After the Punctuation:
Competition, Uncertainty, and Convergent State Policy Change

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Published in
State Politics & Policy Quarterly
19(1): 3 – 28
2019

Abstract

What happens after longstanding policies are overthrown in fierce political battles, events scholars refer to as punctuated equilibrium? Do these new policies remain static and unchanging until the next big punctuation, or do they continue to change in explainable and predictable ways? In this article, we develop a model of postpunctuation policy change grounded in the boundedly-rational decision-making by policymakers. Uncertain about how the new policy will perform, policymakers learn to rely on competing interest groups for information, or, under certain circumstances, looking to other political jurisdictions for cues on how their policy ought to be further refined. We test our predictions by studying changes in charter school laws in the American states from 1996 to 2014. We find evidence of policy change, and even convergence, across states, as well as other evidence suggesting that policies after punctuation do change in ways explained as reactions to political pressures in an environment fraught with uncertainty.

Keywords: interest groups, policy diffusion, charter schools, policy change, state politics

Big changes in longstanding policies that push aside regimes of powerful interest groups, lawmakers, and the privileged constituencies they serve, in favor of freshly mobilized social or economic interests expressing very different political desires, events often referred to as "punctuated equilibrium," has attracted considerable attention from scholars. Yet the follow-up question has received almost none. What happens to the policy *after* punctuation? Is it set in stone and unchanging because a new, stable regime supports it? Or are the defeated interest groups, who benefitted under the old policy, lying in wait for a counter-attack, hoping they can convince lawmakers to shift it back to something resembling the old when the attention of the rest of the political world has moved on to other problems? Perhaps the victorious new interests are hoping to use their momentum to win even more benefits with further amendments to the policy. Or might new policies continue to change for reasons unconnected to group competition, such as diffusion of information about the problems and successes of similar policies in similar jurisdictions?

In this paper we study post-punctuation policy change by investigating how charter school laws – which represent major changes in state education policies – fared after enactment. In many states these laws continued to change significantly over time, many becoming surprisingly similar to each other, a pattern of convergence that needs to be better understood. We develop an explanation for this change by drawing on theories of bounded rationality, interest group influence, and policy diffusion. We argue that policymakers initially try to evaluate a new law in noisy, uncertainty-filled environments by consulting with the interest groups who persuaded them to enact it in the first place. If their recommendations fail to produce results, officials may turn back to opponent interests and shift the new policy back towards something like the old. Yet if policymakers find neither sets of groups to be credible, they may instead change the policies based on how they see similar laws perform in neighboring states, a kind of continuing policy diffusion.

Our results largely support these expectations. We find that proponent interests remain influential over subsequent policy change after the punctuation, regardless of how well the new policy performs, while those opposing it are only influential when it fails to live up to expectations. Furthermore, when the new policy does not match the promises of either set of competing interests, we find that lawmakers will look across state lines to learn from similar policies in neighboring states that appear to be performing well. In other words, cross-state diffusion matters as well.

Post-Punctuation Policy Change

Public policy is rarely static, yet studies of policy change seldom look beyond big, status-quo changing events. This interest in major, even radical policy change is easy to understand because it usually means some dramatic shift has occurred in the power structures supporting it, with long privileged organized interests ousted in favor of newly mobilized ones. It occurs, Kingdon (1984) argued, because new policy entrepreneurs found the right combination of political opportunities to push their ideas. Baumgartner and Jones (1993) argue that big change occurs when the attention of policymakers and the public are drawn to a policy's short comings, sparking doubt about how well it benefits them. Signs of doubt spur other, often marginalized, interest groups to promote alternative policies they argue are better solutions to the problem the original policy was supposed to solve. If enough policymakers are convinced, the result is dramatic change and upheaval for the economic or social interests privileged by it (Baumgartner et al. 2009), an event called "punctuated equilibrium." While punctuations do not occur frequently, Jones et al. (2009) found they do occur in cycles rather than at random, making them interesting to scholars.

How similar policies spread across states lines may also be a sign of punctuated equilibrium taking place, though this is not always acknowledged in the literature (see Boushey 2010).

Whether lawmakers adopt big new policies simply because they see neighboring states doing it to solve similar problems (Savage 1985; Lee and Mooney 1999), or because aggressive policy entrepreneurs backed by multi-state interest groups convince them to do so (Mintrom 1997; Balla 2001), diffusion sometimes involves new policy regimes shoving aside once entrenched interests. It also turns out that cross-border policy punctuation is not only cyclical, it spreads across regions in waves following *S*-curve patterns where a few jurisdictions change first, encouraging more to follow, with a few never-adopting stragglers (Gray 1973; Weyland 2007).[1]

But what happens after the big change? Do these radical new policies settle-in to become static and unchanging in the years after punctuation, the regimes of interests supporting them safe from new threats because the pressure of political attention has turned to other problems? If change *is* still occurring, even if it is incremental as Pressman and Wildavsky (1973) once claimed, is it random, or is it occurring in patterns driven by some of the same combinations of pressures that led to punctuation in the first place? Or might post-punctuation change be driven less by politics and more by incremental tweaks as policymakers learn by trial-and-error in their own state or from studying the mistakes and successes of other states, a kind of ongoing diffusion that exemplifies Lindblom's (1959) famous "muddling through" description of policy implementation?

While Baumgartner and Jones suggest that the combination of interests overthrowing the old regimes may settle down to form quiet policy monopolies of their own, Patashnik (2008) argues that there are good reasons to doubt that new policies remain uncontested and unchanging until the next punctuation in some distant future. One reason is that the organized interests advantaged by the old, displaced policy are unlikely to just vanish into the night. Had they been so weak, the old policy's death would have been undramatic and hardly qualify as a punctuated equilibrium. As Moe (2015) argues, interest groups vested in the old policy will fight to preserve their members'

privileges, even if exactly restoring it is unrealistic. Organizations that do not at least try will hemorrhage members until they die (Gray and Lowery 1993). Concurrently, interests championing the new policy may not have achieved everything they desired with the initial enactment. They may hope their momentum will help them frame the feedback mechanisms lawmakers use to evaluate a new policy in a positive enough light to justify further changes enhancing their policy gains (Patashnik and Zelizer 2013).

Further changes may also occur because policymakers are uncertain about a new law's performance. They may have replaced the old policy because they no longer felt it solved the problem it was meant to solve, but this does not mean they are sure the new one will do any better. Uncertainty is the enemy of re-election sensitive politicians (Kingdon 1973). Just as Savage (1985) found that common problems push states to adopt similar policies, so too might adjustments to a new policy in one state diffuse to others because policymakers see other versions of it better addressing the issue-problem (Nicholson-Crotty and Carley 2016). Policies across states might even come to resemble each other over time just because policymakers fear to go their own way. Overall, though, any theoretical framework for studying why policies might continue to change (assuming they do) after a big policy punctuation, and in which direction they change, must encompass policymaker uncertainty, interest group advocacy, and cross-state diffusion.

Bounded Rationality and Policy Change

Underlying Assumptions

We start with six assumptions drawn from several literatures. First is the long held assumption that legislators are goal-directed in that they want policies to solve identified issue-problems in ways that satisfy key constituencies, either because they want to be seen as good

lawmakers or because they want to be re-elected (Mayhew 1974; Dodd 1977). Regulators are also goal-directed in that they want to implement policy faithful to their statutory mandates and to keep overseeing legislators happy (Brehm and Gates 1997; Gormley and Balla 2001). How well policy is perceived to achieve these goals is the feedback they all use to evaluate its performance.

Jones (2001), however, argues that while policymakers may be goal-directed, they are boundedly rational deciders in that they only have limited time and cognitive ability to focus on and evaluate each policy, which is seen in the episodic nature of policy punctuation. We agree, but we also argue that even when most policymakers have turned their attention to other issue-problems, there are typically some who remain attentive to policies. The literature on legislative organization finds that legislators pick their committee assignments because the policies within a committee's jurisdiction are important to their constituents and thus their re-election prospects (Fenno 1973; Deering and Smith 1997). Regulators, of course, are statutorily required to monitor and evaluate the policies under their jurisdiction. Therefore, our second assumption is that there are always legislators and regulators who evaluate a policy's performance, and will make changes when they do not believe it meets their goals.

Our third assumption, though, is that even this subset of policymakers are boundedly rational in that cognitive limitations restrict their ability to fully evaluate a policy's performance. Furthermore, they are embedded in an environment full of noise that distorts the feedback they receive when trying to make these evaluations (Kingdon 1973; Gormley and Balla 2001). Consequently, even as they remain focused on the policy as others have moved on, they are uncertain about how well it helps them meet their goals. Fourth, because of all of this uncertainty, Simon (1955) argues that people establish aspiration levels for performance, reference points above which a policy is believed to have done an adequate job of meeting goals of constituency

needs and satisfying legal mandates. Performance may not be optimal, but it is good enough and few tweaks are needed.[2] In this sense policymakers are risk-averse, preferring the known policy to unknown alternatives, which often reinforces the status quo (Quattrone and Tversky 1989).

