

# The Rise and Fall of Radical Civil Service Reform in the U.S. States\*

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## **Abstract**

Initiated by a 1996 Georgia statute, “radical” civil service reform quickly swept throughout the United States. I focus on explaining the wax and eventual wane of state efforts to increase the number of their at-will employees at the expense of the population of fully protected merit system employees. Using an event history approach to explain this policy diffusion with state-level variables, I find that electoral competition and gubernatorial powers are the most significant determinants of this kind of policy diffusion. Where previous literature has concluded that these reforms ceased spreading because the new programs were failing to create the promised governmental efficiency, I argue that the institutional conditions for these HRM policies have been less propitious in recent years. This paper signifies an important contribution in that it brings civil service reform back into the scope of policy diffusion literature and identifies political insights into a perpetually important question.

The past two decades have been an interesting and at times tumultuous period for public personnel systems throughout the United States. 1996 marked the beginning of a period of “radical” decentralization of state personnel systems across the Sun Belt, which then spread to other parts of the United States. This year brought the passage of a Georgia statute, supported by then Democratic Governor Zell Miller, which mandated that all new civil servants be hired on an at-will basis, therefore effectively phasing out merit protection for civil servants in that state. The passage of this state law started a process of diffusion that notably spread—to varying degrees—to Florida (Bowman and West, 2006), South Carolina (Nigro and Kellough, 2008), Arkansas (Kellough and Nigro, 2006), North Dakota (Shafritz and Hyde, 2007), and many other states and subunits within states (Ruhil, 2003; Cogburn et al., 2010). While many studies of these individual reforms focus on their effects for state governance, there has been little attempt to uncover what drives radical changes in personnel policies.

Individual case studies help us to understand important operational differences among these recent state-specific reforms, but more general reviews (e.g. Condrey and Maranto, 2001; Condrey and Battaglio, Jr., 2007; Hays and Sowa, 2007), have identified a common dimension on which to characterize so-called radical civil service reform. Traditional merit protection requires a centralized HR system, where decentralization of HR decisions to line agencies usually arrives concomitantly with deregulation and increases the prevalence of employees hired at-will. The dominant, and untested, view in the literature on human resource management (HRM) is that the transition away from centralized merit systems toward decentralized at-will systems is part of a broader movement from traditional management practices toward the privatization espoused by proponents of the new public management (NPM). This view is consistent with the idea that individual political reforms are the product of broader based movements—as I review below, this is a history with a strong tradition in the study of public personnel systems. However, this perspective does not allow adequate contextual understanding of why HR decentralization and the abolition of state merit sys-

tems occurred in some states and not others. Nor does it explain the timing of such reforms. To paraphrase Ruhil and Camões (2003), who look at variation in the state-level *adoption* of merit systems, I am primarily interested in why some states essentially repealed merit systems in favor of decentralized at-will ones at the end of the same century that saw every state besides Texas adopt comprehensive civil service reform. While arguments regarding the potential efficacy of radical reform as a solution to state problems are part of state decisions to adopt them, they are not determinative. Instead, I focus on state institutional and political characteristics as determinants of radical civil service reform across the states. To assess the relative effects of these characteristics on the *adoption* of these reforms, as well as the *timing*, I use the event history modeling approach common to studies of diffusion processes (Berry and Berry, 1990; Karch, 2007; Boehmke and Skinner, 2012; Shipan and Volden, 2012), but thus far underutilized to study this particular type of political reform.

This paper represents the first attempt at a general empirical understanding of this particular phase in the history of civil service reform in the United States. Previous research (Dresang, 1982; Tolbert and Zucker, 1983; Ruhil and Camões, 2003; Ting et al., Forthcoming, e.g.,) has shown that state-level characteristics have had a discernible impact on the adoption of merit systems across the states, but they are largely silent concerning the conditions under which these reforms might eventually be rolled back or abolished altogether. In the next section, I introduce this process of radical reform that has gone largely unconsidered in cross-state empirical studies. I review the literature on so-called “radical civil service reform” and conclude that while it comprehensively catalogues and prognosticates the effects of such reforms on state governance, it offers very little in the way of general explanations for the piecemeal adoption and diffusion of HR decentralization and deregulation across the states. I then more deeply mine the theoretical accounts on the political and institutional determinants of reform for hypotheses regarding the empirical phenomena on which the individual state studies focus—that some states adopted radical reform (by decentralizing their HR systems) before others, while others have yet to eschew centralized merit systems. I

argue that state personnel policies are important arenas for the insulation of political decision making from external forces. As insulation is interesting from a theoretical perspective (Ting et al., Forthcoming), so are the conditions under which insulation might be undone. The third section organizes these hypotheses more formally and describes the methodology for assessing them in the current paper. This includes a clear empirical operationalization of the sometimes muddled concept of radical civil service reform. The penultimate section describes results and the final section concludes with some thoughts about what this research contributes to the literature and some ideas about where scholarship might go next.

## **“Radical” Civil Service Reform**

Scholars of political science and public administration have long been interested in the composition of and selection procedures for governmental personnel (Wilson, 1887; Mosher, 1968; Condrey and Maranto, 2001; Kellough and Nigro, 2006). I review some of this literature further in the subsequent section, but it is sufficient for now to say that the contours of this selection and composition have varied over time and have been the focus of numerous and competing reform movements. The initial historical movement was a rhetorical one against the patrician backgrounds of officeholders toward a Jacksonian democracy, replete with “spoils” and rotation of public jobs among the plebeian masses. From the administrations of Jackson through Garfield, civil service jobs were distributed to loyal supporters as partisan pork. The next movement was away from spoils and towards a “merit” based system, modeled off of the Weberian Prussian and British systems. This was thought to guarantee the “neutral competence” (Kaufman, 1956) that was the foundation for Wilson (1887)’s politics-administration dichotomy. The merit system of personnel, therefore, is intimately associated with the traditional model of public administration.

At least at the federal level, the merit system of HRM established under the Pendleton Act of 1883 held sway, largely unchanged until 1978. Initiated by Jimmy Carter, the Civil Service

Reform Act of 1978 sought to address some of the criticisms the system was beginning to receive on flexibility grounds (Finkle, 1977; Bach, 1977). The critics held that mandatory merit systems unduly tied the hands of HR managers and, especially at the municipal level, contributed to waste and procedural delay. In addition, the late 1970s saw an increased distrust in government, with some attributing poor governmental performance to the merit selection of civil servants (Argyle, 1982). In this context of instability and public malaise, Congress passed the CSRA with Carter's coordination (Pffner and Brook, 2000). The CSRA sought to maintain the principle of merit, while at the same time improving the management of the federal civil service through new institutions and new management techniques such as performance review and merit pay. The content of the CSRA was undoubtedly influenced by developments in the theory and practice of public management and seemed to be a response to the public demands for governmental efficiency and accountability that partially determined these developments.

