Rebuttal Report: Response to Expert Reports of Sean Trende and Nicholas Goedert

Kenneth R. Mayer, Ph.D.

Department of Political Science

University of Wisconsin-Madison

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This report presents my responses to the criticisms that Sean Trende and Professor Nicholas Goedert make of my report.[1]

I. Summary

A. Both Trende and Goedert erroneously argue that Democrats are more geographically concentrated than Republicans in Wisconsin, which creates a natural pro-Republican bias even under a neutrally-drawn district plan. Both arguments are based on unreliable methodologies, flawed measures, and lead to inaccurate conclusions. Trende's methodology for measuring partisan concentration relies on an unorthodox method (the PVI) far more common among political commentators than academics who study spatial patterns of concentration and isolation. Moreover, as he applies it here, Trende relies on fundamentally inaccurate measures of geography that are guaranteed to demonstrate that Democratic wards are closer to one another than are Republican wards.

Goedert's arguments about geographic concentration are analogous to Trende's, and suffer from the same flaws in that they are based on superficial claims that do not rely on actual measures of spatial concentration or isolation. Moreover, Goedert's claims here contradict his own research, in which he finds that even after controlling for urbanization (a proxy for concentration), Republican control of the redistricting process has a large and statistically significant impact on a plan's bias. A model in one of his papers (Goedert 2015) also shows that a court-drawn or bipartisan map in Wisconsin would be expected to produce a *pro-Democratic bias*. The model generates the same expectation for a court-drawn or bipartisan map in a state that resembles the country as a whole. Accordingly, based on Goedert's own analysis, there is no natural pro-Republican tilt in either Wisconsin or the typical U.S. state.

In contrast to Trende's and Goedert's unorthodox techniques, widely (even universally) accepted measures of spatial distributions, such as Global Moran's I (Cho 2003) and the Isolation Index (Reardon 2004), show that Wisconsin's Republicans and Democrats are equally spatially concentrated and equally spatially isolated from each other, and that in some election years *Republicans are more concentrated* than Democrats.

B. Trende criticizes my method of estimating the partisanship of uncontested Assembly districts as biased. But his criticism stems from a superficial and erroneous discussion of a single figure in my report (Figure 2), and he erroneously believes that I set the Assembly votes in uncontested districts to the presidential vote in those districts. He does not take notice of the fact that my analysis was based on a comprehensive multiple regression model that controlled for the very factors that he claims create bias, nor that my model produces extraordinarily accurate forecasts of the actual data, using multiple methods.

[1] "Analysis of the Efficiency Gaps of Wisconsin's Current Legislative District Plan and Plaintiff's Demonstration Plan," July 3, 2015.

C. Trende criticizes my baseline measure of partisanship for not taking into account factors such as incumbency, candidate quality, and spending. This is an inaccurate criticism, because estimating baseline partisanship is *designed* to control for incumbency, campaign spending, and candidate quality. This is the method preferred in the academic literature on redistricting, which seeks to understand the consequences of hypothetical plans (in which candidate quality, spending, and incumbency are unknown). My approach is *identical* to the method used by Professor Gaddie, who produced the baseline partisan estimates used by Wisconsin's map drawers in 2011.

D. Goedert challenges my model for estimating baseline partisanship in 2012, contending that I took into account information that the authors of Act 43 did not have (the 2012 election results). However, my baseline estimates of partisanship are nearly identical to those generated by Gaddie in 2011, indicating the same conclusions follow whether 2012 or pre-2012 data are used in the analysis. In addition, pre-2012 election results are highly correlated with 2012 election results, indicating that it would make no difference if I had used earlier election results. Goedert dismisses the convergence between my estimates and Gaddie's estimates as "mostly coincidental," but offers no evidence or data to support his assertion.

E. Geodert also challenges my efficiency gap calculations for ignoring the effects of incumbency, which he asserts that any author of a redistricting plan would incorporate. His criticism fails to acknowledge that controlling for incumbency is the standard methodology for estimating the partisan consequences of a hypothetical district plan. Nevertheless, I recalculated efficiency gap estimates for both Act 43 and my Demonstration Plan, taking incumbency into account. The substantive conclusions are identical: the efficiency gap for my plan increases slightly (but is still well within acceptable limits), as does the efficiency gap for Act 43. The *difference* between the two plans' efficiency gaps remains enormous.

F. Goedert criticizes my efficiency gap calculations for not including any sensitivity testing to determine whether my results are robust to changes in the statewide electoral environment. I conducted a uniform swing analysis over the range of plausible election results, based on the maximum and minimum statewide Democratic Assembly vote since 1992. This analysis shows that the efficiency gaps of both Act 43 and the Demonstration Plan are robust: Act 43's efficiency gap remains very high across this range, always significantly above the plaintiffs' suggested 7% threshold, and the Demonstration Plan's efficiency gap remains very low, and is always well below the threshold. Goedert is simply incorrect in asserting that the plans' respective efficiency gaps are not robust, and, again, offers no data or evidence to support his claim.

G. Throughout their reports, neither Trende nor Goedert has actually done any analysis that identifies problems with my analysis, or that specifically shows where my analysis is

incorrect. Trende and Goedert merely offer speculative and unsubstantiated criticism, but never offer any substantive data or evidence that supports their arguments. And, as I will show, when they attempt to analyze Wisconsin's political geography, their conclusions are utterly wrong.

II. The Claim that Wisconsin's Political Geography Has a Pro Republican Bias

While I will go into more detail on the specific points each report makes, I focus first on a central argument both Trende and Goedert make: that Wisconsin has a natural distribution of Republicans and Democrats that produces an intrinsic pro-Republican bias in a neutrally-drawn redistricting plan. They claim that because Democrats in Wisconsin happen to be (allegedly) naturally concentrated in small pockets of overwhelming Democratic strength, even a neutrally-drawn map would produce a large pro-Republican efficiency gap. As a result, they conclude, it is not possible to consider a large pro-Republican efficiency gap as evidence of gerrymandering.

I begin by noting that both Trende and Goedert ignore the role that political geography already plays in plaintiffs' proposed test. Under the test's first prong, if the state's motive in enacting its plan was simply to follow the contours of the state's geography, then partisan intent would not be present and plaintiffs would proceed no further in their claim. Similarly, under the test's third prong, if the state can show that its plan's large efficiency gap was necessitated by the geographic distribution of the state's voters, then the plan would be upheld. These points mean that geography is already properly incorporated into plaintiffs' proposal.

There are, additionally, two points that fundamentally negate the utility of this line of attack. First, the geographic concentration argument is predicated on the foundational assumption that a *neutrally-drawn map* would have produced a pro-Republican bias. Even if Trende and Goedert are correct in this assumption (which they are not), they take no position on whether the process in Wisconsin was, in fact, neutral. The record of the federal redistricting trial clearly shows that Act 43 was designed with the predominant purpose of benefiting Republicans and disadvantaging Democrats, and neither Trende nor Goedert contradicts the findings in my report of examples of blatant packing and cracking that are the very DNA of a partisan gerrymander.

And second, even if the state's experts are correct that political geography has produced the pro-Republican bias in Wisconsin's state legislative district plan (which they are not), it is impossible for them to quantify *how much* of an effect geography has had: is it 5%? 10%? 90%? 100%? Neither Trende nor Goedert have actually done any analysis that *demonstrates* that the alleged concentration of Democrats *in Wisconsin* will produce a pro-Republican efficiency gap, or any work that quantifies how concentration is related to efficiency gap calculations. They simply assert (incorrectly) that Democrats are more concentrated than Republicans, and therefore that even a neutral map will produce a pro-Republican bias.

But they are also wrong on the facts. Their argument about geographic concentration is based on flawed data and measures, and has no basis in accepted methods of measuring geographic concentration and isolation. Trende, in particular, uses an unorthodox method with

no support in the peer-reviewed literature, and one that is guaranteed to produce a biased result that shows Democrats far more concentrated than they actually are. Goedert's argument contradicts his own published work, which shows that partisan control of redistricting generates a substantial bias even after partisan concentration is taken into account. His argument, further, falls victim to the Modified Areal Unit Problem, in that it is based entirely on the analysis of wards, ignoring the fact that wards are aggregated into districts. As I demonstrate, this aggregation process completely changes the applicability of Goedert's conclusions.

When I analyze the geographic distribution of Wisconsin's Democrats and Republicans using widely accepted measures of spatial concentration and isolation (Global Moran's I and the Isolation Index), I find that there is very little evidence of significant disparities in how the parties' voters have been distributed in recent election cycles. Republicans are in fact *more concentrated* than Democrats when measured by the 2012 Assembly vote.

A. Trende

Trende spends nearly half of his report (paragraphs 62-105) arguing that Democrats are naturally more concentrated ("clustered") than Republicans in Wisconsin, which creates a natural packing effect. Much of this discussion is entirely irrelevant to Wisconsin (Trende's discussion of patterns in the southern United States, Virginia, and differences between the 1996 and 2008 Democratic coalitions; see paragraphs 62-77). Trende also simply asserts that "there is little doubt that the Democratic vote in Wisconsin is also increasingly concentrated in fewer counties" (paragraph 71). He neither explains the relevance of the *county* vote to the issue of geographic distribution and legislative redistricting, nor why the county vote pattern in 1988 or 1996 is germane to the environment in 2012.

