In The Matter Of:

William Whitford, et al., vs.

Gerald Nichol, et al.

Deposition of SIMON JACKMAN

March 16, 2016

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IN THE UNITED STATES DISTRICT COURT
FOR THE WESTERN DISTRICT OF WISCONSIN

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WILLIAM WHITFORD, et al.,

Plaintiffs,

-vs- Case No. 15-cv-421-bbc

GERALD NICHOL, et al.,

Defendants.

= =

Deposition of:

SIMON JACKMAN

Madison, Wisconsin
March 16, 2016

Reported by: Lisa L. Lafler, RPR, CRR, CLR

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(Attached to original transcript with copies provided to Mr. Keenan and Ms. Greenwood)

(Original transcript filed with Mr. Keenan; copies provided to Mr. Keenan and Ms. Greenwood)

DEPOSITION of SIMON JACKMAN, called as a witness, taken at the instance of the Defendants, under the provisions of the Federal Rules of Civil Procedure, pursuant to Notice, before Lisa L. Lafler, a Registered Professional Reporter, Certified Realtime Reporter, Certified Livenote Reporter, and Notary Public in and for the State of Wisconsin, at the State of Wisconsin Department of Justice, 17 West Main Street, City of Madison, County of Dane, and State of Wisconsin, on the 16th day of March, 2016, commencing at 9:09 in the forenoon.

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SIMON JACKMAN,
called as a witness, being first duly sworn, testified on oath, as follows:
(Exhibit No. 56 marked for identification)

EXAMINATION

BY MR. KEENAN:

Q. Good morning. Professor Jackman, as you remember, I'm Brian Keenan. I'm an attorney for the defendants in this case.
You're here for a second deposition. Since you just had a deposition a few months ago, I'm not going to go over all the preliminary stuff in great detail, but I will say that if you don't understand a question I ask, please make sure to let me know and I'll try to rephrase or we can have the court reporter repeat it. Do you understand?
A. I do.
Q. And then just as a reminder, to respond verbally with yes-no answers and try not to cut me off in my question, I'll try not to cut you off in your answer, so we can get a clean transcript. Do you understand?
A. I do.

Q. Good. So what did you do to prepare for this deposition?
A. After the creation of the rebuttal report, I came to Madison yesterday and we had a meeting with the team to my right here in a building not too far away from here.
Q. So who was at that meeting?
A. Doug, Annabelle, and Ruth.
Q. Okay. And that was it?
A. That was it.
Q. And then how long did that meeting last?
A. Net of lunch, approximately four hours.
Q. We have marked what's been marked as Exhibit 56. I'll give you a copy.
A. Thank you.
Q. If you could just identify what that document is for the record.
A. This is a copy of my rebuttal report.
Q. So I thought we would just go into the report and I'll ask you some questions as we go through it. So if you could turn to page 3 -- and I'm skipping the introduction because I think we'll get to those things during the body.
So we'll start with Section 1, responses to Goedert's criticisms, and the first -- your

paragraph starting "First." Focus on that for now. So you criticize Professor Goedert for believing that a plan's efficiency gap is only relevant to the extent it sheds light on the partisan intent; is that correct?
A. I criticize Professor Goedert for equating the efficiency gap -- or large values of the efficiency gap with partisan intent.
Q. And that's a word that will probably come up, partisan, like partisan gerrymandering is what this case is about.
So you would agree that partisan intent behind a mapmaker's decision cannot be inferred from a large efficiency gap?
A. Not necessarily.
Q. And that would include a large efficiency gap in one election and also a large efficiency gap across all the elections in a plan?
A. Yes.
Q. And you would agree with me that a plan's efficiency gap says nothing about how the mapmakers adhere to traditional districting principles?
A. That's a slightly broader question. There, I think, the set of what we define as traditional

redistricting principles could determine whether I'd say yes or no. But more narrowly on the question of intent, I think our position or my position in response to Goedert is clear. But, I think, I would want to, perhaps, talk about specific redistricting criteria under connection two, the efficiency gap, to answer that question.
Q. Sure. So would the efficiency gap measure how closely a mapmaking body adhered to keeping communities of interest together in the same district?
A. Not necessarily.
Q. And would it measure how a mapmaking body performed on measures of compactness?
A. Again, I'm going to answer not necessarily. It -- X would lead to Y, meaning it's easy to conceive of situations where ignoring compactness, say, or something like that could lead to higher or lower values of the efficiency gap. But the backward inference, observing a higher value or low value of the efficiency gap and then making that inference on its face, the efficiency-gap number, you would want additional information in order to draw such an inference.
Q. Okay.

A. Or at least I would.
Q. And your research shows that large efficiency gaps occur in the absence of any partisan intent. That's correct?
A. No. That's not correct. I -- my research did not -- it was irrelevant to whether -- I -- I computed values of the efficiency gap putting questions of partisan intent completely to one side. I paid no attention to that; certainly, at the time of my initial report, yeah.
Q. Exactly. And the results of your research reveal that large efficiency gaps occur in plans that were enacted with no partisan intent?
A. I'm not in the position -- I don't know what the partisan intent was. So I can't answer that question.
Q. Okay.
A. Yeah.
Q. You would agree that large efficiency gaps occurred in plans that were not enacted under unified partisan control?
A. I'm aware of, if we may cut to the chase, one instance in this state where a court-drawn plan did yield a large value of efficiency gap.
Q. And that was Wisconsin in the 2000's decade?

A. The cycle immediately preceding the plan at issue, yeah.
Q. Your report criticizes Dr. Goedert for not understanding that the efficiency gap is a measure of partisan effect, not partisan intent; is that correct?
A. That's a fair paraphrase.
Q. And why is it your opinion that a large efficiency gap should be a problem when a map is enacted with partisan intent but not when it's enacted with no partisan intent?
A. I think the question of whether intent itself is a trigger for judicial scrutiny is beyond my area of expertise. What I can testify to is a large efficiency gap, though, is certainly evidence of partisan -- systematic, rather, partisan advantage one way or the other, and on that basis, it is something that a court might be interested in.
Q. And that systematic partisan advantage, though, would exist in a state that had a high efficiency gap regardless of the intent that went into enacting the plan?
A. Well, again, that's right. That's right. I would agree with that.
Q. Moving on to the paragraph starting "Second,"

we'll go in order here so hopefully --
A. Okay.
Q. -- this will be logical. You say that, "The appropriate universe for plaintiffs, defendants, and courts is limited to the first elections held under plans." Why do you say that?
A. That is -- it would seem to me that's the operative moment to go to court, as it were, or to begin the process of judicial scrutiny. It's possible you might even begin the process of scrutiny with zero elections, right? The plan was just a plan at that point, perhaps, passed by the legislature, but we're yet to see an election generated underneath it. Seems to me you could -- you could do that.
But the thing about the first plan is that now we have a piece of data generated from the actual plan as it is operating, and it seems to me it's not -- you know, the idea that we would wait for two or three elections under the plan so as to build a more reliable picture of how the plan is performing seems sort of unrealistic. At that point, we're closer to the end of the plan than the beginning and any damage, if you will, or partisan advantage manifest in the plan is being

-- the effects of that are being felt and any harm is being felt.
So it would seem to me that the appropriate moment might be when we've seen one election from the plan. That -- that's probably, I think, hitting the sweet spot between uncertainty as to what the plan will do over the rest of the decade -- over the elections we will observe over the rest of the decade under that plan, if allowed to stand, versus I think the -- the more speculative exercise of taking a plan to court.
And particularly under this criteria, we haven't seen an election yet so we don't know what its efficiency gap is, or if we did, we would be engaged in, I think, a more speculative exercise. So that's why I think the appropriate number in terms of triggering litigation is -- is that one election, that first election.
Q. But, obviously, you'd agree that's just one piece of data about the plan?
A. I do.
Q. And a plan -- you'd agree that a plan would produce a range of results over its lifetime under different electoral conditions, correct?
A. And, indeed, that was considered at great length

in my original report. That's right.
Q. Yeah. Now, is there any particular reason why the -- sorry. Strike that question.
Do you think it's relevant in looking at the number of elections that exceed a particular efficiency-gap threshold in any election under a plan is at all relevant in determining the usefulness of the efficiency gap as a standard going forward into the future?
A. I think that -- that would -- I think there are two senses of the word "threshold" that I'd want to keep distinct. So it's the value we observe -- the value of the efficiency gap that we observe in the first election held under the plan, and we've talked about that being a trigger for judicial scrutiny. And then there's a second sense of the word "threshold," and that is, what is the -- you know, what values of the efficiency gap are we observing in the second, third, fourth?
So I -- so -- so one -- if I were to answer -- the best answer to your question might be to say that conditional on the first election under the plan triggering the threshold that we've promulgated as -- as should apply to those -- that set of first elections. It is, indeed, a

pertinent question to ask what is the behavior of the efficiency gap over -- over the life of the plan; and then, indeed, the question that I concerned myself with in my original report was whether that subsequent sequence of efficiency-gap values lay on the same sign of zero that was -- it was either negative or positive, had the same sign indicating the direction of partisan advantage as we observed in that first election.
So that's, I think, the probative value, if you will, of the sequence of values we observe in elections two, three, four, and five put up against the value we observed -- or the efficiency gap we observe in election one.
Q. And your analysis has examined historical elections under plans and looked at the first election that actually happened under that plan; is that correct?
A. That is correct.
Q. And then analyzed the future elections based on the efficiency gap observed in that first election?
A. Correct.
Q. Okay. Now, for plans that have actually had a chance to run their full course, you've been able

to examine plans from the 1970s, '80s, '90s, and 2000s; is that correct?
A. That's correct.
Q. So the majority of these first elections would have been in 1972, 1982, 1992, and 2002?
A. Yes, and 2012 we have a couple there as well.
Q. Okay. But in the 2012 --
A. Yeah, I know.
Q. -- we haven't been able to see the full results over a full ten-year period, right?
A. Gotcha. Gotcha.
Q. And just looking at Wisconsin in the 2000's decade, the first efficiency gap observed in 2002, I believe, was a negative 7 and a half about; is that --
A. I -- I'd want to look at my original report.
Q. Sure.
A. I think I've got that exactly there. Do you mind? Thanks.
Q. Mr. Jackman's original report was marked as Exhibit 11 previously, and he's referring to a copy of it here.
A. So you asked me about which election?
Q. 2002.
A. Yeah. The estimate of the efficiency gap for

Wisconsin in 2002 is negative 0 -- a negative 0.075.
Q. And that's a good topic. You like to refer to things in proportions; is that correct?
A. Oh, I -- I'm happy to call that minus 7.5. We can multiply by 100 to stop all the decimals and zeroes in the transcript if that's --
Q. It's fine to do it the way you want. I just wanted to establish that negative 7.5 is the same thing as negative 0.075.
A. That's right.
Q. My mind works in percentages.
A. No. No. That's --
MR. POLAND: Just so we can be clear about if we're talking percentages, if we're actually talking decimal points.
MR. KEENAN: Yeah.
THE WITNESS: Sure.
Q. And you were referring to Figure 35 on page 72 of your report?
A. Correct. I was reading -- literally reading that data point off the graph, yeah.
Q. And so when Wisconsin's 2000's plan is analyzed -- when you analyze that plan in your -- in your work, that's treated as a plan that has a negative

7.5 percent efficiency gap in its first election?
A. (No verbal response.)
Q. Is that correct?
A. Correct.
Q. Now, we know that the plan, though, also went on to produce a variety of results, correct?
A. That is correct.
Q. So what were the other efficiency gaps observed in Wisconsin's 2000's plan? We can go in order.
A. Sure. Again, reading off the graph, in 2004, it's close to negative 10 percent. In 2006, it's approximately negative 12 percent. In 2008, it's approximately negative 5 percent. And in 2010, it is approximately negative 4 percent.
Q. Okay. So we have a range from negative 4 to negative 12; is that correct?
A. That is correct.
Q. Now, in your analysis, is there any particular political science reason why negative 0 -- or negative 7.5 percent was the result that was -- happened to be seen in 2002?
A. No. There's nothing from the literature per se that -- that led me to -- oh, you mean the value per se?
Q. Yeah.

A. I'm sorry. I misunderstood the question. Could you ask it again?
Q. Sure. In 2002, Wisconsin saw a negative 7.5 percent efficiency gap. Is there any particular reason why 2002 saw that number of efficiency gap?
A. There's -- no. There's nothing in the literature that would -- would look at a given election and make a -- a -- a sharp prediction other than to say the precise value we would probably not be able to predict, but there's analysis around to suggest that depending on prevailing conditions, you know, in particular who drew the plan, we might -- we might form expectations as to whether we're going to see one side -- you know, positive or negative efficiency-gap values.
Now, I note that in this plan -- this was a plan that was drawn by a court. So, in this case, we wouldn't have particularly strong expectations as to what the sign nor the magnitude of the -- of the first efficiency gap that we see under the plan.
Q. And you'd agree that the plan could conceivably produce an election anywhere from negative 4 to negative 12 percent efficiency gap? The Wisconsin

2000's plan could have produced an efficiency gap anywhere from negative 4 percent to negative 12 percent depending on the electoral circumstances?
MR. POLAND: I'm going to object to the form of the question.
Q. Well, you'd -- let me re -- you'd agree that the Wisconsin 2000's plan was capable of producing a range of results; is that correct?
A. We observed that it, in fact, did.
Q. And, in fact, it did produce negative 4 to negative 12 percent; is that correct?
A. That's correct.
Q. So before the 2012 -- or 2002 election, no one knows what the efficiency gap's going to be, correct?
A. Not with any great precision.
Q. Okay. And so it happened to produce an efficiency gap of negative 7.5 percent. That's correct?
A. That's correct.
Q. But it was capable of producing efficiency gaps that were perhaps as low as negative 4 percent or as high as negative 12 percent. That's correct?
MR. POLAND: Object to the form of the question.
THE WITNESS: You're asking -- do

you want me to answer all the same?
MR. POLAND: Well, it's up to you. I just objected to form. It's just an objection. If you can answer, you can answer.
THE WITNESS: Okay.
A. It -- okay. So it did, indeed, produce that -- that range of values. The value of the first one, we -- we didn't have a -- you know, it would be an interesting analysis to engage in. We've got a little bit of that in the rebuttal report. But certainly at the time I was -- at this stage of my investigation of the efficiency gap, I was not engaged in that exercise nor has it been a particularly strong focus of my work on the efficiency gap thus far.
Q. But under your analysis that you've performed, had the 2010 election result occurred in 20 -- 2002, the Wisconsin plan would present itself as an initial plan with a negative 4 percent efficiency gap; is that correct?
MR. POLAND: Object to the form of the question.
A. It's -- it's a -- it's a -- it's a bit counterfactual for me to try to grasp, frankly.

Had everything that produced the 2010 election holding constant the district lines, which were held constant, would -- would we have seen the same efficiency-gap number? I -- I -- that's a rather speculative counterfactual I'm -- I'm sort of being asked to entertain there and one that I'm not quite sure I can -- I can -- I can answer with any great confidence or precision.
Q. Okay. So you understand that you're -- the standard you're presenting is being asked to be applied by courts that would go into the future, correct?
A. I do.
Q. So it would apply to the 2020 round of redistricting if it was adopted by the courts?
A. Yes.
Q. Okay. And so do we know what type of election's going to occur in 2022?
MR. POLAND: Object to the form of the question. The "type of election" is vague.
A. Are you asking me --
Q. Yeah. Do you know -- we don't know what percentage of the vote the Democrats versus the Republicans are going to get in 2022?

A. No, we don't.
Q. We don't know whether it's going to be a 50/50 election or a wave election one way or the other?
A. I'll -- I'll -- I'll accept what we mean by "wave election" there, but -- but -- what we might mean by wave election there, but, no, we don't know the exact vote share that Democrats or Republicans will get in the 2022 Wisconsin state election.
Q. And that would be the election that would trigger judicial review under the standard that you're advocating?
A. Or may not.
Q. Sure. Yes. It would be the election which determines whether there's judicial review or not?
A. If -- if the standard were adopted and if it tripped the -- the proposed standard.
Q. And before a plan -- there's an election under a plan, is there a way that people can know what type of election's going to occur in the first election under a plan?
A. Well, I -- again, in answer to an earlier question, this is the election -- zero-elections problem. All we have are the plan boundaries. We're yet to see an election conducted under the plan's boundaries. I can imagine a research

agenda that would try to forecast efficiency-gap estimates based on some kind of statistical modeling or based on some sort of forecast as to what we thought was going to happen statewide, what was going to happen seat by seat, taking into account factors like incumbency, or what -- you know, on my feet I can think out loud about what such a research program might look like. But at the end of the day, that would be -- it would be a lot of modeling and it would be considerable uncertainty attaching to any capitalization of the plan before we've seen a real actual election conducted under the district lines.
Q. The first election's just going to be one data point about the plan though, correct?
A. It is one data point. It is one value of the efficiency gap.
Q. And the potential efficiency gaps are going to span a range of possibilities, correct?
A. That's correct.
Q. And is there a way to determine where along the spectrum of that range the first efficiency gap -- the experience under a plan is, on the high end, the low end, or the middle?
A. Before we see it?

Q. Well, after the first election.
A. Oh, after we see it. Yes. We could then look at how it lined up with the now considerable several hundred values of the efficiency gap that we've seen if -- indeed, first election under the plan efficiency gaps that we've now seen from the historical analysis.
Q. So you'd have to refer back to your historical analysis of the prior decades; is that correct?
A. I would, yeah.
Q. Okay. If we move on to the next paragraph in your report -- and you can keep the other report handy just in case you need to refer back to it.
A. Sure, certainly.
Q. There's some discussion of the differences in durability between pro-Democratic efficiency gaps and pro-Republican efficiency gaps; is that correct?
A. That's correct.
Q. Do you have an opinion as to why the efficiency gap shows that Republican plans are more durable than Democratic plans?
A. I don't have a well-formed hypothesis as to why that is the case. The most obvious one that comes to mind is Caprice, that -- that -- that first

value we got is a draw from a distribution that lies actually closer to zero and that those relatively small number of cases where we do see an apparent pro-Democratic advantage in the first election. When the plan is allowed to run its course, we learn that, in fact, that, on average, it tends to be the case that there's no systematic or long-run advantage to Democrats. So that would suggest that the relatively few -- as I said, in the relatively few instances we're seeing such a positive pro-Democratic first value of the efficiency gap, it -- it -- that's why they're not durable or as durable as the ones we see on the other side, yeah.
Q. So why are then the Republican -- pro-Republican advantages more durable than the Democratic advantages seen?
A. The hypothesis that you -- the conclusion that you're sort of led to is that Republican plans, plans that are generating Republican advantage, are consistent with -- they were drawn that way. They're producing the results and they were designed to -- to do so, certainly consistent with our argument, let's say, you know, dispositive with respect to partisan intent -- we've been down

that road -- but it would seem to be consistent with there being a systematic Republican advantage in more plans, particularly in the '90s, 2000s, 2010s period than in the earlier period.
Q. Is it that Republicans are better at gerrymandering than Democrats?
A. I'd resist, perhaps, that exact form of words for what's going on, but something like that might -- might be the -- might be the case, that the -- that the plans that are being drawn to -- that generate Republican advantage are -- yes, have been done, perhaps, more strongly, more systematically. Maybe that does that up better.
Q. Do you have any opinion on whether the underlying political geography on which any map is going to be drawn just happens to be more favorable to the Republicans than the Democrats regardless of who's drawing the lines?
A. I try to resist -- we talk about political geography, but it's not geography in the sense of lakes and rivers and mountains. Political geography arises through the very exercise that we're scrutinizing here, and that is, line drawing, right? We break up states into districts. We note that some districts after that

exercise tend to be more Democratic or more Republican in their election results or other data that might point that way. But I -- I try not to put -- it's almost putting the cart before the horse a little bit to say -- at the same time I'm being asked to examine properties of a -- of a districting system to then ask about was there some underlying, quote, political geography that made it the outcome the way it had to be? It's -- you know, I'm sort of conflating the sort of cause and consequence there.
Q. Sure. And maybe the term "political geography" might be poor.
But what about the distribution of a party's voters throughout the state? Is there any -- do you have an opinion on whether a particular party's voters are more advantageously distributed throughout the state to the other party?
A. Well, what I do know is that's a very active area of debate inside political science and, in particular, among political scientists interested in redistricting. But -- but my position would be to say that, you know, in particular, the words "natural political geography," I tend to bristle at that. The whole point of the exercise is -- is

that the lines subject, you know, constraints -- legal and sometimes and traditional redistricting criteria, that does impose constraints on line drawers, but line drawers also have many, many degrees of freedom to produce the districts they do.
And we have it -- you know, I've done some subsequent analysis that suggests, perhaps, one of the biggest drivers of the efficiency gaps that we observe is who controlled the redistricting process, not so much -- that would suggest that that's -- that's an incredibly important predictor more so than anything to do with the speculation about the distribution of partisans through -- through -- through the state.
Q. And the analysis you just referred to, that's contained in your rebuttal report?
A. It is.
Q. So we'll get to that later.
A. Okay.
Q. We'll talk about that.
But based on your testimony, your analysis has only looked at the results of the elections that have been seen and hasn't factored into account at all the potential distribution of

partisans in a particular state?
A. No. I -- I -- no. That's -- no.
Q. A little bit ambiguous, but --
A. No.
Q. Your analysis just looked at the results seen in various elections. That's correct?
A. Yes.
Q. And it doesn't go back and try to adjust anything to establish any sort of like baseline efficiency gap that would be expected under traditional districting principles?
A. I did not consider alternative plans.
Q. And it measures all plans against a baseline of zero efficiency gap?
A. No. It -- it -- it computes the efficiency gap election by election; and it could be positive, it could be negative, but there's nothing magic about zero. It didn't -- zero didn't play any role in -- in my analysis.
Q. Why do you say that?
A. In the sense that -- it's not like I -- I -- we compute an efficiency-gap number for each election. Some are positive, some are negative. We just let literally the chips fall where they may and observe the distribution of efficiency-gap

values afterwards. But there's nothing -- and zero -- as a theoretical matter, a zero efficiency gap does have a special status, right? That's a plan that shows no advantage one way or the other.

But in terms of doing my analysis, the fact that zero -- you know, the special theoretical status of a zero efficiency gap played -- played no role. It was purely an empirical investigation, an empirical investigation of -- of -- of the efficiency-gap values in that historical data set.

Q. I think we'll move on to Section 2.
A. Okay.
Q. I think maybe it would be helpful to look at the chart on page 6 --
A. Yeah.
Q. -- that talks about true positives, false positives, false negatives, and true negatives, and just have you explain -- maybe I'll just go in order.

What is a true positive for purposes of your test?
A. Okay.

MR. POLAND: So objection; vague.

Can you give him specific questions to take

him through it?

MR. KEENAN: Sure.

Q. I mean, well, first why don't you explain what you did in terms of the -- Section 2? I don't want to characterize it as a particular thing.

What type of tests were you doing in Section 2?
A. I -- okay. What I did was to put ourselves in the position of something akin to a doctor making a diagnosis, almost like a medical test; and so we observed the efficiency gap from the first election under a plan -- and that's a number. And we've also proposed a threshold; and just as you might with your doctor, your cholesterol is above a certain number, the doctor's going to do something. They will suggest you do something, perhaps.

And here it's exactly analogous, right? We are proposing that if we see a first value of the efficiency-gap line above the threshold, that such a plan would invite scrutiny. And, that is to say, if the first election under the plan exceeds that threshold, we say it has tested positive just in the same way that your blood cholesterol, for instance, has tripped a threshold.

And then we can ask about how good an indicator the actual underlying condition, -- that is, partisan advantage one way or the other -- is that test result, right? And so if over the life of the plan -- you know, there are various ways that Markham might be wrong, and the one I considered in my original report was at any point over the life of the plan in election two, three, four, or five did we see a value of the efficiency gap that contradicted the signal we got from the first election. And in such a case, we have a first election has tripped the threshold, so it has tested positive but, in fact, it is a negative case. That plan as allowed to run generated values of efficiency gap that contradicted the initial sign, and so that's a false positive, all right? So such cases would fall in the top right corner of the two-by-two table that appears on the bottom half of page 6.
Q. Maybe I can just stop you. So a false positive is a plan that triggered the threshold, but then actually went on to produce an election with an EG of the opposite sign?
A. Correct.
Q. Okay.

A. A true positive, on the other hand though, right, is now we've tripped the threshold and, indeed, the -- over the life of the plan the subsequent sequence of efficiency-gap values stayed on that same side of zero as, indeed, case in point would be the Wisconsin plan 2002 through 2010 we were discussing.
Q. And then what are the -- what's a false negative?
A. Let's talk about those. So negative is that that first election we've got a small -- in magnitude a small value of the efficiency gap, and so based on the proposed threshold, we'd say there's nothing to see here. Your cholesterol is normal, right? But then as we allow the plan to run, we -- we, indeed, observe that it produces values that are large.

And then a true -- a true negative is just the other case. It tested negative. It looked like there was nothing -- it didn't trigger a threshold in the first election and, indeed, went on to small values of the efficiency gap or even values of the efficiency gap that alternated in sign. Sometimes it looked like there was a Republican advantage. Sometimes it looked like a negative. So that's a true negative; and that is -- you know, you've got low cholesterol and turns out that was the right call. What -- we don't need to make an invention in those -- in that case.

And so this is a conventional way of looking at the behavior of any prognostic procedure that yields a binary outcome, would trip a threshold or not, positive or negative, so it admits this rather simple two-by-two classification of the possibilities, you know, the relationship between what we see with the initial test and then the underlying behavior of -- of the plan over the rest of the decade.

Q. Okay. And so just to clarify on the negative, is the negative based on a sign flip or is it based on a magnitude?

A. Being a true negative, a true negative is -- is -- let me be clear on that. Yeah. A true negative is -- it's -- it's, in fact, bouncing around. It's changing sign over the life of the plan.

Q. And so would a false negative be a plan that came in below the threshold and, thus, escaped your view but then never changed signs?

A. Well, a false -- a false negative is a case that tested negative, but that was the wrong call.

Q. And why was it the wrong call? Is it because it was the same sign throughout its existence?

A. Yeah.

Q. Okay.

A. That's right.

Q. So this is -- these positives and negatives are based on whether a change in the efficiency-gap sign occurs or not?

A. Yeah. Yeah. Describing under the columns "actual," that's what we mean, yeah, yeah.

Q. And why is the sign flip the determining factor for whether a plan should trigger the threshold or not -- or sorry. That was a poor question.

Why is the sign flip the determining factor for whether the threshold is accurately capturing the positives and negatives?

A. Yeah. The answer to that is I -- in my initial report, I seized on that -- I thought that was the -- absolute one of the most rigorous, strenuous tests we could submit the efficiency-gap measure to.

Let's take another analogy from the world of testing, one we might be familiar with. We ask here -- your kid takes a math test and scores 70 percent, say. Now we're asking not just what

will the average test score be in other math tests. You know, what does that 70 percent tell us? Now we're asking what's the probability we will ever see a score below 50, say? And that's a -- that's a -- we're asking just one election, right, taking on the other sign is enough for us to say, no, that has sent us the wrong message.

So I thought -- I thought, as I did my initial report, what's an extremely strenuous test we could submit the efficiency gap to such that -- right? Because at the end of the day what we're in the business of doing is trying to promulgate a standard here that we'd want people to be able to rely on. So we want to have pretty high confidence that when we were calling something a positive, it was, indeed, a positive.

So that's why -- and the -- and a true positive -- what -- a true positive or true negative being, you know, held up to this high -- not just the on average or the median, but just do you ever see an efficiency-gap score taking on -- there's even one election where the efficiency gap bounces over to the other side of zero would be enough to say no.

And so that struck me at the time of my

initial report as -- as one of the more strenuous tests I could submit the efficiency gap and, indeed, what -- what the -- the efficiency gap from the first election submitting -- investigating the prognostic value of that -- that number.

Q. First, a clarification question. In your analysis, are you using the point estimate of the efficiency gap and not the confidence interval in terms of the sign change?

A. Everything -- for instance, the -- if I could direct your attention --

Q. Sure.

A. -- to -- to -- to, say, just for example, to Figure 1 in my rebuttal report on page 8, the shaded regions around each of those lines are, in fact, 95 percent confidence intervals on each of those quantities on the prognostic measures that in turn stem from the fact that we have confidence intervals that are some certainty accompanying the value of the efficiency gap in the first election and, indeed, in subsequent elections as well. So that uncertainty is, if you will, propagated down through other things I say about the efficiency gap or the prognostic value of the first

efficiency gap under a plan.
Q. But the -- would the lines themselves be based on the point estimates?
A. In some cases, yes, yes.
Q. Yeah. I guess maybe an example would help just for my mind.
So say a plan -- in determining whether it's a positive or a negative, a plan was all of the same sign point estimates but, perhaps, some of the confidence interval went to the other side. Would that count as a positive or a negative?
A. Well --
MR. POLAND: I'm going to object. Just object to the form of the question. You can answer, if you understand.
A. As a -- as a practical matter, yes. The way this is done is with -- I don't want to get too technical here, but the way this is done is with Monte Carlo simulation. So the efficiency gap for a given election is only known up to a distribution, right, and we can summarize that distribution with the mean and we call that conventionally the point estimate; and we also summarize the width of that distribution with something like a confidence interval.

But for the purposes of generating, again, downstream quantities, if you will, such as the prognostic value of the first efficiency gap, there's -- I use something that's called a Monte Carlo method and, that is, to sample out of that distribution that characterizes our uncertainty with respect to any given efficiency gap; and, indeed, for all efficiency gaps I do this.
And then -- if you will, then I've got a sequence of efficiency gaps for that decade and they're each being drawn from the predictive distributions -- posterior distributions, rather, and then -- and it's wash, rinse, repeat. You literally are counting how many times you see a sign flip under that draw and you've stacked -- you know, you literally count that across plans and then you take another draw.
So sometimes, right, the efficiency gap you're working with for a given election -- on any given iteration of that scheme, the efficiency gap value you're working with for -- for a particular election will be above the mean or below the mean, but that uncertainty is -- is -- and this is what Monte Carlo methods do for us in -- in the

quantitative sciences, is allow us to propagate uncertainty in quantities up here in the analysis down through the analysis such that bottom-line things like, for instance, the things I'm reporting in Figure 1 reflect the uncertainty and the inputs.
Q. So -- it's not a binary yes-or-no decision whether a plan counts as a positive or a negative. It could vary depending on the particular Monte Carlo simulation?
A. In any given Monte Carlo simulation, the answer is yes. Averaged over Monte Carlo simulations we get -- that's why we attach a probability to that threshold number, the probability that we will see a sign flip given the first election -- efficiency gap above or below a threshold. That's where that language of -- of probability comes from.
Q. And then stepping back, is there a theoretical or reason why you're using a sign flip from positive to negative or negative to positive as the -- the focal point of this analysis?
A. Yeah. And now we're back to the special meaning of zero, right? Right, because zero represents an unbiased -- or a plan that has no apparent advantage one way or the other, right? Seeing

something on the other side of zero, as it were, you know, the plan is generating an election that's got a different message now to -- to the other messages you may have got, particularly the message, say, from the first election.
So that's why -- and -- and -- that's why I thought that was, like I said, a strong test that -- that -- you know, you get a -- to the extent, right -- think about it the other way. If you get all the efficiency-gap values, what we're calling positive, they're all on the same side of zero, you've never seen it tell you anything other than there is partisan advantage for one side or the other here versus, oh, in one election it did.
And so that's why I thought that was a -- you know, the -- your ability to characterize a plan in those terms struck me as really strong. We have never -- in five out of five elections, it never -- given all the vagaries and wave elections, all that stuff, right, we never saw it send a contrary message, and that struck me kind of intuitively as a -- as a -- as a strong set, right? It's not the average. It's not the median. It's did it ever say anything different to what we saw in the first election? Yeah.

Q. And so a false negative, would that cover a plan that -- using a negative 7 percent threshold that its first election was under negative 7 percent, let's just say negative 5 or something like that.
A. Right.
Q. And then it could have subsequent efficiency gaps of negative 3, negative 2, negative 1, negative 4. That's a false negative?
A. That would count.
Q. Yeah.
A. It didn't trip the threshold in election one and went on to state -- nonetheless, went on to rack up values of the efficiency gap all in the same side of zero as the first one.
Q. And that would work the same way for a positive number as well?
A. Yes. I know. There's many senses of the word "positive" and "negative" being thrown around at the moment. But, yes, I know what you mean and you're right, yes.
Q. So why don't we -- maybe I can just get you to explain the -- there's seven different --
A. Yes.
Q. -- measures here and we can go -- go through them one by one starting with --

A. Sure.
Q. -- sensitivity or the true positive rate. What is that?
A. Well, let me just back up by saying these are all quite standard in the literature on assessing diagnostic performance, right, and indeed, the first two are straight out of the -- the -- the medical literature.
So the true positive rate, known in the medical literature as -- as the sensitivity, is simply the proportion of positives that test positive. So it's cases -- in this case, a definition of positive, right, is that we're seeing the plan have a sequence of efficiency-gap values that are all on one side of zero or all on the other side of zero, and the test, right, is what we saw in the first election. Did it trip some threshold? And so it's just a proportion of all those positives that would have tested positive, yeah.
Q. Okay. And just so -- with all these, there's some abbreviations here.
So TP stands for true positive?
A. Correct.
Q. And then FN is false negative?

A. That's right. And that's to help you out with the table, right? Each one of these quantities is essentially adding and dividing different quantities if you had populated the four entries in that two-by-two table. So sometimes we're going by -- by rows and sometimes we're going by -- by columns. But the abbreviations map back to the interior of that table we were just discussing.
Q. And just to be complete, FP is false positive --
A. False positive.
Q. -- where we see it later on?
A. Yep.
Q. And then TN is true negative?
A. Correct.
Q. Okay. So I think I understand true negative now after you've explained it.
A. Okay.
Q. Can you explain what balanced accuracy is?
A. Okay. So balanced accuracy, right? So now we've got a true positive rate. We've got a true negative rate. So balanced accuracy is -- is the average of the two, right, because why would we want to average them? And the answer is because the true positive rate, we're just looking at

positives that test positive. The true negative rate, we're just looking at negatives that test negative. We want to talk about the overall behavior of the test. We've sort of got to put those two together, either the two rows or the two columns together. And in this case, the balanced accuracy measure is a way of combining the performance with respect to positives and the performance with respect to negatives in a single number, and it's called balanced accuracy for -- as opposed to accuracy. We just confuse everybody. That's fine.
Q. Yeah. There's also accuracy. Could you explain what that is?
A. Yeah. That's right. So now -- now these are the -- now we're doing something else which is -- right? There are many ways to -- a surprisingly large number of ways to analyze a two-by-two table and -- and Item 4 there, accuracy, is -- is -- if you will, is summing the diagonal. How many of the elements line up on the diagonal, because they're right calls, right?
So a true positive, it tested positive and, in fact, was positive; and true negative and it was, in fact, negative and you -- you know, what

-- what percentage of your cases fall on the diagonal of this table is essentially the proportion of, if you will, correct calls out of the whole universe of -- of cases being tested, not just positives, not just negatives.
Q. Okay. And, I guess, maybe we should just go on and do all the rest of them. What is the false positive?
A. Okay. The false positive rate is the proportion of -- of negative cases that -- that -- that test positive. That's why we say it's a false positive, right? It's -- it's tested positive, but in -- but in -- but, in fact, it's actually a negative case.
Q. And then the false discovery rate?
A. Right. The false discovery rate is -- and, you know, we call it discovery because we think we've made a discovery that is with our case that has tested positive, but it's -- but it's -- but it's actually negative. So it's of your -- right, the denominator there, your -- your cases that have tested positive, but you -- in the numerator, it's the -- it's the number of false positives.
Q. And then the false omission rate?
A. Right. And this is cases that tested negative but

actually turned out to be positive.
Q. And then you have several figures --
A. Yes.
Q. -- that represent these? Figure 1, it says it's the absolute EG threshold. Does it mean it's the absolute value with --
A. That's right.
Q. -- respect to sign?
A. Yeah. So we don't take into account whether it's Republican advantage or Democratic advantage. It's just tripped because that's what the sign tells us, so yeah.
Q. And why don't we just go to Figure 1.
A. Yep.
Q. And just to make sure I'm understanding this right, on the vertical axis there's the rate. So maybe just explain what does 1.00 mean there?
A. So, for instance, let's take -- or sensitivity is a very good one, right? Remember that sensitivity is the proportion of positives that test positive; and if you set the threshold to zero, then everything tests positive and they fall -- all of -- all of your positives tested positive because everything tested positive and -- and you end up with a sensitivity of 1.0. That's like your

doctor setting the correct level of -- the healthy value of the cholesterol to zero so we all test -- we all have high cholesterol, and that, by definition, captures the people who, in fact, do have high cholesterol or heart disease, right?
So -- so -- and so as you move -- sorry to interrupt, but as we move from left to right in each panel, it's the -- the corresponding measure of prognostic performance is -- is changing and -- but what I've just called rate, you know, panel by panel we could just substitute in whether we're talking about sensitivity, whether we're talking about specificity, and so on across the seven panels there.
Q. And so in using percentages, 1.0 would be 100 percent?
A. Correct. We're back to that again, yes.
Q. And then .75 would be 75 percent --
A. Correct.
Q. -- and so on down the row? And then on the -- the horizontal axis, does that refer to the efficiency gap in the first election held under a plan?
A. That's right.
Q. Okay.
A. On the absolute value of the efficiency gap.

Q. Correct.
A. Okay.
MR. KEENAN: We've been going about an hour. I don't know if you want a break. I can keep going, but --
MR. POLAND: I could use a two-or-three-minute break.
MR. KEENAN: Okay. Let's do that.
THE WITNESS: Yeah. Cool.
(Recess)
MR. KEENAN: We're back on the record.
Q. Going back to Figure 1, which we were examining before the break, just a couple of finalizing things. I take it that the label at the top of each graph refers back to the various tests we were just referring to in your testimony?
A. That is correct.
Q. And then in reading the caption to Figure 1, this says that it spans all the state legislative elections and district plans 1972 to 2014?
A. That's correct.
Q. So this analysis does include the plans enacted in the 2010s?
A. We had the same question last time, and I -- I

would need to check whether I kept them in -- I remember -- and just to -- you know, I'm sure, as you know, we had this discussion last time. We've only observed two and -- and I don't -- you know, I don't think you want the mean. But I would -- and I -- on the basis of our conversation the last time we spoke, I -- I -- I thought I'd kept them out, but I can -- I can -- I can verify whether I did or not.
Q. Yeah. That would be --
A. Off the top -- from memory I can't recall. I'd need to consult something to verify if that's the case.
Q. And that would be fine. Do you have your computer here where you'd be able to do that?
A. I could do that if you wished me to.
Q. I don't need to do it right now, but I think it would be fine at a certain point. We can have you get the computer out and check any information that you don't know offhand that you need to check your computer.
A. Yeah. Yeah.
Q. Okay. So just moving to -- we'll go to Figures 2 and 3. So if you could just explain to me what Figure 2 is.

A. Right. Figure 2 is a -- in effect a rerun of Figure 1 but now restricting our attention to where we've seen the -- the first election under a plan has produced a negative score of the efficiency gap and, of course, a negative score is consistent with the plan having an advantage for Republicans. So it's a subset of the data shown in Figure 1.
And, moreover, that's why some of the lines have a different shape, because now we're coming in from negative values to -- along the horizontal axis -- negative values all the way up to zero versus the previous graph that was with respect to absolute values and went from zero up through positive scores.
Q. And so the right-most line on each of these graphs is zero?
A. Yeah. Each panel the X axis terminates at zero.
Q. And then what is Figure 3?
A. Pardon me?
Q. Figure 3, just referring to that.
A. Figure 3 does the opposite now. Now, it's looking at plans that -- whose first value of the efficiency gap is positive, indicative of Democratic advantage, and now we're considering

the prognostic performance of a threshold; hypothetically, you know, moving the threshold over. You know, it's obviously now bounded on the left at zero right up through, you know, extremely high values of the efficiency gap -- positive values of the efficiency gap left to right.
Q. And I believe you testified to this earlier, but the -- there's a line here and there's also like gray area surrounding the line. Could you just explain what those two things are?
A. Yeah. The -- the line shows what happens when we plug in, you know -- as you correctly referred to them -- all the point estimates and do the computation with the point estimates ignoring the uncertainty accompanying any point estimate of the efficiency gap. And the -- the vertical shading indicates how variable, right, the corresponding prognostic measure is given the uncertainty in the underlying inputs; that is, the uncertainty in the efficiency gap measures themselves. And so those shaded lines span what in statistics we call a 95 percent confidence interval.
Q. Okay. So we'll go back to page 7. I'm referring to the text that's describing these graphs.
A. Yes.

Q. So you say that the .07 threshold is conservative because the rate of false positives is reasonably low at 25 percent and the -- without letting the false emission rate -- omission rate go above 50 percent; is that correct?
A. Yes.
Q. So at the .07 threshold absolute value, the rate of false positives is 25 percent?
A. Yeah. Yep.
Q. And then what -- you say that the false omission rate does not go above 50 percent. Do you know what the actual false omission rate is?
A. Oh, at .07?
Q. Yeah.
A. No. I'm just doing my best to read it off the graph at this -- at this point. But it's -- it's right around -- getting close to .5, perhaps may not have -- it might be around .5, yeah.
Q. And then what would the false discovery rate be? Could you --
A. Okay. At .07, it's roughly 32 percent, meaning that, right, the -- of -- of cases that trip the threshold that they go on to -- the proportion of cases that trip the threshold that are actually negative cases, yep.

Q. And, I guess, maybe if I could just get you to identify the sensitivity.
A. Uh-huh. At .07?
Q. Correct.
A. Okay. Again, I'm reading this off the -- off the graph myself. But, I believe, in the -- in the text, I don't refer to those two measures per se, but I'm -- so I'll just read them off the graph as best I can. About -- about -- again, about -- at .07, the sensitivity is about 32 percent and the specificity is -- is much higher in Figure 1. That's up at about point -- almost .7, high .6s, pushing .7.
Q. And then the balanced accuracy?
A. Uh-huh.
Q. Can you tell me what that is at .07?
A. It's about point -- I'm just seeing if the actual number appears in the report. No. So it is -- again, reading off the graph, it is slightly above .5.
Q. And then the same with --
A. With balanced accuracy?
Q. Right.
A. It's perhaps a tiny bit higher, about, say -- well, again, just this is a rough guess based on

just eyeballing the graph, but about 55 percent.
Q. Is 55 percent the accuracy or the balanced accuracy?
A. Again, I'm just doing my best here with the --
Q. Yeah. Just like you gave slightly about --
A. They're about the same, actually --
Q. Okay.
A. -- as I -- as I kind of lean right in and squint at the graph hard, yeah.
Q. Okay.
A. Yeah. In the -- in the -- yeah, about 55 percent each.
Q. Turning back to page 7 --
A. Uh-huh.
Q. -- the last sentence you say, "To reiterate, the proposed standard for judicial scrutiny is cautious and conservative erring on the side of letting even durably skewed plans stand."
A. Uh-huh.
Q. What do you mean by "durably skewed plan"?
A. Well, a durably skewed plan there is a synonym for an actual positive and the threshold is -- is letting -- at .07, you've set the threshold high that the -- that you're letting -- a lot of actual positives are actually testing negative. So the

-- the false omission rate, things that you should have thrown a flag on but you don't, with the threshold at .07 is -- is actually -- is actually getting up pretty high. What we've done there at .07 is done -- we're literally trading off -- that's the sense in which it's conservative. We're willing to let cases like that go through more so than we're willing to throw a flag when, in fact, we should -- we're quite conservative in setting .07 inviting scrutiny in the first instance.
Q. So durably skewed means a plan that had elections all with the same EG sign?
A. That's correct.
Q. Would I be able to get you to give the point -- sorry, the values at a .1 EG threshold on Figure 1?
A. For -- for each of the seven quantities?
Q. Yeah, for each of the panels. Or is that something that would be easier to do with your computer?
A. I could provide that later on, if we wished --
Q. Okay.
A. -- and take the guesswork out of it, yeah.
Q. Okay.

A. Yeah. Happy to help like that, yep.
Q. And I think, perhaps, I'll have you do the same thing for Figures 2 and 3. We can just get the exact answers from the code.
A. Okay. And the idea is we'll just do that orally or you want me to --
Q. I'm fine asking you the question and having you tell the answer on the record.
A. And just read it off the machine later?
Q. Yes.
A. Is that --
MR. POLAND: We could do that or we could also -- I mean, we could take a break and we can look it all up and we could have that, you know, ready to go.
MR. KEENAN: Whatever's easiest, I mean.
MR. POLAND: Okay.
THE WITNESS: Okay.
Q. I'm not as familiar with how "R" code works and how it would be easiest for you to do it. So going to page 10 --
A. Yes.
Q. -- you talk about an asymmetry in the results. What asymmetry did you see between the

pro-Democratic and pro-Republican?
A. Well, at .07, you're -- you're letting plans that begin life with a Democratic advantage -- so let's just go to that graph. That's Figure 3. You're -- you're making some -- some false discoveries there more so than you would for Republican advantage. In Figure 2, you'll observe that. If you were to compare the panel labeled false discovery in Figure 3 with Figure 2, it's my sense that those are offset by -- by a -- by a -- by a -- a considerable -- they're considerably different from one another.
So the false discovery, right, for plans that trip negative .07, that is Republican advantage, is -- is -- is -- is quite low, but up -- up to about three times as high on -- on -- on the Democratic side.
So you'd be actually submitting -- on that set of plans on the Democratic side, you'd be inviting -- didn't think it would turn out this way, but as it turns out, you'd be inviting more scrutiny of -- of -- of -- of Democratic plans that actually turn out to be negative cases. And that goes back to the earlier point we were talking about about the durability of apparent

pro-Democratic bias in the first election in a sequence under a plan. That's -- those two are essentially analogous things, equivalent things we're seeing, yeah.
Q. So the reasons for this asymmetry, your opinions for the -- about the reasons for this asymmetry would be the same testimony you gave previously to that?
A. Yeah. Yeah. What explains this -- because it is the same phenomena, so the explanation for one is the explanation for this behavior as well.
Q. Go on to Section 3, the plan -- the plan average --
A. Yes.
Q. -- efficiency-gap sign. Maybe you could just explain what type of analysis you did that's listed here in Section 3.
A. Okay. Okay. So this asks a different question to what I've asked hitherto. Now we're asking -- we've got the same threshold testing in mind, what is the value of the efficiency gap we observe under the first election, but now we're asking not do we have to see a sign flip. Now we're asking does the average efficiency-gap value under the plan have the same sign as the first value you

saw? So it's asking about where is the average now rather than will you ever see a draw from that distribution with one or more of the -- of the draws being on the other side of zero to the first draw.
So it's a less strenuous test of the proposed standard, and that's reflected in the behavior of it as a prognostic -- we have -- you know, has better prognostic -- the first election is a better predictor of that subsequent behavior than -- than the more extreme test we were subjecting the first election to in the previous analysis.
Q. Now, in this calculation, the first election's EG will be a component of the plan average, correct?
A. That's right.
Q. So how do you account for that, or do you?
A. Well, that is -- this is what it is, right? You can do it two ways. You can compute the average holding out the first one or you can have the -- have -- you know, are we going to have -- compute an average of five observations or are we going to have to compute an average of four observations, you know, typically? And -- and we could -- we could do it either way and, indeed, I may have played with that. It's ringing a bell that that

might have been something I looked at, but -- but, you know, it's part of the sequence. It's -- it's -- it's -- it's -- the first election is still, nonetheless, indicative of what the average will be, you know.
Q. Sure.
A. We --
Q. Sure. And your calculations include the first election in the calculation?
A. I believe so, but I -- I'm happy to verify that when we take that break and go at some of the code.
Q. And then there is a series -- Figures 4, 5, and 6 here.
A. Yep.
Q. I don't think we need to go into them as much detail as we did for 4.
A. For sure.
Q. But the -- the horizontal/vertical axis and labels correspond to what we talked about before with respect to Figures 1, 2, and 3; is that right?
A. Precisely. And, if you will, even sequentially 1, 2, and 3 have respectively -- they're analogs now with 4, 5, and 6.
Q. All right. So I think we can move on from

Section 3 --
A. Okay.
Q. -- on to Section 4.
A. Oh, right, yes.
Q. Could you explain the analysis that you did that's contained in Section 4?
A. Yeah. Well, it's closely related to what we were just discussing about Section 3. This is the extent to which the first election efficiency-gap reading and -- that is to say, the efficiency-gap value you get from the first election under a plan is -- is predictive of the average efficiency gap you'll see over the totality of elections under the -- under the -- under that plan.
And, for instance, Figure 7 is essentially a summary of that. We're talking about the relationship between two numbers now. The first value of the -- the first election efficiency-gap score and the plan average efficiency gap; and the idea is, you know, let's investigate the relationship between those two quantities.
Q. And I see --
A. You'd like there to be a relationship, or at least one -- one could imagine being interested in the extent to which there is a relationship between

those two given everything I just said, you know.
Q. And I see in this paragraph -- the paragraph that starts Figure 7 on page 15, it says that, "Only plans with a" -- "with three or more elections are included," so that means that the most recent --
A. That's right.
Q. -- round has been excluded?
A. Would be out, yes, would be out, right, and it -- and Figure 7 has the same restriction.
Q. I'm in the middle of that paragraph. There's a sentence that says, "Instead, we see a classic 'regression-to-the-mean' pattern with a positive regression slope of less than one," and it says in parentheses "(as indeed we should given that the first election EG on the horizontal axis contributes to the average plotted on the vertical axis)."
Maybe you can just explain what you mean there to someone who's not as well versed in statistics as you are.
A. Yes. I believe you -- you hit on it in about three or four questions ago; and that is, if you're analyzing the relationship between the average for -- based on a small number of cases, it's a mathematical fact that there's going to be

some reasonably predictable relationship between any one of those data points; the first, the second, but it doesn't really matter, but -- and the average, right? And we can take the absurd case of where we have the average just based on one case in which it's that case and that would give us a perfect relationship. So now we're up to computing an average based on four, typically five cases, and we're asking what's the relationship between the first of that sequence of four or five values and the average of the four or five values?
So that is to say -- and in statistics, okay, regression to the mean, that -- that language refers to a well -- you know, if -- if you have data of that sort, as we do here, one ought to expect some kind of relationship between the two. It would be kind of implausible that the relationship there didn't bear some -- some kind of relationship.
But regression to the mean picks up on the fact that often on any one draw, if it's an extremely low score, it -- the corresponding mean will lie further towards the interior of the data than, you know, a typical score close to -- in

this case, close to -- zero is going to be close to the mean, closer to the mean, and with an extreme value.
You see, the phrase comes from, actually, the very first users of the word "regression" in statistics where people noticed that the children of exceptionally tall parents tended not to have quite as tall, and the children of exceptionally short people, their kids tended not to be -- tended to be shorter than average but not quite as short as -- as the parents, and that's -- the phrase has stuck. And anytime we have sort of patterns like that, we -- we -- in statistics, at least, refer to that with the shorthand regression to the mean, and we have some of that going on in Figure 7.
Q. Sure. And it says that -- continuing on a couple sentences later it says, "The variation in plan average efficiency gaps explained by this regression is quite large --
A. Uh-huh.
Q. -- about 73 percent."
A. Uh-huh.
Q. And then there's some language above the confidence intervals.

What do you mean by "the variation in plan average is explained by regression"?
A. Literally what we mean is, if I could refer to Figure 7 in answering that, the vertical spread of the data, the spread of the data in the vertical dimension is well accounted for by the spread of the data in the horizontal dimension, and that is merely to say that X is a good predictor; in fact, you might even say a very good predictor of Y here. The preceding language about regression to the mean is indicating we shouldn't be too surprised that there's some relationship, right? As you noted in your earlier question, you know, there has to be some kind of relationship between data point one and the mean of the succeeding four or five data points.
But what I'm noting with that comment about the amount of variation explained is that it -- by social science standards, that's a pretty good fit, might be even a very good fit, to the data. You can do a pretty good job, perhaps even a very good job, of predicting plan average efficiency gap given the efficiency gap you see from the first election.
Q. And then it says it's 73 percent. What would we

think of the other 27 percent that's not accounted for here?
A. Yeah. That's where the first election is unusually different from what the plan turned out to be. That's -- that's -- that's where -- so indeed, you know, there's a few cases labeled on the graph where the first election lies a long way from -- from the -- from the mean. So there's a -- there's some of the more extreme examples that are labeled on the graph. But, in general, the pattern is one of a strong relationship between first election efficiency gap and the plan average efficiency gap.
Q. And, I guess, we can look at that Figure 7.
A. Sure.
Q. And you mentioned a couple of labels there. For example, I see VT4 --
A. Uh-huh.
Q. -- listed there. What does VT4 mean?
A. Okay, VT4. VT is Vermont, so it's just the two-letter abbreviation for each state. Then the number is the -- refers to the decade. And the way this works is conventionally that '70s plan is one, '80s are two, '90s are three, '00s are four, and the '10s are five.

Q. Okay. So I'm reading this correctly, Vermont 4, that would be the 19 -- or 2000's plans?
A. '70s, '80s, '90s, yes, yes.
Q. It started out with a negative efficiency gap in its first election of, I don't know, maybe negative .04 or 5?
A. Maybe not that big, but yeah.
Q. All right.
A. Or close.
Q. And then it -- but then its average ended up being --
A. Yes.
Q. -- positive?
A. Right, .5 or -- .05 or 5 percent.
Q. Okay. And then if we look at another one, WA3, would that be Washington from the 1990s?
A. Exactly right, and that's gone the other way where the first election produced a positive value of the efficiency gap, right, of about, let's call it, 6 percent, but has gone on to produce a plan average of, you know, negative -- what is that, yeah, negative 6 percent, yeah.
Q. If we think of the Wisconsin 2000's plan, it had a first election that was negative .75 and the average was fairly close to that as well. Would

its data point then be close to the -- the diagonal -- black diagonal line that goes from corner to corner?
A. Correct.
Q. Okay.
A. Absolutely correct. To the extent the first data point -- if -- indeed, if it was a perfect relationship between the first efficiency gap and the average, if -- if we hit the average dead on every time, all the data would lie on that 45-degree line. But you're right. I think that Wisconsin case would be -- would lie very close to the 45-degree line for the '00 decade.
Q. And then going to the next page --
A. Sure.
Q. -- the top paragraph on page 16 --
A. I'm sorry. Yep.
Q. I'm sorry.
A. No. I got it.
Q. I meant the previous page. The paragraph says, "The historical relationship between first election EG and plan average EG shown in Figure 7 indicates that a first election EG of negative .07 is typically associated with a plan average EG of about negative .053." Did I read that correctly?

A. Yes.
Q. So -- and then I noticed it has a 95 percent confidence interval. That's what CI means, right?
A. That's correct.
Q. Of negative .111 to .004. That seems like a large confidence interval to me. Can you explain why it's such a large range?
A. Well, because it doesn't fit the data perfectly, right? It's not a -- right. The data are -- there's some variability around the fitted regression line, which is the blue line on -- if you've got a color copy of Figure 7 on -- on page 17. It won't be a perfect relationship between the first election efficiency gap.
And the other thing why -- confidence interval why, is we're out in the tail of the data too. Recall -- keep that in mind. Now, when we predict out of a regression model, the imprecision accompanying a prediction is a function of how unusual the hypothetical case you're considering is as -- as an input to the regression.
So the input we're considering is a first election EG of negative .07, right, which is unusual or relatively unusual in -- in -- in these data and, therefore, the regression prediction's

conditional on an unusual event. Subsequent predictions tend to be accompanied with more uncertainty than if we're predicting, say, at the middle of the data set.
So that's why that confidence interval will -- is as large as it is. I -- I point out the -- the words that appear in the -- in the -- in the very next line, that "conditional on a first election efficiency gap of negative .07." Even taking into account the confidence interval accompanying this unusual scenario, the probability that resulting expected plan average efficiency gap is negative -- is 96 and a half percent, all right? So that confidence interval does -- 95 percent does just touch positive territory, as you pointed out in your question to me; but, indeed, that's why the next remark appears indicating that the probability -- we would expect to see a negative average value of the efficiency gap is still above 95 percent and, indeed, it's 96.5.
Q. And then the -- going on it says, "The first election EG of positive .07, there's typically a plan average EG of .037." Do you see that?
A. That's right. That's right.

Q. But, in this case, the probability that the resulting expected plan average is positive is 89.8 percent; is that correct?
A. That's right.
Q. And is this another instance of the asymmetry we've been talking about?
A. Exactly. Now, there's the third manifestation this morning of the -- of that -- of that behavior, that the apparent pro-Democratic advantage, as evident in the first efficiency gap reading under a plan, does not appear to be as durable. Therefore, in this case, as we try to predict the average value of the efficiency gap, we'll see over the life of the plan it's accompanied with more uncertainty, right?
So two things to note there: That the prediction has come much further back in toward zero, right, all right, where we go from negative .07 and the prediction about the average is now negative .053. If we saw positive .07, our prediction for the plan average comes all the way back into .037 and -- and the confidence interval has to at that point have more mass on -- on the other side of zero, yeah.
Q. For both positive and negative .07, we see that

the plan average is closer to zero than the first election; is that correct?
A. Yes, and that's regression to the mean, that regression-to-the-mean phenomenon I was describing.
Q. Is that true for each -- each possible first election EG you calculated?
A. And, indeed, that's what the regression line describes. The -- and the regression line, just so I'm being perfectly clear, is the blue line on Figure 7. And if you -- that provides the -- if you will, the set of predictions about plan average efficiency gap given first election efficiency gap, and you can literally project up from the horizontal axis, hit that blue line, and project over to the vertical axis will give you a prediction in every instance.
Q. So, on average, after we see one data point in the first election, we would expect that the plan average would be closer to zero than what we see in the first election?
A. That's correct.
Q. I guess, I suppose, I'd say for like a positive EG it would be closer to --
A. Less positive.

Q. Less positive, and a negative EG would be less negative?
A. Less negative, yes, yes. But by an amount, though, right? This is the key thing about regression to the mean; that is, it's self-decreasing as we get closer to zero. So if you started close to zero, you wouldn't go as close to zero, right, as if you'd -- if you're out in the tails, and we would just hark back to that discussion, the analogy about regression to the mean, yeah.
Q. The regression back to the mean is larger the further away from zero you are?
A. Correct.
Q. All right. I'm learning. Okay. Going on in the next paragraph, it talks about Wisconsin in 2012 --
A. Right.
Q. -- and the initial efficiency gap of negative .133. Could you explain why you predict that the probability that it will have an average efficiency gap of positive is less than .1 percent?
A. Could you just --
Q. Sure.

A. Oh, oh, right, the end of the paragraph. I'm sorry. I see. Okay. So -- okay. So I'll just walk you through, if you don't mind --
Q. Sure.
A. -- the -- the logic in -- in that -- in that paragraph. Now we -- we take as an input to this exercise the first value of the efficiency gap we see in Wisconsin in 2012. What we have now with reference to Figure 7, we're starting off now at negative .133 on the horizontal axis, right, almost at the very edge of the observed data, all right, and perhaps maybe even slightly to the left of it. I'm not quite sure. And then we project up and we hit the blue line; and then we go over against the vertical axis to get our prediction of what the plan average efficiency gap will be and we arrive at .095, or negative 9.5 percent.
Now, we're able to put a confidence interval on that prediction and that confidence interval is bounded, right? They're both negative numbers, the limits of confidence interval. And, moreover, you can even ask a further question -- and remember, I'm -- let the record show I'm describing a bell-shaped curve with my -- with my finger here, one of the -- how much of that

bell-shaped curve spills over into -- into positive territory. That is -- you would -- right? What's the probability that -- nonetheless, we were at a point estimate of negative -- for the average of negative 9 and a half percent. There's some uncertainty around that. I just want to be perfectly clear, right, that we're up to -- we're better than 99.9 percent sure that given the historical relationship between first plan efficiency gap and average -- plan average efficiency gap, that the Wisconsin plan, if left to run, will -- will have a -- a -- a pro-Republican average efficiency gap.
Q. And --
A. So they're less than 0.1. Perhaps the more dramatic way of putting that might be more than 99.9 of -- of -- of continuing to show Republican advantage.
Q. And then just -- maybe we could just go to Figure 7 and I can ask the same questions on that just to make sure I can understand it and apply it.
A. Sure. Uh-huh.
Q. So maybe we could just take a look at negative .07 on the horizontal.

A. Yeah.
Q. So that horizontal axis refers to the first --
A. That's correct.
Q. -- election efficiency gap? And so if I -- if there's an election with a negative .07 and I go up from there to the blue line --
A. Uh-huh.
Q. -- that would tell me what the expected average efficiency gap would be?
A. That's correct.
Q. Okay.
A. If we were then to project over to the vertical axis, that's right.
Q. And then that would apply for any observed first efficiency gap. I would go to the relevant spot on the horizontal axis and move up to the blue line?
A. That's correct.
Q. Okay. All right. I think it might be helpful to maybe get the computer now and we can talk about the --
A. Oh, because you were ready to --
Q. Move on.
A. -- go on to five and -- yeah. Okay.
MR. KEENAN: So we can take a short

break.
THE WITNESS: Will that be okay before I --
MR. POLAND: Yeah. That's fine.
(Recess)
MR. KEENAN: We're back on the record.
Q. So we're back from a short break, and I was going to follow up with some questions that I postponed earlier --
A. Yes.
Q. -- to allow you to consult with your "R" code to get the answers. Have you been able to do that during the break?
A. I have.
Q. Okay. So I think the first question was in looking at the analysis in Section 2 --
A. Yeah.
Q. -- whether that analysis included the plans that were enacted following the 2010 census or whether they were excluded?
A. They're in.
Q. Included, okay. And then we also had some questions on -- I had some questions on the precise values of some of the graphs that are

contained, like Figure 1, 2, and 3, and were you able to look at that information?
A. Yeah. What we did was to get the number exactly corresponding to .1 --
Q. Correct.
A. -- I believe, on the -- is what you're asking. So I've got those viable for Figures 1, 2, and 3.
Q. Okay. So why don't we just -- we'll go in order, Figure 1, and then we'll start with sensitivity --
A. Exactly.
Q. -- and work our way to the right.
A. Yes. From left to right, the corresponding numbers go: Sensitivity, .20; specificity, .91; balanced accuracy, .56; accuracy, .52; false positive, .08; false discovery, .26; and false omission, .51. And that's all conditional on the -- being at .10 on the horizontal axis.
Q. Okay. So then, I guess, we move to Figure 2, which would be now negative .1.
A. Exactly. The numbers run in sequence. Sensitivity, .17; specificity, .98; balanced accuracy, .58; accuracy, .65; false positive, .02; false discovery, .12; and false omission, .38.
Q. Okay. And then head to Figure 3 --
A. Uh-huh.