Fifth, policymakers also cope with chaotic environments and time limitations by structuring their perceptions of issue-problems in "task environments" of beliefs, relationships, and institutional rules that, in turn, help them create simple search rules to quickly find information directing and justifying their decisions (Denzau and North 1994; Payne and Bettman 2001). Put another way, they employ the simplifying heuristic of finding trusted information sources (ignoring everything else) and make decisions based on how these sources frame a policy (Gigerenzer and Selton 2001). As long as the outcome is good enough, policymakers will use the same heuristic, the same information source, again and again. They are also risk-averse in this sense, preferring known, trusted information sources to unknown alternatives.

One source of information policymakers frequently consult are interest groups. People, businesses, and other organizations advantaged by current policy mobilize as interest groups and utilize their resources, which range from technical information to money to ties to constituencies crucial to a legislator's future, to protect it (Hansen 1991; Wright 1996). Lacking time and, very often, other trusted sources of information, legislators grant access to groups whose information and arguments can reduce their uncertainty, that can convince them that supporting the interest group's policy helps themselves (Hall and Deardorff 2006; Hertel-Fernandez 2014). McKay (2012) even suggests that interest groups take advantage of policymakers' uncertainty and risk-aversion to protect their preferred policies. So our final assumption is that the heuristic policymakers use to cope with uncertainty-filled environments is to consult a few trusted interest groups. Interests disadvantaged by the status quo may argue that other policies would better help

them achieve their goals, but goal-directed officials in competitive task environments only consult familiar groups who reinforce their beliefs that any alternative would be disastrous.

Baumgartner and Jones argue that major change in a policy monopoly occurs when lawmakers' perception of the policy's performance leads them to doubt whether it is helping them achieve their goals. As the problem worsens, their faith in the old policy's ability to solve the underlying problem, and even belief in what that problem really is, erodes and interest groups supporting it lose their credibility with policymakers. As Todd (2001) argues, faced with growing uncertainty in the task environment, any simplifying heuristic can be stretched to encompass similar, if not previously sought out, information sources with different messages. Here this means consulting other, competing interest groups.[3] Consequently, policymakers become inclined to consider alternate policies advocated by interest groups representing other constituencies. Ultimately, Tversky and Fox (1995) argue, policymakers become so alarmed with the policy's inability to solve the problem that they may start evaluating its performance vis-a-vis the promised performance of an alternative policy. Supporting the status quo is now framed as a *loss* compared to the alternative, and policymakers become risk-*accepting* in their support for the latter. Task environment uncertainty, which was rising, now falls, creating the bandwagoning effect in support for radical new policies observed by Baumgartner and Jones and the *S*-curve of enactments seen by scholars in cross-border policy diffusion (Gray 1973; Weyland 2007).

Post-Punctuation Policy Change

We argue that post-punctuation change continues to be shaped by interest group credibility, which in turn is still driven by how well the new law appears to help policymakers achieve their goals and the resources competing interest groups invest in shaping this perception. Unless the new law is exactly what proponent interests (the former challengers) wanted, we assume that they

continue to lobby for "improvements," claiming these will further increase the new policy's value to policymakers. Since boundedly rational policymakers chose to accept proponent interests' framing prior to punctuation, they remain invested in proponents' claims regarding the value of subsequent policy tweaks. In other words, proponents have high credibility immediately after punctuation. Opponent groups, advantaged by the old policy, argue that the new policy's performance is actually poor, and will only improve if it is amended to more closely resemble the old. Unfortunately for them, their credibility was hurt in the fight leading up to the punctuation, and it remains low as the new fight over the policy begins, regardless of the resources they invest in promoting a negative framing of it.

Figure 1 shows how officials might respond to competing interest groups' claims regarding policy performance. If punctuation occurred at time *t*, say an annual legislative session, then the new policy's performance is evaluated next session, *t*+1, by how well it appears to meet policymakers' goals. The continuum at the top of Figure 1 represents this perceived performance. On the left is *G*, policymakers' notion of perfectly fulfilled goals, while *S* is Simon's satisficing point, a good enough performance point that, if attained, leaves officials satisfied with proponents' claims in *t* of what the policy would achieve. The old policy's performance is at *OP*, and interest groups favoring it claimed in *t* that the new would perform even worse, so this claim is marked at *O* to the right of *OP*, far from *G*.

---- Figure 1 here ----

Policymakers have a simple choice in *t+1* as they evaluate the new policy relative to *G*: do nothing, amend the policy as proponents desire, or shift it back to something closer to the old policy. Which option boundedly rational deciders choose depends on its performance, as well as proponent credibility and resources. If, as in the top of Figure 1, performance outcome NP_{t+1} is

between *G* and *S*, policymakers see the distance NP_{t+1} to *OP* as a gain over the old policy, but NP_{t+1} to *G* as a loss (proponents' unfulfilled promises). Proponent credibility in *t*+1, though, only falls if *NP* is to the right of *S*, the good enough performance point. Opponent credibility, however, is a function of actual versus predicted losses rather than just the inverse of proponent claims. Opponents claimed in *t* that the new policy would perform worse than the old, so they are evaluated by comparing losses they predicted, *OP* to *O*, to actual performance, *NP* to *O*.

How this evaluation feeds back on the likelihood of subsequent change is shown in the middle of Figure 1. The vertical axes are the credibilities in policymakers' minds that the gains or losses interest groups claim will happen will actually occur if more changes are made. Credibility is shaped by perceptions of the new policy's performance after the big change in time *t*, perceptions that may be shaped by interest groups' advocacy resources. If the policy in *t*+1 is perceived to be left of *S*, at NP^1_{t+1}, then officials are satisfied with its performance. Because proponent's credibility is unthreatened, their recommendations for new changes will be enacted. If the policy fails to perform closer to *G* in *t*+2, proponent credibility may take a hit, but the hit will be small as long as *NP* is left of *S*. If, though, *NP* is right of *S*, at NP^2_{t+1}, policymakers will be unhappy, but because these boundedly rational officials are still invested in proponent's framing, the credibility hit in *t*+1 is small. Yet if NP^2_{t+2} has not moved left, proponent credibility will decline significantly, if mitigated by intense advocacy, and it is less likely their requests for further changes will be enacted. If *NP* is still right of *S* in *t*+3, proponents will probably not be consulted at all.

As for opponents lobbying to move the policy back in the direction of the old, because policymakers chose in time *t* to invest themselves in framing supporting the new policy, opponents have little credibility, even if they are still resource strong. Risk-averse policymakers in *t*+1 still believe that any of their recommendations will bring losses, so they will not consult opponents in

t+1 when searching in their task environment for information about performance. In this sense policymakers are different from rational deciders, who would always evaluate every advocate's claim in *t*+1. They will not consult opponents in *t*+2 either *unless NP* is perceived to be right of *OP*, like NP^3_{t+2}, which is why the crossing of the credibility curves in the middle of Figure 1 is right-shifted. But if *NP* is right of *OP* in *t*+2 and beyond, opponent credibility will rise, especially if they invest more in advocacy, and policymakers will be more inclined to amend the policy so it begins to resemble the old. Where proponents and opponents are on their credibility curves in *t+1* and beyond defines Figure 1's bottom. Left to the vertical dashed line at *S* means the new policy provides enough gains over *OP* relative to *G* that officials are satisfied and proponents' credibility remains high. We cannot say precisely where this stops being true, but can offer two hypotheses:

Conditional Proponent Hypothesis: Policymakers will enact changes to the policy in *t*+1 if proponents invest in lobbying regardless of the policy's performance, but starting in *t*+2 proponents will only gain further changes if it is perceived to be performing well.

Conditional Opponent Hypothesis: Policymakers will not enact changes for opponents in *t*+1 regardless of advocacy, but will starting in *t*+2 if the policy performs poorly and opponents lobby.

We believe there is another interesting result that might occur. Meseguer (2006) and Weyland (2007) argue that many patterns of policy diffusion are a consequence of boundedly rational decision-making, and that looking at the choices of neighboring states or nations is also a simplifying heuristic used by policymakers. But when would they stop consulting interest groups entirely and rearrange their task environment to take cues from the policy choices of neighboring states? When *neither proponent nor opponent interests are credible*. The diffusion of post-punctuation policy change is thus conditional on low interest group credibility.

