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EXHIBIT 9

IN THE UNITED STATES DISTRICT COURT
FOR THE MIDDLE DISTRICT OF NORTH CAROLINA

UNITED STATES OF AMERICA,

Plaintiff,

v.

THE STATE OF NORTH CAROLINA, *et*
al.,

Defendants.

Civil Action No. 1:13-CV-861

Surrebuttal of Charles Stewart III, Ph.D.

Pursuant to 28 U.S.C. § 1746, I, Charles Stewart III, make the following declaration:

1. On April 11, 2014, I filed an expert report in this case, in which I discussed the racially disparate effects that HB 589 would have on the voters of North Carolina, focusing on the provisions of that law that affected early voting, same-day registration, and out-of-precinct voting.

2. On April 25, 2014 I received from attorneys at the U.S. Department of Justice declarations by Mr. Thomas H. Fetzer, Dr. Donald Schroeder, Dr. Janet R. Thornton, and Mr. Thomas P. Trende; on April 26, 2014, I received a declaration by Dr. Thomas Brook Hofeller from U.S. DOJ attorneys.

3. In this declaration I respond to certain claims and opinions made in these declarations. I focus particularly on claims and opinions in the declarations by Dr. Thornton (the “Thornton Declaration”) and Mr. Trende (the “Trende Declaration”).

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4. My responses may be summarized as follows:

- Opinion 1 in the Trende Declaration is largely irrelevant to providing insights into how changes to election laws affect voter registration and turnout.
- The alternative coding of early voting time spans offered in the Trende Declaration in support of Opinion 1 are minor, and result in a conclusion that is identical to that offered in my original declaration.
- The analysis offered in the Trende Declaration, which compares North Carolina with states that offer no opportunities for in-person early voting, confuses the election administration issues posed by this case.
- The analysis offered in support of Opinion 1 in the Trende Declaration is ad hoc and unrelated to the issues involved in this case, because it offers no statistical analysis concerning racial disparities that are likely to arise on account of implementing HB 589.
- The analysis offered in support of Opinion 2 of the Trende Declaration mischaracterizes the framing of the analysis in my declaration, and inappropriately assumes that if a law does not result in a reduction of registration or turnout, it has not presented an obstacle to voting.
- The analysis in support of Opinion 2 of the Trende Declaration contains a simplistic attempt at statistical analysis to account for how changes to election laws influence voter turnout. This analysis is inconsistent in method and approach with that found in the scientific literature on the topic.
- The analysis of the Trende Declaration which suggests that turnout in North Carolina grew because it has become a “target state” is not based on evidence that is regularly used in the scientific analysis of this topic.
- The Trende Declaration misrepresents the research conducted by Dr. Paul Gronke and myself into how Florida voters responded to a restriction of early voting days in 2012, by mischaracterizing voters’ commitment to early voting as voters’ commitment to voting at all by any method.
- The observation in the Thornton Declaration that factors other than changes to election laws may influence individuals to vote does not diminish the fact that North Carolina’s election laws have an influence on voter turnout.
- The fact presented in the Thornton Declaration that census tracts with early voting sites had a statistically significantly higher percentage of African Americans among the voting age population does not undermine the conclusion that HB 589 will have a disproportionate impact on blacks compared to whites.

- The differences in average wait times for early voting in North Carolina are greater than the national average; the difference is statistically significant at traditional levels used in the social sciences.
- The Thornton Declaration mischaracterizes my analysis of the total number of early voting hours in counties, and performs an analysis that leaves an incorrect impression that she has conducted an independent analysis that is similar to my own.
- The Thornton Declaration incorrectly claims that I assume 3.0% of African-Americans will be unable to vote due to changes in the same-day-registration law.

MATTERS RELATED TO THE TRENDE DECLARATION

I. Opinion 1 in the Trende Declaration is largely irrelevant to providing insights into how changes to election laws affect voter registration and turnout.

5. The purpose of Opinion 1 of the Trende Declaration is to demonstrate that HB 589 moved North Carolina into the “mainstream of American voting laws.” Leaving aside for the moment the question of whether the Trende Declaration establishes this claim, it is largely irrelevant to the issues at the core of my analysis. As I noted when I began the substantive analysis in my declaration, the report I provided was “about whether various changes to North Carolina’s election laws reflected in HB 589 would create a disproportionate burden on black voters in the state.” (¶ 19) The general logic of my declaration starts with the status quo before the passage of HB 589, and then considers the impact of the act on *individual voters* in North Carolina. There is no question that the provisions of HB 589 will place new obstacles before North Carolina voters. The question at hand is whether the obstacles will be disproportionately faced by African American voters. Based on the evidence I examined, the answer to this question is a clear “yes.”

6. Whether the *change* in law embodied in HB 589 moves North Carolina closer to some national average is irrelevant to the question of disproportionate obstacles. North Carolina

voters are not affected by the election laws of other states. Therefore, the comparison is irrelevant. The only relevant comparison is North Carolina before and after the passage of HB 589.

7. The experience of other states may inform our understanding of the likely effects of HB 589, to the degree that other states have enacted similar changes to their laws in the past, and to the degree that we can observe how the voters in these other states were affected by changes in these laws. However, it is important to keep in mind that what matters is whether a *change* in law leads to a *change* in obstacles, not whether a state changes its laws to maintain some sort of aesthetic balance with other states.

8. As a guide to my general criticism of much of the analysis in the Trende Declaration — particularly the analysis that attempts to associate features of North Carolina’s election laws with the turnout of North Carolina voters — it is important to understand the canonical way in which social scientists have been studying how voting laws affect turnout and registration rates for the past forty years.

9. The classic work on how voting laws affect turnout was written by Steven J. Rosenstone and Raymond E. Wolfinger in the late 1970s/early 1980s. The most important of their research appeared in a book, *Who Votes?*,¹ and an article, “The Effect of Registration Laws on Voter Turnout.”² Although the book is a fuller treatment of the subject than the article, both focus on the same data: responses to the 1972 Voting and Registration Supplement (VRS) of the Current Population Survey (CPS).³

¹ Raymond E. Wolfinger and Steven J. Rosenstone, *Who Votes?* New Haven: Yale University Press, 1980.

² Steven J. Rosenstone and Raymond E. Wolfinger. “The effect of registration laws on voter turnout.” *American Political Science Review* (1978): 22-45;

³ See ¶ 48 in my original declaration for a description of the VRS. I refer to this study as the VRS, while some refer to it as the CPS. I prefer VRS as a short hand, because it avoids confusion with the national survey conducted by the University of Michigan’s Center for Political Studies (CPS), which now is more commonly referred to as the

10. The logic of their investigation is straightforward. The outcome (“dependent variable”) they explore is whether a respondent to the VRS reported voting in the 1972 presidential election. The factors explored as potential causes of voting (the “independent variables”) include demographics of the respondents, election laws and administrative practices in the state in which the respondents lived, and other political factors related to the respondent’s state.

11. Table 1 below reprints Table 2 from their article, as an example of the type of estimation they provided.⁴ The numbers under the column “probit estimate” are weighting factors that measure how influential the corresponding independent variables are in determining whether a respondent voted.⁵ Positive values of the probit coefficients indicate a positive relationship between the corresponding independent variable and turnout, holding values of all other independent variables constant; negative values indicate a negative relationship. The numbers under the column “standard error” measure the precision of the coefficients, and are used to assess statistical significance.⁶

American National Election Study (ANES). Note that the references below in the Trende Declaration to the CPS are references to the VRS, not the ANES.

⁴ Rosenstone and Wolfinger, “The effect of registration laws on voter turnout”, p. 32.

⁵ The form of statistical procedure used in this article and reported in the displayed table is called “probit analysis,” which is a type of multivariate statistical procedure that is similar to linear regression, which is more commonly familiar. (*Who Votes?* contains an accessible discussion of the relationship between probit analysis and linear regression in its Appendix C.) Probit analysis is commonly used when the dependent variable is dichotomous, 0 or 1, as is the case when we study whether someone voted (1) or not (0). A challenge with probit analysis is that unlike linear regression, the probit coefficients are not directly interpretable. There is an extra step the researcher needs to perform to convert the probit coefficients into measures of how a unit change of the independent variable affects the probability that one will vote (as in this example). In the research cited here, Wolfinger and Rosenstone devote considerable attention to converting the probit coefficients to more easily interpretable probabilities.

⁶ Statistical significance is established by conducting a “*t*-test,” in which the coefficient is divided by the corresponding standard error. This ratio is called the “*t*-statistic.” The larger the *t*-statistic, the higher the degree of confidence we have that the coefficient is statistically different from zero.

Table 1. Reprint of key table reporting statistical results of the estimation of the effects of voter registration laws on turnout, controlling for demographic factors. (Source: Rosenstone and Wolfinger, p. 32 [Table 2].)

Estimates of the Effect of Demographic Variables and Registration Laws on Turnout in 1972 ^a		
Variable	Probit Estimate	Standard Error
(Constant)	-2.7001	.2410
Education	.1847	.0120
Education squared	.0120	.0050
Age	.0707	.0045
Age squared	-.0006	.0001
Region	-.1371	.0413
Closing date	-.0073	.0015
Irregular office hours	-.1005	.0438
Open evening and/or Saturday	.1253	.0345
No absentee registration	-.0909	.0403
Hours polls open	.0336	.0159
Gubernatorial election	.0634	.0338

Number of cases = 7,936
 Percentage of cases correctly predicted = 71.4
 Log of the likelihood function = -4445.63
 -2 times the log likelihood ratio = 1154.66
 Degrees of freedom = 11

^aEstimates for the variables deleted from this equation are given in Appendix D.