Throughout the 1980s and early-mid 1990s, these developments in public management morphed into what we now call the New Public Management (NPM) philosophy of governance. As this intellectual movement gained in momentum, scholars increasingly interpreted it as driving actual governmental reform, famously including the National Performance Review of President Clinton and Vice President Gore (West, 1986; Hood, 1991; Pffner, 1997; Shafritz and Hyde, 2007). Again, the dominant perspective seemed to propose a causal link between theory and practice with the idea of solving governmental problems. The aforementioned 1996 Georgia statute represents the logical conclusion for the reforms that began at the federal level in 1978—a complete break with the merit system of the past. Nigro and Kellough (2000) describe the Merit Systems Reform Act as having “gone to the edge” by effectively reforming the merit out of the civil service, going much further down the NPM path than had the CSRA or any previous state reform. The act completely decentralizes and deregulates the state civil service by shifting employees from merit protection to unclassified status. Scholarship on the content of this policy identifies the complete decentralization of

HRM as the “important regard” (Nigro and Kellough, 2000, p.42) which makes the reform so dramatic.

The initial spate of literature inspired by Georgia’s reform was normative and either lauded it as indicative of the future of flexible and efficient “managerialism” (Pollitt, 1993) or vilified it as something like a “return to spoils” (Nigro and Kellough, 2000; Gossett, 2002; Condrey, 2002; Condrey and Battaglio, Jr., 2007). Likewise, while it is difficult to pinpoint who coined this phrase for what Georgia did, Condrey and Maranto (2001), Bowman and West (2007), and others, including explicit proponents (Maranto, 2001), have taken to calling it “radical civil service reform.” Although state reforms subsequent to Georgia’s were not *as* radical, a number of states immediately followed Georgia’s lead down the path of HR decentralization.<sup>1</sup>

Besides these descriptive accounts of individual state reforms and the concomitant normative debate regarding their desirability, the bulk of the literature on radical service civil service reform has turned toward assessing the *effects*, both ethical and empirical, of these changes (Bowman and West, 2006; Green et al., 2006; Williams and Bowman, 2007; Nigro and Kellough, 2008; Crowell and Guy, 2010; Goodman and French, 2011). These studies examine, primarily through survey instruments, the effects that shifts toward decentralized HR systems have had on dependent variables such as governmental performance, service delivery, personnel morale, restrictions on employee due process rights, and even citizen trust in government. While this literature has been helpful in coming to terms with the potential effects of radical civil service reform in the states where it has taken hold, it does very little to uncover root causes. To the extent that the extant research traces the development of reforms in individual states such as Georgia and Florida, it focuses implicitly on budgetary pressures and citizen dissatisfaction with government—that is, radical civil service reform can be explained by the fact that key decisionmakers deemed it necessary for “good government.” This perspective is captured nicely in a statement made by Zell Miller himself in his 1996 State of the State address:

Folks, the truth of the matter is that a solution in 1943 (when the system was established) is a problem in 1996. The problem is governmental paralysis, because, despite its name, our present merit system is not about merit. It offers no reward to good workers. It only provides cover for bad workers (State of Georgia, 1996, pp. 6-7).

There is no doubt that budget constraints, ideology, and public dissatisfaction with governmental performance have had significant effects on the initiation of radical civil service reforms. However, previous literature has not shown such sentiments to be determinative of reform and has not systematically explained reform efforts or timing across states. In fact, that these reforms do not seem to be inducing the promised efficiency and governance effects (Coggburn et al., 2010) implies that policy success has not been the overwhelming determinant of diffusion. As stated above, my aim is to occupy this lacuna and examine the extent to which state political and institutional characteristics might generally catalyze decentralization of state personnel systems. Before I introduce my expectations regarding the roles of state characteristics, I briefly review theoretical literature on the antecedent *adoption* of centralized and merit-based systems in the first place.

## Political Explanations of Civil Service Adoption

In the introduction to their compendium of classic works in public administration, Shafritz and Hyde give a summary of the types of approaches normally taken to explain the adoption of merit systems that is worth quoting at length:

...the advent of modern merit systems is either an economic, political, or moral development. Economic historians would maintain that the demands of industrial expansion—a dependable postal service, a viable transportation network, and so on—necessitated a government service based on merit. Political



analysts could argue rather persuasively that it was the demands of an expanded suffrage and democratic rhetoric that sought to replace favoritism with merit. . . . As moral impulses tend to hide economic and political motives, the weight of moral concern undiluted by other considerations is impossible to measure. Nevertheless, the cosmetic effect of moral overtones was of significant aid to the civil service reform movement, because it accentuated the social legitimacy of the reform proposals (Shafritz and Hyde, 2007, p. 4).

In many ways, the description of the difficulty with attributing the adoption of merit systems to “moral overtones” is analogous to attributing political reforms to intellectual developments pursuing “good government.” The textbook account of civil service adoption in the United States focuses on the rise of the merit system as the antidote to politicized administrative decisionmaking. The approach is often descriptive and/or normative (see, e.g., Van Riper (1958); Hoogenboom (1961) for especially detailed accounts) and focused on the Pendleton Act of 1883 and its effect on the federal civil service.<sup>2</sup> The Pendleton Act enshrined the Progressive ideal of the politically neutral and competent civil servant in federal legislation and signaled the ideological defeat of “spoils,” or the explicitly politicized and therefore ephemeral pre-Pendleton workforce. These historical studies, as well as more recent treatments (Skowronek, 1982; Knott and Miller, 1987; Johnson and Libecap, 1994), are not politically naïve. They trace the development, maintenance, and eventual success of Progressive coalitions against the partisan advocates of spoils in the late 19th century. While illuminative of an important era in American political history, these studies fail to account for variation in the eventual adoption of merit systems across the states. A significant weakness of this literature in approaching the topic of this paper is that it does not propose conditions under which reform might occur, nor is it blind to the direction of reform and is too specifically tailored to explaining the particular development of merit systems.

