Rebuttal Report of Kenneth R. Mayer, Ph.D.

January 22, 2019

This report presents my responses to the December 17, 2018 report of Sean Trende, and the December 17, 2018 report of Dr. Brian Gaines.

I. Summary

- With one minor exception, neither Gaines nor Trende responds to or criticizes my analysis of how specific district boundaries in Act 43 were drawn to secure a partisan advantage for Republicans, nor my analysis of packing and cracking. Trende makes a claim about several assembly districts, arguing that the 2016 presidential results show that elections are too unpredictable to forecast the effects of gerrymanders (Trende Rpt. Para. 56). He is incorrect. All of the districts Trende mentions voted as the partisan baseline predicted. In fact, what the 2016 election in these districts shows is how effectively Act 43 insulated Republican Assembly candidates from even major shifts in public opinion and voter preferences.

- Gaines challenges the use of baseline partisanship measures as theoretically complicated and empirically inaccurate, arguing that the concept of partisanship is difficult to measure. He is incorrect, and ignores a long-standing and accepted political science literature that uses measures of baseline partisanship as a fundamental method of analyzing redistricting plans. His argument also contradicts his own published work, in which he has used the baseline method to evaluate redistricting maps. It further ignores that one of the first steps the Assembly's consultant, Dr. Keith Gaddie, took in assisting the legislative aides who drew the maps used in Act 43 was to establish a partisan baseline measure using statewide election results.

- Gaines argues that split-ticket voting undermines the usefulness of baseline partisanship as a measure of district performance. But the literature he cites is outdated (often by decades). Recent work on voter polarization has shown that levels of ticket-splitting are lower than at any time since World War II, and that partisan loyalty is higher. Down-ticket elections are increasingly nationalized, reflecting national partisan forces that lead to consistent voting behavior. And again, Gaines' argument contradicts what Dr. Gaddie and the legislative aides who drew the map actually used to analyze alternative maps. In calculating a partisan baseline, Dr. Gaddie testified that his work for the Assembly was intended to assess district performance under different circumstances. Gaines' opinions raise a fundamental question: Why would the Assembly's consulting expert have calculated a partisan baseline as an initial step in drawing the maps used in Act 43 if it wasn't useful?

- Contrary to Gaines' argument, recent elections in Wisconsin show extremely high levels of partisan consistency. The 2016 presidential vote in a district is an accurate predictor of voting in 2016 Assembly elections (with r^2 measures of 0.99 for

Democrats and 0.97 for Republicans). Voting in Assembly elections is, moreover, highly stable from one election to the next.

- Trende's objection to inference using observational data is an oversimplification. Observational data is commonly used to make inferences. Trende clarified in his deposition testimony that he does not dispute that, apparently backing away from the position in his report that it is "uniquely difficult." He qualifies his agreement with that statement, however, by stating that caution should be exercised in drawing inferences based on observational data. Although it is correct that the validity of those inferences depends on the nature of the underlying theory, Trende proposes a standard for drawing inferences from observational data that would invalidate much of social science research altogether.

- Trende's dismissal of evidence that Act 43 has hurt Democratic competitiveness, fundraising, and ability to recruit challengers ignores the underlying theory and evidence about *why* those outcomes are an expected result. Nationwide evidence, extending back to 1972, confirms the phenomena: large pro-Republican efficiency gaps reduce Democratic fundraising, increase the percentage of legislative districts that Democrats do not contest, and reduce the percentage of Democratic incumbents who return to office.

- Trende claims that Act 43 has had little effect on enacted policy, arguing that the post-Act 43 change is the same as the change from 2010-2012. But he ignores the fact that the overall 4-year change from 2010 (pre-Act 43) to 2014 is the largest ever recorded in Wisconsin. He applies the wrong standard in arguing that "whatever Act 43 has done, it has not transformed Wisconsin into Mississippi, South Carolina, or Georgia." The correct evaluative standard is not whether Wisconsin is Mississippi, or whether Wisconsin has become more (or less) liberal than the rest of the United States. The correct evaluative standard is to ask whether Act 43 has locked in policy changes in Wisconsin, resulting in the Wisconsin legislature enacting more conservative policies over a longer period than observed in recent history, by preventing Democrats from having an opportunity to compete for political power, even in the face of shifts in public opinion.

II. Gaines

Gaines does not respond to any of the conclusions in my report, although he makes two general claims about the use of partisan baseline metrics. First, he argues that the underlying partisanship of a district – he refers to the concept of a "normal vote" – is theoretically complicated, difficult to measure, and unstable. And second, he argues that baseline partisanship is a poor predictor of election outcomes in Wisconsin Assembly districts.

Gaines is incorrect on both counts.

Individual vote choices and aggregate outcomes are a function of more than just partisanship. But it does not follow, in any sense, that baseline partisanship itself is not a useful theoretical concept, nor that it is uninformative as to likely election results in a district. Gaines ignores the basic notion that baseline partisanship measures are *designed* to factor out other variables (incumbency, candidate characteristics, campaign issues, national conditions, etc.) to arrive at an estimate of the vote that is independent of district-specific factors; in fact, as I note below, Gaines himself has made this very point in his own published work. The concept of the normal or baseline vote is at the very core of analyzing redistricting plans, which alter district boundaries so that the office-specific election results are less useful in understanding likely results when districts are reconfigured.

Gaines ignores the fact that there is an entire literature that uses baseline partisanship measures to estimate the effects of redistricting (examples include Cain 1985; Glazer, Grofman, and Robbins 1987; King and Gelman 1994; Ansolabehere and Snyder 2012; Ansolabehere, Snyder and Stewart 2000; Kousser 1996; Desposato and Petrocik 2003; Jacobson 2015), as well as a literature on different methods of estimating district-level partisanship using exogenous election data (Kernell 2009; Levendusky, Pope, and Jackman 2008; McDonald 2014; Jackman 2014; Canes-Wrong, Brady, and Cogan 2002). Gaines' position is, essentially, that because one cannot completely rule out the possibility of all split ticket voting, the underlying partisanship of a district as expressed in voting results is irrelevant in understanding and forecasting election outcomes. This is incorrect; if Gaines were right, it would obliterate the foundation of much of the scholarship on voting behavior written in the last six decades. And significantly, it would undermine and contradict the approach that Dr. Keith Gaddie took in performing his consulting work for the Assembly in 2011, when he assisted legislative aides in drafting the maps that were ultimately included in Act 43. One of the first things Dr. Gaddie did was to assist the legislative aides create partisan baselines for Wisconsin. The aides used this measure to assess the partisan performance of existing and potential Assembly districts in Wisconsin under different scenarios.

Much of Gaines' argument is irrelevant to my analysis and to Wisconsin. He criticizes party registration as a metric for partisanship (Gaines Rpt. at p. 2), though Wisconsin does not have party registration. He claims that "party identification is typically gauged via public-opinion surveys" (*id.* at p. 2), and argues that many Wisconsin voters are independent. This does not respond to my report, which does not rely on any public opinion surveys but instead focuses on actual voting behavior. Gaines' analysis of the stability of party identification, distinctions between "macropartisanship" and "micropartisanship" (*id.* at pp. 4-7) is similarly immaterial to the analysis of Act 43. Gaines engages in a lengthy digression on split-ticket voting in U.S. House and U.S. Senate elections and the 2000 presidential vote in Florida (*id.* at pp. 8-15). But none of this has anything to do with the underlying nature of Act 43, or whether Act 43 is a partisan gerrymander. What matters to the analysis of Act 43 is whether State Assembly districts were drawn in a way that packs and cracks voters based on their partisanship.

While split-ticket voting – usually defined as voting for different parties for President and Congress – occurred over the second half of the 20th Century, it has declined significantly in recent decades. Jacobson noted that split ticket voting in 2012 "was the smallest for any election" since 1952 (Jacobson 2015, 863). In 2017, Smidt found that "[d]espite a declining percentage of Americans claiming a party identification, Americans now exhibit the highest rates of party allegiance when voting across successive presidential elections" (Smidt 2017, 379) and that even independents behave like partisans when voting. Sievert and McKee (2018) found that even down-ticket races at the state level have become more nationalized. One reason for this pattern is an increasingly polarized electorate, in which party loyalties are more closely tied to social and cultural divisions. The result is an

> increase in straight-ticket voting and a growing connection between the results of presidential elections and the results of House, Senate, and even state legislative elections. To a greater extent than at any time in the post-World War II era, the outcomes of elections below the presidential level reflect the outcomes of presidential elections (Abramowitz and Webster 2015, 12).

In any event, the relationship between partisanship – as measured through voting in exogenous races – and voting in State Assembly races need not be perfect in order to be informative. Gaines argues that "[the] average Republican (or Democratic) vote for distinct offices *need not match*, particularly when one compares top-of-the-ballot offices and down-ballot offices" (Gaines Rpt. at p. 18, emphasis added). Of course, in an obvious and trivial sense, the voting totals "need not match." But as a basic empirical matter in Wisconsin, voting patterns for distinct offices *do match* in a way close enough to generate accurate inferences about the nature of baseline partisanship, and more importantly, to forecast outcomes in alternative configurations and predict which party is likely to prevail in any legislative district.