1. The PVI (partisan vote index) is the wrong quantity of interest

As applied to Wisconsin, Trende attempts to demonstrate that over the last 20 years Democrats have become more concentrated. His method relies on a quantity he calls the Partisan Lean Index, which is the party's county or ward vote share minus the party's statewide vote share, and appears to be analogous to the Cook PVI, which is the same quantity calculated using the congressional district vote and the national presidential vote. Trende argues that Democratic wards are closer together than Republican wards, which to him is evidence of geographic clustering that produces a natural pro-Republican redistricting bias.

The PVI (which is how Trende abbreviates the measure) is a quantity that is not commonly used in the academic literature, and when it is, it is used largely as a simple descriptive statistic. What this index does is simply redistribute the ward vote around the statewide average, and thus tells us which areas are more Democratic (or Republican) than the state as a whole, and which areas are less so.[2] It tells us little about overall partisan strength, and

[2] The Cook Political Report notes that it "introduced the Partisan Vote Index (PVI) as a means of providing a more accurate picture of the competitiveness of each of the 435 congressional districts." http://cookpolitical.com/story/5604

is useful only in comparing elections at one level (here, counties or wards) to elections at another (the state).

The PVI is used almost exclusively by political commentators to describe congressional districts (the most widely known is the Cook PVI, which compares the average congressional district vote split over two consecutive elections to the average national presidential vote over those same elections). It is used less frequently in academic research, and then largely as a basic descriptive statistic used to classify districts as competitive or not. It is not used in the context of state legislative redistricting (Trende did not cite any studies that support the use of his measure, and could not identify any in his deposition).

Moreover, Trende appears to have made two errors in his calculation of the PVI.[3] First, while he states that his PVI is based on the top-of-the-ticket race in each year, he uses the gubernatorial elections as his top-of-the-ticket race in 2002, 2010, and 2014, but the U.S. Senate race in 2006, even though there was a gubernatorial race that year. While scholars may differ on whether a gubernatorial or U.S. Senate election is the correct top-ticket race, there is no justification whatsoever for being inconsistent.[4]

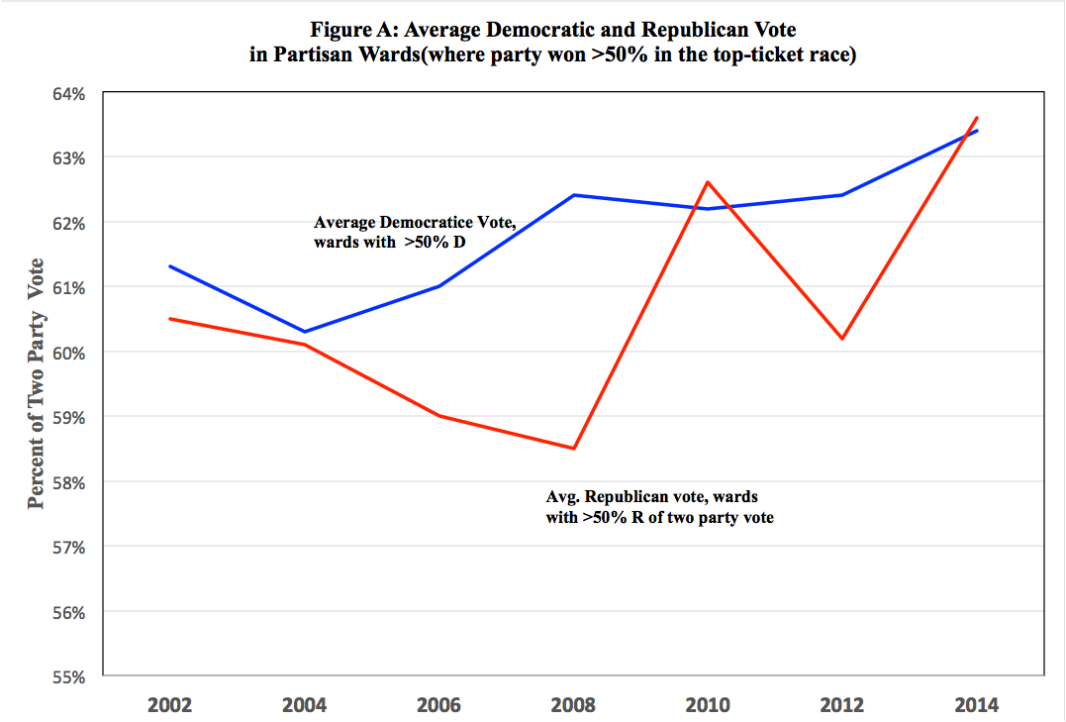
Second, in calculating his 2014 PVI, Trende mistakenly subtracted the *2014* statewide percentages from the *2012* ward totals (this is the code he used to generate the PVI for 2014; the error is highlighted, and "map_2012$r_share" is the ward vote for 2012):

```
map_2014=readOGR("Wards_Final_Geo_111312_2014_ED.shp",
"Wards_Final_Geo_111312_2014_ED")
map_2014=spTransform(map_2014, CRS("+proj=longlat +datum=WGS84"))
map_2014$r_share=map_2014$GOVREP14/(map_2014$GOVREP14 + map_2014$GOVDEM14)
map_2014$pvi=map_2012$r_share -
sum(map_2014$GOVREP14)/(sum(map_2014$GOVREP14) + sum(map_2014$GOVDEM14))
map_2014$pvi[which(is.nan(map_2014$pvi))]=0
```

Instead of the PVI, the actual ward level vote (or party vote share) is a much more direct measure of ward partisanship. I used LTSB ward level data from 2002 to 2014 to calculate the average Democratic percentage of the vote in a Democratic ward (all wards that were more than 50% Democratic in the top-ticket race), and the average Republican vote in wards where Republicans won more than 50% of the top-ticket vote. A graph of this data shows a very different pattern from what Trende claims (Republicans are in red; Democrats in blue):

[3] These occurred in the R file "Wisconsin_clustering_computation.R" that Trende disclosed.

[4] This inconsistency could well affect Trende's results, as the vote percentages were vastly different in the two races in Wisconsin. Democrats garnered 53.8% of the two-party vote in the gubernatorial election, but 60.5% in the Senate race (GAB data).



Here, we see that Democrats and Republicans have moved in almost identical fashion between 2002 and 2014. In 2002, Democrat wards were about 60.8% Democratic, and Republican wards were about 60.5% Republican in the top-ticket races. In 2014, similarly, both Democratic and Republican wards became more partisan: Democratic wards were 63.3% Democratic, and Republican wards 63.6% Republican.

Trende's claim that Democratic wards have become more Democratic, while Republican wards have not become more Republican (paragraphs 91-95), is simply false.

Trende offers no justification or support for why he is relying on the PVI measure rather than more direct indicators of ward partisanship; he merely asserts that it is a relevant quantity. Given that there are far more widely used and relevant measures of district level partisanship, his reliance on it in this context is unsupportable.

2. Trende's "Nearest Neighbor" Method is Inappropriate and Inaccurate

After introducing the PVI, Trende attempts to use it to demonstrate that Democrats have become more closely packed than Republicans (which, he asserts, produces a natural pro-Republican gerrymander). Apart from the irrelevance of the PVI, Trende's analysis uses a fundamentally flawed measure that is guaranteed to exaggerate the extent of Democratic concentrations. Instead of his measure, widely used and academically accepted metrics of concentration and isolation show that Democrats and Republicans are *both* highly segregated, and to about the same extent. Just as there are core areas of high Democratic strength in Milwaukee and Madison, there are similar Republican core areas in the "collar counties" of Waukesha, Ozaukee, and Washington.

The premise of Trende's argument is that pro-Democratic wards are closer to other pro-Democratic wards than are pro-Republican wards to other pro-Republican wards. His method, which I infer from his description, is to identify a pro-Democratic or pro-Republican ward of a certain percentage lean, and then to find the distance to the nearest ward with the *same* partisan lean. He determines the *median* distance between similar wards, and presents two graphs (about paragraph 98 in his report) showing that the median distance between similar Democratic wards is smaller than for Republican wards, and that as Democratic wards become more Democratic, they become closer to one another.

This is reminiscent of the nearest neighbor method used in the study of populations, but it bears little resemblance to how the concept is actually used in the literature, even in its earliest form (Clark and Evans (1954) used it to study the distribution of plant and animal populations).[5] His application of this method is highly unorthodox, unsuited to the study of redistricting, and not based on any accepted peer-reviewed academic work (he does not cite a single study in support of his method).

Trende's method is to start with a ward (call it *i*), calculate its PVI and assign it to a quantile, and then locate the closest ward that shares this PVI quantile (call it *j*). The geographic distance between wards *i and j* (presumably calculated using the ward centroids, although Trende fails to specify this key detail) is then recorded (paragraph 97). The process is repeated for every ward over every election from 2002 to 2014, producing for each election a matrix consisting of every ward and the distance to the nearest ward with the same PVI quantile. He then calculates median distances between wards of the same PVI quantiles, which he claims shows that Democratic wards are, and have been continuing to move, closer together than Republican wards.

There are several problems with this approach. First, and most fundamentally, the proximity of similar wards is simply not a measure of geographic concentration or clustering. Trende's method tells us nothing about which wards are actually *adjacent* to wards of a certain PVI. It only tells us how far these wards tend to be from other wards of the same partisan lean. It is entirely possible for wards of the same partisan makeup to be far apart but still easy to join in the same district (think of a sparsely populated but uniformly partisan area). Likewise, it is entirely possible that wards of the same partisan makeup are close together but quite difficult to combine in the same district (think of a densely populated but politically heterogeneous area). Trende's method cannot distinguish between these scenarios, and as a result it cannot tell us anything about the geographic patterns that actually matter for redistricting.