12. There are three important features of this analysis that bear highlighting, because they have informed subsequent analysis that builds on Wolfinger and Rosenstone's work. First, this work all proceeds from a theoretical and empirical focus on the *individual voter*. The theoretical framing device is to ask, "If a state adopts Law X, what is the probability that a 'representative citizen' will be more likely to vote?" The "representative citizen" is a theoretical construct that makes it simpler to think about how changes to laws will affect *typical* citizens.

13. Second, this research accounts for the possibility that some election laws may have more of an impact on turnout than others. Therefore, it is common to include measures of numerous election laws as independent variables in the analysis. In the case of Table 1, for instance, this article studied the effects of five election laws.⁷

⁷ In addition to being interested in knowing which election laws have a greater effect on turnout, it is important to include measures of multiple laws in a multivariate study of this topic because of the problem of "spurious correlation" that is addressed in the next paragraph. The issue is that a state that adopts Law A to increase turnout may also be more prone to adopt Law B, which also increases turnout. In studying the effects of Law A, we must

14. Third, this research all assumes that election laws are not the only factors affecting whether citizens register and/or vote. As Table 1 illustrates, some of these factors are demographics, such as education, age, and region, and some are political, such as whether there was also a gubernatorial election in the respondent's state in 1972.⁸ There are two major reasons for the inclusion of statistical controls like these in the study of the effects of registration laws on turnout. First, it builds a more realistic statistical model of the turnout decision, which aids in external validity, while also adding the statistical benefit of improving the "efficiency" of the estimated coefficients. Second, it helps to guard against "spurious causality." This second advantage is the more important for estimating the effect of election laws on turnout accurately, so I will say another word about it.

15. This problem of spurious causality can easily be seen with an example. It is well known that the states of the upper Midwest and Great Plains — the Dakotas, Minnesota, and Wisconsin — tend to have very high voter turnout rates. Two of these states, Minnesota and Wisconsin, have Election Day Registration (EDR); North Dakota does not have voter registration at all. It is tempting to conclude that EDR causes higher turnout, as evidenced by the high level of turnout of states that have EDR, compared to states that do not have EDR.

16. However, these states that have EDR also have average levels of education that are higher than the national average. We also know (see Table 1 above) that education is positively correlated with turning out. Therefore, it may be that states like Minnesota, Wisconsin, and North Dakota have high turnout rates because of their voter registration laws (or

control for the presence/absence of Law B; if we do not, then some of the effects of Law B on turnout will be spuriously attributed to the presence of Law A.

⁸ Note that in Table 1, education-squared and age-squared are included in the estimation, to capture curvilinear relationships between turnout and these variables. For instance, if the youngest *and* the oldest voters are less likely to vote than middle-aged voters, that curvilinear relationship needs to be captured this way.

lack thereof), or they may have high turnout rates because their voters are more highly educated than average. How do we judge which hypothesis is correct?⁹

17. Demographic controls are how social scientists adjudicate questions like this. By “controlling for” factors that *both* influence turnout *and* are correlated with the presence of the election laws we are studying, we can get an unbiased estimate of the effects of the election laws *per se*, abstracted from the confounding influence of these other factors. In general, when we control for demographic factors, the measured influence of election laws is diminished, compared to the estimates we get when we do not control for demographic factors.

18. Research since Wolfinger and Rosenstone’s work has built on their pioneering efforts.¹⁰ The empirical innovations have been several. For instance, instead of studying a specific year, some of this research has combined data from across several years of VRS studies, so that the longitudinal dynamic of the passage of election laws can be examined. As computational capacity has grown, more demographic and political controls have been added.

⁹ Although the question of whether EDR increases turnout is used here as an example, below (¶ 74) I reference research by Brians and Grofman that addresses precisely this question. The answer is that EDR does increase turnout, even in the presence of statistical controls.

¹⁰ The scholarly literature that followed Wolfinger and Rosenstone has been vast. (Google Scholar reports that *Who Votes* has been cited 2,324 times.) To cite only the most prominent works that also use the VRS to study the influence of election laws on turnout includes the following: Craig Leonard Brians and Bernard Grofman, “When registration barriers fall, who votes? An empirical test of a rational choice model,” *Public Choice* 99, no. 1-2 (1999): 161-176; Brians and Grofman, “Election day registration’s effect on US voter turnout,” *Social Science Quarterly* 82, no. 1 (2001): 170-183; Benjamin Highton, “Easy registration and voter turnout,” *The Journal of Politics* 59, no. 2 (1997): 565-575; Highton and Wolfinger, “Estimating the effects of the National Voter Registration Act of 1993,” *Political Behavior* 20, no. 2 (1998): 79-104; Stephen Knack and James White, “Election-Day registration and turnout inequality,” *Political Behavior* 22, no. 1 (2000): 29-44; Jan E. Leighley and Jonathan Nagler, “Socioeconomic class bias in turnout, 1964–1988: The voters remain the same,” *American Political Science Review* 86, no. 3 (1992): 725-736; Leighley and Nagler, *Who Votes Now? Demographics, Issues, Inequality, and Turnout in the United States*, Princeton, Princeton University Press, 2003; Glenn E. Mitchell and Christopher Wlezien, “The impact of legal constraints on voter registration, turnout, and the composition of the American electorate,” *Political Behavior* 17, no. 2 (1995): 179-202; Jonathan Nagler, “The effect of registration laws and education on US voter turnout,” *American Political Science Review* 85 (1991): 1393-1405; Eric J. Oliver, “The effects of eligibility restrictions and party activity on absentee voting and overall turnout,” *American Journal of Political Science* 90 (1996): 498-513; Peverill Squire, Raymond E. Wolfinger, and David P. Glass, “Residential mobility and voter turnout,” *American Political Science Review* 81 (1987): 45-65; Wolfinger, Highton, and Megan Mullin, “How postregistration laws affect the turnout of citizens registered to vote,” *State Politics and Policy Quarterly* 5, no. 1 (2005): 1-23.

Finally, as interest in the effect of election laws, regulations, and practices has evolved, the effect of different laws has been examined, such as the effect of the National Voter Registration Act (NVRA) of 1993.

19. It is not always possible to study the effect of voter registration laws on the probability that a citizen will vote using individual-level data. The best example of this is when the topic is studied in an international context, where cross-national survey research may be lacking.¹¹ In these cases, aggregate turnout rates may take the place of data on individual decisions to vote (or not), and aggregate measures of demographic and political factors may be substituted for individual measures of the same phenomena. Direct measures of election laws are always included in this type of analysis.

20. About these studies that rely on aggregate data to examine the influence of election laws on turnout, it is important to remark that the *theoretical* model has not changed. Social scientists still think of the problem as understanding how a *typical citizen* of a country or state is affected by election laws. The aggregate analysis provides some insight into the question, but the gold standard is analysis that studies the behavior of individuals — that is the unit of analysis to which all studies of the effects of voting laws strive.

21. My general reading of the Trende Declaration is done in the context of my familiarity with this large scientific body of work on the question of the influence of election laws on voter turnout. It would be unreasonable to assume that an expert report on the likely effects of HB 589 on North Carolina voters would replicate in every detail the established scientific work just reviewed in the case of North Carolina. However, expert reports should be

¹¹ Two highly cited articles in this vein of research include Robert W. Jackman, “Political institutions and voter turnout in the industrial democracies,” *American Political Science Review* 81 (1987): 405-423 and G. Bingham Powell Jr, “American voter turnout in comparative perspective.” *American Political Science Review* 80 (1986): 17-43.

actively informed by the body of established scientific research on the topic. I have already alluded to the hallmarks of that research that are central to this literature: (1) a theoretical grounding in the idea of the “representative citizen” and how he or she is affected by changes in election laws, (2) disaggregation, that is, exploring the effects of *individual* election laws on turnout, rather than lumping them together, and (3) a careful attention to appropriate statistical controls. There are other standards of inquiry that should apply to all scientific research, such as transparency of method, attention to measurement, and care in the interpretation of estimated effects.

22. As a general matter, my reading of the Trende Declaration leads me to conclude that it offers only minor and tangential analysis of the impact of HB 589 on the individual voters of North Carolina, either voters as a whole or voters analyzed separately by race. Therefore, it offers scant insights into whether the *change* in North Carolina’s election laws will cause a *change* in election obstacles faced by individual voters in the state. It certainly provides no insights into whether those changes are likely to be greater for African Americans or whites.

II. The alternative coding of early voting time spans offered in the Trende Declaration in support of Opinion 1 are minor, and result in a conclusion that is identical to that offered in my original declaration.

23. The Trende Declaration includes a long discussion of the details of early voting laws in the various states.¹² As the Trende Declaration rightly notes, coding states with respect to how many days of early voting they allow involves judgment calls.¹³ In the construction of Figure 11 of my declaration, which compares early voting days in North Carolina to other states, I relied on a summary spreadsheet published by the Early Voting Information Center (EVIC), of

¹² Trende Declaration, ¶¶ 26–44, including Figure 1.

¹³ Trende Declaration, ¶ 30.

which Dr. Paul Gronke is the founder and director.¹⁴ I relied on this data because the EVIC is regarded in the election administration community as the most authoritative impartial source of information about in-person early voting in the United States.

24. The information contained on the EVIC Web site is the opening and closing dates of the early voting period in each state. Figure 11 in my declaration graphs these opening and closing dates. To help organize the graph, I also calculated the number of days between the opening and closing dates and termed this the number of days available for early voting (§ 129), using this measure to sort the states. It is certainly true that local election officials regularly are granted some flexibility within these opening and closing dates for the scheduling of early voting, including some flexibility about the specific dates and times when early voting sites will be open, within the overall early voting window set by state law. Variation in details such as this will affect the experience of individual voters. However, such variation *within a state* will be minor compared to variation *across states* in how many days and hours of early voting are available to a typical voter. As a consequence, these details should not affect the broad picture of how early voting laws impact voting across the states.