<u>A. Baseline Partisanship</u>

In asserting that baseline partisanship is not a meaningful quantity, Gaines makes a claim about ticket splitting in three wards in Wisconsin in the 2016 election: Wards 1 and 2 in Whitefish Bay, and what appears to be Ward 2 in the Town Germantown (Gaines Rpt. at p. 15).[1] But this exercise constitutes cherry-picking of the highest order: Gaines has selected only *three* of Wisconsin's roughly 6,500 wards, with a combined voting age population less than 0.03% of the state total, to make a general (and erroneous) assertion about split-ticket voting. Gaines does nothing to show that these three wards are representative of the other 6,497 wards, rendering his conclusions meaningless from a statistical and qualitative standpoint. Even as an example, his reliance on these wards is inapposite.

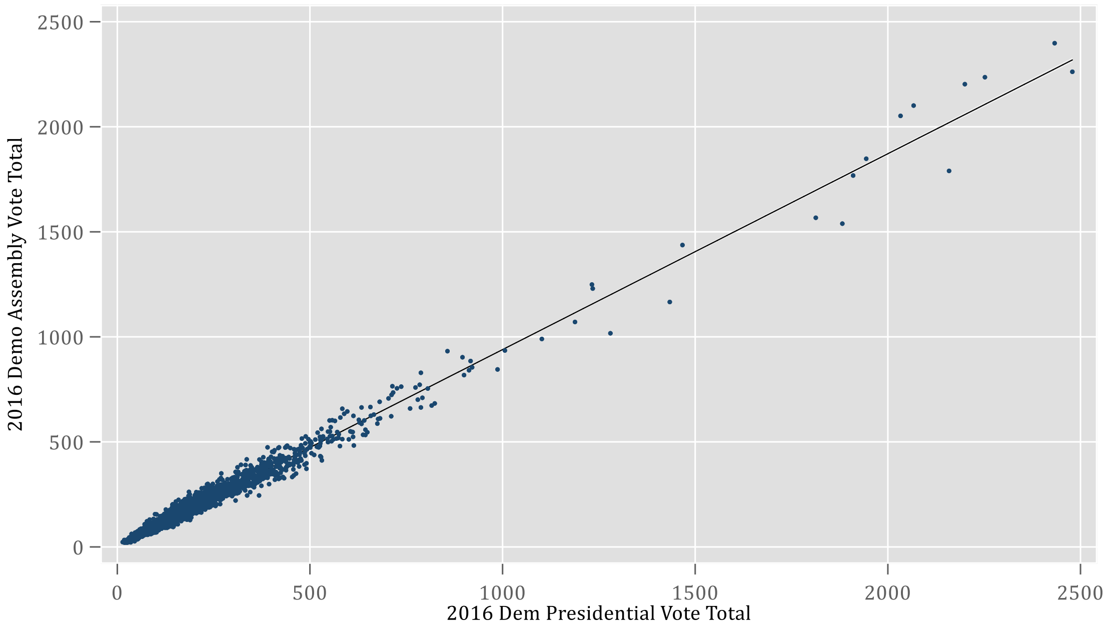
When examined across the state, ward-level votes for president correspond very closely to ward-level votes for the Assembly. In figures 1 and 2 I plot the number of Democratic and Republican votes for Assembly candidates in 2016 by the number of Democratic and

---

[1] Gaines refers only to "a ward in Germantown, Juneau County" where "Donald Trump won nearly 66% of the vote while the Republican Senate candidate Ron Johnson took about 55%" (Rpt. at p. 15). Germantown Ward 2, with a voting age population of 555, is the only ward that comes close to these figures: In 2016, Trump won 65.8% of the vote while Johnson won 57.1%.

Republican votes cast for president in contested districts, using data published by the Wisconsin Legislative Technology Services Bureau ("LTSB").

Figure 1
2016 Dem Presidential and Assembly Vote
Contested Wards

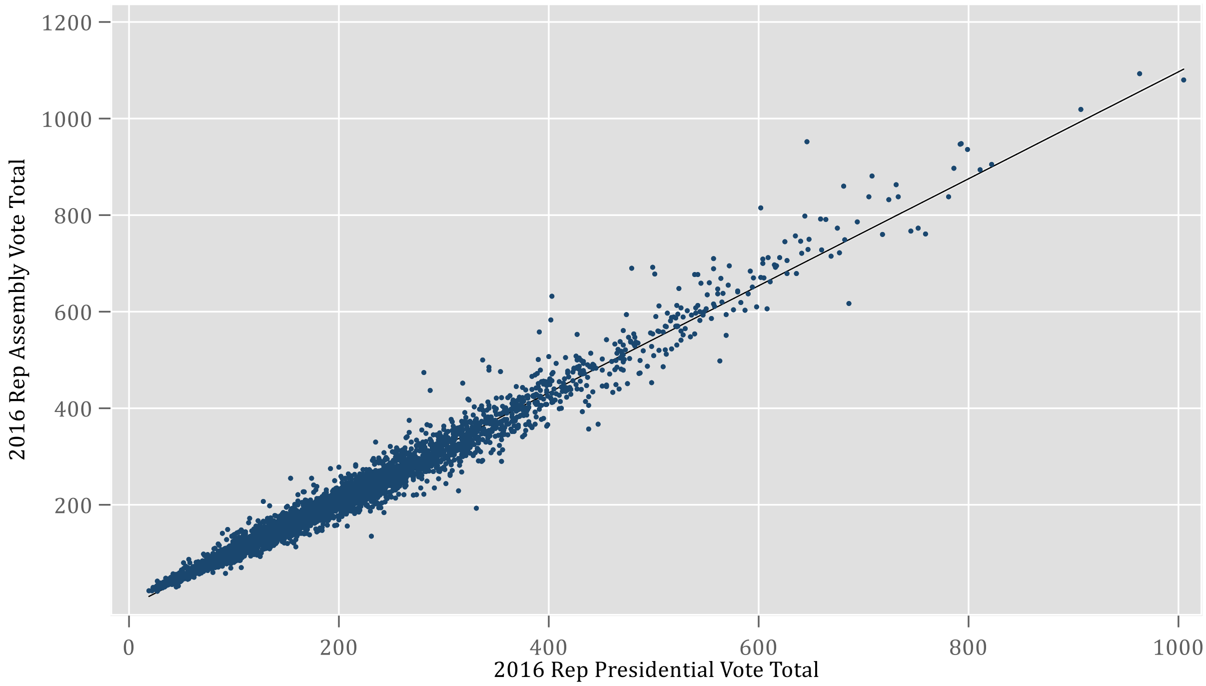


Here, each point represents the raw ward-level votes cast for Democrats in the 2016 presidential and contested Assembly races.[2] The line is the fitted bivariate regression line. The relationship between the Democratic presidential and Democratic Assembly candidates is obvious, and is so strong that the confidence interval drawn around the regression line is almost invisible. The bivariate regression r^2 (weighted by the number of Democratic presidential votes cast in each ward) is 0.99, indicating a near-perfect relationship (the maximum r^2 possible is 1).

The same pattern occurs with Republican vote totals:

[2] Because of the way that the LTSB disaggregates votes from reporting units to wards, there are a small number of wards where both Democratic and Republican Assembly votes were erroneously recorded in uncontested districts. I have removed these from the graphs.

