

Financial Incentives in Vertical Diffusion: The Variable Effects of Obama's Race to the Top Initiative on State Policy Making

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Abstract

A substantial body of empirical work documents the influence of federal monies on state policy making. Less attention, however, has been paid to the conditioning effects of states' prior financial health. Nearly always, apportioned monies cover only a fraction of the costs of federal policy reforms. The capacity of states to deploy supplementary resources, therefore, may inform the willingness of states to comply with the federal government's policy objectives. Focusing on Barack Obama's Race to the Top (RttT) initiative, we present new evidence that state responses to federal initiatives that carry financial rewards systematically vary with the amount of resources already on hand. States that survived the Great Recession with their education budgets largely intact, we find, tended to implement more RttT reforms overall, and especially more reforms that required substantial up-front financial commitments. These patterns of policy adoptions can be meaningfully attributed to RttT, are not the result of either prior or ancillary policy trends, and speak to the general importance of accounting for what states already have, above and beyond what the federal government is willing to offer, when studying the financial incentives of vertical diffusion.

Keywords

executive politics, education policy, vertical diffusion, public policy, federal/state, federalism, political economy

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When policymaking opportunities at the national level falter, whether because of entrenched disagreement or constitutional restriction, federal actors may turn to the states. There, gains may be realized that simply are not possible either in Congress or the federal bureaucracy. Progress, however, need not be uniform. Depending on the inducements offered and states' preexisting capacity to deploy them, policy achievements may vary dramatically across state governments.

The vertical diffusion literature rather exhaustively demonstrates the federal government's ability to alter state policy making (for a review, see Dinan 2014). Much of this work attests to the influence gleaned by financial incentives. According to a variety of studies, policies supported by the federal government diffuse more rapidly when they are accompanied by financial rewards (Welch and Thompson 1980). The relevance of financial incentives, what is more, has been documented in a wide range of domains, including environmental policy (Clark and Whitford 2011; Daley and Garand 2005; Diamond 2009), welfare reform (Albritton 1989), and truth-in-sentencing laws (Allen, Pettus, and Haider-Markel 2004).

Although a large body of research has shown that states respond to federal incentives in the aggregate, much less is known about states' variable responses. Quite obviously, states that voluntarily accept financial support to undertake reforms are a special sample of all states, and understanding the characteristics of this select group is an essential part of assessing the ultimate success of any incentive-based reform. The distributional consequences of a federal policy initiative often matter just as much as its average effects. And for scholars interested in the utility of different federal incentive schemes, it may be just as valuable to know the distinguishing features of who responds as the magnitude of the average response.

This study identifies one such distinguishing feature: the resources states already have at their disposal. Rarely do federal incentive programs cover the full cost of an intervention, and when the provision of federal dollars is insufficient or uncertain, states have every reason to take stock of their own financial coffers before undertaking new reforms. The marginal value of additional federal dollars, after all, can only be evaluated with respect to financial resources already secured. To date, however, no empirical study within the vertical diffusion literature systematically evaluates whether the propensity of states to comply with federal initiatives varies according to the amount of resources on hand.

This article investigates the matter within the context of Barack Obama's Race to the Top (RttT) initiative. Established in 2009, RttT offered the promise of money—from a total cache of \$4.35 billion—in exchange for adopting specific policies prescribed by the Department of Education (ED). While substantial in the aggregate, these rewards covered a small fraction of the total costs of implementing and then sustaining targeted reforms to states' testing regimes, accountability systems, and school intervention protocols (Weiss 2013). To undertake these reforms, therefore, states would need to leverage substantial resources of their own.

For its sizable impact on education policies across the country (Howell and Magazinnik 2017), RttT merits scholarly attention. Because of its timing and design, however, RttT also presents a promising venue in which to evaluate the variable effects

of financial inducements on state policy making. This is no small matter. Numerous unobserved factors, after all, affect states' budgets and their propensity to consider federal funds, including their existing policy portfolios and redistributive preferences. RttT came on the heels of the Great Recession of 2007–08, which differentially (and unexpectedly) depleted state coffers. As analysts, therefore, we are in a position to exploit a plausibly exogenous source of variation in these potential confounders. What is more, in both its design and implementation, RttT reveals the federal government's policy interests and priorities with an unprecedented level of detail and clarity. Because it is possible to track states' progress on these policies over time, RttT also affords remarkably precise measures of state compliance to a federal initiative.

We present evidence that policy activity unleashed by RttT systematically varied according to states' prior financial resources. Regardless of whether they won, lost, or did not apply to the RttT competitions, states whose education resources were disproportionately negatively affected by the Great Recession were less likely to adopt RttT policies. These patterns of policy adoption, we show, do not appear to be either an artifact of pre-RttT policy trends or of a general increase in education policy activity during the Obama Administration. Estimated correlations between resource shocks and policy adoptions were concentrated among the subset of RttT policies that required large, up-front financial commitments; and they appeared largest in the first stage of the RttT competition, when the policy environment was most uncertain and the effects of the Great Recession most immediate. Meanwhile, we find no evidence that the variable effects of the Great Recession on education resources correlate with state adoptions of policies that were not rewarded under the RttT competitions, or that prior changes in education resources correlated with RttT policies before the competition.

We proceed as follows. After reviewing the existing literatures on vertical diffusion and fiscal federalism, we describe the unique features of RttT that facilitate the study of financial considerations in U.S. federalism. We then describe the process by which we collected data on RttT participation rates and policy adoptions. In our empirical section, we present our main findings, extensions, robustness checks, and the results of a variety of placebo tests. We conclude by discussing the generalizability of our findings not only to other federal incentive programs, but to broader developments in contemporary intergovernmental relations.

Vertical Diffusion and Fiscal Federalism

For decades, the federal government has employed financial mechanisms, particularly grants-in-aid, to alter state policy making (Dinan 2014). Whereas grants-in-aid were originally designed to help states pursue their own objectives, the 1960s inaugurated a new phase of federal intervention in subnational affairs, whereby "Congress asserted the national interest and authority in a wide range of governmental functions that had been the province, exclusively or predominantly, of state and local governments" (Sundquist 1969). The federal government advanced these objectives through a blend of both carrots and sticks: Title VI of the Civil Rights Act of 1964 and Title VIII of the Civil Rights Act of 1968, for instance, conditioned the provision of federal grants on

each state's willingness to abide by national anti-discrimination standards; and the Highway Beautification Act of 1965 made it possible for states to lose transportation funds if they did not adopt federal regulations regarding highway billboards (Advisory Commission on Intergovernmental Relations 1984).

The growing vertical diffusion literature documents the consequences of these efforts (Bahl and Saunders 1965; Benton 1992; Harrison 1975; Hedge 1983; O'Brien 1971; Osman 1966; 1968; Smith 1968; Volden 1999; Wilde 1968). For the most part, scholars have found, positive financial inducements accelerate the diffusion of federal policies (Welch and Thompson 1980). Whether by reducing the marginal costs of policy investments (Chubb 1985; Dubnick and Gitelson 1981; Eyestone 1977; Hamilton and Wells 1990; Peterson, Rabe, and Wong 1986) or producing a "flypaper effect," whereby federal expenditures stimulate further state spending within a policy domain (Clark and Whitford 2011), federal grants-in-aid reliably improve the chances that state governments will follow federal policy recommendations. They do so, moreover, across a wide variety of domains, including truth-in-sentencing laws (Allen, Pettus, and Haider-Markel 2004), welfare reform (Albritton 1989), environmental protection (Clark and Whitford 2011), and policies to promote hybrid-electric vehicles (Diamond 2009).