25. Finally, in conducting my analysis, it was important to measure the opening and closing dates of early voting *in 2012*. The EVIC spreadsheet reports the opening and closing dates for early voting periods for 2012. It is not clear which year is reflected in the coding of early voting time periods that appears in the Trende Declaration, because that information is not reported. For instance, Nebraska enacted a law in 2013 that reduced its early voting period to 30

¹⁴ In my original declaration, I provided a URL link to the EVIC web site that hosts the spreadsheet I relied on. To facilitate direct access to the data, I provide the direct link to the spreadsheet here: <https://docs.google.com/spreadsheet/ccc?key=0AqzPUPKRixWSdGtHSXIMMk1qNEFDRVU4X1U2REg2NIE#gid=0>.

days, which is the time period reported in the Trende Declaration (§ 40).¹⁵ There may be other cases where the divergence in the coding of early voting periods between my report and the Trende Declaration are due to Mr. Trende relying on early voting dates that were changed after 2012.

26. It is appropriate to consider the exercise conducted in the Trende Declaration at paragraphs 32–42 as a “sensitivity analysis” of the account given in my original declaration. A sensitivity analysis responds to the question, “If someone with a skeptical eye makes other assumptions about how to code the data, do the findings hold up?”

27. The answer to this question is an unqualified “yes.” In Figure 11 of my original declaration, I show that before the passage of HB 589, North Carolina’s early voting period was the 19th shortest of the 32 states identified with in-person early voting; after HB 589, North Carolina now ranks 5th shortest among the 32 states. In the Trende Declaration (Figure 1), North Carolina ranked as the 19th shortest among 33 states with in-person early voting before HB 589; after HB 589, North Carolina ranked 5th shortest among 33 states. In other words, the analysis in the Trende Declaration substantively replicates the analysis I provided in my original declaration, illustrated by Figure 11.

28. On net, the disagreements in the coding of states with respect to their early voting windows are minor, and cancel each other out with respect to the overall ranking of states. The EVIC spreadsheet notes that Maine had early in-person voting, but did not provide opening and closing dates of the early voting season. The Trende Declaration codes Maine’s early voting period at 38 days. The Trende Declaration bases its coding of Minnesota on a change to election

¹⁵ Nebraska LB 271, approved by the governor April 24, 2013.
<http://nebraskalegislature.gov/FloorDocs/Current/PDF/Slip/LB271.pdf>.

law that occurred after the 2012 election; Minnesota did not have in-person early voting in 2012, so is properly excluded from the analysis.¹⁶

29. The Trende Declaration correctly codes Virginia as not having early in-person voting as it is commonly understood.

30. With the cases of Maine, Minnesota, and Virginia aside, the following table accounts for all the coding differences between my declaration and the Trende Declaration.

Table 2. Coding decision differences about the number of early voting days in the Stewart and Trende Declarations.			
State	Stewart Declaration	Trende Declaration	Difference
Colorado	12	15	-3
Idaho	43	18	25
Illinois	13	21	-8
Kansas	20	14	6
Nebraska	36	30	6
West Virginia	11	10	1

III. The analysis offered in the Trende Declaration, which compares North Carolina with states that offer no opportunities for in-person early voting, confuses the election administration issues posed by this case.

31. The Trende Declaration attempts to cloud the issue about North Carolina shortening its early voting period by arguing that the length of the early voting periods of states that offer in-person early voting should be directly compared with states that do not allow in-person early voting at all. As a consequence, it is suggested that states without early in-person voting at all should be coded as offering zero days of early voting; therefore the rankings of all states with respect to their early voting periods should be shifted by sixteen positions (by the Trende Declaration's accounting). This would make all states with in-person early voting, even

¹⁶ In the 2012 Statutory Overview Report published by the U.S. Election Assistance Commission (pp. 10–11), Minnesota is listed as “N/A” (Not Applicable) in the table summarizing mail-in voting and early voting laws by state. This indicates that Minnesota is a state that did not permit early voting in 2012.

those with severely restricted numbers of early voting days, appear to offer even more days of early voting than they actually do.

32. The problem with this classification decision in the Trende Declaration is that states that offer in-person early voting operate within qualitatively different election administration regimes than states that do not. Indeed, as a general matter, states that offer in-person early voting usually do not offer it passively — they publicize this voting mode and encourage voters to take advantage of it. Thus, the difference between a state that offers *no* days of early in-person voting and a state that offers a *limited number* of days of early in-person voting is a difference of *kind*, not a difference of *degree*.

33. It is my experience that election administrators generally encourage voters to vote early (when the state allows it) because early voting offers administrative advantages over traditional in-precinct voting on Election Day (“Election Day voting”). If voters who prefer to vote in-person shift to early voting, county boards of elections can often consolidate the number of Election Day voting sites, saving money on facility rentals and staff salaries. Local election boards can also rely more heavily on year-round employees to staff early voting sites, compared to Election Day poll workers who are often poorly trained and paid.¹⁷ Administratively, this means that early voting offers improvements in security, registration list maintenance, and access to the polls for people who need assistance.

¹⁷ See Presidential Commission on Election Administration, *Report* (2014), p. 45, available at <https://www.supportthevoter.gov/files/2014/01/Amer-Voting-Exper-final-draft-01-09-14-508.pdf>.

34. In states that offer in-person early voting, voting equipment that might otherwise be used for Election Day voting is redeployed to early voting centers,¹⁸ as the remaining voting equipment is reassigned to consolidated Election Day precincts.

35. One consequence of states offering in-person early voting is that citizens eventually become habituated to voting in the new mode. This habituation is a large part of what accounts for the steady growth in the number of voters using early voting nationwide, in both on- and off-year elections, since 2000.¹⁹

36. The interaction between administrative procedures, voter expectations, and the encouragement of local election officials has led states to differentiate themselves with respect to the portfolio of voting options they offer their residents. States can be thought of as offering five stylized portfolios of voting modes:

- Traditional Election Day voting, with absentee ballots offered for a limited number of excuses.
- Election Day voting with “no excuse” absentee mail ballots
- Election Day voting with in-person early voting
- Roughly equal reliance on Election Day, by-mail absentee ballots, and in-person early voting
- All vote-by-mail (VBM)

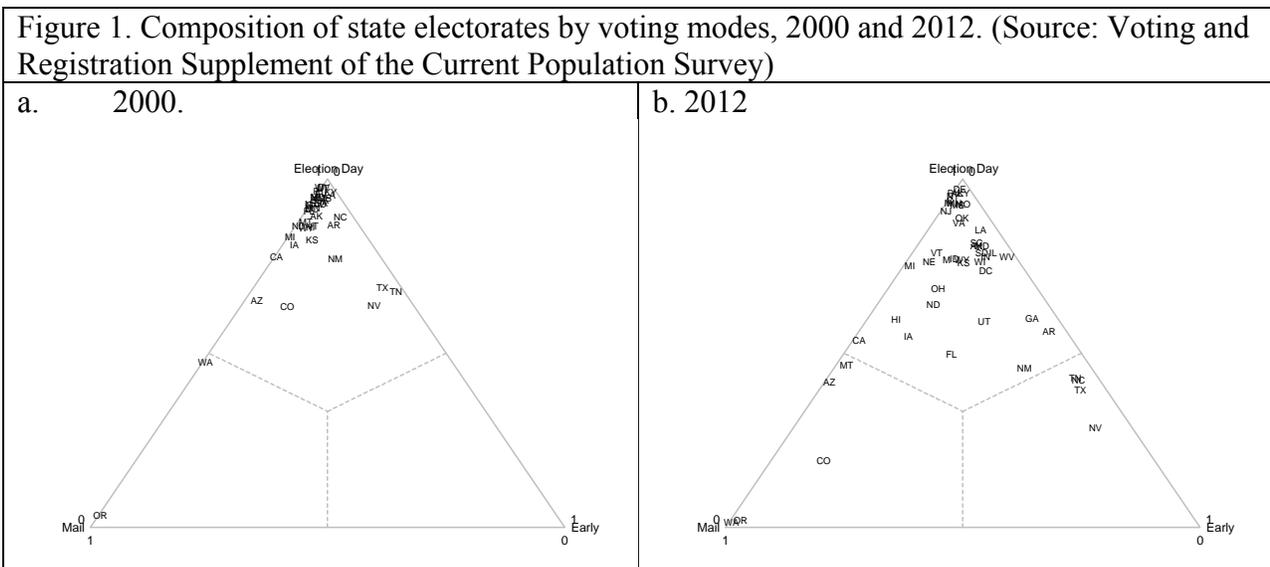
37. The emergence of these portfolios over the past decade-and-a-half is illustrated in Figure 1, below.

38. Figure 1 displays two “ternary plots.” A ternary plot allows one to plot the percentage composition of a whole quantity that can be divided into three parts (in this case, a

¹⁸ Based on conversations with state and local election officials in a number of states over the past several years, it is my understanding that in almost every case, voting equipment used for early voting cannot — and often is not permitted to — be deployed for Election Day voting.

¹⁹ Caltech/MIT Voting Technology Project, *Voting: What has Changed, What Hasn't, & What Needs Improvement*, p. 37. Available at http://vote.caltech.edu/sites/default/files/Voting%20Technology%20Report_1_14_2013.pdf.

state’s electorate), on a two dimensional surface.²⁰ In these particular plots in Figure 1, each state’s electorate is divided into (1) those who reported they voted on Election Day, (2) those who voted early in-person, and (3) those who voted by mail.²¹ If a state is at the very top of the equilateral triangles, then all of its voters reported that they voted in-person on Election Day. If a state is located at the lower left-hand vertex of the plot, then all its citizens reported voting by mail. If a state is located at the lower right-hand vertex of the plot, then all its citizens reported voting early in-person.