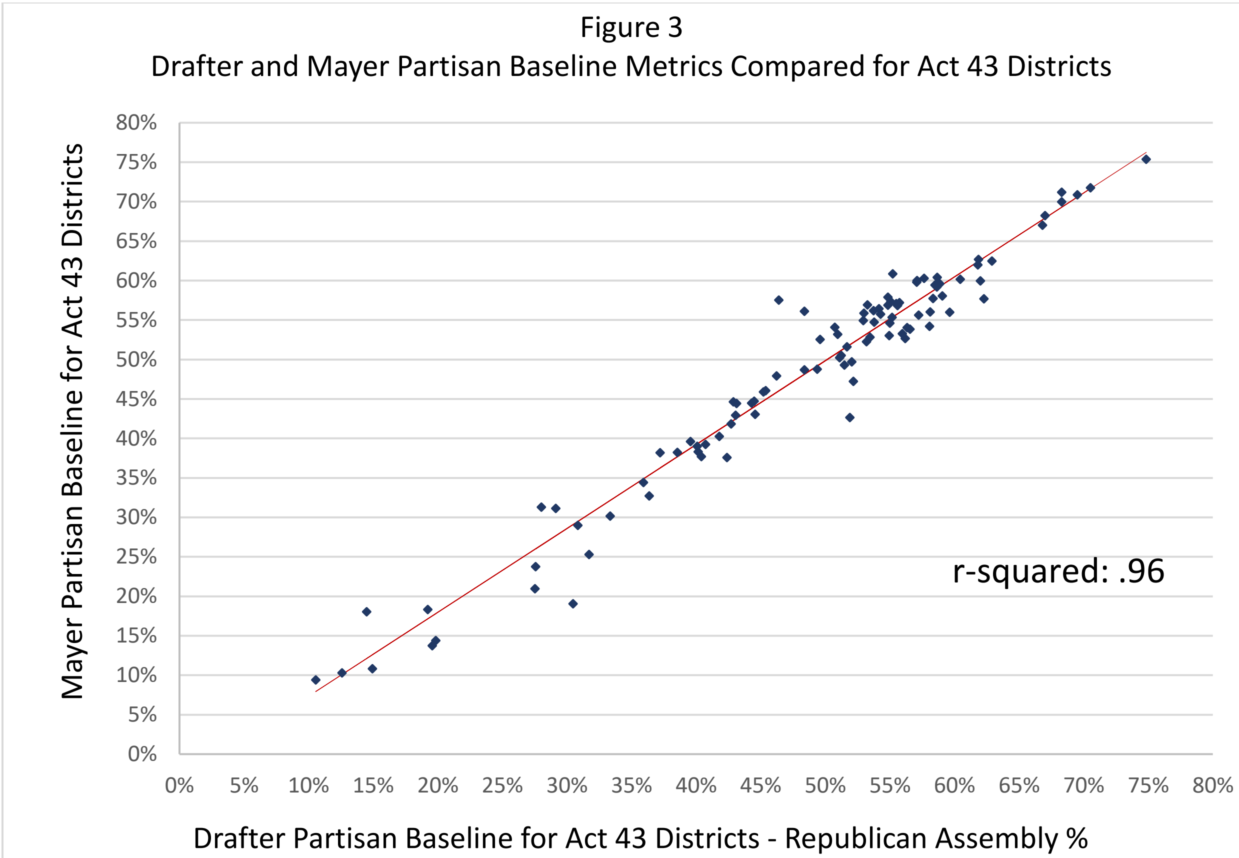
Figure 2
2016 Rep Presidential and Assembly Vote
Contested Wards



Here as well, the confidence interval around the weighted bivariate regression line is imperceptible, reflecting an r^2 of 0.97. Again, this indicates an extremely strong relationship.

There is variation around the regression line as one would expect. Partisanship is not the *only* district-level factor that affects voting behavior in state legislative elections. But it is emphatically the case that partisanship is by a wide margin *the most important* determinant of legislative voting behavior in Assembly elections. Gaines' argument, which is that measures of baseline partisanship tell us little about Assembly vote patterns is, to put it most starkly, wrong.

Baseline partisanship is robust under several different methods that can be used to calculate it. In my original 2015 expert report, I described a model that forecast raw ward-level Assembly vote totals using the presidential vote, demographic factors, incumbency, and geographic fixed effects. I used these results to estimate baseline open-seat measures for Act 43 and alternative district plans. My estimates matched, nearly perfectly, the baseline partisanship measure used by the Act 43 map drawers to evaluate different plans. This measure was based on aggregating a set of statewide election results from 2004-2010. As the figure below shows (figure 7 in my original report), the two measures line up almost exactly. As the Court explicitly found in its opinion, "The drafters' 'partisanship proxy' . . . correlated almost 'identically' with the 'open-seat baseline model' that Professor Mayer developed by way of a regression analysis." (Op. at p. 10 n. 31)



B. Use of Baseline Data in Drawing Act 43

Gaines also ignores the fact that the Act 43 map-drawers relied on precisely this information when drawing the Assembly maps. After confirming that the aggregate 2004-2010 index (the "composite score") was nearly perfectly correlated with a measure that the Legislature's consultant, Dr. Keith Gaddie, constructed using a regression model very similar to mine, the drafters used the composite score to evaluate potential maps. As the Court found:

> The drafters used their composite score to evaluate the statewide maps that they had drawn based on the level of partisan advantage that they provided to Republicans. In many instances, the names of the maps reflected the level of partisan advantage achieved by the districting plan; for instance, there are maps labeled "Assertive" and "Aggressive." [Drafter Adam] Foltz testified that "aggressive" in this context meant "probably that [the map] was a more aggressive map with regard to GOP leaning." (Op. at pp. 10-11)

C. Partisan Stability of Assembly Vote From 2012-2016

I next consider whether vote patterns were stable between elections in Assembly races. 2012 and 2016 presented different electoral environments: In 2012, the Democratic statewide share of the two-party presidential vote was 53.5%; in 2016, it was 49.6%, with statewide turnout down by 8.6%. Moreover, directly comparing raw vote totals will include variance that results from ward-level population changes in the intervening four years (as well as annexations and incorporations that reconfigured existing wards into new municipalities). Still, Democratic and Republican vote totals in Assembly elections remained strongly correlated, as the following figures show:

Figure 4
Democratic Assembly Vote 2012 and 2016

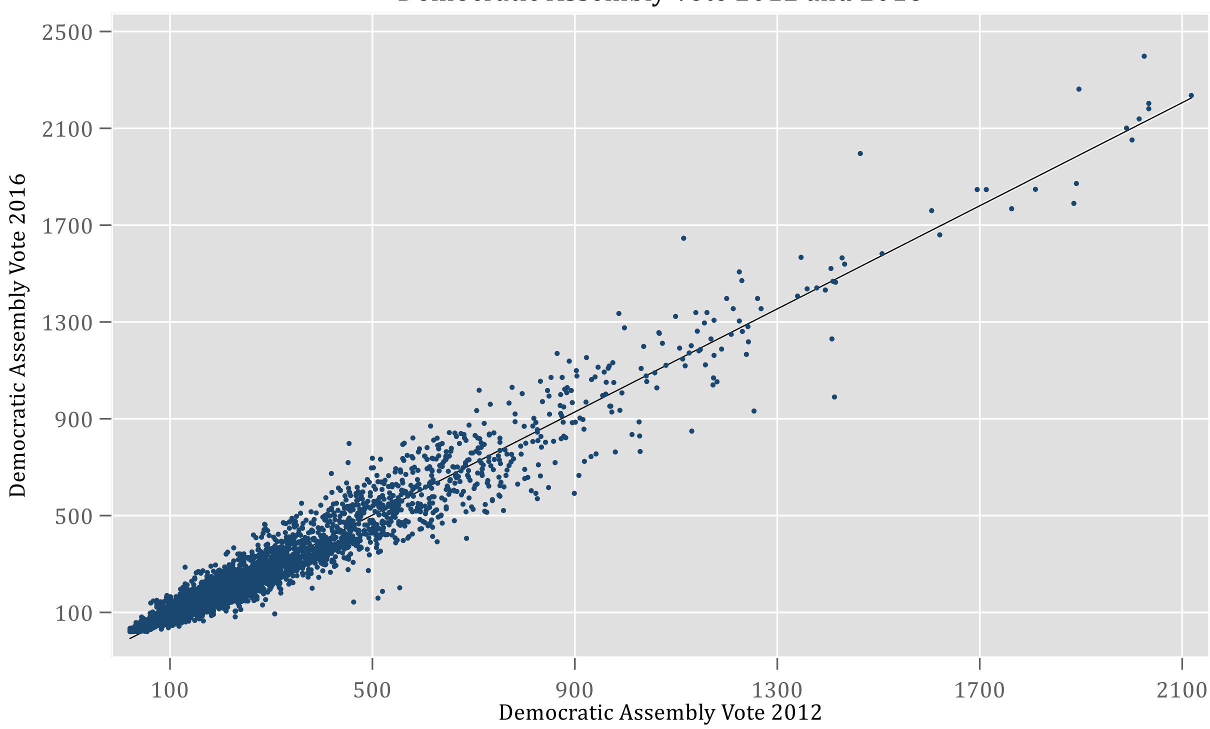
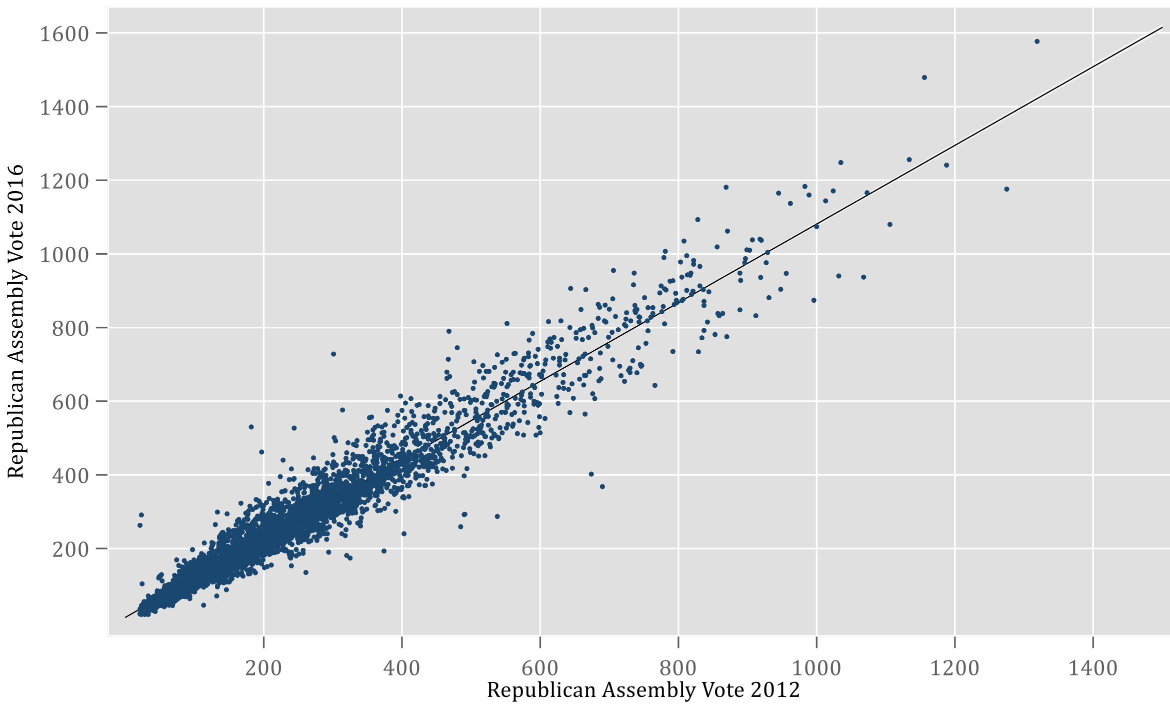