Q. -- and use .10.
A. Correct. We go .11, .95 -- I'm sorry. I'll read each one. Balanced accuracy, .53; accuracy, .64; false positive, .05; false discovery, .43; and false omission, .35.
Q. Okay. Thank you. And now we can turn to Section 5. This deals with party control.
A. Let's go to that then. Great.
Q. And maybe I -- we'll mark two exhibits.
A. Oh, right. Yes, yes, yes.
MR. KEENAN: This will be 57.
(Exhibit Nos. 57 and 58 marked for identification)
Q. First, could you just identify what Exhibit 57 is?
A. 57 appears to be an email from Nicholas Stephanopolous to myself with some other parties cc'd.
Q. And what was Mr. Stephanopolous sending you attached to this email?
A. There were two attachments to the email, two Excel spreadsheets.
Q. And what was your understanding of what the data that would be on those spreadsheets was?
A. One would contain efficiency-gap values for congressional elections. The other contained data

indicating which group, partisan or otherwise, was nominally designated as controlling the redistricting process in a given state in a given year.
Q. And, for the record, I have not made a copy of the congressional EG data attachment, because I wasn't going to ask you about it. So to save some trees, I haven't done that, but if you could identify what Exhibit 58 is.
A. Yes. Exhibit 58 --
Q. And it's a -- it's a two-sided document --
A. Yes. I've got it.
Q. -- so you know.
A. I'm familiar with this. This is a printout of the Excel spreadsheet, the second one I referenced, the party control Excel spreadsheet.
Q. Could you explain the information that's contained on Exhibit 58?
A. Yes. It is organized in -- each record -- each row of the spreadsheet is a state election year combination and it's blank, has no data for election year, it appears, in 1970. But beginning in 1972, it contains an indicator for whether the redistricting plan under, which the corresponding election was held, whether that redistricting plan

was -- came -- was the product of an independent commission, a court, and then there's also indicators for whether it came out of a process controlled by the legislature or the state government more generally, and if so, was that state government under unified Democratic control or unified Republican control or, as we call it, divided government; say, a mismatch between the party of the governor and the parties that were controlling the state legislature would be an indicator -- that would be an instance of what we meant by divided government.
Q. **So did your historical analysis, both in your original report and in the rebuttal report, did it consider elections in the year 1970?**
A. No.
Q. **Okay. So we can ignore those.**
A. Okay. Yes.
Q. **And then if we could -- what does -- maybe you can just explain what a zero or one indicates in a particular column.**
A. It's -- it's -- literally, zero connotes no and one means yes --
Q. **Okay.**
A. -- for -- for the -- for the attribute indicated

by the column header.
Q. **And then we see the state name. That's pretty obvious --**
A. Uh-huh.
Q. **-- I would think. And then the abbreviation for the state.**
A. Uh-huh.
Q. **What does the number in the FIP column stand for?**
A. Oh, that's a FIPS code, which is a Federal Information Processing Standard. Sometimes states are labeled with a -- with their so-called FIP code, and that's helpful to have depending on -- as you would with these data, you'd be merging them against some other data set and in that other data set where the state's labeled by the full name, their postal abbreviation code, or by their FIPS code, and you've got three butts of the cherry there, as it were, to help you if you want to bring other -- other data sets to bear, which is what we're going to do with these data.
Q. **Okay. And so, for example, if I see Wisconsin is listed here with -- on the second page with 55 --**
A. That's its FIPS code.
Q. **And so every time Wisconsin appears in this**

document, it will have a 55 next to it?
A. It should.
Q. **Okay. And every other state will have a unique number associated with it?**
A. Yeah, just as it's got a unique two -- two-letter postal abbreviation too.
Q. **And then just so I understand it, if there's multiple elections under the same plan, are those elections listed multiple different times in this document?**
A. That's the way these data are organized. Perhaps not efficiently, right? It means there are redundant rows, but they're being organized at the level of state election when the more efficient rendering, perhaps, might be, as the question presupposes, you know, election plan, yeah.
Q. **Okay. So just, for example, like Wisconsin 2012 and 2014 will be listed two times even though it's under the same plan?**
A. Let me just -- I'll verify that. Well, so there's -- right. There's an entry for Wisconsin 2012 and another entry for -- where was it? Oh.
Q. **I notice that some of them are a little bit out of order, but --**
A. No. It was just on the back page. Yeah. That --

that's correct.
Q. **But elections under the plans -- same plans should have the same zeros and ones in the same columns?**
A. That's my understanding of the organization of this data set.
Q. **And is it your understanding that this chart would refer to the body that instituted both state legislative plans and congressional plans?**
A. That I don't know.
Q. **But it's your understanding it definitely covers state legislative plans?**
A. That's my understanding of these data.
Q. **All right. And then was this document the source of the information for your party control analysis that is reflected in Section 5 of your report?**
A. That's correct.
Q. **So you can put that aside. I don't know that we'll refer to it, but --**
A. Okay.
Q. **So there has been a change in the party control of the districting process over time, correct?**
A. That's correct.
Q. **And so can I just get you to outline what the party control was in terms of Republicans and Democrats? And then I don't know what the correct**

term should be for a nonpartisan or bipartisan body. What should we call that?
A. All others.
Q. Okay.
A. So everything from commissions to courts to plans that were brought up under divided government, yeah.
Q. Okay.
A. So it's literally -- there's a -- the data are richer than this, but we've -- we've broken it out just into three categories -- collapsing that information into three categories: Unified Democratic, unified Republican, and the rest.
Q. Okay. So if I could get you to identify the breakdown between the three categories for the 1990's plans.
A. Yes. So Figure 8 does -- does this for you. In Figure 8, we see that going back to the 1990s, the proportion of plans brought up under -- that were brought up through the legislature and control of the redistricting -- well, the state government itself, right, where that was Republican governor and Republican legislators. There was a relatively small number of such plans in the -- in the 1990s around -- and the number there, you

know, again, reading off the graph is -- the exact number might appear in the report, but, yeah, about 10 percent. That's right.
That goes up as we -- you know, and these data are just for the three -- the last three decades, 1990s, 2000s, 2010s, left to right, and that goes up. So that by the time we get to 2010, we're up to about 40 percent of plans were produced under that condition we're labeling unified Republican control.
Q. And in the 2000s, is that about 20 percent?
A. Yeah. Let's go ahead and -- that's -- that's about right, yeah.
Q. And then Democrats -- I believe you said that 1990s it started at 30 percent in the report?
A. Yeah.
Q. And then how does that change as we move to the 2000s and then the 2010s?
A. Well, that falls down to a roundabout 20 percent by -- 20 versus 15 into 2000s; and then in 2010, we're down to less than 20 percent designed by -- under unified Democratic control.
Q. Okay.
A. So the point is we essentially invert the preponderance -- the relative preponderance of

plans 1990s and we go from preponderance of -- to the extent they are unified, one side of politics or the other controlling the redistricting process, we go from that being a predominantly Democratic phenomenon in the 1990s to a predominantly, you know, Republican phenomenon by the 2010s, yeah.
Q. And the other institution in the 1990s at 60 percent?
A. Yeah. That's about right, 60, 60, you know, falls slightly to the -- just above the Republican -- unified Republican proportion by the time of the 2010s.
Q. And then in the 2010s is it -- looks about 60 percent as well?
A. No. To my eye --
Q. Sorry. The 2000s. I misspoke.
A. Oh, pardon me, yes, yes. That's right.
Q. And then, I believe you say, it's 40 percent in the 2010s?
A. Uh-huh. Yes.
Q. So could you explain -- and your report references a regression analysis you performed on this data.
A. Sure.
Q. Could you explain what you did?

A. Okay. So in each decade, you run a regression that predicts the magnitude of the efficiency gap based on which one of these three categories, as we were just talking about, the given election falls in; that is, is it an election under a plan that was designed entirely with Democrats controlling the process, with entirely Republicans controlling the process, or in that third category of none of the above, all other possibilities? You run that regression analysis, as I said, and it's a very simple regression analysis. You're essentially just classifying -- you know, you're basically breaking out efficiency gaps by those three categories, and you do that in each of the -- of the three decades. And that leads us to then the analysis that's presented in -- in Figure 9.
Q. Okay. So why don't we talk about what you did to each specific category within a decade to run this analysis.
A. Oh, okay. So you -- literally it's -- it's extraordinarily simple. You just literally clump -- gather up elections according to which one of those three categories they fit in, all right, and then -- and then it's -- it's --

it's -- literally what you're doing is computing the average efficiency gap conditional on who controlled the redistricting, is perhaps the most simple way whereby, quote, who controlled the redistricting, unquote; we mean which one of those three categories, right, with that three-fold classification of control, yeah.
Q. And is this an average of all the elections or is it an average of the plan averages?
A. It's an average of -- they'd be the same, but it's a -- it's each individual election appears as a data point in -- in that analysis.
Q. Okay. So, for example, like all the Republican-drawn plans in the '90s had an average efficiency gap of a certain value --
A. Yes.
Q. -- you just add them all up and divide it by the number and that's your average?
A. That's right.
Q. And you would do that for each of the -- each of the other components of Democrats and the Republicans?
A. Yeah.
Q. And so then you did that for the '90s, the 2000s, and 2010s?

A. That's correct.
Q. Page 19, in the paragraph right underneath the figure has a parenthetical that talks about the omitted category --
A. Yes.
Q. -- being the other institutions. What does it mean to be in an omitted category?
A. Yeah. Right. That's -- that's unhelpful to a nonstatistical reader. So let me -- let me explain.
When we use regression analysis to do something extraordinarily simple, that is, compute three averages, the way we do that with regression analysis is to arbitrarily define one of the three categories as the baseline and then estimate differences -- two differences relative to baseline. So the better word, rather, than omitted, which has prompted the question, I think, the -- the better label there would have been baseline. And then we -- you can estimate the three averages as three averages or you can estimate an overall average and then two differences from -- you can estimate the baseline and then two differences from that baseline. And so that's all -- that's really a function of how

the statistical machinery wants to compute it. Perhaps isn't a helpful way to put it to a lay audience, yeah.
Q. Maybe you can just explain how the other institution served as the baseline in the calculation.
A. It's -- well, it's arbitrary as to which category appears as the baseline. It's really -- you know, everybody -- there's this baseline group that you're either in or not and now we're going to estimate differences, right? So I can recover the average of any group by its baseline plus the difference between baseline and that group, right, and so it doesn't really have -- it's of no statistical -- this is more a math thing than a stats thing, if you will. This is do I want to estimate B or do I want to estimate B and the difference between B and A and add that to get B is A plus the difference between B and A might be one way of putting it. If -- I'm not sure that's helpful, but it's -- it's -- this is really to do with, if you will, tricking regression analysis to do difference of means and, hence, the means by group. And it's -- it's a very standard usage of the term here, one that I understand in this

context might be prompting a question or two.
Q. Sure. And then just to kind of go back to the data set --
A. Sure.
Q. -- the specific plans that are grouped in each category change over time, correct, between the decades?
A. If control of the plan change -- control of the redistricting process changed, yes.
Q. So, for example, in your 1990's decade, the Wisconsin plan is counted as an other institution?
A. Yeah. Yeah. We could verify that.
Q. Because it was drawn by a court?
A. And, indeed, it is.
Q. And then the 2000's plan is also treated as a -- Wisconsin plan is also treated under the other category because it was drawn by a court?
A. And, indeed, it is.
Q. But then in the 2010s, the Wisconsin plan was treated as a Republican plan because it was drawn by Republicans, correct?
A. The 2012 election would be the first election under. So let's just check that one. Oh, indeed, 2014 is the same, you know, and -- and there -- there we've got, yes, unified government and a

flag also for unified Republican government for 20
-- yeah, yeah, for those latter Wisconsin entries
in the data set.
Q. And then why don't we look at Figure 9 then --
A. Sure.
Q. -- which contains like a graphical representation
of the regression analysis.
A. Uh-huh.
Q. What does the solid line represent?
A. Okay. The -- the solid line is just showing the
average efficiency gap by decade, the -- and it's
blue on -- on my version of the report as well.
Q. Yeah. I have a black-and-white copy.
A. That's okay.
Q. And then is that -- are the points there the
average of every election in that decade's
efficiency gap and then the average -- just flat
average of all of them?
A. That's correct.
Q. Okay. Regardless of what type of body implemented
that plan?
A. Yes.
Q. Okay. So then why don't we explain what the
dotted line represents.
A. Okay. So the dotted line is using -- is a

counterfactual exercise, the results of a
counterfactual exercise. The counterfactual being
contemplated is: Suppose partisan control of
redistricting had stayed the way it appeared in --
in -- in the -- in the 1990s. If -- what average
value of the efficiency gap would we see in the
2000s and in the 2010s if instead of the partisan
control of redistricting that we actually had in
the 2000s, we'd had the partisan control that we
had back in the '90s, we -- which, you'll recall,
was to the extent any one party dominated the
other with respect to partisan control, it was --
it was Democrats were -- were controlling more
redistricting plans than Republicans back then.
So it's a -- it's an interesting attempt,
kind of nifty, if I do say so myself, to isolate
the -- the effect of one of the things that's
moving here and, that is, who's controlled the
redistricting versus other things that might be
changing over the period 1990s to -- to 2010, and
so as you ask, you know, what are the efficiency
gap -- on average what would be the efficiency-gap
values we'd see had we got -- had we had the same
partisan control balance as we had in earlier
decades.

Q. Okay. So perhaps we could walk through like the
2000's calculation.
A. Uh-huh.
Q. Did you calculate an average efficiency gap for
all Republican plans that were in place in the
2000s?
A. Yes.
Q. Okay.
A. And then what you do literally is just change the
number of plans, right, back to what the 1990
number plans looks like to sort of readjust the
average to account for the fact that there's --
there's just a different balance of partisan
control of redistricting in the earlier decades,
yeah.
Q. And then you also calculated an average efficiency
gap for Democratic-drawn plans?
A. Yes.
Q. And then also one for the other drawn plans?
A. That's right, yeah, yeah. There were three
averages at the three data points, yeah, yep, and
-- but the counterfactual exercise comprises of
changing the amount of data -- when you get the
overall average reducing those three averages to a
single number, you do so by imagining that we're

back in -- in -- with the -- the -- that we had
the 1990's control of redistricting in place
rather than the ones we actually had in the 2000s
and 2010s.
Q. Sure. And so -- and if I understand it correctly,
you also did the same thing for the 2010s then as
well?
A. Exactly, an analogous exercise for the 2010s.
Q. And 2010's exercise used the percentages from the
1990s; is that correct?
A. Again, it's the same counterfactual. You're
asking if -- if -- in the 2010 round of
redistricting, what if we'd had the same mix of
Democratic control, Republican control, and other
that we'd had -- that we observed in the 1990s?
Had that been in place, what -- how would our
expectations as to efficiency gaps -- how would
they change, yeah.
Q. And then did you -- for the 2010s, did you do a
calculation of what it would look like if you
instead of going all the way back to the 1990s
just went back to the 2000s?
A. I haven't done that.
Q. I think I'd like to get the averages for the three
different buckets --

A. Sure.
Q. -- for each one for each decade. That may be another --
A. That's another -- I can -- yeah, yeah.
Q. -- computer thing. So we can do that at a certain point, and then I may come back to have some questions on this.
A. Sure.
Q. And if I understand it correctly, your method is just to change the number of plans in each bucket to represent what it was like in the 1990s?
A. It's equivalent to doing that, yeah, yeah.
Q. I think we can start on the Section 6, the Chen and Rodden.
A. Okay.
MR. POLAND: Now's probably a good time to ask. What are your thoughts just in terms of the amount of time you have left? Not trying to press you for anything.
MR. KEENAN: Yeah. I'm thinking we'll probably have to take a lunch and come back.
MR. POLAND: Okay. Okay.
MR. KEENAN: But then I don't anticipate it going all the way until like

five or anything. But, I guess, you never know, it's stats, and see how long it takes me to understand things --
MR. POLAND: Okay.
MR. KEENAN: -- and get what I need.
THE WITNESS: Okay.
MR. KEENAN: So I'm thinking maybe we can go until a convenient time for lunch and then break and then come back, you know.
MR. POLAND: That's fine. Sure.
Q. Okay. So back to Chen and Rodden.
A. Uh-huh.
Q. Are you familiar -- were you familiar with Chen and Rodden's work before you were retained to be an expert in this case?
A. Yes.
Q. Okay. And is Professor Rodden a colleague of yours at Stanford?
A. He is. And Jowei Chen was -- is a graduate of our Ph.D. program.
Q. Okay. So I see that you said you respect their contribution to the field; is that correct?
A. Yes.
Q. Let's go to the first critique about simulating

lawful plans. And I take it your criticism is that it doesn't account for majority/minority districts. It has to be created under the Voting Rights Act; is that correct?
A. That's correct.
Q. Okay. Do you have an opinion on whether if Chen and Rodden did account for the Voting Rights Act, whether that would make their results more or less advantageous to Democrats?
A. I don't have a view on that, no.
Q. Okay. Do you know is there literature in the field about whether needing to create majority/minority districts hurts Democrats' abilities to convert statewide vote totals into seats?
A. Yes.
Q. Is there?
A. Yes.
Q. And what does that show?
A. Well, there's a debate. There's a -- that -- that in -- you know, one of the -- and the way I'd characterize it, this is a debate that's been around since I was in graduate school. I remember being exposed to this. But in the name of creating majority/minority districts, you're

inadvertently engaging in -- in packing, and it's pretty simple, pretty simple argument.
Q. And the argument would be that the minorities who are -- minority voters who are in the minority -- majority districts are strong Democratic voters?
A. Yes.
Q. And then you're required to create a district that has a large number of those so that they can secure the representative of choice and, therefore, you're packing Democrats into a district?
A. That -- that's the way the debate goes. That's one of the opening salvos in what's a pretty lively debate inside the profession, yes.
Q. So it's a lively debate. You'd say there hasn't been a resolution one way or the other?
A. Well, it's almost a normative question. I think that's helped -- contributes to its liveliness. You're balancing two things that people care about. One is more minority representation versus not creating lopsided districts and -- yes.
Q. As an empirical matter, is there still a debate as to whether minority/majority districts end up packing Democrats into -- into districts?
A. I -- I wouldn't like to be drawn into trying to

characterize the literature on the -- on the spot.
Q. So we can move on to your second criticism --
A. Sure. Sure.
Q. -- that Chen and Rodden used presidential election results.
A. Yeah.
Q. Are presidential election results indicative of what state legislative election results would be?
A. No. There's considerable divergences.
Q. What's the, I guess, magnitude of the divergence?
A. Oh, again, I'm not a -- I couldn't authoritatively answer that for you. But the mechanism is typically a couple of things. One is -- we're talking about different districts, so it's -- it's -- you know, it's not always -- it's sometimes a technical feat. We're, you know, getting votes for Congress at the level of state legislative district. That's -- that's a technical issue that you can solve or you can't.
But then -- then the more operative factor, I think, is -- is the different incumbency advantages operating on different levels. You might have a Democratic incumbent for a state and you might have a Republican incumbent in the -- in the -- because it's a -- you know, up at the

corresponding congressional district, and so that tends to muddy the waters. And then you also have the fact -- and tiny number stats. This isn't such a big issue. They're off sequence sometimes. Some states go on numbers -- with the off -- off the first state legislative elections. That's not a huge issue, but just yet another complicating factor here.
Q. In terms of establishing a partisan baseline that was not contingent on incumbency effects, would the presidential election results be useful in determining that?
A. Yeah, and that's -- I would tell you is the industry standard for precisely that reason. It's the same two candidates appearing everywhere, and that's why scholars in the field prize those sorts of data. Presidential vote aggregated by, complete the blank, and we're always in search of, you know, state legislative, congressional, county. People -- people really value that sort of data.
Q. Okay. So an analysis that used presidential election results as an input would be relevant to determining the -- the nonincumbent partisan baseline of -- of a particular geographic area?

A. And, indeed, that's precisely the role that presidential vote aggregated to X plays in many redistricting matters, yeah.
Q. And going on to that -- the last sentence in the -- in the paragraph, it says, "In fact, this is exactly what seems to be occurring at the congressional level. Efficiency gaps are about 6 percent more Republican when they're calculated using" --
A. Yeah.
Q. -- "when they're calculating using presidential data than when they are computed on the basis of congressional election results"?
A. Yeah.
Q. Where did you get that fact from?
A. I believe that's a number I found in Stephanopolous and McGee.
Q. Do you know if there's a similar figure for -- for state legislative elections?
A. Versus presidential?
Q. This is for congressional level.
A. Yeah. I got your question now. And the answer is no, I don't, offhand. No, I don't.
Q. All right. Then moving to the third paragraph starting, "Third, Chen and Rodden's simulated maps

do not constitute a representative sample of the entire plan solution space." What do you mean by that?
A. Okay. There's another lively debate inside political science at the moment and as to whether the Chen and Rodden algorithm, in fact, will discover all possible plans. As we might say, to borrow an analogy, the jury's out on -- on that. And I know scholars at Princeton have a different view and there's a sense that we're going to need, perhaps, computer scientists and big-iron computing to maybe sort this one out. But I think there's -- it would be fair to say that there's some -- we don't know whether -- and there's reason to doubt that the Chen and Rodden algorithm generates an exploration of all possible plans.
Q. Is there any research as to whether a different algorithm would lead to different results than the ones that Chen and Rodden discovered?
A. This is very early days in the automated computer-generated redistricting world, so we don't have a lot of guidance on a question of that specific gesture.
Q. So just to be clear, it's not clear whether that would affect Chen and Rodden's results one way or

the other more favorable to Republicans or less favorable?
A. That's right. I think that's fair, yeah.
MR. KEENAN: Mark this as 59.
(Exhibit No. 59 marked for identification)
Q. The first question on Exhibit 59 is if you could identify what this is?
A. This is a paper by Fifield, Higgins, Imai, and Tarr outlining their attempt at automated -- using a computer to explore the space of all possible redistricting plans.
Q. And is this -- is Exhibit 59 the article that's referenced on page 21 of your report in the paragraph starting third where it says Fifield, et al, 2015?
A. Yeah. That's right. That's right.
Q. Do you know if this article is -- has been published in a journal?
A. I don't know the answer to that.
Q. Okay. And so you don't know if it's been -- if this article's been subject to a formal peer-review process?
A. I -- I -- I don't know the answer to that. It may be in the midst of it right now, but -- but I -- I

don't know. I saw it -- this is the form I've seen it in. I haven't seen an update.
Q. And when did you first become aware of the Fifield article?
A. Ooh. Oh, first half of '15, I think, first half of 2015.
Q. So that would be before you were retained as an expert in this case?
A. Right around there. Certainly, my interest was -- was piqued by the prospect of -- of -- of coming on, and I know quite well one of -- one of the authors and they were taking a shot at one of my colleagues, so I -- I -- I took it -- I took it -- I took an interest.
Q. So which author do you know?
A. Kosuke Imai. He's a professor at Princeton.
Q. And then the "shot" you're referring to would be Professor Rodden?
A. Yeah. Yeah.
Q. Okay.
A. Yeah.
Q. Although, I note that in the notes it says they thank Jowei Chen for useful comments and suggestions.
A. Oh, there's plenty of that in our business.

Q. So I think we can put this one aside.
A. Okay. Okay.
MR. KEENAN: We'll mark this one as 60.
(Exhibit No. 60 marked for identification)
Q. We were going to move down for the -- your next critique, which references an article by Fryer and Holden. So I've marked the document as Exhibit 60. Can you identify Exhibit 60 for us?
A. Yes. This -- this is the paper by Fryer and Holden looking at the relationship between respecting compactness criteria and various measures of the quality biasness, whatever. I mean, it's a little imprecise, the bias of redistricting plans.
Q. When did you first become aware of Fryer and Holden's research that's reflected in this article?
A. Richard Holden hails from the same country as I do. He's a professor of -- in the -- at the University of New South Wales in Sydney, Australia, and I ran into him -- I've never been introduced to him and I was -- somewhat thought I'd be curious to meet someone from

Australia, and he's an economist by training. That's why our paths had never really intersected before. And as we started talking, I didn't -- he -- he mentioned to me that he's actually done work on redistricting, and I said, "That's great. Send me a paper." And he did, and that was about, oh, first half of last year as well, yeah.
Q. And you said he's an economist, correct?
A. Uh-huh.
Q. So he's not a political science Ph.D.?
A. No, he's not. No.
Q. Do you know about his coauthor here, Roland Fryer?
A. No. I don't know much about Roland Fryer.
Q. What's your understanding of what Fryer and Holden did in this article which is titled "Measuring the Compactness of Political Districting Plans"?
A. Yeah. Sure. Well, look, I think the key takeaway is -- is -- is to show that if you go after -- if what you try to maximize is compactness, what -- you know, what does that do with the -- with these automated algorithms. So if that was a criteria that you paid most attention to, what would be the consequences for the -- what sort of plans would -- would -- would you generate, is can you -- can you -- can you make a strong statement about that?

And their strong statement is that you get smaller measures of partisan bias almost always.
Moreover, the responsiveness of the electoral system that you get under maximally -- by trying to maximize compactness, and by responsiveness, remember, we mean how your seat share changes as your vote share changes. They find that that goes up as well.
And I think what this paper -- I think it just speaks -- I mean, the sequence of papers we've just seen in Exhibit 59 and 60 speaks to, I think, the unsettled state of the literature at the moment with respect to what one gets out of automated redistricting plans, the state of the art there and how it links up with the things we care about in -- in -- in the -- in the redistricting.
So getting your computer to draw lines is one thing, what criteria are respecting as it does so, and what sort of plans does it produce? We're slowly filling that in as a body of knowledge, and Fryer and Holden is a contribution to that ongoing exploration in the field.
Q. Is it your understanding that Fryer and Holden generated multiple different districts in a state

or just one districting plan?
A. Well, I thought they -- my understanding is they went for the maximally compact one.
Q. So that would just be one -- one plan that was the most maximally compact?
A. That's my -- that's my recollection of the paper, yes.
Q. And then they only looked at -- and their plan was for congressional elections; is that correct?
A. I believe so. Yeah.
Q. And, I believe, it was just for the 2000 congressional elections in California, New York, Pennsylvania, and Texas; is that correct?
A. I'll just verify that. Yeah. They're -- they're examples, right? There's two parts of the paper, the theory, but then actual application to -- to quote/unquote real -- real elections is limited to those -- to those cases, yeah.
Q. And then, as I understand it, they compared the results of their maximally compact plan in terms of bias and responsiveness to the plan that was actually in existence in those states --
A. Yeah.
Q. -- for the 2000 election; is that correct?
A. That's correct.

Q. Okay. So the statements about --
A. Oh.
Q. -- bias being slightly smaller in all states except one and the statements about responsiveness are comparisons between the Fryer and Holden maximally compact districts and then the districts that were actually in place in those four states?
A. Yeah.
Q. Okay.
MR. KEENAN: I think now might be a good time to break for lunch.
MR. POLAND: Break right now? Okay. Let's do that.
(Recess)
MR. KEENAN: Go back on the record.
Q. We're back from our lunch break. And I see, Mr. Jackman, I think you have the numbers we were looking for of the average -- efficiency gaps for the plans as put in place by Democrats, Republicans, and other units for the various decades. So why don't we go through those.
A. Yeah.
Q. You can give me the numbers.
A. Exactly. So of the three decades and three numbers -- and they are, as you said, the average

efficiency gap in the corresponding decade or plans in place corresponding to the top of the redistricting cycle at the start of the decade.
So let's start with the 1990s with plans that fall into that omnibus other category. The average value of the efficiency gap is negative .029, or if -- for clarity, I'll read these as percentages, so minus 2.9 percent. Same decade, 1990s, Democratic control, 4.4 percent.
Q. And that's positive?
A. Positive, yes, consistent with, yeah. Republican control, negative 6.7 percent is the average. Okay. 2000s now, in the same order, other, Democrat, Republican. Other, negative 1.7; Democrats, negative .4.
MR. POLAND: Do you want to say percent just to make it --
A. Percent, negative .4 percent; Republican, negative 4.8 percent. 2010s, other is negative 1.3 percent; Democrats 2.1, and Republicans negative 8.1 percent. So that should be nine numbers three by three.
Q. Okay. And so if I understand this, the efficiency gap -- the average efficiency gap for the plans in the other category has been negative in each

decade?
A. That is correct. That's what I just read to you.
Q. Okay.
A. By a small quantity and lying between the Democratic number and the Republican number.
Q. Is it your opinion that the distribution of partisans geographically is a neutral factor even though the efficiency-gap plans instituted by other bodies has consistently been negative since the 1990s?
A. I'm sorry. Just repeat the question.
Q. Sure. Does the fact that the efficiency gap has been negative -- the average efficiency gap has been negative under the other category plans consistently since the 1990s, does that show you that the distribution of partisans geographically weighs against Democrats?
MR. POLAND: Object to the form of the question.
A. Well, I'm not quite sure what premises or what assumptions we're making about the distribution of partisans over the -- over the three decades.
Q. Sure. Wouldn't you expect if, you know, the normal efficiency gap was going to be zero, that the average for the other category would be about

zero?
A. It -- it -- it is about zero. It's -- I mean, it's very close to zero.
Q. And if we look at Figure 9 --
A. Sure.
Q. -- which is the graphical representation of this --
A. Uh-huh, uh-huh, uh-huh.
Q. -- the 2010's decade predicted number --
A. Uh-huh.
Q. -- the dotted line, that prediction is based on an assumption that the Republicans would only have drafted 10 percent of plans in existence?
A. Uh-huh. Yes.
Q. And that Democrats would have put in place 30 percent of plans?
A. Yes.
Q. And that neutral bodies would have put in place 60 percent of plans?
A. Right.
Q. And with that distribution of control over the districting processes, wouldn't you expect that the average efficiency gap would be positive given that Republicans are only implementing 10 percent of all plans?

A. Another hypothesis might be that the plans they are implementing are especially favorable to them.
Q. So much so that even though they constitute only 10 percent of plans, they have that much effect on the average?
A. Well, under the counterfactual scenario they have that. But the -- perhaps one of the -- if I -- you know, it might be helpful to also realize that the prediction for 2010 is almost the same as the actual for the 1990s, right? So, to my mind, one of the takeaways from this analysis is that factors that might have changed between 1990 and 2010, one of those I often hear advanced is the change in political geography, would seem to me that you can explain a lot of movement by -- if we -- if we -- we get back to the same level of -- it's -- it's about who controlled it -- the redistricting would seem to be the -- you know, the compelling factor if one had to explain why it is the efficiency-gap numbers look the way they do now versus the past.
Q. And one thing that changes over time in this analysis is the category in which a state will fall into in the analysis in the different decades?

A. That's right, as revealed by Figure 8, yes.
Q. We can go to No. 7 --
A. For sure.
Q. -- which is your analysis of Sean Trende's report. I think it may be helpful in this one to have a copy of your first report handy and we can look at -- it's the table of the unambiguously negative -- or unambiguous-as-to-sign plans, which is what's discussed here.
A. Yes. Can you give the actual table --
Q. Yeah.
A. -- in the back or page number it appears on?
Q. Here, page 55.
A. Thank you.
Q. Table 1. And so your analysis finds that of these 17 plans, 5 of them were enacted with unified party control over the districting process?
A. Yes. That's right. That's right.
Q. And so then the implication of that 12 of the 17 plans were implemented without unified partisan control over redistricting?
A. Right, right.
Q. Okay. And so you've listed the five that were enacted with unified partisan control on pages 22 and 23, correct?

A. Correct. That's right.
Q. Okay. So given the fact that 12 of these plans were enacted without unified partisan control, you'd agree that an unambiguous-as-to-sign efficiency gap can occur in the absence of any partisan gerrymandering at all?
A. Well, I'd say this is -- efficiency gaps without ambiguous sign are -- are an element of what constitutes a partisan gerrymander; are necessary but not sufficient for the definition. So I -- I guess, strictly speaking, I would disagree with your statement. Without this I wouldn't say we have a partisan gerrymander, but I think we'd need this -- this is an important constituent development on the way to calling something a partisan gerrymander.
Q. Sure. But there are plans that have been put in place represented on -- in Table 1 --
A. Uh-huh.
Q. -- that presented unambiguous efficiency gaps that were not the product of any sort of partisan gerrymandering on behalf of the districting body?
A. If by partisan -- if partisan intent is equated with control of the redistricting process, which party controlled it, that's right. But I'd agree

with you -- your conclusion. But, like I said, this is an element of establishing whether or not we have a partisan gerrymander. It wouldn't -- it's -- it's not unnecessary, but not sufficient condition.
By that -- so that that there may be ways, and this is not a domain in which I'm an expert, of establishing partisan intent that go beyond simply reading off which party we deemed to have had control of -- of -- of the process.
Q. Okay. And so I'm just going to go through the ones that were identified as having unified partisan control.
A. Uh-huh.
Q. So that's Florida's plan in the 1970s, which I see is the bottom --
A. Uh-huh.
Q. -- listed?
A. Uh-huh, uh-huh.
Q. And we have Florida's plan in the 2000s?
A. Which appears?
Q. At the very top.
A. Uh-huh.
Q. Michigan from the 2000s?
A. Uh-huh.

Q. New York in the 1970s?
A. Uh-huh.
Q. And Ohio in the 2000s?
A. Uh-huh.
Q. And it's your opinion that these state plans are accurately captured by the test, because they had a large initial efficiency gap and then also never changed sign; is that correct?
A. That's right; and, moreover, the reason I singled out these plans is because, as we've discussed earlier, taking into account the -- the confidence intervals and the uncertainty attaching to any efficiency-gap estimate, these -- even taking that into account, these came nowhere near close to ever generating an efficiency-gap estimate with the opposite sign to the ones indicated in the table.
Q. Now, have you taken into account the fact that for Michigan, New York, and Ohio, that those plans also appear on this chart for other redistricting periods --
A. Oh.
Q. -- in a circumstance for which there was no partisan control over the districting process? For example, I see New York is on here four

different times, I believe.
A. Uh-huh, uh-huh.
Q. You've identified the Michigan 2002 plan?
A. Uh-huh.
Q. But the Michigan 1992-to-2002 plan also appears on here; is that correct?
A. Uh-huh.
Q. And then Ohio, you've identified the 2002 plan, but the 1994-to-2000 plan also appears on here?
A. Uh-huh.
Q. Do you have any opinion on how that should affect your analysis of whether the plans implemented with unified partisan control should be seen as partisan gerrymandering?
A. None other than to say I think this is a piece of evidence in support of, you know, whether you have a partisan gerrymandering; I think in these particular cases quite compelling. I think the other important component would be to establish partisan intent through other means, one of which may be partisan control over the process.
But, again, I'm -- I'm straying into a part of this matter that -- that -- where my expertise starts to run out as to how one might establish partisan intent -- partisan control. I can well

imagine, indeed, all of us have two, that would be a critical element of it, but there could well be others.
Q. **Do you have any opinion on whether each state should be judged on different efficiency-gap criteria -- whether states should be judged on the same efficiency-gap standard or whether a different standard should apply to different states?**
A. No.
Q. **But you'd agree with me that the effect on voters or a political party that is disadvantaged by a plan is the same regardless of whether that plan was enacted with partisan intent or not?**
MR. POLAND: Objection; compound.
Q. **Did you understand the question?**
A. If you could repeat it?
Q. **Sure.**
A. I -- I -- okay.
Q. **He can make some objections to the form of my question. It probably was a bad question, so I'll re-ask it. But if you do understand it, you can go ahead and answer when he does that. Will you let me --**
A. Sure.

Q. **-- recollect my thoughts to see what I was asking you about?**
MR. KEENAN: Could you read back what my question was? I may then rephrase it, but --
(Previous question read)
MR. POLAND: Same objection just for the record. You can answer.
A. The efficiency gap measures the consequences of a districting plan and the partisan advantage thereof. It's -- it's a consequence of a districting plan, I think a separate line of inquiry, but not unrelated one, obviously, is to do with -- you tackle the question of intent.
Q. **And, I guess, my question is aimed at the consequence the efficiency gap is measuring is the same regardless of what went into enacting that plan?**
A. Yes.
Q. **And your analysis -- your historical analysis in both the -- in the initial report -- your historical analysis in the initial report measured those consequences irrespective of -- of what type of body enacted the plan?**
A. Yes.

Q. **Moving on in the Trende section of the report -- that's Trende, T-r-e-n-d-e -- there's some discussion here of the differences between the efficiency gap --**
A. Oh, yes, yes.
Q. **-- as calculated in congressional plans and with respect to legislative plans and how it works differently. Did you -- is your -- are your opinions in that -- those paragraphs based on the reasoning in the Stephanopolous and McGee article on the efficiency gap?**
A. Yes, because they are, at this stage at least, the canonical piece of scholarship on the performance of the efficiency gap in that set, and that is the congressional elections setting.
Q. **And, basically, your criticism is that the raw efficiency data should be translated into a number of congressional seats affected?**
A. Up at the congressional level, that's right, and that's -- well, I can elaborate as to why, but --
Q. **And I believe that's in your report --**
A. -- I did in the report, yeah, yeah, yeah.
Q. **-- so I don't need to you repeat what's already in there.**
But would you agree that analyzing how the

efficiency gap works in congressional plans even without converting to seats would shed light on how well the efficiency gap measures partisan gerrymandering?
A. With -- with one important caveat and, I guess, the heart of what that is about; and that is, it's just some states just have so few congressional seats, although they may have many numbers of seats in their state legislature. If we could get up to a state -- larger states and -- you know, let's hark back to the Fryer and Holden, please, for instance. The four states that they chose to look at were all states with large populations and, hence, large number of congressional seats. That's where we're more apples to apples, if you will.
There's still a caveat, though, that the state delegations are part of a larger body in D.C., but that would be sort of a fairly strictly circumscribed set of circumstances where I would think analysis of the efficiency-gap's properties up at the congressional level starts to match up as roughly comparable, perhaps, to what I did with state legislatures.
Q. **Okay. And then you -- further on on page 25 you**

discuss the difference between substituting presidential election results and then using them as an imputation for -- for the results, and we went over last time in your deposition the imputation model you used.
A. Uh-huh.
Q. My question is how big of a difference does it make in determining the vote total of an uncontested seat?
A. I -- I -- I can't give you precise answer. I do know that incumbency, particularly congressional elections, is thought to be, you know, a critical -- critical variable, and that no serious scholar of congressional elections would ever ignore it in modeling congressional election outcomes.
Q. And you say that it produces -- Trende's method would produce errors. I believe it says --
A. Well, certainly less credible.
Q. I was just going to say what -- an error as compared to what?
A. Excuse me?
Q. You say that Trende's method is guaranteed to produce errors.
A. Yeah, yeah, by omitting -- in omitting a variable that everybody in the literature agrees is -- is

critical, such as incumbency. Moreover, just to elaborate this point, the congressional setting is -- is we have a lot of data aggregated up to the level of congressional seats, census aggregates, in a way that are sometimes sketchy for state legislative districts, and that literature also makes a lot of use of those variables. So simply substituting presidential vote at the level of congressional district is -- is -- is a long way from what I think -- where the literature or -- or, you know, what -- how -- you -- just how models for congressional elections are done in -- in political science.
Q. And this is modeling the vote totals for an uncontested seat as if it were contested?
A. Well -- and, indeed, to do that, though, one uses the data in the contested ones to help you extrapolate out, so that's -- that's right.
Q. And so what -- is there an average incumbency advantage in congressional races that's applied, 5 percent, 6 percent, anything like that?
A. Well, it is not plugged in. It is estimated as you go; and that's kind of the point, that it does vary cycle to cycle. But it's something you don't have to make an assumption about. But it's --

recent estimates of incumbency advantage have been close to those numbers you just gave to me.
Q. 5 or 6 percent?
A. In the old days, we used to say 8 and, if anything, it's probably come down a little bit. But the point is you -- you estimate it, you know.
MR. KEENAN: Another exhibit.
(Exhibit No. 61 marked for identification)
Q. And while you're reviewing Exhibit 61, my first question is going to be if you can just identify what it is.
A. It's an email from -- it's copy of an email from Nick Stephanopolous to myself and some other parties cc'd.
Q. And is it your understanding that this email contains a list of the tasks that you were to carry out in your rebuttal report?
A. Yes.
Q. I'd like to direct your attention to No. 2 in the email.
A. Right.
Q. And then there's a sub D at the end of that paragraph --
A. Right.

Q. -- where it says, "Addressing the validity of the Trende analysis of political geography (paras 62 to 105) which relies primarily on data on Wisconsin counties and wards."
A. Uh-huh.
Q. Did you do any analysis of Wisconsin counties and wards in trying to determine the political geography of Wisconsin?
A. No. I did not.
Q. And did you do any analysis in attempting to determine why Wisconsin saw the efficiency gaps it did over the course of the 1990's and 2000's court-drawn plans?
A. No. I did not.
Q. Put that one aside.
A. Okay. Oh, okay.
MR. KEENAN: Go to the next exhibit, 62.
(Exhibit No. 62 marked for identification)
Q. Could you identify Exhibit 62 for us?
A. This is a supplemental or an extra piece of analysis that I ran looking at the sensitivity of the efficiency gap to -- to uniform swing.
Q. Is there a reason why this analysis was not

included in the rebuttal report?
A. Overcommitment on my part. It wasn't -- we weren't quite -- haven't got to it.
Q. You mentioned the term "uniform swing"?
A. Yep.
Q. Could you define what that is?
A. Certainly. Uniform swing in political science refers to a method for constructing counterfactual elections by taking the set of seat shares -- vote shares we observe across seats in a given election and then shifting them all by the same quantity either up or down mimicking a jurisdiction-wide swing; and the word "uniform" arises there because the same swing is being applied to every seat. So it's a very simple technique that assumes away the fact that, you know, in a real election, election to election, the different seats swing by -- by -- by different amounts. And just to be clear, the word "swing" here, also, what do we mean by that? We mean the difference in an election outcome, election one to election two.
Q. And are we looking at the two-party vote share for each candidate in addition?
A. Exactly. So that's the number when we have a bunch of those numbers over each seat, and then we

shift them all up to the left or down, you know, to the right.
Q. And in your report, you state that it's considered to be a simplification. But that it still is a useful tool. Why is it still useful even if it's a simplification?
A. Because it's so easy to do. You can code it up and it zips along extremely quickly and it saves you from -- if you're going to have -- if you're open to the possibility that every -- the more -- frankly, the more politically realistic assumption that each seat is going to change by a different amount from any other seat, then where is that coming from? So instead of now you manipulating many parameters, potentially one for each seat, versus just one for the whole jurisdiction-wide swing. So despite some mythological critique over the years of this technique, it enjoys a long life in political science, and there's a reason in this context as well.
Q. And there isn't currently an accepted methodology of figuring out the amount of swing that would occur in each district individually, is there?
A. The closest we have on that is a work by Gary King and Andy Gelman going -- who originally tried to

get us away from uniform swing back in the -- with a -- with a particular view to redistricting questions in the -- in the 19 -- early 1990s. Their approach makes -- is -- is -- you have to know a lot of statistics and modeling to implement it. You also have to have a lot of data that can inform your best guesses as to -- informed by the model, of course, as to how individual seats differ. And the second fact to note, at least at the presidential level, and -- and it's an open question to how much this has happened at Congress or down at state legislature levels, but a funny thing has happened to the United States since the 1990s; and, that is, uniforms -- swings have become more uniform certainly at the presidential level. So that is sort of reality, as it were, or sort of undercut kind of the -- the mythological imperative there to do better.
And so given that it's so fast to do and it sort of kind of works certainly up at one level of American politics, it -- it -- it still is a go-to method for -- for many people in the redistricting world.
Q. And if I understand -- just so I understand it correctly, in your uniform swing, there's swings

of plus and minus?
A. That's right.
Q. Is the plus -- the plus Democratic vote?
A. Exactly, yes. Plus means in a Democratic direction and negative means in a Republican direction.
Q. And so, for example, in a -- if a seat was one with 50.3 percent of the vote by Democrats and a plus-one swing, you'd make that seat 51.3 percent Democratic?
A. Exactly right.
Q. And then --
A. And the same shift for every seat. And we typically cap it. If a seat is going to go above 100, we can't -- we -- we typically truncate them at a 100 or don't let them go below 30, but you've got the idea right.
Q. So why don't you explain the uniform swing analysis you did that's reflected in Exhibit 62.
A. Okay. Well, there were various components to it; and, essentially, what I set out to do was to demonstrate another robustness check, if you will; how -- we -- we observe -- here's the problem. We observe a value for an efficiency gap in one election, and our problem is we'd like to know how

prognostic that is of -- of what we might see under the plan. And my initial report provided a lot of analysis on that sign flipping and -- and we've talked at length about that.
There's another way you might approach that problem. That is to ask, well, take that election as given and ask, well, let's perturb that election that we actually got and suppose, you know, there's a swing to the Democrats of X percent or a swing away from the Democrats of X percent, what sort of efficiency gap would we get then? And that's -- that's not an unreasonable way to approach this.
The one -- as -- as we've been talking, as we've been discussing, this -- the method of uniform swing is a device for generating counterfactual or hypothetical elections based off an observed set of election results has a -- has a long and durable legacy in -- in the political science world.
Now, so what I did was to say, you know, in response to criticism of -- of why didn't we do that, was one of the criticisms of -- of my initial report, so we did it. I did it.
Q. And maybe I could just stop you and just -- so you

-- you have an initial efficiency gap of the actual election, correct?
A. Based on an actual election.
Q. And then you did uniform swings of different amounts --
A. Uh-huh.
Q. -- on that same election?
A. Yes.
Q. And then you recalculated the efficiency gap based on the uniform swing?
A. Yes, under the new scenario; because note what happens, by the way. As you shift those seat shares by some amount, some now flip past 50, right, and the seats that you originally were saying were going to be Democratic wins become Republican wins or vice versa. So remember the efficiency gap compares seat shares against vote shares, essentially, and so that's why the efficiency-gap numbers will change as you -- as you change the level of statewide vote share. You're also changing who wins seats.
Q. And so just as an example, on a 2.2 percent swing in favor of the Democrats, they would end up winning additional seats -- any seat which they -- which they had a 48 percent share or great -- up

to 50?
A. You've got it exactly. Any seat that previously was within that window now will either go right up to 50 or over. That's right, yeah.
Q. And then in terms of measuring the efficiency gap, the expected seat share will also change; is that correct --
A. Well --
Q. -- based on the vote share?
A. Well, it's purely -- the allocation of seats given votes is purely deterministic, right? So if -- right? If we're talking -- we're in this two-party world. The magic number's 50. If I'm above 50, I win the seat. If I'm below, you win it. And we can just as we move -- as we move vote shares up, now some are more -- more -- more seats are falling over that threshold or fewer depending on however.
Q. Yeah, and I understand that. But then in terms of then calculating the efficiency gap on the --
A. Oh.
Q. -- uniform swing, if Democratic vote went from 50 to 52, the Democrats are now expected to win -- are judged against whether they won 54 seats, correct, because that's what the zero efficiency

gap hypothesis line would call for; is that correct?
A. That's correct. Very good, very good.
Q. Okay. First, why don't we just look at Figure 1 --
A. Uh-huh.
Q. -- and you can explain what these various -- it looks like it's a similar figure multiple times. So maybe we can just look at the first one, swing plus .20, and explain what -- what's reflected here.
A. Yes. So -- right. So there's a variety of swings presented there, but the one on the top left corresponds to where we perturb election results just in -- right? And this is just down on -- on elections in 2012 and 2014, so there's a relatively small number of elections. Each one has an actual efficiency gap corresponding to their actual election outcome, right, the actual election we observed, and so that's what's plotted on the horizontal axis, right?
And then on the -- on the vertical axis is the efficiency gap for that election you get if you apply the designated level of uniform swing. And to use a graphical convention I've used

elsewhere today, the black line in each panel is a 45-degree line, right? So if all the efficiency gaps lined up were the same as the actual ones -- and by the way, the limiting case there is right in the middle of the plot where the uniform swing is zero. We're basically -- that's the trivial null case, if you will. We're just replicating the same election. All the data are on the 45-degree line there. And then the idea is to see -- as -- as we get different efficiency gaps under higher levels of -- of uniform swing, we will start to -- we should expect to see and we do see efficiency gaps looking increasingly different from the ones we got under the actual election. And the goal of this analysis is to sort of understand the pace at which that happens. Higher and higher levels of uniform swing will -- will have to generate different election outcomes. Possibly different values of the efficiency gap would be astonishing if they didn't. The real -- the real thing to -- to try and understand is how much you have to change the election you got to get something different with respect to the efficiency gap.

Q. And is it at a certain point in the uniform swing

where that difference starts to emerge?

A. Yeah. Just purely seat of the pants here. This is not especially rigorous. But the middle-road swings that aren't especially large, right, you see very little -- the data are almost indistinguishable. And, in particular, keep in mind that any given efficiency gap, because of uncontestedness, is equipped with some uncertainty. You know, where the -- the changes in the uncertainty -- in the efficiency-gap measures that we're getting actual to simulated under different levels of uniform swing, that change is often not large relative to your uncertainty about the efficiency-gap number in a given election to begin with.

So you've really got to go out to quite large swings, two and a half, threes, and higher, before that data starts to really open up and we're starting to see considerable divergence from an actual efficiency gap to a hypothetical efficiency gap that might have arisen had the state swung three points one way or the other from -- from what we actually saw.

Q. Why don't we turn to Figure 2.

A. Yes.

Q. And, again, we have a series of somewhat similar graphics. Maybe you could explain what each of these graphics represent.

A. Certainly. So it's the same exercise just with summarizing a different output, right? So, again, we're perturbing observed election results by different amounts of uniform swing with the actual election, of course, being, again, the trivial null case corresponding to a uniform swing of zero in the middle of each panel. The top three panels report the correlation between actual efficiency gaps and the efficiency gaps observed under hypothetical levels of uniform swing across that range of simulated values of uniform swing.

Moreover, the data are broken into three chunks: Elections that had a low value of the efficiency gap, and by that I mean less than .03; medium -- in absolute value. Medium levels of the efficiency gap, and that's in the middle two -- the middle column of the figure, and by medium levels of the efficiency gap I mean .03 to .07 in absolute value. And the column on the right shows us the case of where we began with an election that was exhibiting a high efficiency gap above .07 in absolute value.

And the -- let's just take the first row. The correlations stay between -- actual and simulated efficiency-gap estimates are quite high as we shuck the actual elections even with quite large values of uniform swing. So the takeaway there, say, the top right panel, if you had a high value of the efficiency gap and you considered a fairly broad range of alternative elections held under the same plan, in fact, generated through this methodology called uniform swing, you would end up observing hypothetical values of the efficiency gap that look an awful lot like the ones you actually got.

The efficiency-gap measure is -- is quite robust when it's high to begin with. When it's low, it doesn't take much uniform swing to come up with an efficiency gap value that in some cases has the opposite sign, or even after a while starts to bear very little reliable relationship with the original set of efficiency-gap estimates. So now I'm referring to the top left panel of Figure 2 where some of those correlations start to fall away toward zero. And, remember, zero correlation means there's no relationship between the original efficiency gaps and the simulated efficiency gaps. And about the only place we see that, right, is, again, when you take something that began life -- an election that began life with a low efficiency gap and you subject it to a fairly high level of uniform swing. So this does -- this shows, if you will, the robustness of efficiency-gap estimates as a function of how large they were to begin with to different levels of uniform swing.

The second row of Figure 2 repeats that exercise using the same sign test that I've used throughout my original report and at various parts of the rebuttal as well. And, again, just to -- to move this along, the takeaway there is -- direct your attention to the bottom right panel of Figure 2. There's a series of dots there that tell us that the proportion of simulated efficiency gaps that have the same sign as the actual efficiency gap we saw. It's essentially 100 percent, and only starts to tail away even a little once you get up to quite massive amounts of -- of -- of swing in the neighborhood of minus 5 or 5 -- that might dip down to 90, 97 or 98 percent, or something like that.

So, again, the takeaway, you begin life with

a high level of the efficiency gap. You -- you simulate other elections, even some that depart pretty radically from the one you got under this uniform swing methodology. You -- you make the same conclusion about the efficiency gap under -- under that scenario.

Q. And to be clear, all this analysis is just on the 2012 elections?
A. 2012 and 2014 --
Q. Okay.
A. -- I believe.
Q. Both of them?
A. Yeah.
Q. And --
A. Yeah.
Q. Okay. And then the correlation?
A. Uh-huh.
Q. Is the correlation number represented in Figure 2 equivalent to the difference between the slopes of the lines in Figure 1?
A. You're on absolutely the right track, okay. So if the data -- okay. So I can -- I can map you from Figure 1 to Figure 2 now. Observe that anytime the uniform swing -- okay. Figure 2, anytime the uniform swing is zero, the correlation is 1.0, and

exactly 1.0. There's no confidence interval around that. That corresponds to that middle panel of Figure 1 where we're getting back exactly the same results. So if I were to -- essentially the correlation is 1.0 there where the data coincide and will slowly get -- fall away from 1.0 as we take on larger and larger values of uniform swing towards the -- the corners of our Figure 1, yeah. So your intuition was absolutely correct.
Q. And then those lines, the lines on Figure 1 or you graphically represented, a subset of -- maybe I should say like the Figure 1 represents all plans, correct?
A. All elections.
Q. All elections. And then Figure 2 is broken down into different subsets?
A. Exactly, subsetting the data by the magnitude of the efficiency gap into three -- three classes, low, medium, and high.
Q. And then the lines on Figure 1 --
A. Are -- are all the data together.
Q. And the line -- does the line correspond to the average of all of the plans or -- I may be phrasing that wrong. So if you could maybe just explain to me the -- what the line is supposed to

fit.
A. It's a -- it's a regression line.
Q. And I don't know if you can explain that maybe in like more layman's terms.
A. So there's a line of -- if you will, that's often a delayed interpretation of regression. There's a line of best fit to a -- to two variables that minimizes some of the squared errors.
Q. So there will be plans -- or, I guess, this would be elections on both sides of those lines or both above and below the line?
A. And, indeed, we -- we can observe just as much from -- from Figure 1 if we were to sort of strain our eyes and investigate what's going on in any given panel. But by its nature, that's what regression will do. It will be trying to balance out points that will lie above the line with points that lie below the line --
Q. And --
A. -- approximate -- to a rough approximation.
MR. KEENAN: Maybe we could take a short break.
MR. POLAND: Sure. Absolutely.
(Recess)
MR. KEENAN: Go back on the record.

Q. We're back from a short break. I just have a few more questions here. Then we can send you on your way --
A. Okay.
Q. -- back home.
We put before you what's been marked as Exhibit 63. Could you identify Exhibit 63 for us?
A. It's a copy of an invoice from myself back to plaintiffs' attorneys.
Q. I believe there's -- I put two documents together. There's a two separate invoices; is that correct?
A. Let me just check the dates on them. You are correct. There are two invoices here. That's right, yes.
Q. And the last time you were deposed you produced some documents to your attorneys who gave them to me that included some invoices. Do you remember that?
A. Yes.
Q. And then does Exhibit 63 represent all the invoices after that time that you've sent to plaintiffs' counsel?
A. That's correct. Yes.
Q. And have you been paid for the invoices that you've submitted?

A. Yes, I have.
Q. Okay.
MR. KEENAN: And that's all my questions.
MR. POLAND: We don't have any questions. So we're all set.
MS. GREENWOOD: Read and sign.
MR. POLAND: Yeah. We'll take a look at the transcript and reserve signature.
(Adjourning at 2:09 p.m.)

STATE OF WISCONSIN)
) SS:
COUNTY OF DANE)

I, LISA L. LAFLER, a Registered Professional Reporter, Certified Realtime Reporter, Certified Livenote Reporter, and Notary Public in and for the State of Wisconsin, do hereby certify that the foregoing deposition was taken before me at the State of Wisconsin Department of Justice, 17 West Main Street, City of Madison, County of Dane, and State of Wisconsin, on the 16th day of March, 2016; that it was taken at the request of the Defendants, upon verbal interrogatories; that it was taken in shorthand by me, a competent court reporter and disinterested person, approved by all parties in interest and thereafter converted to typewriting using computer-aided transcription; that said deposition is a true record of the deponent's testimony; that the deposition was taken pursuant to Notice; that said SIMON JACKMAN before examination was sworn by me to testify to the truth, the whole truth, and nothing but the truth relative to said cause.

Dated March 24th, 2016.

____
Notary Public
In and for the State of Wisconsin

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Rebuttal Report

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Introduction

In this rebuttal report, I respond to criticisms made by Sean P. Trende and Professor Nicholas Goedert in their respective expert reports. I also conduct new empirical analyses further confirming the validity of the efficiency gap as a measure of partisan gerrymandering and the reasonableness of the proposed 0.07 threshold. More specifically, my principal contributions are the following:

- *First*, I respond to Goedert's various critiques of the efficiency gap and of the proposed efficiency gap threshold. Among other things, he misunderstands the relevance of efficiency gap data, cherry-picks information from my initial report while ignoring its broader context, and wrongly claims that plaintiffs' test would mandate "hyper-responsiveness" or prevent states from pursuing goals such as competitiveness or proportional representation.
- *Second*, I calculate several widely accepted prognostic measures—all based on the rates of true positives, false positives, true negatives, and false negatives—with respect to the odds of a district plan's efficiency gap changing signs over the plan's lifetime given a certain efficiency gap value in the plan's first election. Based on these measures, I conclude that the proposed 0.07 threshold is highly conservative. In fact, this threshold *sacrifices* some accuracy (which would be maximized at a lower threshold) in order to reduce the proportion of false positives.
- *Third*, I calculate the same prognostic measures with respect to the odds of a district plan's *average* efficiency gap, over its lifetime, having a different sign than that observed in the first election under a plan, given a certain efficiency gap value in this first election. Under this method, the proposed 0.07 threshold appears even more conservative, driving down the share of false positives to below 5%.
- *Fourth*, I compare the values of the efficiency gap in the *first* election under a plan and *on average* over the plan's lifetime. This relationship is impressively tight (r^2=0.73), indicating that a plan's initial bias is a very good predictor of its overall lifetime bias. For Act 43, this analysis allows us to predict that it will *average* a pro-Republican efficiency gap of almost 10% over the 2010 cycle as a whole.
- *Fifth*, I examine to what extent changes in party control over redistricting are responsible for the pro-Republican trend in the efficiency gap since the 1990s. In the current cycle, about *four times* more state house plans were designed by Republicans in full control of state government than in the 1990s. Had the distribution of party control over redistricting remained unchanged, essentially *all* of the pro-Republican movement in the efficiency gap over the last two decades

would not have occurred. It is thus changes in party control, and *not* changes in the country's political geography, that primarily account for Republicans' growing redistricting advantage over the last generation.

- *Sixth*, I address recent work by Chen and Rodden (2013), cited by both Trende and Goedert for the proposition that Republicans enjoy a natural geographic advantage over Democrats. Chen and Rodden's simulated maps are not *lawful* because they ignore the Voting Rights Act and state redistricting criteria; they are based on presidential election results rather than more relevant state legislative election results; they do not constitute a representative sample of the entire plan solution space; and they are contradicted by other recent work (Fryer & Holden 2011) finding that randomly drawn plans *reduce* bias and *increase* electoral responsiveness.
- *Lastly*, I comment on Trende's analysis of particular state legislative and congressional plans. This analysis is marked by conceptual and methodological errors severe enough to render it useless. For example, Trende ignores two of the three prongs of plaintiffs' proposed test; he calculates congressional efficiency gaps without converting them from percentage points to House seats and for House delegations too small to generate reliable estimates; and he simply *substitutes* presidential election results for congressional election results whenever the latter are missing due to uncontested races. None of this work meets accepted standards of social science rigor.

1 Responses to Goedert's criticisms

In his report, Goedert offers several critiques of the efficiency gap and of the 0.07 threshold I recommended in my initial report, based primarily on the alleged instability of the efficiency gap. None of these critiques have merit. In this section, I respond to Goedert's points relying only on the analysis of my initial report and on the existing literature. My new empirical analyses appear in subsequent sections.

First, Goedert appears to believe that a plan's efficiency gap is only relevant to the extent that it sheds light on the partisan intent (or lack thereof) underlying the plan. He writes that "such intent cannot be inferred" from a large efficiency gap, that "a durable bias . . . is not even a sign of deliberate partisan intent," and that the "efficiency gap [is] a standard to measure partisan intent" (pp. 11, 13, 19). But this is not at all the legal function of the efficiency gap in plaintiffs' proposed test. Rather, partisan intent is its own independent inquiry, and the efficiency gap then comes into play at the *second* stage of the test, to determine if a plan's electoral *consequences* are sufficiently severe that it should be deemed presumptively unconstitutional. To put it simply, the efficiency gap is plaintiffs' measure of partisan *effect*, not of partisan *intent*. Goedert's misunderstanding of this basic point infects all of his discussion.

