Ohio is not the entire United States, and there are unique events in Ohio that effect Ohio election results, as there would be in any state. Furthermore, national election results are very easily obtained. The regression could have included any number of national measures of partisan and/or electoral trends rather than a measure of only Ohio. Some possibilities include the national vote share obtained by either party in the previous presidential election, the proportion of votes won by Republicans or Democrats for all U.S. House elections in the prior election, or perhaps an aggregate index that combines presidential and congressional voting nationally. By including only Ohio data, the regression actually measures the correlation between Florida election results and Ohio election results. Some of that correlation will be because of national trends, but another portion of the correlation will be unique to Ohio election results and the idiosyncrasies of Ohio elections that are separate from national trends. Dr. Krosnick incorrectly claims that I believe that it was a wise decision to include the Ohio variable in the regression. KR2 says, "Dr. Barber… note[s] that the result

obtained with this variable to be sensible and expected." What I actually said was that I was not surprised that Ohio elections and Florida elections were correlated. However, I stated in my prior report that the more appropriate variable would measure national electoral trends, not only Ohio electoral trends. Given this, I reaffirm my discomfort with the inclusion of this variable and restate my belief that this decision combined with the decision to analyze counties-elections rather than elections as the unit of analysis makes the regression results unhelpful for answering the question of ballot ordering effects.

Finally, I agree with Dr. Klick that the standard errors of the regression should be clustered since there is correlation across units in each election cycle. Standard errors provide a measure of the degree of certainty we have around an estimated statistical effect. Correlation across observations in the data can lead to the appearance of more certainty (smaller standard errors) than is actually the case. In Florida, it is likely that there is correlation across units (candidates) because all candidates from one party are given first placement on the ballot after their gubernatorial candidate wins the election. Because the treatment (i.e. being placed first on the ballot) is not administered candidate by candidate, but rather to all candidates across the state, there is reason to suspect correlations across observations regarding performance in future elections. The typical way to account for this correlation in the standard errors of a regression is to use "clustering" of standard errors, which allows the regression to consider the presence of this correlation. Without clustering, the regression is forced to assume zero correlation across observations, which seems false in this case.[3]

[3] See Abadie et. al (2017) for a detailed explanation of when to cluster standard errors in regression analysis. Abadie, Alberto, Susan Athey, Guido Imbens, and Jeffrey Wooldridge. (2017). When Should You Adjust Standard Errors for Clustering? NBER Working Paper No. 24003. http://www.nber.org/papers/w24003

Dr. Krosnick elides the degree of agreement within the social sciences on the need to cluster standard errors at the level of the treatment, which in this case is administered statewide every four years after gubernatorial elections (see King and Roberts, 2015 for a summary of the prevalence of clustered standard errors in political science literature).[4] Furthermore, if there is uncertainty as to whether to cluster the standard errors by year or four-year gubernatorial cycle, then why not do both to see if the difference is substantial. Ideally, the standard errors would change very little. If there is a large degree of difference, this suggests that the regression suffers from misspecification and potential omitted variable bias (King and Roberts, 2015). An additional solution would be to use heteroskedasticity robust standard errors, which do not require any decisions as to clustering at years, electoral terms, or any other factors. However, Dr. Krosnick does none of this in his analysis. High degrees of correlation within clusters can lead to false certainty and artificially low standard errors. Including clustered standard errors appropriately accounts for the uncertainty in the model.

Use of CPS Data

Dr. Krosnick suggests an inconsistency in my use of the CPS data to point to demographic differences between Florida, North Dakota, Ohio, and New Hampshire and Dr. Klick's criticism of the CPS data. These two facts are not inconsistent, however. I use the CPS insofar as I was simply pointing to differences reported in Dr. Krosnick's own report (Tables 2-7). Furthermore, the criticism of the CPS is centered around vote overreporting, not

[4] https://gking.harvard.edu/files/gking/files/robust_0.pdf

demographics overall. Thus, my illustration of the dramatic demographic differences in these states is not affected by a concern over how voting is reported in the CPS.

I, Michael Barber, am being compensated for my time in preparing this report at an hourly rate of $400/hour. My compensation is in no way contingent on the conclusions reached as a result of my analysis.

Michael Barber

July 20, 2018