Figure 5
Republican Assembly Vote 2012 and 2016



Each figure plots the 2016 ward-level vote for Democratic and Republican candidates for the Assembly against the 2012 ward-level vote where candidates of that party ran in both 2012 and 2016 (this is a harder test than contested districts alone, as it includes vote totals where candidates ran uncontested in either 2012 or 2016). The black line in each graph is the bivariate regression line of the 2016 vote on the 2012 vote. There is some variance around the regression line, although many of the larger outliers (particularly those below the regression line) are the result of changes to ward boundaries, municipal annexations or incorporations, or population changes rather than substantive changes in voting behavior.[3] However, the relationship between the 2012 and 2016 votes is extremely strong for both parties: for Democrats, the r^2 for the bivariate regression is 0.96, and for Republicans, 0.94.

[3] Some examples: the Town of Menasha in Winnebago County incorporated as the Village of Fox Crossing in 2015-2016 (http://mds.wi.gov/View/Incorporations). This both created wards in 2016 that did not exist in 2012 and eliminated wards that did. The LTSB estimated the 2012 vote in the new Village of Fox Crossing wards, but the disaggregation and reaggregation process resulted in systematic reallocations and differences. See https://data-ltsb.opendata.arcgis.com/datasets/62d5782482cd45f2898fe7e3d4272c10_0?geometry=-104.087%2C42.041%2C-71.26%2C47.494. This incorporation also appears to have altered ward boundaries in what remained of the Town of Menasha: Both State Elections Commission and LTSB data show 3,624 votes cast in Town of Menasha wards in 2016, though these wards had a combined 2010 Census population of 6 and did not report any votes in 2012.

Whatever was the case in Florida in 2000, in recent elections Wisconsin shows very high levels of partisan consistency in voting across office and across elections.

D. Gaines Has Relied on the "Normal Vote" in His Published Work

In claiming that underlying partisanship is difficult to estimate and that the "normal vote" is not a useful measure, Gaines contradicts his own published work, where he has relied on the baseline vote method to analyze redistricting outcomes. In a 2011 report on redistricting in Illinois, Gaines used gubernatorial election results to estimate the underlying partisanship of state legislative districts and to compare the partisan consequences of the 1982 and 1992 Illinois redistricting rounds (Gaines 2011); Gaines referred to the gubernatorial vote as a "[measure]of Republican-ness" in legislative districts (2011, 6).

In 2013, Gaines and his co-authors evaluated a legislative redistricting plan by noting, "[f]or each district, one can estimate the expected outcome in a 'normal' election, that is, an open-seat race where there is no incumbency advantage at play in a year without a strong partisan tide favoring either side" (Gaines, Kuklinski and Mooney 2013, 73-74). In that case, Gaines and his co-authors relied on the presidential vote in 2004 and 2008 to evaluate baseline partisanship. "The key feature," they pointed out, "is that we are using the same data to gauge the districts from the old and new maps, and so focus strictly on how clumps of partisan voters were re-grouped to alter the partisan composition of the districts" (2013, 74). This is a direct application of the normal or baseline vote method.

In addition, Gaines has argued that "as important as familiarity is in determining a person's vote for his/her lawmaker, *political party matters much more*" (Gaines, Kuklinski, and Mooney 2013, 74, emphasis added), thus contradicting his central claim here (Gaines Rpt. at pp. 17-22) that underlying partisanship is not useful in understanding likely outcomes.

In short, Gaines' criticism of baseline partisanship as a metric for evaluating the partisan performance of actual and proposed legislative districts and plans ignores conclusive data of increasing partisan stability across offices and elections; fails to consider overwhelming evidence that in Wisconsin there is an extremely strong relationship between baseline partisanship and voting in State Assembly elections; overlooks a well-established and accepted political science literature relying on baseline measures as a valid method of understanding elections and redistricting; contradicts his own work where he has used statewide elections as a baseline to assess the consequences of redistricting; and perhaps most telling, contradicts the exact method used by the Assembly's own consultant, Dr. Gaddie, in assisting the legislative aides in drafting the Act 43 districts in 2011.

III. Trende

The bulk of Mr. Trende's critique of my report falls into two categories. First, as a general matter, Trende attacks my use of observational data to draw causal inferences. And second, Trende argues that there is no relationship between Act 43 and measures of fundraising, candidate recruitment, and policy liberalism.

"Estimating the partisan effects of Act 43 is an enormously difficult task," Trende states. "It is difficult," he continues, "to know how this law affected fundraising, recruitment, and the ideological direction of the legislature, to say nothing of the effect on Democrats' chances in individual districts" (Trende Rpt. at Para. 98).

Trende's conclusion is uninformed and incorrect. Trende, like Gaines, ignores the existence of an entire and well-accepted political science literature on redistricting and gerrymandering, spanning decades, which is *premised* on using observational data to draw inferences about the partisan effects of redistricting plans and to forecast the probabilities of a party winning in individual districts.

In his response to my report, Trende does not cite one piece of research or theoretical work in support of any of his conclusions. His conclusions are offered only as his own impressions and opinions.

<u>A. Causal Inference Using Observational Data</u>

Trende argues that "it is *uniquely difficult* to draw causal inferences on the basis of observational data, as opposed to experimental data, where researchers can randomize observations and impose some sort of control on what is observed" (Trende Rpt. at Para. 29, emphasis added). This overstates the case considerably. Trende does not offer any specific argument in support of this proposition, nor any citations to the academic literature on research design or inference.

There is nothing magic about experimental data, and nothing inherently flawed about using observational data, as the basis for causal inference, as *all* social science causal inference is probabilistic and depends on the rigor of the underlying theory. Confounders, exogenous factors, and unobserved variables are ubiquitous features of all empirical social science research. As King, Keohane, and Verba put it in their classic work *Designing Social Inquiry*, "no matter how perfect the research design, no matter how much data we collect, no matter how perceptive the observers, no matter how diligent the research assistants, and no matter how much experimental control we will have, we will never know a causal inference for certain" (1994, 79). Indeed, a central point of this influential book is that there is no difference between quantitative inference - the kind based on extensive numerical data - and qualitative inference - the kind based on description (King, Keohane and Verba 1994, 5-6).

The standard Trende proposes would render virtually all redistricting-related inference impossible, since true randomization and controls are rarely practicable.[4] Even quasi-experimental designs, such as matching, will usually be infeasible, since there will rarely be enough cases or enough similarities between states to permit the type of study that Trende apparently prefers. No matter how many factors we attempt to control for, "it is impossible to entirely preclude the possibility that there exist unobserved variables that confound the

[4] An exception would be tests of certain kinds of election-related information, such as treatment-control experiments of different messages and information on turnout. See Gerber, Green and Larimer (2008) as an example.

relationships even after conditioning on many observed covariates" (Imai, Keele, Tingley, and Yamamoto 2011, 771).

When faced with the lack of true or even quasi-experimental designs with redistricting plans, and the fact that any kind of social science inference will be probabilistic, there are two options. One is adopting the nihilist position that there is little point in even trying to understand such plans, a clearly unreasonable stance that would negate decades of highly regarded and peer-reviewed published scholarship analyzing redistricting plans, and ignore centuries of effort devoted to enacting them.