Second, Trende does not explicitly define in his report what a "similar partisan index" (paragraph 97) means. Clearly, Trende is classifying them in some way, defining "similar" as within some range, as his vague discussion of quantiles indicates (paragraph 98). But without specifying the range, it is impossible to know whether his measure has any meaning. Different

[5] Byers and Raferty (1998) use a near neighbor method to estimate the statistical relationship between points in space and how they differ from random distributions, or "clutter," in the context of distinguishing landmines from other objects during aerial reconnaissance. Neither their work nor Clark and Evans (1954) supports Trende's use of the method.

classification methods -- requiring a match of, say, within 0.1 percentage points, or classifying according to deciles or some other method -- are likely to yield very different results than requiring a match of within 0.5 or 1.0 percentage points or using a larger number of categories. His graphs suggest he is using some type of percentile distribution (the x axis label refers to "(.05% is the most Democratic [or Republican] Ward)," but he does not explicitly define why he chose this particular scheme or how he calculated the quantiles. On this point alone, his method lacks validity or replicability.

But there are two additional serious – fatal, in fact – flaws in this method. First, in treating the geographic distances between wards as his quantity of interest, Trende does not take into account the fact that wards in Wisconsin are not uniform in area. Ward areas actually vary widely: some are very small, others are moderate in size, and still others are very large (wards are drawn within specified population limits, but their geographic areas are not similarly constrained).

Table A shows the mean and median areas (in square miles) of Wisconsin wards. The average is 8.41 mi^2, but the range is huge: the smallest ward with a nontrivial population is in the City of Middleton: ward 19, with 690 people in an area of 0.0071 mi^2. The largest ward in the state is in the Town of Winter: ward 2 (in Sawyer County), with 565 people in an area of 227.7 mi^2.

Geographic distances between ward centroids will, obviously, depend on how large the wards are. Although centroid-to-centroid distances will not map perfectly onto area differences (because the distances will vary with the shape and orientation of wards), two large wards – even if they are adjacent – will show up as much farther apart than two smaller wards that might be separated by numerous other wards and municipal boundaries.

The problem is magnified when we observe that ward sizes are correlated with other relevant variables, particularly whether a ward is in a city, and most crucially, whether it is a Democratic or Republican ward:

Table A 2012 Ward Sizes (square miles)[6]	Mean	Median
Statewide Average	8.41	1.12
City of Milwaukee	0.29	0.20
Rest of State	8.83	1.27
Democratic Wards	5.91	0.56
Republican Wards	10.96	3.45

Wards in the city of Milwaukee have a mean area of only 0.29 mi^2, which is 3% of the size of the mean area statewide. Democratic wards (measured by whether the 2012 Democratic presidential vote was above 50%) are, on average, only about half the size of Republican wards (5.91 mi^2 vs. 10.96 mi^2).

In relying on the distance between wards, Trende is thus putting his thumb on the scale; all other things equal, this method will *always* show Democratic wards to be much closer than Republican wards, irrespective of whether this concentration is real or merely an artifact of ward area. To put it most simply, smaller Democratic wards will *always* appear closer than larger Republican wards.

But a second and equally serious problem lurks. Trende does not use the *mean* distance between wards as his quantity of interest, but rather the *median*. He justifies this choice "because outlying wards, such as Menominee County, exert an undue amount of leverage on averages" (paragraph 97).

This is the wrong measure, because the "nearest neighbor" approach is unlikely to pair, say, a ward in Milwaukee with a ward in northwest Wisconsin. Menominee County will not exercise "an undue amount of leverage" because it is an outlying ward. It will exercise an undue amount of leverage because it *has a very large area* (222.8 mi^2), which is something Trende should, but does not, correct for.

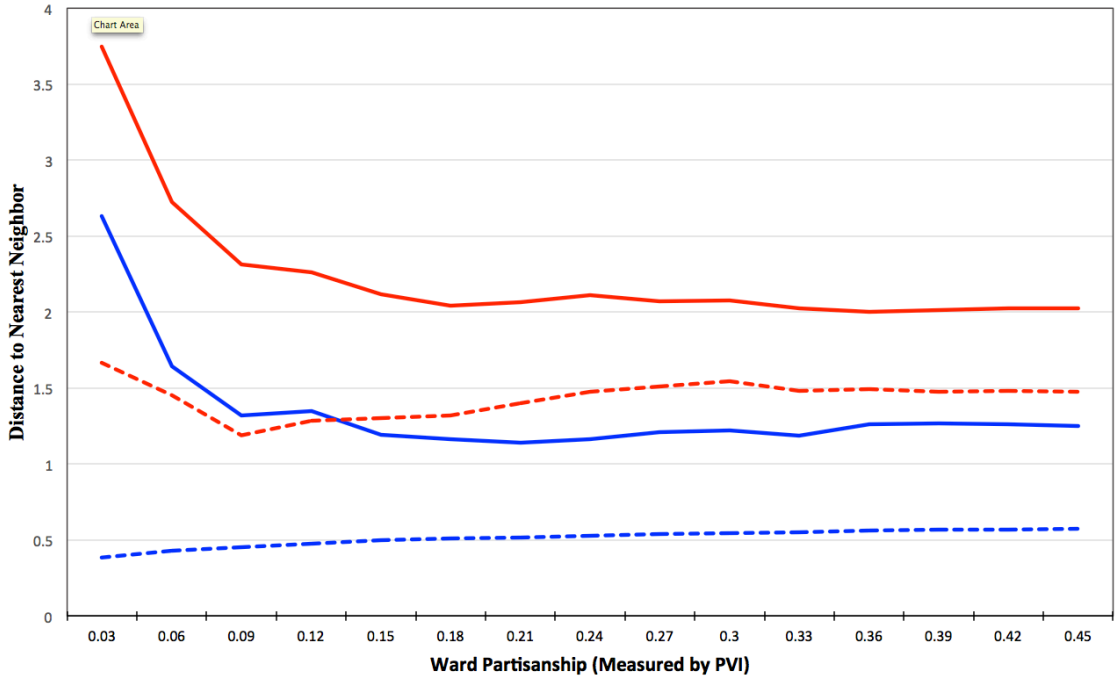
His use of the median rather than the mean further exaggerates the difference between Republican ward distances and Democratic ward distances. The average Republican ward area is 1.9 times larger than the average Democratic ward area (10.96 vs. 5.91 mi^2). But the *median* Republican ward is *6.2* times larger than the median Democratic ward (3.45 mi^2 vs. 0.56 mi^2).

[6] Calculated directly from the LTSB shape files of 2012 wards, obtained from http://legis.wisconsin.gov/gis/data.

Because the disparity is three times larger for the median versus the mean area, Trende is further stacking the deck in favor of his preferred hypothesis.

I was able to replicate Trende's analysis, using LTSB data and the R code he disclosed. When the mean distances between similar wards are included, Figure B is the result for the 2012 Election:[7]

Figure B
Nearest Neighbor Analysis: Mean vs. Median Distance



In this graph, the dotted lines are the median nearest neighbor distances for Democratic (blue) and Republican (red) wards, replicating what Trende did in his median distance graphs around paragraph 98 in his report. Wards become more partisan as we move from right to left.

The *mean* distances are shown with solid lines. While Republican wards remain farther apart than Democratic wards, the mean distances for both parties are much larger than the median distances. Proportionally, Republican and Democratic wards are much closer together in mean than in median distances (which is what one would expect, given the exaggerated difference between median Democratic and Republican ward sizes). Specifically, the mean distance between Republican wards is only about 70% larger than the mean distance between Democratic wards, compared to a 180% difference between the median Republican and Democratic distance.

[7] The pattern Trende identifies is largely constant across all elections; adding the additional cycles will not change the results.

More relevant is the shape of the mean distance lines. They show that Republican and Democratic distances move precisely in parallel, and that strongly Democratic wards are significantly *farther apart* than weaker Democratic wards (as are strongly Republican wards). This is the complete opposite of Trende's claim that stronger Democratic wards are closer together than weaker Democratic wards, and it obliterates the core of Trende's report: the assertion that the pro-Republican bias evident in Act 43 is the natural result of Democrats being more geographically concentrated.

To conclude, Trende's argument about Democratic concentration is based on an irrelevant measure of partisanship (PVI) that is incorrectly calculated, applies a methodology that bears no relationship to any scholarship or actual research on spatial distribution, ignores a key feature of Wisconsin's actual political geography (ward area), relies on an improper distance measure that is enormously biased in favor of his hypothesis, and produces a result that fundamentally misrepresents what the data actually shows. Because of his use of a questionable method and fundamentally flawed measures, Trende's opinions should be regarded as uninformative.