In a compelling empirical paper, Ruhil and Camões (2003) question the accepted wisdom that state legislatures, also motivated by Progressive calls for good government and a diffuse sentiment against machine politics, quickly passed comprehensive civil service reforms using the Pendleton Act as a model. This work destroys the notion that states used Pendleton as an impetus and a model for their own merit systems and passed them as a matter of course. They make a strong case for interpreting state adoption of merit systems as determined by characteristics internal to states, rather than exogenously determined by the federal government. Instead, states established merit systems from 1883<sup>3</sup> to 1940, when they were mandated to do so by the 1939 amendment to the Social Security Act<sup>4</sup> (Ruhil and Camões, 2003). Upon recognizing this variation in adoption timing, Ruhil and Camões (2003) build on an existing empirical account (Dresang, 1982) and construct multivariate empirical models of civil service diffusion. At the end of this section, I will briefly introduce their hypotheses and findings in the interest of motivating the variables I hypothesize might affect the obverse of this initial diffusion.

In a more theoretical paper, Ting et al. (Forthcoming) see civil service adoption as an attempt by partisan majorities to insulate policy gains against future “political drift” (Moe, 1989, 1990). When dominant parties have a sufficiently long time horizon, they become willing to insulate the civil service from the politicization that just may well help them win elections in the short term (Folke, Hirano and Snyder Jr., 2011). This account is more cynical than those that see the original adoption of civil service systems across the states as part of a normatively good convergence to “good government.” However, it allows for specific explanations of reform *timing* that are conspicuously missing from less contextual theoretical accounts. In addition, the analytical model presented in Ting et al. (Forthcoming) generates expectations for when civil service might displace spoils *and* for when spoils might displace existing systems of insulated civil service—the empirical focus of this paper. To help explain civil service adoption, they theorize that “an incumbent might kill its own spoils system (reducing the next generation’s chances of reelection) when its electoral prospects are dim.

This prevents future office holders from the same party from being disadvantaged by the rival party's spoils system" (Ting et al., Forthcoming, p. 4). On the other hand, "an incumbent will move toward a spoils system when its electoral fortunes seem positive, as this will help lock in future victories" (Ting et al., Forthcoming, p. 4). This is a cleaner exposition of Dresang (1982)'s hypothesis that merit systems should be driven by increased competition for state governors' mansions. These authors provide some bivariate evidence that civil service reforms were adopted in states by previously dominant parties after electoral competition began increasing, but do not provide any multivariate analyses of reform adoption. Similarly, Ruhil and Camões (2003) hypothesize that *electoral competitiveness* should have increased the incentives for state politicians to initially adopt civil service reform.

In addition to this primary political variable, Ruhil and Camões (2003) propose that a number of state environmental variables should mediate the determinative effect of political competition on merit adoption. Among these, they find that the proportion of foreign-born residents in a state constituted varying supplies of patronage constituencies and thus mitigated the incentive for politicians in competitive states to abolish spoils. For related reasons, they control for the degree of urbanization in states, as cities were more fertile ground for patronage than the dispersed populations of the plains and more rural states. Given the time period they were studying (1900-1939), the contours of state electoral procedures also mattered in the adoption of merit adoption. In particular, they show that the Australian (secret) ballot reduced the incidence of implicit vote contracts between parties and poor constituencies and therefore reduced a mechanism of patronage, spurring the adoption of merit systems across the states. Finally, they recognize that there might be reason to believe that the economic climate of a state might affect merit adoption either positively or negatively, but find no empirical relationship between the proportion of business failures in a state and the likelihood of that state adopting merit reform.

While these theoretical and empirical explanations of state civil service adoption are illuminating for my endeavor, it is not so straightforward to translate their expectations to

the problem of radical decentralization—that is, the effective repeal of state merit systems. In addition, the work specifically on the spread of radical civil service reform assumes these policies diffuse on the basis of ideologically derived ideas of “good government.” There are, however, clues in this literature regarding some types of state political and demographic variation that could lead to the increased or decreased likelihood of radical decentralization. For example, Hays and Sowa (2007) present a comprehensive catalogue of those states which had decentralized their HR systems and “uncovered” merit workers to at-will status by 2005. To the extent that they try to explain the trends they discover, they focus on idiosyncratic factors, rather than systematic variation. As an example, they cite as a determinant of HRM reforms that “the newer generation of politicians is more likely than not to have a chip on its collective shoulder concerning the bureaucracy” (Hays and Sowa, 2007, pp.16-17). In addition, Hays and Sowa (2007) surmise that the imposition of legislative term limits in some states may serve to aggrandize the “governor as manager” and embolden executives to push for radical civil service reform. It is no exaggeration to say that this is the extent to which expectations regarding the potential political and demographic determinants of radical civil service reform have been specified in the literature. In the following section, I modify the expectations of Ting et al. (Forthcoming) and Ruhil and Camões (2003) to generate testable hypotheses regarding the effects of state characteristics such as electoral competitiveness, the strength of public sector unions, interbranch conflict, state ideology, state interest group populations, and others, on the radical decentralization of civil services in states.

## **Data and Methods**

The empirical dependent variable of interest is, as are most broad policy areas, multifaceted. Even in the literature reviewed above, different authors tend to mean different specific things when they talk about “radical” civil service reform. In general, the concept is muddled in the literature and hardly ever clearly operationalized. Although it is no doubt

an important endeavor, the scope of this paper is empirical and shall not delve too deeply into conceptual criticisms of the extant literature. Instead, I hope to clearly define an empirical referent for radical civil service reform and test hypotheses regarding its presence or absence in the United States from 1996-2005. I am fully aware that this is not the *only* way to operationalize what is meant by radical civil service reform, but, by being clear about what I am counting as radical reform, I can set a baseline for future empirical research. To preview, the empirical focus of this paper is on significant HR decentralization across the states. I ultimately find that electorally dominant majorities appear to decentralize state personnel systems when they can. This research does not show that such situations lead to other flavors of civil service reform, such as privatization and contracting out services. Future research should assess the extent to which political-institutional variation drives state decisions to adopt these other elements of reform.

Many authors, including Condrey and Maranto (2001), unequivocally consider Georgia's 1996 statute to be "radical." This statute will eventually have the effect of phasing out all state employees from the traditional merit system with them instead being classified "at-will." Not every state's radical civil service reform is as radical as that. This raises a problem of categorization and leads to the question of how radical is radical enough. Hays and Sowa (2007) recognize this problem from a normative standpoint, but their exposition makes clear the measurement issue as well:

... there is no accepted standard for how many at-will employees is enough, or what percentage would optimize the competing demands for responsiveness *and* expertise (accountability *and* professionalism). Although not necessarily mutually exclusive, these values represent the contemporary jargon that once dominated the *merit versus patronage* debate (Hays and Sowa, 2007, p. 16—emphasis in original).