39. Drawing our attention to Figure 1a, which shows the composition of voting modes in the 2000 election, note that the bulk of states are clumped together at the top of the graph. This illustrates that in 2000, most states’ voters primarily went to the polls in-person on Election Day. To the degree that states are plotted below the apex of the triangle in Figure 1a, most hug the left side of the triangle, which indicates that in most states in 2000, if citizens did not vote in-person on Election Day, they voted absentee, by mail. Note that almost all of

²⁰ See the following Wikipedia entry for an accessible introduction to ternary plots: http://en.wikipedia.org/wiki/Ternary_plot

²¹ The data plotted in these graphs were generated from the Voting and Registration Supplement of the Current Population Survey, which was described in my original declaration. See paragraph 48 and Exhibit 19.

Oregon’s voters reported voting by mail, owing to that state’s transition to total mail balloting, and that just over half of Washington State’s voters voted by mail — again, because Washington was in the process of transitioning to all VBM. Three states — Nevada, Tennessee, and Texas — stand out as states whose citizens were beginning to abandon Election Day voting in favor of in-person early voting.

40. Drawing our attention now to Figure 1b, we see that by 2012, states had diversified, in terms of how their voters cast ballots. A few states remained clumped at the apex of the triangle, consistent with the maintenance of traditional laws that required voters to vote on Election Day, with few exceptions. Most states had fallen from the apex. Voters in states falling along the left side of the triangle were voting by mail at significant rates. Voters in states falling along the right side of the triangle were voting in person, but during the early voting period at significant rates. A few states (such as Florida and Utah) had fallen straight down the figure, indicating that their voters were using all three voting modes in roughly equal proportions.

41. North Carolina is located with a group of states that includes Tennessee, Texas, and Nevada, in which the modal (i.e., most common) voting mode is in-person early voting.

42. If one is to place states “in a national context,” as the Trende Declaration seeks to do,²² it is necessary to do so in light of qualitative differences and similarities in voting-mode regimes. It may make sense to ask about differences in administrative parameters among the states that have clearly chosen to encourage the early-voting track, but it defies logic to suggest we learn anything by comparing the early voting practices of states whose voters have embraced early voting with states whose voters have not.

²² Trende Declaration ¶ 26.

43. Therefore, to treat states that do not allow in-person early voting as if they *do* offer in-person early voting — only the number of early voting days is set to zero — results in an apples-to-oranges comparison among the states.²³

44. Previously I noted that the interstate comparison of election laws is relevant only if the comparison illuminates the effect of *changes* in North Carolina's election laws on *changes* in the experience of North Carolina voters. The reason why it is relevant to compare the number of days of early voting in North Carolina to other states is so that we can gain insight into the likely effects of HB 589 on North Carolina's voters.

45. In my original declaration, I note that the reduction in early voting days wrought by HB 589 is similar to the reduction in early voting implemented in Florida in 2012. Before Florida made its change, it offered 14 days of early voting, which is in line with what North Carolina had before HB 589 passed. Also note, from Figure 1b, that Florida is one of the states with the greatest proportion of early voters, ranking eighth overall in 2012. As I note in my original declaration, a bipartisan consensus emerged in the Florida legislature after the 2012 election that this reduction in early voting days led to serious problems in the administration of early voting, the most visible of which were hours-long lines to vote early.²⁴

46. North Carolina had *more* early-voting than Florida did in 2012, measured by shares of their respective electorates. The reduction in early voting days in Florida was largely regarded to be a disaster for that state's voters, and for the reputation of the state's ability to manage elections. This interstate comparison is relevant only because it gives us insight into the likely effects of HB 589 in North Carolina — not because it informs us about whether North

²³ The Declaration of Donald Schroeder also falls prey to this inferential fallacy. (See Schroeder Declaration, ¶ 9b.)

²⁴ Stewart Declaration ¶¶ 196, 210.

Carolina is more-or-less in the “mainstream” of states in an irrelevant comparison of state election laws.

IV. The analysis offered in support of Opinion 1 in the Trende Declaration is ad hoc and unrelated to the issues involved in this case, because it offers no statistical analysis concerning racial disparities that are likely to arise on account of implementing HB 589.

47. The analysis pertaining to Opinion 1 in the Trende Declaration appears to be in service of the conclusion contained in paragraph 61, which is that North Carolina during the decade of the 2000s “effectively created an outlier in United States election law, at least insofar as the laws at issue in this litigation are concerned.” However, as far as this statement goes, it is simply a statement of how Mr. Trende has coded an independent variable that is likely to be related to turnout rates and barriers to voting, particularly in North Carolina. It is not a statement about whether North Carolina’s change in election laws reflected in HB 589 will likely reduce turnout and/or increase obstacles to voting, which is the focus of the scientific literature reviewed in paragraphs 8–21, nor is it a statement about whether these obstacles will affect African Americans and whites disparately.

48. One can understand the review of election laws in Opinion 1 to be an attempt to comport with the standards of the canonical method of studying the effect of election laws on voter turnout reviewed above by creating a measure of how “liberal” or “restrictive” those laws are. As a measure of state law restrictiveness, the measure offered in the Trende Declaration falls short because it is not a systematic, comprehensive review of *all* laws or practices that might be implemented by a state to encourage greater turnout by its voters. For instance, it does not code whether a state has Election Day Registration (which North Carolina has never had), despite the fact that research has regularly shown it to have a strong positive influence on

registration and turnout rates.²⁵ Nor does the coding include whether a state has a “permanent” absentee ballot list or automatically mails absentee ballot applications to all voters — practices North Carolina does not use, but which are used in other states. The coding does not take into account whether states have Election Day “vote centers” (which North Carolina does not have), despite the fact that some research has shown that vote centers can enhance the voting convenience, improve election administration, and increase turnout.²⁶

49. Thus, the measure offered in the Trende Declaration of the number of laws at issue in this case is a poor measure of how liberal or restrictive election laws are in a state. Because of how the measure is constructed, all it does is tell us which state is the most like North Carolina. It is unsurprising that the state most like North Carolina on this measure is North Carolina.

50. A more relevant question to ask is whether the types of laws at issue in this litigation are associated with higher turnout levels, and whether minority voters are more likely to be aided by these laws than whites. None of the analysis offered in the Trende Declaration addresses differential effects. As I discuss below, the analysis offered in the Trende Declaration about whether these laws influence the turnout of minority voters does not compare the effect of the laws on white voters. To the degree the analysis has a statistical component, it bears almost none of the marks of the canonical method in this field that I reviewed in paragraphs 8–21, and would be unacceptable for publication that required peer review.

V. The analysis offered in support of Opinion 2 of the Trende Declaration mischaracterizes the framing of the analysis in my declaration, and inappropriately

²⁵ See Brians and Grofman, “When registration barriers fall”; Brians and Grofman, “Election day registration’s effect on U.S. voter turnout”; Leighley and Nagler, *Who Votes Now?*; *Knack and White, op. cit.*

²⁶ Robert M. Stein and Greg Vonnahme, “Engaging the unengaged voter: vote centers and voter turnout,” *Journal of Politics* 70 (2008): 487–497.

assumes that if a law does not result in a reduction of registration or turnout, it has not presented an obstacle to voting.

51. The Trende Declaration begins Opinion 2 by claiming that I contend that the termination of same-day registration would result in a “diminution in African American registration.” (Trende Declaration, ¶ 62) While this is one possible effect of the termination of same-day registration, along with other provisions of HB 589, my analysis made few direct predictions about the resulting number of African American voter registrations under HB 589. My analysis was primarily aimed at the imposition of *disparate obstacles* before African American and white voters in North Carolina by the implementation of HB 589. Those obstacles are generally measured by the percentage of voters in each racial group who will have to face them. Changing registration rates is one measure of the degree of obstacles, but it is not the only one.

52. Experience in the United States and abroad testifies to the ability of voters who face voting obstacles to overcome them in some circumstances, so that they manage to register and/or vote. This does not diminish the fact that an obstacle to vote or register is an obstacle, and that if African Americans are more likely than whites to have to face the obstacle in order to register or to vote, then the obstacle is greater for blacks than it is for whites.

53. The Trende Declaration similarly mischaracterizes my declaration by suggesting that the only outcome I am concerned with is turnout. For instance, the Trende Declaration in paragraph 66 lumps all the changes to North Carolina election law embodied in HB 589 together as part of the measurement of a “liberalized voting regime.” The declaration then claims that my analysis suggests that *all* the changes wrought by HB 589 will predictably lower turnout by African Americans. That is an incorrect generalization of my analysis which limits the range of effects that obstacles might produce.

54. For instance, my analysis of the early voting restrictions focuses on the obstacles that will be placed in front of early voters, who will be required to vote in early voting centers that are likely to become even more congested than they were in 2012. The result will likely be longer waits to vote in the future. These longer waits are a cost imposed on voters *who continue to turn out to vote*. Again, reduced turnout, while a serious consequence of HB 589, is not the only consequence.²⁷

55. The Trende Declaration (¶ 63) faults my analysis for failing to account for voters adjusting their behaviors as a consequence of the obstacles imposed by HB 589. This claim mischaracterizes my declaration.

56. For instance, I discuss at length research conducted by Dr. Paul Gronke and myself about the adjustments that Florida voters made when a similar set of barriers to early voting were passed in that state.²⁸ In that analysis, I note that *some* early voters from 2008, when faced with a restricted set of days in which to vote early in 2012, switched to other modes of voting, or voted later in the early voting period. Other voters — most often those who had previously voted late in the 2008 early voting period — did not vote at all in 2012.