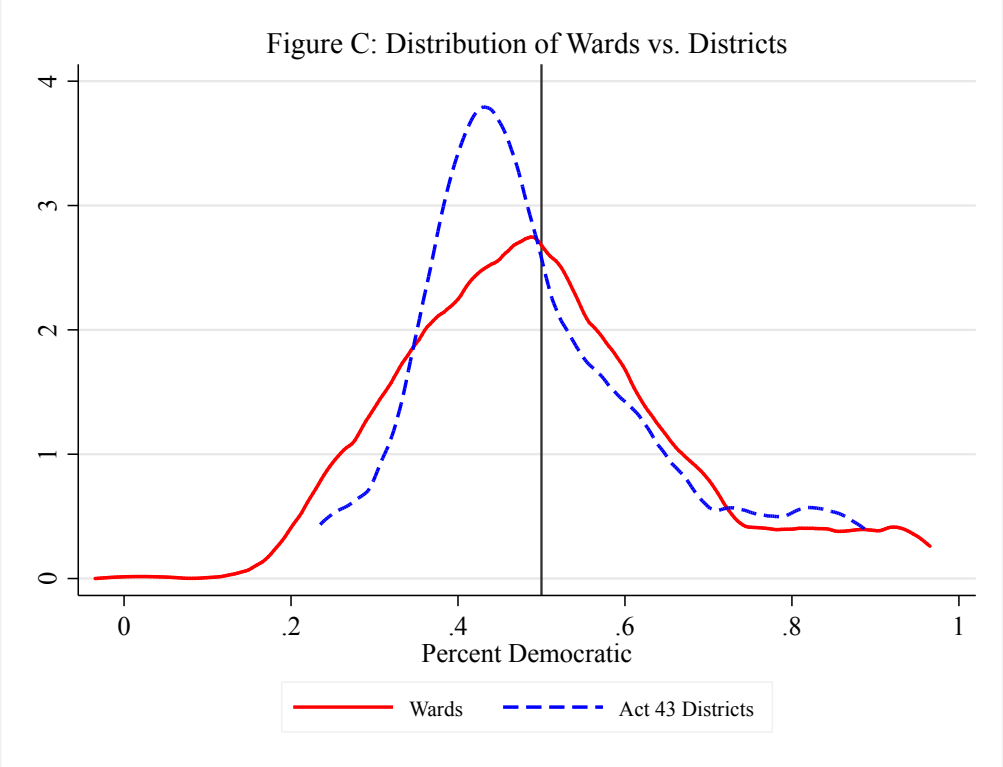
B. Goedert

Goedert, like Trende, asserts that Wisconsin's natural geography creates an intrinsic pro-Republican bias in redistricting (p. 17). He cites his own research that geography produced a pro-Republican bias in the 2012 congressional election (p. 19).

The only analysis Goedert conducts as to Wisconsin is an examination of wards, which he claims shows "the bias inherent in Wisconsin's geography" (p. 21). His analysis is a simple "uniform swing" study of wards in 2012, adjusting the Democratic presidential vote in each ward downward by 3.5% to determine the overall ward distribution in the event of a tied election (Figure 1, p. 22). He asserts that based on this analysis, "Republicans would win 60.2% of wards, comprising 54.4% of the voting population" in a tied election (p. 22). This is the extent of his analysis.

This analysis, however, is a non sequitur, because it fails to aggregate wards to the relevant geographic level, which is *districts*. Goedert's failure to take this into account is an example of the Modified Areal Unit Problem, in which inferences at one level of geography frequently do not hold at other levels of aggregation; see King (1996). In this example, the ward level vote is far less relevant than the district level vote, because it is entirely possible that wards will be aggregated in such a way that the pattern he observes either disappears (or even reverses).

When we examine the distribution of *districts*, which have a population deviation small enough that we can consider them equal (the deviation under Act 43 is 0.76%), we in fact see almost the reverse pattern. The following graph (Figure C) displays Goedert's adjusted ward level presidential vote in a simulated 50-50 election, along with an adjusted baseline forecast for Act 43 districts, using my baseline open seat model, in a simulated tied election. Both wards and districts are weighted based on the number of votes cast in each unit. This allows me to directly compare ward level results to district level results:



What this figure demonstrates is that as wards are aggregated into districts, the distribution substantially changes. The red line is a kernel density plot of the ward Democratic vote percentage in a simulated tied election; it is a continuous version of the histogram Goedert presents in his Figure 1. The dotted blue line shows the predicted Democratic vote in Act 43 districts in a simulated tied election – or, what occurs after the wards are aggregated into Assembly districts. The overall shape of the curves, the mode of each distribution, and even the mean vote percentage vary as we aggregate from wards up to districts. Knowing the ward distribution ultimately does not tell us much about what the distribution of districts will look like; the process of aggregation is crucial.

More significantly, the district distribution is much more tilted in a Republican direction than is the ward distribution. The ward distribution is nearly normal in shape, and has a peak very close to 50% Democratic. In contrast, the *district* distribution is skewed to the right, and has a much higher peak around 42% Democratic, meaning that there are many more districts that Republicans win by relatively small margins (indicating that Democrats are cracked), and many more districts where Democrats win by much larger margins (indicating packing). Accordingly, the district distribution does *not* mirror the underlying distribution of wards. Rather, it reveals that Act 43's designers were able to distort a fairly neutral ward distribution into a far more advantageous district distribution, through gerrymandering.

1. Goedert's Published Work Contradicts His Report

Goedert's own prior work indicates that unified party control of state government has an independent and significant effect on the bias of redistricting plans, even after controlling for

population concentration. This work also indicates that if Wisconsin, or a state resembling the country as a whole, had a court-drawn or bipartisan map in 2012, this map would have had a slight *pro-Democratic* bias. These findings further obliterate the claim that Act 43's extreme partisan tilt resulted from Wisconsin's natural political geography.

In a 2014 article, Goedert analyzes the consequences of different redistricting processes, looking for evidence that partisanship and geography each have an independent effect on the partisan bias of redistricting plans.[8] Using an unorthodox definition of gerrymandering – Goedert defines *any* redistricting plan created in a state with unified party control of state government as a partisan gerrymander – he finds that in states with more than six congressional districts, both urbanization (a proxy for Democratic concentration) and unified party control have a strong and statistically significant effect on the bias of a district plan (2014, 6). Goedert interprets his results as indicating that geography matters, and that higher urban concentration leads to more bias against Democrats (2014, 6). But what his results also show is that *even after taking urbanization into account*, the partisanship of the map drawers introduces a separate and significant bias: Republican-drawn maps are associated with an additional *13.6%* pro-Republican bias.

Geodert updated his 2014 article in a more recent manuscript, which incorporated the results of the 2014 midterm elections. Here, he finds that urbanization *no longer has a statistically significant effect* on the bias of district plans (2015, 6). Yet he stills finds evidence that the partisanship of map-drawers has a significant effect on district plans' bias (in 2014, a Republican-drawn plan adds 12.4% bias, or roughly the same as the 13.6% estimate for 2012).

So, on the one hand, Goedert's own work comes to different conclusions about the impact of urbanization (or Democratic concentration): sometimes it matters, other times it does not. But his work is consistent about the effect of partisan control: when partisans draw maps, they *always* do so in ways that dramatically bias plans in their favor. The clear inference is that geography matters much *less* than partisan control in explaining plans' electoral consequences.

Furthermore, we can use Goedert's regression model to generate a forecast of what would have occurred in 2012 in Wisconsin – as well as in a state resembling the country as a whole – under a neutral process (i.e., a court-drawn or bipartisan plan). His regression model includes the following variables (2015, 11):

1. Whether a district plan was drawn by Democrats or Republicans (court-drawn and bipartisan plans are the excluded category)
2. A state's African American population percentage
3. A state's Hispanic population percentage

[8] Goedert's definition of bias is essentially identical to the efficiency gap. He "compare[s] the mean vote share with the expected seat share under a 'fair' map with zero bias and a historically average seats-votes curve" (2014, 3). In the "historically average seats-votes curve," "a 1% increase in vote share will produce about a 2% increase in seat share," which is the same seat-vote relationship implied by a zero efficiency gap (2014, 3). Goedert's bias estimates are thus largely indistinguishable from the efficiency gap calculations of Stephanopoulos and McGhee (2015).

4. The percentage of a state that is urbanized (according to the Census)
5. The statewide Democratic vote
6. The number of congressional seats.

With the coefficients of this model, and the appropriate data for Wisconsin (or any other state), we can calculate what the expected bias would be for a plan in 2012.[9] The dependent variable here is a measure of bias almost identical to the efficiency gap, with positive values indicating a pro-Democratic bias, and negative values a pro-Republican bias. Because this is a linear regression, we can multiply each coefficient by the value of the independent variable, and then sum the results to generate a forecast from any set of data values. In Table B, I set both Democratic and Republic Gerrymanders to 0, simulating a neutrally-drawn plan:

[9] Goedert generated two models, one for states with fewer than 6 congressional districts, and another for states with more than six. As Wisconsin has 8 districts, I use the latter.

Table B **Goedert's Regression Model for 2012** **Dependent Variable:** **Pro-Democratic Bias in a District Plan**			
Variable Name	**(a)** **Coefficient Value**	**(b)** **Variable value for Wisconsin**	**Value** **(a) x (b)**
Democratic Gerrymander	16.6	0	0
Republican Gerrymander	-13.6	0	0
% Black	-0..29	6.6	-1.914
% Hispanic	0.77	6.5	5.005
% Urbanized	-0.72	70.2	-50.544
Statewide Democratic Congressional Vote	0.11	50.8 (2012)	5.588
Number of Seats	-0.16	8	-1.28
Constant	45.0	1	45
Total	**(sum of all values)**		**1.855**

Goedert's regression model thus predicts that if Wisconsin had a neutrally drawn plan in 2012, the resulting map would have had a *pro-Democratic* bias of 1.855%. In other words, in the absence of unified Republican control over the redistricting process, Wisconsin's demographic, geographic, and political characteristics would have resulted in a small natural *Democratic* advantage. And this is no fluke of the state or the election year. We can also use Goedert's model to predict what would happen in a state resembling the United States as a whole (i.e., a state that is 13.2% black, 17.4% Hispanic, 80.7% urbanized, 51% Democratic, and with

8.7 congressional seats[10]). Substituting these values into the regression model shows that in an "average" state, a neutrally-drawn map would have had a *pro-Democratic bias* of 0.684% in 2012.