Second, Goedert observes that of *all* plans, anytime in the decade, with a *pro-Democratic* efficiency gap of greater than 0.07, a substantial proportion of them switch signs over their lifetimes (p. 11). In making this observation, Goedert cherry-picks a single bit of data from my initial report, and an irrelevant piece of data at that. This fact is irrelevant because it applies to plans no matter when their elections were held, while the appropriate universe for plaintiffs, defendants, and courts is limited to the *first* elections held under plans. It is the first elections that typically will be used in litigation, given Justice Kennedy's admonition in *Vieth* that plans should not be struck down based on a "hypothetical state of affairs," but rather "if and when the feared inequity arose" (*Vieth v. Jubelirer* (2004), p. 420). And the fact is misleading because it applies only to pro-Democratic efficiency gaps above 0.07, and not to the larger set of pro-Republican efficiency gaps above this threshold.

If we consider only plans that exhibit a pro-Democratic efficiency gap above 0.07 in their *first* elections, the probability that they will switch signs over their lifetimes drops by about five percentage points (Jackman Report, p. 61). And if we then turn to plans that exhibit a *pro-Republican* efficiency gap above 0.07 in their first elections—a more sizeable set, for which more accurate estimates are possible—this probability drops all the way to about 15% (Jackman Report, p. 61). In other words, of plans that open with large pro-Republican efficiency gaps, close to 85% of them continue to favor Republicans in every election for the remainder of the cycle. *This* is the most pertinent data point in my report, not the one cherry-picked by Goedert, and it reveals the persistence of many gerrymanders.

Third, Goedert discusses *congressional* district plans throughout his report, even though this case is exclusively about state legislative redistricting (pp. 7-8, 10, 12, 20). In doing so, he makes some of the same errors as does Trende: namely, not converting the efficiency gap from percentage points to House seats, and improperly handling uncontested races (in his case, by not adjusting for the uncontestedness *at all*, and simply treating the races as if all of the vote went to one party and none to the other). I discuss these errors in more detail later in this report.

Fourth, Goedert claims that it is "arbitrary" to focus on the first election after redistricting, and that doing so "biases toward a finding of *EG* durability" by ignoring wave elections (p. 14). As noted above, the first election after redistricting is the critical one for purposes of litigation, since under *Vieth*, it is after this election that a lawsuit will typically commence and have to be decided by the courts. Later elections are largely irrelevant for litigation purposes, since it is unreasonable to expect suits to be brought six or eight or even ten years into a cycle. Moreover, my analysis in no way ignored wave elections; to the contrary, I determined the odds that a plan's efficiency gap would switch signs by examining *all* elections held under the plan, waves and non-waves alike. If anything, the fact that most wave elections over the last forty years have not taken place in the first election after redistricting biases *against* a finding of durability, since these elections may well cause the efficiency gap to flip signs.

Fifth, Goedert is wrong that an efficiency gap of zero represents "'hyper-responsive' representation" (p. 2). In fact, as he has recognized in his own prior work, an efficiency gap of zero corresponds almost exactly to the responsiveness actually displayed by American elections over the course of the twentieth century, under which "a 1% increase in vote share will produce about a 2% increase in seat share" (Goedert 2014, p. 3). Indeed, this correspondence is one of the efficiency gap's most attractive properties, and it explains why Goedert himself calculated a quantity nearly identical to the efficiency gap in his work (Goedert 2014; Goedert 2015).

And sixth, Goedert is wrong as well that plaintiffs' proposed test might discourage states from pursuing worthwhile goals such as competitiveness or proportional representation (pp. 6-10). If a state's aim in redrawing districts was to make them more competitive or to produce more proportional representation, then the partisan intent required by the first prong of plaintiffs' test would not be present. Even if partisan intent were somehow found, the state would likely be able to show that its plan's large efficiency gap was necessitated by its pursuit of competitiveness or proportional representation. And in any event, competitiveness and proportional representation are extremely rare objectives in American redistricting. Only *one* state, Arizona, has a competitiveness requirement, and not a *single* state has a proportional representation criterion. (And needless to say, line-drawers do not tend to seek out either of these goals on their own.)

2 Reliability of a district plan's first efficiency gap

Having rebutted Goedert's criticisms using preexisting data, I now provide further analysis of the reliability of the first efficiency gap (*EG*) observed in the life of a district plan. This played a key role in the determination of the threshold *EG* value in my initial report. In that report, I focused on the probability of a "sign-flip": that is, given the magnitude of the efficiency gap observed in the first election under a district plan, what can we infer about the likelihood that all subsequent efficiency gaps observed under that plan will have the same sign as that from the first election.

Under this approach, just one election that produces an efficiency gap with a different sign from the efficiency gap in the first election will generate a "failure," in the sense we would say that the plan has generated an efficiency gap that conflicts with that from the first election. In short, the "constant sign" analysis in my original report considers the most extreme set of efficiency gap estimates produced under a plan and insists that they have the same sign. In this sense, the "constant sign" analysis I performed is a quite stringent and conservative test of what we can or ought to infer from the efficiency gap observed in the first election under the district plan. Another approach would be to inquire as to the *average* efficiency gap over the life of the district plan. A summary statistic such as the average is—by definition—less sensitive to extreme values. At the same time—and again, by definition—the average measures central tendency or typicality, and is the most widely used summary statistic in existence. I thus consider how well the first *EG* observed under a district plan predicts the average *EG* observed over the life of the plan.

But I first provide some additional analysis of the prognostic properties of the first efficiency gap observed under a district plan. In each instance the test is whether the first *EG* observed under a plan exceeds a given threshold value. The outcome of interest is whether the plan's remaining efficiency gaps have the same sign as the *EG* from the first election. For purposes of this exercise, plans are classified as "positive" (all *EG* scores under the plan have the same sign) or "negative" (*EG* scores differ in sign). With these definitions in place, we can then classify plans according to the accuracy of the prediction implicit in the first *EG* observed under the plan:

	Actual	
Test	Positive	Negative
Positive	True Positive	False Positive
Negative	False Negative	True Negative

The prognostic measures I rely on are conventional measures of predictive or classification accuracy used throughout the quantitative sciences:

1. sensitivity, or the *true positive rate*: proportion of positives that test positive, TP/(TP + FN)

2. specificity, or the *true negative rate*: proportion of negatives that test negative, TN/(TN + FP)

3. *balanced accuracy*, the average of the sensitivity and the specificity

4. *accuracy*, the proportion of cases that are true positives or true negatives, (TP + TN)/(TP + FP + FN + TN).

5. the *false positive rate*; proportion of negative cases that test positive, 1 minus the specificity or FP/(TN + FP).

6. the *false discovery rate*; proportion of cases testing positive that are actually negative, FP/(TP + FP).

7. the *false omission rate*; proportion of cases that test negative that are actually positive, FN/(FN + TN).

Figure 1 shows how these prognostic performance indicators vary as a function of the absolute *EG* threshold (on the horizontal axis in the figure). That is, as we move to the right in each panel of the graph, the test is becoming increasingly stringent: larger absolute values of the efficiency gap in the first election under a district plan are required to trip the increasingly higher threshold. When the threshold is set to zero, all plans trip the threshold (all first-election *EG*s are greater than zero in magnitude, by definition) and so all cases test positive; in this case the sensitivity is 1, while conversely the specificity is 0 and the false positive rate is 1 (all negatives test positive).

The test has better properties as the threshold grows, with the accuracy measures maximized around absolute values of .03 to .04. Yet accuracy is not all in this context. The rate of false positives is quite high at thresholds where the accuracy is high, as is the false discovery rate. At a threshold of .03, for example, over half of plans that would go on to exhibit sign flips in their *EG*s would test positive and be flagged for inspection; of the plans selected for scrutiny, more than a third would turn out to have *EG* sign flips over the life of the plan. The .07 threshold is thus a conservative standard, the point at which the rate of false positives is becoming reasonably low (25%), without letting the false omission rate go above 50%.

It is worth noting the weight being put on false discoveries or false alarms versus the weight on false omissions in this context, which in turn reflects the conservatism and caution of the thinking underlying the .07 threshold. We propose accepting *twice* the rate of false omissions (plans that should have been scrutinized but were not) than the rate of false discoveries (plans that would be flagged for scrutiny given the *EG* observed in the first election, but would then go on to display sign flips). To reiterate: the proposed standard for judicial scrutiny is cautious and conservative, erring on the side of letting even durably skewed plans stand.

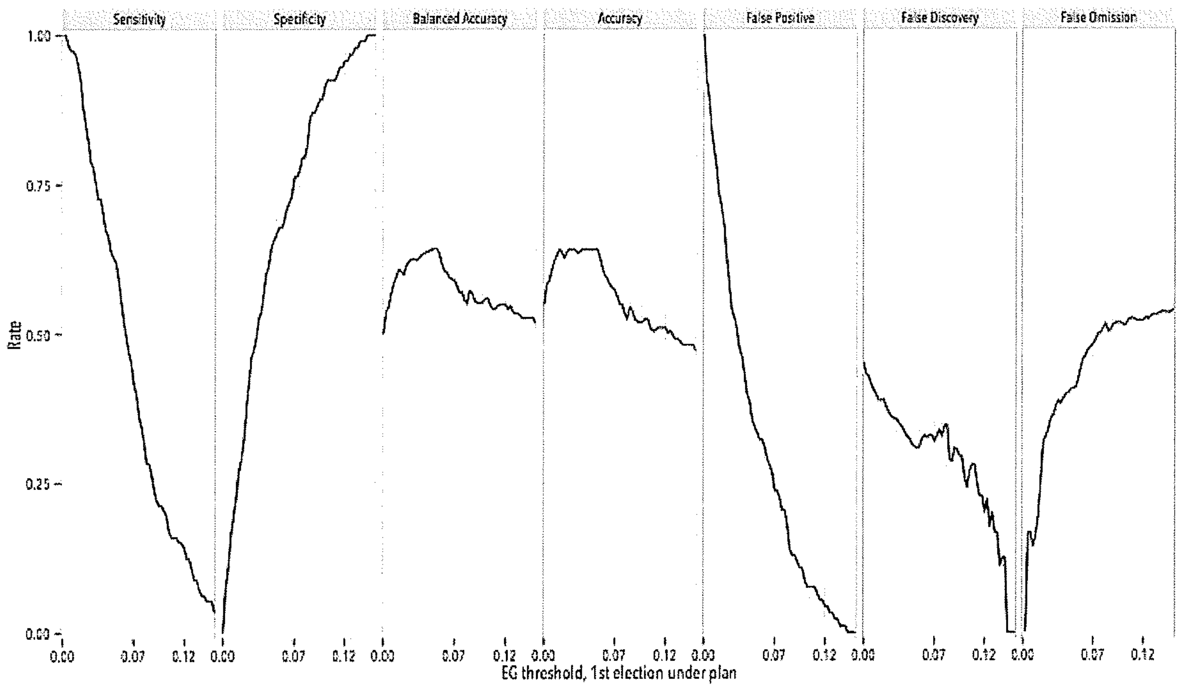

Figure 1: Prognostic performance measures, first efficiency gap under a district plan more extreme than threshold (horizontal axis) as a predictor of whether the subsequent efficiency gaps recorded under the district plan all have the same sign as the first efficiency gap. Vertical lines indicate 95% confidence intervals. Analysis spans all state legislative elections and district plans as per my initial report, 1972-2014.

Figure 2 repeats this analysis, but only considering the performance of *negative* values of the first-election efficiency gap threshold, consistent with Republican advantage (and more relevant to the Wisconsin plan at issue). Here the threshold becomes less stringent as we move across the horizontal axis from left to right, from larger negative thresholds to closer to zero at the right hand edge of each panel. With a large negative threshold (left hand edge of each panel), almost all plans test negative and so the sensitivity is close to zero, the specificity is 1, and the false positive rate is zero. The accuracy measures increase as the threshold becomes less stringent, attaining maxima in the range -.05 to -.02. Again—and consistent with the cautious approach we take—we emphasize that accuracy is not the sole criterion we use to evaluate a decision rule. At low values of the threshold, where accuracy is maximized, the false positive and false discovery rates are relatively high. On the other hand, at the proposed threshold value of -.07, the false positive rate is under 10% (fewer than 10% of plans with efficiency gaps changing signs would be scrutinized), and the false omission rate is about 35% (close to

35% of plans would not be flagged despite having *EG*s of the same sign over their lifetimes). The proposed threshold again errs on the side of restraint, tolerating a higher rate of false omissions than false discoveries.

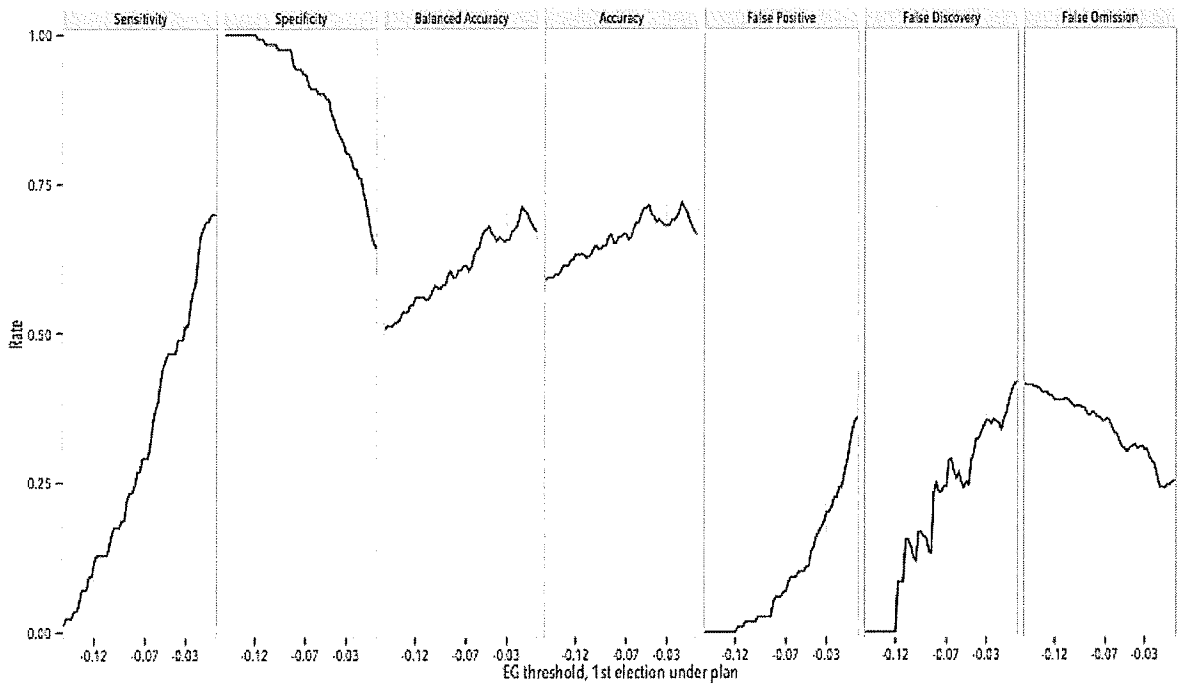

Figure 2: Prognostic performance measures, first efficiency gap under a district plan more extreme than threshold (horizontal axis) as a predictor of whether the subsequent efficiency gaps recorded under the district plan all have the same sign as the first efficiency gap. Vertical lines indicate 95% confidence intervals. Analysis examines negative, first-election threshold values of the efficiency gap, consistent with Republican advantage.

Figure 3 presents the corresponding analysis of *positive* values of the first-election *EG* threshold, consistent with Democratic advantage. Here the proposed threshold becomes more stringent as we move to the right of each panel, in the sense that fewer plans trip the threshold. At high values of the threshold (the right hand edge of each panel), no plans trip the threshold and all are classified as "negatives," leading to a specificity of 1, and false positive and false discovery rates of zero. Once again, accuracy is maximized at a less stringent threshold than the proposed .07 standard, around .03. The false positive rate is much lower at the proposed threshold of .07 than at the accuracy-maximizing threshold of .03. Note that the false discovery rates are moderately large but unstable and estimated with considerable imprecision; this is because there are so few plans exhibiting high (pro-Democratic) levels of *EG* in their first election. Moreover, of the few plans that do trip a given pro-Democratic threshold in their first election, it is reasonably likely that they will record efficiency gaps that will change sign over the life of the plan; this sign-flip or "false discovery" probability is about 35% at the proposed threshold of .07.

Comparing the analyses in Figures 2 and 3, we see an asymmetry in the results. The .07 threshold is more permissive with respect to plans that begin life exhibiting Democratic advantage than it is for plans that initially exhibit Republican advantage. At a +/- .07 threshold, the false discovery rate for plans initially exhibiting Republican advantage is under 10%, but around 35% for plans initially exhibiting Democratic advantage. As Figure 3 shows, it is difficult to find a threshold for apparently pro-Democratic plans that drives the false discovery rate to reliably low levels, if only because the historical record has relatively few instances of these types. We also note that the .07 threshold generates false omission rates of about 30% for both sets of plans.

Because the preceding discussion is somewhat technical, it is worth restating its principal conclusion: It is that an efficiency gap threshold of 0.07 is quite conservative, in that it sacrifices some accuracy (which would be maximized at a threshold of around 0.03) in order to drive down the false positive and false discovery rates. At a threshold of 0.07, in fact, the false positive and false discovery rates are about *half* of the false omission rate, indicating that there are about twice as many plans that are *not* being flagged even though their *EG* signs would remain one-sided throughout the cycle, than there are plans that *are* being flagged even though their *EG* signs would flip. This is further powerful confirmation of the reasonableness of the 0.07 efficiency gap threshold.

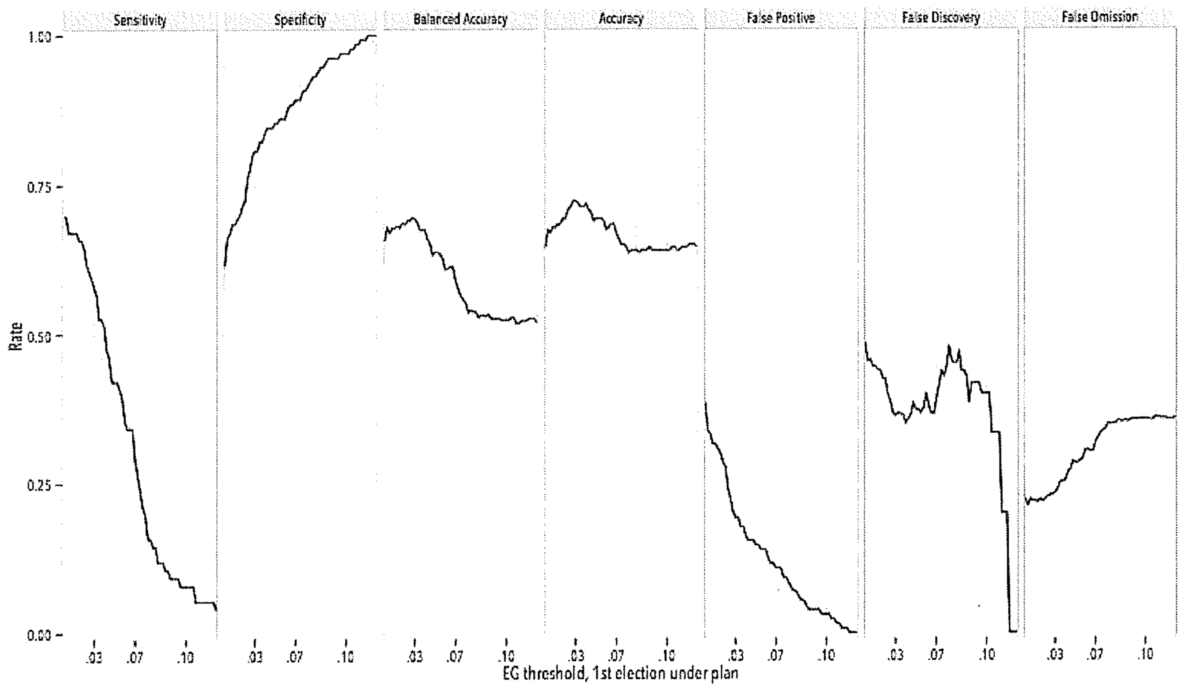

Figure 3: Prognostic performance measures, first efficiency gap under a district plan more extreme than threshold (horizontal axis) as a predictor of whether the subsequent efficiency gaps recorded under the district plan all have the same sign as the first efficiency gap. Vertical lines indicate 95% confidence intervals. Analysis examines positive, first-election threshold values of the efficiency gap, consistent with Democratic advantage.

3 First-election efficiency gap reliability with respect to the plan-average efficiency gap sign

Next we consider a slightly different kind of test; given that the first election under a district plan produces a value of the efficiency gap above or below a given threshold, how likely is it that the *average* value of the efficiency gap produced over the life of the plan lies on the same side of zero as that of the first election? Recall that the sign of the efficiency gap speaks to the corresponding direction of partisan advantage ($EG < 0$ is consistent with Republican advantage; conversely for $EG > 0$). We expect that this will be a less strenuous test than asking if *any* EG has an opposite sign to the first EG observed under a district plan.

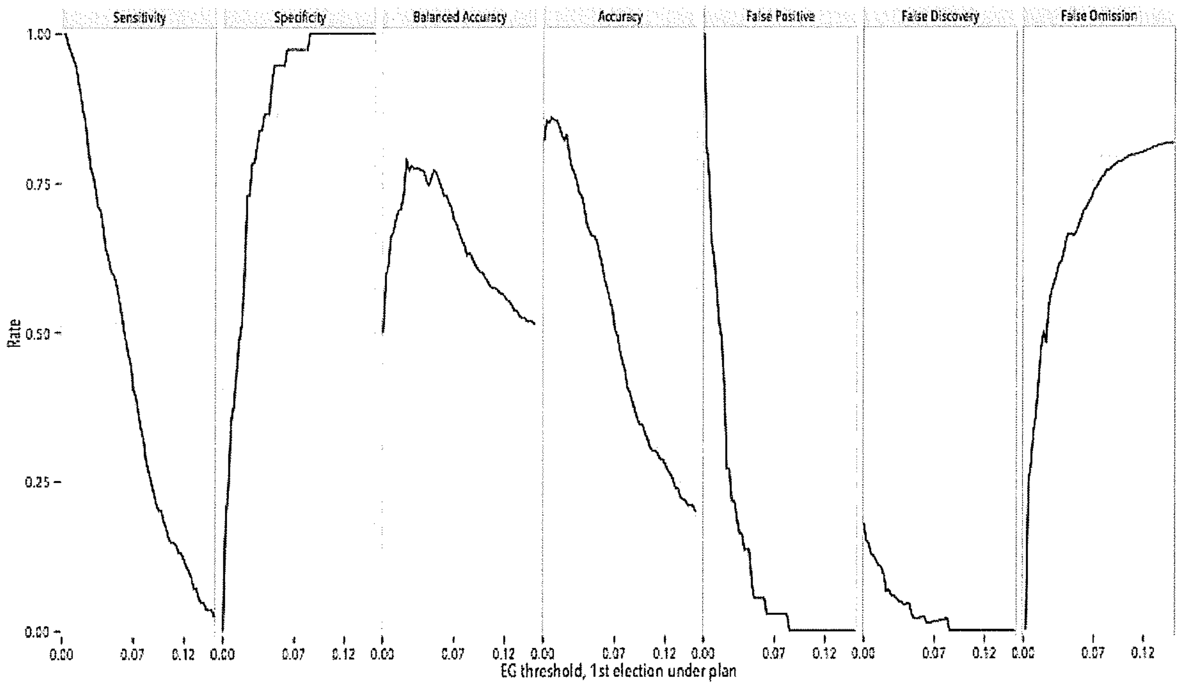

Figure 4: Prognostic performance measures, first efficiency gap under a district plan more extreme than threshold (horizontal axis) as a predictor of whether the average efficiency gap recorded under the district plan has the same sign as the first efficiency gap. Vertical lines indicate 95% confidence intervals. Analysis spans all state legislative elections and district plans as per my initial report, 1972-2014.

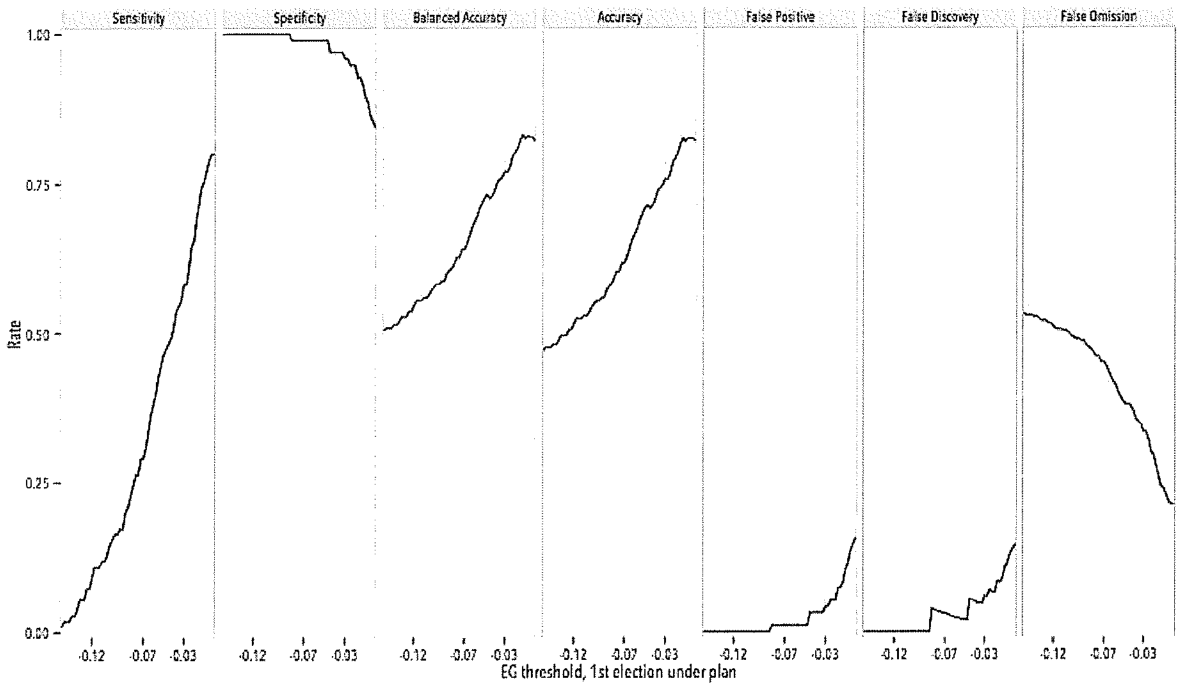

Figure 5: Prognostic performance measures, first efficiency gap under a district plan more extreme than threshold (horizontal axis) as a predictor of whether the average efficiency gap recorded under the district plan has the same sign as the first efficiency gap. Vertical lines indicate 95% confidence intervals. Analysis examines negative, first-election threshold values of the efficiency gap, consistent with Republican advantage.

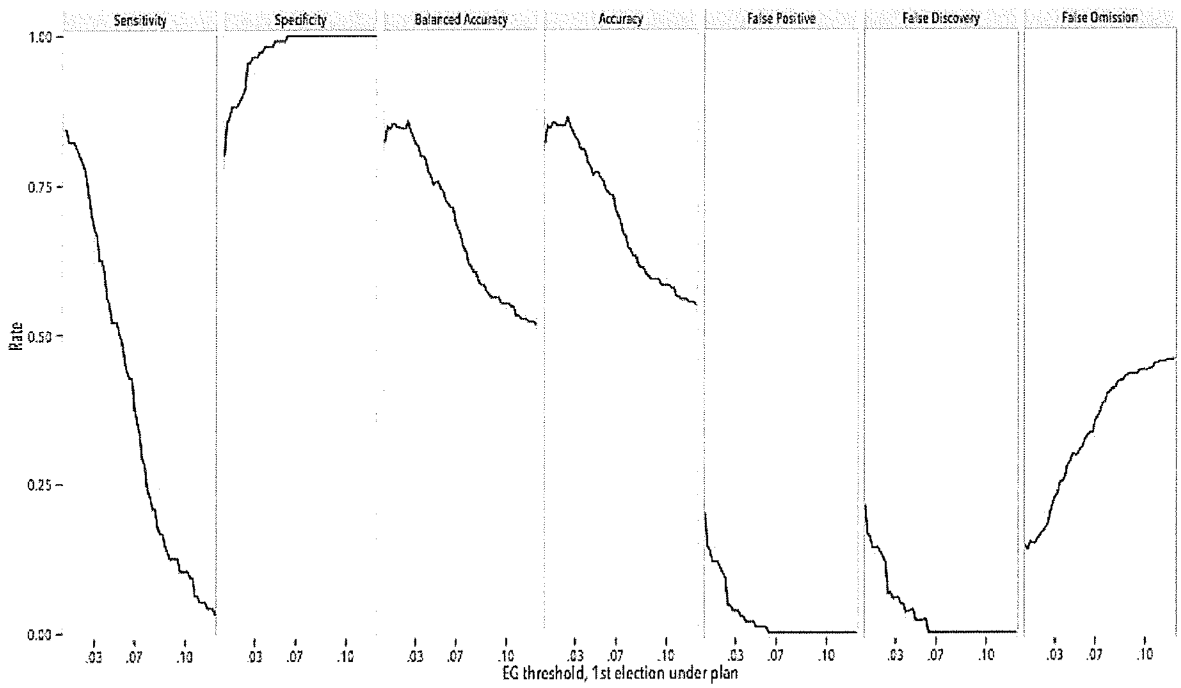

Figure 6: Prognostic performance measures, first efficiency gap under a district plan more extreme than threshold (horizontal axis) as a predictor of whether the average efficiency gap recorded under the district plan has the same sign as the first efficiency gap. Vertical lines indicate 95% confidence intervals. Analysis examines positive, first-election threshold values of the efficiency gap, consistent with Democratic advantage.

Figures 4, 5 and 6 show the prognostic performance of the first-election *EG* with respect to the sign of the corresponding plan's average *EG*, looking at the absolute value of the first-election *EG* (Figure 4), negative first-election efficiency gaps (Figure 5) and positive first-election efficiency gaps (Figure 6). The first thing to observe is the generally superior prognostic performance when it comes to forecasting the sign of the *plan-average* efficiency gap, relative to the prognostic performance with respect to *all* of the plan's efficiency gaps having the same sign. As anticipated, the former is better predicted by the plan's first-election efficiency gap than the latter. Second, the accuracy-versus-caution tradeoff noted earlier is also apparent. The proposed threshold of +/- 0.07 trades away accuracy for very low false positive and false discovery rates, below 5%, at the cost of higher false omission rates, a pattern we observed earlier. Finally, note that at the proposed threshold of +/- 0.07, almost one-half of all plans with a negative (pro-Republican) average *EG* would *not* be candidates for scrutiny (right-hand panel of Figure 5); about one-third of plans with a positive (pro-Democratic) average *EG* also would not trigger the threshold for scrutiny.

4 Relationship between the first-election efficiency gap and the plan-average efficiency gap

I next present analysis on a related issue, the relationship between the magnitudes of the *first* efficiency gap observed under a plan and the *average* efficiency gap we observe over the life of the plan. Does a larger or smaller first-election efficiency gap portend anything for the average value of the efficiency gap generated over the life of a district plan?

Clearly the first value of the efficiency gap and the plan-average efficiency gap are related; the former contributes to the calculation of the latter, and after the first election under a district plan we observe at most four more elections under the plan (given elections every two years in most states and redistricting once a decade). Accordingly we expect a positive correlation between the two quantities. The interesting empirical question—and one with considerable substantive implications for the issue at hand—is *how strong* the relationship is between the first-election efficiency gap and the corresponding plan-average efficiency gap. This speaks to the reliability of the first-election *EG* measure as a predictor of *EG* over the life of the plan.

Figure 7 shows the relationship between the first-election *EG* and the average *EG* observed over the entire plan. Note that we restrict this analysis to plans with at least three elections, so that the first election does not unduly contribute to the calculation of the average; this restriction has the consequence of omitting elections from the most recent round of redistricting after the 2010 Census, which have contributed at most two elections. The black diagonal line on the graph is a 45-degree line: if the relationship between first-election *EG* and plan-average *EG* were perfect, the data would all lie on this line. Instead we see a classic "regression-to-the-mean" pattern, with a positive regression slope of less than one (as indeed we should, given that the first-election *EG* on the horizontal axis contributes to the average plotted on the vertical axis). But the relationship here is especially strong. The variation in plan-average efficiency gaps explained by this regression is quite large, about 73%; after taking into account the uncertainty in the *EG* scores (stemming from the imputation procedures used for uncontested districts; see my initial report) a 95% confidence interval on the variance explained measure ranges from 67% to 74% (the uncertainty has the consequence of tending to make the regression fit slightly less well). That is, even given the uncertainty that accompanies *EG* measures due to uncontestedness, the relationship between first-election *EG* and plan-average *EG* is quite strong.

In particular, at the threshold values of +/- 0.07 there is very little doubt as to the plan-average value of the efficiency gap. The historical relationship between first-election *EG* and plan-average *EG* shown in Figure 7 indicates that a first-election *EG* of -.07 is typically associated with a plan-average *EG* of about -0.053 (95% CI -0.111 to 0.004); the probability that the resulting, expected plan-average *EG* is negative is 96.5%. Conditional on a first-election *EG* of .07 we typically see a plan-average *EG* of about 0.037 (95% CI -0.021 to 0.093); the probability that the resulting, expected plan-average *EG* is positive is 89.8%. This constitutes additional, powerful evidence that (a) first-election *EG* estimates are predictive with respect to the *EG* estimates that will be observed over the life of the plan; and (b) the threshold values of +/- 0.07 are conservative, generating high-confidence predictions as to the behavior of the district plan in successive elections.

In the particular case of Wisconsin in 2012—the first election under the plan in question—I estimated the efficiency gap to be -0.133 (95% CI -0.146 to -0.121). The analysis of historical data discussed above—and graphed in Figure 7—indicates that the plan-average *EG* for this plan will be -0.095 (95% CI -0.152 to -0.032)[1], a quite large value by historical standards, placing the current Wisconsin district plan among the five to ten most disadvantageous district plans for Democrats in the data available for analysis. The probability that the Wisconsin plan—if left undisturbed—will turn out to have a positive, pro-Democratic, average efficiency gap is for all practical purposes zero (less than 0.1%).

1 It is also worth stressing that the confidence interval is computed so as to take into account uncertainty from all known sources: in the underlying efficiency gap scores themselves, the fact that the 2012 *EG* scores for Wisconsin are large by historical standards, and in the regression relationship between first-election *EG* and plan-average *EG*.

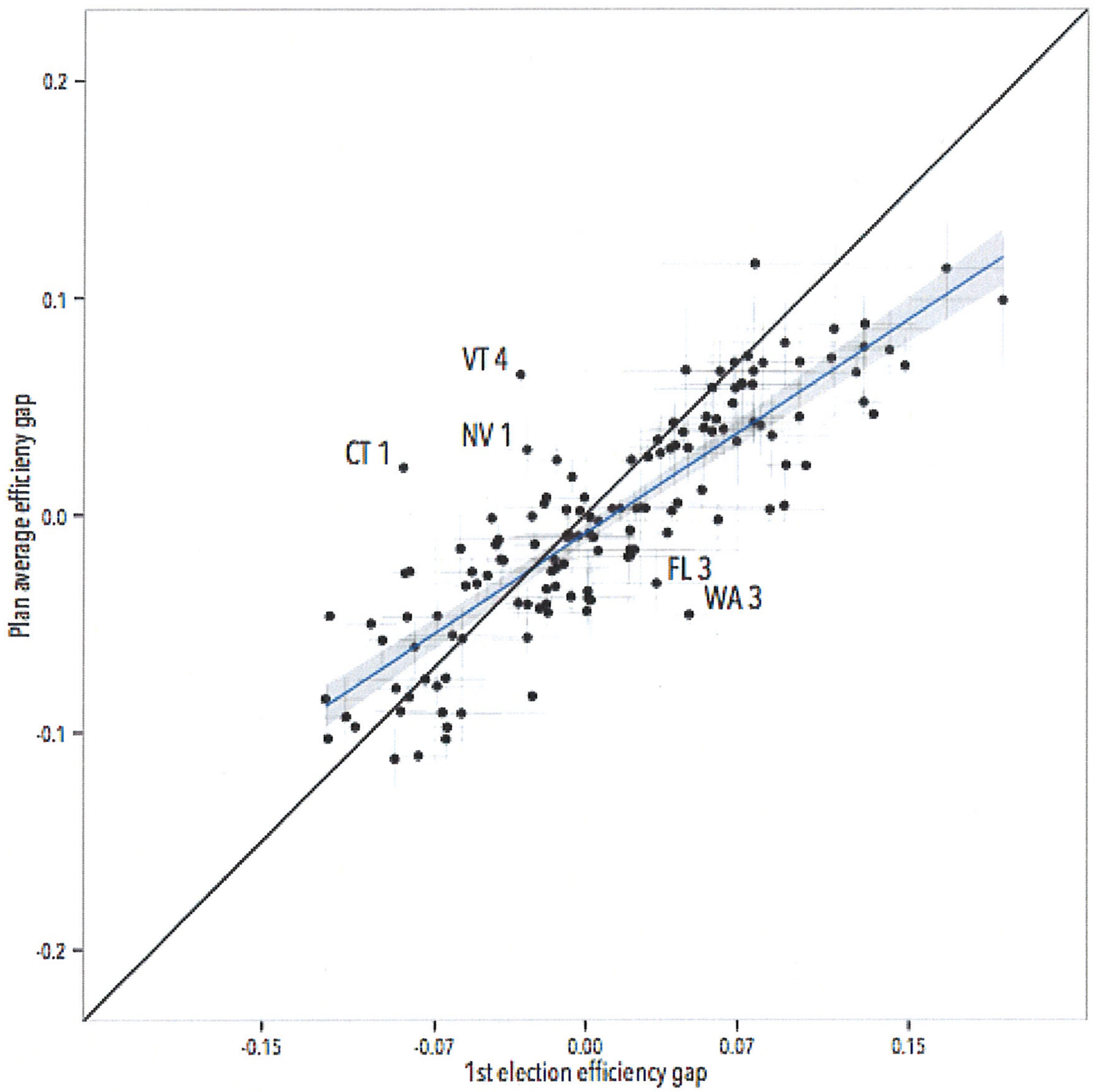

Figure 7: Scatterplot of first-election efficiency gap scores (horizontal axis) and plan-average efficiency gap scores (vertical axis). The diagonal black line is a 45-degree line; the data would lie on this line if first-election efficiency gaps coincided with plan-average efficiency gaps. The solid blue line is a linear regression with slope .64 (95% CI 0.57 to 0.72); the shaded region around the blue line is a 95% confidence interval for the regression line. Vertical and horizontal lines extending from each data point cover 95% confidence intervals in either direction, summarizing the uncertainty in both first-election *EG* and plan-average *EG*, stemming from imputations for uncontested districts. Outliers are labeled (state, plan). Analysis restricted to plans with at least three elections (1972-2010), omitting plans adopted after the 2010 Census. The first-election *EG* for the current Wisconsin plan is -0.133 (95% CI -0.146 to -0.121).

5 Party control as an explanation for change in the efficiency gap

Both Trende and Goedert point out that, on average, state house plans have exhibited pro-Republican efficiency gaps in recent years (Trende, paragraphs 129-30; Goedert p. 19). They then argue that this pro-Republican mean is attributable to a natural pro-Republican political geography in many states. However, as I found in my initial report, the *overall* efficiency gap average, over the entire 1972-2014 period, is very close to zero (Jackman Report, p. 35, 45, 57). There is thus no sign of a natural pro-Republican advantage in the dataset as a whole, nor any evidence (despite Trende and Goedert's unsupported assertions to the contrary) that states' political geography is changing in ways that favor Republicans.

In fact, the one historical change that *is* undeniable is the trend toward unified Republican control over redistricting. As Figure 8 displays, only about 10% of all state house plans were designed by Republicans in full control of the state government in the 1990s, compared to about 30% by Democrats in full control and about 60% by another institution (divided government, a commission, or a court). But in the 2000s, Republicans were fully responsible for slightly *more* plans than were Democrats (about 20% versus about 15%). And in the 2010s, the partisan gap jumped again, to about *40%* of plans designed entirely by Republicans, versus less than 20% designed entirely by Democrats.

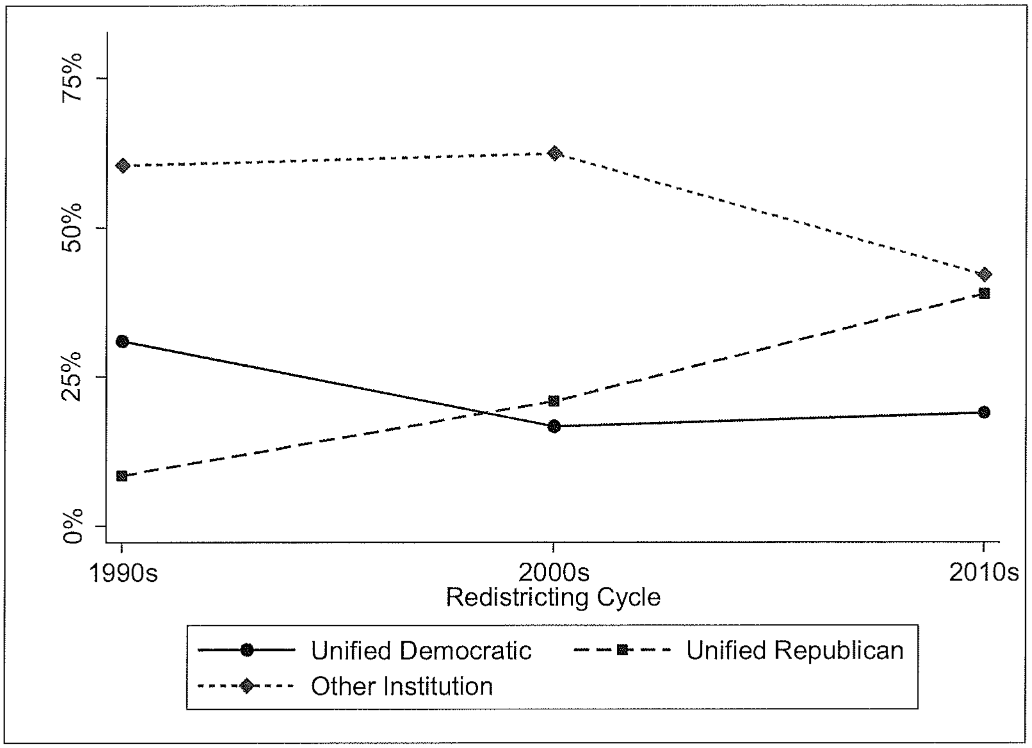

Figure 8: Share of all state house plans, by cycle, designed by Democrats in unified control of state government, by Republicans in unified control of state government, or by another institution (divided state government, commission, or court).

To determine the impact of this change in party control on the change in the efficiency gap over the last generation, I carry out three regressions, one for the 1990 redistricting cycle, one for the 2000 cycle, and one for the 2010 cycle. In each case, state house plans' efficiency gaps are the dependent variable, and unified Democratic control over redistricting and unified Republican control over redistricting are the independent variables. (The omitted category is any other institution responsible for redistricting, such as divided government, a court, or a commission.) Figure 9 then displays the *actual* average efficiency gap for each cycle, as well as the *predicted* average efficiency gap if the distribution of party control over redistricting had remained unchanged since the 1990s.

As is evident from the chart, state house plans' average efficiency gap in the 2000 cycle would have been substantially less pro-Republican (by about 0.5 percentage points) had Republicans not gained control of more state governments in this cycle relative to the 1990s. And in the current cycle, *all* of the efficiency gap's movement in a Republican direction would have been erased had the distribution of party control over redistricting not changed since the 1990s. That is, if the same distribution of party control had existed in this cycle as in the 1990s, state house plans' average efficiency gap would have been very close to zero, not over 3% in a Republican direction. Accordingly, it is the change in party control that appears to account for essentially all of the pro-Republican trend in the efficiency gap over the past two decades—and not, as claimed by Trende and Goedert, a dramatic alteration of the country's political geography.

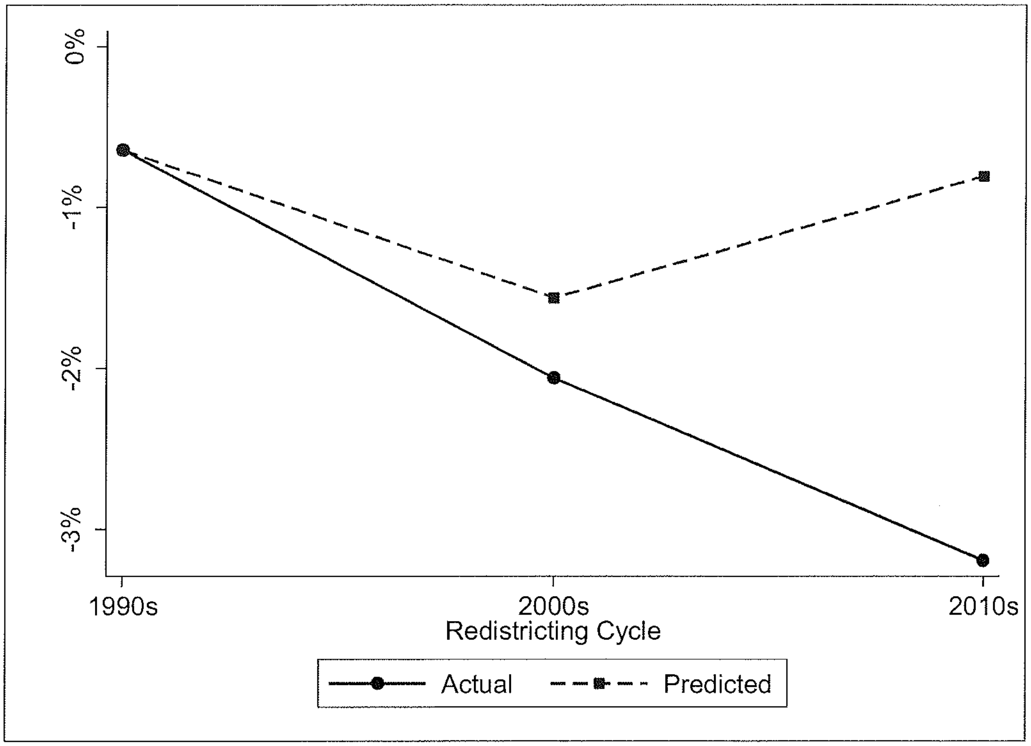

Figure 9: Actual and predicted values of state house plans' average efficiency gaps by cycle. Predicted values calculated assuming that the 1990s distribution of party control over redistricting remained constant in subsequent cycles.

6 Response to the Chen and Rodden map simulations

Both Trende and Goedert cite a recent article by Chen and Rodden (2013) that purports to find, based on simulations of hypothetical district maps, that random redistricting would benefit Republicans because of their more efficient spatial allocation (Trende, paragraphs 89, 126; Goedert, pp. 13, 18, 21). While I respect Chen and Rodden's contribution, there are several issues with their work that make it inapplicable here.

First, Chen and Rodden do not even attempt to simulate *lawful* plans. Rather, they simulate plans "using only the traditional districting criteria of equal apportionment and geographic contiguity and compactness" (Chen and Rodden, 248). They do not take into account Section 2 of the Voting Rights Act, which often requires majority-minority districts to be constructed. They also do not take into account Section 5 of the VRA, which until 2013 meant that existing majority-minority districts could not be eliminated in certain states. And they do not take into account state-level criteria such as respect for political subdivisions and respect for communities of interest, which are in effect in a majority of states (NCSL 2010, pp. 125-27).

Second, Chen and Rodden only use *presidential* election results in their analysis, but these outcomes may diverge from *state legislative* election results due to voter roll-off as well as voter preferences that vary by election level. As Stephanopoulos and McGhee have noted, "If certain voters consistently support Republicans at the presidential level and Democrats at the legislative level, then presidential data may produce more pro-Republican estimates than legislative data" (Stephanopoulos & McGhee, 870). In fact, this is exactly what seems to be occurring; at the congressional level, efficiency gaps are about 6% more Republican when they are calculating using presidential data than when they are computed on the basis of congressional election results.

Third, Chen and Rodden's simulated maps do not constitute a representative sample of the entire plan solution space. Their simulation algorithm has "no theoretical justification," is "best described as ad-hoc," and is not "designed to yield a representative sample of redistricting plans" (Fifield et al. 2015, pp. 2-3; Altman & McDonald 2010, p. 108). The explanation for this lack of representativeness is highly technical and involves the details of the particular simulation approach adopted by Chen and Rodden. But its implication is clear: that no conclusions can yet be drawn about the partisan consequences of randomly drawn maps.

Lastly, Chen and Rodden's results are directly contradicted by Fryer and Holden, who also simulated contiguous, compact, and equipopulous districts for multiple states. Unlike Chen and Rodden, Fryer and Holden found that, "[u]nder maximally compact districting, measures of Bias are slightly *smaller* in all states except [one]" (Fryer & Holden 2011, p. 514). Fryer and Holden also found that "[i]n terms of responsiveness . . . there are large and statistically significant" *increases* in all states, sometimes on the order of a fivefold rise (p. 514). Their analysis thus leads to the opposite inference from Chen and Rodden's: that randomly drawn contiguous and compact districts favor *neither* party and substantially boost electoral responsiveness.

7 Trende's analysis of particular plans

Trende devotes a large portion of his report (paragraphs 106-31) to analyzing the efficiency gaps of particular state legislative and congressional plans. He first examines a set of seventeen state legislative plans that had efficiency gaps favoring the same party over their entire lifespans, arguing that not all of these plans were gerrymanders (paragraphs 106-14). He then cites a series of congressional plans, some of which he claims had large efficiency gaps despite not being gerrymanders, and others of which allegedly had small efficiency gaps despite being gerrymanders (paragraphs 115-24). All of this analysis is riddled with conceptual and methodological errors that, in my judgment, renders it unreliable and unhelpful to the court.

Beginning with the set of seventeen state legislative plans that had efficiency gaps of the same sign throughout their lifespans, Trende asserts that they "would be included in the definition of a gerrymander," and are a "list of gerrymandered states" (paragraphs 109-10). But neither plaintiffs nor I argue that these plans should have been held unconstitutional. That is, neither plaintiffs nor I argue that these plans were designed with partisan intent (the first element of plaintiffs' proposed test), that their initial efficiency gaps exceeded a reasonable threshold (the second element), or that their efficiency gaps could have been avoided (the third element). To the contrary, I simply included these plans in my report to illuminate historical cases in which the efficiency gap's direction did not change over the course of a decade. I never stated or implied that these plans should have been deemed unlawful.

However, if we focus on the plans among the seventeen that likely *would* have failed plaintiffs' proposed test (at least the first two elements), we see that both the test and the efficiency gap perform exceptionally well. Five of the seventeen plans featured unified control by a single party over redistricting (from which, like Goedert (2014) and Goedert (2015), we can infer partisan intent) as well as an initial efficiency gap above 7% (the threshold I recommended in my initial report): Florida in the 1970s, Florida in the 2000s, Michigan in the 2000s, New York in the 1970s, and Ohio in the 2000s. Assuming that these plans' large efficiency gaps were avoidable (a granular inquiry that cannot be carried out here), it would have been quite reasonable for all of these maps to attract heightened judicial scrutiny. In particular:

- Florida's plan in the 1970s was designed exclusively by Democrats, opened with a 9.9% pro-Democratic efficiency gap, averaged a 7.0% pro-Democratic efficiency gap over its lifespan, and never once favored Republicans.

- Florida's plan in the 2000s was designed exclusively by Republicans, opened with a 8.9% pro-Republican efficiency gap, averaged a 11.2% pro-Republican efficiency gap over its lifespan, and never once favored Democrats.
- Michigan's plan in the 2000s was designed exclusively by Republicans, opened with a 12.0% pro-Republican efficiency gap, averaged a 10.3% pro-Republican efficiency gap over its lifespan, and never once favored Democrats.
- New York's plan in the 1970s was designed exclusively by Republicans, opened with a 10.7% pro-Republican efficiency gap, averaged a 9.7% pro-Republican efficiency gap over its lifespan, and never once favored Democrats.
- Ohio's plan in the 2000s was designed exclusively by Republicans, opened with a 8.6% pro-Republican efficiency gap, averaged a 9.0% pro-Republican efficiency gap over its lifespan, and never once favored Democrats.

Accordingly, we see that if my report's set of seventeen plans is analyzed properly, the opposite conclusion emerges from the one advocated by Trende. Only a subset of the seventeen plans likely would have failed plaintiffs' proposed test. But *every member* of this subset turns out to have been an exceptionally severe and durable gerrymander, featuring a very large and consistent efficiency gap over its lifespan. These are *precisely* the historical cases in which judicial intervention may have been advisable.

After commenting on these seventeen state legislative plans, Trende discusses a series of *congressional* plans, all from the 2000 and 2010 redistricting cycles. These congressional plans are entirely irrelevant to this case, which deals only with state legislative redistricting. Neither in their complaint nor in their subsequent filings do plaintiffs ever argue that their approach should be applied to congressional plans. And neither Mayer nor I provide any empirical analysis of congressional plans. In my initial report, in particular, I examined state legislative plans from 1972 to the present, but no congressional plans at all.

This state legislative focus has two explanations. First, and more importantly, each congressional delegation is *not* a legislative chamber in its own right, but rather a portion (often a very small portion) of the U.S. House of Representatives. Methods applicable to entire chambers cannot simply be transferred wholesale to delegations that make up only fractions of Congress. Second, most congressional delegations have many fewer seats than most state houses. The efficiency gap becomes lumpier when there are fewer seats, because each seat accounts for a larger proportion of the seat total, and the efficiency gap thus shifts more as each seat changes hands. This lumpiness is entirely avoided when state legislative plans, which typically have dozens or even hundreds of districts, are at issue.

For these reasons, Stephanopoulos and McGhee make two adjustments when analyzing congressional plans in their work on the efficiency gap. First, they convert the efficiency gap from percentage points to *seats* by multiplying the raw efficiency gap by each state's number of congressional districts. As they explain their method, "What matters in congressional plans is their impact on the total number of *seats* held by each party at the national level. Conversely, state houses are self-contained bodies of varying sizes, for which *seat shares* reveal the scale of parties' advantages and enable temporal and spatial comparability" (Stephanopoulos & McGhee, 869). Second, they only calculate efficiency gaps for states with at least eight congressional districts. Efficiency gaps are lumpier for states with fewer than eight districts, and additionally, congressional "redistricting in smaller states has only a minor influence on the national balance of power" (Stephanopoulos & McGhee, 868).

In his report, Trende fails to make either of these necessary adjustments when examining congressional plans. That is, he does not convert the efficiency gap from percentage points to seats, and he calculates the efficiency gap for small congressional delegations with fewer than eight seats. There is no authority in the literature for his methodological choices, and he is unable to cite any. And his flawed methods have serious substantive consequences that render his results entirely untrustworthy.

Take Trende's failure to convert the efficiency gap from percentage points to House seats. He claims that Alabama's congressional plan had an efficiency gap of -12.5% in 2002, that Arizona's congressional plan had an efficiency gap of 16% in 2012, that Colorado's congressional plan had an efficiency gap of -9% in 2002 and -10% in 2012, that Illinois's congressional plan had an efficiency gap of -9% in 2002, and that Iowa's congressional plan had an efficiency gap of -20% in 2002—all above my suggested 7% threshold for state legislative plans (paragraphs 115-16, 118-19, 121-22). But when converted to seats, *all* of these efficiency gaps become quite small, lower in all cases than the two-seat threshold proposed in the literature for congressional plans (Stephanopoulos & McGhee, 887-88). Specifically, using Trende's own calculations—which, as I discuss below, are incorrect in any event—Alabama had an efficiency gap of -0.9 seats in 2002, Arizona had an efficiency gap of 1.4 seats in 2012, Colorado had an efficiency gap of -0.6 seats in 2002 and -0.7 seats in 2012, Illinois had an efficiency gap of -1.7 seats in 2002, and Iowa had an efficiency gap of -1.0 seats in 2002. *None* of these scores are high enough to rise to presumptive unlawfulness under the literature's suggested two-seat threshold, meaning that we come to exactly the *opposite* conclusion as Trende after making the necessary adjustment.

Next take Trende's consideration of Alabama's congressional plan in 2002 (which had seven districts), Iowa's congressional plan in 2002 (five districts), and Colorado's congressional plans in 2002 and 2012 (seven districts each) (paragraphs 115-16, 119, 122). All four of these plans have fewer than eight districts, and so, based on the literature, should not be included in any efficiency gap analysis because of the measure's lumpiness when applied to so few seats. Trende nowhere acknowledges this limitation, and indeed appears unaware of its existence.

Moreover, Trende's study of congressional plans is marred by two further flaws, one conceptual and the other methodological. The conceptual defect is that, as in his earlier discussion of state legislative plans, he assumes that a large efficiency gap is all that is necessary to render a plan unconstitutional. He writes that efficiency gaps of -12.5%, -9%, -9%, -20%, and 16% "would invite court scrutiny as a Republican gerrymander" or "would invite court scrutiny as a Democratic gerrymander" (paragraphs 115, 116, 118, 119, 121, 122). But again, this is not plaintiffs' proposed test. A large efficiency gap is only a single prong of the test, and does not result in a verdict of unconstitutionality unless it is paired with a finding of partisan intent *and* a finding that it could have been avoided. Trende entirely overlooks these other elements.

The methodological defect is that whenever there were uncontested congressional races, Trende simply *substituted* presidential election results for the missing congressional results. As he put it in his deposition, he "used presidential results" and "imputed those results to the congressional races" whenever the races were uncontested (Trende deposition, p. 83). This is an exceptionally crude method that is guaranteed to produce errors, both because there is voter roll-off from the presidential to the congressional level and because voters may have different presidential and congressional preferences. Of course, presidential results can be used as the *inputs* to a regression model that *predicts* the outcomes of uncontested congressional races. Indeed, this is the preferred approach in the literature, and the approach I employed in my initial report. But presidential results cannot simply be plugged in without any adjustment, and no competent social scientist would have done so.

Accordingly, in my judgment, Trende's examination of particular state legislative and congressional plans is unreliable and entitled to no weight by the court. The state legislative analysis ignores the actual elements of plaintiffs' proposed test, and would have led to the opposite conclusion if these elements had been taken into account. Likewise, the congressional analysis ignores the test's prongs, fails to convert the efficiency gap from percentage points to seats, improperly considers states with small House delegations, improperly substitutes presidential election results whenever congressional results are missing—and deals with federal elections that simply are not part of this case.

Dated December 21, 2015

/s/ Simon Jackman

Simon Jackman, PhD

Department of Political Science

Stanford University

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Case

Vieth v. Jubilerer, 541 U.S. 267 (2004).

From: **Nicholas Stephanopoulos** nicholas.stephanopoulos@gmail.com
Subject: Datasets
Date: Sat Dec 05 2015 05:33:58 GMT+0530 (IST)
To: Jackman jackman@stanford.edu
Cc: Peter Earle peter@earle-law.com, Paul Strauss Pstrauss@clccrul.org, Ruth Greenwood rgreenwood@clccrul.org

N

Simon,

Attached are the two datasets I previously referenced: one containing efficiency gap data at the congressional level, and another containing information on the institution responsible for redistricting at the state legislative level. Please let me know if you have any questions. Thanks very much.