57. Some people who wish to vote in future North Carolina elections will attempt to adapt their behavior to the voting obstacles thrown up by the North Carolina legislature by HB 589. Some who attempt to adapt will be successful in voting, others will be unsuccessful. It is incorrect to suggest that because someone overcomes a voting obstacle, as is suggested in paragraph 63 of the Trende Declaration, no obstacle exists.

²⁷ The portion of the analysis offered in my original declaration that addresses turnout most directly is in paragraph 122, where I discuss the turnout rates of same-day registrants. I note that same-day registrants vote at a rate approaching 100%, compared to registrants who register right before the traditional registration deadline, who only vote at an 80% rate. This is a clear case in which a provision of HB 589 — the elimination of same-day registration — will certainly result in lower turnout.

²⁸ Stewart Declaration, ¶¶ 197–204.

58. In paragraph 65 of the Trende Declaration, it is claimed that “[p]laintiffs’ experts also conflate the midterm electorate with the presidential electorate, notwithstanding the fact that the two electorates have become very different.” This statement, too, mischaracterizes my original declaration.

59. In paragraph 77 and Exhibit 27 of my declaration I note that it is critical to draw comparisons across similar elections, in order to estimate the effects of a policy change on outcomes such as turnout or registration rates. In other words, to properly understand how a change in election laws affects a change in behavior, we need to hold the type of election constant. In Exhibit 27, I include an extended discussion of the difference between presidential election years and midterm congressional years — a discussion that goes beyond that offered in the Trende Declaration.

60. As I noted in my original declaration, the most important thing to keep in mind about the difference between presidential and congressional midterm elections in North Carolina is that midterm elections do not always have contested statewide races on the ballot. When there are no contested statewide races on the ballot, turnout drops.

VI. The analysis in support of Opinion 2 of the Trende Declaration contains a simplistic attempt at statistical analysis to account for how changes to election laws influence voter turnout. This analysis is inconsistent in method and approach with that found in the scientific literature on the topic.

61. Beginning at paragraph 67 of the Trende Declaration, and running to paragraph 135, there is what appears to be an attempt at statistical analysis to account for the impact of “liberalized” election laws on the turnout of African Americans in North Carolina. This analysis first introduces the Voting and Registration Supplement (VRS) of the Current Population Survey

as a data source that allows us to study the effect of election laws on voter turnout.²⁹ In-and-of-itself, the use of the VRS to study the influence of election laws on turnout is unremarkable, because the core findings in the scientific literature are based on research that uses this data. (See footnote 10 above.) However, this is where the comparison of the analysis offered in paragraphs 67–135 in the Trende Declaration with the research covered in paragraphs 8–21 above ends.

62. The analysis appearing in paragraphs 67 to 135 of the Trende Declaration bears few of the marks of the established scientific research on the topic. Rather than conduct individual-level, multivariate statistical analysis that seeks to explain why *individual* Americans registered and/or voted, as a function of the specific election laws in their states, the analysis contained in the Trende Declaration offers up, first, a series of highly aggregated time trends about turnout rates that pertain to a limited number of states. (The standard is to look at all states if one is doing cross-state analysis.) It then draws conclusions about the relationship between the “permissiveness” of voting regimes by conducting an informal review of African American turnout rates compared to an informal classification of states as “middling, low, and high” on a scale of voting regime permissiveness. (See, for instance, ¶ 79.³⁰)

²⁹ Paragraph 69 of the Trende Declaration is incorrect in claiming that “voters are *required* to respond to the CPS” [emphasis added]. Participation in the CPS is voluntary. The Census Bureau undertakes a monumental effort to ensure that CPS panelists continue to respond to the various waves of surveys that are part of the CPS. In recent years, the nonresponse rate to the CPS has ranged between 8% and 9% of contacts. See <http://www.census.gov/cps/methodology/collecting.html>.

³⁰ The logic of the comparison in paragraph 79 of the Trende Declaration is the following. First, choose the three states with the highest African American turnout rates and record the “permissiveness” of the voting regimes in these states. Next, choose the three states with the lowest African American turnout rates and record the “permissiveness” of the voting regimes in these states. Finally, see if the high-turnout states have more permissive regimes than the low-turnout states. If they do, then we would conclude that “regime permissiveness” was positively related to African American turnout rates. There are two serious flaws with the logic of this approach as applied to paragraph 79. First, the analysis only uses six observations in the dataset; the accepted principle in statistical analysis is to use all the relevant data at one’s disposal. Second, the method of choosing states to compare based on whether they have extreme values of the outcome (dependent) variable is called “sampling on the dependent variable,” which is regarded as one of the most common (yet avoidable) research design flaws in the

63. Furthermore, some of the coding of states, in terms of their “permissiveness,” would surprise most experts on election administration. For instance, in paragraph 79, Wisconsin’s voting access laws are termed “middling,” despite the fact it offers Election Day registration, which is the most liberal form of voter registration law available, short of North Dakota’s lack of voter registration laws altogether.

64. Also troubling, when statistical analysis is conducted in this section of the Trende Declaration, it fixates on the statistical precision of how well the data fit to what could be considered control variables, without addressing the influence of the election laws themselves.

65. One example of this is found in paragraphs 87 and 88 of the Trende Declaration. In these paragraphs, the Trende Declaration notes that the turnout rate of African Americans has grown steadily compared to the white turnout rate since 1980. To establish this claim, the Trende Declaration offers the results of a simple OLS regression analysis, in which the “participation rate” of African Americans (as a fraction of the white “participation rate”) is explained using the year as the independent variable. The Trende Declaration trumpets a “variance explained” level of 81.5 percent. The inference one is apparently supposed to draw from this analysis is that because African American participation rates have grown steadily over time, it is impossible for election laws to have had an influence on rising African American participation.

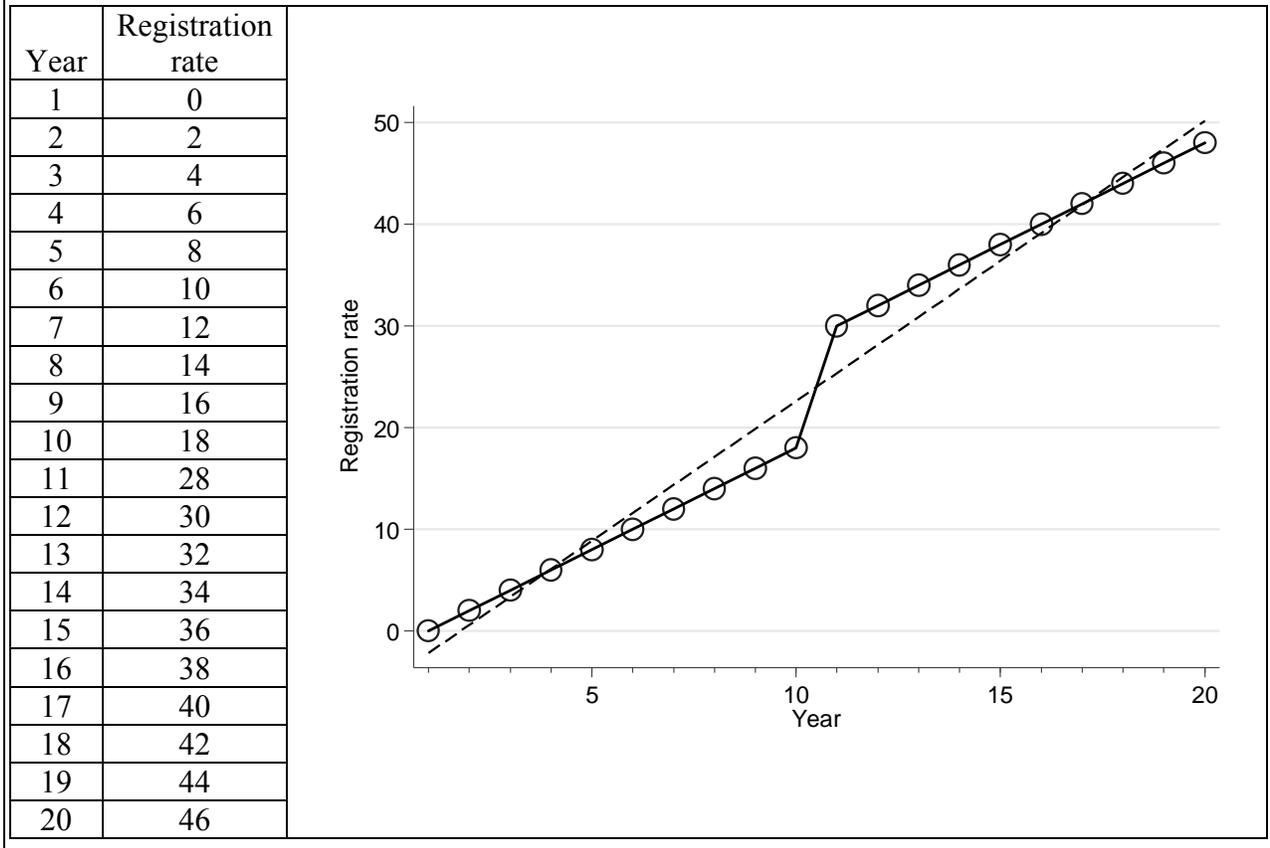
66. Nothing can be further from the truth. To see this point, and the faulty inferential logic of this analysis in the Trende Declaration, consider the following hypothetical. Imagine we are interested in studying whether a program established in a state to encourage more Hispanics to register to vote has been effective, in a context in which Hispanic voter registration has been

social sciences. See Gary King, Robert O. Keohane, and Sidney Verba, *Designing social inquiry: scientific inference in qualitative research*, Princeton: Princeton University Press, 1994, chap. 4.

growing steadily over time in the absence of such a program. In this example, suppose that in Year 1, no Hispanics are registered to vote. In Year 2, due to a rising interest in politics among Hispanics, rising educational levels, and individual mobilization efforts, the registration rate rises to 2%. In Year 3, for similar reasons, the registration rate rises another two points to 4%. Similarly, each year for ten years, because of reasons that are attributable to individual demographic factors, Hispanic registration rates continue to rise by two percentage points over the previous year. By Year 10, the Hispanic registration rate has risen to 18%. Between Years 10 and 11, a program is established to publish more registration materials in Spanish, and outreach programs are targeted to Spanish-speaking communities. As a consequence of this law, there is a one-time surge in the registration rate of an additional 10 percentage points. Thus, in Year 11, the registration rate grows to 30% (i.e., 10 percentage points higher than Year 10 because of the program plus 2 percentage points higher because of the long-term growth rate in registrations due to individual demographic factors). In the years that follow, the older pattern of growth by 2 percentage points each year continues without any further one-time surges.