Goedert's 2014 variant of the model (2015, 13) further predicts that Wisconsin would have had a *pro-Democratic bias* of 4.392% in 2014, and that the average state would have had a *pro-Democratic bias* of 1.589%. At this point, it is hard to see what is left of the thesis that political geography inherently favors Republicans. If anything, Goedert's own published analysis shows that Wisconsin's political geography slightly favors *Democrats*.

C. Accepted Measures of Geographic Concentration and Isolation Show that Democrats and Republicans are Equally Dispersed

In arguing that Republicans in Wisconsin enjoy a natural geographic advantage, both Trende and Geodert use ad hoc, unorthodox measures of concentration that are neither relevant nor accepted by the academic literature. In fact, there exist widely accepted metrics of geographic concentration and dispersion, used by geographers and demographers to study spatial patterns. Two of the most common are Global Moran's I (Anseln 1995; Cho 2003), and the Isolation Index (Glaeser and Vigdor 2012; Reardon 2004). I use these metrics to determine how Democrats and Republicans in Wisconsin are actually distributed.

Moran's I is a measure of spatial autocorrelation, or how values of a variable in space correlate with values in nearby space. It can be calculated for an entire geographic system (Global Moran's I), or for any specific point in space (Local Moran's I). The Isolation Index indicates, for the average member of a group residing in a certain geographic unit (such as a ward), what share of the member's neighbors in the unit belong to the same group (Iceland and Weinberg 2002, 120). It measures how geographically isolated a group is (Reardon 2004, 153), and it can easily be adjusted, by deducting a group's share of the statewide population, to show how much *more* isolated a group is than we would expect given its statewide size (Glaeser and Vigdor 2012, 2). Both Moran's I and the Isolation Index are widely used in studies of residential segregation and sorting (Chung and Brown 2007; Massey and Denton 1989; Glaeser and Vigdor 2012; Dawkins 2007; Reardon 2004; Iceland and Weinberg 2002), epidemiology (Moore and Carpenter 1999), network effects (Cho 2003), and political geography (Glaeser and Ward 2005). The measures are also used by the U.S. Census Bureau itself (Iceland and Weinberg 2002).

Both Moran's I and the Isolation Index are directly applicable to the issue of measuring the geographic distribution of Democrats and Republicans in Wisconsin. In this context, Global Moran's I tells us how likely Democrats are to live clustered next to other Democrats (and Republicans to Republicans), and the Isolation Index, adjusted as noted above, tells us to what extent the average Democrat (or Republican) lives in a ward that is more heavily Democratic (or Republican) than the state as a whole. I use these indices to directly assess the geographic distribution of Democrats, and, more importantly, to compare it to the geographic distribution of Republicans.

[10] Calculated as 435/50.

Global Moran's I is analogous to a correlation coefficient, and ranges from -1 to 1; scores close to 1 indicate a very high spatial correlation (i.e., clustering) of Democrats (or Republicans). The Isolation Index ranges from 0 to 1, and, adjusted as noted above, indicates to what extent the average Democrat or Republican lives in a ward that is more heavily Democratic or Republican than Wisconsin as a whole. In calculating both measures, I use the ward as the basic unit of geography and actual Assembly votes.[11] Because I only have geodata for the current wards, I only estimate Global Moran's I for 2012 and 2014. For the Isolation Index, I compute scores dating back to 2004. Both Global Moran's I and the Isolation Index are asymmetrical, and so must be calculated separately for Democrats and Republicans.

Table C shows the values of the Isolation Index, adjusted as noted above, for Democrats and Republicans in Wisconsin from 2004 to 2014:

	Table C Isolation Index	
	Dem-Rep	**Rep-Dem**
2014	0.23	0.20
2012	0.14	0.12
2010	0.15	0.17
2008	0.15	0.14
2006	0.16	0.17
2004	0.20	0.21

As is evident from Table C, Democrats were slightly less isolated than Republicans in 2004, 2006, and 2010, and slightly more so in 2008, 2012, and 2014. In all cases, the differences in isolation were very small, amounting to only one to three percentage points (out of a scale extending from 0% to 100%). In the 2012 election, for instance, the average Democrat lived in a ward whose Democratic vote share was 14% more Democratic than the state as a whole; analogously, the average Republican lived in a ward whose Republican vote share was 12% more Republican than the entire state. In the previous election, it was Republican voters who were more isolated than Democratic voters (17% versus 15%). This analysis in no way supports the claim that Republicans are more advantageously distributed than Democrats; on the contrary, both parties' supporters are almost identical in their geographic isolation over the last decade, and there is no clear temporal pattern. In some years, Democrats are marginally more isolated than Republicans, and in other years Republicans are marginally more isolated than Democrats.

[11] I calculated Global Moran's I using the method in Bivand and Piras (2015) and the R module spdep available at https://cran.r-project.org/web/packages/spdep/index.html. I calculated the isolation index using a Stata module (seg), available at http://econpapers.repec.org/software/bocbocode/s375001.htm.

The results are very similar with the Global Moran's I, again calculated for Democrats and Republicans in Wisconsin, although only for the two elections (2012 and 2014) for which the geodata is readily available:

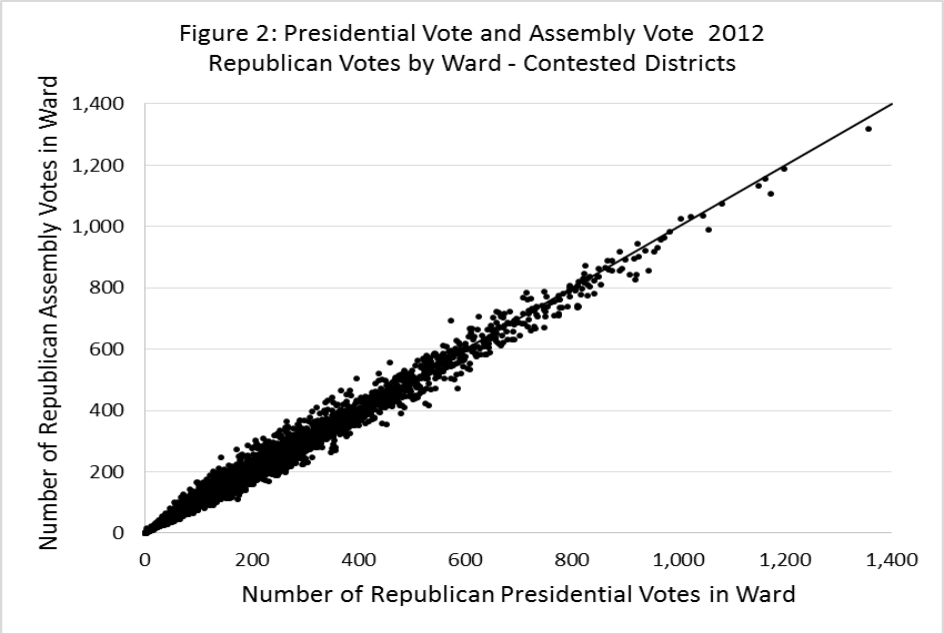
	Table D Global Moran's I	
	Democrats	**Republicans**
2014	0.75	0.68
2012	0.68	0.69

Here, we see that Democrats were slightly less spatially concentrated than Republicans in 2012, but slightly more spatially concentrated in 2014. The differences in both cases are tiny: 0.01 in 2012 and 0.07 in 2014, on a scale that stretches from -1 to 1. The message is quite clear: *both* Democrats and Republicans in Wisconsin tend to live near one another in distinct clusters, but there is no evidence that Democrats are *more* geographically clustered than Republicans.