For all of its considerable strengths, this research has two principal shortcomings. The first concerns the precise object of study. Scholars tend to fix their attention on the size of the financial rewards offered by the federal government. As these rewards increase, scholars show, state policy adoptions tend to rise; and as they decline, so does state policy activity, and usually by common measure. Less attention, though, has been paid to each state's ability to put these rewards—whatever their size—to use. Instead, the literature tends to view subnational governments as interchangeable units, each equivalently capable of leveraging new federal resources for policy change. Rather obviously, though, they are not. As Nicholson-Crotty (2004) recognizes, the influence of financial inducements crucially depends on a state's preexisting policy objectives and its independent financial capacity to achieve them. These joint factors, moreover, can be expected to vary rather significantly across states. By downplaying the differences in state responses to federal interventions, the vertical diffusion literature largely overlooks the possibility of heterogeneous treatment effects, understood either by reference to within-state differences across multiple stages of the policymaking process or to across-state differences within any given stage of policy activity.¹

The second limitation concerns issues of measurement. For a variety of reasons, the existing literature has struggled to gauge the precise levels of state responsiveness to new financial incentives. Quite often, this is because the federal government does not elaborate clear standards for reform, so scholars are left to make judgment calls about how states' efforts conform to federal expectations. But not always. With RttT, ED publicly released a set of policy prescriptions that were described with unprecedented clarity and detail, allowing us to evaluate state policy adoptions according to these criteria. A further advantage of RttT is the sheer number of concrete policies that were incentivized by the competition, which allows for a uniquely fine-grained measure of responsiveness that can range from 0 to nearly 50 distinct interventions; most other studies, by contrast, have only one or at most a handful of indicators of success. And

because RttT offered sums of money commensurate with each state's population in the aftermath of profoundly disruptive shocks to state education resources, RttT offers a rare opportunity to measure the influence of state capacity, as distinct from the size of federal inducements, on the processes of vertical diffusion.

A Description of Race to the Top

Congress funded RttT through the American Recovery and Reinvestment Act (ARRA). Signed into law on February 17, 2009, the ARRA contained \$787 billion in tax cuts and economic stimulus spending. Roughly \$100 billion of the ARRA was allocated for education, of which \$53.6 billion went into the State Fiscal Stabilization Fund (SFSF). Within SFSF, however, \$5 billion was set aside for a competitive grant system, \$4.35 billion of which established RttT.

RttT operated over multiple competitive phases. In each phase, states were asked to describe their past policy achievements and outline their future goals in meeting policy priorities that spanned six major categories: teacher effectiveness, state involvement in education reform, standards and assessments, support for charter schools and other non-traditional public schools, school intervention procedures, and data systems. Within each category, ED established further point breakdowns for policy subcategories. Through direct communications and public announcement, ED provided extensive information about the kinds of policies that satisfied the demands of these categories.²

Participating in the various phases of the competition was entirely voluntary. Applications for Phase 1 were due on January 19, 2010. As summarized in Figure 1 and explicated in further detail in Supplemental Appendix A.2, 40 states and the District of Columbia submitted applications. Finalists were announced on March 4, 2010, and the two official winners were declared on March 29, 2010. Phase 1 winners Tennessee and Delaware were awarded roughly \$500 million and \$100 million, respectively, which that year amounted to 10.0% and 5.7% of the respective states' budgets for K-12 education.³ Phase 2 applications then were due on June 1, 2010. The application criteria were the same for Phase 2, though Phase 1 winners could not apply and other states could resubmit amended applications. A total of 35 states and the District of Columbia participated in Phase 2. Finalists were announced on July 27, 2010, and winners on August 24, 2010. Phase 2 had a total of 10 winners, each of which were awarded prizes of between \$75 million and \$700 million.⁴

Having exhausted the ARRA funds, the president in 2011 sought additional support for RttT. That spring, Congress allotted funds to support a third phase. Phase 3 differed from previous rounds in three important ways. First, only losing finalists from Phase 2 of the competition were allowed to participate. Second, the policy scope of Phase 3 was significantly smaller, as each competing state needed only to reconfirm their commitments to a subset of reforms they had made in their Phase 2 applications. States, however, had some latitude to choose the activities and projects from their Phase 2 application that they planned to focus on pursuing. Finally, a significantly higher percentage of participating states won in Phase 3 of the competition, though the amounts

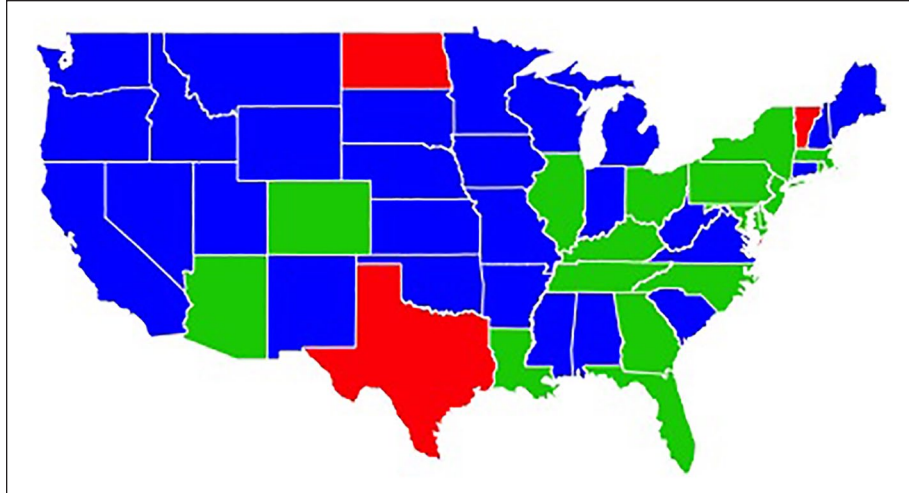


Figure 1. RttT winners, applicants, and non-applicants.

Note. States in green won any round of the competition; blue states applied but never won; red states never applied. Alaska, not shown, never applied to the competition, while Hawaii applied and won in the second round. RttT = Race to the Top.

of these grants were considerably smaller than those from Phases 1 and 2. On December 23, 2011, ED announced Phase 3 winners, which received prizes ranging from \$17.1 million to \$42.8 million. Over all three rounds, 18 states and the District of Columbia were awarded grants totaling \$4.1 billion. These awardees in aggregate serve approximately 2 million students, which account for approximately 45% of all K-12 students in the United States.