67. Figure 2 below contains a chart that shows the registration rate of Hispanics each year in the example just described, along with a graph that plots the registration rate each year. Finally, the “best fit” linear regression line that describes this time series is also graphed using a dashed line.

Figure 2. Fit of a linear trend to a hypothetical time series describing the growth of Hispanic registration rates that are caused by a linear trend and a one-time policy change.



68. Note that the dashed line approximates the graph very closely; the “variance explained” by this regression line is 97.6%.³¹ Nonetheless, the effect of the change in law between years 10 and 11 is also apparent in the graph — the linear regression line fit to the data fails to account for the effects of the law, despite the fact that the r^2 statistic, which measures “variance explained,” is very close to 100%.

69. This simple example helps to illustrate why it is that most instructors I am familiar with who teach statistical methods to political science students, both graduate and undergraduate, strongly caution their students against using r^2 (“variance explained”) to draw

³¹ In other words, when we run a regression with the registration rate as the dependent variable and year as the independent variable, the r^2 statistic is .976.

substantive conclusions. Just because a straight line can closely approximate the data in a scatterplot does not tell us anything about whether deviations from the long-term trend are associated with changes to policy or legislation.

70. Finally, it should be noted that if a multiple regression were conducted on the data illustrated in Figure 2, accounting for levels of Hispanic registration (the “dependent variable”) using *both year and* a change in law as the two independent variables, the “variance explained” would jump to 100%. Furthermore, the regression coefficient that results from this analysis would properly record that after Year 10, registration rates were 10 points higher than before; the coefficients from the regression analysis would also uncover the fact that there was an additional 2 percentage point increase in registration rates each year. This would be the appropriate use of linear regression to study the effect of a policy change on outcomes — the answer is found in the regression *coefficients*, not in goodness-of-fit statistics such as r^2 .

71. In short, reporting in paragraphs 87 to 90 of the Trende Declaration that there are linear trends in registration rates of African Americans over the past three decades in North Carolina has no bearing on the question of whether North Carolina’s election laws affected these registration rates, and whether the laws had differential effects on African American and white voters. This analysis is not conducted according to standard scientific protocols in the study of the effect of election laws on registration and turnout rates, and hence is not informative for understanding these effects.

VII. The analysis of the Trende Declaration which suggests that turnout in North Carolina grew because it has become a “target state” is not based on evidence that is regularly used in the scientific analysis of this topic.

72. Paragraphs 91 to 116 of the Trende Declaration consist of an ad hoc review of evidence which suggests that voting turnout in North Carolina grew in the past decade because

of the emergence of North Carolina as a “target state,” which is more commonly called a “battleground state.” While some of this analysis is based on a synthesis of peer-reviewed scholarly literature, many of the citations are to news accounts and blogs — sources that may help generate hypotheses in social science research, but are rarely relied on to rigorously establish statistical patterns.

73. Most importantly, however, there is no dispute that North Carolina was a battleground state in the 2008 and 2012 presidential elections. The question is whether the effects of election laws are discernible after controlling for competitive factors that may also affect turnout levels.

74. Competitiveness in a state is certainly a factor that should be taken into account when assessing whether election laws also have an effect on turnout rates. Indeed, in their highly cited study on the effect of Election Day Registration (EDR) on turnout in each presidential election from 1972 to 1996, Brians and Grofman included a control for the degree of electoral competitiveness in states as part of their analysis.³² Competitiveness — which was measured by how close the Democratic-Republican vote share was to a 50/50 split in the respondent’s state in the year of the survey — had a statistically significant influence on whether respondents to the survey reported that they turned out to vote. Still, after controlling for whether a respondent lived in a “battleground state,” whether a state had EDR was also shown to be a statistically significant influence on turnout.³³

³² Brians and Grofman, “Election Day Registration’s Effect on U.S. Voter Turnout,” *op. cit.*

³³ In addition to controlling for state partisan competitiveness, the Brians and Grofman article also controls for age, education, income, employment status, marital status, sex, race, year of the election, whether the state’s voter registration deadline is close to Election Day, and a measure for how actively the state’s department of motor vehicles assisted in registering new voters. It should also be said that while *Election* Day Registration is different from *Same* Day Registration, which is what North Carolina had before HB 589 passed, in that EDR is allowed on Election Day while SDR happens during early voting before Election Day, findings pertaining to the relationship between EDR and turnout are relevant in considering the relationship between Same Day Registration and turnout.

75. None of the analysis offered in paragraphs 91 to 116 helps to provide an estimate of the effect of early voting, same-day registration, and out-of-precinct provisional balloting on registration or turnout rates in North Carolina, after controlling for electoral competitiveness. The closest this section comes to tying competitiveness to the effects of specific laws changed by HB 589 is in paragraphs 108 and 109 of the Trende Declaration, where early voting is discussed. However, all these two paragraphs do is provide a quote from a history of the 2008 campaign that supports the idea that early voting was a *target* of the Obama campaign. That quote, however, says nothing about targeting of *African Americans*, or about the targeting of African Americans in North Carolina. The quote supports a general claim about campaign strategy that has no particular bearing on North Carolina. At best, the claim made in paragraphs 108 and 109 offers an intriguing hypothesis that could, in principle, be tested within the structure of the standard statistical models used to test the effects of election laws on registration and turnout. Such analysis is not offered in the Trende Declaration, however.

76. Paragraphs 117 to 125 claim to provide a comprehensive cross-state analysis of whether the election laws changed by HB 589 are associated with an increase in African American turnout across 34 states from 2000 to 2012.³⁴ I interpret the analysis in paragraphs 117 to 125 to be an attempt to follow in the tradition of the research spawned by the early work of Wolfinger and Rosenstone discussed above. However, when compared to the significant political science literature that examines the effects of election laws on voter turnout using VRS

³⁴ Why these 34 states were chosen is not clear. The Trende Declaration claims that these 34 states are the only one for which “the CPS has an unbroken data series of African American voter participation between 2000 and 2012.” (§ 118) It is not clear what is meant by this statement. In my examination of this data, there are black respondents in each state’s sample who responded to the turnout question in the 2000, 2004, 2008, and 2012 versions of the CPS/VRS. Some of the sample sizes are very small in particular years — only 1 black respondent in Montana in 2012, for instance. Therefore, the Trende Declaration probably uses some cut-off point based on the number of African American respondents, but that cut-off is not reported. If the Trende Declaration had analyzed individual responses to the CPS/VRS, it would have been possible to analyze respondents from all states without worrying about the problem of a small number of African Americans in the sample from some states.

data, some of which was cited above in footnote 10, the analysis offered in these paragraphs of the Trende Declaration deviates from the basic principles of that research, and does not add to a scientific understanding of the issue.

77. There are many flaws in the analysis contained in paragraphs 117 to 125. I briefly note just four here. First, if we are to understand whether there are disparate effects of election laws based on race, we need a comparison of the *relative* differences in black and white participation rates across states. There is no such analysis in this section, only a report of changes in African American participation without any comparison with white participation.

78. Second, as noted above, it is common to add demographic and political controls when accounting for the effects of election laws. However, in the analysis offered here, the Trende Declaration examines only a “few basic controls”: whether there was a change in how “contested” a state was between 2000 and 2012³⁵ and African American turnout in 2000. A cursory review of the literature cited in footnote 10, above, would reveal that many more controls are typically entered in studies of this subject, with the two most basic being age and education. Furthermore, the method of controlling for other factors in the Trende Declaration is not truly multivariate — single controls are explored one at a time. Thus, in sum, the strategy of applying statistical controls is highly unusual.

79. Third, the causal sequencing of the variables considered in this section of the Trende Declaration is highly unorthodox. In the best of circumstances, we would observe a change in election laws at one point in time (often termed “time *t*” in the social sciences) and

³⁵ Mr. Trende codes states according to their competitive situation in 2000 and 2012 based on his “experience as a psephologist [i.e., scientific analyst of elections] and knowledge of United States elections.” (¶ 122) In all the scientific analysis of the effect of election laws on registration and turnout of which I am aware, when competitiveness is entered in as a control, the measurement is based on objective criteria that are explained to the reader and readily reproducible. The standard objective measure is based on how close a state’s presidential election returns are to a 50/50 split. See Brians and Grofman, *op. cit.* and Leighley and Nagler, *Who Votes Now?*, chap. 4.

then measure a change in registration or turnout in a future point in time (time $t + 1$). Often we do not observe this temporal ordering, but rather measure the presence of laws and turnout levels at the same point in time. This allows us to establish a correlation between election laws and turnout levels, but conclusions about causal relationships must be treated cautiously.