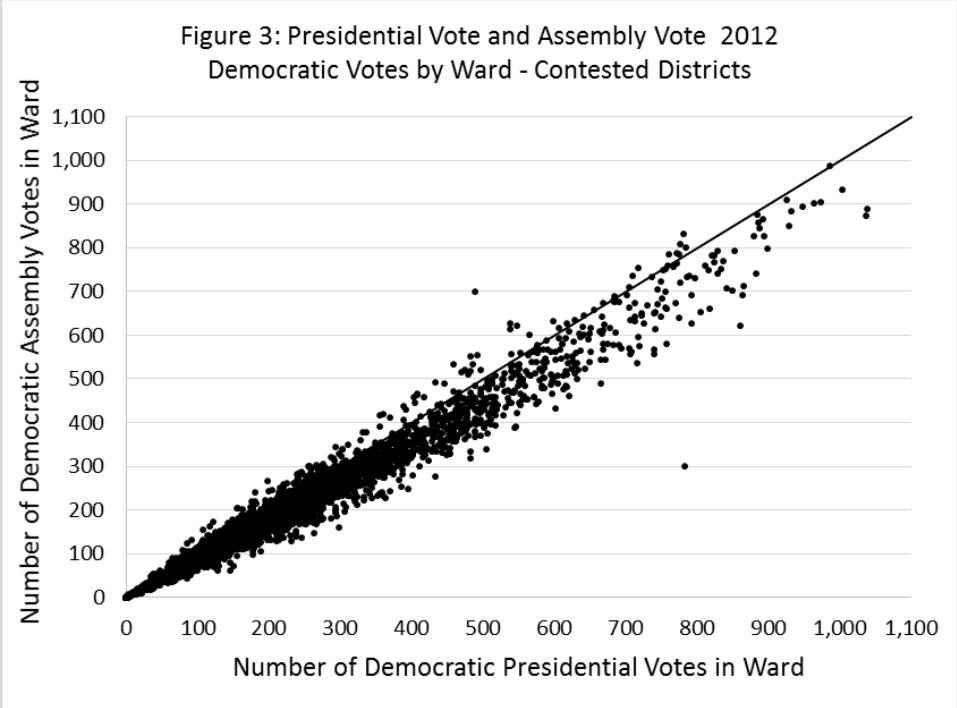
Accordingly, two widely used and accepted measures of geographic distribution show no consistent pattern, and no material difference in how Wisconsin's Democrats and Republicans are dispersed spatially. In no sense, therefore, is it an accurate statement that Democrats are much more concentrated than Republicans – the unsubstantiated claim that comprised the core of both Trende's and Geodert's arguments about natural gerrymanders.

III. Trende's Claim That My Vote Model Is Biased Is Incorrect

Trende claims that there may be "a systematic bias involved in imputing presidential results to state House results" (paragraph 135). As evidence he points to Figures 2 and 3 in my original report, which display the relationship between the ward level presidential vote and the ward level Assembly vote. Trende notes that Figure 2 shows that there is close to a 1:1 relationship between Republican presidential and Assembly votes, as the dots on the graph are distributed around the 45-degree line:



However, Trende claims that the relationship is different for Democratic votes (Figure 3 in my original report):

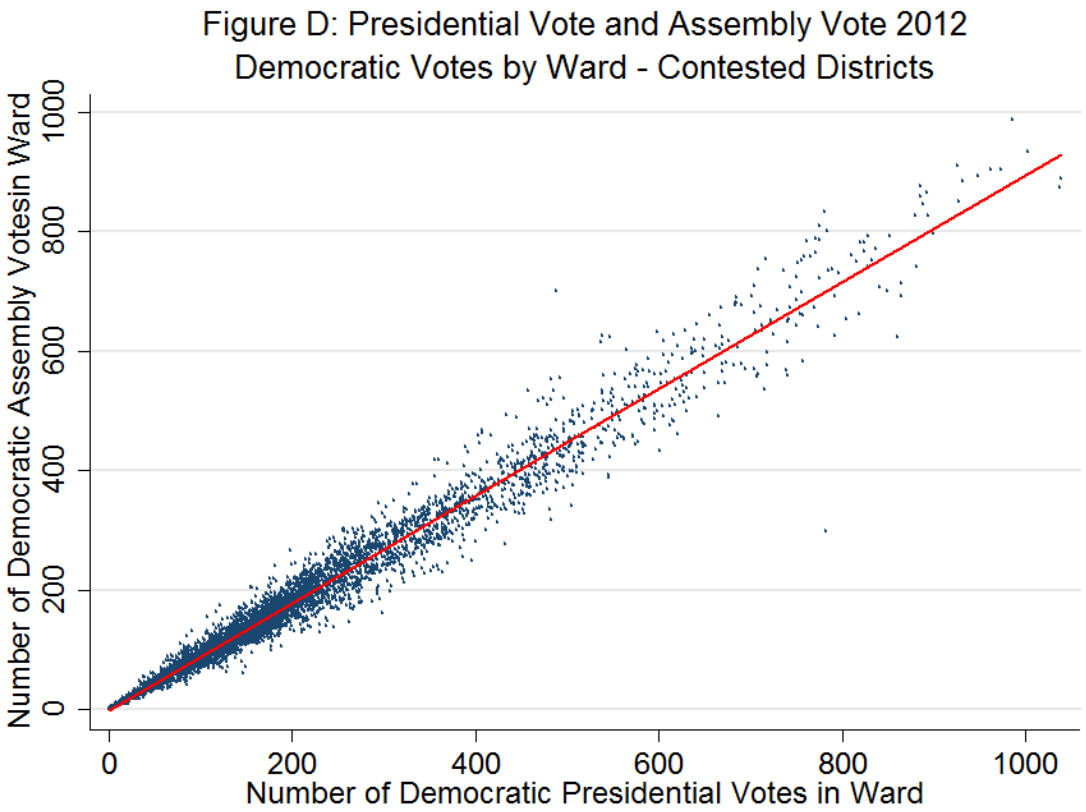


Here, Trende argues, the "dots systematically fall below the line, often creating differences on the order of 10 percent" (paragraph 138). This pattern, he asserts, will "skew the imputation" of votes, resulting in "too many votes [being] imputed in wards reporting a high number of Democratic votes" (paragraph 139).

Trende is completely and unambiguously wrong in this claim, which belies a fundamental lack of understanding of multiple regression and the causes of bias in statistical models. Trende appears to believe that I simply assumed that ward level Democratic Assembly votes are actually *equal* to ward level Democratic presidential votes, or that in estimating the Assembly vote in uncontested wards I merely used the value of the presidential vote (presumably because that is how he imputes the vote in uncontested districts in his own analysis; deposition page 83).

That is wrong. I displayed this graph merely to show that there is in fact a strong relationship between the two variables. The fact that the Democratic Assembly vote tends to fall below the presidential vote is completely irrelevant to any possible bias. In fact, regression analysis estimates the relationship between the two quantities by identifying the *slope* of the line that relates them, not how the relationship varies across a 45-degree line.

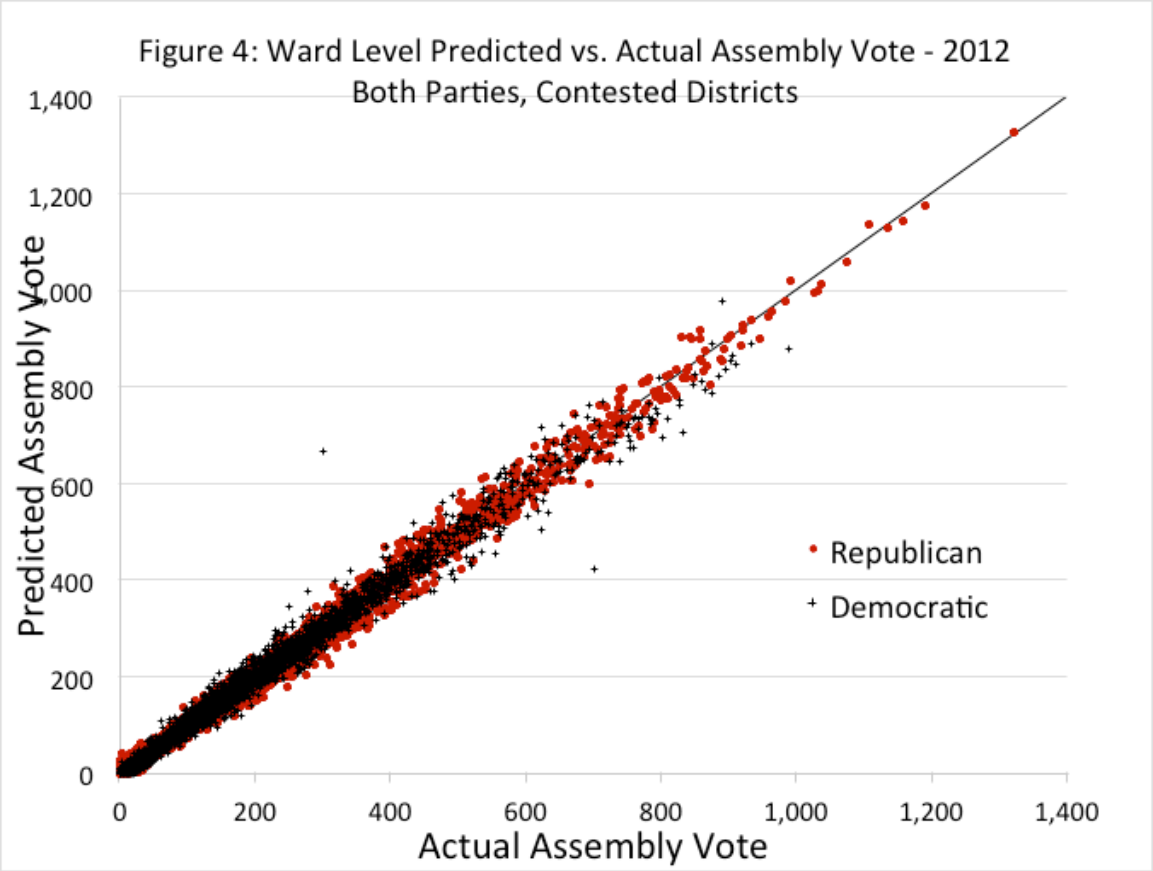
Below (Figure D) is a graph that plots the data in Figure 3 of my original report along with a fitted line of predicted values from a bivariate regression of the Democratic Assembly vote on the Democratic presidential vote. The red line consists of the predicted values of the Democratic Assembly vote in each ward:



Here, we see that the fitted line runs *exactly* down the middle of the plotted points. My regression analysis of the Democratic Assembly vote (Table 1 in my original report) shows that the coefficient for the Democratic presidential vote is 0.931 ($p<0.0001$), which is precisely the pattern than we see in the bivariate relationship above. In a linear model, this coefficient is the

slope of the line that relates the presidential vote to the assembly vote. It is less than 1 (a 45-degree line), indicating that the Assembly vote rises more slowly than the presidential vote; i.e., the predicted Assembly vote will lie below the 45-degree line in Figure 2.