Nick

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Nicholas O. Stephanopoulos
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University of Chicago Law School
nsteph@uchicago.edu
(773) 702-4226
http://www.law.uchicago.edu/faculty/stephanopoulos

Attachments:
Congressional EG Data.xlsx (73.72 kB)
Party Control Data.xlsx (55.87 kB)

EXHIBIT
PENGAD 800-631-6989
S. Jackman
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3/16/16 ue

State	Code	FIP	Year	Commission	Court	Any Unified Govt	Divided Govt	Unified Dem Govt	Unified Rep Govt	Non-Unified Control
Alabama	AL	1	1970							
Alaska	AK	2	1970							
Arizona	AZ	4	1970							
Arkansas	AR	5	1970							
California	CA	6	1970							
Colorado	CO	8	1970							
Connecticut	CT	9	1970							
Delaware	DE	10	1970							
Florida	FL	12	1970							
Georgia	GA	13	1970							
Hawaii	HI	15	1970							
Idaho	ID	16	1970							
Illinois	IL	17	1970							
Indiana	IN	18	1970							
Iowa	IA	19	1970							
Kansas	KS	20	1970							
Kentucky	KY	21	1970							
Louisiana	LA	22	1970							
Maine	ME	23	1970							
Maryland	MD	24	1970							
Massachusetts	MA	25	1970							
Michigan	MI	26	1970							
Minnesota	MN	27	1970							
Mississippi	MS	28	1970							
Missouri	MO	29	1970							
Montana	MT	30	1970							
Nebraska	NE	31	1970							
Nevada	NV	32	1970							
New Hampshire	NH	33	1970							
New Jersey	NJ	34	1970							
New Mexico	NM	35	1970							
New York	NY	36	1970							
North Carolina	NC	37	1970							
North Dakota	ND	38	1970							
Ohio	OH	39	1970							
Oklahoma	OK	40	1970							
Oregon	OR	41	1970							

EXHIBIT
PENGAD 800-631-6989
S. Jackman
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3/16/16 ue

Pennsylvania	PA	42	1970							
Rhode Island	RI	44	1970							
South Carolina	SC	45	1970							
South Dakota	SD	46	1970							
Tennessee	TN	47	1970							
Texas	TX	48	1970							
Utah	UT	49	1970							
Vermont	VT	50	1970							
Virginia	VA	51	1970							
Washington	WA	53	1970							
West Virginia	WV	54	1970							
Wisconsin	WI	55	1970							
Wyoming	WY	56	1970							
Alaska	AK	2	1972							
Arizona	AZ	4	1972							
Arkansas	AR	5	1972							
California	CA	6	1972	0	0	1	0	1	0	0
Colorado	CO	8	1972	0	0	1	0	0	1	0
Connecticut	CT	9	1972	1	0	0	0	0	0	1
Delaware	DE	10	1972	0	0	1	0	0	1	0
Florida	FL	12	1972	0	0	1	0	1	0	0
Georgia	GA	13	1972	0	0	1	0	1	0	0
Hawaii	HI	15	1972							
Idaho	ID	16	1972							
Illinois	IL	17	1972							
Indiana	IN	18	1972	0	0	1	0	0	1	0
Iowa	IA	19	1972	0	1	0	0	0	0	1
Kansas	KS	20	1972	0	0	0	1	0	0	1
Kentucky	KY	21	1972							
Louisiana	LA	22	1972							
Maine	ME	23	1972							
Massachusetts	MA	25	1972	0	0	1	0	1	0	0
Michigan	MI	26	1972	0	1	0	0	0	0	1
Minnesota	MN	27	1972							
Mississippi	MS	28	1972	0	0	1	0	1	0	0
Missouri	MO	29	1972	0	1	0	0	0	0	1
Montana	MT	30	1972							
Nebraska	NE	31	1972							

Nevada	NV	32	1972	0	0	0	1	0	0	1
New Hampshire	NH	33	1972							
New Jersey	NJ	34	1972							
New Mexico	NM	35	1972	0	0	1	0	1	0	0
New York	NY	36	1972	0	0	1	0	0	1	0
North Carolina	NC	37	1972							
North Dakota	ND	38	1972							
Ohio	OH	39	1972	0	0	0	1	0	0	1
Oklahoma	OK	40	1972	0	0	1	0	1	0	0
Oregon	OR	41	1972	0	0	0	1	0	0	1
Pennsylvania	PA	42	1972	1	0	0	0	0	0	1
Rhode Island	RI	44	1972	0	0	1	0	1	0	0
South Carolina	SC	45	1972	0	0	1	0	1	0	0
South Dakota	SD	46	1972							
Tennessee	TN	47	1972	0	1	0	0	0	0	1
Texas	TX	48	1972	0	0	1	0	1	0	0
Utah	UT	49	1972	0	0	0	1	0	0	1
Vermont	VT	50	1972							
Virginia	VA	52	1972							
Washington	WA	53	1972	0	1	0	0	0	0	1
West Virginia	WV	54	1972							
Wisconsin	WI	55	1972	0	0	0	1	0	0	1
Wyoming	WY	56	1972							
Alabama	AL	1	1974	0	1	0	0	0	0	1
Alaska	AK	2	1974							
Arizona	AZ	4	1974							
Arkansas	AR	5	1974							
California	CA	6	1974	0	1	0	0	0	0	1
Colorado	CO	8	1974	0	0	1	0	0	1	0
Connecticut	CT	9	1974	1	0	0	0	0	0	1
Delaware	DE	10	1974	0	0	1	0	0	1	0
Florida	FL	12	1974	0	0	1	0	1	0	0
Georgia	GA	13	1974	0	0	1	0	1	0	0
Hawaii	HI	15	1974							
Idaho	ID	16	1974							
Illinois	IL	17	1974							
Indiana	IN	18	1974	0	0	1	0	0	1	0
Iowa	IA	19	1974	0	1	0	0	0	0	1

Kansas	KS	20	1974	0	0	0	1	0	0	1
Kentucky	KY	21	1974							
Maine	ME	23	1974	0	1	0	0	0	0	1
Maryland	MD	24	1974							
Massachusetts	MA	25	1974	0	0	1	0	1	0	0
Michigan	MI	26	1974	0	1	0	0	0	0	1
Minnesota	MN	27	1974	0	1	0	0	0	0	1
Missouri	MO	29	1974	0	1	0	0	0	0	1
Montana	MT	30	1974	1	0	0	0	0	0	1
Nebraska	NE	31	1974							
Nevada	NV	32	1974	0	0	0	1	0	0	1
New Hampshire	NH	33	1974							
New Jersey	NJ	34	1974							
New Mexico	NM	35	1974	0	0	1	0	1	0	0
New York	NY	36	1974	0	0	1	0	0	1	0
North Carolina	NC	37	1974							
North Dakota	ND	38	1974							
Ohio	OH	39	1974	0	0	0	1	0	0	1
Oklahoma	OK	40	1974	0	0	1	0	1	0	0
Oregon	OR	41	1974	0	0	0	1	0	0	1
Pennsylvania	PA	42	1974	1	0	0	0	0	0	1
Rhode Island	RI	44	1974	0	0	1	0	1	0	0
South Carolina	SC	45	1974	0	0	1	0	1	0	0
South Dakota	SD	46	1974							
Tennessee	TN	47	1974	0	0	1	1	0	0	1
Texas	TX	48	1974	0	0	1	0	1	0	0
Utah	UT	49	1974	0	0	0	1	0	0	1
Vermont	VT	50	1974							
Virginia	VA	51	1974							
Washington	WA	53	1974	0	1	0	0	0	0	1
West Virginia	WV	54	1974	0	0	1	0	1	0	0
Wisconsin	WI	55	1974	0	0	0	1	0	0	1
Wyoming	WY	56	1974							
Alaska	AK	2	1976							
Arizona	AZ	4	1976							
Arkansas	AR	5	1976							
California	CA	6	1976	0	1	0	0	0	0	1
Colorado	CO	8	1976	0	0	1	0	0	1	0

Connecticut	CT	9	1976	1	0	0	0	0	0	1
Delaware	DE	10	1976	0	0	1	0	0	1	0
Florida	FL	12	1976	0	0	1	0	1	0	0
Georgia	GA	13	1976	0	0	1	0	1	0	0
Hawaii	HI	15	1976							
Idaho	ID	16	1976	0	0	0	1	0	0	1
Illinois	IL	17	1976							
Indiana	IN	18	1976	0	0	1	0	0	1	0
Iowa	IA	19	1976	0	1	0	0	0	0	1
Kansas	KS	20	1976	0	0	0	1	0	0	1
Kentucky	KY	21	1976							
Louisiana	LA	22	1976							
Maine	ME	23	1976	0	1	0	0	0	0	1
Massachusetts	MA	25	1976	0	0	1	0	1	0	0
Michigan	MI	26	1976	0	1	0	0	0	0	1
Minnesota	MN	27	1976	0	1	0	0	0	0	1
Mississippi	MS	28	1976	0	0	1	0	1	0	0
Missouri	MO	29	1976	0	1	0	0	0	0	1
Montana	MT	30	1976	1	0	0	0	0	0	1
Nebraska	NE	31	1976							
Nevada	NV	32	1976	0	0	0	1	0	0	1
New Hampshire	NH	33	1976							
New Jersey	NJ	34	1976							
New Mexico	NM	35	1976	0	0	1	0	1	0	0
New York	NY	36	1976	0	0	1	0	0	1	0
North Carolina	NC	37	1976							
North Dakota	ND	38	1976							
Ohio	OH	39	1976	0	0	0	1	0	0	1
Oklahoma	OK	40	1976	0	0	1	0	1	0	0
Oregon	OR	41	1976	0	0	0	1	0	0	1
Pennsylvania	PA	42	1976	1	0	0	0	0	0	1
Rhode Island	RI	44	1976	0	0	1	0	1	0	0
South Carolina	SC	45	1976	0	0	1	0	1	0	0
South Dakota	SD	46	1976							
Tennessee	TN	47	1976	0	0	1	1	0	0	1
Texas	TX	48	1976	0	0	1	0	1	0	0
Utah	UT	49	1976	0	0	0	1	0	0	1
Vermont	VT	50	1976							

Virginia	VA	51	1976							
Washington	WA	53	1976	0	1	0	0	0	0	1
West Virginia	WV	54	1976	0	0	1	0	1	0	0
Wisconsin	WI	55	1976	0	0	0	1	0	0	1
Wyoming	WY	56	1976							
Alabama	AL	1	1978	0	1	0	0	0	0	1
Alaska	AK	2	1978							
Arizona	AZ	4	1978							
Arkansas	AR	5	1978							
California	CA	6	1978	0	1	0	0	0	0	1
Colorado	CO	8	1978	0	0	1	0	0	1	0
Connecticut	CT	9	1978	1	0	0	0	0	0	1
Delaware	DE	10	1978	0	0	1	0	0	1	0
Florida	FL	12	1978	0	0	1	0	1	0	0
Georgia	GA	13	1978	0	0	1	0	1	0	0
Hawaii	HI	15	1978							
Idaho	ID	16	1978	0	0	0	1	0	0	1
Illinois	IL	17	1978							
Indiana	IN	18	1978	0	0	1	0	0	1	0
Iowa	IA	19	1978	0	1	0	0	0	0	1
Kansas	KS	20	1978	0	0	0	1	0	0	1
Kentucky	KY	21	1978							
Maine	ME	23	1978	0	1	0	0	0	0	1
Maryland	MD	24	1978							
Massachusetts	MA	25	1978	0	0	1	0	1	0	0
Michigan	MI	26	1978	0	1	0	0	0	0	1
Minnesota	MN	27	1978	0	1	0	0	0	0	1
Missouri	MO	29	1978	0	1	0	0	0	0	1
Montana	MT	30	1978	1	0	0	0	0	0	1
Nebraska	NE	31	1978							
Nevada	NV	32	1978	0	0	0	1	0	0	1
New Hampshire	NH	33	1978							
New Jersey	NJ	34	1978							
New Mexico	NM	35	1978	0	0	1	0	1	0	0
New York	NY	36	1978	0	0	1	0	0	1	0
North Carolina	NC	37	1978							
North Dakota	ND	38	1978							
Ohio	OH	39	1978	0	0	0	1	0	0	1

Oklahoma	OK	40	1978	0	0	1	0	1	0	0
Oregon	OR	41	1978	0	0	0	1	0	0	1
Pennsylvania	PA	42	1978	1	0	0	0	0	0	1
Rhode Island	RI	44	1978	0	0	1	0	1	0	0
South Carolina	SC	45	1978	0	0	1	0	1	0	0
South Dakota	SD	46	1978							
Tennessee	TN	47	1978	0	0	1	1	0	0	1
Texas	TX	48	1978	0	0	1	0	1	0	0
Utah	UT	49	1978	0	0	0	1	0	0	1
Vermont	VT	50	1978							
Virginia	VA	51	1978							
Washington	WA	53	1978	0	1	0	0	0	0	1
West Virginia	WV	54	1978	0	0	1	0	1	0	0
Wisconsin	WI	55	1978	0	0	0	1	0	0	1
Wyoming	WY	56	1978							
Alaska	AK	2	1980							
Arizona	AZ	4	1980							
Arkansas	AR	5	1980							
California	CA	6	1980	0	1	0	0	0	0	1
Colorado	CO	8	1980	0	0	1	0	0	1	0
Connecticut	CT	9	1980	1	0	0	0	0	0	1
Delaware	DE	10	1980	0	0	1	0	0	1	0
Florida	FL	12	1980	0	0	1	0	1	0	0
Georgia	GA	13	1980	0	0	1	0	1	0	0
Hawaii	HI	15	1980							
Idaho	ID	16	1980	0	0	0	1	0	0	1
Illinois	IL	17	1980							
Indiana	IN	18	1980	0	0	1	0	0	1	0
Iowa	IA	19	1980	0	1	0	0	0	0	1
Kansas	KS	20	1980	0	0	0	1	0	0	1
Kentucky	KY	21	1980							
Louisiana	LA	22	1980							
Maine	ME	23	1980	0	1	0	0	0	0	1
Massachusetts	MA	25	1980	0	0	1	0	1	0	0
Michigan	MI	26	1980	0	1	0	0	0	0	1
Minnesota	MN	27	1980	0	1	0	0	0	0	1
Mississippi	MS	28	1980	0	0	1	0	1	0	0
Missouri	MO	29	1980	0	1	0	0	0	0	1

Montana	MT	30	1980	1	0	0	0	0	0	1
Nebraska	NE	31	1980							
Nevada	NV	32	1980	0	0	0	1	0	0	1
New Hampshire	NH	33	1980							
New Jersey	NJ	34	1980							
New Mexico	NM	35	1980	0	0	1	0	1	0	0
New York	NY	36	1980	0	0	1	0	0	1	0
North Carolina	NC	37	1980							
North Dakota	ND	38	1980							
Ohio	OH	39	1980	0	0	0	1	0	0	1
Oklahoma	OK	40	1980	0	0	1	0	1	0	0
Oregon	OR	41	1980	0	0	0	1	0	0	1
Pennsylvania	PA	42	1980	1	0	0	0	0	0	1
Rhode Island	RI	44	1980	0	0	1	0	1	0	0
South Carolina	SC	45	1980	0	0	1	0	1	0	0
South Dakota	SD	46	1980							
Tennessee	TN	47	1980	0	0	1	1	0	0	1
Texas	TX	48	1980	0	0	1	0	1	0	0
Utah	UT	49	1980	0	0	0	1	0	0	1
Vermont	VT	50	1980							
Virginia	VA	51	1980							
Washington	WA	53	1980	0	1	0	0	0	0	1
West Virginia	WV	54	1980	0	0	1	0	1	0	0
Wisconsin	WI	55	1980	0	0	0	1	0	0	1
Wyoming	WY	56	1980							
Alabama	AL	1	1982	0	0	1	0	1	0	0
Alaska	AK	2	1982	0	0	0	1	0	0	1
Arizona	AZ	4	1982							
Arkansas	AR	5	1982	0	0	1	0	0	1	0
California	CA	6	1982	0	0	1	0	1	0	0
Colorado	CO	8	1982	1	0	0	0	0	0	1
Connecticut	CT	9	1982	0	0	1	0	1	0	0
Delaware	DE	10	1982	0	0	0	1	0	0	1
Florida	FL	12	1982	0	0	1	0	1	0	0
Georgia	GA	13	1982	0	0	1	0	1	0	0
Hawaii	HI	15	1982	1	0	0	0	0	0	1
Idaho	ID	16	1982	0	0	0	1	0	0	1
Illinois	IL	17	1982	0	0	1	0	1	0	0

Indiana	IN	18	1982	0	0	1	0	0	1	0
Iowa	IA	19	1982	1	0	0	0	0	0	1
Kansas	KS	20	1982	0	0	0	1	0	0	1
Maine	ME	23	1982	0	0	0	1	0	0	1
Maryland	MD	24	1982							
Massachusetts	MA	25	1982	0	0	1	0	1	0	0
Michigan	MI	26	1982	0	1	0	0	0	0	1
Minnesota	MN	27	1982	0	1	0	0	0	0	1
Missouri	MO	29	1982	1	0	0	0	0	0	1
Montana	MT	30	1982	1	0	0	0	0	0	1
Nebraska	NE	31	1982							
Nevada	NV	32	1982	0	0	0	1	0	0	1
New Hampshire	NH	33	1982							
New Jersey	NJ	34	1982							
New Mexico	NM	35	1982	0	0	1	0	1	0	0
New York	NY	36	1982	0	0	0	1	0	0	1
North Carolina	NC	37	1982							
North Dakota	ND	38	1982							
Ohio	OH	39	1982	0	0	0	1	0	0	1
Oklahoma	OK	40	1982	0	0	1	0	1	0	0
Oregon	OR	41	1982	0	0	0	1	0	0	1
Pennsylvania	PA	42	1982	1	0	0	0	0	0	1
Rhode Island	RI	44	1982	0	0	1	0	1	0	0
South Carolina	SC	45	1982	0	0	1	0	1	0	0
South Dakota	SD	46	1982							
Tennessee	TN	47	1982	0	0	1	0	1	0	0
Texas	TX	48	1982	0	1	0	0	0	0	1
Utah	UT	49	1982	0	0	1	0	0	1	0
Vermont	VT	50	1982							
Virginia	VA	51	1982							
Washington	WA	53	1982	0	0	1	0	0	1	0
West Virginia	WV	54	1982	0	0	1	0	1	0	0
Wisconsin	WI	55	1982	0	1	0	0	0	0	1
Wyoming	WY	56	1982							
Alabama	AL	1	1984	0	0	1	0	1	0	0
Alaska	AK	2	1984	0	0	0	1	0	0	1
Arizona	AZ	4	1984							
Arkansas	AR	5	1984	0	0	1	0	0	1	0

California	CA	6	1984	0	0	1	0	1	0	0
Colorado	CO	8	1984	1	0	0	0	0	0	1
Connecticut	CT	9	1984	0	0	1	0	1	0	0
Delaware	DE	10	1984	0	0	0	1	0	0	1
Florida	FL	12	1984	0	0	1	0	1	0	0
Georgia	GA	13	1984	0	0	1	0	1	0	0
Hawaii	HI	15	1984	1	0	0	0	0	0	1
Idaho	ID	16	1984	0	1	0	0	0	0	1
Illinois	IL	17	1984	0	0	1	0	1	0	0
Indiana	IN	18	1984	0	0	1	0	0	1	0
Iowa	IA	19	1984	1	0	0	0	0	0	1
Kansas	KS	20	1984	0	0	0	1	0	0	1
Kentucky	KY	21	1984	0	0	1	0	1	0	0
Louisiana	LA	22	1984							
Maine	ME	23	1984	0	0	0	1	0	0	1
Massachusetts	MA	25	1984	0	0	1	0	1	0	0
Michigan	MI	26	1984	0	1	0	0	0	0	1
Minnesota	MN	27	1984	0	1	0	0	0	0	1
Mississippi	MS	28	1984	0	0	1	0	1	0	0
Missouri	MO	29	1984	1	0	0	0	0	0	1
Montana	MT	30	1984	1	0	0	0	0	0	1
Nebraska	NE	31	1984							
Nevada	NV	32	1984	0	0	0	1	0	0	1
New Hampshire	NH	33	1984							
New Jersey	NJ	34	1984							
New Mexico	NM	35	1984	0	0	1	0	1	0	0
New York	NY	36	1984	0	0	0	1	0	0	1
North Carolina	NC	37	1984							
North Dakota	ND	38	1984							
Ohio	OH	39	1984	0	0	0	1	0	0	1
Oklahoma	OK	40	1984	0	0	1	0	1	0	0
Oregon	OR	41	1984	0	0	0	1	0	0	1
Pennsylvania	PA	42	1984	1	0	0	0	0	0	1
Rhode Island	RI	44	1984	0	0	1	0	1	0	0
South Carolina	SC	45	1984	0	0	1	0	1	0	0
South Dakota	SD	46	1984							
Tennessee	TN	47	1984	0	0	1	0	1	0	0
Texas	TX	48	1984	0	0	1	0	1	0	0

Utah	UT	49	1984	0	0	1	0	0	1	0
Vermont	VT	50	1984							
Virginia	VA	51	1984							
Washington	WA	53	1984	0	0	1	0	0	1	0
West Virginia	WV	54	1984	0	0	1	0	1	0	0
Wisconsin	WI	55	1984	0	0	0	1	0	0	1
Wyoming	WY	56	1984							
Alabama	AL	1	1986	0	0	1	0	1	0	0
Alaska	AK	2	1986	0	0	0	1	0	0	1
Arizona	AZ	4	1986							
Arkansas	AR	5	1986	0	0	1	0	0	1	0
California	CA	6	1986	0	0	1	0	1	0	0
Colorado	CO	8	1986	1	0	0	0	0	0	1
Connecticut	CT	9	1986	0	0	1	0	1	0	0
Delaware	DE	10	1986	0	0	0	1	0	0	1
Florida	FL	12	1986	0	0	1	0	1	0	0
Georgia	GA	13	1986	0	0	1	0	1	0	0
Hawaii	HI	15	1986	1	0	0	0	0	0	1
Idaho	ID	16	1986	0	1	0	0	0	0	1
Illinois	IL	17	1986	0	0	1	0	1	0	0
Indiana	IN	18	1986	0	0	1	0	0	1	0
Iowa	IA	19	1986	1	0	0	0	0	0	1
Kansas	KS	20	1986	0	0	0	1	0	0	1
Kentucky	KY	21	1986	0	0	1	0	1	0	0
Maine	ME	23	1986	0	0	0	1	0	0	1
Maryland	MD	24	1986							
Massachusetts	MA	25	1986	0	0	1	0	1	0	0
Michigan	MI	26	1986	0	1	0	0	0	0	1
Minnesota	MN	27	1986	0	1	0	0	0	0	1
Missouri	MO	29	1986	1	0	0	0	0	0	1
Montana	MT	30	1986	1	0	0	0	0	0	1
Nebraska	NE	31	1986							
Nevada	NV	32	1986	0	0	0	1	0	0	1
New Hampshire	NH	33	1986							
New Jersey	NJ	34	1986							
New Mexico	NM	35	1986	0	0	1	0	1	0	0
New York	NY	36	1986	0	0	0	1	0	0	1
North Carolina	NC	37	1986							

North Dakota	ND	38	1986								
Ohio	OH	39	1986	0	0	0	1	0	0	1
Oklahoma	OK	40	1986	0	0	1	0	1	0	0
Oregon	OR	41	1986	0	0	0	1	0	0	1
Pennsylvania	PA	42	1986	1	0	0	0	0	0	1
Rhode Island	RI	44	1986	0	0	1	0	1	0	0
South Carolina	SC	45	1986	0	0	1	0	1	0	0
South Dakota	SD	46	1986							
Tennessee	TN	47	1986	0	0	1	0	1	0	0
Texas	TX	48	1986	0	0	1	0	1	0	0
Utah	UT	49	1986	0	0	1	0	0	1	0
Vermont	VT	50	1986	0	0	1	0	0	1	0
Virginia	VA	51	1986							
Washington	WA	53	1986	0	0	1	0	0	1	0
West Virginia	WV	54	1986	0	0	1	0	1	0	0
Wisconsin	WI	55	1986	0	0	0	1	0	0	1
Wyoming	WY	56	1986							
Alaska	AK	2	1988	0	0	0	1	0	0	1
Arizona	AZ	4	1988							
Arkansas	AR	5	1988	0	0	1	0	0	1	0
California	CA	6	1988	0	0	1	0	1	0	0
Colorado	CO	8	1988	1	0	0	0	0	0	1
Connecticut	CT	9	1988	0	0	1	0	1	0	0
Delaware	DE	10	1988	0	0	0	1	0	0	1
Florida	FL	12	1988	0	0	1	0	1	0	0
Georgia	GA	13	1988	0	0	1	0	1	0	0
Hawaii	HI	15	1988	1	0	0	0	0	0	1
Idaho	ID	16	1988	0	1	0	0	0	0	1
Illinois	IL	17	1988	0	0	1	0	1	0	0
Indiana	IN	18	1988	0	0	1	0	0	1	0
Iowa	IA	19	1988	1	0	0	0	0	0	1
Kansas	KS	20	1988	0	0	0	1	0	0	1
Kentucky	KY	21	1988	0	0	1	0	1	0	0
Louisiana	LA	22	1988							
Maine	ME	23	1988	0	0	0	1	0	0	1
Massachusetts	MA	25	1988	0	0	1	0	1	0	0
Michigan	MI	26	1988	0	1	0	0	0	0	1
Minnesota	MN	27	1988	0	1	0	0	0	0	1

Mississippi	MS	28	1988	0	0	1	0	1	0	0
Missouri	MO	29	1988	1	0	0	0	0	0	1
Montana	MT	30	1988	1	0	0	0	0	0	1
Nebraska	NE	31	1988							
Nevada	NV	32	1988	0	0	0	1	0	0	1
New Hampshire	NH	33	1988							
New Jersey	NJ	34	1988							
New Mexico	NM	35	1988	0	0	1	0	1	0	0
New York	NY	36	1988	0	0	0	1	0	0	1
North Carolina	NC	37	1988							
North Dakota	ND	38	1988							
Ohio	OH	39	1988	0	0	0	1	0	0	1
Oklahoma	OK	40	1988	0	0	1	0	1	0	0
Oregon	OR	41	1988	0	0	0	1	0	0	1
Pennsylvania	PA	42	1988	1	0	0	0	0	0	1
Rhode Island	RI	44	1988	0	0	1	0	1	0	0
South Carolina	SC	45	1988	0	0	1	0	1	0	0
South Dakota	SD	46	1988							
Tennessee	TN	47	1988	0	0	1	0	1	0	0
Texas	TX	48	1988	0	0	1	0	1	0	0
Utah	UT	49	1988	0	0	1	0	0	1	0
Vermont	VT	50	1988	0	0	1	0	0	1	0
Virginia	VA	51	1988							
Washington	WA	53	1988	0	0	1	0	0	1	0
West Virginia	WV	54	1988	0	0	1	0	1	0	0
Wisconsin	WI	55	1988	0	0	0	1	0	0	1
Wyoming	WY	56	1988							
Alabama	AL	1	1990	0	0	1	0	1	0	0
Alaska	AK	2	1990	0	0	0	1	0	0	1
Arizona	AZ	4	1990							
Arkansas	AR	5	1990	0	0	1	0	0	1	0
California	CA	6	1990	0	0	1	0	1	0	0
Colorado	CO	8	1990	1	0	0	0	0	0	1
Connecticut	CT	9	1990	0	0	1	0	1	0	0
Delaware	DE	10	1990	0	0	0	1	0	0	1
Florida	FL	12	1990	0	0	1	0	1	0	0
Georgia	GA	13	1990	0	0	1	0	1	0	0
Hawaii	HI	15	1990	1	0	0	0	0	0	1

Idaho	ID	16	1990	0	1	0	0	0	0	1
Illinois	IL	17	1990	0	0	1	0	1	0	0
Indiana	IN	18	1990	0	0	1	0	0	1	0
Iowa	IA	19	1990	1	0	0	0	0	0	1
Kansas	KS	20	1990	0	0	0	1	0	0	1
Kentucky	KY	21	1990	0	0	1	0	1	0	0
Maine	ME	23	1990	0	0	0	1	0	0	1
Maryland	MD	24	1990							
Massachusetts	MA	25	1990	0	0	1	0	1	0	0
Michigan	MI	26	1990	0	1	0	0	0	0	1
Minnesota	MN	27	1990	0	1	0	0	0	0	1
Missouri	MO	29	1990	1	0	0	0	0	0	1
Montana	MT	30	1990	1	0	0	0	0	0	1
Nebraska	NE	31	1990							
Nevada	NV	32	1990	0	0	0	1	0	0	1
New Hampshire	NH	33	1990							
New Jersey	NJ	34	1990							
New Mexico	NM	35	1990	0	0	1	0	1	0	0
New York	NY	36	1990	0	0	0	1	0	0	1
North Carolina	NC	37	1990							
North Dakota	ND	38	1990							
Ohio	OH	39	1990	0	0	0	1	0	0	1
Oklahoma	OK	40	1990	0	0	1	0	1	0	0
Oregon	OR	41	1990	0	0	0	1	0	0	1
Pennsylvania	PA	42	1990	1	0	0	0	0	0	1
Rhode Island	RI	44	1990	0	0	1	0	1	0	0
South Carolina	SC	45	1990	0	0	1	0	1	0	0
South Dakota	SD	46	1990							
Tennessee	TN	47	1990	0	0	1	0	1	0	0
Texas	TX	48	1990	0	0	1	0	1	0	0
Utah	UT	49	1990	0	0	1	0	0	1	0
Vermont	VT	50	1990	0	0	1	0	0	1	0
Virginia	VA	51	1990							
Washington	WA	53	1990	0	0	1	0	0	1	0
West Virginia	WV	54	1990	0	0	1	0	1	0	0
Wisconsin	WI	55	1990	0	0	0	1	0	0	1
Wyoming	WY	56	1990							
Alaska	AK	2	1992	0	1	0	0	0	0	1

Arizona	AZ	4	1992	0	0	0	1	0	0	1
Arkansas	AR	5	1992	0	0	1	0	1	0	0
California	CA	6	1992	0	1	0	0	0	0	1
Colorado	CO	8	1992	1	0	0	0	0	0	1
Connecticut	CT	9	1992	1	0	0	0	0	0	1
Delaware	DE	10	1992	0	0	0	1	0	0	1
Florida	FL	12	1992	0	0	1	0	1	0	0
Georgia	GA	13	1992	0	0	1	0	1	0	0
Hawaii	HI	15	1992	1	0	0	0	0	0	1
Idaho	ID	16	1992	0	0	0	1	0	0	1
Illinois	IL	17	1992	0	0	1	0	0	1	0
Indiana	IN	18	1992	0	0	0	1	0	0	1
Iowa	IA	19	1992	1	0	0	0	0	0	1
Kansas	KS	20	1992	0	0	0	1	0	0	1
Kentucky	KY	21	1992	0	0	1	0	1	0	0
Louisiana	LA	22	1992	0	0	1	0	1	0	0
Maine	ME	23	1992	0	1	0	0	0	0	1
Massachusetts	MA	25	1992	0	0	0	1	0	0	1
Michigan	MI	26	1992	0	1	0	0	0	0	1
Minnesota	MN	27	1992	0	1	0	0	0	0	1
Mississippi	MS	28	1992	0	0	1	0	1	0	0
Missouri	MO	29	1992	1	0	0	0	0	0	1
Montana	MT	30	1992	1	0	0	0	0	0	1
Nebraska	NE	31	1992							
Nevada	NV	32	1992	0	0	1	0	1	0	0
New Hampshire	NH	33	1992	0	0	1	0	0	1	0
New Jersey	NJ	34	1992	1	0	0	0	0	0	1
New Mexico	NM	35	1992	0	0	1	0	1	0	0
New York	NY	36	1992	0	0	0	1	0	0	1
North Carolina	NC	37	1992	0	0	1	0	1	0	0
North Dakota	ND	38	1992	0	0	0	1	0	0	1
Ohio	OH	39	1992	0	0	0	1	0	0	1
Oklahoma	OK	40	1992	0	0	1	0	1	0	0
Oregon	OR	41	1992	0	0	0	1	0	0	1
Pennsylvania	PA	42	1992	1	0	0	0	0	0	1
Rhode Island	RI	44	1992	0	0	1	0	1	0	0
South Carolina	SC	45	1992	0	1	0	0	0	0	1
South Dakota	SD	46	1992	0	0	1	0	0	1	0

Tennessee	TN	47	1992	0	0	1	0	1	0	0
Texas	TX	48	1992	0	0	1	0	1	0	0
Utah	UT	49	1992	0	0	1	0	0	1	0
Vermont	VT	50	1992	0	0	0	1	0	0	1
Virginia	VA	51	1992	0	0	1	0	1	0	0
Washington	WA	53	1992	1	0	0	0	0	0	1
West Virginia	WV	54	1992	0	0	1	0	1	0	0
Wisconsin	WI	55	1992	0	1	0	0	0	0	1
Wyoming	WY	56	1992	0	0	0	1	0	0	1
Alabama	AL	1	1994	0	1	0	0	0	0	1
Alaska	AK	2	1994	0	1	0	0	0	0	1
Arizona	AZ	4	1994	0	0	0	1	0	0	1
Arkansas	AR	5	1994	0	0	1	0	1	0	0
California	CA	6	1994	0	1	0	0	0	0	1
Colorado	CO	8	1994	1	0	0	0	0	0	1
Connecticut	CT	9	1994	1	0	0	0	0	0	1
Delaware	DE	10	1994	0	0	0	1	0	0	1
Florida	FL	12	1994	0	0	1	0	1	0	0
Georgia	GA	13	1994	0	0	1	0	1	0	0
Hawaii	HI	15	1994	1	0	0	0	0	0	1
Idaho	ID	16	1994	0	0	0	1	0	0	1
Illinois	IL	17	1994	0	0	1	0	0	1	0
Indiana	IN	18	1994	0	0	0	1	0	0	1
Iowa	IA	19	1994	1	0	0	0	0	0	1
Kansas	KS	20	1994	0	0	0	1	0	0	1
Kentucky	KY	21	1994	0	0	1	0	1	0	0
Maine	ME	23	1994	0	1	0	0	0	0	1
Maryland	MD	24	1994	0	0	1	0	1	0	0
Massachusetts	MA	25	1994	0	0	0	1	0	0	1
Michigan	MI	26	1994	0	1	0	0	0	0	1
Minnesota	MN	27	1994	0	1	0	0	0	0	1
Missouri	MO	29	1994	1	0	0	0	0	0	1
Montana	MT	30	1994	1	0	0	0	0	0	1
Nebraska	NE	31	1994							
Nevada	NV	32	1994	0	0	1	0	1	0	0
New Hampshire	NH	33	1994	0	0	1	0	0	1	0
New Jersey	NJ	34	1994	1	0	0	0	0	0	1
New Mexico	NM	35	1994	0	0	1	0	1	0	0

New York	NY	36	1994	0	0	0	1	0	0	1
North Carolina	NC	37	1994	0	0	1	0	1	0	0
North Dakota	ND	38	1994	0	0	0	1	0	0	1
Ohio	OH	39	1994	0	0	0	1	0	0	1
Oklahoma	OK	40	1994	0	0	1	0	1	0	0
Oregon	OR	41	1994	0	0	0	1	0	0	1
Pennsylvania	PA	42	1994	1	0	0	0	0	0	1
Rhode Island	RI	44	1994	0	0	1	0	1	0	0
South Carolina	SC	45	1994	0	1	0	0	0	0	1
South Dakota	SD	46	1994	0	0	1	0	0	1	0
Tennessee	TN	47	1994	0	0	1	0	1	0	0
Texas	TX	48	1994	0	0	1	0	1	0	0
Utah	UT	49	1994	0	0	1	0	0	1	0
Vermont	VT	50	1994	0	0	0	1	0	0	1
Virginia	VA	51	1994	0	0	1	0	1	0	0
Washington	WA	53	1994	1	0	0	0	0	0	1
West Virginia	WV	54	1994	0	0	1	0	1	0	0
Wisconsin	WI	55	1994	0	1	0	0	0	0	1
Wyoming	WY	56	1994	0	0	0	1	0	0	1
Alaska	AK	2	1996	0	1	0	0	0	0	1
Arizona	AZ	4	1996	0	0	0	1	0	0	1
Arkansas	AR	5	1996	0	0	1	0	1	0	0
California	CA	6	1996	0	1	0	0	0	0	1
Colorado	CO	8	1996	1	0	0	0	0	0	1
Connecticut	CT	9	1996	1	0	0	0	0	0	1
Delaware	DE	10	1996	0	0	0	1	0	0	1
Florida	FL	12	1996	0	0	1	0	1	0	0
Georgia	GA	13	1996	0	0	1	0	1	0	0
Hawaii	HI	15	1996	1	0	0	0	0	0	1
Idaho	ID	16	1996	0	0	0	1	0	0	1
Illinois	IL	17	1996	0	0	1	0	0	1	0
Indiana	IN	18	1996	0	0	0	1	0	0	1
Iowa	IA	19	1996	1	0	0	0	0	0	1
Kansas	KS	20	1996	0	0	0	1	0	0	1
Kentucky	KY	21	1996	0	0	1	0	1	0	0
Louisiana	LA	22	1996	0	0	1	0	1	0	0
Maine	ME	23	1996	0	1	0	0	0	0	1
Massachusetts	MA	25	1996	0	0	0	1	0	0	1

Michigan	MI	26	1996	0	1	0	0	0	0	1
Minnesota	MN	27	1996	0	1	0	0	0	0	1
Mississippi	MS	28	1996	0	0	1	0	1	0	0
Missouri	MO	29	1996	1	0	0	0	0	0	1
Montana	MT	30	1996	1	0	0	0	0	0	1
Nebraska	NE	31	1996							
Nevada	NV	32	1996	0	0	1	0	1	0	0
New Hampshire	NH	33	1996	0	0	1	0	0	1	0
New Jersey	NJ	34	1996	1	0	0	0	0	0	1
New Mexico	NM	35	1996	0	0	1	0	1	0	0
New York	NY	36	1996	0	0	0	1	0	0	1
North Carolina	NC	37	1996	0	0	1	0	1	0	0
North Dakota	ND	38	1996	0	0	0	1	0	0	1
Ohio	OH	39	1996	0	0	0	1	0	0	1
Oklahoma	OK	40	1996	0	0	1	0	1	0	0
Oregon	OR	41	1996	0	0	0	1	0	0	1
Pennsylvania	PA	42	1996	1	0	0	0	0	0	1
Rhode Island	RI	44	1996	0	0	1	0	1	0	0
South Carolina	SC	45	1996	0	1	0	0	0	0	1
South Dakota	SD	46	1996	0	0	1	0	0	1	0
Tennessee	TN	47	1996	0	0	1	0	1	0	0
Texas	TX	48	1996	0	0	1	0	1	0	0
Utah	UT	49	1996	0	0	1	0	0	1	0
Vermont	VT	50	1996	0	0	0	1	0	0	1
Virginia	VA	51	1996	0	0	1	0	1	0	0
Washington	WA	53	1996	1	0	0	0	0	0	1
West Virginia	WV	54	1996	0	0	1	0	1	0	0
Wisconsin	WI	55	1996	0	1	0	0	0	0	1
Wyoming	WY	56	1996	0	0	0	1	0	0	1
Alabama	AL	1	1998	0	1	0	0	0	0	1
Alaska	AK	2	1998	0	1	0	0	0	0	1
Arizona	AZ	4	1998	0	0	0	1	0	0	1
Arkansas	AR	5	1998	0	0	1	0	1	0	0
California	CA	6	1998	0	1	0	0	0	0	1
Colorado	CO	8	1998	1	0	0	0	0	0	1
Connecticut	CT	9	1998	1	0	0	0	0	0	1
Delaware	DE	10	1998	0	0	0	1	0	0	1
Florida	FL	12	1998	0	0	1	0	1	0	0

Georgia	GA	13	1998	0	0	1	0	1	0	0
Hawaii	HI	15	1998	1	0	0	0	0	0	1
Idaho	ID	16	1998	0	0	0	1	0	0	1
Illinois	IL	17	1998	0	0	1	0	0	1	0
Indiana	IN	18	1998	0	0	0	1	0	0	1
Iowa	IA	19	1998	1	0	0	0	0	0	1
Kansas	KS	20	1998	0	0	0	1	0	0	1
Kentucky	KY	21	1998	0	0	1	0	1	0	0
Maine	ME	23	1998	0	1	0	0	0	0	1
Maryland	MD	24	1998	0	0	1	0	1	0	0
Massachusetts	MA	25	1998	0	0	0	1	0	0	1
Michigan	MI	26	1998	0	1	0	0	0	0	1
Minnesota	MN	27	1998	0	1	0	0	0	0	1
Missouri	MO	29	1998	1	0	0	0	0	0	1
Montana	MT	30	1998	1	0	0	0	0	0	1
Nebraska	NE	31	1998							
Nevada	NV	32	1998	0	0	1	0	1	0	0
New Hampshire	NH	33	1998	0	0	1	0	0	1	0
New Jersey	NJ	34	1998	1	0	0	0	0	0	1
New Mexico	NM	35	1998	0	0	1	0	1	0	0
New York	NY	36	1998	0	0	0	1	0	0	1
North Carolina	NC	37	1998	0	0	1	0	1	0	0
North Dakota	ND	38	1998	0	0	0	1	0	0	1
Ohio	OH	39	1998	0	0	0	1	0	0	1
Oklahoma	OK	40	1998	0	0	1	0	1	0	0
Oregon	OR	41	1998	0	0	0	1	0	0	1
Pennsylvania	PA	42	1998	1	0	0	0	0	0	1
Rhode Island	RI	44	1998	0	0	1	0	1	0	0
South Carolina	SC	45	1998	0	1	0	0	0	0	1
South Dakota	SD	46	1998	0	0	1	0	0	1	0
Tennessee	TN	47	1998	0	0	1	0	1	0	0
Texas	TX	48	1998	0	0	1	0	1	0	0
Utah	UT	49	1998	0	0	1	0	0	1	0
Vermont	VT	50	1998	0	0	0	1	0	0	1
Virginia	VA	51	1998	0	0	1	0	1	0	0
Washington	WA	53	1998	1	0	0	0	0	0	1
West Virginia	WV	54	1998	0	0	1	0	1	0	0
Wisconsin	WI	55	1998	0	1	0	0	0	0	1

Wyoming	WY	56	1998	0	0	0	1	0	0	1
Alaska	AK	2	2000	0	1	0	0	0	0	1
Arizona	AZ	4	2000	0	0	0	1	0	0	1
Arkansas	AR	5	2000	0	0	1	0	1	0	0
California	CA	6	2000	0	1	0	0	0	0	1
Colorado	CO	8	2000	1	0	0	0	0	0	1
Connecticut	CT	9	2000	1	0	0	0	0	0	1
Delaware	DE	10	2000	0	0	0	1	0	0	1
Florida	FL	12	2000	0	0	1	0	1	0	0
Georgia	GA	13	2000	0	0	1	0	1	0	0
Hawaii	HI	15	2000	1	0	0	0	0	0	1
Idaho	ID	16	2000	0	0	0	1	0	0	1
Illinois	IL	17	2000	0	0	1	0	0	1	0
Indiana	IN	18	2000	0	0	0	1	0	0	1
Iowa	IA	19	2000	1	0	0	0	0	0	1
Kansas	KS	20	2000	0	0	0	1	0	0	1
Kentucky	KY	21	2000	0	0	1	0	1	0	0
Louisiana	LA	22	2000	0	0	1	0	1	0	0
Maine	ME	23	2000	0	1	0	0	0	0	1
Massachusetts	MA	25	2000	0	0	0	1	0	0	1
Michigan	MI	26	2000	0	1	0	0	0	0	1
Minnesota	MN	27	2000	0	1	0	0	0	0	1
Mississippi	MS	28	2000	0	0	1	0	1	0	0
Missouri	MO	29	2000	1	0	0	0	0	0	1
Montana	MT	30	2000	1	0	0	0	0	0	1
Nebraska	NE	31	2000							
Nevada	NV	32	2000	0	0	1	0	1	0	0
New Hampshire	NH	33	2000	0	0	1	0	0	1	0
New Jersey	NJ	34	2000	1	0	0	0	0	0	1
New Mexico	NM	35	2000	0	0	1	0	1	0	0
New York	NY	36	2000	0	0	0	1	0	0	1
North Carolina	NC	37	2000	0	0	1	0	1	0	0
North Dakota	ND	38	2000	0	0	0	1	0	0	1
Ohio	OH	39	2000	0	0	0	1	0	0	1
Oklahoma	OK	40	2000	0	0	1	0	1	0	0
Oregon	OR	41	2000	0	0	0	1	0	0	1
Pennsylvania	PA	42	2000	1	0	0	0	0	0	1
Rhode Island	RI	44	2000	0	0	1	0	1	0	0

South Carolina	SC	45	2000	0	1	0	0	0	0	1
South Dakota	SD	46	2000	0	0	1	0	0	1	0
Tennessee	TN	47	2000	0	0	1	0	1	0	0
Texas	TX	48	2000	0	0	1	0	1	0	0
Utah	UT	49	2000	0	0	1	0	0	1	0
Vermont	VT	50	2000	0	0	0	1	0	0	1
Virginia	VA	51	2000	0	0	1	0	1	0	0
Washington	WA	53	2000	1	0	0	0	0	0	1
West Virginia	WV	54	2000	0	0	1	0	1	0	0
Wisconsin	WI	55	2000	0	1	0	0	0	0	1
Wyoming	WY	56	2000	0	0	0	1	0	0	1
Illinois	IL	17	2006	0	0	1	0	1	0	0
Alaska	AK	2	2002	1	0	0	0	0	0	1
Arizona	AZ	4	2002	1	0	0	0	0	0	1
Arkansas	AR	5	2002	0	0	1	0	0	1	0
Illinois	IL	17	2008	0	0	1	0	1	0	0
Colorado	CO	8	2002	1	0	0	0	0	0	1
Connecticut	CT	9	2002	1	0	0	0	0	0	1
Delaware	DE	10	2002	0	0	0	1	0	0	1
Florida	FL	12	2002	0	0	1	0	0	1	0
Illinois	IL	17	2004	0	0	1	0	1	0	0
Hawaii	HI	15	2002	1	0	0	0	0	0	1
Idaho	ID	16	2002	1	0	0	0	0	0	1
California	CA	6	2008	0	0	1	0	1	0	0
Indiana	IN	18	2002	0	0	0	1	0	0	1
Iowa	IA	19	2002	1	0	0	0	0	0	1
Kansas	KS	20	2002	0	0	1	0	0	1	0
Kentucky	KY	21	2002	0	0	0	1	0	0	1
Maine	ME	23	2002	0	0	0	1	0	0	1
Illinois	IL	17	2002	0	0	1	0	1	0	0
Alabama	AL	1	2010	0	0	1	0	1	0	0
Michigan	MI	26	2002	0	0	1	0	0	1	0
Minnesota	MN	27	2002	0	1	0	0	0	0	1
Missouri	MO	29	2002	1	0	0	0	0	0	1
Montana	MT	30	2002	1	0	0	0	0	0	1
Nebraska	NE	31	2002							
Nevada	NV	32	2002	0	0	0	1	0	0	1
New Hampshire	NH	33	2002	0	1	0	0	0	0	1

New Jersey	NJ	34	2002	1	0	0	0	0	0	1
New Mexico	NM	35	2002	0	1	0	0	0	0	1
New York	NY	36	2002	0	0	0	1	0	0	1
Delaware	DE	10	2012	0	0	1	0	1	0	0
North Dakota	ND	38	2002	0	0	1	0	0	1	0
Ohio	OH	39	2002	0	0	1	0	0	1	0
Oklahoma	OK	40	2002	0	0	0	1	0	0	1
Oregon	OR	41	2002	0	0	0	1	0	0	1
Pennsylvania	PA	42	2002	1	0	0	0	0	0	1
Massachusetts	MA	25	2012	0	0	1	0	1	0	0
South Carolina	SC	45	2002	0	1	0	0	0	0	1
South Dakota	SD	46	2002	0	0	1	0	0	1	0
Tennessee	TN	47	2002	0	0	0	1	0	0	1
Texas	TX	48	2002	0	1	0	0	0	0	1
Utah	UT	49	2002	0	0	1	0	0	1	0
Vermont	VT	50	2002	0	0	0	1	0	0	1
Virginia	VA	51	2002	0	0	1	0	0	1	0
Washington	WA	53	2002	1	0	0	0	0	0	1
West Virginia	WV	54	2014	0	0	1	0	1	0	0
Wisconsin	WI	55	2002	0	1	0	0	0	0	1
Wyoming	WY	56	2002	0	0	1	0	0	1	0
Alaska	AK	2	2004	1	0	0	0	0	0	1
Arizona	AZ	4	2004	1	0	0	0	0	0	1
Arkansas	AR	5	2004	0	0	1	0	0	1	0
California	CA	6	2006	0	0	1	0	1	0	0
Colorado	CO	8	2004	1	0	0	0	0	0	1
Connecticut	CT	9	2004	1	0	0	0	0	0	1
Delaware	DE	10	2004	0	0	0	1	0	0	1
Florida	FL	12	2004	0	0	1	0	0	1	0
Georgia	GA	13	2004	0	1	0	0	0	0	1
Hawaii	HI	15	2004	1	0	0	0	0	0	1
Idaho	ID	16	2004	1	0	0	0	0	0	1
West Virginia	WV	54	2008	0	0	1	0	1	0	0
Indiana	IN	18	2004	0	0	0	1	0	0	1
Iowa	IA	19	2004	1	0	0	0	0	0	1
Kansas	KS	20	2004	0	0	1	0	0	1	0
Kentucky	KY	21	2004	0	0	0	1	0	0	1
Louisiana	LA	22	2004	0	0	0	1	0	0	1

Maine	ME	23	2004	0	0	0	1	0	0	1
Illinois	IL	17	2012	0	0	1	0	1	0	0
Michigan	MI	26	2004	0	0	1	0	0	1	0
Minnesota	MN	27	2004	0	1	0	0	0	0	1
West Virginia	WV	54	2004	0	0	1	0	1	0	0
Missouri	MO	29	2004	1	0	0	0	0	0	1
Montana	MT	30	2004	1	0	0	0	0	0	1
Nebraska	NE	31	2004							
Nevada	NV	32	2004	0	0	0	1	0	0	1
New Hampshire	NH	33	2004	0	1	0	0	0	0	1
New Jersey	NJ	34	2004	1	0	0	0	0	0	1
New Mexico	NM	35	2004	0	1	0	0	0	0	1
New York	NY	36	2004	0	0	0	1	0	0	1
North Carolina	NC	37	2008	0	0	1	0	1	0	0
North Dakota	ND	38	2004	0	0	1	0	0	1	0
Ohio	OH	39	2004	0	0	1	0	0	1	0
Oklahoma	OK	40	2004	0	0	0	1	0	0	1
Oregon	OR	41	2004	0	0	0	1	0	0	1
Pennsylvania	PA	42	2004	1	0	0	0	0	0	1
West Virginia	WV	54	2006	0	0	1	0	1	0	0
South Carolina	SC	45	2004	0	1	0	0	0	0	1
South Dakota	SD	46	2004	0	0	1	0	0	1	0
Tennessee	TN	47	2004	0	0	0	1	0	0	1
Texas	TX	48	2004	0	1	0	0	0	0	1
Utah	UT	49	2004	0	0	1	0	0	1	0
Vermont	VT	50	2004	0	0	0	1	0	0	1
Virginia	VA	51	2004	0	0	1	0	0	1	0
Washington	WA	53	2004	1	0	0	0	0	0	1
Arkansas	AR	5	2014	0	0	1	0	1	0	0
Wisconsin	WI	55	2004	0	1	0	0	0	0	1
Wyoming	WY	56	2004	0	0	1	0	0	1	0
North Carolina	NC	37	2002	0	0	1	0	1	0	0
Alaska	AK	2	2006	1	0	0	0	0	0	1
Arizona	AZ	4	2006	1	0	0	0	0	0	1
Arkansas	AR	5	2006	0	0	1	0	0	1	0
Alabama	AL	1	2006	0	0	1	0	1	0	0
Colorado	CO	8	2006	1	0	0	0	0	0	1
Connecticut	CT	9	2006	1	0	0	0	0	0	1

Delaware	DE	10	2006	0	0	0	1	0	0	1
Florida	FL	12	2006	0	0	1	0	0	1	0
Georgia	GA	13	2006	0	1	0	0	0	0	1
Hawaii	HI	15	2006	1	0	0	0	0	0	1
Idaho	ID	16	2006	1	0	0	0	0	0	1
California	CA	6	2004	0	0	1	0	1	0	0
Indiana	IN	18	2006	0	0	0	1	0	0	1
Iowa	IA	19	2006	1	0	0	0	0	0	1
Kansas	KS	20	2006	0	0	1	0	0	1	0
Kentucky	KY	21	2006	0	0	0	1	0	0	1
Maine	ME	23	2006	0	0	0	1	0	0	1
California	CA	6	2002	0	0	1	0	1	0	0
North Carolina	NC	37	2006	0	0	1	0	1	0	0
Michigan	MI	26	2006	0	0	1	0	0	1	0
Minnesota	MN	27	2006	0	1	0	0	0	0	1
Missouri	MO	29	2006	1	0	0	0	0	0	1
Montana	MT	30	2006	1	0	0	0	0	0	1
Nebraska	NE	31	2006							
Nevada	NV	32	2006	0	0	0	1	0	0	1
New Hampshire	NH	33	2006	0	1	0	0	0	0	1
New Jersey	NJ	34	2006	1	0	0	0	0	0	1
New Mexico	NM	35	2006	0	1	0	0	0	0	1
New York	NY	36	2006	0	0	0	1	0	0	1
California	CA	6	2010	0	0	1	0	1	0	0
North Dakota	ND	38	2006	0	0	1	0	0	1	0
Ohio	OH	39	2006	0	0	1	0	0	1	0
Oklahoma	OK	40	2006	0	0	0	1	0	0	1
Oregon	OR	41	2006	0	0	0	1	0	0	1
Pennsylvania	PA	42	2006	1	0	0	0	0	0	1
North Carolina	NC	37	2004	0	0	1	0	1	0	0
South Carolina	SC	45	2006	0	1	0	0	0	0	1
South Dakota	SD	46	2006	0	0	1	0	0	1	0
Tennessee	TN	47	2006	0	0	0	1	0	0	1
Texas	TX	48	2006	0	1	0	0	0	0	1
Utah	UT	49	2006	0	0	1	0	0	1	0
Vermont	VT	50	2006	0	0	0	1	0	0	1
Virginia	VA	51	2006	0	0	1	0	0	1	0
Washington	WA	53	2006	1	0	0	0	0	0	1

Rhode Island	RI	44	2006	0	0	1	0	1	0	0
Wisconsin	WI	55	2006	0	1	0	0	0	0	1
Wyoming	WY	56	2006	0	0	1	0	0	1	0
Alaska	AK	2	2008	1	0	0	0	0	0	1
Arizona	AZ	4	2008	1	0	0	0	0	0	1
Arkansas	AR	5	2008	0	0	1	0	0	1	0
Massachusetts	MA	25	2006	0	0	1	0	1	0	0
Colorado	CO	8	2008	1	0	0	0	0	0	1
Connecticut	CT	9	2008	1	0	0	0	0	0	1
Delaware	DE	10	2008	0	0	0	1	0	0	1
Florida	FL	12	2008	0	0	1	0	0	1	0
Georgia	GA	13	2008	0	1	0	0	0	0	1
Hawaii	HI	15	2008	1	0	0	0	0	0	1
Idaho	ID	16	2008	1	0	0	0	0	0	1
Delaware	DE	10	2014	0	0	1	0	1	0	0
Indiana	IN	18	2008	0	0	0	1	0	0	1
Iowa	IA	19	2008	1	0	0	0	0	0	1
Kansas	KS	20	2008	0	0	1	0	0	1	0
Kentucky	KY	21	2008	0	0	0	1	0	0	1
Louisiana	LA	22	2008	0	0	0	1	0	0	1
Maine	ME	23	2008	0	0	0	1	0	0	1
Illinois	IL	17	2010	0	0	1	0	1	0	0
Michigan	MI	26	2008	0	0	1	0	0	1	0
Minnesota	MN	27	2008	0	1	0	0	0	0	1
North Carolina	NC	37	2010	0	0	1	0	1	0	0
Missouri	MO	29	2008	1	0	0	0	0	0	1
Montana	MT	30	2008	1	0	0	0	0	0	1
Nebraska	NE	31	2008							
Nevada	NV	32	2008	0	0	0	1	0	0	1
New Hampshire	NH	33	2008	0	1	0	0	0	0	1
New Jersey	NJ	34	2008	1	0	0	0	0	0	1
New Mexico	NM	35	2008	0	1	0	0	0	0	1
New York	NY	36	2008	0	0	0	1	0	0	1
Massachusetts	MA	25	2004	0	0	1	0	1	0	0
North Dakota	ND	38	2008	0	0	1	0	0	1	0
Ohio	OH	39	2008	0	0	1	0	0	1	0
Oklahoma	OK	40	2008	0	0	0	1	0	0	1
Oregon	OR	41	2008	0	0	0	1	0	0	1

Pennsylvania	PA	42	2008	1	0	0	0	0	0	1
West Virginia	WV	54	2012	0	0	1	0	1	0	0
South Carolina	SC	45	2008	0	1	0	0	0	0	1
South Dakota	SD	46	2008	0	0	1	0	0	1	0
Tennessee	TN	47	2008	0	0	0	1	0	0	1
Texas	TX	48	2008	0	1	0	0	0	0	1
Utah	UT	49	2008	0	0	1	0	0	1	0
Vermont	VT	50	2008	0	0	0	1	0	0	1
Virginia	VA	51	2008	0	0	1	0	0	1	0
Washington	WA	53	2008	1	0	0	0	0	0	1
Oregon	OR	41	2012	0	0	1	0	1	0	0
Wisconsin	WI	55	2008	0	1	0	0	0	0	1
Wyoming	WY	56	2008	0	0	1	0	0	1	0
Arkansas	AR	5	2012	0	0	1	0	1	0	0
Alaska	AK	2	2010	1	0	0	0	0	0	1
Arizona	AZ	4	2010	1	0	0	0	0	0	1
Arkansas	AR	5	2010	0	0	1	0	0	1	0
Alabama	AL	1	2002	0	0	1	0	1	0	0
Colorado	CO	8	2010	1	0	0	0	0	0	1
Connecticut	CT	9	2010	1	0	0	0	0	0	1
Delaware	DE	10	2010	0	0	0	1	0	0	1
Florida	FL	12	2010	0	0	1	0	0	1	0
Georgia	GA	13	2010	0	1	0	0	0	0	1
Hawaii	HI	15	2010	1	0	0	0	0	0	1
Idaho	ID	16	2010	1	0	0	0	0	0	1
Illinois	IL	17	2014	0	0	1	0	1	0	0
Indiana	IN	18	2010	0	0	0	1	0	0	1
Iowa	IA	19	2010	1	0	0	0	0	0	1
Kansas	KS	20	2010	0	0	1	0	0	1	0
Kentucky	KY	21	2010	0	0	0	1	0	0	1
Maine	ME	23	2010	0	0	0	1	0	0	1
Oregon	OR	41	2014	0	0	1	0	1	0	0
West Virginia	WV	54	2010	0	0	1	0	1	0	0
Michigan	MI	26	2010	0	0	1	0	0	1	0
Minnesota	MN	27	2010	0	1	0	0	0	0	1
Missouri	MO	29	2010	1	0	0	0	0	0	1
Montana	MT	30	2010	1	0	0	0	0	0	1
Nebraska	NE	31	2010							

Nevada	NV	32	2010	0	0	0	1	0	0	1
New Hampshire	NH	33	2010	0	1	0	0	0	0	1
New Jersey	NJ	34	2010	1	0	0	0	0	0	1
New Mexico	NM	35	2010	0	1	0	0	0	0	1
New York	NY	36	2010	0	0	0	1	0	0	1
Massachusetts	MA	25	2008	0	0	1	0	1	0	0
North Dakota	ND	38	2010	0	0	1	0	0	1	0
Ohio	OH	39	2010	0	0	1	0	0	1	0
Oklahoma	OK	40	2010	0	0	0	1	0	0	1
Oregon	OR	41	2010	0	0	0	1	0	0	1
Pennsylvania	PA	42	2010	1	0	0	0	0	0	1
Massachusetts	MA	25	2010	0	0	1	0	1	0	0
South Carolina	SC	45	2010	0	1	0	0	0	0	1
South Dakota	SD	46	2010	0	0	1	0	0	1	0
Tennessee	TN	47	2010	0	0	0	1	0	0	1
Texas	TX	48	2010	0	1	0	0	0	0	1
Utah	UT	49	2010	0	0	1	0	0	1	0
Vermont	VT	50	2010	0	0	0	1	0	0	1
Virginia	VA	51	2010	0	0	1	0	0	1	0
Washington	WA	53	2010	1	0	0	0	0	0	1
Massachusetts	MA	25	2014	0	0	1	0	1	0	0
Wisconsin	WI	55	2010	0	1	0	0	0	0	1
Wyoming	WY	56	2010	0	0	1	0	0	1	0
Alaska	AK	2	2012	1	0	0	0	0	0	1
Arizona	AZ	4	2012	1	0	0	0	0	0	1
Rhode Island	RI	44	2004	0	0	1	0	1	0	0
California	CA	6	2012	1	0	0	0	0	0	1
Colorado	CO	8	2012	1	0	0	0	0	0	1
Connecticut	CT	9	2012	1	0	0	0	0	0	1
Rhode Island	RI	44	2002	0	0	1	0	1	0	0
Florida	FL	12	2012	0	0	1	0	0	1	0
Georgia	GA	13	2012	0	0	1	0	0	1	0
Hawaii	HI	15	2012	1	0	0	0	0	0	1
Idaho	ID	16	2012	1	0	0	0	0	0	1
Georgia	GA	13	2002	0	0	1	0	1	0	0
Indiana	IN	18	2012	0	0	1	0	0	1	0
Iowa	IA	19	2012	1	0	0	0	0	0	1
Kansas	KS	20	2012	0	0	1	0	0	1	0

Kentucky	KY	21	2012	0	0	0	1	0	0	1
Louisiana	LA	22	2012	0	0	1	0	0	1	0
Maine	ME	23	2012	0	0	0	1	0	0	1
Massachusetts	MA	25	2002	0	0	1	0	1	0	0
Michigan	MI	26	2012	0	0	1	0	0	1	0
Minnesota	MN	27	2012	0	1	0	0	0	0	1
Rhode Island	RI	44	2012	0	0	1	0	1	0	0
Missouri	MO	29	2012	1	0	0	0	0	0	1
Montana	MT	30	2012	1	0	0	0	0	0	1
Nebraska	NE	31	2012							
Nevada	NV	32	2012	0	1	0	0	0	0	1
New Hampshire	NH	33	2012	0	0	1	0	0	1	0
New Jersey	NJ	34	2012	1	0	0	0	0	0	1
New Mexico	NM	35	2012	0	1	0	0	0	0	1
New York	NY	36	2012	0	0	0	1	0	0	1
North Carolina	NC	37	2012	0	0	1	0	0	1	0
North Dakota	ND	38	2012	0	0	1	0	0	1	0
Ohio	OH	39	2012	0	0	1	0	0	1	0
Oklahoma	OK	40	2012	0	0	1	0	0	1	0
Vermont	VT	50	2012	0	0	1	0	1	0	0
Pennsylvania	PA	42	2012	1	0	0	0	0	0	1
West Virginia	WV	54	2002	0	0	1	0	1	0	0
South Carolina	SC	45	2012	0	0	1	0	0	1	0
South Dakota	SD	46	2012	0	0	1	0	0	1	0
Tennessee	TN	47	2012	0	0	1	0	0	1	0
Texas	TX	48	2012	0	0	1	0	0	1	0
Utah	UT	49	2012	0	0	1	0	0	1	0
Vermont	VT	50	2014	0	0	1	0	1	0	0
Virginia	VA	51	2012	0	0	0	1	0	0	1
Washington	WA	53	2012	1	0	0	0	0	0	1
Rhode Island	RI	44	2008	0	0	1	0	1	0	0
Wisconsin	WI	55	2012	0	0	1	0	0	1	0
Wyoming	WY	56	2012	0	0	1	0	0	1	0
Alabama	AL	1	2014	0	0	1	0	0	1	0
Alaska	AK	2	2014	1	0	0	0	0	0	1
Arizona	AZ	4	2014	1	0	0	0	0	0	1
Rhode Island	RI	44	2010	0	0	1	0	1	0	0
California	CA	6	2014	1	0	0	0	0	0	1

Colorado	CO	8	2014	1	0	0	0	0	0	1
Connecticut	CT	9	2014	1	0	0	0	0	0	1
Rhode Island	RI	44	2014	0	0	1	0	1	0	0
Florida	FL	12	2014	0	0	1	0	0	1	0
Georgia	GA	13	2014	0	0	1	0	0	1	0
Hawaii	HI	15	2014	1	0	0	0	0	0	1
Idaho	ID	16	2014	1	0	0	0	0	0	1
Maryland	MD	24	2002	0	0	1	0	1	0	0
Indiana	IN	18	2014	0	0	1	0	0	1	0
Iowa	IA	19	2014	1	0	0	0	0	0	1
Kansas	KS	20	2014	0	0	1	0	0	1	0
Kentucky	KY	21	2014	0	0	0	1	0	0	1
Maine	ME	23	2014	0	0	0	1	0	0	1
Maryland	MD	24	2006	0	0	1	0	1	0	0
Maryland	MD	24	2010	0	0	1	0	1	0	0
Michigan	MI	26	2014	0	0	1	0	0	1	0
Minnesota	MN	27	2014	0	1	0	0	0	0	1
Missouri	MO	29	2014	1	0	0	0	0	0	1
Montana	MT	30	2014	1	0	0	0	0	0	1
Nebraska	NE	31	2014							
Nevada	NV	32	2014	0	1	0	0	0	0	1
New Hampshire	NH	33	2014	0	0	1	0	0	1	0
New Jersey	NJ	34	2014	1	0	0	0	0	0	1
New Mexico	NM	35	2014	0	1	0	0	0	0	1
New York	NY	36	2014	0	0	0	1	0	0	1
North Carolina	NC	37	2014	0	0	1	0	0	1	0
North Dakota	ND	38	2014	0	0	1	0	0	1	0
Ohio	OH	39	2014	0	0	1	0	0	1	0
Oklahoma	OK	40	2014	0	0	1	0	0	1	0
Maryland	MD	24	2014	0	0	1	0	1	0	0
Pennsylvania	PA	42	2014	1	0	0	0	0	0	1
Mississippi	MS	28	2004	0	0	1	0	1	0	0
South Carolina	SC	45	2014	0	0	1	0	0	1	0
South Dakota	SD	46	2014	0	0	1	0	0	1	0
Tennessee	TN	47	2014	0	0	1	0	0	1	0
Texas	TX	48	2014	0	0	1	0	0	1	0
Utah	UT	49	2014	0	0	1	0	0	1	0
Mississippi	MS	28	2008	0	0	1	0	1	0	0

Virginia	VA	51	2014	0	0	0	1	0	0	1
Washington	WA	53	2014	1	0	0	0	0	0	1
Mississippi	MS	28	2012	0	0	1	0	1	0	0
Wisconsin	WI	55	2014	0	0	1	0	0	1	0
Wyoming	WY	56	2014	0	0	1	0	0	1	0

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A New Automated Redistricting Simulator Using Markov Chain Monte Carlo*

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Abstract

Legislative redistricting is a critical element of representative democracy. A number of substantive scholars have used simulation methods to sample redistricting plans under various constraints in order to assess their impacts on partisanship and other aspects of representation. However, surprisingly few simulation methods exist in the literature, and the standard algorithm has no theoretical justification. To fill this gap, we propose a new automated redistricting simulator using Markov chain Monte Carlo. We formulate redistricting as a graph-cut problem and adopt the Swendsen-Wang algorithm for sampling contiguous districts. We then extend this basic algorithm to incorporate various constraints including equal population and geographical compactness. Finally, we apply simulated and parallel tempering to improve the mixing of the resulting Markov chain. The proposed algorithms, therefore, are designed to approximate the population of redistricting plans under various constraints. Through a small-scale validation study, we show that the proposed algorithm outperforms the existing standard algorithm. We also apply the proposed methodology to the data from New Hampshire and Mississippi. The open-source software is available for implementing the proposed methodology.