These authors come to this conclusion after having surveyed HR personnel in all 50 states to

get a sense about how much “radical” civil service reform they had seen in each state. They used structured and semi-structured interviews with key HR decisionmakers to ascertain the extent to which the HR system had recently decentralized in a state, whether the state government was increasing the proportion of new employees hired at-will, what the judicial standards for the range of grievable actions was, and whether the surveyed employees felt a subjective decline in job security. Table 1 represents the findings from this cross-state snapshot of state HR systems in 2005. Clearly, there are multiple components to the changes in HR systems captured in the Hays and Sowa research. As an empirical first cut and a clear decision rule, I have interpreted the first column of this table—capturing the extent to which the HR system had recently significantly decentralized— as indicating the presence or absence of radical civil service reform by 2005. The choice of decentralization as indicative of a radical movement away from merit is not without justification. Previous research (Condrey and Maranto, 2001; Condrey and Battaglio, Jr., 2007; Hays and Sowa, 2007) has identified decentralization as an important underlying dimension of reform away from merit protection for civil servants. Condrey and Battaglio, Jr. (2007, p.427) even imply that decentralized environments (as measured by the Hays and Sowa survey) go hand in hand with at-will environments, with one likely setting the conditions for the other.

The information from Hays and Sowa (2007) is sufficient for assessing whether a state had adopted radical civil service reform (as indicated by a significant decentralization of their HR system) by 2005, but it does not tell us about the timing of these reforms. Therefore, I mined individual primary and secondary sources for these states to determine the legislative or administrative action that spurred the decentralization of each state’s HR system.<sup>5</sup> I can then use this information to model the adoption and timing of radical civil service reform as a function of the covariates suggested by the hypotheses proposed below. As is common for empirical models of policy adoption across states (see Berry and Berry (2007), Karch (2007), Boehmke and Skinner (2012), and Shipan and Volden (2012) for some recent reviews), I use an event history analysis (EHA) approach to modeling the determinants of policy adoption.

Similar approaches are taken in Tolbert and Zucker (1983) and Ruhil and Camões (2003), so they are not foreign to studies of civil service adoption specifically. The dependent variable is the duration of time that states maintain a traditional merit system after the first “event,” the 1996 Georgia statute. The covariates, or independent variables, specified below can then be assessed based on their statistical impact on this duration before adoption (Beck, Katz and Tucker, 1998; Box-Steffensmeier and Jones, 2004).

The clearest expectation from the existing literature is that electoral competitiveness should have an effect on radical civil service reform. An increase in electoral competitiveness is commonly found to drive the adoption of merit systems (Dresang, 1982; Ruhil and Camões, 2003; Ting et al., Forthcoming). This expectation is driven by the idea that current incumbent parties may use merit systems to insulate policy gains against future dominant parties and their merit employees. Merit systems are thus a form of policy implementation insurance familiar to the literature on the “politics of structural choice” (Moe, 1989, 1990). However, the obverse of this hypothesis should inform the empirical analysis of the repeal of merit. Therefore, I expect that states with less competitive political systems should be more likely to radically decentralize their HR systems than more competitive states.

**Hypothesis 1** *Electoral competitiveness should decrease the probability that a state adopts radical civil service reform. Conversely, electoral dominance, as measured below by the size of legislative majority parties, should significantly increase the incidence of HR decentralization.*

(+)

Here, I operationalize long-term electoral competitiveness of the legislative branches of each state. For this legislative competitiveness, I draw on data collected and provided by Carl Klarner on the size of majority parties across state legislatures (Klarner, 2003). The variable for the size of the majority party is calculated by taking the absolute value of the difference between the numbers of Democrats and Republicans in each chamber for each year and dividing by the total number of legislators in that chamber. I calculate this relative size of the

majority party for each chamber<sup>6</sup> and then take the mean across chambers for each state-year. I assume that the larger this average size of the majority party is, the less electorally competitive the legislative branch is in that state-year. Therefore, a small majority party in a state denotes a more competitive electoral environment in a state-year. The theoretical literature reviewed above (Ting et al., Forthcoming) emphasizes that it is the *perception* of dominance or competitiveness that drives party strategies to insulate policy implementation via merit reform or not. Similarly for the current paper, perceptions of long-term electoral competition—driven by past events—should be more determinative than short-term fluctuations in electoral environments. For this reason, I calculate the moving average over the previous 10 years of data<sup>7</sup> (current value included) on legislative majority size and use this variable to capture party perceptions of future electoral competition.

There is a critical difference between studying the relationship between party competition and the adoption of merit and that between party dominance and the scaling back of merit. Quite simply, public sector unions have political and economic interests in maintaining merit protection—once it exists—for large numbers of state employees. A recent review (Kearney, 2009) catalogues instances in which public sector unions have overtly affected state personnel policies. Therefore, the extent to which the strength and influence of public sector unions vary across the states may condition the ability of dominant electoral parties to decentralize personnel policy in “radical” ways. This suggests an interaction term between the moving average of majority size and the strength of public sector unions in a state. I operationalize the latter variable as the proportion of all state employees belonging to a public sector union (Hirsch and Macpherson, 2011). This discussion implies the following conditional hypothesis:

**Hypothesis 2** *The effect of electoral dominance (from **Hypothesis 1**) on the probability of reform should become less positive and/or insignificant as public sector union membership increases in a state. (- on interaction term)*

Previous research has emphasized that changes to civil service systems are not institu-



tionally neutral (Dresang, 1982). Governors and legislatures struggle over the control of state bureaucracy, and control of the composition of the civil service should be an important weapon for either side in this struggle. As a very general proposition, I suggest that governors prefer decentralized/at-will systems to merit ones, especially when they possess strong institutional powers, such as those of appointment and removal. On the other hand, legislatures might generally prefer to insulate the civil service from the governors' powers of appointment and removal and would therefore be more likely to prefer civil service systems than governors would be. One might think that institutionally powerful governors might be more able to impose their structural will on legislators and we should therefore expect them to have more success in adopting radical civil service reform. While this is plausible, I take a more strategic view and recognize that legislators are important actors here as well. I presume that state legislatures may be more willing to cede to the governor her preferred HR system when they see her utilization of such a tool as less of a threat to the legislature's implementation prerogatives. Therefore, I expect that

**Hypothesis 3** *Institutionally weak governors have much to gain from radical civil service reform and legislatures are more willing to cede them this power; therefore, the extent to which a state's governor is institutionally weak increases the probability of adoption. (-)*

Governors' institutional powers are measured as an index and collected from Thad Beyle's gubernatorial powers database,<sup>8</sup> which includes information concerning gubernatorial selection methods, tenure, term limits, appointment powers, and institutional veto powers. This index ranges from a low of 2.5 to a high of 4.3.