In the middle of Figure 1, around the point where the curves cross right of *S*, neither groups' credibility levels are high. There is uncertainty, even fear, now among risk-averse policymakers starting in *t*+2 after changes sought by proponents appear to have failed. In this circumstance, policymakers may abandon their normal heuristic of listening to interest groups and look for alternative cues. They may willingly pay the costs in time and resources to find new information, but Jones (2001, pp. 114-117) argues that an easy heuristic overwhelmed policymakers might use is to look at the choices of others like themselves. Diffusion research now informs post-punctuation policy change because copying the choices officials made in similar jurisdictions is an easy new heuristic to employ (see Gilardi et al. 2009).[4]

Yet mere mimicry of neighboring states is too weak an explanation, even if it is a simplifying heuristic. Again, policymakers are goal-directed and care about performance, wanting to know that similar policies in neighboring states are better able to meet their goals before amending their policy to match those of these neighbors (Nicholson-Crotty and Carley 2016). We cannot specify the exact point in Figure 1's credibility curves where neither group is consulted, or predict how policymakers in one state learn what happens in others, but if many states converge to a similar policy, and it appears to be working, this may be a sign to policymakers they can learn from their neighbors' success and mistakes. So:

Diffusion and Learning Hypothesis: Starting in *t*+2, if the new policy is not performing as proponents or opponents predict, officials will amend their policy to resemble similar policies in neighboring states if those states exhibit signs of improved policy performance.

Research Design

One reason that the evolution of post-punctuation policy may not receive much attention is because change is hard to operationalize. Except for government budgets, which are nicely measured in dollars, few policies come in fine interval variable form. Fortunately, we have one that does – state charter school laws. While the idea that public education might improve if parents could choose where to enroll their children, rather than have the choice determined for them by geography, has many antecedents, charter schooling as a policy of reform was primarily embraced by advocates in the 1990s who believed that markets and choice are effective solutions to social problems (Henig 1994). Successfully framing traditional K–12 education as a failed policy, these advocates settled on charter schooling, publicly-funded schools free enough from regulation to design innovative curricula they can use to recruit students, as the alternative for reform most likely to appeal to policymakers (Nathan 1996; Henig 2008). When national leaders proved reluctant to embrace them, advocates refocused their efforts on states where politicians hoping to portray themselves as reformers happily pushed it, often over fierce resistance from teacher's unions and school board associations (Bulkley 2005). Minnesota enacted the first law in 1991, with 47 states (including the District of Columbia) doing so by 2017.

For several reasons education scholars believe that enacting charter school laws qualifies as punctuated equilibrium. First, it involved changing fundamental beliefs about traditional education policy, accepting the new idea that allowing parents to choose schools for their children will reform and improve a state's education performance (Kirst 2007). Second, as Baumgartner and Jones argue is often true, it required changing policymaking venues, in this case from the national arena to the states (Bulman and Kirp 1999). Third, it was accompanied by a sustained burst of press coverage shaping and re-shaping public opinion through the 1990s (Henig 2008, p. 185). Fourth, it broke up a policy monopoly dominated by interest groups such as teacher's unions

(Holyoke et al. 1999). Finally, enactments occurred in a pattern of a few early adoptions followed by an explosion, an *S*-pattern of cumulative adoptions which Boushey (2010) argues is often the hallmark of punctuated equilibrium.[5]

Measuring Charter School Policy Change

Since 1996, the Center for Education Reform, a national interest group promoting choice in education, has measured various aspects of state laws to create an index of how supportive of charter schooling each is. The coding is done by a team of experts, many of whom are education policy specialists, and yields an interval measure of each state's policy. While there has been some criticism of using scales to study education policy generally (Scott and Barber 2002; Chi and Welner 2008), the Center's scores (commonly called CER scores) have been accepted and used by scholars (e.g., Wong and Shen 2002; Stoddard and Corcoran 2007). After Shober et al. (2006) revealed that parts of the index are contradictory, Holyoke et al. (2009) re-analyzed the components to create a new index using only those elements consistently measuring how permissive a state's law is when it comes to opening a school, operating it, and implementing novel curricula.[6] This reduced CER index ranges from a restrictive, highly regulated 0 to a very permissive, pro-charter score of 30. Because freedom to expand and innovate, giving parents more choices on where to enroll their children and placing greater competitive pressure on traditional schools, best reflects the free market philosophy advocates claim underlies charter schooling, a measure of a state law's flexibility is the most appropriate measure for our dependent variable.

---- Figure 2 here ----

The Center has published these scores at least every two years starting in 1996, providing us with a consistent interval measure of policy for every state from 1996 to 2014, allowing us to answer the question - what comes after enactment? Just looking at the two-year change in CER

scores for the 21 states (including DC) enacting laws between 1994 and 1997 (including 1994 enactors allows us to see their scores in the first observed year afterwards) gives us a chance to see how these laws change over a period of at least seventeen years, revealing two interesting trends.[7] In five states (Alaska, Hawaii, Kansas, Rhode Island, and Wyoming) there was almost no change at all, all five maintaining very low scores changing less than 5 points on the thirty point scale. In other words, the five states enacting the most restrictive laws, those departing least from the old policy, continue to be restrictive. What is more interesting are the other sixteen where, as seen in Figure 2, mid-range policy convergence appears to be taking place. In 1996, these states' scores ranged 27.5 points, from 30 (Arizona and DC) to 2.5 (Arkansas), but by 2014 the range was just 13.33, 23.5 to 10.17. Fourteen actually ended within the 10 to 20 range, and eleven between 14 and 18. Not only are their policies continuing to change after punctuation, they are converging in a way that suggests the change is far from random. Perhaps lawmakers are responding to uncertainty by learning from other states as the Diffusion and Learning Hypothesis predicts.

Operationalizing Variables

We construct several variables to test our hypotheses regarding ongoing change in charter laws. Descriptive statistics are in Table 1. Again, our dependent variable is each state's CER score measured every two years from 1996 to 2014, giving us ten data panels, capturing whether legislators are amending statutes, or regulators are changing rules, related to charter schooling. Every state adopting a law in 1994 or later is included, entering our data set after it enacts its law. First we operationalize the hypotheses derived from our model.[8] Proponents of the original charter school policy are presumably proponents of further positive change in a state's CER score, which in this case are choice in education organizations, charter schools, charter school operators, and the constituencies they serve. We develop three indicators capturing their political strength. The

first is the sheer number of charter schools in each state each observed year, which, like the CER scores, comes from reports published by the Center for Education Reform. The more schools there are, the more entrepreneurs and staff there are advocating for stronger laws enhancing their autonomy from state and local regulations. The second indicator is total charter school enrollments. This data, which comes from the Department of Education's National Center for Education Statistics (NCES), is a proxy for the number of parents of charter school students.[9] Since we presume parents *want* their children in charters instead of traditional schools, we assume they also strongly support giving the schools significant flexibility to create innovative programs for them.[10]

---- Table 1 here ----

Our third indicator is the number of educational management organizations (EMOs) operating in each state. These are for-profit corporations or nonprofits that operate charter schools, usually many schools, and since the cost of operation affects their bottom lines, we assume EMOs will lobby for flexible, regulation-lite environments. For years, Alex Molnar has collected and presented data on the number of EMOs operating in each state, which we obtain for the relevant years and use as our third measure of pro-charter advocacy strength.[11] To streamline our statistical models, we use a factor analysis of these three indicators to identify and capture the underlying dimension, which becomes our measure of proponent strength.[12]

In terms of opponent interests, the most consistent foes of charter schooling have been teachers' unions such as the National Education Association (NEA) and the American Federation of Teachers (AFT). While teachers unions have not always opposed efforts at education reform (see Hoxby 1996), when it comes to charter schooling unions have generally been opponents, or at least outspoken critics (Bulman and Kirp 1999). We are not able to obtain data on AFT membership, but the NEA has made available the number of members it has in each state in their

annual *Handbooks*, so we use this as one indicator of opposition.[13] For a second, Lott and Kenny (2013) argue that campaign contributions are good indicators of teachers' union strength (also see Hartney and Flavin 2011), so we obtain teachers' union contributions data for relevant years from the Institute for Money in State Politics.[14] Finally, to try capturing AFT members, and because public unions often support each other, our last measure is data on the percentage of public employees in each state represented by unions, developed by Hirsch and MacPherson (2003).[15] We factor analyze the variables and the underlying dimension is our opponent strength measure.[16]

We predicted that both proponent and opponent advocates' influence will be conditional on how successful each state's new policy is. While K-12 student grades and SAT scores might be used as performance indicators, both are inconsistent and controversial. Since graduation is the ultimate goal, we use the annual change in high school graduation rates obtained from NCES. Again, choice-in-education advocates pushed charter schooling as a reform for a state's entire education system, so for the policy to be seen as a success, overall state graduation rates must rise.[17] To capture the conditional effect of graduation rates on interest group influence, we multiply the two-year change in graduation rates by proponent strength for the Conditional Proponent Hypothesis. As charter advocate resource strength increases, conditioned on improvements, their credibility in the eyes of policymakers grows and CER scores should rise. The Conditional Opposition Hypothesis predicts the reverse for falling graduation rates and teacher's union resource strength. For both hypotheses, though, we predict that the interaction effect should not appear until t+2 after policy punctuation. In t+1 proponents should be influential *regardless* of graduation rates and, because we assume they have no credibility in t+1, opponents should be powerless regardless of performance.[18]

The final variables operationalizing our theoretical predictions regard the diffusion of policy ideas from other states, which may influence policymakers when they believe their own policy, and thus the advice they are getting from interest groups, is failing. The literature on state-to-state diffusion emphasizes the influence of neighboring jurisdictions, so for each observed state we find the CER score for every border state with a law and calculate the average. Since this effect is predicted to be conditioned on changing graduation in these neighbors, we calculate the average change in those states and multiply it by the first measure. The Diffusion and Learning Hypothesis interaction term should show a positive effect as the state's law comes to resemble its neighbors.