And, as is immediately apparent from the actual results of my regression (Figure 4 in my original report, which plots the actual vs. predicted ward level votes), there is no bias in the results. In this graph, the 45-degree line is where the *predicted* Assembly vote would fall if it were exactly equal to the actual Assembly vote:



Trende's criticism on this point is utterly misinformed. No one with a solid understanding of quantitative methods or regression analysis would have made it.

IV. Trende's Claim That My Efficiency Gap Calculations Ignore Incumbency, Candidate Quality, and Campaign Spending

In paragraphs 140-143, Trende criticizes my efficiency gap calculations for failing to take into account factors that can affect election results, such as get-out-the vote drives, candidate quality, recruitment, and campaign spending.

Trende offers no evidence that these factors would actually have a material effect on my estimates if I had more directly taken them into account. And he ignores the fact that any

estimation of the results of a hypothetical district plan utilizes baseline estimates that, in effect, average out the effects of these factors (Gelman and King 1990; 1994). That is to say, my regression model *does* implicitly incorporate these factors, in its analysis of the relationship between the presidential vote (where none of these variables will affect the vote) and the Assembly vote (where they are all incorporated into the estimates).

Moreover, Trende's criticism overlooks the point that my model is based on precisely the same information that the authors of Act 43 considered in estimating the likely partisan effects of the new districts. In particular, Gaddie's analysis of the partisan effects in the new Act 43 districts was functionally equivalent to mine and based on exactly the same considerations.

Like his complaints about alleged bias in the regression analysis that I discuss above, Trende's criticism is uninformed and betrays a lack of knowledge of how hypothetical district plans are evaluated.

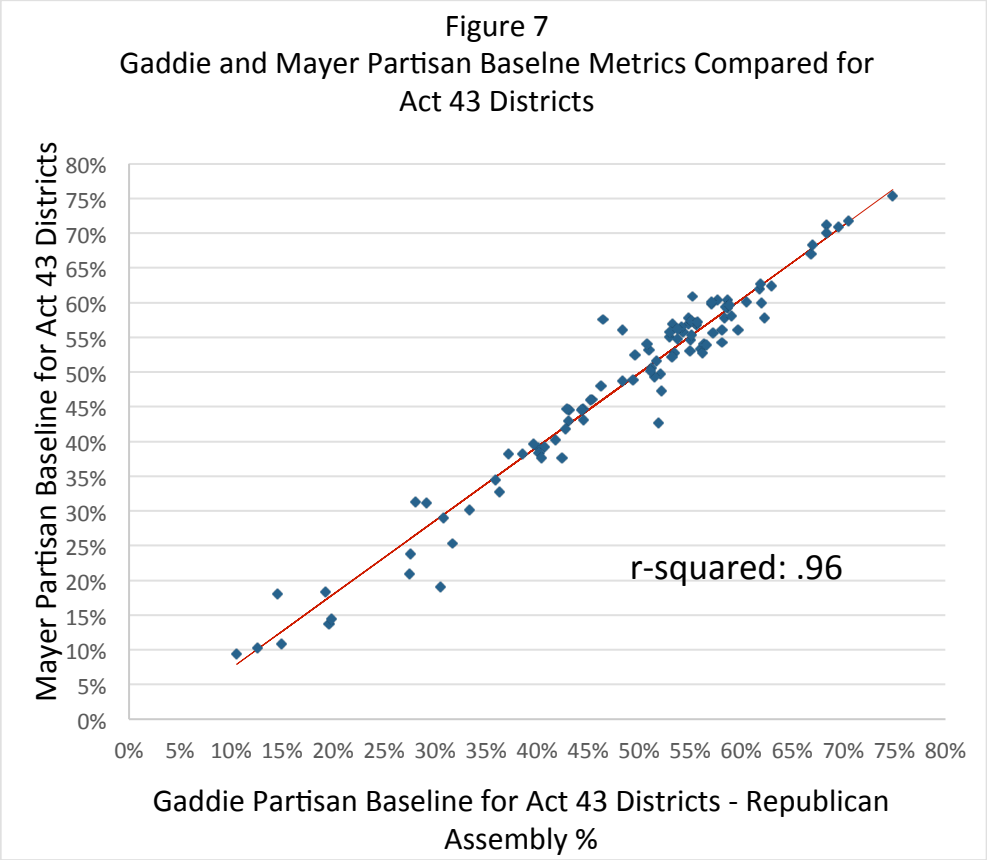
V. Goedert's Claim That My Efficiency Gap Calculations Incorporate Information Not Available to Act 43's Designers, and Ignore the Effects of Incumbency

Goedert criticizes my analysis for incorporating information that map drawers did not have (2012 election results), and for ignoring information that map drawers would have taken into account (incumbency in particular).

The first criticism is incorrect, as Act 43's designers in fact had information functionally equivalent to the 2012 election results in their possession, in the form of Gaddie's Act 43 district level estimates. These estimates, like my own, are baseline measures of partisanship, and they correlate almost perfectly with my results (r^2=0.96). In his deposition, Gaddie described in detail his method, which like mine assumed that all seats would be contested and that no incumbents would run (Gaddie Deposition, pp. 197, 198, 201, 202, 204):

> Let's suppose we have a seat with an incumbent and a seat without an incumbent and each one has an Assembly election. The party of the incumbent is presumably going to do a little stronger in the district where they have an incumbent than in an open seat. So I can't really take -- Let's suppose I move precincts from the open seat into that incumbent seat. I can't really take those open seat Assembly votes, add them, compare them to the percentage for the incumbent running for the same party, get an accurate estimation of the partisanship and the competitiveness of the district. So we attempt to create a substitute measure. Statewide elections are held in all precincts, they're held in all constituencies, so one thing that we often do is we do what we call reconstituted elections, or proxy elections, where we'll take one election or a composite of elections, like I described previously, and attempt to create some measure of partisan competitiveness, an expected vote or what we call a normal vote, what the vote would usually do without an incumbent in the district." (Gaddie Deposition, pp. 204-5)

To highlight the similarity between Gaddie's pre-2012 estimates and my own estimates using 2012 election results, below is a graph plotting the two sets of data (Figure 7 in my original report, p. 30):



This graph shows that the information the Act 43 authors relied on when drawing their map (the Gaddie estimates) and my estimates, are nearly identical. This is largely because they are both estimates of the same underlying quantity – the baseline partisanship of a hypothetical Assembly district. Goedert dismisses the nearly perfect correlation as "mostly coincidental" (p. 17), but offers no analysis or data to support this conclusion. It is simply an assertion offered without evidence.

And it is an entirely unpersuasive assertion for the additional reason that election results in Wisconsin (and in most states) are extremely highly correlated from one election to the next. For example, Wisconsin's counties remained geographically constant between 2008 and 2012, and Trende supplied information about the presidential vote in each county in each of these years. The 2008 county level presidential vote and the 2012 county level presidential vote are almost perfectly correlated (r^2=0.96), indicating that it would make no difference whether Act 43 was assessed using the former or the latter.[12] Either way, the same conclusion would follow: that

[12] Ward level 2008 and 2012 results cannot easily be compared because ward boundaries were redrawn after the 2010 Census.

the map is an extreme Republican gerrymander, and that the authors of Act 43 had information in their possession that predicted it.

Second, Goedert claims that map drawers do not ignore incumbency when drawing maps. That will generally be true when map drawers are trying to figure out which incumbent should be included in which district. But when it comes to estimating the likely partisanship of the new districts, ignoring incumbency (that is, controlling for it) is precisely what the drawers of Act 43 did, as Gaddie noted in his description of his methods. This approach is sensible since incumbents can be defeated, retire, run for higher office, or switch parties over a plan's decade-long lifespan. A map's authors will typically want to ensure that their projections do not depend on particular incumbents continuing to run in particular districts.

In any event, *including* incumbency in no way changes my substantive conclusions about Act 43 or the Demonstration Plan. I recalculated the efficiency gap for both maps, using my baseline partisan estimate and then incorporating incumbency into the model. For Act 43, I used the actual incumbents who ran in the plan's districts, with the adjustments noted in my report to account for paired incumbents and those who lost in primaries (p. 18, footnote 14).[13] For my plan, I geocoded incumbents' home addresses[14] and then identified which districts had incumbents residing in them using Maptitude for Redistricting. Table E shows the resulting efficiency gap calculations, and compares them to the open seat baseline I generated in my report:

Table E

Efficiency Gap Calculations

with Incumbents

	Demonstration Plan	**Act 43**
Baseline Efficiency Gap	2.20%	11.69%
Efficiency Gap with Incumbency	3.71%	13.04%

The efficiency gap increases marginally for both plans (by 1.5% for the Demonstration Plan and 1.4% for Act 43), in large part because there were more Republican (50) than

[13] I recalculated vote estimates using predicted values of Democratic and Republican Assembly votes when one of the parties had an incumbent running.
[14] This information was provided to me by counsel.