By definition, radical civil service reform constitutes a radical shift to the status quo when it is adopted. Theories of lawmaking in separation of powers systems (e.g., Fiorina, 1996) hold that policy change of any kind, never mind of the radical variety, is far less likely under divided government than unified. Such theories are not without empirical (Mayhew, 1991) or theoretical (Krehbiel, 1998) challenge, but they are worth testing in this context. To test

the following hypothesis, I simply include a variable indicating whether divided government is present in a state in a given year or not.

**Hypothesis 4** *Divided government decreases the probability that states adopt radical reform of any kind, including radical civil service reform. (-)*

Similar to the previous hypothesis, legislatures where both chambers are controlled by the same party, regardless of which party controls the governor's mansion, would be better able to radically change the status quo with legislation than legislatures with more difficult inter-chamber bargaining environments. Therefore, we can expect that

**Hypothesis 5** *Having a unified legislature increases the probability that a state adopts radical reform of any kind, including radical civil service reform. (+)*

Implicit in their explanations of radical civil service reform in “good government” terms is the previous literature's assumption that reform is ideologically driven. Descriptions of the Georgia reform (Condrey, 2002) and the Florida reform (Bowman and West, 2006) explicitly mention that the imposition of decentralized systems might have been more in line with conservative citizen expectations. In contemporary political discourse, such reforms are seen as conservative insofar as they are driven by efficiency concerns over distributive ones and seek to approximate the business world in governmental settings. As an indicator of citizen ideology, I use the Berry et al. (1998) measure, which ranges from 0-100, where higher numbers represent increased liberalism.

**Hypothesis 6** *Ideologically liberal states should be less likely to adopt radical civil service reform than ideologically conservative ones. (-)*

Related to citizen ideology is the extent to which elected politicians might be ideologically predisposed for or against radical reform. To control for this, via the proxy of political parties to ideological views, I include a measure of the percentage of Democrats across state

chambers for each year in the data (Klarner, 2003).

**Hypothesis 7** *As the percentage of Democrats increases within or across state legislatures, the adoption of radical civil service reform becomes less likely. (-)*

In addition to citizen ideology and partisan composition of state legislatures, I test whether citizen preferences are transmitted to politicians via registered interest groups. Since decentralized/at-will systems more approximate the business world than the traditional conception of government, I expect economic interest groups to positively affect the probability of radical civil service adoption. I therefore include the number of economic interest groups registered to lobby in each state in 1990 (for 1996) and 1997 (for 1997-2005). I expect the number of such interest groups to positively affect the probability of adoption. For these numbers, I used information provided in Gray and Lowery (1996) and Boehmke (2008) and transform the variable by taking its natural log.

**Hypothesis 8** *The greater number of economic interest groups in a state, the more likely that state will be to adopt reform. (+)*

Ruhil and Camões (2003) find economic variables to partially determine civil service adoption between 1900 and 1939. In addition, anecdotal accounts of the incentive to move towards at-will systems emphasize the potential for such a reform to ameliorate budget crises. I have operationalized a state's economic health with a measure of each state's yearly budget surplus (positive values) or deficit (negative values) and a percentage of their total gross state product (Klarner, 2003).

**Hypothesis 9** *Economically healthy states should be less likely than poorer states to adopt radical civil service reform (-)*

In addition to testing whether these state characteristics have significant effects on the adoption of radical civil service reform, I am also interested in whether mechanisms of spatial

diffusion condition a state's reform choice. Although there are many ways to test for diffusion effects (Shipan and Volden, 2008), I operationalize spatial diffusion by simply counting the number of a state's contiguous neighbors that have radically decentralized their HR systems by each year in the data. In the conclusion I discuss ways to make this test more nuanced by adding potential important information to the structure of this covariate.

Of primary concern for this paper are the political-institutional hypotheses 1-3, with the others essentially stating the expected effects of control variables that one might reasonably expect to affect the adoption of radical civil service reform. It is important to note that some of these covariates do not vary much within states over time, while some vary considerable within states and across years. Because of this, I have chosen to estimate standard logit (Beck, Katz and Tucker, 1998) models, which are much more amenable to the inclusion of time varying covariates than parametric, or even semi-parametric, event history models (Box-Steffensmeier and Jones, 2004).

## Results

What follows is an exploratory empirical analysis, guided by the theoretical literature cited above. Here, I am able to show support for many of the stipulated hypotheses, but I see the true contribution to the literature being the approach of looking for systematic determinants of administrative reforms. To preview the contours of the results: a governor's institutional powers score, unified legislature, the partisan composition of the legislature, economic interest group activity, and number of contiguous adopters all have statistically significant effects consistent with those predicted *ex ante*. Also interestingly, the analyses indicate, against expectations, that while controlling for other determinants of reform, states with more liberal citizenries are more likely to decentralize their personnel systems in radical ways than those with more conservative citizens. Most significantly, given its place in the literature on merit system *adoption*, I find that the prospect for electoral dominance induces

majorities to pursue radical reform, *but only* when they are unconstrained by the presence of powerful public sector unions.

Table 3 presents coefficients for two related models of radical civil service adoption across the states. The data that are modeled in each are time-series cross-sectional in nature and include time varying covariates. The dependent variable ( $Y_{it}$ ) is an indicator for whether state  $i$  adopts radical civil service reform in time  $t$ . Once a state adopts reform, they are no longer “at risk” for adopting it in the future and drop out of the analysis. In conjunction with the exclusion of Texas and Nebraska, this contributes to there being 449 observations across 48 states and 10 years. Beck, Katz and Tucker (1998) describe this data structure as binary dependent variable time-series cross-sectional (BTSCS) and suggest that this is best thought of as being analogous to discrete time duration data (Box-Steffensmeier and Jones, 2004). The simplest and most familiar way to model the effects of covariates on this type of dependent variable is with a binary response model such as logit or probit. Inclusion of the contiguity variable controls for the spatial dependence of the adoption of radical civil service reform across the states, but does not account for potential effects of duration dependence—that states may be more or less likely to adopt reform as a function of time. To control for this in the context of a logit model, I include a quadratic transformation of a duration until adoption variable. The rightmost columns of Table 3 present results from this model. Here, we see that most of the effects (both null and significant) maintain from the first model. This suggests that duration dependence is not a significant contributor to adoption when controlling for the included covariates. Therefore, I rely on the output from the model without the duration and duration squared terms for the model interpretations to come.<sup>9</sup>

The political-institutional determinants of radical civil service reform perform largely as expected in either logit specification. The coefficient for the constitutive term of *majority size* gives the effect of this variable on the propensity of a state government to adopt radical civil service reform *when public sector unions are nonexistent*. As expected we see that this effect is positive and statistically significant, but substantive interpretation is not so

straightforward. First, there are no state-years in the actual data where public sector union membership is zero—In fact, the variable ranges from 6.6% of state employees (Mississippi in 2001) to 73.1% (Connecticut in 1996). The negative coefficient of the interaction of *majority size* and *public sector union membership* tells us that the positive effect of *majority size* decreases as union membership increases (as expected from Hypothesis 2). Figure 1 takes this significant interaction into account and shows how the marginal effect of majority size on radical reform changes across the actual range of union strength. Here, we see that there are thresholds at which union strength precludes more dominant majorities (as measured by *majority size*) from having positive effects on reform. Figure 1 also shows (by plotting a kernel density estimate of the distribution of the *public sector union membership* variable) that there are many cases in the data of union strength being low enough for majority size to have the effect expected by Hypothesis 1.