Keywords: gerrymandering, graph cuts, Metropolis-Hastings algorithm, simulated tempering, parallel tempering, Swendsen-Wang algorithm

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1 Introduction

Legislative redistricting is a critical element of representative democracy. Previous studies have found that redistricting influences turnout and representation (e.g., Abramowitz, 1983; Gelman and King, 1994; Ansolabehere *et al.*, 2000; McCarty *et al.*, 2009; Barreto *et al.*, 2004). From a public policy perspective, redistricting is potentially subject to partisan gerrymandering. After the controversial 2003 redistricting in Texas, for example, Republicans won 21 congressional seats in the 2004 election (Democrats won 11) whereas they had only 15 seats in 2002 (Democrats won 17). To address this concern, numerous remedies, including geographical compactness and partisan symmetry requirements, have been proposed (see Grofman and King, 2007; Fryer and Holden, 2011, and references therein).

The development of automated redistricting algorithms, which is the goal of this paper, began in the 1960s. Vickrey (1961) argued that such an "automatic and impersonal procedure" can eliminate gerrymandering (p. 110). After *Baker v. Carr* (1962) where the Supreme Court ruled that federal courts may review the constitutionality of state legislative apportionment, citizens, policy makers, and scholars became interested in redistricting. Weaver and Hess (1963) and Nagel (1965) were among the earliest attempts to develop automated redistricting algorithms (see also Hess *et al.*, 1965). Since then, a large number of methods have been developed to find an *optimal* redistricting plan for a given set of criteria (e.g., Garfinkel and Nemhauser, 1970; Browdy, 1990; Bozkaya *et al.*, 2003; Chou and Li, 2006; Fryer and Holden, 2011). These optimization methods serve as useful tools when drawing district boundaries (see Altman *et al.*, 2005, for an overview).

However, the main interest of substantive scholars has been to characterize the *distribution* of possible redistricting plans under various criteria for detecting instances of gerrymandering and understanding the causes and consequences of redistricting (e.g., Engstrom and Wildgen, 1977; O'Loughlin, 1982; Cirincione *et al.*, 2000; McCarty *et al.*, 2009; Chen and Rodden, 2013). In 42 of the 50 U.S. states, for example, state politicians control the redistricting process and approve redistricting plans through standard statutory means. Therefore, an important institutional and policy policy question is how to effectively constrain these politicians through means such as compactness requirements (e.g., Niemi *et al.*, 1990), in order to prevent the manipulation of redistricting for partisan ends. Simulation methods allow substantive scholars to answer these questions by approximating distributions of possible electoral outcomes under various institutional constraints.

Yet, surprisingly few simulation algorithms exist in the methodological literature. In fact, most, if not all, of these existing studies use essentially the same Monte Carlo simulation algorithm where a geographical unit is randomly selected as a "seed" for each district and then neighboring units are added to contiguously grow this district until it reaches the pre-specified population threshold (e.g., Cirincione *et al.*, 2000; Chen and Rodden, 2013). Unfortunately, no theoretical justification is given for these existing simulation algorithms, and some of them are best described as ad-hoc. A commonly used algorithm of this type is proposed by Cirincione *et al.* (2000) and implemented by Altman and McDonald (2011) in their open-source software. We hope to improve this state of the methodological literature.

To fulfill this methodological gap, in Section 2, we propose a new automated redistricting simulator using Markov chain Monte Carlo (MCMC). We formulate the task of drawing districting boundaries as the problem of graph-cuts, i.e., partitioning an adjacency graph into several connected subgraphs. We then adopt a version of the Swendsen-Wang algorithm to sample contiguous districts (Swendsen and Wang, 1987; Barbu and Zhu, 2005). We further extend this basic algorithm to incorporate various constraints commonly imposed on redistricting plans, including equal population requirements and geographical compactness. Finally, we apply simulated and parallel tempering to improve the mixing of the resulting Markov chain (Marinari and Parisi, 1992; Geyer and Thompson, 1995). Therefore, unlike the existing algorithms, the proposed algorithms are designed to yield a representative sample of redistricting plans under various constraints. The open-source software, an R package `redist`, is available for implementing the proposed methodology (Fifield *et al.*, 2015).

In Section 3, we conduct a small-scale validation study where all possible redistricting plans under various constraints can be enumerated in a reasonable amount of time. We show that the proposed algorithms successfully approximate this true population distribution while the standard algorithm fails even in this small-scale redistricting problem. We also conduct an empirical study in realistic settings using redistricting and U.S. Census data from New Hampshire and Mississippi. In this case, the computation of the true population distribution is not feasible and so we evaluate the empirical performance of the proposed algorithms by examining several standard diagnostics of MCMC algorithms. Lastly, Section 4 gives concluding remarks.

2 The Proposed Methodology

In this section, we describe the proposed methodology. We begin by formulating redistricting as a graph-cut problem. We then propose a Markov chain Monte Carlo (MCMC) algorithm to uniformly sample redistricting plans with n contiguous districts. Next, we show how to incorporate various constraints such as equal population and geographical compactness. Finally, we improve the mixing of the MCMC algorithm by applying simulated and parallel tempering. A brief comparison with the existing algorithms is also given.

2.1 Redistricting as a Graph-cut Problem

Consider a typical redistricting problem where a state consisting of m geographical units (e.g., census blocks or voting precincts) must be divided into n contiguous districts. We formulate this redistricting problem as that of graph-cut where an adjacency graph is partitioned into a set of connected subgraphs (Altman, 1997; Mehrotra *et al.*, 1998). Formally, let $G = \{V, E\}$ represent an adjacency graph where $V = \{\{1\}, \{2\}, \ldots, \{m\}\}$ is the set of nodes (i.e., geographical units of redistricting) to be partitioned and E is the set of edges connecting neighboring nodes. This means that if two units, $\{i\}$ and $\{j\}$, are contiguous, there is an edge between their corresponding nodes on the graph, $(i, j) \in E$.

Given this setup, redistricting can be seen equivalent to the problem of partitioning an adjacency graph G. Formally, we partition the set of nodes V into n blocks, $\mathbf{v} = \{V_1, V_2, \ldots, V_n\}$ where each block is a non-empty subset of V, and every node in V belongs to one and only one block, i.e., $V_k \cap V_\ell = \emptyset$ and $\bigcup_{k=1}^{n} V_k = V$. Such a partition $\mathbf{v}$ generates an adjacency subgraph $G_\mathbf{v} = (V, E_\mathbf{v})$ where $E_\mathbf{v}$ is a subset of E. Specifically, an edge (i, j) belongs to $E_\mathbf{v}$ if and only if $(i, j) \in E$ and nodes $\{i\}$ and $\{j\}$ are contained in the same block of the partition, i.e., $\{i\}, \{j\} \in V_k$. Because $E_\mathbf{v}$ is obtained by removing some edges from E or "cutting" them, redistricting represents a graph cut problem. Finally, since each resulting district must be contiguous, a valid partition consists of only connected blocks where for any two nodes $\{i\}$ and $\{j\}$ in a connected block $V_k \in \mathbf{v}$, there exists a path of edges within V_k that joins these two nodes. Formally, there exists a set of nodes $\{\{i\} = \{i_0\}, \{i_1\}, \{i_2\}, \ldots, \{i_{m'-1}\}, \{i_{m'}\} = \{j\}\} \subset V_k$ such that, for all $\ell \in \{1, \ldots, m'\}$, $(i_{\ell-1}, i_\ell) \in E_\mathbf{v}$.

Figure 1 presents two illustrative examples, one of which is used in our validation study in Section 3.1. These examples are taken from actual Florida precinct data in

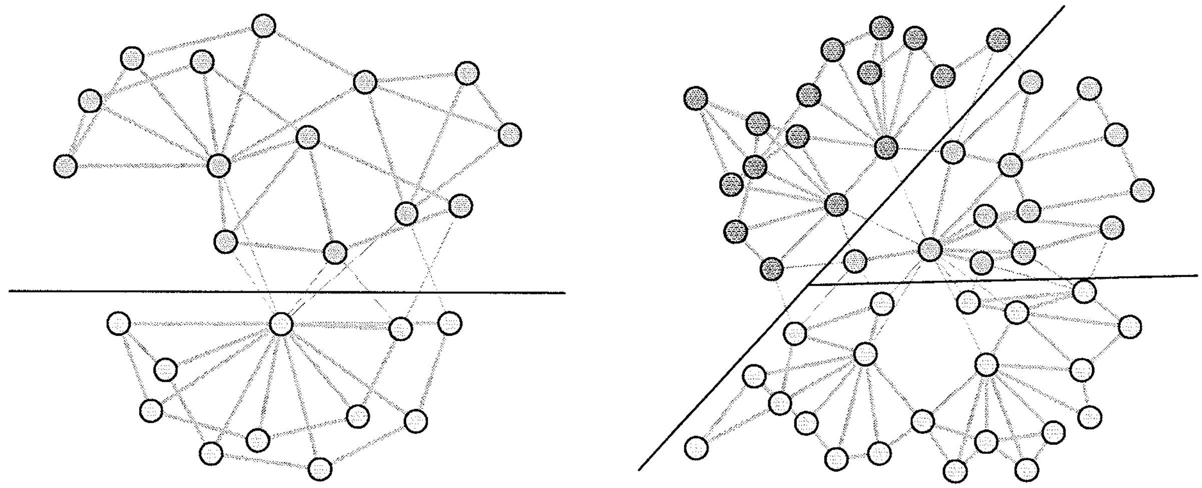

Figure 1: Redistricting as a Graph-cut Problem. A state is represented by an adjacency graph where nodes are geographical units and edges between two nodes imply their contiguity. Under this setting, redistricting is equivalent to removing or cutting some edges (light grey) to form connected subgraphs, which correspond to districts. Different districts are represented by different colors. Two illustrative examples, one of which is used in our validation study in Section 3.1, are given here.

an attempt to create realistic, albeit small, examples. A state is represented by an adjacency graph where nodes are geographical units and edges between two nodes imply their contiguity. The figure demonstrates that redistricting a state into n districts is equivalent to removing some edges of an adjacency graph (light grey) and forming n connected subgraphs.

2.2 The Basic Algorithm for Sampling Contiguous Districts

We propose a new automated simulator to uniformly sample valid redistricting plans with n contiguous districts. The contiguity of valid partitions dramatically increases the difficulty of developing such an algorithm. Intuitive methods for constructing partitions at random – e.g., randomly assigning precincts to districts – have a minuscule chance of yielding contiguous districts, and enumerating all partitions with contiguous districts is too large of a problem to be tractable in realistic redistricting settings. For more discussion, see Section 3.1.

Our MCMC algorithm is designed to obtain a dependent but representative sample from the uniform distribution of valid redistricting plans. In particular, we modify and extend Algorithm 1 of Barbu and Zhu (2005), which combines the Swendsen-Wang algorithm (Swendsen and Wang, 1987) with a Metropolis-Hastings step (Metropolis *et al.*, 1953; Hastings, 1970). This algorithm begins with a valid partition $\mathbf{v}_0$ (e.g., an actual redistricting plan adopted by the state) and transitions from a valid partition $\mathbf{v}_{t-1}$ to another partition $\mathbf{v}_t$ at each iteration t. Here, we describe the basic algorithm for sampling contiguous districts. Later in the paper, we extend this basic algorithm in a couple of important ways so that common constraints imposed on redistricting can be incorporated and the algorithm can be applied to states with a larger number of districts.

Figure 2 illustrates our algorithm using the 50 precinct example with 3 districts given in the right panel of Figure 1. Our algorithm begins by randomly "turning on" edges in $E_{\mathbf{v}_{t-1}}$; each edge is turned on with probability q. In the left upper plot of Figure 2, the edges that are turned on are indicated with darker grey. Next, we identify components that are connected through these "turned-on" edges and are on the boundaries of districts in $\mathbf{v}_{t-1}$. Each such connected component is indicated by a dotted polygon in the right upper plot. Third, among these, a subset of non-adjacent connected components are randomly selected as shown in the left lower plot (two in this case). These connected components are reassigned to adjacent districts to create a candidate partition. Finally, the acceptance probability is computed based on two kinds of edges from each of selected connected components, which are highlighted in the left lower plot: (1) "turned-on" edges, and (2) "turned-off" edges that are connected to adjacent districts. We accept or reject the candidate partition based on this probability.

Our algorithm guarantees that its stationary distribution is equal to the uniform

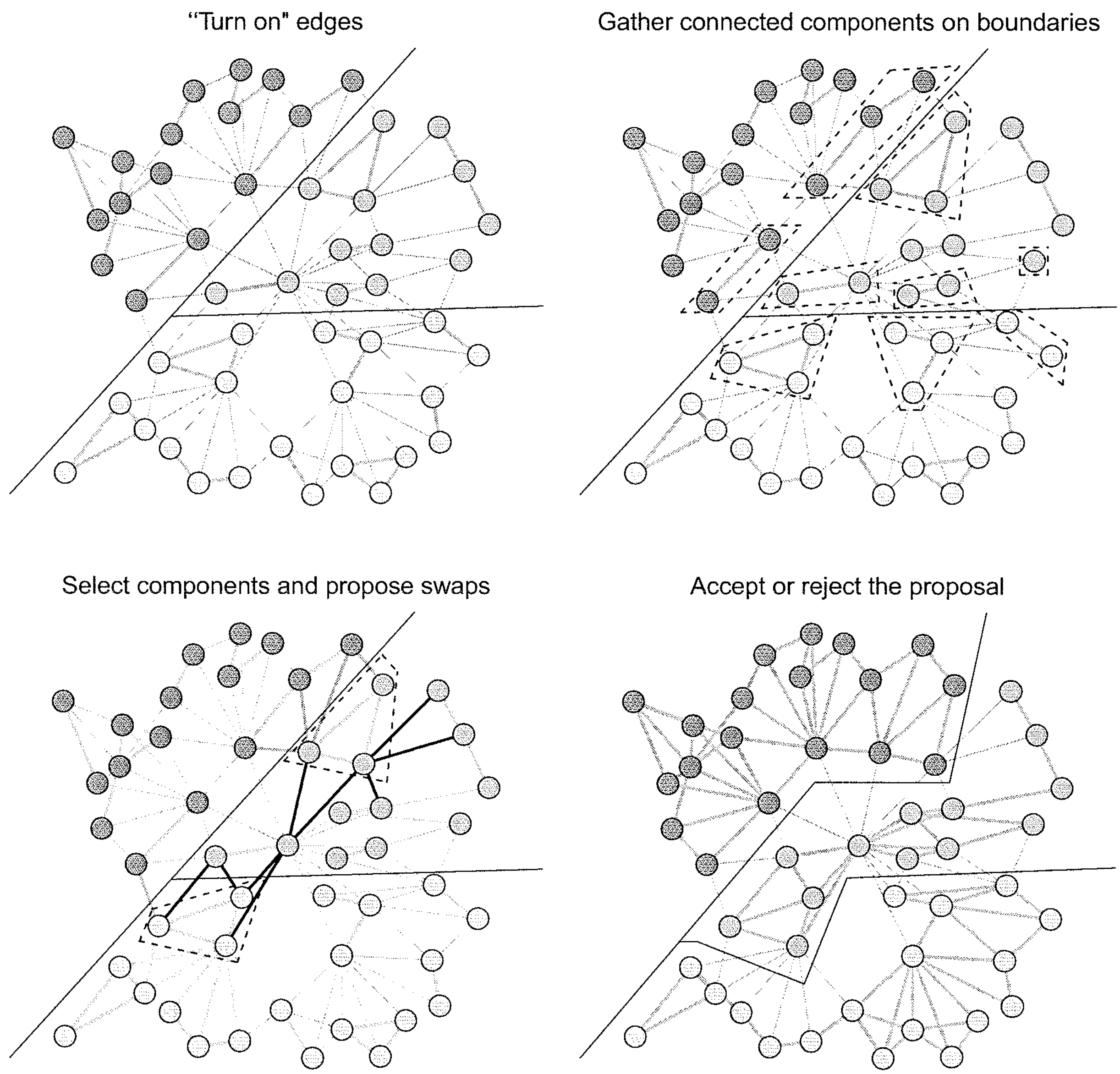

Figure 2: The Basic Algorithm for Sampling Contiguous Districts. The plots illustrate the proposed algorithm (Algorithm 1) using the 50 precinct data given in the right panel of Figure 1. First, in the left upper plot, each edge other than those which are cut in Figure 1 is "turned on" (dark grey) independently with certain probability. Second, in the right upper plot, connected components on the boundaries are identified (dashed polygons). Third, in the left lower plot, a certain number of non-adjacent connected components on boundaries are randomly selected (dashed polygons) and the acceptance ratio is calculated by counting certain edges (colored edges). Finally, in the right lower plot, the proposed swap is accepted using the Metropolis-Hastings ratio.

distribution of all valid partitions, thereby yielding a uniformly sampled sequence of redistricting plans with contiguous districts. We now formally describe this algorithm.

ALGORITHM 1 (SAMPLING CONTIGUOUS REDISTRICTING PLANS) *We initialize the algorithm by obtaining a valid partition* $\mathbf{v}_0 = \{V_{10}, V_{20}, \ldots, V_{n0}\}$ *and then repeat the following steps at each iteration* t,

Step 1 ("Turn on" edges): *From the partition* $\mathbf{v}_{t-1} = \{V_{1,t-1}, V_{2,t-1}, \ldots, V_{n,t-1}\}$, *obtain the adjacency graph* $G_{\mathbf{v}_{t-1}} = (V, E_{\mathbf{v}_{t-1}})$. *Obtain the edge set* $E^*_{\mathbf{v}_{t-1}} \subset E_{\mathbf{v}_{t-1}}$ *where each edge* $e \in E_{\mathbf{v}_{t-1}}$ *is independently added to* $E^*_{\mathbf{v}_{t-1}}$ *with probability* q.

Step 2 (Gather connected components on boundaries): *Find all components that are connected within* E^*_{t-1} *and adjacent to another block in the partition* $\mathbf{v}_{t-1}$. *Let* C *denote this set of connected components where for all* $C_\ell \in C$, *there exists* $k \in \{1, 2, \ldots, n\}$ *such that* $C_\ell \cap V_{k,t-1} = \emptyset$ *and* $(i, j) \in E$ *for some* $\{i\} \in C_\ell$ *and* $\{j\} \in V_{k,t-1}$.

Step 3 (Select non-adjacent connected components): *Randomly select a set of* r *non-adjacent connected components* C^* *from* C *such that* $\mathbf{v}_{t-1} \setminus C^*$ *is a valid partition where each block of nodes* $V_{\ell,t-1} \setminus C^*$ *is connected in* $G_{\mathbf{v}_{t-1}}$. *The sampling is done such that each eligible subset of* C *is selected with equal probability.*

Step 4 (Propose swaps): *Initialize a candidate partition* $\mathbf{v}^*_t = (V^*_{1t}, V^*_{2t}, \ldots, V^*_{nt})$ *by setting* $V^*_{kt} = V_{k,t-1}$. *For each component* $C^*_\ell \in C^*$ *with* $\ell \in \{1, \ldots, r\}$, *find the block* $V_{k,t-1} \in \mathbf{v}_{t-1}$ *that contains* C^*_ℓ, *and let* $A(C^*_\ell, \mathbf{v}_{t-1})$ *denote the set of blocks in* $\mathbf{v}_{t-1}$ *that are adjacent to* C^*_ℓ, *not including the block that contains* C^*_ℓ. *Propose to assign* C^*_ℓ *from block* $V_{k,t-1}$ *to an adjacent block* $V_{j',t-1}$ *with probability* $1/|A(C^*_\ell, \mathbf{v}_{t-1})|$. *If* C^*_ℓ *is assigned to block* $V_{k',t-1}$, *set* $V^*_{k't} = V_{k',t-1} \cup C^*_\ell$ *and* $V^*_{kt} = V_{k,t-1} \setminus C^*_\ell$. *If* $V^*_{kt} = \emptyset$, *go back to Step 3. Observe that, after each proposed swap,* $\mathbf{v}^*_t$ *remains a connected set partition.*

Step 5 (Accept or reject the proposal): *Set*

$$\mathbf{v}_t = \begin{cases} \mathbf{v}^*_t, & \textit{with probability} \quad \alpha(\mathbf{v}_{t-1} \to \mathbf{v}^*_t), \\ \mathbf{v}_{t-1}, & \textit{with probability} \quad 1 - \alpha(\mathbf{v}_{t-1} \to \mathbf{v}^*_t). \end{cases} \tag{1}$$

The acceptance probability is given by the Metropolis criterion

$$\alpha(\mathbf{v}_{t-1} \to \mathbf{v}^*_t) = \min\left(1,\ (1-q)^{|B(C^*, E_{\mathbf{v}^*_t})| - |B(C^*, E_{\mathbf{v}_{t-1}})|}\right) \tag{2}$$

where $B(C^*, E_{\mathbf{v}}) = \{(i,j) \in E_{\mathbf{v}} : \exists C^*_\ell \in C^*, C^*_\ell \subset V_k \in \mathbf{v} \; s.t. \{i\} \in C^*_\ell, \{j\} \in V_k \setminus C^*_\ell\}$ *denotes the set of edges in* $E_{\mathbf{v}}$ *that need to be cut to form connected components* C^*.

In the Appendix, we prove the following theorem, which states that if the Markov chain produced by the proposed algorithm is ergodic, the stationary distribution of the chain is uniform on the population of all valid partitions $\Omega(G, n)$ (Tierney, 1994).

THEOREM 1 *If every valid partition can be obtained through a sequence of moves given by Algorithm 1, then the stationary distribution of the resulting Markov chain is uniform on all valid partitions.*

The acceptance ratio given in equation (2) is based on the Metropolis-Hastings detailed balance condition (Metropolis *et al.*, 1953; Hastings, 1970),

$$\alpha(\mathbf{v}_{t-1} \to \mathbf{v}^*_t) \quad = \quad \min\left(1, \; \frac{\pi(\mathbf{v}^*_t \to \mathbf{v}_{t-1})}{\pi(\mathbf{v}_{t-1} \to \mathbf{v}^*_t)}\right), \tag{3}$$

where $\pi(\mathbf{v} \to \mathbf{v}^*)$ denote the probability that, starting from partition $\mathbf{v}$, an iteration of Algorithm 1 described above obtains a candidate partition $\mathbf{v}^*$ through Steps 1–4. Computing numerators and denominators of this ratio separately is combinatorially expensive. However, following Barbu and Zhu (2005), we show in the Appendix that substantial cancellation occurs, yielding a simple expression given in equation (2). Indeed, we only need to find all edges within $E_{\mathbf{v}_{t-1}}$ and $E_{\mathbf{v}^*_t}$ that join a node in a connected component of $C^*_\ell \in C^*$ to a node not contained in the block. Since components in C^* are not adjacent, this will ensure that the node not contained in C^*_ℓ will not be contained in a block in C^*.

Several additional remarks are in order. First, when implementing this algorithm, Step 2 requires the three operations: (1) identify all nodes that form a boundary of multiple partitions by comparing $G_{\mathbf{v}_{t-1}}$ with the original adjacency graph G, (2) identify all connected components that include at least one such node via the breadthfirst or depth-first search algorithm, and (3) identify the partition to which each connected component belongs.

Second, in Step 3, we typically choose a positive integer r by randomly sampling it from a distribution with $\Pr(r = 1) > 0$ at each iteration. If $r = 1$, then the ergodicity of the Markov chain is guaranteed but the algorithm moves slowly in the sample space. When $r > 1$, the algorithm can mix faster by proposing multiple swaps. However, depending on the adjacency graph G, the algorithm may fail to reach some valid partitions. Thus, we allow r to take a value greater than 1 while keeping the probability of $r = 1$ positive (e.g., a truncated poisson distribution).

Third, in the original algorithm of Barbu and Zhu (2005), r is set to 1 and instead the authors use a small value of q to create larger connected components. This alternative strategy to improving mixing of the algorithm, though sensible in other settings, is not applicable to the current case. The reason is that larger connected components typically include more units from the interior of each block. This in turn dramatically lowers the acceptance probability.

Finally, while this basic algorithm yields a sample of redistricting plans with contiguous districts, it does not incorporate common constraints imposed on redistricting process, including equal population and geographical compactness. In addition, our experience shows that the algorithm does not scale for states with a medium or larger number of districts. Therefore, we now describe two important modifications to the basic algorithm.

2.3 Constraints and Reweighting

In a typical redistricting process, several additional constraints are imposed. Two most commonly applied constraints are equal population and geographical compactness. We first consider the equal population constraint. Suppose that we use p_i to denote the population size for node $\{i\}$ where the population parity for the state is given by $\bar{p} \equiv \sum_{i=1}^{m} p_i/n$. Then, the population equality constraint can be written as,

$$P_{\mathbf{v}} = \max_{1 \leq k \leq n} \left| \frac{\sum_{i \in V_k} p_i}{\bar{p}} - 1 \right| \leq \delta \tag{4}$$

where δ determines the degree to which one wishes to impose the constraint. For example, $\delta = 0.03$ implies that the population of all districts must be within 3% of the population parity.

Next, we consider the geographical compactness. No consensus exists about the exact meaning of compactness and several alternative definitions have been proposed in the literature (see Niemi *et al.*, 1990). Here, we adopt the measure recently proposed by Fryer and Holden (2011). Let w_i be the population density of node $\{i\}$ and d_{ij} represent the distance between the centroids of nodes $\{i\}$ and $\{j\}$. The measure, which is called the relative proximity index, is based on the sum of squared distances among voters in each district relative to its minimum value. Then, the compactness constraint can be written as,

$$R_{\mathbf{v}} = \frac{\sum_{k=1}^{n} \sum_{i,j \in V_k, i<j} w_i w_j d_{ij}^2}{\min_{\mathbf{v}' \in \Omega(G,n)} \sum_{k=1}^{n} \sum_{i,j \in V_k', i<j} w_i w_j d_{ij}^2} \leq \epsilon \tag{5}$$

where $V_k' \in \mathbf{v}'$, ϵ determines the strength of this constraint, and $\Omega(G,n)$ is the set of all redistricting plans with n contiguous districts. Fryer and Holden (2011) develops an approximate algorithm to efficiently compute the minimum of the sum of squared distances, i.e., the denominator of equation (5). The authors also show that this measure is invariant to geographical size, population density, and the number of districts of a state, thereby allowing researchers to compare the index across different states and time periods.

How can we uniformly sample redistricting plans under these additional constraints? One possibility is to discard any candidate partition that does not satisfy the desired constraints. In Algorithm 1, after Step 4, one could check whether the candidate partition $\mathbf{v}_t^*$ satisfies the constraints and if not go back to Step 3. However, such a strategy often dramatically slows down the algorithm and worsens mixing. Alternatively, researchers could run Algorithm 1 without any modification and then simply discard any sampled redistricting plans that do not meet the constraints. The problem of this approach is that many sampled plans may be discarded when strong constraints are imposed.

To overcome this difficulty, we propose to modify Algorithm 1 in the following manner. We first oversample the redistricting plans that are likely to meet the constraints. This means that fewer sampled plans are discarded due to the failure to satisfy the constraints. We then reweight the remaining valid redistricting plans such that they together approximate the uniform sampling from the population of all valid redistricting plans under the constraints. To do this, we consider the Gibbs distribution from statistical physics,

$$P(\mathbf{v}) \quad = \quad \frac{1}{z(\beta)} \exp\left(-\beta \sum_{V_k \in \mathbf{v}} \psi(V_k)\right) \tag{6}$$

where $\beta \geq 0$ is the inverse temperature and $z(\beta)$ is the normalizing constant. The function $\psi(\cdot)$ is chosen so that it reflects the constraint of interest. For example, we use $\psi(V_k) = |\sum_{i \in V_k} p_i/\bar{p} - 1|$ and $\psi(V_k) = \sum_{i,j \in V_k} w_i w_j d_{ij}^2$ for the equal population and geographical compactness constraints, respectively.

Algorithm 1 can be modified easily to sample from the non-uniform stationary distribution given in equation (6). In particular, we only need to change the acceptance probability in equation (2) of Step 5 to,

$$\alpha(\mathbf{v}_{t-1} \to \mathbf{v}_t^*) \quad = \quad \min\left(1, \frac{g_\beta(\mathbf{v}_t^*)}{g_\beta(\mathbf{v}_{t-1})} \cdot (1-q)^{|B(C^*, \mathbf{v}_t^*)| - |B(C^*, \mathbf{v}_{t-1})|}\right) \tag{7}$$

where $g_\beta(\mathbf{v}) \equiv \exp\left(-\beta \sum_{V_k \in \mathbf{v}} \psi(V_k)\right)$. Lastly, we reweight the sampled plans by $1/g_\beta(\mathbf{v})$ to approximate the uniform sampling from the population of all possible valid redistricting plans. If we resample the sampled plans with replacement using this importance weight, then the procedure is equivalent to the sampling/importance resampling (SIR) algorithm (Rubin, 1987).

2.4 Simulated and Parallel Tempering

One major drawback of the reweighting approach is that when each plan is weighted according to equation (6) the algorithm may have a harder time moving through the sample space. We use simulated and parallel tempering to improve the mixing of Algorithm 1 in such situations (Marinari and Parisi, 1992; Geyer and Thompson, 1995). We begin by describing how to apply simulated tempering in this context.

Recall that we want to draw from the distribution given in equation (6). We initialize a sequence of *inverse temperatures* $\{\beta^{(\ell)}\}_{\ell=0}^{r-1}$ where $\beta^{(0)}$ corresponds to the *cold temperature*, which is the target parameter value for inference, and $\beta^{(r-1)} = 0$ represents the *hot temperature* with $\beta^{(0)} > \beta^{(1)} > \cdots > \beta^{(r-1)} = 0$. After many iterations, we keep the MCMC draws obtained when $\beta = \beta^{(0)}$ and discard the rest. By sampling under warm temperatures, simulated tempering allows for greater exploration of the target distribution. We then reweight the draws by the importance weight $1/g_{\beta^{(0)}}(\mathbf{v})$.

Specifically, we perform simulated tempering in two steps. First, we run an iteration of Algorithm 1 using the modified acceptance probability with $\beta = \beta^{(l)}$. We then make another Metropolis-Hastings decision on whether to change to a different value of β. The details of the algorithm are given below.

ALGORITHM 2 (SIMULATED TEMPERING) *Given the initial valid partition* $\mathbf{v}_0$ *and the initial temperature value* $\beta_0 = \beta^{(\kappa_0)}$ *with* $\kappa_0 = r - 1$, *the simulated tempering algorithm repeats the following steps at each iteration* t,

Step 1 (Run the basic algorithm with the modified acceptance probability): *Using the current partition* $\mathbf{v}_{t-1}$ *and the current temperature* $\beta_{t-1} = \beta^{(\kappa_{t-1})}$, *obtain a valid partition* $\mathbf{v}_t$ *by running one iteration of Algorithm 1 with the acceptance probability given in equation* (7).

Step 2 (Choose a candidate temperature): *We set* $\kappa_t^* = \kappa_{t-1} - 1$ *with probability* $u(\kappa_{t-1}, \kappa_{t-1} - 1)$ *and set* $\kappa_t^* = \kappa_{t-1} + 1$ *with probability* $u(\kappa_{t-1}, \kappa_{t-1} +$

1) $= 1 - u(\kappa_{t-1}, \kappa_{t-1} - 1)$ where $u(\kappa_{t-1}, \kappa_{t-1} - 1) = u(\kappa_{t-1}, \kappa_{t-1} + 1) = 1/2$ when $1 \leq \kappa_{t-1} \leq r - 2$, and $u(r-1, r-2) = u(0,1) = 1, u(r-1, r) = u(0, -1) = 0$.

Step 3 (Accept or reject the candidate temperature): *Set*

$$\kappa_t = \begin{cases} \kappa_t^*, & \text{with probability } \gamma(\kappa_{t-1} \to \kappa_t^*), \\ \kappa_{t-1}, & \text{with probability } 1 - \gamma(\kappa_{t-1} \to \kappa_t^*) \end{cases} \tag{8}$$

where

$$\gamma(\kappa_{t-1} \to \kappa_t^*) = \min\left(1, \frac{g_{\beta^{(\kappa_t^*)}}(\mathbf{v}_t)\ u(\kappa_t^*, \kappa_{t-1})\ w_{\kappa_t^*}}{g_{\beta^{(\kappa_{t-1})}}(\mathbf{v}_t)\ u(\kappa_{t-1}, \kappa_t^*)\ w_{\kappa_{t-1}}}\right) \tag{9}$$

where w_ℓ is an optional weight given to each $l \in \{0, 1, \ldots, r-1\}$.

Much like simulated tempering, parallel tempering is also useful for improving mixing in MCMC algorithms and for sampling from multimodal distributions (Geyer, 1991). Parallel tempering differs from simulated tempering in that instead of varying the temperature within a single Markov chain, we run r copies of Algorithm 1 at r different temperatures, and after a fixed number of iterations we exchange the corresponding temperatures between two randomly selected adjacent chains using the Metropolis criterion. This algorithm has an advantage over Algorithm 2 in that we do not need to choose the prior probability of β, which typically has a significant effect on the mixing performance. However this advantage comes at the expense of increased computation as we are now running r chains instead of just one.

The nature of parallel tempering suggests that it should be implemented in a parallel architecture, which can be used to minimize computation time. Altekar *et al.* (2004) describe such an implementation using parallel computing and MPI, which we use as the basis for implementing our algorithm described below.

Algorithm 3 (Parallel Tempering) *Given r initial valid partitions $\mathbf{v}_0^{(0)}, \mathbf{v}_0^{(1)}, \ldots, \mathbf{v}_0^{(r-1)}$ and a sequence of r decreasing temperatures $\beta^{(0)} > \beta^{(1)} > \cdots > \beta^{(r-1)} = 0$ with $\beta^{(0)}$ the target temperature for inference, and swapping interval T, the parallel tempering algorithm repeats the following steps every iteration $t \in \{0, T, 2T, 3T, \ldots\}$,*

Step 1 (Run the basic algorithm with the modified acceptance probability): *For each chain $i \in \{0, 1, \ldots, r-1\}$, using the current partition $\mathbf{v}_t^{(i)}$ and* *the corresponding temperature* $\beta^{(i)}$, *obtain a valid partition* $\mathbf{v}_{t+T}^{(i)}$ *by running* T *iterations of Algorithm 1 with the acceptance probability given in equation* (7). *This step is executed concurrently for each chain*

Step 2 (Propose a temperature exchange between two chains): *Randomly select two adjacent chains* j *and* k *and exchange information about the temperatures* $\beta^{(j)}, \beta^{(k)}$ *and the unnormalized likelihoods of the current partitions* $g_{\beta^{(j)}}\left(\mathbf{v}_{t+T}^{(j)}\right), g_{\beta^{(k)}}\left(\mathbf{v}_{t+T}^{(k)}\right)$ *using MPI*

Step 3 (Accept or reject the temperature exchange): *Exchange temperatures (i.e* $\beta^{(j)} \leftrightarrows \beta^{(k)}$*) with probability* $\gamma\left(\beta^{(j)} \leftrightarrows \beta^{(k)}\right)$ *where*

$$\gamma\left(\beta^{(j)} \leftrightarrows \beta^{(k)}\right) = \min\left(1, \frac{g_{\beta^{(j)}}\left(\mathbf{v}_{t+T}^{(k)}\right) g_{\beta^{(k)}}\left(\mathbf{v}_{t+T}^{(j)}\right)}{g_{\beta^{(j)}}\left(\mathbf{v}_{t+T}^{(j)}\right) g_{\beta^{(k)}}\left(\mathbf{v}_{t+T}^{(k)}\right)}\right) \tag{10}$$

All previously generated samples are assumed to have been generated at the current temperature of the chain

We note that the mixing performance of Algorithm 3 is affected by the choice of the temperature sequence $\beta^{(i)}$. While no sequence has been shown to be optimal in the literature, sequences with power-law spacing have been shown heuristically to produce reasonable results. For this reason, we used the sequence $\beta^{(i)} = \left(\beta^{(0)}\right)^{\frac{i}{r-1}}, i \in \{0, 1, \ldots, r-1\}$ for our implementation.

2.5 Comparison with the Existing Algorithms

A number of substantive researchers used Monte Carlo simulation algorithms to sample possible redistricting plans under various criteria in order to detect the instances of gerrymandering and understand the causes and consequences of redistricting (e.g., Engstrom and Wildgen, 1977; O'Loughlin, 1982; Cirincione *et al.*, 2000; McCarty *et al.*, 2009; Chen and Rodden, 2013). Most of these studies use a similar Monte Carlo simulation algorithm where a geographical unit is randomly selected as a "seed" for each district and then neighboring units are added to contiguously grow this district until it reaches the pre-specified population threshold. A representative of such algorithms, proposed by Cirincione *et al.* (2000) and implemented by Altman and McDonald (2011) in their open-source `BARD` package, is given here.

Algorithm 4 (The Standard Redistricting Simulator (Cirincione *et al.*, 2000)) *For each district, we repeat the following steps.*

Step 1: *From the set of unassigned units, randomly select the seed unit of the district.*

Step 2: *Identify all unassigned units adjacent to the district.*

Step 3: *Randomly select one of the adjacent units and add it to the district.*

Step 4: *Repeat Steps 2 and 3 until the district reaches the predetermined population threshold.*

Additional criteria can be incorporated into this algorithm by modifying Step 3 to select certain units. For example, to improve the compactness of the resulting districts, one may choose an adjacent unassigned unit that falls entirely within the minimum bounding rectangle of the emerging district. Alternatively, an adjacent unassigned unit that is the closest to emerging district can be selected (see Chen and Rodden, 2013).

Nevertheless, the major problem of these simulation algorithms is their adhoc nature. For example, as the documentation of BARD package warns, the creation of earlier districts may make it impossible to yield contiguous districts. More importantly, the algorithms come with no theoretical result and are not even designed to uniformly sample redistricting plans even though researchers have a tendency to assume that they are. In contrast, the proposed algorithms described in Sections 2.2–2.4 are built upon the well-known theories and strategies developed in the literature on the Markov chain Monte Carlo methods. The disadvantage of our algorithms, however, is that they yield a dependent sample and hence their performance will hinge upon the degree of mixing. Thus, we now turn to the assessment of the empirical performance of the proposed algorithms.

3 Empirical Performance of the Proposed Algorithms

In this section, we assess the performance of the proposed algorithms in two ways. First, we conduct a small-scale validation study where, due to its size, all possible redistricting maps can be enumerated in a reasonable amount of time. We show that our algorithms can approximate the target distribution well when the standard algorithm commonly used in the literature fails. Second, we use the actual redistricting data to examine the convergence behavior of the proposed algorithms in more realistic settings using the redistricting data from New Hampshire (two districts) and Mississippi (four districts). For these data, the computation of the true population distribution is not feasible. Instead, we evaluate the empirical performance of the proposed algorithms by examining the standard diagnostics of MCMC algorithms.

To conduct these analyses, we integrate precinct-level data from two sources. We utilize precinct-level shape files and electoral returns data from the Harvard Election Data Archive to determine precinct adjacency and voting behavior. We supplement this data with basic demographic information from the U.S. Census Bureau P.L. 94–171 summary files, which are compiled by the Census Bureau and disseminated to the 50 states in order to obtain population parity in decennial redistricting.

3.1 A Small-scale Validation Study

We conduct a validation study where we analyze the convergence of our algorithm to the target distribution on the 25 precinct set, which is shown as an adjacency graph in Figure 1. Due to the small size of these sets, all possible redistricting plans can be enumerated in a reasonable amount of time. We begin by considering the problem of partitioning each of these graphs into two districts. We apply the proposed algorithm

(Algorithm 1) with the starting map obtained randomly by running the standard algorithm (Algorithm 4) once. In addition, we apply the standard algorithm, as implemented in the BARD package (Altman and McDonald, 2011), to compare its performance with that of our proposed algorithm. We then consider partitions of the 25 precinct set into three districts. The results of the proposed algorithm are based on a single chain of 10,000 draws while those of the standard algorithm are based on the same number of independent draws.

Before we give results, it should be noted that, even for this small-scale study, the enumeration of all valid partitions is a non-trivial problem. For partitions of 25 precincts into three districts, of the roughly $3^{25}/6 \approx 1.41 \times 10^{11}$ possible partitions, $82,623$ have three contiguous districts, and $3,617$ have district populations within 20% of parity.

A brief description of our enumeration algorithm is as follows. In the case of two districts, we choose an initial starting node and form a partition where one district is that initial node and the other district is the complement, provided the complement is connected. We then form connected components of two nodes comprised of that starting node and and nodes that are adjacent to that node. We identify all valid partitions where one district is a two-node component and the other district is the complement of the component. We continue forming connected components of incrementally increasing sizes and finding valid partitions until all possible partitions are found. In the case of three precincts, if the complement of a connected component is comprised of two additional connected components, we store that partition as valid. If the complement is a single connected component, we apply the two-district algorithm on the complement. After this enumeration, we identify which partitions have districts with populations within a certain percentage of parity.

Figure 3 presents the results of the validation study with three districts and 25

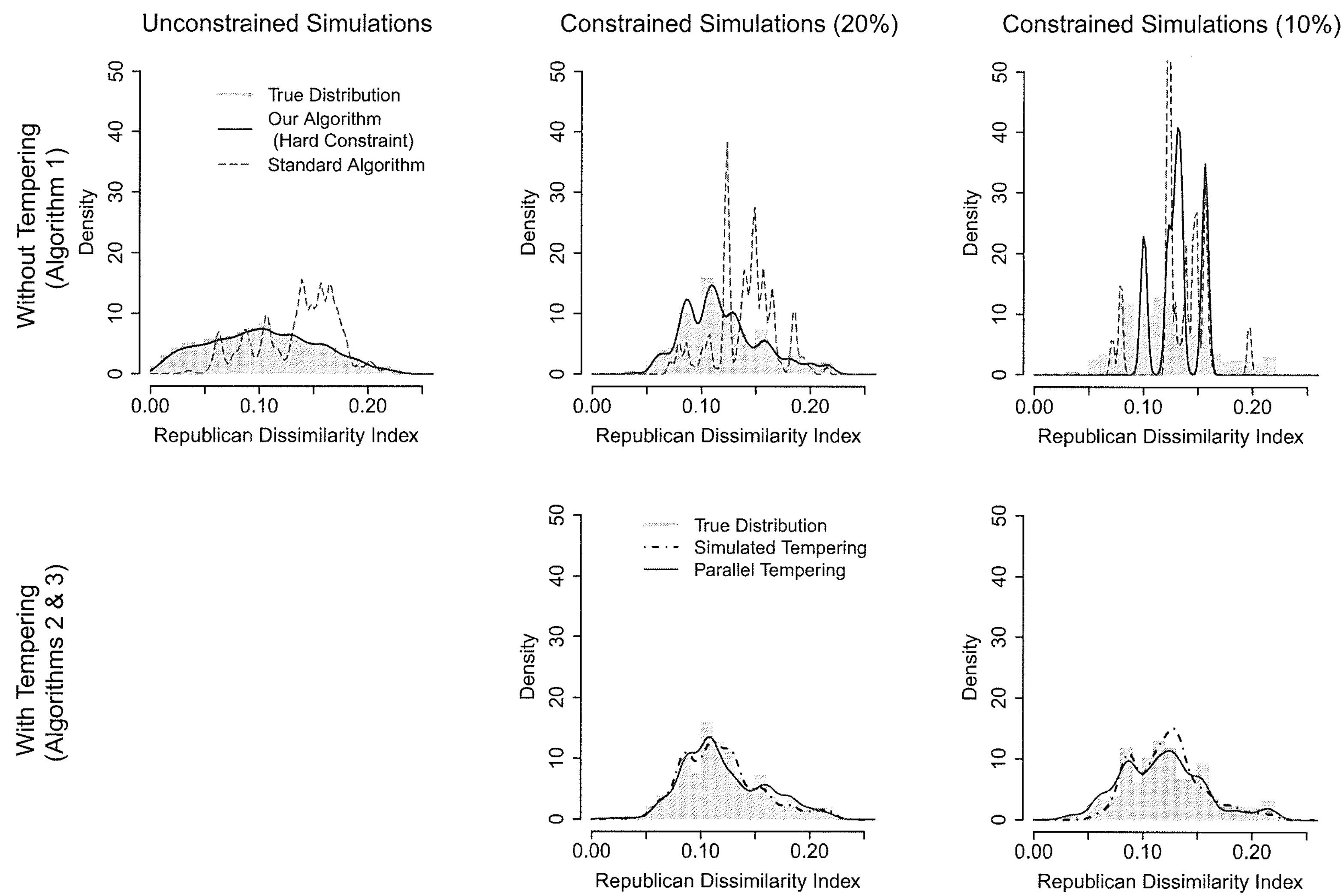

Figure 3: A Small-scale Validation Study with Three Districts. The underlying data is the 25 precinct set shown in the left plot of Figure 1. The plots in the first row show that the proposed algorithm (Algorithm 1; solid black lines) approximates well the true population distribution (grey histograms) when no (left plot) or weak (middle plot) equal population constraint is imposed. However, the algorithm exhibits poor performance when a stronger equal population constraint (right plot) is imposed. Finally, the standard algorithm (Algorithm 4; red dashed lines) fails to approximate the target distribution in all cases. In contrast, in the plots of the second row, the proposed algorithm with simulated tempering (Algorithm 2; black dot-dashed line) approximates the true population distribution well even when a stronger constraint is placed. The same exact pattern is observed for the parallel tempering algorithm (Algorithm 3; blue solid line). The results for each algorithm is based on a single chain of 10,000 draws.

precincts. We apply the proposed algorithm (Algorithm 1) with the starting map obtained randomly from the standard algorithm (Algorithm 4) (upper panel). These algorithms are also implemented with the simulated tempering (Algorithm 2; black dot-dashed lines) and parallel tempering (Algorithm 3; blue solid lines) strategies (the lower panel).

To implement these algorithms, we specify a sequence of temperatures $\{\beta^{(\ell)}\}_{\ell=0}^{r}$. For the population deviation of 20%, we chose a target temperature of $\beta^{(r)} = 5.4$, and for the population deviation of 10%, we chose a target temperature of $\beta^{(r)} = 9$. In both cases, we use $\beta^{(0)} = 0$. We choose these setups so that the rejection ratio is in the recommended 20–40% range (Geyer and Thompson, 1995) and the target temperature value is chosen based on the number of plans that meet the population constraint. In both cases, we use a subset of draws taken under the target temperature. We then resample the remaining draws using the importance weights $1/g_{\beta^{(\ell)}}(\mathbf{v})$, and finally subset down to the set of remaining draws that fall within the population target.

The left-upper plot of Figure 3 shows that when no constraint is imposed the proposed algorithm approximates the target distribution well while the sample from the standard algorithm is far from being representative of the population. In the plots of the middle and right columns, we impose the equal population constraint where only up to 20% and 10% deviation from the population parity is allowed, respectively. It is no surprise that the standard algorithm completely fails to approximate the true distribution as well in these caes (the middle and right plots in the upper panel). In contrast, the proposed algorithms with simulated and parallel tempering approximate the true population distribution well. Even when a stronger constraint, i.e., 10%, is placed, the proposed algorithms with simulated tempering (Algorithm 2) and parallel tempering (Algorithm 3) maintain a good approximation.

Finally, Figure 4 compares the runtime between the proposed basic algorithm

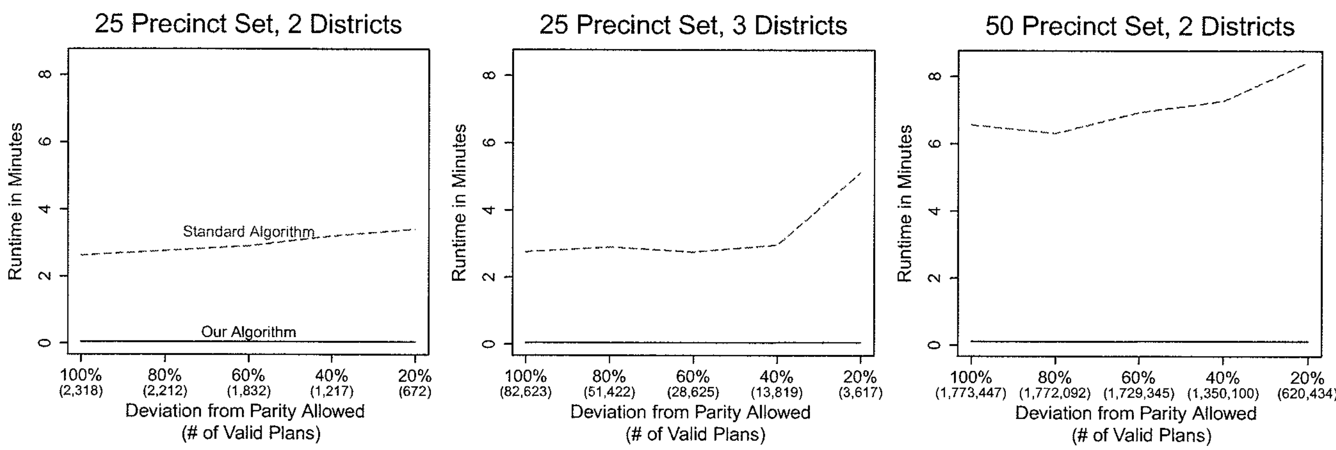

Figure 4: Runtime Comparison between the Proposed and Standard Algorithms in the Small-scale Validation Study. The runtime is compared between the proposed basic algorithm (Algorithm 1; solid black lines) and the standard algorithm (Algorithm 4; red dashed lines) under various settings. Each algorithm is run until it yields 10,000 draws. The runtime is much greater for the standard algorithm than the proposed algorithm. It also increases much more quickly for the former as the number of precincts and the strength of equal population constraint increase.

(Algorithm 1; solid black lines) and the standard algorithm (Algorithm 4; red dashed lines) under various validation study settings. In addition to the 25 precinct set, we also include the 50 precinct set, which is shown in the right plot of Figure 1. Each algorithm is run until it yields 10,000 draws using a node on a Linux server with 2.66 GHz Nehalem processors and 3GB RAM (no parallel computing is used). We find that under all settings we consider here the runtime for the proposed algorithm is at least 50 times shorter than that for the standard algorithm. This difference increases as the number of precincts and the strength of equal population constraint (x-axis) increase. In sum, in terms of computational speed, the proposed algorithm scales much better than the standard algorithm.

3.2 An Empirical Study

The scale of the validation study presented above is small so that we can enumerate all possible redistricting plans in a reasonable amount of time. This allowed us to examine how well each algorithm is able to approximate the true population distri-

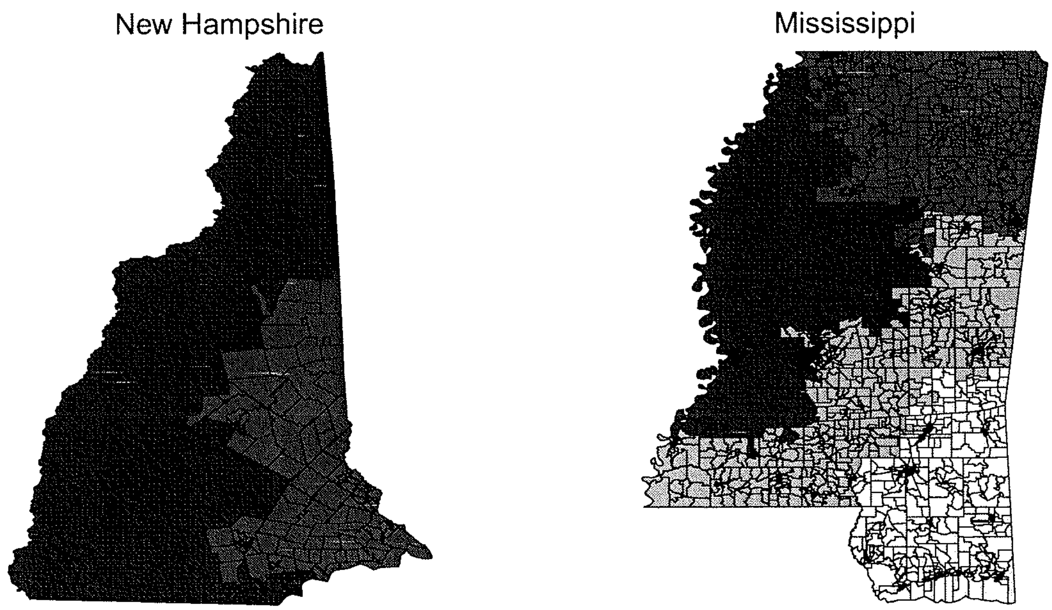

Figure 5: Precinct-level Maps of New Hampshire (327 precincts, two congressional districts) and Mississippi (1,969 precincts, four congressional districts). Colors correspond to precinct congressional district assignments in 2010. In New Hampshire, Democrats and Republicans each hold a single congressional seat. In Mississippi, Republicans hold three congressional seats while Democrats hold a single seat.

bution. However, the scale of the study is too small to be realistic. Below, we apply the proposed algorithms to the 2008 election data and conduct standard convergence diagnostics of MCMC algorithms. While we cannot compare the distribution of sampled maps with the true population distribution, this empirical study enables us to investigate the performance of the proposed methods in realistic settings.

New Hampshire. We first consider New Hampshire. The state has two congressional districts and consists of 327 precincts, and so this is one of the simplest realistic redistricting problems. The left panel of Figure 5 shows the implemented statewide redistricting plan as of 2010. Under this plan, Democrats and Republicans won a single congressional seat each. In 2008, Obama won 54% of votes in this state while his 2012 voteshare was 52%. Redistricting in New Hampshire is determined by its state legislature and plans are passed as standard statutes, which makes them subject to gubernatorial veto. We apply the proposed basic algorithm (Algorithm 1), simulated tempering algorithm (Algorithm 2), and parallel tempering algorithm (Algorithm 3). The target population consists of all redistricting plans with contiguous districts and a maximum of 1% deviation from the population parity.

A total of 10 chains are run until 500,000 draws are obtained for each of the three algorithms. Inference is based on a total of 22,970 draws, which is the lowest number of draws across the three algorithms that both satisfy the population constraint and were drawn under the target temperature value, $\beta^{(r)} = 27$. For starting values, we use independent draws from the standard algorithm (Algorithm 4 as implemented in the BARD package). For both the simulated and parallel tempering algorithms, after some preliminary analysis, we have decided to allow $\beta^{(\ell)}$ to take values between 0 and 27, using power-law spacing, with the target temperature value of 27. As in the small-scale verification study, we only use draws taken under the target temperature, and then reweight according to the importance weights $1/g_{\beta^{(\ell)}}(\mathbf{v})$ before selecting all remaining draws that fall within the target parity deviation of 1%.

Figure 6 presents the results. The figure shows the autocorrelation plots (left column), the trace plots (middle column), and the Gelman-Rubin potential scale reduction factors (Gelman and Rubin, 1992; right column) for the basic algorithm (top panel), the simulated tempering algorithm (middle panel) and the parallel tempering algorithm (bottom panel). We use the logit transformed Republican dissimilarity index for all diagnostics. Both the simulated and parallel tempering algorithms significantly outperform the basic algorithm. The former has a lower autocorrelation and mixes better. In addition, the potential scale reduction factor goes down quickly, suggesting that all the chains with different starting maps become indistinguishable from each other after approximately 1,500 draws.