We also use several control variables. The first set regards each state's political environment. While Democrats have occasionally supported particular charter schools, they have generally been skeptics of the policy and have often pushed for greater government oversight and control (Bulman and Kirp 1999). We obtain data on the percentage of Democrats in both chambers of each state's legislature and average them. Since governors like Tom Ridge (R-PA), Tommy Thompson (R-WI), and Pete Wilson (R-CA) were big advocates for the initial enactment of charter school policies, they may be decisive later as well, protecting their legacy, so we code 0 each state with a Republican governor in the observed year and use it as another control.[19] While people of different ideological stripes supporting charter schools, conservatives are more consistent advocates of market-driven solutions to social problems, so politicians wanting to appeal to them may continue to support flexible laws. We therefore use the state citizen ideology measure created by Berry et al. (1998) as a special control (see below).[20]

Other education-oriented variables may be important, so we use data on current state K-12 enrollments and the number of K-12 teachers for each observed year from NCES for two more control variables. Finally, we also use several economic variables, the first being state budget

deficit for the observed year, and the other being gross state product, which Gray and Lowery (1993) argue is crucial for assessing interest group influence, two of our key variables.[21]

Analysis and Discussion

Analyzing post-punctuation charter school policy change means we have to estimate data arranged in cross-sectional time-series panels using random-coefficient models. These control for state-level effects varying from state-to-state but hardly at all from year-to-year by estimating separate slopes for each state.[22] Since the degree to which people living in a state are conservative or liberal is a statewide effect barely changing over time, we use the citizen ideology measure to estimate the steepness of each state's slope rather than use it to directly estimate policy change. Furthermore, since the charter proponent and teachers' union variables are interacted with the same graduation rate variable they are too highly collinear with each other to be in the same model. We therefore estimate one set of models for proponents and another for opponents. Finally, since a state's CER score cannot be greater than 30, we would not expect to see its score increase when it is already near 30, or decrease when already near 0. Therefore, our proponent models are only estimated on those states and years where the CER score is in the top one-third of CER scores (N = 268), while our opponent models use cases appearing in the middle and upper thirds (N = 206).

Recall that the Conditional Proponent Hypothesis not only predicts that in most years the interaction of graduation rates and proponent strength should have a positive effect, but in the first year after enactment proponent strength alone should have an independent positive effect. The interaction term and the changing graduation rates variable should not. Therefore, before estimating the full model we first estimate the effects of our variables on state CER scores in just the t+1 year. Since this is a very small N model, we use OLS regression with standard errors

clustered by states.[23] The results are in the first column of Table 2, and, so far, support our Conditional Proponent Hypothesis. Charter school advocates appear credible, and therefore influential, in the first observed year after the policy's adoption, as seen in that charter school proponent strength is the only significant variable, and it is positive.

---- Table 2 here ----

The results of our full model for proponent influence are in the last column of Table 2, with the results of a baseline model without the interaction terms in the second column for reference. Unfortunately, our prediction that in subsequent years proponent strength is conditioned on strong policy performance is not supported. The proponent strength variable is significant in the baseline model, showing a positive independent effect, and in the full model as well, showing a positive effect when change in state graduation rates is 0 (since it is part of the interactive variable, proponent strength cannot exhibit an independent effect in the full model). It seems proponent interests are so strong and entrenched after punctuation that they remain influential in the years after *regardless* of how well the new policy performs. It is worth noting, though, that the positive effect of proponent strength is not that strong, a one-unit change in the variable only resulting in an estimated 1.15 increase in a state's CER score on a 30-point scale.[24] Democratic legislative control and gross state product appear to have stronger negative effects, and overall student enrollment a larger positive one.[25] Nonetheless, advocates for strong, flexible charter laws continue to have influence. Perhaps this explains Georgia, where a weak policy of 5 in 1996 rose to 17 in 2014 as charter school numbers grew from 2 to 114 with 1.7 million students enrolled. Similarly, in Colorado the CER score was 15 in 1996 but 22 in 2004, even though graduation rates remained largely unchanged at just over seventy percent.[26] Once a new organized interest emerges to support a new policy, the harder it is to shake-off its influence later.

Our results for the Conditional Opponent Hypothesis are harder to evaluate. As predicted, and as seen in Table 3's initial year model, teacher's unions exerted no influence over the policy in t+1, presumably having been discredited in the battle leading up to the policy punctuation. This is not what we would expect if policymakers were fully rational and always considered all claims and options. Also, as expected, teacher's unions exhibit no independent influence, as seen in the baseline model. Furthermore, as predicted, this variable is not significant in the full model, though it is surprisingly positive. Of course the Conditional Opponent Hypothesis's main predicted is that the effect of rising teacher's union strength would exhibit a significant negative effect when conditioned on declining graduation rates, meaning the interaction term should be negative and significant. Unfortunately, while it is negative in the full model, it is not significant.

---- Table 3 here ----

Rather than accept this result at face value, which would under-cut much of our theoretical model, we follow the advice of Brambor, Clark, and Golder (2006) who argue that marginal effects of interactive terms made from two continuous variables ought to be graphed because some regions may still exhibit significant effects even when the coefficients are not. We therefore plot in Figure 3a the marginal effect of teacher's union strength on state CER scores as graduation rates fall. While the average estimate (the solid line) is clearly declining, indicating a growing negative effect, the decline is small and the upper 95% confidence band never drops below 0, meaning we cannot be sure that the real estimate is really even negative.

---- Figure 3a and 3b ----

This suggests that sharper, if less nuanced, results might be found by recasting the modifying variable in the interaction (changing graduation rates) as a binary indicator coded 1 when a state's rates are falling and 0 otherwise. This, we argue, is not a substantively meaningless

change as interest groups often engage in a little hyperbole to bolster their arguments, such as arguing that charter school laws are failing just because graduation rates are falling while avoiding murky details such as whether they fell a lot or just a little. Remember that our argument is about changes in law based on whether and how interest groups are shaping the perceptions of lawmakers. So in the last column of Table 3 we present the results of estimating state CER scores using the binary graduation rate indicator interacted with the continuous teacher's union variable.[27] While the other coefficients are largely the same in significance and direction, the interactive term is negative and significant while the teacher's union variable is not, which is exactly what the Conditional Opponent Hypothesis predicts. In Figure 3b we predict each state's CER score as teacher's union strength increases, given that the state had declining graduation rates. The negative effect is clear, though the overall magnitude is only a little over 2 points on the CER scale.

This effect, while small may have been felt in Delaware, a state with a high CER score of 27.5 in 1996, but as the 21st Century began its graduation rate fell from 81% to 72%. At the same time the Delaware State Education Association membership grew from 9,285 to over 11,000 by 2006 and remained high. By 2008 the state's CER score began to quickly decline, bottoming out at 18 (mid-range) in 2014. And back in Colorado, after contributions made by the Colorado Education Association rose to $2,423,946 by 2014, by which time the state's CER score fell from 22 in 2004 back down to 19.33. As with the proponent models, K-12 enrollment, Democratic control of the legislature, and GSP also all exhibit significant effects.

Finally, the Diffusion and Learning Hypothesis predicts that officials should only pay attention to developments in other states when their own graduation rates neither rise nor fall significantly, discrediting both proponents and opponents, starting in t+2. The results in column one of Tables 2 and 3 show the predicted non-effect in t+1, the first observed year after enactment.