The logit coefficients capture the change in the log odds of adoption and are not intuitive to interpret as substantive effects. However, we can substantively interpret the effects of changes in covariates on the changes in the probability of adoption by simply taking the inverse of the logit function of  $-\hat{X}_\beta$ . To get a sense of how important variables change the probability of adoption, we can calculate probabilities as we change one variable of interest at a time and hold all other covariates constant at their means, modes, or other values of interest. Figure 2 displays how changes in *majority size* affect the probability of adoption for the modal value of union strength (15.6% public sector union membership, or half of a standard deviation above its minimum). Here, we can a tangible sense of how majority parties, sensing electoral dominance, might be able to take advantage of a weak union presence to reverse merit protection. This empirical finding sits nicely as a complement to recently published work on civil service adoption (Ting et al., Forthcoming).

Table 3 also provides strong support for Hypothesis 3 regarding the effect of gubernatorial powers on radical civil service adoption. In both specifications, states with institutionally weak governors are more likely to see radical civil service reform than are states with powerful

governors. Holding the other continuous variables constant at their means and indicator variables at their modes, a transition from the least powerful governorship (North Carolina at 2.5) to the most powerful (Maryland at 4.3) induces a 20 fold decrease in the probability of adoption. This probability goes from 0.1 for the least powerful governor to less than 0.002 for the most. This is a meaningful change in the probability of adoption, especially since the adoption of radical civil service reform is a relatively rare event. We can also examine the interrelation of substantive effects that are easy to attain in a logit model. For example, the number of neighboring states that have already adopted radical reform serves to radically shift the probability curve up (with an increase in contiguous adopters) or down (with a decrease in contiguous adopters). The hypothetical situation where we compare the weakest governor state to the strongest becomes more dramatic if we assume they each have 3 neighboring states having already adopted (probabilities of adoption of 0.33 and .008, respectively).

A number of the control variables suggested above also attain statistical significance as determinants of radical civil service reform. Although divided government does not have the expected effect, unified legislatures appear to be more likely to enact reform than divided ones. Legislatures dominated by Democrats are less likely to adopt; but, controlling for this, states with citizens with more liberal public opinions are surprisingly more prone to this type of reform.<sup>10</sup> As expected, the number of economic interest groups also affects adoption across models, but state economic health—against the conventional wisdom—does not appear to drive radical civil service reform. Overall, the analyses presented in this section provide support for the idea presented in this paper that there are systematic political-institutional features of state government that can explain the rise—and eventual fall—of radical civil service reform across the states. This kind of quantitative and systematic approach is conspicuously absent in the literature on radical reform, but it has a history of illuminating (Ruhil and Camões, 2003; Ting et al., Forthcoming) the complementary process of merit protection in the civil service. I argue that the lessons we have learned

from this complementary literature can be fruitfully applied to decentralization as well as to centralization.

## Discussion

Students of politics know all too well that institutions of governance are consequential to political outcomes. This paper argues that the inhabitants of these institutions are consequential as well, and the evidence presented herein suggests that partisan majorities recognize this. Thus, radical civil service reform is a normatively important topic that has affected the governance and personnel systems of many states (Bowman and West, 2006; Kellough and Nigro, 2006; Shafritz and Hyde, 2007; Nigro and Kellough, 2008; Coggburn et al., 2010). Despite attracting much attention by scholars and practitioners alike, there have been too few attempts at a systematic understanding of why some states adopted these types of reforms where others did not. In addition, extant scholarship focusing on ideological determinants of adoption has been unable to explain the variation in adoption times and the apparent spatio-temporal diffusion of the reform policies. In contrast to the literature documenting the particular aspects of the reforms and their specific consequences on state policymaking, I have taken a much broader view of the diffusion process in this paper.

The results generated from this research should inform future work on the determinants of radical civil service reform—which is too consequential a reform for policy implementation and governance for us to lack a reasonable theory for its imposition and diffusion. Here, I have attempted to go beyond the simplistic argument that reforms occur when politicians deem them necessary for “good government.” Instead, there is considerable support for the effects of important political-institutional variables on the adoption of a particular kind of “radical” reform across the states—the significant decentralization of state HR systems. Based on their levels of statistical and substantive significance and their concordance with theoretical expectations developed herein, I have much confidence that political-institutional



factors have conditioned the diffusion of this kind of reform across the United States.

In sum, I argue that electorally dominant majorities appear to try to decentralize personnel systems (and thereby de-insulate governmental employees from political control). This is consistent with a general theoretical argument (see, e.g., de Figueiredo Jr., 2002) that political majorities see institutional structures as variably advantageous given their power position. Strong majorities prefer policy implementation to be responsive to their policy goals and favor reforms that make this responsiveness easier to attain (“spoils,” e.g.). On the other hand, weaker majorities, foreseeing future electoral loss, have been shown to prefer to insulate policymaking through reforms such as administrative procedures acts (McCubbins, Noll and Weingast (1999); de Figueiredo and Vanden Bergh (2004) and merit protection of civil servants (Ruhil and Camões, 2003; Ting et al., Forthcoming). Crucially, the ability of strong majorities to succeed in de-insulating policymaking is conditioned by the political strength (or lack thereof) of public sector unions across the U.S. states. In addition, the varying institutional powers of governors have influenced the diffusion of radical civil service reform as conceptualized in this paper. The interpretation I have offered is a strategic one. State legislatures seem to be more willing to decentralize personnel systems when they deem governors too weak to take institutional advantage. This interpretation has the advantage of simultaneously explaining the adoption of radical civil service reform, as well as the wane of this adoption. While my analyses only go through 2005, the pace and extent of diffusion has slowed considerably in recent years. An explanation based on the results herein might hold that there are just not many weak governor states left for adoption to spread to, conditional on the adoption being driven by other determinants as well.