Due to unique features of its design and implementation, RttT presents a superb opportunity to clarify the role of financial incentives in vertical diffusion. For starters, RttT allows for accurate and precise measures of state compliance. In each phase, states were asked to describe their past policy achievements and outline their future goals in meeting policy priorities that spanned the competition's six major categories. Within each category, ED established further point breakdowns for policy subcategories. Through direct communications and public announcements, ED provided extensive information about the kinds of policies that satisfied the demands of these categories. (See Supplemental Appendix A.3 for the summary document released by ED.) After it selected the winners in each phase of the competition, ED released the scores, by section, for all applicants. Since state applications to the competition were made public, we can construct a measure of compliance with federal objectives that is not only accurate, but extremely precise: the number of promises and the types of promises made capture far more information about responsiveness to federal influence than the binary outcomes observed in most studies of vertical diffusion; and whereas past studies only document compliance patterns at a single point in time, we track

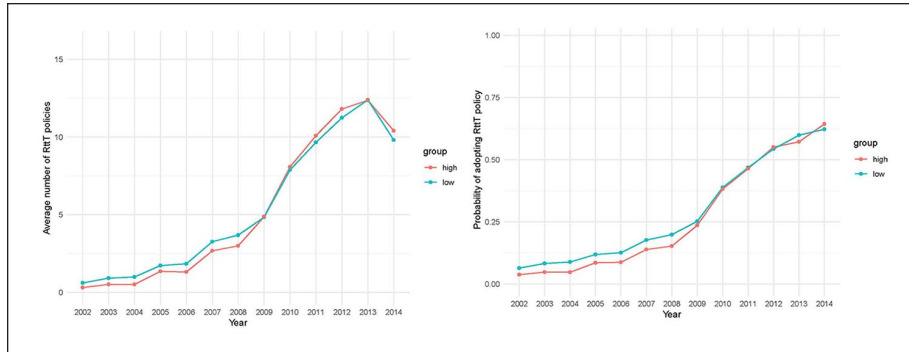


Figure 2. RttT policies adopted by year.

Note. We compute the difference between states' 2009 and 2007 logged per-pupil education revenues and construct the "high" group as the states that were above the median of this value; the "low" group is those that were at or below the median. The "high" states, shown in red, are the ones that gained most (or suffered least) over the course of the Recession. RttT = Race to the Top.

them at multiple stages of the process, from initial policy commitment through final implementation.

The timing of RttT also facilitates the study of financial incentives on vertical diffusion. Given the fungibility of resources and the adaptive responses of states, it is obviously difficult to assess the influence of existing state budgets net of any fiscal incentives offered by the federal government. In the case of RttT, however, these challenges at least attenuate. Between 2007 and 2009, states across the nation rather unexpectedly lost substantial tax revenues, which, in some instances, had devastating consequences for their education budgets. According to some estimates (see, for example, Leachman and Mai 2014), as late as 2012, state and local education funding levels in 35 states remained lower than where they stood in 2007. That said, not all states lost money. While the education budgets of most states declined during this period, the size of these losses varied dramatically, and some states, notably North Dakota and Alaska, experienced large windfalls in state revenues (see Figures 1 and 2 of Leachman and Mai 2014). The timing of RttT, as such, allows us to assess how variable fiscal shocks to state budgets impacted states' responsiveness to financial rewards for policy adoption.

Data

We collected an original dataset tracking the implementation of RttT policies in every state and year from 2002 to 2014. To do so, we identified a set of relevant policies using a document released by ED in November 2009 that specified the criteria of the competition. Each section of this document corresponded to a policy innovation on which states were assessed: for example, in section D(2)(i), states were graded on the basis of their support for policies to measure student growth for each individual

student. The final result was a panel dataset with an observation for every policy, state, and year, with the dependent variable of policy adoption coded 1 if the policy was in place at any time of that year, and 0 otherwise.

To construct this dataset, we first consulted the applications states submitted to the competition, which were publicly available from ED. Here, states often explicitly cited the legislation enacting the policies in question. We also relied on secondary sources such as reports written by research and advocacy organizations tracking states' progress on specific policies over time (e.g., the National Alliance for Public Charter Schools, Center for Education Reform, and Education Counts). Where necessary, we used media reports on state legislation to fill in gaps. Finally, we used states' online legislative databases and the LexisNexis State Capital database to confirm that the text of the legislation met the requirements for the variable definition.

For this panel, we were only interested in policies that were encoded in state law, not those practiced informally or adopted administratively. We applied this conservative definition of implementation for a few reasons. First, non-legislative policies are more difficult to observe and assess consistently, so including them would result in a noisier implementation measure. Relatedly, just as administrative actions may be more easily undertaken in the short run than passing legislation, they can be overturned or ignored in the aftermath of a new election. By focusing on legislative action, we run the risk of underestimating RttT's impact, but can be confident that we are capturing a deeper and more enduring type of policy change.

Policies lacking variation across states and those that were excessively subjective or vague were excluded from this analysis. Applying our selection criteria, we ultimately isolated 20 distinct policies that covered such topics as charter schools, data management, intervention into low performing schools, and the use of test scores for school personnel policy.⁵ Table 1 enumerates these policies and summarizes their adoption rates before and after RttT was announced.

Not all policies required equal levels of commitment. On one end of the spectrum, a number of policies required only a small investment of financial resources to get off the ground. For instance, it was relatively cheap for states to change protocols for evaluating teachers and to commit to common national educational standards, even if they did face vocal and well-organized opposition from teachers' unions, Tea Party activists, and parent groups. Other policies, meanwhile, required substantial up-front investments, such as those that funded new data systems linking students to teachers while complying with federal privacy regulations—a herculean task for less technologically advanced states. Because we expect that the promise of financial resources might differentially affect states' propensity to adopt policies involving large up-front investments versus those that could be implemented at low short-term cost, we also coded this variable for each policy. These “expensive” policies are indicated with a checkmark in Table 1.⁶

To gauge whether the effects we observe are unique to RttT policies rather than secular trends, we also tracked education policies that were not part of Obama's agenda or the RttT package, but that had been discussed and considered by education reformers over the same time period. This control set included exit exams that students must pass

Table 1. Race to the Top Initiative Policy Descriptions and Rates of Adoption.

Application section	Up-front investment	Adoption rate (overall)	Adoption rate (2002–09)	Adoption rate (2010–14)
Section B: Standards and Assessments				
<i>standardsconsortium</i> (B)(1)(i)		0.42	0.12	0.91
State is in a consortium working to develop common K-12 standards.				
<i>standardsadopt</i> (B)(1)(ii)		0.34	0.00	0.87
State will have adopted common K-12 standards by 2010.				
<i>commonassessments</i> (B)(2)		0.41	0.14	0.85
State is in a consortium working to develop assessments aligned with common K-12 standards.				
Section C: Data Systems to Support Instruction				
<i>data1</i> (C)(1)	✓	0.98	0.93	1.00
State has a longitudinal data system that includes a unique identifier for every student.				
<i>data8</i> (C)(1)	✓	0.65	0.38	0.82
State has a longitudinal data system that includes a way to match teachers to students.				
Section D: Great Teachers and Leaders				
<i>pathwaysroutes</i> (D)(1)(i-ii)		0.04	0.00	0.10
State legally allows and currently uses alternative routes to teacher certification.				
<i>measuregrowth</i> (D)(2)(i)		0.26	0.04	0.61
State has a system to measure student growth for each individual student.				