To fully test this hypothesis, we estimate another set of models using only observations in the middle third of the graduation rate variable's range (and so the graduation variable is not used as an independent variable). We also cannot use both proponent and opponent interaction terms because of the multicollinearity it produces, so we simply use the independent charter advocate and teacher's union variables. The results are in Table 4. As predicted, in states where graduation rates are not going up or down appreciably, neither charter advocates nor opponents appear to have any influence over state policies. We take this to mean that, despite any resource strength they might have, failure to predict significant changes in graduation rates (either up or down) eroded their credibility in the eyes of policymakers.

---- Table 4 and Figure 4 here ----

The baseline model shows that neighbor state CER scores and their graduation rates have no independent effects. In the full model we see that increases in border state graduation rates does negatively influence the observed state's CER score when those states' average scores are 0 (which never actually occurs), and border state CER scores have no effect when their average change in graduation is 0. It seems that if the policy is not performing well in neighbor states, policymakers have little to learn from them. Yet the interaction term suggests that as border state graduation rates rise, the marginal effect of border state CER scores on the observed state's score also rises. Plotting this marginal effect in Figure 4 supports this interpretation where we see the effect of border state CER scores being negative on the observed state when their graduation rates are falling, but it turns positive when rates rise and their policies are appearing to perform well. Indeed, the Diffusion and Learning Hypothesis turns out to be the most strongly supported of our bounded rationality-derived hypotheses. From this we tentatively conclude that this diffusion of

policy change is not just mimicry, but actual learning as policymakers study the experiences of states close to their own. Change is *not* occurring because of proponent and opponent advocacy.

Conclusion

Continuing policy change after major, regime overthrowing punctuations has received little attention in the scholarly literature in comparison to the large body of work on why dramatic change occurs in the first place and spreads across borders. Here we explored subsequent policy change in the aftermath of a punctuation in state education policy to see what, if anything, happens in the years after the dust of intense political conflict begins to settle. It turns out that the dust often does not settle at all. As our research shows in the case of charter school laws, policies are anything but static in the years following their high profile and controversial enactments. Not only do they often continue to change, but do so in ways that are understandable and predictable.

We argued that these post-punctuation changes occur because boundedly rational policymakers are trying to evaluate the new policy's performance in an environment full of confusing and conflicting information, including information from interest groups still competing to influence the policy. Risk-averse policymakers, we predicted, prefer getting their advice on further policy tweaks from the interest groups that originally convinced them to overthrow the old policy regime, at least as long as the new policy appears to perform better than the old one. Actually, our analysis found that these proponent interests remained influential regardless of whether the policy lived up to expectations. We also predicted that if the new policy's performance consistently disappoints over time, risk-averse policymakers may once again seek out the advice of interest groups that supported the old, displaced policy. Consequently, they may amend the new law to resemble aspects of the old, even if there is no real return to the old policy. In this way,

we ground part of our explanation for ongoing policy change in theories of boundedly rational deciders and information-based interest group influence. While our predictions were not fully supported, we feel this direction is worth further attention. Future work might explore how interest groups shape policymaker perceptions of what the new policy ought to fully achieve (*G* in Figure 1), or whether legislators might largely hand-off ongoing policy change to regulators given certain circumstances, a kind of transaction politics also subject to interest group influence.

We also predicted, and found evidence for, an entirely different explanation for post-punctuation policy change. If policymakers decide that neither set of competing interests has any credibility, they may look for other information sources on how the policy might be improved, such as the way similar problems are handled in neighboring states. As policy adjustments diffuse across state borders, a kind of convergence appeared to take place, with charter laws coming to resemble each other over time, seen in Figure 2. This finding is also important because some scholars argue that research on policy diffusion needs to be put on a firmer theoretical foundation and more clearly linked to other theories of policy change (see Karch 2007). Here we support the argument that diffusion patterns can be explained by theories of bounded rationality. Policymakers trying to adjust their policies in noisy political environments, so that at least their own goals are served, may dispense with contradictory interest group advice entirely, and instead find it is easier to simplify task environments by learning from the successes and mistakes of other states. This too should be the subject of future research.

Figure 1: Predicted changes in policy post-punctuation

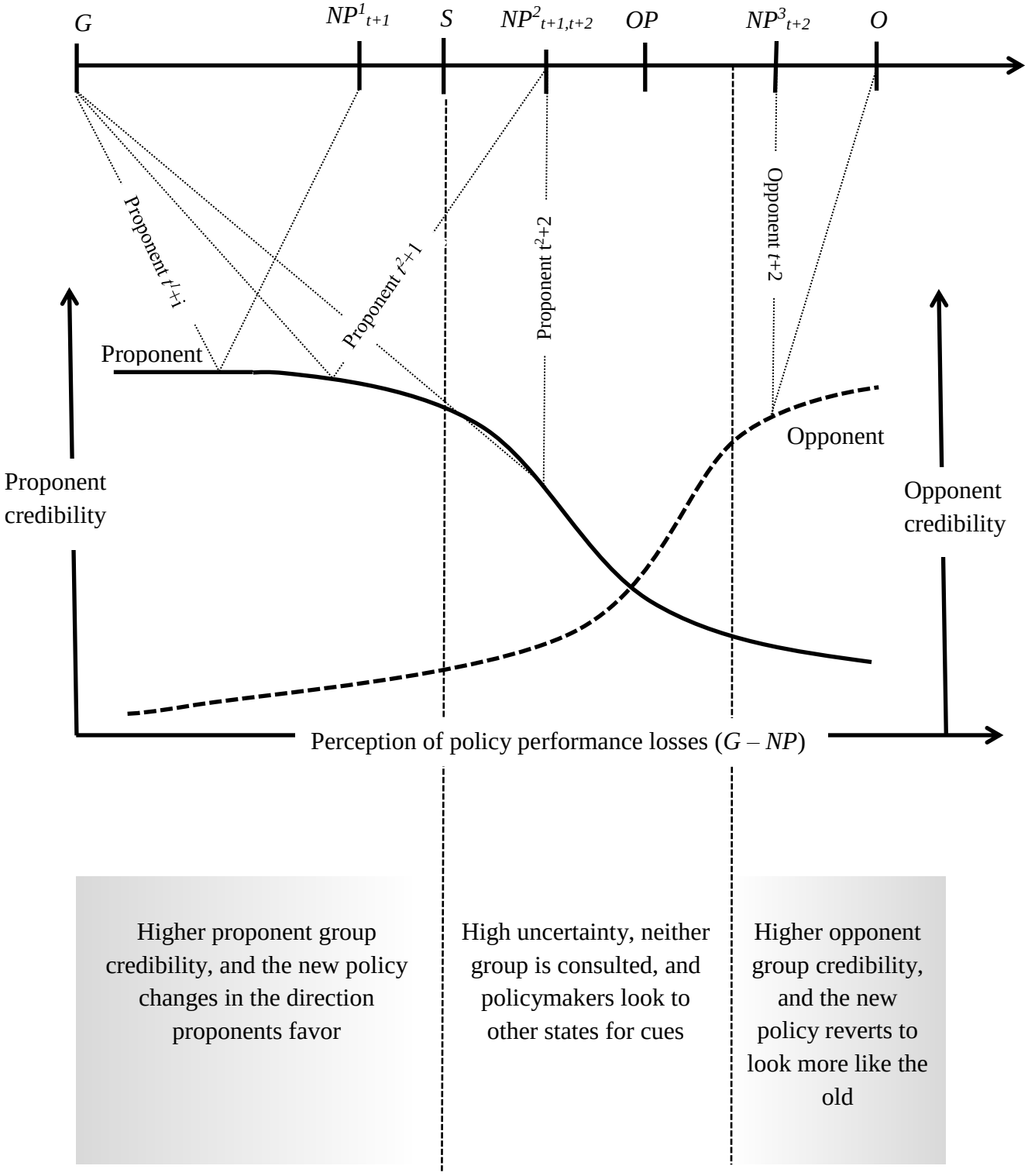

Figure 2: Change in charter school laws of states adopting between 1994 to 1997 with more than five points of change