This research sees what happened in Georgia in 1996 and spread—to varying degrees—to at least fourteen additional states as less a general “return to spoils” (Condrey and Battaglio, Jr., 2007) than an institutionally determined consequence of political actors behaving in political ways. The existing case-study literature focuses on the ideological undertones of personnel decentralization and establishes ideology perhaps as a pre-condition for radical

reform. Despite possessing many advantages, these research designs left scholars with very little idea about why states adopted radical reform when they did, or about why some states never adopted such reforms. The present research is admittedly lacking in specific information about particular reforms and does not examine their effects at all, but it does point to specific factors that have systematically made reform more or less likely. Taken in conjunction with the extant literature, we may now make out the proverbial forest as well as describe its component trees.

In terms of future research, this type of study would most likely benefit from more nuanced empirical modeling of the diffusion process. The data structure for this paper has been monadic, which allows for some nuanced tests of diffusion processes, but not others (Boehmke, 2009*b*). A dyadic data structure would make it easier to test mechanisms more complex than the simple contiguity variable allows in this context. For example, states may be more willing to learn (or emulate) from states that with similar ideological compositions, or some other non-geographic proximity (Shipan and Volden, 2008). In addition to this extension, an astute researcher could collect more nuanced information regarding the individual components of radical civil service reform. Where the dependent variable here is an indicator for whether or not states adopted reform in a certain year, there exists more detailed information about the types of provisions states adopted. Boehmke (2009*a*) describes pooling methods for analyzing the diffusion of multiple and distinct components of state policy in general, including radical civil service reform. Such a strategy would allow us to discern whether different political and institutional variables drive distinct aspects of civil service reform across the states. Finally, the approach taken in this paper can be extended to other types of structural reform, including even legislation regarding the structure and role of public sector unions in state governance in the first place.

## Notes

<sup>1</sup>Although there are additional components of “radical” HR reform (such as privatization and changes in the legal rights of employees), decentralization is a common theme among the case studies reviewed from the literature.

<sup>2</sup>This is, of course, a crude account of the details of these studies. The Pendleton Act was preceded by more than 30 years of interest group activity and the persistent efforts of individual political leaders, such as representatives Charles Sumner (R-MA) and Thomas A. Jenckes (R-RI), and George William Curtis of the New York Civil Service Reform League (Ruhil and Camões, 2003). In addition, Dorman B. Eaton, chair of the first Civil Service Commission established by President Grant in 1871, studied the British civil service and came back to write a book which beams the intellectual foundation of the legislative civil service reform movement (Eaton, 1880; Shafritz and Hyde, 2007).

<sup>3</sup>New York’s law passed in the same year as Pendleton and is considered by many to be the model for the federal act.

<sup>4</sup>Ting et al. (Forthcoming) use a more restrictive definition of what constitutes a general civil service reform act and determine that states did not necessarily comply with this federal mandate. They code West Virginia, for example, as not having passed a civil service act until 1989. Both coding schemes of course recognize that Texas has *never* passed a comprehensive civil service act.

<sup>5</sup>The coding is as follows: Georgia, 1996; South Carolina, 1997; Pennsylvania, 1998; Vermont, 1999; Iowa, 1999; Oklahoma, 1999; Arkansas, 2001; Missouri, 2001; North Dakota, 2001; Florida, 2001; Washington, 2002; Virginia, 2002; Kansas, 2003; Colorado, 2003; North Carolina, 2005.

<sup>6</sup>Nebraska has a unicameral legislature and therefore is excluded from the analyses below.

<sup>7</sup>Alternatively, I have used 8 year and 5 year moving averages, as well as an unlagged current value of the majority size variable— all with little substantive effect on the results

reported below.

<sup>8</sup><http://www.unc.edu/~beyle/gubnewpwr.html>

<sup>9</sup>In addition to the logit models presented here, I also specified parametric (exponential, Gompertz, and Weibull) and semi-parametric (Cox) event history models. For the most part, the results substantively comport with those presented below.

<sup>10</sup>I agree with an anonymous reviewer that this result might indicate the efficacy of decentralization as a reform strategy. Liberal citizenries presumably demand more services from state line agencies and if decentralization gives more discretion to these agencies, they might be better able to deliver these services effectively. At least this result indicates that liberal citizenries might reasonably expect this outcome, controlling for state partisan politics.

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Table 1: 2005 Snapshot of State Personnel Systems (from Hays and Sowa (2007, pp.8-9))

State	HR Decentralization	↑ At-Will Employees	Range of Grievable Issues	Activist Governor	“Decline in Job Security”
AL	Partial	No	Agency Specific	No	Yes
AK	Centralized	No	Restricted	Yes	No
AZ	Partial	Yes	Restricted	Yes	Yes
<b>AR</b>	<b>Significant</b>	Yes	Restricted/Agency Specific	No	Yes
CA	Partial	No	Expansive	Yes	Yes
<b>CO</b>	<b>Significant</b>	Yes	Restricted	Yes	Yes
CT	Partial	No	Expansive	No	No
DE	Partial	Yes	Expansive	No	No
<b>FL</b>	<b>Significant</b>	Yes	Restricted	Yes	Yes
<b>GA</b>	<b>Significant</b>	Yes	Restricted	No	Yes
HI	Centralized	No	Expansive	No	No
ID	Partial	Yes	Agency Specific	No	Yes
IL	Partial	No	Expansive	No	Yes
IN	Recentralizing	Yes	Restricted	Yes	Yes
<b>IA</b>	<b>Significant</b>	Yes	Expansive	No	No
<b>KS</b>	<b>Significant</b>	Yes	Expansive/Agency Specific	Yes	Yes
KY	Centralized	Yes	Expansive	Yes	No
LA	Partial	No	Restricted	No	Yes
ME	Recentralizing	No	Expansive	Yes	Yes
MD	Partial	No	Expansive	No	No
MA	Partial	Yes	Expansive	Yes	Yes
MI	Partial	No	Expansive	No	Yes
MN	Partial	No	Expansive	No	Yes
MS	Partial	Yes	Restricted	Yes	Yes
<b>MO</b>	<b>Significant</b>	Yes	Agency Specific	Yes	Yes
MT	Partial	No	Restricted	No	No
NE	Centralized	Yes	Restricted	No	Yes
NV	Partial	No	Expansive	No	No
NH	Partial	No	Expansive	No	No
NJ	Partial	Yes	Expansive	No	Yes
NM	Centralized	No	Expansive	No	No
NY	Partial	No	Expansive	No	No
<b>NC</b>	<b>Significant</b>	Yes	Restricted	No	Yes
<b>ND</b>	<b>Significant</b>	No	Restricted	No	No
OH	Partial	Yes	Restricted	No	No
<b>OK</b>	<b>Significant</b>	Yes	Restricted	No	Yes
OR	Partial	Yes	Expansive	Yes	Yes
<b>PA</b>	<b>Significant</b>	No	Expansive	No	No
RI	Centralized	Yes	Expansive but Not Utilized	Yes	Yes
<b>SC</b>	<b>Significant</b>	Yes	Restricted	Yes	Yes
SD	Centralized	No	Expansive	No	No
TN	Centralized	No	Restricted	No	No
TX	Complete	Yes	Not Applicable	No	Yes
UT	Partial	Yes	Expansive	Yes	No
<b>VT</b>	<b>Significant</b>	Yes	Restricted	Yes	Yes
<b>VA</b>	<b>Significant</b>	No	Restricted	No	Yes
<b>WA</b>	<b>Significant</b>	Yes	Restricted	Yes	Yes
WV	Partial	Yes	Restricted	Yes	Yes
WI	Partial	No	Expansive	Yes	Yes
WY	Partial	Yes	Restricted	No	No