Mississippi. Next, we analyze the 2008 election data from Mississippi. This state has a total of four congressional districts and 1,969 precincts, thereby providing a

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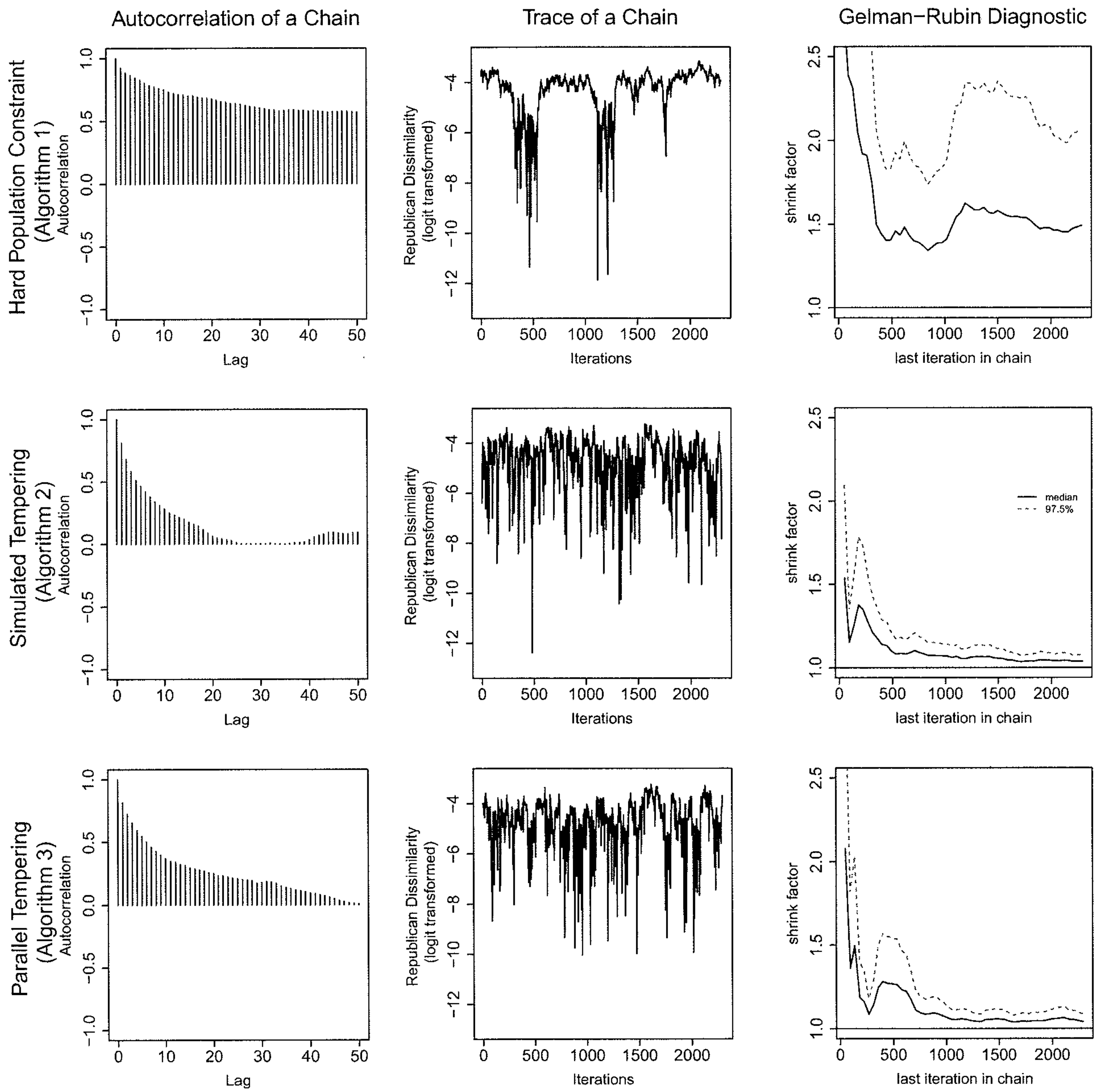

Figure 6: Convergence Diagnostics of the Proposed Algorithm for the 2008 New Hampshire Redistricting Data. The proposed basic algorithm (Algorithm 1; top panel), the simulated tempering algorithm (Algorithm 2; middle panel), and the parallel tempering algorithm (Algorithm 3; bottom panel) are applied to the New Hampshire data with 327 precincts and 2 congressional districts. The target population consists of all redistricting plans with contiguous districts and a maximum of 1% deviation from the population parity. A total of 10 chains are run with different starting maps for each algorithm until 500,000 draws are obtained, and inference is based on a total of 22,970 draws (the number of draws in the simulated tempering algorithm that are both drawn under the target temperature and satisfy the target population constraint). For the logit transformed Republican dissimilarity index, the autocorrelation plots (left column), the trace plots (middle column), and the Gelman-Rubin potential scale reduction factors (right column) are presented. The simulated and parallel tempering algorithms outperform the basic algorithm across all three diagnostics.

more challenging example when compared to New Hampshire. The right-hand panel of Figure 5 shows the implemented redistricting plan in Mississippi as of 2010. In 2008, 43% of the electorate voted for Obama while his voteshare in the 2012 election for this state was 44%. Redistricting in Mississippi is determined by its state legislature subject to gubernatorial veto.

One important feature of Mississippi is its sizable African-American population (37% of the population). This group is concentrated in the capital city, Jackson, and in surrounding areas in the west of the state, which poses a special challenge to the algorithms. Democrats typically win this seat, shaded in blue in Figure 5, while Republicans typically win the other three seats in Mississippi. Mississippi is also one of the nine states fully covered by Section V of the Voting Rights Act, which obligates political officials to submit its proposed redistricting plan to the U.S. Department of Justice. However, following the Supreme Court's decision in *Shelby County v. Holder* (2013) to strike down the pre-clearance formula determining Section V coverage, Mississippi is no longer subject to Section V requirements by default.

Here, we utilize parallel tempering (Algorithm 3) to examine its algorithmic performance for Mississippi. After some preliminary analysis, we chose to anneal $\beta^{(\ell)}$ between 0 and -225 in unequally spaced increments, with the target temperature of $\beta^{(\ell)} = -225$. We run a total of 10 chains for 200,000 simulations each, keeping every 5th draw. Inference is then based off of a total of 138,840 draws, which is the number of remaining simulations drawn under the target $\beta^{(\ell)}$ that fall within 5% of population parity.

Figure 7 presents the results of this analysis. The same set of diagnostics are conducted for the Republican dissimilarity index (top row) and the African-American dissimilarity index (bottom row). The figure shows that although the Mississippi data pose a much more challenging application than the New Hampshire data, the

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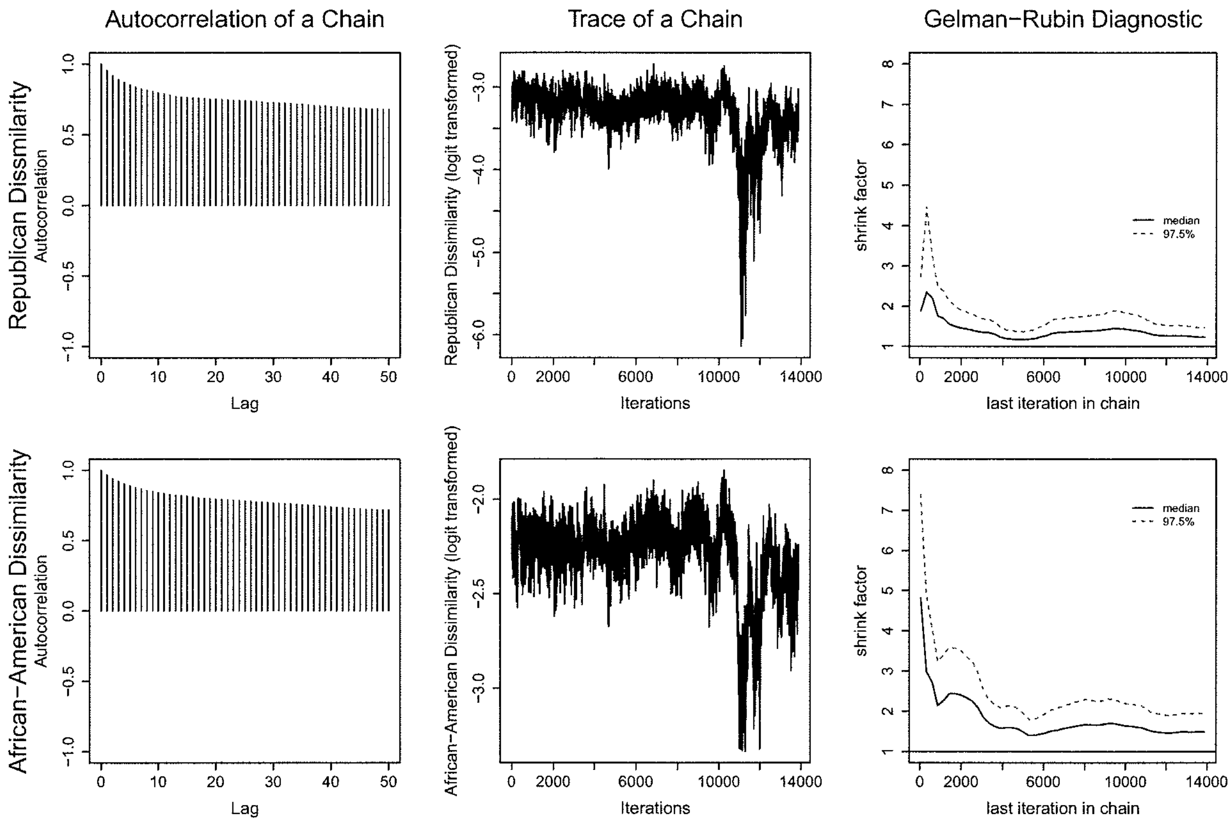

Figure 7: Convergence Diagnostics of the Proposed Algorithm for the 2008 Mississippi Redistricting Data. The information identical to that of Figure 6 is displayed here for two statistics, Republican dissimilarity index and African-American dissimilarity index (both logit transformed). See the caption of Figure 6 for details. The data is obtained from 138,840 draws of the parallel tempering algorithm (Algorithm 3).

parallel tempering algorithm still performs reasonably well. In particular, the potential scale reduction factor (in the plots given in the right column) is relatively low and remains stable for the Republican dissimilarity index, suggesting that the impact of the starting values has mostly disappeared. Because African American voters are geographically concentrated, the algorithm has a harder time mixing for the African-American dissimilarity index. Nevertheless, the scale reduction factor still stabilizes at a reasonably low value, suggesting that the impact of the starting values is limited in this application.

4 Concluding Remarks

Over the last half century, a number of automated redistricting algorithms have been proposed in the methodological literature. Most of these algorithms have been designed to find an optimal redistricting plan given a certain set of criteria. However, many substantive researchers have been interested in characterizing the distribution of redistricting plans under various constraints. Unfortunately, few such simulation algorithms exist and even the ones that are commonly used by applied researchers have no theoretical justification.

In this paper, we propose a new automated redistricting simulator using Markov chain Monte Carlo. Unlike the existing standard algorithm, the proposed algorithms have a theoretical justification and approximate the target distribution well in a small-scale validation study. Even in more realistic settings where the computational challenge is greater, our initial analyses shows a promising performance of the proposed algorithms. Nevertheless, it is still unclear whether these algorithms scale to those states with an even greater number of districts than those considered here. In the future, we plan to investigate whether simulated and parallel tempering strategies can overcome the computational challenge posed by those large states.

Another promising line of research is to examine the factors that predict the redistricting outcome. For example, substantive researchers are interested in how the institutional features of redistricting process (e.g., bipartisan commission vs. state legislature) determines the redistricting process. Such an analysis requires inferences about the parameters that are underlying our generative model. In contrast, in this paper we restricted our attention to the question of how to simulate redistricting plans given these model parameters. Therefore, a different approach is required to address this and other methodological challenges.

Appendix: Proof of Theorem 1

Let $\Gamma(C^*, G_{\mathbf{v}})$ denote all sets of connected components C obtainable through "turning on" edges in $E_{\mathbf{v}}$ such that $C^* \subset C$. Let $p(C \mid G_{\mathbf{v}})$ denote the probability that C is obtained through Steps 1 and 2 of Algorithm 1. Let $p(C^* \mid C)$ denote the probability that, given C, its particular subset C^* is selected at Step 3. Note that this probability does not depend on the partition $\mathbf{v}$. Then, it follows that

$$\pi(\mathbf{v}_{t-1} \to \mathbf{v}_t^*) = \sum_{C' \in \Gamma(C^*, G_{\mathbf{v}_{t-1}})} p(C^* \mid C') p(C' \mid G_{\mathbf{v}_{t-1}}) \prod_{\ell=1}^{r} \frac{1}{|A(C_\ell^*, \mathbf{v}_{t-1})|} \quad (11)$$

$$\pi(\mathbf{v}_t^* \to \mathbf{v}_{t-1}) = \sum_{C' \in \Gamma(C^*, G_{\mathbf{v}_t^*})} p(C^* \mid C') p(C' \mid G_{\mathbf{v}_t^*}) \prod_{\ell=1}^{r} \frac{1}{|A(C_\ell^*, \mathbf{v}_t^*)|} \quad (12)$$

We now simplify equations (11) and (12) to identify common terms, which then cancel each other in equation (3). First, we show

$$|A(C_\ell^*, \mathbf{v}_{t-1})| = |A(C_\ell^*, \mathbf{v}_t^*)| \quad (13)$$

for any connected component $C_\ell^* \in C^*$ where $l \in \{1, \ldots, r\}$.

Suppose that, without loss of generality, C_ℓ^* is adjacent to blocks $V_{1,t-1}, V_{2,t-1}, \ldots, V_{|A(C_\ell^*, \mathbf{v}_{t-1})|, t-1} \in \mathbf{v}_{t-1}$, and C_ℓ^* is contained in block $V_{|A(C_\ell^*, \mathbf{v}_{t-1})|+1, t-1} \in \mathbf{v}_{t-1}$. The check that $V_{kt}^* \neq \emptyset$ in Step 4 of the algorithm ensures that $C_\ell^* \neq V_{|A(C_\ell^*, \mathbf{v}_{t-1})|+1, t-1}$. Since $\mathbf{v}_{t-1}$ is a connected set partition, there must exist $\{i_{|A(C_\ell^*, \mathbf{v}_t^*)|+1}\} \in C_\ell^*$ and $\{j_{|A(C_\ell^*, \mathbf{v}_t^*)|+1}\} \in V_{|A(C_\ell^*, \mathbf{v}_{t-1})|+1, t-1} \setminus C_\ell^*$ that are adjacent in $G_{\mathbf{v}_{t-1}}$. Moreover, there exist pairs of adjacent nodes $(\{i_1\}, \{j_1\}), \ldots, (\{i_{|A(C_\ell^*, \mathbf{v}_{t-1})|}\}, \{j_{|A(C_\ell^*, \mathbf{v}_{t-1})|}\})$ with $\{i_k\} \in C_\ell^*, \{j_k\} \in V_{k,t-1}$ where $1 \leq k \leq |A(C_\ell^*, \mathbf{v}_{t-1})|$. Since C^* is comprised of non-adjacent connected components, it follows that nodes $\{j_1\}, \ldots, \{j_{|A(C_\ell^*, \mathbf{v}_{t-1})|}\}, \{j_{|A(C_\ell^*, \mathbf{v}_{t-1})|+1}\}$ do not change block assignment when transitioning from $\mathbf{v}_{t-1}$ to $\mathbf{v}_t^*$, and thus, are contained in distinct blocks in $\mathbf{v}_t^*$. Thus, the connected component C_ℓ^* is adjacent to all blocks corresponding to a node in $\{\{j_1\}, \ldots, \{j_{|A(C_\ell^*, \mathbf{v}_t^*)|}\}, \{j_{|A(C_\ell^*, \mathbf{v}_t^*)|+1}\}\}$ except for the block containing C_ℓ^*: $|A(C_\ell^*, \mathbf{v}_{t-1})|$ blocks in total. Hence, $|A(C_\ell^*, \mathbf{v}_t^*)| \geq |A(C_\ell^*, \mathbf{v}_{t-1})|$. Moreover, for any block $V_{k,t-1} \notin A(C_\ell^*, \mathbf{v}_{t-1})$ such that $C_\ell^* \not\subset V_{k,t-1}$, the corresponding block $V_{k,t}^*$ obtained by swapping connected components in C^* will not be contained in $A(C_\ell^*, \mathbf{v}_t^*)$; by definition, for any $\{i\} \in C_\ell^*$, $\{j\} \in V_{k,t-1}$, $(i,j) \notin E$, and since connected components in C^* are not adjacent, it follows that no edge connects a vertex in $V_{k,t}^*$ to a vertex in C_ℓ^*. This proves equation (13).

Next, through a proof by contradiction, we show that

$$\Gamma(C^*, G_{\mathbf{v}_{t-1}}) = \Gamma(C^*, G_{\mathbf{v}_t^*}). \quad (14)$$

By showing this, we also conclude that $\mathbf{v}_{t-1}$ can be a candidate partition when starting from $\mathbf{v}_t^*$, i.e., $\pi(\mathbf{v}_{t-1} \to \mathbf{v}_t^*) > 0$ implies $\pi(\mathbf{v}_t^* \to \mathbf{v}_{t-1}) > 0$. Suppose that there exists a set of connected components $C' \in \Gamma(C^*, G_{\mathbf{v}_{t-1}})$ such that $C' \notin \Gamma(C^*, G_{\mathbf{v}_t^*})$. This means that there exists $C'_\ell \in C'$ that can be formed by turning on edges in $E_{\mathbf{v}_{t-1}^*}$ but not in $E_{\mathbf{v}_t^*}$. Thus, there exists $\{i\}, \{j\} \in C'_\ell$ such that $(i,j) \in E_{\mathbf{v}_{t-1}}$ and

$(i,j) \notin E_{\mathbf{v}_t^*}$. However, according to Step 4 of the algorithm, the only edges deleted in the transition between $\mathbf{v}_{t-1}$ and $\mathbf{v}_t^*$, are those connecting a vertex in $\{i\}$ in C^* to a vertex $\{j\} \notin C^*$. Since $C^* \subset C' \in \Gamma(C^*, G_{\mathbf{v}_{t-1}})$, $\{i\}$ and $\{j\}$ cannot be contained in the same component of C', a contradiction. An analogous argument shows that there is no connected component $C' \in \Gamma(C, \mathbf{v}_t^*)$ such that $C' \notin \Gamma(C, \mathbf{v}_{t-1})$. This proves equation (14).

Third, we decompose $p(C \mid G_{\mathbf{v}})$. For a partition $\mathbf{v}$, let $\Lambda(C, E_{\mathbf{v}})$ denote all subsets of edges of $E_{\mathbf{v}}$ such that, when only those edges in a subset are turned on, the set of connected components C is formed (Step 2). Note that C can be formed if and only if the partition $\mathbf{v}$ satisfies $E_C \subset E_{\mathbf{v}}$, and $\Lambda(C, E_{\mathbf{v}})$ is identical for all such partitions. Specifically, $\Lambda(C, E_{\mathbf{v}_{t-1}}) = \Lambda(C, E_{\mathbf{v}_t^*})$. To see this, observe that every set of edges $E_{\mathbf{v}}^* \in \Lambda(C, E_{\mathbf{v}})$ must connect nodes within each connected component in C, and must not include any edges joining a connected component to a node not included in the connected component. For any connected component $C_\ell \in C$, there must be a block $V_k \in \mathbf{v}$ such that $C_\ell \subset V_k$. Since $E_{\mathbf{v}}$ contains all edges joining two nodes in V_k, it follows that any set of edges connecting nodes in C is contained in $E_{\mathbf{v}}$.

Given a set of "turned-on" edges $E_{\mathbf{v}}^* \in \Lambda(C, E_{\mathbf{v}})$, define $\overline{E}_{\mathbf{v}}^* \equiv E_{\mathbf{v}} \setminus E_{\mathbf{v}}^*$ as the set of "turned-off" edges. Observe that, for $E_{\mathbf{v}_{t-1}}^* \in \Lambda(C, E_{\mathbf{v}_{t-1}})$, $E_{\mathbf{v}_t^*}^* \in \Lambda(C, E_{\mathbf{v}_t})$ with $E_{\mathbf{v}_{t-1}}^* = E_{\mathbf{v}_t}^*$, $\overline{E}_{\mathbf{v}_{t-1}}^*$ may be different from $\overline{E}_{\mathbf{v}_t^*}^*$. That is, if the candidate partition $\mathbf{v}^*$ is obtained from $\mathbf{v}_{t-1}$ by assigning connected component $C' \in C$ from block V_ℓ to block $V_{\ell'}$, $\overline{E}_{\mathbf{v}_t^*}^*$ may contain an edge that connects a node in C' to an adjacent node in $V_{\ell'}$, whereas this edge cannot occur in $\overline{E}_{\mathbf{v}_{t-1}}^*$. Define

$$
\begin{aligned}
B(C^*, \overline{E}_{\mathbf{v}}^*) &\equiv \{(i,j) \in \overline{E}_{\mathbf{v}}^* : \{i\} \in C^*, \{j\} \notin C^*\} \\
&= \{(i,j) \in \overline{E}_{\mathbf{v}}^* : \exists C_\ell^* \in C^*, C_\ell^* \subset V_k \in \mathbf{v} \ s.t. \{i\} \in C_\ell^*, \{j\} \in V_k \setminus C_\ell^*\}
\end{aligned} \tag{15}
$$

as the set of edges in $\overline{E}_{\mathbf{v}}^*$ that connect a block of nodes in C^* to a vertex not in C^*, i.e., those edges that need to be "cut" to form blocks of vertices C^*. Since $C^* \subset C$, for partition $\mathbf{v}$, $B(C^*, E_{\mathbf{v}})$ is the same for every set of turned-on edges in $\Lambda(C, E_{\mathbf{v}})$, and is the same across all sets of connected components in $\Gamma(C^*, G_{\mathbf{v}})$. Then, we can write $p(C \mid G_{\mathbf{v}})$ as:

$$
p(C \mid G_{\mathbf{v}_{t-1}}) = \prod_{e \in B(C^*, E_{\mathbf{v}_{t-1}})} (1 - q_e) \sum_{E_{\mathbf{v}_{t-1}}^* \in \Lambda(C, E_{\mathbf{v}_{t-1}})} \prod_{e \in E_{\mathbf{v}_{t-1}}^*} q_e \prod_{e \in \overline{E}_{\mathbf{v}_{t-1}}^* \setminus B(C^*, E_{\mathbf{v}_{t-1}})} (1 - q_e) \tag{16}
$$

where we allow the edge cut probability to differ across edges.

Finally, we show that, for any $E_{\mathbf{v}_{t-1}}^* \in \Lambda(C, E_{\mathbf{v}_{t-1}})$, $E_{\mathbf{v}_t^*}^* \in \Lambda(C, E_{\mathbf{v}_t})$ with $E_{\mathbf{v}_{t-1}}^* = E_{\mathbf{v}_t}^*$,

$$
E_{\mathbf{v}_{t-1}}^* \setminus B(C^*, E_{\mathbf{v}_{t-1}}) = E_{\mathbf{v}_t^*}^* \setminus B(C^*, E_{\mathbf{v}_t^*}) \tag{17}
$$

Consider any edge $e \in E_{\mathbf{v}_{t-1}} \setminus B(C^*, E_{\mathbf{v}_{t-1}})$. This edge can either join two nodes within a single connected component or joins two nodes in two distinct connected components. In the former case, both nodes are contained in a single block of $\mathbf{v}_{t-1}$, and since connected components are reassigned to form the candidate partition $\mathbf{v}_t^*$, it follows that both nodes are contained in a single block $V^* \in \mathbf{v}_t^*$. Hence, $e \in E_{\mathbf{v}_t^*}$, and since does not join a node in connected component in C^* to a node in a connected component that is not in C^*, it follows that $e \in E_{\mathbf{v}_t^*} \setminus B(C^*, E_{\mathbf{v}_t^*})$. In the latter case, observe that, since $e \in E_{\mathbf{v}_{t-1}}$, both connected components must be contained within the same block of $\mathbf{v}_{t-1}$. Since they do not belong to C^*, neither component is reassigned to a block, and hence, are contained within the same block $V_{kt}^* \in \mathbf{v}_t^*$. Thus, $e \in E_{\mathbf{v}_t^*}$, and since does not join a node in connected component in C^* to a node in a connected component that is not in C^*, it follows that $e \in E_{\mathbf{v}_t^*} \setminus B(C^*, E_{\mathbf{v}_t^*})$. In both cases, $e \in E_{\mathbf{v}_t^*} \setminus B(C^*, E_{\mathbf{v}_t^*})$. Thus, $E_{\mathbf{v}_{t-1}} \setminus B(C^*, E_{\mathbf{v}_{t-1}}) \subset E_{\mathbf{v}_t^*} \setminus B(C^*, E_{\mathbf{v}_t^*})$. By the same argument, $E_{\mathbf{v}_t^*} \setminus B(C^*, E_{\mathbf{v}_t^*}) \subset E_{\mathbf{v}_{t-1}} \setminus B(C^*, E_{\mathbf{v}_{t-1}})$, and we have shown equation (17). By this observation, we can now write,

$$p(C \mid G_{\mathbf{v}_t^*}) = \prod_{e \in B(C^*, E_{\mathbf{v}_t^*})} (1-q_e) \sum_{E^*_{\mathbf{v}_{t-1}} \in \Lambda(C, E_{\mathbf{v}_{t-1}})} \prod_{e \in E^*_{\mathbf{v}_{t-1}}} q_e \prod_{e \in E^*_{\mathbf{v}_{t-1}} \setminus B(C^*, E_{\mathbf{v}_{t-1}})} (1-q_e). \tag{18}$$

Using equation (16) and the fact that the set of edges $B(C^*, \mathbf{v}_{t-1})$ is identical across all sets of connected components $C_\ell \in C^*$, we can write as:

$$\pi(\mathbf{v}_{t-1} \to \mathbf{v}_t^*) = \prod_{e \in B(C^*, E_{\mathbf{v}_{t-1}})} (1-q_e) \sum_{C \in \Gamma(C^*, \mathbf{v}_{t-1})} \left(\sum_{E^*_{\mathbf{v}_{t-1}} \in \Lambda(C, E_{\mathbf{v}_{t-1}})} \prod_{e \in E^*_{\mathbf{v}_{t-1}}} q_e \prod_{e \in \overline{E}^*_{\mathbf{v}_{t-1}} \setminus B(C^*, E_{\mathbf{v}_{t-1}})} (1-q_e) \right) \times p(C^* \mid C) \prod_{\ell=1}^{r} \frac{1}{|A(C_\ell^*, \mathbf{v}_{t-1})|} \tag{19}$$

Similarly, we find that:

$$\pi(\mathbf{v}_t^* \to \mathbf{v}_{t-1}) = \prod_{e \in B(C^*, E_{\mathbf{v}_t^*})} (1-q_e) \sum_{C \in \Gamma(C^*, \mathbf{v}_{t-1})} \left(\sum_{E^*_{\mathbf{v}_{t-1}} \in \Lambda(C, E_{\mathbf{v}_{t-1}})} \prod_{e \in E^*_{\mathbf{v}_{t-1}}} q_e \prod_{e \in \overline{E}^*_{\mathbf{v}_{t-1}} \setminus B(C^*, E_{\mathbf{v}_{t-1}})} (1-q_e) \right) \times p(C^* \mid C) \prod_{\ell=1}^{r} \frac{1}{|A(C_\ell^*, \mathbf{v}_{t-1})|}. \tag{20}$$

Thus, many terms cancel out and we obtain the following expression for the acceptance probability:

$$\alpha(\mathbf{v} \to \mathbf{v}^*) = \min \left(1, \frac{\prod_{e \in B(C^*, \mathbf{v}_t^*)} (1-q_e)}{\prod_{e \in B(C^*, \mathbf{v}_{t-1})} (1-q_e)} \right). \tag{21}$$

In the special case that edges are turned on with equal probability, i.e., $q = q_e$ for all e, this ratio can be computed by counting the number of edges connecting nodes in blocks of C^* to nodes outside of those blocks:

$$\alpha(\mathbf{v} \to \mathbf{v}^*) = \min \left(1, (1-q)^{|B(C^*, \mathbf{v}_t^*)| - |B(C^*, \mathbf{v}_{t-1})|}\right). \tag{22}$$

□

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Measuring the Compactness of Political Districting Plans

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Abstract

We develop a measure of compactness based on the distance between voters within the same district relative to the minimum distance achievable – which we coin the relative proximity index. Any compactness measure which satisfies three desirable properties (anonymity of voters, efficient clustering, and invariance to scale, population density, and number of districts) ranks districting plans identically to our index. We then calculate the relative proximity index for the 106th Congress, requiring us to solve for each state's maximal compactness; an NP-hard problem. The correlation between our index and the commonly-used measures of dispersion and perimeter is -.37 and -.29, respectively. We conclude by estimating seat-vote curves under maximally compact districts for several large states. The fraction of additional seats a party obtains when their average vote increases is significantly greater under maximally compact districting plans, relative to the existing plans.

Keywords: Compactness, gerrymandering, power diagrams, redistricting.
JEL Codes: H70, K19

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1 Introduction

The architecture of political boundaries is at the heart of the political process in the United States.[1] When preferences over political candidates are sufficiently heterogeneous, altering the landscape of political districts can have large effects on the composition of elected officials. Prior to the 2003 Texas redistricting, the congressional delegation was comprised of 17 Democrats and 15 Republicans; after the 2004 elections there were 11 Democrats and 21 Republicans.[2] Politically and racially motivated districting plans are believed to be a significant reason for the lack of adequate racial representation in state and federal legislatures, and there is a debate as to whether the creation of majority-minority districts to ensure some level of minority representation have led to fewer minority-friendly policies (see Shotts, 2002 for an excellent overview and critique).

There are several factors which weigh on the constitutionality of districting plans: (i) equal population (the Supreme Court first established this principle for congressional districts in Wesberry v. Sanders, 376 US 1 (1964)), (ii) contiguity (which is a requirement in 49 state constitutions), and (iii) compactness. The latter consideration – distinct from the mathematical notion of a finite subcover of a topological space – refers to how "oddly shaped" a political district is. The Supreme Court has acknowledged the importance of compactness in assessing districting plans for nearly half a century.[3] Yet, despite its importance as a factor in adjudicating gerrymandering claims, the court has made it clear that no manageable standards have emerged (see the judgment of Scalia, J., in *Vieth v. Jubelirer*). There is no consensus on how to adequately measure compactness.[4]

In this paper, we propose a simple index of compactness based on the distance between voters within the same political district in a state relative to the minimum such distance achievable by any districting plan in that state – which we coin the relative proximity index.[5] The index satisfies three desirable properties: (i) voters are treated equally (*anonymity*), (ii) increasing the distances between voters within a political district leads to a larger value of the index (*clustering*), and

[1] Article I, §4 of the United States Constitution provides that "The Times, Places and Manner of holding Elections for Senators and Representatives shall be prescribed in each State by the Legislature thereof; but the Congress may at any time by Law make or alter such Regulations, except as to the Places of choosing Senators."

[2] In the US, political boundaries are typically redrawn every 10 years, after the decennial census. The 2003 "mid-decade" redistricting in Texas is a notable exception. The US Supreme Court recently held that this was not unconstitutional in *Jackson, et al. v. Perry, et al.* (docket number 05-276).

[3] The Apportionment Acts of 1842, 1901 and 1911 contained a compactness requirement. In Davis v. Bandemer, 476 US 173 (1986)) Justices Powell and Stephens pointed to compactness as a major determinant of partisan gerrymandering, and Justices White, Brennan, Blackmun and Marshall cited it as a useful criterion. Nineteen state constitutions still contain a compactness requirement (Barabas and Jerit, 2004).

[4] An important argument against the use of compactness as a districting principle is that it may disadvantage certain population subgroups. As Justice Scalia put it in *Vieth v. Jubelirer*: "Consider, for example, a legislature that draws district lines with no objectives in mind except compactness and respect for the lines of political subdivisions. Under that system, political groups that tend to cluster (as is the case with Democratic voters in cities) would be systematically affected by what might be called a "natural" packing effect. See Bandemer, 478 U. S., at 159 (O'Connor, J., concurring in judgment)." First, the courts use compactness as one of several criterion. Second, it is an open question whether or not more compact districting plans have a positive or negative effect on racial or political representation.

[5] For the emprical analysis and characterization of the optimally compact district plan we use Euclidean distance. But since many of our results are proven in an arbitrary metric space, one can extend much of the analysis here by using driving distance or what many legal scholars refer to as "communities of interest."

(iii) the index be invariant to the scale, population density, and the number of districts in a state (*independence*). In a technical Appendix, we show that any compactness index that satisfies these properties ranks districting plans identically to the relative proximity index.

The relative proximity index has several advantages over existing measure of compactness. First, it is the only compactness index which permits meaningful comparisons across states. Second, the index does not assume (implicitly or otherwise) that voters are uniformly distributed across political districts. Many previously proposed measures adopt a geometric approach (perimeter length of political districts, e.g.) and fail to consider the distribution of voters within a state. Third, our measure is constructed at the state level. Some measures apply to political districts.[6] Yet, the districting problem is fundamentally about partitioning; the shape of one element of the partition affects the shapes of the other elements. Analyzing individual pieces of a larger partition in isolation can be misleading. Fourth, though our index is simple, it is based on desirable properties that compactness measures should satisfy. Existing measures have been proposed in a relatively ad hoc fashion. At a minimum, our approach is a more principled way of narrowing the field of competing measures.

We apply the index to the districting plans of the 106th congress using tract-level data from the US census. In doing so, we are required to calculate each state's maximal compactness. This number is the denominator of our index. But calculating this number by brute force, enumerating the set of all feasible partitions and maximizing compactness over this set, is impossible.[7] Similar partitioning problems arise in applied mathematics (computer vision), computer science and operations research (the k-way equipartition problem), and computational biology (gene clustering), which have given rise to several important algorithms and candidate functionals. Unfortunately, none of these techniques are directly applicable to our districting problem as they are either designed for very small samples (≈100) or do not require partitions to be of even approximately equal size.

We develop an algorithm for approximating this partitioning problem which is suitable for very large samples and guarantees nearly equal populations in each partition. The algorithm is based on *power diagrams* – a generalization of classic Voronoi diagrams – which have been used extensively in algebraic and tropical geometry (Passare and Rullgard, 2004; Richter-Gebert, Sturmfels and Theobald, 2003), condensed matter physics, and toric geometry/string theory (Diaconescu, Florea, and Grassi, 2002). Power diagrams are a powerful tool to partition Euclidean space into cells by minimizing the distance between points in a cell and the centroid of that cell. We prove that maximally compact districts are power diagrams and that the line separating two adjacent districts are perpendicular to the line connecting their centroids, and all such lines separating three adjacent districts meet at a single point. It follows that the resulting districts are convex polygons.

The empirical results we obtain on the compactness of districting plans are interesting and in some cases quite surprising. The five states with the most compact districting plans are Idaho,

[6] See Young (1988), however, and section 2.2 below.

[7] A back of the envelope calculation reveals that, for California alone, the cardinality of this set is larger than the number of atoms in the observable universe.

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2 Background and Previous Literature

2.1 A Brief Legal History of Compactness

Compactness has played a fundamental role in the jurisprudence of gerrymandering, both racial and political. Since *Gomillion v. Lightfoot* 364 U.S. 339 (1960), where the court struck down Alabama's plan to redraw the boundaries of the city of Tuskegee, the court has recognized compactness as a relevant factor in considering racial gerrymandering claims. In *Gomillion* the court referred to the proposed district as "an uncouth 28-sided figure." Although *Gomillion* is considered by many to be a jurisprudential high-water mark, the role of compactness in considering racial gerrymandering claims has been affirmed in other decisions.[8] As Justice O'Connor put it: "we believe that reapportionment is one area in which appearances do matter."

Compactness has also played an important role in partisan gerrymandering claims. It has been recognized by the court as a "traditional" districting principle. In *Davis v. Bandemer*, Justices Powell and Stevens described compactness as a major criterion (at 173), and Justices White, Brennan, Blackmun and Marshall described it as an important criterion (at 2815). In *Vieth*, the plurality acknowledged compactness as a traditional districting principle. Justice Kennedy, in his concurring opinion, states that compactness is an important principle in assessing partisan gerrymandering claims: "We have explained that "traditional districting principles," which include "compactness, contiguity, and respect for political subdivisions," are "important not because they are constitutionally required...but because they are objective factors that may serve to defeat a claim that a district has been gerrymandered on racial lines." ...In my view, the same standards should apply to claims of political gerrymandering, for the essence of a gerrymander is the same regardless of whether the group is identified as political or racial."

Despite different views about what a judicially manageable standard is or might be, the court has been unanimous that it must include some notion of compactness.

2.2 Existing Measures of Compactness

There is a large literature in political science on the measurement of compactness. Niemi et al (1990) provide a comprehensive account of the various measures which have been proposed (see also Young (1988)).[9] Niemi et al (1990) classify existing measures into four categories: (i) dispersion measures, (ii) perimeter measures, (iii) population measures, and (iv) other miscellaneous measures.[10] The important take-away is that all of these measures either fail to account for the population distribution or are not invariant to geographical size. As such, meaningful comparisons across states or time cannot be made.

[8] In *Shaw v. Reno* 113 S. Ct. 2816. 92-357 (1993), the court upheld a challenge to North Carolina's redistricting plan on the basis that the ill-compactness of the districts was indicative of racial gerrymandering. See also *Thornburg v. Gingles* 478 U.S. 30 (1986) or *Growe v. Emison* 278 U.S. 109 (1993).

[9] Some of these measures were originally proposed for purposes other than to do with legislative districts - but were later applied by other authors to that issue. We cite the original authors.

[10] We draw heavily on their summary and classification.

One class of dispersion measures are based on length versus width of a rectangle which circumscribes the district (Harris, 1964; Eig and Setizinger, 1981; Young, 1988). A second uses circumscribing figures other than rectangles and considers the area of these figures.[11] At least two "moment-of-inertia" measures have been suggested. Schwartzberg (1966) and Kaiser (1966) consider the variance of the distances from each point in the district to the districts areal center. Boyce and Clark (1964) consider the mean distance from the areal center to a point on the perimeter reached by equally spaced radial lines.

A second set of measures are those based on perimeters. The sum of perimeter lengths was suggested by Adams (1977), Eig and Setizinger (1981) and Wells (1982), but this measure is potentially intractable for reasons highlighted in the classic work of Mandelbrot (1967) on the length of the coastline of Great Britain. In fact, a fractal dimension based measure was proposed by Knight (2004). Various authors have proposed measures which compare the perimeter to the area of the district. Cox (1927) considers the ratio of the district area to that of a circle with the same perimeter.[12]

There are three population-based measures. Hofeller and Groffman (1990) propose two: the ratio of the district population to the convex hull of the district, and the ratio of the district population to the smallest circumscribing circle. Weaver and Hess (1963) suggest the population moment of inertia, normalized to lie in the unit interval.

Niemi et al's (1990) final miscellaneous category includes three measures: (1) the absolute deviation of district area from average area in the state (Theobald 1970); a measure based on the number of reflexive and non-reflexive interior angles (Taylor 1973); and the sum of all pairwise distances between the centers of subunits of the district, weighted by subunit population (Papayanopolous 1973). Finally, Mehrotra, Johnson and Nemhauser (1998) use a branch-and-price algorithm to compute a districting plan for South Carolina. Their objective function is how far people are from a graph-theoretic measure of the center of the district.

3 The Relative Proximity Index

3.1 Basic Building Blocks

Let $\mathbf{S}$ denote a collection of states with typical element $S \in \mathbf{S}$. A finite set S, whose elements we call individuals or voters, is a metric space with associated distance function $d_{ij} \geq 0$, which measures the distance between any two elements $i, j \in S$. Let $V_S = \{v_1^S, ..., v_n^S\}$ denote a finite partition of S into elements $v_i \in V_S$ which we shall refer to as "voting districts", or "districts". We will routinely refer to the partition V_S as a "districting plan" for state S and allow n to represent a

[11] Reock (1961) proposes a circle, Geisler (1965) a hexagon, Horton (1932) and Gibbs (1961) a circle with diameter equal to the districts longest axis, still others use the smallest convex figure (see Young (1988)).

[12] For variants of Cox (1927) see Attneave and Arnoult (1956), Horton (1932), Schwartzberg (1966), or Pounds (1972).

generic integer. We restrict voting districts to be equal in size, up to integer rounding.[13] [14] Let $\mathcal{V}_S$ denote the set of all partitions of S which satisfy this restriction. We say a districting plan V_S is *feasible* if and only if $V_S \in \mathcal{V}_S$.

Definition 1 *A compactness index for a state S is a map $c : V_S \mapsto \mathbb{R}_+$.*

3.2 The Relative Proximity Index

Consider voter i in element $v \in V_S$ and define:

$$\pi\left(V_S\right) = \sum_{v \in V} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2 \tag{1}$$

Similarly, let $V_S^* = \underset{V_S \in \mathcal{V}_S}{\arg\min} \left\{\pi\left(V_S\right)\right\}$. The Relative Proximity Index (RPI), for a partition of state S, V_S, is given by

$$RPI = \frac{\pi(V_S)}{\pi(V_S^*)}.$$

The RPI is well defined so long as $\pi(V_S^*) \neq 0$ which holds so long as all voters are not located at the same point. In the non-degenerate case, the RPI ranges from 1 to infinity; higher numbers indicate less compactness. The index has an intuitive interpretation: a value of 3 implies that the current districting plan is roughly 3 times less compact than a state's maximal compactness. Further, Theorem 1 in Appendix A shows that any index that satisfies three axioms – anonymity of voters, efficient clustering, and invariance to scale, population density, and number of districts – ranks districting plans identically to the RPI.

3.3 A Constructive Example

[insert figure 1]

Consider the state depicted in Figure 1. The nodes represent voters. There are two voting districts separated by the bold dashed line. Voters are spread evenly across the state; each adjacent voter is 1 kilometer apart. Voter 1 is 1 kilometer away from voters 2 and 4, $\sqrt{2}$ kilometers away from voter 5, $\sqrt{5}$ kilometers away from voter 6, and so on.

There are two steps involved in calculating the Relative Proximity index. First, we calculate the numerator. For voter 1 the sum of squared distances is 5, since she is 1 kilometer away from voter 2 and 2 kilometers away from voter 3–and they are the only other voters in her district. For

[13] This was first held as a requirement by the Court in *Baker*, and is becoming a very strict constraint. For instance, a 2002 Pennsylvania redistricting plan was struck down because one district had 19 more people (not even voters) than another. The 2004 Texas redistricting had each district with the same number of people up to integer rounding. Yet, the population may grow at drastically different rates across political districts between redistrictings. For instance, in the 2000 census, a typical state had a 23% difference in the population of its smallest and largest district.

[14] In symbols: $\left|v_i^S\right| \in \left\{\lfloor |S| / |V_S| \rfloor, \lceil |S| / |V_S| \rceil\right\}$ for all $v_i^S \in V_S$, where $\lceil x \rceil = \inf\left\{n \in \mathbb{Z} | x \leq n\right\}$ and $\lfloor x \rfloor = \sup\left\{n \in \mathbb{Z} | n \leq x\right\}$.

voter 2 the total is $1^2 + 1^2 = 2$ and for voter 3 it is $1^2 + 2^2 = 5$. Voters 4,5 and 6 are symmetric to voters 1,2 and 3 respectively. Thus the numerator of our index is $2(5 + 2 + 5) = 24$.

The second step in calculating RPI is to account for state specific topography. This will represent the denominator of our index. There are nine other feasible partitions in addition to $\{\{1, 2, 3\}, \{4, 5, 6\}\}$.[15] We perform the same calculation as above for each of those partitions and then take the min of these ten values. The minimizing partition is $\{\{1, 4, 5\}, \{2, 3, 6\}\}$–although $\{\{1, 2, 4\}, \{3, 5, 6\}\}$ achieves the same value. That value turns out to be $2\left(1^2 + 2 + 1^2 + 2 + 1^2 + 1^2\right) = 16$. The index is thus $24/16 = 3/2$.

The example provides a snap-shot of the Relative Proximity Index and previews some of its properties. For instance, because the index is calculated relative to a state specific baseline, *neither the size of states nor their population density can solely alter the index.* If we increased the distance between any two nodes in figure 1 to 2 kilometers, the index would not change. Similarly, if we imputed 10 more individuals to each node – thinking of them in terms of neighborhoods rather than households – the index would be unaltered.

4 Implementing the Relative Proximity Index

In this section, we apply the relative proximity index to the districting plans of the 106th congress.

4.1 The Minimum Partitioning Problem

Calculating the denominator of the relative proximity index is a complicated combinatorial problem. When partitioning n voters into d districts the number of feasible partitions is $\left(\frac{(n-1)!}{(n/d-1)!(n-n/d)!}\right)^{d-1}$. So, for California alone, using data at the tract level, involves $n = 6,800$ and $d = 53$. The cardinality of the set of feasible partitions is $78.4 \times 10^{59,351}$. Technically speaking, the problem is *NP-hard.*

Similar problems arise in fields such as applied mathematics (computer vision), computer science and operations research (k-way equipartitioning problem), and computational biology (gene clustering). The celebrated Mumford-Shah functional is a candidate functional designed to segment images (Mumford and Shah, 1989). The structure of the functional contains two penalty functions: one to ensure that the continuous approximation is close to the discrete problem, and another to penalize perimeter length. While the Mumford-Shah functional is a powerful tool for myriad problems, it cannot guarantee even nearly equal population size across districts.

If our objective function was simply distance, rather than distance squared, the problem is precisely the k-way equipartition problem which has received considerable attention in computer science and related to a literature in computational biology employing minimum spanning trees to partition similar genes into clusters.[16] Good algorithms for the k-way equipartition problem when

[15] They are: $\{\{1,2,4\},\{3,5,6\}\}$, $\{\{1,2,5\},\{3,4,6\}\}$, $\{\{1,2,6\},\{3,4,5\}\}$, $\{\{1,3,4\},\{2,5,6\}\}$, $\{\{1,3,5\},\{2,4,6\}\}$, $\{\{1,3,6\},\{2,4,5\}\}$, $\{\{1,4,5\},\{2,3,6\}\}$, $\{\{1,4,6\},\{2,3,5\}\}$, $\{\{1,5,6\},\{2,3,4\}\}$.

[16] Without the constraint that each district have an equal number of voters the problem is the *min-sum k-clustering problem* which was shown by Sahni and Gonzales (1976) to be NP-complete. An approximation for it in a general metric space which runs in $n^{O(1/\epsilon)}$ time has been found by Bartal, Charikar and Raz (2001). It is also closely related

sample sizes are small (≈ 100) can be found in Ji and Mitchell (2005) and Mitchell (2003). This restriction makes these algorithms impractical for our purposes.

Below, we develop an algorithm to approximate the minimum partitioning problem for large samples, based on *power diagrams* (a concept we make precise below), that guarantees nearly equal populations in each partition and runs in $O\left(n \log\left(n'\right)\right)$ time, where n' is the number of voters and n is the number of districts in a state.

4.2 Optimally Compact Districting Plans and Power Diagrams

In this section, we show that optimally compact districting plans are *power diagrams*, a generalization of Voronoi diagrams due to Aurenhammer (1987). Consider a set of *generator points* $m_1, \ldots, m_n$ in a finite dimensional Euclidean space. The *power* of a point/voter $x \in S$ with respect to a generator point m_i is given by the function $pow_\lambda(x, m_i) = \|x - m_i\|^2 - \lambda_i$, where $\|\cdot\|$ is the Euclidean norm. The total number of voters assigned to generator point m_i is called its *capacity*, denoted K_{m_i}. A *power diagram* is an assignment of voters to generator points such that point x is assigned to generator point m_i if and only if $pow_\lambda(x, m_i) < pow_\lambda(x, m_j)$ for all $j \neq i$. Let the points assigned to generator point m_i be denoted D_i, which is referred to as a *cell*. Note that no two D_is can intersect, and furthermore, every $x \in S$ is in some D_i, and hence $\{D_1, \ldots, D_n\}$ is a partition of S. Note also that the dividing line between cells D_i and D_j in a power diagram satisfies $\|x - m_i\|^2 - \|x - m_j\|^2 = \lambda_i - \lambda_j$.

When $\lambda_i = \lambda$ for all i then the power diagram is a Voronoi diagram. Power diagrams are thus a generalization of Voronoi diagrams.

Definition 2 *An optimally compact districting plan for state S is a feasible districting plan, V_S, with an associated total distance $\sum_{v \in V_S} \sum_{i,j \in v} (d_{ij})^2$ such that there does not exist another feasible districting plan, V_S' with an associated total distance $\sum_{v \in V_S'} \sum_{i,j \in v} (d_{ij})^2$ such that $\sum_{v \in V_S'} \sum_{i,j \in v} (d_{ij})^2 <$ $\sum_{v \in V_S} \sum_{i,j \in v} (d_{ij})^2$.*

We can now state our second key result.

Theorem 2 *Optimally compact districting plans are power diagrams.*

Proof. See Appendix B. ■

This theorem follows from three lemmas which partially characterize an optimal districting plan and establish that these characteristics imply a power diagram. The first lemma shows that our objective function is equivalent to a variant of the k-means objective function. This is important because it allows one to focus attention on district centroids.

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to the classic graph partitioning problem, which is also known to be NP-hard.

The second lemma shows that any pair of districts are separated by a line perpendicular to a line connecting their centroids. This separating line is the locus of points at which the power of the two centroids are equal. It represents all points in which one is indifferent between placing voters in one district and the other. Finally, we establish that all such lines separating any three adjacent districts meet at a single point; they are concurrent.

To see that these properties imply a power diagram, recall that a power diagram is a set of lines dividing a euclidean space into a finite number of cells. The line separating two adjacent cells are such that the power of the points along this locus is equal to their respective centroids. And the power of a point is measured as a function of the difference between a point and the centroid of its district – which we have already established is equivalent to our objective function. It is important to note that if the line separating two adjacent districts was not perpendicular to the line connecting their centroids then one could not be indifferent between points being in one district and the other everywhere along the line. This holds for all such pairs of districts, which implies concurrent lines. Taken together, these imply that optimally compact districtings are power diagrams[17]. Notice, since all subsets of a convex set formed by drawing straight lines are convex, it follows that the resulting districts must be convex polygons.

Theorem 2 provides an important insight for building an algorithm, allowing us to use all we know about a partial characterization of optimally compact districts. There are three important caveats. First, we have not yet proven that there is a unique power diagram for every set of starting values. Second, we are only able to map optimal districting plans into power diagrams when distance is quadratic, because this guarantees that optimal districting involves straight lines. Mathematically, this is an obvious limitation. Practically, however, it boils down to assuming that courts punish outliers in a district more. Given this assumption, we are hard pressed to find a principled reason for courts to prefer higher order exponents.

Third, power diagrams do not guarantee a global optimum to the minimum partitioning problem because their structure depends on exogenously given starting values.

[insert figure 2]

Panel A of figure 2 depicts the optimally compact districting plan for a hypothetical state. There are nine voters, arranged so the state is a lattice. The stars represent centroids of the resulting districts. Note that the line separating districts 1 and 2 is perpendicular to a line connecting their centroids (the same is true for districts 1 and 3, and also 2 and 3). This is an illustration of the Perpendicular Line Lemma alluded to above. The Concurrent Line Lemma is also illustrated by the intersection of the lines separating districts 1,2 and 3 at a single point. The partition depicted

[17] Aurenhammer et al. (1998) prove a closely related theorem, taking squared distance from the centroid as the objective function. Their proof proceeds by showing that if an algorithm can be designed to find a power diagram then it is an optimal partition. By contrast, we provide a constructive proof based on the parallel and concurrent line lemmas. We could, of course, state our lemma on the equivalence of the objective functions and then appeal to their result, but our current proof provides more information about optimal districtings.

is indeed the globally optimal partition. Once one knows that, the centroids of the districts are easy to compute.

In our problem, however, we do not know the optimal districts in advance, and so we must choose generator points which will not in general be the centroids of the optimal districting plan. An important part of the approximation problem is selecting and improving upon the generator points. To illustrate this point, consider panel B of Figure 2 which chooses alternative generator points than those used to partition the panel A. The generator point used for district 1 differs from that used above resulting in four voters being placed in district 1 and only 2 in districting 2, thereby violating the equal size constraint.

4.3 An Algorithm Based on Power Diagrams

The algorithm we propose is a modification of the second algorithm presented in Aurenhammer et. al (1998). Since we know by Theorem 2 that local optima of the RPI are power diagrams, we search within the set of power diagrams for one that is a feasible districting. However, as power diagrams are generated around sites, which we call $z_1, \ldots, z_n$, it is necessary to update the locations of the sites as well as the design of the districts.

We provide a complete formal treatment in the appendix, and here give a heuristic description of the algorithm. The algorithm takes the centroids of existing districts as starting generator points and computes a power diagram. Power diagrams do not require partitions (cells) to be even roughly equal so, after constructing the diagram, the algorithm adjusts the district boundaries until the number of voters within each district is equal up to integer rounding. We then recalculate the centroids of the new districts and check to see if any pair of individuals can switch districts and reduce the objective function (total squared distances). The algorithm continues to check until there are no more pairs that can be switched and reduce the objective function by a pre-determined $\varepsilon > 0$. The algorithm then repeats itself – recalculating centroids, drawing power diagrams, adjusting boundaries, etc – until it reaches a value within preset bounds for a stopping rule.

4.4 The Compactness of Political Districting Plans of the 106th Congress

The ideal data to estimate the relative proximity index would contain the geographical coordinates of every household in the US, their political district, some measure of distance between any two households within a state, and a precise definition of communities of interest. This information is not available.

In lieu of this, we use tract-level data from the 2000 US Census from the Geolytics database which contains the latitude and longitude of the geographic centroid of each tract, the political district each centroid is in, and its total population.[18] Census tracts are small, relatively permanent

[18]For roughly 5,000 census tracts, information on congressional district was not provided. In these cases, we mapped the coordinates of the centroid of the tract and manually keypunched the congressional district to which it belonged.

statistical subdivisions of a county. The spatial size of census tracts varies widely depending on the density of settlement, but they do not cross county boundaries. Census tracts usually have between 2,500 and 8,000 persons and, when first delineated, are designed to be homogeneous with respect to population characteristics, economic status, and living conditions. The latter consideration is our main interest in using this level of aggregation (relative to blocks or block-groups), as census tracts are more likely to contain some notion of communities of interest.

An important consideration in the application of RPI is how to handle tracts of different densities. The equal representation constraint – districting plans must have the same number of individuals in each district up to integer rounding – is predicated on individuals, not tracts. Our algorithm, described below, addresses this issue by allowing one to divide tracts into arbitrarily small units. There is an important trade-off between computational burden and the variance in population across districts, a burden that lessens with technological progress.

For ease of implementation, we have chosen not to split any tracts. As a robustness check, we split tracts of small states into 4 smaller parts and assigned them to the same longitude and altered their latitude by 0.001 degrees. In all cases, accuracy (and computing time) were substantially increased with little effect on the RPI.

To calculate the RPI for each state, we begin with the numerator of the index: $\sum_{v \in V} \sum_{i,j \in v} (d_{ij})^2$, where i and j are population centroids of tracts and v are voting districts. We weight the total distances by the population density of each tract. An identical calculation is performed for the denominator, but V is constructed by our power diagram algorithm.

The empirical results we obtain on the compactness of districting plans are displayed in Table 1. The first column list each state, the second provides the relative proximity index, the third and fourth give the maximum deviation from equal partitions in the actual data and that resulting from our algorithm – an indication of the degree to which the equal size constraint holds. The final columns report the results from a bootstrapping technique which we describe below. It is important to realize that for every state, the elements of our partitions are more balanced than what appears in the actual districting plans. Further, the largest deviation from equal partitions in the actual data (Florida 0.46) is substantially larger than our largest deviation (California 0.22).

Table 1 illustrates that the five states with the most compact districting plans are Idaho, Washington, Arkansas, Mississippi, and New Hampshire. The five most compact states are Idaho, Nebraska, Arkansas, Mississippi, and Minnesota. The five least compact states are Tennessee, Texas, New York, Massachusetts, and New Jersey. The districting plan that solves the minimum partitioning problem is more than forty percent more compact than the typical districting plan. The rank correlation between the rRelative Proximity Index and the most popular indices of compactness, dispersion and perimeter, is -.37 and -.29, respectively.

Axiom III (invariance to scale, population density, and number of districts – see Appendix A) ensures that the RPI can be compared across states, but it does not guarantee that the distribution of RPI values across states are the same. It is entirely plausible that Texas finds it "easier" (a lower percentile of the distribution of RPI values from feasible partitions) to obtain a given value of RPI than say, Florida. Thus, gleaning an understanding of how "sensitive" RPI values are for a given state is difficult.

To try and address this issue, we calculated 200 RPI values for each state by randomly generating starting values for the algorithm. Columns 5 and 6 in Table 1 report the means and associated standard deviations from this process. The final column reports what percentile in the distribution our original RPI value lies, if the distribution of RPI values is assumed to be normal. In all but one case, our original estimates are higher than the mean of the simulated distribution and in most cases, under the normality assumption, we are at the far extreme of the right tail of the distribution. There are four notable exceptions: Oklahoma, Oregon, Rhode Island, and Wisconsin. In these states, our estimate of RPI is at the median or below in the simulated distribution. This is likely due to the fact that the current partitions of these states generate starting values that are highly non-optimal. To obtain maximal compactness in these states, a significant restructuring is likely needed.

To understand what state demographics are correlated with compactness, we estimate a state-level OLS regression where the dependent variable is the RPI and the independent variables are percent black, percent Asian, percent Hispanic, population density, difference in presidential vote shares between Democrats and Republicans, and whether or not the state is required to submit their districting plans to the Department of Justice under the preclearance provision of Section 5 of the Voting Rights Act. States which are more compact tend to be states with a larger share of blacks and a larger difference between the percent who vote Republican and Democrat. The latter is intuitive: states with more to gain from altering the design of political districts tend to do it more. Whether or not a state is forced to submit their districting plans is also highly correlated with compactness. Consistent with Axiom II (efficient clustering – see Appendix A), RPI is uncorrelated with population density.

Beyond the technical considerations, perhaps the best evidence in favor of our approach can be illustrated visually. Figures 3-11 present side-by-side comparisons of congressional district maps for actual districting plans and those obtained from our algorithm.[19] Figures 3 and 4 illustrate this comparison for the least and most compact states, Tennessee and Idaho, respectively. Tennessee, under the current districting plan, resembles the salamander-shaped districts drawn by Eldridge Gerry that gave rise to name "gerrymandering." Under the algorithm, however, Tennessee is transformed into a neat set of convex polygons. Idaho is at the other extreme. Because it need only cut the state into two equal parts, the existing cut and our preferred cut are very similar to one another. Further, our partition provides a more equitable distribution of voters across the districts, which explains why the calculate RPI is slightly less than one.

These figures illustrate three key points. First, the geometric properties discussed above (the perpendicular and concurrent line lemmas and the convexity of political districts) are immediately apparent. Second, those states which rank relatively high (resp. low) in terms of the RPI appear to quite different (resp. similar) to the partition resulting from our algorithm. Third, Figures 5 and 8

[19] A complete set of maps are available at http://www.economics.harvard.edu/faculty/fryer/fryer.html

(Hawaii and Nevada), suggest that communities of interest are an important consideration. In the actual plans, Honolulu and Las Vegas are their own districts while the rest of the state is contained in the other. The issues faced by residents of the outer islands might well be more similar than those of residents in Honolulu. This serves to highlight why compactness is only one factor which weighs on the redistricting question. RPI in its current implementation ignores this consideration. An RPI with a more general notion of distance or carefully selected starting values for the power diagram can address this issue.

5 Election Counterfactuals

Thus far, we have derived an index of compactness, shown how one implements the index, and provided some basic facts about the most and least compact districting plans and what correlates with these plans. We conclude our analysis with some suggestive evidence on the impact of maximally compact districting plans on election outcomes in four large states.

In winner-take-all election contests, such as elections for representatives for the U.S. Congress and for electoral votes for the U.S. Presidency, the winner of a contest is determined by which candidate receives the plurality of the votes. In most of these cases, only the top two parties need to be considered, yielding an easy condition for an election win in a district.

Assuming there are n districts, labeled $i \in [1, ..., n]$, let ϕ_i denote the proportion of the two-party vote received by the candidate from the first party (in examples to follow, the Democratic Party). The candidate's victory can then be expressed as $s_i = w_i \mathbb{I}(\phi_i > \frac{1}{2})$, where w_i denotes how many seats are determined by the vote; 1 for single-member districts, or 3 or more for the Electoral College, for example. Two important summary statistics are the average district vote, $\Phi = \frac{1}{n} \sum_{i=1}^{n} \phi_i$, and the seat share, $S = \frac{\sum_{i=1}^{n} s_i}{\sum_{i=1}^{n} w_i}$.

Many other statistics can be generated using the vote and seat outcomes directly, but we are particularly interested in partisan bias and responsiveness. Namely: $Bias = 2E(S|\Phi = 0.5) - 1$ estimates the deviation from the median share of seats if each side receives an identical average district vote; $Responsiveness = \frac{dS}{d\Phi}|\Phi$ estimates how a small shift in the average district vote would translate into a shift in the share of seats. This estimate is taken either at the observed average district vote or the median vote.

5.1 Data and Statistical Framework

We use voter tabulation district (VTD) level election return data from US elections of the 105th and 106th Congresses for four large states; California, New York, Pennsylvania, and Texas. These states were chosen because of their large number of congressional districts (roughly 30 or greater) and the availability of vote shares by VTD. There are approximately 300 VTDs in a typical congressional district, though there is substantial variation. In our data, for instance, California has 7,000 VTDs for 50 districts; Texas has 8,000 for 30. Pennsylvania has 9,000 for 20, and New York contains 13,000 for 30 districts.

The intuition behind our approach is straightforward. Consider Figure 9, which depicts the existing districting plan of New York and the plan derived from our algorithm. To fix ideas, concentrate on the western portions of the state. There are roughly 433 VTDs in each congressional district in New York. Suppose an election takes place. Currently, a congressional representative is chosen by aggregating the votes from the VTDs within each district. In Figure 9, this amounts to adding votes from roughly 433 voting centers in districts 27 through 31. Now, suppose we want to estimate how these representatives will change if the districting plan were drawn to maximize compactness. To do this, we simply take note of which VTDs are in the new partitions and aggregate within each new district. In short, we disaggregate down to the VTD level, take note of the new districting lines, and then aggregate up taking these boundaries into account. As before, the winner of the new districts (in Figure 9 this now amounts to district 4, 6, 8, and 17) is determined by aggregating the votes from VTDs.

There are a few complications. First, we need to assign candidates to the new districts in a reasonable manner. Second, we need to take into account the results of previous elections and whether or not the candidate is an incumbent – as both of these factors weigh heavily on the prediction of future elections. Third, we need to think about how to get standard errors on our estimates.

To formalize the intuition above, we employ techniques from elementary Bayesian statistics developed in Gelman and King (1994). We provide a terse synopsis of their approach below.[20] The crux of the Gelman-King method is a linear model with two distinct error components of the form:

$$\phi_i = X\beta + \gamma_i + \varepsilon_i. \tag{2}$$

The vector X consists of an intercept term, results from the previous election, and an incumbent dummy.

To derive precise predictions in this framework, more structure has to be placed on the error terms. Let $\gamma_i \sim N(0, \sigma_\gamma^2)$ represent the systematic error component; an expression of the unobserved variables that took place before the election campaign began and would be identical if the election were to be re-run again. This might include the result in the previous election, the race of the candidates, or a relevant change in election law. The unpredictability of the behavior of voters is also a source of systematic error.