80. The Trende Declaration's approach to temporal sequencing of the input and output variables is entirely different from what one typically encounters. The outcome variable (African American turnout) is measured as a *change* from 2000 to 2012. The independent variable is the state of election laws *at the end of the period*, in 2012. Therefore, we are left with the odd situation in which an outcome is associated with an independent variable in such a way that the change in turnout, which is the outcome variable, temporally precedes the laws that are being tested for having a causal effect on the outcome. For proper causal analysis, it should be the other way around.

81. Fourth, the analysis is confined to 34 states, which reduces the statistical power of the tests dramatically. The ability to discern the effects of election laws on registration and turnout rates depends on the ability to estimate effects that are in the range of 3-to-4 percentage points. Such precision typically only comes with sample sizes that run into the thousands of observations. That is why the canonical approach to the study of the effects of election laws on registration and turnout proceed from such large data sets, analyzing tens of thousands of individuals.

VIII. The Trende Declaration misrepresents the research conducted by Dr. Paul Gronke and myself into how Florida voters responded to a restriction of early voting days in 2012, by mischaracterizing voters' commitment to early voting as voters' commitment to voting at all by any method.

82. The Trende Declaration addresses research that Dr. Paul Gronke and I conducted into how Florida voters responded to a restriction of early voting days in that state — restrictions that are similar to those imposed by HB 589.

83. The Trende Declaration begins the consideration of our research by misrepresenting a characterization we make of early voters in Florida. In paragraph 139 of the Trende Declaration, it is claimed that “Dr. Stewart notes that the decline did not actually come from the voters who voted on the days where early voting was cut. These voters were the most committed voters.” Stating that Dr. Gronke and I said that the earliest of the early voters in Florida were “the most committed voters” misrepresents what we wrote. Here is the complete sentence that the Trende Declaration is referring to:

The earliest early voters in Florida in 2008, plus those who voted in the middle weekend, were the most committed to this mode of voting; in 2012, they were the *least likely* to be deterred by reports of long waits to vote early once the early voting centers opened. [emphasis in original]³⁶

84. The Trende Declaration misconstrues our statement about voters who were the most committed *to early voting* into a one that makes it appear that we claim these were the most committed voters in the Florida electorate.

85. In short, what we characterize as a commitment to a *mode of voting*, the Trende Declaration morphs into a statement about commitment to voting itself. By inference, this disparages the civic commitment of those who voted later in the early voting period, as well as others who voted using other modes.

86. Furthermore, paragraph 140 of the Trende Declaration implies that Dr. Gronke and I characterize voters who voted in 2008 but not in 2012 as “less motivated,” citing paragraph 206 of my original declaration. There is nothing in that paragraph about voters being more-or-

³⁶ Stewart Declaration, ¶ 203.

less motivated as a consequence of when or how they voted. Rather, paragraph 206 of my original declaration suggests that the empirical patterns seen in Florida in 2012 are likely to repeat in North Carolina in 2016: the earliest early voters will be forced to vote in early vote centers that will be even more congested than they were in 2012. This may *deter* some from voting, “out of fear that the lines to vote will be unreasonable.”

87. There were reports in Florida during the 2012 election season of lines to vote at the early voting centers of up to five hours on the last Saturday of the early voting period.³⁷ There are many explanations besides “lack of motivation” for why some people would choose to forego voting under these circumstances, such as the press of family obligations or job responsibilities.

88. To conclude, very little in the Trende Declaration is relevant to the questions at issue here, which are narrowly focused on the likely effects of *particular laws* upon *individual* voters in North Carolina. The empirical issues also revolve around analysis that directly compares recent behavior of African American and white voters in the state. The analysis offered in the Trende Deposition is, by-and-large, aggregated at a very high level, inattentive to direct comparisons of blacks and whites, and lacking of the degree of methodological rigor associated with scientific studies of the effects of election laws on outcomes such as voter turnout.

MATTERS RELATED TO THE THORNTON DECLARATION

IX. The observation in the Thornton Declaration that factors other than changes to election laws may influence individuals to vote does not diminish the fact that North Carolina’s election laws do have an influence on voter turnout.

³⁷ Stephen Ansolabehere and Charles Stewart III, “Waiting in Line to Vote,” White Paper prepared for the Presidential Commission on Election Administration, July 28, 2013, p. 9, available at <http://vote.caltech.edu/sites/default/files/WP%20114.pdf>.

89. Just as with the Trende Declaration, much of the Thornton Declaration is devoted to suggesting that factors other than election laws may influence turnout decisions in North Carolina. (See, for instance, Thornton Declaration, ¶ 20.) As I previously noted (¶¶ 14–17), the scientific literature on turnout that systematically examines the tie between election laws and voter turnout also controls for non-legal factors, such as voter demographics, electoral dynamics particular to an election, and the partisan competitiveness of states, as a matter of course. In all the studies previously cited, after accounting for these other demographic and political factors, election laws still had measurable effects on turnout. To state that “the decision by individuals to register to vote and to participate in voting are not solely tied to the availability of one-stop voting”³⁸ is arguing with a straw person.

90. In addition, the Thornton Declaration uses the growth of North Carolina’s voter registration in the 1990s — before North Carolina passed the election laws that were repealed or revised by HB 589 — as evidence that something other than North Carolina’s voting laws must explain the growth in North Carolina’s registration rates.³⁹ What goes unsaid is that the most likely explanation for the rapid rise in North Carolina’s registration rate in the 1990s is passage of the federal National Voter Registration Act (NVRA), which resulted in a liberalization of voter registration procedures in some states.

91. In research published in 1995 — in other words, immediately after the passage of the NVRA — about the impact of legal constraints on voter registration rates, Mitchell and Wlezien show that North Carolina at that time had among the most restrictive registration laws in the nation, and therefore was one of the states that would likely experience the greatest gains in

³⁸ Thornton Declaration, ¶ 20.

³⁹ Thornton Declaration, ¶¶ 20, 21.

registration rates were restrictive registration laws to be loosened up.⁴⁰ If it is in fact the case that the NVRA is responsible for the rapid rise of voter registration rates in North Carolina in the 1990s, this hardly refutes the impact that laws that were passed later had on raising North Carolina's registration rates even further.

X. The fact presented in the Thornton Declaration that census tracts with early voting sites had a statistically significantly higher percentage of African Americans among the voting age population does not undermine the conclusion that HB 589 will have a disproportionate impact on blacks compared to whites.

92. In paragraphs 25 to 34 of the Thornton Declaration, it is reported that the census tracts in which early voting sites are located tend to have higher percentages of African American residents than census tracts that do not have early voting sites. This pattern is fully consistent with my conclusion that African Americans will face greater obstacles by the restriction of the number of early voting days than whites. *If the location of early voting sites does not change in the future*, the location of early voting sites is a constant; therefore, early voting site location cannot have an influence on the differential burden of the early voting restrictions. Stated another way, Africans Americans, more than whites, will end up voting in the same early voting sites that they used in 2012, only now they will be more congested.

93. The analysis presented in paragraphs 25 to 34 of the Thornton Declaration concerning the location of early voting sites reaches similar conclusions to the analysis offered in paragraphs 64 to 74 of the Hofeller Declaration, insofar as the Hofeller analysis shows correlations between the siting of early voting centers and African American population. As with the analysis in the Thornton Declaration, this analysis in the Hofeller Declaration is

⁴⁰ Glenn E. Mitchell and Christopher Wlezien, "The impact of legal constraints on voter registration, turnout, and the composition of the American electorate," *Political Behavior* 17 (1995): 179–202. According to Mitchell and Wlezien, only Arizona, Georgia, and New Mexico had more "room to grow," in terms of liberalizing registration laws in order to increase turnout.

consistent with my conclusion that the obstacle imposed by restricting the number of early voting days will have a greater impact on African Americans than on whites.

XI. The differences in average wait times for early voting in North Carolina are greater than the national average; the difference is statistically significant at traditional levels used in the social sciences.

94. The Thornton Declaration notes at paragraph 37 that I do not provide a “margin of error” for the 5.9% point-estimate I calculate of the percentage of North Carolina respondents who waited “more than 1 hour” to vote in Table 9 of my declaration (p. 74). Dr. Thornton then attempts to calculate the 95% margin of error, but does so incorrectly.⁴¹ (I had made this estimate using data from the Survey of the Performance of American Elections [SPAЕ].) The correct margin of error is ± 3.4 percentage points.⁴² Thus, the true population proportion has a 95% probability of lying within the interval of 2.5% to 9.3%. However, it bears noting that according to the “central limit theorem” which allows us to make inferences about population statistics when we have only a sample of the population to go on, the most likely population value of this proportion is 5.9%.

95. The Thornton Declaration notes, at footnote 35, that the number of observations in Exhibit 42 of my declaration vary slightly from the counts in Table 9, whereas the number of observations should be identical. Upon a review of the output of my analysis, I have discovered that the counts in Table 9 are correct. I have attached to this report, as Exhibit 1, an update of the original Exhibit 42, which now reflects the correct number of counts that are associated with all the proportions in the exhibit.

⁴¹ If p is the proportion for which we are trying to construct the margin of error, and n is the number of observations used to calculate the proportion then the formula for the 95% margin of error is the following: $1.96 \times \sqrt{\frac{p(1-p)}{n}}$.

⁴² That is because, using the formula from the previous footnote, $1.96 \times \sqrt{\frac{0.059(1-0.059)}{188}} = 0.034$.