Democratic (24) incumbents running in 2012. With twice as many incumbents, Republicans will win more seats than in the open seat baseline even though the Republican vote percentage remains below 50% in both cases. It is thus apparent that taking incumbency into account has no effect on my conclusion that Act 43 was an egregious partisan gerrymander; the substantive inferences are identical, with or without incumbency.[15]

VI. Goedert's Claim That I Did Not Perform Sensitivity Testing for Act 43's or the Demonstration Plan's Efficiency Gaps

Goedert criticizes the efficiency gap calculations for both Act 43 and the Demonstration Plan, arguing that I "provide no estimates for the efficiency gap of the demonstration plan under the range of plausible election outcomes facing legislators at the time they were drawing the map" (p. 16), and that I conduct no "sensitivity testing" of my calculations of Act 43's efficiency gap.

I note that Goedert has not provided any actual analysis showing that this sensitivity testing would have materially altered my conclusions, or even any citations showing that such testing is necessary to evaluate the adequacy of my calculations.

Still, it is possible to show that my calculations are robust to significant changes in the electoral environment. Using Jackman's historical estimates of the statewide Assembly vote in Wisconsin, I can determine the plausible variation of the overall vote over the course of a decade. Since 1992, the statewide Democratic percentage of the Assembly vote has ranged from a high of 54.6% (in 2006) to a low of 46.4% (in 2010). The Democratic share of the statewide vote in 2012 was 51.2% in my baseline calculations, which suggests a plausible range of -5% to +3% in conducting a sensitivity analysis. In effect, this approach asks whether Act 43's and the Demonstration Plan's efficiency gaps would be durable in the face of massive Democratic *or* Republican waves – an extremely rigorous test that exceeds what is normally found in the literature.

Following Goedert's method of applying a uniform swing (p.21), I can estimate the effects that these swings will have on the efficiency gap, both for Act 43 and for the Demonstration Plan. To maintain consistency and to address his concern that I did not incorporate incumbency in my baseline, I estimate the effects using the incumbent baseline (that is, including the incumbents who ran in 2012).

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[15] We can use these calculations to determine how many more Democratic legislators would have been elected in 2012 if either the Demonstration Plan, or a plan with an efficiency gap of exactly zero, had been in place. Under the open-seat baseline, 9.49% more Democrats would have been elected under the Demonstration Plan (11.69% - 2.20%), and 11.69% more under a plan with an efficiency gap of exactly zero. Similarly, under the incumbent baseline, 9.33% more Democrats would have been elected under the Demonstration Plan (13.04% - 3.71%), and 13.04% more under a plan with an efficiency gap of exactly zero. In all cases, these are very large differences, amounting to anywhere from nine to thirteen Assembly seats.

The results are shown in the following two tables, the first for the Demonstration Plan (Table F), and the second for Act 43 (Table G). For the Demonstration Plan, the efficiency gap remains well below the plaintiffs' suggested 7% threshold, even when the statewide vote reaches the most extreme values either party has seen over the last three decades. Specifically, the efficiency gap goes to 3.9% in the event of a Democratic wave akin to that of 2006, and to -2.0% if a Republican wave like that of 2010 occurs. For Act 43, however, the efficiency gap remains extremely large and above the threshold at all times, ranging from 10.7% in a Democratic wave to 8.8% in a Republican wave. Moreover, the sensitivity testing shows that even if the Democrats obtained over *54%* of the statewide Assembly vote – equal to their best performance in a generation – they *still* would not capture a majority of the Assembly, gaining only 48 seats. Act 43's gerrymandering thus effectively insulates the Republican Assembly majority from all plausible shifts in voter sentiment.

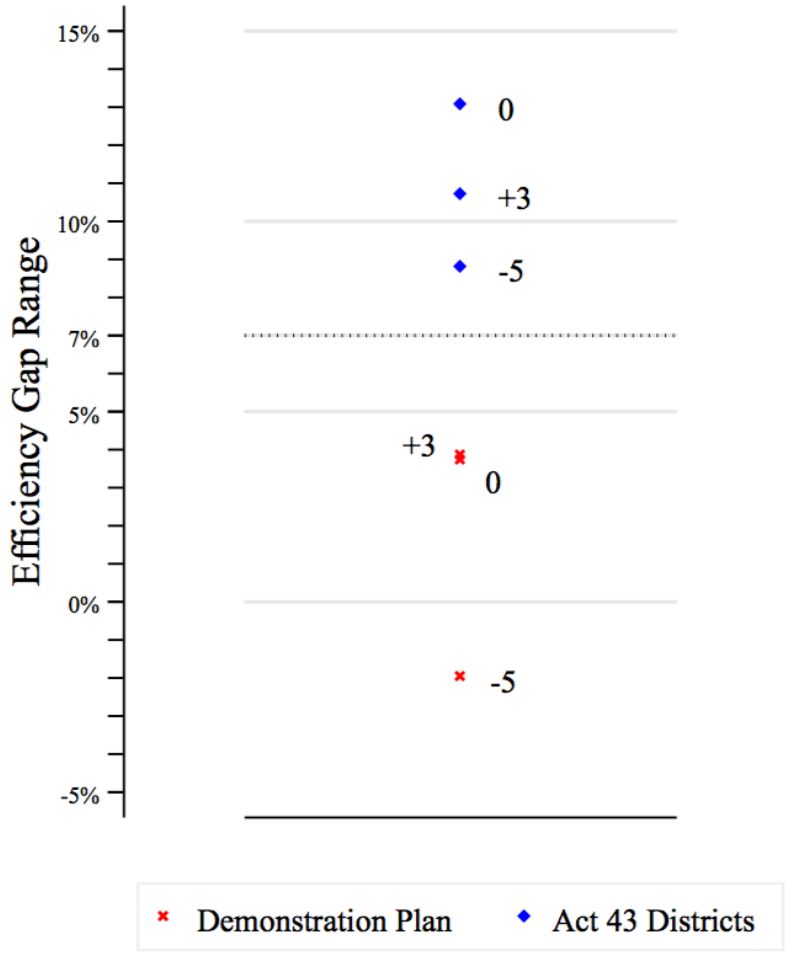
Table F
Efficiency Gap Estimates, Uniform Swing
Demonstration Plan

	D Minus 5	**My Plan Incumbent Baseline**	**D Plus 3**
party split (R-D)	51-48	48-51	43-56
Rep share of Seats	52%	48%	43%
Wasted Republican Votes	737,557	659,821	659,390
Wasted Democratic Votes	681,900	765,561	769,546
Gap	(55,657)	105,740	110,156
Total Democratic Votes	1,336,168	1,484,631	1,573,709
Total Republican Votes	1,502,745	1,366,132	1,284,164
Total Votes	2,838,913	2,850,763	2,857,873
Efficiency Gap (gap/total votes)	**-1.96%**	**3.71%**	**3.85%**

	Table G Efficiency Gap Estimates, Uniform Swing Act 43 Districts		
	D Minus 5	**Act 43 Actual**	**D Plus 3**
Party Split (R-D)	64-35	60-39	51-48
Rep share of Seats	65%	61%	52%
Wasted Republican Votes	585,668	504,553	560,840
Wasted Democratic Votes	835,968	876,153	866,725
Gap	250,300	371,600	305,885
Total Democratic Votes	1,316,158	1,462,397	1,550,141
Total Republican Votes	1,527,115	1,388,286	1,304,989
Total Votes	2,843,273	2,850,684	2,855,130
Efficiency Gap (gap/total votes)	**8.80%**	**13.04%**	**10.71%**

Figure E below shows these results graphically: the red x's are the efficiency gap estimates for the Demonstration Plan, and the blue diamonds the estimates for Act 43. The dotted line is at plaintiffs' suggested threshold of 7%. The figure clearly demonstrates that even across huge partisan swings, the efficiency gap under Act 43 remains very large, and the efficiency gap for the Demonstration Plan remains very small. This is further powerful confirmation of the durability of Act 43's bias – and the durable *lack* of bias of the Demonstration Plan.

Figure E: Sensitivity Analysis
-5 to +3 Democratic Swing



VII. **Conclusion**

In their criticism of my report, both Trende and Goedert offer nothing but supposition, speculation, irrelevant discourse about Wisconsin political history, extraneous discussion of congressional redistricting in other parts of the United States, wildly inapposite and inaccurate conjecture about the geographic concentration of Democrats as a possible source of the pro-Republican bias of Act 43, unreliable methodologies, and minor quibbles that have no consequences for my conclusions. Neither Trende nor Goedert has conducted any valid analysis of either Act 43 or the Demonstration Plan – in fact, they make no mention at all of the specifics of the Demonstration plan.

Most significantly, nothing in their reports undercuts my fundamental conclusion that Act 43 constituted an egregious and durable gerrymander, and that it was entirely possible to draw a neutral map that met or exceeded Act 43 on all legal dimensions. If anything, the sensitivity

testing substantially bolsters this conclusion, since it shows that Act 43's large efficiency gap and the Demonstration Plan's small one are durable in the face of enormous changes in Wisconsin's electoral environment.

Dated: December 21, 2015

/s/ Kenneth R. Mayer

Kenneth R. Mayer, Ph.D.

Department of Political Science

University of Wisconsin-Madison

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