(continued)

Table 1. (continued)

Application section	Up-front investment	Adoption rate (overall)	Adoption rate (2002–09)	Adoption rate (2010–14)
<i>evalsystem1</i> (D)(2)(ii) State has an educator evaluation system with multiple rating categories to differentiate effectiveness for teachers and principals.		0.25	0.04	0.59
<i>evalsystem2</i> (D)(2)(ii) Educator evaluation system takes into account student growth.		0.26	0.03	0.62
<i>annualevals</i> (D)(2)(iii) Annual evaluation systems inform decisions about teacher pay and promotion for principals and teachers.		0.21	0.06	0.44
<i>evalsprofdev</i> (D)(2)(iv)(a) Annual evaluation systems inform decisions on professional development for principals and teachers.		0.25	0.05	0.57
<i>evalsreward1</i> (D)(2)(iv)(b) Annual evaluation systems inform decisions on compensation for principals and teachers.		0.13	0.00	0.33
<i>evalsreward2</i> (D)(2)(iv)(b) Annual evaluation systems allow highly effective teachers to gain additional responsibilities.		0.07	0.00	0.18
<i>evalsteture</i> (D)(2)(iv)(c) Annual evaluation systems inform tenure decisions for teachers.		0.17	0.01	0.43

(continued)

Table 1. (continued)

Application section	Up-front investment	Adoption rate (overall)	Adoption rate (2002–09)	Adoption rate (2010–14)
<i>evalsfire</i> (D)(2)(iv)(d) Annual evaluation systems inform removal of ineffective teachers and principals.		0.17	0.02	0.42
<i>preprogram</i> (D)(4) State has a plan to link student growth data to teachers and to link that information to teacher credentialing programs, and to expand most effective programs in the state.	✓	0.03	0.01	0.06
Section E: Turning Around the Lowest-Achieving Schools <i>lowachievereintervene</i> (E)(1) State has authority to intervene directly in the lowest-achieving schools.		0.37	0.26	0.58
Section F: General Selection Criteria <i>innovativeschoolsnumber</i> (F)(2)(i) State law does not prohibit increasing number of charter schools.	✓	0.32	0.27	0.43
<i>innovativeschoolsauth</i> (F)(2)(ii) State law requires student achievement to be a factor in charter school renewal.		0.40	0.31	0.56
<i>innovativeschoolsbuild</i> (F)(2)(iv) State provides charter schools with funding for facilities and other support.	✓	0.33	0.28	0.40

in order to leave third grade as well as to graduate from high school, and tax credits for donations to nonprofit organizations that provide private school scholarships.

We supplemented our own data collection with education finance data from the National Center for Education Statistics (NCES), which tracks states' annual revenues, or the funds available for expenditure by the local education agencies in the state.⁷ This includes funds from state sources (both direct funds from state governments and funds in lieu of taxation), from the federal government (including direct grants-in-aid to schools or agencies, funds distributed through a state or intermediate agency, and revenues in lieu of taxes to compensate a school district for nontaxable federal institutions within a district's boundary), and from localities (including revenues from local property and nonproperty taxes, investments, and student activities such as textbook sales, transportation and tuition fees, and food service revenues). Finally, we constructed a panel of political and economic variables from the U.S. Election Atlas and the Census, including the partisan composition of the state legislatures, the governor's party, and the Democratic presidential vote share in the 2008 election, as well as data on the financial impact of the recession, including mortgages and unemployment. Table 2 provides descriptive statistics on these variables.

Figure 2 summarizes the trends in states' RttT policy adoptions over time by the education revenue losses they realized over the Recession. In the left panel, we plot the average total number of policies states had in place in every year, and in the right panel, we plot the average probability of adopting an RttT policy. By both measures, states that gained the most (or lost the least) during the Recession lagged slightly behind harder-hit states in their adoption of RttT policies before 2009, but they caught up, even slightly surpassed them, in the wake of the economic downturn.

Empirical Strategy

The relationship between states' education budgets and their propensity to adopt RttT policies is clouded by a number of confounding factors: for instance, more Democratic states tend to allocate more money to education while simultaneously sharing more of Obama's policy preferences, and richer states may be more willing to experiment with education reform more generally, not RttT policies specifically. These spurious correlations do not represent the causal effect of states' own resources on their susceptibility to vertical diffusion. The ideal statistical test of this effect, therefore, would randomly assign education budgets to states and then measure the marginal effect of having more funding on the propensity to adopt RttT policies in the post-competition period.

Of course, we are in no position to conduct such an analysis. But by introducing an exogenous shock to states' education budgets, and thereby creating variation in their capacity for education reform that was uncorrelated with pre-Recession trends in revenues, the Great Recession of 2007–09 puts us as close to this idealized experiment as we are likely to come. The variation caused by the Great Recession was significant, driven by across-state differences in household debt-to-income ratios (Mian and Sufi 2011), credit constraints (Chodorow-Reich 2014; Goetz and Gozzi 2010; Greenstone, Mas, and Nguyen 2014; Guerrieri and Lorenzoni 2011), and policy uncertainty (Shoag

Table 2. Descriptive Statistics.

	Minimum	Maximum	Median	<i>M</i>	<i>SD</i>
Δ Per-Pupil Education Revenue, 2007–09 (dollars)					
From Federal, State, and Local Sources	–509	3,371	931	1,078	768
Federal	–288	889	92	163	219
State	–739	2,755	418	499	669
Local	–565	3,312	277	422	606
Δ Total Per Capita State Revenue, 2007–09 (dollars)	–500	2,014	266	324	451
Per-Pupil Education Revenue, 2010–14 (dollars)					
From Federal, State, and Local Sources	7,830	28,926	12,770	13,743	3,976
Federal	758	3,538	1,397	1,490	473
State	3,185	18,205	5,963	6,484	2,593
Local	314	26,163	4,864	5,795	3,726
Total Per-Capita State Revenue, 2010–14 (dollars)	6,339	23,518	8,196	8,854	2,719
Political Controls					
Proportion of State House– Democrats, 2010–14	0.13	0.92	0.47	0.48	0.17
Democratic Governor, 2010–14	0	1	0	0.42	0.50
Democratic Vote Share, 2000 Presidential Election	0.26	0.85	0.46	0.46	0.10
Democratic Vote Share, 2008 Presidential Election	0.32	0.93	0.52	0.51	0.11
Number of RttT Policies Adopted (2002–09)					
Overall	0	15	2	2.04	2.08
Winners	0	15	3	2.84	2.46
Applicants (Non-winners)	0	7	1	1.61	1.72
Non-applicants	0	4	1	1.25	1.14
Number of RttT Policies Adopted (2010–14)					
Overall	2	18	11	10.36	4.09
Winners	6	18	14	13.18	3.21
Applicants (Non-winners)	2	16	9	9.17	3.56
Non-applicants	3	10	5	5.35	1.90

Note. RttT = Race to the Top.

and Veuger 2016). Since the Recession originated in the collapse of a speculative housing bubble, its impacts can be captured in part by the change in the average mortgage from 2007 to 2009. Another, more general measure of its impact is the change in the unemployment rate over the same period. As shown in Figure 3, both variables correlate strongly with changes in states' per-pupil education revenues from 2007 to 2009; in other words, the extent to which states' education resources suffered during this time was a direct consequence of how hard-hit they were by the Recession.