96. Although the Thornton Declaration attempts to cast doubt on whether one can trust the statistical estimates of wait times based on only 146 Election Day and 188 early voters, the fact is that statistical tests of significance factor in sample size, to establish how precisely the estimate produced by analyzing a sample approximates the corresponding population statistic. The larger the sample size, the greater the precision. However, as the central limit theorem establishes, the sample mean approaches the population mean very rapidly as the sample size grows. It approaches so rapidly, that the rule of thumb often used in applied statistics is that once the sample size exceeds 20 (so long as the sampled observations are independently drawn), the sample mean is likely very close to the population mean.

97. However, my conclusion that early voting wait times in North Carolina were longer in 2008 and 2012 than the national average is not based only on the percentage of North Carolina early voters who reported waiting more than an hour to vote, as is suggested in paragraph 37 of the Thornton Declaration. Instead, it is based on an analysis that compares all early voters in the North Carolina sample with all early voters in the remaining sample of the SPAE. This comparison has a sufficiently large number of observations to establish with precision whether the average wait times of the two groups are statistically significantly different from each other. Regardless of how the statistical test is performed, we can conclude that North Carolina early voters very likely waited longer to vote than early voters in the rest of the country.

98. We can first construct a *chi-squared* test of whether the distribution of early voting wait times in Table 9 of my declaration for North Carolina likely came from the same distribution of early voting wait times for the remaining sample of early voters nationwide. According to this *chi-squared* test, we can conclude that the probability that the nationwide

distribution of early voting wait times and the North Carolina distribution of early voting wait time came from the same population is less than 0.00006%.⁴³

99. Second, we can conduct a multiple regression analysis to test whether average values of the wait-time response for early voters in North Carolina are greater than the average values of the wait-time response for early voters in other states. When we run that regression, we discover that the probability that North Carolina early voters waited longer (on average) than early voters nationwide is 99.9999%.⁴⁴

XII. The Thornton Declaration mischaracterizes my analysis of the total number of early voting hours in counties, and performs an analysis that leaves an incorrect impression that she has conducted an independent analysis that is similar to my own.

100. The Thornton Declaration states that I “purport[] that the hours open in each county is related to the size of the population. However, I also found the hours open are statistically significantly correlated with having a higher proportion African-American VAP in a county.”⁴⁵

101. First, this characterization of my analysis of the total number of early voting hours available in each of the 100 counties in North Carolina implies that I did not analyze predictors of early voting hours other than county size. This is incorrect.

⁴³ More formally $\chi^2 = 34.3$ with 4 degrees of freedom; $p < .0000006$.

⁴⁴ The following reports the regression output. Note that the t -statistic for the coefficient associated with being a North Carolina voter is much greater than 1.96, which is the threshold for being within the traditional 95% confidence interval used in the social sciences.

Variable	Coefficient	t -statistic
Voter is from North Carolina	0.44	4.98
Intercept	2.18	103.1
N = 3,330		
$r^2 = .01$		
s.e.r. = 1.19		

⁴⁵ Thornton Declaration, ¶ 35.

102. In Exhibit 45 of my report, I perform a multiple regression that predicts the total number of hours available for early voting in each North Carolina county in 2012 in terms of (1) the number of registered voters in the county, (2) the percentage of the vote received by President Obama in 2012, (3) the percentage of the county's registered voters in 2012 who were non-white, and (4) the fraction of the county's voters who used early voting in 2010. I report regression results one independent variable at a time (bivariate regression) before I combine all four independent variables into a single multiple regression. I show that the total number of early voting hours is statistically significantly related to the number of registered voters in the county and the Democratic vote share for president in 2012. It was not related to race or prior early voting usage in the multivariate analysis.

103. The Thornton Declaration claims to find a statistically significant positive relationship between the "hours open" in a county and the proportion of the county's voting age population (VAP) that is African American. I find no such relationship. Because the Thornton Declaration does not report the data on which this analysis was performed, nor does it describe the procedure by which it calculated open hours in each county, I do not know why our findings are divergent on this point.⁴⁶

XIII. The Thornton Declaration incorrectly claims that I assume 3.0% of African-Americans will be unable to vote due to changes in the same-day-registration law.

104. Paragraph 39 of the Thornton Declaration claims that I assume that 3.0% of African-Americans will be unable to vote in the future because of changes to the same-day registration law by HB 589. This sentence is misleading.

⁴⁶ It is unclear from the Thornton Declaration, for instance, whether the hours-open measure was calculated by summing up the available hours at each early voting location in a county, or whether some other method was used. Further, it is unclear whether Dr. Thornton cross-checked the open hours reported in the state-provided hard drive with the hard copy of the open hours scheduled at each county's early voting site that was made available through the discovery process. See ¶ 183 n. 105 of my declaration.

105. I calculate the 3.0% figure by noting that had HB 589 been in effect for the 2012 election, “31,127 African Americans would no longer have been able to register during the same-day registration period, which was revoked by HB 589...”⁴⁷ Then using 1,046,424 — the number of African Americans who turned out to vote in North Carolina in 2012 — as the denominator, I state the following:

Using this turnout figure as the denominator, the raw numbers . . . can be expressed in the following percentage terms: Burdens on registration would have affected 3.0% of the number of African Americans who turned out to vote; . . .⁴⁸

106. After terming my description of documented facts as “speculative,” the Thornton Declaration then chides me for not adjusting for future factors, “such as the expected future levels of effects such as feelings of citizen empowerment, interest in and concern about the election, and political mobilization by parties, candidates and other political organizations.”⁴⁹ Reporting verifiable facts is thus termed “speculative,” while at the same time it is suggested that speculation is scientific analysis.

107. Finally, at paragraph 41 of the Thornton Declaration, it is noted that neither I, nor the other Plaintiffs’ experts, address why the number of out-of-precinct ballots rose from 2008 to 2012. In response, I will note that, first, *why* the number of out-of-precinct ballots rose is immaterial to the fact that African Americans availed themselves of the procedure more often than whites (proportionally) in each year. Second, the growth in out-of-precinct voting would seem to suggest that something happened in the administration of North Carolina elections

⁴⁷ Stewart Declaration, ¶ 19. Upon a review of my report, I realize I should have used the number 30,612 instead of 31,127. The number 31,127 is the total number of African Americans who were registered while early voting was ongoing, which includes a small number of new registrants who were recorded during this period without voting early — probably because they registered by mail and the registrations were recorded during this time, despite the fact they could not vote in the 2012 presidential election. The correct number, 30,612, is taken from Table 7 of my original declaration, which reports the number of black registrants during the early voting period who also voted early. Dividing 30,612 into 1,046,424 yields a result that 2.9% of African Americans would have been affected by the end of same-day registration.

⁴⁸ *Ibid.*, ¶ 20.

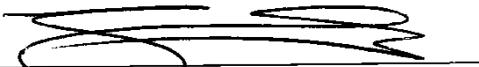
⁴⁹ Thornton Declaration, ¶ 39.

between 2008 and 2012 to cause more North Carolinians of both races (but more often African Americans than whites) to find themselves out of their residential precinct when they went to vote. This increase suggests that the practice of ending out-of-precinct voting may only present obstacles to an even larger fraction of voters in future years.

CONCLUSION

108. Having reviewed the declarations by Mr. Thomas H. Fetzer, Dr. Thomas Brook Hofeller, Dr. Donald Schroeder, Dr. Janet R. Thornton, and Mr. Thomas P. Trende, I make no changes to the substance of my analysis or to my conclusions.

I declare under penalty of perjury that the foregoing is true and correct. Executed this 2 day
of May, 2014.



Charles Stewart III

Exhibit 1. Reported voting wait times in 2008 and 2012 (updated).
(Source: Survey of the Performance of American Elections.)

	2008					
	Nationwide		North Carolina		N.C. minus Nationwide	
	Election Day	Early	Election Day	Early	Election Day	Early
Not at all	44.1%	33.3%	48.5%	20.2%	4.4	-13.1
Less than 10 minutes	27.4%	28.0%	16.0%	24.1%	-11.4	-3.9
10-30 minutes	15.9%	20.4%	16.5%	24.2%	0.6	3.8
30 min. - 1 hr.	8.5%	11.4%	14.8%	21.5%	6.3	10.1
More than 1 hr.	4.0%	7.0%	4.2%	10.0%	0.2	3.0
N	6,099	1,679	71	92		

	2012					
	Nationwide		North Carolina		N.C. minus Nationwide	
	Election Day	Early	Election Day	Early	Election Day	Early
Not at all	39.6%	39.1%	31.5%	25.7%	-8.1	-13.4
Less than 10 minutes	32.2%	28.5%	38.7%	26.3%	6.5	-2.2
10-30 minutes	18.4%	19.1%	24.9%	25.5%	6.5	6.4
30 min. - 1 hr.	7.5%	9.8%	4.9%	21.0%	-2.6	11.2
More than 1 hr.	2.3%	3.5%	0.0%	1.5%	-2.3	-2.0
N	5,958	1,651	75	96		

	Combined					
	Nationwide		North Carolina		N.C. minus Nationwide	
	Election Day	Early	Election Day	Early	Election Day	Early
Not at all	41.9%	36.1%	39.4%	22.9%	-2.5	-13.2
Less than 10 minutes	29.7%	28.2%	28.1%	25.1%	-1.6	-3.1
10-30 minutes	17.1%	19.8%	21.0%	24.8%	3.9	5.0
30 min. - 1 hr.	8.0%	10.6%	9.6%	21.3%	1.6	10.7
More than 1 hr.	3.2%	5.3%	2.0%	5.9%	-1.2	0.6
N	12,057	3,330	146	188		

General Information

Court	United States District Court for the Middle District of North Carolina; United States District Court for the Middle District of North Carolina
Federal Nature of Suit	Civil Rights - Voting[441]
Docket Number	1:13-cv-00660