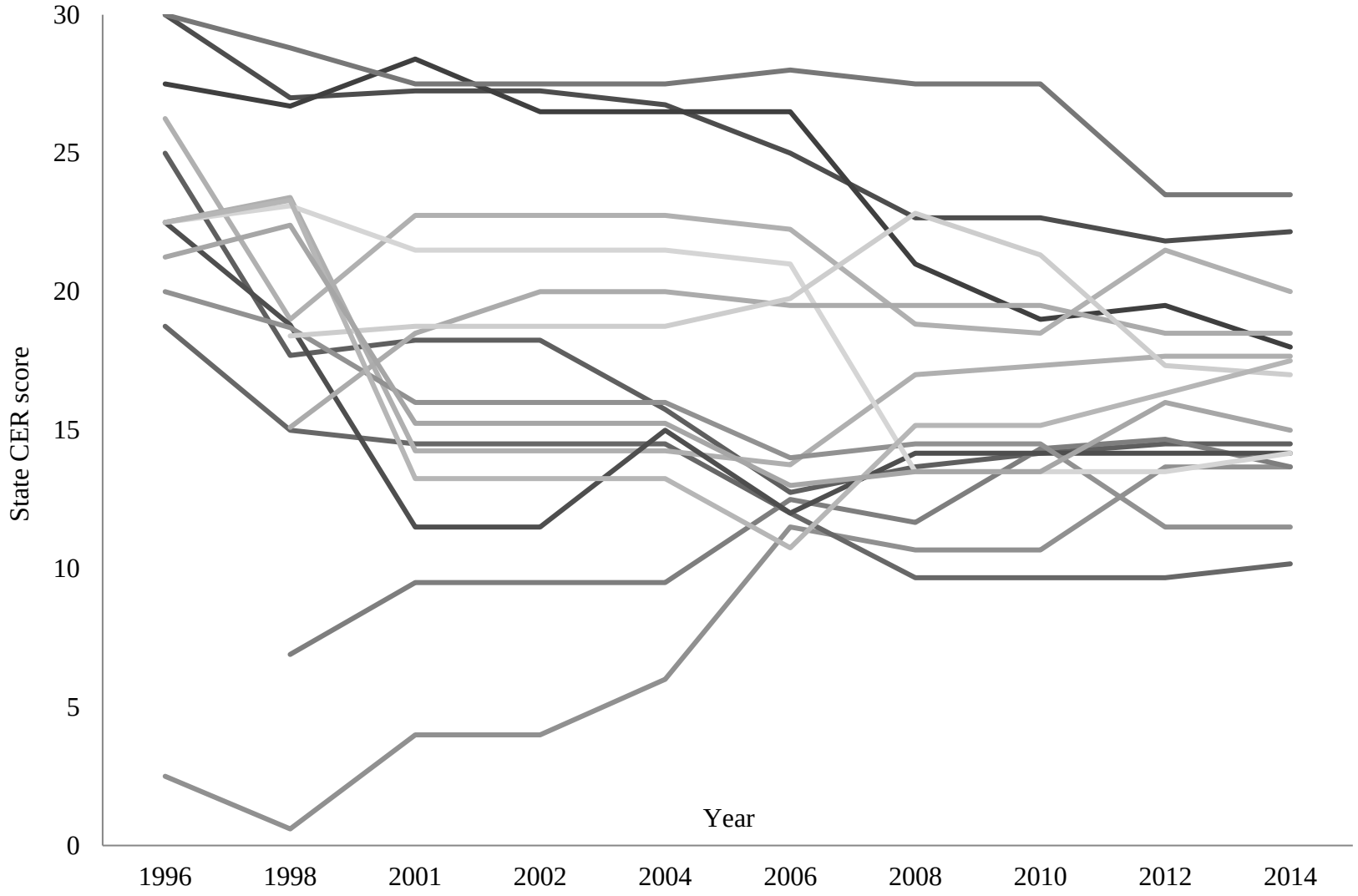

Table 1: Summary statistics for variables

Variable name	Mean	Standard deviation	Minimum value	Maximum value
State CER score (dependent variable)	13.69	7.37	0	30
Charter school proponents index	0.00	0.96	–0.45	8.67
Teachers' union opponents index	0.01	0.75	–1.35	3.35
Change in graduation rates	0.01	0.05	–0.21	0.20
Average of neighbor state CER scores	14.89	5.37	0	30
Average of neighbor state changing graduation rates	0.01	0.03	–0.12	0.15
Student enrollments (divided by 10,000)	96.62	111.20	8.44	644.16
Number of teachers (divided by 10,000)	6.06	6.42	0.66	33.32
Percentage of legislative Democrats	0.25	0.09	0	0.46
Governor is a Democrat	0.42 (mode is 0)	0.49	0	1
State deficit (divided by 1,000)	–1.59	9.74	–127.29	19.19
Gross state product	251.47	317.42	14.66	2,311.6
Citizen ideology	50.56	15.88	8.45	95.97

NOTE: Student enrollments, number of teachers, and state deficit have all been reduced so that they yield more meaningful coefficients in the statistical analysis.

Table 2: Estimates of charter school proponent strength in states with middle to low CER scores

Independent variables	Initial year model	Baseline model	Full model
Charter school proponents	36.25*	1.12*	1.16*
	(16.53)	(0.48)	(0.53)
Change in graduation rates	–3.19	–2.25	–2.52
	(68.78)	(3.83)	(4.24)
Charter school proponents *X* change in graduation rates	98.63	–	–2.48
	(207.34)		(8.89)
Average of neighbor state CER scores	–0.21	0.02	0.01
	(0.26)	(0.07)	(0.07)
Average of neighbor state changing graduation rates	–	5.75	–21.59
		(5.33)	(18.20)
Average of neighbor state CER scores *X* average of neighbor state changing graduation rates	–	–	1.70
			(1.08)
Percentage of legislative Democrats	10.75	–15.14***	–15.61***
	(15.72)	(4.58)	(4.56)
Governor is a Democrat	–4.15	0.63	0.67
	(2.85)	(0.36)	(0.36)
Student enrollment	–0.11	0.09***	0.09***
	(0.17)	(0.03)	(0.03)
Number of teachers	2.64	–0.52	–0.43
	(3.00)	(0.34)	(0.36)
State deficit	–1.01	–0.02	–0.02
	(0.62)	(0.02)	(0.02)
Gross state product	–0.03	–0.02***	–0.02***
	(0.04)	(0.00)	(0.00)
Constant	27.66**	15.10***	15.34***
	(9.17)	(2.18)	(2.20)
F-test (column 1) / Wald χ^2	6.17***	45.29***	48.21***
N	27	268	268

** p < 0.05* *** p < 0.01* **** p < 0.005*

Table 3: Estimates of teachers' union strength on change in states with mid to high CER scores

Independent variables	Initial year model	Baseline model	Full model	Binary interaction
Teachers' union opponents	–19.39	0.18	1.58	0.33
	(15.90)	(0.66)	(6.90)	(0.65)
Change in graduation rates (reversed) / Binary=1 for falling graduation rates	6.67	4.06	4.81	0.38
	(18.03)	(4.53)	(4.51)	(0.44)
Teachers' union opponents *X* change in graduation rates or binary indicator	14.06	–	–1.35	–1.23*
	(15.98)		(7.05)	(0.62)
Average of neighbor state CER scores	0.47	0.12	0.11	0.14
	(0.27)	(0.09)	(0.09)	(0.09)
Average of neighbor state changing graduation rates	–	3.08	–34.56	–26.38
		(6.16)	(20.76)	(20.71)
Average of neighbor state CER score *X* average of state graduation rate change	–	–	2.33	1.68
			(1.24)	(1.24)
Percentage of legislative Democrats	8.25	–11.38*	–12.21*	–12.27*
	(20.93)	(5.45)	(5.41)	(5.38)
Governor is a Democrat	2.97	–0.23	–0.14	–0.18
	(3.37)	(0.48)	(0.48)	(0.47)
Student enrollment	–0.02	0.07**	0.06*	0.07**
	(0.14)	(0.03)	(0.03)	(0.03)
Number of teachers	0.20	–0.51	–0.39	–0.48
	(2.53)	(0.40)	(0.40)	(0.40)
State deficit	0.14	–0.02	–0.02	–0.02
	(0.23)	(0.02)	(0.02)	(0.02)
Gross state product	0.02	–0.01***	–0.01***	–0.01***
	(0.01)	(0.00)	(0.00)	(0.00)
Constant	0.87	13.97***	13.54**	17.69***
	(23.50)	(4.90)	(4.87)	(2.31)
F-test (column 1) / Wald χ^2	12.06***	52.33***	57.15***	61.51***
N	20	206	206	206

** p < 0.05* *** p < 0.01* **** p < 0.005*

Table 4: Estimates of diffusion variables in states without significant change in graduation rates

Independent variables	Baseline model	Full model
Charter school proponents strength	0.00 (0.53)	0.10 (0.52)
Teachers' union strength	0.99 (0.81)	1.39 (0.81)
Average of neighbor state CER scores	0.03 (0.09)	–0.01 (0.09)
Average of neighbor state changing graduation rates	6.63 (6.81)	–64.19* (27.85)
Average of neighbor state CER scores *X* average of neighbor state changing graduation rates	–	4.04** (1.54)
Percentage of legislative Democrats	–16.55*** (5.63)	–17.16*** (5.55)
Governor is a Democrat	0.60 (0.45)	0.72 (0.45)
Student enrollment	0.08*** (0.03)	0.08** (0.03)
Number of teachers	–0.32 (0.42)	–0.24 (0.41)
State deficit	–0.04 (0.02)	–0.04 (0.02)
Gross state product	–0.02*** (0.00)	–0.02*** (0.00)
Constant	16.09*** (2.59)	16.76*** (2.57)
Wald χ^2	48.67***	57.31***
N	214	214