The other is careful attention to theory and appropriate care in making empirical claims, while not shying away from the task of studying and attempting to comprehend crucial empirical patterns. Predictive reliability, consistency with theoretical expectations, and careful identification of the quantities of interest can produce reliable and replicable inferences, even if they do not meet the impossible condition of absolute certainty. Trende appears to believe as long as it is *possible* that something other than Act 43 produced the effects I observed in my report, I cannot make any claim that Act 43 was a causal factor. That is the wrong standard, as it is *always* possible that some other causal factor was in play, and no amount of data or rigor could eliminate this possibility. The important features of my argument are that: (a) Act 43 was intentionally designed to create maximum partisan advantage for Assembly Republicans, through careful and methodical packing and cracking of Democratic voters in a way that insulated Republican candidates from statewide swings in the vote and more efficiently translated Republican votes into seats, thereby placing the Democratic Party, and Democratic voters, at a disadvantage (this is the very point of partisan gerrymandering);[5] and that (b) Act 43 produced individual and aggregate effects that are entirely consistent with that objective, and that also match predictions of a large and reliable empirical literature on redistricting, political parties, and elections.

Trende hedges throughout his report, repeatedly asserting without evidence that the patterns are "only correlation" (Trende Rpt. at Para. 28) or "at best a correlation" (*id.* at Para. 29), that "it could be" that other factors explain observed fundraising patterns (*id*. at Para. 30), that "it is by no means obvious that [Act 43] is the most likely" factor in explaining the increase in uncontested Republican seats (*id.* at Para. 38), and that my results "should be approached with caution" (*id.* at Para. 98).

---

[5] Despite what Trende claims, there are some inferential elements that are, for all practical purposes, known with certainty. One is that Act 43 was enacted by the Wisconsin legislature. The other is that Act 43 was *intended* to extract maximum partisan advantage for Republicans. The former is an obvious fact from the observable public record. The latter is a reliable inference based on what is observably known about the data the individuals who drew the Act 43 map relied on, and on what these individuals said about what they were doing. As the Court explicitly found, "the defendants intended and accomplished an entrenchment of the Republican Party likely to endure for the entire decennial period." (Op. at pp. 54-55)

In the end, Trende asserts that "[e]lections are enormously complicated events, which political scientists frequently struggle to unravel." (*Id.* at Para. 98) This is not quite true. In fact, elections provide a wealth of observational data that social scientists have used to explain voting patterns, forecast results, and understand the causes and consequences of electoral outcomes. I cite specific examples from the peer-reviewed political science literature in this rebuttal report; a complete search of the entire academic universe would yield thousands of additional examples.

In making the claims in my report, I am not insisting that Act 43 is the *only* factor that explains the data, nor am I making a claim about the precise size of the effect. Instead, I am pointing out numerous aggregate measures that are consistent with a broad and generally accepted peer-reviewed, published academic literature and the conclusions offered by scholars who have studied redistricting plans and the nature of their partisan effects. Trende is incorrect in arguing that these relationships are nothing more than correlation (something that is an oversimplified and overused excuse for not making causal claims in any event).[6]

A more accurate way to phrase Trende's objection is that correlation does not necessarily imply causation. That is a correct and oft-repeated statistical truism: correlation does not *necessarily* imply causation. However, when there are valid theoretical reasons to connect cause and effect, the criticism loses much of its power. Examples of obviously spurious correlations abound (*see* Tufte 1974); the researcher's task is to assess the relationships according to underlying theory and to draw conclusions based on careful evaluation of appropriate data. One need not be a quantitative social scientist to know this; Henry David Thoreau observed more than 150 years ago that "some circumstantial evidence is very strong, as when you find a trout in the milk."[7]

B. Fundraising Disadvantages

Trende dismisses the fundraising data I use in my report, insisting that the data "show little correlation between fundraising and the implementation of Act 43" (Trende Rpt. at p. 6). While he concedes that there is a relationship between the gap in Assembly campaign committee fundraising and Act 43, he claims that it is "at best, a correlation" and that "Republican fundraising, relative to the Democrats, was about the same in 2012 as it was in 2006, 2000, and 1998." (Trende Rpt. at Para. 28)

---

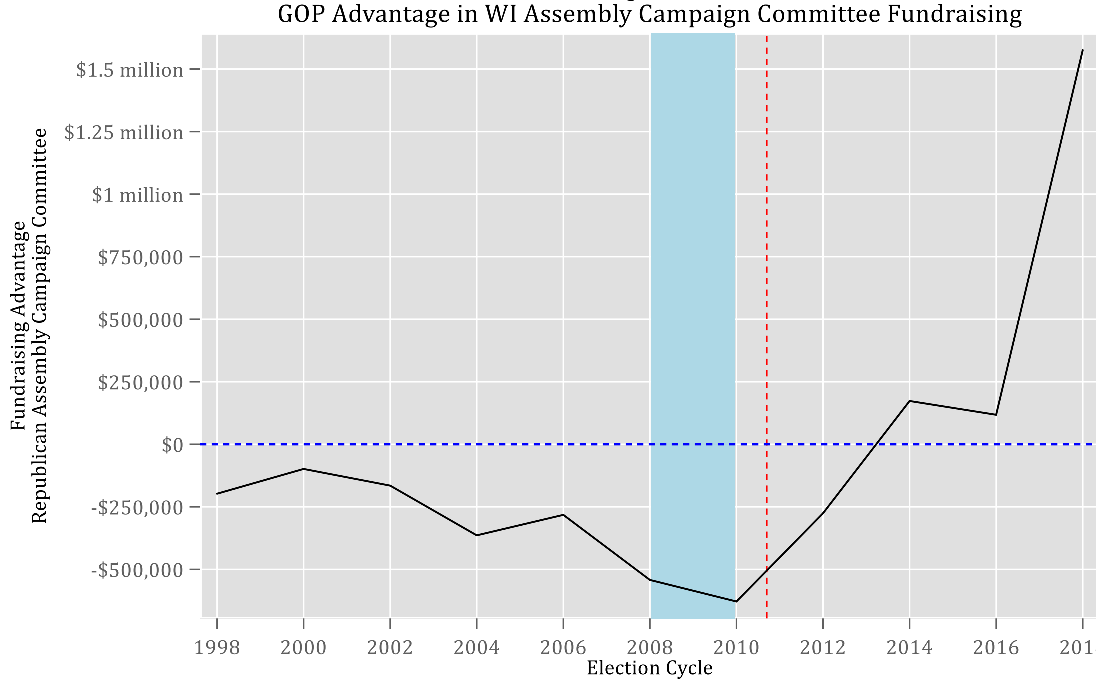
[6] "Many social scientists," write King, Keohane and Verba, critically, "are uncomfortable with causal inference. They are so wary of the warning that 'correlation is not causation' that they will not state causal hypotheses or draw causal inferences" (1994, 75). I note that the claims "correlation is not causation," and "causal inference is invalid without experimental evidence" were both made by apologists for the tobacco industry in the 1950s to dispute evidence that cigarette smoking causes cancer (*see* Cornfield et al. 1959). This study is so influential that it has been reprinted multiple times (*see* Tufte 1970, and Cornfield et al. 2009).

[7] Henry David Thoreau, *Journal II*, November 11, 1850, 94.

This is incorrect and misreads the data. Consider Figure 6, a visual representation of the data in Trende's Table 1, showing fundraising by the Assembly Democratic Campaign Committee and the Republican Assembly Campaign Committee. Here, the net Republican fundraising advantage is plotted by election cycle. Values below 0 indicate a net Democratic advantage, while values above 0 indicate a net Republican advantage. The dotted red line is the approximate date of the enactment of Act 43 (August 2011). The shaded blue region (2008-2010) indicates Democratic control of the Assembly.

The pattern is stark: through the 2012 election cycle, Democrats retained a fundraising advantage every cycle, *even when they were in the minority*. The greatest Democratic advantage (-$628,000) occurred in 2010 when they controlled the Assembly. Upon enactment of Act 43, the Democratic advantage shrank, shifting to a net Republican advantage in 2014. By 2018, the GOP advantage was nearly $1.6 million. Contrary to Trende's claim that "Republican fundraising improves in 2014, not 2012 as we might expect with a redistricting-related effect" (Trende Rpt. at Para. 28), the fundraising gap moves decidedly in the GOP direction between 2010 and 2012, as Republican fundraising went up in that period (from $295,506 in the 2012 cycle to $349,250 in 2014) while Democratic fundraising went down (from $922,854 in 2010 to $624,852 in 2012).

Figure 6
GOP Advantage in WI Assembly Campaign Committee Fundraising



Is it possible to draw an inference from this data that Act 43 is a cause of the observed change? It is unlikely that contributions to the Republican Assembly Campaign Committee after 2012 came with a letter attached saying "Because of Act 43." But it was not the loss (or gain) of majority status in 2010 that accounted for the dramatic post-2010 shift, as the Assembly Democratic Campaign Committee raised more money than its Republican counterpart from 1998-2012. It was not the immediate effect of the vote swing in a single election, as based on votes for statewide office Democrats performed better electorally in 2016 and 2018 than they did in 2014 or 2010 but faced dramatically worse fundraising competitiveness. And over the 1998-2018 span, the gap does not depend on whether Assembly elections took place in a presidential or gubernatorial cycle, or whether the Governor was a Democrat or Republican.