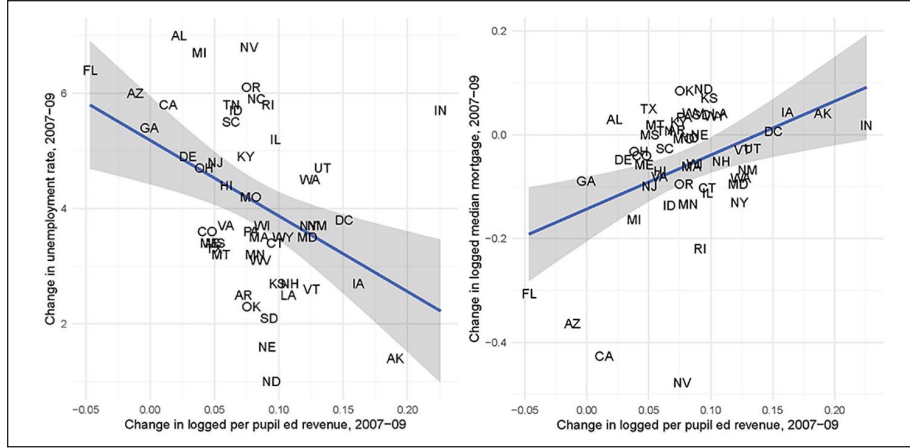


Figure 3. Bivariate relationship between recession shock and education revenues: (a) unemployment and (b) mortgages.

Fortunately for us, this variation is neither a function nor a continuation of any secular pre-2007 trends: as shown in Figure 4, changes in education revenues from 2007 to 2009 are completely uncorrelated with changes from 2004 to 2006.

We use the exogenous shock of the Recession to assess the relationship between resources and state policy adoptions. We estimate a linear probability model with the following form:

$$\begin{aligned} \Pr(Y_{itp} = 1) = & \beta_0 + \beta_1 \Delta EdRevenue_i + \beta_2 EdRevenue_{it} + \beta_3 Won_{it} + \beta_4 Lost_{it} \\ & + \beta_5 DemGovernor_{it} + \beta_6 DemHouse_{it} + \beta_7 DemPresidentialVote_{it} \\ & + \beta_8 PolicyInPlace2008_{ip} + \eta_t + \gamma_p + \varepsilon_{itp} \end{aligned}$$

where the dependent variable is a binary indicator for having policy p in place in state i and year t from 2010 to 2014. Our main independent variable of interest is the difference between the logged per-pupil education revenue from state, federal, and local sources in 2009, by which point most of the losses associated with the economic downturn had been realized, and that in 2007, the baseline level before the Recession hit. Calculated as such, this variable represents the logged proportion of 2009 to 2007 per-pupil education revenues.⁸ Won_{it} and $Lost_{it}$ are binary indicators for winning RttT and applying but never winning, respectively, at any time up to and including year t ; the omitted category in every year is therefore states that never applied in the time up to and including year t . To account for non-independence of observations within states, we cluster standard errors by state throughout the analysis.

Although the shock to education budgets was uncorrelated with prior trends in education resources, how states adjusted to that shock was not random. Thus,

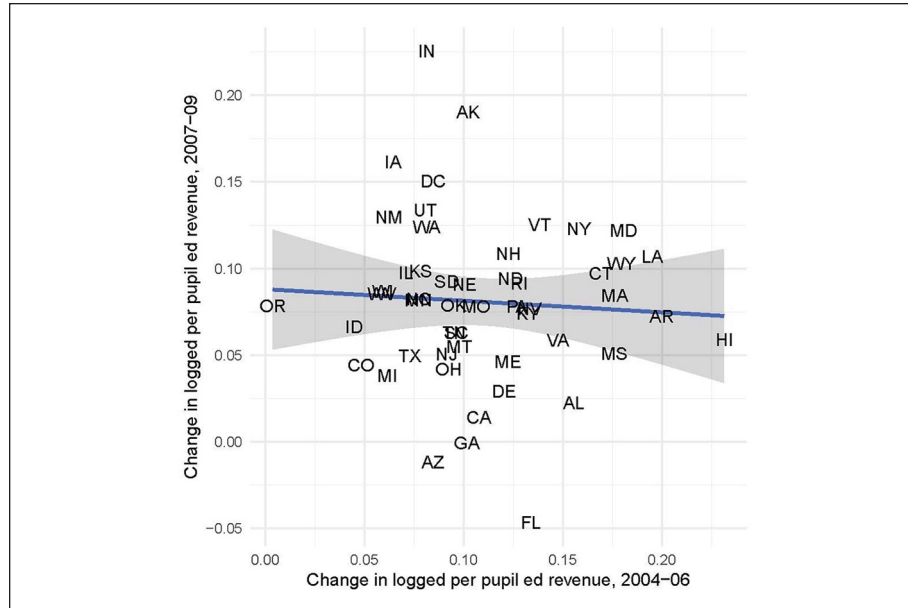


Figure 4. Bivariate relationship between recession and pre-recession trends in education revenues.

we control for a few additional variables that may be correlated with both education revenue changes and the propensity to adopt policies championed by a Democratic president. First, we account for the partisan climate in the state, measured by the state's Democratic vote share in the last presidential election (2008), the proportion of seats in the state's lower legislative chamber held by Democrats, and a binary indicator for whether the governor is a Democrat. Second, we condition on the number of policies satisfying RttT's requirements that each state already had in place in 2008, the year before the announcement of the competition. Finally, in some of our models, we include a control for education revenue levels in year t in addition to changes from 2007 to 2009, since they may be correlated with states' relative losses in the Recession and also condition their ability to adopt new policies.

To purge our estimates of the influence of aggregate temporal trends in the dependent and independent variables, and to account for the fact that different policies had different baseline adoption rates, we also include year and policy fixed effects. Thus, our estimates in the fully specified model represent the deviation from the year and policy-specific average probability of RttT policy adoption that is caused by the exogenous shock to a state's financial resources. This is the causal effect of interest under the assumption that there are no omitted time-variant policy-specific confounders affecting both education revenues and adoption rates.

Main Results

As shown in Table 3, our main estimates are consistent across a variety of specifications, including the inclusion of policy and year fixed effects as well as controls for the levels of education revenue at a state's disposal. Notably, though perhaps not surprisingly, the outcomes of the competition correlate with state policy adoptions. Winners were consistently more likely than non-applicants to adopt RttT policies: depending on the specification, their probabilities of adoption varied by 39 to 43 percentage points for these two groups. This difference is observed even when controlling for whether the policy was in place in 2008, which mitigates concerns about selection into the competition.

Of course, winners were chosen in part for having the most well-developed plans for future policy implementation, and upon winning they became contractually bound by the promises they made in their applications. What is therefore somewhat more surprising—though consistent with ED's aims—is that applicants that did not win funding at time t were nonetheless more likely—by 21 to 22 percentage points across all specifications—to have an RttT policy in place at that time. In part, this can be explained by the fact that states adopted policies in order to be competitive for RttT, before knowing whether or not they would receive funding. These early efforts may have also spurred investments and interest group mobilization that were hard to reverse even when financial support did not come through from ED.