Table 2: Descriptive Statistics

	count	mean	sd	min	max
Majority Size (10 yr Moving Avg)	449	0.26	0.077	0.044	0.68
Public Sector Union Membership	460	0.35	0.18	0.066	0.73
Governor's Institutional Powers Score	460	3.48	0.41	2.50	4.30
Divided Government	460	0.57	0.50	0	1
Unified Legislature	460	0.73	0.45	0	1
Citizen Ideology	460	47.6	15.1	8.45	86.5
% Dems in Legislature	449	0.51	0.16	0.11	0.89
# Economic IGs	460	485.8	261.4	154	1232
Budget Surplus as % of GSP	460	0.27	1.05	-6.96	9.78
# Contiguous Adopters	460	0.52	0.81	0	4

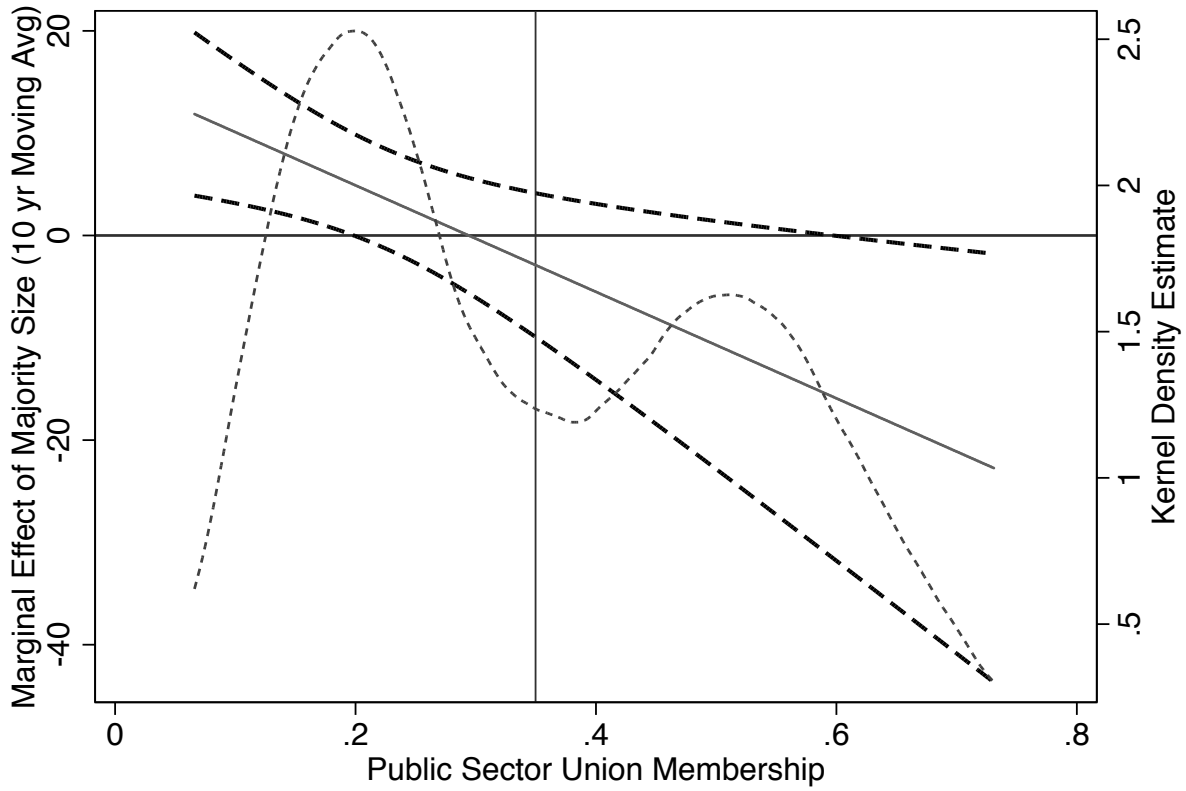
Table 3: Models of Radical Civil Service Adoption (S.E. clustered by state)

	Logit		BTSCS Logit	
adopt				
Majority Size (10 yr Moving Avg)	15.30**	(6.040)	13.38**	(6.459)
Public Sector Union Membership	5.483	(5.033)	4.108	(5.423)
Majority Size * Public Sector Union Membership	-52.04**	(23.75)	-45.37*	(25.64)
Governor's Institutional Powers Score	-2.062**	(0.801)	-2.097**	(0.870)
Divided Government	0.126	(0.873)	0.161	(0.917)
Unified Legislature	1.448*	(0.832)	1.297	(0.824)
% Dems in Legislature	-6.737**	(3.141)	-6.431**	(3.192)
Citizen Ideology	0.104**	(0.0525)	0.0964*	(0.0532)
LN(# Economic IGs)	1.247**	(0.616)	1.193*	(0.654)
Budget Surplus as % of GSP	-0.807	(0.625)	-0.626	(0.769)
# Contiguous Adopters	0.753**	(0.317)	0.644*	(0.365)
Duration			0.644	(0.678)
Duration Squared			-0.0486	(0.0510)
Constant	-9.104*	(4.901)	-9.606**	(4.659)
Observations	449		449	
Log-likelihood	-49.98		-49.27	
Chi <sup>2</sup>	51.10		63.55	
df	11		13	

Standard errors in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Figure 1: Marginal Effects on Radical Reform ( $Y = 1$ )



Thick dashed lines give 90% confidence interval.  
Thin dashed line is a kernel density estimate of Public Sector Union Membership.

Figure 2: Predicted Probabilities of Radical Reform ( $Y = 1$ )

Low Public Sector Union Membership (Minimum + 1/2 SD)