C. Nationwide Data on Fundraising, Recruitment, and Competitiveness

Trende, similarly, dismisses data on Assembly candidate fundraising and recruitment, concluding that there is no correlation between levels or gaps before and after Act 43.

It is possible to bring additional data to this analysis. I obtained from Dr. Christopher Warshaw (George Washington University) data on lower-house state-level redistricting in 41 states in election cycles from 1972-2016. The data, based largely on work by Dr. Simon Jackman for this litigation,[8] includes extensive information about lower-house candidates and elections. I use this data in an analysis of the overall effects of redistricting plans, using the efficiency gap as a key independent variable, and different measures of aggregate effects as dependent variables. The benefit of this approach is that it establishes a more general result that is not specific to a single state or year.

As the data are organized by state and year, they constitute a cross-sectional time series. I apply a fixed-effects model using state and year variables in addition to the substantive independent variables of interest; *see* Greene (2012, 359-364). The state and year variables control for state-specific and cycle-specific factors generally, and will isolate the effect of efficiency gap values on aggregate party outcomes beyond actual election results.

The outcome variables I use as dependent variables in this analysis are the following:

- The percentage of campaign contributions received by Democratic candidates[9]
- The percentage of lower house seats uncontested by Democratic candidates
- The percentage of Democratic incumbents who return to office[10]

The key independent variable is the efficiency gap calculation for each state and year in lower house elections, using the method described in Jackman (2015). Recall that by convention, negative efficiency gap values indicate a pro-Republican bias in a district plan, while positive values indicate a pro-Democratic bias. The theoretical expectations for each outcome variable are as follows:

---

[8] The data covers 41 states, excluding those with nonpartisan elections, extremely high rates of uncontested elections, multimember districts, or runoff rules (Jackman 2015, 20).
[9] Data from the National Institute for Money in State Politics, beginning in 1998.
[10] This measures both incumbents who retire and those who are defeated in the election.

Table 1 Expected Relationship Between Efficiency Gap and Aggregate Outcomes		
Outcome Variable	Expected Effect of Efficiency Gap (expected sign of coefficient)	Explanation
Share of Campaign Contributions Received by Democratic Candidates	Positive	Large negative (pro-Republican) efficiency gap scores diminish Democratic candidate fundraising capacity
Percentage of Seats Uncontested by Democratic Candidates	Negative	Large negative (pro-Republican) efficiency gap scores diminish ability of Democratic candidates to meaningfully compete for seats, and reduce party capacity to recruit quality candidates
Percentage of Democratic Incumbents who Return	Positive	Large negative (pro-Republican) efficiency gap scores reduce value of office for Democratic incumbents, increase incentives to retire, and weaken party

As a robustness check, in addition to the full set of states and years, I estimate the model only for redistricting plans enacted when one party is in full control of the state government. This captures an intent factor, and only includes plans where a party had the ability to enact a partisan gerrymander.

The results are shown in tables 2 and 3.

Over the entire period, the results confirm theoretical expectations that negative efficiency gap scores impose aggregate harms on Democratic candidates and incumbents (which, again, is the very point of a partisan gerrymander). Negative efficiency gaps (indicating a pro-Republican bias) reduce the Democratic share of campaign contributions, increase the percentage of seats uncontested by Democrats (the negative coefficient multiplied by a negative efficiency gap produces a positive change), and reduce the percentage of Democratic incumbents who return to office. All of these coefficients are statistically significant. What the results show is that a change of -0.1 in the efficiency gap reduces the Democratic share of contributions by 7.1%, increases the percentage of Democratic uncontested seats by 8.1%, and reduces the percentage of Democratic incumbents who return to office by 3.5%. To put this in scale, the Jackman data calculate the efficiency gap for Wisconsin as -.13 in 2012.

Table 2
Effect of Efficiency Gap on Aggregate Outcomes
All Cycles and States 1972-2016

	Dependent Variable					
Independent Variable	Democratic % of Campaign Contributions (1998-2016)		% of Seats Uncontested by Democrats		% of Democratic Incumbents who Return	
Current Efficiency Gap	0.71^{***}		-0.81^{***}		0.35^{***}	
se	(0.19)		(.06)		(0.08)	
n	351	351	820	779	818	779
*** p < 0.001, * p< 0.1						

It is important to emphasize the fact that although the efficiency gap is calculated using current election results, those results occur *after* decisions about making campaign contributions, contesting a district, and strategic retirements have been made. These decisions will be conditioned on expectations of how a party will perform, but will predate actual election results.

Table 3 shows the results for elections held under redistricting plans enacted by one party through unified control of a state government. A partisan gerrymander is more likely in these conditions (unified control is generally considered a necessary condition for a partisan gerrymander to have occurred). We would therefore expect the impact of large efficiency gaps to be greater in this model.

Table 3
Effect of Efficiency Gap on Aggregate Outcomes
Redistricting Plans Enacted by Unified Party Control, 1972-2016

	Dependent Variable					
Independent Variable	Democratic % of Campaign Contributions (1998-2016)		% of Seats Uncontested by Democrats		% of Democratic Incumbents who Return	
Current Efficiency Gap	1.10^{***}		-1.02^{***}		0.18^{*}	
se	(0.34)		(.08)		(0.11)	
n	151	351	372	352	371	352
*** p < 0.001, * p< 0.1						

Table 3 confirms as much. The effect of large negative efficiency gaps is greater under unified control. The effect of a -0.13 efficiency gap (as occurred in Wisconsin in 2012) is associated with a 14.3% decrease in the Democratic share of campaign contributions, a 13.3% increase in the share of seats uncontested by Democrats, compared to a neutral plan, and a 2.3% decrease in the return rate for Democratic incumbents, compared to a neutral plan.

It is possible to calculate how the estimates in table 3 would, in expectation, change Democratic fundraising, levels of uncontested races, and incumbent return in a state that moved from a nearly neutral map (efficiency gap less than ± 0.01) to an efficiency gap of -.13 (the value for Wisconsin in 2012).

Table 4
Comparison of Neutral Plan to Map With Efficiency Gap = -0.13

	Democratic % of Campaign Contributions	% of Seats Uncontested by Democrats	% of Returning Democratic Incumbents
Actual Median Value When |Efficiency Gap| < 0.01	56%	11%	77%
Expected Value When Efficiency Gap = -0.13	46.8%	21.5%	72.4%
Δ	-9.2%	10.5%	-4.6%

Table 4 shows the impact a map like Act 43 would have on Democratic competitiveness and electoral success. Compared to a neutral map, a map with an efficiency gap of -0.13 would reduce the Democratic share of campaign contributions by over 9%, would nearly double the share of seats Democrats would not contest, and would reduce the share of returning incumbents by nearly 5%.

This broader dataset confirms that biased redistricting plans like Act 43 impose an observable and significant harm on the electoral fortunes and competitive capacities of the disadvantaged political parties.

D. Policy Changes

In paragraphs 44-48, Trende attempts to minimize the observed shift in policy liberalism in Wisconsin (the Caughey-Warshaw index) by claiming that the shift was both expected, given Republican control of the legislature, and insignificant because Act 43 "has not transformed Wisconsin into Mississippi, South Carolina, or Georgia." (Trende Rpt. at Para. 48) This both misreads the meaning of the shift and obfuscates its importance.

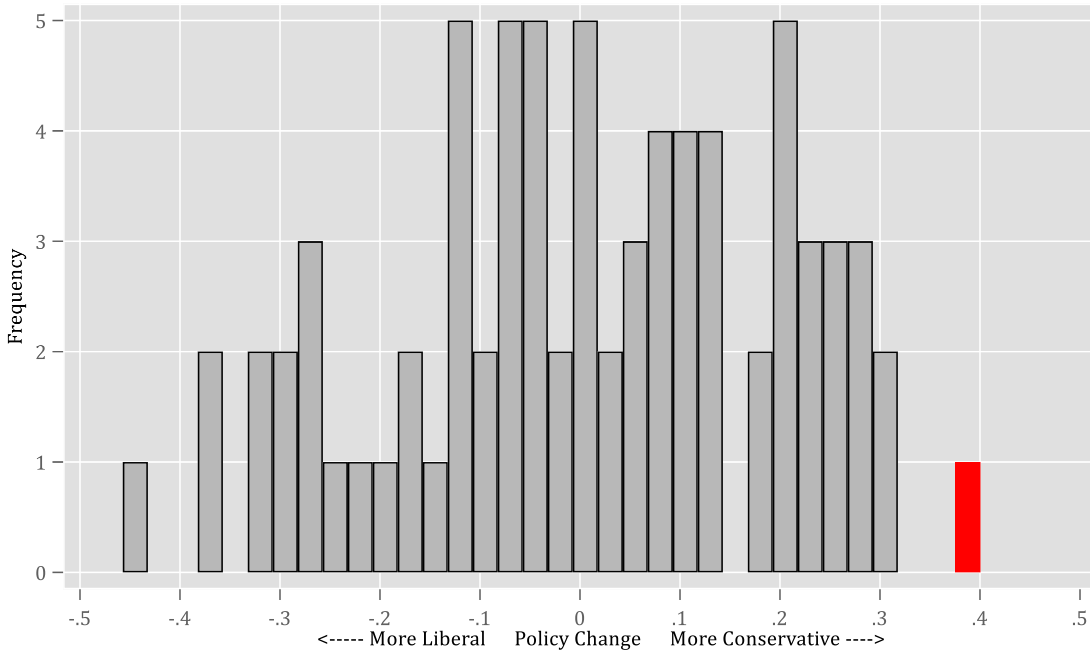
As I noted in my report, there is a significant difference between a legislative majority that results from a shift in voter preferences, and one obtained by intentionally denying the other party seats through a district plan that packs and cracks voters in a way that reduces its ability to translate votes into seats and puts candidates, incumbents, and voters at a competitive disadvantage:

> Reducing by design the number of seats a political party wins will have consequences for that party's ability to compete for office and influence legislative outcomes. The reduced ability to compete results from fewer incumbents, more difficulty in recruiting quality challengers to run against incumbents in the other party, less ability to attract campaign contributions, and fewer subsequent opportunities to win seats. The reduced influence over legislative outcomes is the result of holding fewer seats, and of being stuck in a legislative minority position. These two forces – reduced competitiveness and reduced influence – reinforce each other in a regressive feedback loop: less ability to compete leads to less legislative influence, which in turns leads to less competitiveness, and so on. It is one thing, of course, when this cycle is the result of a lack of public support (or votes). It is another when this cycle is the result of intentional action by a political party (e.g., drawing a legislative map) that renders the other party *unable* – because its supporters have been cracked and packed – to translate public support and votes into seats. (Mayer Rpt. at pp.4-5)

Trende dismisses the post-Act 43 shift in the policy liberalism index, claiming that the 2010-2012 change is "roughly the same" as the shift from 2012-2013, that compared to other states Wisconsin is "still a relatively liberal state," and that the 2014 measure (0.668) is about the same as it was in 2006, 2007, and 2008. (Trende Rpt. at paras. 45-47)

This misses the point, which is not merely that a Republican legislative majority enacted conservative policies. Rather, it is that Act 43 locked in this majority by foreclosing the ability of Democratic candidates, officeholders, and voters to have a meaningful opportunity to compete for political power. And, as well, that the policy shift was larger than had ever occurred before: Trende cannot dispute the fact that the four-year change in the policy index from 2010-2014 is the largest conservative shift observed in the entire index (which began in 1935), as shown in Figure 7, a histogram of all 4-year policy shifts between 1939 and 2014:

Figure 7
4-Year Change in State Policy Liberalism 1939-2014



Here, the red bar at the right of the histogram is the 2010-2014 change in policy liberalism. It is clearly an outlier.

Trende, finally, claims that the standard deviations of the Caughey-Warshaw index mean that "it is entirely unclear. . . that Wisconsin has been transformed at all." (Trende Rpt. at Para. 48) This is a mistaken interpretation of what the standard deviation means in this case. The policy liberalism index is not a sample of a broader set of data, where the standard deviation is a measure of dispersion allowing for the calculation of confidence intervals. Instead, the standard deviation reflects uncertainty in how the underlying quantities reflect an unobserved variable. Using the methodology described in Levendusky, Pope, and Jackman (2008, 741), the probability is 0.838 that the 2014 policy liberalism index is more conservative than the 2010 index, matching the unanimous view of Wisconsin politics over the past decade.

This, too, is consistent with the peer-reviewed, published academic literature. Caughey, Tausanovitch and Warshaw find that (1) *no matter what the margin of victory is*, switching party control in a single legislative district significantly changes the ideology of the member representing that district (2017, 458). In this regard, there is little difference between the ideology of a legislator winning by a margin of more than 10 points and a legislator from a highly competitive seat winning by a margin of less than 1 point; (2) *even after controlling for legislative majorities, statewide voting behavior, and the party of the governor*, the efficiency gap by itself has a significant effect on the median legislator's ideology (2017, 463-4). That is, larger efficiency gaps produce larger changes in the aggregate ideology of a legislative majority; and (3) *even after controlling for legislative majorities, statewide voting behavior, and the party of the governor*, the efficiency gap has a significant effect on state policy conservatism, with larger efficiency gaps producing larger shifts in policy (2017, 464-5).

In short, the degree of gerrymandering – the size of the efficiency gap, and the way that gerrymanders insulate advantaged legislative majorities from electoral consequences – by itself has a significant effect on legislative majorities, legislator ideology, and policy, and locks in those consequences:

> [E]fficiency gaps can deny the majority of voters the opportunity to reverse past policies that they dislike or to enact large policy changes themselves. In short, efficiency gaps can degrade the disfavored party's influence on the political process, both in the short term and over the longer term as well. (Caughey, Tausanovitch and Warshaw 2017, 468)

In the 2018 election, Wisconsin Democrats won majorities in all five statewide races held: Governor, Attorney General, Treasurer, Secretary of State, and U.S. Senate, defeating Republican incumbents in the Governor and Attorney General races. Yet despite winning consistent majorities of the statewide vote, the partisan makeup of the State Assembly barely changed, with Democrats winning only one additional seat and Republicans maintaining a 63-36 majority.

In response to an incoming Democratic Governor and Attorney General, Republican majorities in the Wisconsin legislature convened in extraordinary session after the election, and enacted three bills (SB 883, SB 884, and SB 886) that curtailed powers of the now-Democratic Governor and Attorney General. Among the changes made:[11]

- Reducing the number of appointments the Governor makes to the Governing Board of the Wisconsin Economic Development Corporation, from 6 out of 12, to 6 out of 16. The Senate Majority Leader and Assembly Speaker (both Republicans) appoint four members each, up from 3 each under the previous law.

- Removing the Governor's authority to appoint the Chief Executive Officer of WEDC.

- Prohibiting the Department of Health Services from asking for waivers under federal programs or requesting authorization for pilot programs or demonstration projects unless a statute directs the agency to submit the request, and requiring prior legislative approval of statutorily directed requests.

- Limiting the rulemaking authority of state agencies.

[11] Wisconsin Legislative Council, Act Memo, *2017 Wisconsin Act 369: Various Changes to State Law*¸ December 17, 2018. Wisconsin Legislative Council, Act Memo, *2017 Wisconsin Act 370: Requests for and Implementation of Certain Federal Approvals, Modifications to Certain Public Assistance Programs, Allocation of TANF Funds, and Funding for the Fast Forward Program*, December 17, 2017.

- Eliminating the Attorney General's authority to settle civil actions without legislative approval.
- Eliminating the Attorney General's authority to propose plans for spending settlement funds, and requiring all such funds to be deposited in the state general fund.
- Eliminating the ability of the Attorney General to concede the unconstitutionality of a state statute without approval of the legislature.
- Allowing the legislature to retain private counsel at its own discretion to defend statutes in court, without the need to obtain approval of the Attorney General.
- Allowing the legislature to secure building space outside the Wisconsin State Capitol, without the need to obtain approval of the Department of Administration.
- Eliminating the Office of the Solicitor General in the Attorney General's Office.

Taken together, these actions corrode what Robert Dahl called the "democratic bargain:" the norm that election winners will not use their power to deny rights to the losers, and that election *losers* will transfer political power voluntarily and will not change the rules post-hoc to prevent the winners from exercising legitimate authority. (Schmitter and Karl 1991, 82)

These actions are also linked to Act 43, in that they allowed one party (in this case, Republicans who were swept in all five 2018 Wisconsin statewide elections) to insure control of the legislature and insulate itself against changes in voter preferences, locking in their policy and political powers even in the face of statewide electoral losses. Tellingly, two of the provisions — one allowing the legislature to retain private counsel instead of relying on legal representation by the Wisconsin Department of Justice, and another allowing the legislature to secure building space outside the Wisconsin State Capitol without approval of the Department of Administration — appear to allow the legislature to employ the same practices that it used in 2011 when it retained a private law firm to advise it in the creation of the maps used in Act 43, and used that law firm's private office space to conduct legislative work, restricting access to only a handful of Republican legislative aides, consultants, legislative leaders, and their private attorneys. This practice came under heavy criticism by the three-judge panel in the *Baldus* litigation over Act. 43. *Baldus v. GAB*, 843 F. Supp. 2d 955, 958-60 (E.D. Wis. 2012).

E. Trende's Evaluation of Assembly Districts in 2016

Trende argues that the 2016 presidential election results in several Assembly Districts demonstrate that elections are too unpredictable to forecast the long-term effects of gerrymanders. (Trende Rpt. At Para. 56) Trende claims that in several districts (10 districts in which Democratic voters were cracked, and 1 in which they were packed)[12], the presidential vote did not correspond closely to the 2016 Assembly vote, concluding that "[it] is difficult to see how a solid line between packing and cracking is maintained over the course of a redistricting map's lifespan." (Trende Rept. Para. 56)

The line is more solid and easier to see than Trende acknowledges. In the eleven districts he mentions, all of the 2016 and 2018 results are consistent with the baseline estimates. In the 10 districts with a baseline of less than 50%. Republicans won *all* of them in 2016 and 2018. In the single district Trende mentions with a baseline above 50%, the Democratic candidate won in 2016 and 2018. In every district, the result was what the baseline forecast nearly 8 years after the district lines were drawn.