For our purposes, however, the key finding concerns the estimated relationship between a state's education revenues and its policy activity. Whether they won, lost or never applied to the competition, we find, all states coming out of the Recession with greater financial resources were more likely to adopt RttT policies. According to the fully specified model (Column 3b), a one-unit change in the logged proportion of 2009 to 2007 per-pupil education revenues was associated with a 0.82-unit increase in the probability of adopting an RttT policy. To put this on a more readily interpretable scale, the range of the independent variable was from a minimum of -0.05 to a maximum of 0.23 , with a standard deviation of 0.05 ; thus, a one standard deviation increase in the independent variable was associated with a 4 percentage point increase in the probability of RttT policy adoption. Traveling the full range of the independent variable—from the state with the largest realized losses to the one with the largest gains—was associated with a 23 percentage point increase in the probability of adopting a RttT policy.

The average revenue effects presented thus far mask considerable variation across RttT policies. As shown in Table 4, those policies that involved a large up-front investment of financial resources generated the largest effects. Here, we present results from the fully specified models in columns 3a and 3b of Table 3, estimated separately on the "cheap" and "expensive" subgroups discussed in the "Data" section above. Within the expensive policy group, which included reforms such as setting up data systems that tracked individual student growth and increasing funding to charter schools and successful teacher preparation programs, a one standard deviation increase in the logged proportion of 2009 to 2007 per-pupil revenues was associated with a 6 percentage

Table 3. Effect of Per-Pupil Education Revenues on Probability of RttT Policy Implementation, 2010–2014.

	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Change in logged per-pupil education revenue, 2007–09	0.904** (0.278)	0.929** (0.294)	0.771* (0.312)	0.821* (0.320)	0.769* (0.314)	0.818* (0.324)
Logged per-pupil state education revenue		–0.015 (0.074)		–0.029 (0.070)		–0.028 (0.071)
Won RttT	0.430*** (0.034)	0.428*** (0.035)	0.389*** (0.038)	0.385*** (0.039)	0.391*** (0.038)	0.387*** (0.039)
Applied to RttT and lost	0.215*** (0.029)	0.212*** (0.032)	0.214*** (0.030)	0.208*** (0.034)	0.214*** (0.030)	0.208*** (0.034)
Democratic governor	–0.002 (0.043)	–0.002 (0.042)	–0.002 (0.039)	–0.001 (0.039)	–0.002 (0.039)	–0.001 (0.039)
Proportion of state House, Democrats	–0.153 (0.136)	–0.150 (0.139)	0.050 (0.144)	0.056 (0.148)	0.051 (0.146)	0.056 (0.149)
Democratic vote share, 2008	0.281 (0.249)	0.294 (0.249)	0.076 (0.258)	0.099 (0.257)	0.071 (0.260)	0.095 (0.260)
presidential election						
Policy was in place in 2008	0.505*** (0.028)	0.506*** (0.028)	0.511*** (0.028)	0.512*** (0.028)	0.483*** (0.031)	0.484*** (0.031)
Constant	0.057 (0.105)	0.190 (0.679)	–0.052 (0.104)	0.205 (0.646)	–0.114 (0.116)	0.141 (0.665)
Policy fixed effects	No	No	No	No	Yes	Yes
Year fixed effects	No	No	Yes	Yes	Yes	Yes
R ²	.209	.209	.236	.236	.430	.430
N	4,762	4,762	4,762	4,762	4,762	4,762

Note. Standard errors clustered by state in parentheses. Significance tests are two-tailed. Dependent variable is a binary indicator for whether a given policy was in place in a state and year. Democratic vote share and proportion of state House variables are scaled from 0 to 1. Unit of analysis is policy by state by year. RttT = Race to the Top.

* $p < .05$. ** $p < .01$. *** $p < .001$.

Table 4. Effect of Per-Pupil Education Revenues on RttT Policy Implementation, 2010–14 Expensive and Cheap Policy Subsets.

	Expensive policies		Cheap policies	
	(1a)	(1b)	(2a)	(2b)
Change in logged per-pupil education revenue, 2007–09	1.152*** (0.303)	1.276*** (0.312)	0.644 (0.388)	0.666 (0.398)
Logged per-pupil state education revenue		–0.072 (0.050)		–0.013 (0.092)
Won RttT	0.262*** (0.064)	0.251*** (0.062)	0.439*** (0.049)	0.437*** (0.051)
Applied to RttT and lost	0.094 (0.054)	0.079 (0.053)	0.257*** (0.042)	0.254*** (0.047)
Democratic governor	0.008 (0.028)	0.010 (0.028)	–0.004 (0.048)	–0.004 (0.048)
Proportion of state House, Democrats	–0.243* (0.109)	–0.228* (0.109)	0.160 (0.183)	0.163 (0.187)
Democratic vote share, 2008 presidential election	0.200 (0.218)	0.261 (0.223)	0.038 (0.312)	0.048 (0.311)
Policy was in place in 2008	0.564*** (0.040)	0.566*** (0.040)	0.431*** (0.046)	0.431*** (0.046)
Constant	0.144 (0.107)	0.789 (0.460)	–0.189 (0.137)	–0.075 (0.865)
Policy fixed effects	Yes	Yes	No	No
Year fixed effects	Yes	Yes	Yes	Yes
R ²	.592	.593	.374	.374
N	1,362	1,362	3,400	3,400

Note. Standard errors clustered by state in parentheses. Significance tests are two-tailed. Dependent variable is a binary indicator for whether a given policy was in place in a state and year. Democratic vote share and proportion of state House variables are scaled from 0 to 1. Unit of analysis is policy by state by year. RttT = Race to the Top.

* $p < .05$. ** $p < .01$. *** $p < .001$.

point increase in the probability of RttT policy adoption; going from the minimum to the maximum of the independent variable was associated with a 32 to 36 percentage point increase. By contrast, the point estimates for the cheap policy subgroup are roughly half the size in magnitude and do not approach standard thresholds for statistical significance.

The patterns we observe among policies involving a large up-front investment of resources and those that did not tell two different stories about how financial considerations operated in the context of the competition. Cheap policies were taken up by both winners and losers, by 44 and 25 percentage points more than non-applicants, respectively, according to the fully specified model. Because these reforms did not strain states' resources in the short run, neither current levels nor recent changes in education revenues explain the decision to adopt them. Rather, it seems that participation in the competition alone was enough to mobilize states to action. By contrast, for expensive policies, the effect of winning the competition is dramatically reduced compared to the cheap policy subgroup, and the effect of applying and losing becomes statistically indistinguishable from zero. For these more financially demanding reforms, the outcomes of the competition hardly mattered if the independent financial resources were unavailable to support policy change. Viewed slightly differently, the results speak to the important spillover effects induced by the design of the competition: rather than merely controlling participants' behavior through the use of external financial levers, RttT defined the pool of policies on which *all states* with the financial freedom to undertake reform spent their own education dollars.

Robustness Checks and Extensions

To rule out the possibility that our results are driven by secular trends, we estimate our main models on a sample of control policies that were not part of RttT, but that were in the national conversation: policies related to third grade and high school exit exams, and to educational tax credits. The patterns observed in this sample, shown in Table 5, do not replicate our main results. As an additional check, we estimate our models on RttT policies before they became part of the president's agenda: over the period from 2003 to 2008. As shown in Table 6, we again see no revenue effects in this analysis. Taken together, these results suggest that RttT was responsible for the main effects of states' revenues on policy adoptions.