** p < 0.05* *** p < 0.01* **** p < 0.005*

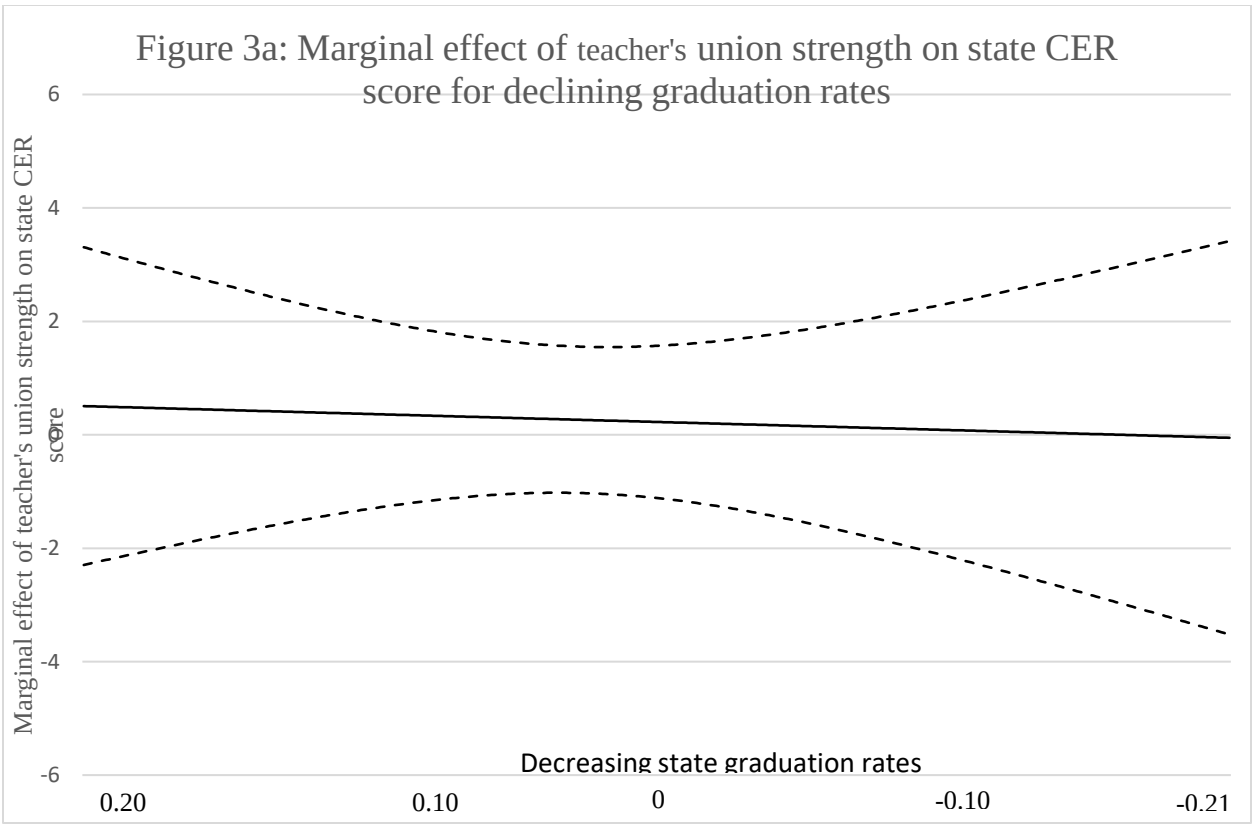
Figure 3a: Marginal effect of teacher's union strength on state CER score for declining graduation rates
Marginal effect of teacher's union strength on state CER score
6
4
2
0
-2
-4
-6
Decreasing state graduation rates
0.20
0.10
0
-0.10
-0.21

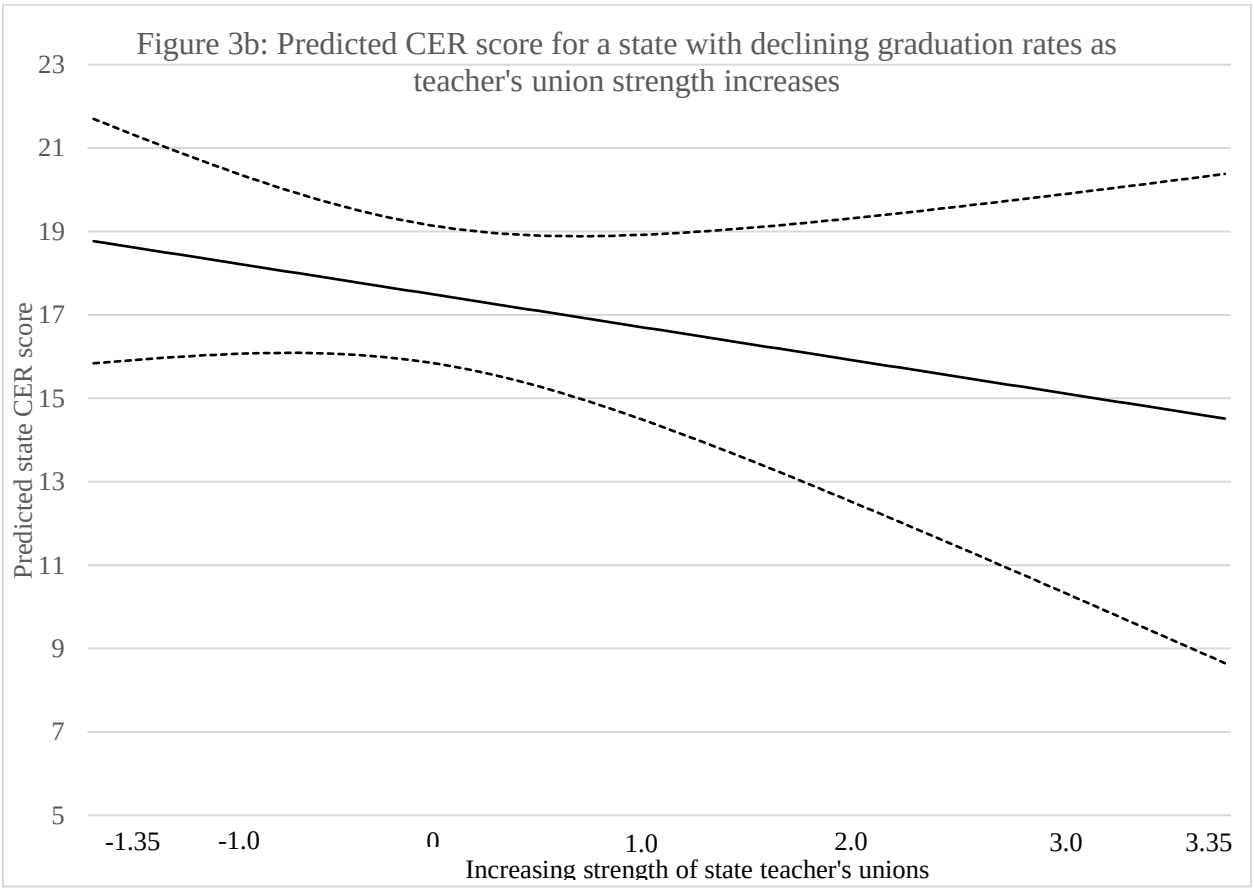
Figure 3b: Predicted CER score for a state with declining graduation rates as teacher's union strength increases
Predicted state CER score
23
21
19
17
15
13
11
9
7
5
-1.35
-1.0
0
1.0
2.0
3.0
3.35
Increasing strength of state teacher's unions