In fact, contrary to Trende's assertion that gerrymanders are hard to forecast long term, the 2016 election (and 2018 as well) demonstrates that Act 43 operated entirely as its drafters intended, which was to insulate Republican candidates from shifting electoral tides and changing voter preferences. By cracking and packing Democratic voters, Act 43 created districts that produced reliable results through multiple election cycles, protecting Republican candidates and incumbents from statewide swings. This is precisely what a gerrymander is designed to do.

The 13th Assembly District captures this dynamic. The Chen baseline for this district was 59.7%. The Republican candidate won in 2012 with 60.5% of the vote in 2012, ran uncontested in both 2014 and 2016, and won again in 2018.[13]

These results strongly confirm Dr. Simon Jackman's conclusion in his 2015 expert report, that the effect of a large efficiency gap endures throughout the decade in which a gerrymandered map is in effect. (Jackman 2015, 60-62)

____

Kenneth R. Mayer
January 22, 2019

---

[12] The cracked districts are Districts 13, 22, 23, 24, 29, 50, 67, 70, 82 and 90; the packed district is District 95.

[13] Trende mischaracterizes what I wrote in describing my analysis of Assembly District 13 in Act 43. Trende claims that I described the district as "an overwhelmingly Republican area of Waukesha county." (Trende Rpt. at Para. 56) This is incorrect. What I wrote was that Act 43 cracked Democratic voters in Milwaukee County by combining them with "an overwhelmingly Republican area in Waukesha County." (Mayer Rpt. at p. 17)

Sources

Abramowitz, Alan I., and Steven Webster. 2015. "The Rise of Negative Partisanship and the Nationalization of U.S. Elections in the 21st Century." *Electoral Studies* 41:12-22.

Ansolabehere, Stephen and James M. Snyder. 2012. "The Effects of Redistricting on Incumbents." *Election Law Journal* 11:490-502 (No. 4).

Ansolabehere, Stephen and James M. Snyder, and Charles Stewart, III. 2000. "Old Voters, New Voters, and the Personal Vote: Using Redistricting to Measure the Incumbency Advantage. *American Journal of Political Science* 44:17-34.

Ansolabehere, Stephen and James M. Snyder, and Charles Stewart, III. 2001. "Candidate Positioning in U.S. House Elections." *American Journal of Political Science* 45:136-159.

Cain, Bruce E. 1985. "Assessing the Partisan Effects of Redistricting." *American Political Science Review* 71:320-333.

Canes-Wrone, Brandice, David W. Brady, and John F. Cogan. 2002. "Out of Step, Out of Office: Electoral Accountability and House Members' Voting." *American Political Science Review* 96:127-140.

Caughey, Devin, Chris Tausonovitch, and Christopher Warshaw. 2017. "Gerrymandering and the Political Process: Effects on Roll-Call Voting and State Policies." *Election Law Journal* 16:453-469.

Cornfield Jerome, William Haenszel, E. Culer Hammond, Abraham M. Lillenfield, Michael B. Shimkin, and Ernst L. Wynder. 1959. Smoking and Lung Cancer: Recent Evidence and a Discussion of Some Questions." *Journal of the National Cancer Institute* 22:173-203.

Cornfield Jerome, William Haenszel, E. Culer Hammond, Abraham M. Lillenfield, Michael B. Shimkin, and Ernst L. Wynder. 2009. "Smoking and Lung Cancer: Recent Evidence and a Discussion of Some Questions." Reprinted in *International Journal of Epidemiology* 38:1175-91.

Desposato, Scott W. and John R. Petrocik. 2003. "The Variable Incumbency Advantage: New Voters, Old Voters, and the Personal Vote." *American Journal of Political Science* 47:18-32.

Gaines, Brian J. 2011. "What is Fair Redistricting." In *Rethinking Redistricting: A Discussion About the Future of Legislative Mapping in Illinois*. Institute of Government and Public Affairs, University of Illinois.

Gaines, Brian J., James H. Kuklinksi, and Christopher Z. Mooney. 2013. "Revisiting Redistricting: Who Should Be Afraid of Partisan Mapmaking?" *Illinois Report 2013*. Institute of Government and Public Affairs, University of Illinois. https://igpa.uillinois.edu/sites/igpa.uillinois.edu/files/reports/IR13_web_Full.pdf.

Gelman, Andrew, and Gary King. 1990. "Estimating the Electoral Consequences of Legislative Redistricting." *Journal of the American Statistical Association* 85:274-282 (June)

Gelman, Andrew, and Gary King. 1994. "A Unified Method of Evaluating Electoral Systems and Redistricting Plans." *American Journal of Political Science* 38:514-554 (No. 2, May)

Gerber, Alan S., Donald P. Green, and Christopher W. Larimer. 2008. "Social Pressure and Voter Turnout: Evidence from a Large-Scale Field Experiment." *American Political Science Review* 102:33-48.

Glazer, Amihai, Bernard Grofman, and Marc Robbins. 1987. "Partisan and Incumbency Effects of 1970s Congressional Redistricting." *American Journal of Political Science* 31:680-707.

Greene, William H. 2012. *Econometric Analysis*, 7th ed. Saddle River, NJ: Prentice Hall.gr

Grofman, Bernard, and Gary King. 2007. "The Future of Partisan Symmetry as a Judicial Test of Partisan Gerrymandering after *LULAC v. Perry*. *Election Law Journal* 6:2-35.

Imai, Kosuke, Luke Keele, Dustin Tingley, and Teppei Yamamoto. 201.. "Unpacking the Black Box of Causality: Learning about Causal Mechanisms from Experimental and Observational Studies." *American Political Science Review* 105:765-789.

Jackman, Simon. 2014. "The Predictive Power of Uniform Swing." *PS: Political Science and Politics* 47:317-321.

Jackman, Simon. 2015. *Assessing the Current Wisconsin State Legislative Districting Plan*. July 7.

Jacobson, Gary C. 2003. "Terror, Terrain, and Turnout: Explaining the 2002 Midterm Elections." *Political Science Quarterly* 118:1-22.

Jacobson, Gary C. 2015. "It's Nothing Personal: The Decline of the Incumbency Advantage in US House Elections." *Journal of Politics* 77:861-873.

Jacobson, Gary C. and Jamie L. Carson. 2016. *The Politics of Congressional Elections* 9th ed. Lanham, MD: Rowman & Littlefield.

Kernell, Georgia. 2009. "Giving Order to Districts: Estimating Voter Distributions with National Election Returns." *Political Analysis* 17:215-235.

King, Gary, Robert O. Keohane, and Sidney Verba. 1994. *Designing Social Inquiry: Scientific Inference in Qualitative Research*. Princeton: Princeton University Press.

Kousser, J. Morgan. 1996. "Estimating the Partisan Consequences of Redistricting Plans – Simply." *Legislative Studies Quarterly* 21:521-541.

Levendusky, Matthew S., Jeremy C. Pope, and Simon D. Jackman. 2008. "Measuring-District Level Partisanship with Implications for the Analysis of U.S. Elections." *The Journal of Politics* 70:736-753 (No. 3, July)

McDonald, Michael P. 2006. "Drawing the Line on District Competition." *PS; Political Science and Politics* 39:99-104 (No 1, January)

McDonald, Michael P. 2007. "Redistricting and Competitive Districts." In Michael P. McDonald and John Samples, eds., *The Marketplace of Democracy: Electoral Competition and American Politics*. Washington, DC: Brookings Institution Press.

McDonald, Michael D. 2014. "Presidential Vote Within State Legislative Districts." *State Politics & Policy Quarterly* 14:196-204.

McDonald, Michael D. and Robin E. Best. 2015. "Unfair Partisan Gerrymanders in Politics and Law: A Diagnostic Applied to Six Cases." *Election Law Journal* 4:312-330.

Schmitter, Phillipe C., and Terry Lynn Karl. 1991. "What Democracy Is. . . and is Not." *Journal of Democracy* 2:75-88.

Sievert, Joel, and Seth C. McKee. 2018. "Nationalization in U.S. Senate and Gubernatorial Elections." *American Politics Research*. https://doi.org/10.1177/1532673X18792694

Smidt, Corwin D. 2017." Polarization and the Decline of the American Floating Voter." *American Journal of Political Science* 61:365-381.

Tufte, Edward R., ed. 1970. *The Quantitative Analysis of Social Problems*. New York: Addison Wesley.

Tufte, Edward R. 1974. *Data Analysis for Politics and Policy*. Englewood Cliffs, NJ: Prentice Hall.