Throughout our main analysis, we have reported linear probability model estimates for ease of interpretation and because they do not demand the more strenuous assumptions required for consistency and unbiasedness of the nonlinear models. However, we recovered similar results under a logit model (see Supplemental Appendix Table C.1).

We also estimated our main models using alternative measures of states' education budgets on the expensive and cheap policy subsets. These results are presented in Supplemental Appendix Tables C.2 and C.3. Because we might be concerned that federal revenues were adjusted endogenously to balance out states' losses, we first reestimate our fully specified model using the sum of revenues from state and local sources, without including federal dollars (Columns 1a and 1b). When we do so, our

Table 5. Effect of Per-Pupil Education Revenues on Implementation of Control (Non-RttT) Policies, 2010–14.

	(1a)	(1b)
Change in logged per-pupil education revenue, 2007–09	0.514 (0.894)	0.651 (0.977)
Logged per-pupil state education revenue		–0.084 (0.113)
Won RttT	0.196* (0.074)	0.185** (0.065)
Applied to RttT and lost	0.123* (0.061)	0.106* (0.049)
Democratic governor	–0.071 (0.054)	–0.070 (0.054)
Proportion of state House, Democrats	0.220 (0.260)	0.237 (0.261)
Democratic vote share, 2008 presidential election	–0.483 (0.538)	–0.411 (0.543)
Policy was in place in 2008	0.830*** (0.046)	0.825*** (0.045)
Constant	0.048 (0.191)	0.804 (1.001)
Policy fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
R ²	.606	.607
N	689	689

Note. Standard errors clustered by state in parentheses. Significance tests are two-tailed. Dependent variable is a binary indicator for whether a given policy was in place in a state and year. Democratic vote share and proportion of state House variables are scaled from 0 to 1. Unit of analysis is policy by state by year. RttT = Race to the Top.

* $p < .05$. ** $p < .01$. *** $p < .001$.

Table 6. Effect of Per-Pupil Education Revenues on RttT Policy Implementation, 2003–08.

	(1a)	(1b)
Change in logged per-pupil education revenue, 2000–02	0.008 (0.107)	–0.023 (0.114)
Logged per-pupil state education revenue		0.027 (0.023)
Democratic governor	0.003 (0.009)	0.003 (0.009)
Proportion of state House, Democrats	0.033 (0.059)	0.040 (0.057)
Democratic vote share, 2000 presidential election	0.004 (0.085)	–0.035 (0.076)
Policy was in place in 2002	0.933*** (0.024)	0.932*** (0.024)
Constant	–0.023 (0.032)	–0.247 (0.196)
Policy fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
R ²	.523	.523
N	5,051	5,051

Note. Standard errors clustered by state in parentheses. Significance tests are two-tailed. Dependent variable is a binary indicator for whether a given policy was in place in a state and year. Democratic vote share and proportion of state House variables are scaled from 0 to 1. Unit of analysis is policy by state by year. RttT = Race to the Top.

* $p < .05$. ** $p < .01$. *** $p < .001$.

estimates remained largely unchanged. When we focus on state sources of revenue alone, however, we no longer see statistically significant effects, suggesting that local revenues are an important part of the story (Columns 2a and 2b). Finally, we use a state's total revenues instead of revenues for education specifically (Columns 3a and 3b), recognizing that states had the freedom to reallocate funds to education from other sources if their education coffers were particularly hard-hit. On this measure, too, we see null effects, suggesting that it was the money available for education reform specifically that drove states' decision making.

The gradual rollout of the competition may have meant that winners experienced different effects depending on when they applied. Round 1 winners received significantly more money than did Round 2 and 3 winners, but they were also at the forefront of implementing some novel reforms. Later winners, by contrast, received less financial support from ED, but they were able to capitalize on some of the investments made by early adopters and to learn from their successes and mistakes. We therefore account for both treatment effect heterogeneity and horizontal diffusion effects in our analysis.

In Supplemental Appendix Table C.6, we reestimate the fully specified model in Table 3, with three binary indicators for winners in each round of the competition. In the last two columns of Supplemental Appendix Table C.6, we also interact these variables with the change in education revenue from 2007 to 2009. We find that Round 1 winners were most likely to adopt RttT policies, followed by Round 2 and 3 winners (the latter two are not distinguishable from each other at $p < .05$). This finding is unsurprising, given that Round 1 winners were selected in no small part on the likelihood that they would follow through with their proposals, and that they were given the most financial support to achieve their goals. But we also see that it was within this first set of winners that prior resources mattered the most, as evidenced by the large and positive interaction that is exclusively observed between education revenues and winning Round 1.

Finally, to account for the possibility that late RttT adopters were responding not only to federal incentives, but to the decisions of other states—and not only to their own economic concerns, but the economic concerns of those states—we estimated our fully specified model with the addition of controls that account for commonly recognized pathways of horizontal diffusion. First, to capture diffusion over geographical space, for every policy p in state i and year t , we coded the proportion of i 's neighboring states that had policy p in place in year t .⁹ Of course, policies might diffuse across similar or ideologically proximate states rather than physically proximate states. We therefore also constructed a variable for every policy p in state i and year t that is the weighted sum of adoptions of p in all other states in the same year, where the weight is proportional to the similarity of those states to state i along some meaningful political and economic dimensions.¹⁰ After controlling for these two indices—and for their one-year lags, to allow for a slower rate of diffusion—our main estimates of interest remain nearly unchanged, even as we observe some evidence of horizontal diffusion (see Supplemental Appendix Table C.7).

We have seen that all applicants to RttT, winners and losers alike, were more likely to adopt RttT policies than those states that never applied. Might financial resources also have affected causally prior outcomes, such as who chose to apply, or the types of policy promises they made in their applications? We do not see evidence that this was the case. In Supplemental Appendix Table C.4, we estimate a linear probability model, regressing the binary decision to apply to RttT on the change in education revenue variable and the same set of statistical controls. Across-state differences in resources, we find, had no effect on a state's probability of participating in the competition. As shown in Supplemental Appendix Table C.5, neither did resource shocks correlate with states committing to any given RttT policy in their applications to the competition.

Lessons and Applications

For federal policy actors, incentive-based initiatives like RttT have a number of attractive qualities: because they draw heavily from participating states' own resources, these initiatives are less costly than centralized reforms; and because of their voluntary nature and flexibility, they tend to be easier to enact. RttT's demonstrated success, moreover, may encourage federal actors to turn their attention to the states, particularly as deepening partisan divides impede legislative action within Congress. But if states' responsiveness to incentives is conditioned by existing resources—and any number of other, as-yet unstudied factors—then this policy tool may increase policy heterogeneity across the country. At the same time that they enable the states most willing and able to undertake reform, incentive programs may not reach those least equipped to undertake costly policy reform on their own. And if resistance derives from lack of resources, then such programs may entrench the very inequalities they were designed to address.