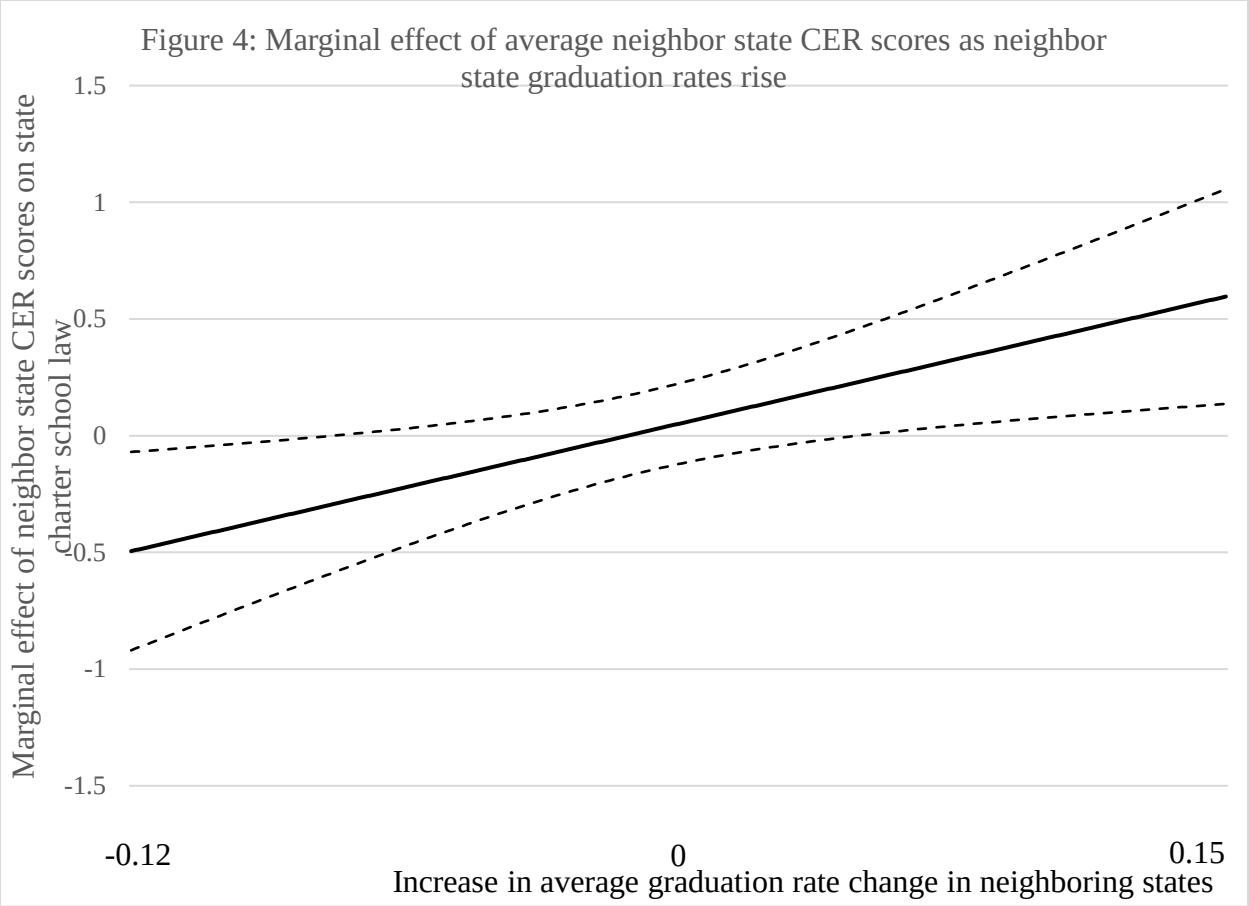
Figure 4: Marginal effect of average neighbor state CER scores as neighbor state graduation rates rise
Marginal effect of neighbor state CER scores on state charter school law
1.5
1
0.5
0
-0.5
-1
-1.5
-0.12
0
0.15
Increase in average graduation rate change in neighboring states

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[1] Boushey (2010), however, notes that policy diffusion does not follow an *S*-curve pattern when states start adopting the same policy in response to mandates from the federal government.

[2] This assumption is even more pronounced in the version of bounded rationality known as Prospect Theory developed by Kahneman and Tversky (1979).

[3] Todd discusses "cue dimensions" where deciders abruptly jump from one source in the same cue-type to another, like jumping from one interest group to another but continuing to use groups as cues. This parallels Jones's argument (2001, p. 204) that boundedly rational deciders lurch around in their decisions.

[4] Not all diffusion scholars accept the idea that states primarily look to their neighbors for inspiration and lessons. Grossback, Nicholson-Crotty, and Peterson (2004) argue that lawmakers are actually more inclined to look at developments in ideologically similar states.

[5] Our graph of enactments showing this *S*-pattern is included in our online appendix.

[6] The five indicators we use are rankings regarding the degree to which a state's law allows multiple government agencies to approve charter schools, does so without imposing a third party consent, provides a waiver from most state and local laws, provides legal operating autonomy from regulatory oversight, and guarantees public per-pupil funding. See our online appendix.

[7] Figures for all states enacting laws from 1991 to 1997, and all states with charter laws, are in our online appendix.

[8] Given its unique idiosyncrasies, we do not include the District of Columbia.

[9] We obtained the data from reports published by the National Alliance for Public Charter Schools at http://dashboard.publiccharters.org/dashboard/students/page/overview/state/ .

[10] We assume, of course, that parents care a great deal about their children's education. No parent is forced to send their students to a charter school, so it is a proactive choice made in what they believe to be in the best interests of their children. We feel it is little different than the emphasis parents put on selecting good school districts when buying a house, which evidence from the National Household Education Survey finds is one of the most important factors in buying a house for half of surveyed parents (see Henig and Sugarman 1999, p. 16).

[11] The Molnar reports are now available at http://necp.colorado.edu/publications/all, though a keyword search for "education management organizations" is required. Since Molnar never produced a report for 2014, we repeat the 2012 data.

[12] A factor analysis with an eigenvalue of 1.99. The factor loading scores are 0.91 for the number of charter schools, 0.92 for charter school enrollments, and 0.55 for EMOs.

[13] For 2012 and 2014 NEA state membership data we rely on the work of an organization called the Education Intelligence Agency. While we know this organization is skeptical of teacher unions, the data on state NEA membership was obtained by their researchers calling state NEA affiliates and asking, so we believe the data to be accurate. See http://www.eiaonline.com/NEAMembership2013-14.pdf.

[14] Using the Institute's database at http://www.followthemoney.org, we find all teachers' union contributions made to candidates for state office, like the legislature, for both years in each cycle. For instance, 1998 observations use data for 1997 and 1998 contribution cycles.

[15] Their data and supporting documentation can be found at http://unionstats.com/.

[16] The eigenvalue is 0.93. The loading of the three variables on it are 0.70 for NEA membership, 0.54 for public union coverage, and 0.44 for NEA contributions.

[17] The data from NCES is the percentage of 9th graders who are then recorded as graduating after 12th grade. Therefore our graduation rate change variable is calculated by calculating the difference between the percentage in one year from the percentage in the following year.

[18] We reverse the graduation rate variable when interacting it with the union strength variable, so higher values are lower rates and thus make the multiplicative term consistent.

[19] This comes from the annual publication *The Book of the States*.

[20] The data is at https://rcfording.wordpress.com/state-ideology-data/.

[21] The deficit variable was created by finding total state revenues and total state expenditures, subtracting the latter from the former. This data comes from the *Fiscal Survey of the States* published by the National Association of State Budget Officers. Data on gross state product comes from http://www.usgovernmentrevenue.com/compare_state_revenue.

[22] Our data is cross-sectional, time-series, so using OLS regression might not account for the grouped-by-state nature of our data and thus miss unobserved state-level factors (Ahn and Schmidt 1995). Grouping the data, though, requires either random-effects or fixed-effects. Neither is perfect, but Clark and Linzer (2015) note that fixed-effects uses dummy variables for each state. Since the number of observations in our models is small, this risks reducing each model's explanatory power by expanding the standard errors. They also note that fixed-effects models are

also sensitive to changes in random error when the number of panels for each state is relatively small, which is true here, especially if independent variables for each state change little over time. Random-effects models, on the other hand, assume that state differences are Normally distributed and so do not use dummies as state controls. While this makes the risky assumption that there is no correlation between the independent variables and any systematic state-to-state effect, we feel this concern is less threatening to the integrity of our results.

[23] The border state average graduation rate variable correlates too highly with the observed state's graduation rate to be included in these small *N* models.

[24] We tried re-estimating the same model for even later years, but the results did not change.

[25] It is worth noting that there are differences between the states, as seen in the fact that in the proponent model the standard deviation of the state slopes is 6.54 on the 30-point scale, and 5.05 in the opponents model and 9.57 in the diffusion model. However, the additional effect of citizen ideology on these slopes is almost negligible in the proponent and opponent models, being 0.09 and 0.08 respectively, though it is a little larger in the diffusion model at 0.11.

[26] It is worth noting that we re-estimated the baseline model with the three proponent strength indicators rather than the factor variable. Charter school enrollments was positive and significant, and number of schools and EMOs were both positive but only significant at $p < 0.10$. In the opponent baseline model, none of the three components were statistically significant, but neither is the factor variable in the baseline model in Table 3.

[27] Specifically, the predicted value of a state's CER score when the binary indicator is 0 is 17.69, and the linear prediction, or slope, when the indicator is 1 is –0.91.