The second source of error is a random component which can be explained by random events during the election, such as the weather on election day or the reaction of the public to an unintentional gaffe. Let $\varepsilon_i \sim N(0, \sigma_\varepsilon^2)$.

There are two key assumptions in the Gelman-King Method. First, errors are expressed in terms of two parameters: σ^2, the sum of the individual variances σ_γ^2 and σ_ε^2, and λ, the proportion of the total variance attributed to the systematic component; $\lambda = \sigma_\gamma^2/(\sigma_\gamma^2 + \sigma_\varepsilon^2)$. Second, the counterfactual assumes that the regrouping of voters into new districts will not have a systematic effect on voting behavior.

[20] For more details, see Gelman and King (1994).

Estimating λ and σ^2

In practice, a districting map is constant over a series of elections. Thus, λ and σ^2 are found by taking the mean of individual estimators from each year. In each year, σ^2 is the variance of the random error term in Equation (2) and λ, the fraction of the error attributed to systematic error, is estimated by including the results of the previous election as an explanatory variable in the current one. By calculating this for each election that did not follow a redistricting (i.e. where the electoral map is identical), and taking the mean, we have an estimator for λ.[21]

Generating Hypothetical Future Elections

To predict the properties of a subsequent election using the same districting plan, a series of hypothetical elections are simulated using the estimates for β and σ^2. A new set of explanatory variables X is used to demonstrate the conditions at the election. Since no information can be derived about the nature of the systematic error component beforehand, one error term is used, $\omega = \gamma + \varepsilon$, with variance σ^2. Thus, a single hypothetical election is then generated by drawing from

$$\phi_{hyp} = \mathbf{X}_{hyp}\beta + \delta_{hyp} + \omega \tag{3}$$

where β is the posterior distribution, with mean $\widehat{\beta} = (X'X)^{-1}X'\phi$ and (with a normality assumption) variance $\Sigma_\beta = \sigma^2(X'X)^{-1}$. The δ term is used to produce hypothetical elections whose average district vote is desired to be different from the original. Integrating out the conditional parameters β and γ one obtains the marginal distribution:

$$\phi_{hyp}|\phi \sim N(\lambda\mathbf{v} + (\mathbf{X}_{hyp} - \lambda\mathbf{X})\widehat{\beta} + \delta, (\mathbf{X}_{hyp} - \lambda\mathbf{X})\Sigma_\beta(\mathbf{X}_{hyp} - \lambda\mathbf{X})'^2)\sigma^2 I).$$

To evaluate the election system, let $\mathbf{X}_{hyp} = \mathbf{X}$; to evaluate under counterfactual conditions, set $\mathbf{X}_{hyp}$ to the desired explanatory variables.

Comparing Districting Plans

With the above statistical model in hand, we can predict elections under different partitions of a state into voting districts. The procedure is as follows. First, we estimate the model in equation (2). Second, having generated a new map through our algorithm, we determine the values for the explanatory variables for each district, either by aggregating and averaging the previous values in each precinct or by making sensible predictions for their value (e.g. incumbency). In terms of vote shares, we simply aggregate the VTDs in the new partitions. For incumbency, we assign each incumbent to the latitude and longitude of the centroid of their district. Under the new districting plan, if there is one such incumbent per district, s/he becomes the incumbent. In the rare cases where there was more than one incumbent assigned to a district under a new districting plan, we break the tie by choosing the incumbent closest to the resulting centroid and replacing another district with the other incumbent to keep the numbers constant. Finally, with our new map we

[21] Ideally, one would have historical votes for many years to tease out the systematic error component. We have only two years of such data.

simulate the model 1000 times; deriving the relevant parameters is straightforward.

5.2 Analyzing Seat-Vote Curves

Using the methodology described above, Figures 13-16 provide seat-vote curves for California, New York, Pennsylvania, and Texas under each state's actual districting plan and the plan that maximizes its compactness. The vertical axis depicts the proportion of seats won by democrats. The horizontal axis depicts the share of votes that the democrats earned in the election. Each figure reports two interesting quantities: Vote is the average district vote the Democrats received in the election; and Seats report the fraction of seats the Democrats received in the election (not the hypothetical seat share). The dark line represents our estimate of the seat vote curve, the two parallel lines around it are 95% confidence intervals. Visually, one can see that there is a marked difference between the seat-vote curves estimate, from the actual data and those estimated from the partition developed by our algorithm, in California and New York. The slope of the curve is significantly steeper in both these states. Texas and Pennsylvania are also slightly steeper, but the difference is much less dramatic.

To get a better sense of the magnitudes involved, Table 2 presents our estimates of Bias and Responsiveness for the actual partition of our four states and those gleaned from the algorithm. We also report the t-statistic on the difference between them. Under maximally compact districting, measures of bias are slightly smaller in all states except Pennsylvania, though none of the differences are statistically significant. In terms of responsiveness, however, there are large and statistically significant differences between the existing partitions and those that are maximally compact. New York, in particular, has a five fold increase; from .482 to 2.51. In other words, under the current partition, a 1% increase in vote share for Democrats results in a .482% increase in seats under the current system. When maximally compact, however, a 1% increase results in a 2.51% increase. The next largest change is California - increasing from 1.086 to 1.731. Pennsylvania and Texas show smaller increases, which are statistically significant at the 10% level.

6 Concluding Remarks

There will be continued debate about the design of districting plans. We have developed a simple but principled measure of compactness. Our measure can be used to compare districting plans across state and time, a feature not found in existing measures, and our algorithm provides a way of approximating the most compact plan. Further, the impact a maximally compact districting plan can have on the responsive of votes is encouraging. These are first steps toward a more scientific understanding of districting plans and their effects. Extensions and generalizations abound.

Perhaps the most obvious extension is to consider higher dimensional spaces, generalized distance functions, and communities of interest. Aurenhammer and Klein (2000) provide a comprehensive survey of Voronoi Diagrams and how to incorporate generalized notions of distance, including p-norms, convex and "airlift" distances, and non-planar spaces. These extensions are not only mathematically interesting and elegant, they have real-world content. Consider the following thought experiment. Suppose there is a city on a hill.[22] On the West side is mild, long incline toward the rest of the city, which is in a plane. On the East side is a steep cliff, either impassable or with just a narrow, winding road that very few people use. While the next residential center to the East is much closer to the hilltop on a horizontal plane, it is much further on all sorts of distances that we think might matter: transportation time, intensity of social interactions, sets of shared local public goods and common interests, etc. Thus, for all practical purposes, one probably wants to include the hilltop in a Western district rather than an Eastern one. More general notions of distance can handle this. A similar situation arises when there is a "natural" boundary (river or highway, e.g.) that effectively segregates / reduces communication between two population centers that are geographically very close. Conversely, there could be something (e.g., a tunnel or subway) that makes two non-connected regions effectively close to each other or, there may be other notions of communities and shared interest that lend themselves to a natural clustering. It is imperative to note that the derivation of our index only assumed a general metric space – many of these ideas fit squarely within our framework. The empirical application of the index, however, required us to only consider Euclidean distances. The challenge ahead is to incorporate more general notions of distance into an empirically tractable algorithm.

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7 Technical Appendix

7.1 Appendix A: An Axiomatic Derivation of the Relative Proximity Index

We now describe three properties which any compactness index should satisfy and discuss each in turn. We provide formal mathematical statements of these in the appendix.

Axiom I (Anonymity) Axiom I, an anonymity condition in the same spirit as that typically used in social choice theory (Arrow, 1970), requires that all individuals be treated equally. That is, any compactness index should not depend on the particular identities (race, political affiliation, wealth, etc.) of voters. Consider a state S with associated partition V and compactness index $c(V, S)$. For any bijection $h : S \to S$ and compactness index $c_h(V, S)$, $c_h(V, S) = c(V, S)$.

Axiom II (Clustering)

Compactness is fundamentally a mathematical partitioning problem; deciding who to group with whom in a political district. Clustering is the quintessential objective (Bartal, Charikar, and Raz, 2001).[23] Our second axiom requires that if two states with the same number of voters, voting districts, and the same value for the minimum partitioning problem have different weighted intra-district distances, then the state with the larger value is less compact.

Let $\gamma_k = \sum_{i,j \in v} \alpha_{ij} \left(d_{ij}\right)^{\delta}$, for $k = \{1, ..., n\}$ and let $g\left(\gamma_1, ..., \gamma_n\right) : \mathbb{R}^n \to \mathbb{R}$ be a monotonic, increasing function. Consider two states, S_1 and S_2 and partitions V and V' respectively such that S_1 and S_2 have: the same number of voters, the same number of districts and

$$\min_{V \in \mathcal{V}_{S_1}} g_{S_1}\left(\gamma_1, ..., \gamma_n\right) = \min_{V \in \mathcal{V}_{S_2}} g_{S_2}\left(\gamma_1, ..., \gamma_n\right).$$

Then

$$g_{S_1}\left(\gamma_1, ..., \gamma_n\right) > g_{S_2}\left(\gamma_1, ..., \gamma_n\right) \Longrightarrow c\left(V, S_1\right) > c\left(V', S_2\right).$$

Density independence means that if we replicate a state by multiplying the number of people in each household by λ, the index of compactness is unaltered. For instance, when comparing two voting districts (Cambridge, MA, and New York, NY, e.g.) who differ in their population density, the index provides the same cardinal measure of compactness.

Scale independence provides a similar virtue, permitting comparisons across states that differ in the distances between individuals (Massachusetts and Texas, say), allowing one to increase the distances between all individuals in a state by a constant with no resulting change in the index. Independence with respect to the number of districts is also vital in making cross-state comparisons.

Axiom III (Independence)

[23] Other common objectives are distance from the geographic centroid of each partition or distance from a representative (typically the center of a cluster and not necessarily the center of the partition).

Our final axiom requires that any measure of compactness of a state be insensitive to its physical size, population density, and number of districts. This is vital for making cross-state comparisons of districting plans. Before stating the property formally, we need some further notation. We say that a state $\widehat{S}$ is an n-Replica of S if and only if $\forall i \in S, \exists j_1, ..., j_n \in \widehat{S}$ such that $d_{ij} = 0, \forall i$ and $d_{j_i j_k} = 0, \forall i, k$. It is also useful to have a shorthand for the realized value of the minimum partitioning problem. Consider two partitions of state S, V and V' with ρ and ρ' elements respectively. Let $V_S^{\min_\rho}$ and $V_S^{\min_{\rho'}}$ be the respective minimizing partitions.

Consider $S, \widehat{S} \in S$ with cardinality $|S|$ and $\left|\widehat{S}\right|$ respectively.

1. *(Scale) If $d_{ij} = \lambda d_{ij}$, for all $i, j \in S, \widehat{S}$. Then* $c(V, S) = c(V, \widehat{S})$, for all V.
2. *(Density) If $\left|\widehat{S}\right| = \lambda |S|$ and $\widehat{S}$ is a λ-replica of S then* $c(V, S) = c(V, \widehat{S})$, for all V.
3. *(Number of Districts)*

$$\text{If } \frac{\sum_{v \in V_S^\rho} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2}{V_S^{\min_\rho}} = \frac{\theta \sum_{v \in V_S^{\rho'}} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2}{V_S^{\min_{\rho'}}} \Longrightarrow c(V, S) = \theta c(V', S).$$

7.1.1 Uniqueness Result

Let $O_c = (\mathbb{R}_+, \succeq)$ denote the ordered set generated by the relative proximity index c, and let $O_{\widehat{c}}$ denote the ordered set over elements $V_S \in \mathcal{V}_S$ generated by any other compactness index. We say that two indices, c and $\widehat{c}$, are ordinally isomorphic if $O_c = O_{\widehat{c}}$. We are now equipped to state our main result. The proof of this, as with all others, can be found in Appendix A.

Theorem 1 *(1) The Relative Proximity Index satisfies Anonymity, Clustering, and Independence; (2) Suppose $\delta = 2$ and $g_{S_i}(\cdot)$ is symmetric for all i, then any compactness index which satisfies Anonymity, Clustering and Independence is ordinally isomorphic to the Relative Proximity Index.*

Proof of Theorem 1, Part 1:

That the RPI satisfies the three axioms follows from five simple lemmas which we now state and prove.

Lemma 1 *The Relative Proximity Index satisfies Anonymity.*

Proof. Consider a partition V of state S and an associated compactness index $c(V, S)$. Now consider a bijection $h : S \to S$.

$$\sum_{v \in V_S} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2$$

is unchanged since h is a bijection and hence there are the same number of points in each element of V and they are at the same points. For identical reasons the denominator of the RPI does not change, and hence $c(V, S) = c_h(V, S)$ for any bijection h. ■

Lemma 2 *The Relative Proximity Index satisfies Clustering.*

Proof. Let there be two partitions, V_S^1 and $V_{S'}^2$ such that

$$\sum_{v \in V_S^1} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2 > \sum_{v \in V_{S'}^2} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2 \tag{4}$$

Clustering requires:

$$c(V_S^1, S) > c(V_S^2, S)$$

Suppose, by way of contradiction, that (4) holds, and

$$c(V_1, S) < c(V_2, S). \tag{5}$$

That is

$$\frac{\sum_{v \in V_S^1} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2}{\min_{V \in \mathcal{V}_S} \sum_{v \in V} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2} < \frac{\sum_{v \in V_S^2} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2}{\min_{V \in \mathcal{V}_S} \sum_{v \in V} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2} \tag{6}$$

The denominators are identical and hence the supposition requires:

$$\sum_{v \in V_S^1} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2 < \sum_{v \in V_{S'}^2} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2, \tag{7}$$

a contradiction. ■

Lemma 3 *The Relative Proximity Index satisfies Density Independence.*

Proof. Consider S and $\widehat{S}$, with $|S|$ and $\left|\widehat{S}\right|$ respectively with $\widehat{S}$ a λ-replica of S. We need to show that $RPI\,(V, S) = RPI(V, \widehat{S})$ for all $V \in \mathcal{V}_S, V \in \mathcal{V}_{\hat{S}}$. That is

$$\frac{\sum_{v \in V_S} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2}{\min_{V \in \mathcal{V}_S} \sum_{v \in V} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2} = \frac{\sum_{v \in V_{\hat{S}}} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2}{\min_{V \in \mathcal{V}_{\hat{S}}} \sum_{v \in V} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2},$$

for all $V \in \mathcal{V}_S, V \in \mathcal{V}_{\hat{S}}$. By the definition of a λ-replica, the right-hand side of the above equation is simply

$$\frac{\lambda \sum_{v \in V_S} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2}{\lambda \min_{V \in \mathcal{V}_S} \sum_{v \in V} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2},$$

which is clearly equal to the left-hand side for any partition. ■

Lemma 4 *The Relative Proximity Index satisfies Scale independence.*

Proof. Scale Independence requires that for two states, S and $\widehat{S}$ with $d_{jk} = \lambda d_{jk}$, for all $j, k \in S, \widehat{S}$. Then $c(V, S) = c(V, \widehat{S})$, for all $V \in \mathcal{V}_S, V \in \mathcal{V}_{\hat{S}}$. That is

$$\frac{\sum_{v \in V_S} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2}{\min_{V \in \mathcal{V}_S} \sum_{v \in V} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2} = \frac{\sum_{v \in V_{\hat{S}}} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2}{\min_{V \in \mathcal{V}_S} \sum_{v \in V} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2},$$

for all $V \in \mathcal{V}_S, V \in \mathcal{V}_{\hat{S}}$. Scale independence means that the right-hand side of the above equation is simply

$$\frac{\sum_{v \in V_S} \sum_{i \in v} \sum_{j \in v} (\lambda d_{ij})^2}{\min_{V \in \mathcal{V}_S} \sum_{v \in V} \sum_{i \in v} \sum_{j \in v} (\lambda d_{ij})^2} = \frac{\lambda^2 \sum_{v \in V_S} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2}{\lambda^2 \min_{V \in \mathcal{V}_S} \sum_{v \in V} \sum_{i \in v} \sum_{j \in v} d_{ij}^2}.$$

which is clearly equal to the left-hand side for any partition. ■

Lemma 5 *The Relative Proximity Index satisfies Number of Districts independence.*

Proof. Follows immediately from the definition of independence with respect to number of districts. ■

We can now prove the second part of Theorem 7.1.1. It is proved by transforming a given state so that it can be compared to another state. Anonymity and Independence ensure that this can be done in a way which does not alter the compactness index, and Clustering then allows a comparison of two districting plans to be made based on their total intra-cluster pairwise distances.

Proof of Theorem 1, Part 2.

Proof. From part 1 we have $RPI(V, S_m) > RPI(\hat{V}, S_n) \Rightarrow c(V, S_m) > c(\hat{V}, S_n)$, for any m, n. Suppose part 2 is not true. This implies that

$$c(V, S_m) > c\left(\hat{V}, S_n\right) \text{ and } RPI(V, S_m) < RPI\left(\hat{V}, S_n\right), \tag{8}$$

or

$$c(V, S_m) < c\left(\hat{V}, S_n\right) \text{ and } RPI(V, S_m) > RPI\left(\hat{V}, S_n\right),$$

for some m, n.

If $S_m = S_n$ then the argument is straightforward. Begin with the first pair of inequalities. Note that Equality implies that $\mu_{ij} = \mu$ for all i, j and that symmetry of g implies combined with Equality implies that g is additively separable in its arguments. Then by Equality and Clustering we have

$$\sum_{v \in V_{S_m}} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2 > \sum_{v \in \hat{V}_{S_n}} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2 \Longrightarrow c(V, S_m) > c\left(\hat{V}, S_n\right),$$

since $RPI(V, S_m) < RPI\left(\hat{V}, S_n\right)$ and

$$S_m = S_n \Rightarrow \min_{V \in \mathcal{V}_{S_m}} \sum_{v \in V_{S_m}} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2 = \min_{V \in \mathcal{V}_{S_n}} \sum_{v \in \hat{V}_{S_n}} \sum_{i \in v} \sum_{j \in v} (d_{ij})^2,$$

we have

$$\sum_{v\in V_{S_m}} \sum_{i\in v} \sum_{j\in v} (d_{ij})^2 < \sum_{v\in \hat{V}_{S_n}} \sum_{i\in v} \sum_{j\in v} (d_{ij})^2 .$$

By Clustering this implies that $c(V, S_m) < c\left(\hat{V}, S_n\right)$–a contradiction. Identical reasoning rules out the case where

$$c(V, S_m) < c\left(\hat{V}, S_n\right) \text{ and } RPI(V, S_m) > RPI\left(\hat{V}, S_n\right).$$

Now consider the case in which $S_m \neq S_n$, and suppose that S_m contains γ_m districts and S_n contains γ_n districts. Consider the following transformation of state n. First, make a $\lambda-$replica of S_n and a $\mu-$replica of S_m so that the number of voters is the same as in state the transformed S_m. Note that $c(V, S_m)$ and $RPI(V, S_m)$ are unchanged due to Independence. In a slight abuse of notation we will continue to use V and S_m in reference to the $\mu-$replicated state. Second, expand or contract the state in the sense that the distance between any two points, d_{ij} say, in state S_n is αd_{ij} in state $S_{n'}$. Note that any partition of state n is a well defined partition of state $S_{n'}$ as it contains the same voters, scaled by α. Choose α such that

$$\alpha = \frac{|n| \min_{V\in \mathcal{V}_{S_n}^{\gamma_m}} \sum_{v\in \hat{V}_{S_n}} \sum_{i\in v} \sum_{j\in v} (d_{ij})^2}{\mu\,|m| \min_{V\in \mathcal{V}_{S_m}} \sum_{v\in V_{S_m}} \sum_{i\in v} \sum_{j\in v} (d_{ij})^2},$$

where $|n|$ and $|m|$ are the number of voters in states S_n and S_m respectively, and the γ_m superscript denotes a partition into γ_m elements. Note that

$$\min_{V\in \mathcal{V}_{S_m}} \sum_{v\in V_{S_m}} \sum_{i\in v} \sum_{j\in v} (d_{ij})^2 = \min_{V\in \mathcal{V}_{S_{n'}}^{\gamma_m}} \sum_{v\in V_{S_{n'}}} \sum_{i\in v} \sum_{j\in v} (d_{ij})^2 . \tag{9}$$

Third, select a feasible partition of $S_{n'}$ with γ_m elements, and denote this partition $\hat{V}'$. Suppose

$$\sum_{v\in \hat{V}'_{S_{n'}}} \sum_{i\in v} \sum_{j\in v} (d_{ij})^2 = \theta \sum_{v\in \hat{V}_{S_n}} \sum_{i\in v} \sum_{j\in v} (d_{ij})^2 ,$$

and that

$$\min_{V\in \mathcal{V}_{S_n}^{\gamma_m}} \sum_{v\in \hat{V}_{S_n}} \sum_{i\in v} \sum_{j\in v} f(d_{ij}) = \beta \min_{V\in \mathcal{V}_{S_n}^{\gamma_n}} \sum_{v\in \hat{V}_{S_n}} \sum_{i\in v} \sum_{j\in v} f(d_{ij}) .$$

Hence

$$\frac{\sum_{v\in \hat{V}'_{S_{n'}}} \sum_{i\in v} \sum_{j\in v} (d_{ij})^2}{\min_{V\in \mathcal{V}_{S_n}^{\gamma_m}} \sum_{v\in \hat{V}_{S_n}} \sum_{i\in v} \sum_{j\in v} (d_{ij})^2} = \frac{\theta}{\beta} \frac{\sum_{v\in \hat{V}_{S_n}} \sum_{i\in v} \sum_{j\in v} (d_{ij})^2}{\min_{V\in \mathcal{V}_{S_n}^{\gamma_n}} \sum_{v\in \hat{V}_{S_n}} \sum_{i\in v} \sum_{j\in v} (d_{ij})^2}$$

By Independence

$$c\left(\hat{V}', S_{n'}\right) = \frac{\theta}{\beta} c\left(\hat{V}, S_n\right)$$

and

$$RPI\left(\hat{V}', S_{n'}\right) = \frac{\theta}{\beta} RPI\left(\hat{V}, S_n\right).$$

From (8)

$$c\left(V, S_m\right) > \frac{\beta}{\theta} c\left(\hat{V}', S_{n'}\right) \text{ and } RPI\left(V, S_m\right) < \frac{\beta}{\theta} RPI\left(\hat{V}', S_{n'}\right). \tag{10}$$

But since S_m and $S_{n'}$ have the same number of voters, the same number of districts, and (9) holds, it follows that (10) implies that c violates Clustering.

Identical reasoning rules out the case where

$$c\left(V, S_m\right) < c\left(\hat{V}, S_n\right) \text{ and } RPI\left(V, S_m\right) > RPI\left(\hat{V}, S_n\right),$$

and hence the proof is complete. ■

7.2 Appendix B: Proofs and Description of Algorithm

7.2.1 Proof of Theorem 2

Let districts of state S be denoted $D_1, \ldots, D_d$. A districting plan is *feasible* if $|D_i| = n$ for all $i \in \{1, \ldots, d\}$. The set of feasible districtings is $\mathcal{V}$. Let the centroid of district D_i be m_i, so $m_i = \frac{1}{n}\sum_{x \in D_i}(x)$. Define the functions:

$$\psi(D_i) = \sum_{x \in D_i} \|x - m_i\|^2, \qquad \Psi(D_1, \ldots, D_d) = \sum_{i=1}^{d} \psi(D_i)$$

We say that districting is optimally compact if it minimizes $\Psi(D_1, \ldots, D_d)$ over all $(D_1, \ldots, D_d) \in \mathcal{V}$. For $z_1, \ldots, z_d \in \mathbb{R}^2$, let:

$$\psi_{z_i}(D_i) = \sum_{x \in D_i} \|x - z_i\|^2, \qquad \Psi_{z_1, \ldots, z_d}(D_i) = \sum_{i=1}^{d} \psi_{z_i}(D_i)$$

A Power Diagram with sites $z_1, \ldots, z_d$ is a partition of $\mathbb{R}^2$ into districts $D_1, \ldots, D_d$ such that for fixed constants $\lambda_1, \ldots, \lambda_d \in \mathbb{R}$,

$$D_i = \left\{ q \in \mathbb{R}^2 : i = \arg\min_j \left[\|q - z_j\|^2 - \lambda_j\right] \right\}$$

It is clear that a power diagram is described by its edges and the fact that if x is on the same side as D_i of any complete set of linear separators between D_i and other districts then $x \in D_i$, and otherwise not. The edges of D_i are described by the set of $q \in \mathbb{R}^2$ such that $\|q - z_i\|^2 - \lambda_i = \|q - z_i\|^2 - \lambda_j$, or $\|q - z_i\|^2 - \|q - z_i\|^2 = \lambda_i - \lambda_j$.

Lemma 6 *$\Psi(D_1,\ldots,D_d)$ is proportional to the RPI for $(D_1,\ldots,D_d)\in\mathcal{V}$, so minimizing one is equivalent to minimizing the other. Specifically,*

$$\sum_{i=1}^{d}\sum_{x\in D_i}\sum_{y\in D_i}\|x-y\|^2 = 2n\sum_{i=1}^{d}\sum_{x\in D_i}\|x-m_i\|^2 .$$

Proof of Lemma 6.

$$\begin{aligned}
\sum_{i=1}^{d}\sum_{x\in D_i}\sum_{y\in D_i}\|x-y\|^2 &= \sum_{i=1}^{d}\sum_{x\in D_i}\sum_{y\in D_i}\left(\|x\|^2+\|y\|^2-2x\cdot y\right)\\
&= \sum_{i=1}^{d}\sum_{x\in D_i}\left(n\|x\|^2-2nm_i\cdot x+\sum_{y\in D_i}\|y\|^2\right)\\
&= \sum_{i=1}^{d}\left(\sum_{x\in D_i}\left(n\|x\|^2-2nm_i\cdot x\right)+n\sum_{y\in D_i}\|y\|^2\right)\\
&= \sum_{i=1}^{d}\left(\sum_{x\in D_i}\left(2n\|x\|^2-2nm_i\cdot x\right)\right)\\
&= \sum_{i=1}^{d}\left(2n\sum_{x\in D_i}\left(\|x\|^2-m_i\cdot x\right)\right)\\
&= \sum_{i=1}^{d}2n\left(\sum_{x\in D_i}\left(\|x\|^2\right)-n\|m_i\|^2\right)\\
&= \sum_{i=1}^{d}\left(2n\left(\sum_{x\in D_i}\left(\|x\|^2-2m_i\cdot x+\|m_i\|^2\right)\right)\right)\\
&= \sum_{i=1}^{d}\left(2n\left(\sum_{x\in D_i}\|x-m_i\|^2\right)\right)\\
&= 2n\sum_{i=1}^{d}\sum_{x\in D_i}\|x-m_i\|^2
\end{aligned}$$

■

Lemma 7 *For all $(D_1,\ldots,D_d)\in\mathcal{V}$,*

$$(m_1,\ldots,m_d)=\arg\min_{(z_1,\ldots,z_d)}\Psi_{z_1,\ldots,z_d}(D_1,\ldots,D_d)$$

Proof of Lemma 7. It suffices to show that substituting m_i for z_i minimizes the expression on the right. Its first order condition with respect to the z_i is:

$$\forall D_i, \quad 2\sum_{x\in D_i}(x - z_i) = 0 \quad \Rightarrow \quad z_i = \frac{1}{n}\sum_{x\in D_i} x = m_i$$

■

Lemma 8 *In an optimally compact districting, every pair of adjacent districts is separated by a line perpendicular to a line connecting their centroids.*

Proof of Lemma 8. Let $(D_1, \ldots, D_d)$ be optimally compact. Without loss of generality we can prove the lemma for districts D_1 and D_2. By isometry we can assume that $m_1 = (0,0)$ and $m_2 = (\xi, 0)$. Pick $v_1 = (x_1, y_1) \in D_1$ and $v_2 = (x_2, y_2) \in D_2$. Let $D_1' = D_1 \cup \{v_2\} - \{v_1\}$ and $D_2' = D_2 \cup \{v_1\} - \{v_2\}$. By the optimality of $(D_1, \ldots, D_d)$ and the optimality lemma,

$$\psi(D_1) + \psi(D_2) \le \psi(D_1') + \psi(D_2') \le \psi_{m_1}(D_1') + \psi_{m_2}(D_2')$$

$$\Rightarrow \quad \|v_1 - m_1\|^2 + \|v_2 - m_2\|^2 \le \|v_1 - m_2\|^2 + \|v_2 - m_1\|^2$$

$$\Rightarrow \quad -2v_1 \cdot m_1 - 2v_2 \cdot m_2 \le -2v_1 \cdot m_2 - 2v_2 \cdot m_1$$

$$\Rightarrow \quad (v_2 - v_1) \cdot (m_1 - m_2) \le 0$$

$$\Rightarrow \quad (x_2 - x_1) \cdot (-\xi) + (y_2 - y_1) \cdot (0) \le 0$$

$$\Rightarrow \quad x_1 \le x_2$$

Since v_1 and v_2 are arbitrary, we can pick them such that v_1 is the point in D_1 with greatest x_1 and v_2 is the point in D_2 with least x_2, showing that there is a line of the form $x = c$ for $c \in \mathbb{R}$ separating the two districts. Isometrics preserve perpendicularity, so applying one moving m_1 and m_2 away from $(0,0)$ and $(\xi, 0)$ leaves the separator between D_1 and D_2 perpendicular to the segment connecting m_1 and m_2. ■

Lemma 9 *Let $(D_1, \ldots, D_d)$ be optimal. For every three districts, there exist three concurrent lines each of which separates two of the three districts, with one line separating each pair of districts.*

Proof of Lemma 9. Without loss of generality we prove this for the three districts D_1, D_2, and D_3. By the Straight Line Lemma, there exist linear separators between D_1 and D_2, D_2 and D_3, and D_3 and D_1 perpendicular to the lines connecting their centroids. We can characterize these lines by the equations $\|r - m_1\|^2 - \|r - m_2\|^2 = \mu_{1,2}$, $\|s - m_2\|^2 - \|s - m_3\|^2 = \mu_{2,3}$, and $\|t - m_3\|^2 - \|t - m_1\|^2 = \mu_{3,1}$, for free variables $r, s, t \in \mathbb{R}^2$. If the lines are concurrent, that means there exist $q \in \mathbb{R}^2$ satisfying all three equations. Adding them together gives $\mu_{1,2} + \mu_{2,3} + \mu_{3,1} = 0$. Therefore, if the lines are concurrent then for all r, s, and t on the lines,

$$\|r - m_1\|^2 - \|r - m_2\|^2 + \|s - m_2\|^2 - \|s - m_3\|^2 + \|t - m_3\|^2 - \|t - m_1\|^2 = 0$$

Assume there is no choice for $\mu_{1,2}$, $\mu_{2,3}$, and $\mu_{3,1}$ such that the lines are concurrent. Then, for all r, s, and t on the three edges,

$$\|r - m_1\|^2 - \|r - m_2\|^2 + \|s - m_2\|^2 - \|s - m_3\|^2 + \|t - m_3\|^2 - \|t - m_1\|^2 \neq 0$$

If any one of $\mu_{1,2}$, $\mu_{2,3}$, or $\mu_{3,1}$ induces an optimal separator at both the values ν_1 and ν_2 in $\mathbb{R}^2$, then it must also at the value $\lambda\nu_1 + (1-\lambda)\nu_2$ for $\lambda \in [0,1]$. So the expression above is either strictly greater or strictly less than 0 for all permissible values of r, s, and t. We assume without loss of generality that it is greater. Then, there exist $v_1 \in D_1$, $v_2 \in D_2$, and $v_3 \in D_3$ such that when substituted for r, s, and t, respectively, the above expression reaches a positive infimum. The expression cannot be at an infimum unless the extreme values of r, s, and t are specifically chosen to be in D_1, D_2, and D_3, respectively, otherwise $\|r - m_1\|^2 - \|r - m_2\|^2$, for example, could be decreased by moving r in the direction $m_1 - m_2$ while still separating D_1 and D_2. Therefore,

$$\|v_1 - m_1\|^2 - \|v_1 - m_2\|^2 + \|v_2 - m_2\|^2 - \|v_2 - m_3\|^2 + \|v_3 - m_3\|^2 - \|v_3 - m_1\|^2 > 0$$

$$\Leftrightarrow \|v_1 - m_1\|^2 + \|v_2 - m_2\|^2 + \|v_3 - m_3\|^2 > \|v_1 - m_2\|^2 + \|v_2 - m_3\|^2 + \|v_3 - m_1\|^2$$

Let $D_1' = D_1 \cup \{v_3\} - \{v_1\}$, $D_2' = D_2 \cup \{v_1\} - \{v_2\}$, and $D_3' = D_3 \cup \{v_2\} - \{v_3\}$. Then,

$$\psi(D_1) + \psi(D_2) + \psi(D_3) > \psi_{m_1}(D_1') + \psi_{m_2}(D_2') + \psi_{m_3}(D_3') > \psi(D_1') + \psi(D_2') + \psi(D_3')$$

This contradicts the optimality of $D_1, \ldots, D_d$, and the lemma follows.

■

Proof of Theorem 4.2. We prove that any optimal districting is a power diagram with cites equal to their centroids, $m_1, \ldots, m_d$. For any pair of districts D_i and D_j, we can pick $\mu_{i,j}$ such that $\|q - m_i\|^2 - \|q - m_j\|^2 = \mu_{i,j}$ is a linear separator between the districts, and if we add a third district D_j, we can similarly pick $\mu_{j,k}$ and $\mu_{k,i}$ such that the districting lines are concurrent, or $\mu_{i,j} + \mu_{j,k} + \mu_{k,i} = 0$. Note that $\mu_{a,b} = -\mu_{b,a}$. We prove that there exist constants $\lambda_1, \ldots, \lambda_d$ such that $\lambda_i - \lambda_j = \mu_{i,j}$ by induction. This is obviously true when $n = 2$. Assume it is true for districts $D_1, \ldots, D_k$. For $i, j < k+1$,

$$\mu_{i,k+1} = \mu_{i,j} + \mu_{j,k+1} = \lambda_i - \lambda_j + \mu_{j,k+1}$$

$$\Rightarrow \lambda_i - \mu_{i,k+1} = \lambda_j - \mu_{j,k+1}$$

Thus, $\lambda_i - \mu_{i,k+1}$ is constant over choice of i, call the constant λ_{k+1}. That makes $\mu_{i,k+1} = \lambda_i - \lambda_{k+1}$ for any i, and the induction is complete. Clearly any $x \in D_i$ is on the m_i side of a boundary line between D_i and another district, so it follows that optimal districtings are power diagrams. ■

7.2.2 Algorithm Details

The algorithm we propose is a modification of the second algorithm presented in Aurenhammer et. al (1998). Since we know by Theorem 2 that local optima of the RPI are power diagrams, we search within the set of power diagrams for one that is a feasible districting. However, as power diagrams are generated around sites, which we call $z_1, \ldots, z_n$, it is necessary to update the locations of the sites as well as the design of the districts.

First we explain the (Aurenhammer et al, 1998) algorithm for finding a power diagram which minimizes $\Psi_{z_1,\ldots,z_d}(D_1, \ldots, D_d)$ with $|D_i| \approx n$ for all i. Since a power diagram is defined by its sites and their weights, $\lambda_1, \ldots, \lambda_d$, assuming fixed sites each district D_i is a function of $\lambda_1, \ldots, \lambda_d$, or $D_i = D_i(\lambda_1, \ldots, \lambda_d)$. We suppress this dependence for simplicity. Let

$$\xi(\lambda_1, \ldots, \lambda_d) = \sum_{i=1}^{d}(n - |D_i|) \cdot \lambda_i + \Psi_{z_1,\ldots,z_d}(D_1, \ldots, D_d).$$

Aurenhammer et al, (1998) simplifies the problem by continuing as if each D_i does not change locally with respect to each λ_i everywhere, as this is true almost everywhere (at all but finitely many points). Therefore, $|D_i|$ and $\Psi_{z_1,\ldots,z_d}(D_1, \ldots, D_d)$ are locally constant with respect to λ_i, so,

$$\frac{\partial \xi}{\partial \lambda_i} = n - |D_i|.$$

Let $\Lambda = (\lambda_1, \ldots, \lambda_d)$. Using some choice of Λ_0, we can update it by gradient descent,

$$\Lambda_{t+1} = \Lambda_t + \epsilon_t \cdot \nabla \xi(\Lambda_t).$$

In our implementation we set Λ_0 to be the zero vector. It remains to pick the step sizes $\{\epsilon_t\}_{t \geq 0}$. To do this, one first determines an overestimate of the minimum value of ξ, call it $\overline{\xi}$. This can be done by setting $\overline{\xi} = \Psi_{z_1,\ldots,z_d}(D_1, \ldots, D_d)$ for any feasible districting $(D_1, \ldots, D_d)$. We use the notation $D_i(\Lambda_t)$ to mean one of the districts induced by the power diagram weights contained in the vector Λ_t, and let:

$$\epsilon_t = \frac{\overline{\xi} - \xi(\Lambda_t)}{\sum_{i=1}^{d} |D_i(\Lambda_t)|^2}$$

This step size is iterated until the minimum is either reached or missed, which happens when

$\sum_{i=1}^{d} |D_i(\Lambda_t)| \cdot |D_i(\Lambda_{t+1})| > 0$. Then, $\bar{\xi}$ is updated by solving the equation:

$$\frac{\bar{\xi} - \xi(\Lambda_t)}{\sum_{i=1}^{d} |D_i(\Lambda_t)|^2} = \frac{\bar{\xi} - \xi(\Lambda_{t+1})}{\sum_{i=1}^{d} |D_i(\Lambda_{t+1})|^2}$$

ϵ_{t+1} is chosen accordingly. This algorithm is repeated until the $|D_i|$'s are within some predetermined error bound around n.

Once optimal districts $D_1, \ldots, D_d$ for sites $z_1, \ldots, z_d$ are chosen, by Lemma 7 (see Appendix A) the function $\Psi_{z_1,\ldots,z_d}(D_1, \ldots, D_d)$ is improved by moving the z_i's to the centroids of the D_i's and keeping the $\lambda_1, \ldots, \lambda_d$ constant. Yet, all of the D_i's are not necessarily of size n, so they need to be adjusted by the above procedure. This process is repeated until moving the $z_1, \ldots, z_d$ still leaves the sizes of the D_i's within the prescribed error bound.

Note: The algorithm described in Aurenhammer et al. (1998) tends to fail when one of the districts is randomly set to size 0. Our solution to this issue was to move z_i to a random new location if $|D_i|$ became zero during any point in the process. Random new locations were chosen using a uniform distribution function ranging from the minimum to the maximum of the longitude and the latitude of the state in question.

7.3 Appendix C: A Guide to Programs

All programs to compute feasible districtings minimizing the RPI are written for MATLAB. There are two main programs, Main.m and Compute_Index.m, and support programs District.m, getRandGP.m, Psi.m, Weighted_Assign.m, Weighted_FirstTryAssign.m, and Weighted_PowerDiagram.m. We briefly describe each below.

Main.m and Compute_Index.m are both shell programs which call District.m, the actual algorithm, and store its output in text files. Typing Compute_Index(filename, Iterations) reads demographic data about a state from a text file, say 'indiana.out', and creates a new districting Iterations times. The file should have the latitudes and longitudes of the census tracts of the states in columns two and three (respectively), the FIPS code of the state repeated in every entry of column four, the current districts of all census tracts in column five, and the populations of all census tracts in column six. Compute_Index.m generates two output files. The first, in this case 'indiana.out.output' contains the latitudes and longitudes of the census tracts in the first two columns, and their new district numbers in the subsequent columns. Each column after the second represents a different iteration of the algorithm. The second output file, in this case 'indiana.out.stats', contains statistics from each iteration of the algorithm on a different row. The first column has the RPI's, the second has the accuracy of the districting, and the third has the accuracy of the current districting. Accuracy is measured:

$$\max_{i \in \{1,\ldots,d\}} \left| \frac{|D_i| - n}{n} \right|$$

Compute_Index.m has the following hard-coded parameters which are passed to District.m:

outside_tol_ratio, tol_ratio, outside_bail, and bail. tol_ratio and bail are the stopping criteria for the sub-routine Weighted_Assign.m which creates the best districting around randomly-initiated sites. If the accuracy falls below tol_ratio or the number of iterations of the gradient-descent procedure rises above bail, the algorithm terminates. Likewise, outside_tol_ratio and outside_bail are the stopping criteria for the larger districting algorithm. If the accuracy of the districting falls below outside_tol_ratio or the number of times the sites are moved rises above outside_bail, the algorithm terminates. The set values for outside_tol_ratio, tol_ratio, outside_bail, and bail are .9 times the real accuracy, whichever is the lesser between .9 times the real accuracy or .05, 35 times the number of districts in the state, and 35 times the number of districts in the state.

Main(filename) reads a list of states and iterations for each state to be run by Compute_Index. The file is of the form:
states, bootstraps
alabama 4
arizona 7
arkansas 3
california 1
Names of states and numbers of iterations are separates by tabs. If 'arizona' is written in this file, Compute_Index will open a file called 'arizona.out'. Main.m creates an additional file called index.txt which lists the FIPS code for every state next to the best RPI the algorithm has found for it such that the accuracy for the districting corresponding to that RPI is better than the state's current accuracy.

This procedure yields an RPI > 1 and an accuracy better than the current accuracy nearly all of the time for all states other than Connecticut, Idaho, Minnesota, and Nebraska, which already are well-districted and usually require quite a few bootstraps to improve on the current districting.