Given their distributional consequences, when does it make sense to deploy financial incentive programs? One key consideration is whether the aim is to spur innovation or to enact essential policies already known to be effective. An explicit goal of RttT was to encourage states to experiment with relatively untested reforms in order to discover new ways to improve student outcomes. Indeed, a major focus of the competition was on building the infrastructure needed to measure what works. And in light of the diffusion effects documented here and elsewhere (Howell and Magazinnik 2017), letting the wealthiest states shoulder the burden of experimentation seems particularly appealing. Less appropriate, however, are federal incentive programs that involve well-tested, but expensive, remedies that require large up-front investments, or where the neediest states are the ones with the least internal capacity—infrastructure development, for instance, or basic poverty alleviation.

Although this article focuses on financial incentives, it offers lessons to a larger class of federalism projects. Money, after all, is only one of many resources that the federal government can use to jump-start policy change among subnational governments. Another is authority. In the domain of immigration policy, Magazinnik (2019) analyzes an initiative that allowed local governments to voluntarily assume the power—and the associated expense—of enforcing federal immigration law. The

program increased total immigration enforcement efforts across the country, but it mainly did so among localities that already had the most restrictionist policy, as these preference outliers disproportionately selected into the program. In a similar vein, the federal government may grant regulatory leniency. Rather than the authority to carry out national mandates, the federal government can allow states to opt out of them. Recent work has documented presidents' strategic use of waivers and exemptions to promote their agendas in the domains of endangered species protection (Kerosky 2018) and welfare, Medicaid, and education (Mann 2016). Scholarship on these programs also suggests that preexisting differences—in this case, in ideology and partisanship—condition subnational governments' responsiveness to federal initiatives.

As partisan divisions continue to impede reforms that are exclusively funded by the federal government, future empirical and normative work should consider how policies that strategically deploy smaller amounts of federal resources to subnational governments in ostensibly neutral ways nonetheless yield divergent outcomes. The resulting inequalities are concerns in their own right. But if these inequalities deepen partisan divisions, as some have suggested (see, for example, McCarty, Poole, and Rosenthal 2016), such initiatives may alter political realities in ways that become self-justifying.

Conclusion

Obama's RttT was one of the most consequential—and creative—federal interventions into state education policy making ever undertaken. By deploying a modest sum of money alongside a robust public relations campaign, the Obama Administration managed to radically alter the education landscape. Policies that stood little chance of enactment within Congress and that had stalled in many state legislatures rather suddenly sprung to life, with wide-ranging consequences for the governance, oversight, and operations of public schools nationwide.

The consequences of RttT, however, were not uniformly felt across the country. Some states implemented nearly all of the policies supported by ED, whereas others did so only selectively. A key source of the observed variation, it turns out, is a basic factor that shapes nearly all state-federal relations, but that the existing literature on vertical diffusion largely overlooks.

The availability of prior resources, we find, crucially affects the willingness of states to undertake policy actions that are only partially subsidized by the federal government. In the aftermath of RttT, states whose education resources were especially depleted by the Great Recession were less likely to enact reforms supported by ED, and especially those that presented substantial up-front costs. These effects appear reasonably robust to a variety of alternative measurement and modeling strategies. That we see no relationship between education resources and policymaking activities outside of RttT policies in the aftermath of the competitions, or within RttT policies beforehand, suggests that the patterns of findings relate to vertical diffusion and not merely independent state policymaking dynamics.

In 2015, the oversight and funding operations of RttT drew to a close. It is unclear, exactly, how this affected the production of education policies around the nation.

Coming on the heels of the Great Recession, the introduction of RttT provided a unique opportunity to leverage plausibly exogenous variation in state resources to investigate the distributional consequences of a federal incentive program. By the time RttT rolled to a close, however, states had adapted to the budgetary disruptions caused by the Great Recession. Moreover, participating states had ample time to adjust the scale of their programmatic initiatives in full anticipation of RttT's termination. These endogenous changes, unfortunately, all but preclude the possibility of recovering causal estimates of the distributional effects of RttT's removal from (as opposed to its entrance to) the policy landscape.

It is also not at all clear that the causal effects that we can recover generalize to the larger population of federal grants-in-aid or other forms of financial assistance. RttT, after all, covered only a modest portion of the total costs of these education policies. Had it covered more, then the observed empirical findings in this article might attenuate or even switch signs. Were the federal government to pay the entire costs of new education reforms, especially destitute states might rally behind initiatives that stood any chance of benefiting their residents. To gauge the conditional effects of state resources on state policies given variable amounts of federal aid, clearly, scholars will need to collect data on multiple policy interventions.

What RttT makes abundantly clear, though, is that the size of financial rewards is not the only relevant factor in shaping the pace or scope of vertical diffusion. The resources that states already have on hand crucially affect their willingness to undertake subsidized policy actions. And to the extent that these prior conditions vary across states—and they decidedly do—we can expect average treatment effects associated with fiscal federalism to mask considerable state-level variation.

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Supplemental Material

Supplemental material for this article is available online.

Notes

1. Two notable exceptions include the studies by Nicholson-Crotty (2004), which argues that the effectiveness of federal grants in stimulating further subnational spending is

conditioned by goal congruence between recipient jurisdictions and the national government, and Nicholson-Crotty and Staley (2012), which argues that competitive federalism and credit-claiming considerations offer a better explanation for states' participation in Race to the Top (RttT) than financial need or partisanship. Several other studies control for the influence of internal factors when estimating the effect of intergovernmental grants on state policy adoptions (e.g. Daley and Garand 2005), but nonetheless report only average treatment effects for all states.

2. See Supplemental Appendix A.1 for more details about the competition.
3. Prize packages were based primarily on the share of the state's population of children aged 5 to 17. Further details are available at: <http://www.ed.gov/news/press-releases/delaware-and-tennessee-win-first-race-top-grants>.
4. For empirical investigations into who applied and who won the RttT competitions, see Manna and Ryan (2011) and McGuinn (2010). For a General Accounting Office report on the subject, see <http://www.gao.gov/new.items/d11658.pdf>.
5. See Supplemental Appendix B for a detailed explanation of the criteria our research team used for coding each policy, as well as a list of our sources.
6. To be sure, some policies that were "cheap" in the short term could have had significant fiscal consequences for states in the long term: for instance, forcing teacher promotion and firing decisions to be informed by their evaluations could have effects on the labor market down the line. However, since RttT funding was provided on a relatively short time horizon—and since these downstream effects are more difficult to predict and quantify—we focus here on states' decision making over the near term.
7. Specifically, NCES defines "revenues" as, "Additions to assets that do not incur an obligation that must be met at some future date, do not represent exchanges of fixed assets, and are available for expenditure by the local education agencies in the state."
8. We use revenues rather than debt, an alternative indicator of financial vulnerability, because it is a purer measure of the resources a state has at its disposal, unconfounded by potential changes in its spending priorities.
9. For example, for the policy of intervening in persistently low-achieving schools in the state of California in 2012, we check whether Arizona, Nevada, and Oregon had the policy in place; since Arizona and Nevada did in 2012 but Oregon did not, our independent variable takes a value of 0.67.
10. Specifically, we computed the Euclidean distance in the same political and economic covariates that we used as controls throughout the analysis between state i and every other state, having standardized them first. We then constructed a weight that is the inverse of this Euclidean distance divided by the sum of all distances (so that the weight sums to one). Finally, we computed the weighted sum of all adoptions of policy p in states other than i and year t . See Case, Rosen, and Hines (1992) for more on this approach.

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