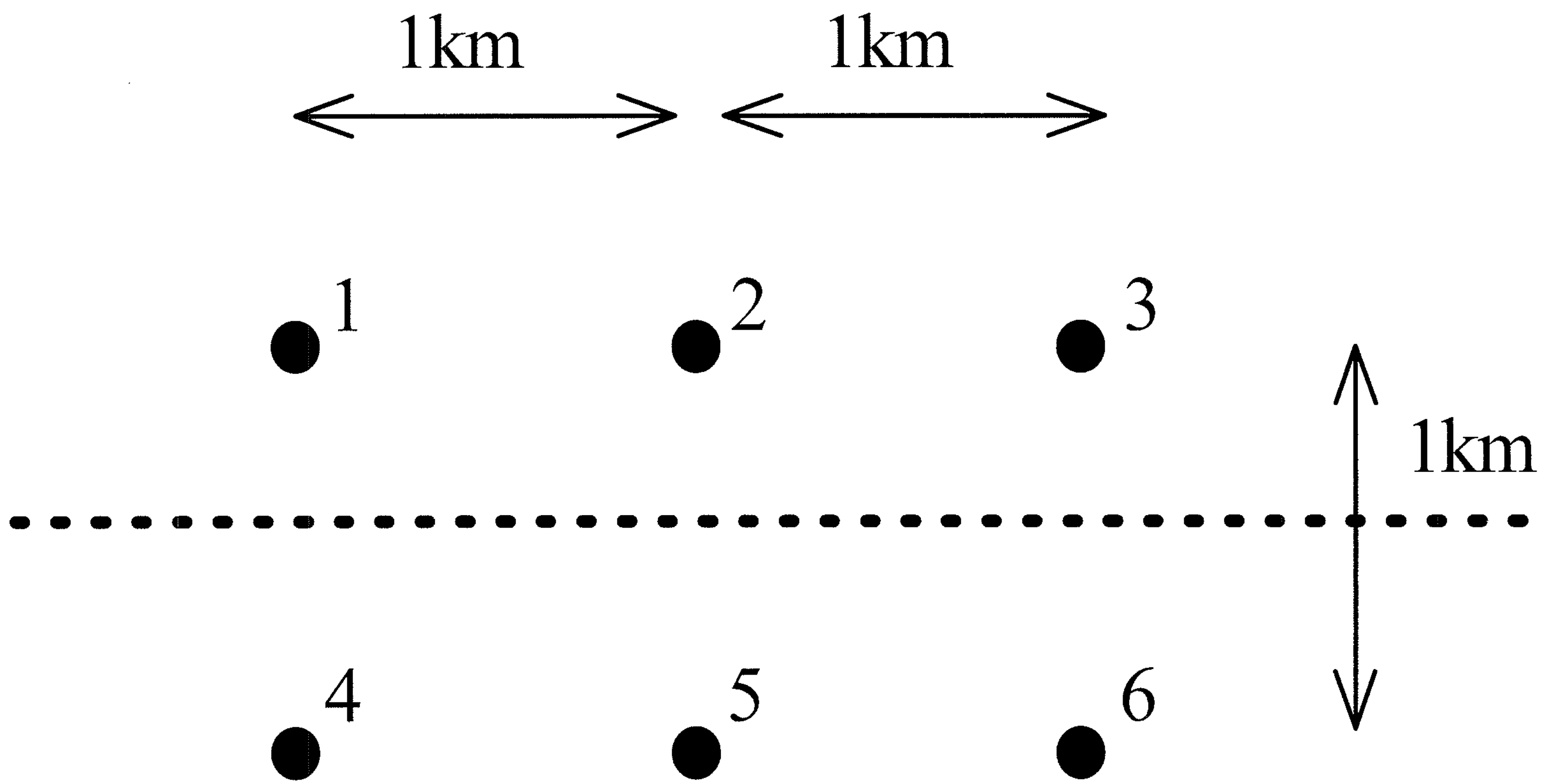

Figure 1: A Simple Example

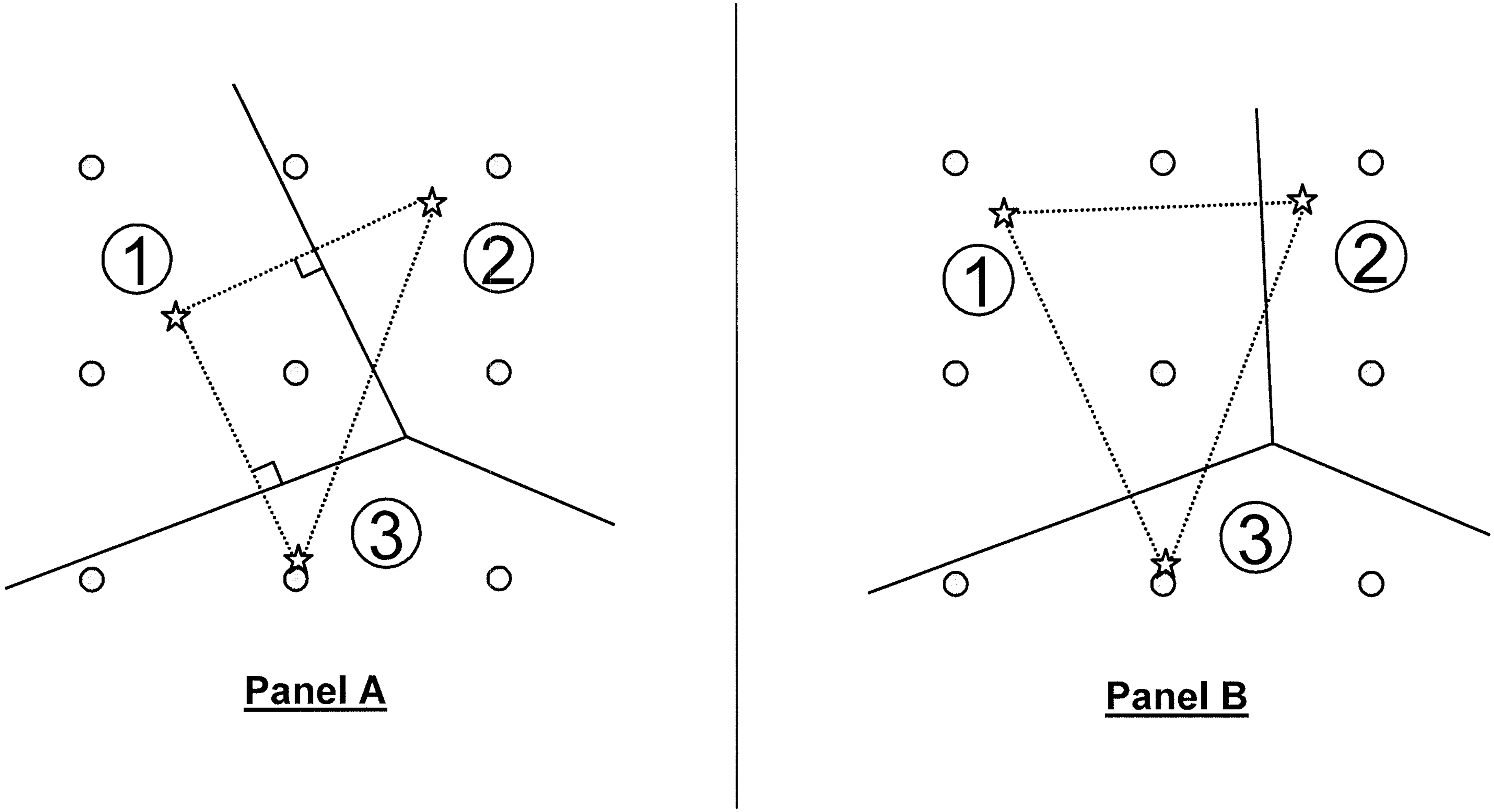

Figure 2: Good and Bad Generator Points

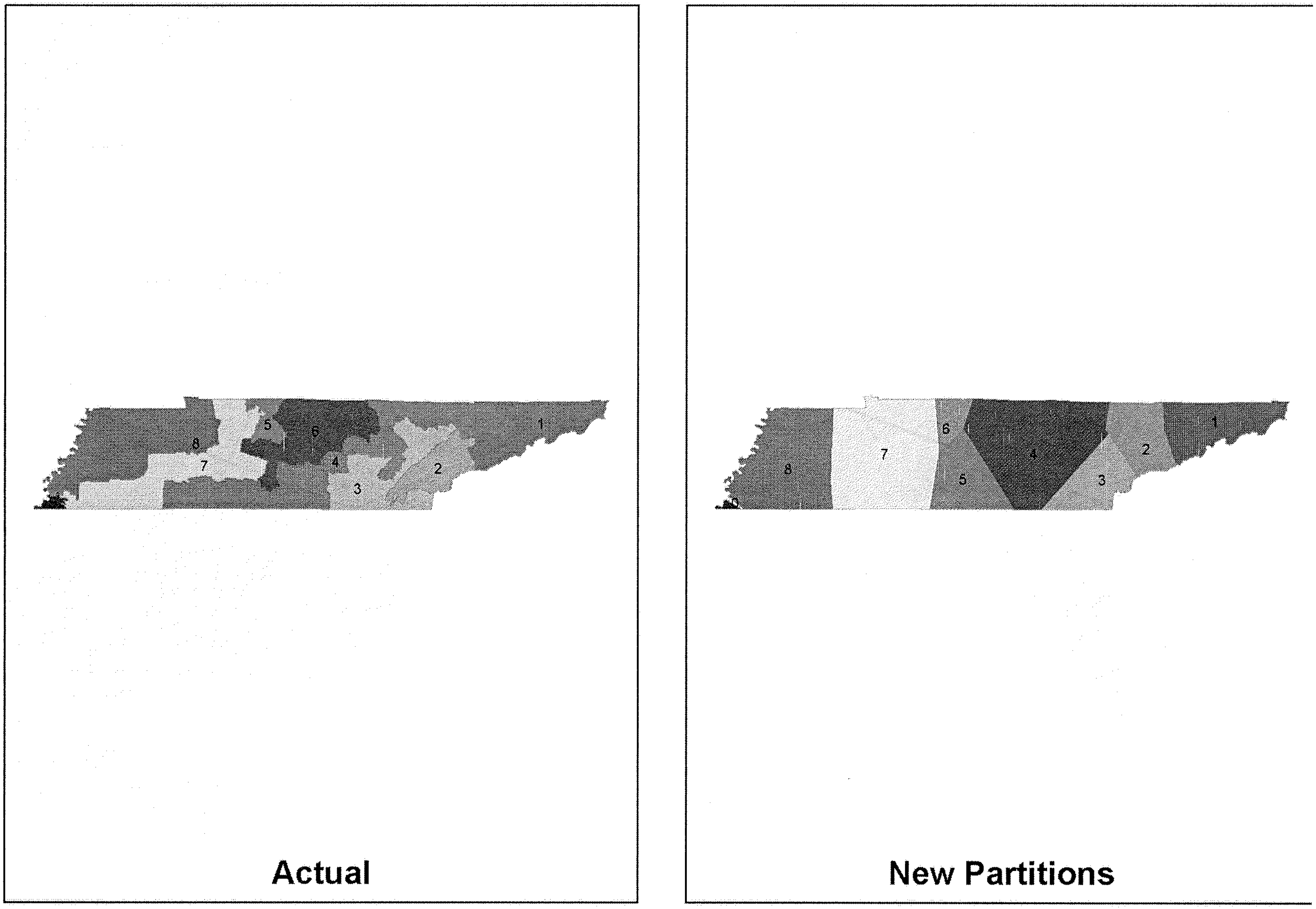

Figure 3: Tennessee 106th Congress Districting Plans, Actual v. Algorithm

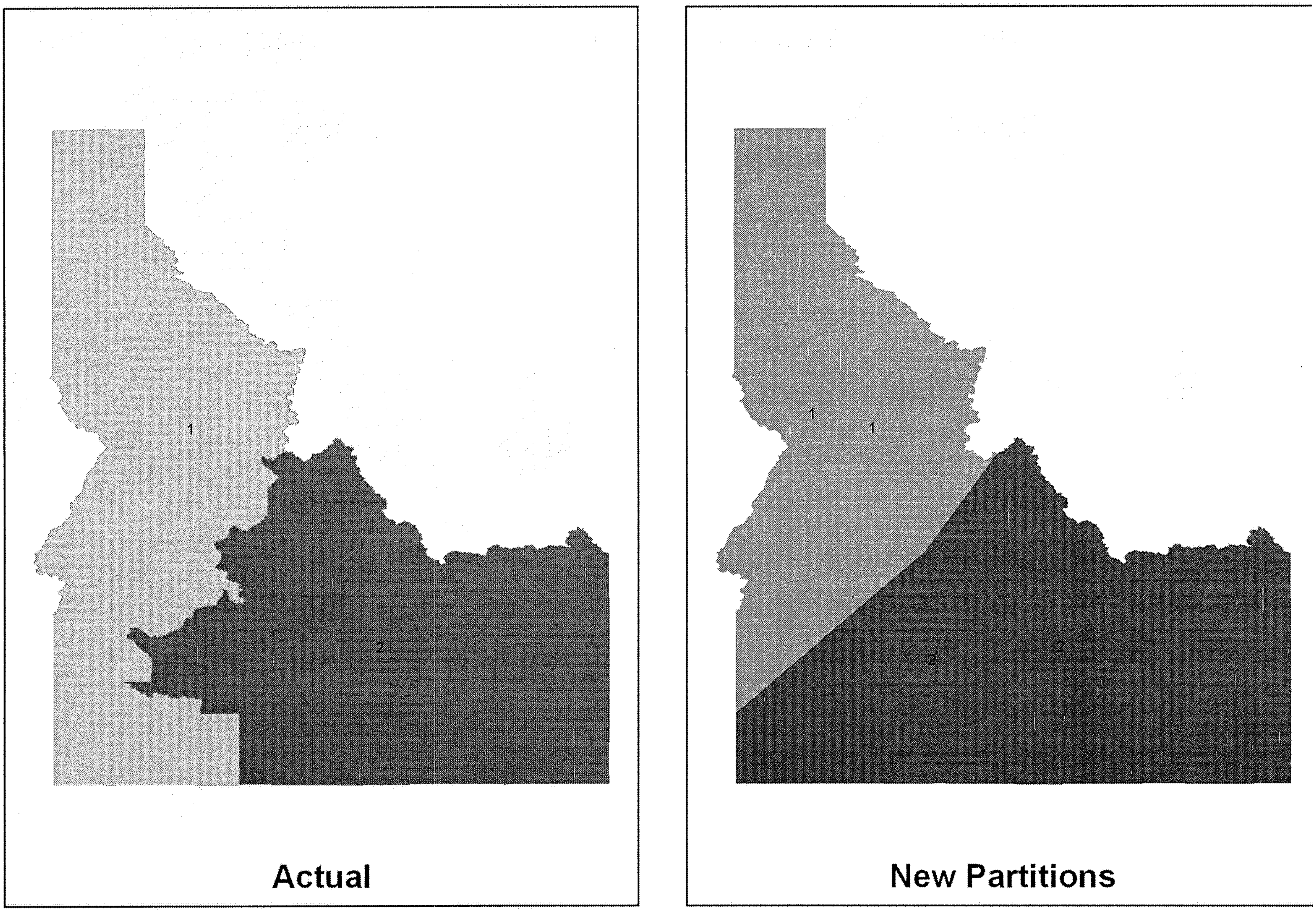

Figure 4: Idaho 106th Congress Districting Plans, Actual v. Algorithm

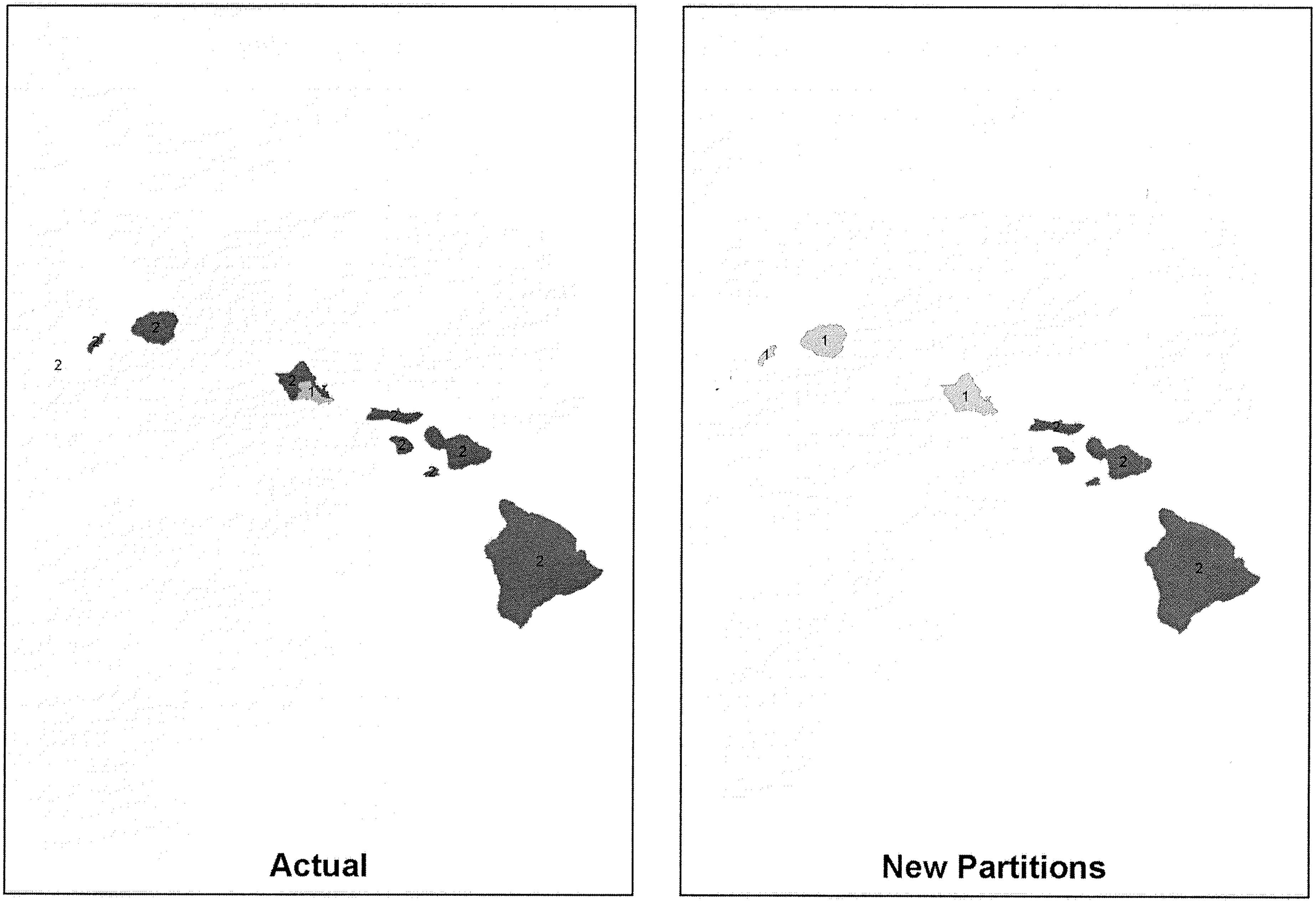

Figure 5: Hawaii 106th Congress Districting Plans, Actual v. Algorithm

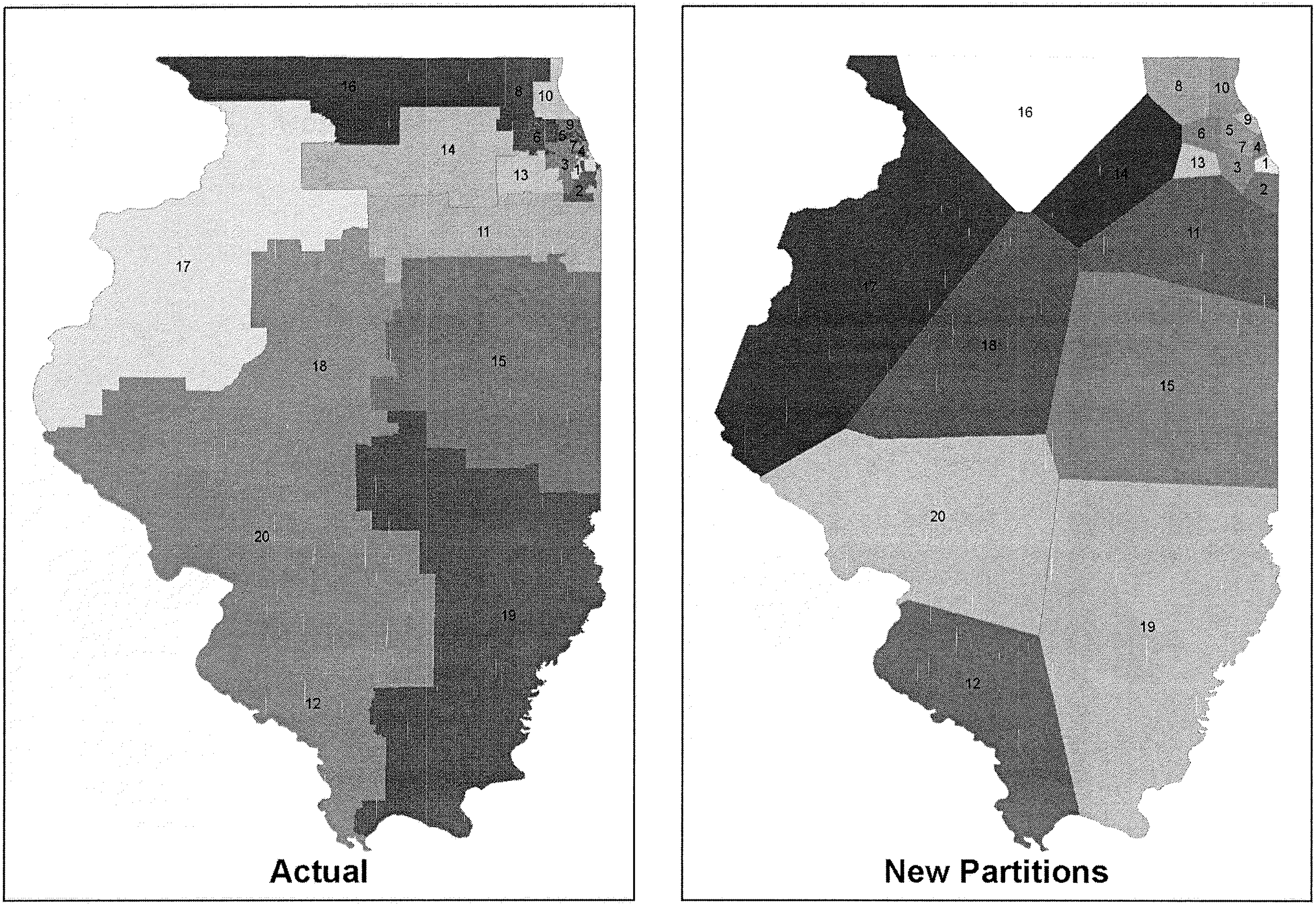

Figure 6: Illinois 106th Congress Districting Plans, Actual v. Algorithm

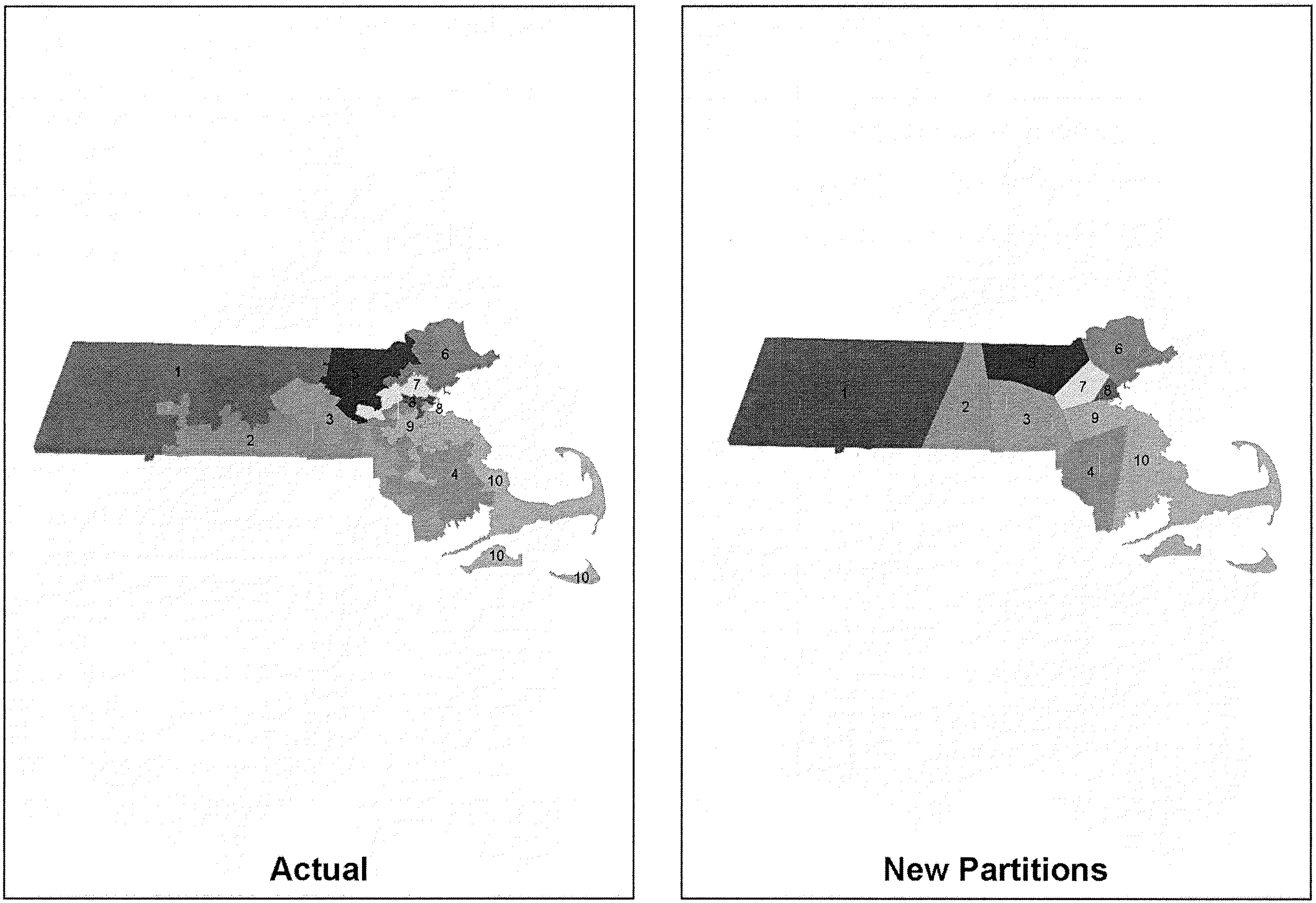

Figure 7: Massachusetts 106th Congress Districting Plans, Actual v. Algorithm

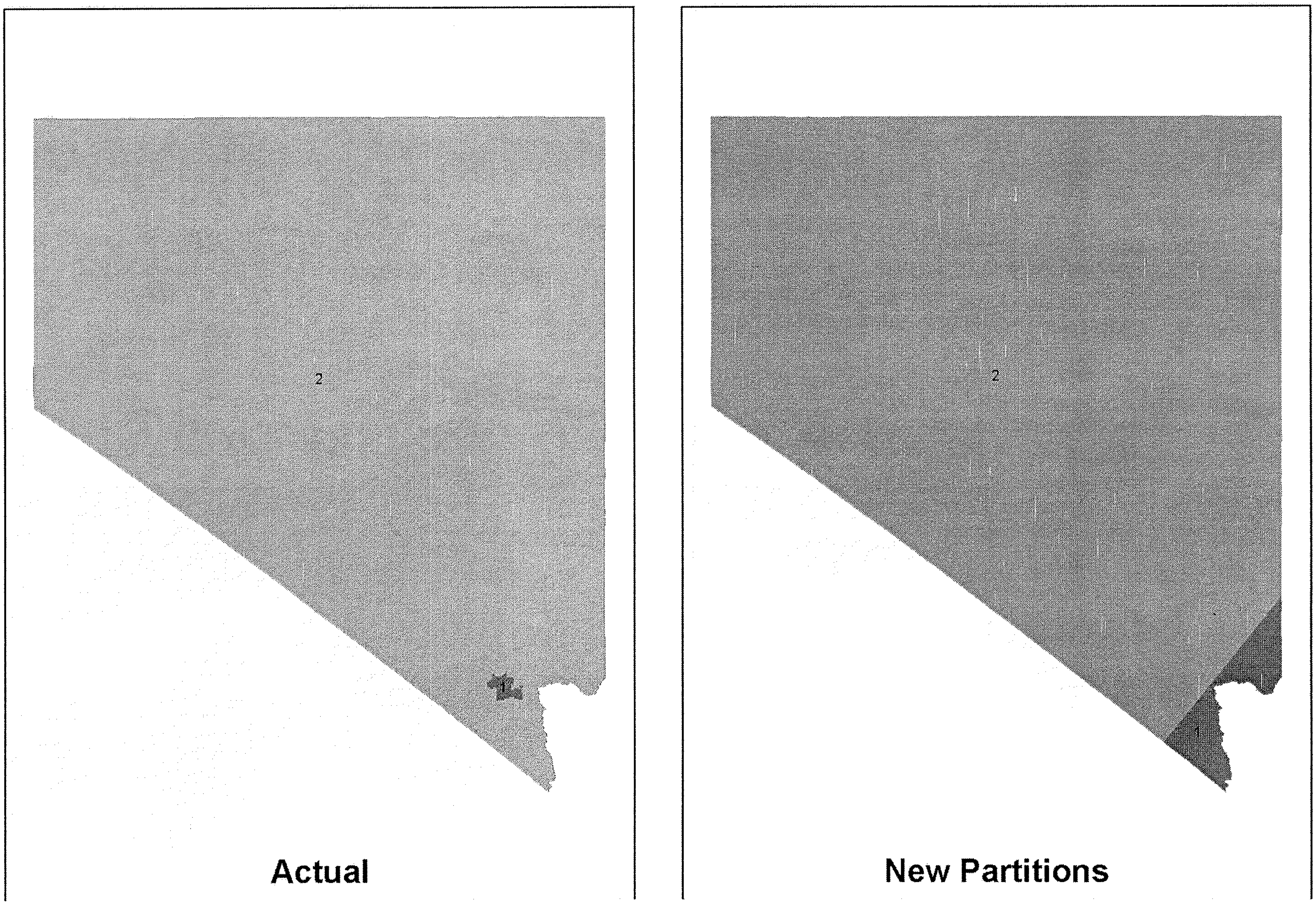

Figure 8: Nevada 106th Congress Districting Plans, Actual v. Algorithm

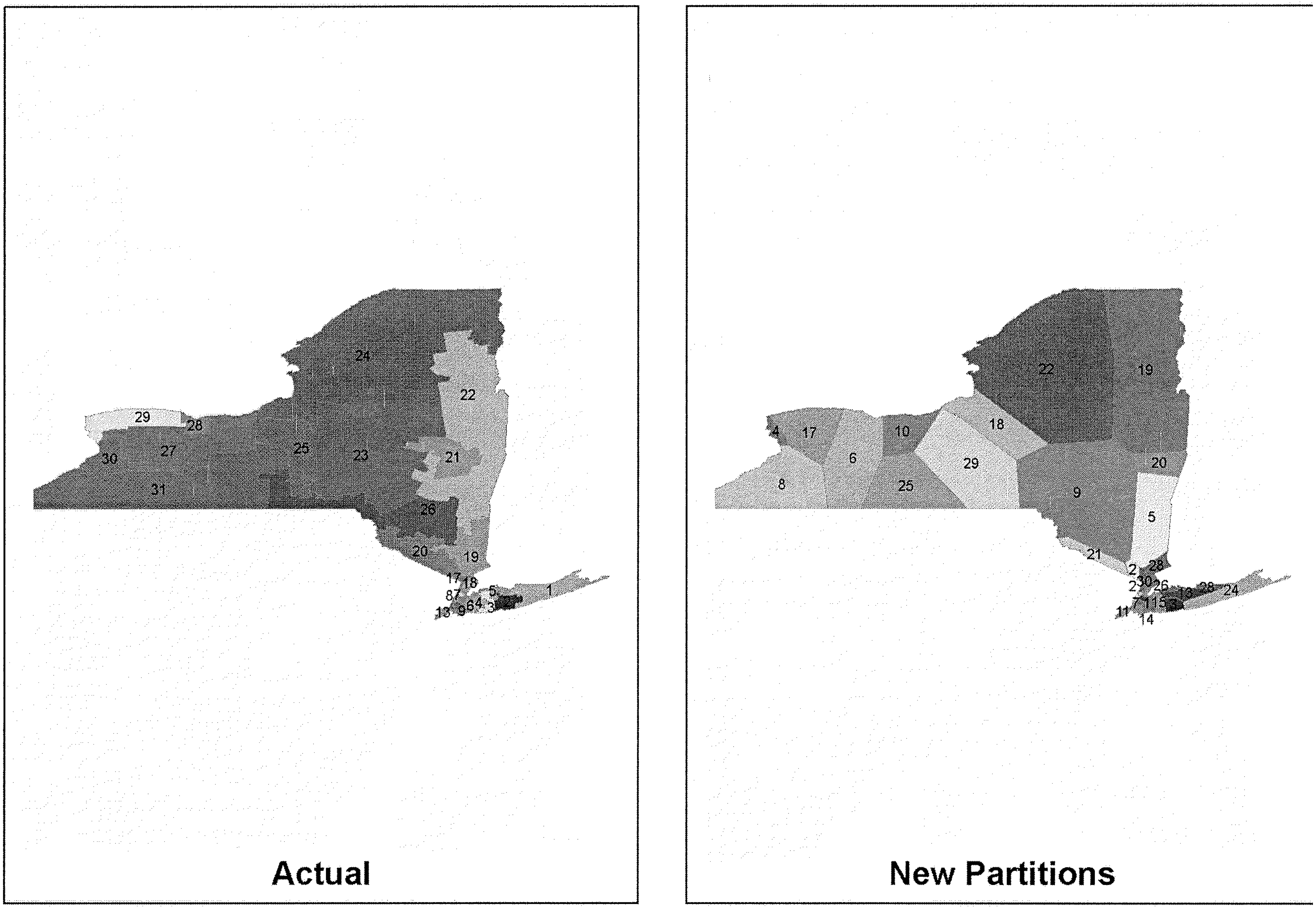

Figure 9: New York 106th Congress Districting Plans, Actual v. Algorithm

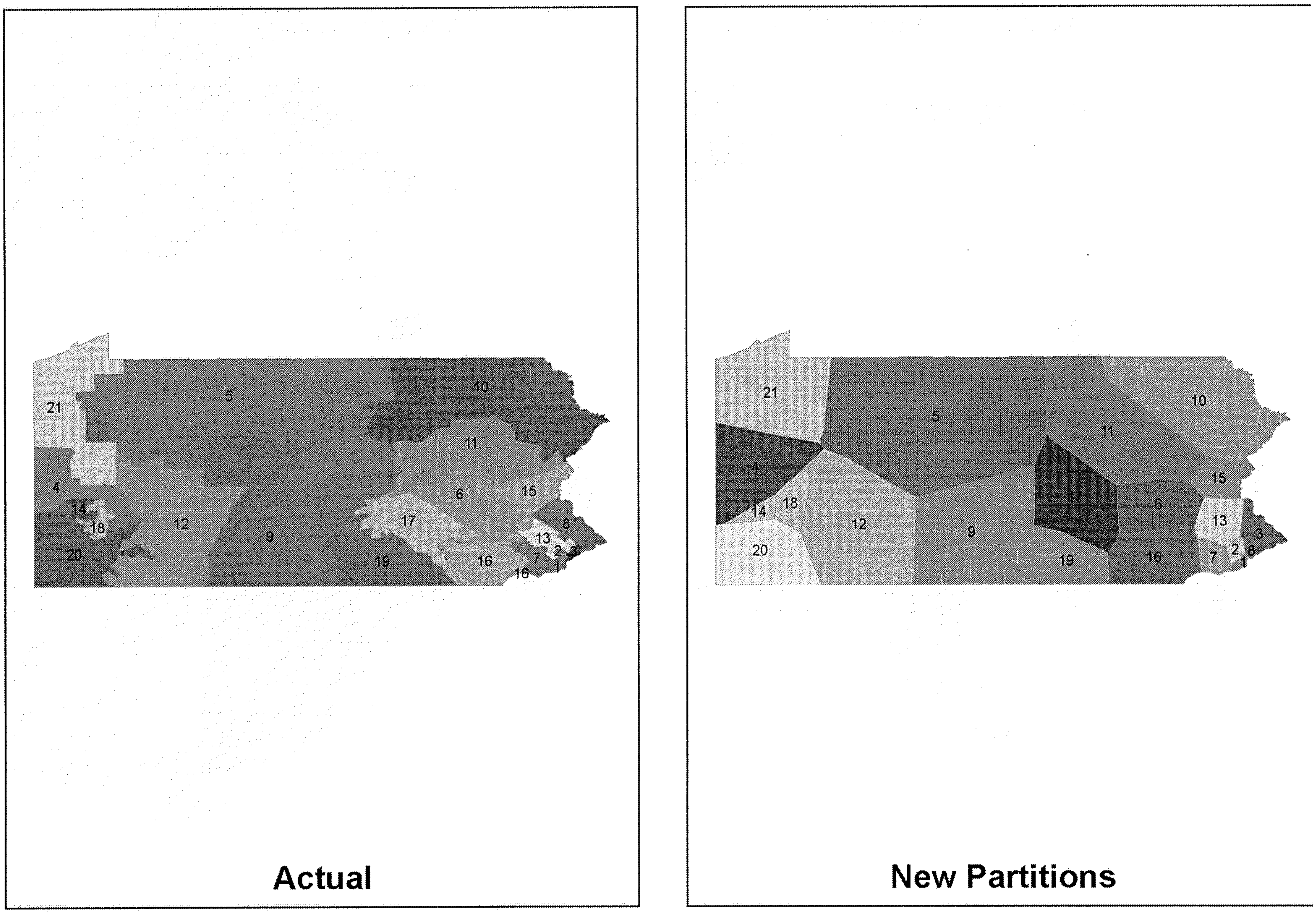

Figure 10: Pennslyvania 106th Congress Districting Plans, Actual v. Algorithm

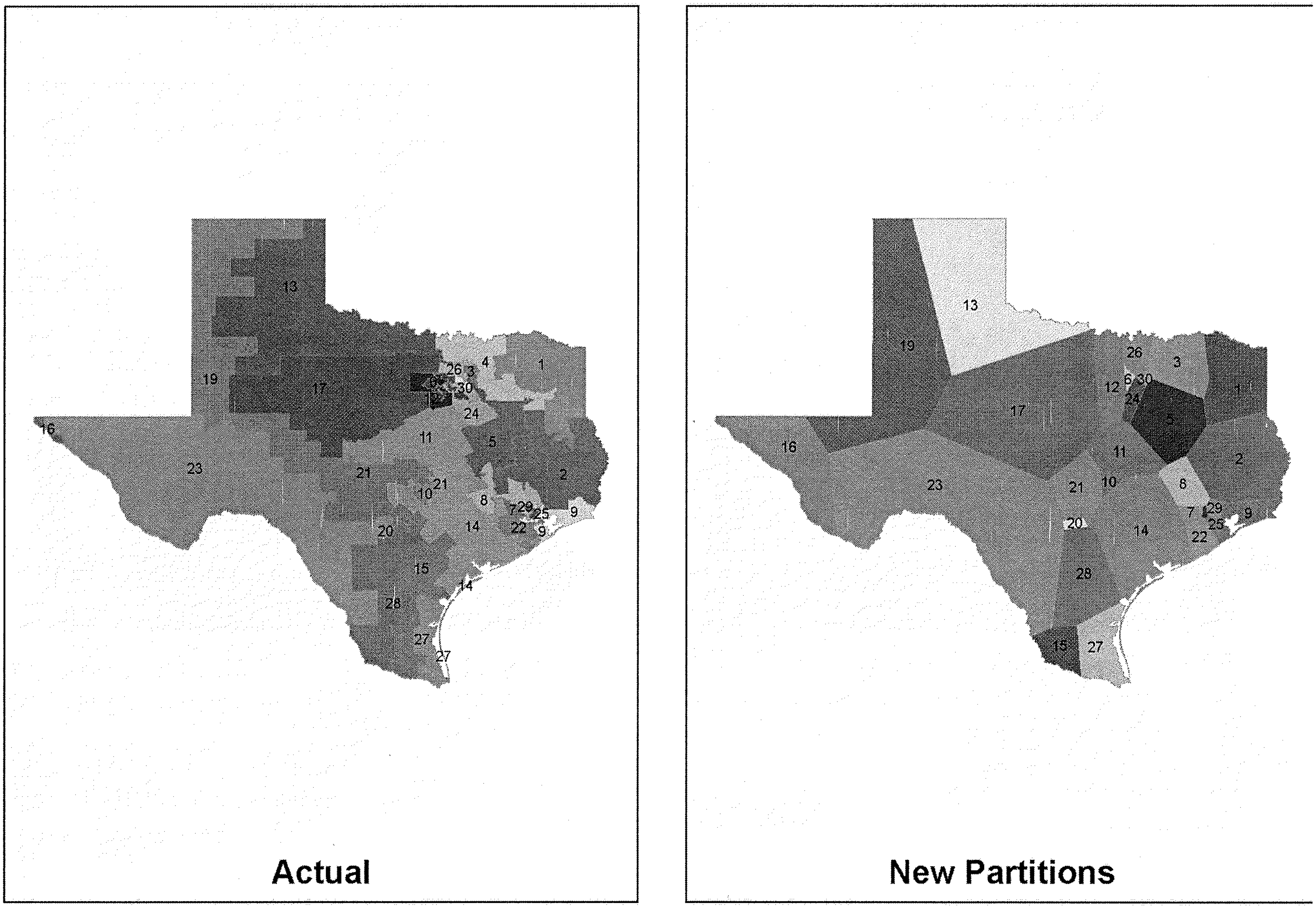

Figure11: Texas 106th Congress Districting Plans, Actual v. Algorithm

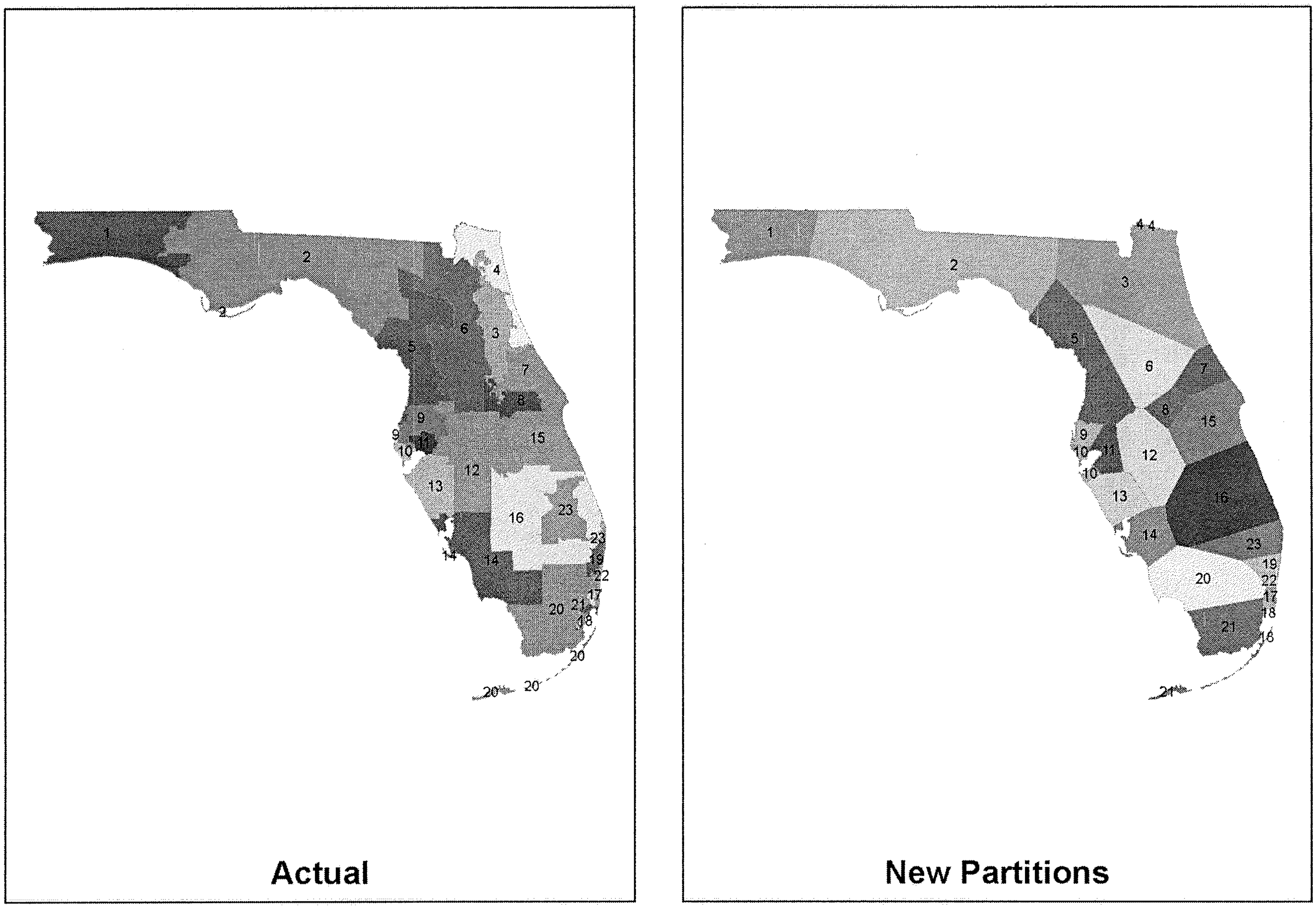

Figure 12: Florida 106th Congress Districting Plans, Actual v. Algorithm

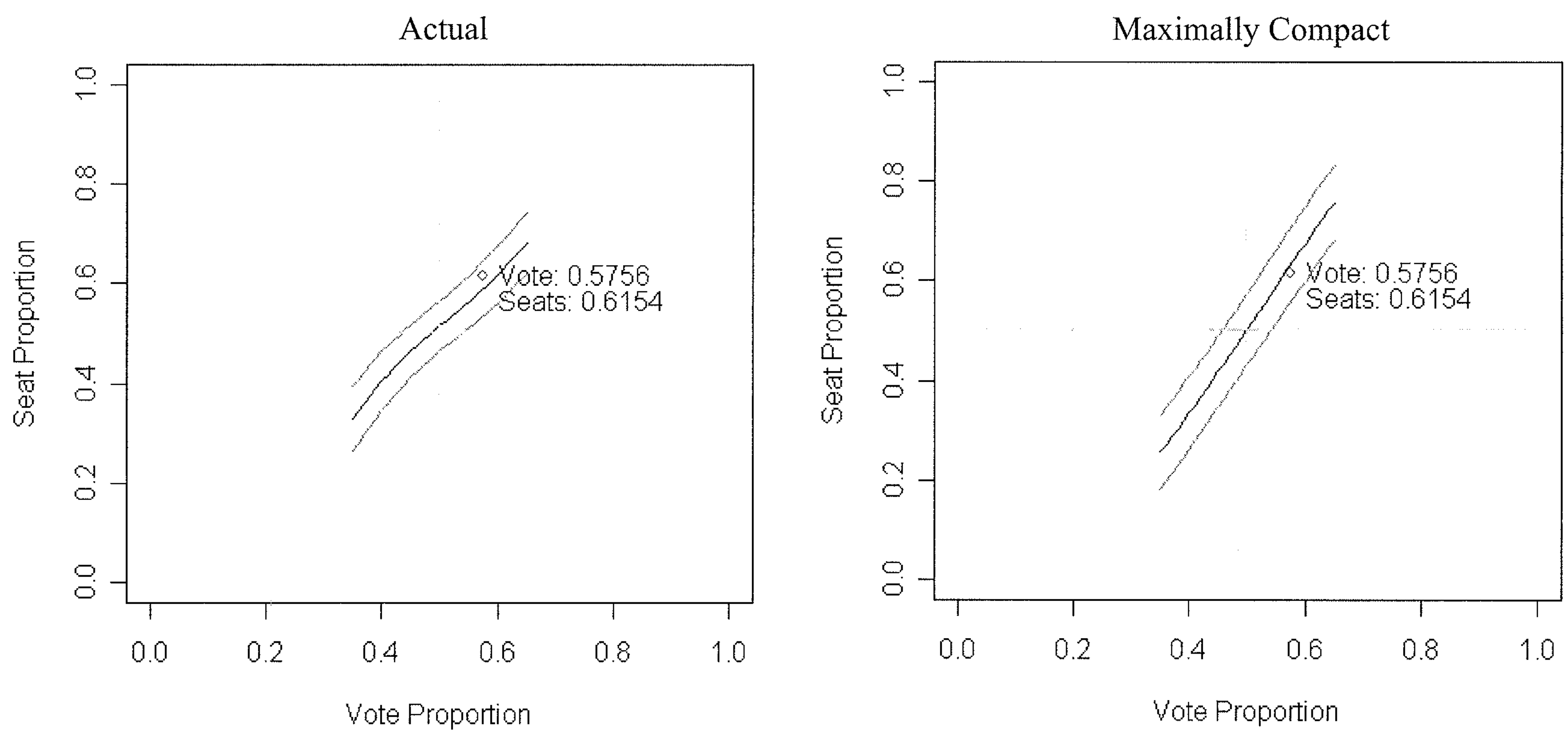

Figure 13: Seat-Vote Curves for California, Actual v. Maximally Compact

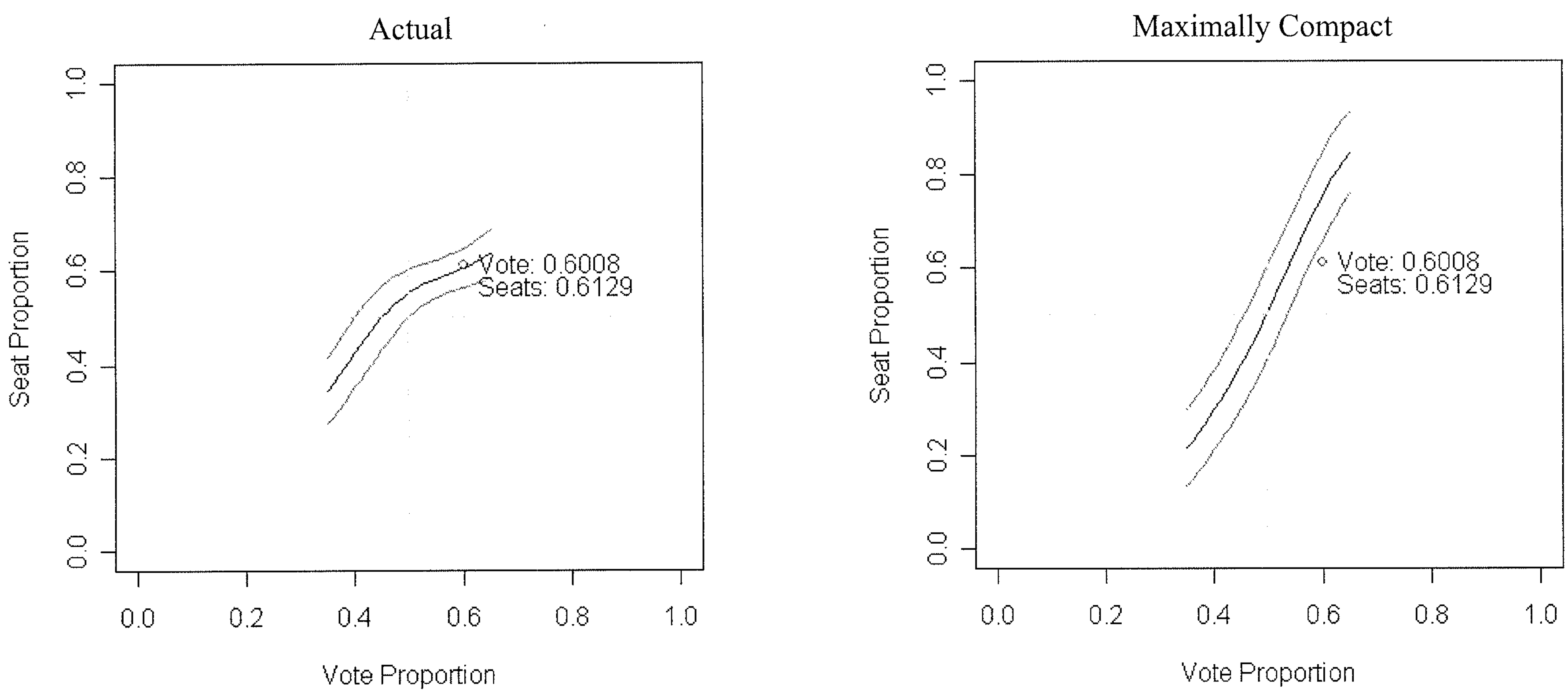

Figure 14: Seat-Vote Curves for New York, Actual v. Maximally Compact

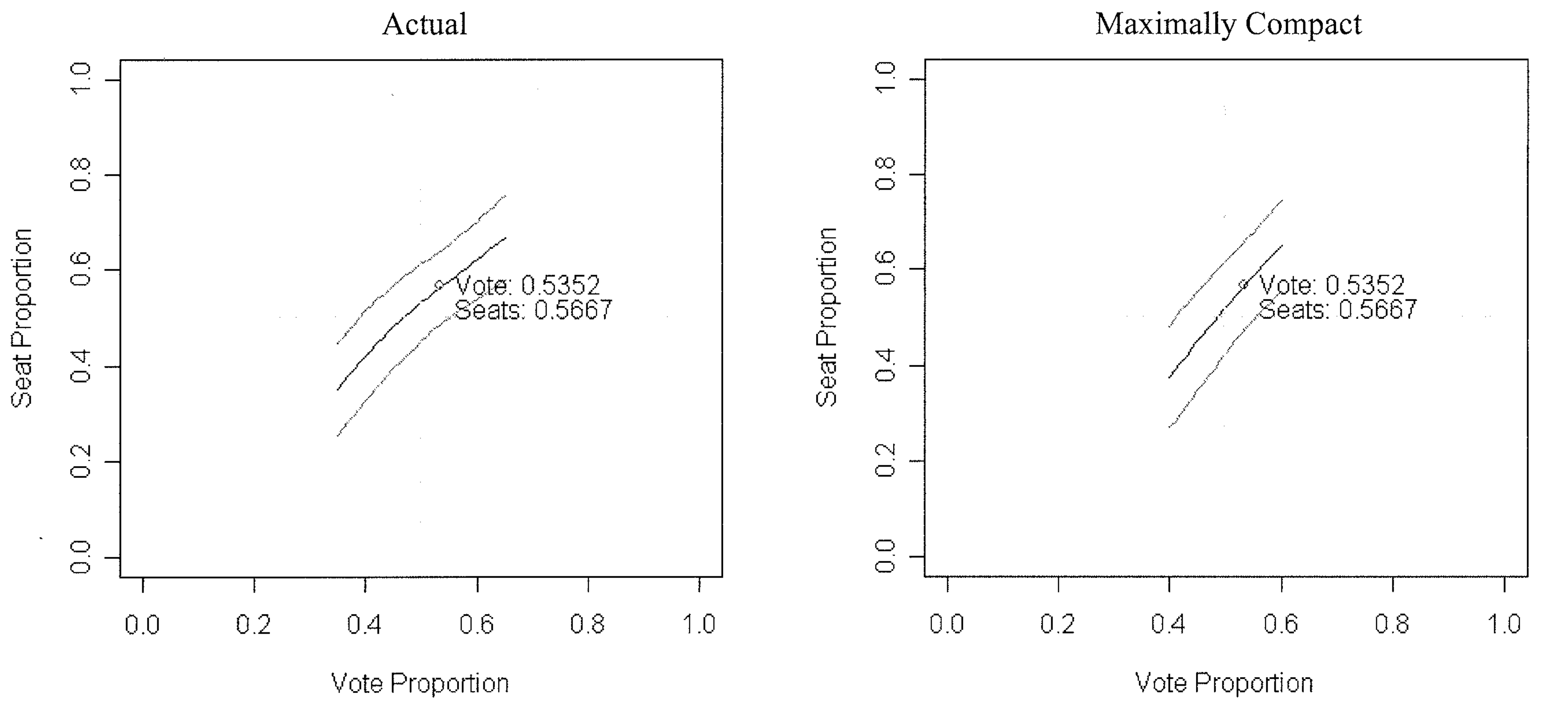

Figure 15: Seat-Vote Curves for Texas, Actual v. Maximally Compact

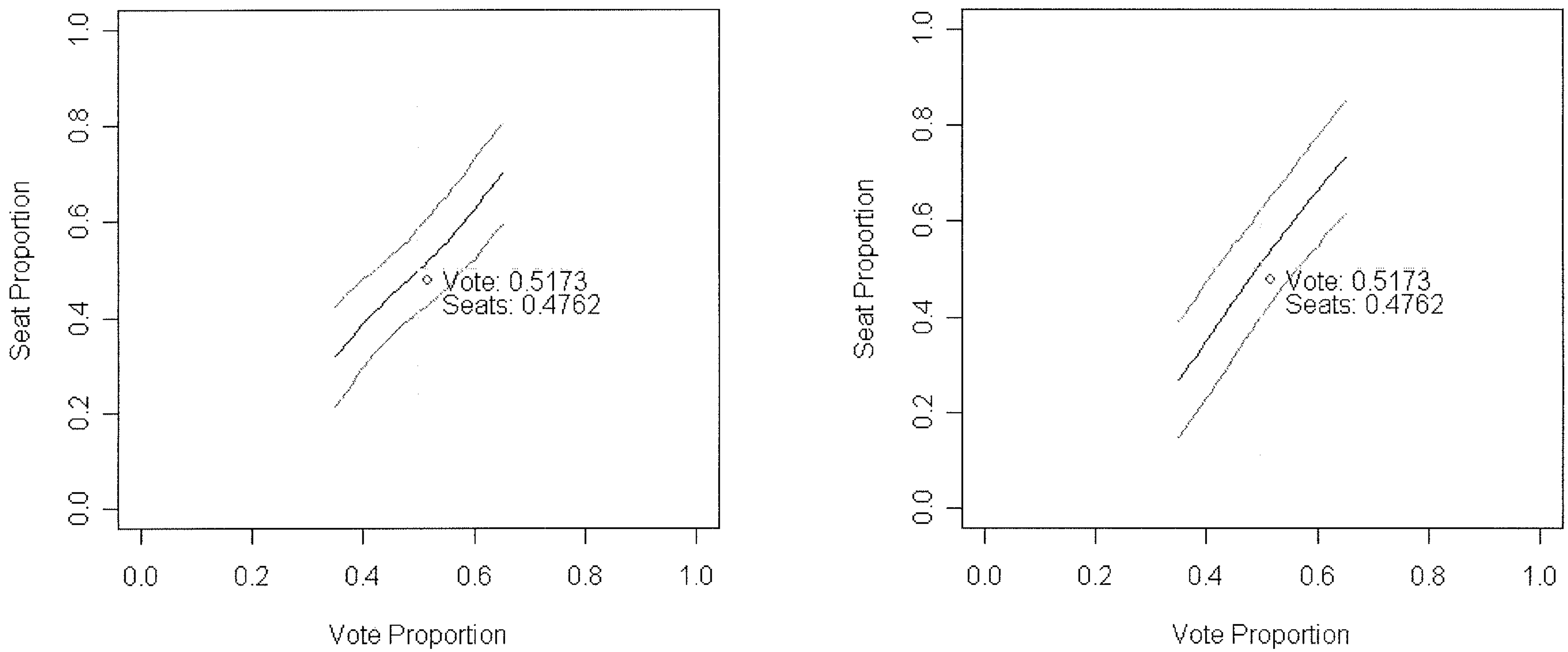

Figure 16: Seat-Vote Curves for Pennsylvania, Actual v. Maximally Compact

Table 1: The Relative Proximity Index, 2000

State Name	RPI	Max Deviation (Actual)	Max Deviation (Algorithm)	Mean RPI	Standard Deviation RPI	Percentile
Alabama	1.21	0.27	0.05	0.99	0.03	1.00
Arizona	1.34	0.20	0.15	1.27	0.04	0.97
Arkansas	1.08	0.14	0.05	0.78	0.01	1.00
California	1.49	0.17	0.04	0.96	0.03	1.00
Colorado	1.59	0.15	0.05	1.28	0.02	1.00
Connecticut	1.36	0.02	0.01	1.09	0.35	0.78
Florida	1.39	0.46	0.07	0.83	0.08	1.00
Georgia	1.24	0.14	0.09	0.90	0.01	1.00
Hawaii	1.59	0.09	0.04	1.48	0.02	1.00
Idaho	0.97	0.10	0.02	0.80	0.02	1.00
Illinois	1.43	0.29	0.11	0.98	0.07	1.00
Indiana	1.49	0.20	0.06	1.05	0.02	1.00
Iowa	1.38	0.06	0.05	1.29	0.01	1.00
Kansas	1.11	0.08	0.05	0.95	0.01	1.00
Kentucky	1.51	0.14	0.05	1.22	0.01	1.00
Louisiana	1.15	0.13	0.05	0.79	0.43	0.80
Maine	1.39	0.04	0.03	1.15	0.01	1.00
Maryland	1.52	0.22	0.04	1.25	0.02	1.00
Masschussetts	1.87	0.10	0.05	1.54	0.01	1.00
Michigan	1.24	0.13	0.04	0.99	0.02	1.00
Minnesota	1.05	0.16	0.05	0.90	0.02	1.00
Mississippi	1.02	0.18	0.05	0.87	0.01	1.00
Missouri	1.38	0.23	0.05	1.01	0.16	0.99
Nebraska	1.01	0.05	0.04	0.89	0.23	0.70
Nevada	1.38	0.08	0.05	1.19	0.01	1.00
New Hampshire	1.10	0.01	0.00	1.09	0.00	0.95
New Jersey	2.27	0.21	0.05	1.69	0.02	1.00
New Mexico	1.23	0.06	0.04	1.14	0.01	1.00
New York	1.83	0.21	0.10	1.45	0.45	0.80
North Carolina	1.33	0.28	0.04	1.15	0.09	0.97
Ohio	1.62	0.13	0.05	1.42	0.01	1.00
Oklahoma	1.24	0.09	0.05	1.42	0.36	0.31
Oregon	1.26	0.09	0.04	1.21	0.28	0.56
Pennsylvania	1.81	0.25	0.22	1.27	0.05	1.00
Rhode Island	1.18	0.03	0.02	1.18	0.01	0.55
South Carolina	1.22	0.21	0.04	1.27	0.02	0.00
Tennessee	2.91	0.25	0.04	2.59	0.04	1.00
Texas	1.90	0.30	0.22	1.24	0.07	1.00
Utah	1.46	0.06	0.04	1.40	0.01	1.00
Virginia	1.38	0.22	0.07	1.14	0.04	1.00
Washington	1.17	0.15	0.06	0.77	0.03	1.00
West Virginia	1.68	0.06	0.05	1.61	0.01	1.00
Wisconsin	1.40	0.11	0.08	1.22	0.58	0.62

Notes: RPI values were calculated using tract-level data from the 2000 Census. Max Deviation 1 minus the total population of the largest congressional district divided by the total population of the smallest congressional district. Mean RPI was calculated as the mean of 200 repititions of the RPI -- each having different starting values.

Table 2: Partisan Bias and Responsiveness, Actual versus Maximally Compact Districtings

State	Bias (Actual)	Bias (Algorithm)	t-statistic on Difference	Responsiveness (Actual)	Responsiveness (Algorithm)	t-statistic on Difference
California	.028	.007	.469	1.086	1.731	-4.327**
	(.010)	(.045)		(.069)	(.132)	
New York	.103	.018	1.051	0.482	2.51	-6.540**
	(.014)	(.080)		(.036)	(.308)	
Pennsylvania	-0.0027	.031	-.363	1.138	1.562	-1.800*
	(.021)	(.076)		(.128)	(.198)	
Texas	.062	.039	.334	0.8872	1.305	-1.717*
	(.024)	(.064)		(.103)	(.221)	

Notes: Estimates are based on voter tabulation district level election return data for the 105th and 106th congress.

From: **Nicholas Stephanopoulos** nicholas.stephanopoulos@gmail.com
Subject: Items for rebuttal report
Date: Sat Dec 05 2015 05:24:27 GMT+0530 (IST)
To: Jackman jackman@stanford.edu
Cc: Peter Earle peter@earle-law.com, Paul Strauss Pstrauss@clccrul.org, Ruth Greenwood rgreenwood@clccrul.org

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Simon,

Based on our conversation, here's a list of tasks we'd like for you to carry out in your rebuttal report. We may add further items to this list, and you should also let us know as soon as possible if you have additional ideas. Again, the report is due on 12/21, so we'd like to receive a draft by 12/18. I'll also send you in a separate message (1) a dataset of congressional efficiency gaps; and (2) a dataset of the institution responsible for redistricting in each state. Thanks very much.

Nick

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1. *Further investigate the stability of the efficiency gap.* You may wish to do this by (a) determining the average lifetime size of a plan's EG given the first (or the first two) observed EG values for the plan; and (b) carrying out sensitivity testing for the first observed EG value for a plan, using uniform vote swings in either direction, and thus determining the plan's expected average EG size and expected odds of switching EG signs over its lifetime (per Stephanopoulos & McGhee). You should address the implications of this analysis for setting the actionable EG threshold.

2. *Further investigate the relationship between political geography and the efficiency gap:* You may wish to do this by (a) analyzing the observed distribution of EGs over the modern redistricting era; (b) determining the extent to which the pro-Republican trend in the EG in recent years is attributable to Republican control over redistricting in more states; (c) addressing the validity of the Chen/Rodden analysis of political geography, which relies on simulated district plans; and (d) addressing the validity of the Trende analysis of political geography (paras. 62-105), which relies primarily on data on Wisconsin counties and wards.

3. *Address the relationship between the efficiency gap calculated using district vote totals and the measure calculated using the assumption of equal turnout:* You may wish to do this by focusing on states with no uncontested races, which allow both metrics to be calculated easily.

4. *Address the specific redistricting cases raised in Trende's report (paras. 106-131):* You may wish to do this by (a) examining the cases that were cited from your own report; and (b) examining the mostly congressional cases that Trende discusses.

5. Address any other points you believe are worthwhile: Finally, you should comment on any other aspects of the Goedert and Trende reports that, in your view, warrant a response.

--

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Sensitivity of the Efficiency Gap to Uniform Swing

How sensitive is the efficiency gap to reasonable swings in vote shares? In his report, Goedert asserts that it is extremely sensitive (pp. 11-15), but his claim is based on a small number of examples (pp. 12-13) as well as his own work at the congressional level involving only two elections (Goedert 2015). Sections 1-4 of my rebuttal report show that the first efficiency gap observed under a plan is a reliable indicator of the efficiency gap's magnitude and direction over the remainder of the plan's lifespan. These sections, however, are based on historical efficiency gap data rather than the "sensitivity testing for future results" deemed "crucial" by Goedert (p. 13). Accordingly, we conduct sensitivity testing here of exactly the kind earlier carried out by Stephanopoulos & McGhee (pp. 889-90, 898-99) and recommended by Goedert. This testing confirms the findings in Sections 1-4 of my rebuttal report, and further corroborates my conclusions therein about the efficiency gap's durability and reliability.

Methodologically, we investigate the behavior of the efficiency gap when we perturb it by mimicking "uniform swing" across a jurisdiction. That is, a given election produces a set of vote shares across districts. A new hypothetical election is considered in which all vote shares move up or down by a predetermined quantity (i.e., the "swing"); since all districts move by the same amount, this technique is known as uniform swing. In real-world elections swings are never precisely uniform, and so this method is widely considered to be a simplification; on the other hand, modeling or predicting swing district by district is quite difficult, especially for state legislative elections where we often lack useful district-level predictors of swing (or, more tellingly, predictors of the way the swing in a given state legislative district might depart from the statewide swing).

We restrict the following exercise to elections since the 2010 round of redistricting. For each election we simulate a series of uniform swings, evenly spaced between -5% to +5%, a quite

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large set of swings by the standards of state legislative elections. For instance, swings in Wisconsin state legislative elections from 1972 to 2014 are estimated to range between -7.6 percentage points from 2008 to 2010 (Democratic share of two-party vote, averaged by district) and +5.0 percentage points from 2004 to 2006. Similarly, Stephanopoulos & McGhee found that a swing of +/- 5.5 percentage points covered the vast majority of state legislative elections from 1972 to 2012 (p. 874).

At each level of uniform swing, we record the new vote shares and seat shares (some seats change hands if the swing pushes Democratic two-party vote share to the other side of 50%) and recompute the efficiency gap. We then examine how much the simulated efficiency gaps—generated under different levels of uniform swing—depart from the efficiency gap observed under the actual election. In particular, if relatively small swings produce large changes in *EG*, we might rightly be concerned about the stability and reliability of the efficiency gap as a characterization of a district plan. Keep in mind that this exercise keeps the district plan as it is and simply shifts vote shares up and down over a range of hypothetical levels of statewide swing, held constant over districts.

Figure 1 shows the relationships between efficiency gaps estimated using actual election results in state legislative elections held since the 2010 round of redistricting, and efficiency gaps estimated using a range of uniform swings. When uniform swing is zero, the simulation exercise leaves the actual election results unperturbed, and we simply recover the original efficiency gap estimates; all the data in the panel labelled "Swing +0.0" lies on the 45-degree line. As we increase the magnitude of hypothetical levels of uniform swing, the relationship between the observed efficiency gaps and the simulated efficiency gaps weakens, but only by a moderate amount. Even at high levels of uniform swing (approaching +/- five percentage points), the relationship between observed efficiency gaps and simulated efficiency gaps remains of significant strength; the blue line in each panel of Figure 1 is a regression line and in every case has a large and unambiguously positive slope, indicating a positive correlation between actual and simulated efficiency gaps.

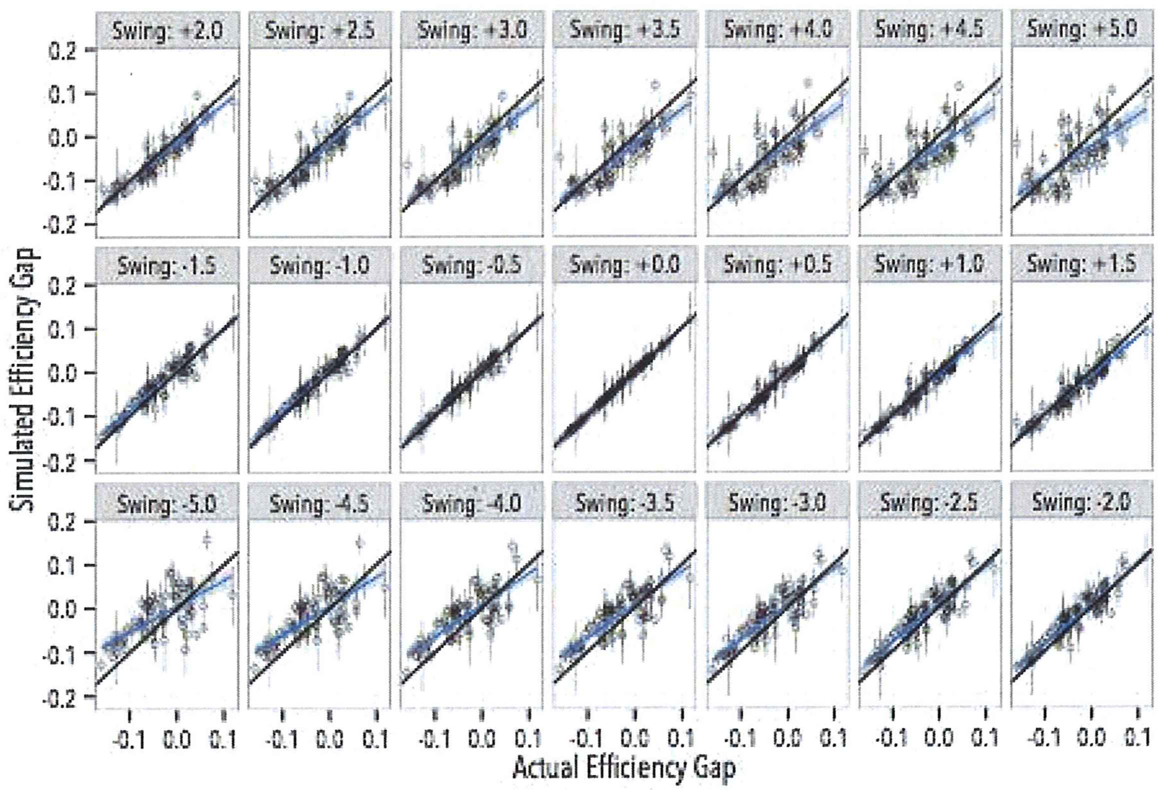

Figure 1: Actual efficiency gaps from state legislative elections 2012 to 2014 (horizontal axis), and corresponding simulated efficiency gaps generated by varying levels of uniform swing. Vertical lines indicate 95% confidence intervals. Dark diagonal lines are at forty-five degrees, the fit to the data that would result if actual and simulated efficiency gaps were equal (as is the case when the simulated level of uniform swing is set to zero, as in the middle panel of the second row). The blue line indicates a regression fit. For small to even moderately large values of uniform swing, there is a high degree of correspondence between the actual and simulated efficiency gaps.

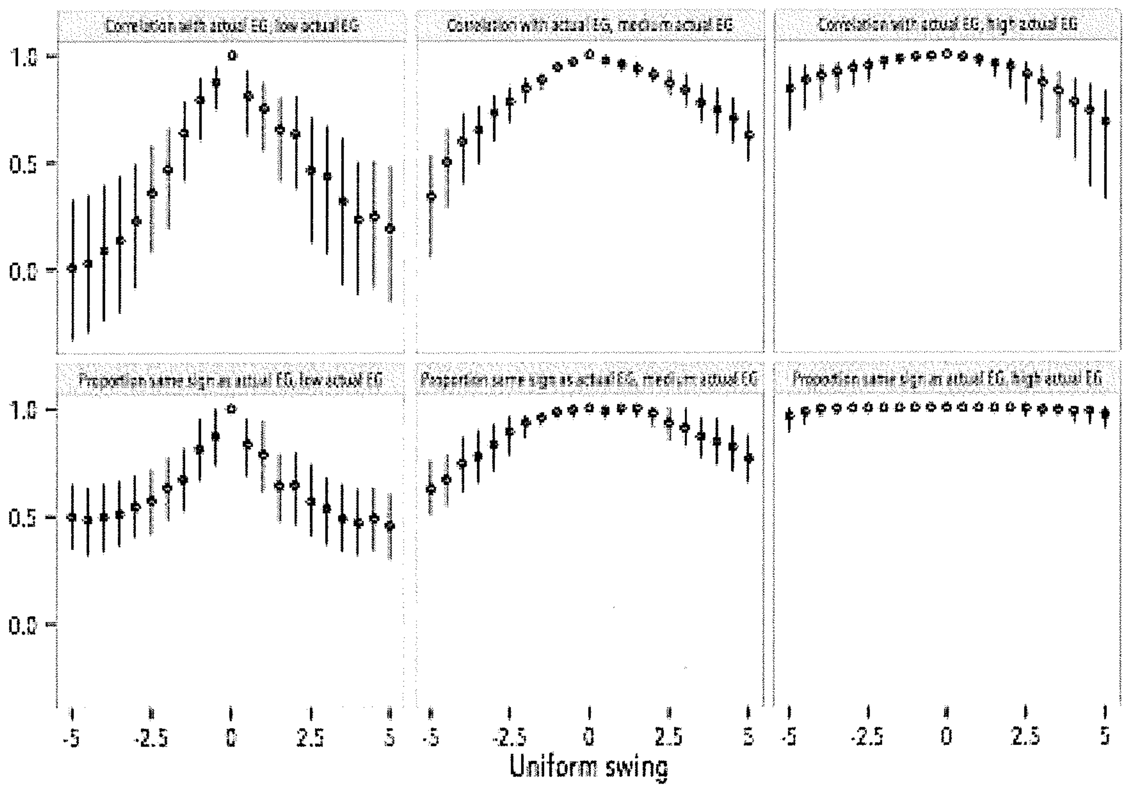

Figure 2: Correlation between actual efficiency gaps and simulated efficiency gaps (top row) and proportion of simulated efficiency gaps with same sign as actual efficiency gaps (bottom row), by hypothetical levels of uniform swing (horizontal axis). Vertical lines are 95% confidence intervals. The three columns correspond to actual efficiency gaps that are low in magnitude (less than .03 in absolute value; left column), medium (.03 to .07 in absolute value, middle column) and high (above .07 in absolute value, right column). When uniform swing is zero, the simulated efficiency gaps correspond to the actual efficiency gaps, and so the correlation between the two sets of efficiency gaps is exactly 1.0 and 100% of the simulated efficiency gaps have the same sign as the actual efficiency gaps.

The top row of Figure 2 displays correlations between actual efficiency gaps and simulated efficiency gaps, under different hypothetical levels of uniform swing (horizontal axis), with separate panels for low, medium, and high values of actual efficiency gaps. Note that when uniform swing is zero, the simulated efficiency gaps correspond to the actual efficiency gaps, and so the correlation between the two sets of efficiency gaps is exactly 1.0. As levels of uniform swing increase, the correlation between actual and simulated efficiency gaps diminishes. Small efficiency gaps (less than .03 in absolute value) are less resistant to perturbations from uniform swing; at high levels of uniform swing for small actual efficiency gaps, the correlation between actual efficiency gaps and simulated efficiency gaps approaches zero. However, larger values of the efficiency gap are much more robust to perturbations from uniform swing. In fact, for large actual efficiency gaps (greater than .07 in magnitude), the correlation between actual and simulated efficiency gaps stays impressively large over the entire range of uniform swing levels considered here (top right panel of Figure 2).

The bottom row of Figure 2 displays the proportion of simulated efficiency gaps that have the same sign as actual efficiency gaps, under a range of hypothetical levels of uniform swing (horizontal axis), again with separate panels for low, medium, and high values of actual efficiency gaps. Again we see that small efficiency gaps—less than .03 in magnitude and hence relatively close to zero—are reasonably likely to flip signs under moderate to large values of hypothetical uniform swing: about half of these small efficiency gap estimates flip signs when subjected to reasonably large statewide swings one way or the other. But large efficiency gaps—those greater than .07 in magnitude—show great resistance to flipping signs even in the face of moderate or even large hypothetical statewide swings (lower right panel of Figure 2). None of the large efficiency gaps flip signs when swings are below 2.5 percentage points and *barely any* flip signs even we consider larger statewide swings. Just 11% of actual efficiency gaps greater than .07 in magnitude flip signs when exposed to a very large, hypothetical statewide swing of minus five percentage points and only 9% flip signs when we consider a statewide swing of positive five percentage points.

In short, efficiency gap estimates display a high level of resistance to perturbations from even large levels of uniform swing. This further bolsters our confidence that the efficiency gap is measuring a durable property of a district plan. Moreover, the analysis reported here demonstrates that efficiency gaps are especially reliable when they are large, as is the case for the efficiency gaps generated under the Wisconsin plan. The efficiency gap changes if vote totals change, even if the district plan remains constant; this is "hardwired" into the definition and accompanying arithmetic of the efficiency gap. But to reiterate a conclusion from my original report: the amount of election-to-election variation in the efficiency gap is small relative to the variation in the efficiency gap across plans.

SIMON JACKMAN

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Attention: Ruth Greenwood
Chicago Lawyers' Committee for Civil Rights Under Law
100 N. Lasalle Street, Suite 600,
Chicago, IL 60602
Date: 12/7/15

Project Title: Wisconsin's 2011 state legislative districting plan
Project Description: assessing the efficiency gap as an indicator of partisan gerrymandering, historical analysis, comparisons of the Wisconsin plan with historical and contemporaneous precedents, actionable threshold.

Description	Hours	Rate	Cost
Arizona analysis, writeup, October 2015	2.50	$ 250	$ 625
Deposition preparation, phone calls	2.00	$ 250	$ 500
Deposition preparation, Madison	4.70	$ 250	$ 1,175
Deposition preparation, solo	2.00	$ 250	$ 500
Deposition	4.00	$ 250	$ 1,000
Deposition transcript review, comments	3.00	$ 250	$ 750
Rebuttal report preparation	1.00	$ 250	$ 250
Hour totals			$ 4,800
Airfare, SFO-MSN-SFO			$ 447
TOTAL			**$ 5,247**

Sincerely yours,

Simon Jackman

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Until March 12:
65 High St,
Oxford, OX1 4EL
UNITED KINGDOM

After March 12:
89 Endeavour St,
Red Hill, ACT 2603
AUSTRALIA

Attention: Ruth Greenwood
Chicago Lawyers' Committee for Civil Rights Under Law
100 N. Lasalle Street, Suite 600,
Chicago, IL 60602
Date: **2/23/16 (corrects invoice of 2/15/16)**

Project Title: Wisconsin's 2011 state legislative districting plan
Project Description: rebuttal report, analysis, report writing.

Date	Description	Hours
12/13/15	phone conversation, Ruth G, Nick S., Paul S.	0.60
12/18/15	rebuttal preparation	3.00
12/19/15	rebuttal preparation	4.00
12/20/15	rebuttal preparation	4.00
12/21/15	rebuttal preparation	8.00
12/22/15	rebuttal preparation	12.60
12/23/15	rebuttal preparation	0.50
12/24/15	rebuttal preparation	3.00
12/25/15	rebuttal preparation	2.50
1/6/16	summary judgement review, graphs	1.50
	Total	39.70
	At rate of **$250/hr**	**$9925.00**

Sincerely yours,

Simon Jackman

Simon Jackman